Paris School of Economics –

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Evaluating the Non-Monetary Impacts of Major Events, Infrastructure, and Institutions

submitted by

Christian Krekel,

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List of Abbreviations

ATT .... Average Treatment Effects on the Treated
DID .... Difference-in-Difference
IIS .... Ipsos Interactive Services Panel
IOC .... International Olympic Committee
LPM .... Linear Probability Model
LSA .... Life Satisfaction Approach
PSM .... Propensity-Score Matching
SHP .... Swiss Household Panel
SOEP .... Socio-Economic Panel
SWB .... Subjective Wellbeing

Chapter 0

Introduction

0.1 Preliminary Remarks

Since the mid 2000s, there has been a growing interest amongst policy-makers to use indicators of subjective well-being in order to monitor social progress and evaluate particular policies and programmes. The Office for National Statistics in the UK is a leading example: after taking office in 2010, the Conservative and Liberal Democrat coalition government under Prime Minister David Cameron asked the national statistics agency to "devise a new way of measuring well-being in Britain... [to measure] progress as a country, not just by how our economy is growing, but by how our lives are improving" (Cameron, D. 2010). Following recommendations by Dolan and Metcalfe 2012, it now routinely asks people how they think and feel about their lives, including items on evaluative (life satisfaction), experience (happiness, anxiousness), and eudemonic (worthwhileness) measures of subjective well-being in its surveys. Similar recommendations were made by the *Commission on the Measurement of Economic Performance and Social Progress* in France, advocated by President Nicolas Sarkozy and chaired by Joseph Stiglitz, Amartya Sen, and Jean Paul Fitoussi (Stiglitz et al. 2009). Initiatives at the international level include the OECD's *Better Life Initiative* and its work programme on *Measuring Well-Being and Progress*, as well as the UN's *World Happiness Report* edited by John Helliwell, Richard Layard, and Jeffrey Sachs. These initiatives are backed up by an ever growing body of empirical evidence on how to measure subjective well-being, its correlates, and some of its causes and consequences (Dolan et al. 2008).

The rationale behind this development is that "traditional" indicators of social progress such as per capita income only insufficiently capture phenomena like environmental degradation or income inequality: for the former, there exist no market prices; to the latter, aggregate or per-capita indicators are invariant.[1] Both phenomena, however, are clearly detrimental to quality of life (Burkhauser et al. 2016; Rehdanz and Maddison 2008). At the same time, using traditional indicators to evaluate particular policies and programmes is inaccurate in case that individuals adapt to circumstances (Clark, Diener, et al. 2008; Clark et al. 2016) or make relative comparisons (Clark, Frijters, et al. 2008; Clark and Senik 2010; Senik 2009). In such situations, evaluations based on income alone may be misleading, as welfare implications of policies and programmes may differ between the short-run and the long-run, or policies and programmes may turn out to be zero-sum welfare games altogether. This can then yield inaccurate predictions about the distribution of welfare as well as inaccurate predictions about behaviour. Measures based on subjective well-being, on the contrary, may capture a wide array of non-monetary impacts, and in doing so, may fruitfully complement traditional indicators. Clearly, for a complete account of the costs and benefits of particular policies and programmes, both their monetary and their non-monetary impacts need to be taken into account. An ever growing body of literature is therefore using measures of subjective well-being for policy and programme evaluation.

In this dissertation, I contribute to this literature by evaluating the non-monetary impacts of major events (the Fukushima Daiichi meltdown, the Olympic Games), infrastructure (urban land use, wind turbines), and institutions (instructional time) on subjective well-being (in particular life satisfaction), attitudes, and behaviours. A central theme present in all chapters of this dissertation is the use of geo-referenced panel data, either standalone by making use of the geographical coordinates of households within the primary data (the German Socio-Economic Panel in most cases) or by matching the primary with secondary data through geographical coordinates, thereby calculating either the distances between households and infrastructure or the prevalence of infrastructure in buffers around households. Special attention is paid to identifying causal effects using recent methods in applied microeconometrics, in particular difference-in-differences models, partly combined with propensity-score and spatial matching techniques.

For the purpose of this dissertation, and in accordance with Diener et al. (1999), we define *life satisfaction* (an evaluative measure of subjective well-being) as *cognitive evaluations of the*

1. I coin these indicators "traditional", although they have only been gathered in a reliable manner during the last 60 years or so. Simon Kuznets, founder of the modern GDP concept, argued as early as 1934: "The welfare of a nation can scarcely be inferred from a measurement of national income" (Kuznets, S. 1934).

circumstances in life.[2] The literature is not entirely settled on whether life satisfaction is equal to utility or merely one component in an individual's utility function, besides others such as income (Becker et al. 2008; Benjamin et al. 2012). It has been shown that individuals do not necessarily make choices that are consistent with life satisfaction maximisation, for example when making locational decisions (Glaeser et al. 2016), be it consciously or unconsciously due to prediction errors, especially in intertemporal contexts (Odermatt and Stutzer 2015). An extensive treatment of the validity of subjective well-being indicators for policy and programme evaluation is beyond the scope of this dissertation; rather, in this dissertation, I am using life satisfaction and other subjective well-being indicators as a vehicle to measure the non-monetary impacts of major events, infrastructure, and institutions, for which I only require them to be valid approximations of individual welfare. Adler et al. (2015), using a large population survey, show that people, by and large, tend to make life choices that score high on life satisfaction.

In what follows, I am walking the reader through the different chapters of this dissertation. In doing so, I am giving a short background and motivation for each chapter, elaborate the research questions, and provide an overview of the data and methods used to answer it. Finally, I am giving a brief outline of the results, and highlight the specific contribution of each chapter to the literature.

0.2 A Walk Through the Chapters

0.2.1 Chapter 1: The Fukushima Daiichi Meltdown

On March 11, 2011, one of the worst accidents in the history of the civil use of nuclear energy happened: a natural disaster triggered the Fukushima Daiichi meltdown. While physical damages were mainly limited to Japan and its surrounding sea areas, the meltdown triggered political action in a country more than 5,000 miles distant: Germany. In response to the catastrophe, the conservative government of Chancellor Angela Merkel made a sharp U-turn in its energy policy, shutting down the eight oldest reactors and taking back the lifetime extension of the remainder that it had just granted one year earlier.

In Chapter 1, we evaluate the impact of the Fukushima Daiichi meltdown on environmental concerns, subjective well-being, risk aversion, and political preferences in Germany, and compare

2. Besides evaluative measures, there are also experience measures of subjective well-being such as happiness or anxiousness as well as eudemonic measures such as worthwhileness of things in life, which we will also selectively be looking at.

them to those in Switzerland and the UK, where no policy action occurred.[3] To do so, we use data from the German Socio-Economic Panel (years 2010 and 2011), the Swiss Household Panel (years 2009 to 2012), and Understanding Society (years 2010 to 2012). We estimate difference-in-differences models, exploiting the exact dates of the catastrophe, and in case of Germany, of the policy action as cut-offs to allocate individuals into either the treatment or control group. Our identification strategy rests on the assumption that the dates of the events are exogenous to the interview dates of the respondents. We additionally stratify the identified effects by geographical distances between the respondents' places of residence and the nearest nuclear power plant, using geographical coordinates of households and reactors, respectively.

We do not find much evidence that subjective well-being was significantly affected in Germany, Switzerland, or the UK. However, we find that environmental concerns significantly increased among Germans. Moreover, the share of Germans who consider themselves as "very risk averse" increased significantly, in particular among people who live in close proximity to nuclear power plants or for whom the next reactor belongs to one of the eight oldest. Likewise, support for the Greens – a party that traditionally opposes nuclear power and advocates its abolition – increased significantly in all three countries. Finally, the announcement and (partial) implementation of the exit from nuclear power in Germany led to a decrease in environmental concerns there, approximately by the same size that they had increased after the catastrophe. The results are insensitive to interview types, moving behaviour, and time trends. Importantly, they withstand a series of placebo tests, including placebo policy dates, time periods, and outcomes. Both a comparative media analysis and, using the same data and empirical model, a case study that directly compares the disaster effect of Chernobyl with that of Fukushima and that includes a placebo policy action after Chernobyl suggest that the identified policy effect after Fukushima is not merely driven by a decrease in media attention.

These findings are interesting for several reasons: first, they are interesting because they show that disasters do not only have negative effects locally, but can also impose negative external effects on distant countries, even if those countries are far away and presumably not directly affected. Second, these negative external effects exist even in case that the objective risk of a similar disaster does not change, pointing towards the importance of subjective risk perceptions or individual risk tolerance when assessing similar situations. Finally, policy action, if credible and implemented swiftly, can alleviate or even reverse concerns in the population.

3. This chapter is also available as the following journal article: Goebel, J., C. Krekel, T. Tiefenbach, and N. R. Ziebarth, "How Natural Disasters Can Affect Environmental Concerns, Risk Aversion, and Even Politics: Evidence from Fukushima and Three European Countries," *Journal of Population Economics*, 28(4), 1137–1180, 2015.

0.2.2 Chapter 2: Urban Land Use

In Chapter 2, we look at something different: cities and urban infrastructure.[4] In major cities, space is a scarce commodity, and urbanisation puts increasing pressure on areas that provide important ecosystem services. Acknowledging that urban areas such as parks and green spaces contribute to their climate and environmental policy objectives, many supranational, national, and regional policies are put in place to promote their preservation.[5] This is backed up by a growing body of empirical evidence that documents the important role of urban green areas for residential well-being and health (see Bell et al. 2008 and Croucher et al. 2008 for reviews). In contrast to this evidence stand studies that show the disamenity value of vacant land or abandoned areas in inner cities (Bixler and Floyd 1997; Branas et al. 2011; Kuo et al. 1998).

We evaluate the impact of urban land use on residential well-being in major German cities and value different types of urban land use monetarily using the so-called *life satisfaction approach* (Kopmann and Rehdanz 2013; Luechinger and Raschky 2009; Rehdanz and Maddison 2008), which trades off the effect of urban land use on life satisfaction with that of income. To do so, we merge panel data from the German Socio-Economic Panel (years 2000 to 2012) with cross-section data from the European Urban Atlas (year 2006), and calculate both geographical distances between households and different types of urban land use as well as shares of different types of urban land use in buffers around households. Since our combined dataset includes several waves of data on residential well-being, but only one on urban land use, we cannot clearly identify causal effects. Instead, we use fixed-effects regressions with both individual and city fixed effects to have the effects identified by movers, who we can show to move mostly for reasons unrelated to their surroundings. Robustness checks excluding city fixed effects, excluding movers to have the effects identified by stayers (in a plain ordinary-least-squares framework), or regressing the likelihood to move on different types of urban land use suggest that endogeneity due to simultaneity plays, if any, only a minor role.

We find that access to urban green areas is positively associated with life satisfaction, whereas access to abandoned areas is negatively associated with it. In contrast, access to forests and waters do not seem to matter much for residential well-being. The relationships are concave in nature, and in terms of effect heterogeneity, small effects at the aggregate level hide much larger effects for older residents. We finish the chapter with a small policy case study in which we contrast the willingness-to-pay of residents in order to increase the number

4. This chapter is also available as the following journal article: Krekel, C., J. Kolbe, and H. Wuestemann, "The Greener, The Happier? The Effect of Urban Land Use on Residential Well-Being," *Ecological Economics*, 121, 117–127, 2016.

5. See European Commission 2013 and Federal Ministry for the Environment, Nature Conservation, Building, and Nuclear Safety 2007 for recent initiatives by the European Union and Germany.

of parks in their surroundings with the cost of doing so, and we show – in a plain partial-equilibrium framework – that there is a substantial net well-being benefit arising from reducing the undersupply of parks in major German cities.

Although the effects of urban land use on residential well-being have been studied before, we contribute to the literature in several important ways: first, our empirical strategy brings us closer to identifying causal effects. Second, we use data on urban land use rather than cover, and data on actual usage is less prone to measurement error and, presumably, much more consistent in terms of provision of utility, as this type of data adds a second stage of verification, namely a check by local authorities that, for example, what is classified through satellite imagery as a park is actually used as one. Moreover, our data allow jointly estimating the effects of different types of urban land use on residential well-being. Finally, merging both datasets through geographical coordinates allows calculating exact distances and coverages, and measuring access based on exact distances and coverages is more precise than measuring access based on, for example, aggregated areas.

0.2.3 Chapter 3: Wind Turbines

We look at energy infrastructure next. Since the 1990s, there has been a world-wide trend towards renewable resources for electricity generation, with wind power being at the forefront of this development. The economic rationale behind its deployment is to avoid negative externalities associated with conventional technologies, most notably CO2 emissions. Wind turbines, however, are not free of externalities themselves, particularly interference with landscape aesthetics (Devine-Wright 2005; Jobert et al. 2007; Wolsink 2007). As with the amenity value of parks, typically, no market prices exist for these externalities, so that they have to be quantified using other methods, including, for example, stated (Groothuis et al. 2008; Jones and Eiser 2010; Meyerhoff et al. 2010) or revealed preference approaches (Gibbons 2015; Heintzelman and Tuttle 2012).

In Chapter 3, we evaluate the impact of wind turbines on residential well-being and value their negative externalities monetarily using, once again, the life satisfaction approach.[6] To do so, we merge data from the German Socio-Economic Panel (years 2000 to 2012) with a unique and novel dataset on more than 20,000 installations in Germany. As the combined dataset includes the geographical coordinates of both households and wind turbines as well as interview and construction dates, we employ a difference-in-differences design in which individuals are

6. This chapter is also available as the following journal article: Krekel, C., and A. Zerrahn, "Does the Presence of Wind Turbines Have Negative Externalities for People in Their Surroundings? Evidence from Well-Being Data," *Journal of Environmental Economics and Management*, 82, 221–238, 2017.

allocated to the treatment group in the interview year in which a wind turbine is present in a pre-defined radius around their households, and to the control group otherwise. Our identification strategy rests on the assumptions that *(i)* assignment to treatment is independent of outcome conditional on both observables and unobservables (*conditional ignorability*), and that *(ii)* treatment and control group would have followed a common time trend in the absence of treatment (*common trend assumption*). We try to make sure that the former holds by focussing only on large installations that are typically built by utilities rather than private persons. To make sure that the latter holds, we apply propensity-score and spatial matching techniques based on exogenous weather data to match treatment and control group prior to running our difference-in-differences regressions.

We find that the construction of wind turbines in a pre-defined radius of 4,000 metres around households has a significant and sizeable negative effect on life satisfaction. There is only weak evidence for distance and cumulation effects in addition to the main effect: it seems that the presence of a single wind turbine in the surroundings of households is sufficient to trigger substantial negative external effects. These seem to be both spatially and temporally limited (although the latter may arise as a statistical artefact due to lack of power), and greater for house owners than for renters; in fact, for the latter they turn out to be insignificant altogether. To dig deeper into this issue, we conduct a complementary hedonic analysis and find that renters are more swiftly compensated through a reduction in real estate prices; for house owners, however, this channel does not operate. Interestingly, the size of the negative externalities, when quantified monetarily, is similar between house owners and renters. Robustness checks including placebo regressions and view shed analyses based on digital terrain models as well as sensitivity analyses that evaluate the extent to which endogenous residential sorting affects our estimates confirm our findings.

To the best of our knowledge, this is the first study that evaluates the impact of the construction of wind turbines on the subjective well-being of individuals in their surroundings in a formal treatment effect analysis framework. The methodology developed in this study can be easily transferred to evaluating the impact of various other infrastructure projects. Finally, the finding that the life satisfaction approach and the hedonic method complement each other in the given context is a methodological contribution to the existing literature, in particular Luechinger 2009.

0.2.4 Chapter 4: The Olympic Games

Chapter 4 deals with a major sports event: the 2012 Olympic Summer Games in London.[7] Given the public interest in the Olympics and the large public subsidies they typically require (the 2012 Olympic Summer Games in London required government subsidies of more than $15 billion alone to cover direct costs), a significant body of empirical literature seeks to evaluate their economic impact (see Baade and Matheson 2016 for a review).[8] The majority of these studies find little or no multiplier effect of hosting the Olympics on investment and tourism, and thus little evidence of any tangible long-term economic impact. In fact, this has led proponents to argue that one of the main reasons for hosting the Olympics is its intangible and potentially long-lasting impact on the well-being of residents in the host city (Department for Culture, Media & Sport 2013).

In Chapter 4, we therefore ask: do the Olympics make people in the host city happier? To shed light on this question, we collected panel data on the subjective well-being of more than 26,000 Londoners, Parisians, and Berliners in the summers of 2011, 2012, and 2013 in order to evaluate the impact of the 2012 Olympic Summer Games in London on four outcomes: life satisfaction, happiness, anxiousness, and worthwhileness. We estimate difference-in-differences models in which individuals are allocated to the treatment group if they live in London, and to the control group otherwise. Our identification strategy rests on the assumption that the subjective well-being of Londoners would have followed the same time trend as that of Parisians and Berliners in the absence of the Olympics. Although this *common trend assumption* is not formally testable, we show that all three cities followed a similar time trend in subjective well-being in the year 2011, one year prior to the Olympics, albeit on different levels (which is no threat to identification).

We find that the Olympics have a significant and sizeable positive effect on life satisfaction, and in most specifications, on happiness as well. It is only short-lived, though, vanishing after one year at the latest. We find no evidence that the identified effect is driven by relative sporting success: rather, it seems that hosting itself matters for well-being. This finding remains robust regardless of whether we employ *(i)* a difference-in-differences design that uses time periods within the year 2012 only, *(ii)* a difference-in-differences design that uses the years 2011 and 2012 to net out unobserved heterogeneity at the individual level, or *(iii)* a difference-in-differences design that splits the year 2012 into three time periods – before, during, and after

7. This chapter is also available as the following discussion paper: Dolan, P. H., G. Kavetsos, C. Krekel, D. Mavridis, R. Metcalfe, C. Senik, S. Szymanski, and N. R. Ziebarth, "The Host with the Most? The Effects of the Olympic Games on Happiness," *CEP Discussion Paper*, 1441, 2016.

8. See National Audit Office 2012 for a detailed overview of the costs of the 2012 Olympic Summer Games in London.

the Olympics – and then interacts these with the time-invariant treatment dummy, respectively. It also remains robust in case that only Berliners are chosen as the control group (as Paris itself was bidding to host the event), or alternatively, in case that both Berliners and Parisians are chosen as separate control groups. Further robustness checks, including the use of a balanced panel, inverse probability weighting, and propensity-score matching to account for selection into the follow-up survey; the use of additional economic and exogenous weather controls to account for confounding factors that could induce divergent time trends; and the use of placebo regressions with both placebo outcomes and placebo time periods confirm our findings.

To the best of our knowledge, this is the first study that evaluates the impact of the Olympics on subjective well-being in a formal treatment effect analysis framework. As with the previous chapter, the methodology developed in this study can be easily transferred to evaluating the impact of various other major events. A possible extension might be the use of synthetic control methods to create credible control groups.

0.2.5 Chapter 5: Instructional Time

In the last chapter, we study how educational institutions shape student behaviours.[9] A growing body of empirical literature documents the importance of instructional time for student learning and performance, whereby raising instructional time is often found to have positive effects on cognitive skills (Bellei 2009; Cortes and Goodman 2014; Taylor 2014) and standardised test scores (Andrietti 2016; Cattaneo et al. 2016; Huebener et al. 2016). It therefore features high on the policy agenda in many countries, despite being a relatively costly input into the educational production function (Organisation for Economic Co-operation and Development 2016). Yet, outcomes other than student learning and achievement have scarcely been studied, and particularly little is known about how raising instructional time affects student leisure activities and behaviours (Patall et al. 2010).

We ask: can raising instructional time crowd out student pro-social behaviour such as volunteering? To shed light on this question, we evaluate the impact of a large educational reform in Germany that has raised instructional time for high school students as a quasi-natural experiment. Starting with school cohorts in the early 2000s, the number of years required to obtain the university entrance qualification has been reduced from 13 to 12. The taught curriculum, however, has remained the same, leading to a substantial rise in weekly instructional hours. At the same time, this feature allows isolating the "pure" effect of raising

9. This chapter is also available as the following discussion paper: Krekel, C., "Can Raising Instructional Time Crowd Out Student Pro-Social Behaviour? Evidence from Germany," *SOEPpapers on Multidisciplinary Panel Data Research*, 903, 2017.

instructional time on student pro-social behaviour, excluding potentially confounding changes to the educational system that are typically accompanied by similar reforms. We employ difference-in-differences models that exploit variation in the implementation of the reform across federal states and school cohorts to estimate causal effects. Graphical evidence supports that students in treatment and control group followed a common time trend in pro-social behaviour prior to the reform.

Using data on youth and adolescents aged 17 to 20 from the German Socio-Economic Panel (years 2000 to 2014), we find that the rise in weekly instructional hours has a significant and sizeable negative effect on volunteering, leading almost every fifth student to change her behaviour from volunteering at least once a month to volunteering less often or not at all. This change is primarily driven by students that volunteer weekly, and it affects both the intensive and the extensive margin of volunteering: while half of students cut back on their activities, the other half give them up completely. Students with lower-educated parents are particularly likely to reduce their engagement. We find no similar crowding out of scholastic involvement, but no substitution either. The rise in weekly instructional hours also affects political interest. Results are robust to time trends and seasonal variation; selection (both within and between schools through moving) and implementation; and potentially confounding other reforms that are implemented during the observation period. They also withstand a series of placebo tests.

These findings are significant for several reasons: first, they are significant because of the sheer number of students affected by this and similar reforms that make changes to instructional time. Second, they are significant because of the macroeconomic value of volunteering (Organisation for Economic Co-operation and Development 2015). Likewise, pro-social behaviour, and in particular volunteering, is beneficial for both individuals and society at large (Putnam 2000; Wilson and Musick 2012). Finally, to the extent that students from disadvantaged backgrounds are disproportionally affected, the decrease in volunteering for these groups might further increase educational inequalities, and thus inequalities in later life outcomes.

The impact of raising instructional time on student pro-social behaviour has not been studied so far. For a more complete cost-benefit account of raising instructional time, however, its impact on student leisure activities and behaviours, in particular on beneficial ones, should be taken into account.

0.3 Brief Overview

Table 0.1 summarises the dissertation: it shows the titles of the different chapters along with their co-authors, provides information on data and identification strategies, and finally, presents the main findings. It also gives on overview about the publication status of the different chapters.[10]

10. The chapters in this dissertation might differ slightly from their published versions.

Table 0.1: Overview

	Chapter 1	Chapter 2	Chapter 3	Chapter 4	Chapter 5
Publication Title	How Natural Disasters Can Affect Environmental Concerns, Risk Aversion, and Even Politics: Evidence from Fukushima and Three European Countries	The Greener, The Happier? The Effect of Urban Land Use on Residential Well-Being	Does the Presence of Wind Turbines Have Negative Externalities for People in Their Surroundings? Evidence from Well-Being Data	The Host with the Most? The Effects of the Olympic Games on Happiness	Can Raising Instructional Time Crowd Out Student Pro-Social Behaviour? Evidence from Germany
Co-Author(s)	Jan Goebel, Tim Tiefenbach, Nicolas R. Ziebarth	Jens Kolbe, Henry Wuestemann	Alexander Zerrahn	Paul H. Dolan, Georgios Kavetsos, Dimitris Mavridis, Robert Metcalfe, Claudia Senik, Stefan Szymanski, Nicolas R. Ziebarth	–
Primary Data	German Socio-Economic Panel, Swiss Household Panel, Understanding Society	German Socio-Economic Panel	German Socio-Economic Panel	Self-Collected Data on Residential Well-Being from IPSOS Mori	German-Socio-Economic Panel
Secondary Data	–	European Urban Atlas	Self-Collected Data on Wind Turbines from Federal Environmental Agencies in Germany	–	–
Time Period	Mostly 2010 to 2011	2000 to 2012	2000 to 2012	2011 to 2013	2000 to 2014
Data Structure	Panel	Panel	Panel	Panel	Panel
Identification Strategy	Difference-in-Differences Design	Fixed-Effects Regressions	Difference-in-Differences Design, Combined With Propensity-Score and Spatial Matching Techniques	Difference-in-Differences Design	Difference-in-Differences Design
Main Results	Large positive effect on environmental concerns; no effect on subjective well-being; moderate positive effect on being very risk averse; increased support for Green party	Large positive effects of urban green areas, and large negative effects of abandoned areas or vacant land on life satisfaction	Large negative effect on life satisfaction, with some evidence for non-persistence	Large positive effect on life satisfaction and happiness, but short-lived	Large negative effect on student volunteering; no effect on scholastic involvement; small effect on political interest
Own Contribution	Idea; empirical analysis for Germany (excluding processing of geographical data), including robustness checks; joint write-up	Idea; empirical analysis (excluding processing of geographical data), including robustness checks; most write-up	Idea; empirical analysis (excluding processing of geographical data and sampling of data on installations), including robustness checks; joint write-up	Empirical analysis, including robustness checks; joint write-up	-

Continued on next page

Continued from previous page

	Chapter 1	Chapter 2	Chapter 3	Chapter 4	Chapter 5
Discussion Paper	*Obsolete*	*Obsolete*	*Obsolete*	Dolan, P. H., G. Kavetsos, C. Krekel, D. Mavridis, R. Metcalfe, C. Senik, S. Szymanski, and N. R. Ziebarth, "The Host with the Most? The Effects of the Olympic Games on Happiness," *CEP Discussion Paper*, 1441, 2016.	Krekel, C., "Can Raising Instructional Time Crowd Out Student Pro-Social Behaviour? Evidence from Germany," *SOEPpapers on Multidisciplinary Panel Data Research*, 903, 2017.
Journal Article	Goebel, J., C. Krekel, T. Tiefenbach, and N. R. Ziebarth, "How Natural Disasters Can Affect Environmental Concerns, Risk Aversion, and Even Politics: Evidence from Fukushima and Three European Countries," *Journal of Population Economics*, 28(4), 1137–1180, 2015.	Krekel, C., J. Kolbe, and H. Wuestemann, "The Greener, The Happier? The Effect of Urban Land Use on Residential Well-Being," *Ecological Economics*, 121, 117–127, 2016.	Krekel, C., and A. Zerrahn, "Does the Presence of Wind Turbines Have Negative Externalities for People in Their Surroundings? Evidence from Well-Being Data," *Journal of Environmental Economics and Management*, 82, 221–238, 2017.	*Submitted*	–

Chapter 1

The Fukushima Daiichi Meltdown

Abstract

We study the impact of the Fukushima disaster on environmental concerns, well-being, risk aversion, and political preferences in Germany, Switzerland, and the UK. In these countries, overall life satisfaction did not significantly decrease, but the disaster significantly increased environmental concerns among Germans. One underlying mechanism likely operated through the perceived risk of a similar meltdown of domestic reactors. After Fukushima, more Germans considered themselves as "very risk averse." However, drastic German policy action shut down the oldest reactors, implemented the phaseout of the remaining ones, and proclaimed the transition to renewables. This shift in energy policy contributed to the subsequent decrease in environmental concerns, particularly among women, Green party supporters, and people living in close distance to the oldest reactors. In Germany, political support for the Greens increased significantly, whereas in Switzerland and the UK, this increase was limited to people living close to reactors.[*]

∗. This chapter is also available as the following journal article: Goebel, J., C. Krekel, T. Tiefenbach, and N. R. Ziebarth, "How Natural Disasters Can Affect Environmental Concerns, Risk Aversion, and Even Politics: Evidence from Fukushima and Three European Countries," *Journal of Population Economics*, 28(4), 1137–1180, 2015.

> Fukushima taught us that we have to deal with risks differently.
>
> Angela Merkel during the press conference announcing the nuclear phaseout on May 30, 2015.

> I conducted a re-assessment of [the remaining risks of] nuclear energy.
>
> Angela Merkel in her State-of-the-Union Address on June 9, 2011

1.1 Introduction

On March 11, 2011, a worst-case scenario in the history of the civil use of nuclear energy occurred: a natural disaster triggered the Fukushima Daiichi nuclear accident. At about 3 pm JST, the Tohoku earthquake, magnitude 9.0, struck off the east coast of Japan at an underwater depth of about 30 km (19 mi). It was the strongest earthquake to hit Japan since record-keeping began, triggering a gigantic tsunami with waves up to 40 m (133 ft). The tsunami's dimensions by far exceeded the safety measures of the Fukushima Daiichi nuclear power plant, whose 5.7 m (19 ft) sea walls were easily topped by up to 15 m (49 ft) high waves hitting the plant. Although the safety measures met regulatory requirements, in total, three of the six reactors fully melted down, leading to a major release of radioactive material into the environment.

In the subsequent days, the dimensions of the catastrophe became apparent. Within 2 days, nearly 200,000 people were evacuated, an estimated 4.5 million were without electricity, and 1.5 million without water. In September 2011, the Japanese Policy Agency concluded that the entire disaster, inclusive of the earthquake, tsunami, and meltdown, resulted in 16,000 deaths, thousands of injured or missing people, and 400,000 collapsed or partially collapsed buildings (Institute of Nuclear Power Operations 2011). However, no short-term physical health damages from radiation were observed as of 2013 (World Health Organisation 2013).

While physical damages were mainly limited to Japan and its surrounding sea areas, the disaster triggered political action in a country more than 5000 miles distant: Germany. In response to the meltdown, the conservative government of Chancellor Angela Merkel made a sharp U-turn in its energy policy. In consensus with the liberal opposition, immediately after the disaster, the oldest reactors in Germany were temporarily shut down (*Atommoratorium*) – despite their reputation of being among the safest in the world and despite the marginal tsunami risk in Germany. On May 30, 2011, the government announced the *Nuclear Phase-Out Bill* (*Atomausstieg*) that would permanently shut down these oldest reactors. Moreover, the bill implemented the staggered phaseout of the remaining reactors and will lead to a complete shutdown of all nuclear power plants in Germany by 2022. It represents a direct and immediate response to the unexpected and exogenous Fukushima catastrophe. Figure 1.1 shows the timeline of the policy events following the disaster.

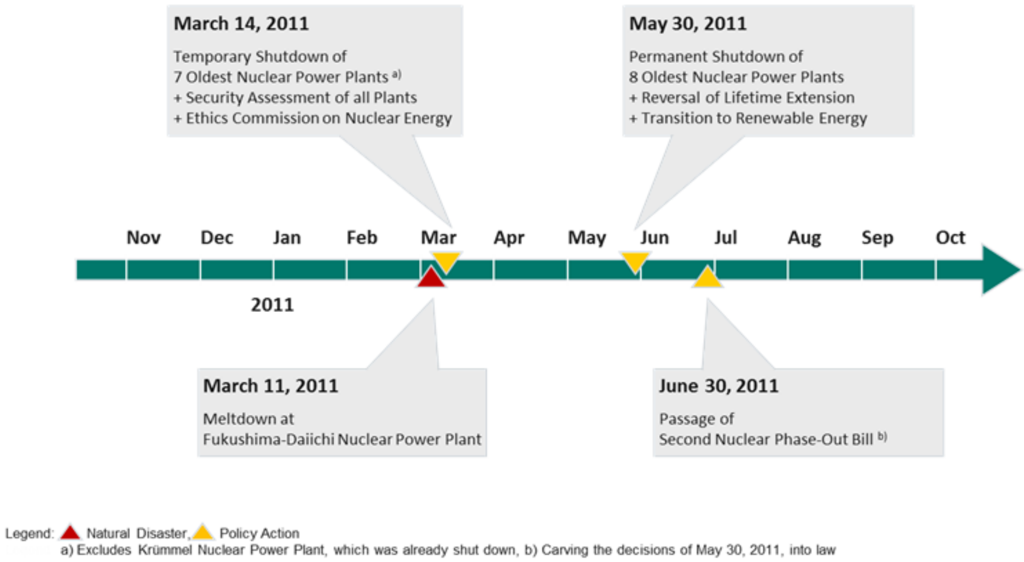


Figure 1.1: Timeline of Policy Action Following Fukushima

This paper estimates the impact of the Fukushima catastrophe on environmental concerns, well-being, risk aversion, and political preferences in three European countries: Germany, Switzerland, and the UK. We exploit the timing of the disaster that is exogenous to the survey interview dates. In addition, we stratify the estimated effects by the geographic distances between respondents' residencies and the nearest nuclear power plant. Our findings show the following:

First, while we do not find much evidence that subjective well-being was significantly affected in Germany, Switzerland, or the UK, we do find that environmental concerns significantly increased among Germans. These findings demonstrate that disasters do not only have negative local effects but also impose negative externalities on distant countries, even if those countries are presumably not directly affected. For Germany, similar effects have been reported by Berger (2010) after the Chernobyl disaster and by Richter et al. (2013) after the Fukushima disaster. However, we enrich the existing literature by (i) netting out individual unobserved heterogeneity, (ii) stratifying the effects by the distances to nuclear power plants, and (iii) providing empirical evidence on the operating channels, with a particular focus on risk aversion. In addition, we analyze how political preferences changed in (vi) three European countries that differ in their energy policies and their policy responses. Finally, we (vii) provide a battery of robustness checks to verify the validity of the results.

Second, while the objective risk of a similar disaster did not change, subjective risk perceptions or individual risk tolerance may have changed. Indeed, we find that the share of Germans who considered themselves as "very risk averse" increased significantly after Fukushima, in particular among people who lived in close proximity to reactors. This suggests that the perception of local risk factors may outweigh potential risk factors further away in other countries.

Third, after Fukushima, support for the Green party significantly increased in all three European countries, again most strongly among people who lived in close proximity to reactors. Phasing out of nuclear energy has been one of the core political objectives of the Green party since the 1980s. Party representatives have always warned about the risks of nuclear disasters, which were called "zero-risk events" by others.

Fourth, we find that immediate policy action can alleviate, or even reverse, concerns in the population. A representative survey conducted on March 14, 2011 found that 70% of all Germans believed that a nuclear catastrophe similar to Fukushima could also happen in Germany. Accordingly, 71% were in favor of a complete phaseout of nuclear energy, up from 62% in August 2010 (Infratest Dimap 2010, 2011a, 2011b). In line with these survey data, SOEP data show that after the announcement and implementation of the *Atomausstieg*, environmental concerns significantly decreased again – by approximately the same share that they had increased after Fukushima. Again, a representative survey conducted in June 2011 underlines these findings, showing that 54% of all Germans support the complete phaseout of nuclear energy and also the quick political decision process (Infratest Dimap 2011c).

While this study exploits household panel data and provides empirical evidence for three European countries, the focus is on Germany for two reasons: The first, technical reason is that we can use the German Socio-Economic Panel (Socio-Economic Panel 2013), which is a representative long-running panel study with a rich set of concern, well-being, risk, and political preference questions. As such, we are able to net out individual unobserved heterogeneity in our Difference-in-Difference (DID) models. Specifically, we exploit the exact interview date and the fact that about 50% of all SOEP respondents were surveyed before and after the Fukushima disaster in 2011. Established in 1984, the SOEP also allows us to directly compare the impact of the Fukushima and Chernobyl disasters using the same data, models, and variables as well as the same institutional and cultural setting. In particular, it allows us to conduct a falsification test by inserting a placebo policy after the Chernobyl disaster, using the same time span as elapsed between the Fukushima disaster and the real policy action (*Atomausstieg*) taken thereafter. This lends credibility to the identified policy effect after the Fukushima disaster, and in particular, provides evidence against the claim that this effect is driven merely by a decrease in media attention or a return to baseline after the catastrophe.

The second, institutional reason is that the German *Atomausstieg* as a reaction to Fukushima was worldwide unique. For decades, debates about nuclear energy, especially its risks, costs, and benefits, have been an integral part of the political debate in Germany. A complete nuclear phaseout has always been one of the key policy objectives of the Greens who have been part of the German parliament since 1982.

The findings of this paper are of general interest and not limited to Germany. As in Germany, there have been debates and referenda about nuclear energy in various industrialized countries. Massive protests prevented the *Carnsore Point* nuclear power plant in Ireland from starting operations in the 1970s. Austria decided to mothball the already finished *Zwentendorf* nuclear power plant after a negative referendum in 1978. As a reaction to the 1986 Chernobyl disaster, the Philippines decided against operating a new nuclear power plant, while Italy shut down its four operating ones. Currently, there exist 435 nuclear power plants in 30 countries; half of them are located in the USA where nuclear energy is less controversial than in Europe. New reactors are planned in 21 of these 30 countries. Eleven countries that currently do not operate nuclear power plants plan to use nuclear energy in the future, including Turkey, Poland, Indonesia, and Vietnam. On the contrary, in addition to Germany, nuclear energy is being phased out in Switzerland, Belgium, and Spain. Thus, although population attitudes and cultures differ across countries, the German example is informative for a wider set of countries.

1.2 Literature Review

This paper contributes to several strands in the economic literature. First, it adds to the literature on well-being (e.g. Benjamin, Heffetz, Kimball, and Szembrot (2014), Benjamin, Heffetz, Kimball, and Rees-Jones (2014), and Bond and Lang (2014)). Events studied in this field include economic growth (Oswald 1997; Oswald and Wu 2011; Deaton 2012), unemployment (Winkelmann and Winkelmann 1998; Kassenboehmer and Haisken-DeNew 2009; Knabe et al. 2010; Luechinger et al. 2010; Marcus 2013), (relative) income (Frijters et al. 2004; Senik 2009; Clark et al. 2009; Clark and Senik 2010), or pollution (Luechinger 2009).

Second, the paper adds to the small, but growing, field on the effects of natural disasters, terrorism, and nuclear accidents (e.g. Eckel et al. (2009), Cassar et al. (2011), and Cameron and Shah (2013)). For example, Luechinger and Raschky (2009) use subjective well-being measures to economically value the costs of flood disasters. Pesko (2014a, 2014b) assesses the impact of Hurricane Katrina and terrorism on stress and risky behaviors, while Pesko and Baum (2016) assess the impact of 9/11 on similar outcomes.[11] Draca et al. (2011) study the impact of the 2005 terrorist attacks in London on crime.

While all papers cited above study directly affected populations, several papers study negative externalities and spillover effects on other, presumably unaffected, countries. Such negative externalities and spillover effects provide an economic justification for a supra-national regulation of policies on climate change, counter-terrorism, and nuclear energy. For example, Metcalfe et al. (2011) show that 9/11 negatively affected mental well-being of residents in the UK. Schüller (2013) shows that negative attitudes towards immigration increased in Germany post-9/11.

While Danzer and Danzer (2016) find negative long-term effects of the Chernobyl disaster on subjective well-being and mental health in Ukraine, three other papers assess the impact of the disaster on a variety of well-being, health, and labor market outcomes in other countries: (i) Almond et al. (2009) find negative long-term effects of prenatal exposure on cognitive abilities in Sweden, (ii) Halla and Zweimüller (2014) find negative long-term effects of prenatal exposure on labor market outcomes in Austria, and (iii) Berger (2010) finds that environmental concerns increased in West Germany immediately after the disaster.

Several studies on the consequences of the Fukushima disaster exist. However, almost all of them focus on Japan (Glaser 2011; Hippel 2011; Hommerich 2012; Huenteler et al. 2012; Kawashima and Takeda 2012; Thomas 2012; Uchida et al. 2014; Vivoda 2012; Yamamura 2012; Aoki and Rothwell 2013; Bauer et al. 2013; Buesseler et al. 2012; Csereklyei 2014; Ohtake and Yamada 2013; Rehdanz et al. 2013; Richter et al. 2013; Rieu 2013; Tiefenbach and Kohlbacher 2013, 2015; Wang et al. 2013; Welsch and Biermann 2014). Specifically, three (unpublished) studies focus on well-being.[12] Rehdanz et al. (2013) use household panel data from the Japanese KHPS and find that, post-Fukushima, people living closer to the disaster report lower levels of subjective well-being than people living further away. Hanaoka et al. (2015) exploit the *Japan Household Panel Survey on Consumer Preferences and Satisfaction* and variation in affected Japanese regions. They find that respondents in more affected regions became *more*

11. Other studies assess the indirect effects of the War Against Terrorism by showing that combat exposure increases (i) risky behaviors, such as smoking or drug use, among affected soldiers (Cesur et al. 2014), as well as (ii) sleep disorders, psychological problems, and the risk of migraine headache (Cesur et al. 2015).

12. In addition to these three studies focusing on well-being, Bauer et al. (2013) study the impact of the shutdown of reactors on housing prices in Germany. They find that housing prices decreased by between 6 and 12%.

risk tolerant.

Using the SOEP and similar outcome variables as this study, Richter et al. (2013) also investigate the effects of Fukushima and the nuclear phaseout in Germany. However, the two studies differ substantially with respect to the methodological approach. First, Richter et al. (2013) solely use the year 2011 and do not exploit the panel structure of the data, whereas this study nets out individual unobserved heterogeneity through individual fixed effects. Second, Richter et al. (2013) use a simple before-after comparison, whereas this study uses a Difference-in-Difference (DID) model. Moreover, this study provides empirical evidence on operating channels, with a particular focus on risk aversion and political preferences, while providing heterogeneity analyses and using additional concern and well-being measures. Third, Richter et al. (2013) solely focus on Germany, whereas this study focuses on Germany and two additional countries that differ in their energy policies and in their policy responses. Section 1.5.2.10 replicates Richter et al. (2013) and highlights some methodological differences to this study.

This paper also adds to the literature on energy economics (Greenstone and Gayer 2009; Cesur et al. 2013; Strielkowski et al. 2013), political economy (Anderson et al. 2013), and their interplay (Ockwell 2008; Büscher 2009; Acemoglu et al. 2011; Wangler 2012). The latter is a growing field that deals primarily with the policies and consequences of renewables and climate change (Cullen 2013; Pindyck 2013; Marron and Toder 2014; Murray et al. 2014).

Finally, this paper contributes to the literature on the determinants and impacts of risk aversion (Lusk and Coble 2008; Eckel et al. 2009; Cassar et al. 2011; Malmendier and Nagel 2011; Huang et al. 2013; Hanaoka et al. 2015; Cohn et al. 2015; Vieider et al. 2015; Cameron and Shah 2013) and environmental concerns. Studies in the former field find that, for example, education is positively associated with risk aversion (Jung 2015) and that people who are less favourable towards a particular technology impose a higher risk premium on it (Rottenstreich and Hsee 2001). Studies in the latter field find that females and higher educated people are more concerned about the environment (Czap and Czap 2010; Tatić and Činjarević 2010; Urban and Ščasný 2012; Aklin et al. 2013). If individuals with more educational attainment are more risk averse, and if individuals with more educational attainment are more concerned about the environment, then risk aversion and environmental concerns should also be positively correlated (which is the case in our final sample). Interestingly, income itself does not seem to play a big role for environmental concerns (Urban and Ščasný 2012; Aklin et al. 2013). Owen et al. (2012) show that personal experiences with extreme weather events, however, positively affect preferences for environmental regulation.[13]

1.3 Data

This study uses household panel data provided by the German Socio-Economic Panel (SOEP)for Germany, the Swiss Household Panel (SHP) for Switzerland, and Understanding Society for the UK, all of which are described in more detail below (Budowski et al. 2001; University of Essex, Institute for Social and Economic Research and National Centre for Social Research 2012).

1.3.1 SOEP

The Socio-Economic Panel (2013) is a representative panel study of private households in Germany, conducted annually since 1984. About 20,000 individuals in more than 11,000 households are surveyed every year. All respondents aged 17 and older answer an individual questionnaire covering about 150 questions on different topics, such as demographic, educational, and labor market characteristics, health, well-being, concerns, attitudes, and perceptions. For further details about the survey content and design, see Wagner et al. (2007).

The baseline specification exploits the panel dimension of the SOEP and focuses on respondents interviewed in 2010 and 2011. In total, the SOEP contains 20,178 person-year observations from 10,177 different individuals which were interviewed in both years and have no missings on their observables. In 2011, roughly half of those 10,177 individuals were interviewed before and after the Fukushima disaster. Since only individuals who were interviewed in both years contribute to the identifying variation in the individual fixed effects models, we naturally use a balanced panel when we use the years 2010 and 2011.

For extended analyses, in particular for the analysis of medium-term effects, we use data from 2009 to 2012, obtaining 57,492 person-year observations. Moreover, in a falsification test, we compare Fukushima to Chernobyl. To do so, we exploit the same data, models, and variables but focus on respondents who were interviewed between 1984 and 1989, obtaining 62,540 person-year observations. In these extended analyses with more than two waves of panel data, we routinely use unbalanced panels, as we would otherwise select on a specific subsample of respondents – those not prone to panel attrition – and would potentially discard useful information.

13. Table 1.18 in Section 1.8 shows simple socioeconomic determinants of environmental concerns. The findings largely confirm the previous literature: Relative to the mean share of very environmentally concerned people in the population, which is 31%, environmental concerns are (i) 7 ppt higher among females and (ii) 3 ppt higher among disabled individuals, whereas they decrease (iii) by 0.5 ppt for each child in the household, (iv) by 21 ppt for individuals with the lowest educational degree (less than secondary) relative to the highest educational degree (tertiary), and (v) by 6 ppt for individuals who are full-time employed relative to being irregularly employed.

1.3.2 Dependent Variables: Concerns, Well-Being, Risk Aversion, and Political Preferences

1.3.2.1 Concerns

We exploit several concern measures that are routinely surveyed by the SOEP. Our first dependent variable is based on the question: "What is your attitude towards environmental protection? Are you concerned about it? (a) Very concerned, (b) Somewhat concerned, (c) Not concerned at all." We collapse the answer to this question into a binary measure that is one when respondents are "very concerned" about environmental protection and zero otherwise. As seen in Section 1.7, 31% of all respondents are very concerned about the environment. Analogously, we generate a binary measure indicating whether respondents are "very concerned" about climate change. It is based on a similar question which asks: "What is your attitude towards climate change? Are you concerned about it?" 30% of all respondents are very concerned about climate change. Finally, for placebo tests, we generate binary measures indicating whether respondents are "very concerned" about their job security, health, the economy, and crime (see Section 1.7).

1.3.2.2 Well-Being

First, we use the standard 11 categorical *life satisfaction* item, measured on a scale from 0 to 10. It is based on the question: "How satisfied are you with your life, all things considered?". Its mass point lies between the values 5 and 9. On average, 86% of all respondents fall into these categories. In psychology, life satisfaction is often defined as the cognitive evaluation of the circumstances of life (Diener et al. 1999). To the extent that the Fukushima disaster (or the ensuing policy action) affects this evaluation, we may find an impact on this item over and beyond concerns. The SOEP also measures affective well-being, asking respondents to rate how often they felt *happy* or *sad* during the 4 weeks prior to the interview. These are standard items that are used in many nationally representative household panels, and it has become common practice to ask about them with respect to a pre-specified time period in the past. The five answer categories range from "very seldom" to "very often." We collapse the two highest categories, "often" and "very often," and, accordingly, generate two dichotomous variables. Section 1.7 shows that (only) 13% of all Germans are "often" or "very often" happy, whereas 54% are "often" or "very often" sad. For these questions to be useful, we have to take their retrospective nature into account, and we do so by using the dates 4 weeks after the Fukushima disaster and the policy action as the cut-off dates, i.e. April 11 instead of March 11, 2011, for the Fukushima disaster and June 30 instead of May 30, 2011, for the policy action.

1.3.2.3 Risk Aversion

We collapse the standard 11 categorical *risk attitude* scale – ranging from 0 (extremely risk averse) to 10 (extremely risk loving) – and generate four risk aversion measures (Dohmen et al. 2011). We categorize respondents in category 0 as "extremely risk averse" (5%), respondents in categories 0 to 1 as "very risk averse" (10%), respondents in categories 0 to 2 as "moderately risk averse" (22%), and respondents in the lowest four categories simply as "risk averse" (37%). Note that these measures are not mutually exclusive but capture different, overlapping parts of the distribution of risk aversion: for example, the share of respondents included in the "very risk averse" category is a subset of the share included in the "moderately risk averse" category.

1.3.2.4 Political Party Preferences

We obtain the binary indicator *Supports Political Party* from the question that asks respondents "Do you lean towards a particular party?" On average, 49% of all Germans do. For respondents who answer "yes" to this question, we obtain specific political party measures, such as *Supports SPD* (30%), *Supports The Greens* (15%), *Supports CDU/CSU* (40%), *Supports FDP* (5%), and *Supports The Left* (7%).[14] Finally, to understand how intensely respondents support a party, *Strong Political Party Support* and *Weak Political Party Support* are generated from the five categorical answers to the question "And to what extent [do you lean towards this party]?" On average, 44% support a party strongly and 6% support a party weakly.

1.3.2.5 Distance to Reactors

By using the exact geographical coordinates of all SOEP households, we exploit the distances between respondents' residencies and the nearest nuclear power plant. As such, we can additionally differentiate the estimated effects to learn more about operating channels. Nuclear power plant distance measures are used for Germany, Switzerland, and the UK.

Figure 1.2 depicts nuclear power plants in and around Germany along with radii of 25 km (16 mi), 50 km (31 mi), and 100 km (62 mi). Three distance variables indicate whether respondents live (i) *within 50 km to reactor* (28% of all Germans), (ii) *within 50 km to 80 km to* (20% of all Germans), or whether the (iii) *nearest reactor is among eight oldest* (49% of all Germans). The

14. At the time of the disaster, Germany's parliament comprised five political parties. With 44% of the votes, the Christian-Democrats (CDU/CSU) and the Free Democratic Party (FDP) formed the governing coalition. The Social Democrats, the Greens, and The Left formed the opposition. Accordingly, we generate a variable "Supports Government."

eight oldest German reactors were temporarily shut down on March 14, 2011 and permanently shut down on May 30, 2011. To account for potential residential sorting based on environmental concerns, we run robustness checks that exclude individuals who moved outside a 50 km (31 mi) radius of their birth place and individuals who moved previously (see Columns (2) and (3) of Table 1.20 in Section 1.8).[15]

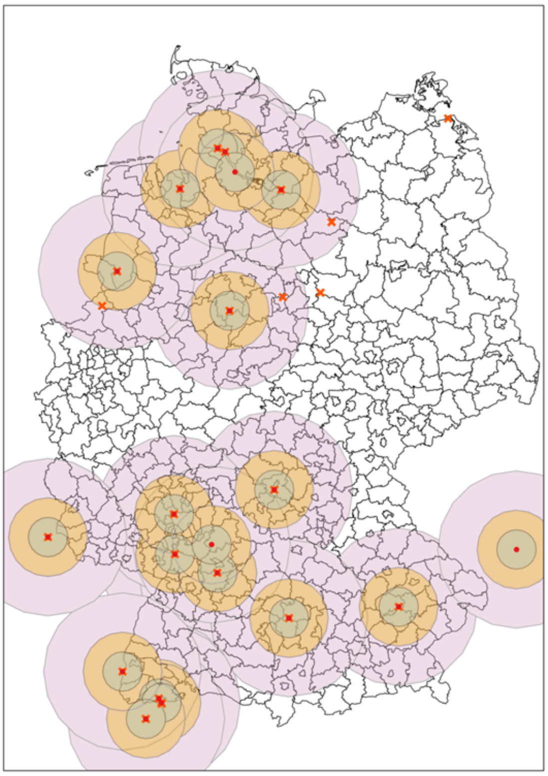


Figure 1.2: Nuclear Power Plants and Waste Sites in Germany

1.3.3 Sociodemographic Covariates

The regression models control for the ***Demographic Characteristics*** *age*, *female*, and for being *married*, *single*, or *disabled*. In addition, individuals *without German nationality* and the *number of children* in the household are indicated. ***Education and Labor Market Characteristics*** measure whether respondents are still *in education* or have a *schooling degree*, and whether they are *full-time employed*, *part-time employed*, *out of the labor force*, on *maternity leave*, or *unemployed*. The full descriptive statistics for Germany, Switzerland, and the UK are in Section 1.7.

15. Traditionally, (inter-generational) geographic mobility is very low in Germany. In a given year, only about 1% of SOEP respondents move.

1.4 Empirical Model and Identification

To the extent that our dependent variables are binary, we run Linear Probability Models (LPM).[16] Specifically, we employ the following model:

$$\begin{aligned} y_{it} = &\beta_0 + \beta_1 PostMarch11_{i,2011} * 2011 + \beta_2 PostMay30_{i,2011} * 2011 \\ &+ \beta_3 PostMarch11_{i,2011} + \beta_4 PostMay30_{i,2011} + X_{it}{'}\gamma \\ &+ \delta_y + \varphi_m + t + \varepsilon_{it} \end{aligned} \tag{1.1}$$

where y_{it} represents the dependent variable for individual i at time t.

$PostMarch11_{i,2011}$ is a dummy variable that indicates whether a respondent's interview in 2011 occurred after or on March 11 – the day of the Fukushima disaster. Note that this dummy is time-*invariant* and indicates the treatment group. This means that all respondents who were interviewed after or on March 11 (in 2011) *always* have a one on this variable regardless of when they were interviewed in the other years. Consequently, this variable nets out potentially existing systematic differences between respondents who were interviewed before and after March 11, 2011.

$PostMay30_{i,2011}$ is similarly constructed and stands for the day when the government officially announced the *Atomausstieg*, permanently shutting down the eight oldest reactors in Germany and phasing out the remaining ones.

2011 is a year dummy variable that takes on the value one if the interview took place in 2011, and zero otherwise.

The coefficients of the interaction terms between $PostMarch11_{i,2011}$, $PostMay30_{i,2011}$, and 2011, β_1 and β_2, measure the Average Treatment Effects on the Treated (ATT).

All models routinely control for year and month fixed effects, $\delta_y + \varphi_m$, a linear time trend, t, and a vector of socioeconomic characteristics, X_{it}. A more refined version of the model above (which already exploited the panel structure through the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$), is to explicitly consider individual fixed effects via μ_i. In doing so, we net out individual unobserved heterogeneity, which is a key methodological difference to Berger (2010) and Richter et al. (2013). Robustness checks augment the baseline model with split time trends in addition to individual fixed effects:

16. The results are robust to running Logit Models with marginal effects instead of Linear Probability Models (see, for example, Table 1.29 in case of environmental concerns).

$$
\begin{aligned}
y_{it} = &\beta_0 + \beta_1 PostMarch11_{i,2011} * 2011 + \beta_2 PostMay30_{i,2011} * 2011 \\
&+ t * PostMarch11_{i,2011} + t * PostMay30_{i,2011} + X_{it}{}'\gamma \\
&+ \delta_y + \varphi_m + t + \varepsilon_{it}
\end{aligned} \tag{1.2}
$$

Note that when including individual fixed effects, the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ drop out of the estimation.

1.4.1 Identification of the Fukushima Effect

The main identifying assumption is that, conditional on the vector of socioeconomic characteristics, X_{it}, year and month fixed effects, $\delta_y + \varphi_m$, the linear time trend, t, and conditional on netting out individual unobserved heterogeneity, μ_i, the interview date is exogenous to the Fukushima disaster. It is very likely that this identifying assumption holds.

The Fukushima disaster happened on March 11, 2011. Most SOEP interviews are carried out during the first 6 months of the year.[17] Hence, in 2011, roughly half of all SOEP respondents completed their interviews before and after March 11. Table 1.15 plots the mean values of all covariates along with their scale-free normalized differences. Imbens and Wooldridge (2009) suggest that a normalized difference above 0.25 indicates covariate imbalance. This is not the case for any of our covariates. Thus, we conclude that the sample is well-balanced on observables.[18] Note that even systematic differences in observables and unobservables would not necessarily be a threat to the identification strategy. First, we control for observables. Second, the time-invariant variable $PostMarch11_{i,2011}$ nets out systematic differences between respondents before and after the exogenous event. Moreover, since we use panel data and focus on a relatively short time horizon, our individual fixed effects models net out time-invariant individual unobserved heterogeneity. In fact, our estimates vary little depending on the approach employed which reinforces the exogeneity assumption of the interview date. Finally, it is difficult to think of an unobservable that systematically affected the outcomes in

17. In practice, the fieldwork company that carries out the interviews assigns each interviewer two to three tranches of respondents with addresses to schedule and conduct interviews over a defined time period of a couple of months. Within that time period, each interviewer coordinates the specific interview independently with the respondent. This approach guarantees a relative balance of interviewee characteristics over the year.

18. We also checked the covariate balance with respect to the May 30 and June 30, 2011, cutoff dates for the policy action. Again, none of the normalized differences exceeded the threshold of 0.25. In addition, we calculated the normalized differences and means for the important outcome variable "environmental concerns" by the policy dates May 30 and June 30 in 20*10*. We found that, if anything, respondents interviewed later in the year reported higher levels of environmental concerns. For the May 30 cutoff date, the mean levels are 0.29 (pre) and 0.32 (post), whereas they are 0.30 (pre) and 0.32 (post) for the June 30 cutoff date. The differences are, however, not statistically significant and the normalized differences are below 0.05 in both cases.

2011, but not in 2010, and was correlated with the date of Fukushima. Basically, this would be an event that is unrelated to but strongly correlated with Fukushima, which – to the best of our knowledge – did not exist.

It is, however, important that self-administered interviews were not systematically postponed due to the Fukushima disaster. In fact, the Fukushima disaster happened on a Friday at 7:45 a.m. CEST. Interviews where an interviewer is physically present are typically scheduled several days in advance. It is conceivable, though unlikely, that (environmentally sensitive) respondents postponed their scheduled interviews. Even if this happened, it would not bias the disaster estimates since interviews carried out on or after March 11 fall into the treatment group. Comparing the distribution of interviews (excluding self-administered interviews) carried out on Fridays to the interviews carried out on Friday March 11, 2011, we find no abnormal decrease.[19] Additionally, in robustness checks, we exclude all interviews that were not scheduled and where a trained interviewer was not present (see Table 1.19 and Column (4) of Table 1.20 in Section 1.8). Excluding March 11 interviews does not affect the estimates either.

Since the Fukushima disaster was exogenous to interview dates, in principle, an adjustment for pre-post differences in the sample composition is not necessary.[20] In a totally randomized setting, one could rely on cross-sectional data to estimate the effect of the disaster. The use of panel data allow us to compare (i) LPM treatment effects unadjusted for observables with (ii) LPM treatment effects adjusted for observables, as well as (iii) pooled LPM-OLS estimates not exploiting the panel structure with (iv) LPM-FE estimates eliminating individual unobserved heterogeneity. Comparing (i) with (iv) serves as a robustness check for the disaster date exogeneity assumption and yields information on potential confounding factors.

1.4.2 Identification of Nuclear Phaseout Effect

Compared to the identification of the disaster effect, the identification of the nuclear phaseout effect is more challenging for a number of reasons: First, we observe a series of policy events rather than a single event (see Figure 1.1). The initial event, the *Atommoratorium*, which temporarily shut down the eight oldest reactors, was announced and implemented on March 14, 2011, only 3 days after the Fukushima disaster. Empirically, it is basically impossible to disentangle the effect of the *Atommoratorium* from the disaster. One could hypothesize that

19. On March 11, 2011, 80 interviews were carried out, the week before 81, the week after 64, and on the last Friday of March, 78 interviews were conducted.

20. As a referee correctly pointed out, there may be unobserved third factors that vary systematically across seasons and may affect environmental concerns, e.g., air pollution. Ziebarth et al. (2014) show that between 1999 and 2008 pollution patterns follow very regular seasonal patterns in Germany. Unless there was an unusual and longer-term spike in air pollution exactly at the time of Fukushima, monthly fixed effects should net out seasonal pollution effects.

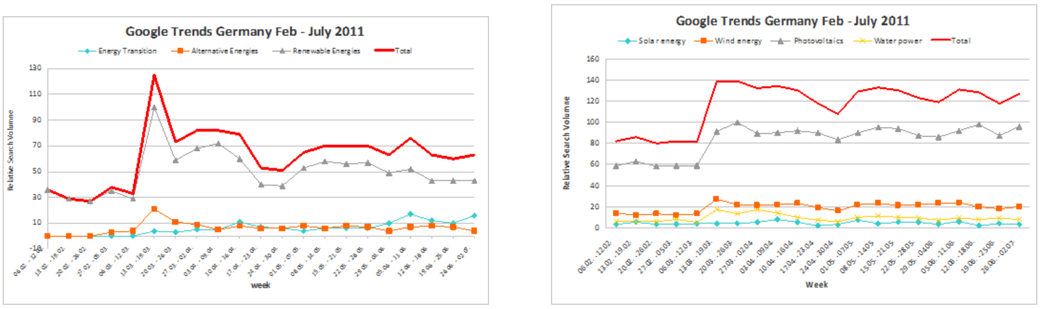
its impact may have operated in both directions – either reinforced or reduced environmental concerns.

To the extent that respondents now fear the adverse environmental consequences of relying more on conventional electricity generation, their environmental concerns might increase, yielding an upper bound of the identified Fukushima effect. To the extent that they are relieved, in the sense that they do not fear the adverse environmental consequences of a similar disaster happening near them, their environmental concerns might decrease, yielding a lower bound. We cannot provide conclusive evidence which bounding is the case, but there are three reasons why it is more likely that the Fukushima effect is bounded from below: first, adverse environmental consequences of relying more on conventional electricity generation are a rather abstract phenomenon, as opposed to a nuclear disaster with widespread media coverage. Second, the temporary shutdown, at the time of announcement, was communicated to last three months only, probably not enough for fears about such adverse environmental consequences to substantiate. Finally, as we show later, the permanent shutdown decreases concerns about the environment, and it follows that the temporary shutdown, to a lesser extent, should have gone into the same direction.

When estimating the models with March 14 as the relevant policy action date, the coefficients barely change, which could suggest that the *Atommoratorium* had little impact on Germans' concerns.[21] Hence, we consider the unexpected and widely covered announcement of the *Nuclear Phase-Out Bill* on May 30, 2011 as the crucial policy action date.

Second, a decrease in media coverage and thus disaster-related consciousness in the population may have reduced environmental concerns. We provide the following robustness checks that support our view that the unexpected and drastic turnaround in energy policy contributed significantly to the decrease in environmental concerns: Figure 1.3a plots the results of a Google trend search using the three German keywords Energy Transition (*Energiewende*), Alternative Energies (*Alternative Energien*), and Renewable Energies (*Erneuerbare Energien*). Figure 1.3b shows the relative search volume for the Solar Energy (*Solarenergy*), Wind Energy (*Windenergie*), Photovoltaics (*Photovoltaic*), and Water Power (*Wasserkraft*). Google trend search has been shown to have good predictive power for economic indicators (Askitas and Zimmermann 2009; d'Amuri and Marcucci 2012). The graphs clearly show two main developments: (a) Immediately after Fukushima, the search volume spiked. (b) In the subsequent week(s), it sharply decreased, but remained relatively stable at a level clearly higher than before Fukushima. The

21. We also tested whether the release of the report of the Reactor Safety Commission on May 17, 2011 (the so-called *Reaktorsicherheitskommission*), which gave a rather negative safety outlook for German reactors, had any impact on environmental concerns, but do not find any evidence for that.



(a) (b)

Figure 1.3: "Renewable Energy" Google Trend Search for Germany, Feb to July 2011

latter point is crucial and shows that, in our opinion, it is extremely unlikely that the entire decrease in environmental concerns can be traced back to a decrease in media coverage and thus disaster-related consciousness in the population. Moreover, recall that due to the incompetent management of the catastrophe by Tokyo Electric Power Company (TEPCO) – the operating firm of the Fukushima Daiichi nuclear power plant – Fukushima and nuclear safety remained in the media spotlight for a very long time.

Figure 1.4 shows that the decrease in environmental concerns started to kick in about 6 weeks after the disaster before the announcement of the *Nuclear Phase-Out Bill*. This may be attributed to or reflected by the decrease in search volume observed in Figure 1.3. However, Figure 1.4 also shows a clear, structural, and additional decrease in environmental concerns below the zero y-axis line immediately after May 30, which is of a magnitude similar to the increase after March 11. Recall that, among others, the empirical models routinely control for month fixed effects, linear time trends, and individual time-invariant unobserved heterogeneity. Moreover, we run a series of placebo regressions with alternative policy action dates in 2011 and 2010. We also show that the findings are robust to the inclusion of separate linear time trends after the disaster and the policy action, as well as to the inclusion of a quadratic time polynomial. A simple "return-to-the-baseline" effect would be captured by these controls.

Finally and importantly, we use the same data, models, and variables as well as the same institutional and cultural setting to assess the medium to long-run effects of the Chernobyl disaster in 1986. The idea is to, first, show that our specification identifies a similar disaster effect for Chernobyl as for Fukushima and, second, use Chernobyl as a falsification test. More specifically, we insert a placebo policy after the Chernobyl disaster, using the same time span as elapsed between the Fukushima disaster and the real policy action (*Atomausstieg*) taken thereafter. We use July 15, 1986 (e.g., 81 days after Chernobyl) as the placebo nuclear phaseout date. Non-significance of this placebo policy action, together with a similar disaster effect for

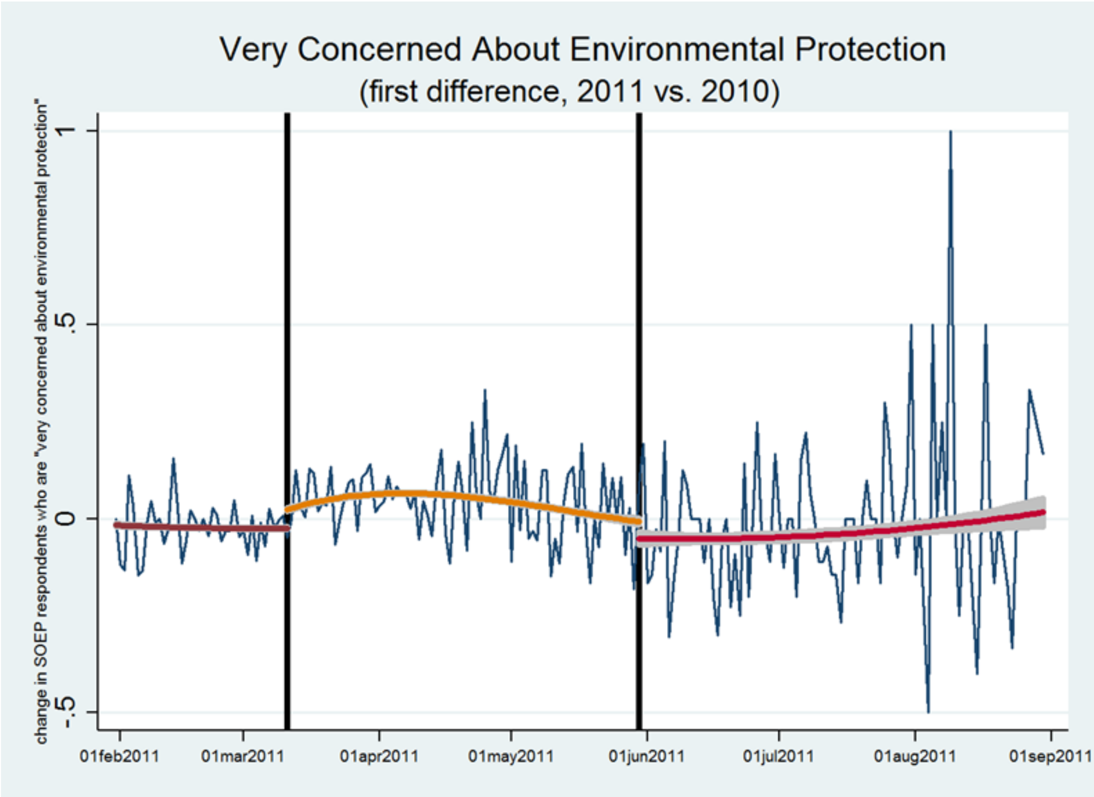


Figure 1.4: Change in Concerns About Environmental Protection

Chernobyl and a similar media reporting between Chernobyl and Fukushima, would provide *prima facie* evidence against the claim that our identified policy action after Fukushima is driven merely by a decrease in media attention or a return to baseline after the catastrophe.

We find that (a) environmental concerns were still significantly elevated at the end of 1986 and (b) there is no significantly negative effect of the placebo nuclear phaseout date on environmental concerns. Concerns that Chernobyl and Fukushima cannot be compared due to different media coverage and exposure can be dismissed by Figure 1.5. Figure 1.5 shows the number of newspaper articles with the keywords “Fukushima” and “Chernobyl” in one of the leading (internationally available) German newspapers, *Frankfurter Allgemeine Zeitung (FAZ)*. The normalized graph impressively shows that the spike and subsequent decrease in media coverage was almost perfectly identical in 2011 and 1986. Thus, we consider Chernobyl as a valid counterfactual.

In sum, one can say that the identification of the effect of the policy action is challenging and likely confounded by a decrease in media coverage. However, a series of robustness checks suggests that part of the significant and surprisingly large decrease in environmental concerns

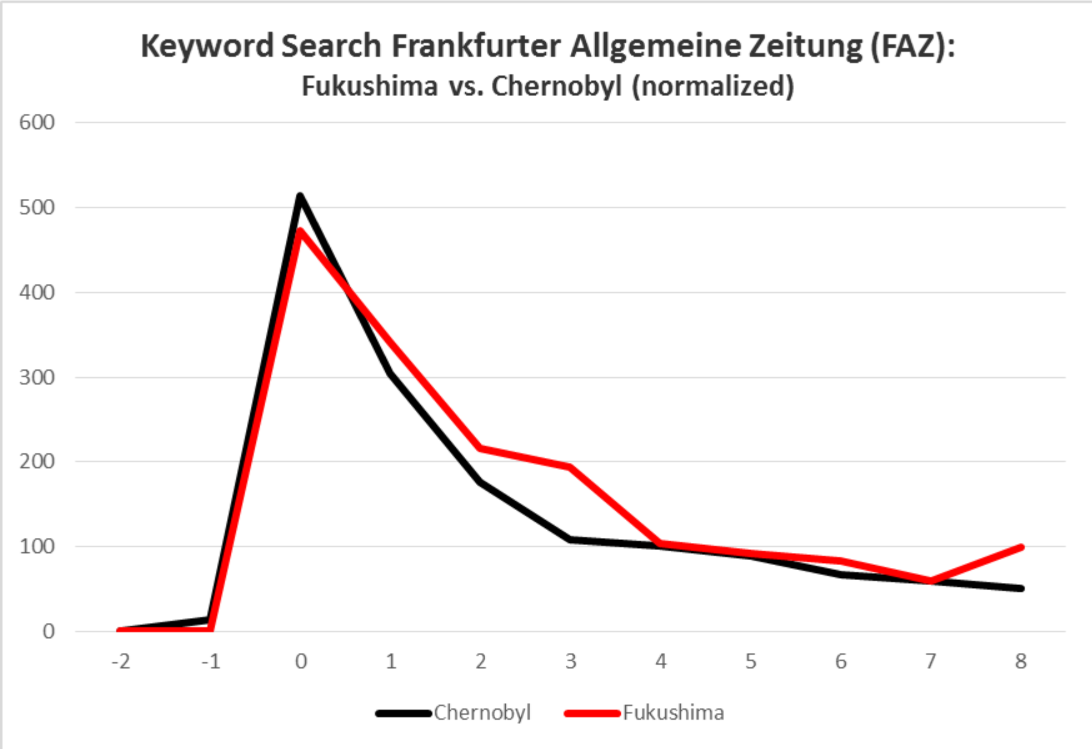


Figure 1.5: Newspaper Articles on Fukushima vs. Chernobyl in Weeks Before and After Disaster

can be explained by the unexpected and drastic energy policy turnaround by the center-right German government.

Table 1.28 lists potentially confounding events for our main outcome variables in Germany, Switzerland, and the UK in the first half of 2011, that is, during the relevant observation period: as can be seen, there are little confounding events with respect to environmental concerns, but quite a few elections, which might be confounding factors for political preferences. Our results on political preferences should thus be interpreted with caution.

1.5 Results

1.5.1 Descriptives

Figures 1.4, 1.6, and 1.9 anticipate and illustrate one of our main findings. Figure 1.9 is in Section 1.8, and is a nonparametric representation of an unbalanced OLS model. Figures 1.4 and 1.6 are nonparametric representations of a model with individual fixed effects. The x-axes display the interview dates in 2011; the first black vertical bar indicates the Fukushima disaster and the second vertical black bar indicates the announcement of the *Nuclear Phase-Out Bill*.

Figure 1.9 plots the share of respondents, on a given day, who reported being very concerned about the environment. We observe a distinct jump in this share after the Fukushima disaster. After the announcement of the *Nuclear Phase-Out Bill*, however, the share decreased again. Note that the grey underlined confidence intervals widen towards the end of the year, as only 10% (or 1100) of all interviews were carried out after August 1, 2011.

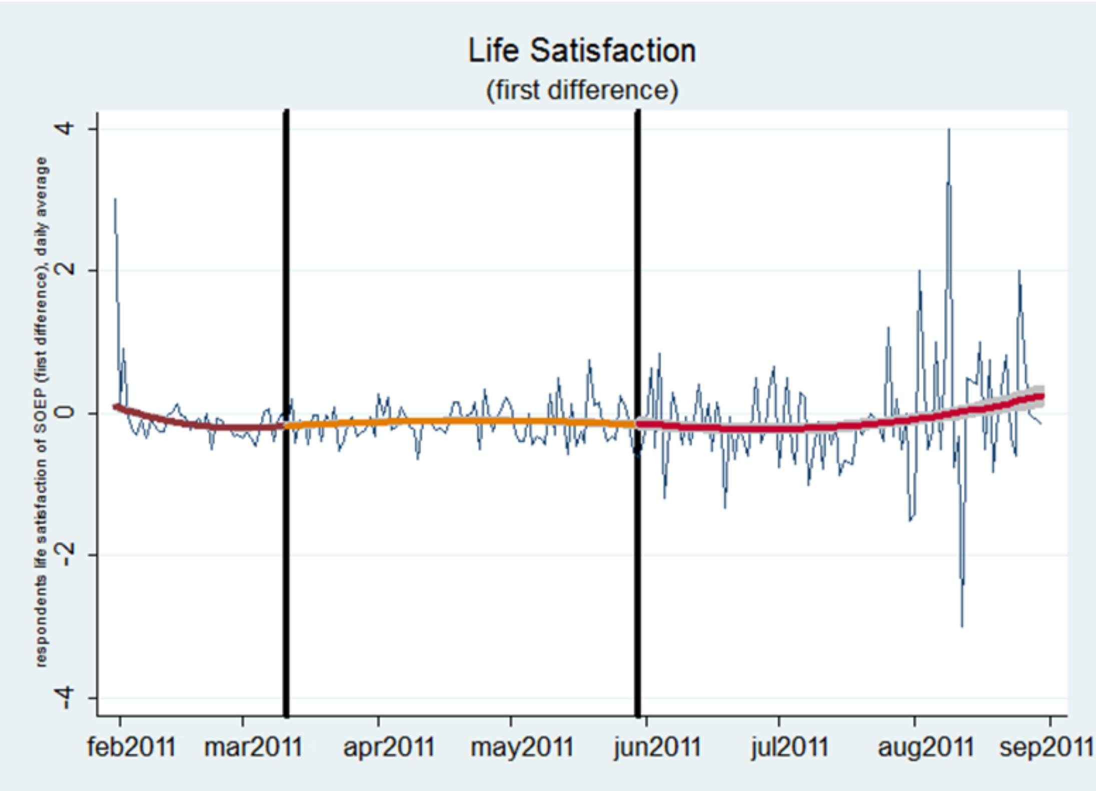


Figure 1.6: Change in Life Satisfaction

Figure 1.4 exploits the panel structure of the data. The y-axis displays the individual-level change in responses between the 2011 and 2010 interviews, where the 2011 interview determines the location on the y-axis. In other words, Figure 1.4 plots the change in the daily share of people who are very concerned about the environment by their 2011 interview date, relative to their 2010 answers. This is the graphical representation of the difference-in-differences model that exploits exact cut-off dates to allocate individuals into treatment and control group.

Figure 1.4 illustrates that, while there was zero change in environmental concerns before the Fukushima disaster, environmental concerns significantly increased by 5 to 10 ppt thereafter. They started to decline smoothly again after about 6 weeks. However, after the conservative government announced the *Nuclear Phase-Out Bill*, making a sharp and unexpected U-turn in their energy policy, environmental concerns significantly and sharply decreased once more, now falling below the horizontal zero-change line on the y-axis.

Figure 1.6 is set up analogously to Figure 1.4, but plots changes in life satisfaction. It is easy to see that the curve is flat around the zero-change line on the y-axis. No changes in life satisfaction that could be attributed to the disaster or the policy action are observable.

1.5.2 Regression Results

1.5.2.1 Baseline Specifications

Table 1.1 shows our baseline specifications, where we focus on just 2 years, 2010 and 2011. The first two columns estimate OLS-LPM and the next two columns FE-LPM models.[22] Thus, the first two columns are the (covariate-adjusted) regression equivalent to Figure 1.9, and the last two columns are the (covariate-adjusted) regression equivalent to Figure 1.4. The binary dependent variable is environmental concerns and indicates whether respondents are "very concerned" about environmental protection. For the sake of clarity and brevity, we suppress the coefficient estimates of those covariates that are not of principal interest; these can be found in Table 1.18 of Section 1.8. As shown in the bottom of Table 1.1, in the even numbered columns covariates, X_{it}, are included in the regressions, whereas they are excluded in the odd numbered columns. We learn the following from Table 1.1:

First, across all four models, we consistently find that environmental concerns significantly increased by about 7 ppt immediately after the Fukushima disaster. Relative to the baseline level of environmental concerns before Fukushima, this represents an increase of about 23%.[23]

Second, after the sharp and unexpected U-turn in energy policy – the permanent shutdown of the eight oldest reactors and the announcement of the *Nuclear Phase-Out Bill* – environmental concerns decreased significantly by about 10 ppt. Relative to the baseline level of environmental concerns between March 11 and May 30, which was 38%, this represents a decrease by about 26%, i.e., a decrease that roughly equals the increase immediately after Fukushima. Obviously, the *Nuclear Phase-Out Bill* helped to counterbalance environmental concerns triggered by the Fukushima catastrophe. Note that, in our preferred specification, we routinely employ year fixed effects, month fixed effects, a linear time trend, and individual fixed effects. Since we also rely

22. We routinely cluster standard errors at the interview date level (Bertrand et al. 2004; Lee and Card 2008). We do so because of the event-study-like design of our empirical model, which allocates individuals into treatment and control group based on a cut-off date, and observations recorded on particular dates are correlated. However, clustering at the individual, household, or state level does not alter the results.

23. When using March 14 as disaster date, the results remain largely robust. The results are also robust to collapsing the three categorical environmental concern questions differently. When we ran the same models but collapse the categories "somewhat" and "very concerned," we find that the share of at least "somewhat" concerned Germans increased by 2 ppt from a baseline level of 89% after Fukushima.

on sharp timely variation, the effects are identified by changes in respondents' environmental concerns from 2010 to 2011 – net of monthly shocks and a time trend – for the treatment group that was interviewed just after the exogenous disaster, relative to the control group that was interviewed just before the disaster.

Third, we find no evidence that (i) respondents differ in their observables before and after the March 11 and May 30 interview dates (see Table 1.15), (ii) the correction for observables matters, and (iii) the correction for unobservables matters. Across all models, the estimates remain almost identical whether or not we include covariates, X_{it}. The OLS vs. FE estimates are likewise almost identical.

As discussed in detail below, Section 1.8 provides batteries of robustness checks including specifications that test the exogeneity of the interview date and employs several variants of time trends as well as placebo policy and Fukushima dates.

1.5.2.2 Effects on Measures of Subjective Well-Being

1.5.2.2.1 Life satisfaction The first column of Table 1.2 uses the standard 11 categorical life satisfaction measure as dependent variable. This model is the regression equivalent to Figure 1.6. As already suspected in Figure 1.6, we do not find any evidence that the disaster or the phased out had an effect on life satisfaction. Typically, studies consistently find that individual income or unemployment have strong effects on life satisfaction (Winkelmann and Winkelmann 1998; Frijters et al. 2004; Kassenboehmer and Haisken-DeNew 2009; Knabe et al. 2010). One may interpret our finding as evidence that disasters may affect specific individual concerns, even in distant geographical regions, but not satisfaction with life in general, at least as long as individuals are not directly affected. This is in line with Berger (2010) and with the empirical evidence from the UK and Switzerland (see below).

1.5.2.2.2 Happiness The finding from Column (1) is reinforced in Column (2) where we make use of a collapsed version of the "happiness" affective well-being measure (see Section 1.3). We do not find evidence that the share of people who felt "very often" or "often" happy changed significantly as a result of the disaster or the policy action.

1.5.2.2.3 Sadness In contrast, after Fukushima, the share of respondents who felt "sad" increased by about 5 ppt (Column (3)). This is understandable, as the magnitude of the disaster affected people around the globe emotionally. This may be reflected in Column (3).

1.5.2.2.4 Concerns about climate change The sharp and unexpected U-turn in the energy policy entailed a long-term, large-scale plan under which Germany would gradually replace nuclear with renewable energy. Angela Merkel created the term "Energy Transition" (*Energiewende*) for this ambitious plan. Since the *Energiewende* is inherently linked to climate change politics and was largely communicated to the public with this spin, Column (4) tests whether concerns about climate change shifted. Indeed, concerns about climate change significantly increased after the Fukushima disaster but significantly decreased after the policy action, which entailed the announcement of the *Energiewende*. This is a surprising finding, as one would expect concerns about climate change, if anything, to increase after the disaster (due to the temporary shutdown), and even more so, to increase after the policy action (due to the permanent shutdown of nuclear power plants and their replacement with conventional electricity generation technologies). The fact that concerns about climate change closely mimic environmental concerns might suggest that respondents may not be able to cognitively distinguish both concepts. In fact, concerns about climate change are a rather abstract phenomenon, and are typically not part of a respondent's daily concerns. This explanation might be supported by the fact that both types of concerns show very similar summary statistics. Finally, their very position in the survey might lead to respondent behaviour that does not make an explicit distinction between both types of concerns: the item on concerns about climate change follows immediately that on environmental concerns.

1.5.2.3 Effects on Risk Aversion and Evidence on Operating Channels

1.5.2.3.1 Impact on the German risk aversion distribution, or: does Merkel represent German attitudes? Next, we test whether Germans became more or less risk averse after the Fukushima disaster. The quote by Chancellor Merkel above strongly suggests that she became more risk averse; at least she declared many times in public that, before Fukushima, she believed that the remaining risk of a nuclear accident was zero but that she re-assessed her opinion and changed her mind and willingness to tolerate small high-stakes risks (Bundesregierung 2011d, 2011b, 2011e, 2011a, 2011c).

Table 1.3 provides tests on the entire risk aversion distribution. We employ four different binary outcome variables that represent collapsed versions of the right tail of the risk aversion distribution. In other words, we test whether the share of Germans who self-categorized as *risk averse* (0–3/10 on scale), *moderately risk averse* (0–2/10 on scale), *very risk averse* (0–1/10 on scale), or *extremely risk averse* (0/10 on scale) changed after the meltdown and the nuclear phaseout.

As seen, while there is little evidence that more Germans self-categorized as a 3 or 2 on a scale from 0 to 10, the empirical models show a movement into the very right tail of the risk aversion distribution. This means that more Germans self-categorized as a 1 or even a 0 on the risk assessment scale. Immediately after Fukushima, the share of "very risk averse" people increased by 1.6 ppt from a baseline of 10 %, and the share of "extremely risk averse" people increased by 2 ppt from a baseline of 5%. After it became certain that the oldest nuclear reactors would remain permanently shut down and that the remaining ones would be shut down in the future, extreme risk aversion decreased again to the pre-Fukushima baseline level. Finally, in Table 1.24, we demonstrate that netting out unobserved individual heterogeneity via individual fixed effects matters for risk aversion. The unbalanced OLS models show attenuated and insignificant effects, while focusing on changes in risk aversion for the same individuals over 2 years yields larger and more precisely estimated effects. This finding is in line with Hanaoka et al. (2015).

While one might expect that risk perception changes as a result of the disaster and the ensuing policy action, the case for a change in risk aversion is less clear. We argue that the interplay between three reasons might cause a change in risk aversion: (i) a huge catastrophic event with large-scale loss of lives as a trigger, (ii) a widespread and prolonged media coverage of the event as a framing (Tversky and Kahneman 1981), and (iii) a sceptic pre-disposition of the population towards nuclear power (there is a long history of the anti-nuclear movement in Germany), as people who are less favourable towards a particular technology are also shown to impose a higher risk premium on it (Rottenstreich and Hsee 2001).

1.5.2.3.2 Do risk aversion and concern effects differ by exogenous distances to reactors? Operating channels Now, we exploit the exact distances between respondents' residencies and the nearest nuclear power plant, as illustrated in Figure 1.2. The idea is to stratify the risk aversion and concern results by exogenous distance indicators to learn more about operating channels. While people may sort into or out of close proximity to nuclear plants based on their preferences, the exogenous nature of the disaster is very likely uncorrelated with such endogenous residential sorting in the short run. We exploit this fact below in Tables 1.4 and 1.5. The exogeneity assumption would be violated if a significant share of respondents deliberately moved away from or towards a nuclear power plant in the months after the Fukushima disaster. We run robustness checks to test for endogenous residential sorting by excluding movers.[24]

24. In columns (2) and (3) of Table 1.20 in Section 1.8, we show that the main results are robust to excluding individuals who live outside a 50-km radius of their birth place and individuals who moved in the previous time period.

Technically, in Tables 1.4 and 1.5, we add distance indicators both in levels and as interaction terms with $PostMarch11_{i,2011} * 2011$ and $PostMay30_{i,2011} * 2011$. As discussed in Section 1.3, we generate three distance indicators, *within 50 km to reactor*, *within 50 km to 80 km to reactor*, and *nearest reactor among eight oldest*. In addition to the three-way interaction terms, we also add the corresponding two-way interactions terms to the model, i.e., [*variable of interest*] $*2011$, [*variable of interest*] $* PostMarch11_{i,2011}$, and [*variable of interest*] $* PostMay30_{i,2011}$. Note that, since we also include individual fixed effects, the distance indicators in levels drop out when excluding movers, and are only identified by movers otherwise. Also note that we consistently include lagged distance indicators, that is, distance indicators lagged by one time period in order to take on pre-treatment values (which reduces concerns about endogeneity). Finally, note that, instead of using separate regressions with triple interaction terms for households within a 50km radius and for households within a 50 to 80km radius to the nearest nuclear power plant, an alternative way to look at these heterogeneous effects would be to include all of these triple interaction terms in a single regression, with the reference category then being all households living further away than 80km to the nearest nuclear power plant: when doing so, the results remain virtually identical, both for environmental concerns and for risk aversion. They also remain virtually identical when controlling for rental prices in these heterogeneity (spatial) analyses.[25]

A priori, one could hypothesize that the Fukushima disaster changed respondents' risk *perceptions* and environmental concerns via altering their subjective assessment of

i The probability of a nuclear disaster outside of Germany or

ii The probability of a nuclear disaster inside of Germany

Finally, one could hypothesize that the

iii Perceived risk did not change, but people adjusted the degree to which they are *willing to tolerate* these risks due to Fukushima, i.e., they became more or less risk averse.

Empirically, it is very challenging to unambiguously discriminate between these channels as there is evidence that concern and risk perception may be distinct phenomena – especially in case of concerns about nuclear accidents (Slovic 1987; Sjöberg 1998). Obviously, the findings in Table 1.3 reinforce hypothesis (iii), as does the quote of Angela Merkel.

25. The final sample includes both households that are house owners and households that are renters. To control for rents while not losing households that are house owners, we generate a new variable for rents that includes actual rents for renters and hypothetical rents for house owners. The latter is a special category in the SOEP which is obtained from an item that asks respondents who are house owners to convert their house prices into rents by estimating them.

Table 1.1: Effects of the Meltdown and the Permanent Shutdown on Environmental Concerns in Germany

	Very concerned about the environment			
	OLS (1)	OLS (2)	FE (3)	FE (4)
$PostMarch11_{i,2011} * 2011$	0.0677***	0.0671***	0.0713***	0.0713***
("after meltdown")	(0.0164)	(0.0163)	(0.0088)	(0.0088)
$PostMay30_{i,2011} * 2011$	-0.1026***	-0.1040***	-0.0984***	-0.0994***
("after shutdown")	(0.0287)	(0.0287)	(0.0159)	(0.0159)
$PostMarch11_{i,2011}$	0.0013	0.0022		
	(0.0112)	(0.0111)		
$PostMay30_{i,2011}$	0.0029	0.0045		
	(0.0185)	(0.0183)		
2011	-0.0017**	-0.0017*	-0.0018***	-0.0018***
	(0.0009)	(0.0009)	(0.0005)	(0.0005)
Controls				
Demographic characteristics	No	Yes	No	Yes
Educational characteristics	No	Yes	No	Yes
Labor market characteristics	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	0.0037	0.0138	0.0061	0.0075
N	20,178	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop out in the FE models. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.2: Effects on Alternative Well-Being Measures in Germany

	Life satisfaction (1)	Happiness (2)	Sadness (3)	Very concerned about climate change (4)
$PostMarch11_{i,2011} * 2011$	0.0344	0.0047	0.0453***	0.0575***
(“after meltdown”)	(0.0318)	(0.0075)	(0.0102)	(0.0072)
$PostMay30_{i,2011} * 2011$	-0.0455	-0.0158	-0.0374	-0.0551***
(“after shutdown”)	(0.0526)	(0.0188)	(0.0264)	(0.0141)
Controls				
Demographic characteristics	Yes	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes	Yes
Labor market characteristics	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	0.0198	0.0089	0.0065	0.0058
N	20,178	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. Happiness and sadness measures refer to the four weeks prior to the interview (see Section 1.3). April 11, 2011, and June 30, 2011, are the cutoff dates for these models. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1, except for column (1), which is an ordered probit model.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.3: Effects on Risk Aversion in Germany

	Risk averse (1)	Moderately risk averse (2)	Very risk averse (3)	Extremely risk averse (4)
$PostMarch11_{i,2011} * 2011$	-0.0013	-0.0048	0.0163***	0.0200***
("after meltdown")	(0.0095)	(0.0079)	(0.0060)	(0.0041)
$PostMay30_{i,2011} * 2011$	-0.0100	-0.0100	0.0021	-0.0273***
("after shutdown")	(0.0163)	(0.0123)	(0.0104)	(0.0080)
Controls				
Demographic characteristics	Yes	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes	Yes
Labor market characteristics	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	0.0129	0.0091	0.0085	0.0064
N	20,178	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. The dependent variables are dummy variables which equal one if the individual is risk averse (0–3/10 on the risk attitude scale), moderately risk averse (0–2/10 on the risk attitude scale), strongly risk averse (0–1/10 on the risk attitude scale), and extremely risk averse (0/10 on the risk attitude scale). The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.4: Effects on Environmental Concerns by Distance to Reactors in Germany

	Very concerned about the environment		
	Within 50 km to reactor	Within 50 km to 80 km to reactor	Nearest reactor among eight oldest
$PostMarch11_{i,2011} * 2011 *$ [columnheader]	-0.0167	0.0351***	-0.0005
	(0.0136)	(0.0127)	(0.0115)
$PostMay30_{i,2011} * 2011 *$ [columnheader]	-0.0175	-0.0612**	-0.0698***
	(0.0279)	(0.0286)	(0.0271)
$PostMarch11_{i,2011} * 2011$ (“after meltdown”)	0.0757***	0.0641***	0.0714***
	(0.0092)	(0.0095)	(0.0104)
$PostMay30_{i,2011} * 2011$ (“after shutdown”)	-0.0918***	-0.0870***	-0.0592***
	(0.0185)	(0.0169)	(0.0225)
Controls			
Socioeconomic characteristics	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes
R^2	0.0077	0.0080	0.0080
N	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. All distance dummy variables as indicated in the column headers are lagged by one time period to have the pre-treatment values (see Descriptive Statistics in Section 1.7). The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1. $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ are time-invariant dummy variables which drop out in the FE models. As such, all three-way interaction terms could be regarded as two-way interactions terms.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.5: Effects on Risk Aversion by Distance to Reactors in Germany

	Very risk averse		
	Within 50 km to reactor	Within 50 km to 80 km to reactor	Nearest reactor among eight oldest
$PostMarch11_{i,2011} * 2011 *$	0.0279***	-0.0073	0.0156**
[columnheader]	(0.0081)	(0.0077)	(0.0075)
$PostMay30_{i,2011} * 2011 *$	-0.0763***	0.0187	-0.0489***
[columnheader]	(0.0173)	(0.0225)	(0.0176)
$PostMarch11_{i,2011} * 2011$	0.0086	0.0178***	0.0084
("after meltdown")	(0.0065)	(0.0062)	(0.0070)
$PostMay30_{i,2011} * 2011$	0.0245	-0.0018	0.0287
("after shutdown")	(0.0218)	(0.0117)	(0.0247)
Controls			
Socioeconomic characteristics	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes
R^2	0.0095	0.0085	0.0089
N	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. All distance dummy variables as indicated in the column headers are lagged by one time period to have the pre-treatment values (see Descriptive Statistics in Section 1.7). The dependent variable is a dummy variable which equals one if the individual is very risk averse (0–1/10 on the risk attitude scale). The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1. $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ are time-invariant dummy variables which drop out in the FE models. As such, all three-way interaction terms could be regarded as two-way interactions.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.6: Effects on Life Satisfaction by Distance to Reactors in Germany

	Life satisfaction		
	Within 50 km to reactor	Within 50 km to 80 km to reactor	Nearest reactor among eight oldest
$PostMarch11_{i,2011} * 2011 *$ [columnheader]	-0.0072	0.0215	0.0042
	(0.0393)	(0.0496)	(0.0390)
$PostMay30_{i,2011} * 2011 *$ [columnheader]	-0.1105	-0.0505	0.0752
	(0.0976)	(0.0938)	(0.0870)
$PostMarch11_{i,2011} * 2011$ ("after meltdown")	0.0777	0.0825	0.0534
	(0.0691)	(0.0796)	(0.0336)
$PostMay30_{i,2011} * 2011$ ("after shutdown")	-0.0314	-0.0580	-0.0877
	(0.0561)	(0.0539)	(0.0718)
Controls			
Socioeconomic characteristics	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes
R^2	0.0095	0.0085	0.0089
N	20,178	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. All distance dummy variables as indicated in the column headers are lagged by one time period to have the pre-treatment values (see Descriptive Statistics in Section 1.7). The dependent variable is life satisfaction on a 0 to 10 scale. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1. $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ are time-invariant dummy variables which drop out in the FE models. As such, all three-way interaction terms could be regarded as two-way interactions.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

In Table 1.4, the Fukushima and policy effects are stratified by the respondents' distance to the nearest power plant, using concern levels as outcome variable. We use pre-treatment values, i.e. values lagged by one time period, for both distances to plants and types of plants in order to avoid stratifying by endogenous variables. We see that respondents who lived close to one of the oldest reactors were not significantly more concerned after Fukushima (as compared to the general population) but significantly less after the policy decision to not power up the oldest reactors again. Respondents in the 50 to 80 km (31 to 50 mi) distance to the next nuclear plant reacted significantly more strongly in both directions after Fukushima and the phaseout.[26] Here, the reactions are not only a function of the distance to the next plant but also of the temporary vs. permanent shutdown of the oldest reactors.

Column (1) of Table 1.5 investigates the risk aversion results for respondents who live within 50 km distance of the next nuclear power plant.[27] Column (2) stratifies on respondents who live between 50 and 80 km distance to the next plant. Column (3) stratifies on respondents whose closest reactor is among the eight oldest that were immediately (but at that time only temporarily) shut down on March 14 and then permanently shut down on May 30, 2011. We use pre-treatment values, i.e. values lagged by one time period, for both distances to plants and types of plants in order to avoid stratifying by endogenous variables.

The findings show that respondents who live within a 50-km radius of nuclear reactors were those who became more risk averse. Post-Fukushima, they were much more likely to indicate the risk aversion categories 0 or 1 on a scale from 0 to 10. After the phaseout, their subjective risk aversion level shifted again, now away from the right tail. This finding is reinforced by the fact that the subsequent decrease in risk aversion is driven by respondents whose next reactor was one of the eight oldest. This subgroup saw an increase in risk aversion after Fukushima and then experienced a decrease when it became clear that their next reactor would remain permanently shut down.

Table 1.6 replicates the above heterogeneity analysis for life satisfaction as outcome: in accordance with our insignificant average treatment effects for life satisfaction in our main analysis, we do not find significant heterogeneous impacts on this evaluative measure of subjective well-being by distance to the nearest nuclear power plant, neither for the disaster nor for the policy action.

26. The fact that we do not find differential effects for those in close proximity, with less than 50 km, may be explained by concern level-based sorting into residencies close to nuclear plants. We also tried stratifying by the following three measures: (a) whether the closest nuclear power plant will be shut down before 2022 and (b) whether the closest nuclear power plant will not be shut down (exploiting the fact that some Germans live in close distance to nuclear power plants in France and Switzerland, which are not affected by the policy action in Germany). However, we did not find evidence for differential effects by (a) and (b).

27. The results are robust to alternate cutoff radii.

The finding that individuals' general risk aversion depends on the degree of local risk factors is fascinating. Although the exact interpretation hinges on what exactly the SOEP risk aversion scale measures (c.f. Dohmen et al. 2011), the finding provides evidence for the validity of both hypotheses (ii) and (iii). It may also be seen in stark contrast to Hanaoka et al. (2015) who estimate post-Fukushima changes in risk preferences for Japan and who differentiate the effects by affected regions. They show that (directly) affected individuals became *less* risk averse after the disaster.

To make sense of the diverging results, it is important to keep in mind that – as compared to Germany – the institution of nuclear energy is deeply embedded in Japanese society. First, from a political party perspective, anti-nuclear energy campaigns have no tradition in Japan (apart from support by the Communist party). Until Fukushima, Japan did not even have a Green party. Second, from a geopolitical perspective, nuclear energy is much more important to Japan than it is to Germany. Unlike Germany, Japan is not embedded in a political economic union such as the EU and wants to maintain an independent energy supply. Further, since Fukushima, the massive imports of fuel and liquid gas are a heavy burden, not only on Japan's trade deficit but also on its image as a leading player in the development of "clean, CO_2-free" energy. Many Japanese believe an energy policy without nuclear energy would be unrealistic. This opinion is supported by recent survey data: only 15% of Japanese citizens support a nuclear phaseout, whereas 71% of all Germans do (World Nuclear Association 2015).

Overall, the findings in Table 1.4 underline those in Table 1.5 and suggest that local environmental risk factors matter, at least after important events related to these local risk factors.

To summarize and reiterate, the differential risk aversion, scaring, and relieving effects strongly speak in favor of hypotheses (ii) and (iii) above. The operating channels through which distant disasters affect individual's concerns appear to work primarily through the (re-)evaluation of local risks and an adjustment in the willingness to tolerate small risks with high stakes. Further evidence is provided by representative polls which indicate that, immediately after the Fukushima disaster, 70% of Germans believed that Fukushima could also happen in Germany, whereas, in July 2009, some 44% indicated "trust" or "big trust" in the safety of reactors in Germany (Infratest Dimap 2009, 2011b).

1.5.2.3.3 Concern effect heterogeneity by sociodemographics Next, in Table 1.7, we investigate effect heterogeneity by sociodemographics. We stratify the concern levels in our preferred specification in column (4) of Table 1.1 by being risk averse, being female, being above

40 years old, and being a Green party supporter.[28] We use pre-treatment values, i.e. values lagged by one time period, for being risk averse and being a Green party supporter in order to avoid stratifying by endogenous variables. Table 1.7 provides clear and strong evidence that women (i) incurred an about 4 ppt greater scaring effect after the disaster and (i) an about 6 ppt greater relieving effect after the policy action as compared to men. However, we fail to find differential treatment effects by age and for risk aversion (columns (1) and (3)). The latter point is interesting in light of the discussion about the conceptual idea behind risk aversion as compared to concerns. Our finding that changes in concern levels and risk aversion are *not* correlated, while the geographic distance matters for both, supports the hypothesis that concerns and risk perception may indeed be distinct phenomena (Slovic 1987; Sjöberg 1998).

Column (4) of Table 1.7 shows the effects for Green party supporters. Those are presumably individuals who are strongly in favor of a nuclear phaseout and have always warned about the dangers of a nuclear accident and its consequences. Green party supporters are also known for their high environmental consciousness. We find that – in contrast to the general population – immediately after Fukushima, their concern levels did *not* significantly increase. Since those individuals were always concerned about nuclear accidents and had always high environmental concern levels, they obviously were not particularly surprised by Fukushima. In contrast, after the sharp turnaround in conservatives' attitudes towards nuclear energy and the unexpected phaseout decision, Green supporters were significantly more relieved than the rest of the population. In fact, (female) Green supporters are driving the observed phaseout relief effect.

1.5.2.4 Effects of Fukushima on Political Party Support

We now take the last findings a step further and exploit a rich battery of political support questions in the SOEP. To be precise, we generate nine political support outcome variables as discussed in Section 1.3. Those measures are used as dependent variables in a regression framework as in Eq. 1.1 along with individual fixed effects. That is, we run the model in column (4) of Table 1.1 with the outcome variables as indicated in the column headers of Table 1.8.

28. We use 40 years to split the sample into those individuals who actively (age 15 and above) experienced the Chernobyl catastrophe and those who did not. In doing so, we distinguish individuals born before and after 1971. This is also the point in time at which the anti-nuclear movement in Germany started to take pace, with widespread protests against the Breisach, Esenshamm, Neckarwestheim (built), and Bonn plants (Radkau 1983).

Table 1.7: Effects on Environmental Concerns by Sociodemographics in Germany

	Very concerned about the environment			
	Risk averse	Female	Above 40	Supports the Greens
$PostMarch11_{i,2011} * 2011 *$	-0.0083	0.0409***	0.0035	0.0060
[columnheader]	(0.0126)	(0.0099)	(0.0115)	(0.0217)
$PostMay30_{i,2011} * 2011 *$	0.0382	-0.0616***	-0.0188	-0.1018**
[columnheader]	(0.0238)	(0.0225)	(0.0256)	(0.0418)
$PostMarch11_{i,2011} * 2011$	0.0753***	0.0499***	0.0687***	0.0662***
("after meltdown")	(0.0099)	(0.0104)	(0.0119)	(0.0122)
$PostMay30_{i,2011} * 2011$	-0.1147***	-0.0672***	-0.0861***	-0.0417*
("after shutdown")	(0.0183)	(0.0196)	(0.0253)	(0.0231)
Controls				
Socioeconomic characteristics	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	0.0183	0.0084	0.0076	0.0069
N	20,178	20,178	20,178	11,859

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. All sociodemographic dummy variables as indicated in the column headers are lagged by one time period to have the pre-treatment values (see Descriptive Statistics in Section 1.7). The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1. $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ are time-invariant dummy variables which drop out in the FE models. As such, all three-way interaction terms could be regarded as two-way interactions terms.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.8: Effects on Political Outcomes in Germany

	Political party support							Support intensity	
	General (1)	SPD (2)	Greens (3)	CDU/CSU (4)	FDP (5)	Left (6)	Government (7)	Strong (8)	Weak (9)
$PostMarch11_{i,2011} * 2011$	0.0057	-0.0102*	0.0175***	0.0084	-0.0097**	0.0031	0.0008	0.0369***	-0.0198***
(“after meltdown”)	(0.0073)	(0.0057)	(0.0054)	(0.0055)	(0.0043)	(0.0036)	(0.0039)	(0.0140)	(0.0065)
$PostMay30_{i,2011} * 2011$	0.0058	-0.0204*	-0.0055	0.0035	0.0111	0.0011	0.0134*	-0.0373	0.0166
(“after shutdown”)	(0.0116)	(0.0115)	(0.0110)	(0.0095)	(0.0069)	(0.0072)	(0.0071)	(0.0254)	(0.0123)
Controls									
Socioeconomic characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0122	0.0079	0.0187	0.0072	0.0220	0.0079	0.0071	0.0058	0.0100
N	20,178	11,859	11,859	11,859	11,859	11,859	11,859	11,415	11,415

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop out in the FE models. The dependent variables are dummy variables that equal one if the individual supports a political party in general or supports the SPD, Greens, CDU/CSU, FDP, the Left, or the government, respectively; and if this support is strong or weak, respectively. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Column (1) shows no evidence that overall political party support changed after Fukushima or the phaseout decision. For individuals who support a political party, columns (2) to (6) test whether support for the six parties that are represented in the German parliament *Bundestag* increased or decreased. In line with anecdotal evidence and intuition, after Fukushima, support for the Green party – whose main political objective has always been the phaseout of nuclear energy – significantly increased by about 1.8 ppt from a baseline level of 15%. The pro-nuclear Free Democratic Party (FDP) lost about 1 ppt from a low baseline of 5%, and the center-left Social-Democrats (SPD) lost about 1 ppt from a baseline of 30%. There was no significant movement for the other parties, i.e., the center-right CDU/CSU and the far left party "The Left."

In sum, the Greens gained voter sympathies mostly from the center-left voter spectrum, while the pro-nuclear FDP lost support. This finding is in line with actual election outcomes in two German states. In Baden-Württemberg, a prosperous traditionally conservative state in the south of Germany, a state election was held on March 27, 2011, i.e., two weeks after Fukushima. In this election, the Greens doubled their voter shares by 12.5 ppt and became the second biggest party with 24.2% of the total votes. They gained over proportionally from former SPD voters and non-voters (Tagesschau 2011). In Rheinland-Pfalz, elections also took place on the same day and the Greens gained 10.8 ppt from a baseline of 4.6%.

While our empirical specifications do not suggest that support for parties in general increased – i.e., the extensive margin remained stable – the last two columns of Table 1.8 provide evidence that there was movement on the intensive margin. In other words, people who were already politically interested and in favor of a political party intensified their support after Fukushima, at least according to self-reports in the SOEP. We also find some evidence that the government may have slightly benefited from the decision to pass a phaseout bill quickly.

1.5.2.5 Well-Being and Green Party Effects by Distances to Reactors in Switzerland and the UK

As a last exercise, we test whether and how residents in Switzerland and the UK reacted to the Fukushima disaster (there was no comparable policy action in any of these countries). To do so, we exploit the panel data sets *Understanding Society* and the *Swiss Household Panel (SHP)*. The results are presented in Tables 1.9 and 1.10. For both countries, we exploit the exogenous timing of the disaster alone and in combination with respondents' distances to the nearest reactor lagged by one time period. Dependent variables measure well-being and support for the Green party.[29] Maps of the UK and Switzerland along with the location of their nuclear reactors are in Figures 1.7 and 1.8.[30] The descriptive statistics for the UK and Swiss are in Section 1.7, and the models that we run are essentially identical to Eq. 1.1. Tables 1.16 and 1.17 of Section 1.8 show the means of the covariates separately for pre- and post-March 11 interview dates along with the normalized differences. As for Germany, there is not much evidence for a significant covariate imbalance. All normalized differences are well below 0.25.

Table 1.9 shows the results for Switzerland which borders Germany and operates four nuclear power plants (see Figure 1.7). The odd columns only show the main effect for $PostMarch11_{i,2011} * 2011$, and the even columns additionally stratify on the distance to the next reactor. The first two columns exploit *life satisfaction* as an outcome variable, the next two columns *supports the Greens*, and the last two columns a binary variable which indicates whether respondents value environmental protection higher than economic growth. While not perfectly comparable, the latter variable is similar to the one surveyed in Germany.

In line with the findings from Germany, there is no evidence that life satisfaction was negatively affected by the disaster (columns (1) and (2)). Contrarily, and again in line with the findings from Germany, voter sympathies for the Greens increased by a significant 2.6 ppt and particularly among respondents who live within 25-50 km of nuclear reactors (column (4)).

Finally, the share of respondents who value environmental protection more than economic growth increased by a significant 1.9 ppt over the entire year (Column (5)). The latter finding reinforces that the policy action in Germany significantly contributed to the decrease in environmental concerns in 2011.[31] Recall that for Germany and 2011, we find a significant decrease in environmental concerns following the *Nuclear Phase-Out Bill*, resulting in a level of environmental concerns at the end of 2011 (and in 2012) that was not significantly higher than before Fukushima (see Figure 1.4 and Tables 1.1 and 1.11 below).

29. Unfortunately, we cannot exploit risk aversion measures for the UK and Switzerland since they were only surveyed for one cross section (that also only includes very few post-Fukushima respondents) in both countries (and we show that addition of individual fixed effects matters in Table 1.24 , also see Hanaoka et al. (2015).

30. In Figure 1.7, the yellow triangle that lies outside the Swiss territory is the French nuclear power plant *Fessenheim*, which lies within a 100 km radius of Switzerland.

31. Note that environmental concerns were only surveyed in waves 11 and 13 of the SHP. Since respondents of each wave are interviewed between September and February, the models are essentially comparing individual responses between September 2009 and February 2010 to responses between September 2011 and February 2012. The employed fixed effects models net out individual unobserved heterogeneity and solely focus on changes in the responses between these two waves. *Understanding Society* does not include environmental concerns which is why we cannot test if they remain elevated for the UK.

Table 1.9: Effects of the Meltdown on Well-Being, Political Outcomes, and Environmental Concerns in Switzerland

	Waves 11–14 (2009–2012)				Waves 11 vs. 13 (2009 and 2011)	
	Life satisfaction		Vote Green		Environmental protection more important than GDP	
	(1)	(2)	(3)	(4)	(5)	(6)
$PostMarch11_{i,2011} * 2011$ ("after meltdown")	0.036	0.005	0.026***	0.010	0.019***	0.021
	(0.028)	(0.042)	(0.008)	(0.011)	(0.005)	(0.014)
$PostMarch11_{i,2011} * 2011 *$ 0–25 km radius to reactor		0.067		0.019		-0.004
		(0.045)		(0.013)		(0.016)
$PostMarch11_{i,2011} * 2011 *$ 25–50 km radius to reactor		0.033		0.026**		-0.005
		(0.037)		(0.011)		(0.016)
$PostMarch11_{i,2011} * 2011 *$ 50–80 km radius to reactor		0.001		0.010		0.003
		(0.043)		(0.013)		(0.019)
Controls						
Socioeconomic characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	No	No
R^2	0.011	0.011	0.010	0.011	0.011	0.011
N	14,104	14,104	14,104	14,104	9474	9474

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: The dependent variables are defined as follows: Models (1) and (2) use life satisfaction on a scale from 0 to 10. Models (3) and (4) use a dummy variable which equals one if the individual votes for either the Swiss Ecology Party or the Green Liberals when asked: "If there was an election for the National Council tomorrow, for which party would you vote?". Models (5) and (6) use a dummy variable which equals one if the individual prefers environmental protection over economic growth. The controls include age, age squared, being female, being married, being disabled, having Swiss citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being regularly employed, being in a managerial position, being in a contracted position, being self-employed, being unemployed, being retired, being a student, being out of the labour force, and the household income. Since the Swiss Household Panel only conducts interviews from September to February in a given wave, we cannot employ month fixed effects along with $PostMarch11_{i,2011}$ and $PostMarch11_{i,2011} * 2011$.

Source: SHP w11–14, 2009–2012, balanced panel, own calculations

Table 1.10: Effects of the Meltdown on Well-Being and Political Outcomes in the UK

	Waves 2-3 (2010-2012)			
	Happiness		Vote or support Green party	
	(1)	(2)	(3)	(4)
$PostMarch11_{i,2011} * 2011$	0.009	0.008	-0.001	-0.002
("after meltdown")	*(0.010)*	*(0.011)*	*(0.003)*	*(0.003)*
$PostMay30_{i,2011} * 2011$	0.004	0.010	0.000	0.001
("placebo nuclear phase-out")	*(0.008)*	*(0.010)*	*(0.002)*	*(0.003)*
$PostMarch11_{i,2011} * 2011 *$ 0–25 km		-0.036		0.026***
radius to reactor		*(0.026)*		*(0.009)*
$PostMarch11_{i,2011} * 2011 *$ 25–50		0.002		-0.003
km radius to reactor		*(0.021)*		*(0.006)*
$PostMarch11_{i,2011} * 2011 *$ 50–80		0.009		0.003
km radius to reactor		*(0.018)*		*(0.005)*
$PostMay30_{i,2011} * 2011 *$ 0–25 km		-0.002		-0.013
radius to reactor		*(0.035)*		*(0.012)*
$PostMay30_{i,2011} * 2011 *$ 25–50 km		-0.024		-0.000
radius to reactor		*(0.025)*		*(0.007)*
$PostMay30_{i,2011} * 2011 *$ 50–80 km		-0.024		0.001
radius to reactor		*(0.021)*		*(0.006)*
Controls				
Socioeconomic characteristics	Yes	Yes	Yes	Yes
Individual, year, and month fixed effects, linear time trend	Yes	Yes	Yes	Yes
R^2	0.006	0.006	0.003	0.003
N	**46,406**	**46,406**	**46,406**	**46,406**

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: The dependent variables are defined as follows: Models (1) and (2) use happiness on a scale from 1 to 4. Models (3) and (4) use a dummy variable which equals one if the individual votes for or supports the Green Party. The controls include age, age squared, being female, being married, being disabled, the number of children in the household, having no qualification, having a secondary degree, having a tertiary degree, having another qualification, being in paid employment, being self-employed, being unemployed, being retired, being on maternity leave, being retired, being a student, and the household income.

Source: Understanding Society w2–3, 2010–2012, balanced panel, own calculations

Table 1.10 provides the effects for the UK, for the main model and the refined version where we stratify on the distance to the next nuclear reactor (Figure 1.8). Once more, we do not find evidence that happiness changed after Fukushima, neither in the UK as a whole nor for people living in close proximity to plants. However, as for Germany and Switzerland, Green voter support increased by a significant 2.6 ppt among UK residents who lived in close distance to a reactor (column 4). Finally, including May 30 as placebo phaseout date shows that the effects for the UK are small and insignificant in size.

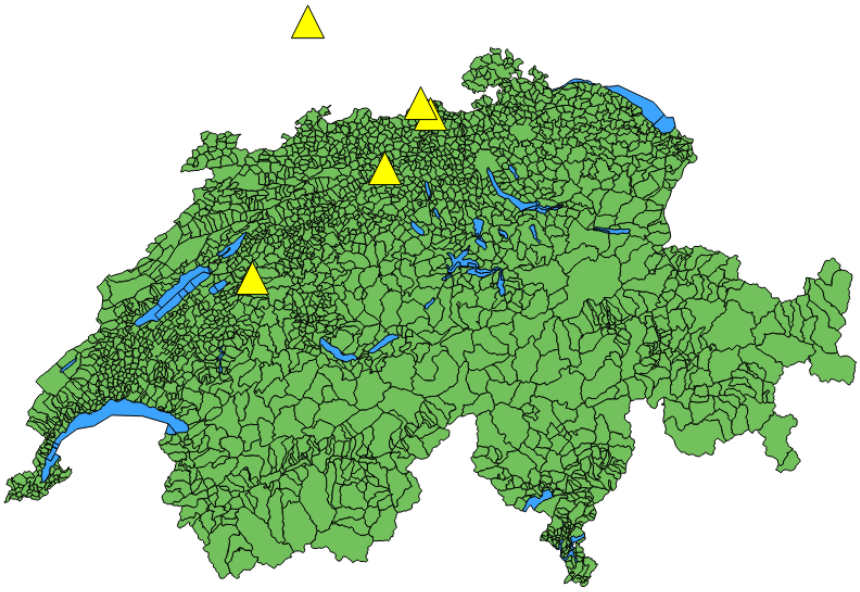


Figure 1.7: Nuclear Power Plants and Respondents' Residency (SHP) in Switzerland

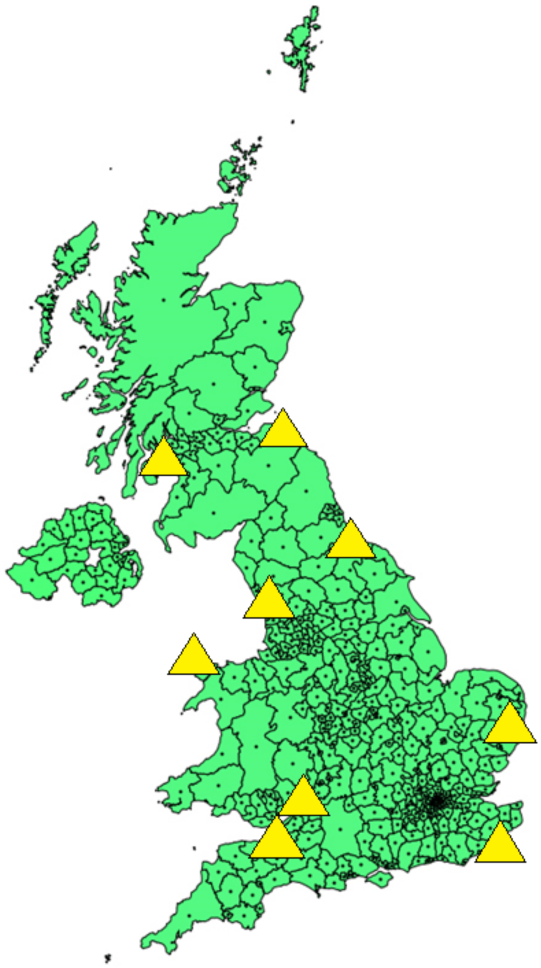


Figure 1.8: Nuclear Power Plants and Respondents' Residency (Understanding Society) in the UK

1.5.2.6 Robustness Checks

Tables 1.20, 1.21, and 1.22 in Section 1.8 provide a series of robustness checks for our standard estimate for Germany, which is the fixed effects model in column (4) of Table 1.1. In our baseline specifications, we chose the date when the *Nuclear Phase-Out Bill* was unexpectedly announced by the hitherto pro-nuclear conservative government, May 30, 2011. However, the parliament in Germany formally passed the bill with a large majority of 513/600 votes on June 30, 2011. In column (1) of Table 1.20, we employ this alternative policy date which delivers robust results.

Column (2) excludes individuals who live outside a 50-km radius of their birthplace, and column (3) excludes individuals who moved in the previous time period. Excluding movers, in particular those individuals who moved away from their birthplace, eliminates potential endogenous residential sorting into different regions based on environmental concerns. The results are robust.

In column (4) of Table 1.20, we focus on pre-scheduled interviews only. Self-completed interviews without the presence of a trained interviewer may induce measurement error in the interview date. In addition, respondents may have postponed the completion of the questionnaire due to the Fukushima catastrophe. Excluding almost half of all interviews does not alter the results. In Table 1.19, we demonstrate the robustness of the results using three additional interview mode specifications.

The first two columns of Table 1.21 add (i) a linear time trend that starts after the disaster, March 11, as well as (ii) a monthly time trend that starts after the policy action, May 30, in addition to the linear time trend over the entire observation period. In column (3), we include a quadratic time polynomial in addition to the linear time trend. One concern with the identification of the policy action may be that after the sharp increase in environmental concerns, they would have decreased even without the announcement of the *Nuclear Phase-Out Bill* due to a decrease in media coverage and disaster-related consciousness (see Section 1.4.2 for further discussions). All three specifications show that the identification of the policy action is largely robust to the inclusion of split and linear time trends, as well as quadratic time polynomials in addition to year, month, and individual fixed effects.

We also checked whether environmental concerns significantly affect panel attrition over the 2 years. This is not the case; panel attrition between 2010 and 2011 accounts only for 5% of the 2010 sample.

Finally, we estimate a pure Regression Discontinuity (RD) model, using only the year 2011 and cutoff dates of 45 days around the disaster and the policy action. Table 1.25 in Section 1.8

shows the results. Both effects are robust to this specification.

1.5.2.7 Placebo Regressions

We employ several placebo regressions in Table 1.22. In column (1), we use our baseline specification but employ placebo disaster and policy action dates, March 11 and May 30, 2010. In column (2), we do the same with placebo disaster and policy action dates for 2012, March 11 and May 30, 2012. All estimates are close to zero in size and statistically insignificant.

Columns (3) to (6) employ alternative placebo regressions using dependent variables that are arguably unrelated to the Fukushima catastrophe. To be precise, we use questions about concerns about (i) job security, (ii) health, (iii) the economy, and (iv) crime. Otherwise, the specifications are identical to our baseline specifications. All estimates are small in size and statistically insignificant.

Finally, in Table 1.22 of Section 1.8, we show the results of a series of placebo regressions for 2011 using May 15, June 15, and July 15 as alternative policy action dates. All of the estimates are small in size and statistically insignificant.

Whereas, so far, we have looked at short-run effects, covering the year just before and the year of the disaster, we next look at medium to long-run effects, and in this context, compare the Fukushima to the Chernobyl disaster. For this purpose, we include one year after (in case of Fukushima) and up to three years after the event (in case of Chernobyl). Moreover, we make more extensive use of pre-treatment information by including, in both cases, an additional year pre-treatment.

1.5.2.8 Medium-Run Effects

Table 1.11 tests medium to long-run effects and compares the identified effects of the Fukushima disaster to those of the Chernobyl disaster using the same data set, variables, and estimation techniques.

In columns (1) and (2), we test whether environmental concerns increased significantly in the medium-run due to the Fukushima catastrophe. For this purpose, we use the years 2009 to 2012 and estimate (unbalanced) OLS and (balanced) FE models. When estimating effects over a longer time period, we face a trade-off between considering unobservables through individual fixed effects and considering populations who did not participate in the survey at least once pre- and post-Fukushima. For example, for the years 2009 to 2012, we have a total of 57,492 person-year observations, but only 7935 individuals participated in all four waves from 2009 through 2012.

Table 1.11: Comparison of Meltdown and (Placebo) Policy Effects Between Fukushima and Chernobyl

	Very concerned about the environment				
	Fukushima OLS 2009-2012	Fukushima FE 2009-2012		Chernobyl OLS 1984-1989	Chernobyl FE 1984-1989
$PostMarch11_{i,2011}*$ 2011	0.0712***	0.0797***	$PostApril26_{i,1986}*$ 1986	0.1025***	0.1213***
("after meltdown")	*(0.0116)*	*(0.0104)*	("after meltdown")	*(0.0175)*	*(0.0151)*
$PostMay30_{i,2011}*$ 2011	-0.0871***	-0.1078***	$PostJuly15_{i,1986}*$ 1986	-0.0183	-0.0247
("after shutdown")	*(0.0191)*	*(0.0156)*	("placebo policy")	*(0.1686)*	*(0.0251)*
2010	0.0338***	0.0348***	1985	-0.0291***	-0.3000***
	(0.0054)	*(0.0042)*		*(0.0100)*	*(0.0088)*
2011	0.0003	0.0092	1986	-0.0869***	-0.1034***
	(0.0082)	*(0.0068)*		*(0.0116)*	*(0.0102)*
2012	0.0003	-0.0002	1987	0.0671***	0.0580***
	(0.0002)	*(0.0001)*		*(0.0108)*	*(0.0097)*
			1988	0.0494***	0.0375***
				(0.0106)	*(0.0092)*
			1989	0.1068***	0.0984***
				(0.0113)	*(0.0093)*
Controls					
Socioeconomic characteristics	Yes	Yes		Yes	Yes
Year and month fixed effects	Yes	Yes		Yes	Yes
Linear time trend	Yes	Yes		Yes	Yes
R^2	*0.0121*	*0.0087*		*0.0655*	*0.0281*
N	***57,492***	***57,492***		***62,540***	***62,540***

Standard errors are in parentheses and clustered at the interview date level

***** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$ for Fukushima and $PostApril26_{i,1986}$ and $PostJuly15_{i,1986}$ for Chernobyl, all of which drop out in the FE models. To save space, they are not displayed. While April 26, 1986, was the date of the disaster, the actual treatment status for Chernobyl is defined by interviews that took place after April 28, 1986, as the disaster only became public two days later. $PostJuly15_{i,1986}$ is the placebo policy date for Chernobyl, 81 days after the disaster. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant.

Source: SOEP v29, 1984–1989 and 2009–2012, unbalanced panel, own calculations

Note that the 2012 dummy variable identifies the general change in environmental concerns in 2012 and assumes that there were no other events that could have affected concerns in 2012. The estimated 2012 effect is small in size and statistically insignificant. The same is true for the 2011 effect, which we obtain when adding up the 2011 estimate and the disaster and policy action estimates.

1.5.2.9 Comparison to Long-Run Effects of the Chernobyl Disaster

Next, we replicate our baseline specification for the Chernobyl disaster to assess its medium to long-term effects on environmental worries in the German population. This also serves as a falsification test for the identified policy effect: in this test, we insert a placebo policy after the Chernobyl disaster, using the same time span as elapsed between the Fukushima disaster and the real policy action (*Atomausstieg*) taken thereafter, i.e. July 15, 1986, or 81 days after the disaster. Non-significance of this placebo policy action, together with a similar disaster effect for Chernobyl and a similar media reporting between Chernobyl and Fukushima, as shown above, provide *prima facie* evidence against the claim that our identified policy action after Fukushima is driven merely by a decrease in media attention or a return to baseline.

Columns (3) and (4) of Table 1.11 basically replicate columns (1) and (2) but employ the years 1984 to 1989 and April 28, 1986, the disaster date of the Chernobyl catastrophe.[32] As seen, after Chernobyl, the share of respondents who were "very concerned about environmental protection" increased by a highly significant 10 to 12 ppt. Relative to the baseline level of environmental concerns before Chernobyl, this represents an increase of about 25% – almost exactly the same increase that we find after Fukushima.[33] Thus, we argue that the two disasters are comparable in terms of their effects, particularly since we focus on Germany and use the same dataset, variable definitions, and estimation techniques. More importantly, the estimate for the placebo policy action in 1986 which would have occurred on July 15 – exactly 81 days after the disaster when the *Nuclear Phase-Out Bill* was announced in 2011 – is small in size and statistically insignificant. Moreover, the coefficients for 1987, 1988, and 1989 have a size of 4 to 10 ppt and are highly significant. This means that – in contrast to post-Fukushima – we seem to observe a persistent jump in environmental concerns post-Chernobyl.[34]

In Figure 1.10, we non-parametrically illustrate this persistent increase in environmental

32. Although the Chernobyl catastrophe happened on the evening of April 26, it took 2 days, until April 28, before the media started reporting about it.

33. The baseline level of environmental concern before Chernobyl (40%) was higher than before Fukushima (28%).

34. Also note that Metcalfe et al. (2011), who study the impact of 9/11 on mental well-being in the UK, still report a relatively large coefficient of 0.18 (which is significant at the 10% level) one year after the attacks. Since the immediate effect was 0.24, this implicitly means that we do not observe a "return-to-the baseline" effect for 9/11 in the UK.

concerns.[35] In the visual analogue to column (3) of Table 1.11, it is easy to see that environmental concerns substantially increased after Chernobyl and remained at their elevated level.

1.5.2.10 Replication of Richter et al. (2013)

In a research note, Richter et al. (2013) use the 2011 cross section of the SOEP to estimate the impact of Fukushima and the nuclear phaseout on life satisfaction and environmental worries in Germany. The research in this paper and in Richter et al. (2013) were carried out independently and without knowing from each other. Both working papers were published in summer 2013 in the *SOEPpapers Series* (#590 and #599).

One major difference between this paper and Richter et al. (2013) is that this paper focuses on risk aversion and political outcomes. In addition, it estimates the well-being and voter effects for the UK and Switzerland. Moreover, as already discussed in detail in Section 1.2, the identification approaches differs significantly. One main difference is that this paper exploits the SOEP panel structure and nets out unobserved individual heterogeneity.

Column (1) in Tables 1.26 and 1.27 (Section 1.8) replicate the main estimation results of Richter et al. (2013) on life satisfaction on environmental worries. The next four columns then (i) cluster differently and (ii) add time trends, (iii) month fixed effects, as well as (iv) individual fixed effects along with adding the year 2010 to the estimation sample.

With regard to life satisfaction, Richter et al. (2013) find a significantly positive effect of the nuclear phase out (but no effect of Fukushima itself). Table 1.26 shows that this positive nuclear phase out effect on life satisfaction disappears when one either adds linear time trends, month fixed effects, or individual fixed effects (columns (3) to (5)).

With regard to environmental concerns, Richter et al. (2013) find a significantly positive effect of Fukushima but only a small insignificant effect of the nuclear phase out (column (1)). Table 1.27 shows that one obtains our findings when one considers time trends or individual unobserved heterogeneity (columns (3) and (5)).

1.6 Discussion and Conclusion

This research shows that natural disasters can have significant effects on concerns, risk aversion, and voting preferences in presumably unaffected distant countries. Our findings show that the Fukushima disaster significantly increased environmental concerns among Germans. However,

35. As in Figure 1.4, we report daily averages. However, since we plot the daily averages over several years and most respondents were interviewed in the first months of a year, we observe jumps in the graph. To smooth them out, we disregard days with fewer than five interviews.

there is no empirical evidence that general well-being in the German, Swiss, or British population decreased as a result of the disaster. This finding is in line with research in the field of well-being which shows that life satisfaction measures are relatively robust to large-scale disasters and crises (Berger 2010; Deaton 2012; Ohtake and Yamada 2013; Tiefenbach and Kohlbacher 2015). Empirical checks on potential operating channels suggest that the effect on individuals' environmental concerns worked primarily through the (re-)assessment of risks of domestic reactors. After Fukushima, Germans were significantly more likely to report that they were "extremely risk averse" – in particular people who lived close to nuclear reactors – suggesting that humans adjust their risk tolerance levels after unexpected large-scale disasters. This finding is in line with recent research from China and Indonesia (Huang et al. 2013; Cameron and Shah 2013).

In contrast not only to the Swiss reaction to Fukushima, but also the German reaction to the Chernobyl disaster in 1986, Germans' environmental concerns decreased again after the hitherto pro-nuclear governing center-right coalition made a drastic and sharp turnaround in its energy policy. On May 30, 2011, Angela Merkel announced that a new bill would permanently shut down the eight oldest reactors and implement the staggered phaseout of the remaining ones. The bill was combined with a large-scale government program supporting the transition to renewables. We find that the reduction in environmental concerns was particularly strong among individuals who lived in close proximity to the eight oldest reactors, supporters of the Green party, and women.

Finally, we show that the disaster increased political support for the Green party in Germany, Switzerland, and the UK. It has always been one of the Green party's main objectives to phase out nuclear energy. While the increase in voter sympathies was universal in Germany, in Switzerland and the UK, it was concentrated among people who live in close distance to nuclear reactors. For Germany, we also find that the intensity of political party support increased significantly, while there is no evidence that the disaster triggered more political interest at the extensive margin.

In terms of magnitude, given a baseline of about 31% and an effect size of about 7 percentage points, the share of individuals who report to be very concerned about the environment increases by about 28% due to the Fukushima disaster in Germany; the policy action has a similar magnitude in the opposite direction. Compared to the Chernobyl disaster, which we also study using the same specification, the effect size is smaller (7 versus 12 percentage points), which makes sense since Germany is presumably not directly affected. Berger (2010) obtains 8 percentage points for Chernobyl using a different specification.

Complementary evidence shows that Germans are actually willing to pay for nuclear-free energy production, most likely in return for a lower level of environmental concerns. In representative polls, 70% claim that they would be willing to pay higher energy prices in return for the transition to renewables (Infratest Dimap 2011b). Part of the *Energiewende* is a fixed subsidy for every kilowatt hour (kWh) produced by renewables. The 18bn € annual cost of this policy is paid by consumers through a tax on electricty.[36] In 2013, this tax amounted to 5.3 Eurocent per kWh (Bundesregierung 2013). Since the average household consumes about 3500 kWh per year, it effectively pays 185 € or 15 € per month for the transition to renewables (EnergieAgentur NRW 2012). While this represents a federal mandatory tax, a study by Check24 (2012) finds that, before Fukushima, 37% of all consumers who switched their energy provider chose electricity from renewables. Immediately after Fukushima, this share doubled to 74% and was still 64% 1 year after the disaster. This is suggestive evidence that a change in environmental concerns and attitudes might translate into actual behaviour.

An obvious question is whether the results of this study carry over to other (non- European) countries. Complementary evidence for Switzerland and the UK is consistent with the German experience, but the effects are less pronounced. Besides such external validity issues, policy implications of this paper are clearly limited: its main point is to show that (i) disasters can have negative external effects on other countries, even if those countries are far away and not directly affected; (ii) these negative external effects exist even if the objective risk of a similar event has not changed as a result of the disaster; and (iii) policy action, if taken credibly and swiftly, can alleviate some of these negative external effects. The good news in terms of policy, therefore, is that policy is able to have an impact at all, even in the face of such a catastrophe.

How changes in concerns and risk tolerance ultimately translate into changes in actual economic behavior is a field for future research. The German experience teaches us that it can result in a rarely observed abrupt shift in long-term policies: a complete phaseout of nuclear energy.

36. Meanwhile, the *Energiewende* is exemplary with at least 65 countries – among them the USA – copying the subsidy (called "Einspeisevergütung") for renewables (Renewable Energy Policy Network for the 21st Century 2013).

1.7 Appendix to Chapter 1

Table 1.12: Descriptive Statistics – Germany (SOEP)

	Mean	Std. Dev.	Min.	Max.	Obs.
Dependent Variables					
Very concerned about the environment	0.3090	0.4621	0	1	20,178
Very concerned about climate change	0.3002	0.4584	0	1	20,178
Very concerned about job security	0.1121	0.3155	0	1	11,526
Very concerned about health	0.2022	0.4016	0	1	20,178
Very concerned about the economy	0.2946	0.4559	0	1	20,178
Very concerned about crime	0.3458	0.4756	0	1	20,178
Life satisfaction	6.9952	1.7295	0	10	20,178
Happiness	0.1349	0.3417	0	1	20,178
Sadness	0.5414	0.4983	0	1	20,178
Risk averse	0.3741	0.4839	0	1	20,178
Moderately risk averse	0.2194	0.4138	0	1	20,178
Very risk averse	0.1007	0.3009	0	1	20,178
Extremely risk averse	0.0501	0.2181	0	1	20,178
Supports political party	0.4949	0.5000	0	1	20,178
Supports SPD	0.3014	0.4589	0	1	11,859
Supports the Greens	0.1479	0.3550	0	1	11,859
Supports CDU/CSU	0.4045	0.4908	0	1	11,859
Supports FDP	0.0471	0.2119	0	1	11,859
Supports the Left	0.0739	0.2616	0	1	11,859
Supports the government	0.4537	0.4979	0	1	11,859
Strong political party support	0.4437	0.4968	0	1	11,415
Weak political party support	0.0590	0.2357	0	1	11,415
Demographic Characteristics					
Age	51.4397	17.0461	18	101	20,178
Age squared/100	29.3660	17.7809	3.2400	102.0100	20,178
Female	0.5260	0.4993	0	1	20,178
Married	0.6385	0.4805	0	1	20,178
Single	0.2119	0.4087	0	1	20,178
Disabled	0.1425	0.3496	0	1	20,178
No German nationality	0.0443	0.2057	0	1	20,178
Number of children in household	0.8252	1.1950	0	12	20,178
Educational Characteristics					
In school	0.0127	0.1119	0	1	20,178
Lower than secondary degree	0.1315	0.3380	0	1	20,178
Secondary degree	0.5337	0.4989	0	1	20,178
Tertiary degree	0.3221	0.4673	0	1	20,178
Labor Market Characteristics					
Full-time employed	0.3946	0.4888	0	1	20,178
Part-time employed	0.1198	0.3247	0	1	20,178
Out of the labor force	0.4164	0.4930	0	1	20,178

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	Mean	Std. Dev.	Min.	Max.	Obs.
On maternity leave	0.0178	0.1324	0	1	20,178
Unemployed	0.0507	0.2194	0	1	20,178
Household income	2832	1583	0	47,256	20,178
Heterogeneity					
Within 50 km to reactor (lagged)	0.2834	0.4506	0	1	20,178
Within 50 km to 80 km to reactor (lagged)	0.2000	0.4000	0	1	20,178
Nearest reactor among eight oldest (lagged)	0.4935	0.5000	0	1	20,178
Risk averse (lagged)	0.3741	0.4839	0	1	20,178
Above 40 (lagged)	0.7350	0.4413	0	1	20,178
Supports the Greens (lagged)	0.1347	0.3415	0	1	11,859

Source: SOEP v29, 2010–2011, own calculations

Table 1.13: Descriptive Statistics – Switzerland (SHP)

	Mean	Std. Dev.	Min.	Max.	Obs.
Dependent Variables					
Life Satisfaction	8.0269	1.3094	0	10	14,104
Votes Green	0.1466	0.3537	0	1	14,104
Env. protection more important than GDP	0.5088	0.4999	0	1	9474
Demographic Characteristics					
Age	51.5468	16.0513	16	96	14,104
Age squared/100	29.1470	16.5089	2.5600	92.1600	14,104
Female	0.5255	0.4994	0	1	14,104
Single	0.2119	0.4087	0	1	14,104
Married	0.6326	0.4821	0	1	14,104
Divorced	0.0915	0.2884	0	1	14,104
Separated	0.0132	0.1141	0	1	14,104
Widowed	0.0488	0.2154	0	1	14,104
Registered partnership	0.0020	0.0445	0	1	14,104
Children under 17 in household	0.5610	0.9592	0	5	14,104
No Swiss nationality	0.0552	0.2284	0	1	14,104
Educational Characteristics					
Incomplete school education	0.0137	0.1162	0	1	14,104
Elementary school	0.0664	0.2489	0	1	14,104
Compulsory secondary education	0.4275	0.4947	0	1	14,104
A-level equiv., master, technical school	0.2265	0.4186	0	1	14,104
Voc./acad. high school degree or higher	0.2660	0.4419	0	1	14,104
Labor Market Characteristics					
Regularly employed	0.5177	0.4997	0	1	14,104
Managerial position	0.1078	0.3101	0	1	14,104
Contract position	0.0564	0.2308	0	1	14,104
Self-employed	0.0904	0.2868	0	1	14,104
Unemployed	0.0095	0.0970	0	1	14,104
Student	0.0200	0.1400	0	1	14,104
Retired	0.2136	0.4098	0	1	14,104
Disabled (not working)	0.0160	0.1253	0	1	14,104
Domestic tasks or care	0.0305	0.1719	0	1	14,104
Other non-working	0.0184	0.1345	0	1	14,104
Household income	125.2957	119.2922	0.5000	6185.4000	14,104
Heterogeneity					
Within 0–25 km radius to reactor	0.2683	0.4431	0	1	14,104
Within 25–50 km radius to reactor	0.3883	0.4874	0	1	14,104
Within 50–80 km radius to reactor	0.1766	0.3814	0	1	14,104

Source: SHP w11–14, 2009–2012, own calculations

Table 1.14: Descriptive Statistics – UK (Understanding Society)

	Mean	Std. Dev.	Min.	Max.	Obs.
Dependent Variables					
Happiness	2.9462	0.5451	1	4	46,406
Supports Green party	0.0353	0.1845	0	1	46,406
Demographic Characteristics					
Age	48.5198	17.1784	16	99	46,406
Age squared/100	26.4926	17.2949	256.0000	9801.0000	46,406
Female	0.5619	0.4962	0	1	46,406
Single	0.2638	0.4407	0	1	46,406
Married	0.5573	0.4967	0	1	46,406
Separated	0.0213	0.1444	0	1	46,406
Divorced	0.0982	0.2976	0	1	46,406
Widowed	0.0594	0.2364	0	1	46,406
Number of children in household	0.5246	0.9280	0	9	46,406
Educational Characteristics					
Higher degree	0.2363	0.4248	0	1	46,406
Degree	0.1249	0.3306	0	1	46,406
A-level (etc.)	0.1973	0.3980	0	1	46,406
GCSE (etc.)	0.2088	0.4065	0	1	46,406
Other qualification	0.1020	0.3026	0	1	46,406
No qualification	0.1306	0.3370	0	1	46,406
Labor Market Characteristics					
Paid employment	0.4903	0.4999	0	1	46,406
Self-employed	0.0757	0.2645	0	1	46,406
Unemployed	0.0499	0.2178	0	1	46,406
Retired	0.2338	0.4233	0	1	46,406
Family care, home, maternity leave	0.0659	0.2482	0	1	46,406
Student	0.0431	0.2031	0	1	46,406
Sick or disabled	0.0353	0.1845	0	1	46,406
Other	0.0059	0.0766	0	1	46,406
Household income	3685.0700	2909.1600	0	20,000	46,406
Heterogeneity					
Within 0-25 km radius to reactor	0.0305	0.1720	0	1	46,406
Within 25-50 km radius to reactor	0.1074	0.3096	0	1	46,406
Within 50-80 km radius to reactor	0.1908	0.3929	0	1	46,406

Source: Understanding Society w2–3, 2010–2012, own calculations

1.8 Online Appendix to Chapter 1

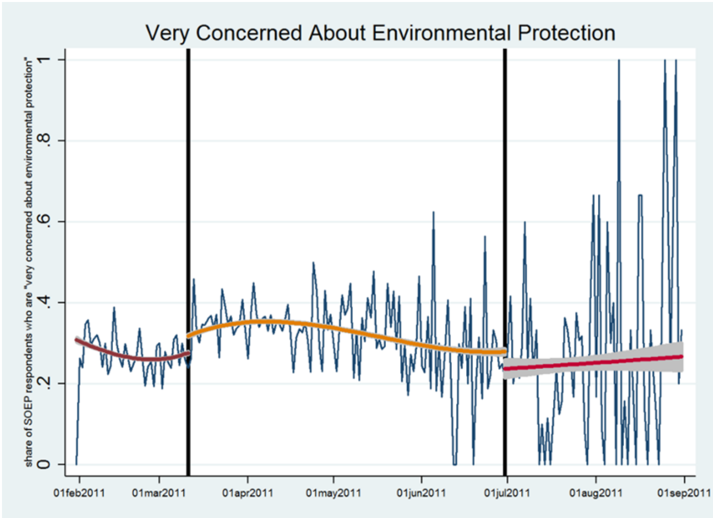

Figure 1.9: SOEP Respondents Who Are Very Concerned About the Environment in 2011

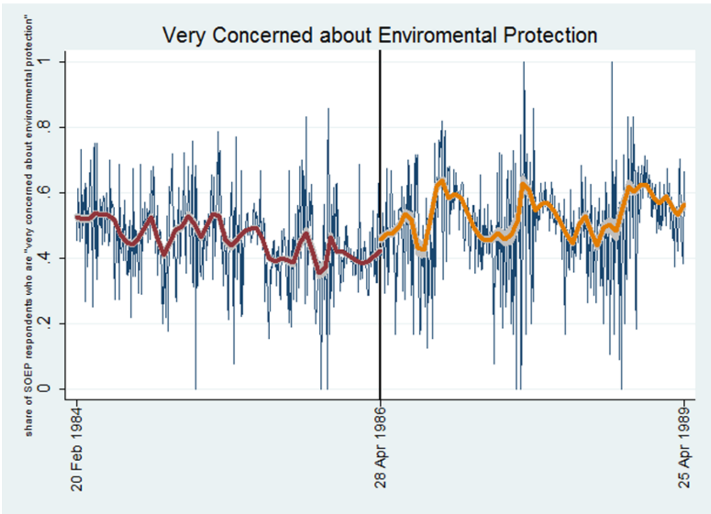

Figure 1.10: SOEP Respondents Who Are Very Concerned About Environmental Protection in 1986

Table 1.15: Balancing Properties between Treatment and Control Group, 2010-2011, Germany

	Interview after March 11, 2011 (treatment group) Mean	Interview before March 11, 2011 (control group) Mean	Normalized Difference
Demographic Characteristics			
Age	51.8324	50.3973	0.1368
Age squared/100	29.8230	28.1530	0.1459
Female	0.5282	0.5201	0.0159
Married	0.6367	0.6432	0.0169
Single	0.2086	0.2208	0.0445
Disabled	0.1464	0.1324	0.0651
No German nationality	0.0420	0.0502	0.0482
Number of children in household	0.8356	0.7977	0.0344
Educational Characteristics			
In school	0.0132	0.0114	0.0328
Lower than secondary degree	0.1384	0.1132	0.0733
Secondary degree	0.5336	0.5339	0.0052
Tertiary degree	0.3148	0.3415	0.0510
Labor Market Characteristics			
Full-time employed	0.3810	0.4306	0.0985
Part-time employed	0.1152	0.1318	0.0460
Out of the labor force	0.4337	0.3705	0.1353
On maternity leave	0.0182	0.0168	0.0061
Unemployed	0.0542	0.0413	0.0127
Household income	2,756.2142	3,041.9783	0.1602
Heterogeneity			
Within 50 km to reactor (lagged)	0.2837	0.2825	0.0038
Within 50 km to 80 km to reactor (lagged)	0.1978	0.2059	0.0226
Nearest reactor among eight oldest (lagged)	0.4846	0.5168	0.0735
Risk averse (lagged)	0.4040	0.3939	0.0211
Above 40 (lagged)	0.7412	0.7186	0.0557
Supports the Greens (lagged)	0.1284	0.1498	0.0512
N	***14,656***	***5,522***	–

Note: The last column shows the normalized difference which has been calculated according to $\Delta s = (\bar{s}_1 - \bar{s}_0) / \sqrt{{\sigma_1}^2 + {\sigma_0}^2}$, with $\bar{s}_1$ and $\bar{s}_0$ denoting average covariate values for treatment and control group, respectively. σ denotes the variance. As a rule of thumb, normalized differences exceeding 0.25 indicate non-balanced observables that might lead to sensitive results (Imbens and Wooldridge 2009).

Source: SOEP v29, 2010–2011, own calculations

Table 1.16: Balancing Properties between Treatment and Control Group, 2009–2012, Switzerland

	Interview after March 11, 2011 (treatment group) Mean	Interview before March 11, 2011 (control group) Mean	Normalized Difference
Demographic Characteristics			
Age	52.5468	50.5468	0.0883
Age squared/100	30.1779	28.1160	0.0885
Female	0.5255	0.5255	0.0000
Single	0.2031	0.2208	0.0307
Married	0.6354	0.6298	0.0083
Divorced	0.0946	0.0885	0.0150
Separated	0.0129	0.0135	0.0035
Widowed	0.0518	0.0458	0.0196
Registered partnership	0.0023	0.0017	0.0090
Children under 17 in household	0.5172	0.6048	0.0647
No Swiss nationality	0.0547	0.0557	0.0031
Educational Characteristics			
Incomplete school education	0.0095	0.0179	0.0510
Elementary school	0.0613	0.0715	0.0290
Compulsory secondary education	0.4290	0.4260	0.0043
A-level equiv., master, technical school	0.2266	0.2265	0.0002
Voc./acad. high school degree or higher	0.2737	0.2582	0.0247
Labor Market Characteristics			
Regularly employed	0.6900	0.7131	0.0357
Managerial position	0.1044	0.1112	0.0155
Contract position	0.0519	0.0610	0.0278
Self-employed	0.0888	0.0920	0.0080
Unemployed	0.0092	0.0098	0.0041
Student	0.0155	0.0245	0.0459
Retired	0.2300	0.1971	0.0568
Disabled (not working)	0.0169	0.0150	0.0104
Domestic tasks or care	0.0278	0.0332	0.0222
Other non-working	0.0199	0.0170	0.0149
Household income	127.1445	123.4470	0.0219
Heterogeneity			
Within 0–25 km radius to reactor	0.2683	0.2683	0.0000
Within 25–50 km radius to reactor	0.3883	0.3883	0.0000
Within 50–80 km radius to reactor	0.1771	0.1761	0.0018
N	***7,052***	***7,052***	–

Note: The last column shows the normalized difference which has been calculated according to $\Delta s = (\bar{s}_1 - \bar{s}_0) / \sqrt{{\sigma_1}^2 + {\sigma_0}^2}$, with $\bar{s}_1$ and $\bar{s}_0$ denoting average covariate values for treatment and control group, respectively. σ denotes the variance. As a rule of thumb, normalized differences exceeding 0.25 indicate non-balanced observables that might lead to sensitive results (Imbens and Wooldridge 2009).

Source: SHP w11–14, 2009–2012, own calculations

Table 1.17: Balancing Properties Between Treatment and Control Group, 2010–2012, UK

	Interview after March 11, 2011 (treatment group) Mean	Interview before March 11, 2011 (control group) Mean	Normalized Difference
Demographic Characteristics			
Age	48.7660	48.1026	0.0273
Age squared/100	2,672.4283	2,609.9976	0.0255
Female	0.5630	0.5599	0.0044
Single	0.2623	0.2663	0.0064
Married	0.5582	0.5557	0.0035
Separated	0.0215	0.0210	0.0026
Divorced	0.0988	0.0972	0.0039
Widowed	0.0592	0.0599	0.0020
Number of children in household	0.5268	0.5210	0.0044
Educational Characteristics			
Higher degree	0.2408	0.2289	0.0198
Degree	0.1285	0.1189	0.0205
A-level (etc.)	0.1993	0.1940	0.0095
GCSE (etc.)	0.2043	0.2165	0.0210
Other qualification	0.1005	0.1045	0.0094
No qualification	0.1266	0.1373	0.0222
Labor Market Characteristics			
Paid employment	0.4864	0.4970	0.0150
Self-employed	0.0773	0.0730	0.0115
Unemployed	0.0505	0.0490	0.0048
Retired	0.2380	0.2267	0.0190
Family care, home, maternity leave	0.0660	0.0659	0.0002
Student	0.0410	0.0467	0.0197
Sick or disabled	0.0351	0.0356	0.0019
Other	0.0058	0.0062	0.0037
Household income	3,716.5781	3,631.6910	0.0207
Heterogeneity			
Within 0–25 km radius to reactor	0.0327	0.0269	0.0240
Within 25–50 km radius to reactor	0.1117	0.1000	0.0270
Within 50–80 km radius to reactor	0.2057	0.1657	0.0729
N	***29,183***	***17,223***	–

Note: The last column shows the normalized difference which has been calculated according to $\Delta s = (\bar{s}_1 - \bar{s}_0) / \sqrt{{\sigma_1}^2 + {\sigma_0}^2}$, with $\bar{s}_1$ and $\bar{s}_0$ denoting average covariate values for treatment and control group, respectively. σ denotes the variance. As a rule of thumb, normalized differences exceeding 0.25 indicate non-balanced observables that might lead to sensitive results (Imbens and Wooldridge 2009).

Source: Understanding Society, w2–3, 2010–2012, own calculations

Table 1.18: Determinants of Environmental Concerns

	OLS	FE
Age	0.0077***	
	(0.0015)	
Age Squared/100	-0.0074***	
	(0.0000)	
Female	0.0710***	
	(0.0090)	
Married	-0.0145	-0.0185
	(0.0111)	*(0.0350)*
Single	0.0035	-0.0062
	(0.0160)	*(0.0496)*
Disabled	0.0516***	0.0343*
	(0.0098)	*(0.0198)*
No German Nationality	0.0096	-0.0563
	(0.0180)	*(0.1649)*
Number of Children in Household	-0.0108***	-0.0047*
	(0.0041)	*(0.0025)*
In School	0.0625*	-0.0157
	(0.0343)	*(0.0508)*
Lower Than Secondary Degree	-0.0126	-0.2148***
	(0.0107)	*(0.0620)*
Secondary Degree	-0.0086	-0.0324
	(0.0126)	*(0.0378)*
Full-Time Employed	-0.0610***	-0.0576***
	(0.0137)	*(0.0202)*
Part-Time Employed	-0.0250	-0.0101
	(0.0155)	*(0.0190)*
Out of the Labor Force	-0.0164	-0.0241
	(0.0148)	*(0.0166)*
On Maternity Leave	0.0160	0.0014
	(0.0259)	*(0.0268)*
Unemployed	0.0004	-0.0270
	(0.0173)	*(0.0196)*
Household Income	-0.0198***	-0.0118
	(0.0052)	*(0.0104)*
Distance to the Next Nuclear Power Plant/10,000	-0.0010**	-0.0007
	(0.0004)	*(0.0045)*
R^2	*0.0138*	*0.0075*
N	***20,178***	***20,178***

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. Models control for individual, year, and month fixed effects as well as a time trend.

Table 1.19: Effects on Environmental Concerns in Germany (Robustness Checks I)

	Very Concerned About the Environment			
	Only Pre-Scheduled Interviews (1)	Excludes CAPI Interviews (2)	Excludes CAPI+Postal Interviews (3)	Includes Only CAPI Interviews (4)
$PostMarch11_{i,2011} * 2011$	0.0793***	0.0597***	0.0464**	0.0985***
("after meltdown")	*(0.0150)*	*(0.0091)*	*(0.0232)*	*(0.0201)*
$PostMay30_{i,2011} * 2011$	-0.1248***	-0.1070***	-0.1776***	-0.1053***
("after shutdown")	*(0.0248)*	*(0.0168)*	*(0.0340)*	*(0.0344)*
Controls				
Demographic characteristics	Yes	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes	Yes
Labor market characteristics	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	*0.0153*	*0.0076*	*0.0271*	*0.0199*
N	***9,311***	***15,206***	***4,487***	***4,824***

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop out in the individual FE models. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. There are ten different categories of interviews, but only four are of quantitative relevance: (a) oral interviews, pre-scheduled and carried out by a professional interviewer at the respondent's home (19%), (b) self-administered interviews, filled out by the respondent without the help of an interviewer (29%), (c) written interviews, sent in by mail (20%), and (d) CAPI interviews, pre-scheduled and carried out by a professional interviewer at the respondent's home at a computer (25%). Column (1) excludes categories (b) and (c). Column (2) excludes category (d). Column (3) excludes categories (c) and (d). Column (4) excludes all categories except for (d). Each column stands for one FE model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.20: Effects on Environmental Concerns in Germany (Robustness Checks II)

	Very Concerned About the Environment			
	Uses Alternative Policy Date for Phase-Out	Excludes Individuals That Moved Out of 50 km Radius to Birth Place	Excludes Individuals That Moved in Previous Period	Includes Only Pre-Scheduled Interviews
	(1)	(2)	(3)	(4)
$PostMarch11_{i,2011} * 2011$	0.0610***	0.0874***	0.0752***	0.0794***
(“after meltdown”)	*(0.0086)*	*(0.0121)*	*(0.0090)*	*(0.0149)*
$PostMay30_{i,2011} * 2011$		-0.1338***	-0.1114***	-0.1203***
(“after shutdown”)		*(0.0213)*	*(0.0163)*	*(0.0246)*
$PostJune30_{i,2011} * 2011$	-0.0959*** *(0.0213)*			
Controls				
Demographic characteristics	Yes	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes	Yes
Labor market characteristics	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	*0.0061*	*0.0130*	*0.0082*	*0.0144*
N	***20,178***	***9,837***	***19,904***	***9,424***

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop out in the individual FE models. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Column (1) uses June 30, 2011, as the relevant policy date for the phase-out, as the phase-out bill was formally passed by the parliament on that date. Column (2) excludes individuals that moved out of a 50 km radius to their birth place. Column (3) excludes individuals that moved in the previous period. Column (4) includes only pre-scheduled interviews. Each column stands for one FE model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.21: Effects on Environmental Concerns in Germany (Robustness Checks III)

	Very Concerned About the Environment		
	Adds Linear Time Trend After Meltdown (1)	Adds Linear Time Trend After Policy (2)	Adds Quadratic Time Polynomial (3)
$PostMarch11_{i,2011} * 2011$	0.0717***	0.0733***	0.0880***
("after meltdown")	*(0.0094)*	*(0.0088)*	*(0.0140)*
$PostMay30_{i,2011} * 2011$	-0.0997***	-0.1144***	-0.0587***
("after shutdown")	*(0.0159)*	*(0.0154)*	*(0.0277)*
Controls			
Demographic characteristics	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes
Labor market characteristics	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes
R^2	*0.0075*	*0.0086*	*0.0078*
N	***20,178***	***20,178***	***20,178***

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop in the individual FE models. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Column (1) adds a linear time trend which starts after the meltdown. Column (2) adds a linear time trend which starts after the policy action. Column (3) adds a quadratic time polynomial to the linear time trend. Columns (1) and (2) stand for FE models similar to Eq. 1.2. Column (3) stands for a FE model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.22: Placebo Dates and Placebo Concerns in Germany

	Placebo Years		Placebo Dependent Variables: Very Concerned About			
	Very Concerned About the Environment		Job Security	Health	the Economy	Crime
	2010 (1)	2012 (2)	2011 (3)	2011 (4)	2011 (5)	2011 (6)
$PostMarch11_{i,2010} * 2010$	0.0105					
	(0.0077)					
$PostMay30_{i,2010} * 2010$	0.0060					
	(0.0133)					
$PostMarch11_{i,2012} * 2012$		-0.0035				
		(0.0262)				
$PostMay30_{i,2012} * 2012$		0.0649				
		(0.0500)				
$PostMarch11_{i,2011} * 2011$			-0.0088	0.0078	0.0087	-0.0075
("after meltdown")			*(0.0124)*	*(0.0080)*	*(0.0127)*	*(0.0116)*
$PostMay30_{i,2011} * 2011$			0.0277	-0.0114	0.0386	0.0328
("after shutdown")			*(0.0189)*	*(0.0152)*	*(0.0263)*	*(0.0255)*
Controls						
Socio-Economic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year, Month, and Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	*0.0041*	*0.0126*	*0.0281*	*0.0043*	*0.0979*	*0.0125*
N	***21,944***	***17,754***	***11,526***	***20,178***	***20,178***	***20,178***

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $Post - May30_{i,2011}$, which drop out in the individual FE models. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Columns (1) and (2) use the placebo years 2010 and 2012. Columns (3) to (6) use placebo dependent variables, which are dummy variables that equal one if the individual is very concerned about job security, health, the economy, and crime. Each column stands for one FE model similar to Eq. 1.1.

Source: SOEP v29, 2009–2012, balanced panel, own calculations

Table 1.23: Placebo Policy Dates in Germany

	Very Concerned About the Environment					
	Real Policy Dates		Placebo Policy Dates			
	(1)	(2)	(3)	(4)	(5)	(6)
$PostMarch11_{i,2011} * 2011$	0.0713***	0.0610***	0.0724***	0.0728***	0.0728***	0.0726***
("After Meltdown")	*(0.0088)*	*(0.0086)*	*(0.0147)*	*(0.0147)*	*(0.0147)*	*(0.0147)*
$PostMay30_{i,2011} * 2011$	-0.0994***					
("After Permanent Shutdown")	*(0.0159)*					
$PostJune30_{i,2011} * 2011$		-0.0959***				
		(0.0213)				
$PostMay15_{i,2011} * 2011$			-0.0158			
			(0.0246)			
$PostMay17_{i,2011} * 2011$				-0.0010		
				(0.0238)		
$PostJune15_{i,2011} * 2011$					-0.0089	
					(0.0298)	
$PostJuly15_{i,2011} * 2011$						0.0443
						(0.0380)
Controls						
Socio-Economic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year, Month, and Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	*0.0075*	*0.0061*	*0.0083*	*0.0083*	*0.0083*	*0.0083*
N	***20,178***	***20,178***	***20,178***	***20,178***	***20,178***	***20,178***

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$, $PostMay15_{i,2011}$, $PostMay17_{i,2011}$, $PostMay30_{i,2011}$, $PostJune15_{i,2011}$, $PostJune30_{i,2011}$, and $PostJuly15_{i,2011}$, which drop out in the individual FE models. Columns (1) and (2) use the real policy dates, May 30 and June 30, 2011. Columns (3), (4), (5), and (6) use the placebo policy dates May 15, May 17, June 15, and July 15, 2011. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one FE regression model similar to Eq. 1.1.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Table 1.24: Effects on Risk Aversion in Germany

	Very Risk Averse			
	OLS	OLS	FE	FE
	(1)	(2)	(3)	(4)
$PostMarch11_{i,2011} * 2011$	0.0059	0.0036	0.0163***	0.0163***
(“After Meltdown”)	*(0.0092)*	*(0.0084)*	*(0.0060)*	*(0.0060)*
$PostMay30_{i,2011} * 2011$	-0.0087	-0.0070	0.0016	0.0021
(“After Shutdown”)	*(0.0144)*	*(0.0138)*	*(0.0104)*	*(0.0104)*
$PostMarch11_{i,2011}$	-0.0038	0.0032		
	(0.0071)	*(0.0066)*		
$PostMay30_{i,2011}$	0.0066	0.0045		
	(0.0089)	*(0.0087)*		
2011	-0.0014**	-0.0015***	-0.0026***	-0.0026***
	(0.0006)	*(0.0005)*	*(0.0003)*	*(0.0003)*
Controls				
Demographic Characteristics	No	Yes	No	Yes
Educational Characteristics	No	Yes	No	Yes
Labor Market Characteristics	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes
R^2	*0.0033*	*0.0535*	*0.0072*	*0.0085*
N	***20,178***	***20,178***	***20,178***	***20,178***

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$, which drop out in the individual FE models. The dependent variable is a dummy variable which equals one if the individual is very risk averse (0–1/10 on the risk attitude scale). The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one regression model similar to Eq. 1.1.

Source: SOEP v29, 2009–2012, balanced panel, own calculations

Table 1.25: Effects on Environmental Concerns in Germany in a Pure RD Design

	Very Concerned About the Environment			
	OLS (1)	OLS (2)	OLS (3)	OLS (4)
$PostMarch11_{i,2011}$ ("After Meltdown")	0.0660*** *(0.0099)*			
$PostMay30_{i,2011}$ ("After Shutdown")		-0.0494** *(0.0209)*		
$PostJune30_{i,2011}$ ("Alternative Policy Date")			-0.0552** *(0.0265)*	
$PostJuly30_{i,2011}$ ("Placebo Policy Date")				0.0018 *(0.0418)*
Controls				
Demographic Characteristics	Yes	Yes	Yes	Yes
Educational Characteristics	Yes	Yes	Yes	Yes
Labor Market Characteristics	Yes	Yes	Yes	Yes
R^2	*0.0164*	*0.0195*	*0.0186*	*0.0223*
N	***11,881***	***3,503***	***1,989***	***1,035***

Standard errors are in parentheses and clustered at the interview date level
**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant treatment dummy variables $PostMarch11_{i,2011}$, $PostMay30_{i,2011}$, $PostJune30_{i,2011}$, and $PostJuly30_{i,2011}$. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one local linear regression model around the respective cut-off date, plus minus 45 days, i.e. the identification of the effects is based on a pure regression discontinuity design. Note that this approach relies only on cross sectional data for 2011.

Source: SOEP v29, 2011, own calculations

Table 1.26: Effects on Life Satisfaction in Germany (Richter et al. (2013) – Replication I)

	Original Model	Augmented Models			
Life Satisfaction	OLS	OLS (+ Clustering)	OLS (+ Linear Time Trend)	OLS (+ Month Fixed Effects)	OLS (+ Individual Fixed Effects)
Before Fukushima accident: 02/01/2011–03/10/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*	*(Ref.)*	*(Ref.)*
Fukushima accident:	0.055	0.055	-0.008	0.016	0.134
03/11/2011–06/05/2011	*(0.036)*	*(0.046)*	*(0.047)*	*(0.063)*	*(0.842)*
Nuclear phase-out:	0.131***	0.131**	-0.059	-0.129	0.100
06/06/2011–09/30/2011	*(0.046)*	*(0.057)*	*(0.102)*	*(0.166)*	*(0.843)*
Observations	17,571	17,571	17,571	17,571	35,964
R^2	0.333	0.333	0.334	0.334	0.077

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: OLS and ordered logit estimates are estimated. The dependent variable is general life satisfaction (coded: 0–10); cross sectional weights for all waves are applied. Considered covariates are: Health, gender, age, age (squared), log household income, child in household, marital status, employment status, education, worries about own economic situation and overall economic development, state dummies and regional dummy (East).

Table 1.27: Effects on Life Satisfaction in Germany (Richter et al. (2013) – Replication II)

	Original Model	Augmented Models			
Very Concerned About Environment	OLS	OLS (+ Clustering)	OLS (+ Linear Time Trend)	OLS (+ Month Fixed Effects)	OLS (+ Individual Fixed Effects)
Before Fukushima accident: 02/01/2011–03/10/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*	*(Ref.)*	*(Ref.)*
Fukushima accident:	0.060***	0.060***	0.051***	0.048**	0.046***
03/11/2011–06/05/2011	*(0.010)*	*(0.011)*	*(0.013)*	*(0.019)*	*(0.017)*
Nuclear phase-out:	-0.017	-0.017	-0.044*	-0.048	-0.041**
06/06/2011–09/30/2011	*(0.011)*	*(0.012)*	*(0.026)*	*(0.050)*	*(0.021)*
Observations	20,021	20,021	20,021	20,021	38,429
R^2	0.081	0.081	0.082	0.082	0.018

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Standard errors are in parentheses and clustered at the individual level. OLS and ordered logit estimates are estimated. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. Cross sectional weights for all waves are applied. Considered covariates are: Health, gender, age, age (squared), log household income, child in household, marital status, employment status, education, worries about own economic situation and overall economic development, state dummies and regional dummy (East).

Table 1.28: Potentially Confounding Events in Germany, Switzerland, and the United Kingdom in 2011

Date	Potentially Confounding Event
Germany, 2011, first half year	
January 7	Due to extreme weather conditions, several river banks burst.
February 20	Hamburg, federal state election, 2011
March 1	Karl-Theodor zu Guttenberg (CDU) resigns as defence minister.
March 20	Saxony-Anhalt, federal state election, 2011
March 27	Baden-Württemberg, federal state election, 2011; Rhineland-Palatinate, federal state election, 2011
April 3	Guido Westerwelle (FDP) resigns as party leader.
May 12	Philipp Rösler (FDP) becomes new federal minister for the economy. Daniel Bahr (FDP) becomes new federal minister for health.
May 12	Winfried Kretschmann (Greens) becomes minister president of Baden-Wuerttemberg (first ever minister president by Greens).
May 22	Bremen, federal state election, 2011
July 1	Bundestag ends conscription.
July 7	Bundestag allows preimplantation genetic diagnosis.
Switzerland, 2011, first half year	
–	No particular events to be reported, except campaigning (federal elections held on October 23).
United Kingdom, 2011, first half year	
13 January	Labour wins the Oldham East and Saddleworth by-election.
26 January	David Cameron announces that Sinn Féin's Gerry Adams has resigned from the British parliament and has accepted the position of Crown Steward and Bailiff of the Manor of Northstead. Speaker John Bercow later clarifies that Adams has been appointed to the role following a denial of his acceptance.
5 February	David Cameron criticises "state multiculturism" in his first speech as prime minister on radicalisation and causes of terrorism.
3 March	Voters in Wales approve plans to give the Welsh Assembly more powers.

Continued on next page

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Date	Potentially Confounding Event
3 March	Labour wins the Barnsley Central by-election, with the Liberal Democrats finishing in sixth place.
26 March	Hundreds of thousands of people march in London against government budget cuts with the protests later turning violent.
24 April	Senior Liberal Democrat minister Chris Huhne threatens legal action over "untruths" told by Conservative MP's opposed to the Alternative Vote System, 11 days before the referendum. He also warns that the dispute could damage the coalition government.
5 May	Elections are held for the Scottish Parliament, Welsh Assembly, and the Northern Ireland Assembly. Local elections are held on the same day together with the referendum on whether to adopt the Alternative Vote electoral system for elections to the House of Commons.
6 May	The Scottish National Party secures election victory, winning an overall majority in the Scottish parliament elections. The counting of votes in local elections in England and Northern Ireland continue with Labour making gains and the Liberal Democrats losing seats. Voters reject proposals to introduce the Alternative Voting System. Labour candidate Jon Ashworth wins the Leicester South by-election.
7 May	Counting for the Northern Ireland Assembly election ends with the DUP and Sinn Féin winning most of the 108 seats, with 38 and 29 respectively. The Welsh Labour Party wins 30 of the 60 Welsh Assembly seats in Thursday's election and plans to form a one-party government.
10 June	Sinn Féin's Paul Maskey wins the West Belfast by-election.
1 July	Labour's Iain McKenzie wins the Inverclyde by-election with a majority reduced from 14,416 in 2010 to 5,838.
Sources:	Wikipedia, BBC 2017

Table 1.29: Effects of the Meltdown and the Permanent Shutdown on Environmental Concerns in Germany, Logit Models With Marginal Effects

	Very concerned about the environment	
	Logit, Marginal Effect (1)	Logit, Marginal Effect (2)
$PostMarch11_{i,2011} * 2011$	0.0655***	0.0653***
("after meltdown")	*(0.0153)*	*(0.0153)*
$PostMay30_{i,2011} * 2011$	-0.0704***	-0.0737***
("after shutdown")	*(0.0228)*	*(0.0226)*
$PostMarch11_{i,2011}$	-0.0013	-0.0001
	(0.0112)	*(0.0112)*
$PostMay30_{i,2011}$	-0.0182	-0.0143
	(0.0140)	(0.0140)
2011	-0.0306***	-0.0296***
	(0.0099)	*(0.0099)*
Controls		
Demographic characteristics	No	Yes
Educational characteristics	No	Yes
Labor market characteristics	No	Yes
Year fixed effects	Yes	Yes
Month fixed effects	Yes	Yes
Linear time trend	Yes	Yes
$PseudoR^2$	0.0029	0.0103
N	20,178	20,178

Standard errors are in parentheses and clustered at the interview date level

**** p<0.01, ** p<0.05, * p<0.1*

Note: Treatment status is defined by the time-invariant dummy variables $PostMarch11_{i,2011}$ and $PostMay30_{i,2011}$. The dependent variable is a dummy variable which equals one if the individual is very concerned about the environment. The controls include age, age squared, being female, being married, being disabled, having German citizenship, the number of children in the household, being in education, having less than a secondary degree, having a secondary degree, having a tertiary degree, being full-time employed, being part-time employed, being out of the labour force, being on maternity leave, being unemployed, the household income, and the distance to the nearest nuclear power plant. Each column stands for one model similar to Eq. 1.1, without individual fixed effect.

Source: SOEP v29, 2010–2011, balanced panel, own calculations

Chapter 2

Urban Land Use

Abstract

We investigate the effect of urban land use on residential well-being in major German cities, using panel data from the German Socio-Economic Panel and cross-section data from the European Urban Atlas. We reduce concerns about endogeneity by employing fixed-effects (within) estimators, including both individual and city of residence fixed effects, and by controlling for a rich set of confounders. The results show that access to green urban areas, such as parks, is positively associated with, whereas access to abandoned areas, such as brownfields, is negatively associated with life satisfaction. The effects are strongest for residents who are older. We calculate the marginal willingness-to-pay of residents to have access to green urban and abandoned areas, as well as the optimal value of green urban and abandoned areas in their surroundings. We provide a policy case study, while discussing limitations and avenues for future research.*

∗. This chapter is also available as the following journal article: Krekel, C., J. Kolbe, and H. Wuestemann, “The Greener, The Happier? The Effect of Urban Land Use on Residential Well-Being,” *Ecological Economics*, 121, 117–127, 2016.

2.1 Introduction

In major cities, space is a scarce commodity, and urbanisation puts increasing pressure on areas that provide important ecosystem services. Acknowledging that urban areas, such as parks and green space, contribute to their climate and environmental policy objectives, the European Commission promotes their preservation by incorporating them into national and regional policies across the European Union (European Commission 2013), whereas the Federal Government in Germany promotes their preservation by incorporating them into its national strategy on biodiversity protection (Federal Ministry for the Environment, Nature Conservation, Building, and Nuclear Safety 2007).

These ongoing policy initiatives, meant to preserve urban ecosystem services, are encouraged by a growing body of literature that highlights their benefits for residents in their surroundings, suggesting that urban areas, such as parks and green space, have positive effects on residential well-being and health (see Bell et al. (2008) and Croucher et al. (2008) for reviews). Using cross-section data on residential well-being from the Household, Income, and Labour Dynamics Survey in Australia and the amount of green space in the collection districts of major Australian cities, Ambrey and Fleming (2013) show that green space is positively associated with life satisfaction.[37] Smyth et al. (2008) and Smyth et al. (2011) confirm that green space per capita is positively associated with happiness in urban China, whereas, in a case study of Adelaide, Australia, Sugiyama et al. (2008) show that residents who rate to live in greener areas report higher mental and physical health. Importantly, these effects seem to be heterogeneous: Ambrey and Fleming (2013) suggest that single parents and people with lower levels of education benefit more in terms of life satisfaction, whereas, in the United Kingdom, Richardson and Mitchell 2010 find that men benefit more in terms of lower rates of cardiovascular and respiratory diseases, and Mitchell and Popham (2008) find that low-income households benefit more in terms of reduced health inequalities (Jorgensen and Anthopoulou 2007). Maas et al. (2006) confirm the heterogeneous effect for people with lower levels of education in the Netherlands, and also add that older residents benefit more in terms of general health (Jorgensen et al. 2002). Most of these studies, however, use cross-section data, with the exception of White et al. (2013), who find positive effects of green space on life satisfaction and mental health in England.[38] In Berlin, Germany, Bertram and Rehdanz (2015) relate self-collected survey data on subjective well-being, in particular life satisfaction, cross-sectionally to urban land use data, in particular green

37. In related studies, using the same dataset and empirical strategy, the authors also find that there is a positive relationship between scenic amenity and protected areas on the one hand and life satisfaction on the other (Ambrey and Fleming 2011, 2012).

38. Alcock et al. (2014) are a spin-off of White et al. (2013), focusing on residents who move.

space, obtained from the European Urban Atlas, and find similar life satisfaction maximising amounts of green space in a one kilometre radius around households as we do (and also document that there is, on average, an under-supply of urban green). We differ from this study by being able to focus on all major German cities with inhabitants equal to or greater than 100,000, by being able to employ fixed-effects (within) estimators with both individual and city of residence fixed effects in order to reduce concerns about endogeneity, and by being able to exploit the exact geographical coordinates of households as documented in the German Socio-Economic Panel. We also look at types of urban land use other than urban green space.

In sharp contrast to these studies stands another stream of literature that investigates the disamenity value of vacant or abandoned areas in post-industrial cities. Using a quasi-experimental difference-in-differences design, Branas et al. (2011) show that the greening of vacant lots in Philadelphia, Pennsylvania, reduces certain crimes and improves the self-reported health of residents in their surroundings. Bixler and Floyd (1997) and Kuo et al. (1998) suggest similar effects when it comes to common space on the one hand and perceived safety and fear of crime on the other. These results are supported by studies on the relationship between violent crimes and vacancies: Cui and Walsh (2015), using a difference-in-differences design and a more comprehensive dataset from Pittsburgh, Pennsylvania, that allows for exact proximity analysis, report an increase of roughly 19% for violent crimes once dwellings become vacant. Although these studies do not directly investigate the effect of vacant or abandoned areas on life satisfaction, they still suggest that vacant or abandoned areas are associated with lower life satisfaction, as health and safety are important determinants of subjective well-being (Krekel and Poprawe 2014).

Generally, for environmental qualities associated with green and abandoned areas, as well as other types of urban land use, no market prices exist. Therefore, they are typically valued using stated preference approaches, such as contingent valuation and discrete choice experiments, or revealed preference approaches, such as hedonic pricing (see Brander and Koetse (2011) for a review).

We investigate the effect of urban land use on residential well-being in Germany and value different land use categories monetarily, using the *life satisfaction approach* (Welsch 2007). To this end, we merge panel data from the German Socio-Economic Panel for the time period between 2000 and 2012 with cross-section data from the European Urban Atlas for the year 2006. Trading off the impact of different land use categories on life satisfaction against the impact of income, the life satisfaction approach allows us to calculate the marginal willingness-to-pay of residents in order to have access to different land use categories in their surroundings, as well as the life-satisfaction maximising amounts of them. As this approach has already been applied to value various other public goods and bads monetarily, including air pollution (Ferreira et al. 2013; Ambrey et al. 2014), noise pollution (Praag and Baarsma 2005; Rehdanz and Maddison 2008), as well as scenic amenity (Ambrey and Fleming 2011) and natural land areas (Kopmann and Rehdanz 2013), we contribute to a steadily growing stream of literature.

Specifically, the richness of our data allows us to contribute to the literature on the relationship between urban land use and residential well-being in several ways. First, using the German Socio-Economic Panel allows us to estimate the effect of urban land use on residential well-being by employing fixed-effects (within) estimators, with individual and city of residence fixed effects, while controlling for a rich set of observables. This reduces concerns about endogeneity, especially simultaneity, as the effect is identified by movers, who we can show are moving primarily for reasons unrelated to different land use categories in their surroundings. Second, using the European Urban Atlas allows us to employ data on land use rather than cover. This has the advantage that information based on actual usage is much more consistent in terms of provision of utility than information based on, for instance, cover. Moreover, this dataset allows us to jointly estimate the effects of different land use categories on residential well-being. We focus on green urban areas, forests, waters, and abandoned areas.[39] Third, merging both datasets through geographical coordinates allows us to calculate the exact distances between households and different land use categories, as well as the exact coverages of different land use categories in a pre-defined radius around households. This has the advantage that measuring access based on distances and coverages is much more precise than based on aggregated areas, which simply sum up the amounts of different land use categories in a district. Moreover, using both distances and coverages serves as a robustness check, as they are substitutes for measuring access to different land use categories. Finally, the literature on vacant land focuses mostly on its effect on health and safety. As health and safety are known to be important determinants of subjective well-being, the results of this study may also contribute to this stream of literature.

The rest of this paper is organised as follows. Section 2.2 describes the data and provides detailed definitions of the different land use categories employed. Section 2.3 introduces the empirical model and discusses identification issues. Section 2.4 presents the results, while Section 2.5 gives policy implications. Section 2.6 discusses the results and limitations of this study against the status quo of the literature, and concludes by providing avenues for future

39. Green urban areas are defined as "land for predominantly recreational use", including, for example, gardens and parks. There is an important distinction between green urban areas and forests, as forest within an urban setting, showing traces of recreational use, are classified as green urban areas. Abandoned areas are defined as "areas in the vicinity of artificial surfaces still waiting to be used or re-used", including, for example, waste land and gaps between new construction areas or leftover land (European Environment Agency 2011, p. 21).

research.

2.2 Data

2.2.1 Data on Residential Well-Being

The German Socio-Economic Panel is a comprehensive and representative panel study of private households in Germany, including about 20,000 individuals in more than 11,000 households.[40] It provides information on all household members, covering Germans living in the old and new federal states, foreigners, and recent immigrants (Wagner et al. 2007; Wagner et al. 2008). Most importantly, it provides information on the geographical locations of the places of residence of individuals, allowing to merge data on residential well-being with data on urban land use through geographical coordinates.[41] As such, the dataset is not only representative of individuals living in Germany today, but also provides the necessary geographical reference points for our analysis.[42]

To investigate the effect of urban land use on residential well-being, we select *satisfaction with life* as the dependent variable. The indicator is obtained from an eleven-point single-item Likert scale that asks respondents "How satisfied are you with your life, all things considered?". It has been found to validly reflect the quality of respondent's lives (Diener et al. 2013), and it is the indicator commonly used to value public goods monetarily, using the *life satisfaction approach*, which is named after it. Conceptually, life satisfaction, which is equivalent to subjective well-being (Welsch and Kühling 2009) or experienced utility (Kahneman et al. 1997), is defined as the cognitive evaluation of the circumstances of life (Diener et al. 1999).

2.2.2 Data on Urban Land Use

The European Urban Atlas, provided by the European Environment Agency, is a comprehensive and comparative cross-section study of urban land use in Europe, including data for major German cities (European Environment Agency 2011). We restrict the data to the 32 major German cities with greater than or equal to 100,000 inhabitants in order to avoid confounding the effect of urban land use on residential well-being with that of urbanisation. Based on

40. In our baseline specification, we end up with 37,608 observations for our OLS and 33,782 observations for our FE model. This reduced number of observations arises from our focus on major German cities with inhabitants equal to or greater than 100,000 and from controlling for a rich set of observables that are not available for every respondent over the entire thirteen-year observation period.

41. The German Socio-Economic Panel provides the geographical coordinates at the street block level, which is very accurate in urban areas.

42. The dataset is subject to rigorous data protection regulation. It is never possible to derive the household data from the geographical coordinates, as they are never visible to the researcher at the same time. See Göbel and Pauer (2014) for more information.

satellite imagery, in this dataset, urban areas greater than 0.25 hectare are assigned exclusively to well-defined land use categories.[43] A major advantage of having data on land use rather than cover is that information based on usage is far more homogeneous in terms of provision of utility and neighbourhood effects, as this type of data adds a second stage of verification, namely a check by local authorities that, for example, what is classified through satellite imagery as a park is actually used as one.

The definitions of the land use categories *green urban areas*, *forests*, *waters*, and *abandoned areas* are given in Table 2.1.

Table 2.1: Independent Variables of Interest

Variables	Descriptions	Examples	Categories
Green urban areas	Includes all land for predominantly recreational use[a]; not included are private gardens within housing areas, cemeteries, agricultural areas, green fields not managed for recreational use, sports and leisure facilities	Zoos, gardens, parks, castle parks, suburban natural areas used as parks	1.4.1
Forests	Includes all (even privately owned) areas with ground coverage of tree canopy greater than 30% and tree height greater than five metres	-	3
Waters	Includes all water bodies exceeding one hectare	Lakes, rivers, canals	4
Abandoned areas	Includes all areas in the vicinity of artificial surfaces still waiting to be used or re-used; not included are areas showing any signs of recreational or agricultural use	Waste land, removed former industrial areas, gaps between new construction areas or leftover land	1.3.4

[a] Incorporates playgrounds located within green urban areas

Source: European Urban Atlas 2006

43. The European Urban Atlas provides exact geographical coordinates in form of shapefiles.

The European Urban Atlas defines *green urban areas* as "land for predominantly recreational use" (European Environment Agency 2011, p. 21). Included are, for example, zoos, gardens, parks, and castle parks, as well as suburban natural areas used as parks. Moreover, forests and other green fields are considered green urban areas in case that there are traces of recreational use and they are surrounded by urban structures. Thus, forests within an urban setting, such as patches of parks densely canopied by trees, fall into this land use category. Not included are, for example, private gardens within housing areas, cemeteries, agricultural areas, and other green fields not managed for recreational use. Finally, sports and leisure facilities, such as golf courses and allotment gardens, are not considered green urban areas. As this land use category concentrates on publicly accessible land that provides space for social interaction, the results of this study are comparable to results of studies analysing the social value of public green space.

The land use category *forests* incorporates all (even privately owned) areas with ground coverage of tree canopy greater than 30% and tree height greater than five metres, including other kinds of vegetation at their borders, unless they are themselves part of green urban areas. The land use category *waters* incorporates all water bodies, such as lakes, rivers, and canals, exceeding one hectare. Notably, within parks, lakes are considered as waters and do not count among the green urban area surrounding them.

The European Urban Atlas defines *abandoned areas* as "areas in the vicinity of artificial surfaces still waiting to be used or re-used" (European Environment Agency 2011, p. 21).[44] Included are, for example, waste land, removed former industrial areas, and gaps between new construction areas or leftover land. As the European Urban Atlas distinguishes between land use patterns as opposed to land cover information, within this land use category, different types of land cover can occur. Not included are, for example, areas showing any signs of recreational or agricultural use. Importantly, privately owned green or brown fields used for recreational purposes do not fall into this land use category; they are classified as urban fabric (private gardens). In other words, this land use category does not mix up amenities and disamenities by including areas for recreational activities. As it is difficult to determine land without current use based on satellite imagery alone, assignment to this land use category often relies on locally gathered information based on actual usage (Lavalle et al. 2002, p. 45).

To investigate the effect of urban land use on residential well-being, we define two independent variables that measure access to the different land use categories. First, we define the *distance* to them, measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category, respectively. Second, we define the *coverage* of

44. In some studies, abandoned areas are referred to as *land without current use.*

them, measured as the hectares covered by the land use category in a pre-defined radius of 1,000 metres around households, respectively. Using both distance and coverage serves as a robustness check, given that distances do not make any assumptions, contrary to coverages.

For simplicity, the definition of the *coverage* is illustrated in Figure 2.1.

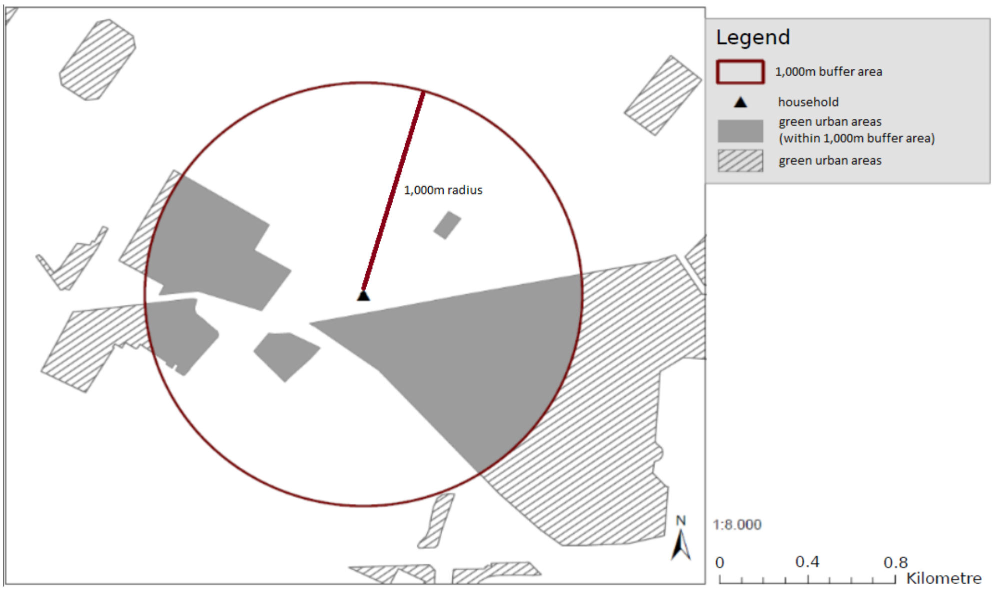

Figure 2.1: Data – Definition of Coverage

We merge the data on residential well-being with the data on urban land use and add controls at the micro level, originating from the German Socio-Economic Panel, at the macro level, originating from the Federal Statistical Office, and at the geo level, originating from our own calculations. The controls at the micro level include demographic characteristics, human capital characteristics, and economic conditions including income at the individual level, as well as household characteristics and housing conditions including rental prices at the household level.[45] The controls at the macro level include macroeconomic conditions, namely the unemployment rate and the average household income, at the city level. The controls at the geo level include the location of the household within the city in terms of distance to the city centre and distance to the city periphery.[46]

The descriptive statistics of the final sample are given in Table 2.2.

45. Since the final sample includes both house owners and renters, to control for rental prices and not lose households that are house owners, we generated a new variable for rental prices that comprises both actual rents (for households that are renters) and hypothetical rents (for households that are house owners). The latter is obtained from a special item in the SOEP in which house owners are asked to convert their house prices into fictitious rents. Although this approach may be subject to measurement error and bias, the bias resulting from excluding one group of individuals, namely house owners, entirely is most likely to be greater than the bias resulting from this conversion.

46. The city centre is defined as the geographical location of the town hall.

Table 2.2: Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Dependent Variable					
Satisfaction With Life	6.8091	1.3842	0	10	37,608
Independent Variables of Interest					
Distance to Green Urban Areas in 100 Metres	2.4853	2.2912	0	40.0303	37,608
Distance to Forests in 100 Metres	18.4860	16.3851	0	90.4961	37,608
Distance to Waters in 100 Metres	12.4058	9.4976	0	84.2749	37,608
Distance to Abandoned Areas in 100 Metres	9.2744	6.1020	0	52.4960	37,608
Coverage of Green Urban Areas in Hectares	22.2950	20.1047	0	192.4058	37,608
Coverage of Forests in Hectares	12.2865	26.0796	0	261.2102	37,608
Coverage of Waters in Hectares	6.9684	13.1860	0	148.3892	37,608
Coverage of Abandoned Areas in Hectares	1.3945	2.2849	0	34.2859	37,608
Other Independent Variables - Geo Level					
Distance to City Centre in 100 Metres	57.3659	39.3028	0.3008	252.0285	37,608
Distance to City Periphery in 100 Metres	33.2043	23.3049	0.0587	118.3960	37,608
Other Independent Variables - Micro Level					
Age	49.4702	17.3950	17	99	37,608
Is Female	0.5201	0.4765	0	1	37,608
Is Married	0.5969	0.4845	0	1	37,608
Is Divorced	0.0872	0.2752	0	1	37,608
Is Widowed	0.0761	0.2520	0	1	37,608
Has Very Good Health	0.1022	0.2907	0	1	37,608
Has Very Bad Health	0.0399	0.2010	0	1	37,608
Is Disabled	0.1191	0.3230	0	1	37,608
Has Migration Background	0.1501	0.3881	0	1	37,608
Has Tertiary Degree	0.3608	0.4332	0	1	37,608
Has Lower Than Secondary Degree	0.1301	0.3373	0	1	37,608

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Variables	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Is in Education	0.0211	0.1412	0	1	37,608
Is Full-Time Employed	0.4203	0.4862	0	1	37,608
Is Part-Time Employed	0.0912	0.2839	0	1	37,608
Is on Parental Leave	0.0312	0.1531	0	1	37,608
Is Unemployed	0.0803	0.2421	0	1	37,608
Net Individual Income[a]	1,301.2855	2,306.4550	0	50,000.0860	37,608
Has Child in Household	0.2401	0.3850	0	1	37,608
Rental Price[a]	688.6512	338.5719	0	8,248.0000	37,608
Lives in House[b]	0.2322	0.4089	0	1	37,608
Lives in Small Apartment Building	0.1021	0.3087	0	1	37,608
Lives in Large Apartment Building	0.3367	0.4513	0	1	37,608
Lives in High Rise	0.0363	0.1897	0	1	37,608
Number of Rooms per Individual	1.711	0.8815	0.2500	13	37,608
Other Independent Variables - Macro Level					
Unemployment Rate in City	11.9809	3.9593	4.5000	20.8000	37,608
Average Household Income in City [a]	1,484.1110	244.8841	1,047.2000	2,050.4000	37,608

[a] Monthly in Euro, [b] Detached, Semi-Detached, or Terraced

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The respective coverage is measured as the hectares covered by the land use category of interest in a pre-defined radius of 1,000 metres around households. All figures are rounded to four decimal places.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own tabulations

2.3 Empirical Model

2.3.1 Regression Equation

We employ a linear regression model estimated by the fixed-effects (within) estimator, with individual and city of residence fixed effects, and robust standard errors clustered at the city of residence level.[47]. The specification test by Wu (1973) and Hausman (1978), as well as the robust version of this test by Wooldridge (2002) indicate that fixed effects are strictly preferable to random effects. Specifically, both tests reject the null hypothesis of identical parameter estimates between a fixed and a random effects model at the 1% significance level.

We employ the following regression equation:

$$\begin{aligned} y_{it} = \beta_0 + \mathbf{MIC'_{it}}\boldsymbol{\beta}_1 + \mathbf{MAC'_{it}}\boldsymbol{\beta}_2 + \mathbf{GEO'_{it}}\boldsymbol{\beta}_3 + \\ + \mathbf{LUC'_{i}}\boldsymbol{\delta}_1 + \mathbf{LUC^{2\prime}_{i}}\boldsymbol{\delta}_2 + \eta_c + \mu_i + \epsilon_{it} \end{aligned} \tag{2.1}$$

where y is *satisfaction with life* as the regressand; β_0 is the constant; $\beta_1 - \beta_3$ and $\delta_1 - \delta_2$ are the coefficients; MIC, MAC, and GEO are the vectors of controls at the micro, macro, and geo level, respectively; η_c and μ_i are (time-invariant) unobserved heterogeneity or fixed effects at the city of residence and individual level, respectively; ϵ_{it} is the idiosyncratic disturbance of resident i in time period t; and LUC is a vector of either the distances to or the coverages of the different land use categories, respectively, as the regressors of interest.[48]

Following the literature on the use of green space (see, for example, Schipperijn, Stigsdotter, et al. (2010) and Schipperijn, Ekholm, et al. (2010)), we estimate one set of models including distances and another one including coverages. The rationale behind this approach is that, in this literature, both proximities and sizes are seen as proxies for the use of green space.[49] The intuition behind this is simple. Take, for example, a household that is surrounded by a high coverage of green space: it is very likely that this household is also located at a close distance to

47. Notably, using a linear regression model introduces measurement error, as *satisfaction with life* is a discrete, ordinal variable. However, this has become common practice, as discrete models for ordinal variables are not easily applicable to the fixed-effects (within) estimator, and the bias resulting from this measurement error has been found to be negligible (see, for example, Ferrer-i-Carbonell and Frijters (2004) for panel data and Brereton et al. (2008) and Ferreira and Moro (2010) for repeated cross-section data)

48. When adding year fixed effects or a linear time trend to account for the fact that life satisfaction might systematically differ between years or change over time, respectively, the results remain qualitatively the same as in the baseline specification (see Table 2.10 in Section 2.7 for these results).

49. Ambrey and Fleming (2013) even argue that coverages can be interpreted as the synthesis of proximities and sizes.

green space.[50] As such, in accordance with this literature, we consistently interpret distances and coverages as different measures of the same concept, namely access to different land use categories.[51] Thus, we estimate both distances and coverages in separate models.

2.3.2 Identification Issues

Typically, when estimating the effect of urban land use on residential well-being, there are three sources from which endogeneity – correlation between the error terms and the regressors that leads to biased and inconsistent parameter estimates – can rise.

First, endogeneity can arise from *omitted variables*, meaning that an observable with explanatory power for the outcome is omitted from the regression, for example, the type of dwelling in which a resident lives. This observable can be either time-variant or time-invariant. We account for time-variant omitted variables by including a rich set of time-variant regressors as controls, all of which have been shown to affect the regressand in the literature.[52][53] Second, endogeneity can arise from *unobserved heterogeneity*, meaning that a time-invariant unobservable with explanatory power for the outcome is omitted from the regression, for example, the baseline level of happiness (see, for example, Clark, Diener, et al. (2008) for a discussion) or personality of a resident. We account for this type of endogeneity by including individual and city of residence fixed effects. Third, endogeneity can arise from *endogenous residential sorting (self-selection or reverse causality)*, meaning that a resident with a higher (lower) preference for a particular land use category self-selects into an urban area with a higher (lower) access to it, whereby the preference is correlated with the outcome. For example, happier (unhappier) residents might move to an urban area with more (less) green urban areas, which, in turn, makes them even happier (unhappier). This can happen either prior to the observation period, so that we have an issue of *preference heterogeneity*, which we already account for by including individual fixed effects, or during the observation period, so that we have an issue of *simultaneity*: this issue is rarely discussed in the literature, and including fixed effects alone does not solve it.

To account for simultaneity, we would need a source of exogenous variation (that is, an instrument) that changes the presence of a particular land use category (that is, relevance of the

50. In other words, there should be a negative correlation between distances and coverages, indicating that they are substitutes rather than complements, which is also what we find; for example, -0.5113 for green urban areas.

51. Notably, when including both distances and coverages in the same regression equation, we find that one of them systematically becomes insignificant, although which one differs for different land use categories (see Table 2.11 in Section 2.7 for this result).

52. See Frey (2010) for a review of the relevant controls.

53. We automatically account for all time-invariant variables, both observable and unobservable, by including individual and city of residence fixed effects.

instrument) in an urban area without at the same time affecting the well-being of its residents (that is, exogeneity of the instrument). Unfortunately, our merged dataset is a quasi-panel, which includes only one observation on the different land use categories, with no variation over time. This is simply due to data limitations, as the European Urban Atlas, to date, includes only one wave. However, even if more than one wave was available, we would need a source of *exogenous* variation, such as an urban land reform, to solve the issue of simultaneity and establish causality. To our knowledge, such a source of exogenous variation does not exist for Germany during the observation period.

Given these data and institutional limitations, we cannot completely solve the issue of simultaneity, but we can try to work around it and evaluate the extent to which it plays a role in the given context. We work around it by including both individual and city of residence fixed effects to have the effects identified by movers (of the 6,194 individuals in our final sample, 418 move at some point), who are moving primarily for reasons unrelated (that is, orthogonal) to the different land use categories in their surroundings.[54] In fact, 81% of them are moving primarily for reasons that are not directly linked to their location.[55] We take this as initial evidence that simultaneity plays only a minor role, which is also found in other contexts (see, for example, Chay and Greenstone (2005) for the context of air pollution).[56] As a robustness check, we regress a dummy variable that equals one in the time period in which a resident moves, and zero otherwise, on the distances to the different land use categories in order to test whether these distances affect moving behaviour: none of the parameter estimates is significant (the same is true when using coverages instead of distances).[57] We take this as additional evidence that simultaneity plays only a minor role.

Although we can show that, by identifying impacts through movers and by tabulating their primary moving reasons, simultaneity due to endogenous residential sorting seems to play only a minor role, we still cannot fully exclude it. In particular, we cannot exclude the case of *conditional* endogenous residential sorting, which perceives moving as a two-step process: individuals might move primarily for reasons unrelated to their surroundings in a first step, but

54. Note that city of residence fixed effects are the smallest administrative area fixed effects that are readily usable in the German Socio-Economic Panel. Using individual times city fixed effects yields similar results as when using both individual and city of residence fixed effects separately.

55. The German Socio-Economic Panel includes an item that asks respondents whether they moved in the previous time period, as well as a follow-up item that asks respondents about the primary reason for moving. This follow-up item commingles reasons for moving home with reasons for choice of location by providing answer possibilities in both domains, including notice given by the landlord; buying a house or an apartment; inheritance; job reasons; marriage, breakup, or other family reasons; the size of the dwelling; the price of the dwelling; the standard of the dwelling; the standard of the location; the standard of the surroundings; and other reasons. We combine all categories except for the standard of the location and the standard of the surroundings into one category that we assume not to be directly linked to the location of respondents.

56. The results are robust to excluding city of residence fixed effects. See Table 2.12 in Section 2.7 for this result.

57. See Table 2.13 in Section 2.7 for this result.

conditional on moving anyway, might optimise with respect to their surroundings at their new location in a second step. As we have no information on secondary moving reasons, we cannot look into this issue, and should cautiously interpret our identified impacts as associations.

2.4 Results

The effects of the distances to and the coverages of the different land use categories on life satisfaction can be seen in Tables 2.3 and 2.4, respectively.[58]

As can be seen in Table 2.3, the distance to green urban areas has a significantly negative effect on life satisfaction at the 1% level, whereas the distance to abandoned areas has a significantly positive effect on it at the same level. Both effects are non-linear: increasing the distance to green urban areas significantly decreases life satisfaction, whereas increasing the distance to abandoned areas significantly increases it, at a decreasing rate, respectively. This is in line with the notion of diminishing marginal returns to utility or disutility in neoclassical theory.[59] Both effects are, however, rather small: increasing the distance to green urban areas by 100 metres, given a mean distance of 249 metres, decreases life satisfaction only by 4% of a standard deviation, whereas increasing the distance to abandoned areas by 100 metres, given a mean distance of 927 metres, increases it only by 3% of a standard deviation. As can be seen in Table 2.4, almost the same picture arises when looking at the effects of the coverages of green urban and abandoned areas in a pre-defined radius of 1,000 metres around households on life satisfaction. The sizes of these effects, again rather small, are slightly different, though: increasing the coverage of green urban areas by one hectare, given a mean coverage of 22 hectares, increases life satisfaction by 0.6% of a standard deviation, whereas increasing the coverage of abandoned areas by one hectare, given a mean coverage of one hectare, decreases it by 4% of a

58. Regarding controls, having very good health has a significantly positive effect on life satisfaction at the 1% level, whereas being older, having very bad health, and being disabled has a significantly negative effect on it at the 5% and 1% level, respectively. Moreover, being on parental leave has a significantly positive effect on life satisfaction at the 1% level, whereas individual income has a significantly positive effect on it at the 1% level. Finally, being unemployed and the unemployment rate are most detrimental to life satisfaction and among the largest regression coefficients (Clark and Oswald 2004; Blanchflower 2008). See Section 2.7 for the full tables.

59. However, the effect of the squared distance to green urban areas is significant at the 10% level only in the baseline specification.

Table 2.3: Results – Final Sample, Satisfaction With Life, FE Model, Distances

Regressors	Satisfaction With Life FE
Distance to Green Urban Areas	-0.0391***
	(0.0129)
Distance to Forests	-0.0031
	(0.0042)
Distance to Waters	0.0023
	(0.0078)
Distance to Abandoned Areas	0.0289***
	(0.0103)
Distance to Green Urban Areas Squared	0.0018*
	(0.0009)
Distance to Forests Squared	-0.0000
	(0.0000)
Distance to Waters Squared	-0.0002
	(0.0004)
Distance to Abandoned Areas Squared	-0.0011**
	(0.0005)
Controls	Yes
Constant	Yes
Number of Observations	29,729
Number of Individuals	6,194
Adjusted R^2	0.0563

Robust standard errors in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own calculations

Table 2.4: Results - Final Sample, Satisfaction With Life, FE Model, Coverages

Regressors	Satisfaction With Life FE
Coverage of Green Urban Areas	0.0059***
	(0.0021)
Coverage of Forests	-0.0029
	(0.0021)
Coverage of Waters	-0.0043
	(0.0039)
Coverage of Abandoned Areas	-0.0401***
	(0.0132)
Coverage of Green Urban Areas Squared	-0.0001***
	(0.0000)
Coverage of Forests Squared	0.0000
	(0.0001)
Coverage of Waters Squared	0.0001
	(0.0003)
Coverage of Abandoned Areas Squared	0.0015*
	(0.0008)
Controls	Yes
Constant	Yes
Number of Observations	29,729
Number of Individuals	6,194
Adjusted R^2	0.0565

Robust standard errors in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective coverage is measured as the hectares covered by the land use category of interest in a pre-defined radius of 1,000 metres around households. All figures are rounded to four decimal places. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own calculations

standard deviation.[60]

The effect sizes compare well to those found in the literature: for example, for green urban areas, we find that a one-hectare increase in green spaces in a pre-defined radius of 1,000 metres or an area of about 314 hectares around households raises life satisfaction, measured on a zero-to-ten scale, on average by about 0.0059 points, which compares very well to White et al. (2013), who find an effect size of about 0.0012 points for a similar increase.[61] Despite using a similar empirical specification (individual fixed effects), differences in the effect sizes might arise due to differences in the urbanity of the final sample: we focus on major German cities with inhabitants greater than or equal to 100,000 (mean coverage of green spaces of about seven percent per area), whereas the authors focus on lower-level super-output areas in England with inhabitants greater than or equal to 10,000 (mean coverage of green spaces of about 65 percent per area). Bertram and Rehdanz (2015) find that a one-hectare increase in green spaces in a pre-defined radius of 1,000 metres or an area of about 314 hectares around households raises life satisfaction, measured on a zero-to-ten scale, on average by about 0.0452 points, which is much larger than what we find. The authors, however, use a different empirical specification (ordinary least squares) and focus on Berlin only.

Although the effects are rather small in size, small effects at the individual level can translate into substantial effects at the community level: on average, in our final sample consisting of major German cities with inhabitants greater than or equal to 100,000, a one-hectare increase in green spaces in a pre-defined radius of 1,000 metres or an area of about 314 hectares around households would affect 6,089 people, yielding a 35.9 increase in aggregated life satisfaction (given the very small effect size of the squared term, taking the non-linearity into account, it would still translate into a 34.7 increase in aggregated life satisfaction). There is also some evidence that these effects are persistent in nature (Alcock et al. 2014).

Up to now, the effects of the distances to and the coverages of green urban and abandoned

60. Clearly, an increase in one category of urban land use by one hectare comes at the expense of all other, remaining categories in a pre-defined radius of 1,000 metres or an area of about 314 hectares around households, relative to their respective initial shares, which can differ for different households in different cities. Assume that all 21 categories of urban land use recorded in the European Urban Atlas have the same initial shares in that area, namely about 14.95 hectares ($314ha/21$). An increase in one category of urban land use by one hectare then decreases the respective initial shares of all other, remaining categories by 0.05 hectares ($1ha/20$). How this affects the estimates, that is, whether they are lower or upper bounds of the true effects, depends on the distribution of all these other categories, and is difficult to say *ex-ante*. If these categories have, on average, positive effects on life satisfaction, the estimated effects are lower bounds; otherwise, they are upper bounds. See Section 2.7 Table 2.9 for the average distribution of the most important other categories of urban land use in terms of size within a one kilometre radius around households.

61. White et al. (2013) show that a one-percent increase in green spaces in an area of about 400 hectares around households raises life satisfaction, measured on a one-to-seven scale, on average by about 0.0020 points. Given a mean amount of green spaces of about 259 hectares in that area, after rescaling the life-satisfaction measure from a one-to-seven to a zero-to-ten scale, this translates into an effect size of about 0.0012 points for a one-hectare increase ($((((0.0020 \times 11)/7)/2.59ha) \times 1ha = 0.0012)$.

areas on life satisfaction were estimated jointly for all residents. Naturally, the question arises whether the rather small effects for average residents hide potentially larger effects for different types of residents. To shed light on this question, in Tables 2.5 and 2.6, they are estimated separately for different population sub-groups, including residents who are female, who are older, who live in low-income households, and who have at least one child in the household.[62]

As can be seen in Tables 2.5 and 2.6, there is evidence that the effects of the distances to and the coverages of both green urban and abandoned areas on life satisfaction are stronger for residents who are older, whereas only the effects of abandoned areas are, for some of our measures, stronger for residents who live in high-income households and residents who do not have a child in the household. Moreover, there is some evidence that the effects are stronger for residents who are male. To sum up, it seems that, although the evidence is partly different from what we expected, especially as we expected residents who have at least one child in the household to show stronger effects, they clearly differ for different types of residents. In fact, it seems that the rather small effects for average residents translate into substantial effects for older residents, being up to five times more sizeable: increasing the distance to green urban areas by 100 metres, given a mean distance of 249 metres, decreases the life satisfaction of older residents by 18% of a standard deviation, whereas increasing the distance to abandoned areas by 100 metres, given a mean distance of 927 metres, increases it by 7% of a standard deviation.[63]

What would residents be willing to pay in order to have better access to green urban areas, and to avoid having abandoned areas around them? To answer this question, we value the effects of the distances to and the coverages of green urban and abandoned areas on life satisfaction monetarily, using the *life satisfaction approach.* Compared to both stated and revealed preference approaches, the life satisfaction approach has a number of advantages. Compared to stated preference approaches, such as contingent valuation or discrete choice experiments, it avoids bias resulting from the complexity of or attitudes towards the public good, which might lead to superficial or symbolic valuation. Rather than asking individuals to value a complex public good in a hypothetical situation, the life satisfaction approach does not rely on the ability of individuals to consider all relevant consequences of a change in the provision of the public good, reducing the cognitive burden that is typically associated with

62. For these heterogeneity analyses, we split the final sample by mean gender (53% are female), age (50% are above 49 years old), monthly net household income (50% have a monthly net household income lower than 2,500 Euro), and presence of children in the household (24% have at least one child in the household). Alternatively, instead of using split samples, one could also use the full sample with interaction terms between the variables of interest and the stratifying variables: the results remain qualitatively the same regardless of the approach used.

63. We also found some evidence that the effects of the distances to and the coverages of both green urban and abandoned areas on life satisfaction tend to be stronger, respectively, in cities with lower shares of them, and *vice versa*.

Table 2.5: Results – Sub-Samples, Satisfaction With Life, FE Models, Distances

	Satisfaction With Life							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to Green Urban Areas	-0.0378*	-0.0450*	-0.1842***	-0.0169	-0.0186	-0.0487	-0.0133	-0.0901***
	(0.0202)	(0.0244)	(0.0354)	(0.0145)	(0.0253)	(0.0301)	(0.0225)	(0.0203)
Distance to Forests	0.0033	-0.0048	-0.0122	-0.0029	0.0041	-0.0034	-0.0273***	0.0095
	(0.0098)	(0.0095)	(0.0149)	(0.0078)	(0.0082)	(0.0082)	(0.0092)	(0.0078)
Distance to Waters	0.0141	-0.0004	0.0331	-0.0068	0.0169	-0.0020	0.0012	0.0054
	(0.0092)	(0.0086)	(0.0202)	(0.0089)	(0.0201)	(0.0121)	(0.0185)	(0.0090)
Distance to Abandoned Areas	0.0217	0.0318**	0.0671***	0.0129	0.0689***	-0.0052	0.0167	0.0278**
	(0.0144)	(0.0151)	(0.0114)	(0.0155)	(0.0152)	(0.0172)	(0.0208)	(0.0131)
Distance to Green Urban Areas Squared	0.0007	0.0022**	0.0093***	0.0004	0.0002	0.0035	0.0004	0.0051***
	(0.0010)	(0.0011)	(0.0013)	(0.0010)	(0.0008)	(0.0028)	(0.0010)	(0.0011)
Distance to Forests Squared	-0.0001	-0.0000	0.0000	0.0000	-0.0001	0.0001	0.0002	-0.0002
	(0.0002)	(0.0003)	(0.0003)	(0.0000)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Distance to Waters Squared	-0.0002	-0.0000	-0.0008	0.0002	-0.0005	0.0001	0.0001	-0.0003
	(0.0003)	(0.0002)	(0.0010)	(0.0005)	(0.0007)	(0.0003)	(0.0005)	(0.0004)
Distance to Abandoned Areas Squared	-0.0009	-0.0015*	-0.0031***	-0.0005	-0.0023***	-0.0002	-0.0002	-0.0009
	(0.0008)	(0.0009)	(0.0009)	(0.0004)	(0.0003)	(0.0005)	(0.0003)	(0.0010)

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	Satisfaction With Life							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	15,462	14,267	15,756	13,973	15,162	14,567	7,138	22,591
Number of Individuals	3,221	2,973	3,283	2,911	3,159	3,035	1,487	4,707
Adjusted R^2	0.0548	0.0612	0.0688	0.0521	0.0525	0.0591	0.0462	0.0599

(1) Female Sub-Sample, (2) Male Sub-Sample, (3) Old-Age Sub-Sample, (4) Young-Age Sub-Sample, (5) High-Income Sub-Sample, (6) Low-Income Sub-Sample, (7) Child Sub-Sample, (8) Non-Child Sub-Sample

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. All figures are rounded to four decimal places. The impacts are identified by 219 movers in model (1), 199 movers in model (2), 145 movers in model (3), 259 movers in model (4), 264 movers in model (5), 216 movers in model (6), and 130 movers in model (7). The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own calculations

Table 2.6: Results – Sub-Samples, Satisfaction With Life, FE Models, Coverages

	Satisfaction With Life							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coverage of Green Urban Areas	0.0060	0.0052	0.0181***	0.0035	0.0038**	0.0093**	0.0087	0.0055*
	(0.0048)	(0.0042)	(0.0052)	(0.0031)	(0.0019)	(0.0044)	(0.0076)	(0.0032)
Coverage of Forests	-0.0011	-0.0022	-0.0005	-0.0009	0.0011	-0.0033	0.0065	-0.0031
	(0.0029)	(0.0045)	(0.0034)	(0.0024)	(0.0042)	(0.0053)	(0.0057)	(0.0045)
Coverage of Waters	-0.0052	-0.0010	-0.0025	-0.0031	-0.0149***	0.0063	-0.0245***	0.0032
	(0.0055)	(0.0046)	(0.0067)	(0.0036)	(0.0042)	(0.0053)	(0.0059)	(0.0044)
Coverage of Abandoned Areas	-0.0401	-0.0363	-0.0823***	-0.0220	-0.2011***	0.0012	-0.0123	-0.0675***
	(0.0289)	(0.0283)	(0.0230)	(0.0198)	(0.0398)	(0.0321)	(0.0301)	(0.0172)
Coverage of Green Urban Areas Squared	-0.0000	-0.0001	-0.0003***	-0.0000	-0.0002	-0.0002***	-0.0001	-0.0003***
	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0004)	(0.0000)	(0.0002)	(0.0000)
Coverage of Forests Squared	0.0000	0.0000	0.0000	0.0000	-0.0001	0.0000	-0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Coverage of Waters Squared	0.0001	0.0000	0.0000	0.0001	0.0001	-0.0000	0.0003***	-0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0000)	(0.0002)
Coverage of Abandoned Areas Squared	0.0002	0.0015	0.0036***	-0.0003	0.0161***	0.0003	-0.0004	0.0026**
	(0.0012)	(0.0012)	(0.0010)	(0.0016)	(0.0023)	(0.0015)	(0.0022)	(0.0012)

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	Satisfaction With Life							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	15,462	14,267	15,756	13,973	15,162	14,567	7,138	22,591
Number of Individuals	3,221	2,973	3,283	2,911	3,159	3,035	1,487	4,707
Adjusted R^2	0.0548	0.0611	0.0623	0.0532	0.0528	0.0601	0.0486	0.0601

(1) Female Sub-Sample, (2) Male Sub-Sample, (3) Old-Age Sub-Sample, (4) Young-Age Sub-Sample, (5) High-Income Sub-Sample, (6) Low-Income Sub-Sample, (7) Child Sub-Sample, (8) Non-Child Sub-Sample

Robust standard errors in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective coverage is measured as the hectares covered by the land use category of interest in a pre-defined radius of 1,000 metres around households. All figures are rounded to four decimal places. The impacts are identified by 219 movers in model (1), 199 movers in model (2), 145 movers in model (3), 259 movers in model (4), 264 movers in model (5), 216 movers in model (6), and 130 movers in model (7). The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own calculations

stated preference approaches. Moreover, it does not reveal to individuals the relationship between life satisfaction and the public good, reducing the incentive to answer in a strategical or socially desirable way. Contrary to revealed preference approaches, such as hedonic pricing, it avoids bias resulting from the assumption that the market for the private good taken to be the complement of the public good is in equilibrium, which is violated in the presence of low variety of private goods, slow adjustment of prices, incomplete information, and transaction costs. Rather than assuming that the provision of the public good is reflected in market transitions, the life satisfaction approach requires only that life satisfaction constitutes a valid approximation of welfare. Finally, it avoids bias resulting from misprediction of utility, which is common to both stated and revealed preference approaches (Frey and Stutzer 2013).[64]

What would residents be willing to pay in order to have better access to green urban areas, and to avoid having abandoned areas around them? To answer this question, we value the effects of the distances to and the coverages of green urban and abandoned areas on life satisfaction monetarily, using the *life satisfaction approach*. Compared to both stated and revealed preference approaches, the life satisfaction approach has a number of advantages. Compared to stated preference approaches, such as contingent valuation or discrete choice experiments, it avoids bias resulting from the complexity of or attitudes towards the public good, which might lead to superficial or symbolic valuation. Rather than asking individuals to value a complex public good in a hypothetical situation, the life satisfaction approach does not rely on the ability of individuals to consider all relevant consequences of a change in the provision of the public good, reducing the cognitive burden that is typically associated with stated preference approaches. Moreover, it does not reveal to individuals the relationship between life satisfaction and the public good, reducing the incentive to answer in a strategical or socially desirable way. Contrary to revealed preference approaches, such as hedonic pricing, it avoids bias resulting from the assumption that the market for the private good taken to be the complement of the public good is in equilibrium, which is violated in the presence of low variety of private goods, slow adjustment of prices, incomplete information, and transaction costs. Rather than assuming that the provision of the public good is reflected in market transitions, the life satisfaction approach requires only that life satisfaction constitutes a valid approximation of welfare. Finally, it avoids bias resulting from misprediction of utility, which is common to both stated and revealed

64. Naturally, the life satisfaction approach is not entirely free of methodological issues itself. For example, for life satisfaction to constitute a valid approximation of welfare, the data should be at least ordinal in nature. Moreover, the micro-econometric function that relates life satisfaction to the public good should be correctly specified. However, these requirements are typically met in practice (Welsch and Kühling 2009).

preference approaches (Frey and Stutzer 2013).[65]

We can calculate the marginal willingness-to-pay ($MWTP$) of residents in order to change the access to green urban and abandoned areas in their surroundings, using the formula:

$$\begin{aligned} MWTP &= \left. \frac{\frac{\partial y}{\partial measure}}{\frac{\partial y}{\partial income}} \right|_{\partial y = 0} \\ &= \frac{\bar{X}_{income}(\hat{\beta}_{measure} + 2\hat{\beta}_{measure^2}\bar{X}_{measure})}{\hat{\beta}_{income}} \end{aligned} \tag{2.2}$$

where y is *satisfaction with life* as the regressand; $\bar{X}$ is the respective mean; $\hat{\beta}$ is the respective regression coefficient; *measure* is either the distance to or the coverage of green urban and abandoned areas, respectively; and *income* is the monthly net individual income in Euro.

We find that, *ceteris paribus*, residents are, on average, willing to pay 42 Euro of monthly net individual income in order to increase the coverage of green urban areas in a pre-defined radius of 1,000 metres around households by one hectare, given a mean coverage of 22 hectares, whereas they are, on average, willing to pay 1,050 Euro in order to decrease the coverage of abandoned areas by one hectare, given a mean coverage of one hectare.[66] Moreover, we find that, *ceteris paribus*, residents are, on average, willing to pay 900 Euro in order to decrease the distance between households and green urban areas by 100 metres, given a mean distance of 249 metres, whereas they are, on average, willing to pay 254 Euro in order to increase the distance between households and abandoned areas by 100 metres, given a mean distance of 927 metres. Note that the marginal willingness-to-pay is hypothetical and does not imply feasibility, neither that it is feasible for residents to actually pay the amount, given their budget constraints, nor that it is feasible for urban planners to actually implement the change, given their urban building, treasury, and policy constraints.

We can also calculate the optimal values (X^*) of the distances to and the coverages of green urban and abandoned areas, using the following formula:[67]

65. Naturally, the life satisfaction approach is not entirely free of methodological issues itself. For example, for life satisfaction to constitute a valid approximation of welfare, the data should be at least ordinal in nature. Moreover, the micro-econometric function that relates life satisfaction to the public good should be correctly specified. However, these requirements are typically met in practice (Welsch and Kühling 2009).

66. Notably, the calculated marginal willingness-to-pay of 42 Euro in order to increase the coverage of green urban areas is slightly higher than the 25 Euro calculated by Bertram and Rehdanz (2015), but is much less than the 1,806 Euro calculated by Ambrey and Fleming (2013), converted with an exchange rate of 1,5130 EUR/AUD, as of December 12, 2014.

67. Notably, the values are optimal in the sense that they maximise life satisfaction.

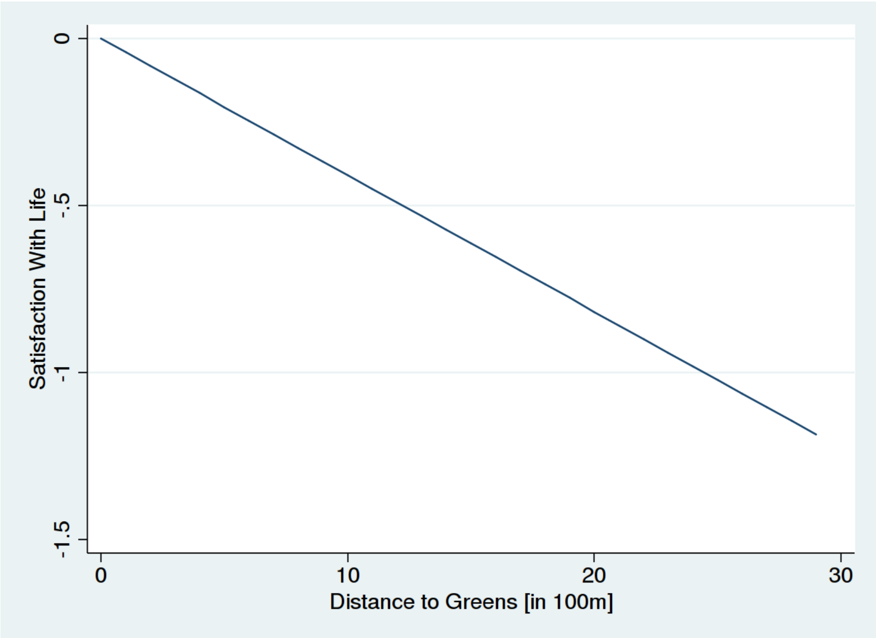

Figure 2.2: Results – Optimal Value of Distance to Green Urban Areas

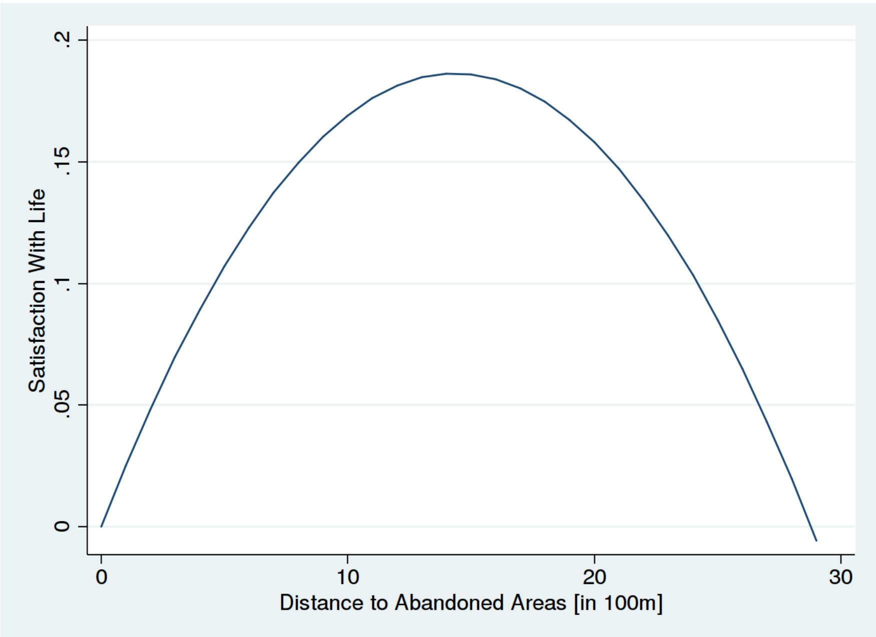

Figure 2.3: Results – Optimal Value of Distance to Abandoned Areas

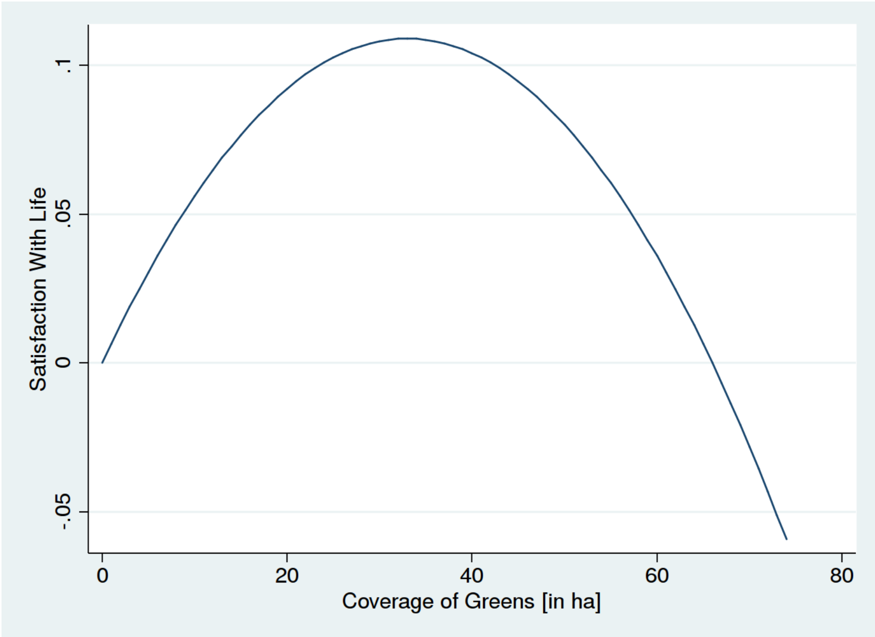

Figure 2.4: Results – Optimal Value of Coverage of Green Urban Areas

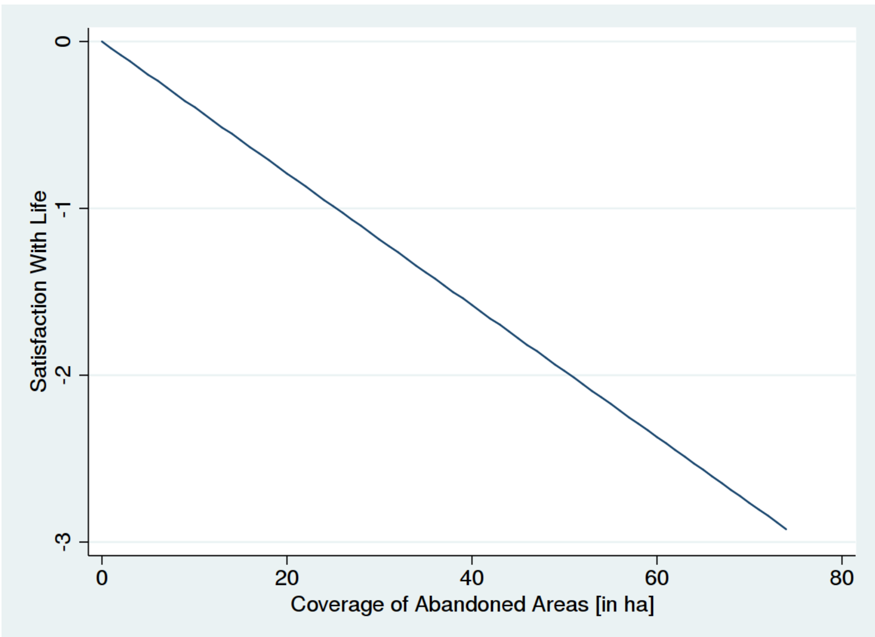

Figure 2.5: Results – Optimal Value of Coverage of Abandoned Areas

$$X^*_{measure} = -\frac{\hat{\beta}_{measure}}{2\hat{\beta}_{measure^2}} \tag{2.3}$$

where $\hat{\beta}$ is the respective regression coefficient and *measure* is either the distance to or the coverage of green urban and abandoned areas, respectively.

We find that, *ceteris paribus*, the optimal value of the coverage of green urban areas in a pre-defined radius of 1,000 metres around households is, on average, 30 hectares, whereas the optimal value of the coverage of abandoned areas is, on average, zero hectares. Moreover, we find that, *ceteris paribus*, the optimal value of the distance between households and green urban areas is, on average, zero metres, whereas it is, on average, 1,314 metres for abandoned areas.[68]

The intuition behind the optimal values of zero hectares and metres, respectively, for the coverage of abandoned areas and the distance to green urban areas is straightforward: the life satisfaction of residents is maximised, everything else held constant, whenever there are no abandoned areas in their surroundings and whenever they live closest to the nearest green urban area.

2.5 Policy Implications

For urban planning and development, we can calculate the net well-being benefit in pecuniary terms that arises, on average, when increasing the coverage of green urban areas in a pre-defined radius of 1,000 metres around households by one hectare. This is especially interesting in view of the fact that there is, on average, an under-supply of green urban areas in major German cities; the mean and optimal value is 22 and 30 hectares, respectively. We know that the gross well-being benefit in pecuniary terms that arises, on average, when increasing the coverage of green urban areas in a pre-defined radius of 1,000 metres around households by one hectare is 3,068,856 Euro annually.[69] The costs of the construction and maintenance of green urban areas

68. The optimal values of zero for the coverage of abandoned areas and the distance to green urban areas come from the assumption that the effects of the squared coverage of abandoned areas and the squared distance to green urban areas on life satisfaction are insignificant, given that they are significant at the 10% level only.

69. We obtain this number from the following thought experiment: We describe a circle around a new green urban area of one hectare size such that all households within this circle have the new green urban area in a pre-defined radius of 1,000 metres around them. We know that residents are, on average, willing to pay 42 Euro of monthly net individual income in order to increase the coverage of green urban areas in a pre-defined radius of 1,000 metres around households by one hectare. We know that the average population density is 2.177 individuals per square metre, yielding 6,089 individuals within the circle around the new green urban area. We obtain the gross well-being benefit in pecuniary terms as $(12 \times 42 \times 6,089) = 3,068,856$. See Figure 2.6 for an illustration.

differ between cities and neighbourhoods depending on the type of facilities and intensity of usage. We take parks in Berlin as an example. The average construction costs of parks range from 5 Euro per square metre for parks located near the city periphery, with average quality and no particular infrastructure, to 201 Euro per square metre for parks located near the city centre, with high quality and cost-intensive infrastructure, yielding average construction costs of an additional hectare of park between 3,333 and 134,000 Euro annually (Senate Department for Urban Development and the Environment 2010). The average life span of parks is 15 years, after which major reinvestments become necessary. The average maintenance costs of parks range from 2 Euro per square metre annually for parks with no particular infrastructure to 7 Euro per square metre annually for parks with cost-intensive infrastructure, yielding average maintenance costs of an additional hectare of park between 20,000 and 70,000 Euro annually (Senate Department of Finance 2013). As such, the average total costs of an additional hectare of park range between 23,333 and 204,000 Euro annually. Thus, the net well-being benefit in pecuniary terms that arises, on average, when increasing the coverage of green urban areas in a pre-defined radius of 1,000 metres around households by one hectare ranges between 2,864,856 and 3,045,523 Euro annually.

Naturally, this cost-benefit analysis is only a crude back-of-the-envelope calculation based on a partial-equilibrium analysis, as it does not take into account the effects of the new green urban area on the house prices and rents in its surroundings, as well as other externalities. Moreover, taking the example of parks in Berlin, we implicitly assume that green urban areas are equivalent to parks; there is, however, quite some heterogeneity in this land use category, which can include, for example, zoos, gardens, parks, and castle parks, as well as suburban natural areas used as parks, all of which are likely to differ in their effect on residential well-being. Nevertheless, the above cost-benefit analysis shows that there is a substantial net well-being benefit in pecuniary terms from reducing the under-supply of green urban areas in major German cities, and, as the heterogeneity analysis suggests, urban areas with high shares of elderly might profit the most. A straightforward, and potentially cost-effective, way to reduce this under-supply would be to transform life-satisfaction reducing abandoned areas, ideally already in possession of the city, into life-satisfaction raising green urban areas. However, even in case that vacant land (which is expensive in densely populated areas) has to be purchased first, it is straightforward to show that, when assuming an infinite lifetime of a park and a reasonable discount rate, purchasing vacant land in order to transform it into a park brings with it a positive net present value.

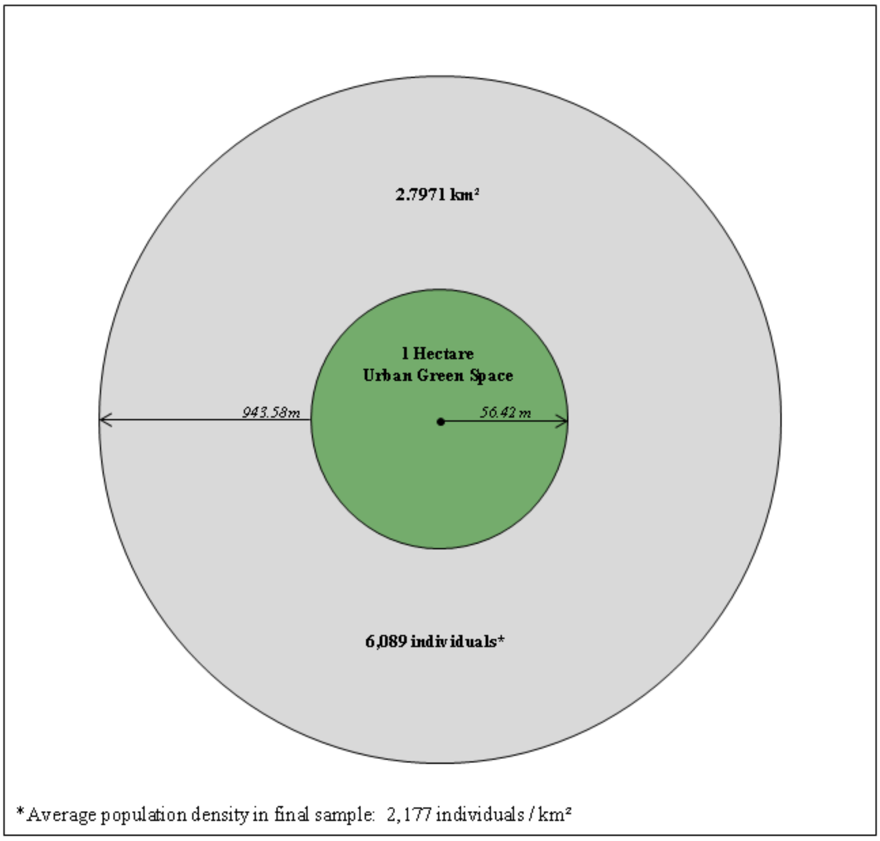

Figure 2.6: Thought Experiment

2.6 Discussion

We show that, for the 32 major German cities with greater than or equal to 100,000 inhabitants, access to green urban areas matters for residential well-being, but access to abandoned areas matters even more, whereas access to forests and waters does not matter much. In fact, coverage of and, even more so, proximity to green urban areas are significantly positively associated with, whereas proximity to and, even more so, coverage of abandoned areas are significantly negatively associated with life satisfaction. Moreover, these relationships are concave in nature. Finally, the effects are strongest for residents who are older. While the positive effect of green urban areas on life satisfaction might be explained by their provision of publicly accessible land for recreation and social interaction, the negative effect of abandoned areas might be explained by the negative effect of vacant land on mental and physical health identified in earlier studies (see, for example, Branas et al. (2011) and Garvin et al. (2013)). Moreover, there is a considerable emerging literature on vacant land and social segregation, (perceived) unsafety, and (fear of) crime in response to land use characteristics and neighbourhood physical environment (see, for example, Bixler and Floyd (1997), Kuo et al. (1998), Branas et al. (2011), and Branas et al. (2012)). All these aspects might be important transmission mechanisms through which the negative effect of abandoned areas on life satisfaction might arise.

Our results on green urban areas confirm the results of a similar study by White et al. (2013). White et al. (2013) show that green urban areas do not only have a positive effect on the mental health of residents in England, but also on their life satisfaction. However, besides the fact that the authors only investigate the effects of green urban areas and waters on residential well-being, there are other important differences between their study and ours. White et al. (2013), using panel data from the British Household Panel Study, adopt a similar approach in terms of the empirical model, especially when it comes to using fixed-effects (within) estimators, but, using cross-section data from the General Land Use Database, adopt a different approach in terms of the data on urban land use. In fact, their data are based on aggregated areas, which are, in turn, based on population densities. As a result, these areas differ from each other in size and shape, implying that more densely populated areas are smaller than less densely populated ones, *et vice versa*. On the contrary, our data are, among others, based on coverages, which are, in turn, based on pre-defined radii around households. As a result, these areas are equal to each other in size and shape. Moreover, they are free from methodological issues that arise when aggregating geographical information. This is a strong advantage, especially when considering the geographical location and mobility of households.[70] Nevertheless, White et al. (2013), like us, have only cross-section data on urban land use, essentially relying on residents who move from one urban area to another in order to provide variation in and therewith identify the effect of green urban areas on residential well-being. As a result, White et al. (2013), like us, cannot account for simultaneity and therewith cannot claim that the identified effects are causal; in fact, their empirical model is more prone to simultaneity than ours, as they do not include both individual and city of residence fixed effects. In any case, this issue has been found to be minor in other contexts, and we conduct several robustness checks to show that it is also minor here.

Naturally, our data on urban land use are not entirely free of limitations themselves. First, they only include objects of a minimum size of 0.25 hectare. This introduces measurement error, as the accumulation of objects of smaller sizes is neglected, which is especially problematic for coverages in case that radii are small. However, the bias resulting from this measurement error is likely to be minor, as the pre-defined radius of 1,000 metres around households is rather small. Second, the European Urban Atlas is only available for the year 2006, whereas the German Socio-Economic Panel is available for the time period between 2000 and 2012. This

70. See Holt et al. (1996) for a review of issues regarding the use of aggregated data.

introduces measurement error, as the data on urban land use are cross-section data and the data on residential well-being are panel data, implying that single-year observations of urban land use are assigned to multiple-year observations of residential well-being. However, the bias resulting from this measurement error is, again, likely to be minor, as the presence of the different land use categories is rather persistent over time.[71] Another aspect that could limit our findings is bias resulting from omitted or unobserved variables. For example, the amenity value of privately owned open space is often discussed in the literature (see, for example, Bolitzer and Netusil (2000) and Irwin and Bockstael (2001), as well as Walsh (2007) and Strong and Walsh (2008) for theoretical models on endogenous, private provision of open space), and our data on urban land use provide only information on public open space, ignoring privately owned green or brown fields. However, considering that such time-invariant unobserved heterogeneity between cities should be captured by the city of residence fixed effects, the bias resulting from omitted or unobserved variables in form of privately owned open space is, once again, likely to be minor.

In view of these limitations, there is a lot of room for further research. Most importantly, further research should be directed towards establishing the causality of the identified effects, potentially by exploiting novel panel data on and exogenous variation in urban land use, which might become available in the future. Moreover, further research should be directed towards incorporating the role that the quality of the different land use categories plays for residential well-being. Taken together, the spatial analysis of the relationship between urban land use and residential well-being remains a promising field of research.

71. When narrowing down the observation period around the year 2006, the results remain qualitatively the same as in the baseline model.

2.7 Online Appendix to Chapter 2

Table 2.7: Results – Final Sample, Satisfaction With Life, OLS/FE Models, Distances

	Satisfaction With Life			
Regressors	OLS	OLS	FE	FE
Distance to Green Urban Areas	0.0008	-0.0122**	-0.0293***	-0.0391***
	(0.0071)	(0.0060)	(0.0102)	(0.0129)
Distance to Forests	-0.0101***	-0.0042**	0.0007	-0.0031
	(0.0023)	(0.0021)	(0.0051)	(0.0042)
Distance to Waters	-0.0083***	-0.0019	0.0067	0.0023
	(0.0019)	(0.0031)	(0.0062)	(0.0078)
Distance to Abandoned Areas	0.0302***	0.0289***	0.0192***	0.0289***
	(0.0029)	(0.0052)	(0.0044)	(0.0103)
Distance to Green Urban Areas Squared	0.0015	0.0003	0.0012**	0.0018*
	(0.0019)	(0.0007)	(0.0006)	(0.0009)
Distance to Forests Squared	0.0003***	0.0000	-0.0000	-0.0000
	(0.0000)	(0.0001)	(0.0002)	(0.0000)
Distance to Waters Squared	0.0001	0.0001	-0.0002	-0.0002
	(0.0006)	(0.0003)	(0.0004)	(0.0004)
Distance to Abandoned Areas Squared	-0.0004***	-0.0006***	-0.0006***	-0.0011**
	(0.0001)	(0.0001)	(0.0002)	(0.0005)
Distance to City Centre	-0.0044***	-0.0031***	-0.0002	-0.0007
	(0.0012)	(0.0008)	(0.0018)	(0.0031)
Distance to City Periphery	-0.0058***	-0.0003	0.0046	-0.0005
	(0.0008)	(0.0017)	(0.0040)	(0.0036)
Distance to City Centre Squared	0.0002***	0.0002**	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Distance to City Periphery Squared	-0.0000	-0.0000	-0.0000	0.0000
	(0.0003)	(0.0001)	(0.0000)	(0.0000)
Age		-0.0612***		-0.0242**
		(0.0025)		(0.0109)
Age Squared		0.0008***		-0.0004**
		(0.0001)		(0.0002)
Is Female		0.0513***		
		(0.0168)		
Is Married		0.1243***		-0.0117
		(0.0296)		(0.0642)
Is Divorced		-0.1398***		-0.0987
		(0.0382)		(0.1021)
Is Widowed		0.0367		-0.2217*
		(0.0481)		(0.1202)
Has Very Good Health		0.9802***		0.3605***
		(0.0312)		(0.0288)
Has Very Bad Health		-2.2118***		-1.2101***
		(0.0456)		(0.0456)
Is Disabled		-0.3102***		-0.1598***
		(0.0213)		(0.0412)
Has Migration Background		-0.0451**		
		(0.0213)		
Has Tertiary Degree		0.0339		-0.1112
		(0.0242)		(0.0817)
Has Lower Than Secondary Degree		-0.1502***		-0.0236
		(0.0215)		(0.0989)

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	Satisfaction With Life			
Regressors	OLS	OLS	FE	FE
Is in Education		-0.0602		0.1196
		(0.0753)		(0.0811)
Is Full-Time Employed		-0.1964***		0.0417
		(0.0412)		(0.0422)
Is Part-Time Employed		-0.0712**		-0.0319
		(0.0348)		(0.0417)
Is on Parental Leave		0.3717***		0.2822***
		(0.0652)		(0.0760)
Is Unemployed		-0.9302***		-0.5312***
		(0.0478)		(0.0449)
Log Net Individual Income[a]		0.1195***		0.0436***
		(0.0224)		(0.0124)
Has Child in Household		-0.0024		0.0256
		(0.0263)		(0.0397)
Rental Price[a]		-0.0002***		-0.0001
		(0.0000)		(0.0003)
Lives in House[b]		0.0102		0.0112
		(0.0517)		(0.0287)
Lives in Small Apartment Building		0.0312		0.0127
		(0.0411)		(0.0385)
Lives in Large Apartment Building		-0.0301		-0.0182
		(0.0284)		(0.0304)
Lives in High Rise		-0.0464		-0.0187
		(0.0789)		(0.0470)
Number of Rooms per Individual		0.1217***		0.0142
		(0.0142)		(0.0195)
Unemployment Rate in City		-0.0367***		-0.0211***
		(0.0024)		(0.0038)
Average Household Income in City		0.0000		0.0000
		(0.0002)		(0.0001)
Constant	Yes	Yes	Yes	Yes
Number of Observations	37,608	29,729	37,608	29,729
Number of Individuals	7,836	6,194	7,836	6,194
Adjusted R^2	0.0412	0.0495	0.0519	0.0563

[a] Monthly in Euro, [b] Detached, Semi-Detached, or Terraced

Robust standard errors in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. All figures are rounded to four decimal places. The impacts are identified by 418 movers in the fixed-effects model.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own calculations

Table 2.8: Results – Final Sample, Satisfaction With Life, OLS/FE Models, Coverages

	Satisfaction With Life			
Regressors	OLS	OLS	FE	FE
Coverage of Green Urban Areas	-0.0002	0.0031***	0.0041*	0.0059***
	(0.0017)	(0.0009)	(0.0022)	(0.0021)
Coverage of Forests	0.0078***	0.0021**	-0.0015	-0.0029
	(0.0011)	(0.0011)	(0.0027)	(0.0021)
Coverage of Waters	0.0021	0.0013	-0.0009	-0.0043
	(0.0018)	(0.0017)	(0.0036)	(0.0039)
Coverage of Abandoned Areas	-0.0476***	-0.0336***	-0.0317***	-0.0401***
	(0.0063)	(0.0062)	(0.0121)	(0.0132)
Coverage of Green Urban Areas Squared	-0.0000	-0.0001***	-0.0001*	-0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)
Coverage of Forests Squared	-0.0001***	-0.0000**	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Coverage of Waters Squared	-0.0000	-0.0000	0.0000	0.0001
	(0.0002)	(0.0001)	(0.0002)	(0.0003)
Coverage of Abandoned Areas Squared	0.0022**	0.0011	0.0017	0.0015*
	(0.0011)	(0.0018)	(0.0018)	(0.0008)
Age		-0.0513***		-0.0227**
		(0.0027)		(0.0107)
Age Squared		0.0012***		-0.0004***
		(0.0001)		(0.0001)
Is Female		0.0575***		
		(0.0181)		
Is Married		0.1401***		-0.0054
		(0.0289)		(0.0698)
Is Divorced		-0.1502***		-0.0976
		(0.0400)		(0.1021)
Is Widowed		0.0382		-0.2143*
		(0.0513)		(0.1165)
Has Very Good Health		0.9872***		0.3702***
		(0.0301)		(0.0323)
Has Very Bad Health		-2.2231***		-1.2300***
		(0.0478)		(0.0497)
Is Disabled		-0.3117***		-0.1517***
		(0.0275)		(0.0462)
Has Migration Background		-0.0427**		
		(0.0226)		
Has Tertiary Degree		0.0324		-0.1104
		(0.0235)		(0.0788)
Has Lower Than Secondary Degree		-0.1411***		-0.0176
		(0.0256)		(0.1043)
Is in Education		-0.0652		0.1201
		(0.0787)		(0.0875)
Is Full-Time Employed		-0.1901***		0.0413
		(0.0378)		(0.0419)
Is Part-Time Employed		-0.0754**		-0.0318
		(0.0367)		(0.0485)
Is on Parental Leave		0.3712***		0.2771***
		(0.0587)		(0.0622)
Is Unemployed		-0.9321***		-0.5178***
		(0.0401)		(0.0432)

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	Satisfaction With Life			
Regressors	OLS	OLS	FE	FE
Log Net Individual Income[a]		0.1157***		0.0445***
		(0.0252)		(0.0153)
Has Child in Household		-0.0031		0.0202
		(0.0268)		(0.0378)
Rental Price[a]		-0.0001***		-0.0001
		(0.0000)		(0.0002)
Lives in House[b]		0.0113		0.0128
		(0.0413)		(0.0298)
Lives in Small Apartment Building		0.0367		0.0117
		(0.0489)		(0.0358)
Lives in Large Apartment Building		-0.0501		-0.0165
		(0.0317)		(0.0265)
Lives in High Rise		-0.0698		-0.0182
		(0.0634)		(0.0451)
Number of Rooms per Individual		0.1236***		0.0136
		(0.0112)		(0.0232)
Unemployment Rate in City		-0.0327***		-0.0175***
		(0.0021)		(0.0043)
Average Household Income in City		0.0000		0.0002
		(0.0002)		(0.0003)
Constant	Yes	Yes	Yes	Yes
Number of Observations	37,608	29,729	37,608	29,729
Number of Individuals	7,836	6,194	7,836	6,194
Adjusted R^2	0.0458	0.486	0.0018	0.0565

[a] Monthly in Euro, [b] Detached, Semi-Detached, or Terraced

Robust standard errors in parentheses

**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: The respective coverage is measured as the hectares covered by the land use category of interest in a pre-defined radius of 1,000 metres around households. The impacts are identified by 418 movers in the fixed-effects model. All figures are rounded to four decimal places.

Source: German Socio-Economic Panel 2000-2012, individuals aged 17 or above, own calculations

Table 2.9: Average Distribution of Most Important Other Categories of Urban Land Use in Terms of Size Within 1 Kilometre Radius Around Households

Variables	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Total Area Covered in Hectares					
Agricultural + Semi-natural areas + Wetlands	27.2822	45.1591	0	273.7259	37,608
Continuous Urban Fabric (S.L. > 80%)	49.1374	45.1862	0	223.7759	37,608
Discontinuous Dense Urban Fabric (S.L. : 50% - 80%)	67.3958	39.2417	0	250.4687	37,608
Discontinuous Medium Density Urban Fabric (S.L. : 30% - 50%)	13.2002	22.5722	0	185.2490	37,608
Industrial, commercial, public, military and private units	52.2394	30.8274	0.5731	229.1328	37,608
Other roads and associated land	26.9876	8.8824	1.4003	60.4525	37,608
Railways and associated land	5.9737	9.9744	0	107.4742	37,608
Sports and leisure facilities	18.3938	16.9474	0	136.2992	37,608
Percentage of Landscape					
Agricultural + Semi-natural areas + Wetlands	0.0884	0.1464	0	0.8858	37,608
Continuous Urban Fabric (S.L. > 80%)	0.1592	0.1462	0	0.7242	37,608
Discontinuous Dense Urban Fabric (S.L. : 50% - 80%)	0.2188	0.1276	0	0.8105	37,608
Discontinuous Medium Density Urban Fabric (S.L. : 30% - 50%)	0.0428	0.0731	0	0.5995	37,608
Industrial, commercial, public, military and private units	0.1694	0.0997	0.0019	0.7415	37,608
Other roads and associated land	0.0875	0.0287	0.0076	0.1956	37,608
Railways and associated land	0.0194	0.0323	0	0.3478	37,608
Sports and leisure facilities	0.0596	0.0549	0	0.4411	37,608

Note: The most important categories of urban land use are those that are greater than one hectare in a one kilometre radius around households. All figures are rounded to four decimal places.

Source: German Socio-Economic Panel 2000–2012, individuals aged 17 or above, own tabulations

Table 2.10: Systematic Time Differences or Time Changes in Life Satisfaction, Distances

	Satisfaction With Life			
Regressors	(i)	(ii)	(iii)	(b)
Distance to Greens	-0.0422***	-0.0410***	-0.0410***	-0.0391***
	(0.0134)	(0.0134)	(0.0134)	(0.0129)
Distance to Forests	-0.0019	-0.0020	-0.0019	-0.0031
	(0.0050)	(0.0050)	(0.0050)	(0.0042)
Distance to Waters	0.0053	0.0049	0.0048	0.0023
	(0.0067)	(0.0067)	(0.0067)	(0.0078)
Distance to Abandoned Areas	0.0255***	0.0259***	0.0258***	0.0289***
	(0.0099)	(0.0099)	(0.0099)	(0.0103)
Distance to Greens Squared	0.0013**	0.0012*	0.0012*	0.0018*
	(0.0006)	(0.0006)	(0.0006)	(0.0009)
Distance to Forests Squared	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0000)
Distance to Waters Squared	-0.0001	-0.0001	-0.0001	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0004)
Distance to Abandoned Areas Squared	-0.0009**	-0.0009**	-0.0009**	-0.0011**
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Individual Fixed Effects	Yes	Yes	Yes	Yes
City of Residence Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes
Number of Observations	29,729	29,729	29,729	29,729
Number of Individuals	6,194	6,194	6,194	6,194
Adjusted R^2	0.1848	0.1900	0.1899	0.0563

(i) Includes Year Fixed Effects, (ii) Includes Linear Time Trend (Years), (iii) Includes Linear Time Trend (Months), (b) Baseline Model

Robust standard errors in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city. All figures are rounded to four decimal places.

Source: SOEP 2000-2012, individuals aged 17 or above, own calculations

Table 2.11: Robustness Check: Estimating both Distances and Coverages in one Model

	Satisfaction With Life		
Regressors	(i)	(b1)	(b2)
Distance to Greens	-0.0367**	-0.0391***	
	(0.0145)	(0.0129)	
Distance to Forests	-0.0047	-0.0031	
	(0.0050)	(0.0042)	
Distance to Waters	0.0039	0.0023	
	(0.0082)	(0.0078)	
Distance to Abandoned Areas	0.0139	0.0289***	
	(0.0112)	(0.0103)	
Distance to Greens Squared	0.0010**	0.0018*	
	(0.0005)	(0.0009)	
Distance to Forests Squared	0.0000	-0.0000	
	(0.0001)	(0.0000)	
Distance to Waters Squared	-0.0001	-0.0002	
	(0.0002)	(0.0004)	
Distance to Abandoned Areas Squared	-0.0006	-0.0011**	
	(0.0004)	(0.0005)	
Coverage of Greens	0.0043		0.0059***
	(0.0027)		(0.0021)
Coverage of Forests	-0.0031		-0.0029
	(0.0023)		(0.0021)
Coverage of Waters	-0.0036		-0.0043
	(0.0038)		(0.0039)
Coverage of Abandoned Areas	-0.0368**		-0.0401***
	(0.0173)		(0.0132)
Coverage of Greens Squared	-0.0001		-0.0002***
	(0.0001)		(0.0000)
Coverage of Forests Squared	0.0000		0.0000
	(0.0000)		(0.0001)
Coverage of Waters Squared	0.0001		0.0001
	(0.0000)		(0.0003)
Coverage of Abandoned Areas Squared	0.0013**		0.0015*
	(0.0006)		(0.0008)
Individual Fixed Effects	Yes	Yes	Yes
City of Residence Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Constant	Yes	Yes	Yes
Number of Observations	29,729	29,729	29,729
Number of Individuals	6,194	6,194	6,194
Adjusted R^2	0.0526	0.0553	0.0565

(i) Includes Both Distances and Coverages, (b1) Baseline Model (Distances), (b2) Baseline Model (Coverages)

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The respective coverage is measured as the hectares covered by the land use category of interest in a pre-defined radius of 1,000 metres around households. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city. All figures are rounded to four decimal places.

Source: SOEP 2000-2012, individuals aged 17 or above, own calculations

Table 2.12: Robustness Check: Exclusion of City of Residence Fixed Effects, Distances

	Satisfaction With Life	
Regressors	(i)	(b)
Distance to Greens	-0.0414***	-0.0391***
	(0.0133)	(0.0129)
Distance to Forests	-0.0031	-0.0031
	(0.0047)	(0.0042)
Distance to Waters	0.0063	0.0023
	(0.0065)	(0.0078)
Distance to Abandoned Areas	0.0229**	0.0289***
	(0.0097)	(0.0103)
Distance to Greens Squared	0.0013**	0.0018*
	(0.0006)	(0.0009)
Distance to Forests Squared	-0.0000	-0.0000
	(0.0001)	(0.0000)
Distance to Waters Squared	-0.0001	-0.0002
	(0.0002)	(0.0004)
Distance to Abandoned Areas Squared	0.0000**	-0.0011**
	(0.0000)	(0.0005)
Individual Fixed Effects	Yes	Yes
City of Residence Fixed Effects	No	Yes
Controls	Yes	Yes
Constant	Yes	Yes
Number of Observations	29,729	29,729
Number of Individuals	6,194	6,194
Adjusted R^2	0.0512	0.0563

(i) Excludes City of Residence Fixed Effects, (b) Baseline Model

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city. All figures are rounded to four decimal places.

Source: SOEP 2000-2012, individuals aged 17 or above, own calculations

Table 2.13: Robustness Check: Effect of Different Types of Urban Land Use on Moving Behaviour, Distances

Regressors	Has Moved
Distance to Greens	-0.0049
	(0.0051)
Distance to Forests	-0.0042*
	(0.0021)
Distance to Waters	0.0024
	(0.0021)
Distance to Abandoned Areas	0.0047
	(0.0030)
Distance to Greens Squared	0.0003
	(0.0002)
Distance to Forests Squared	0.0000
	(0.0000)
Distance to Waters Squared	-0.0001
	(0.0001)
Distance to Abandoned Areas Squared	-0.0001
	(0.0001)
Individual Fixed Effects	Yes
City of Residence Fixed Effects	Yes
Controls	Yes
Constant	Yes
Number of Observations	29,729
Number of Individuals	6,194
Adjusted R^2	0.0323

Robust standard errors in parentheses

**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: The respective distance is measured as the Euclidean distance in 100 metres between households and the border of the nearest land use category of interest. The impacts are identified by 418 movers. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, log net individual income, having a child in the household, rental price, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the city, and the average household income in the city. All figures are rounded to four decimal places.

Source: SOEP 2000-2012, individuals aged 17 or above, own calculations

Chapter 3

Wind Turbines

Abstract

Throughout the world, governments foster the deployment of wind power to mitigate negative externalities of conventional electricity generation, notably CO_2 emissions. Wind turbines, however, are not free of externalities themselves, particularly interference with landscape aesthetics. We quantify these negative externalities using the life satisfaction approach. To this end, we combine household data from the German Socio-Economic Panel Study (SOEP) with a novel panel dataset on over 20,000 installations. Based on geographical coordinates and construction dates, we establish causality in a difference-in-differences design. Matching techniques drawing on exogenous weather data and geographical locations of residence ensure common trend behaviour. We show that the construction of wind turbines close to households exerts significant negative external effects on residential well-being, although they seem both temporally and spatially limited. Robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm our results.*

*. This chapter is also available as the following journal article: Krekel, C., and A. Zerrahn, "Does the Presence of Wind Turbines Have Negative Externalities for People in Their Surroundings? Evidence from Well-Being Data," *Journal of Environmental Economics and Management*, 82, 221–238, 2017.

3.1 Introduction

Since the 1990s, there has been a world-wide trend towards renewable resources for electricity generation. In OECD countries, the share of renewables, excluding hydro power, quadrupled from 1.8% to 7.2% between 1990 and 2012 (International Energy Agency 2013). Wind power has been a major driver of this development: in the same time period, capacity and production grew by more than 20% annually (International Energy Agency 2013, ibid.). In Germany, for example, more than 20,000 wind turbines contributed 9% to total electricity consumption in 2014 (Federal Ministry for Economic Affairs and Energy 2015). Also in non-OECD countries, wind power plays an ever increasing role, for example, in China, being the world's biggest market by 2012 (World Wind Energy Association 2013). The economic rationale behind this trend is to avoid negative environmental externalities common to conventional electricity generation technologies. Beyond noxious local emissions from burning fossil fuels, carbon dioxide emissions are responsible for global climate change. Nuclear power is subject to unclear long-term storage of waste and low-probability but high-impact accidents.

While wind power is largely free of emissions, waste, and risks, it is not free of externalities itself. Thereby, it is important to distinguish between *wind power* and *wind turbines*. Wind power, that is, electricity generated by wind turbines, might require costly changes within the electricity system, including the need to build more flexible backup capacities or expand the transmission grid. Wind turbines, in contrast to large centralised conventional power plants, which foster out-of-sight-out-of-mind attitudes, are more spatially dispersed and in greater proximity to consumers, increasing the salience of energy supply (Pasqualetti 2000; Wüstenhagen et al. 2007). In fact, beyond unpleasant noise emissions (Bakker et al. 2012; McCunney et al. 2014) and impacts on wildlife (Pearce-Higgins et al. 2012; Schuster et al. 2015), most importantly, wind turbines have been found to have negative impacts on landscape aesthetics (Devine-Wright 2005; Jobert et al. 2007; Wolsink 2007). In general, no market prices exist for these negative externalities, so that they must be valued using alternative methods such as stated (Groothuis et al. 2008; Jones and Eiser 2010; Meyerhoff et al. 2010) or revealed preference approaches (Gibbons 2015; Heintzelman and Tuttle 2012).

We investigate the effect of the presence of wind turbines on residential well-being and quantify their negative externalities using the so-called *life satisfaction approach*. To this end, we combine household data from the German Socio-Economic Panel Study (SOEP) with a novel panel dataset on more than 20,000 installations for the time period between 2000 and 2012. Trading off the decrease in life satisfaction caused by the presence of wind turbines against the increase caused by income, we value the negative externalities monetarily. As this approach has already been applied to various other environmental externalities, including air pollution (Ambrey et al. 2014; Ferreira et al. 2013; Levinson 2012), landscape amenities (Kopmann and Rehdanz 2013), noise pollution (Rehdanz and Maddison 2008; Praag and Baarsma 2005), or flood disasters (Luechinger and Raschky 2009), we contribute to a steadily growing stream of literature.

To estimate the causal effect of the presence of wind turbines on residential well-being, we employ a difference-in-differences design that exploits variation in wind turbine construction across space and over time: residents are allocated to the treatment group if a wind turbine is constructed within a pre-defined radius around their households, and to the control group otherwise. To ensure comparability of the treatment and control group, we apply, first, propensity-score matching based on socio-demographic characteristics, macroeconomic conditions, and exogenous weather data; and second, spatial matching techniques based on geographical locations of residence.

We show that the construction of a wind turbine within a radius of 4,000 metres has a significant and sizeable negative effect on life satisfaction. For larger radii, no negative externalities can be detected. Importantly, the effect seems to be non-persistent, vanishing after five years at the latest, and does not intensify with proximity or cumulation of installations. Robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm these results. We arrive at a monetary valuation of these negative externalities for the current resident population between 564 Euros per affected household in total and 258 Euros per affected household and year, depending on the specification. Complementary hedonic regressions indicate a willingness-to-pay to avoid wind turbine construction in surroundings, which is capitalised in rental prices, of up to 200 Euros.

To our knowledge, there exists only one working paper that investigates the effect of the presence of wind turbines on residential well-being, Möllendorff and Welsch (2015), showing that they have a temporary negative impact. However, it differs from our paper in at least two important aspects: the authors do not account for self-selection of residents, and the data are only analysed at the post code level, i.e. life satisfaction is regressed on the number of wind turbines in a given post code area.

The rest of this paper is organised as follows: Section 3.2 reviews the literature on negative externalities of wind turbines and different valuation approaches. Section 3.3 describes the data, and Section 3.4 the empirical model. Results are presented in Section 3.5, and discussed in Section 3.6. Finally, Section 3.7 concludes and outlines avenues for future research.

3.2 Literature Review

3.2.1 Stated and Revealed Preference Approaches

Throughout contingent valuation studies, landscape externalities in form of visual disamenities are found to be a crucial trigger of opposition to wind turbine projects (Groothuis et al. 2008; Jones and Eiser 2010; Meyerhoff et al. 2010). Opposition is found to be shaped by two potentially opposing forces: proximity and habituation. Concerning proximity, most studies find a significant willingness-to-pay to locate planned installations further away from places of residence (Drechsler et al. 2011; Jones and Eiser 2010; Meyerhoff et al. 2010; Molnarova et al. 2012). Concerning habituation, evidence is more mixed: while some papers detect decreasing acceptance (Ladenburg 2010; Ladenburg et al. 2013), others find unchanged attitudes (Eltham et al. 2008) or adaptation (Warren et al. 2005; Wolsink 2007) over time.

Likewise, hedonic studies, drawing on variations in real estate prices, find evidence for negative externalities caused by the construction of wind turbines, for example, in the United States (Heintzelman and Tuttle 2012), Denmark (Jensen et al. 2014), the Netherlands (Dröes and Koster 2014), Germany (Sunak and Madlener 2014), and England and Wales (Gibbons 2015). The decrease in real estate prices is found to range between 2% and 16%.

3.2.2 Life Satisfaction Approach

The life satisfaction approach (LSA) is an alternative to stated and revealed preference approaches. It specifies a microeconometric function relating self-reported life satisfaction to the environmental disamenity to be valued, along with income and other variables. Parameter estimates are then used to calculate the implicit marginal rate of substitution, that is, the amount of income a resident is willing to pay in order to avoid the environmental disamenity (Frey et al. 2004). Conceptually, life satisfaction, which is also referred to as subjective well-being (Welsch and Kühling 2009) or experienced utility (Kahneman et al. 1997), can be defined as cognitive evaluation of the circumstances of life (Diener et al. 1999).

Compared to contingent valuation studies, the LSA avoids bias resulting from the expression of attitudes or the complexity of valuation. Stated preference approaches, in particular, are subject to symbolic valuation: what is measured may be intrinsic attitudes rather than extrinsic preferences. At the same time, they are prone to framing and anchoring effects (Kahneman and Sugden 2005). The LSA, in contrast, does not ask residents to monetarily value a complex environmental disamenity in a hypothetical situation, which reduces cognitive burden. Likewise, it does not reveal the relationship of interest, mitigating the incentive to answer in a strategic or socially desirable way (Kahneman and Sugden 2005; Horst 2007).

Compared to hedonic studies, the LSA avoids bias resulting from the misconception that the real estate market is in, or close to, equilibrium. Typically, this occurs in case of slow adjustment of prices, incomplete information, and transaction costs (especially direct and indirect migration costs). It also avoids potentially distorted future risk expectations common to market transactions, as well as bias resulting from the misprediction of utility (Frey et al. 2004; Frey and Stutzer 2014).

Intuitively, the LSA is not entirely free of methodological issues itself. For subjective well-being data to constitute a valid approximation of welfare, they have to be at least ordinal. Moreover, the microeconometric function relating self-reported life satisfaction to the environmental disamenity has to be specified correctly. These requirements are typically met in practice (Welsch and Kühling 2009).

There is more debate about whether self-reported life satisfaction is an approximation of welfare in the first place. Recent research shows that people do not necessarily make choices that maximise their life satisfaction, for example, when making moving decisions (Glaeser et al. 2016). This seems to suggest that life satisfaction is not equal to utility, but rather one component in an individual's utility function, besides others such as income (Becker et al. 2008; Benjamin et al. 2012). An emerging stream of literature argues that one other such component could be sense of meaning or purpose in life (White and Dolan 2009). On the other hand, individuals might make prediction errors when trying to maximise their life satisfaction, be it white noise or systematic. This might be even more so the case when trading off losses in well-being today for gains in the future (Odermatt and Stutzer 2015).

An extensive treatment of the validity of subjective well-being measures is beyond the scope of this paper. To be clear, we do not advocate to use life satisfaction as an exclusive criterion for environmental policy evaluation, but only use it as a vehicle to measure a negative externality. The life satisfaction approach itself does not hinge on the assumption that life satisfaction is equal to utility: rather, it assumes that it is a valid approximation. Adler et al. (2015), using a large population survey, show that people by and large tend to make life choices that score high on life satisfaction.

3.3 Data

3.3.1 Data on Residential Well-Being

We use panel data from the German Socio-Economic Panel Study (SOEP) for the time period between 2000 and 2012. The SOEP is a representative panel of private households in Germany, covering about 20,000 individuals in more than 11,000 households in its current wave (Wagner et al. 2007; Wagner et al. 2008). Importantly, it provides information on the geographical locations of the places of residence, allowing to merge data on residential well-being with data on wind turbines.[72] Our dependent variable is *satisfaction with life*, which is obtained from an eleven-point single-item Likert scale that asks "How satisfied are you with your life, all things considered?".[73]

3.3.2 Data on Wind Turbines

At the heart of our analysis lies a novel panel dataset on onshore wind turbines in Germany. For its creation, we drew on a variety of dispersed sources, mostly the environmental authorities in the sixteen federal states. If data were not freely accessible, we contacted the body in charge for granting access and filed a request for disclosure.[74] We brought together data on more than 20,000 wind turbines with construction dates ranging between 2000 and 2012. The core attributes rendering an observation suitable for our empirical analysis are *(i)* the exact geographical coordinates, *(ii)* the exact construction dates, and *(iii)* information on the size of the installation.

The exact geographical coordinates constitute the distinctly novel feature of our dataset: postal codes or addresses, as provided by the public transparency platform on renewable energy installations in Germany, would render an exact matching between individuals and installations impossible.[75] Moreover, the exact construction dates of installations are required in order to contrast them with the interview dates of individuals. Finally, we focus only on installations that exceed a certain size threshold: small installations are less likely to interfere with landscape aesthetics. It is also more likely that they are owned by private persons, and we might therefore measure effects other than negative externalities. Naturally, there is some degree of arbitrariness

72. The SOEP is subject to rigorous data protection legislation. It is never possible to derive the household data from coordinates since they are never visible to the researcher at the same time. See Göbel and Pauer (2014) for more information.

73. We also examined whether wind turbine construction has an effect on health, using self-assessed health, as well as the mental and physical health items from the Short-Form (SF12v2) Health Survey, which has been incorporated into the SOEP. Overall, we found little evidence that these outcomes are affected.

74. See 3.8.3.2 for a detailed account and information on data protection.

75. The public transparency platform on renewable energy installations can be found at `www.netztransparenz.de/de/Anlagenstammdaten.htm` (in German), accessed June 1, 2015.

in determining a size threshold: beyond those without any information on size at all, we exclude all installations with a hub height of less than 50 metres or a capacity of less than 0.5 megawatts. In doing so, we focus only on large installations that are typically constructed by utilities.[76]

Out of more than 20,000, we are left with a set of 10,083 wind turbines relevant for our analysis. These constitute the *included group*.[77] The other 10,554 constitute the *excluded group*.

3.3.3 Merge

We merge the data on residential well-being with the data on wind turbines by calculating the distances between households and the nearest installation. Specifically, a treatment radius around each household is specified within which wind turbines of the *included group* trigger the household members to be allocated to the treatment group. If no such wind turbine is located within the treatment radius, the household members are allocated to the control group.

We subsequently check for each individual and year whether a wind turbine from the *excluded group* is located within the treatment radius at the interview date. Turbines from the *excluded group* receive special attention as households in their proximity should be discarded: they do not belong to either the treatment or control group. If both a turbine from the *included* and *excluded group* are present, however, then the individual is allocated to the treatment group if the first turbine built is from the *included group*, and discarded otherwise. See Figure 3.3 in Section 3.8 for a graphical illustration.

Some further data adjustments are made. Due to the unavailability of up-to-date data, only years up to 2010 are included for the state of *Mecklenburg-Vorpommern*, up to 2011 for *Saxony*, and up to 2012 for all other states. Moreover, we discard individuals for which the interview date is given with insufficient accuracy in the year in which the first wind turbine is constructed in their surroundings: for those individuals, we cannot be sure whether they should be allocated to the treatment or control group. Finally, we discard individuals who "start" in the treatment group, for example, if they enter the panel while a wind turbine is already present in their surroundings: for them, no pre-treatment information to base inference on is given. Note that the size of the treatment and control group depends on the treatment radius chosen.

76. We also focus only on installations that are built past 2000: before that, the SOEP does not provide information on the geographical locations of places of residence.

77. See Table 3.10 in Section 3.8 for descriptive statistics.

3.4 Empirical Model

3.4.1 Treatment Radius

As default treatment radius, we choose 4,000 metres, motivated by three considerations. First, we consider this radius close enough for wind turbines to unfold negative impacts. Second, it allows for a sufficient sample size, especially when stratifying the final sample to study different sub-groups. Finally, there is no uniform legislation in Germany that could serve as reference. Across time and states, the so-called *impact radius*, based on which intrusions into the environment are evaluated, varies between 1,500 and 6,000 metres for a wind turbine with a hub height of 100 metres. Beyond the 4,000 metres default treatment radius, we carry out various sensitivity analyses with other radii.

In addition, to achieve a clear-cut distinction between treatment and control group at the margin, we introduce a ban radius of 8,000 metres, twice the length of the treatment radius: residents who experience the construction of a turbine within the ban radius, but outside the treatment radius, are discarded.

3.4.2 Identification Strategy

To establish causality, we have to make three identifying assumptions. First, the interview date is random and unrelated to the construction date. In other words, residents should not strategically postpone interviews due to construction. We checked the distribution of interviews, and it seems that this is not the case. Second, in the absence of treatment, treatment and control group would have followed a common trend in the outcome over time. While this *common trend assumption* is not formally testable, as the counterfactual is not observable, we apply propensity-score and spatial matching techniques, as described in Sub-Section 3.4.3, to ensure comparability between treatment and control group. In addition, we control for confounders that could cause remaining differences in time trends.[78] Finally, conditional ignorability implies that, conditional on covariates, construction is independent of the outcome, and therefore exogenous. In our setting, endogeneity may arise through two channels: endogenous construction or endogenous residential sorting. In other words, for certain residents it could be systematically more likely that either new wind turbines are constructed in their surroundings, or that they move away from or towards existing installations. In both cases, estimates would be biased if such endogenous assignment to treatment or control group is correlated with the outcome.

78. Implicitly, we also require the *stable unit treatment value assumption* to hold: whether a wind turbine is constructed in the surroundings of one household should not depend on the outcome of another household. There is no a priori reason to believe that this is the case.

We argue that both channels are mitigated.

Concerning endogenous construction, the siting process in Germany is driven by business interests of project developers, which must adhere to governmental zoning law and the regulations on ecological impacts. Negotiation with affected residents or the legal right to appeal is, in general, not provided for. As such, we omit residents who live near small wind turbines; those installations are more likely to be built and run by private persons. Instead, we focus only on large installations that are typically constructed by utilities. Moreover, we omit residents who are farmers: these are more likely to let land to commercial operators.[79] Finally, we control for individual fixed effects and a rich set of time-varying observables at the micro level, originating from the SOEP, and at the macro level, originating from the Federal Statistical Office. The micro controls include demographic characteristics, human capital characteristics, and economic conditions at the individual level, as well as household characteristics and housing conditions at the household level; the macro controls include macroeconomic conditions and neighbourhood characteristics at the county level.[80] In doing so, we net out systematic differences between treatment and control group over time and at any point in time, ensuring common trend behaviour.[81]

In case of endogenous residential sorting, residents with lower (higher) preferences for wind turbines self-select into areas with greater (smaller) distances to them, whereby the preferences are correlated with the outcome. This can happen either prior to the observation period, so that we have an issue of *preference heterogeneity*, which we already account for by including individual fixed effects, or during the observation period, so that we have an issue of *simultaneity*.

In our baseline specification, we work around simultaneity by excluding residents who move, motivated by two reasons. First, if residential sorting is endogenous to wind turbine construction, the direction of bias resulting from the inclusion of movers is unclear. Depending on the type of move, theory predicts an attenuation or augmentation of estimates. For instance, hypothesising that wind turbines exert a negative effect on residential well-being, the most adversely affected individuals are most likely to move away from installations, leading to a downward bias. On the other hand, individuals who move from the control to the treatment group may exhibit a lower aversion against wind turbines, leading to an upward bias. To this

79. We do not find that wind turbine construction increases income from renting out or leasing of nearby residents. The results are robust to the inclusion of farmers.

80. The results are robust to replacing the macro controls with state-year fixed effects. Moreover, they are robust to including linear and quadratic time trends, both individually and jointly, and to including month and quarter-of-year fixed effects.

81. The results are robust to omitting all of these controls, which reinforces the notion of ignorability, that is, wind turbine construction as an exogenous event.

end, estimating for stayers provides clearer and undistorted evidence. Sub-Section 3.5.7 provides a robustness check including movers. Besides that, endogenous residential sorting seems to be a quantitatively minor issue: geographical mobility in Germany is traditionally low. As a matter of fact, in our final sample, between 4 and 7% of all individuals move per year. Therefore, we expect bias resulting from the exclusion of movers not to overly blur results.

In general, our empirical strategy can be characterised as intention-to-treat approach: the definition of our treatment variable proxies the effect of the presence of wind turbines on residential well-being by a treatment radius. It implicitly assumes that every wind turbine is visible to every resident at any time, which is unlikely to be the case. For example, local topography and land cover might block the view from a household to a wind turbine.[82] On the other hand, households might adopt mitigating behaviour to block the view themselves, for example, by planting a tree or building a fence. Finally, we only have information on private households: some individuals, however, might spend considerable amounts of time outside their homes, for example, at work. They might thus be less permanently affected. Moreover, wind turbines can also unfold negative externalities on actual and potential temporary visitors like tourists, or non-use values, which cannot readily be captured by our approach. Therefore, our estimates can be interpreted as a lower bound, specifically for individuals who do not move away.

3.4.3 Matching Treatment and Control Group

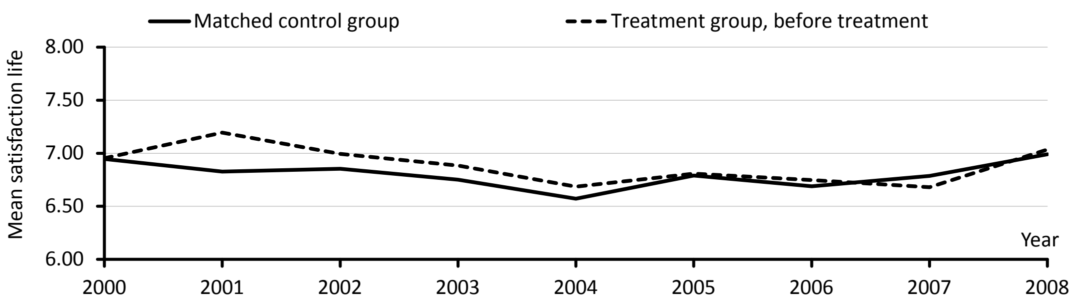
Under the basic definition, the treatment group is relatively small, and concentrated in remote and rural areas, whereas the much larger control group is dispersed over the whole country. Individuals may thus not be comparable to each other, questioning the assumption of a common time trend between treatment and control group. We therefore restrict both treatment and control group to individuals living in rural areas, excluding individuals living in city counties (*kreisfreie Städte*) and counties ranked in the top two deciles according to population density.[83] Moreover, we use two types of matching, prior to running our difference-in-differences regressions. See Figure 3.4 in Section 3.8 for a graphical illustration of both types of matching.

The first type of matching is *propensity-score matching.* Specifically, we use one-to-one nearest-neighbour matching on macro controls, including the unemployment rate, average monthly net household income, and population density at the county level, as well as state dummies. We match residents on the mean values of these variables, taken over the entire observation period. Alternatively, one could match individuals on their values in either the first

82. We investigate this issue further in Sub-Section 3.5.6 by performing a view shed analysis.
83. The results are robust to the inclusion of individuals living in urban areas.

year of the observation period or, in case that individuals enter the panel at a later point, in the first year in which they enter the panel. The resulting point estimates are similar in terms of significance, and slightly smaller in size.[84] We also match on a variable that captures local wind power adequacy, defined as the average annual energy yield of a wind turbine in kilowatt hours per square metre of rotor area, based on weather data from 1981 to 2000 (German Meteorological Service 2014). It encompasses a multitude of exogenous climatic and geographical factors. Specifically, it is based on wind velocity and aptitude, taking into account between-regional factors, such as coasts, and within-regional factors, such as cities, forests, and local topographies. Wind power adequacy is recorded on the basis of 1 kilometre × 1 kilometre tiles, distributed over the entire country. We match households with the nearest tile, and calculate the mean expected annual energy yield of a wind turbine from the 25 tiles surrounding it. See Figure 3.5 in Section 3.8 for a graphical illustration of this calculation. As variables used in the propensity-score matching take on mean or initial values, and as they may change over time, we routinely control for them in addition in all regressions.

Figure 3.1 visualises how the dependent variable, *satisfaction with life*, evolves over time. The annual mean life satisfaction is shown for the matched control group (solid line) and the treatment group prior to treatment (dashed line).[85] All graphs control for confounders. As can be seen, the matched control and pre-treatment group co-move in a similar pattern over time; there is no evidence for a diverging time trend.



Source: SOEP v29, 2000–2012, individuals aged 17 or above, own calculations

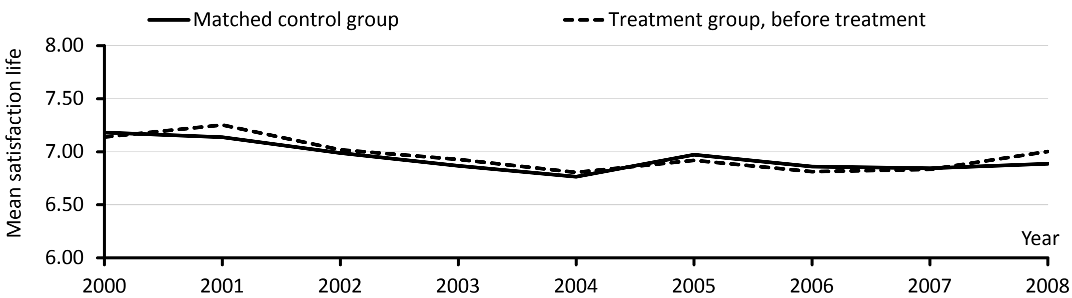
Figure 3.1: Common Time Trend (Propensity-Score Matching)

The second type of matching is *spatial matching*. It is based on the first law of geography, which states that closer things are more similar to each other. In this vein, it follows the idea that residents in close proximity to wind turbines are sufficiently similar to those living close but slightly farther away. We define a matching radius around each place of residence: individuals

84. Table 3.22 in Section 3.8 presents the results of this alternative matching procedure.
85. The horizontal axis is restricted to the time period between 2000 and 2008. Thereafter, the pre-treatment group mean is based only on very few observations, and hardly delivers insightful information.

who are neither treated nor discarded, but experience the construction of a wind turbine within the matching radius, constitute the control group. In other words, we match residents who live close to an installation and close enough to be treated with those who live close but not close enough to be treated. We choose 10,000 and 15,000 metres as matching radii, whereby the latter serves as default. Through spatial matching, the scope of the analysis is narrowed down to residents who are comparable in terms of local living conditions. Likewise, potential positive effects of wind turbines, in particular local economic benefits, can be mitigated: while both treatment and control group could profit to a certain extent from a wind turbine, only the treatment group within 4,000 metres distance is likely to be negatively affected by its presence.

Figure 3.2 is constructed analogously to Figure 3.1, using the default matching radius of 15,000 metres. Again, there is no evidence for a diverging time trend between matched control and pre-treatment group. A similar picture arises for the matching radius of 10,000 metres.



Source: SOEP v29, 2000–2012, individuals aged 17 or above, own calculations

Figure 3.2: Common Time Trend (Spatial Matching, 15,000 metres)

The descriptive statistics for the propensity-score matching specification are given in Table 3.1:[86] it shows the means of all covariates, overall and separately for treatment and control group, along with their scale-free normalised differences. Imbens and Wooldridge (2009) suggest that a normalised difference above 0.25 indicates covariate imbalance. Clearly, this is not the case for any of our covariates. Thus, we conclude that the final sample is well-balanced on observables.[87]

86. See Table 3.11 in Section 3.8 for the spatial matching specifications.

87. Note that covariance imbalance between treatment and control group would not necessarily be a threat to our identification strategy: we control for a rich set of time-varying observables. Moreover, including individual and year fixed effects net out systematic differences in both time-invariant observables and unobservables between individuals and years, respectively.

Table 3.1: Descriptive Statistics for Propensity-Score Matching (PSM)

	Mean		
	Treatment Group	Control Group, PSM	Normalised Difference
Variables	(T)	(C)	(T)-(C)
Micro Controls			
Age	54.2053	52.3441	0.0875
Is Female	0.4991	0.5026	0.0050
Is Married	0.7829	0.7216	0.1006
Is Divorced	0.0481	0.0654	0.0530
Is Widowed	0.0735	0.0733	0.0006
Has Very Good Health	0.0566	0.0631	0.0194
Has Very Bad Health	0.0433	0.0481	0.0163
Is Disabled	0.1447	0.1243	0.0421
Has Migration Background	0.0881	0.0845	0.0089
Has Tertiary Degree	0.2828	0.3065	0.0369
Has Lower Than Secondary Degree	0.1844	0.1773	0.0130
Is in Education	0.0101	0.0184	0.0498
Is Full-Time Employed	0.3758	0.3779	0.0030
Is Part-Time Employed	0.1112	0.0770	0.0829
Is on Parental Leave	0.0068	0.0060	0.0069
Is Unemployed	0.0732	0.0954	0.0566
Log Monthly Net Individual Income[a]	6.4513	6.3143	0.1009
Has Child in Household	0.2277	0.2652	0.0616
Log Annual Net Household Income[a]	10.3718	10.2929	0.0984
Lives in House[b]	0.5538	0.5283	0.0376
Lives in Small Apartment Building	0.0896	0.0866	0.0067
Lives in Large Apartment Building	0.1589	0.1745	0.0312
Lives in High Rise	0.0113	0.0145	0.0211
Number of Rooms per Individual	1.7996	1.7686	0.0245
Macro Controls			
Unemployment Rate	12.0116	13.7700	0.2139
Average Monthly Net Household Income[a]	1,364.0120	1,311.0680	0.1959
Number of Observations	3,975	2,662	-
Number of Individuals	498	488	-

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Note: The third column shows the normalised difference, which is calculated as $\triangle x = (\bar{x}_t - \bar{x}_c) \div \sqrt{\sigma_t^2 + \sigma_c^2}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate for the treatment and control group, respectively. σ^2 denotes the variance. As a rule of thumb, a normalised difference greater than 0.25 indicates a non-balanced covariate, which might lead to sensitive results (Imbens and Wooldridge 2009). All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, own tabulations.

3.4.4 Regression Equation

We employ a linear model estimated by the fixed-effects (within) estimator.[88] The specification test by Wu (1973) and Hausman (1978), as well as the robust version by Wooldridge (2002) indicate that a fixed-effects specification is strictly preferable over a random-effects one: all tests reject the null of identical coefficients at the 1% significance level.[89] Robust standard errors are clustered at the federal state level.

Regression Equation (3.1) estimates the overall treatment effect, with $Construction_{it,r}$ as the regressor of interest. $Construction_{it,r}$ is a dummy variable that equals one in time period t if a wind turbine is present within treatment radius r around the household of individual i, and zero else. Regression Equation (3.2) estimates the treatment effect intensity, with the interaction $Construction_{it,r} \times Intensity_{it,r}$ as the regressor of interest. $Intensity_{it,r}$ is a place holder for different measures of treatment intensity: $InvDist_{it,r}$ is the inverse of the distance to the nearest installation in kilometres, $RevDist_{it,r}$ is the treatment radius minus the distance to the nearest installation, and $Cumul_{it,r}$ is the number of installations within the treatment radius. As more or more closely located wind turbines can be constructed during the observation period, the intensity can change over time. The two distance measures make different parametric assumptions. Regression Equation (3.3) estimates the treatment effect persistence. The regressor of interest, $Trans_{it-\tau,r}$, is a dummy variable that equals one in time period t, which is τ periods after the construction of the first turbine within the treatment radius, and zero else.

$$\begin{aligned} y_{it} = \beta_0 + \mathbf{MIC'_{it}}\boldsymbol{\beta}_1 + \mathbf{MAC'_{it}}\boldsymbol{\beta}_2 + \delta_1 Construction_{it,r} + \\ + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \end{aligned} \tag{3.1}$$

$$\begin{aligned} y_{it} = \beta_0 + \mathbf{MIC'_{it}}\boldsymbol{\beta}_1 + \mathbf{MAC'_{it}}\boldsymbol{\beta}_2 + \delta_1 Construction_{it,r} \times Intensity_{it,r} + \\ + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \end{aligned} \tag{3.2}$$

$$y_{it} = \beta_0 + \mathbf{MIC'_{it}}\boldsymbol{\beta}_1 + \mathbf{MAC'_{it}}\boldsymbol{\beta}_2 + \sum_{\tau=1}^{9} \delta_\tau Trans_{it-\tau,r} +$$

88. Note that using a linear model introduces measurement error, as *satisfaction with life* is a discrete, ordinal variable. However, this has become common practice, as discrete models for ordinal variables are not easily applicable to this type of estimator, and the bias resulting from this measurement error has been found to be negligible (see, for example, Ferrer-i-Carbonell and Frijters (2004) for panel data, and Brereton et al. (2008) and Ferreira and Moro (2010) for repeated cross-section data).

89. The empirical values of the test-statistic, 204.20 and 220.38 under propensity-score matching and 211.12 and 243.20 under spatial matching, exceed the critical value 56.06 of the χ^2-distribution with 34 degrees of freedom.

$$+\sum_{n=1}^{12}\gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \tag{3.3}$$

where y_{it} is *satisfaction with life* as the regressand; MIC_{it} and MAC_{it} are vectors of controls at the micro and macro level, respectively; and $Year_{2000+n}$ is a full set of yearly dummy variables. μ_i captures time-invariant unobserved heterogeneity at the individual level. ϵ_{it} is the idiosyncratic disturbance. $Construction_{it,r}$, $Construction_{it,r} \times Intensity_{it,r}$, and $Trans_{it-\tau,r}$ are the regressors of interest. The corresponding average treatment effects on the treated are captured by δ_1 and δ_τ.

3.5 Results

3.5.1 Overall Treatment Effect

Table 3.2 reports the results of our difference-in-differences propensity-score and spatial matching specifications using the default treatment radius of 4,000 metres. For convenience, we only show our treatment variable here; detailed tables showing all covariates can be found in Section 3.8.

For both matching specifications, a central result emerges: the presence of a wind turbine within the default treatment radius of 4,000 metres around households has a significant negative effect on life satisfaction at the 1% and 5% level, respectively. The size of this effect is also economically significant: under propensity-score matching, for instance, life satisfaction decreases by 8% of a standard deviation. Combining propensity-score with the spatial matching yields point estimates that are very similar to those of the standalone spatial matching specifications, regardless of matching radius chosen, and significant at the 5% level.[90] The baseline specification thus provides evidence for significant negative local externalities.[91]

What happens if we increase the treatment radius? For 8,000 and 10,000 metres under propensity-score matching, coefficient estimates are negative but considerably smaller in size, $\delta_1 = -0.0348$ and $\delta_1 = -0.0074$, respectively, and insignificant at any conventional level. Likewise, no effect can be detected in case of a 15,000 metres treatment radius.[92] An analogous result emerges for an increased treatment radius of 8,000 metres under spatial matching. This corroborates that we indeed systematically pick up negative local externalities triggered by the

90. See Table 3.19 in Section 3.8 for the combined matching specification.
91. In Figure 3.6 in Section 3.8, we illustrate the identified effect graphically. Here, we carried out a post-estimation analysis in an event study framework: we re-estimated the baseline specifications, normalised the point in time of treatment to $t = 0$, and calculated the mean predicted life satisfaction for periods $t - 5$ to $t + 5$.
92. For larger treatment radii, we apply no ban radius. See Section 3.8 for detailed results.

Table 3.2: Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	PS	S $(10,000m)$	S $(15,000m)$
$\text{Construction}_{\text{it},4000}$	-0.1405***	-0.1088***	-0.1138**
	(0.0399)	(0.0222)	(0.0366)
Micro Controls	yes	yes	yes
Macro Controls	yes	yes	yes
Number of Observations	6,637	8,609	16,378
Number of Individuals	986	1,317	2,586
of which in treatment group	498	506	506
of which in control group	488	811	2,080
Adjusted R^2	0.0657	0.0678	0.0632

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

presence of wind turbines rather than local peculiarities: while closer proximity serves as a proxy for an undesired impact, for larger distances such an effect cannot be detected anymore.

3.5.2 Treatment Effect Intensity

We explore treatment effect intensity next. In Table 3.3, for inverse distance, reverse distance, and cumulation, coefficient estimates have the expected sign, but none is significant for any matching specification.[93] It seems the presence of a wind turbine in a 4km radius is sufficient for negative externalities to arise, and specific intensity measures matter little in addition.

To explore this finding further, we investigate closer treatment radii below 4,000 metres under spatial matching (with propensity-score matching, the control group would have to be determined anew for each treatment radius, rendering comparability difficult). Specifically, we use 2,000, 2,500, and 3,000 metres as treatment radii, and in addition analyse different distance bands around treated individuals. For example, in band $[2,000; 3,000]$, only individuals experiencing wind turbine construction between 2,000 and 3,000 metres around their places of

93. The results for spatial matching with a 10,000 metres matching radius are analoguous. See Section 3.8 for detailed results.

residence are assigned to the treatment group; residents with wind turbines in closer proximity are dropped. Analogously, we specify bands between 2,000 and 4,000 metres, 2,500 and 4,000 metres, and 3,000 and 4,000 metres. Table 3.4 reports the results for both spatial matching radii. For distances below 4,000 metres, no significant effects are detected, and neither is for the $[2,000; 3,000]$ band. For larger bands, however, coefficient estimates are negative, significant at the 1% or 5% level, and large in size.[94]

This finding can have several explanations. First, results can be driven by smaller sample sizes. In the baseline 4,000 metres specification, there are 506 treated individuals, decreasing to only 183 for 2,000 metres. Beyond such a potential statistical artifact, residents in closer proximity may exhibit certain peculiarities: some could effectively profit from installations, for instance, by directed compensation measures. The turbine planning process in Germany prescribes an ecological impact compensation scheme, which could include, for example, a landscape upgrade by planting trees beside a road or the demolition of an abandoned building. As a rule of thumb, compensations should be close to impacts and in the same domain. Alternatively, individuals in particularly close distance could also actively erect wind turbines in their surroundings, and profit monetarily.[95] Although unlikely, we cannot fully exclude this case since we do not have information on the ownership structure of particular installations.

Concerning size and significance of coefficient estimates, this result is in line with the treatment effect for the default 4,000 metres radius: while the effect is much stronger within the $[2,000; 4,000]$ band, it is insignificant for closer distances. Concerning directed compensation measures or active wind turbine erection by residents, results are in line with a lower-bound interpretation: as it cannot be excluded that some individuals in closer distances may profit, estimates are, if anything, attenuated, given that a significant negative overall treatment effect remains a robust finding. As discussed above, this lower-bound interpretation is consistent with the intention-to-treat definition of the treatment variable.[96]

In this respect, insignificant coefficient estimates for the different intensity measures are explained by non-significance of effects for smaller distances: if coefficients are insignificant for individuals living closer to wind turbines, treatment intensity increasing in proximity is obsolete.

94. Alternatively, instead of estimating separate sub-samples, one could interact the main effect with a dummy variable for the respective distance band: the results remain qualitatively the same.

95. We do not find that wind turbine construction decreases electricity costs of nearby residents. Recall that we do not find that it increases income from renting out or leasing either.

96. Impact compensation tends to be greater the closer and the larger the project. In this regard, point estimates for closer distance bands and for cumulation could be downward biased.

Table 3.3: Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching *Construction*$_{it,4000}\times$ *Intensity*

Dependent Variable: Satisfaction With Life

	PS			S $(15,000m)$		
Regressors\Intensity Measure	$\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	$\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$
$\text{Construction}_{\text{it,4000}}$ × Intensity	-0.2090	-0.0128	-0.0178	-0.1862*	-0.0181	-0.0174
	(0.1605)	(0.0550)	(0.1556)	(0.0940)	(0.0338)	(0.0106)
Micro Controls	yes	yes	yes	yes	yes	yes
Macro Controls	yes	yes	yes	yes	yes	yes
Number of Observations	6,637	6,637	6,637	16,378	16,378	16,378
Number of Individuals	986	986	986	2,586	2,586	2,586
of which in treatment group	498	498	498	506	506	506
of which in control group	488	488	488	2,080	2,080	2,080
Adjusted R^2	0.0650	0.0646	0.0659	0.0630	0.0629	0.0630

Robust standard errors clustered at the federal state level in parentheses

**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: $\text{Construction}_{\text{it,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The intensity measures are defined as follows: $\text{InvDist}_{it,4000}$ is the inverse distance, $\text{RevDist}_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, $\text{Cumul}_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4,000 metres, all in interview year t. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Table 3.4: Results – FE Models, Closer Proximity and Distance Bands, Spatial (S) Matching $Construction_{it,r/b}$

Dependent Variable: Satisfaction With Life

Treatment radius r	S $(10,000m)$ Construction$_{it,r}$	S $(15,000m)$ Construction$_{it,r}$	# treated
2,000	-0.0254	0.0232	183
	(0.1278)	(0.1107)	
2,500	-0.0119	-0.0169	274
	(0.0717)	(0.0613)	
3,000	-0.0450	-0.0442	356
	(0.0575)	(0.0589)	
4,000	-0.1088***	-0.1138**	506
	(0.0222)	(0.0366)	
Treatment band b	Construction$_{it,b}$	Construction$_{it,b}$	# treated
$[2,000; 3,000]$	-0.0783	-0.0827	243
	(0.0549)	(0.0614)	
$[2,000; 4,000]$	-0.1711***	-0.1749**	411
	(0.0423)	(0.0551)	
$[2,500; 4,000]$	-0.1860**	-0.1869**	329
	(0.0635)	(0.0754)	
$[3,000; 4,000]$	-0.1735**	-0.1799*	232
	(0.0725)	(0.0842)	

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it,r}}$ ($\text{Construction}_{\text{it,b}}$) is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of r metres (treatment band b in metres) in interview year t, and zero else. The treatment band $[x_1; x_2]$ comprises only those households that are located between x_1 and x_2 metres from the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include micro controls, macro controls, dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

3.5.3 Treatment Effect Persistence

Intuitively, the question arises whether the presence of wind turbines has a persistent effect on residential well-being. Table 3.5 reports results on persistence for all matching specifications, including coefficient estimates for up to nine transition periods after the construction of a wind turbine within the default treatment radius of 4,000 metres. As can be seen, the effect seems to be temporally limited. It is significant at the 1% or 5% level from transition period two, that is, one year after the construction of a wind turbine, to at most transition period five. The size of the effect in each time period is somewhat larger than the size of the combined effect. Presumably, this is because negative externalities take some time to unfold, whereafter adaptation starts to kick in.

Note that a non-significant effect in transition period one is not surprising. While we use the construction date as reported in the data sources, in reality there might be some blur, which is picked up by the first-period coefficient: a wind turbine is usually not erected within a single day, and it is not stated explicitly whether the construction date marks the beginning or the end of the construction process. Additional sensitivity checks including a dummy variable for the time period before the construction of a wind turbine, on the contrary, provide no evidence of anticipation effects.[97]

This finding can have several explanations. First, current residents may adapt to the presence of wind turbines in their surroundings (it is difficult to make any inference on future residents, or temporary visitors, as they do not appear in the data). Alternatively, they may adjust to their presence, for example, by adopting mitigating behaviour such as planting a tree or building a fence. Second, the decay effect may be due to disamenities related to the construction process rather than the presence wind turbines. We believe that this is less likely to be the case, though, as the construction process of wind turbines is rather quick. Moreover, the non-significant effect in transition period one and the prolonged significant effects in transition periods thereafter point against this explanation. Finally, results may be driven by smaller sample sizes, as the treatment group size decreases over time. For a lag of nine years, construction from 2000 to 2003 is possible, whereas for shorter intervals more years are relevant. Note, however, that the point estimates remain reasonably robust as significance decreases. Non-significance may thus arise as a statistical artifact due to loss of power rather than a genuine decay effect.

97. See also Sub-Section 3.5.5 for placebo tests using leads of the treatment variable.

3.5.4 Heterogeneity Analysis

To gain a more diverse picture, we apply our treatment effect analysis to different sub-groups. Table 3.6 reports the results for house owners versus renters, as well as for residents who are very concerned about the environment or climate change, respectively, versus residents who are not. The indicators on environmental and climate change concerns are obtained from single-item three-point Likert scales that ask respondents to rate how concerned they are about “environmental protection” or “climate change”, respectively. We collapse these items into binary indicators that equal one for the highest category of concerns, and zero otherwise. Throughout all models, we use the difference-in-differences spatial matching specification with the default matching radius of 15,000 metres; results are robust to using the matching radius of 10,000 metres.

Stratifying along real estate ownership, the coefficient estimate for house owners shows a significant negative effect (first column), which is not the case for renters (second column).[98] The size of the coefficient estimate is somewhat larger than at the aggregate level. Sensitivity analyses including land price at the county level as an additional control leave results on average and for the different sub-groups unchanged. One explanation for this finding may be that renters are more swiftly compensated through a decrease in rents, as the negative external effect is internalised through the price mechanism in rental markets, whereas for house owners this channel does not operate. In case of full internalisation for renters, we may not be able to detect any residual negative effect of the externality on life satisfaction. We explore this possibility in more detail by performing an additional hedonic analysis in Section 3.6.[99]

Stratifying along environmental concerns, coefficient estimates for non-concerned individuals show significant negative effects (fourth column for environment, sixth for climate change), which is not the case for concerned individuals (third and fifth column, respectively). Again, the size of coefficient estimates is higher than at the aggregate level. In this respect, we interpret environmental concerns as referring to more global rather than local impacts. Generally, wind turbines are regarded as environmentally friendly, and findings for residents who are environmentally aware are in line with that interpretation. Likewise, less environmentally aware individuals may have lower preferences for emission-free electricity production and, thus, be

98. Since stratifying the sample by home ownership greatly reduces the number of observations in the sub-sample for renters, the insignificant impact on renters could be driven by power losses. In fact, the standard error for renters is between two to three times as large as that for house owners. We checked this formally by using an interaction between the main treatment dummy and a dummy for renters in the full sample containing both house owners and renters (of course, we also included the main treatment dummy and the dummy for renters themselves in this regression): the results remain qualitatively the same, suggesting that the insignificant impact on renters is not a statistical artefact due to power losses.

99. In this context, Luechinger (2009) provides a discussion of this complementarity between the life satisfaction approach and the hedonic method in the context of air pollutant emissions from fossil-fuelled power plants.

Table 3.5: Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching $Trans_{it\text{-}\tau,4000}$

Dependent Variable: Satisfaction With Life

Regressors\Transitoriness Measure	PS $\text{Trans}_{it-\tau,4000}$	# treated	S $(10,000m)$ $\text{Trans}_{it-\tau,4000}$	S $(15,000m)$ $\text{Trans}_{it-\tau,4000}$	# treated
$\text{Trans}_{it-1,4000}$	-0.0546	498	-0.0401	-0.0392	506
	(0.0642)		(0.0657)	(0.0642)	
$\text{Trans}_{it-2,4000}$	-0.1616**	444	-0.1212**	-0.1262**	450
	(0.0697)		(0.0482)	(0.0697)	
$\text{Trans}_{it-3,4000}$	-0.192**	424	-0.1381***	-0.1506**	430
	(0.0609)		(0.0411)	(0.0609)	
$\text{Trans}_{it-4,4000}$	-0.2242**	376	-0.1808**	-0.1902*	382
	(0.0917)		(0.0687)	(0.0917)	
$\text{Trans}_{it-5,4000}$	-0.2253**	335	-0.1311	-0.1472	341
	(0.0924)		(0.0837)	(0.0924)	
$\text{Trans}_{it-6,4000}$	-0.2637	288	-0.1664	-0.1519	291
	(0.1495)		(0.1264)	(0.1495)	
$\text{Trans}_{it-7,4000}$	-0.2215	240	-0.0963	-0.0744	243
	(0.1271)		(0.0941)	(0.1271)	
$\text{Trans}_{it-8,4000}$	0.0305	204	0.1847	0.2104	207
	(0.1846)		(0.1483)	(0.1846)	
$\text{Trans}_{it-9,4000}$	-0.0679	167	0.0378	-0.0778	170
	(0.2816)		(0.2452)	(0.2816)	
Micro Controls	yes		yes	yes	
Macro Controls	yes		yes	yes	
Number of Observations	6,637		16,378	16,378	
Number of Individuals	986		1,317	2,586	
of which in control group	488		811	2,080	
Adjusted R^2	0.0659		0.0680	0.0635	

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Trans}_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$, and zero else. For example, $\text{Trans}_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Table 3.6: Results – Sub-Samples, FE Models, Spatial Matching ($15,000m$) $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{Construction}_{\mathrm{it,4000}}$	-0.1261**	-0.0937	-0.0711	-0.1356**	0.0634	-0.2127***
	(0.0488)	(0.1132)	(0.0686)	(0.0436)	(0.0499)	(0.0605)
Micro Controls	yes	yes	yes	yes	yes	yes
Macro Controls	yes	yes	yes	yes	yes	yes
Number of Observations	12,570	3,808	3,934	12,350	5,469	10,909
Number of Individuals	2,047	700	1,380	2,400	722	1,864
of which in treatment group	388	155	308	488	148	358
of which in control group	1,659	545	1,072	1,912	587	1,506
Adjusted R^2	0.0635	0.0733	0.0668	0.0636	0.0669	0.0650

(1) House-owners, (2) Non-house-owners, (3) Worries environment high, (4) Worries environment not high,
(5) Worries climate change high, (6) Worries climate change not high

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: $\mathrm{Construction}_{\mathrm{it,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Table 3.7: Results – Robustness (Placebo Tests), FE Models, Propensity-Score (PS) and Spatial (S) Matching *Construction*$_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	PS	PS	PS	PS	S $(15,000m)$	S $(15,000m)$	S $(15,000m)$	S $(15,000m)$
F3.Construction$_{\text{it},4000}$ *(Third Lead)*			0.0806	0.0956			0.0772	0.1083
			(0.0894)	(0.1109)			(0.0843)	(0.1119)
F2.Construction$_{\text{it},4000}$ *(Second Lead)*		-0.0208		-0.0470		-0.0163		-0.0335
		(0.0535)		(0.1104)		(0.0399)		(0.1008)
F1.Construction$_{\text{it},4000}$ *(First Lead)*	-0.0650			0.0474	-0.0593			0.0421
	(0.0505)			(0.0949)	(0.0536)			(0.0939)
Construction$_{\text{it},4000}$				-0.1354***				-0.1239***
				(0.0396)				(0.0313)
Micro Controls	yes	yes	yes	yes	yes	yes	yes	yes
Macro Controls	yes	yes	yes	yes	yes	yes	yes	yes
Number of Observations	6,189	5,843	5,274	5,274	15,235	14,408	12,988	12,988
Number of Individuals	897	872	819	819	2,306	2,246	2,090	2,090
of which in treatment group	496	492	479	479	504	500	486	486
of which in control group	401	380	340	340	1,802	1,746	1,604	1,604
Adjusted R^2	0.0536	0.0517	0.0499	0.0503	0.0561	0.0541	0.0531	0.0532

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: Construction$_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

more sensitive towards intrusions into their surroundings.

3.5.5 Robustness: Placebo Tests

To check the robustness of our results regarding confounding factors, we conduct placebo tests. Specifically, we include up to three leads of the treatment variable, first individually and then jointly in combination with the contemporary treatment variable, in both our default difference-in-differences propensity-score and spatial matching specifications. Table 3.7 reports the results.

As can be seen, none of the leads is significant at any conventional level, both in the propensity-score – first to third column – and spatial – fifth to seventh column – matching specification. They are also much smaller in size, and in case of the third lead even of opposite sign. When included jointly in combination with the contemporary treatment variable – fourth and eighth column – they remain insignificant without clear pattern in terms of sign and size. The contemporary treatment variable, however, is still significant at the 1% level, negative, and large in magnitude. We take this as evidence that our estimates indeed systematically pick up the effect of wind turbine construction rather than confounding factors.[100]

3.5.6 Robustness: View Shed Analysis

To check the robustness of our results regarding actual visual relationships between households and installations, we combined our geographical information on households and wind turbines with a digital terrain model for Germany (Federal Agency for Cartography and Geodesy 2016b) in order to perform a view shed analysis. In this type of analysis, for every household, it is established, based on location-specific elevated terrain, to which extent there is a direct visual relationship between the household and the nearest wind turbine (i.e. whether the household is located within the view shed of the wind turbine). This also provides further insight into disentangling the identified negative externalities into landscape aesthetics and other channels. To be clear, a digital terrain model includes only geographical barriers to visibility such as location-specific elevated terrain, while excluding natural ones such as forests and trees as well as man-made structures such as houses and fences, all of which may equally be barriers to visibility. However, to the extent that the latter are built on purpose in order to block visibility, individuals who built them are presumably those that are most adversely affected. In this vein, our estimates can be interpreted as a lower bound.

We created a new treatment group of households that are located within the default treat-

100. This is also evidence that the construction of a wind turbine is a rather sudden, short-lived, and unanticipated event.

ment radius of 4,000 metres and that have a direct view of wind turbines, as well as a corresponding new measure of treatment intensity – the visible height of wind turbines from the viewpoint of households. Based on these, we performed a view shed analysis. The results are presented in Table 3.8.

As can be seen, the point estimates using the new treatment group definition are very similar to those using the old, in both our propensity-score – first column – and spatial – third column – matching specification. In fact, they are only slightly smaller in size and slightly less significant; the latter is most likely due to the loss of observations resulting from wind turbines covered by terrain. Moreover, the second and fourth column show that, when using the new treatment group definition and interacting the main effect with the visible height of the nearest installation, life satisfaction drops significantly for each metre rise in visibility. Interestingly, from all measures of treatment intensity, the visible height of wind turbines from the viewpoint of households is the only measure that turns out significant.[101]

We take this as evidence that the identified negative externalities associated with the construction of wind turbines are indeed foremost driven by negative impacts on landscape aesthetics. Moreover, the aggravating effect of the visible height of the nearest installation suggests that they are mainly driven by households that stand in direct visual relationship to them; however, the vast majority of households in our sample (about 92%) can see at least part of the nearest installation.

3.5.7 Robustness: Residential Sorting

So far, we have excluded movers from all our analyses. To evaluate the extent to which simultaneity and resulting endogeneity plays a role, we conduct two robustness checks on a sample augmented by movers.

First, we analyse moving reasons. Descriptive statistics, as recorded in the SOEP, indicate that about 87% of moves are due to reasons that are not linked to geographical location. To dig deeper, we estimate linear probability models that regress a dummy indicating a move since the last period on the treatment dummy. Otherwise, the models are equivalent to our baseline specifications. Results show that the construction of a wind turbine in the default treatment radius of 4,000 metres has no significant effect on the probability of moving; see Table 3.20 in Section 3.8. In either matching specification, point estimates are close to zero. Thus, we do not find empirical evidence that residential sorting is endogenous to wind turbine construction.

101. We also recalculated all of our other intensity measures, including the inverse and reverse distance to the nearest installation, as well as the cumulative number of installations around the household, for the new treatment group. We did the same for our measures of treatment persistence. Tables 3.23 and 3.24 in Section 3.8 present the results of these additional analyses.

Table 3.8: Results – Robustness (View Shed Analysis), FE Models, Propensity-Score (PS) and Spatial (S) Matching $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	PS	PS	S $(15,000m)$	S $(15,000m)$
$\text{ConstructionVisible}_{\text{it,4000}}$	-0.1388**		-0.1082**	
	(0.0471)		(0.0381)	
$\text{ConstructionVisible}_{\text{it,4000}} \times \text{HeightVisible}_{\text{it,4000}}$		-0.0013**		-0.0010**
		(0.0005)		(0.0004)
Micro Controls	yes	yes	yes	yes
Macro Controls	yes	yes	yes	yes
Number of Observations	6,273	6,273	16,013	16,013
Number of Individuals	939	939	2,538	2,538
of which in treatment group	451	451	458	458
of which in control group	488	488	2,080	2,080
Adjusted R^2	0.0623	0.0624	0.0615	0.0616

Robust standard errors clustered at the federal state level in parentheses
**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: $\text{ConstructionVisible}_{\text{it,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t and the household has a direct view on it, and zero else. $\text{HeightVisible}_{\text{it,4000}}$ is the corresponding visible height of the wind turbine from the viewpoint of the household in metres. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: Federal Agency for Cartography and Geodesy (2016b), SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Table 3.9: Robustness (Residential Sorting - Sample Includes Movers) – FE Models, Spatial (S) Matching, $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

	S $(10,000m)$			S $(15,000m)$		
	No movers	All movers	All movers except TT	No movers	All movers	All movers except TT
Control spell	$\text{Construction}_{\text{it},4000}$	$\text{Construction}_{\text{it},4000}$	$\text{Construction}_{\text{it},4000}$	$\text{Construction}_{\text{it},4000}$	$\text{Construction}_{\text{it},4000}$	$\text{Construction}_{\text{it},4000}$
1	-0.1086***	-0.0761**	-0.0882***	-0.1143**	-0.0732	-0.0809*
	(0.0229)	(0.0313)	(0.0269)	(0.0367)	(0.0516)	(0.0428)
2	-0.1111***	-0.0712**	-0.0844***	-0.1160***	-0.0713	-0.0799*
	(0.0203)	(0.0287)	(0.0259)	(0.0343)	(0.0482)	(0.0399)
4	-0.1224***	-0.0729**	-0.0873***	-0.1236***	-0.0798	-0.0894**
	(0.0235)	(0.0242)	(0.0214)	(0.0340)	(0.0478)	(0.0398)
6	-0.1031**	-0.0603**	-0.0763***	-0.1349***	-0.0913*	-0.1027**
	(0.0332)	(0.0205)	(0.0213)	(0.0314)	(0.0413)	(0.0356)

Robust standard errors clustered at the federal state level in parentheses

**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

Note: $\text{Construction}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. Rows show results for different minimum control spells, that is the timespan an individual must have remained within the control to be included in the analysis. Columns two and five contain specifications without movers, columns three and six specifications including all movers, and columns four and seven specifications including all movers except individuals who move from the treatment to the treatment group. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Second, we re-estimate our baseline spatial matching specification including both movers and non-movers. In doing so, we extend our baseline data quality requirements and exclude individuals who violate one of them before or after a move, for instance when relocating to an urban area, as well as individuals who moved in the period prior to their first observation. An additional dummy captures the effect of having moved as such. To ensure comparability with the treatment group, we impose a minimum timespan an individual must have remained in the control group: it varies between one year, which constitutes no additional restriction, and six years.

Table 3.9 summarises the results. The additional requirement of a minimum spell in the control group leaves baseline findings without movers virtually unchanged. However, the inclusion of movers attenuates estimates, while significance is preserved for the 10,000 metres spatial matching specification. Selectively adding subgroups of movers to the baseline model shows that individuals who relocate from the treatment to the treatment group (i.e. individuals who move within the treatment group and do not change group allocation due to their move) trigger the strongest attenuation. When dropping such movers, estimates are larger and statistically significant for both matching radii.

Recall that our research design can be characterised as an intention-to-treat approach: treated individuals can be expected to be unequally strongly affected. While we do not find empirical evidence for *endogenous residential sorting* with respect to turbine construction, theory predicts that individuals when relocating due to other reasons – and transaction costs can be regarded as partially sunk – take wind turbines into account and optimise with regard to their actual impacts. Such *conditional residential sorting*, together with the intention-to-treat character of our analysis, provides the lens through which to understand findings from the robustness check: especially for treat-to-treat and control-to-treat movers, one can expect that relocation occurs to sites in which turbines are less salient, thus attenuating estimates of treatment effects. For control-to-control and to a lesser extent for treat-to-control movers, indirect effects can be expected to likewise attenuate estimates: as turbine construction reduces the choice set for relocating, utility will decline, thus yielding a smaller wedge between control and treated individuals.

Taken together, while theory predicts that simultaneity leads to a bias whose direction is unclear, empirical evidence does not support endogenous residential sorting with respect to treatment. Rather, if an individual decides to move and transaction costs can be regarded as partially sunk, self selection into less affected sites is rational, and attenuating estimates.

3.6 Discussion

Our findings provide empirical evidence that the presence of wind turbines does entail negative externalities, though limited in both space and time. It is not unequivocal where exactly to delineate effectiveness of these externalities, though: clearly, they impact residents in their surroundings who choose to stay. However, they also influence all potential residents of the area who decide not to move there, just as developers who decide not to project new residences. Likewise, the recreational value of the landscape can be devalued, with impacts on both visitors and potential visitors. Finally, non-use values of natural and cultural landscapes as well as species can be affected.

A monetisation of the negative external effects of wind turbines, let alone a comprehensive cost-benefit account, is therefore difficult to conduct.[102] Based on our findings, however, we can draw some modest conclusions for affected residents in their immediate surroundings who decide not to move away. Also here, some caveats apply. First, the impact of income on life satisfaction may comprise more subtle aspects like relative comparisons to the past or to others. Moreover, evidence suggests that quantifications using well-being data may overestimate the monetary effect of an environmental externality. Likewise, the life satisfaction approach has been shown to result in relatively low trade-off ratios between the externality to be valued and individual characteristics such as whether an individual is unemployed (Luechinger 2009). Numbers derived here are thus an informed point of reference.

We provide both a lower and an upper bound for the monetised negative externalities. For the lower bound, we draw on results from the 10,000 metres radius matching, as in Table 3.5, where only coefficient estimates for transition periods two to four are significantly negative at a conventional level. The log annual net household income for the treatment group amounts to 10.4, as in Table 3.1. A one per cent increase in annual income thus corresponds to 319.5 Euro. Trading off the positive coefficient of income against the three negative coefficients of the treatment, each affected household is on average impacted by a monetised externality of about 564 Euro in total; 155 Euro for the second year, 177 Euro for the third, and 232 Euro for the fourth. For the upper bound, we suppose a permanent effect and take the coefficient estimate largest in size from the propensity-score matching. Applying the same calculus, the monetised negative externality amounts to 258 Euro per year for each affected household.

Recall that in our heterogeneity analysis, we found the negative external effect on the well-being of house owners to be significant and stronger than at the aggregate level, whereas it

102. Additionally, effects of intermittent wind power, that is electricity generated by wind turbines, within the electricity system are nontrivial to quantify (Borenstein 2012; Hirth et al. 2016).

was insignificant for renters. We conjectured that renters may be more swiftly compensated through a decrease in rents, as the negative external effect is internalised through the price mechanism in rental markets. To put this to test, we conducted an additional hedonic analysis: we re-estimated our baseline specifications using log annual net rents as outcome while controlling for a wide range of dwelling and amenity characteristics.[103] We find that wind turbine construction is associated with a decrease of about 4% in annual net rents, which amounts to a decrease of approximately 200 Euro per year for each affected household, similar to our upper-bound estimate obtained from using well-being data. However, this effect is only present in our spatial matching specifications. With propensity-score matching, estimates are small and insignificant.[104]

It thus seems that where exactly externalities are internalised depends on the type of household: for house owners, internalisation through rental prices does not work, so that it must occur through other channels such as well-being. For renters, however, this is not ex-ante clear: it may occur through rental prices in case that rental markets are sufficiently dynamic and prices are flexible. In this case, a decrease in rental prices implies an increase in real income that offsets any change in well-being. Otherwise, in case that rental markets are rigid, internalisation may occur through well-being. Importantly, both cases are only corner solutions: there is a continuum of combinations of changes in both rental prices and well-being, and this continuum also has a temporal dimension, as rental markets may change over time. Thus, when studying externalities, the life satisfaction approach and the hedonic method fruitfully complement each other.

103. We estimated the following log-level hedonic regression:

$$ln(R_{dt}) = \beta_0 + \mathbf{DC}'_{dt}\beta_1 + \mathbf{AC}'_{dt}\beta_2 + \delta Construction_{dt,4000} + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \eta trend_{st} + \epsilon_{dt}$$

where R_{dt} is the annual rent of dwelling d at time t; DC_{dt} is a vector of dwelling characteristics, including whether it is a detached, semi-detached, or terraced house, a small apartment or large building, or a high rise, as well as the number of rooms per individual; AC_{dt} is a vector of amenity characteristics, including whether the dwelling has a kitchen, an indoor bath or shower, an indoor toilet, central or floor heating, a balcony or terrace, a basement, a garden, or a boiler; $Construction_{dt,4000}$ is a treatment dummy variable as is the main specification; $Year_{2000+n}$ is a full set of year dummy variables; $trend_{st}$ are state-specific linear time trends; and ϵ_{dt} is the idiosyncratic disturbance. We exclude households that have parts of their rents subsidised, or pay no rents at all, as well as non-private households such as nursing homes in order to not distort our estimates. We follow a similar approach as Luechinger (2009) and use net rents rather than net rents per square metre. Note, however, that we implicitly account for the dwelling size by controlling for dwelling type and for the number of rooms per individual. Robust standard errors are clustered at the county times year level.

104. Table 3.21 in Section 3.8 presents the results of the additional hedonic analysis.

3.7 Conclusion

In many countries, wind power plays an ever increasing role in electricity generation. The economic rationale behind this trend is to avoid negative environmental externalities common to conventional technologies: wind power is largely free of emissions from fossil fuel combustion, as well as waste and risks from nuclear fission. For instance, the German Environment Agency calculated for 2012 that onshore wind energy saved approximately 39 million tons of CO_2 emissions in Germany (German Environment Agency 2014). With current estimates of damage costs between roughly 50 and 100 Euro per ton (Foley et al. 2013; Bergh and Botzen 2014, 2015), avoided externalities are large. For wind power to play an effective role, however, wind turbines must be constructed in large numbers, rendering them more spatially dispersed. In fact, the greater proximity of wind turbines to consumers has been found to have negative externalities itself, most importantly negative impacts on landscape aesthetics.

Against this background, we investigated the effect of the presence of wind turbines on residential well-being in Germany, combining household data from the German Socio-Economic Panel Study (SOEP) with a unique and novel panel dataset on more than 20,000 wind turbines for the time period between 2000 and 2012. Employing a difference-in-differences design that exploits the exact geographical coordinates of households and turbines, as well as their interview and construction dates, we established causality. To ensure comparability of the treatment and control group, we applied propensity-score and spatial matching techniques based, among others, on exogenous weather data and geographical locations of residence. We showed that the construction of a wind turbine in the surroundings of households has a significant negative effect on life satisfaction. Importantly, this effect is both spatially and temporally limited. The results are robust to using different model specifications. Additional robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm our results.

We arrived at a monetary valuation of the negative externalities between 564 Euro per household in total when supposing a vanishing effect, and 258 Euro per household and year when supposing a permanent disamenity. An additional hedonic analysis confirms the level of this valuation. From a policy perspective, thus, opposition against wind turbines cannot be neglected. It remains the task of policy-makers to communicate benefits of avoided external costs, moderate decision-making processes, and consider distributional implications and potential compensation measures.

Several limitations and open points provide room for further research. First, our data on view sheds and concrete visibility from places of residence is somewhat limited. Advanced digital surface models taking into account natural and man-made structures could provide richer evidence. Second, data on the ownership structure of wind turbines could allow disentangling the nexus between positive and negative spillovers, thus allowing for a more pronounced determination of external effects. Both caveats, however, are consistent with a lower-bound interpretation of our findings: residents in the treatment group might actually not be affected, and wind turbines in community ownership might have potentially positive monetary or idealistic effects on nearby residents. Beyond that, avenues for future research lie in the transfer of the empirical strategy applied in this study to other energy infrastructure, such as biomass plants or transmission towers.

3.8 Online Appendix to Chapter 3

3.8.1 Descriptive Statistics for Wind Turbines in the *Included Group*

Table 3.10: Descriptive Statistics

	[#]	Capacity [kW]			Total height [m]			Share
		min	max	average	min	max	average	
Germany	10083	200	7500	1571	51	239	123	49 %
Baden-Württemberg	309	500	3000	1425	66	186	124	77 %
Bavaria	434	500	3370	1705				68 %
Berlin	1			2000			138	100 %
Brandenburg	2401	500	7500	1683	83	239	133	71 %
Bremen	2	2000	2500	2250	118	143	131	3 %
Hamburg	7	270	6000	3096	66	198	156	12 %
Hesse	343	500	3000	1616	85	186	138	51 %
Lower Saxony	631	300	2500	1674	67	170	118	34 %
Mecklenburg-Vorpommern	726	500	2500	1005				59 %
North Rhine-Westphalia	956	500	2500	1358				33 %
Rhineland-Palatinate								0 %
Saarland	2	2300	2300	2300	145	145	145	1 %
Saxony	491	299	3158	1528	51	186	116	59 %
Saxony-Anhalt	2029	300	7500	1683	56	199	126	77 %
Schleswig-Holstein	1489				63	183	106	55 %
Thuringia	262	600	3075	1741				41 %

Note: capacity, total height, and shares rounded to integers. Blanks if no information available. The share describes the percentage of turbines in the *included group* within each federal state of Germany.

Source: see Online 3.8.3.2.

3.8.2 Descriptive Statistics

Table 3.11: Descriptive Statistics for Spatial Matching (S)

Variables	Mean Treatment Group (T)	Control Group, S ($10,000m$) (C1)	Control Group, S ($15,000m$) (C2)	Normalised Difference (T)-(C1)	Normalised Difference (T)-(C2)
Micro Controls					
Age	54.1815	53.2244	53.1816	0.0455	0.0474
Is Female	0.5009	0.5078	0.5131	0.0098	0.0172
Is Married	0.7793	0.7637	0.7613	0.0263	0.0303
Is Divorced	0.0479	0.0365	0.0411	0.0403	0.0233
Is Widowed	0.0744	0.0573	0.0689	0.0487	0.0152
Has Very Good Health	0.0577	0.0645	0.0690	0.0202	0.0329
Has Very Bad Health	0.0429	0.0402	0.0390	0.0098	0.0139
Is Disabled	0.1446	0.1525	0.1372	0.0157	0.0149
Has Migration Background	0.0874	0.0830	0.1253	0.0112	0.0871
Has Tertiary Degree	0.2829	0.2333	0.2813	0.0803	0.0024
Has Lower Than Secondary Degree	0.1840	0.1727	0.1715	0.0210	0.0232
Is in Education	0.0100	0.0159	0.0142	0.0367	0.0274
Is Full-Time Employed	0.3745	0.3508	0.3752	0.0349	0.0009
Is Part-Time Employed	0.1109	0.1034	0.1067	0.0171	0.0095
Is on Parental Leave	0.0067	0.0056	0.0101	0.0099	0.0260
Is Unemployed	0.0749	0.0682	0.0590	0.0184	0.0450
Log Monthly Net Individual Income[a]	6.4477	6.4098	6.4792	0.0279	0.0230
Has Child in Household	0.2262	0.2374	0.2623	0.0187	0.0595
Log Annual Net Household Income[a]	10.3719	10.3546	10.4101	0.0215	0.0468
Lives in House[b]	0.5537	0.6099	0.6011	0.0829	0.0699
Lives in Small Apartment Building	0.0886	0.0853	0.0855	0.0073	0.0071
Lives in Large Apartment Building	0.1593	0.1306	0.1320	0.0574	0.0544
Lives in High Rise	0.0112	0.0113	0.0123	0.0007	0.0071
Number of Rooms per Individual	1.8012	1.8657	1.8712	0.0496	0.0532

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	Mean				
	Treatment Group	Control Group, S ($10,000m$)	Control Group, S ($15,000m$)	Normalised Difference	Normalised Difference
Variables	(T)	(C1)	(C2)	(T)-(C1)	(T)-(C2)
Macro Controls					
Unemployment Rate	12.0172	10.4592	10.1886	0.1988	0.2314
Average Monthly Net Household Income[a]	1,363.3050	1,403.8010	1,428.5320	0.1451	0.2252
Number of Observations	4,005	4,604	12,373	-	-
Number of Individuals	506	811	2,080	-	-

[a] In Euro/Inflation-Adjusted (Base Year 2000), [c] Detached, Semi-Detached, or Terraced

Note: The third column shows the normalised difference, which is calculated as $\triangle x = (\bar{x}_t - \bar{x}_c) \div \sqrt{\sigma_t^2 + \sigma_c^2}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate for the treatment and control group, respectively. σ^2 denotes the variance. As a rule of thumb, a normalised difference greater than 0.25 indicates a non-balanced covariate, which might lead to sensitive results (Imbens and Wooldridge 2009). All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, own tabulations.

3.8.2.1 Graphs

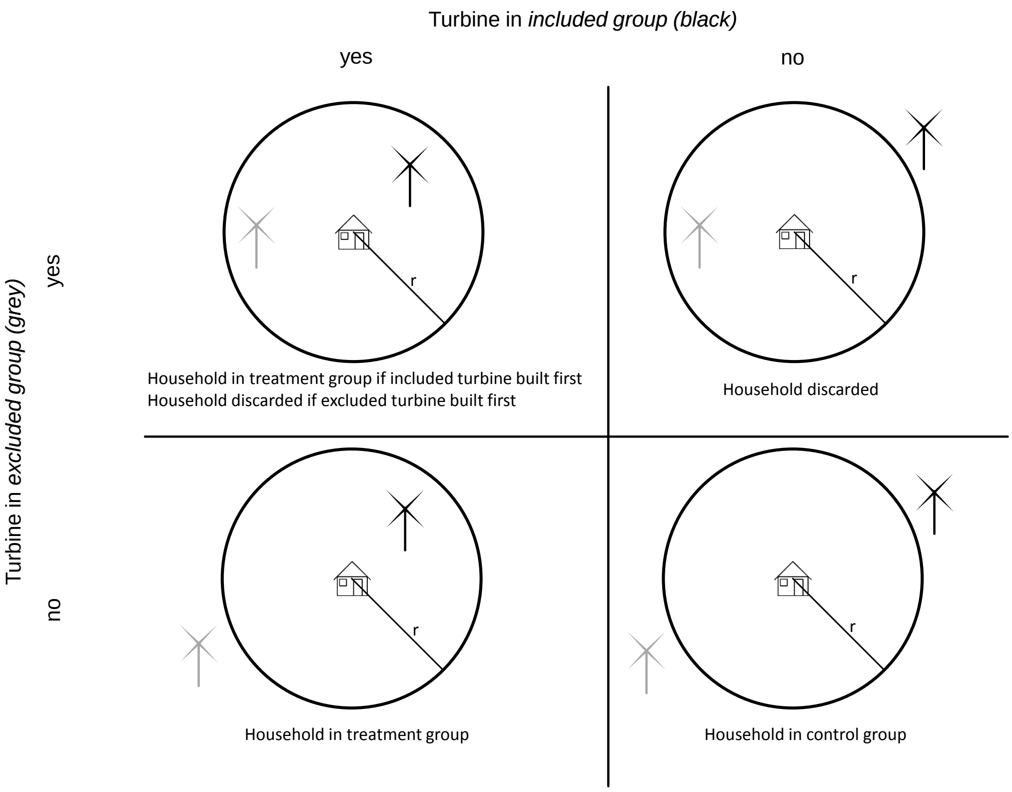


Figure 3.3: Households around which a wind turbine of the *excluded group* is constructed first are discarded, the others are allocated to either the treatment or control group

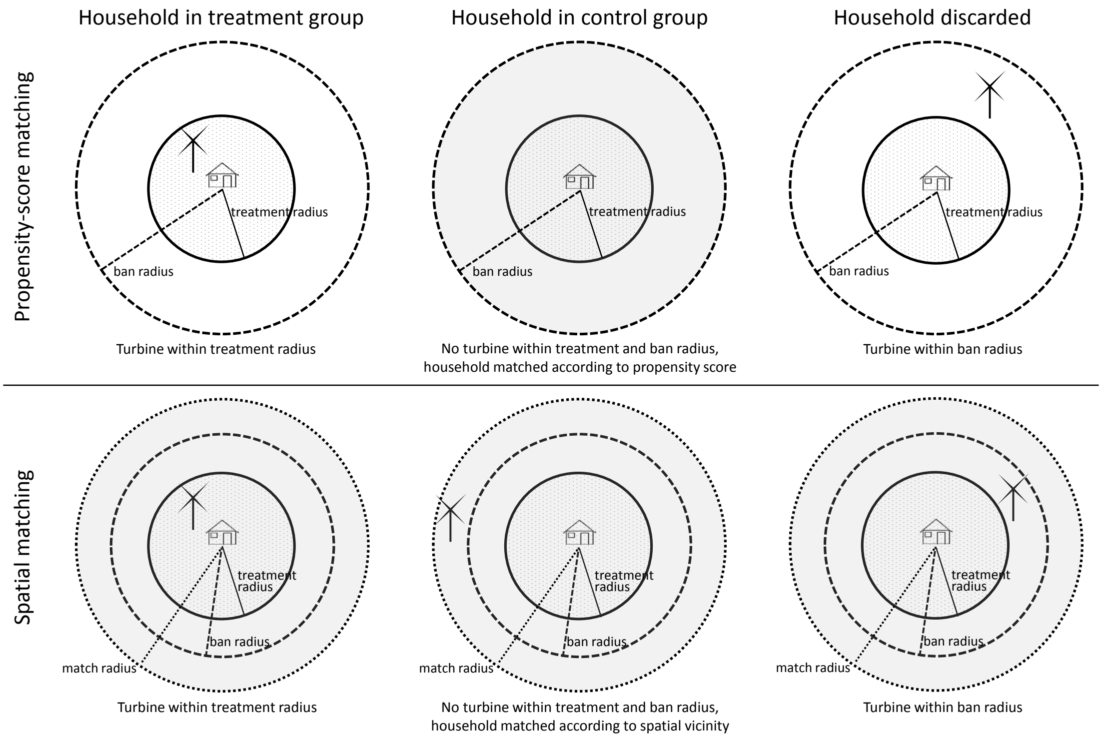
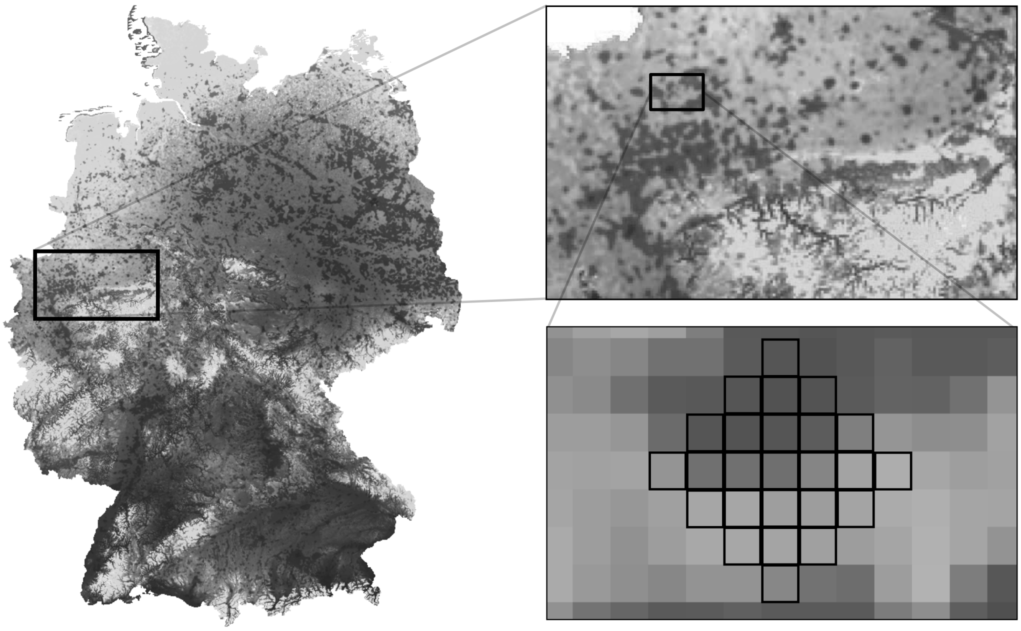


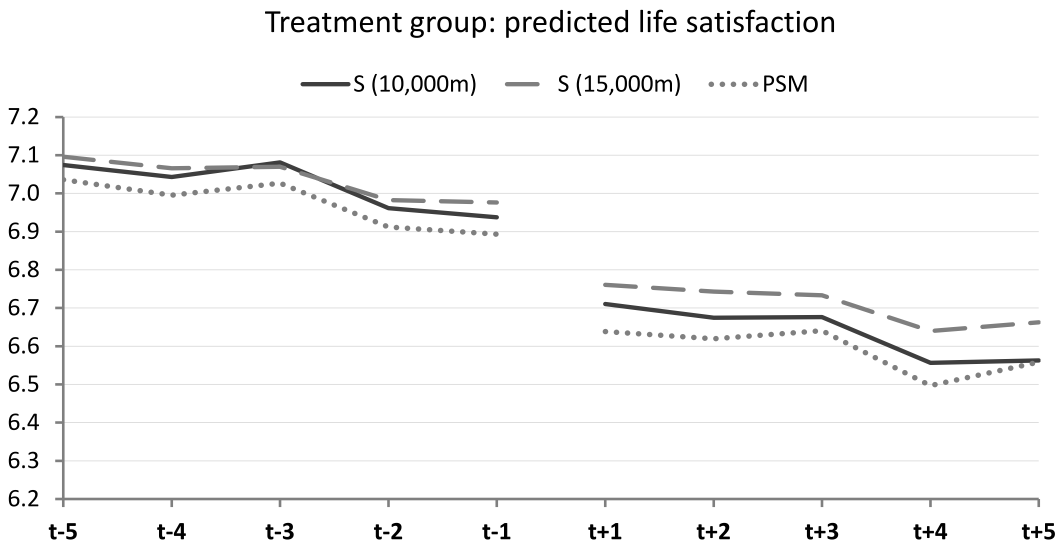
Figure 3.4: Empirical Model – Matching Strategy



Note: Calculation for each household of the mean expected annual energy yield of a wind turbine from the 25 one kilometre times one kilometre tiles surrounding it. Coding ranging from dark (lowest expected annual wind yield) to light (highest expected annual wind yield).

Source: German Meteorological Service (2014), own visualisation.

Figure 3.5: Calculation of Mean Expected Annual Energy Yield



Note: Prediction of life satisfaction, normalisation of point in time of treatment to $t = 0$, and calculation of mean predicted life satisfaction for periods $t - 5$ to $t + 5$.

Source: SOEP v29, 2000-2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Figure 3.6: Predicted Mean Life Satisfaction Before and After Treatment

3.8.3 Detailed Results

Table 3.12: Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	PS	S $(10,000m)$	S $(15,000m)$
$\text{Construction}_{\text{it},4000}$	-0.1405***	-0.1088***	-0.1138**
	(0.0399)	(0.0222)	(0.0366)
Age	-0.0689	-0.0792***	-0.0142
	(0.0425)	(0.0197)	(0.0199)
Age Squared	0.0001	0.0002	-0.0001
	(0.0004)	(0.0002)	(0.0002)
Is Female			
Is Married	0.0903	-0.1502	0.1175
	(0.1449)	(0.1856)	(0.2095)
Is Divorced	0.2802	-0.0721	0.1241
	(0.4173)	(0.0945)	(0.2315)
Is Widowed	-0.1891	-0.7490**	-0.2608
	(0.2035)	(0.3319)	(0.2513)
Has Very Good Health	0.2967***	0.2833***	0.3674***
	(0.0693)	(0.0536)	(0.0424)
Has Very Bad Health	-1.3187***	-1.2854***	-1.2141***
	(0.1184)	(0.0887)	(0.1000)
Is Disabled	-0.0137	-0.0101	-0.2080**
	(0.1113)	(0.0881)	(0.0691)
Has Migration Background			
Has Tertiary Degree	-0.0087	-0.0303	-0.1976
	(0.1926)	(0.2628)	(0.1660)
Has Lower Than Secondary Degree	-0.0008	0.1677	0.2274
	(0.3042)	(0.2073)	(0.2062)
Is in Education	0.3740	0.1739	0.3345
	(0.4008)	(0.2544)	(0.2033)

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Dependent Variable: Satisfaction With Life

Regressors	PS	S $(10,000m)$	S $(15,000m)$
Is Full-Time Employed	0.0001	0.0213	0.0841
	(0.1182)	(0.0780)	(0.0655)
Is Part-Time Employed	-0.1220	-0.0534	-0.0426
	(0.1056)	(0.0904)	(0.0644)
Is on Parental Leave	0.0709	-0.0308	0.1516
	(0.2157)	(0.2097)	(0.1289)
Is Unemployed	-0.5000***	-0.4325***	-0.4542***
	(0.1233)	(0.0864)	(0.0772)
Log Monthly Net Individual Income[a]	0.0538	0.0523	0.0385
	(0.0539)	(0.0436)	(0.0282)
Has Child in Household	0.1555*	0.1997***	0.0897**
	(0.0741)	(0.0521)	(0.0374)
Log Annual Net Household Income[a]	0.1738	0.2503***	0.2003***
	(0.1173)	(0.0695)	(0.0537)
Lives in House[b]	-0.0135	0.0057	0.0086
	(0.0954)	(0.0484)	(0.0414)
Lives in Small Apartment Building	0.0051	0.0234	0.0159
	(0.0935)	(0.0575)	(0.0395)
Lives in Large Apartment Building	-0.0262	-0.0060	0.0144
	(0.0765)	(0.0421)	(0.0298)
Lives in High Rise	0.1176	0.0925	0.0720
	(0.2136)	(0.2107)	(0.1805)
Number of Rooms per Individual	0.0011	-0.0157	0.0136
	(0.0416)	(0.0402)	(0.0210)
Unemployment Rate	-0.0199	-0.0353***	-0.0081
	(0.0133)	(0.0102)	(0.0105)
Average Monthly Net Household Income[a]	0.0008	0.0004	-0.0006
	(0.0006)	(0.0008)	(0.0005)
Number of Observations	6,637	8,609	16,378
Number of Individuals	986	1,317	2,586

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Dependent Variable: Satisfaction With Life

Regressors	PS	S $(10,000m)$	S $(15,000m)$
of which in treatment group	498	506	506
of which in control group	488	811	2,080
F-Statistic	2,462.5200	9,891.2100	5,251.8600
R^2	0.0704	0.0715	0.0652
Adjusted R^2	0.0657	0.0678	0.0632

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000-2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.13: Results - FE Models, Propensity-Score Matching
$Construction_{it,8000/10000/15000}$

Dependent Variable: Satisfaction With Life

Regressors	r=8000	r=10000	r=15000
$\text{Construction}_{\text{it,r}}$	-0.0348	-0.0074	0.1303
	(0.0508)	(0.0645)	(0.1858)
Age	-0.2886	0.0093	-0.0512
	(0.0373)	(0.0192)	(0.0559)
Age Squared	0.0000	-0.0004	-0.0003
	(0.0002)	(0.0003)	(0.0004)
Is Female			
Is Married	-0.2568	-0.6604	-0.6631
	(0.2547)	(0.4986)	(0.6816)
Is Divorced	0.1843	-0.1972	-0.2746
	(0.2606)	(0.5383)	(0.6366)
Is Widowed	-0.6568*	-0.6836	-0.8520
	(0.3032)	(0.4503)	(0.6821)
Has Very Good Health	0.3276***	0.3398***	0.2804**
	(0.0814)	(0.0781)	(0.0872)
Has Very Bad Health	-1.3464***	-1.3147***	-1.2396***
	(0.1025)	(0.1574)	(0.2896)
Is Disabled	-0.0255	-0.1951	-0.2450**
	(0.0873)	(0.1407)	(0.0861)
Has Migration Background			
Has Tertiary Degree	-0.0026	-0.2182	-0.9182
	(0.1907)	(0.3084)	(0.7468)
Has Lower Than Secondary Degree	0.0054	1.1626**	-0.7703***
	(0.1663)	(0.4427)	(0.1394)
Is in Education	-0.1457	0.6630	0.6402
	(0.1904)	(0.4731)	(0.3646)
Is Full-Time Employed	0.0649	0.1354	-0.0820
	(0.1087)	(0.1375)	(0.1928)
Is Part-Time Employed	0.0473	-0.0249	-0.0756
	(0.0927)	(0.1128)	(0.2193)
Is on Parental Leave	0.0912	0.0431	0.0286
	(0.1369)	(0.1654)	(0.2412)
Is Unemployed	-0.4316***	-0.5374**	-0.4905***
	(0.1183)	(0.2060)	(0.0978)
Log Monthly Net Individual Income[a]	-0.0017	-0.0169	-0.0445
	(0.0444)	(0.0485)	(0.0677)
Has Child in Household	0.1246	0.2017	-0.0008
	(0.0927)	(0.1189)	(.01474)
Log Annual Net Household Income[a]	0.2628***	0.2074**	0.1571
	(0.0482)	(0.0736)	(0.1164)
Lives in House[b]	0.0011	-0.0209	0.0106
	(0.0617)	(0.0469)	(0.1294)
Lives in Small Apartment Building	0.0152	-0.0098	0.0156

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Dependent Variable: Satisfaction With Life

Regressors	r=8000	r=10000	r=15000
	(0.0752)	(0.0.0626)	(0.1340)
Lives in Large Apartment Building	-0.0178	-0.0356	0.0303
	(0.1077)	(0.0867)	(0.1010)
Lives in High Rise	0.0437	-0.0186	0.1251
	(0.1478)	(0.0008)	(0.3441)
Number of Rooms per Individual	0.0418	0.0643	0.0491
	(0.0292)	(0.0368)	(0.0469)
Unemployment Rate	-0.0376***	-0.0270*	-0.0455***
	(0.0089)	(0.0132)	(0.0116)
Average Monthly Net Household Income[a]	-0.0012*	-0.0009	0.0006
	(0.0006)	(0.0008)	(0.0009)
Number of Observations	9,389	6,254	2,767
Number of Individuals	1,357	939	423
of which in treatment group	684	474	212
of which in control group	673	465	211
F-Statistic	5,951.5600	7,431.9500	1,373.6400
R^2	0.0698	0.0816	0.0798
Adjusted R^2	0.0665	0.0766	0.0683

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\mathrm{Construction}_{\mathrm{it,r}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of r metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.14: Results – FE Models, Spatial Matching (S) (10,000m, 15,000m) $Construction_{it,8000}$

Dependent Variable: Satisfaction With Life

Regressors	S $(10,000m)$	S $(15,000m)$
$\text{Construction}_{\text{it,8000}}$	-0.0642	-0.0452
	(0.0372)	(0.0447)
Age	-0.0242	-0.0030
	(0.0266)	(0.0248)
Age Squared	-0.0001	-0.0001
	(0.0002)	(0.0002)
Is Female		
Is Married	-0.4424	-0.0844
	(0.5476)	(0.4607)
Is Divorced	-0.0619	0.0909
	(0.4789)	(0.5164)
Is Widowed	-0.8117	-0.4189
	(0.5315)	(0.4720)
Has Very Good Health	0.3484***	0.3920***
	(0.0741)	(0.0518)
Has Very Bad Health	-1.3571***	-1.2564***
	(0.1412)	(0.1378)
Is Disabled	-0.0327	-0.1994**
	(0.1207)	(0.0831)
Has Migration Background		
Has Tertiary Degree	-0.1510	-0.2413
	(0.1510)	(0.2108)
Has Lower Than Secondary Degree	0.1362	0.2324
	(0.1975)	(0.1761)
Is in Education	-0.0400	0.2268
	(0.2082)	(0.1824)
Is Full-Time Employed	0.1017	0.1417
	(0.0831)	(0.0779)
Is Part-Time Employed	0.0588	0.0545
	(0.0783)	(0.0597)
Is on Parental Leave	-0.0244	0.0714
	(0.1257)	(0.0862)
Is Unemployed	-0.4511***	-0.4796***
	(0.0998)	(0.0747)
Log Monthly Net Individual Income[a]	0.0188	0.0056
	(0.0373)	(0.0395)
Has Child in Household	0.2174**	0.0976
	(0.0760)	(0.0568)
Log Annual Net Household Income[a]	0.2354**	0.1812***
	(0.0793)	(0.0453)
Lives in House[b]	0.0098	0.0172
	(0.0230)	(0.0413)
Lives in Small Apartment Building	0.0534	0.0102

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Dependent Variable: Satisfaction With Life

Regressors	S $(10,000m)$	S $(15,000m)$
	(0.0539)	(0.0432)
Lives in Large Apartment Building	-0.0571	-0.0008
	(0.0368)	(0.0580)
Lives in High Rise	0.1087	0.0110
	(0.0820)	(0.1546)
Number of Rooms per Individual	0.0095	0.0230
	(0.0210)	(0.0185)
Unemployment Rate	-0.0445***	-0.0230**
	(0.0080)	(0.0070)
Average Monthly Net Household Income[a]	-0.0005	-0.0010*
	(0.0007)	(0.0005)
Number of Observations	8,643	14,485
Number of Individuals	1,241	2,193
of which in treatment group	698	698
of which in control group	543	1,495
F-Statistic	26,893.1900	14,555.3300
R^2	0.0740	0.0676
Adjusted R^2	0.0704	0.0654

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it},8000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 8,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.15: Results – FE Models, Propensity-Score Matching
$Construction_{it,4000}\times$ Intensity, $Trans_{it\text{-}\tau,4000}$

Dependent Variable: Satisfaction With Life

Regressors	Intensity $\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	Transition $\text{Trans}_{it-\tau,4000}$	# treated
$\text{Construction}_{\text{it},4000}$ × Intensity	-0.2090	-0.0128	-0.0178		
	(0.1605)	(0.0550)	(0.1556)		
$\text{Trans}_{it-1,4000}$				-0.0546	498
				(0.0642)	
$\text{Trans}_{it-2,4000}$				-0.1616**	444
				(0.0697)	
$\text{Trans}_{it-3,4000}$				-0.192**	424
				(0.0609)	
$\text{Trans}_{it-4,4000}$				-0.2242**	376
				(0.0917)	
$\text{Trans}_{it-5,4000}$				-0.2253**	335
				(0.0924)	
$\text{Trans}_{it-6,4000}$				-0.2637	288
				(0.1495)	
$\text{Trans}_{it-7,4000}$				-0.2215	240
				(0.1271)	
$\text{Trans}_{it-8,4000}$				0.0305	204
				(0.1846)	
$\text{Trans}_{it-9,4000}$				-0.0679	167
				(0.2816)	
Age	-0.0738	-0.0790	-0.0738	-0.0672	
	(0.0438)	(0.0446)	(0.0444)	(0.0413)	
Age Squared	0.0001	-0.0001	0.0001	0.0010	
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	
Is Female					
Is Married	-0.0946	0.1056	0.1116	0.0986	
	(0.1456)	(0.1451)	(0.1399)	(0.1530)	

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Regressors	Intensity InvDist$_{it,4000}$	RevDist$_{it,4000}$	Cumul$_{it,4000}$	Transition Trans$_{it-\tau,4000}$	# treated
Is Divorced	0.2825	0.2913	0.3020	0.3110	
	(0.4115)	(0.4110)	(0.4142)	(0.4034)	
Is Widowed	-0.1842	-0.1696	-0.1615	-0.1833	
	(0.2078)	(0.2079)	(0.2026)	(0.2078)	
Has Very Good Health	0.2967***	0.2955***	0.2963***	0.2971***	
	(0.0694)	(0.0698)	(0.0696)	(0.0694)	
Has Very Bad Health	-1.3164***	-1.3166***	-1.3222***	-1.3280***	
	(0.1189)	(0.1201)	(0.1197)	(0.1135)	
Is Disabled	0.0149	0.0137	0.0128	0.0212	
	(0.1101)	(0.1103)	(0.1099)	(0.1132)	
Has Migration Background					
Has Tertiary Degree	-0.0016	0.0038	0.0035	-0.0284	
	(0.1923)	(0.1920)	(0.1915)	(0.1914)	
Has Lower Than Secondary Degree	0.0029	0.0032	-0.0021	-0.0131	
	(0.3066)	(0.3092)	(0.3069)	(0.3061)	
Is in Education	0.3658	0.3658	0.3670	0.3770	
	(0.4006)	(0.4004)	(0.4029)	(0.3998)	
Is Full-Time Employed	-0.0022	-0.0024	-0.0046	0.0022	
	(0.1181)	(0.1180)	(0.1178)	(0.1120)	
Is Part-Time Employed	-0.0154	-0.0156	-0.0148	-0.0113	
	(0.1052)	(0.1059)	(0.1064)	(0.1056)	
Is on Parental Leave	0.0743	0.0768	0.0784	0.0727	
	(0.2203)	(0.2242)	(0.2201)	(0.2144)	
Is Unemployed	-0.5049***	-0.5080***	-0.5075***	-0.5013***	
	(0.1224)	(0.1208)	(0.1209)	(0.1241)	
Log Monthly Net Individual Income[a]	0.0540	0.0541	0.0539	0.0532	
	(0.0536)	(0.0532)	(0.0533)	(0.0552)	
Has Child in Household	0.1509	0.1491*	0.1479*	0.1546*	
	(0.0742)	(0.0753)	(.0743)	(0.0791)	
Log Annual Net Household Income[a]	0.1720	0.1726	0.1760	0.1744	
	(0.1181)	(0.1170)	(0.1178)	(0.1184)	
Lives in House[b]	-0.0134	-0.0144	-0.0134	-0.0136	
	(0.0957)	(0.0958)	(0.0958)	(0.0954)	

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Regressors	Intensity $\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	Transition $\text{Trans}_{it-\tau,4000}$	# treated
Lives in Small Apartment Building	0.0043	0.0028	0.0041	0.0046	
	(0.0945)	(0.0960)	(0.0954)	(0.0927)	
Lives in Large Apartment Building	-0.0260	-0.0264	-0.0255	-0.0272	
	(0.0769)	(0.0774)	(0.0770)	(0.0761)	
Lives in High Rise	0.1176	0.1180	0.1181	0.1120	
	(0.2107)	(0.0774)	(0.2103)	(0.2111)	
Number of Rooms per Individual	0.0007	0.0002	0.0006	0.0008	
	(0.0415)	(0.0411)	(0.0413)	(0.0421)	
Unemployment Rate	-0.0222	-0.0241	-0.0237	-0.0159	
	(0.0142)	(0.0146)	(0.0148)	(0.0127)	
Average Monthly Net Household Income[a]	0.0008	0.0008	0.0007	0.0009	
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	
Number of Observations	6,637	6,637	6,637	6,637	
Number of Individuals	986	986	986	986	
of which in treatment group	498	498	498		
of which in control group	488	488	488	488	
F-Statistic	3,052.8700	2,800.3000	2,605.900	8,865.0800	
R^2	0.0698	0.0694	0.0697	0.0719	
Adjusted R^2	0.0650	0.0646	0.0659	0.0659	

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The intensity measures are defined as follows: $\text{InvDist}_{it,4000}$ is the inverse distance, $\text{RevDist}_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, $\text{Cumul}_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4,000 metres, all in interview year t. $\text{Trans}_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$, and zero else. For example, $\text{Trans}_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.16: Results – FE Models, Spatial Matching (10,000m)
Construction$_{it,4000}$× Intensity, Trans$_{it\text{-}\tau,4000}$

Dependent Variable: Satisfaction With Life

Regressors	Intensity InvDist$_{it,4000}$	RevDist$_{it,4000}$	Cumul$_{it,4000}$	Transition Trans$_{it-\tau,4000}$	# treated
Construction$_{\mathrm{it},4000}$ × Intensity	-0.1604	-0.0078	-0.0142		
	(0.1038)	(0.0411)	(0.0113)		
Trans$_{it-1,4000}$				-0.0401	506
				(0.0657)	
Trans$_{it-2,4000}$				-0.1212**	450
				(0.0482)	
Trans$_{it-3,4000}$				-0.1381***	430
				(0.0411)	
Trans$_{it-4,4000}$				-0.1808**	382
				(0.0689)	
Trans$_{it-5,4000}$				-0.1311	341
				(0.0837)	
Trans$_{it-6,4000}$				-0.1644	291
				(0.1264)	
Trans$_{it-7,4000}$				-0.0963	243
				(0.0941)	
Trans$_{it-8,4000}$				0.1847	207
				(0.1483)	
Trans$_{it-9,4000}$				0.0378	170
				(0.2452)	
Age	-0.0821***	-0.0853***	-0.0818***	-0.0793***	
	(0.0204)	(0.0210)	(0.0206)	(0.0199)	
Age Squared	-0.0002	0.0002	0.0002	0.0002	
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	
Is Female					
Is Married	-0.1501	-0.1450	-0.1400	-0.1467	
	(0.1841)	(0.1831)	(0.1837)	(0.1970)	

Continued on next page

Regressors	Intensity InvDist$_{it,4000}$	RevDist$_{it,4000}$	Cumul$_{it,4000}$	Transition Trans$_{it-\tau,4000}$	# treated
Is Divorced	-0.0729	-0.0686	-0.0606	-0.0546	
	(0.0969)	(0.1003)	(0.0948)	(0.0970)	
Is Widowed	-0.7476**	-0.7395*	-0.7347*	-0.7428*	
	(0.3327)	(0.3314)	(0.3292)	(0.3372)	
Has Very Good Health	0.2839***	0.2839***	0.2842***	0.2834***	
	(0.0537)	(0.0543)	(0.0539)	(0.0539)	
Has Very Bad Health	-1.2847***	-1.284***	-1.2884***	-1.2901***	
	(0.0891)	(0.0897)	(0.0895)	(0.0862)	
Is Disabled	-0.0099	-0.0110	-0.0113	-0.0037	
	(0.0874)	(0.0874)	(0.0863)	(0.0911)	
Has Migration Background					
Has Tertiary Degree	-0.0253	-0.0214	-0.0218	-0.0495	
	(0.2624)	(0.2616)	(0.2620)	(0.2641)	
Has Lower Than Secondary Degree	0.1702	0.1709	0.1672	0.1619	
	(0.2083)	(0.2090)	(0.2078)	(0.2104)	
Is in Education	0.1693	0.1695	0.1696	0.1811	
	(0.2552)	(0.2554)	(0.2078)	(0.2554)	
Is Full-Time Employed	0.0203	0.0206	0.0187	0.0273	
	(0.0776)	(0.0770)	(0.0777)	(0.0803)	
Is Part-Time Employed	-0.0544	-0.0537	-0.0541	-0.0492	
	(0.0905)	(0.0910)	(0.0913)	(0.0920)	
Is on Parental Leave	-0.0255	0.1514	-0.0219	-0.0315	
	(0.2121)	(0.2139)	(0.2114)	(0.2087)	
Is Unemployed	-0.4343***	-0.4450***	-0.4360***	-0.4321***	
	(0.0878)	(0.0883)	(0.0881)	(0.0882)	
Log Monthly Net Individual Income[a]	0.0526	0.0529	0.0527	0.0519	
	(0.0434)	(0.0432)	(0.0434)	(0.0441)	
Has Child in Household	0.1969***	0.1958***	0.1951***	0.1958***	
	(0.0525)	(0.0525)	(0.0519)	(0.0551)	
Log Annual Net Household Income[a]	0.2497***	0.2506***	0.2523***	0.2492***	
	(0.0702)	(0.0698)	(0.0700)	(0.0709)	
Lives in House[b]	0.0056	0.0049	0.0057	0.0052	
	(0.0483)	(0.0482)	(0.0486)	(0.0481)	

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Regressors	Intensity $\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	Transition $\text{Trans}_{it-\tau,4000}$	# treated
Lives in Small Apartment Building	0.0229	0.0220	0.0229	0.0225	
	(0.0575)	(0.0575)	(0.0576)	(0.0569)	
Lives in Large Apartment Building	-0.0062	-0.0068	-0.0060	-0.0066	
	(0.0421)	(0.0422)	(0.0486)	(0.0420)	
Lives in High Rise	0.0919	0.0915	0.0922	0.0947	
	(0.2100)	(0.2101)	(0.2103)	(0.2103)	
Number of Rooms per Individual	-0.0158	-0.0160	-0.0160	-0.0155	
	(0.0402)	(0.0404)	(0.0403)	(0.0401)	
Unemployment Rate	-0.0360***	-0.0362***	-0.0369***	-0.0323**	
	(0.0096)	(0.0097)	(0.0100)	(0.0113)	
Average Monthly Net Household Income[a]	0.0004	0.0004	0.0004	0.0004	
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	
Number of Observations	8,609	8,609	8,609	8,609	
Number of Individuals	1,317	1,317	1,317	1,317	
of which in treatment group	506	506	506		
of which in control group	811	811	811	811	
F-Statistic	10,029.0400	9,702.5400	9,832.3100	10,774.6900	
R^2	0.0711	0.0709	0.0711	0.0725	
Adjusted R^2	0.0704	0.0672	0.0674	0.0680	

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The intensity measures are defined as follows: $\text{InvDist}_{it,4000}$ is the inverse distance, $\text{RevDist}_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, $\text{Cumul}_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4,000 metres, all in interview year t. $\text{Trans}_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$, and zero else. For example, $\text{Trans}_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.17: Results – FE Models, Spatial Matching (15,000m)
Construction$_{it,4000}$× Intensity, Trans$_{it\text{-}\tau,4000}$

Dependent Variable: Satisfaction With Life

Regressors	Intensity InvDist$_{it,4000}$	RevDist$_{it,4000}$	Cumul$_{it,4000}$	Transition Trans$_{it-\tau,4000}$	# treated
Construction$_{\mathrm{it},4000}$ × Intensity	-0.1862*	-0.0181	-0.0174		
	(0.0940)	(0.0338)	(0.0106)		
Transition$_{it-1,4000}$				-0.0392	506
				(0.0642)	
Transition$_{it-2,4000}$				-0.1262**	450
				(0.0697)	
Transition$_{it-3,4000}$				-0.1506**	430
				(0.0609)	
Transition$_{it-4,4000}$				-0.1902*	382
				(0.0917)	
Transition$_{it-5,4000}$				-0.1472	341
				(0.0924)	
Transition$_{it-6,4000}$				-0.1519	291
				(0.1495)	
Transition$_{it-7,4000}$				-0.0744	243
				(0.1271)	
Transition$_{it-8,4000}$				0.2104	207
				(0.1846)	
Transition$_{it-9,4000}$				-0.0778	170
				(0.2816)	
Age	-0.0158	-0.0176	-0.0156	-0.0146	
	(0.0204)	(0.0207)	(0.0202)	(0.0193)	
Age Squared	-0.0001	-0.0001	-0.0001	-0.0001	
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	
Is Female					
Is Married	0.1184	0.1217	0.1231	0.1194	
	(0.2084)	(0.2069)	(0.2088)	(0.2104)	

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Regressors	Intensity $\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	Transition $\text{Trans}_{it-\tau,4000}$	# treated
Is Divorced	0.1241	0.1262	0.1298	0.1356	
	(0.2309)	(0.2069)	(0.2305)	(0.2302)	
Is Widowed	-0.2560	-0.2547	-0.2532	-0.2566	
	(0.2503)	(0.2486)	(0.2498)	(0.2524)	
Has Very Good Health	0.3675***	0.3673***	0.3675***	0.3673***	
	(0.0426)	(0.0428)	(0.0425)	(0.0423)	
Has Very Bad Health	-1.2137***	-1.2141***	-1.2161***	-1.216***	
	(0.1001)	(0.1002)	(0.1001)	(0.0991)	
Is Disabled	-0.2078**	-0.2083**	-0.2086**	-0.2042**	
	(0.0687)	(0.0686)	(0.0687)	(0.0715)	
Has Migration Background					
Has Tertiary Degree	-0.1954	-0.1934	-0.1934	-0.2098	
	(0.1668)	(0.1674)	(0.1673)	(0.1681)	
Has Lower Than Secondary Degree	0.2284	0.2286	0.2266	0.2234	
	(0.2061)	(0.2062)	(0.2061)	(0.2076)	
Is in Education	0.3323	0.3327	0.3327	0.3395	
	(0.2027)	(0.2025)	(0.2036)	(0.2021)	
Is Full-Time Employed	0.0833	0.0830	0.0822	0.0873	
	(0.0656)	(0.0657)	(0.0659)	(0.0650)	
Is Part-Time Employed	-0.0434	-0.0434	-0.0431	-0.0408	
	(0.0642)	(0.0643)	(0.0647)	(0.0640)	
Is on Parental Leave	0.1517	0.1514	0.1525	0.1525	
	(0.1291)	(0.1293)	(0.1294)	(0.1299)	
Is Unemployed	-0.4554***	-0.4562***	-0.4565***	-0.4542***	
	(0.0774)	(0.0773)	(0.0774)	(0.0766)	
Log Monthly Net Individual Income[a]	0.0386	0.0388	0.0386	0.0383	
	(0.0281)	(0.0281)	(0.0282)	(0.0280)	
Has Child in Household	0.0881**	0.0875**	0.0868**	0.0867**	
	(0.0373)	(0.0374)	(0.0371)	(0.0381)	
Log Annual Net Household Income[a]	0.2002***	0.2009***	0.2021***	0.1994***	
	(0.0541)	(0.0539)	(0.0540)	(0.0538)	
Lives in House[b]	0.0086	0.0083	0.0087	0.0083	
	(0.0415)	(0.0417)	(0.0417)	(0.0412)	

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Regressors	Intensity InvDist$_{it,4000}$	RevDist$_{it,4000}$	Cumul$_{it,4000}$	Transition Trans$_{it-\tau,4000}$	# treated
Lives in Small Apartment Building	0.0157	0.0153	0.0158	0.0153	
	(0.0397)	(0.0398)	(0.0396)	(0.0394)	
Lives in Large Apartment Building	0.0144	0.0141	0.0146	0.0140	
	(0.0301)	(0.0304)	(0.0302)	(0.0297)	
Lives in High Rise	0.0715	0.0710	0.0716	0.0732	
	(0.1780)	(0.1795)	(0.1798)	(0.1808)	
Number of Rooms per Individual	0.0135	0.0133	0.0134	0.0138	
	(0.0210)	(0.0211)	(0.0211)	(0.0211)	
Unemployment Rate	-0.0083	-0.0082	-0.0089	-0.0059	
	(0.0100)	(0.0098)	(0.0098)	(0.0112)	
Average Monthly Net Household Income[a]	-0.0006	-0.0006	-0.0006	-0.0006	
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	
Number of Observations	16,378	16,378	16,378	16,378	
Number of Individuals	2,586	2,586	2,586	2,586	
of which in treatment group	506	506	506		
of which in control group	2,080	2,080	2,080	2,080	
F-Statistic	4,299.3200	4,088.2000	5,747.9200	8,860.9700	
R^2	0.0650	0.0650	0.0649	0.0659	
Adjusted R^2	0.0630	0.0629	0.0630	0.0635	

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: Construction$_{\mathrm{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The intensity measures are defined as follows: InvDist$_{it,4000}$ is the inverse distance, RevDist$_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, Cumul$_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4,000 metres, all in interview year t. Trans$_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$, and zero else. For example, Trans$_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.18: Results – Sub-Samples, FE Models, Spatial Matching ($15,000m$) $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{Construction_{it,4000}}$	-0.1261**	-0.0937	-0.0711	-0.1356**	0.0634	-0.2127***
	(0.0488)	(0.1132)	(0.0686)	(0.0436)	(0.0499)	(0.0605)
Age	-0.0188	0.0025	-0.1069**	0.0043	-0.0388	-0.0004
	(0.0166)	(0.0446)	(0.0410)	(0.0259)	(0.0270)	(0.0332)
Age Squared	-0.0001	0.0001	0.0006**	-0.0003	0.0002	-0.0003
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Is Female						
Is Married	0.0589	0.3851	-0.0522	0.0620	0.3197	-0.0734
	(0.0946)	(0.7317)	(0.1953)	(0.1471)	(0.4429)	(0.1527)
Is Divorced	0.0391	0.4838	-0.5064	0.1950	-0.0679	0.2127
	(0.2112)	(0.6903)	(0.8270)	(0.3434)	(0.4314)	(0.2987)
Is Widowed	-0.5247*	0.0895	-0.9141	-0.2729	-0.4955	-0.3157
	(0.2652)	(0.7342)	(0.7701)	(0.1820)	(0.8712)	(0.2506)
Has Very Good Health	0.3674***	0.3737**	0.4583***	0.3490***	0.3639***	0.3686***
	(0.0503)	(0.1615)	(0.1345)	(0.0449)	(0.0636)	(0.0658)
Has Very Bad Health	-1.3017***	-1.0011***	-1.1366***	-1.2267***	-1.3264***	-1.1695***
	(0.1269)	(0.1538)	(0.2749)	(0.1051)	(0.1891)	(0.0952)
Is Disabled	-0.1545	-0.3634*	-0.3932	-0.1647	-0.3259***	-0.1430
	(0.0934)	(0.1811)	(0.2154)	(0.1039)	(0.0691)	(0.1332)
Has Migration Background						
Has Tertiary Degree	-0.2054	-0.3403	-0.4993*	-0.0646	-0.2762	-0.1930
	(0.1951)	(0.2783)	(0.2485)	(0.1469)	(0.3597)	(0.1417)
Has Lower Than Secondary Degree	0.3635*	-0.3660	0.6399	0.2814	-0.1533	0.4471*
	(0.1882)	(0.3417)	(1.0752)	(0.1900)	(0.3664)	(0.2403)
Is in Education	0.1265	1.0588**	0.6272	0.3490*	0.3120	0.3212
	(0.1735)	(0.3595)	(0.5650)	(0.1690)	(0.2717)	(0.2403)
Is Full-Time Employed	-0.0462	0.6159***	0.1730	0.1174*	0.0846	0.0753
	(0.0871)	(0.0913)	(0.1622)	(0.0620)	(0.1230)	(0.0699)

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Dependent Variable: Satisfaction With Life

Regressors	(1)	(2)	(3)	(4)	(5)	(6)
Is Part-Time Employed	-0.0561	0.0547	-0.0196	-0.0034	-0.1111	0.0057
	(0.0602)	(0.1327)	(0.1663)	(0.0853)	(0.1104)	(0.0932)
Is on Parental Leave	0.1815	0.2686	0.1355	0.1546	0.0187	0.2277*
	(0.1016)	(0.4238)	(0.2755)	(0.1321)	(0.2173)	(0.1239)
Is Unemployed	-0.4953***	-0.2808*	-0.3720	-0.4486***	-0.4415**	-0.4850***
	(0.1131)	(0.1304)	(0.2070)	(0.0720)	(0.1523)	(0.1133)
Log Monthly Net Individual Income[a]	0.0693	-0.0393	0.0789	0.0094	0.0771	0.0149
	(0.0399)	(0.0767)	(0.0890)	(0.0331)	(0.0541)	(0.0380)
Has Child in Household	0.1105*	-0.0186	0.1073	0.1133**	0.0124	0.1367**
	(0.0555)	(0.1371)	(0.1434)	(0.0477)	(0.0738)	(0.0509)
Log Annual Net Household Income[a]	0.2405***	0.1759	0.0596	0.2240***	0.3090***	0.1357**
	(0.0645)	(0.1271)	(0.0938)	(0.0599)	(0.0905)	(0.0439)
Lives in House[c]	-0.0099	0.0679	-0.0006	0.0145	-0.0116	0.0175
	(0.0455)	(0.0678)	(0.0807)	(0.0594)	(0.0497)	(0.0602)
Lives in Small Apartment Building	-0.0011	0.0506	-0.0312	0.0232	0.0047	0.0204
	(0.0521)	(0.0871)	(0.0898)	(0.0522)	(0.0741)	(0.0518)
Lives in Large Apartment Building	-0.0091	0.0335	-0.0251	0.0277	-0.0076	0.0262
	(0.0310)	(0.0816)	(0.0873)	(0.0460)	(0.0682)	(0.0515)
Lives in High Rise	0.0597	0.1164	0.2536	0.0279	0.0481	0.0819
	(0.1908)	(0.3136)	(0.3930)	(0.1849)	(0.3097)	(0.1575)
Number of Rooms per Individual	0.0216	0.0104	-0.0228	0.0132	-0.0330	0.0302
	(0.0229)	(0.0493)	(0.0697)	(0.0231)	(0.0505)	(0.0333)
Unemployment Rate	-0.0081	-0.0178	-0.0259	-0.0102	-0.0113	-0.0037
	(0.00149)	(0.0155)	(0.0360)	(0.0155)	(0.0163)	(0.0104)
Average Monthly Net Household Income[a]	-0.0003	-0.0019	-0.0004	-0.0007	-0.0011**	-0.0002
	(0.0005)	(0.0012)	(0.0012)	(0.0006)	(0.0004)	(0.0007)
Number of Observations	12,570	3,808	3,934	12,350	5,469	10,909
Number of Individuals	2,047	700	1,380	2,400	722	1,864
of which in treatment group	388	155	308	488	148	358
of which in control group	1,659	545	1,072	1,912	587	1,506
F-Statistic	3,393.8100	1,464.5000	1,796.3600	25,074.9900	2,300.6900	4,097.3100

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Dependent Variable: Satisfaction With Life

Regressors	(1)	(2)	(3)	(4)	(5)	(6)
R^2	0.0660	0.0816	0.0749	0.0662	0.0728	0.0679
Adjusted R^2	0.0635	0.0733	0.0668	0.0636	0.0669	0.0650

[a] In Euro/Inflation-Adjusted (Base Year 2000), [b] Detached, Semi-Detached, or Terraced

(1) House-owner subsample, (2) Non-house-owner subsample, (3) Worries environment high, (4) Worries environment not high, (5) Worries climate change high, (6) Worries climate change not high

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

3.8.3.1 Additional Results

Table 3.19: Results – FE Models, Combining Propensity-Score (PS) With Spatial (S) Matching $Construction_{it,4000}$

Dependent Variable: Satisfaction With Life

Regressors	PS + S $(10,000m)$	PS + S $(15,000m)$
$\text{Construction}_{\text{it,4000}}$	-0.1136**	-0.1173**
	(0.0453)	(0.0476)
Micro Controls	yes	yes
Macro Controls	yes	yes
Number of Observations	4,812	5,731
Number of Individuals	631	774
of which in treatment group	498	498
of which in control group	133	276
Adjusted R^2	0.0405	0.0412

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.20: Robustness (Residential Sorting: Linear Probability Models) – FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Construction_{it,4000}$

Dependent Variable: Moving

Regressors	PS	S $(10,000m)$	S $(15,000m)$
$\mathrm{Construction}_{\mathrm{it},4000}$	-0.0072	-0.0060	-0.0051
	(0.0069)	(0.0061)	(0.0054)
Micro Controls	yes	yes	yes
Macro Controls	yes	yes	yes
Number of Observations	6,613	8,571	16,316
Number of Individuals	978	1,313	2,580
of which in treatment group	498	506	506
of which in control group	480	807	2,074
Adjusted R^2	0.0102	0.0097	0.0046

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\mathrm{Construction}_{\mathrm{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is a dummy variable that is equal to one in the time period in which an individual moves, and zero else; thus, we are estimating linear probability models here. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.21: Hedonic Analysis – Propensity-Score (PS) and Spatial (S) Matching, $Construction_{dt,4000}$

Dependent Variable: Log Annual Net Rent

Regressors	PS	S $(10,000m)$	S $(15,000m)$
$\text{Construction}_{\text{dt,4000}}$	0.0034	-0.0421**	-0.0437**
	(0.0236)	(0.0199)	(0.0186)
Dwelling Controls	yes	yes	yes
Amenities Controls	yes	yes	yes
State-Specific Linear Time Trends	yes	yes	yes
Number of Observations	1,503	1,615	3,167
Number of Individuals	261	282	563
of which in treatment group	126	128	128
of which in control group	135	154	435
Adjusted R^2	0.1204	0.1966	0.1852

Robust standard errors clustered at the county times year level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{dt,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is the log annual net rent. The dwelling controls include dummy variables for whether an individual lives in a detached, semi-detached, or terraced house, a small apartment building, a large apartment building, or a high rise, as well as for the number of rooms per individual. The amenities controls include dummy variables for whether the dwelling has a kitchen, an indoor bath or shower, an indoor toilet, central or floor heating, a balcony or terrace, a basement, a garden, or a boiler. All regressions include dummy variables for interview years and a constant. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.22: Robustness (Alternative Matching Procedure: Matching on First Observation) – FE Model, Propensity-Score (PS) Matching, $Construction_{it,4000}$

Dependent Variable: Life Satisfaction

Regressors	PS
$\text{Construction}_{\text{it,4000}}$	-0.0779**
	(0.0323)
Micro Controls	yes
Macro Controls	yes
Number of Observations	6,060
Number of Individuals	988
of which in treatment group	498
of which in control group	490
Adjusted R^2	0.0710

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{Construction}_{\text{it,4000}}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t, and zero else. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own calculations.

Table 3.23: Results – Robustness (View Shed Analysis: Treatment Intensity), FE Models, Propensity-Score (PS) and Spatial (S) Matching

Dependent Variable: Satisfaction With Life

	PS			S ($15,000m$)		
Regressors\Intensity Measure	$\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$	$\text{InvDist}_{it,4000}$	$\text{RevDist}_{it,4000}$	$\text{Cumul}_{it,4000}$
$\text{ConstructionVisible}_{\text{it},4000}$ × Intensity	-0.2075	-0.0168	-0.0173	-0.1739	-0.0178	-0.0158
	(0.1685)	(0.0564)	(0.0156)	(0.0970)	(0.0346)	(0.0103)
Micro Controls	yes	yes	yes	yes	yes	yes
Macro Controls	yes	yes	yes	yes	yes	yes
Number of Observations	6,273	6,273	6,273	16,013	16,013	16,013
Number of Individuals	939	939	939	2,538	2,538	2,538
of which in treatment group	451	451	451	458	458	458
of which in control group	488	488	488	2,080	2,080	2,080
Adjusted R^2	0.0617	0.0613	0.0616	0.0613	0.0612	0.0613

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{ConstructionVisible}_{\text{it},4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4,000 metres in interview year t and the household has a direct view on it, and zero else. The intensity measures are defined as follows: $\text{InvDist}_{it,4000}$ is the inverse distance, $\text{RevDist}_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, $\text{Cumul}_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4,000 metres, all in interview year t. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

Table 3.24: Results – Robustness (View Shed Analysis: Treatment Persistence), FE Models, Propensity-Score (PS) and Spatial (S) Matching

Dependent Variable: Satisfaction With Life

	PS		S $(15,000m)$	
Regressors\Transitoriness Measure	$\text{Trans}_{it-\tau,4000}$	# treated	$\text{Trans}_{it-\tau,4000}$	# treated
$\text{TransVisible}_{it-1,4000}$	-0.0496	451	-0.0330	458
	(0.0686)		(0.0534)	
$\text{TransVisible}_{it-2,4000}$	-0.1492*	401	-0.1106*	406
	(0.0716)		(0.0501)	
$\text{TransVisible}_{it-3,4000}$	-0.1910***	384	-0.1468**	389
	(0.0563)		(0.0568)	
$\text{TransVisible}_{it-4,4000}$	-0.2525**	339	-0.2128*	344
	(0.1002)		(0.1009)	
$\text{TransVisible}_{it-5,4000}$	-0.2309**	303	-0.1505	308
	(0.0996)		(0.0942)	
$\text{TransVisible}_{it-6,4000}$	-0.2466	258	-0.1226	260
	(0.1457)		(0.1419)	
$\text{TransVisible}_{it-7,4000}$	-0.2001	215	-0.0473	217
	(0.1421)		(0.1177)	
$\text{TransVisible}_{it-8,4000}$	0.0268	180	0.2198	182
	(0.1882)		(0.1604)	
$\text{TransVisible}_{it-9,4000}$	-0.0839	151	0.0682	153
	(0.3192)		(0.2780)	
Micro Controls	yes		yes	
Macro Controls	yes		yes	
Number of Observations	6,273		16,013	
Number of Individuals	939		2,538	
of which in control group	488		2,080	
Adjusted R^2	0.0625		0.0618	

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: $\text{TransVisible}_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$ and the household has a direct view on it, and zero else. For example, $\text{TransVisible}_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. The controls include age, age squared, being female, being married, being divorced, being widowed, having very good health, having very bad health, being disabled, having migration background, having a tertiary degree, having less than a secondary degree, being in education, being full-time employed, being part-time employed, being on parental leave, being unemployed, income, having a child in the household, living in a house, living in a small apartment building, living in a large apartment building, living in a high rise, the number of rooms per individual, the unemployment rate in the county, and the average household income in the county. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 3.1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places.

Source: SOEP v29, 2000–2012, individuals aged 17 or above, sources in Online 3.8.3.2, own tabulations.

3.8.3.2 Details on Data Sources for Wind Turbines and Data Protection

Data for several wind turbines is taken from the renewables installations master data (EEG Anlagenstammdaten) for Germany, which the German transmission system operators (TSOs) are obliged to publish. This dataset collects all renewables installations which are subject to the Renewable Energy Act support scheme. However, it comprises geographical coordinates only for a small number of installations. Sources:

TSO: 50Hertz Transmission
http://www.50hertz.com/de/EEG/Veroeffentlichung-EEG-Daten/EEG-Anlagenstammdaten (in German), accessed June 1, 2015.
TSO: Amprion
http://www.amprion.net/eeg-anlagenstammdaten-aktuell (in German), accessed June 1, 2015.
TSO: TenneT TSO
http://www.tennet.eu/de/kunden/eegkwk-g/erneuerbare-energien-gesetz/eeg-daten-nach-52.html (in German), accessed June 1, 2015.

For geographical information, we largely rely on data by State offices for the environment of the German federal states and counties, which we report on state or county (*Landkreis*) level in the following. If a German disclaimer applies, we provide the original text and an own translation. An asterisk indicates freely accessible sources; all other data were retrieved on request and may be subject to particular non-disclosure requirements.

Baden-Württemberg*:
Basis: data from the spatial information and planning system (RIPS) of the State Office for the Environment, Land Surveying, and Nature Conservation Baden-Württemberg (LUBW). [Grundlage: Daten aus dem Räumlichen Informations- und Planungssystem (RIPS) der Landesanstalt für Umwelt, Messungen und Naturschutz Baden-Württemberg (LUBW)]
http://udo.lubw.baden-wuerttemberg.de/public/pages/home/welcome.xhtml (in German), accessed June 1, 2015.

Berlin*:
NEB Neue Energie Berlin GmbH & Co. KG. http://www.windenergie-berlin.de/index.htm (in German), accessed June 1, 2015. Coordinates retrieved via Open Street Maps.

Brandenburg:

State Office for the Environment, Public Health, and Consumer Protection Brandenburg (Landesamt für Umwelt, Gesundheit und Verbraucherschutz Brandenburg)

Bremen:
Senator for the Environment, Construction and Transportation

Hamburg:
Office for Urban Development and the Environment

Hesse:
Data source: Hessian State Information System Installations (LIS-A) – Hessian Ministry for the Environment, Energy, Agriculture, and Consumer Protection (Datengrundlage: Hessisches Länderinformations-system Anlagen (LIS-A) - Hessisches Ministerium für Umwelt, Energie, Landwirtschaft und Verbraucherschutz)

Lower Saxony:
Administrative district Ammerland: Construction Office
Administrative district Aurich: Office for Construction and Nature Conservation
Administration Union Greater Braunschweig (Zweckverband Großraum Braunschweig)
Administrative district Cloppenburg
City of Delmenhorst: Municipal Utilities Delmenhorst
Administrative district Harburg: Administrative Department for District and Business Development
Administrative district Holzminden
Administrative district Lüchow-Dannenberg: Office for Construction, Immission Control, and Monument Preservation
Administrative district Oldenburg
City of Osnabrück: Office for the Environment and Climate Protection
Administrative district Osterholz: Construction Office
Administrative district Osterode: Energieportal (energy gateway)
Administrative district Peine
Administrative district Stade: Office for Construction and Immission Protection
Administrative district Vechta: Office for Planning, the Environment, and Construction

Mecklenburg-Vorpommern*:
State Office for the Environment, Nature Conservation, and Geology (Landesamt für Umwelt, Naturschutz und Geologie). `http://www.umweltkarten.mv-regierung.de/atlas/script/`

`index.php` (in German), accessed June 1, 2015.

North Rhine-Westphalia:

State Office for Nature Conservation, the Environment, and Consumer Protection NRW (Landesamt für Natur, Umwelt und Verbraucherschutz NRW)

Rhineland-Palatinate:

Ministry for Economic Affairs, Climate Protection, Energy, and State Planning Rhineland-Palatinate (Ministerium für Wirtschaft, Klimaschutz, Energie und Landesplanung Rheinland-Pfalz)

Saarland:

State Office for Land Surveying, Geographical Information, and Regional Development (Landesamt für Vermessung, Geoinformation und Landentwicklung)

Saxony:

Saxon Energy Agency – SAENA GmbH (Sächsische Energieagentur – SAENA GmbH)

Saxony-Anhalt:

State Administration Office Saxony-Anhalt (Landesverwaltungsamt Sachsen-Anhalt)

Schleswig-Holstein:

State Office for Agriculture, the Environment and Rural Areas (Landesamt für Landwirtschaft, Umwelt und ländliche Räume Schleswig Holstein)

Thuringia:

Thuringian State Administration Office (Thüringer Landesverwaltungsamt),
Thüringer Energienetze*
`www.thueringer-energienetze.com/Kunden/Netzinformationen/Regenerative_Energien.aspx` (in German), accessed June 1, 2015.

Chapter 4

The Olympic Games

Abstract

We show that hosting the Olympic Games in 2012 had a positive impact on the life satisfaction and happiness of Londoners during the Games, compared to residents of Paris and Berlin. Notwithstanding issues of causal inference, the magnitude of the effects is equivalent to moving from the bottom to the fourth income decile. But they do not last very long: the effects are gone within a year. These conclusions are based on a novel panel survey of initially about 26,000 individuals with 49,600 observations who were interviewed during the summers of 2011, 2012, and 2013, i.e. before, during, and after the event. The results are robust to controlling for a rich set of observables, including macroeconomic characteristics in each country, to using the exact cut-off dates of the event, to selection into the follow-up survey, and to the number of medals won.*

∗. This chapter is also available as the following discussion paper: Dolan, P. H., G. Kavetsos, C. Krekel, D. Mavridis, R. Metcalfe, C. Senik, S. Szymanski, and N. R. Ziebarth, "The Host with the Most? The Effects of the Olympic Games on Happiness," *CEP Discussion Paper*, 1441, 2016.

4.1 Introduction

Can large scale events, such as the Olympic Games, make people happier? The original Olympic Games were staged every four years in Olympia in Ancient Greece as a religious and athletic festival from around the 8th century BC until 393AD.[105] Centuries later, Baron Pierre de Coubertin created a committee to restart the Olympic Games, and the first modern Olympiad was celebrated in Athens in 1896. The Games in Rio de Janeiro are the 28^{th} summer Games in the modern period, and there have been 22 winter Games. From the outset, the International Olympic Committee (IOC) has invited cities around the world to act as hosts of the event.

Until the 1960s, the Olympics were relatively modest affairs with limited finance and investment. The television era of watching sport, combined with the capacity to reach a global audience, however, has enhanced the prestige of the event. This has encouraged fierce competition amongst cities to host the Games, and resulted in a significant rise in expenditure on staging the event. The 1956 Summer Olympics in Melbourne cost approximately $63 million (in 2016 prices), including construction costs.[106] In contrast, the 2012 Summer Olympics in London required government subsidies of $15 billion alone to cover the direct costs (National Audit Office 2012).[107]

Given the public interest in the Olympics and the large public subsidies that they now require, a significant academic literature has sought to measure the economic impact of the Games. Much of this literature is devoted to rebutting the claim (often made by economic consultancies on behalf of government officials in order to justify public subsidies) that the Olympics generate substantial multiplier effects by stimulating investment and tourism. Most

105. The widely used date for the first Olympic Games is 776 BCE. However, the first known list of champions dates from the fifth century BCE and the method of calculating the date was refined by Aristotle and Eratosthenes about 100 years after that. Other ancient writers disputed this date (Nelson 2009).

106. The Official Report of the Organizing Committee for the Games of the XVI Olympiad, Melbourne (1956: 35-39) reported a total cost of Australian pounds 4.5 million, including 2.4 million of construction expenditures; http://library.la84.org/6oic/OfficialReports/1956/OR1956.pdf.

107. The NAO's post-Games review also cited several additional sources of costs not included in the official budget, including land acquisition, the costs of the legacy program, the costs of government departments and agencies incurred on Olympics-related tasks, and contributions to turning the Olympic Village into affordable housing (National Audit Office 2012, p. 26-27).

academic studies find little evidence of any tangible long-term economic impact.[108] In a recent review, Baade and Matheson (2016, p. 202) state that "*the overwhelming conclusion is that in most cases the Olympics are a money-losing proposition for host cities*".

Given these findings, many proponents of the Games now suggest that one of its main contributions are the intangible impact on the people who host them. The UK government's assessment of the 2012 Summer Olympics in London focused on intangibles such as "inspiring a generation of children and young people", community engagement, and enthusiasm for volunteering (Department for Culture, Media & Sport 2013). There is also evidence that citizens are willing to pay substantial sums to host these events (Atkinson et al. 2008). A national opinion poll conducted immediately after the 2012 Summer Olympics found that 55% of respondents believed that the public expenditure on the Games had been well worth the investment.[109] Arguably, an important part of the value of public expenditure is the legacy effect, i.e. the long-term benefits of the Olympics.[110]

We study the nature and the extent of the hypothesized "intangible" impact of the Olympic Games on the inhabitants of the host city.[111] We also enquire into whether the effects, if any, persist for at least one year after the Olympics. To achieve these aims, we use measures of subjective wellbeing (SWB) that have been developed and tested by economists and psychologists for about two decades in order to assess how people think and feel about their lives. There is

108. This argument has several dimensions. The general economic principles are addressed by Crompton (1995), Porter (2001) and Siegfried and Zimbalist (2000). Computable General Equilibrium modelling has identified negligible or even negative impacts in the cases of London 2012 (Blake 2005) and Sydney 2000 (Giesecke and Madden 2007). Ex post studies of local employment and wages (Baade and Matheson 2002; Coates and Humphreys 1999, 2003) find little evidence of impact related to sports infrastructure in general, while Jasmand and Maennig (2008) find evidence of income growth effects associated with the 1972 Munich Olympics, but no employment effects. Tourism effects of major sporting events such as the Olympics and the FIFA World Cup have been studied by Fourie and Santana-Gallego (2011) who find evidence of significant increases in tourist arrivals prior to the major sporting event but no long-run impact after the event. Teigland (1999) documents the absence of anticipated long-term tourism benefits following the 1994 Lillehammer Winter Olympics. There is some evidence that sports facilities in general and construction associated with the Olympics in particular have a positive effect on property values: on the London Games, see Kavetsos (2012b), and for other examples see Feng and Humphreys (2012), Ahlfeldt and Maennig (2010) and Ahlfeldt and Kavetsos (2014). Billings and Holladay (2012) find no significant effects of hosting the Olympics on GDP per capita. Preuss (2004) offers an economic history of financing and expenditure on the Olympics Games since Munich 1972.

109. "A new Guardian/ICM poll has revealed that 55% of Britons say the Games are "well worth" the investment because they are doing a valuable job in cheering the country during hard times, outnumbering the 35% who regard them as a costly distraction from serious economic problems." The headline to the article reads "London 2012's Team GB success sparks feel good factor" www.theguardian.com/sport/2012/aug/10/london-2012-team-gb-success-feelgood-factor.

110. The concept of "legacy" has become increasingly important in the rationalization and celebration of the Olympic Games, and this was particularly pronounced in the case of London 2012. The Final Report of the IOC Coordination Commission on the Games mentions the word no less than 90 times in its 127-page report. The concept was used in a number of contexts, including leaving a sporting legacy in the UK (increased participation in sport), a legacy for East London (regeneration of a depressed region), volunteering (increased community engagement of the population), growth in tourist arrivals, and increased foreign direct investment (International Olympic Committee Coordination Commission 2013). The legacy issue is clearly important given the large public subsidy devoted to hosting the Olympics.

111. We evaluate the impacts of the Olympics on residential well-being in the host city rather than population well-being in the host country, given that residents in the host city are the relevant policy group, as cities are bidding to host the Olympics rather than countries.

an accumulation of evidence on how to measure SWB, its correlates, and some of its causes.[112] Economists are showing increasing interest in the use of SWB measures, as these might capture a richer array of intangible effects than allowed for by considering stated preferences or preferences revealed through marker behaviors. To make causal inferences, economists typically rely on clear exogenous variation. We consider the choice of the host city a natural experiment, and therefore the basis for our identification strategy.

Accordingly, we designed our own surveys and collected panel data in three European capitals, interviewing 26,000 residents over three years from 2011 to 2013, totaling up to 50,000 individual interviews. This allows us to estimate the intangible impact of the Olympics on citizens in the host city using a difference-in-differences design. Our treatment city is London, which hosted the 2012 Summer Olympics: Paris and Berlin represent our two control cities. We experiment both with pooling Paris and Berlin based on their broad similarity to London, and with treating them differently in recognition of Paris as the 'favorite', but failed, bidder for the 2012 Summer Olympics. As such, Paris could be seen as a negative treatment. Alternatively, it could be seen as the more credible control city as it was second in line to win the bid.[113] In addition to exploiting the choice of the host city as a natural experiment, and in addition to being able to net out unobserved heterogeneity in our panel data, we randomized in all three cities the day when subjects were surveyed, i.e. before, during, or after the precise period of the Games.

Our main result is that the Olympic Games increased happiness among Londoners during the Games, relative to Parisians and Berliners. In terms of potential "legacy" effects, we find that the effect of the Olympic Games is short-lived. Whilst the effects are especially strong around the opening ceremony, we see no lasting change in happiness when we go back to our respondents the following year. These results are robust to controlling for observables, selection into the survey and attrition, and how we chose the counterfactual and the actual timing of the Olympic Games.

Our findings are important for three reasons: first, although the Olympics aim at providing

112. Earlier research defined this account of welfare as 'experienced utility' (see Kahneman et al. 1997). Since then there has been increasing interest among policymakers in using measures of SWB to monitor progress and evaluate policies (e.g. Stiglitz et al. 2009; Her Majesty's Treasury 2003; Dolan and Metcalfe 2012; Organisation for Economic Co-operation and Development 2013; National Research Council 2014). Economists have been interested in using SWB to measure the intangible costs and benefits of policies and events (see Di Tella et al. 2001; Praag and Baarsma 2005; Oswald and Powdthavee 2008; Cattaneo et al. 2009; Luechinger and Raschky 2009; Stevenson and Wolfers 2009; Metcalfe et al. 2011; Ludwig et al. 2012; Bayer and Juessen 2015; Göbel et al. 2015; Eibich et al. 2016; Krekel and Zerrahn 2016; Krekel et al. 2016) and how people's choices link to their SWB (Rayo and Becker 2007; Ifcher and Zarghamee 2011; Benjamin et al. 2012, 2014; Benjamin, Heffetz, Kimball, and Szembrot 2014; Adler et al. 2015; Feddersen et al. 2016). In a study in similar spirit to ours, Kavetsos and Szymanski (2010) examine the cross-sectional impact of sporting impacts on life satisfaction.

113. Another, more technical, reason to include two control cities is that, ex-ante, we could not anticipate whether there may be confounding events taking place in the control city, so that including two of them at the same time serves as having one of them as a potential backup.

amusement, it is not ex-ante clear whether they raise SWB in the host city at all, for example, due to congestion or fear of terrorism. In fact, we observe that anxiety during the summer months of 2012 actually increased in some specifications. Second, it is well established in the literature that hosting the Olympics has negligible tangible impacts on, for example, local economic growth or job creation, neither in the short-run nor in the long-run. Our findings complement these results by focusing on intangible impacts, which, for a complete cost-benefit analysis, have to be taken into account. Finally, given the negligible tangible impacts, potential host cities typically make the case for hosting the Olympics by stressing its intangible impacts, in particular in the long-run, due to legacy effects. We can show that such legacy effects in terms of SWB are non-existent as well.

The paper is organized as follows. The next section describes the data collection in the three cities during the three years and the survey items. Section 4.3 derives the empirical model and identification strategy. Section 4.4 presents the main results. Section 4.5 examines their robustness with respect to selection into surveys, choice of control group, and extended controls. Here, we also conduct a series of placebo tests using both placebo outcomes and time periods. Section 4.6 shows heterogeneous effects with respect to socio-demographics and medals won. Finally, Section 4.7 discusses legacy effects, and Section 4.8 concludes.

4.2 Data

4.2.1 Sample

We use a quasi-experimental design, surveying an overall panel of over 26,000 individuals in London (host), Paris, and Berlin over the summer periods of 2011 (before), 2012 (during), and 2013 (after/legacy). Paris and Berlin were selected as comparable cities because: (a) they are both capital cities, with diversified economies encompassing industry, finance, education, public administration, transport, and tourism; (b) they are all located in North West Europe, and belong to the three largest nations in the region; (c) they have all hosted the Olympic Games before (London in 1908 and 1948, Paris in 1900 and 1924, and Berlin in 1936)[114]; (d) they have all expressed interest in hosting the Olympics in recent years (Berlin bid for the 2000 Games and lost to Sydney, Paris bid for the 2008 Games (losing to Beijing) and for the 2012 Games (which London won)[115]; (e) they are cities of broadly similar size and wealth (for example, a Eurostat survey in 2006 ranked London, Paris, and Berlin respectively 1^{st}, 2^{nd}, and

114. Berlin won the bid to host the 1916 Games but these were canceled due to World War One. London won the bid to host the 1944 Games but these were canceled due to World War Two.

115. At the time of writing Paris is once again bidding to host the Summer Games, now in 2024.

10^{th} among European metropolitan areas).

We survey a panel of individuals in these three cities over three periods: (a) in 2011 (8^{th} August to 30^{th} September), the year before the Games; (b) in 2012 (20^{th} July to 2^{nd} October), the year in which the Games took place (Olympics: 27^{th} July to 12^{th} August; Paralympics: 29^{th} August to 9^{th} September); and (c) in 2013 (23^{rd} July to 12^{th} September), the year after the Games, capturing legacy effects or adaptation processes. Note that the time period of our data collection in 2012 does not coincide with any other major events in the three countries around that time, such as general or local elections.

We employed a mixed methodology approach using a combination of online surveys and telephone interviews. In all cities, each surveyed individual was interviewed using the same mode in all three waves—either online or over the telephone. The online survey made use of the Ipsos Interactive Services Panel (IIS), without imposing any quotas in the first wave. The online sample was released on a rolling weekly basis in order to sustain a good level of response over the duration of a wave. The telephone sample was generated via random digit dialing. Loose quotas (+/- 30%) on age, gender, and work status were set according to the population profile. Despite those quotas being fairly broad, it should be noted that the sample is not representative of the populations of these cities as a whole. In London, the quotas were set according to the London broadband population, while in Paris and Berlin they were set according to the general population. Given the challenges associated with developing and retaining participants within our own three-year panel, participants were incentivized to take part in all three waves of the survey by being automatically included in a random prize draw. Separate prize draws of a monetary sum of £/€500, £/€1,000 and £/€1,500 were offered in each of the three cities and waves, respectively.

4.2.2 Subjective Wellbeing Questions

The survey, specifically designed for this study, contains three different types of measures of individual SWB: (1) evaluation (i.e. life satisfaction); (2) experiences (both happiness and anxiety yesterday); and (3) eudemonia (i.e. sense of worthwhileness). To date, the SWB literature has focused on high-level evaluative measures of SWB, such as life satisfaction (Dolan et al. 2008), mainly due to data availability in large-scale surveys. Experience measures (happiness, anxiety, etc.) are close to the measure of experienced utility discussed by Kahneman et al. (1997) and Bentham's utilitarianism. Evaluation is closer to decision-utility, and is not the same as experienced utility for many reasons (Kahneman and Deaton 2010; Dolan and Metcalfe 2012). Some philosophers, dating back to Aristotle, argue that eudemonia (e.g., worthwhile activities and purpose in life) is the most important element of happiness. If we are to confidently show whether or not the Games have an effect on SWB, we need to tap into SWB in these various ways.

Following Dolan and Metcalfe (2012), whose recommendations are incorporated by the Office for National Statistics to measure SWB in the UK, and also in the spirit of Stiglitz et al. (2009), Organisation for Economic Co-operation and Development (2013), and National Research Council (2014), we included the following four SWB questions into our surveys:[116]

(a) *Evaluative*: Overall, how satisfied are you with your life nowadays?

(b) *Experience*: Overall, how happy did you feel yesterday?

(c) *Experience*: Overall, how anxious did you feel yesterday?

(d) *Eudemonic*: Overall, how worthwhile are the things that you do in your life?

All responses are on an eleven-point scale, with zero denoting 'not at all' and ten denoting 'completely/very much'.[117]

By employing these evaluative, experience, and eudemonic SWB measures, we remain consistent with the official measures recommended by the Office for National Statistics in the UK – the so-called *ONS-4* – for evaluating the impacts of particular policies and programmes with respect to SWB. There is reason to believe that the impact of the Olympics, due to their amusement factor, is more likely to be measurable on experience rather than evaluative measures. Moreover, given that most individuals may be informed about Olympics-related events through news reporting the evening before the day of the interview, using lagged experience measures, as we do, may be less prone to measurement error.

At this point, we are agnostic about whether the Olympics may have long-run impacts in terms of experience measures. To the extent that the Olympics affect behaviour, which can lead to habit formation, there may well be legacy effects for these measures. So far, however, descriptive evidence suggests that the Olympics did not affect behaviour, at least when it comes to sports-related activities: according to a survey conducted by YouGov on behalf of the company Pro Bono Economics, only 7% of 2,000 respondents said they had been inspired to take up sport by the Olympics (The Guardian 2017).

116. The joint use of these four measures of subjective wellbeing for the purpose of impact evaluation is novel, although some of them, in particular life satisfaction, have been used for this purpose before. In fact, large national household panels like the German Socio-Economic Panel Study (SOEP) (Wagner et al. 2007; Wagner et al. 2008) have started asking respondents about their life satisfaction as early as 1984. In our survey, we use the measurement scale of the SOEP (a 11-point Likert-Scale running from zero to ten).

117. Experimental evidence has shown that zero-to-ten scales of subjective wellbeing measures are more reliable than shorter versions (Kroh 2006).

4.3 Empirical Strategy

4.3.1 Model

To estimate the effect of the Olympic Games on subjective wellbeing, we employ a difference-in-differences (DID) design. Specifically, we employ three different models: the first model looks at the year 2012 only and compares the periods before, during, and after the Olympics in London with those in Paris and Berlin. It is specified in Equation 4.1:

$$\begin{aligned} SWB_i = \beta_0 + \beta_1 London \times OlympicsPeriod + \beta_2 London \times PostOlympicsPeriod + \\ + \beta_3 London + \beta_4 OlympicsPeriod + \beta_5 PostOlympicsPeriod + X_i'\gamma + \phi_d + \epsilon_i \end{aligned} \tag{4.1}$$

where SWB_i is the standardized self-reported subjective wellbeing of individual i; *London* is a time-invariant dummy variable that equals one if the individual was interviewed in London, and zero otherwise; and *OlympicsPeriod* and *PostOlympicsPeriod* are dummy variables that equal one if the individual was interviewed during and after the exact time of the Olympics (within year 2012), respectively, and zero otherwise. The base category is the 2012 pre-Olympics period in Paris and Berlin.

The second model makes use of the panel structure of the data and utilizes both years 2011 and 2012. Netting out time-invariant unobserved heterogeneity, this model compares individual-level *changes* of respondents in London with those in Paris and Berlin. Here, we estimate two types of specifications:

Equation 4.2 takes the entire sampling period in 2012 in London as the treatment period, both before (anticipation), during, and after (adaptation/legacy) the Games. If the main identifying assumption is fulfilled, *London* $\times$ 2012 can be interpreted as the average treatment effect on the treated; or put differently, the average causal effect that the Olympics had on the subjective wellbeing of individuals in the host city.

$$SWB_{it} = \beta_0 + \beta_1 London \times 2012 + \beta_2 2012 + X_{it}'\gamma + \phi_m + \phi_d + \mu_i + \epsilon_{it} \tag{4.2}$$

where SWB_{it} is again the standardized self-reported subjective wellbeing of individual i in year t; *London* is a time-invariant dummy variable that equals one if the individual was interviewed in London, and zero otherwise; and 2012 is a dummy variable that equals one if the individual was interviewed in the year 2012, and zero otherwise.

Equation 4.3 uses the panel structure in the same way as Equation 4.2, but follows Equation

4.1 in dividing the year 2012 into three time periods (before, during, and after the Olympics), each of them interacted with the *London* dummy.

$$\begin{aligned}SWB_{it} = \beta_0 + \beta_1 London \times PreOlympicsPeriod_{2012} + \beta_2 London \times OlympicsPeriod_{2012} + \\ + \beta_3 London \times PostOlympicsPeriod_{2012} + \beta_4 PreOlympicsPeriod + \\ + \beta_5 OlympicsPeriod + \beta_6 PostOlympicsPeriod + X'_{it}\gamma + \phi_m + \phi_d + \epsilon_{it}\end{aligned} \tag{4.3}$$

Note that these specifications pool both Paris and Berlin into a single control group, given our discussion on the broad similarities of these capital cities and our primary interest in estimating the effect of Games on host vs. non-host cities. In our robustness section, we relax this assumption by (a) excluding Paris and considering Berlin as the only control group (as Paris had an inherent interest in hosting the Games), and (b) considering Paris itself as a separate treatment group.

In all models, we control for a rich set of time-varying individual observables, X, that include demographics (age, gender, marital status), human capital characteristics (educational level), and economic conditions (employment status, log annual gross household income, home ownership). To proxy changing economic circumstances in the three cities over time (note that we are only looking at a very short time horizon of three years, and in our baseline specifications, of two years), we include each country's change in quarterly real GDP since the first quarter of 2008—that is, just before the onset of the recent economic crisis—as control. This also accounts for potentially idiosyncratic impacts of the crisis on the three countries. In our robustness section, we go one step further and include additional economic and meteorological controls to further account for divergent economic developments between cities and meteorological conditions, respectively.

By including individual fixed effects, μ_i, we routinely net out individual unobserved heterogeneity. Moreover, we control for both calendar-month and day-of-the-week fixed effects, ϕ_m and ϕ_d, as reports of SWB might differ systematically between different months of the year and different days of the week (Taylor 2006; Kavetsos et al. 2014).[118] Finally, we control for mode of interview (online or phone).[119] Robust standard errors are clustered at the interview date level.

118. Note that in Equation 4.1, we can only control for day-of-the-week fixed effects, as month fixed effects are almost perfectly collinear with the period during and after the Olympics.

119. In some waves/cities we randomized the framing and ordering of the happiness, anxiety and worthwhileness measures. We routinely control for such variations in the respective regressions throughout our analysis.

4.3.2 Identification

Regardless of model, our empirical strategy is based on a difference-in-differences design in which we make relative comparisons of the SWB of Londoners with that of Parisians and Berliners. The main identifying assumption, therefore, is that—controlling for time-varying observables, X, calendar-month and day-of-the-week fixed effects, ϕ_m and ϕ_d, and individual fixed effects, μ_i—in the absence of treatment, the SWB of Londoners would have followed the same trend as the SWB of Parisians and Berliners. As the counterfactual is not observable, the *common trend assumption* is not formally testable. One can, however, provide evidence for the plausibility of this assumption by plotting the development of SWB in all three cities prior to the Olympic Games.

Figure 4.1 shows the development of average SWB by calendar week in the pre-Olympics year 2011.[120] Importantly, given the design of our survey, the SWB developments are shown for the same summer months in 2011 as the ones in which the Olympics take place in 2012. A common time trend seems to be given for all measures. Note that differences in levels between the three cities are of minor importance, as they will be netted out by the city fixed effects.

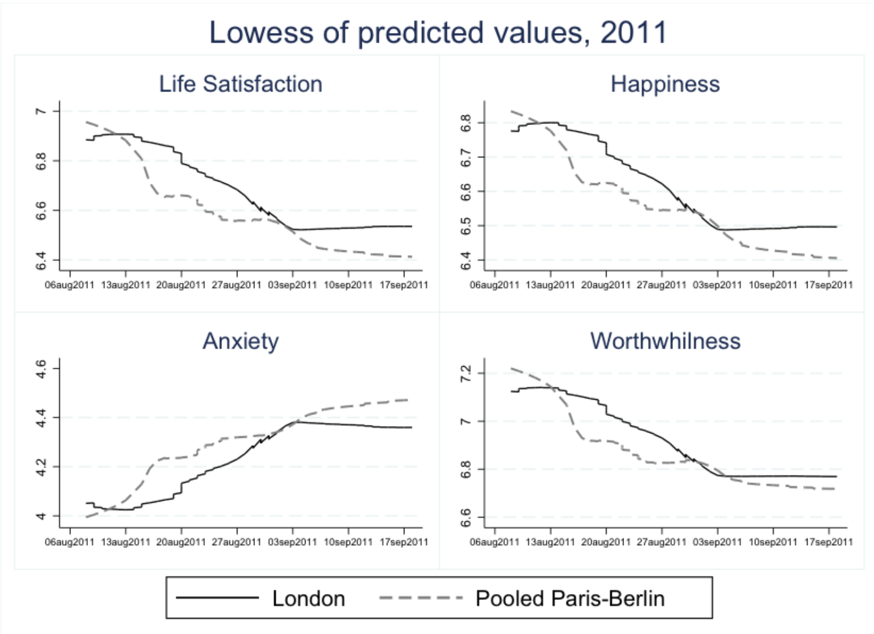


Figure 4.1: SWB in 2011 in London vs. Paris/Berlin

Table 4.20 lists potentially confounding events for our main outcome variables in the UK,

120. In this lowess iterative smoothing for 2011, controls include gender, age, age^2, employment status, education level, marital status, log annual gross household income, home ownership, and a dummy for survey mode. Standard errors clustered at the date level.

France, and Germany in 2012, in the summer months of July, August, and September, that is, during the relevant observation period: as can be seen, there are little confounding events with respect to our subjective well-being indicators. Some sports success in the UK (notably the Tour de France victory of Bradley Wiggins) fall just into the beginning of our observation period, but, arguably, can hardly be the explanation of our identified impacts when using our specification that exploits the exact cut-off dates of the Olympic Games that started about a week later.

4.4 Baseline Results

4.4.1 Descriptive Evidence

In total, our sample contains 50,262 survey responses (London: 17,170; Paris: 19,437; and Berlin: 13,655). Table 4.11 in Section 4.9 offers descriptive statistics of outcomes and covariates by city and wave. As with all panel surveys, panel attrition reduces the number of observations over the three waves. In the first wave, in 2011, 26,142 unique respondents were interviewed in the three cities. A little bit more than half of those respondents, 56% (or 14,838), also participated in the second wave in 2012. Section 4.10 shows and discusses attrition rates.

Given our focus on the 2012 Olympic Games, we start by plotting the SWB measures for 2012. Figure 4.2 shows the fitted daily means for the four SWB measures over the period of the Games in 2012.[121] In all graphs, the first vertical line depicts the day of the opening ceremony (27 July 2012), whereas the second vertical shows the day of the closing ceremony (12 August 2012). For both life satisfaction and happiness, there seems to be a clear jump during the Olympic period in all cities. The impact is most pronounced in the case of London. There also appears to be decline in anxiety and increase in self-reported sense of purpose, although there is no clear difference between London and the other cities.

These effects appear to be strongly associated with the opening and closing ceremonies. All measures of SWB improve in the run up to the opening ceremony and fall off rapidly after the closing ceremony. The opening and closing ceremonies are both the two most watched and the two most expensive events in terms of ticket prices.[122] The apparent return to "normality" after the Olympics are completed is already suggestive of small or missing legacy effects.

121. This is based on a linear regression of SWB measures on the controls, including gender, age, age^2, employment status, education level, marital status, log annual gross household income, home ownership, and a dummy for survey mode. Standard errors clustered at the date level. Figure 4.2 plots the local polynomial estimation of the predicted values for each SWB measure.

122. See www.theguardian.com/media/2012/aug/13/top-olympics-tv-events-ceremonies and http://news.bbc.co.uk/2/shared/bsp/hi/pdfs/15_10_10_athletics.pdf, retrieved August 15, 2015.

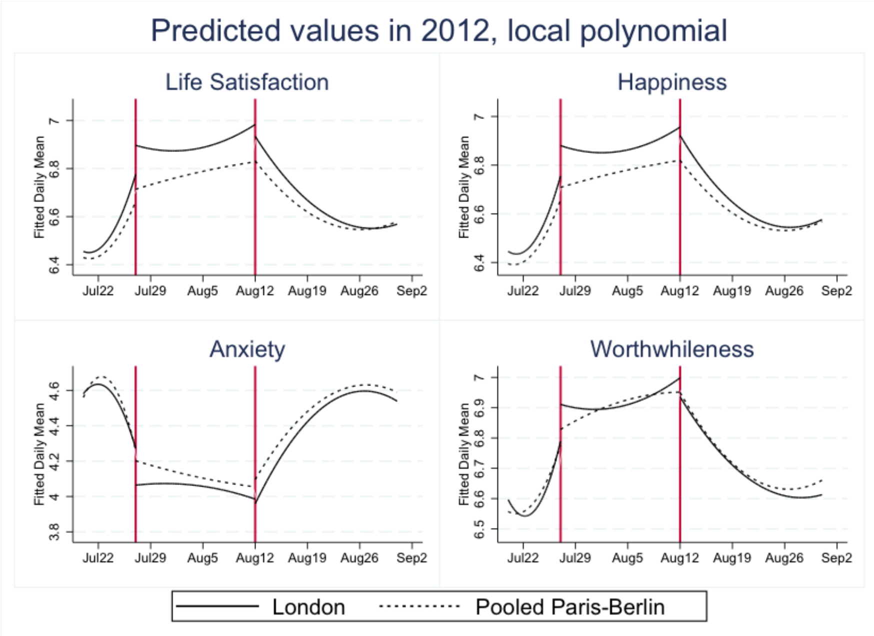


Figure 4.2: SWB in 2012 in London vs. Paris/Berlin

In Figure 4.3, we present graphical evidence based on the mean difference in SWB for each individual who is observed in 2011 and 2012. The change in SWB responses is then averaged by calendar dates in 2012 and plotted.[123] This is the equivalent to the model in Equation 4.2 (it is based on the same regression specification, including controls). Figure 4.3 suggests that the SWB effects of the Olympics are restricted to life satisfaction and happiness and limited to the residents in the host city. Once again we observe a large opening ceremony and closing ceremony effect among Londoners. Here, we do not observe significant impacts on anxiety or sense of purpose.[124] While Figure 4.2 provided suggestive evidence that SWB increased in all three cities during the Olympics, this effect disappears in Figure 4.3 where we focus on individual-level changes.

4.4.2 Regression Results

Table 4.1 shows the regression estimates for Equation 4.1. This model focuses on the year 2012 and differentiates the periods before, during, and after the Games. London is the treatment

123. The mean differences between 2012 and 2011 are calculated as follows. First, the predicted values are obtained for each daily date and city in each year following the same linear regression as used in Figure 4.2, which regresses the SWB measures on the controls, including gender, age, age^2, employment status, education level, marital status, log annual gross household income, home ownership, and a dummy for survey mode. Standard errors clustered at the date level. Second, the mean difference is calculated as the value of the 2012 predicted daily value minus the same daily predicted value in 2011.

124. Figure 4.4 and Figure 4.5 in Section 4.11 plots each city separately. The same broad picture appears.

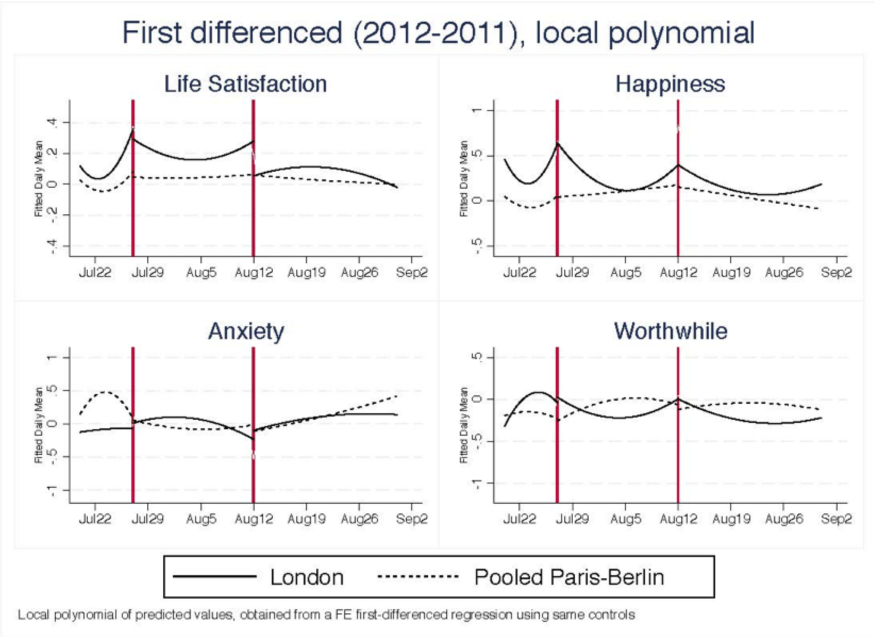


Figure 4.3: Changes in SWB between 2012 and 2011 in London vs. Paris/Berlin

city and responses of Londoners are contrasted with those in Paris and Berlin. This is the regression equivalent of Figure 4.2. We report two separate sets of results—with and without controls—for all four measures of SWB and display the main variables of interest.[125]

The first two columns show that, compared to the pre-Olympics period, life satisfaction increases during the Olympics in London relative to Paris and Berlin, regardless of whether or not we control for covariates. The effect size is 0.117 SDs without controls and 0.088 SDs with controls. We do not find any statistically significant effect for the post-Olympics period, suggesting that there are no immediate legacy effects of the Games as far as life satisfaction is concerned. The evidence for happiness in Columns (3) and (4) is, however, not statistically significant. The measure for anxiety (Columns (5) and (6)) increases during the Olympics, and the effect seems to be considerable: 0.118 SD (Columns (5) and (6)). One could speculate that fear of terror attacks may play a role here. Finally, the results for worthwhileness in the last two columns are small and statistically insignificant. Note, however, that there is a stable and considerable reduction in worthwhileness in the post-Olympics period in London relative to the other cities. This coincides with the fall in life satisfaction after the end of the Games, following the strong increase, and could be interpreted as a "hangover" after this big sports and social event.

Next, we estimate Equation 4.2 which compares individual-level changes between 2011 and

125. Table 4.12 in Section 4.9 includes the full set of controls.

Table 4.1: Impact of Olympics on SWB (2012)

	Life Satisfaction		Happiness		Anxiety		Worthwhile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
London×OlympicsPeriod	0.117**	0.088**	0.079	0.053	0.118**	0.118**	0.025	0.028
	(0.048)	(0.042)	(0.043)	(0.042)	(0.048)	(0.049)	(0.046)	(0.043)
London×PostOlympicsPeriod	0.03	0.053	-0.026	0.001	0.099	0.084	-0.085**	-0.081**
	(0.046)	(0.039)	(0.042)	(0.04)	(0.051)	(0.05)	(0.038)	(0.037)
London	-0.07	0.138**	-0.023	0.002	-0.134***	-0.265***	-0.028	0.521***
	(0.038)	(0.056)	(0.027)	(0.049)	(0.036)	(0.057)	(0.024)	(0.044)
OlympicsPeriod	0.17***	0.148***	0.031	0.023	-0.274***	-0.257***	0.193***	0.166***
	(0.032)	(0.032)	(0.028)	(0.024)	(0.037)	(0.033)	(0.032)	(0.026)
PostOlympicsPeriod	0.063**	0.014	0.057**	0.004	-0.068**	-0.059	0.114***	0.098***
	(0.03)	(0.033)	(0.026)	(0.024)	(0.032)	(0.034)	(0.026)	(0.029)
Constant	-0.088***	-0.009	-0.125***	-0.035	0.186***	0.126**	-0.138***	0.055
	(0.026)	(0.039)	(0.024)	(0.025)	(0.02)	(0.053)	(0.023)	(0.038)
N	14,500	14,500	14,500	14,500	14,500	14,500	14,500	14,500
R^2	0.006	0.10	0.026	0.09	0.011	0.036	0.007	0.067
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses

**** $p<0.01$, ** $p<0.05$*

Note: Estimates for each measure of SWB based on Eq. 4.1, without and with controls. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, and day-of-the-week effects.

2012 for respondents in London with those in Paris and Berlin. The results are shown in Table 4.2.[126] A central finding emerges: in line with Figure 4.3, two of the four SWB measures show a statistically significant and positive effect for London in 2012. The results are almost identical whether or not we include the controls, which reinforces the notion of a quasi-natural experiment and that the covariates are orthogonal to the treatment. Overall, Table 4.2 supports the hypothesis that the Olympics generated a rise in SWB for Londoners in 2012 in terms of the evaluative component (life satisfaction, Columns (1) and (2)) by around 0.07 SDs, an even larger increase in terms of the positive experiential component (happiness, Column (3) and (4)) by around 0.084SDs. In contrast to the finding in Table 4.1 above, when netting out individual-level unobserved heterogeneity, any evidence of a significant anxiety effect due to the Games disappears. Columns (7) and (8) suggest a significant reduction in worthwhileness in London. This is possibly connected to a post-Olympics social "hangover" that we observed in Table 4.1.

We now estimate Equation 4.3 which, as with Equation 4.2, compares individual-level changes between 2011 and 2012 for respondents in London with those in Paris and Berlin, but this time we use the exact cut-off dates for the Olympics in 2012 in order to identify any specific effects related to the exact period during which the Games were staged (i.e. from the opening ceremony to the closing ceremony).

The results are presented in Table 4.3 and show that life satisfaction increased significantly in London, and specifically during the periods of the Olympics (about 0.09 SDs); a significant, yet reduced, effect is also found in London's post-Olympics period (about 0.03 SDs).[127] Self-reported happiness increased in London in all three time periods within the 2012 wave (0.135 and 0.11 SDs in the pre-Olympics and Olympics periods), with again a significant, yet decreased, effect in the Post-Olympics period (0.05 SDs). For anxiety, the results show that this decreased in London in the time leading up to the Games, and increased when these were over. For our measure of purpose, the estimates show a decrease occurring in London in the post-Olympics period.

In a nutshell, our regression analysis therefore suggests two punchlines. First, there was a general increase in SWB for Londoners in 2012 relative to Parisians and Berliners, which may have been associated with the experience of hosting the Games and which encompassed both the pre- and post-Games period. Second, the Games had a positive impact of SWB among Londoners that was specific to the period during which the Games were staged. In other words, there was a general SWB effect in London that can be associated with hosting the Games, and there is evidence that this effect was at its most intense during the staging of the Games. The

126. Table 4.13 in Section 4.9 once again includes the full set of controls.
127. Table 4.13 in Section 4.9 includes the full set of controls.

Table 4.2: Impact of Olympics on SWB (Panel: 2011, 2012)

	Life Satisfaction		Happiness		Anxiety			Worthwhile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
London×2012	0.059***	0.07***	0.086***	0.084***	0.009	0.024	-0.051***	-0.044***
	(0.01)	(0.011)	(0.014)	(0.015)	(0.017)	(0.019)	(0.015)	(0.016)
2012	0.013**	0.005	0.083***	0.043	0.045***	0.022	-0.052***	-0.054***
	(0.006)	(0.013)	(0.017)	(0.022)	(0.009)	(0.015)	(0.008)	(0.013)
Constant	-0.026***	-0.098	-0.129***	-1.228***	-0.014**	0.484	0.015***	-0.409
	(0.003)	(0.30)	(0.021)	(0.448)	(0.005)	(0.428)	(0.003)	(0.336)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.004	0.007
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses

**** $p<0.01$, ** $p<0.05$*

Note: Estimates for each measure of SWB based on Eq. 4.2, without and with controls. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

Table 4.3: Impact of Olympics on SWB (Panel: 2011, 2012) – Exact Cut-Off Dates

	Life Satisfaction		Happiness		Anxiety		Worthwhile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
London×PreOlympicsPeriod$_{2012}$	0.038	0.049	0.142***	0.135***	-0.161***	-0.146***	0.049	0.062
	(0.033)	(0.035)	(0.044)	(0.046)	(0.048)	(0.048)	(0.041)	(0.04)
London×OlympicsPeriod$_{2012}$	0.093***	0.105***	0.107***	0.111***	0.019	0.029	-0.027	-0.019
	(0.016)	(0.017)	(0.024)	(0.023)	(0.027)	(0.028)	(0.021)	(0.021)
London×PostOlympicsPeriod$_{2012}$	0.033**	0.046***	0.059***	0.053***	0.029	0.042**	-0.088***	-0.08***
	(0.013)	(0.014)	(0.017)	(0.018)	(0.019)	(0.02)	(0.02)	(0.021)
PreOlympicsPeriod$_{2012}$	-0.006	-0.003	0.057**	0.018	0.14***	0.089**	-0.112***	-0.076***
	(0.018)	(0.032)	(0.027)	(0.039)	(0.024)	(0.037)	(0.016)	(0.023)
OlympicsPeriod$_{2012}$	0.024**	0.017	0.082***	0.039	-0.009	-0.035**	-0.048***	-0.043**
	(0.01)	(0.017)	(0.018)	(0.022)	(0.012)	(0.018)	(0.012)	(0.017)
PostOlympicsPeriod$_{2012}$	0.007	-0.002	0.065***	0.036	0.071***	0.054***	-0.049***	-0.059***
	(0.008)	(0.013)	(0.02)	(0.024)	(0.012)	(0.017)	(0.011)	(0.014)
Constant	-0.026***	-0.149	-0.114***	-1.284***	-0.014***	0.651	0.015***	-0.481
	(0.003)	(0.294)	(0.022)	(0.447)	(0.005)	(0.435)	(0.003)	(0.325)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.003	0.013	0.008	0.017	0.004	0.009	0.005	0.008
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses

**** $p<0.01$, ** $p<0.05$*

Note: Estimates for each measure of SWB based on Eq. 4.3, without and with controls. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

fact that the results do not differ much between models that control for observables and those that do not reinforces the notion of a quasi-natural experiment. We now test for the robustness of estimates.

4.5 Robustness

4.5.1 Selection into Surveys

One possible concern with our baseline results is that the identified impact of the Olympics might be driven by attrition and/or selection. Note that no question in either wave explicitly asked about the Olympics. Hence, there is no *a priori* reason to believe that respondents in London were primed, selected, or selected themselves into the panel based on a favorable disposition to hosting the Olympics. However, if more positively (or negatively) disposed individuals were more likely to respond in the second wave of the panel, there would be potential for bias.

We check this issue in three ways. First, we estimate Equation 4.2 for a balanced panel. Second, we weigh respondents by the inverse probability of participating in the follow-up survey.[128] Third, we adopt a propensity-score matching (PSM) approach: here, we match respondents in the three cities one-to-one based on their likelihood to participate in the follow-up survey, which we predicted using our standard set of observables.[129] Then, we re-estimate our DID model using only the matched respondents. Using such 'statistical clones' is the most restrictive matching procedure. The results are presented in Table 4.4.[130] When considering the number of observations, there is clearly some overlap between the three approaches.

The results based on the balanced panel (Table 4.4, Panel A) are similar, both in terms of significance and size, to those of the unbalanced panel (Table 4.2). This is also the case for the inverse probability weights (Table 4.4, Panel B). Similarly, for the PSM approach (Table 4.4, Panel C), the contemporaneous effects of the Olympics in London in 2012 remain significantly positive on both life satisfaction and happiness. The sizes of the coefficients, however, are somewhat attenuated. The specifications in Columns (5) and (6) show a significant increase in anxiety in London in 2012, which is the only difference to our baseline specification and the specification using the balanced panel. The fact that we do not find consistent results for anxiety across all specifications, however, suggests that all anxiety interpretations should be

128. To create inverse probability weights, we first predict the likelihood to participate in the follow-up survey based on our standard set of observables, and then weigh all regressions by the inverse of this likelihood (Kalton and Flores-Cervantes 2003).
129. See Table 4.19 in Section 4.10 for the balancing properties of observables after the PSM.
130. For Table 4.4 and all other robustness tests the relevant specification is given by Equation 4.2, i.e. the model that estimates the Olympics effect over the entire 2012 summer period.

Table 4.4: Robustness for Attrition (Panel: 2011, 2012)

	Life Satisfaction		Happiness		Anxiety		Worthwhile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Balanced Panel				
London×2012	0.059***	0.07***	0.086***	0.084***	0.009	0.024	-0.051***	-0.044***
	(0.01)	(0.011)	(0.014)	(0.015)	(0.017)	(0.019)	(0.015)	(0.016)
2012	0.013**	0.005	0.083***	0.043	0.045***	0.022	-0.052***	-0.054***
	(0.006)	(0.013)	(0.017)	(0.022)	(0.009)	(0.015)	(0.008)	(0.013)
Constant	-0.016***	-0.076	-0.119***	-1.253***	-0.022***	0.476	0.013***	-0.414
	(0.004)	(0.308)	(0.021)	(0.463)	(0.008)	(0.439)	(0.005)	(0.346)
N	29,248	29,248	29,248	29,248	29,248	29,248	29,248	29,248
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.004	0.007
N of People	14,820	14,820	14,820	14,820	14,820	14,820	14,820	14,820
Controls	No	Yes	No	Yes	No	Yes	No	Yes
				Panel B: Inverse Probability Weights				
London×2012	0.062***	0.072***	0.07***	0.079***	0.009	0.022	-0.047***	-0.041**
	(0.01)	(0.011)	(0.014)	(0.015)	(0.018)	(0.018)	(0.016)	(0.016)
2012	0.011	-0.001	0.082***	-0.034	0.038***	0.03**	-0.04***	-0.049***
	(0.007)	(0.013)	(0.015)	(0.023)	(0.011)	(0.015)	(0.009)	(0.013)
Constant	-0.036***	-0.167	-0.042	-1.601***	-0.066***	0.594	-0.046***	-0.28
	(0.013)	(0.316)	(0.026)	(0.493)	(0.023)	(0.441)	(0.013)	(0.377)
N	28,956	28,956	28,956	28,956	28,956	28,956	28,956	28,956
R^2	0.003	0.011	0.013	0.017	0.004	0.007	0.005	0.007
N of People	14,528	14,528	14,528	14,528	14,528	14,528	14,528	14,528
Controls	No	Yes	No	Yes	No	Yes	No	Yes
				Panel C: Propensity Score Matching				
London×2012	0.034**	0.051***	0.062***	0.06***	0.046**	0.063***	-0.056***	-0.038***
	(0.013)	(0.014)	(0.015)	(0.014)	(0.03)	(0.023)	(0.018)	(0.019)
2012	0.02***	-0.019	0.085***	0.014	0.02	0.009	-0.054***	-0.091***
	(0.008)	(0.015)	(0.02)	(0.028)	(0.011)	(0.018)	(0.01)	(0.019)
Constant	-0.003	-0.298	-0.20***	-1.694	-0.025***	0.595	0.034***	-1.064
	(0.003)	(0.451)	(0.026)	(0.684)	(0.007)	(0.629)	(0.004)	(0.587)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.004	0.007
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses
**** p<0.01, ** p<0.05*

Note: Regressions are based on Eq. 4.2. Panel A estimates coefficients based on the balanced sample; Panel B weights responses with the inverse probability of participating in wave two of the survey (i.e. 2012); Panel C matches respondents in the three cities one-to-one based on their likelihood to participate in the follow-up survey and estimates Equation 4.2 for those respondents. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

treated with caution.

4.5.2 Choice of Control Group

As mentioned, the city of Paris had bid for 2012 Olympics and in 2005 was favorite to be selected, only to lose to London. Parisians might therefore not be considered an appropriate control group. It is, in fact, possible that the positive life satisfaction and happiness effects identified previously are "contaminated" by a reduction in SWB in Paris. We thus re-estimate Equation 4.2 by (a) excluding Paris and using only the Berlin sample as the control group, and (b) including $Paris \times 2012$ as an additional treatment to $London \times 2012$.

Table 4.5, Panel A, presents the results comparing London to Berlin, excluding the Paris sample. We consistently find significant increases in life satisfaction and happiness in London in 2012, no significant effects for anxiety, but significant reductions in worthwhileness. However, as shown above, the latter finding is very likely due to a post-Olympics reduction in worthwhileness in London. Overall, these results confirm our baseline specification. Notably, for both life satisfaction and happiness, the size of the effect is somewhat reduced compared to the baseline estimates in Tables 4.1 and 4.2. When excluding Paris from the control group, the estimates of life satisfaction nearly halve.

Table 4.5, Panel B, presents the results adding Paris as a separate treatment variable, $Paris \times 2012$. The $London \times 2012$ estimates are very robust and the usual interpretations hold up. Those of $Paris \times 2012$, however, suggest evidence for a significant reduction in life satisfaction and happiness in Paris in 2012. No significant effects of $Paris \times 2012$ are estimated for the measures of anxiety and worthwhileness. Overall, these results suggest that the London Olympics effect is robust to the choice of control group.

4.5.3 Extended Economic and Meteorological Controls

Recall that our regressions control for the quarterly real GDP change since the first quarter of 2008. To further control for potentially divergent economic developments between the three cities, we obtain data on daily stock market index closing values, and include them as additional controls into our preferred specification. For the UK, we take the FTSE100, for France the CAC40, and for Germany the DAX30, all obtained from Yahoo Finance (http://finance.yahoo.com).

Moreover, given that we have daily data, we also control for weather-related factors which have been shown to have an instantaneous effect on subjective wellbeing and could thus explain differences in responses between cities (Feddersen et al. 2016). We obtain data on daily precipitation (in inches) and daily maximum temperature (in Fahrenheit) from the National

Table 4.5: Robustness for Berlin as Control Group (Panel: 2011, 2012)

	Life Satisfaction		Happiness		Anxiety			Worthwhile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: London Treatment				
London×2012	0.033**	0.041***	0.048***	0.071***	0.003	0.015	-0.062***	-0.058***
	(0.013)	(0.015)	(0.018)	(0.02)	(0.018)	(0.019)	(0.019)	(0.019)
2012	0.039***	0.052***	0.122***	0.095***	0.05***	0.033	-0.048***	-0.037**
	(0.01)	(0.017)	(0.025)	(0.025)	(0.015)	(0.021)	(0.011)	(0.017)
Constant	-0.043***	0.169	-0.132***	-1.066	-0.031***	0.322	0.082***	0.076
	(0.003)	(0.40)	(0.032)	(0.583)	(0.007)	(0.535)	(0.004)	(0.427)
N	24,884	24,884	24,884	24,884	24,884	24,884	24,884	24,884
R^2	0.004	0.013	0.011	0.022	0.002	0.011	0.006	0.011
N of People	16,379	16,379	16,379	16,379	16,379	16,379	16,379	16,379
Controls	No	Yes	No	Yes	No	Yes	No	Yes
				Panel B: London and Paris Treatment				
London×2012	0.033**	0.044***	0.048***	0.06***	0.003	0.007	-0.061***	-0.055***
	(0.013)	(0.015)	(0.018)	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)
Paris×2012	-0.043***	-0.033**	-0.062***	-0.031	-0.01	-0.022	-0.018	-0.015
	(0.014)	(0.016)	(0.016)	(0.019)	(0.017)	(0.02)	(0.013)	(0.016)
2012	0.039***	0.039**	0.117***	0.074***	0.05***	0.044**	-0.042***	-0.039***
	(0.01)	(0.015)	(0.021)	(0.024)	(0.015)	(0.019)	(0.01)	(0.015)
Constant	-0.026***	-0.149	-0.088***	-1.274***	-0.014**	0.451	0.015***	-0.432
	(0.003)	(0.297)	(0.021)	(0.447)	(0.005)	(0.422)	(0.003)	(0.335)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.003	0.012	0.008	0.017	0.002	0.007	0.004	0.007
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses

**** $p<0.01$, ** $p<0.05$*

Note: Regressions based on Eq. 4.2. Panel A excludes the Paris sample entirely; Panel B includes the Paris sample as an additional treatment as performed for the case of London. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

Centers for Environmental Information of the National Oceanic and Atmospheric Administration (www.ncdc.noaa.gov). We gather measurements from different weather stations in and around the three cities, and average them to obtain a daily representative measure for each city.

Table 4.6 replicates Table 4.2 including these additional controls. As can be seen, the results remain robust: the coefficients for life satisfaction and happiness have the expected sign, and are very similar in terms of size and significance the ones in our baseline specification. The same is true for worthwhileness.

Table 4.6: Impact of Olympics on SWB (Panel: 2011, 2012) – Additional Controls

	Life Satisfaction	Happiness	Anxiety	Worthwhile
London×2012	0.073***	0.087***	0.023	-0.048***
	(0.011)	(0.015)	(0.018)	(0.016)
2012	0.002	0.039	0.049**	-0.06***
	(0.017)	(0.024)	(0.022)	(0.018)
Constant	-0.20	-1.376***	1.021***	-0.469
	(0.378)	(0.508)	(0.458)	(0.405)
N	40,458	40,458	40,458	40,458
R^2	0.012	0.017	0.007	0.008
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses
**** p<0.01, ** p<0.05*

Note: Estimates for each measure of SWB based on Eq. 4.2 with controls, including: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects. They also include the daily stock market index closing value in each country, as well as the daily amount of rain and the daily maximum temperature in each city.

4.5.4 Placebo Tests

Next, we conduct a series of placebo or confirmation tests. In Table 4.7, Columns (1) to (4) employ *placebo outcomes*, whereas Columns (5) to (7) employ *placebo time periods*. Column (1) replicates Equation 4.2 by using a linear probability model with a binary indicator as outcome that is equal to one if the respondent has thought about her finances the day before; Columns (2) and (3) then use our standard indicators of feelings of happiness and anxiousness the respondent reports to have had when these thoughts occurred, respectively, as outcomes. We would not expect the Olympics to affect these outcomes, and in fact, we do not find any significant effects for them. This is also *prima facie* evidence that our effects are not driven by divergent economic developments between the three cities: if this were the case, we would likely find significant effects for these outcomes.

In considering outcomes plausibly connected to the Olympics, we use a measure of national pride which has previously been related to major sports events (Kavetsos 2012a). We find a strong, significant, and positive effect on this measure (Column 4), which offers supportive evidence that the effects we are measuring in our baseline specification are indeed Olympics-related.

Columns (5) and (6) replicate Equation 4.1—originally focussing on the year 2012 only—by using placebo time periods. We use the Olympics dates in 2012 to define treatment periods in 2011 (Column 5) and 2013 (Column 6), respectively. Both specifications point towards the fact that there is no "Olympics effect" in summer 2011 or 2013. For brevity, we only show results based on the life satisfaction measure; however, similar conclusions also hold for happiness.

Column (7) replicates Equation 4.2 by using the years 2011 and 2013. In this specification we do not find a significant effect, which is again supportive evidence that we are indeed measuring the impact of the Olympics in our original specification of Equation 4.2 comparing 2012 to 2011. Finally, the results of Column (7) are once more evidence against the fact that our main effects are driven by divergent economic developments; if this were the case, significant differences between 2012 and 2011 would likely be present when considering differences between 2013 and 2011.

4.6 Heterogeneity

4.6.1 Socio-Demographics

We first focus on heterogeneous effects based on socio-demographic characteristics (gender, age, income). We follow a similar approach to previous estimations—building on Equation 4.2—and interact the variables of interest with the main *London* $\times$ 2012 treatment indicator.

Table 4.8 reports these heterogeneity estimates. For brevity, we only report the coefficient of the main treatment coefficient (*London* $\times$ 2012) and that of its interaction with the socio-demographic characteristic in question. First, regarding gender (Panel A) and age (Panel B), there do not seem to be any heterogeneous effects. Second, the case of income (Panel C) suggests that the Olympics increased life satisfaction and worthwhileness of wealthier respondents significantly more.[131]

4.6.2 Medals Won

An outstanding question is the degree to which the London treatment variable captures the impact of national athletes' performance or an impact of the Games *per se*. Team Great Britain exceeded expectations in 2012 (even after having done exceptionally well in Beijing in 2008) and was ranked 3^{rd} in the medal table with a total of 65 medals (of which 29 were Gold). Its official target was to be placed 4^{th} with 48 medals.[132] It had ranked 4^{th} (47 medals) in the 2008 Beijing Games and 9th (30 medals) in the 2004 Athens Games. France's and Germany's performance was rather stable: France ranked 7^{th} in 2012 (34 medals; 11 Gold), having ranked 10^{th} in 2008 (41 medals) and 7th in 2004 (33 medals); and Germany 6th in 2012 (44 medals; 11 Gold), having ranked 5^{th} in 2008 (41 medals) and 6^{th} in 2004 (48 medals).

To address the impact of medals won on SWB, we run our baseline specification of Equation 4.2 and additionally interact the main effect with the daily number of medals won by respondents' nation on the day before the interview; i.e. medals won by France for Parisians, by Germany for Berliners, and by Great Britain for Londoners. In other words, we are estimating whether the positive treatment effect for London is amplified by the relative performance of British Athletes on the day before the interview.

Table 4.9 presents the results: Panel A considers all lagged medals irrespective of rank (i.e. gold, silver, and bronze), whereas Panel B considers lagged gold medals only, as these carry more weight in the medal table and attract considerable media attention. Our estimates for the *London* $\times$ 2012 treatment effect are robust to the inclusion of either measure of performance, showing a significant increase in both life satisfaction and happiness in London in 2012. These results continue to hold if we consider lagged (gold) medals accumulated up to the day before an interview took place.[133] This finding confirms previous research which shows, in a large sample of cross-national surveys, a significant hosting effect of major sports events on life satisfaction

131. In the 2013 wave of the online survey, we included additional questions to shed more light on heterogeneous effects. These related to the medium through which individuals in all three cities observed the Olympics (e.g., watching on TV at home; listening to the radio at home, watching/listening on the internet at home; reading the newspaper (online); watching live events on a public screen) and whether respondents in London participated in a Games-related event (e.g., attending a free Olympic event, attending a ticketed event, taking part in Games-related sports/physical activity; taking part in Games-related cultural event/activity; volunteering during the Games; taking part in a Games-related community event/activity). Estimating Equation 4.2-type models and interacting these with the London treatment effect does not significantly alter our main result. We found that those who volunteered during the Games reported higher levels of happiness, although reverse causality might also be at play here. These specific results should be viewed with caution because of attrition of the sample in wave 3; e.g. engaging in these behaviors in 2012 but not being recorded as such in 2013 (see Section 4.7 for further discussion on the 2013 wave).

132. See www.telegraph.co.uk/sport/olympics/9374912/Team-GB-medal-target-for-London-2012-Olympics-is-fourth-place-with-48-medals-across-12-sports.html

133. Table 4.15 in Section 4.9 presents the results of these additional analyses.

Table 4.7: Placebo and Confirmation Tests

	Thoughts about Finances	When Thinking about Finances: Happy	When Thinking about Finances: Anxious	National Pride	Life Satisfaction		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
London×2012	0.022	0.027	-0.046	0.177***			
	(0.02)	(0.022)	(0.026)	(0.017)			
2012	-0.07**	0.064	-0.06	0.008			
	(0.034)	(0.035)	(0.043)	(0.019)			
London×OlympicsPeriod					0.056	0.071	
					(0.05)	(0.06)	
London×PostOlympicsPeriod						0.034	
						(0.061)	
London					-0.135***	-0.147***	
					(0.025)	(0.055)	
OlympicsPeriods					0.145***	0.024	
					(0.034)	(0.038)	
PostOlympicsPeriods						-0.029	
						(0.042)	
London×2013							-0.041
							(0.024)
2013							-0.02
							(0.018)
Constant	-0.899	0.303	-1.405	-1.906***	-1.515***	-1.968***	-0.34
	(0.752)	(0.828)	(1.041)	(0.539)	(0.121)	(0.226)	(0.192)
N	37,400	28,453	28,468	30,778	25,958	9,070	35,028
R^2	0.008	0.016	0.013	0.024	25,958	9,070	0.018
N of People	25,988	21,145	21,158	24,062	0.107	0.116	26,006
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses
**** $p<0.01$, ** $p<0.05$*

Note: Estimates in Columns 1-4 based on Eq. 4.2; in Columns 5-7 on Eq. 4.1. Column (1) estimates a linear probability model using a binary indicator equal to one if the respondent has thought about her finances the day before, and zero otherwise, as outcome; Columns (2) and (3) then use feelings of happiness and anxiousness (0-10 scale), respectively, which the respondent reports to have had when thinking about her finances, as outcomes respectively. Column (4) uses national pride (0-10 scale) as outcome. Columns (5) and (6) replicate Eq. 4.1 in 2011 and 2013, respectively; Column (7) replicates Eq. 4.2 by using the years 2011 and 2013 only. Note that the difference between Column (5) and (6) arises due to the fact that the observation period in 2011 starts later compared to that in 2013. Controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode,, day-of-the-week and calendar-month effects.

Table 4.8: Heterogeneity – Demographic Characteristics

	Life Satisfaction	Happiness	Anxiety	Worthwhile
		Panel A: Gender		
London×2012×Men	-0.001	-0.019	0.024	0.002
	(0.018)	(0.02)	(0.026)	(0.019)
London×2012	0.057***	0.087***	0.008	-0.05***
	(0.013)	(0.016)	(0.022)	(0.018)
N	40,458	40,458	40,458	40,458
R^2	0.011	0.009	0.004	0.006
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes
		Panel B: Age		
London×2012×Age	-0.001	0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
London×2012	0.089***	0.062	0.047	-0.084**
	(0.026)	(0.038)	(0.036)	(0.033)
N	40,458	40,458	40,458	40,458
R^2	0.011	0.009	0.004	0.006
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes
		Panel C: Income		
London×2012×Income	0.018**	0.018	0.022	0.026**
	(0.009)	(0.014)	(0.014)	(0.012)
London×2012	-0.134	-0.105	-0.207	-0.316**
	(0.098)	(0.152)	(0.149)	(0.134)
N	40,458	40,458	40,458	40,458
R^2	0.011	0.009	0.004	0.006
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses

**** p<0.01, ** p<0.05*

Note: Regressions based on Eq. 4.2, with heterogeneous effects included as an additional treatment. Panel A includes gender treatment; Panel B age treatment; and Panel C income treatment. Regressions controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

regardless of sporting success (Kavetsos and Szymanski 2010). Likewise, for the Olympics, sporting success does not appear to matter for SWB.

4.7 Legacy

The concept of "legacy" has become increasingly important in the rationalization and celebration of the Olympic Games. On the SWB measures, however, our graphical evidence in Figures 4.2 and 4.3 suggested a limited legacy effect of the Olympics in London. Next, we incorporate the third wave of our survey collected in 2013 into our estimations to assess whether there is any statistical evidence in favor of a legacy effect in London. Despite our efforts and incentives to retain participants in the sample, attrition rates are significant in the third wave. As a result, our analyses including 2013 should be interpreted with caution.

Table 4.10 presents the results of a DID specification similar to that of Equation 4.2 , the only additions being the inclusion of $London \times 2013$ treatment along with a year fixed effect for 2013.

The $London \times 2012$ coefficients are in line with the findings in Table 4.2. They show positive and statistically significant effects on life satisfaction and happiness, no statistically significant effect on anxiety, and a negative effect on worthwhileness. The $London \times 2013$ coefficients imply no persistent Olympics effect in London in 2013 for life satisfaction and happiness, once all the controls are included in Columns (2) and (4). However, our model suggests there may have been a decrease in anxiety in London in 2013 as well as a decrease in worthwhileness. As mentioned, these results should be interpreted with caution due to high attrition rates and our inability to control for year-country shocks in 2013. Overall, however, and in line with the findings in Figures 4.2 and 4.3, there seems to be little evidence for a significant legacy effect of the Games on SWB.[134]

4.8 Conclusion

Every time there is the prospect of hosting a future Olympic Games, potential bidders ask themselves "is it worth it?" And once the Games are over, every host city asks itself "was it really worth it?" We do not rely on imagination or memory to answer these questions, but rather on whether reports of SWB change in response to hosting the Games. We explore a novel and newly constructed international panel dataset that measures the different components of

134. The same conclusion is reached if we repeat the estimations on legacy using the balanced sample, inverse probability weights, or a PSM approach, as performed in Table 4.4. Table 4.16 in Section 4.9 presents the results of these additional analyses.

Table 4.9: The Impact of Medals on SWB

	Life Satisfaction		Happiness		Anxiety			Worthwhile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Lagged Daily Medals								
London×2012×Medals	-0.006	-0.005	-0.004	-0.003	0.012***	0.011***	-0.014***	-0.013***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Medals	0.003	0.002	-0.005	-0.005	-0.002	-0.002	0.009***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
London×2012	0.074***	0.083***	0.103***	0.101***	-0.027	-0.009	-0.019	-0.014
	(0.012)	(0.013)	(0.018)	(0.018)	(0.021)	(0.022)	(0.018)	(0.019)
2012	0.007	0.002	0.092***	0.049**	0.049***	0.024	-0.068***	-0.064***
	(0.008)	(0.014)	(0.019)	(0.023)	(0.011)	(0.016)	(0.009)	(0.014)
Constant	-0.026***	-0.097	-0.093***	-1.228***	-0.014***	0.484	0.015***	-0.406
	(0.003)	(0.298)	(0.021)	(0.449)	(0.005)	(0.43)	(0.003)	(0.334)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.013	0.008	0.017	0.002	0.007	0.005	0.008
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Lagged Daily Gold Medals								
London×2012×Gold	-0.009	-0.008	-0.021	-0.021	0.032**	0.031**	-0.04***	-0.038***
	(0.01)	(0.01)	(0.012)	(0.012)	(0.013)	(0.012)	(0.01)	(0.009)
Gold	0.008	0.007	0.001	0.003	-0.018	-0.017	0.028***	0.025***
	(0.009)	(0.009)	(0.01)	(0.01)	(0.011)	(0.01)	(0.008)	(0.008)
London×2012	0.064***	0.075***	0.109***	0.106***	-0.016	0.001	-0.024	-0.018
	(0.011)	(0.012)	(0.016)	(0.017)	(0.02)	(0.021)	(0.017)	(0.018)
2012	0.01	0.002	0.083***	0.043	0.052***	0.028	-0.065***	-0.063***
	(0.007)	(0.013)	(0.019)	(0.023)	(0.01)	(0.015)	(0.008)	(0.014)
Constant	-0.026***	-0.098	-0.13***	-1.169**	-0.014**	0.482	0.015***	-0.407
	(0.003)	(0.299)	(0.022)	(0.45)	(0.005)	(0.43)	(0.003)	(0.334)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.005	0.008
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses
**** $p<0.01$, ** $p<0.05$*

Note: Regressions based on Eq. 4.2, with medals included as an additional treatment. Panel A considers daily lagged medals; Panel B considers daily lagged Gold medals only. Regressions controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

Table 4.10: Legacy (Panel: 2011, 2012, 2013)

	Life Satisfaction		Happiness		Anxiety		Worthwhile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
London×2012	0.057***	0.067***	0.084***	0.077***	0.006	0.019	-0.05***	-0.044***
	(0.01)	(0.011)	(0.015)	(0.014)	(0.018)	(0.02)	(0.016)	(0.016)
London×2013	0.036***	-0.01	0.062***	0.005	-0.075***	-0.098***	-0.035**	-0.065**
	(0.012)	(0.022)	(0.019)	(0.027)	(0.024)	(0.032)	(0.018)	(0.026)
2012	0.013**	-0.01	0.084***	0.069***	0.046***	0.018	-0.052***	-0.056***
	(0.006)	(0.01)	(0.021)	(0.019)	(0.011)	(0.013)	(0.009)	(0.012)
2013	0.036***	-0.003	0.056***	0.026	0.084***	0.054***	-0.066***	-0.074***
	(0.009)	(0.015)	(0.021)	(0.022)	(0.013)	(0.02)	(0.012)	(0.019)
Constant	-0.021***	-0.32	-0.128***	-0.02	-0.02***	0.128	0.065***	0.191
	(0.003)	(0.17)	(0.024)	(0.184)	(0.007)	(0.171)	(0.011)	(0.164)
N	49,528	49,528	49,528	49,528	49,528	49,528	49,528	49,528
R^2	0.002	0.011	0.007	0.014	0.002	0.006	0.003	0.005
N of People	26,036	26,036	26,036	26,036	26,036	26,036	26,036	26,036
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses

**** $p<0.01$, ** $p<0.05$*

Note: Regressions based on Eq. 4.2, with London×2013 included as an additional treatment along with a 2013 fixed effect. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

SWB. We exploit a quasi-experimental design to identify the causal effects of the 2012 Olympics on people's SWB in the host city of London. To do so, we elicit SWB from a total of 26,000 individuals in London, Paris, and Berlin over the summers of 2011, 2012, and 2013.

Our findings yield evidence that the 2012 Olympics increased the life satisfaction and happiness of Londoners in the short-run (i.e. during the Olympic period), particularly around the opening and closing ceremonies. Clearly, our identified effects are only average treatment effects: it may well be that, for some population sub-groups, hosting the Olympics did actually decrease SWB, for example, due to congestion. On average, however, our identified effects point towards positive effects of hosting. One of the key lessons of our study is that impacts are only short-run: we are not able to detect legacy effects in terms of SWB of hosting the Olympics. There were no consistent changes (either positive or negative) in anxiety or worthwhileness during the Olympic period for Londoners in comparison to Berliners or Parisians either. Our findings are robust to selection into the survey, and they also withstand a series of placebo tests, including placebo outcomes and time periods.

In terms of magnitude, the increases in life satisfaction – ranging between eight and ten percent of a standard deviation, depending on the specification – are quite large compared to standard estimates in the SWB literature. Notwithstanding important issues of causal inference, according to the specifications in Equations 4.1 and 4.2, the effect is equivalent to moving from the bottom income decile to at least the fourth income decile. They appear even larger when considering their prevalence, that is, they affect a large amount of people (although only for a relatively short amount of time). This complements existing evidence which shows that hosting the Olympic Games has a negligible economic impact on the host city, by providing evidence that hosting them has relatively large but only short-run impacts in terms of SWB. For a complete accounting of the costs and benefits of hosting, policy-makers should take both economic and intangible impacts into account.

Our study suffers from a number of limitations. Our sample is not strictly representative of the populations in London, Paris, and Berlin. We can control for observable differences between the achieved sample and the wider population, but there might be unobservables we are missing, and which would challenge any claims about generalizability. The sample is clearly of those proximate to the Games and policy makers might be interested in the impact on the broader UK, French, and German populations, so extrapolating these findings to the country-level also requires some caution.

Overall, many cities spend substantial resources attracting and then hosting the Olympic Games, but the evidence to date suggests that the Olympics do not have a significant economic benefit to the host city. This paper presents the first causal evidence of a positive wellbeing effect of the Olympic Games on local residents during the hosting of the Games. The effects do not last very long, however, and the Games show no effect on SWB a year later. The host with the most. But not for long.

4.9 Appendix to Chapter 4: Descriptive Statistics

Table 4.11: Descriptive Statistics

	London			Paris			Berlin		
	2011	2012	2013	2011	2012	2013	2011	2012	2013
Life Satisfaction	6.515	6.690	6.756	6.668	6.675	6.724	6.681	6.733	6.846
	(2.00)	(1.951)	(1.951)	(1.794)	(1.748)	(1.753)	(1.993)	(1.977)	(1.939)
Happiness	6.448	6.683	6.791	6.724	6.710	6.803	6.497	6.632	6.771
	(2.15)	(2.07)	(2.10)	(1.873)	(1.812)	(1.812)	(2.236)	(2.166)	(2.165)
Anxiousness	4.252	4.296	4.064	4.324	4.436	4.464	4.197	4.328	4.402
	(2.722)	(2.667)	(2.686)	(2.564)	(2.512)	(2.531)	(2.685)	(2.583)	(2.582)
Worthwhileness	6.865	6.716	6.822	6.699	6.594	6.611	7.226	7.181	7.273
	(2.048)	(2.087)	(2.081)	(1.752)	(1.704)	(1.754)	(1.93)	(1.892)	(1.861)
Age	28.925	32.515	35.124	28.140	30.390	32.240	26.532	29.482	31.876
	(14.929)	(14.379)	(14.259)	(15.20)	(14.981)	(14.984)	(14.688)	(14.613)	(14.452)
Male	0.407	0.413	0.431	0.472	0.476	0.465	0.429	0.436	0.450
	(0.491)	(0.493)	(0.495)	(0.499)	(0.499)	(0.499)	(0.495)	(0.496)	(0.498)
Annual Income (log)	10.386	10.434	10.446	10.310	10.396	10.398	10.006	10.076	10.163
	(0.786)	(0.755)	(0.748)	(0.694)	(0.661)	(0.643)	(0.83)	(0.832)	(0.812)
Married	0.418	0.451	0.483	0.356	0.371	0.375	0.332	0.367	0.396
	(0.493)	(0.498)	(0.50)	(0.479)	(0.483)	(0.484)	(0.471)	(0.482)	(0.489)
With Partner	0.146	0.135	0.115	0.213	0.202	0.190	0.167	0.169	0.159
	(0.353)	(0.342)	(0.319)	(0.409)	(0.402)	(0.392)	(0.373)	(0.374)	(0.365)
Separated	0.023	0.020	0.014	0.022	0.019	0.020	0.029	0.024	0.023
	(0.15)	(0.141)	(0.119)	(0.146)	(0.135)	(0.139)	(0.167)	(0.152)	(0.149)
Divorced	0.071	0.082	0.084	0.083	0.089	0.098	0.100	0.112	0.115
	(0.256)	(0.274)	(0.277)	(0.276)	(0.285)	(0.297)	(0.299)	(0.316)	(0.3199)
Widowed	0.029	0.035	0.039	0.026	0.030	0.034	0.022	0.027	0.031
	(0.168)	(0.185)	(0.192)	(0.16)	(0.17)	(0.182)	(0.146)	(0.162)	(0.174)
In School	0.053	0.021	0.012	0.084	0.060	0.042	0.126	0.089	0.069
	(0.224)	(0.142)	(0.107)	(0.278)	(0.237)	(0.201)	(0.332)	(0.285)	(0.253)
Professional Degree	0.148	0.141	0.174	0.153	0.033	0.185	0.052	0.319	0.316
	(0.355)	(0.348)	(0.379)	(0.36)	(0.177)	(0.388)	(0.223)	(0.466)	(0.465)
University Degree	0.429	0.442	0.416	0.102	0.522	0.000	0.436	0.400	0.429
	(0.495)	(0.497)	(0.493)	(0.303)	(0.50)	(0.00)	(0.496)	(0.49)	(0.495)
Other Higher	0.200	0.181	0.178	0.515	0.242	0.631	0.234	0.212	0.188
Education Degree	(0.40)	(0.385)	(0.383)	(0.50)	(0.428)	(0.483)	(0.423)	(0.409)	(0.391)
Part-Time Employed	0.120	0.126	0.127	0.071	0.064	0.062	0.128	0.128	0.130
	(0.325)	(0.332)	(0.333)	(0.257)	(0.244)	(0.24)	(0.334)	(0.334)	(0.337)
Self-Employed	0.096	0.092	0.104	0.036	0.030	0.026	0.091	0.083	0.087
	(0.294)	(0.289)	(0.305)	(0.187)	(0.17)	(0.158)	(0.288)	(0.276)	(0.282)
Unemployed:	0.059	0.041	0.036	0.049	0.043	0.042	0.056	0.046	0.047
Looking for Job	(0.235)	(0.199)	(0.187)	(0.216)	(0.202)	(0.201)	(0.229)	(0.21)	(0.212)
Unemployed:	0.085	0.088	0.073	0.038	0.036	0.036	0.044	0.041	0.034
Permanently	(0.278)	(0.284)	(0.259)	(0.191)	(0.187)	(0.187)	(0.206)	(0.198)	(0.181)
Retired	0.134	0.170	0.205	0.172	0.200	0.244	0.123	0.161	0.187
	(0.341)	(0.376)	(0.404)	(0.377)	(0.40)	(0.429)	(0.328)	(0.367)	(0.39)
Lives: Flat Share	0.346	0.301	0.261	0.422	0.389	0.375	0.719	0.702	0.680
	(0.476)	(0.459)	(0.439)	(0.494)	(0.488)	(0.484)	(0.449)	(0.458)	(0.467)
Lives: Relatives	0.077	0.048	0.039	0.058	0.053	0.041	0.034	0.027	0.022
	(0.266)	(0.214)	(0.194)	(0.233)	(0.225)	(0.198)	(0.180)	(0.161)	(0.147)
Lives: Other	0.013	0.012	0.014	0.025	0.002	0.013	0.038	0.034	0.043
	(0.115)	(0.108)	(0.119)	(0.157)	(0.045)	(0.115)	(0.191)	(0.181)	(0.203)
N	9,402	4,663	2,857	9,629	5,945	3,672	6,927	3,892	2,541

Standard deviations in parentheses

Note: Averages (proportions for the case of binary variables).

Table 4.12: Table 4.1 with Full Set of Controls

	Life Satisfaction	Happiness	Anxiety	Worthwhileness
London×OlympicsPeriod	0.088**	0.053	0.118**	0.028
	(0.042)	(0.042)	(0.049)	(0.043)
London×PostOlympicsPeriod	0.053	0.001	0.084	-0.081**
	(0.039)	(0.04)	(0.05)	(0.037)
London	0.138**	0.002	-0.265***	0.521***
	(0.056)	(0.05)	(0.057)	(0.044)
OlympicsPeriod	0.148***	0.023	-0.257***	0.166***
	(0.032)	(0.024)	(0.033)	(0.026)
PostOlympicsPeriod	0.014	0.004	-0.059	0.098***
	(0.033)	(0.024)	(0.034)	(0.029)
Age	-0.036***	-0.026***	0.016***	-0.01**
	(0.004)	(0.004)	(0.003)	(0.004)
Age^2	0.001***	0.001***	-0.001***	0.001***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Male	-0.051***	-0.04**	-0.103***	-0.121***
	(0.018)	(0.017)	(0.022)	(0.016)
Annual Income (log)	0.209***	0.162***	-0.106***	0.098***
	(0.011)	(0.011)	(0.013)	(0.013)
Married	0.26***	0.272***	0.031	0.28***
	(0.024)	(0.026)	(0.028)	(0.024)
With Partner	0.188***	0.246***	-0.009	0.16***
	(0.02)	(0.023)	(0.024)	(0.027)
Separated	-0.052	-0.074	0.047	0.045
	(0.056)	(0.055)	(0.066)	(0.055)
Divorced	0.061	0.08	-0.015	0.115***
	(0.041)	(0.041)	(0.033)	(0.038)
Widowed	0.075	0.124**	-0.038	0.134**
	(0.042)	(0.052)	(0.062)	(0.06)
In School	0.082**	0.059	0.001	0.136***
	(0.04)	(0.04)	(0.045)	(0.045)
Professional Degree	-0.011	-0.045	0.074**	0.042
	(0.03)	(0.032)	(0.033)	(0.031)
University Degree	0.036	-0.024	0.083***	0.08***
	(0.025)	(0.024)	(0.026)	(0.02)
Other Higher Education Degree	0.045	-0.001	0.017	0.099***
	(0.032)	(0.033)	(0.025)	(0.026)
Part-Time Employed	0.005	0.007	-0.026	0.005
	(0.032)	(0.027)	(0.029)	(0.029)
Self-Employed	0.061	0.017	-0.076**	0.163***
	(0.034)	(0.03)	(0.037)	(0.033)
Unemployed: Looking for Job	-0.361***	-0.265***	0.16***	-0.283***
	(0.049)	(0.047)	(0.045)	(0.06)
Unemployed: Permanently	-0.221***	-0.188***	0.113***	-0.284***
	(0.05)	(0.043)	(0.032)	(0.044)
Retired	0.045	0.068	-0.05	-0.027
	(0.04)	(0.04)	(0.036)	(0.033)
Lives: Flat Share	-0.149***	-0.087***	0.064***	-0.029
	(0.019)	(0.018)	(0.023)	(0.024)
Lives: Relatives	-0.249***	-0.171***	0.116**	-0.11***
	(0.038)	(0.039)	(0.047)	(0.04)
Lives: Other	-0.171**	-0.117	0.03	0.044
	(0.028)	(0.025)	(0.038)	(0.026)
Change in Quarterly GDP	-0.04	-0.054**	0.156***	-0.016
since 2008Q1	(1.887)	(2.215)	(2.262)	(1.809)
Constant	-0.009	-0.035	0.126**	0.055
	(0.039)	(0.025)	(0.053)	(0.038)
N	14,500	14,500	14,500	14,500
R^2	0.10	0.09	0.036	0.067
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses

Note: Estimates for each measure of SWB based on Eq. (1). Regressions routinely include controls for interview mode, and day-of-the-week and calendar-month effects.

Table 4.13: Table 4.2 with Full Set of Controls

	Life Satisfaction	Happiness	Anxiety	Worthwhileness
London×2012	0.07***	0.084***	0.024	-0.044***
	(0.011)	(0.015)	(0.019)	(0.016)
2012	0.005	0.043	0.022	-0.054***
	(0.013)	(0.022)	(0.015)	(0.013)
Age	-0.03**	0.04	0.003	-0.005
	(0.014)	(0.02)	(0.017)	(0.016)
Age^2	0.001**	-0.001	-0.001	0.001
	(0.0001)	(0.002)	(0.001)	(0.001)
Annual Income (log)	0.068***	0.037***	-0.051***	0.044***
	(0.012)	(0.014)	(0.014)	(0.012)
Married	0.111***	0.227***	-0.072	-0.026
	(0.039)	(0.037)	(0.042)	(0.047)
With Partner	0.059**	0.131***	-0.041	0.015
	(0.026)	(0.028)	(0.03)	(0.022)
Separated	0.126***	0.193***	0.114**	0.027
	(0.042)	(0.054)	(0.052)	(0.051)
Divorced	0.157***	0.02	0.003	0.022
	(0.05)	(0.062)	(0.053)	(0.045)
Widowed	-0.028	0.021	-0.163	-0.105
	(0.085)	(0.116)	(0.095)	(0.073)
In School	-0.017	-0.021	0.035	0.026
	(0.031)	(0.044)	(0.046)	(0.036)
Professional Degree	0.021	0.025	-0.003	0.012
	(0.019)	(0.019)	(0.021)	(0.019)
University Degree	0.008	-0.015	0.045	0.009
	(0.021)	(0.021)	(0.023)	(0.019)
Other Higher Education Degree	0.005	-0.009	-0.001	0.005
	(0.02)	(0.019)	(0.024)	(0.017)
Part-Time Employed	-0.055**	-0.041	0.028	-0.067**
	(0.023)	(0.025)	(0.027)	(0.028)
Self-Employed	-0.037	-0.067	0.075	-0.072**
	(0.033)	(0.036)	(0.039)	(0.035)
Unemployed: Looking for Job	-0.287***	-0.176***	0.124***	-0.117***
	(0.031)	(0.035)	(0.034)	(0.032)
Unemployed: Permanently	-0.104***	-0.044	0.131***	-0.151***
	(0.033)	(0.042)	(0.049)	(0.033)
Retired	0.002	-0.036	0.095**	0.017
	(0.042)	(0.057)	(0.043)	(0.047)
Lives: Flat Share	0.001	-0.036	-0.035	0.0255
	(0.025)	(0.026)	(0.03)	(0.028)
Lives: Relatives	-0.081**	-0.033	-0.013	0.006
	(0.039)	(0.041)	(0.045)	(0.039)
Lives: Other	0.005	-0.031	0.039	0.046
	(0.037)	(0.035)	(0.043)	(0.035)
Change in Quarterly GDP	3.859**	3.517	2.502	1.731
since 2008Q1	(1.887)	(2.215)	(2.262)	(1.809)
Constant	-0.098	-1.228***	0.484	-0.409
	(0.30)	(0.448)	(0.428)	(0.336)
N	40,458	40,458	40,458	40,458
R^2	0.012	0.017	0.007	0.007
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses
**** $p<0.01$, ** $p<0.05$*

Note: Estimates for each measure of SWB based on Eq. (2). Regressions routinely include controls for interview mode, and day-of-the-week effects.

Table 4.14: Table 4.3 with Full Set of Controls

	Life Satisfaction	Happiness	Anxiety	Worthwhileness
London×PreOlympicsPeriod$_{2012}$	0.049	0.135***	-0.146***	0.062
	(0.035)	(0.046)	(0.048)	(0.04)
London×OlympicsPeriod$_{2012}$	0.105***	0.111***	0.029	-0.019
	(0.017)	(0.023)	(0.028)	(0.021)
London×PostOlympicsPeriod$_{2012}$	0.046***	0.053***	0.042**	-0.08***
	(0.014)	(0.018)	(0.02)	(0.021)
PreOlympicsPeriod$_{2012}$	-0.003	0.018	0.089**	-0.076***
	(0.032)	(0.039)	(0.037)	(0.023)
OlympicsPeriod$_{2012}$	0.017	0.039	-0.035**	-0.043**
	(0.017)	(0.022)	(0.018)	(0.017)
PostOlympicsPeriod$_{2012}$	-0.002	0.036	0.054***	-0.059***
	(0.013)	(0.024)	(0.017)	(0.014)
Age	-0.056***	0.012	0.025	0.011
	(0.018)	(0.022)	(0.020)	(0.017)
Age^2	0.001***	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Annual Income (log)	0.074***	0.057***	-0.055***	0.049***
	(0.015)	(0.018)	(0.019)	(0.015)
Married	0.192***	0.345***	-0.061	0.001
	(0.042)	(0.050)	(0.058)	(0.065)
With Partner	0.094***	0.187***	-0.079**	0.027
	(0.031)	(0.036)	(0.037)	(0.029)
Separated	0.163**	0.176**	0.090	0.054
	(0.064)	(0.070)	(0.071)	(0.075)
Divorced	0.126	0.002	0.013	0.023
	(0.075)	(0.081)	(0.071)	(0.058)
Widowed	-0.045	0.069	-0.117	-0.156
	(0.120)	(0.167)	(0.130)	(0.098)
In School	-0.006	0.024	0.032	0.016
	(0.042)	(0.055)	(0.053)	(0.041)
Professional Degree	0.002	0.033	0.001	-0.006
	(0.022)	(0.024)	(0.026)	(0.024)
University Degree	-0.040	-0.027	0.025	-0.021
	(0.025)	(0.026)	(0.029)	(0.021)
Other Higher Education Degree	-0.040	-0.023	0.052	-0.033
	(0.027)	(0.027)	(0.031)	(0.022)
Part-Time Employed	-0.094***	-0.038	0.051	-0.065
	(0.027)	(0.030)	(0.034)	(0.036)
Self-Employed	-0.047	-0.064	0.069	0.011
	(0.043)	(0.041)	(0.049)	(0.047)
Unemployed: Looking for Job	-0.304***	-0.185***	0.123***	-0.114**
	(0.036)	(0.042)	(0.043)	(0.044)
Unemployed: Permanently	-0.059	0.022	0.060	-0.158***
	(0.040)	(0.046)	(0.054)	(0.040)
Retired	-0.012	0.039	0.078	0.098
	(0.046)	(0.046)	(0.049)	(0.052)
Lives: Flat Share	-0.002	-0.023	0.014	0.007
	(0.031)	(0.034)	(0.039)	(0.032)
Lives: Relatives	-0.077	0.014	-0.025	-0.007
	(0.040)	(0.046)	(0.058)	(0.043)
Lives: Other	0.049	-0.018	0.098	0.039
	(0.045)	(0.047)	(0.051)	(0.046)
Change in Quarterly GDP	1.621	-1.009	6.768**	-0.168
since 2008Q1	(2.539)	(2.938)	(3.025)	(2.361)
Constant	-0.149	-1.284***	0.651	-0.481
	(0.294)	(0.447)	(0.435)	(0.325)
N	40,458	40,458	40,458	40,458
R^2	0.013	0.017	0.009	0.008
N of People	26,030	26,030	26,030	26,030
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses
**** p<0.01, ** p<0.05*

Note: Estimates for each measure of SWB based on Eq. (3). Regressions routinely include controls for interview mode, and day-of-the-week effects.

Table 4.15: The Impact of Medals on SWB, Additional Analyses: Cumulated Medals

	Life Satisfaction		Happiness		Anxiety			Worthwhile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Cumulated Medals, Lagged by One Interview Day							
London×2012×Medals	-0.000	-0.000	-0.001**	-0.001	0.002***	0.002***	-0.002***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Medals	0.001	0.000	0.000	0.000	-0.001**	-0.001**	(0.001)	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
London×2012	0.060***	0.069***	0.107***	0.103***	-0.023	-0.006	-0.030	-0.025
	(0.012)	(0.013)	(0.016)	(0.017)	(0.017)	(0.019)	(0.018)	(0.019)
2012	0.006	0.003	0.078***	0.043	0.060***	0.037**	-0.074***	-0.073***
	(0.008)	(0.014)	(0.018)	(0.023)	(0.011)	(0.017)	(0.010)	(0.014)
Constant	-0.026***	-0.098	-0.128***	-1.176**	-0.014**	0.493	0.015***	-0.415
	(0.003)	(0.300)	(0.021)	(0.451)	(0.005)	(0.431)	(0.003)	(0.334)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.005	0.008
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel B: Cumulated Gold Medals, Lagged by One Interview Day							
London×2012×Gold	-0.001	0.000	-0.002	-0.001	0.006***	0.006***	-0.006***	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Gold	0.002	0.001	0.000	-0.001	-0.005***	-0.006***	0.006***	0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
London×2012	0.055***	0.064***	0.099***	0.096***	-0.016	0.003	-0.034	-0.028
	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)
2012	0.007	0.002	0.084***	0.048**	0.063***	0.042**	-0.073***	-0.073***
	(0.008)	(0.014)	(0.018)	(0.023)	(0.011)	(0.017)	(0.010)	(0.015)
Constant	-0.026***	-0.099	-0.094***	-1.173**	-0.014**	0.490	0.065***	-0.414
	(0.003)	(0.300)	(0.021)	(0.451)	(0.005)	(0.430)	(0.012)	(0.334)
N	40,458	40,458	40,458	40,458	40,458	40,458	40,458	40,458
R^2	0.002	0.012	0.008	0.017	0.002	0.007	0.005	0.008
N of People	26,030	26,030	26,030	26,030	26,030	26,030	26,030	26,030
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses
**** p<0.01, ** p<0.05*

Note: Regressions based on Eq. 4.2, with medals included as an additional treatment. Panel A considers cumulated medals, lagged by one interview day; Panel B considers cumulated Gold medals only, lagged by one interview day. Regressions controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

Table 4.16: Robustness for Attrition (Panel: 2011, 2012, 2013)

	Life Satisfaction		Happiness		Anxiety		Worthwhile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Balanced Panel							
London×2012	0.047***	0.059***	0.082***	0.082***	-0.008	-0.003	-0.048**	-0.042**
	(0.015)	(0.015)	(0.022)	(0.020)	(0.024)	(0.025)	(0.019)	(0.019)
London×2013	0.031**	-0.044*	0.062***	-0.021	-0.079***	-0.126***	-0.033*	-0.095***
	(0.014)	(0.025)	(0.022)	(0.028)	(0.025)	(0.034)	(0.019)	(0.030)
2012	0.014*	-0.005	0.090***	0.086***	0.041***	0.000	-0.043***	-0.035**
	(0.008)	(0.014)	(0.023)	(0.024)	(0.014)	(0.021)	(0.011)	(0.016)
2013	0.036***	0.002	0.059***	0.041	0.079***	0.013	-0.049***	-0.046*
	(0.009)	(0.023)	(0.021)	(0.027)	(0.014)	(0.031)	(0.015)	(0.026)
Constant	0.007	0.175	-0.123***	0.742**	-0.049***	-0.283	0.062***	0.784*
	(0.005)	(0.362)	(0.030)	(0.370)	(0.011)	(0.503)	(0.014)	(0.435)
N	26,916	26,916	26,916	26,916	26,916	26,916	26,916	26,916
R^2	0.002	0.012	0.007	0.014	0.002	0.007	0.002	0.004
N of People	9,135	9,135	9,135	9,135	9,135	9,135	9,135	9,135
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel B: Inverse Probability Weights							
London×2012	0.061***	0.074***	0.088***	0.096***	0.012	0.025	-0.054***	-0.045***
	(0.011)	(0.011)	(0.015)	(0.016)	(0.019)	(0.020)	(0.016)	(0.015)
London×2013	0.039***	-0.034	0.069***	-0.008	-0.058**	-0.095***	-0.051***	-0.097***
	(0.013)	(0.023)	(0.020)	(0.030)	(0.025)	(0.035)	(0.018)	(0.026)
2012	0.013*	-0.011	0.075***	0.048**	0.043***	0.011	-0.044***	-0.058***
	(0.007)	(0.012)	(0.020)	(0.023)	(0.012)	(0.016)	(0.009)	(0.013)
2013	0.041***	-0.002	0.057***	0.005	0.072***	0.020	-0.050***	-0.075***
	(0.010)	(0.018)	(0.021)	(0.027)	(0.014)	(0.026)	(0.013)	(0.021)
Constant	-0.006	-0.144	-0.050	-0.384	-0.044***	-0.487	0.074***	0.093
	(0.004)	(0.346)	(0.040)	(0.388)	(0.010)	(0.411)	(0.012)	(0.341)
N	37,771	37,771	37,771	37,771	37,771	37,771	37,771	37,771
R^2	0.002	0.011	0.007	0.011	0.002	0.004	0.002	0.004
N of People	14,530	14,530	14,530	14,530	14,530	14,530	14,530	14,530
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: Propensity Score Matching							
London×2012	0.070**	0.060*	0.087**	0.080*	-0.002	0.013	-0.051	-0.046
	(0.034)	(0.034)	(0.043)	(0.043)	(0.051)	(0.052)	(0.039)	(0.039)
London×2013	0.075***	0.038	0.068*	0.029	-0.083**	0.009	-0.082**	-0.121*
	(0.028)	(0.056)	(0.035)	(0.059)	(0.038)	(0.063)	(0.032)	(0.065)
2012	0.025	0.018	0.068**	0.069*	-0.062***	-0.076***	-0.039*	-0.027
	(0.017)	(0.021)	(0.034)	(0.035)	(0.021)	(0.024)	(0.023)	(0.027)
2013	0.032**	0.010	0.060	0.053	0.017	0.027	-0.007	
	(0.015)	(0.024)	(0.036)	(0.039)	(0.018)	(0.031)	(0.016)	
Constant	0.014*	-0.016	-0.115	0.130	-0.009	-0.446	0.038***	0.838*
	(0.008)	(0.290)	(0.071)	(0.322)	(0.010)	(0.432)	(0.009)	(0.503)
N	18,123	18,123	18,123	18,123	18,123	18,123	18,123	18,123
R^2	0.003	0.016	0.008	0.020	0.003	0.012	0.002	0.007
N of People	13,454	13,454	13,454	13,454	13,454	13,454	13,454	13,454
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at the date level reported in parentheses
**** $p<0.01$, ** $p<0.05$, * $p<0.10$*

Note: Regressions are based on Eq. 4.2. Panel A estimates coefficients based on the balanced sample; Panel B weights responses with the inverse probability of participating in wave three of the survey (i.e. 2013); Panel C matches respondents in the three cities one-to-one based on their likelihood to participate in wave three of the survey and estimates Equation 4.2 for those respondents. Regressions with controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

4.10 Appendix to Chapter 4: Attrition

Attrition across the three years is important as only 35% of wave one respondents were also interviewed in the last wave – see Table 4.17. Attrition was somewhat more pronounced in London, where 31% of the initial sample was interviewed in the last year; compared to 38% and 37% in Paris and Berlin, respectively.

Is attrition selective? To enquire we estimate the four SWB outcomes of interest conditional on staying in the sample. This is tantamount to asking whether 'happier' individuals are more likely to remain in the sample or to drop out of it, and whether this differs in London compared to the other two cities. Any of these results would likely bias our results.

As shown by Table 4.18, some selection bias is actually at play. Individuals who are happier and less anxious are more likely to stay in the sample. There is, however, no evidence of a selection bias that would differ across countries (although life satisfaction is weakly correlated to remaining in the sample in London).

Table 4.17: Number of Individuals Interviewed

	Wave 1	Wave 2	Wave 3
	Sample Attrition: Entire Sample		
Only Wave 1	11,165		
Only Waves 1 & 2	5,695	5,695	
Only Waves 1 & 3	139		139
All Waves	9,143	9,143	9,143
Total	26,142	14,838	9,282
% of Initial	100	56.76	35.51
	Sample Attrition: London		
Only Wave 1	4,679		
Only Waves 1 & 2	1,879	1,879	
Only Waves 1 & 3	42		42
All Waves	2,883	2,883	2,883
Total	9,483	4,762	2,925
% of Initial	100	50.22	30.84
	Sample Attrition: Paris		
Only Wave 1	3,541		
Only Waves 1 & 2	2,402	2,402	
Only Waves 1 & 3	62		62
All Waves	3,656	3,656	3,656
Total	9,661	6,058	3,718
% of Initial	100	62.71	38.48
	Sample Attrition: Berlin		
Only Wave 1	2,945		
Only Waves 1 & 2	1,414	1,414	
Only Waves 1 & 3	35		35
All Waves	2,604	2,604	2,604
Total	6,998	4,018	2,639
% of Initial	100	57.42	37.71

In Wave 1 (2011), interviews were conducted from August 8 to September 30. In Wave 2 (2012), interviews were conducted from July 20 to October 2. In Wave 3 (2013), interviews were conducted from July 23 to September 12.

Table 4.18: Testing for Differences in Attrition

	Life Satisfaction	Happiness	Anxiety	Worthwhile
Present (in all 3 Waves)	0.031	0.060**	-0.062**	0.030
	(0.026)	(0.027)	(0.025)	(0.025)
London	-0.106***	-0.027	0.031	-0.195***
	(0.021)	(0.021)	(0.020)	(0.020)
Paris	-0.013	0.123***	0.034	-0.263***
	(0.020)	(0.021)	(0.020)	(0.019)
Present×London	0.066	0.018	-0.047	0.032
	(0.035)	(0.036)	(0.034)	(0.034)
Present×Paris	0.012	-0.034	0.040	-0.033
	(0.032)	(0.033)	(0.033)	(0.031)
Constant	-0.007	-0.093***	-0.014	0.176***
	(0.016)	(0.017)	(0.015)	(0.015)
N	26,135	26,115	26,113	26,094
R^2	0.002	0.004	0.002	0.012
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at the date level reported in parentheses

**** p<0.01, ** p<0.05*

Note: "Present"=1 when individual is present in all three waves; =0 otherwise. Regressions controls include: gender, age, age^2, employment status, education level, marital status, log income, home ownership, change in quarterly GDP since 2008Q1, controls for interview mode, day-of-the-week and calendar-month effects.

Table 4.19: Balancing Properties of Observables after Propensity-Score Matching

	Mean London	Mean Paris & Berlin Pooled	Scale-free Normalised Difference
Age	31.65	30.492	0.056
Male	0.415	0.46	0.065
Annual Income (log)	10.448	10.246	0.188
Married	0.446	0.378	0.097
With Partner	0.14	0.185	0.087
Separated	0.019	0.021	0.01
Divorced	0.076	0.10	0.06
Widowed	0.033	0.031	0.008
In School	0.026	0.07	0.146
Professional Degree	0.149	0.142	0.014
University Degree	0.514	0.432	0.116
Other Higher Education Degree	0.142	0.255	0.202
Part-Time Employed	0.117	0.091	0.061
Self-Employed	0.084	0.052	0.091
Unemployed: Looking for Job	0.046	0.043	0.01
Unemployed: Permanently	0.084	0.04	0.129
Retired	0.166	0.191	0.047
Lives: Flat Share	0.308	0.524	0.318
Lives: Relatives	0.053	0.039	0.047
Lives: Other	0.01	0.014	0.026
N	10,438	18,624	—

Note: The last column shows the normalised difference, calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) \div \sqrt{\sigma_t^2 + \sigma_c^2}$, where $\bar{x}_t$ and $\bar{x}_c$ denote the sample mean of the covariate of the treatment and control group, respectively, and σ^2 denotes the variance. As a rule of thumb, a normalised difference greater than 0.25 indicates a non-balanced covariate, which might lead to sensitive results Imbens and Wooldridge 2009.

4.11 Appendix to Chapter 4: Additional Figures

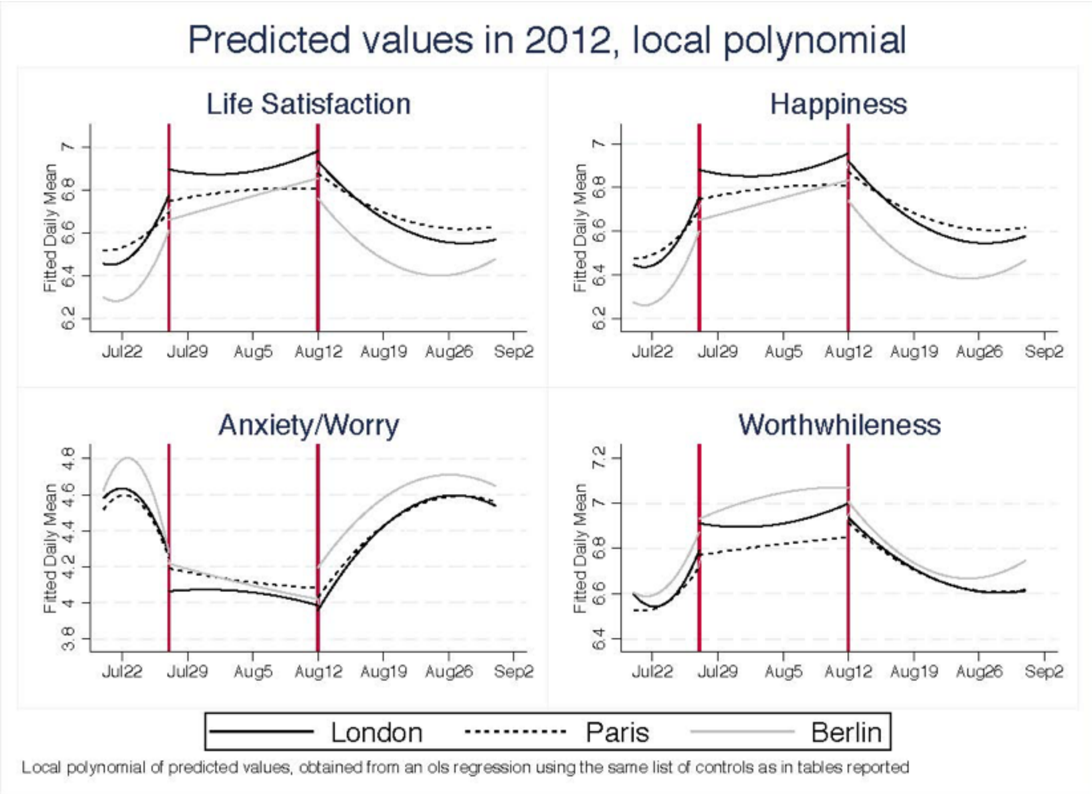


Figure 4.4: SWB in 2012 in London, Paris, Berlin

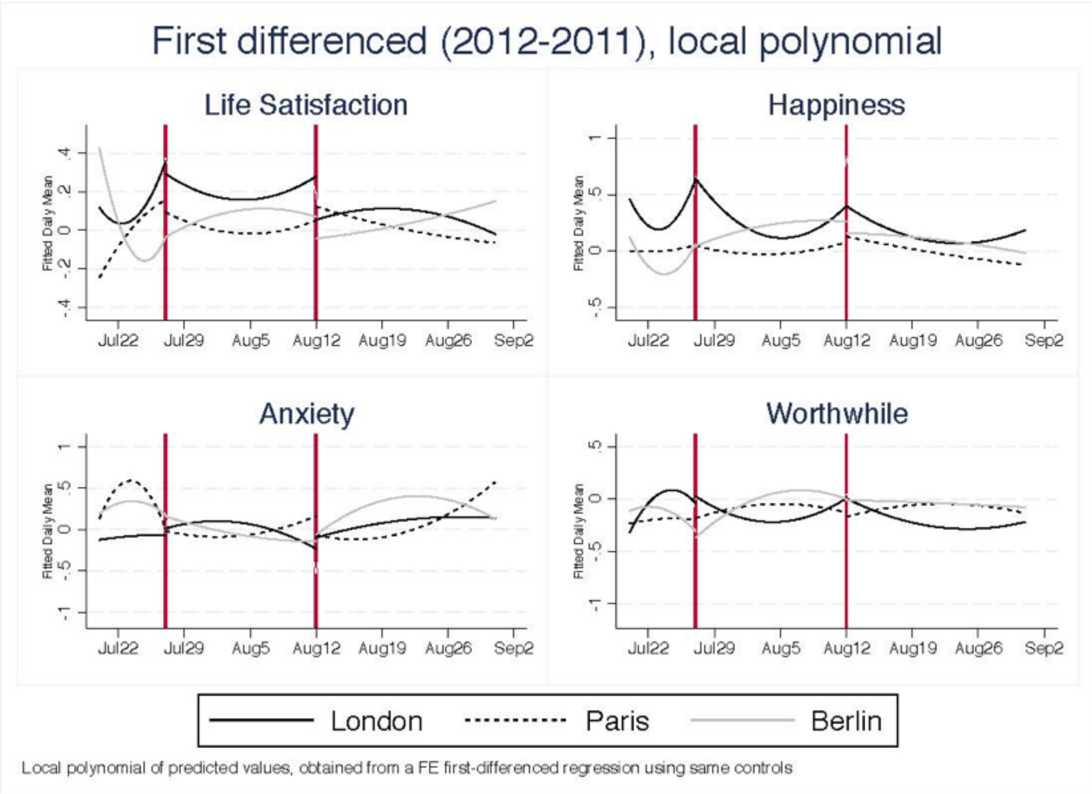


Figure 4.5: Changes in SWB between 2012 and 2011 in London, Paris, Berlin

Table 4.20: Potentially Confounding Events in the United Kingdom, France, and Germany in 2012

Date	Potentially Confounding Event
United Kingdom, 2012, July to September	
6 July	Andy Murray makes it to the final of the 2012 Wimbledon Championships Men's singles, becoming the first Briton to do so in 74 years. He is defeated at the final two days later by Roger Federer.
7 July	Britain's Jonathan Marray and Denmark's Frederik Nielsen win Wimbledon's men's doubles final by three sets to two. Marray becomes the first Briton to win such a match since 1936.
22 July	Bradley Wiggins wins the 2012 Tour de France bicycle race, the first British rider ever to do so.
12 August	Golfer Rory McIlroy wins the 2012 US PGA Championship at Kiawah Island.
10 September	Andy Murray wins the US Open Tennis Championship, the first British man to win a Grand Slam tournament since 1936.
France, 2012, July to September	
16 July 2012	The commission on renewal and ethics in public life is formed by François Hollande.
August	France posts zero growth in the second quarter of 2012, as in the previous two.
Germany, 2012, July to September	
July 3	Success for German players in the Wimbledon tennis singles: in the Men's section, Florian Mayer and Philipp Kohlschreiber reach the quarter finals; in the Women's section, Sabine Lisicki reaches the quarter finals, and Angelique Kerber reaches the semi finals.
August 9 to 12	Hanse Sail in Rostock
September 15 to 19	gamescom in Cologne
September 18 to 23	photokina in Cologne
September 20 to 27	Frankfurt Motor Show in Frankfurt
September 22 to October 7	Oktoberfest in Munich
Sources:	Wikipedia, BBC 2017

Chapter 5

Instructional Time

Abstract

We study whether raising instructional time can crowd out student pro-social behaviour. To this end, we exploit a large educational reform in Germany that has raised weekly instructional time for high school students by 12.5% as a quasi-natural experiment. We find that this rise has a negative and sizeable effect on volunteering, both at the intensive and at the extensive margin. It also affects political interest. There is no similar crowding out of scholastic involvement, but no substitution either. Impacts on student subjective well-being are negative but insignificant. We conclude that instructional time plays an important role in shaping student pro-social behaviour.*

*. This chapter is also available as the following discussion paper: Krekel, C., "Can Raising Instructional Time Crowd Out Student Pro-Social Behaviour? Evidence from Germany," *SOEPpapers on Multidisciplinary Panel Data Research*, 903, 2017.

5.1 Introduction

A growing body of empirical literature documents the importance of instructional time for student learning and performance (Patall et al. 2010). Raising instructional time – the allocated number of hours per year that students spend in formal classroom settings – is often found to have positive effects on cognitive skills such as maths and language ability (Bellei 2009; Cortes and Goodman 2014; Taylor 2014), as well as standardised maths, reading, and scientific literacy test scores (Andrietti 2016; Cattaneo et al. 2016; Huebener et al. 2016).[135] Differences in instructional time between countries are also found to account for some of the observed international gaps in student achievement (Lavy 2015; Woessmann 2003).[136] Thus, despite being a relative costly input into the educational production function, raising instructional time features high on the policy agenda in many countries (Organisation for Economic Co-operation and Development 2016). Yet, outcomes other than student learning and achievement have scarcely been studied (Patall et al. 2010), and particularly little is known about how changes in instructional time might influence student leisure activities and behaviours. Can raising instructional time have hidden costs by – unintentionally – crowding out student leisure activities and behaviours that parents, educators, and policy-makers alike would otherwise consider worth promoting?

In this paper, we are interested in a particular type of student behaviour: pro-social behaviour, defined as voluntary behaviour intended to benefit one or more individuals other than oneself (Eisenberg et al. 2013). This type of behaviour can cover a broad range of actions such as helping, sharing, and other forms of cooperation (Batson and Powell 2003), and is distinct from altruism in that it is not purely motivated by increasing another individual's welfare, but can be motivated by, for example, empathy or reciprocity. Pro-social behaviour, and in particular volunteering, is linked to various positive outcomes: at the societal level, it can help build social capital through fostering cooperation and trust and through promoting citizenship (Putnam 2000), and social capital such as trust is linked to higher levels of subjective well-being in societies (Helliwell 2007; Helliwell and Wang 2011; Helliwell et al. 2011). At the individual level, it is found to nurture important cognitive and non-cognitive skills that can improve individual labour market outcomes, to have positive physical and mental health benefits, and to raise subjective well-being over and beyond other benefits (Wilson and Musick 2012), as confirmed

135. There is growing evidence that the effect of raising instructional time on student learning and performance is heterogeneous, and in particular, that higher-performing students tend to benefit relatively more (Cattaneo et al. 2016; Huebener et al. 2016).

136. The importance of instructional time for student achievement varies between educational systems, and in particular, between developed and developing countries (Woessmann 2016), pointing towards potentially important complementarities in educational production, for example, between instructional time and teacher quality or effective classroom management techniques (Rivkin and Schiman 2015).

in both observational (Binder and Freytag 2013; Meier and Stutzer 2008) and experimental studies (Dunn et al. 2008; Aknin et al. 2008). Specifically for youth, there is evidence that volunteering from an early age on enhances psychological development by raising self-esteem and self-confidence and by discouraging risky behaviours (Hart et al. 2007; Wilson and Musick 2012).

To study the effect of raising instructional time on student pro-social behaviour, we exploit a large educational reform in Germany as a quasi-natural experiment: starting with school cohorts in the early 2000s, the number of school years required to obtain the university entrance qualification has been reduced from 13 to 12.

In Germany, secondary education, which is compulsory until the age of 16, is tripartite: after joint primary education, which typically takes four years, students are tracked into different school types according to their abilities: lower, intermediate, or upper track schools. Some federal states offer schools that combine the lower and intermediate track, or comprehensive schools or alternative school types that postpone tracking. In any case, only the upper track leads to the university entrance qualification. The reform affected only students in this track – hereafter simply referred to as *high school students* – and reduced their duration in secondary education from nine to eight years of schooling. It aimed at reducing the graduation age of high school students, which was high in international comparison, to enable an earlier entry into the labour market. This, in turn, aimed at counteracting demographic change, especially an eroding contributor base and a shortage of skilled labour.

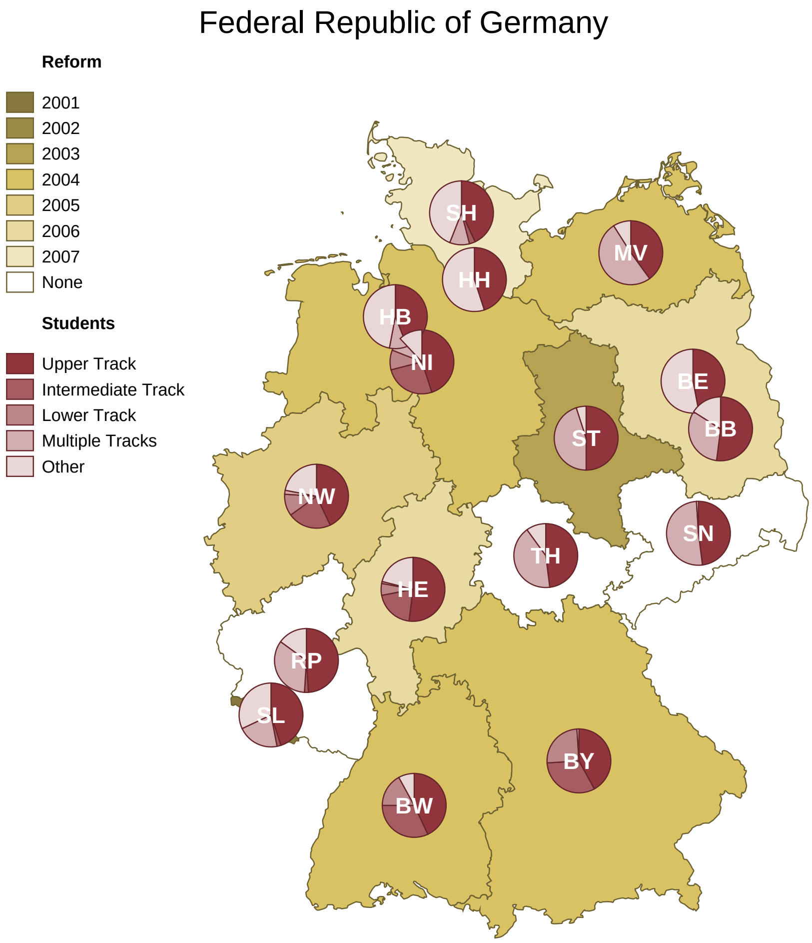
This reform – commonly referred to as "G8", where "G" stands for upper track schools (*Gymnasien*) and "8" for the reduced number of years of schooling (as opposed to the old "G9" system) – has two features that make it particularly interesting for us: first, the overall curriculum and thus total instructional time required to obtain the university entrance qualification has not changed, which, in turn, has lead to a 12.5% rise in weekly instructional hours across all subjects plus a rise in accompanying coursework. [137] Importantly, there have been no changes to the taught curriculum that target pro-social behaviour. From a time use perspective, the available evidence on the implementation of the reform suggests that this 12.5% rise in weekly instructional hours was real: rather than leaving the total number of school hours per week constant and going through the curriculum at a faster pace, the reform was implemented by appending more school hours and going through the materials at the same pace (Homuth 2017),

137. Starting from the fifth grade (the first year of secondary education), high school students generally have to complete at least 265 year-week hours before being allowed to take the university entrance qualification exam (Standing Conference of the Ministers of Education and Cultural Affairs 2016). Thus, average instruction hours per year increased from 1,051 to 1,184, compared to 950 in upper secondary education in England and 1,038 in the US (Organisation for Economic Co-operation and Development 2014). The rise in weekly instructional hours can be calculated as $(((265/(8))/(265/(9))) - 1) \times 100$.

significantly reducing the time available for leisure. In terms of an educational production function, this means that learning intensity has increased, whereas other inputs such as class size, instructional materials, and teacher quality have not been changed as a result of the reform. This allows us to estimate the "pure" effect of raising instructional time on student pro-social behaviour, excluding potentially confounding changes to the educational system that are typically accompanied by similar reforms. Second, since education in Germany is the responsibility of the 16 federal states, there has been a staggered implementation of the reform: while some federal states implemented it as early as 2001 (*Saarland*), others waited until 2007 (*Schleswig-Holstein*); yet others have never fully implemented it (*Rhineland-Palatine*), or as in case of *Saxony* and *Thuringia*, have always required only 12 school years to obtain the university entrance qualification (Standing Conference of the Ministers of Education and Cultural Affairs 2016). This allows the estimation of the causal effect of raising instructional time on student pro-social behaviour by exploiting variation in the implementation of the reform.

Figure 5.1 shows this variation across federal states and school cohorts (the different years of the implementation of the reform are highlighted in different shades of gold). It also shows the share of students in the different tracks (different shades of red): in school year 2013/14, of 5,187,960 students in total, 2,329,990 (45%) are in the upper track; with few exceptions, they make up the largest share of students in each federal state.

Using survey data on youth and adolescents from the German Socio-Economic Panel Study (SOEP) and a difference-in-differences approach, we find that the 12.5% rise in weekly instructional time significantly crowds out student pro-social behaviour: it has a negative and sizeable effect on volunteering, decreasing the likelihood to volunteer at least once a month by about six percentage points. Given that almost 34 percent of students report to volunteer at least monthly, this amounts to a decrease of about 18 percent in this share. In other words, the rise in instructional time leads almost every fifth student to change her behaviour from volunteering at least monthly to volunteering less often or not at all. This change is primarily driven by students who report to volunteer on a weekly basis, and it affects both the intensive and extensive margin of volunteering: while half of the students cut back on their activities, the other half give them up completely. Students with lower educated parents are up to three times more likely to cut back on their activities. We find no similar crowding out of scholastic involvement, but no substitution either. Impacts on student subjective well-being are negative but insignificant. Interestingly, we find that the rise in instructional time has a differential impact on student political interest: it leads to a depolarisation at both ends of the spectrum, decreasing the share of students that report to be at least fairly interested in politics while at



Note: The figure shows variation in the implementation of the reform across states and over time. It also reports the shares of students in the different tracks for each state, as of school year 2013/14. The category *multiple tracks* includes students in schools combining the *intermediate* and *lower track*; *other* includes students in comprehensive and Waldorf schools. The states are *Baden-Wuerttemberg* (BW), *Bavaria* (BY), *Berlin* (BE), *Brandenburg* (BB), *Bremen* (HB), *Hamburg* (HH), *Hesse* (HE), *Lower Saxony* (NI), *Mecklenburg-West Pomerania* (MV), *North Rhine-Westphalia* (NW), *Rhineland-Palatinate* (RP), *Saarland* (SL), *Saxony* (SN), *Saxony-Anhalt* (ST), *Schleswig-Holstein* (SH), and *Thuringia* (TH). Different years of implementation of the reform are highlighted in different shades of gold, shares of students in different tracks of secondary school in different shades of red.

Source: Federal Agency for Cartography and Geodesy 2016a, Federal Statistical Office 2016b, Standing Conference of the Ministers of Education and Cultural Affairs 2016, own calculations.

Figure 5.1: Implementation of Reform, Variation Across States and Over Time

the same time decreasing the share that report to be not interested at all. The size of these changes is very strong: every third student switches category. The results are robust to a different model specification, time trends, and seasonal variation; selection and implementation; and potentially confounding other reforms that are implemented during the observation period. They also withstand a series of placebo tests. We conclude that instructional time plays an important role in shaping student pro-social behaviour.

This finding is significant for several reasons: first, in the given context, it is significant because of the sheer number of students affected. In Germany, in school year 2013/14 alone, the reform affected 2,329,990 high school students, about half of the entire student population in secondary education (Federal Statistical Office 2016b). Second, it is significant because of the important role pro-social behaviour, and in particular volunteering, plays, both for individuals, as described above, and for society at large: the OECD estimates the economic value of volunteering for Germany in 2013 to be around USD 117.6 billion or 3.3% of real GDP (Organisation for Economic Co-operation and Development 2015), roughly comparable to the UK and the US. Finally, to the extent that students from disadvantaged backgrounds are disproportionally affected, the decrease in volunteering for these groups might further increase educational inequalities, and thus inequalities in later life outcomes.

We contribute to three strands of literature: first, we contribute to the economic literature on the external, non-monetary effect of education on civic engagement, which focuses on the effect of years of education on predominantly political interest, information, and participation (Dee 2004; Dhillon and Peralta 2002; Milligan et al. 2004; Pelkonen 2012; Siedler 2010), as well as reciprocity (Fehr and Gachter 2000; Kosse et al. 2014).[138] Here, the study most closely related to ours is Gibson 2001: the author uses a sample of twins to hold unobservable family characteristics constant, showing that more years of education are associated with a lower probability of volunteering and supply of volunteer hours. We complement this study by focusing on intensity rather than amount of instruction.[139] Second, we contribute to the literature on instructional time (Bellei 2009; Cortes and Goodman 2014; Cortes et al. 2015; Herrmann and Rockoff 2012; Taylor 2014), and in particular, to the stream that exploits the "G8" reform as a source of exogenous variation: since the first data became available, the reform has been used – due to its features – as a laboratory for empirical research in educational economics. The more sophisticated studies use difference-in-differences approaches that exploit variation in its implementation across federal states and school cohorts; they examine its effects on graduation

138. See Lochner 2011 and Oreopoulos and Salvanes 2011 for reviews.

139. Next to this literature in economics stands a large body of literature in political science on the relationship between education and political participation, especially voter turnout. See, for example, Henderson and Chatfield 2011, Hillygus 2005, Persson 2014, and Sondheimer and Green 2010, to name just a few.

age, grade repetition, and graduation rates (Huebener and Marcus 2015), post-secondary educational choices (Meyer et al. 2015), and student performance (Andrietti 2016; Homuth 2012; Huebener et al. 2016). Here, the studies most closely related to ours are Dahmann and Anger 2014 and Dahmann 2015: we use the same dataset and a similar specification as these authors, who show that the reform affects personality traits, and to some extent, cognitive skills. So far, the potentially negative effects on leisure activities of youth and adolescents have played only a minor role relative to educational outcomes, although this point has sparked considerable controversy amongst students, parents, and educators alike (see, for example, Süddeutsche Zeitung 2010 for a feature), and continues to do so today. The study that is content-wise closest related to ours is Meyer and Thomsen 2015: the authors use self-collected cross-section data on students from the double graduation cohort (which might be subject to implementation effects) in the federal state of *Saxony-Anhalt* two years after graduation, showing that students in this cohort and state indeed feel more pressured and tend to spend less time on leisure activities such as jobbing or volunteering. More generally, the impact of instructional time on student pro-social behaviour has received little attention so far. Finally, implicitly, we also contribute to the literature on the relationship between pro-social behaviour, in particular volunteering, and subjective well-being using observational data (Binder and Freytag 2013; Meier and Stutzer 2008).

The rest of this paper is organised as follows: Section 5.2 describes the data used in the empirical analysis, Section 5.3 the empirical model and identification strategy. The results, including robustness checks, are presented in Section 5.4. Section 5.5 discusses them against the background of recent trends in the educational sector, and gives policy implications.

5.2 Data

5.2.1 German Socio-Economic Panel Study

The German Socio-Economic Panel Study (SOEP) is a representative panel of private households in Germany. It has been conducted annually since 1984, and includes about 20,000 individuals in more than 11,000 households in its current wave. The SOEP provides rich information on all household members, covering Germans living in the old and new federal states, foreigners, and recent immigrants (Wagner et al. 2007; Wagner et al. 2008). Most importantly, it provides information on the volunteering, scholastic involvement, political interest, and subjective well-being of youth and adolescents, as well as on their demographic, educational, and parental household characteristics.

During fieldwork, typically, two types of questionnaires are administered: an individual questionnaire is filled out by each household member aged 18 and above; a separate household questionnaire is filled out by the household head. The former covers personal characteristics such as education, leisure activities, and attitudes, the latter household and neighbourhood characteristics that apply to all household members equally. Moreover, since 2000, a separate youth questionnaire including both prospective and retrospective items on childhood and schooling is administered to youth in the year in which they turn 17. This is when individuals enter the SOEP at the earliest. If they enter at a later point in time, they are administered – in addition to the individual questionnaire – a supplementary biography questionnaire that includes most of the items of the youth questionnaire in order to complement missing information.

The youth questionnaire is our main data source: it includes items on the volunteering, scholastic involvement, political interest, and subjective well-being of youth annually from 2006 onwards. To increase sample size, we complement these data with data on adolescents from the individual questionnaire, which includes – for the observation period under consideration – the same items on volunteering biannually from 2001 onwards, on political interest biannually from 2000 onwards (with few exceptions), and on subjective well-being annually from 2000 onwards. The supplementary biography questionnaire complements items on scholastic involvement.[140] The SOEP also provides readily usable, generated items on educational trajectories of respondents, including the year and federal state in which they started school, the type of school they are currently attending, and in case they have already graduated, the year and federal state in which they have graduated, as well as the degree they have obtained. In case the year or state of school enrolment is missing, we impute it using their date of birth or state of residence, respectively.[141] If we have multiple observations of the same individual, we only include the observation at the youngest age.

We restrict our sample to the years 2000 to 2014, and to individuals aged 17 to 20 in order to create a homogeneous age group and avoid confounding effects associated with entrance into tertiary education. We focus only on high school students and graduates, as only those have been affected by the reform. In doing so, we omit students from comprehensive schools: as we

140. In robustness checks, we account for between-survey differences at any point in time by including a dummy variable for the respective survey: the results remain robust (see column (a) of Table 5.16 in Section 5.6 for this result). Moreover, we account for within-survey differences over time by routinely controlling for school cohorts.

141. When benchmarking the imputed values with the original ones, we find that they match in about 99% of cases for the state and 66% of cases for the year of school enrolment. Obviously, for the latter, there is some discretion on side of parents (we account for differences in cut-off dates for school enrolment across states and over time): if we assume that parents have a tendency to redshirt, that is, to strategically postpone school enrolment in order to provide their children with educational advantages due to relative and absolute maturity (Bedard and Dhuey 2006; Black et al. 2011), their children are correctly allocated to the treatment group. Enrolling in school prematurely, on the contrary, is very rare in Germany.

cannot clearly identify whether these students are attending or graduated from the academic track, we take a conservative approach and omit them altogether. Moreover, we omit all individuals from federal states where the reform has never been implemented, or during years in which it has been implemented only partially. Finally, we omit all individuals with missing observations on outcomes and covariates. Depending on how many observations on outcomes are available, this gives us a sample of 2,010 students for volunteering and subjective well-being, 1,765 for scholastic involvement, and 2,315 for political interest.[142]

5.2.1.1 Outcomes

5.2.1.1.1 Volunteering We select *volunteering* as our main outcome for pro-social behaviour. The indicator is obtained from a single-item five-point Likert scale that asks respondents "How often do you do volunteer work in clubs or social services during free time?". Possible answers include "daily" (about 6% of respondents), "every week" (16%), "every month" (12%), "less often" (30%), and "never" (36%). We create a binary indicator that equals one if respondents volunteer at least once a month, that is, if they volunteer daily, weekly, or monthly, and zero else. About 34% of respondents do so.[143]

Figure 5.2 shows the development of this outcome over the observation period. The x-axis denotes the interview year, and the y-axis the fitted annual mean, covariate adjusted for observables described in Sub-Section 5.2.1.2.

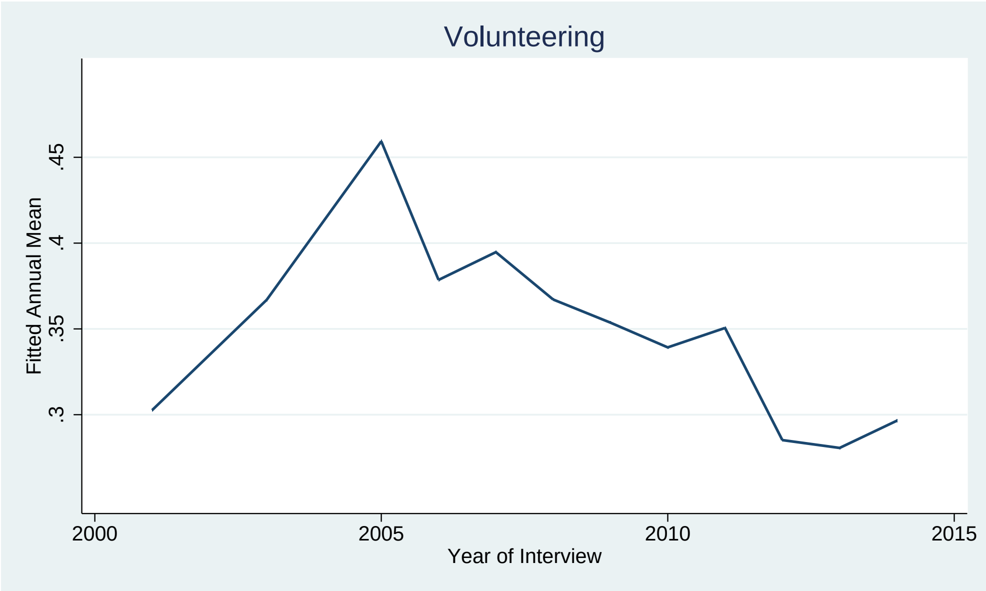
We can see that, over the past decade, there has been an initial rise in the share of students that volunteer at least once a month, up until the year 2005, whereafter this share started to decline until slightly below its initial value in the year 2014.[144]

As this indicator is framed in such a way as to refer to activities outside school, we select various ways of *scholastic involvement* as additional outcomes to cover activities inside, in line with a broad definition of pro-social behaviour. The respective indicators are obtained from a battery of binary items that asks respondents "Besides normal classes, there are also other ways to get involved in school. Have you ever – before or right now – been involved in one or more of the following ways?" Possible answers include "student representative" (about 3% of respondents), "class representative" (41%), "school magazine" (10%), "drama or dance group" (20%), "choir or orchestra" (33%), "sports group" (28%), "other voluntary group" (37%), and

142. If not stated otherwise, descriptive statistics are given on the sample for volunteering.

143. In our baseline specification, we use a binary indicator because it splits the share of students that volunteer at least monthly and the share of those that volunteer less often in approximately equal shares. To dig deeper, we also use binary indicators for each answer possibility: these turn out mostly insignificant, as the reform shifts (almost) the entire volunteering distribution, as we shall see later on.

144. Figure 5.9 in Section 5.6 shows the development of volunteering for students in the lower and intermediate track: compared to those in the upper track, these students tend to volunteer less. The rise in the share that volunteers at least once a month prolongs much longer, up until the year 2009, whereafter it starts to decline.



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.2: Pro-Social Behaviour, *Outside of School*, Over Time

"none" (20%). We create a binary indicator for each activity that equals one if respondents have ever been engaged in it, and zero else.

5.2.1.1.2 Political Interest Pro-social behaviour is voluntary behaviour that benefits one or more individuals other than oneself (Eisenberg et al. 2013), and it is distinct from altruism in that it does not have to be purely motivated by increasing another individual's welfare, but can be motivated by, for example, reciprocity. We adopt a broad definition of pro-social behaviour here, and are also interested in political behaviour and participation, which can (but, of course, does not necessarily have to) be motivated by the willingness to benefit specific groups of society or society as a whole. Apart from items on voting intentions in federal elections, the SOEP does not include specific items on political behaviour, for example, on membership in political parties or participation in youth organisations.[145] However, it regularly asks respondents about their degree of interest in politics more generally. As interest has long been seen as a necessary condition for subsequent behaviour (Fishbein and Ajzen 1975), we select *political interest* as outcome to proxy for political behaviour. The indicator is obtained from a single-item four-point Likert scale that asks respondents "Generally speaking, how much are you interested in politics?". Possible answers include "very much" (about 7% of respondents), "much" (26%), "not so much" (51%), and "not at all" (15%). We create a binary indicator for each of these categories.

5.2.1.1.3 Subjective Well-Being To measure student subjective well-being, we employ an evaluative measure, namely life satisfaction, measured on the standard eleven-point single-item Likert scale that asks respondents "How satisfied are you with your life, all things considered?". The indicator has its mass point at around 7.8, suggesting that most students are pretty happy with their lives (in the overall population, the mass point is at around 7). This is in accordance with the classic result of a U-shape in life satisfaction over the life course, with both younger and older people being relatively more satisfied with their lives than middle-aged individuals.

5.2.1.2 Covariates

We routinely control for age and whether a student has graduated in all our regressions. The mean age of students is 17.5, and only 4% of them have already graduated. We also routinely control for age dummy variables to account for non-linearities of outcomes with respect to age.

145. As federal elections (normally) happen only once every four years, the sample size is not large enough to analyse these items. The SOEP also asks respondents whether they lean towards a specific party, and if so, towards which party they lean and to what extent. As there is no *a priori* reason to believe that an increase in instructional time changes political orientation, we do not analyse these items.

Moreover, in our preferred specification, we control for a rich set of other demographic and parental household characteristics. These include gender (about 53% of students are female), migration background (about 19% have a migration background, either direct or indirect), and place of residence (about 13% live in East Germany and 28% live in rural areas). When it comes to their parents, about 53% of students have at least one parent with a tertiary degree, 13% have a parent that is a blue-collar worker, and 65% have a parent that works full time. Finally, about 19% of students are risen by a single parent, and about 17% are the only child. The average number of children in the household is 2.4. See Table 5.10 in Section 5.6 for more descriptive statistics.

5.3 Empirical Strategy

To investigate whether raising instructional time can crowd out pro-social behaviour, we exploit the recent educational reform in Germany that reduced the number of school years required to obtain the university entrance qualification as a quasi-natural experiment. Specifically, we set up a difference-in-differences design that exploits variation in the implementation of the reform across federal states and school cohorts: students are allocated to the treatment group if they belong to a school cohort in a federal state which was affected by the reform (or, in other words, if they enrolled in the year in which the reform was implemented or any year thereafter in the respective federal state), and to the control group else. Thus, students in the treatment group are exposed to a higher average weekly instructional time of 12.5% plus accompanying coursework than those in the control group. For both groups, however, the taught curriculum is the same. From 2,010 students in our sample on volunteering, 762 are in the treatment and 1,248 are in the control group; for scholastic involvement, these are 743 and 1,022 out of 1,765 students, and for political interest 781 and 1,534 out of 2,315. Table 5.1 shows the distribution of students by age in both treatment and control group over time for volunteering, our main outcome.

Table 5.1: Distribution of Students by Age in Groups for Volunteering

	Treatment Group				Control Group				
Year	Age 17	Age 18	Age 19	Age 20	Age 17	Age 18	Age 19	Age 20	Total
2001	0	0	0	0	71	89	78	72	310
2002	0	0	0	0	0	0	0	0	0
2003	0	0	0	0	25	20	34	25	104
2004	0	0	0	0	0	0	0	0	0
2005	0	0	0	0	102	10	4	4	120
2006	0	0	0	0	95	0	0	0	95
2007	0	0	0	0	130	15	17	5	167
2008	1	0	0	0	84	8	3	0	96
2009	4	0	0	0	96	13	4	1	118
2010	47	0	0	0	81	0	0	0	81
2011	134	13	2	0	36	24	53	23	285
2012	165	0	0	0	8	0	0	0	8
2013	182	6	7	1	5	0	8	5	214
2014	200	0	0	0	0	0	0	0	0
Total	733	19	9	1	733	179	201	135	2,010

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

5.3.1 Regression Equation

We employ linear probability models, which are estimated using ordinary least squares with robust standard errors clustered at the federal state level.[146] More specifically, following Dahmann and Anger 2014 and Dahmann 2015, we use the following specification:

$$y_{isc,(17-20)} = \beta_0 + \beta_1 Reform_{sc} + \boldsymbol{\beta}_2' \mathbf{X_{isc,(17-20)}} + \\ + \sum_{s=1}^{16} \gamma_s State_s + \sum_{c=1}^{14} \delta_c Cohort_c + \epsilon_{isc,(17-20)} \tag{5.1}$$

where y is the pro-social behaviour of student i in federal state s and school cohort c, measured at age 17 to 20; *Reform* is a dummy variable that equals one if the student belongs to a school cohort in a federal state which was affected by the reform, and zero else; and X is a

146. In our preferred specification, less than 1% of predicted values lie outside the [0;1] interval. Moreover, the results are similar when using a probit model, as shown in Table 5.6. Out-of-sample prediction, therefore, seems to be less of an issue. Finally, the results remain the same when using weighted regressions and bootstrapped standard errors. See Table 5.14 in Section 5.6 for these results.

vector of controls, including demographic, educational, and parental household characteristics. We routinely include a full set of federal state and school cohort dummy variables.[147] Our regressor of interest is β_1, which captures the reform effect. It can be interpreted as the average treatment effect on the treated, and is causal if the identifying assumptions described in Sub-Section 5.3.2 hold.

This difference-in-differences design has two features. First, it is generalised in the sense that treatment can occur at different points in time for different individuals. In fact, at any point in time over the observation period, we compare students who are affected by the reform with those who are not (yet) affected. Thus, towards the beginning of the observation period, the treatment group is relatively small, and as the reform gradually fades in, it increases as more and more observations on affected students become available, and *vice versa* for the control group. Second, this difference-in-differences design is pseudo in the sense that we only observe each student once. This is due to the fact that individuals enter the SOEP in the year in which they turn 17 at the earliest.[148] In other words, at the point of the first interview, students are near school completion, or even shortly thereafter. As a consequence, we cannot observe their pre-treatment outcomes, which would have had to be recorded prior to enrolment.[149]

This difference-in-differences design imposes stronger identifying assumptions than a conventional one. For example, as we do not observe the same individuals over time but compare different ones at the same points in time, we cannot readily net out unobserved heterogeneity amongst individuals by including individual fixed effects; rather, in case there is unobserved heterogeneity, we have to assume that there is a balance in unobservable characteristics between treatment and control group, and that this balance remains constant over time (this is sometime referred to as *bias stability*) (Heckman et al. 1999). In Sub-Section 5.3.2 we provide evidence that, although our identifying assumptions are stronger, they are likely to hold.

5.3.2 Identification

Our main identifying assumption is that, in the absence of treatment, the pro-social behaviour of students in the treatment group would have followed the same time trend as that of students

147. We also routinely include controls for sub-samples, as the SOEP consists of 16 random samples, which partly focus on different population strata.
148. The SOEP also includes several mother-child questionnaires, which have been administered since 2003. However, these questionniares, which are highly age-specific and cover the age span from birth to 10, are completed by the mother and do not include the items that are relevant for this study. A separate student questionnaire, covering ages 11 and 12, has been administered since 2014 only (and does not include these items either).
149. Strictly speaking, even if we would observe their pre-treatment outcomes, it is questionable whether we could use them effectively: the kind of pro-social behaviour we are interested in plays a relatively minor role prior to age 12.

in the control group. Although this *common trend assumption* is not formally testable as the counterfactual is not observable, in Sub-Sections 5.3.2.1 and 5.3.2.2, we provide evidence that it is likely to hold.[150]

5.3.2.1 Balancing on Observables

The first piece of evidence comes from Table 5.2: it shows the means of all covariates, overall and separately for treatment and control group, along with their scale-free normalised differences. Here, covariate imbalance between treatment and control group could indicate a deviation from a common time trend.

Imbens and Wooldridge (2009) suggest that a normalised difference above 0.25 indicates covariate imbalance. This is not the case for most of our covariates: only the age is above the threshold, and whether a student has graduated comes close. This is no surprise, though, given that the reform explicitly aimed at reducing the number of school years, thus indirectly reducing the graduation age. In fact, Huebener and Marcus 2015 estimate that the reform decreased the graduation age by about 10 months. Thus, we conclude that the sample is well-balanced on observables, and therefore most likely on unobservables as well. Finally, we routinely control for age dummy variables and whether a student has graduated in all our regressions in order to rule out any age and graduation effects.[151]

5.3.2.2 Graphical Evidence

Next, we take a closer look at how volunteering, our main variable of interest, evolves over time. Figure 5.3 is constructed similarly as Figure 5.2: it shows the development of volunteering over

150. Implicitly, we also require *ignorability* and the *stable unit treatment value assumption* to hold: the former implies that treatment assignment is independent of the outcome, the latter that whether a student is treated or not should not depend on the outcome of another student. Both are likely to be true: the rise in instructional time for a student does not depend on the amount of volunteering of that student, neither does it depend on the amount of volunteering of another student. Moreover, there should be no variation in treatment intensity between students. Again, this is likely to be true as the reform aimed at reducing the number of school years only while holding everything else constant. For the vast majority of students in the first school cohorts affected, the resulting rise in instructional time was present from the point of enrolment onwards. Only students in the federal states of *Saxony-Anhalt* and *Mecklenburg-West Pomerania* had already started school when the reform was implemented. In fact, these students were in grades seven to nine, in which some schools allocated a disproportionally higher share of the overall rise in instructional time, potentially yielding a different treatment intensity for these students. In Sub-Section 5.4.2, we explore this possibility in more detail.

151. Note that covariance imbalance between treatment and control group would not necessarily be a threat to our identification strategy: we control for a rich set of time-varying observables in our preferred specification. Moreover, including federal state and school cohort dummy variables nets out systematic differences in both time-invariant observables and unobservables between federal states and school cohorts, respectively.

Table 5.2: Descriptive Statistics

Variables	Mean	Mean: Treatment Group	Mean: Control Group	Mean: Normalised Difference
Age	17.5105	17.0525	17.7901	0.6680
Has Graduated	0.0383	0.0039	0.0593	0.2265
Is Female	0.5338	0.5525	0.5224	0.0426
Has Migration Background	0.1935	0.2231	0.1755	0.0844
Lives in East	0.1284	0.1168	0.1354	0.0397
Lives in Countryside	0.2831	0.3018	0.2716	0.0472
Parent Has Tertiary Degree	0.5259	0.4698	0.5601	0.1282
Parent is Blue-Collar Worker	0.1264	0.1050	0.1394	0.0744
Parent is Full-Time Employed	0.6532	0.5682	0.7051	0.2032
Parent is Single	0.1940	0.0480	0.0266	0.0801
Is Only Child	0.1701	0.1759	0.1667	0.0172
Number of Children in Household	2.4114	2.5354	2.3357	0.1281
Number of Observations	2,010	762	1,248	-

Note: The last column shows the normalised difference, which is calculcated as $\triangle x = (\bar{x}_t - \bar{x}_c) \div \sqrt{\sigma_t^2 + \sigma_c^2}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate for the treatment and control group, respectively. σ^2 denotes the variance. As a rule of thumb, a normalised difference greater than 0.25 indicates a non-balanced covariate, which might lead to sensitive results (Imbens and Wooldridge 2009). See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

the observation period. The x-axis denotes the interview year. Different from Figure 5.2, there are now two y-axes: the left y-axis denotes the fitted annual mean, covariate adjusted for observables, whereas the right y-axis denotes the percentage of the interviewed who were treated. The vertical line marks the interview year before the first observations of the treated become available.

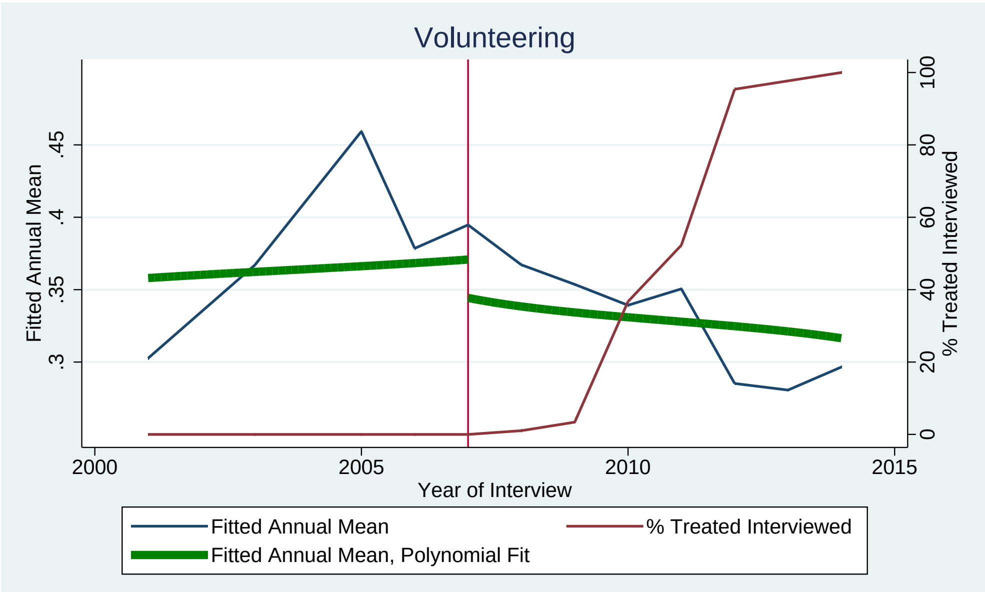
It is clearly visible that the vertical line marks a structural break, dividing the observation period into two: using local-mean polynomial smoothing, we can see that there is a clear upwards trend in volunteering in the first half of the observation period, whereas in the second, this trend is reversed. Moreover, the trend reversal coincides with an increasing share of the treated amongst the interviewed.

Figure 5.4 takes Figure 5.3 one step further: it decomposes, in the second half of the observation period, the overall mean into that of the treatment and control group, respectively. It also plots – in addition to that of the overall mean – the polynomial fit of the control group mean.[152]

We can make three observations. First, when focusing on the control group mean only, it becomes clear that part of the trend reversal in volunteering probably would have come about in the absence of the reform: the polynomial fit of the control group mean tilts downwards irrespective of whether the share of the treated amongst the interviewed increases.[153] Second, the treatment group mean is systematically lower than the control group mean, and as the share of the treated amongst the interviewed increases, the difference between the polynomial fit of the overall and that of the control group mean increases as well. This is already suggestive that part of the trend reversal in volunteering is indeed driven by the reform; in our regressions, we are measuring the mean difference between the control group and the treatment group mean in the second half of the observation period. Finally, important for identification, the treatment group mean, when fading in, evolves in parallel to the control group mean, when fading out. This is suggestive of a common trend between treatment and control group.

To illustrate this common trend in more detail, we plot the overall mean for different federal states that implemented the reform quite late during the observation period. Figures 5.5 and 5.6 are constructed similarly to Figure 5.3: the first figure shows the overall mean for two groups of states in which the first observations of the treated become available in the same interview year, pooled together (the first group, labelled "Gr. 1", includes the states of *Baden-Wuerttemberg*, *Bavaria*, *Bremen*, *Hesse*, and *Lower Saxony*; the second group, labelled "Gr. 2",

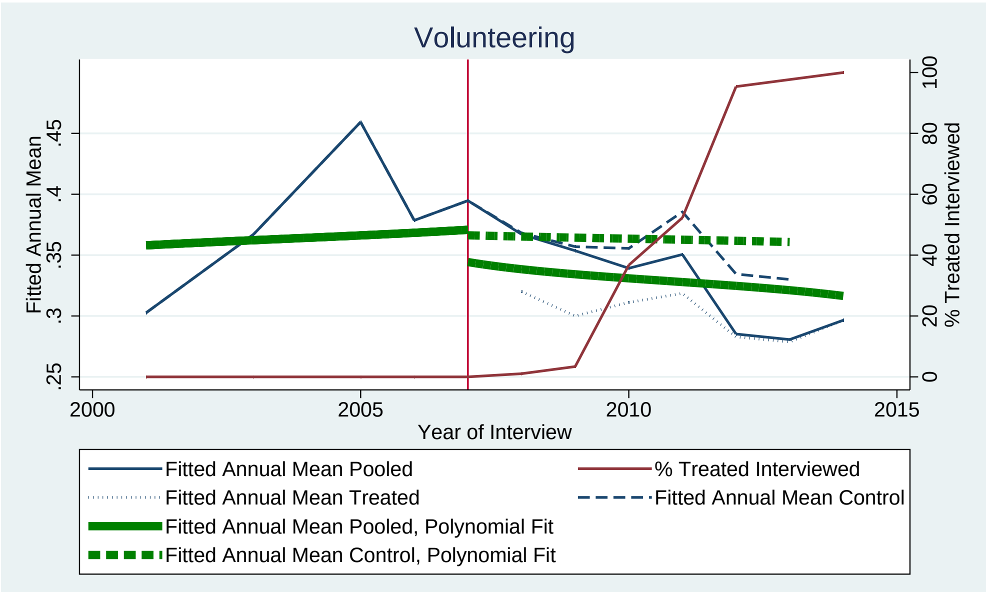
152. See Figure 5.10 in Section 5.6 for a similar illustration of political interest.
153. This also raises the question to what extent the identified reform effect is driven by time trends. In Sub-Section 5.4.2, we explore this possibility in more detail.



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.3: Graphical Evidence - Pro-Social Behaviour, *Outside of School*, Over Time, 1 of 2



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.4: Graphical Evidence - Pro-Social Behaviour, *Outside of School*, Over Time, 2 of 2

includes the states of *Berlin*, *Brandenburg*, and *Schleswig-Holstein*). The second figure shows them separately for two large area states in which this is not the case (the first state, labelled "St. 1", is the state of *Schleswig-Holstein*; the second state, labelled "St. 2", is the state of *North Rhine-Westphalia*).[154]

Again, we can make three observations. First, irrespective of whether we plot the overall mean for groups of states pooled together or separately for single states, there is a common trend between these states before the first observations of the treated become available. Second, the interview year before the first observations of the treated become available marks a structural break. Finally, after this structural break, these states once again exhibit common trend behaviour.[155]

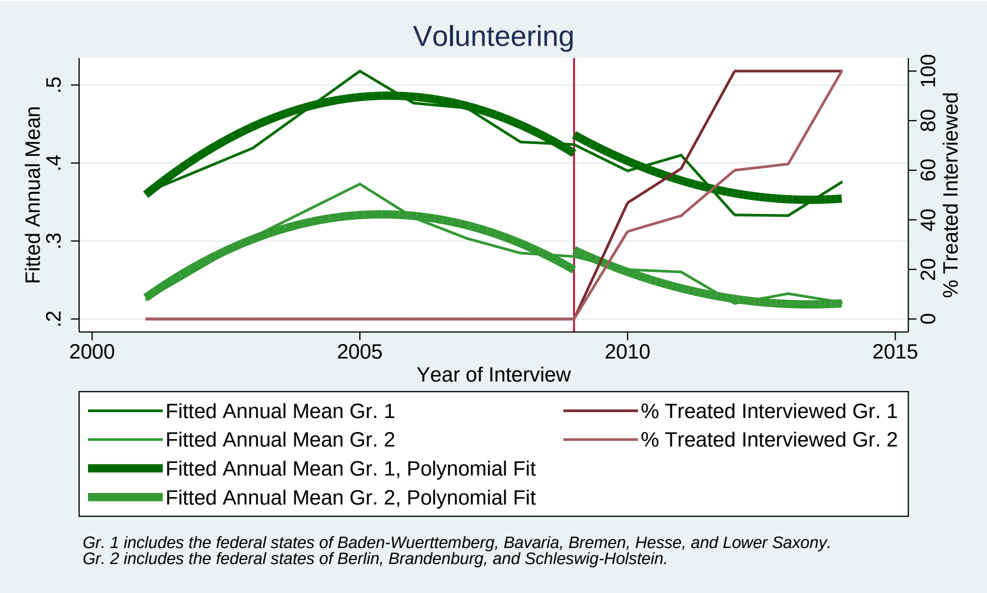
Taken together, the balancing properties of observables and the graphical evidence is clearly supportive of a common trend between treatment and control group. Moreover, in case there is unobserved heterogeneity, there seems to be a balance in unobservable characteristics between them that remains constant over time.

5.4 Results

5.4.1 Baseline Results

We now turn to our baseline results in Table 5.3: column (1) includes only the reform dummy variable, our regressor of interest; columns (2) and (3) then successively add age dummy variables and a graduation dummy variable in order to account for age and graduation effects. Finally, column (4) includes all of the above, along with a rich set of other demographic and parental household characteristics; it is our preferred specification, and the regression equivalent to Figure 5.3.[156]

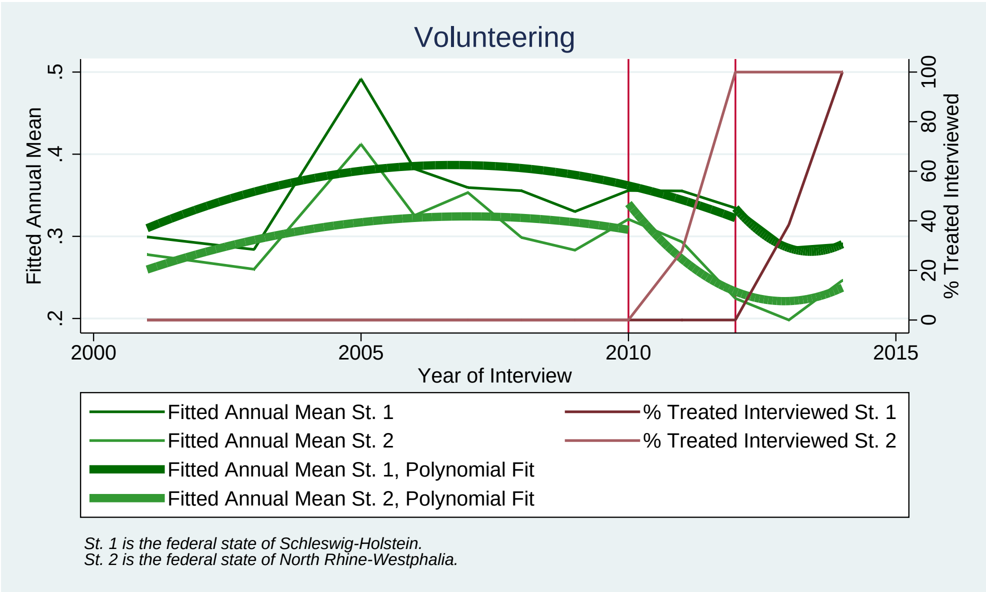
154. Again, see Figure 5.11 in Section 5.6 for a similar illustration of political interest.
155. The latter point is also suggestive evidence that the *stable unit treatment value assumption* is likely to hold: common trend behaviour post-treatment implies that treatment intensity is likely to be the same across federal states.
156. Note that in these and all subsequent models, both R^2 and Adjusted R^2 are relatively low. This, however, is of little concern in our context: we are not interested in the extent to which these models predict outcomes in general, but rather, we want to test whether a specific reform significantly affected the outcomes of treated versus untreated students (for both of which, equally, we cannot perfectly predict outcomes).



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.5: Graphical Evidence - Pro-Social Behaviour, *Outside of School*, Common Trend, 1 of 2



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.6: Graphical Evidence - Pro-Social Behaviour, *Outside of School*, Common Trend, 2 of 2

Table 5.3: Baseline Results - Pro-Social Behaviour, *Outside of School*

	Volunteering			
Regressors	(1)	(2)	(3)	(4)
Reform	-0.0661***	-0.0592***	-0.0590***	-0.0579***
	(0.0126)	(0.0128)	(0.0132)	(0.0144)
Age 17		-0.0454	-0.0382	-0.0373
		(0.0478)	(0.0446)	(0.0373)
Age 18		-0.0260	-0.0186	-0.0165
		(0.0296)	(0.0287)	(0.0294)
Age 19		0.0432	0.0493	0.0538
		(0.0452)	(0.0486)	(0.0535)
Has Graduated			-0.0178	-0.0236
			(0.0333)	(0.0419)
Other Demographic Characteristics	No	No	No	Yes
Parental Characteristics	No	No	No	Yes
Household Characteristics	No	No	No	Yes
Number of Observations	2,010	2,010	2,010	2,010
R^2	0.0517	0.0537	0.0538	0.0767
Adjusted R^2	0.0295	0.0301	0.0296	0.0483

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

5.4.1.1 Volunteering

Table 5.3 shows that the reform has a negative and sizeable effect on volunteering across the board, which is significant at the 1% level: in our preferred specification, it decreases the likelihood to volunteer at least once a month by about six percentage points. The size of this effect is also economically significant: given that almost 34 percent of all students in the sample report to volunteer at least monthly, it amounts to a decrease of about 18 percent in this share. In other words, the reform led almost every fifth student to change her behaviour from volunteering at least monthly to volunteering less often or not at all. The fact that the sign, size, and significance level is similar across all models reinforces the notion of a quasi-natural experiment.[157]

Another way to look at this result is through the lens of an event study. Figure 5.7 plots the fitted annual mean of volunteering, obtained from running Equation 5.1, respectively, in the years just before and the years just after the implementation of the reform (which varies by federal state), whereby the year of implementation is normalised to be at $t = 0$. As can be seen, in the years running up to the reform, the share of students that volunteer at least once per month follows a rather stable path, averaging between 36% and 38%. In the year of the implementation of the reform, at $t = 0$, this share falls sharply to about 30%, and remains stable in the years thereafter.

The remainder of the coefficients behave as expected: age turns out insignificant, which suggests that restricting the sample to students aged 17 to 20 in order to achieve a homogeneous age group and avoid age effects has worked. Related, having graduated has a positive but insignificant effect on volunteering; however, only a small share (4%) of students in the sample has already graduated. In fact, the mean age of students in the final sample is 17.5, which is well below the mean age after graduation of 19.7.[158] Confounding graduation effects therefore seem to be less of an issue.[159]

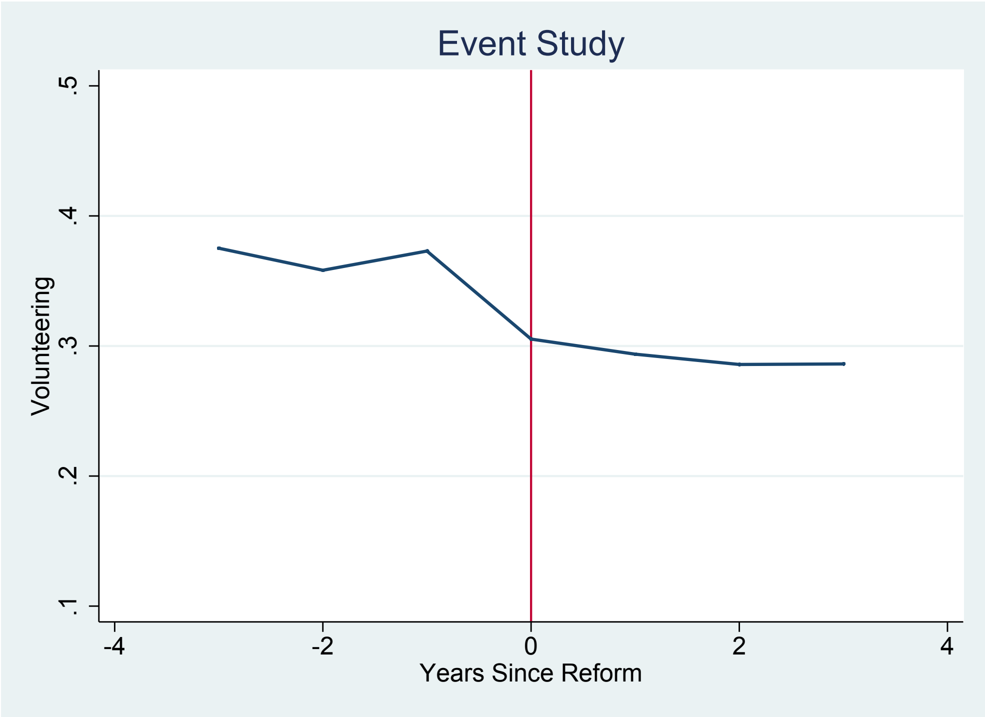
As our outcome is a binary indicator constructed from a categorical variable, it would be interesting to see how the overall frequency distribution of volunteering changes due to the reform. Figure 5.8 illustrates this: it compares the means of the different frequencies of volunteering before and after the reform.

We can make three observations. First, the reform seems to affect the entire frequency

157. This is also suggestive evidence that *ignorability* is likely to hold, even unconditionally: the fact that our estimates vary so little depending on covariates implies that treatment is likely to be exogenous.

158. The mean age at graduation is likely to be lower: most interviews are carried out between January and June, and students typically graduate in June. Thus, there may be quite some lag between when students graduate and when we observe them after graduation.

159. See Table 5.11 in Section 5.6 for the full set of controls.

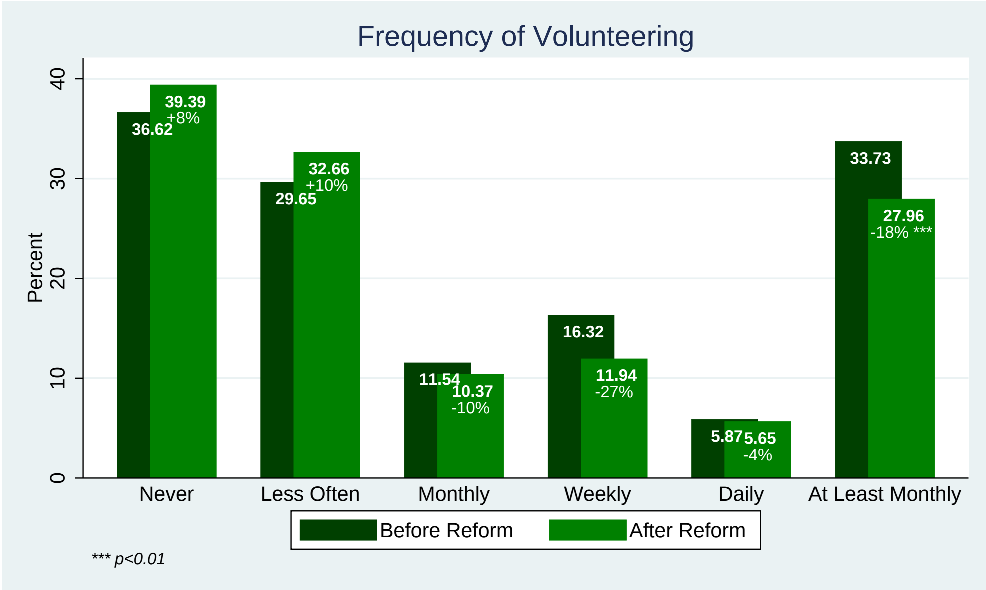


Note: The figure shows the fitted mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables, respectively, in the years just before and in the years just after the reform. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.7: Baseline Results - Pro-Social Behaviour, *Outside of School*, Event Study

distribution of volunteering, as all categories are affected, although to different degrees. Second, the driving force behind the decrease in the share of students that volunteer at least once a month seem to be students that volunteer weekly, followed by those that volunteer monthly: the share of the former drops by about 27%, the share of the latter by about 10%. On the contrary, the share of students that report the highest frequency of volunteering sees almost no reduction (fewer than 4%). This, however, is only a small fraction: about 6% report to volunteer daily, as opposed to about 16% and 12% reporting to volunteer weekly and monthly, respectively. Second, these reductions seem to be met with almost equal rises by approximately 10% in both the share of students that volunteer less often and the share of students that volunteer never; the difference between these flows is significant. This implies that the reform affected both the intensive and the extensive margin of volunteering: while some students seem to cut back on their activities, others seem to give them up completely. At the same time, this might point towards potential effect heterogeneities, and indeed, although there is little evidence that the



Note: The figure shows the change in the frequency distribution of volunteering due to the reform. The respective change is estimated from a separate regression in which the respective frequency of volunteering serves as the outcome. The regressions routinely control for demographic, educational, and parental household characteristics, as well as for sub-samples and for federal states and school cohorts. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.8: Graphical Evidence - Pro-Social Behaviour, *Outside of School*, Change in Distribution

effects vary much by student demographics and achievement, we find that, in line with findings from other OECD countries (Organisation for Economic Co-operation and Development 2015), students with lower educated parents seem to be up to three times more likely to cut back on their activities (see Table 5.13 in Section 5.6 for this result).

The question arises whether there is a similar crowding out of scholastic involvement as for volunteering. Alternatively, one could ask whether the crowding out of volunteering is matched by an increase in scholastic involvement. In other words, is there a substitution of activities outside school with activities inside? Table 5.4 shows that neither seems to be the case: it takes our preferred specification, column (4) in Table 5.3, and uses the likelihood of various ways of scholastic involvement as outcomes. Clearly, the reform has no significant effect on any of them, and neither is there a clear pattern in terms of sign. To get a sense of whether the reform affects the extensive margin of scholastic involvement, we also tested an alternative outcome: a binary indicator that equals one if respondents have ever been engaged in any of the activities in columns (a) to (g), and zero else. Again, the reform has no significant effect on this alternative outcome.[160] A potential caveat of this analysis is that we have slightly less observations for scholastic involvement than for volunteering: the sample size decreases from 2,010 to 1,765 students. This decrease, however, is mostly driven by students in the control group: 743 are now in the treatment and 1,022 are in the control group.

5.4.1.2 Political Interest

Finally, we ask how the reform affects political interest, which we take as a proxy for political behaviour. Table 5.5 sheds light on this question. Once again, we take our preferred specification, column (4) in Table 5.3, and use the likelihood of being interested in politics with a particular strength, including strongly, fairly, weakly, or not at all, as outcomes. We also combine the first two categories to form a new one, namely being moderately interested in politics.[161]

Interestingly, we find that the reform seems to have a differential impact on political interest: it has a significantly positive effect on being weakly interested at the 5% level. At the same time, however, it has a significantly negative effect on not being interested at all as well as on being moderately interested at the 10% level. Although these effects are significant at the 10% level only, we still interpret them as important, given that our sample size is relatively small. In other words, there is a depolarisation at both ends of the spectrum: the reform decreases the share of students that report to be moderately, that is, at least fairly, interested in politics by about 10 percentage points while at the same time decreasing the share that report to be not interested at all by about six percentage points. Taken together, this equals the incremental 16 percentage points of those being weakly interested. These migration flows are very strong: every third student switches from the higher category to the lower, and *vice versa*.[162]

160. In another specification, we excluded the activities in columns (a) and (b). Arguably, these activities should react inelastically to changes in instructional time: by German school law, there has to be a student and a class representative. The result, however, remains the same.
161. In this analysis, we have slightly more observations: the sample size increases from 2,010 to 2,315, 781 of which are now in the treatment and 1,534 are in the control group.
162. In robustness checks, we include dummy variables for state and federal elections, either individually or jointly: the results remain robust, and if anything, the effect for being moderately interested in politics becomes significant at the 5% level (see Tables 5.17, 5.18, and 5.19 in Section 5.6 for these results).

Table 5.4: Baseline Results - Pro-Social Behaviour, *Inside of School*

	Scholastic Involvement							
Regressors	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Reform	0.0082	0.0196	-0.0171	-0.0144	0.0495	0.0466	-0.0044	0.0081
	(0.0078)	(0.0372)	(0.0340)	(0.0457)	(0.0650)	(0.0442)	(0.0453)	(0.0401)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1,765	1,765	1,765	1,765	1,765	1,765	1,765	1,765
R^2	0.0408	0.0398	0.0346	0.1094	0.0846	0.0402	0.0389	0.0436
Adjusted R^2	0.0088	0.0078	0.0023	0.0796	0.0541	0.0081	0.0068	0.0117

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Student Representative, (b) Class Representative, (c) School Magazine, (d) Drama or Dance Group, (e) Choir or Orchestra, (f) Sports Group, (g) Other Voluntary Group, (h) None

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

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	Scholastic Involvement							
Regressors	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.5: Baseline Results - Political Interest

Regressors	Political Interest (a)	(b)	(c)	(d)	(e)
Reform	-0.0373	-0.0670	0.1631**	-0.0587*	-0.1043*
	(0.0329)	(0.0480)	(0.0609)	(0.0282)	(0.0518)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,315	2,315	2,315	2,315	2,315
R^2	0.0510	0.0593	0.0535	0.0562	0.0966
Adjusted R^2	0.0249	0.0334	0.0275	0.0302	0.0717

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Strong, (b) Fair, (c) Weak, (d) None, (e) Modest

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

A potential explanation for this differential impact on political interest is that the reform crowds out political interest on one side of the spectrum, namely for those already interested in politics, while at the same time encouraging others on the other side to become politically active, especially those who have not been so previously, for example, by joining a protest group or party that opposes the reform. In fact, the reform has sparked considerable controversy amongst students, parents, and educators alike (some in anticipation of the adverse effects presented in this study), and continues to do so today. This has led some federal states to announce its revocation, and others like the federal state of *Rhineland-Palatine* not to implement it in the first place.

How does the reform affect student subjective well-being? Table 5.12 in Section 5.6 sheds light on this question: we observe that it has a negative but insignificant effect on the life satisfaction of students. This might suggest that, although the reform significantly reduced the available leisure time of students, in order to remain on the same welfare level, students reacted by reducing their commitment to voluntary activities, which might alleviate any life satisfaction reducing time pressures but, at the same time, might also have a negative impact on life satisfaction itself, as volunteering is often found to be positively associated with subjective well-being (Binder and Freytag 2013; Meier and Stutzer 2008). The former effect, however, seems to dominate the latter.

5.4.2 Robustness Checks

In the following, we conduct a number of robustness checks to confirm the robustness of our baseline results. Specifically, we test whether they remain robust to a different model specification, time trends, and seasonal variation; selection and implementation; and potentially confounding other reforms that are implemented during the observation period. We also conduct a series of placebo tests. All robustness checks build on our preferred specification, column (4) in Table 5.3. For the sake of brevity, we focus on volunteering, our main variable of interest.

5.4.2.1 Model Specification, Time Trends, and Seasonal Variation

First, we turn to a different model specification. In column (1) of Table 5.6, we use a probit instead of a linear model. As can be seen, the reform still has a negative effect on volunteering, which is significant at the 1% level. The size of the coefficient, however, is slightly larger.

Figure 5.4 suggests that some of the decline in volunteering during the observation period probably would have come about in the absence of the reform, which raises the question to what extent the identified reform effect is driven by time trends. To be clear, this is not a threat to our identification strategy as long as time trends do not affect treatment and control group differentially, and time trends are not correlated with the outcome. To explore this possibility nevertheless, in columns (2) and (3) of Table 5.6, we include a linear and quadratic time trend, respectively. Then, in column (4), we include both of them at the same time. As can be seen, the reform still has a negative effect on volunteering, which is significant at the 1% level, across all models, and the size of the coefficients is very similar. We go even one step further: in column (5), we include both state-specific linear and quadratic time trends, counting up the years for each state individually, and in column (6), we include both treatment-specific linear

Table 5.6: Robustness Checks 1 of 4 (Model Specification/Time Trends/Seasonal Variation) - Pro-Social Behaviour, *Outside of School*

	Volunteering							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	-0.0653***	-0.0568***	-0.0568***	-0.0581***	-0.0434*	-0.0619**	-0.0580***	-0.0645***
	(0.0145)	(0.0149)	(0.0147)	(0.0145)	(0.0204)	(0.0285)	(0.0143)	(0.0151)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,010	2,010	2,010	2,010	2,010	2,010	2,010	2,010
(Pseudo) R^2	0.0630	0.0770	0.0776	0.0780	0.0805	0.0955	0.0771	0.0805
Adjusted R^2		0.0481	0.0488	0.0486	0.0453	0.0551	0.0472	0.0468

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(1) Probit Model (Marginal Effect), (2) Adds Linear Trend, (3) Adds Quadratic Trend, (4) Adds Linear and Quadratic Trends, (5) Adds State-Specific Linear and Quadratic Trends, (6) Adds Treatment-Specific Linear and Quadratic Trends, (7) Adds Quarterly Dummy Variables, (8) Adds Monthly Dummy Variables

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

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	Volunteering							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

and quadratic time trends, counting up the years for each state individually starting from the interview year in which the first observations of the treated amongst the interviewed become available. Arguably, both specifications are very restrictive in the sense that they take out much variation in the data, which is in part reflected by lower significance levels. The point estimates remain, nevertheless, quite robust.

Finally, we turn to seasonal variation. Again, this is not a threat to our identification strategy as long as treatment and control group are not systematically interviewed at different dates, and interview dates are not correlated with the outcome. To explore this possibility nevertheless, in columns (7) and (8) of Table 5.6, we include quarterly and monthly dummy variables, respectively. As expected, the sign, size, and significance level of the reform effect in both models is very similar to that in our preferred specification.[163]

5.4.2.2 Selection and Implementation

Next, we turn to selection, which may come in two flavours: within-sample and out-of-sample selection. First, students may self-select from the treatment into the control group within the sample, for example, by moving from one federal state to another in order to avoid the reform.[164] Alternatively, students may self-select out of the sample altogether, for example, by dropping out of high school. To be clear, this is not a threat to our identification strategy as long as self-selection is not correlated with the outcome. Assuming that students who move or drop out are those who are most adversely affected by the reform, our estimates are downward biased and can be interpreted as a lower bound.

We believe that within-sample selection is unlikely to be an issue: moving from one federal state to another is associated with high monetary and non-monetary costs for both students and parents. Besides, geographic mobility in Germany is traditionally low: in a given year, only about 6% of respondents in the SOEP move. This is even more so the case in a selective sample like ours, comprising families with children that attend high school: in a given year, only about 3% of them move. Nevertheless, in column (1) of Table 5.7, we evaluate how movers affect our estimates: here, we exclude all students who move during the observation period. As

163. One might argue that, at the time of interview, students in the treatment group are relatively closer to their high school finals than those in the control group, which might, in turn, partially or even fully account for the identified reform effect. To rule out this non-random measurement error, we follow the approach by Dahmann and Anger 2014, restricting our sample to students aged 17 and interacting our main effect with monthly dummy variables. We do not find a clear pattern in terms of sign, size, and significance level for these interactions; the point estimate of the main effect remains robust, but its significance is greatly reduced, most likely due to loss of observations (about a quarter of our sample). We take this as evidence that non-random measurement error due to time of interview is, if anything, a minor issue.

164. Implicitly, we assume that students self-select from the treatment into the control group, as they have a preference to avoid the reform. To be more precise, it is unlikely that students themselves *self*-select; rather, it is their parents who – probably after joint decision-making with their children – decide on taking this action. For simplicity, we refer to students throughout.

it turns out, this does not change our estimates much: the reform still has a negative effect on volunteering, which is significant at the 1% level; the size of the effect is somewhat reduced.[165] A more serious problem arises, however, for students living close to a state border: rather than move to avoid the reform, they may transfer to a school in a neighbouring state that has not yet implemented it, and commute. In column (2) of Table 5.7, we exclude all students who live within a 10km radius to a state border (about 27%).[166] As it turns out, the size of the effect becomes larger, presumably since some of these students are allocated to the treatment group although, in fact, they should be allocated to the control group.[167]

Rather than geographically sorting between schools, students may also sort within them, for example, by skipping a grade in order to avoid the reform. Unfortunately, we do not have information on whether a student skipped a grade. We argue, however, that sorting within schools is more of a theoretical problem for three reasons: first, in general, skipping a grade is not entirely discretionary to students, and requires considerable effort in terms of previous academic achievement. Second, those students that are allowed to skip a grade are presumably those that are the least affected by the reform, and thus have the lowest incentive to avoid it. Finally, skipping a grade leads students to graduate in the same cohort as their former peers, which – in terms of time to graduation – has no advantage. Moreover, as we argue below, this double cohort has certain features that render grade-skipping to avoid the reform an unattractive strategy. Related, students may also sort within schools by repeating a grade. Although this is not a feasible strategy to avoid the reform, it could nevertheless affect our estimates, as students could switch from the control to the treatment group. Assuming that students who must repeat a grade under the old regime are likely to struggle even more under the new one, omitting them would bias our estimates downwards. Again, this issue applies only to a small subset of students, namely those that are in the last pre-treatment cohorts preceding the first treatment ones. Nevertheless, in column (3) of Table 5.7, we dig deeper into this issue: here, we exclude all students who repeat a grade (about 7%). We find that the reform still has a negative effect on volunteering, which is significant at the 1% level. As expected, the size of the effect is somewhat reduced.[168]

165. In column (a) of Table 5.15 in Section 5.6, we regress the probability of moving on the reform: the effect is small and insignificant. We take this as evidence that the reform has no effect on moving behaviour *per se*.

166. Similar results are obtained when using a 20 or a 30km radius.

167. Related, a staggered self-selection of federal states is also thinkable: first, they decide on whether to implement the reform or not; then, they decide on when to implement it. Again, as long as self-selection is not correlated with the outcome, this does not threaten our identification strategy. Moreover, Dahmann and Anger 2014 convincingly show that federal states which implement the reform early do not systematically differ from those that do so late regarding their proportion of high school students, governing party, next election date, and GDP per capita.

168. As with moving, in column (b) of Table 5.15 in Section 5.6, we regress the probability of repeating a grade on the reform: the effect is small and insignificant.

Table 5.7: Robustness Checks 2 of 4 (Selection/Implementation) - Pro-Social Behaviour, *Outside of School*

	Volunteering							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	-0.0482***	-0.0845**	-0.0408***	-0.0556***	-0.0566***	-0.0893***	-0.0589***	-0.0690***
	(0.0154)	(0.0357)	(0.0128)	(0.0158)	(0.0151)	(0.0302)	(0.0143)	(0.0188)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1,947	1,469	1,866	1,840	2,010	2,010	2,010	2,010
R^2	0.0740	0.0942	0.0829	0.0735	0.0768	0.0770	0.0767	0.0768
Adjusted R^2	0.0446	0.0569	0.0524	0.0423	0.0479	0.0481	0.0478	0.0479

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(1) Excludes Individuals Who Move, (2) Excludes Individuals Who Live Within 10 km to State Border, (3) Excludes Individuals Who Repeat Grade, (4) Excludes Individuals Who Drop Out, (5) Includes Dummy Variable for Double Cohorts, (6) Includes Dummy Variable for First Treatment Cohorts, (7) Includes Dummy Variable for Special Treatment Cohorts in Saxony-Anhalt and Mecklenburg-West Pomerania, (8) Includes Dummy Variable for Last Pre-Treatment Cohorts

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

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	Volunteering							
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Finally, we turn to out-of-sample selection: clearly, if dropping out of high school were a deliberate strategy to avoid the reform, it would be the one with the highest opportunity costs, as students would effectively forego their university entrance certificate. In column (4) of Table 5.7, we evaluate how drop-outs affect our estimates: here, we exclude all students who drop out of high school (about 8%). As it turns out, the sign, size, and significance level of the effect is very similar to that in our preferred specification.[169]

Although the reform has been swiftly integrated into the German secondary education landscape, there may have been various implementation effects – confounding one-off effects arising from the implementation of the reform into regular school business. This is particularly true for students in double, first treatment, and last pre-treatment cohorts, across all federal states.[170] Moreover, in the federal states of *Saxony-Anhalt* and *Mecklenburg-West Pomerania*, students in the first treatment cohorts had already started school when the reform was implemented. For example, for students in the double cohort, such implementation effects may be due to increased competition for educational and post-educational resources; for students in the first treatment cohort, they may be due to inexperience of teachers in delivering material at a faster pace, or insecurity on side of students; and for students in the last pre-treatment cohort, they may be due to increased motivation not to repeat a grade, and be affected by the reform. On the other hand, teachers may treat students in these cohorts in a more easy way. Although it is unlikely that such implementation effects are the driving force behind the aggregate effect, they can still affect our estimates.

In columns (5) to (8) of Table 5.7, we explore this possibility in more detail: here, we include state-specific controls individually for students in double, first treatment, and last pre-treatment cohorts, as well as for students in the first treatment cohorts in the federal states of *Saxony-Anhalt* and *Mecklenburg-West Pomerania*. If anything, we find that controlling for cohorts that might suffer from implementation effects slightly increases the aggregate effect in our preferred specification. Confounding implementation effects, therefore, seem to be less of an issue.[171]

169. Once again, in column (c) of Table 5.15 in Section 5.6, we regress the probability of dropping out on the reform: the effect is small and insignificant. We take this as evidence that the reform has no effect on dropping out. This is in line with Huebener and Marcus 2015 who find that the reform does not affect drop-out rates.

170. We define the first treatment cohorts as the cohorts succeeding the double cohorts in order to avoid mixing up implementation effects.

171. In column (3) of Table 5.14 in Section 5.6, we go even one step further and control for all cohorts that might suffer from implementation effects at the same time: the result remains the same.

5.4.2.3 Other Reforms

Over the past two decades, there have been various other reforms in the German secondary education landscape, some of which fall into the observation period, and could potentially be confounding.[172] For example, having long been standard in the majority of states, the remainder has only recently moved towards state-wide harmonised high school finals by introducing central exit examinations. Others, trying to open up the traditionally less permeable and rigid German education system, introduced changes to the grade at which tracking takes place, or reduced tracking altogether by combining the lower and intermediate tracks into a single one. Yet others have introduced changes to the choice of subjects available to high school seniors. Probably the biggest change in recent decades, however, has been the abolishment of mandatory military or civil service right after finishing secondary education: in 2011, it was replaced with the (non-mandatory) Federal Volunteer Service.

To be clear, it is unlikely that any of these reforms systematically biases our estimates for two reasons: first, it would have to be correlated with the outcome. More importantly, however, it would have to affect the treatment and control groups differentially. This would be the case if reforms were correlated, for example, if reducing the number of years required to obtain a high school degree went hand in hand with restricting the subject choice available to high school seniors. Alternatively, one could argue that states which are more prone to reform may be the first to reduce the number of high school years, and may also be inclined to introduce other reforms shortly after, or the other way around.

To rule out this possibility, in columns (1) to (5) of Table 5.8, we include state-time-specific controls for these potentially confounding other reforms. As expected, the sign, size, and significance level of the coefficients is very similar to that in our preferred specification. Likewise, excluding students who have already graduated, and who might thus be participating in the Federal Volunteer Service, leaves results unchanged (see column (b) of Table 5.16 in Section 5.6 for this result). Confounding other reforms, therefore, seem to be less of an issue.[173]

5.4.2.4 Placebo Tests

Finally, as a last exercise, we conduct placebo tests: in columns (1) and (2) of Table 5.9, we lag the first treatment cohort by one and two, respectively; in columns (3) and (4), we randomly allocate treatment status to school cohorts and federal states, respectively, keeping the other constant. Finally, in column (5), we completely perturb both school cohorts and federal states,

172. See Huebener and Marcus 2015 for a detailed overview of these reforms.
173. In column (4) of Table 5.14 in Section 5.6, we go even one step further and control for all potentially confounding reforms at the same time: the result remains the same.

Table 5.8: Robustness Checks 3 of 4 (Other Reforms) - Pro-Social Behaviour, *Outside of School*

	Volunteering				
Regressors	(1)	(2)	(3)	(4)	(5)
Reform	-0.0529***	-0.0559***	-0.0563***	-0.0576***	-0.0550***
	(0.0164)	(0.0193)	(0.0138)	(0.0128)	(0.0161)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,010	2,010	2,010	2,010	2,010
R^2	0.0769	0.0768	0.0769	0.0767	0.0772
Adjusted R^2	0.0480	0.0479	0.0480	0.0478	0.0483

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(1) Includes Dummy Variable for Changes in Central Exit Examinations, (2) Includes Dummy Variable for Changes in Tracking at Grade Seven, (3) Includes Dummy Variable for Changes in Two-Tier System, (4) Includes Dummy Variable for Changes in Subject Choice, (5) Includes Dummy Variable for Federal Volunteer Service

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.9: Robustness Checks 4 of 4 (Placebo Tests) - Pro-Social Behaviour, *Outside of School*

	Volunteering				
Regressors	(1)	(2)	(3)	(4)	(5)
Reform	-0.0113	-0.0223	0.0013	0.0201	-0.0113
	(0.0290)	(0.0342)	(0.0231)	(0.0252)	(0.0261)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	1,966	1,929	2,010	2,010	2,010
R^2	0.0773	0.0801	0.0766	0.0761	0.0761
Adjusted R^2	0.0503	0.0511	0.0482	0.0475	0.0488

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(1) Placebo School Cohorts (c-1), (2) Placebo School Cohorts (c-2), (3) Placebo School Cohorts (Random), (4) Placebo Federal States (Random), (5) Placebo School Cohorts and Federal States (Random)

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

and then randomly allocate treatment status. As can be seen, none of the coefficients is significant at any conventional level. For the first two columns, we can see that the coefficients are negative, pointing towards the overall trend reversal in volunteering we see during the observation period; the fact that the coefficient of the second column is slightly larger than that of the first one suggests that there are no ex-ante behavioural changes due to anticipation effects (Ashenfelter's dip). Note that in both of these columns, we lose observations that fall out of the observation period window. For the last three columns, we cannot observe a clear pattern of coefficients.

5.5 Discussion and Policy Implications

In sum, we find robust empirical evidence that raising instructional time (and thereby reducing time available for leisure) has significantly crowded out student pro-social behaviour, a behaviour that is linked to various positive outcomes – both at the societal and individual level – and that parents, educators, and policy-makers alike would otherwise consider worth promoting. In the given context, an about 13 percent rise in weekly instructional hours had a negative and sizeable effect on volunteering, decreasing the share of students that volunteer at least once a month by about 18 percent. In other words, it led almost every fifth student to change her behaviour from volunteering at least monthly to volunteering less often or not at all. Students that volunteer on a regular basis are most adversely affected, and there is evidence that students with lower educated parents are up to three times more likely to disengage. While half of students cut back on their activities, the other half give them up completely. Impacts on student subjective well-being are negative but insignificant, potentially pointing towards the fact that, in order to remain on the same welfare level and avoid any time pressure, students reduced their engagement in voluntary activities. Since voluntary activities are positively associated with subjective well-being, this might explain the negative coefficient. We find no similar crowding out of involvement in activities within school, but no substitution either. Finally, there is some evidence that raising instructional time also has the potential to affect political interest, which we take as a proxy for political behaviour.

Naturally, the question arises whether the results are driven by an increase in the intensity of instruction or a decrease in the availability of leisure time. The available evidence clearly suggests that the number of weekly instructional hours has increased thus decreasing the number of weekly available leisure hours, as opposed to implementing the reform through a rise in the pace of instruction while leaving the number of weekly available leisure hours constant

(Homuth 2017). A reduction in the number of weekly available leisure hours then reduces the time available for volunteering activities. Another way to look at whether this is true is to standardise (i.e. divide) our primary, time-dependent outcome – volunteering at least once per month – by the available leisure time per month:[174] 24×7 week hours in total minus about 6×5 week hours in school under the old regime equals 138 hours in available time per week or 552 hours in available time per month *versus* 24×7 week hours in total minus about 7×5 week hours in school under the new regime equals 133 hours in available time per week or 532 hours in available time per month.[175] Table 5.20 in Section 5.6 replicates our baseline results for our primary outcome standardised by the available leisure time per month: as expected, the coefficients remain significant but are greatly attenuated, suggesting indeed that the results are driven by a decrease in the availability of leisure time.[176] This is in accordance with the fact that, in our baseline specification, the likelihood to volunteer at least once per month decreases by about 18 percent as the number of weekly instructional hours increases by about 13 percent – an almost linear relationship.

Why are these findings important? First of all, in the given context, they are important because of the large number of students affected. In Germany, in school year 2013/14 alone, of 2,329,990 high school students in total (Federal Statistical Office 2016b), about 786,000 volunteer at least monthly. We estimate that the rise in instructional time decreases this share by about 134,000: 75,000 cut back on their activities, and 59,000 give them up completely. It is difficult to measure the economic value of volunteering for society: there exist various definitions of volunteering, and at least as many ways to measure it, for example through national accounts, labour force surveys, or social or time use surveys. It is clear, however, that this value is substantial.[177] Through time use surveys, the OECD estimates the economic value of volunteering for Germany in 2013 to be around USD 117.6 billion or 3.3% of real GDP (Organisation for Economic Co-operation and Development 2015).[178] We can calculate back-of-the-envelope that losing between 59,000 and 134,000 volunteers is equal to losing volunteer

174. Standardisation by the available time per week leads to similar but somewhat larger coefficients as the available time per month is divided by four weeks for both students who are affected and students who are not affected by the reform.

175. Implicitly, this accounts for sleeping time as there could, hypothetically, be substitution effects with sleep.

176. Needless to say, standardisation by the available leisure time introduces measurement error: if a respondent indicates to volunteer weekly, this could mean that she volunteers once a week up to, theoretically, six times a week, given that the next higher category is volunteering daily (at, on average, 3.5 volunteering events per week the respondent might make the mental switch and indicate to volunteer daily). The same holds true for volunteering monthly. At the same time, there is only a very small share of students that volunteer daily (they are also the least affected), which would be the most precise measure.

177. See The Economist 2014 for a recent feature.

178. This figure is roughly comparable to the UK (2.5%) and to the US (3.7 %).

work worth between USD 85.9 million and USD 195 million.[179] These figures are likely to be a lower bound for two reasons: first, volunteering in the general population is less prevalent than in the population under scrutiny.[180] Second, to the extent that volunteering during youth and adolescence contributes to habit formation (Hart et al. 2007) and has positive peer effects (Wilson and Musick 1997), impacts may be permanent rather than temporary. Besides these negative effects for society *per se*, the decrease in volunteer work can also have negative micro implications: a growing body of evidence documents the importance of volunteering for individual labour market outcomes. For example, in a recent correspondence testing study, Baert and Vujić 2016 show that job seekers who indicate volunteering on their resumes receive one third more interview invitations, and that this volunteering premium is higher for women. A leading professional social network, LinkedIn 2016, using data on members, estimates that one in five managers hire someone because of their volunteer experience. Sauer 2015, using a structural model and longitudinal data for the US, estimates that an extra year of pro-social engagement increases wage offers in future full-time (part-time) work by 2.6% (8.5%) for women aged 25 to 55, in line with Freeman 1997 who estimates that volunteering raises paid work hours by between 3% and 7%. There is evidence that being engaged from an early age on enhances psychological development by raising self-esteem and self-confidence and by discouraging risky behaviours (Hart et al. 2007; Wilson and Musick 2012). The physical and mental health benefits of volunteering (Wilson and Musick 2012), as well as its subjective well-being returns are well established (Binder and Freytag 2013; Meier and Stutzer 2008). Finally, to the extent that students from disadvantaged backgrounds are disproportionally affected, the role that volunteering can play in the production process of skills, for example, through generating early life skills that complement other skills later on (Cunha and Heckman 2007), or in the selection process for further education, as is for example the case in the German scholarship system or for admissions to US colleges, the decrease in volunteering for these groups might further increase educational inequalities, and thus inequalities in later life outcomes.[181]

To be clear, we are not advocating that raising instructional time is a bad idea *per se*: it is often found to have positive impacts on student learning and performance, especially

179. There were 80.8 million people living in Germany in 2013 (Federal Statistical Office 2016a). Thus, assuming the distribution of activities in the general population is similar to that in the population under scrutiny, the loss in volunteer work can be calculated as $(117,600,000,000 \times 59,000)/80,800,000$ and $(117,600,000,000 \times 134,000)/80,800,000$, respectively, for the 59,000 students giving up and for the 134,000 students cutting back *and* giving up their activities.

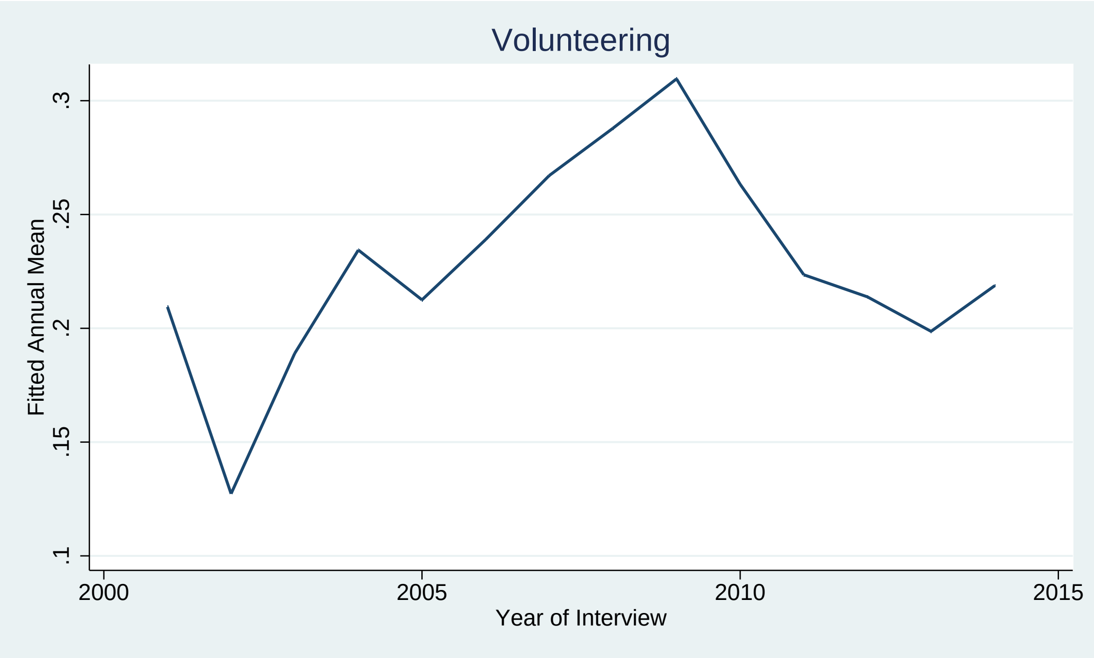
180. In the general population, only about 23% of individuals report to volunteer at least once a month, according to the OECD. In the SOEP, this share is even lower: 20%. Again, both figures are roughly comparable to the UK (18%) and to the US (30%).

181. See Behavioural Insights Team 2016 for a recent impact evaluation of programmes that promote social action: it shows that such programmes can nurture skills such as empathy or grit that are critical for educational success.

when the additional time is used effectively, and there surely is an optimal amount of weekly instructional hours that balances student learning with student leisure activities and behaviours. For a more complete cost-benefit account of raising instructional time, however, its impacts on student leisure activities and behaviours, in particular on beneficial behaviours such as volunteering, should be taken into account. Education policy could consider, for example, providing volunteering opportunities such as high school community service within schools, or encouraging it through the curriculum, for example, by introducing volunteering days.

There are many limitations to this study, which is only a cautious exploration into the relationship between instructional time and student pro-social behaviour. The most obvious is that we cannot say anything about how persistent the identified effects are. The fact that controlling for graduation status reduces the size of the coefficient estimates only slightly suggests that they are rather permanent, though, in line with findings on habit formation (Hart et al. 2007). Once more data become available, it would be interesting to test this formally. External validity is another issue. The fact that the UK and the US exhibit similar profiles regarding instructional time and volunteering demographics than Germany (Bureau of Labor Statistics 2015; Organisation for Economic Co-operation and Development 2015) might point towards the fact that findings are rather transferable.

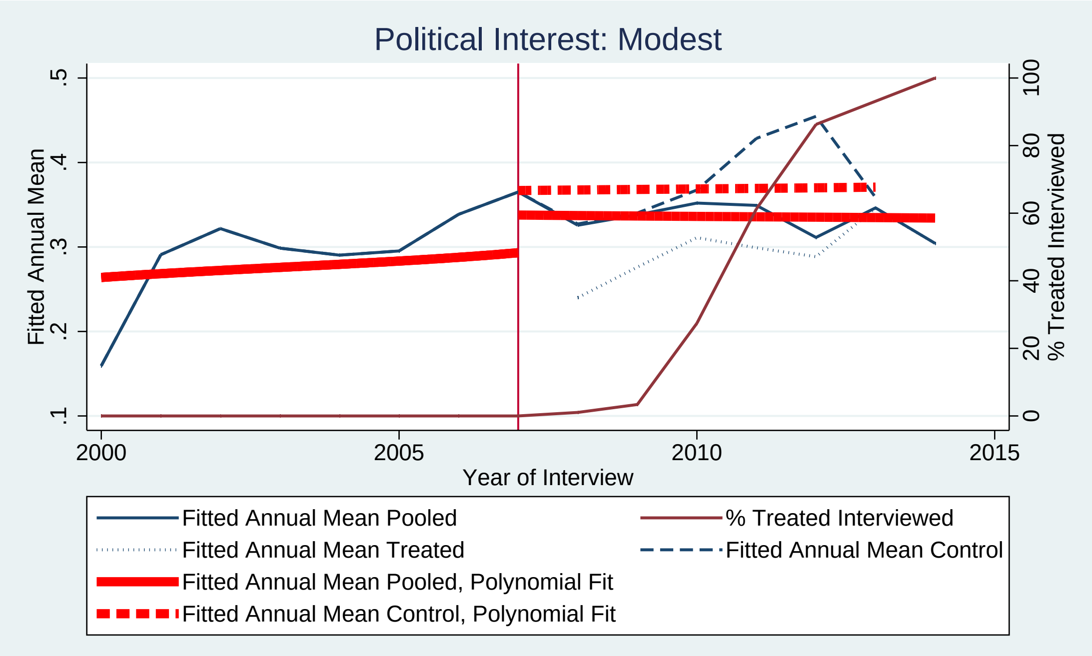
5.6 Online Appendix to Chapter 5



Note: The figure shows the fitted annual mean of volunteering, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (lower and intermediate track) aged 17 to 20, own calculations.

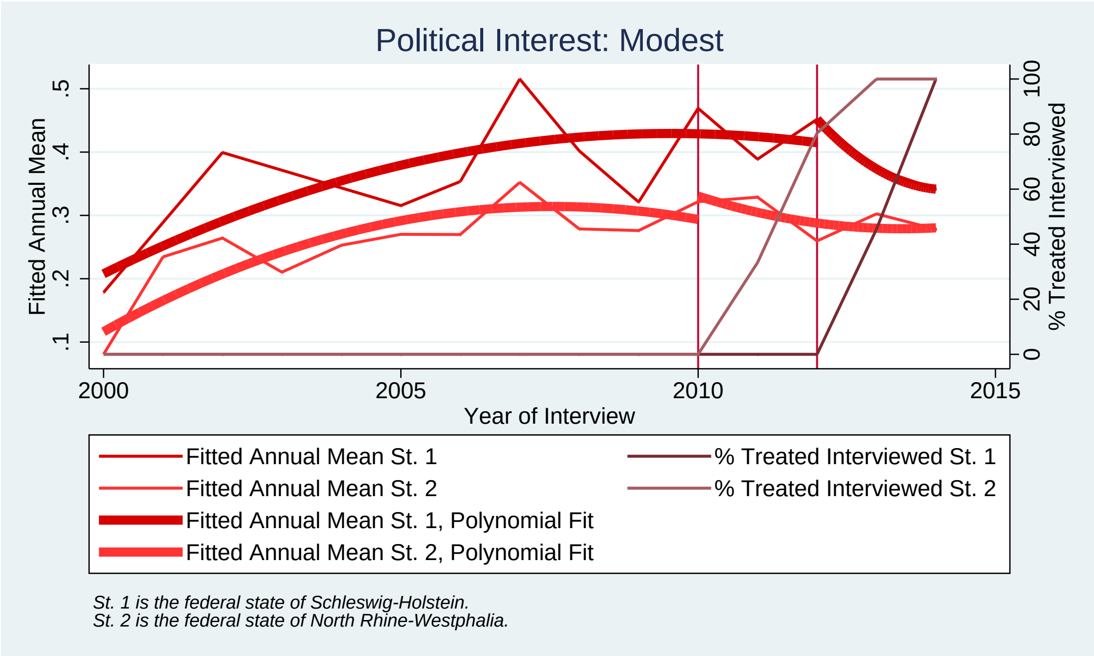
Figure 5.9: Graphical Evidence - Volunteering, Over Time



Note: The figure shows the fitted annual mean of political interest, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.10: Graphical Evidence - Political Interest, Over Time



Note: The figure shows the fitted annual mean of political interest, covariate-adjusted for demographic, educational, and parental household characteristics, as well as for sub-samples and a full set of federal state and school cohort dummy variables. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Figure 5.11: Graphical Evidence - Political Interest, Common Trend

5.6.1 Data

Table 5.10: Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Dependent Variables					
Pro-Social Behaviour, Outside of School					
Volunteering	0.3373	0.4729	0	1	2,010
Pro-Social Behaviour, Inside of School					
Scholastic Involvement: Student Representative	0.0266	0.1610	0	1	1,765
Scholastic Involvement: Class Representative	0.4164	0.4931	0	1	1,765
Scholastic Involvement: School Magazine	0.1014	0.3020	0	1	1,765
Scholastic Involvement: Drama or Dance Group	0.1994	0.3997	0	1	1,765
Scholastic Involvement: Choir or Orchestra	0.3275	0.4694	0	1	1,765
Scholastic Involvement: Sports Group	0.2776	0.4480	0	1	1,765
Scholastic Involvement: Other Voluntary Group	0.3700	0.4829	0	1	1,765
Scholastic Involvement: None	0.1894	0.3920	0	1	1,765
Political Interest					
Political Interest: Strong	0.0639	0.2447	0	1	2,315
Political Interest: Fair	0.2644	0.4411	0	1	2,315
Political Interest: Weak	0.5227	0.4996	0	1	2,315
Political Interest: None	0.1490	0.3562	0	1	2,315
Political Interest: Modest	0.3283	0.4697	0	1	2,315

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Variables	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Subjective Well-Being					
Life Satisfaction	7.7603	1.4762	0	10	2,010
Independent Variables					
Reform	0.3791	0.4853	0	1	2,010
Age	17.5105	0.9302	17	20	2,010
Has Graduated	0.0383	0.1920	0	1	2,010
Is Female	0.5338	0.4990	0	1	2,010
Has Migration Background	0.1935	0.3952	0	1	2,010
Lives in East	0.1284	0.3346	0	1	2,010
Lives in Countryside	0.2831	0.4506	0	1	2,010
Parent Has Tertiary Degree	0.5259	0.4995	0	1	2,010
Parent is Blue-Collar Worker	0.1264	0.3323	0	1	2,010
Parent is Full-Time Employed	0.6532	0.4761	0	1	2,010
Parent is Single	0.1940	0.3956	0	1	2,010
Is Only Child	0.1701	0.3759	0	1	2,010
Number of Children in Household	2.4114	1.1009	1	12	2,010

Note: See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

5.6.2 Baseline Results

Table 5.11: Baseline Results - Pro-Social Behaviour, *Outside of School*

	Volunteering			
Regressors	(1)	(2)	(3)	(4)
Reform	-0.0661***	-0.0592***	-0.0590***	-0.0579***
	(0.0126)	(0.0128)	(0.0132)	(0.0144)
Age 17		-0.0454	-0.0382	-0.0373
		(0.0478)	(0.0446)	(0.0373)
Age 18		-0.0260	-0.0186	-0.0165
		(0.0296)	(0.0287)	(0.0294)
Age 19		0.0432	0.0493	0.0538
		(0.0452)	(0.0486)	(0.0535)
Has Graduated			-0.0178	-0.0236
			(0.0333)	(0.0419)
Is Female				-0.0213
				(0.0246)
Has Migration Background				-0.0836*
				(0.0421)
Lives in East				-0.1807***
				(0.0328)
Lives in Countryside				0.0026
				(0.0274)
Parent Has Tertiary Degree				0.0597**
				(0.0202)
Parent is Blue-Collar Worker				-0.0891**
				(0.0326)
Parent is Full-Time Employed				0.0059
				(0.0201)
Parent is Single				-0.0706***
				(0.0178)
Is Only Child				-0.0030
				(0.0384)
Number of Children in Household				0.0209
				(0.0137)
Number of Observations	2,010	2,010	2,010	2,010
R^2	0.0517	0.0537	0.0538	0.0767
Adjusted R^2	0.0295	0.0301	0.0296	0.0483

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.12: Baseline Results - Life Satisfaction

Regressors	Life Satisfaction
Reform	-0.2714
	(0.2323)
Demographic Characteristics [a]	Yes
Parental Characteristics	Yes
Household Characteristics	Yes
Number of Observations	2,010
R^2	0.0584
Adjusted R^2	0.0205

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

Robust standard errors clustered at the federal state level in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

5.6.3 Heterogeneous Results

Table 5.13: Heterogeneous Results (Students With Lower Educated Parents) - Pro-Social Behaviour, *Outside of School*

Regressors	Volunteering
Reform × Lower Educated Parents	-0.1017**
	(0.0462)
Lower Educated Parents	0.0023
	(0.0333)
Reform	-0.0520***
	(0.0129)
Demographic Characteristics [a]	Yes
Parental Characteristics	Yes
Household Characteristics	Yes
Number of Observations	2,010
R^2	0.0775
Adjusted R^2	0.0482

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. We define students in households with lower educated parents as students who reside in households where at least one parent has less than a secondary degree. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

5.6.4 Robustness Checks

Table 5.14: Robustness Checks (1/6) - Pro-Social Behaviour, *Outside of School*

	Volunteering				
Regressors	(1)	(2)	(3)	(4)	(5)
Reform	-0.0665***	-0.0578***	-0.1781**	-0.0451*	-0.1016***
	(0.0192)	(0.0115)	(0.0682)	(0.0232)	(0.0343)
Cohort 1					
Cohort 2					
Cohort 3					
Cohort 4					
Cohort $\geq$ 5					
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,010	2,010	2,010	2,010	2,010
R^2	0.0774	0.0775	0.0780	0.0775	0.0774
Adjusted R^2	0.0490	0.0769	0.0476	0.0467	0.0470

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(1) Uses Weights, (2) Uses Bootstrapped Standard Errors, (3) Includes All Dummy Variables From 5 to 8 in Table 5.6, (4) Includes All Dummy Variables From 1 to 5 in Table 5.7, (5) Adds Cohort Dummy Variables

Robust standard errors clustered at the federal state level in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

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	Volunteering				
Regressors	(1)	(2)	(3)	(4)	(5)

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.15: Robustness Checks (2/6) - Selection and Implementation Effects

			Probability
Regressors	(a)	(b)	(c)
Reform	0.0168	0.0386	-0.0135
	(0.0130)	(0.0334)	(0.0232)
Demographic Characteristics [a]	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes
Number of Observations	2,010	2,010	2,010
R^2	0.0590	0.0785	0.2932
Adjusted R^2	0.0300	0.0501	0.2714

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Moving, (b) Repeating Grade, (c) Dropping Out

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.16: Robustness Checks (3/6) - Pro-Social Behaviour, *Outside of School*

	Volunteering	
Regressors	(a)	(b)
Reform	-0.0537***	-0.0524***
	(0.0150)	(0.0147)
Demographic Characteristics [a]	Yes	Yes
Parental Characteristics	Yes	Yes
Household Characteristics	Yes	Yes
Number of Observations	2,010	1,933
R^2	0.0775	0.0756
Adjusted R^2	0.0486	0.0464

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated* (drops out in last column)

(a) Controls for Between-Survey Differences, (b) Excludes Graduates

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. Column (a) controls for between-survey differences by including a control for the respective survey. Column (b) restricts the samples to current students. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.17: Robustness Checks (4/6) - Political Interest, Controlling for Federal State Elections

	Political Interest				
Regressors	(a)	(b)	(c)	(d)	(e)
Reform	-0.0383	-0.0674	0.1644**	-0.0587*	-0.1057*
	(0.0317)	(0.0494)	(0.0625)	(0.0281)	(0.0541)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,315	2,315	2,315	2,315	2,315
R^2	0.0536	0.0595	0.0547	0.0562	0.0980
Adjusted R^2	0.0271	0.0331	0.0283	0.0298	0.0728

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Strong, (b) Fair, (c) Weak, (d) None, (e) Modest

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All models include dummy variables for federal state elections in the respective years. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.18: Robustness Checks (5/6) - Political Interest, Controlling for Federal Elections

	Political Interest				
Regressors	(a)	(b)	(c)	(d)	(e)
Reform	-0.0398	-0.0735	0.1717**	-0.0584*	-0.1133**
	(0.0335)	(0.0457)	(0.0569)	(0.0285)	(0.0499)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,315	2,315	2,315	2,315	2,315
R^2	0.0522	0.0620	0.0573	0.0562	0.1011
Adjusted R^2	0.0257	0.0358	0.0309	0.0298	0.0759

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Strong, (b) Fair, (c) Weak, (d) None, (e) Modest

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All models include dummy variables for federal elections in the respective the years. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.19: Robustness Checks (6/6) - Political Interest, Controlling for Both Federal State and Federal Elections

	Political Interest				
Regressors	(a)	(b)	(c)	(d)	(e)
Reform	-0.0409	-0.0741	0.1734**	-0.0584*	-0.1150**
	(0.0322)	(0.0473)	(0.0583)	(0.0284)	(0.0520)
Demographic Characteristics [a]	Yes	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,315	2,315	2,315	2,315	2,315
R^2	0.0550	0.0623	0.0587	0.0562	0.1028
Adjusted R^2	0.0281	0.0356	0.0319	0.0293	0.0772

[a] Including *Age 17*, *Age 18*, *Age 19*, and *Has Graduated*

(a) Strong, (b) Fair, (c) Weak, (d) None, (e) Modest

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All models include dummy variables for both federal state and federal elections in the respective years. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

Table 5.20: Additional Results - Pro-Social Behaviour, *Outside of School*, Standardised by Available Leisure Time Per Month

	Volunteering			
Regressors	(1)	(2)	(3)	(4)
Reform	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age 17		-0.0001	-0.0001	-0.0001
		(0.0001)	(0.0001)	(0.0001)
Age 18		-0.0000	-0.0000	-0.0000
		(0.0001)	(0.0001)	(0.0001)
Age 19		0.0001	0.0001	0.0001
		(0.0001)	(0.0001)	(0.0001)
Has Graduated			-0.0000	-0.0000
			(0.0001)	(0.0001)
Other Demographic Characteristics	No	No	No	Yes
Parental Characteristics	No	No	No	Yes
Household Characteristics	No	No	No	Yes
Number of Observations	2,010	2,010	2,010	2,010
R^2	0.0500	0.0520	0.0520	0.0751
Adjusted R^2	0.0277	0.0283	0.0278	0.0466

Robust standard errors clustered at the federal state level in parentheses

**** p<0.01, ** p<0.05, * p<0.1*

Note: All models include a constant, controls for sub-samples, and a full set of federal state and school cohort dummy variables. The outcome – volunteering at least once per month – is standardised (i.e. divided) by available time per month (standardisation by available time per week leads to similar but somewhat larger coefficients because available time per month is divided by four weeks for both students who are affected and students who are not affected by the reform): 24 × 7 week hours in total minus about 6 × 5 week hours in school under the old regime equals 138 hours in available time per week or 552 hours in available time per month *versus* 24 × 7 week hours in total minus about 7 × 5 week hours in school under the new regime equals 133 hours in available time per week or 532 hours in available time per month. The controls include age, having graduated, being female, having migration background, living in East Germany, living in the countryside, having at least one parent who has a tertiary degree, having at least one parent who is a blue-collar worker, having at least one parent who is full-time employed, having a single parent, being the only child, and the number of children in the household. All figures are rounded to four decimal places. See Section 5.2.1 for a description of the variables used.

Source: SOEP, 2001–2014, students (upper track) aged 17 to 20, own calculations.

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	Volunteering			
Regressors	(1)	(2)	(3)	(4)

Chapter 6

Conclusion

6.1 Preliminary Remarks

First of all, I would like to thank the reader who kept reading until this point. This effort shall not go entirely unrewarded. In what follows, I will wrap up this dissertation by summarising the main results of the different chapters, discussing their strengths and weaknesses, and pointing towards potential avenues for future research.

6.2 Wrap Up

6.2.1 Chapter 1: The Fukushima Daiichi Meltdown

In Chapter 1, we evaluated the impact of the Fukushima Daiichi meltdown on environmental concerns, subjective well-being, risk aversion, and political preferences in Germany, and compared them to those in Switzerland and the UK, where, in stark contrast to the shutdown of the oldest nuclear power plants in Germany and the reduction of the lifetime for the remainder, no immediate policy action occurred. To do so, we used the main representative household panels in the three countries, and estimated difference-in-differences models that exploit the exact dates of the catastrophe, and in case of Germany, of the policy action as cut-offs to allocate individuals into treatment and control group. Besides the standard identifying assumptions of difference-in-differences models – *(i)* assignment to treatment is independent of outcome conditional on both observables and unobservables (*conditional ignorability*), and *(ii)* treatment and control group would have followed a common time trend in the absence of treatment (*common* *trend assumption*) – we have to make the additional assumption that the dates of the events are unrelated to the interview dates of the respondents. In essence, this assumption is a refined version of *conditional ignorability*, implying that individuals should not strategically select into interviews in relation to outcomes. For example, individuals that are relatively more concerned about the environmental consequences of the catastrophe should not postpone their interviews. We believe that these identifying assumptions are met: the catastrophe was clearly exogenous, and in case of the Germany, the policy action arguably unanticipated, given that the same government had just extended the lifetime of nuclear power plants one year earlier. Likewise, we can show that the dates of the interviews were similarly distributed before and after the dates of the events, and that individuals before and after did not systematically differ from each other.

We did not find much evidence that subjective well-being was significantly affected in Germany, Switzerland, or the UK. However, we found that environmental concerns significantly increased among Germans. Moreover, the share of Germans who consider themselves as "very risk averse" increased significantly, in particular among people who live in close proximity to nuclear power plants or for whom the next reactor belongs to one of the eight oldest. Likewise, support for the Greens – a party that traditionally opposes nuclear power and advocates its abolishment – increased significantly in all three countries. Finally, the announcement and (partial) implementation of the exit from nuclear power in Germany led to a decrease in environmental concerns there, approximately by the same size that they had increased after the catastrophe.

On the one hand, these results are, to some degree, intuitive. On the other hand, they may cast a series of interesting points. The most obvious is that disasters do not only have negative effects locally, but can also impose negative external effects on distant countries, even if those countries are far away and presumably not directly affected. This is nothing new, and it has been shown in various contexts (for example, the impact of September 11 on mental well-being in the UK). What is new here is that these negative external effects exist even in case that the objective risk of a similar disaster does not change: clearly, the risk of an earthquake-induced (let alone earthquake-caused-tsunami-induced) nuclear disaster in Germany is limited. This points towards the importance of subjective risk perceptions or individual risk tolerance when assessing situations. What is also new here is that policy action, if credible and implemented swiftly, can alleviate or even reverse concerns in the population. This has not been studied before, largely because the given context was so unique. On the one hand, this may be a downside in terms of external validity. On the other hand, this may point towards a promising avenue for future research.

6.2.2 Chapter 2: Urban Land Use

In Chapter 2, we evaluated the impact of urban land use on residential well-being in major German cities and valued different types of urban land use monetarily using the so-called *life satisfaction approach*. To do so, we merged longitudinal household data from the German Socio-Economic Panel with cross-section data from the European Urban Atlas, and calculated different metrics of proximity to different types of urban land use. We used fixed-effects regressions with both individual and city fixed effects to have the effects identified by movers, who we can show to move mostly for reasons unrelated to their surroundings. Robustness checks excluding city fixed effects, excluding movers to have the effects identified by stayers (in a plain ordinary-least-squares framework), or regressing the likelihood to move on different types of urban land use confirmed our results.

We found that access to urban green areas is positively associated with life satisfaction, whereas access to abandoned areas is negatively associated with it. In contrast, access to forests and waters do not seem to matter much for residential well-being. The relationships are concave in nature, and in terms of effect heterogeneity, small effects at the aggregate level cast much larger effects for older residents. Finally, we calculated that there is a substantial net well-being benefit arising from reducing the undersupply of parks in major German cities.

Although our empirical strategy brings us closer to causality than previous studies, it is clear that it is far from perfect: movers may be moving mostly for reasons unrelated to their surroundings, but it may well be that, when moving anyway, they do take their surroundings into account and optimise with respect to the urban landscape around them. Unfortunately, we cannot evaluate the extent to which such *conditional* endogenous residential sorting is at play. Likewise, we cannot evaluate the quality of different types of urban land use. Both causality and quality are the big unknowns in the literature: once data on quality and plausible exogenous variations in different types of urban land use become available, there is huge potential for further research. This is even more so given the interest of urban planners and policy-makers in this topic. A further promising avenue for future research is to look at how urban landscape fragmentation, that is, the interplay of different types of urban land use and its variety, affect residential well-being. All this research, ultimately, asks the question: “What makes a happy city?”.

6.2.3 Chapter 3: Wind Turbines

In Chapter 3, we looked at energy infrastructure. Here, we evaluated the impact of wind turbines on residential well-being and valued their negative externalities monetarily using, once again, the life satisfaction approach. To do so, we merged data from the German Socio-Economic Panel with a unique and novel dataset on more than 20,000 installations in Germany, and exploited the geographical coordinates of both households and wind turbines as well as interview and construction dates in a difference-in-differences design. We focused only on large installations that are typically built by utilities rather than private persons, and applied propensity-score and spatial matching techniques based on exogenous weather data to match treatment and control group. Identification then rested on the standard assumptions that *(i)* assignment to treatment is independent of outcome conditional on both observables and unobservables (*conditional ignorability*), and that *(ii)* treatment and control group would have followed a common time trend in the absence of treatment (*common trend assumption*).

We found that the construction of wind turbines around households has a significant and sizeable negative effect on the life satisfaction of household members. However, the effect is both spatially and temporally limited (although the latter may arise as a statistical artefact due to lack of power), and greater for house owners than for renters. In fact, for renters, it turns out insignificant altogether, as they are more swiftly compensated through a reduction in real estate prices. Interestingly, the size of the negative externalities, when quantified monetarily (using the life satisfaction approach and hedonic method for house owners and renters, respectively), is similar between both groups.

Although we try to go great lengths in terms of testing our results for robustness and sensitivity, it is clear that the study has limitations. The most obvious one is that we have no information on the ownership structure of wind turbines, so that we cannot fully exclude the case that some of the installations in our final sample are built by private persons that generate monetary or non-monetary returns from them. Related, our data on view sheds and concrete visibility from places of residence is limited: the digital terrain model includes only geographical barriers to visibility such as location-specific elevated terrain, while excluding natural ones such as forests and trees as well as man-made structures such as houses and fences, all of which may equally be barriers to visibility. Both limitations, however, are in line with a lower-bound interpretation of our results. A promising avenue for future research is to transfer the methodology developed in this study to evaluating the non-monetary impacts of other infrastructure projects such as biomass plants.

6.2.4 Chapter 4: The Olympic Games

In Chapter 4, we asked: do the Olympics make people in the host city happier? To shed light on this question, we looked at the example of the 2012 Olympic Summer Games in London. More specifically, we collected panel data on the subjective well-being of Londoners, Parisians, and Berliners in the summers of 2011, 2012, and 2013, and estimated difference-in-differences models that compare the change in the subjective well-being of Londoners with that of Parisians and Berliners over time. Identification rested on the standard assumptions that, after controlling for confounders (both observable and unobservable), the subjective well-being of Londoners would have followed the same time trend as that of Parisians and Berliners in the absence of the Olympics. We provided graphical evidence that this is likely to hold, showing common pre-treatment trends in outcomes.

We found that the Olympics have a significant and sizeable positive effect on life satisfaction, and in most specifications, on happiness as well. It is only short-lived, though, vanishing after one year at the latest. We found no evidence that the identified effect is driven by relative sporting success: rather, it seems that hosting itself matters for well-being. This result turns out to be robust regardless of model specification or control group choice. Further robustness checks, including the use of a balanced panel, inverse probability weighting, and propensity-score matching to account for selection into the follow-up survey; the use of additional economic and exogenous weather controls to account for confounding factors that could induce divergent time trends; and the use of placebo regressions with both placebo outcomes and placebo time periods confirmed also confirmed it.

Clearly, the validity of our results hinges crucially on the *common trend assumption*: are the three cities really comparable to each other at the outset, and more importantly, would they have remained comparable to each other over time (that is, would a potentially uneven distribution of unobservables or bias have remained constant over time) in the absence of treatment? We believe that both is the case: although on different levels, pre-treatment outcomes move in a cointegrated manner. Moreover, controlling for changes in economic fundamentals (changes in quarterly GDP since the first quarter of 2008 and daily stock market closing values) as well as meteorological fundamentals (daily precipitation and maximum temperature) leave our results unchanged. Finally, there have been no confounding events during the relatively short study period. Still, there are limitations: the most obvious one is that our final sample is not strictly representative of the populations in London, Paris, and Berlin. Likewise, the design of our study does not allow extrapolating findings to the general UK, French, and German populations. We nevertheless believe that our study is an important contribution, both content-wise and methodologically, and a promising avenue for future research is to study the impact of the Olympics on other outcomes such as national pride, health or health-related behaviour, risk attitude, and social trust.

6.2.5 Chapter 5: Instructional Time

Finally, in the last chapter, we asked whether raising instructional time can crowd out student pro-social behaviour. To shed light on this question, we evaluated the impact of a large educational reform in Germany that has raised instructional time for high school students as a quasi-natural experiment, using a difference-in-differences design that exploits variation in the implementation of the reform across federal states and school cohorts. The beauty of this reform is that it has compressed the taught curriculum into fewer years of schooling without actually having made any changes to the content itself. This allows the isolation of the "pure" effect of raising instructional time on student pro-social behaviour, excluding potentially confounding changes to the educational system that are typically accompanied by similar reforms. Plotting pre-treatment pro-social behaviour of students in different federal states at different points in time shows a common pre-treatment trend in the outcome, suggesting that the *common trend assumption* is likely to be satisfied.

Using data on youth and adolescents from the German Socio-Economic Panel, we found that the rise in instructional time has a significant and sizeable negative effect on volunteering, leading almost every fifth student to change her behaviour from volunteering at least once a month to volunteering less often or not at all. This change is primarily driven by students that volunteer weekly, and it affects both the intensive and the extensive margin of volunteering: while half of students cut back on their activities, the other half give them up completely. Students with lower-educated parents are particularly likely to reduce their engagement. We found no similar crowding out of scholastic involvement, but no substitution either. The rise in weekly instructional hours also affects political interest.

Clearly, this study has many limitations, and it is only a cautious exploration into unintended consequences of well-intended educational reforms that attempt to make educational systems more efficient, for example, by raising instructional time or reducing the school-starting age. Such reforms aim at equipping students with more skills in lesser amounts of time in order to make them enter the labour market earlier, and ultimately, make them and national economies more competitive as well as social security systems more sustainable. Extreme examples include educational systems in South-East Asia such as in, for example, China, Taiwan, and South Korea. Obviously, there is a cost to more efficiency: creativity may be one, and free time activities, in particular beneficial ones such as volunteering, may be others. These costs are often discussed, but seldom subject to empirical investigations. This study is one of the few. Some limitations include that we cannot, at this point, test for the persistence of the identified effects, as we must wait for more waves of data to become available. Another limitation is external validity, and whether the uncovered relationship also holds in other institutional settings. We believe that this is at least the case in educational systems that resemble the German such as in, for example, Austria. Promising avenues for future research include investigating the effect of school-starting age on pro-social behaviour, and trying to quantify the effect of youth pro-social behaviour on later labour market outcomes. Both could fruitfully complement the given chapter in future versions.

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