DEMAND AND WELFARE IN HEALTH ECONOMICS

A Dissertation

by

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ABSTRACT

In this dissertation I present three projects related to the topic demand and welfare in health economics by leveraging changes in technology, institutions and policies through quasi-experimental and experimental approaches.

In the first essay "Television, Health, and Happiness", which is joint work with Adrian Chadi, we study the consequences of television consumption. Watching television is the most time-consuming human activity besides work but its role for individual well-being is unclear. Negative consequences portrayed in the literature raise the question whether this popular activity constitutes an economic good or whether it is an economic bad and hence serves as a prime example of irrational behavior reducing individual health and happiness. We are the first to comprehensively address this question by exploiting a large-scale natural experiment in West Germany, where households in a few geographically restricted areas received commercial television via terrestrial frequencies. Rich panel data allow us to determine how signal availability over time changes individual time-use and well-being. Contrary to previous research, we find no health impact when television consumption increases. For life satisfaction, we even find positive effects. Additional data support the notion that television is not an economic bad and that non-experimental evidence seems to be driven by negative selection.

The second essay "Vaccines at Work", which is joint work with Roberto Mosquera and Adrian Chadi, is investigating the causes and consequences of vaccination. Influenza vaccination could be a cost-effective way to reduce costs in terms of human lives and productivity losses, but low take-up rates and vaccination unintentionally causing moral hazard may decrease its benefits. We ran a natural field experiment in cooperation with a bank in Ecuador, where we experimentally manipulated incentives to participate in a health intervention, which allows us to determine the personal consequences of being randomly encouraged to get vaccinated using administrative firm data. In a first stage, we find strong evidence that opportunity costs and peers matter to increase vaccination demand. In the second stage, contrary to the company's expectation, vaccination did

not reduce sickness absence during the flu season. Getting vaccinated was ineffective with no measurable health externalities from coworker vaccination. We rule out meaningful individual health effects when considering several thresholds of expected vaccine effectiveness. Using a dataset of administrative records on medical diagnoses and employee surveys, we find evidence consistent with vaccination causing moral hazard, which could decrease the effectiveness of vaccination.

The third essay studies "The Unintended Consequences of Health Insurance" in a universal health care system. Universal healthcare is associated with desirable health and equity outcomes and often allows individuals to purchase supplementary private health insurance. While the purchase of private health insurance is clearly beneficial in the absence of public insurance, it is more difficult to evaluate individual costs and benefits when baseline coverage exists for everyone. The perceived benefits of insurance and the increase in health costs due to premium payments can lead to hidden costs and unintended consequences of supplementary health insurance. To study those costs, I use a regression kink design in conjunction with a policy implemented in Australia in 2000 to overcome selection. The policy punishes agents for delaying the purchase of private health until later in life. Following the policy-guided instrumentation of insurance purchase, it appears that private health insurance does not cause moral hazard. There is a zero effect on medical expenditures despite evidence of adverse selection. Supplementary insurance does not change mortality or work expenses but it changes the budget. We observe an increase in student debt which is consistent with premium payments crowding out debt repayments. There is a loss of gross income from private health insurance which is consistent with income under-reporting to reduce expenses from premium payments.

DEDICATION

To my family, and especially my mother Christine Hoffmann who was always there when I needed her. My deep gratitude goes to Anke Konrad as a pillar of support throughout this journey.

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The analyses in Chapter 1 were conducted in part by Adrian Chadi of the Department of Economics at the University of Konstanz; the analyses in Chapter 2 were conducted in part by Roberto Mosquera of the Economics Department, Universidad de las Américas and Adrian Chadi of the Department of Economics at the University of Konstanz.

All other work conducted for the dissertation was completed by the student independently.

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1. TELEVISION, HEALTH, AND HAPPINESS

1.1 Introduction

> "We know today that television makes you fat, stupid, sad and violent."
>
> (Ursula von der Leyen)

Television consumption and modern forms of it via the Internet are one of the most time-consuming daily activities worldwide. Over the lifespan, watching TV even surpasses work under plausible assumptions.[1] Since TV consumption is voluntary, one might hypothesize that spending so much time on this activity yields large individual benefits. On the contrary, a sizable body of research suggests that TV consumption is a threat to both individual health and happiness (Dietz and Gortmaker 1985; Hu et al. 2003; Hancox et al. 2004; Frey et al. 2007; Bruni and Stanca 2008; Benesch et al. 2010; Cuñado and Gracia 2012). While at the social level positive consequences of TV are discussed in research on education and gender (Gentzkow and Shapiro 2008; Jensen and Oster 2009), numerous potentially detrimental effects of TV consumption are also well documented, such as reduced political involvement, destruction of social capital, higher divorce rates, increased household debt and reductions in IQ (Gentzkow 2006; Olken 2009; Chong and La Ferrara 2009; Baker and George 2010; Hernæs et al. 2019). Consistent with these findings and exemplified by our introductory quote from the current President of the EU Commission, there is a wide-held belief that television is generally harmful. However, it is unclear whether television consumption is individually detrimental and if so: why do individuals then watch so much television in the first place?

The puzzling empirical contradiction of individuals engaging in an activity that could be both socially and individually harmful is reconciled in the happiness literature as a self-control problem

1. Assume that an average individual works 40 years, 250 workdays per year, and 8 hours per day. Further assume the same person watches 3 hours of TV per day, which is below official numbers in many countries, such as the United States. To surpass our lifetime work estimate of 80,000 hours (=40×250×8), this fictitious person needs to watch TV for 74 years or less when the person watches more hours per day. Millions of United States citizens born in the 1940s easily exceed that number.

with TV watching being interpreted as a case of irrational behavior (Frey 2008, 2018).[2] This idea of using happiness data to examine irrational choices has been applied by economists in various contexts, such as in the case of smoking, for which cigarette taxes and bans have been shown to serve as self-control devices making smokers happier (e.g. Gruber and Mullainathan 2005; Odermatt and Stutzer 2015).[3] Given a large general interest in discussing self-control problems in economics (e.g. Laibson et al. 1998; Frederick et al. 2002; Loewenstein et al. 2003), it is no surprise that there are also intense discussions about policy solutions for such phenomena, reaching from asymmetric paternalism to nudges to other types of intervention (e.g. Camerer et al. 2003; Thaler and Sunstein 2009). To draw such policy conclusions for the case of television, it is imperative to first clarify whether watching TV actually belongs into the category of irrational behaviors that imply negative individual consequences. If not, and agents are rationally benefitting from consuming an economic good, then social consequences are a problem of negative externalities that can be addressed by internalizing those costs. Whereas previous research seems clear that TV consumption is an economic bad, implying that individuals do not benefit at all from pursuing this activity, there is reason to question this wide-spread belief and in particular the empirical evidence provided so far. Arguably, unhappy or unhealthy individuals could sort themselves into higher levels of TV consumption, making it difficult for the empirical researcher to determine whether television consumption is individually harmful when the data are of non-experimental nature. While we are not the first to recognize this possible sorting problem, we are the first to credibly address it with data from a natural experiment.[4] By doing so, we find evidence inconsistent with the idea that television

2. Evidence for negative effects of television on individual happiness are based on survey data containing measures of both happiness and TV viewing. In an oft-cited study, Bruni and Stanca (2008) ask: "Why do rational people allocate their time and resources without maximizing their well-being?"

3. In other research strands, there are discussions on whether and how reported happiness may reveal individual choices, so that such measures could be interpreted as a production input factor of preferences and, hence, are of interest in itself (Benjamin et al. 2014). Evidence indeed shows that happiness is a determinant of economic choices and behavior, be it time preferences (Ifcher and Zarghamee 2011), work effort (Oswald et al. 2015) or voting (Liberini et al. 2017). Note that we treat the terms happiness and life satisfaction synonymously, in line with many contributions in the field of happiness research, while we consider well-being to be a broader term that also incorporates health.

4. Most of the studies on health and happiness do point out that the identification of the causal effect of TV watching is difficult and practically impossible with the empirical approaches used so far. For example, Frey et al. (2007) mention in their study a lack of a "natural experiment" to find out about causality. In this context, see Kataria and Regner (2011) for a comment on identification issues in the research on TV and happiness. In regard of health, see DellaVigna and La Ferrara (2015) who point out that studies from outside economics typically "lack a convincing

is an economic bad or that individuals behave irrationally.

In our study, we investigate the consequences of television consumption on happiness and health by exploiting the occurrence of a natural experiment in West Germany. We use this unique setting in conjunction with detailed longitudinal information on the provision of television, individual time-use and well-being measures through multiple sources of data, thereby providing credible evidence on the individual implications of TV in ways not possible so far. By discovering a natural experiment with unique historical facets, we add a novel research setting to a set of studies using regional heterogeneity in the provision of media during periods of implementation or expansion, such as the case of cable-TV in the US (Gentzkow 2006; Baker and George 2010; Campante and Hojman 2013).[5] In contrast to research in economics exploiting variation in terrestrial TV signals from West Germany reaching into East Germany (Hyll and Schneider 2013; Hennighausen 2015; Bursztyn and Cantoni 2016; Slavtchev and Wyrwich 2017; Hornuf et al. 2017; Laudenbach et al. 2018; Friehe et al. 2018, 2019), we are the first to exploit the setup of West Germany with its regional variation in terrestrial signals of private TV, thereby studying individual behavior within a fully developed country at the center of Europe. Arguably, it is not a surprise that natural experiments with strong variation in TV consumption rarely present findings from the developed world. For a country like the US, we would have to return to the middle of the 20th century, when systematic surveys were far less common as today, to find strong variation in TV viewing. Due to an overlap of nation-wide surveys in West Germany with our historical incidence, our setup is one of the few that allows to comprehensively check whether television consumption actually increases after signal reception.

The historic natural experiment on television in West Germany starts with a baseline of a de-facto ban of commercial TV until the early 1980s and, by international standards, low levels of

design" to credibly determine the effects of TV. They conclude: "Surprisingly given the interest in health economics, the evidence is limited" (p. 744).

5. For a review of different settings used in the research on TV so far, see DellaVigna and La Ferrara (2015) who emphasize the significant developments in methodological respects and list influential papers that have been published in major economics journals over the last years. Apart from research on TV, there is related work by economists on the impact of media, e.g. by Strömberg (2004), DellaVigna et al. (2014), Adena et al. (2015), Yanagizawa-Drott (2014) for radio as well as by Bauernschuster et al. (2014) and Falck et al. (2014) for the internet.

TV consumption with two hours per day for the average German. As a result of a Supreme Court decision in 1981, private television became legal in Germany and several new channels emerged.[6] Despite new technological opportunities such as cable and satellite, for years most citizens could not watch any of the new programs due to the failure of the responsible and later dismantled public institution (the Deutsche Bundespost) to roll out private TV in a timely manner. In consequence, there was a time window of several years, in which commercial TV providers searched for options other than cable or satellite to reach their potential viewership. They found a cost-effective way: terrestrial frequencies of public media stations that by chance were still available. However, in the late 1980s almost all powerful frequencies were in use by public broadcasts for which the stations were built in the decades before, so this opportunity was clearly limited. Only a few stations still had open frequencies that offered the opportunity to send out terrestrial signals to millions of households. Due to a German Supreme Court ruling, there was no opportunity for commercial TV to expand upon pre-existing terrestrial frequencies since transmitter stations in West Germany could only be built for public media. In consequence, a technically limited transmitter reach created naturally emerging borders that split citizens into receivers and non-receivers of private TV via antenna.[7]

Due to available technical data on all terrestrial stations transmitting commercial TV in Germany, we determine broadcast signals in a precise fashion to distinguish between TV treatment and control regions. Following recent studies on the impact of media, we use special software based on the Longley-Rice signal propagation model to identify regions with reception and without by considering not only technical data, e.g. the power of the station, but also geographical information such as mountains or valleys. A major benefit of our empirical setting is the fact that two large longitudinal household studies of the German population were ongoing during our investigation

6. We use the terms private and commercial TV simultaneously. While public TV in Germany is partly financed by mandatory fees, private TV channels do not receive fees but have to rely on revenues from advertisement and are in private ownership. For example, in our investigation period, media tycoon and later prime minister of Italy Silvio Berlusconi was one of the owners of private TV channel Tele5.

7. The success of the TV channel RTLplus with David Hasselhoff in the role of the channel's first superstar is a testament of an exogenously triggered increase in TV consumption, as nation-wide market shares more than doubled within the year of 1989 due to heavy TV consumption in just a few areas of the country (see Section 2).

period. First, the German Socio-Economic Panel (SOEP) provides us with detailed survey information on the situation of individuals at the time of the signal introduction, including the county of residence. We merge our technical calculations with the SOEP data at the regional level and compare how individual behavior responds in regions where commercial TV via terrestrial frequencies suddenly became available vs. regions where it did not. We study the implications of TV access for a set of daily individual time-use activities which allows us to inspect whether private TV reception increases TV consumption in our setting. The SOEP questionnaire also includes several outcome variables of interest capturing the individuals' overall satisfaction with their lives and indicators for their health. To capture variations in health, we analyze information on use of medical services (doctor visits, hospital stays) as well as subjective self-assessments (health satisfaction). By exploiting the panel structure of the data, we employ an individual fixed-effects approach to examine how TV consumption changes at the individual level due to commercial TV reception and how well-being is affected as a result of watching more television, without any influence of time invariant individual or regional characteristics. Second, we merge the signal calculations with the German Income and Expenditure Sample (EVS) at the municipality level.[8] The sample drawn from the Federal Statistical Office provides us with household expenses on components relevant to individual health which allows us to investigate the consequences of TV consumption on health-related behavior.

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8. The German federal statistical office conducts the EVS for various purposes, especially to inform public policies. For example, the data are taken into account when determining the level of social benefits paid do welfare recipients in Germany. Apart from that, the data has been the basis for numerous studies on savings and consumption in particular (e.g. Fuchs-Schundeln 2008; Friehe and Mechtel 2014).

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In line with the historical background, we find that private TV significantly increases TV consumption. According to our time-use analysis, increased TV consumption due to private TV may be to the detriment of time spent on housework, suggesting a substitution effect between those two activities. For our main outcomes, we contradict previous findings on the consequences of TV. Individual happiness improves due to TV consumption in terms of life satisfaction, while individuals do not suffer health impairments from watching TV more often. Television consumption does not reduce health satisfaction nor does it lead to an increase in doctoral visits, be it on the extensive margin or overall. This main conclusion does not change if we inspect long-run effects by exploiting the longitudinal nature of our empirical setup in a dynamic treatment analysis. In fact, we can rule out negative health effects for a time window of several years of television treatment.

Our contributions are several fold: First, we provide a textbook example how a negative result from the literature is flipped completely with a credible empirical setting. Using evidence from a complementary survey we conducted ourselves in 2015, we confirm a negative relationship between TV viewing and both health and life satisfaction decades later. By juxtaposing different pieces of evidence, we offer an explanation for previous findings, according to which there might be a selection of unhealthy or unhappy types of individuals into the group of intense TV viewers. Second, we provide a new setting for research on the effects of TV based on a unique natural experiment that took place in a large and fully developed Western country, allowing findings with generalizability from the relatively recent past. The setting further allows for longitudinal analyses of the long-run impacts of TV in a time window of several years until the regional disparity in access to private TV due to terrestrial frequencies became irrelevant.[9] Third, the data include information on possible behavioral changes, enabling us to empirically verify whether the new opportunity to watch TV due to technological advancements actually affected media consumption. In fact, the available time-use data inform us how individuals re-adjust their daily activities to have more time for watching TV, which is a novelty in the research on the causal impact of television in representative populations of adults, while consumption data inform us about the possible effects of television on health-related behavior. Fourth, we complement ongoing research on TV consumption with policy-relevant findings on important outcomes which so far have not received the attention from economists that they arguably deserve. Thereby, we contribute to the debate about the impact of media on society, which has focused mainly on the social costs and benefits but less on the individual. According to our results, TV watching does not appear to be an economic bad. Individuals seem to make a rational choice in a sense that television does yield a benefit to them. This supports the notion of individual welfare maximization. While externalities for soci-

9. The rise of Germany's No.1 private TV channel RTL ended in 1993 when market shares reached a historic peak (see Figure A1), suggesting that the channel could not substantially benefit from further growth in viewership as a result of increasing proliferation of cable and satellite. As receiving private TV via terrestrial frequencies became relevant for millions of Germans throughout the second half of 1988, our setting provides us with a treatment phase of roughly four years.

eties could be either positive or negative, our findings explain why TV consumption is one of the most popular activities, despite its possible social costs.

The remainder of our paper is structured as follows. Section 2 illustrates the early phase of private TV in West Germany and describes the natural experiment (with supplementary details on the history in Appendix A). Section 3 describes the data, which rely on technical calculations of local TV signal reach and survey data to analyze its implications (Appendix B provides supplementary information on technical details and checks). Section 4 presents a replication of earlier findings in the literature and Section 5 contains the main results, including checks and extensions (with Appendix C providing more information on the EVS data and Appendix D offering supplementary output). Section 6 offers a discussion of the findings, with a focus on the content of television, to learn more about external validity. Section 7 concludes by illustrating implications for public policy and provides alternative interpretations of our evidence, thereby addressing the question whether the proliferation of television could be seen as a success story or not.

1.2 Background

The historical development of commercial TV in West Germany involves a variety of different actors, such as media tycoons, politicians, some transmitter stations with limited reach, a TV superstar with a speaking car and Germany's Supreme Court. We focus on those relevant aspects of the history to understand the occurrence of a true and original natural experiment in the center of Europe.[10]

Long before the rise of commercial TV in Europe, Germans have been very skeptical towards television as a technology. Many individuals in Germany associate TV and its proliferation with the stultification of the masses ("Volksverdummung"), which could explain why there was no resistance to the legal ban of private TV for many decades. As a result of this consensus, TV only

10. Harrison and List (2004) provide a nice and not-so-serious definition: "Natural experiments arise when the experimenter simply observes naturally occurring, controlled comparisons of one or more treatments with a baseline. The common feature of these experiments is serendipity: policy makers, nature, or television game-show producers conspire to generate these comparisons." As we document, it appears that the history of commercial TV in West Germany contains all three of these ingredients. For a timeline of events (Figure A1) and documentation of the proliferation of TV in Germany based on excerpts from historical media reports, see Appendix A.

existed in a limited form with a few public TV channels. In contrast to other developed countries at the time, such as the US, watching TV took a relatively minor role in the daily lives of West German citizens with two hours per day allowing for substantial increases in TV consumption (Oltmanns 1993). The television landscape started to change dramatically in the 1980s. First, Germany's Supreme Court clarified its position on commercial television in a 1981 decision by recognizing a modern development allowing media providers to overcome the scarcity of transmission avenues. Thereby, the court referred to the justification of the ban on private TV which until then was based on the idea that terrestrial broadcasting via frequencies can only work for a limited number of media offers. This technical bottleneck was no longer an issue in times of cable and satellite emerging as alternative transmission avenues. Second, a new conservative federal government under the leadership of Chancellor Helmut Kohl decided to proliferate commercial TV in Germany, which starkly contrasted the policies of the former social-democratic government that was poised to protect the monopoly of public TV.

When the first commercial TV channels emerged in 1984, only a few thousand households in Germany were able to watch the new programs. In order to change this quickly, the new conservative government assigned the task to roll out commercial TV to the Deutsche Bundespost. However, this public institution failed to provide German households with those new private TV channels in a timely manner and was dismantled in 1994. The Bundespost focused on cable, as the preferred avenue to reach potential TV consumers, and invested large sums of money into what critics called a "billion-dollar grave". In the late 1980s, still only a minority of Germans watched private TV via cable, whereas satellite TV was no alternative yet. The result was a time window of several years, in which both politicians who supported private TV as well as officials of the emerging TV channels had the incentive to find an alternative way to reach German households. While politicians were interested in good relations with the media, the media companies had the goal to obtain a first-mover advantage in the emerging and growing media landscape in one of Europe's economically most relevant countries. It soon became clear that there was a simple way: terrestrial frequencies on public-media transmitter stations that were not in use yet.

It was apparent, however, that powerful frequencies were extremely rare, given that most of those frequencies were in use by the public media broadcasts for which the stations were built. The stations that could be used for private TV in the late 1980s were mainly constructed in the 1960s to provide the country with a second public TV channel in the aftermath of a 1961 Supreme Court decision. Due to the ban of private TV, it was unforeseeable during the construction phase that decades later there could be a strong commercial desire for more frequencies. Therefore, almost all of the powerful terrestrial frequencies with significant reach were in use by public media broadcasts in the late 1980s. There were only a small number of stations that coincidentally had still an open slot with a powerful frequency (Table D1).

Apart from this first technical limitation of the availability of powerful frequencies, there was a legal limitation on the expansion of terrestrial broadcasting. According to the Supreme Court decision in 1961, the management of Germany's network of transmitter stations was seen as a politically sensitive issue and, hence, building new stations was a public task that should be organized independently from political influences. In consequence, there was no legal option for any commercial TV provider to expand upon the existing network of transmitter stations.

Powerful frequencies ensured that households without particular technical equipment could easily watch the program, which was different for frequencies with low power.[11] At the state level, German politicians realized the importance of the powerful frequencies to reach a significant number of households and they allowed opening up public-media frequencies for non-public TV. As an example, consider the densely populated federal state of North Rhine-Westphalia (NRW). The available powerful frequencies in that state were called "juicy" by the media due to their extraordinary desirability. The representatives of private TV channels applied for those frequencies to the state government which would decide about the frequency usage. After the frequencies were given to TV companies in 1988, millions of citizens in some areas of NRW suddenly were able to watch commercial TV due to strong terrestrial signals available in the western part of the state. At the same time, other citizens, including those in the eastern parts of NRW, could not receive those

11. Figure D1 shows an example of a 1980s TV set, which could be used to watch *RTLplus* via terrestrial television signals in 1989. Usually, TV sets with indoor antenna receive terrestrial broadcasts provided a powerful signal.

terrestrial signals due to a higher distance to transmitting stations. Similar to other empirical TV settings, e.g. in Brazil (see La Ferrara et al. 2012), favoritism played a role in our context, as media tycoons exerted enormous efforts to convince state officials to receive frequencies. However, such form of favoritism only affected the decision which one of the commercial TV providers received the "juicy" frequency but not whether a powerful frequency was technically available or not. This was determined by ex-ante pre-determined factors and the coincidence of still available capacities at transmitter stations that were built many years earlier for the sole purpose of public TV and radio broadcasts.

Figure 1 shows the private TV signal reach across West Germany in 1989.[12] Due to limitations in signal reach, naturally emerging borders split the country into areas of potential receivers (colored) and non-receivers (not colored). In addition to the NRW frequencies in the West, there were a few powerful frequencies in use for private TV in the North of Germany. There were also some smaller areas throughout Germany in which it was technically possible to receive private TV via terrestrial frequencies, but it is an empirical question whether the power was sufficient to affect individual TV consumption in significant ways.

The year 1989 was crucial for the proliferation of commercial TV in Germany. Among various competitors, RTLplus became the country's number one private TV channel in this year and remained at the top for decades. The channel's market share reached 10%, which is very large since only a minority of households in Germany could watch this program (KEK 1998).[13] The program organizers behind RTLplus were able to establish their own superstar, David Hasselhoff,

12. This graphical illustration is a product of our own calculations and aligns with ad-hoc maps, drawn by technical experts and shown to us. In personal communications with experts of terrestrial frequencies in Germany, we discovered that there is no exact calculation of access patterns describing the reach of private TV in Germany so far. As a result, we are the first who have done this laborious task. A comparison reveals that 1989 was the first year in which private TV was widespread in Germany. We provide details on the most powerful private TV frequencies (with at least a power of 10kW) in that year in Table D1.

13. Based on our own calculations using SOEP data, we find that the terrestrial RTLPlus signals reached 29.31% of the individuals. This is an optimistic estimate, as we consider signals from all stations, including low-power frequencies. As documented in Appendix A, satellite played no role at the time. For cable TV, official statistics are available for 1988. Accordingly, 14.8% of households in West Germany had access. This number includes West Berlin, which had the highest state-level access rate with 31.6%. While cable access rates could have increased further in 1989, those numbers would still be far above the actual rate of RTLPlus cable viewers, as many households with access did not order cable TV due to high connection fees.

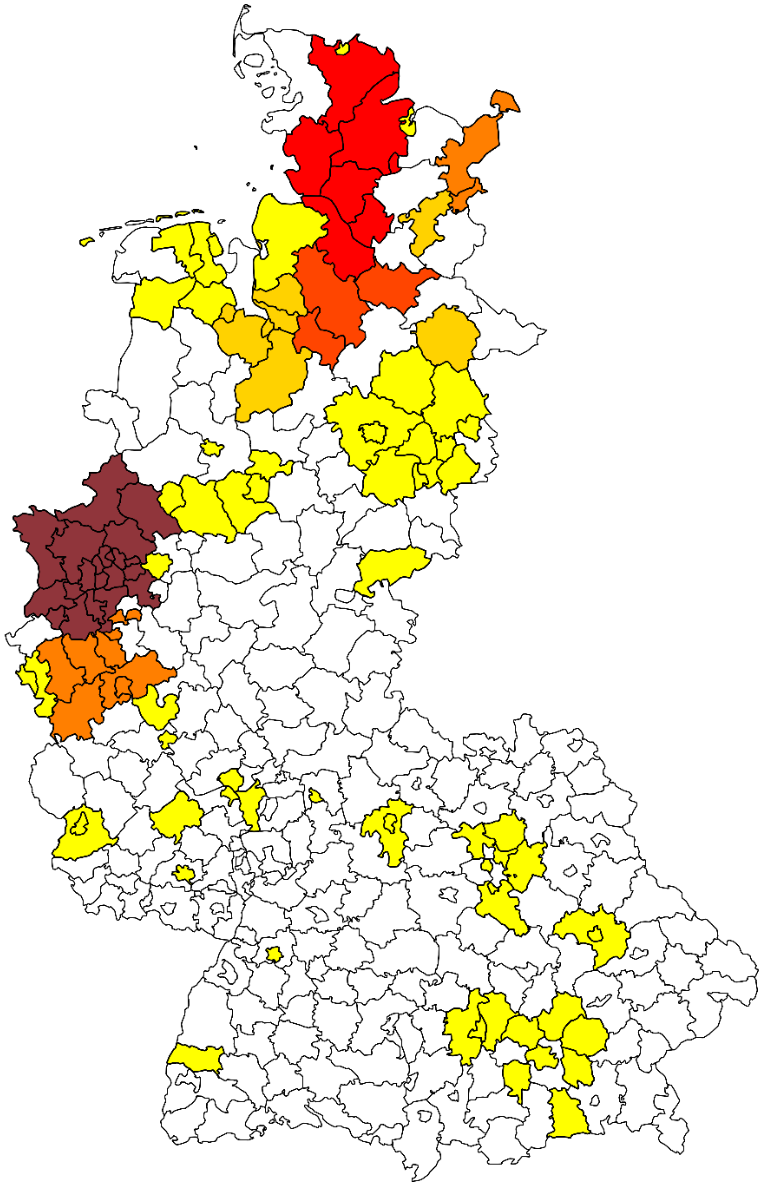
with a popular TV show called "Knight Rider".[14] Germany's number two commercial TV channel Sat.1 did not do as well. While the owner Leo Kirch received powerful frequencies in the North of Germany, he was unsuccessful in the state of NRW. The situation in 1989 was even worse for the third private TV broadcast, Tele5, owned by Italian media tycoon Silvio Berlusconi. Tele5 did not receive any of the powerful frequencies (see Table D1).

Given the importance of powerful frequencies, it is not surprising that media companies demanded more terrestrial frequencies and new transmitter stations. However, it was legally impossible to build additional transmitter stations to reach more German households with private TV. The legal framework was clear about the fact that construction of stations was only allowed for the purpose of public media broadcasts. Given that the Supreme Court justified the broadcast of private TV and the revision of the ban with new technical developments that go beyond terrestrial broadcasting, commercial TV channels could only receive slots on public-media transmitters that were still open. Attempts by these commercial TV providers to expand terrestrial TV in Germany thus were doomed to fail and did fail. In particular, Silvio Berlusconi exerted enormous efforts to expand terrestrial television in Germany for his channel Tele5 but finally failed and he sold his shares during the 1990s when he left the German TV market.[15]

14. Knight Rider, like many other shows, was produced in the US and dubbed into German. An intriguing aspect of the success story of David Hasselhoff in Germany was that the actor's popularity through RTLplus allowed him to start a music career. Although the public media did not play his music, at that time, he reached the top position in Germany's music charts in April of 1989.

15. As part of our historical documentation (Appendix A), several media reports covered the role of Berlusconi in the German TV market. First, he tried to convince officials in NRW to get the powerful frequencies, and, then, offered to expand the net of terrestrial frequencies in Germany, both approaches were unsuccessful.

Figure 1.1: Private TV via Terrestrial Frequencies in Germany of 1989



Notes: The map illustrates counties of West Germany with and without reception of private TV via terrestrial frequencies based on the Longley-Rice propagation model in 1989. In counties without private TV reception (white), the aggregated mean value of all square-kilometer-based signal strength values is below 55 dBuV/m. In counties with private TV reception, the aggregated mean value of all square-kilometer-based signal strength values is at least 55 dBuV/m. These counties are colored according to the effective radiated power (ERP) in kW of the frequency that provides the county with the strongest signal, using six intervals (maroon: min. 200 kW, red: 100 – 200 kW, red-orange: 50 – 100 kW, orange: 20 – 50 kW, gold: 10 – 20 kW, yellow: 0 – 10 kW) in line with Table 3. All signals of private TV, independent of the channel, are included. See Appendix B for more information and additional illustrations.

1.3 Data

1.3.1 TV Signals

We obtained copies from the original documentation containing information about the transmitter stations for public television and radio using terrestrial frequencies in West Germany (NDR 1987, 1988, 1989, 1990, 1991, 1992). These so-called "Wittsmoor lists" are annual overviews of information about all transmitters, including basic facts such as geographic position and effective radiated power (ERP) in kilowatts (kW). While related studies have used the 1989 Wittsmoor list for geographically precise investigations into the effects of terrestrial TV signals from Western stations broadcasting public TV to East German households (Crabtree et al. 2015; Bursztyn and Cantoni 2016), we are the first to consider information on private TV channels within West Germany by constructing a longitudinal dataset on signal reach. We benefit from additional information from the official records of each transmitter station, which – like the Wittsmoor list – are stored in Hamburg at the NDR ("Norddeutscher Rundfunk"). Those records contain information, such as the height of the transmitter station and the month when the broadcast started.[16] We are also the first to collect information on antenna patterns of the transmitter stations. Since patterns can differ substantially across stations, this information further increases precision when calculating TV signal reach.[17]

To determine the TV signal reach, we employ the so-called Communication System Planning Tool (CSPT), which was developed for the US Department of Defense. The CSPT is an add-on to the geographic analysis software ArcGIS to calculate the signal reach based on the Longley-Rice signal propagation model. To consider geographical information for the entire region of West Germany, we rely on digital maps incorporated in ArcGis version 9.3 and employ a resolution of

16. Records also include hand-written notes, such as information on initial phases of lower power. In some cases, stations did not immediately broadcast signals with full power, especially for stations that potentially could reach the territory of other countries.

17. For example, a transmitter station can send out its signal with the exact same strength in all directions or the signal can be aimed at a certain direction. See Appendix B for more information and an exemplary antenna pattern. Figure D2 displays the power of the signal from the transmitter station Wesel used by RTLplus which was mainly directed to the south-east, away from the Dutch territory in the North and in the West of Wesel, and instead targeting the populated Ruhrgebiet area.

1000m. Thereby, our calculation of TV signals takes topographic aspects of the terrain into account, such as mountains and the earth's curvature.

The aim of the signal calculation procedure is to determine whether a TV signal from a transmitter reached a region in West Germany or not. For this purpose, we obtained additional digital maps that allow us to identify borders of both counties and municipalities in West Germany.[18] We determine the signal strength for each square kilometer of West Germany first, and then aggregate this information at the regional level. We thereby obtain a mean value for each region, be it a municipality or a county. This average signal strength value allows us to determine whether individuals in a given region likely could watch a certain TV channel with their TV set.[19]

To separate treatment regions with access to terrestrial private TV from those without access, we determine two important technical thresholds. The first parameter is the minimum strength of a signal (dBuV/m) at which a region is considered to receive a TV signal. 55 dBuV/m is our default value. As discussed in the sensitivity analysis in Appendix B, we obtained this value empirically by employing various alternative thresholds for the first stage while considering related literature in the process. It turns out that our main findings are insensitive to this choice (Table B2). For the second parameter, the power underlying the frequency, we have limited ex-ante knowledge from the literature about the role of less powerful frequencies. For example, in research on Western TV signals in East Germany (Bursztyn and Cantoni 2016), ERP values of below 100kW are not used, given that less powerful frequencies could only reach those areas close to the border that were already provided with Western TV on more powerful frequencies. In our case of West Germany, low powerful frequencies could in principle reach households, so we pay special attention to the parameter of minimum ERP. Our expectation from studying the historical records was that 20kW of ERP could serve as a suitable threshold to define a powerful and, thus, relevant frequency (see

18. We use the oldest available border files from 1997 provided by the German Federal Agency for Cartography and Geodesy.

19. We follow the procedure in Bursztyn and Cantoni (2016). To illustrate the result of this procedure, in Appendix Figures D3 and D4 are maps for the signal reach of the most powerful RTLplus frequency (on the transmitter station Wesel) for both county and municipality level. In addition to averaging all cell values within a region, we also determine the median of signal strength as an alternative measure. Our findings are insensitive to this decision (Table B1).

Section 2). This is in line with the media attention that the "juicy" frequencies of NRW received. According to the technical documents available to us, the historic media coverage can only refer to the two following stations: Wesel with 200kW and Dusseldorf with 20kW. However, the latter frequency was actually below usual ERP levels of public TV broadcasts, for which most of the TV antennas in the 1980s were configured. Therefore, we inspect how results change with a sequential increase in the ERP threshold from 10 to 20, 50, 100 and 200kW, thereby excluding comparatively weaker frequencies, to ascertain which signals did affect TV consumption in our period of investigation.

Finally, we consider the month-exact start information for any private TV channel on each frequency in our longitudinal data on TV signal reach. This data includes binary variables for each region and year from 1987 to 1992. It reflects whether a region received a private TV signal or not.[20] The month-exact information identifies cases when the start of a private TV channel in a given year took place but the SOEP fieldwork of the same year already concluded.[21]

1.3.2 SOEP

The main data source is the German Socio-Economic Panel, which is Germany's largest panel survey. We use version 29 (SOEP 2013) of this ongoing longitudinal investigation into the lives of the German people (Wagner et al. 2007). The SOEP contains representative data for the adult population of Germany from 1984 onwards. Fieldwork happens mainly in the first few months of each year when interviewers visit participating households.[22] Prior to re-unification, West Germany had more than 300 counties with varying numbers of SOEP participants in each county. To identify the impact of television on individual outcomes, we use our longitudinal data on the reach of private

20. We prefer a dummy variable for TV signal reception instead of a linear signal strength. See Appendix B for more information and sensitivity analyses (Table B3).

21. For example, the transmitter station Hennstedt provided the surrounding area with private TV since November of 1988, according to the official records. Since the transmitter start month was later than the data collection for the 1988 SOEP, we consider 1989 as the first treatment year for SOEP participants living in regions receiving private TV from the 100kW frequency of the station. See Appendix B for an overview of relevant stations with information on timing, including official start dates and the occurrence of an initial phase of low power.

22. Due to early SOEP interviews within the year of 1989, the events surrounding the fall of the Berlin wall do not play any role in our main analysis from 1987 to 1989. Note that we focus on West Germany, which in our definition always excludes (West) Berlin.

TV signals and merge that information with the SOEP data on the county-year level.[23]

To examine implications for individual health, the SOEP includes information on doctor visits reported by participants for the last three months prior to the interview.[24] A second health-specific outcome variable in the SOEP are hospital stays reported by participants for the entire year prior to the annual interview. Because this question was not included in the questionnaire of 1990, we have no information on hospital stays for our crucial year of 1989. We do not use this variable in our main specification, but we provide supplementary analyses where we adjust the time window of our investigation. The SOEP also contains subjective self-assessments in the form of health satisfaction (wording: "How satisfied are you with your health?") and life satisfaction (wording: "How satisfied are you with your life, all things considered?"), which are routinely asked in each SOEP questionnaire. Respondents in the SOEP always assess their satisfaction levels on a scale ranging from 0 ("completely dissatisfied") to 10 ("completely satisfied").

For the purpose of investigating individual daily time-use changes as a result of private TV reception, we exploit the SOEP time-use battery. This survey module contains information about the hours per day an individual engages in different activities, e.g. childcare (Table D2). The questions are asked for workdays (including Saturday) and Sundays. In contrast to recent SOEP waves, questionnaires until 1989 included an item called "TV/Video". We use all responses from this item to establish a manipulation variable called "watching TV" with a broad understanding of television, in which we include watching videos.[25] We cumulate the reported numbers of hours by the respondents for a typical workday multiplied by six and add a typical Sunday to obtain weekly

23. Regional identifiers are available for data users after signing a special agreement with the SOEP organizers. Analyses of regional data are possible via remote access using SOEPremote and on-site at the DIW Berlin.

24. In 1988, this question changed. Before 1988, participants could respond to have visited no doctor or provide the doctor visits for different types of doctors (dentist, etc.). Since 1988, the wording in the SOEP aggregates all doctor visits without any separation into types of doctors: "Have you visited doctors in the last 3 months? If yes, please indicate how often." For the pre-1988 data, we aggregate all cases of different types of doctor visits to generate a variable reflecting the total number of doctor visits. Given many survey items due to 11 different doctor categories, this exercise leads to a relatively large number of missing values. Using year fixed effects, ameliorates this issue. Note that the binary indicator for having visited any doctor is not subject to this missing-value issue.

25. At the time, individuals in Germany mainly used their video recorders to watch self-recorded TV shows and movies. A check based on EVS data reveals that ownership of video recorders is not affected by receiving private TV via terrestrial frequencies, while we find significant positive effects of receiving private TV on the number of TV sets owned by EVS participants (see Appendix C).

time-use. While this serves as our main time-use variable of interest, we run sensitivity analyses to check the role of this particular definition (Appendix B). We also analyze the remaining time of a 24-hours day after subtracting the sum of all reported hours spent on all activities and interpret this residual as sleeping time.

A potential caveat of the time-use information is its hour-based measurement. Changes of half an hour for instance could remain unreported. Given negative views and the social stigma attached to watching TV in Germany (Appendix A), it is likely that many individuals stick to their reported hours of TV consumption from the previous interview and do not increase their self-reports even if they actually do watch more after receiving commercial TV. We believe that the merger of the workday information with the Sunday information mitigates this issue somewhat, given that reporting on having watched TV on a weekend might be less stigmatizing than during the week, but we still expect changes in TV consumption to be underreported.[26]

Table D2 shows descriptive statistics for the resulting main sample from 1987 to 1989. We expand this period beyond 1989 for additional reduced-form analyses without the TV consumption variable. As an important data restriction to allow for clustering of standard errors at the regional level, we ensure that our analysis is not affected by individuals moving between regions. Each individual observation is included in the analysis only if the person is observed in the same county, in which he or she lived in 1989. This restriction maximizes the number of observations in this particular year and implies a left-skewed distribution of observations across years in our main sample. In Appendix B, we show that the results are insensitive to this restriction.[27]

26. Self-reported time-use on TV consumption typically reveal much lower estimates about watching behavior in comparison to electronic measures for the same population (Frey et al., 2007). In contrast, electronic measures might yield over-reporting due to individuals that activate the TV in the background while they do not actually watch or listen. Note that we find in sensitivity analyses (Table B6) that TV consumption is affected more heavily on Sunday than during the workweek.

27. Selective relocations are no issue in the context of our natural experiment on private TV signals via terrestrial frequencies. This is because of i) the social stigma associated with (private) TV in Germany (see Appendix A), ii) the uncertainty surrounding private TV on terrestrial frequencies, and iii) the fact that everybody in Germany could expect to get the option to watch private TV eventually. To the best of our knowledge, there was no case of a person moving from one place to another within Germany just to watch private TV earlier.

Table 1.1: Means of Summary Statistics in TV Regions across Survey Years

	20kW Private TV		t-test	200kW Private TV		t-test
	No Signal	Signal	(p-value)	No Signal	Signal	(p-value)
1987						
Life satisfaction	7.20	7.21	0.90	7.21	7.15	0.38
Health satisfaction	6.86	6.91	0.65	6.87	6.89	0.92
Female	0.50	0.51	0.58	0.50	0.51	0.77
Age	43.16	42.79	0.50	43.16	42.48	0.31
Household size	3.26	3.08	0.00	3.23	3.16	0.30
Children in household (Y/N)	0.74	0.74	0.64	0.73	0.81	0.30
Years of education	10.59	10.79	0.00	10.63	10.64	0.81
Married	0.69	0.67	0.08	0.69	0.70	0.71
Care	0.03	0.03	0.60	0.03	0.02	0.26
Log(Income)	7.23	7.23	0.88	7.23	7.23	0.90
1988						
Life satisfaction	7.16	7.06	0.13	7.15	7.06	0.14
Health satisfaction	6.77	6.70	0.11	6.76	6.70	0.23
Female	0.50	0.52	0.31	0.50	0.51	0.70
Age	43.22	43.02	0.72	43.22	42.91	0.71
Household size	3.24	3.08	0.00	3.21	3.17	0.52
Children in household (Y/N)	0.72	0.71	0.36	0.71	0.78	0.38
Years of education	10.60	10.80	0.00	10.63	10.67	0.37
Married	0.68	0.66	0.07	0.67	0.69	0.28
Care	0.03	0.03	0.80	0.03	0.03	0.31
Log(Income)	7.25	7.27	0.62	7.25	7.27	0.82
1989						
Life satisfaction	7.13	7.24	0.02	7.13	7.27	0.09
Health satisfaction	6.69	6.83	0.16	6.71	6.74	0.52
Female	0.50	0.52	0.26	0.50	0.52	0.43
Age	43.40	42.96	0.42	43.37	42.86	0.45
Household size	3.22	3.07	0.00	3.20	3.16	0.70
Children in household (Y/N)	0.71	0.71	0.45	0.70	0.76	0.42
Years of education	10.63	10.79	0.00	10.66	10.68	0.47
Married	0.68	0.66	0.11	0.68	0.68	0.79
Care	0.03	0.03	0.65	0.03	0.02	0.05
Log(Income)	7.30	7.30	0.97	7.30	7.30	0.50
Observations	18,107	1,225		18,672	660	

Source: SOEP data are from 1987 to 1989.

Table 1 illustrates differences between characteristics of SOEP respondents across treatment and control regions for the three main years of our investigation phase. For a TV signal definition of 20kW and more, there are a few significant differences. This check provides an indication that the frequencies with low power might be endogenous and, hence, less useful as an instrument. This empirical insight conforms to the historical circumstances as documented in the media coverage of the 1980s (Appendix A), according to which some low-power frequencies were not in use by private TV, albeit available, depending on politically motivated actions. For a TV signal definition that includes only maximum frequency power, randomization is more plausible.

1.3.3 EVS and Own Survey

In addition to the SOEP, we use the German Income and Expenditure Sample ("Einkommens- und Verbrauchsstichprobe", EVS) that covers the period of the late 1980s and early 1990s.[28] Every five years, the German Federal Statistical Office asks tens of thousands of representative households in Germany to report in detail on their income and consumption behavior. The data includes a variety of different expenditure items, from pharmaceutical products to TV sets. The EVS waves of 1988 and 1993 are of interest to investigate possible differences in behavior due to exposure to private TV signals. We describe the process of merging the EVS data with the TV signal information in detail in Appendix C where we discuss limitations and present results from a complementary TV signal check, for which we use information on purchases of TV sets.

We further conducted a representative telephone survey of the German public on the topic of television in 2015.[29] One goal of this survey was to obtain information from individuals about TV program content perceptions in Germany and to assess differences between public and private TV (see Section 6). Another objective of the survey was to obtain new data on the link between well-being and TV consumption in present time. To infer weekly TV consumption, we asked interviewees the question: "How much time per day (in hours respectively minutes) do you spend on average watching TV?" We inspect associations between self-reported hours of weekly TV con-

28. The 1988 EVS wave was specifically prepared for this project by Germany's Federal Statistical Office to allow a regional analysis at the municipality level.

29. The survey was conducted at and financed by the Institute for Labour Law and Industrial Relations in the European Union, a research institute located at the University of Trier in Germany.

sumption and life satisfaction as well as health satisfaction as the two dependent variables in a complementary analysis. The latter two variables are observed on an 11-point scale, in the same way as in the SOEP.

1.4 Reproducing Findings from the Literature

1.4.1 Model

First, we reproduce previous findings on TV and its empirical relationship with (ill) health and (low) life satisfaction. We use data from our own 2015 survey as well as the SOEP data from the 1980s for a first correlational inspection. This allows us to juxtapose the historical evidence from the SOEP with recent evidence on correlations between well-being and TV consumption. We use the following model, which allows us to test the role of individual fixed-effects:

$$Well\ Being_{it} = \theta_0 + \theta_1 \mathbb{1}(FE) + \theta_2 TV\ Watching_{it} + u_{it} \tag{1.1}$$

Initially, we employ a simple ordinary least squares (OLS) regression linking TV consumption to outcomes reflecting individual well-being (i.e. health and life satisfaction), which is possible for both datasets. For our own survey, the fixed effects indicator $\mathbb{1}(FE)$ is zero. When we use the panel structure of the SOEP data, we transition to a fixed effects model with the binary $\mathbb{1}(FE)$ set to one to control for individual time-invariant characteristics.

1.4.2 Results

Panel A of Table 2 shows the relationship between TV watching and satisfaction outcomes from our 2015 survey. Consistent with findings in the literature, we find that higher TV consumption is linked to lower health satisfaction and life satisfaction scores on average. The results imply that zero TV consumption is connected to the highest satisfaction scores. Adding covariates does not qualitatively change the finding.[30]

30. The results also hold if we consider survey factors, such as weekday of the interview.

Table 1.2: TV Consumption and Well-Being Associations

Panel A) Own survey (2015)

	Health satisfaction	Health satisfaction	Life satisfaction	Life satisfaction
Pooled OLS	-0.040***	-0.027***	-0.032***	-0.030***
	(0.008)	(0.009)	(0.006)	(0.006)
N	511	511	511	511
Control variables	NO	YES	NO	YES

Panel B) SOEP (1987-1989)

	Health satisfaction	Health satisfaction	Life satisfaction	Life satisfaction
Pooled OLS	-0.022***	-0.011***	-0.007***	-0.004*
	(0.003)	(0.002)	(0.002)	(0.002)
Individual Fixed Effects	-0.003	-0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
N	20,311	20,311	20,288	20,288
	Visited A Doctor	**Visited A Doctor**	**Doctor Visits**	**Doctor Visits**
Pooled OLS	0.001***	0.001***	0.027***	0.016***
	(0.000)	(0.000)	(0.004)	(0.004)
Individual Fixed Effects	0.001	0.000	0.002	0.002
	(0.001)	(0.001)	(0.006)	(0.007)
N	20,314	20,314	15,920	15,920
Control variables	NO	YES	NO	YES

Notes: The independent variable is weekly TV consumption in hours. In Panel A, the dependent variables are health and life satisfaction on a 0 to 10 scale. The set of control variables contains gender, age, quadratic age, household size, and living in West Germany in 1989. See Table D3 for descriptive statistics on all variables considered. In Panel B, the dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The baseline specification contains year-fixed effects. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own data collected in 2015 (Panel A) and SOEP data are from 1987 to 1989 (Panel B).

Panel B of Table 2 shows that the relationship for the SOEP participants in the 1980s are similar to the ones in our survey from 2015. Again, lower health and life satisfaction is connected to more hours of TV watching. However, we can already observe that the coefficient on life satisfaction attenuates as soon as we add control variables. When we include fixed-effects, the results change even more drastically. There is no negative effect of changes in TV consumption on individual health or life satisfaction. This is evidence opposing the idea of negative effects from TV viewing for well-being and it demonstrates the importance to consider time-invariant characteristics when analysing differences in health and happiness. Our longitudinal findings indicate a selection of unhappy and unhealthy types into the group of more intense TV viewers. Since the endogeneity between TV watching and well-being cannot be addressed by an even more comprehensive set of controls, we turn to the private TV signal from the natural experiment.

1.5 Exploiting Exogenous Variation in TV Watching

1.5.1 Model

To identify the causal effect of TV on well-being, we exploit differential timing and geographical occurrence of new TV signals though an instrumental variable (IV) fixed-effects approach as follows:

$$TV\ Watching_{it} = \rho_0 + \rho_i + \rho_t + \rho_1 Private\ TV_{it} + \epsilon_{it} \tag{1.2}$$

$$Well\ Being_{it} = \gamma_0 + \gamma_i + \gamma_t + \gamma_1 Private\ TV_{it} + \eta_{it} \tag{1.3}$$

In the first stage of our model, we regress TV Watching on the Private TV signal (2) and in the reduced form we use the health and happiness indicators as dependent variables (3). To arrive at the local average treatment effect of TV Watching on Well-Being we use only the exogenous variation of $TV\widehat{Watching}_{it}$ from the Private TV signal, assuming that the TV signal only affects Well-Being through TV Watching:

$$Well\ Being_{it} = \beta_0 + \beta_i + \beta_t + \beta_1 TV\widehat{Watching}_{it} + u_{it} \tag{1.4}$$

By employing individual fixed-effects (ρ_i, γ_i, β_i), we exploit individual changes in private TV reception resulting in individuals watching more or less TV. We also control for any time trend in Well-Being or TV Watching using time fixed effects (ρ_t, γ_t, β_t).

In the following, we conduct first-stage regressions to inspect how different definitions of our instrument affect time-use, before we turn to reduced-form and IV results for our main outcomes. Afterwards, we discuss the exclusion restriction underlying the IV analysis and report results from several sensitivity analyses.

1.5.2 Time-use

Table 3 shows the manipulation of TV consumption through potential instruments of access to private TV. In line with historical market shares of German TV channels in the late 1980s, having the opportunity to watch terrestrial private TV channels increases time spent watching TV. This finding is robust when adding control variables in column two. The first definition of our private TV instrument in row one includes the two big NRW frequencies (see Section 2). Row two shows the instrument based on all private TV frequencies, including those with very low power. We observe a significant but comparatively weak effect, in line with our expectation that more powerful signals are more likely to ensure TV program reception and, thus, alter behavior. Varying the kW threshold of frequency powers sequentially supports this idea, as more power generally increases the hours watched per week from 0.9 hours per week for all transmitters and up to 1.6 hours per week for the most powerful frequency of 200 kW. To ensure a maximum F statistic for a relevant first stage, we focus on the highest ERP threshold of 200 kW, which is in line with our randomization check (Section 3.2) and with further evidence from the EVS based on TV ownership as manipulation variable (Appendix C).

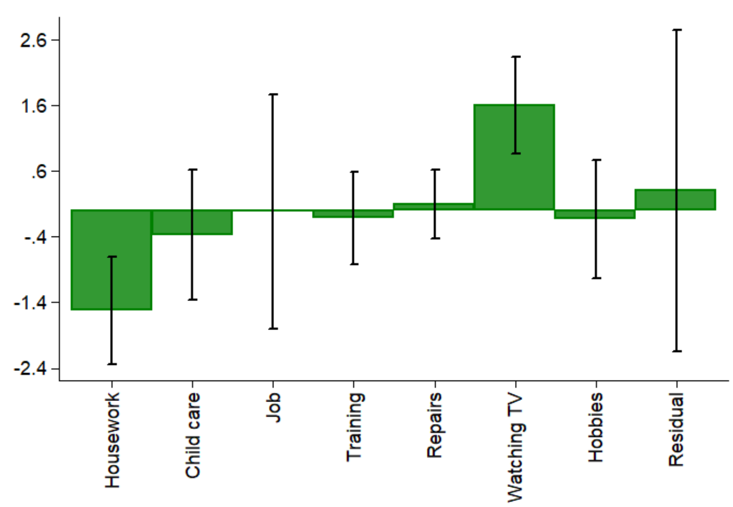
Table 1.3: Effect of Private TV Signal on TV Consumption

	TV Consumption	TV Consumption
NRW Private TV	1.125*** (0.417)	1.155*** (0.406)
F	7.290	8.123
ALL Private TV	0.896*** (0.300)	0.920*** (0.295)
F	8.940	9.734
10kW Private TV	0.906*** (0.334)	0.958*** (0.327)
F	7.344	8.585
20kW Private TV	0.887** (0.363)	0.938*** (0.359)
F	6.003	7.023
50kW Private TV	1.222*** (0.357)	1.249*** (0.347)
F	11.696	12.960
100kW Private TV	1.325*** (0.395)	1.331*** (0.386)
F	11.223	11.903
200kW Private TV	1.604*** (0.374)	1.611*** (0.362)
F	18.404	19.803
N	20,333	20,333
Control variables	NO	YES

Notes: Dependent variable is weekly TV consumption in hours. The baseline model is an individual fixed effects specification with year-fixed effects showing potential first stages. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. F is the Kleibergen-Paap F statistic. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

In Figure 2, we expand our time-use analysis to other activities. We make use of the 200 kW instrument of private TV access via terrestrial frequencies. For a comparison we include TV consumption as part of the seven time-use items and the residual interpreted as hours of sleep. We do not find any changes in this variable which contradicts the idea that individuals substitute sleep to watch more TV. The same is true for child care, work, training, repairs and hobbies. This suggests that respondents differentiate between hobbies and TV watching since the latter increases significantly. As the second significant finding in Figure 2, we observe that access to private TV leads to a reduction of about one and a half hours of time spent on housework per week. While TV signals cannot affect this activity other than through the effect of watching TV, the results suggest that housework is a possible substitute. This interpretation is further supported by the observation of a similar effect size compared to the increase in hours of TV watching.

Figure 1.2: Private TV Signal and Time-Use



Notes: Dependent variables reflect time in hours per week that a person spends on various activities taken from the SOEP time-use battery, with the exception of the residual time variable (which is 24 times 7 minus the total sum of all seven activities). The Private TV is a 200 kW dummy variable indicating whether the respondent lives in a county for which a TV signal based on terrestrial frequencies was calculated. The baseline specification contains year-fixed effects and county-level clustered standard errors. 95 percent confidence interval levels are displayed.
Source: SOEP data are from 1987 to 1989.

1.5.3 Main Results

Table 4 presents the intent-to-treat effects of TV on individual well-being. We regress well-being indicators on TV signal receipt in an individual fixed effects model as shown in column one. In column two, we add a set of control variables. The results show no evidence for any health impairments from the health satisfaction measure due to the opportunity to watch more TV. Similarly, both visiting the doctor (extensive margin) and the amount of doctoral visits show no significant effect from getting a terrestrial private TV signal.

Table 1.4: Intent-to-treat Terrestrial TV on Well-Being

	Health satisfaction		**Life satisfaction**	
	(1)	(2)	(4)	(5)
Private TV	0.107 (0.105)	0.116 (0.106)	0.272*** (0.078)	0.278*** (0.081)
N	20,311	20,311	20,288	20,288
	Visited A Doctor		**Doctor Visits**	
	(7)	(8)	(10)	(11)
Private TV	-0.010 (0.017)	-0.011 (0.018)	0.018 (0.309)	0.025 (0.310)
N	20,314	20,314	15,920	15,920
Control variables		YES		YES

Notes: The independent variable is the 200 kW private TV signal. The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The baseline model is an individual fixed effects specification with year-fixed effects showing reduced form estimates. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table 4 also shows the effect of TV signal reception on life satisfaction. Here, we obtain a significantly positive effect that is robust to the inclusion of control variables. Receiving private TV increases individual happiness, which contrasts previous results in the literature as well as our initial correlates (Section 4.2). Overall, unhappy individuals in bad shape seem to select themselves into the group of intensive-viewers. When we get rid of the selection problem, television appears irrelevant for health conditions and it even benefits individual happiness.

Table 1.5: Instrumental Variable Effects of TV Consumption on Well-Being

	Health satisfaction	Health satisfaction	Life satisfaction	Life satisfaction
TV Consumption	0.067	0.072	0.169***	0.172***
	(0.064)	(0.065)	(0.063)	(0.064)
F	18.404	19.803	18.404	19.803
N	19,190	19,190	19,166	19,166
	Visited A Doctor	**Visited A Doctor**	**Doctor Visits**	**Doctor Visits**
TV Consumption	-0.007	-0.007	0.010	0.013
	(0.011)	(0.011)	(0.171)	(0.168)
F	18.404	19.803	18.404	19.803
N	19,197	19,197	14,088	14,088
Control variables	NO	YES	NO	YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is weekly TV consumption in hours and the instrument is the 200 kW private TV signal. The baseline model is an individual fixed effects specification with year-fixed effects showing instrumental variable estimates. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * p < 0.10, ** p < 0.05, *** p < 0.01.
Source: SOEP data are from 1987 to 1989.

Table 5 shows IV fixed effects to obtain the local average treatment effect. We use the occurrence of private TV signals via terrestrial frequencies as an instrument for TV consumption, which is the endogenous variable on the second stage of our fixed-effects IV model. We find no effect on individual health from watching more TV, rejecting the expectation of corresponding health impairments, throughout all the specifications. As indicated by the F statistics, this zero result is not due to weak manipulation of TV watching. The life satisfaction result supports the idea that increases in TV consumption (if exogenously manipulated) increases individual happiness, which is robust to adding control variables. One more hour of TV consumption per week increases the LSF score by about 0.17 points. To put that into perspective, happiness research based on SOEP data shows significant reductions in life satisfaction when individuals become unemployed, with effect sizes that roughly vary between 0.5 and 1 on the 11-point scale (Clark et al. 2008, Kassenboehmer and Haisken-DeNew 2009, Chadi 2010). This suggests that increasing TV consumption by 3 to 6 weekly hours could compensate the unemployed for their unhappiness.

1.5.4 Discussion of Exclusion Restriction and Sensitivity Analyses

For a credible interpretation of the IV results, the exclusion restriction has to hold. The results are only valid under the assumption that TV consumption is the only channel through which well-being is affected by the television signal. We discuss possible violations of this assumption, including ideas that may be to some extent speculative, before we provide a summary of sensitivity analyses regarding our main results in the second part of this subsection.

First, the program on private TV might be politically biased in favor of those individuals who are more likely to receive a certain channel. Given the historical incidence (Appendix A), one could speculate that RTLplus possibly had a rather social-democrat leaning audience, which became happier due to politically convenient coverage. However, as we discuss later in Section 6, the political content was certainly not the focus of commercial TV organizers, but rather entertainment offers based on fiction were dominant in their programs. Second, there may be a direct health effect linked to TV signals and possible radiation. This is unlikely to affect our results, even if those health concerns were justified, since individuals are only exposed to additional frequencies from transmitter stations, which already broadcasted public media programs on even more powerful frequencies.[31] Further, our results throughout the paper show that health is not reduced due to signal reception. Third, , one might conjecture that the probability of receiving TV signals on powerful frequencies is more likely in densely populated areas and that individual life satisfaction increases in big cities for other reasons during our period of investigation, which then would induce a spurious result. However, our sensitivity analyses show that regions with large cities are not differently affected (Table B4). In summary, the discussion of our empirical checks together with the balance of covariates check (Table 1) increase our confidence that the exclusion restriction holds for the instrument used in our empirical analyses.

We further conduct a series of checks to be certain that our main findings are robust (see Appendix B for details). First, we change the calculation method for the TV signals at the regional level. Aggregation via mean (our default) or median on the raster level or calculating the signal on the municipality level and aggregating signals at the county level or directly calculating the signal at the county level does not change the results. Only calculating the signal based on the centroid of each county results in a drop in power but still with an F above 10, the result is qualitatively the same (Table B1). We interpret this as confirmation of our precise calculation procedure based on square-kilometer raster method, as higher precision indeed goes along with a stronger manipulation of TV consumption. Second, we vary the signal threshold for counties to be defined as treatment regions (50-55-60-65 dBuV/m) and we find that the results are robust (Table B2). Third, we check our decision to focus on signal strength thresholds for a binary distinction between treatment and control regions, instead of employing a linear signal strength variable. The results conform qualitatively to the results established via binary treatment indicator (Table B3). Fourth, we vary the definition of the control group to minimize the likelihood that households in the "control" areas had private TV access through alternative ways. First stage results are robust or become even stronger when we exclude, for example, counties with early cable projects and regions at the border (Tables

31. In case of the transmitter station Wesel, which was built in the 1960s construction phase to establish Germany's second public TV channel ZDF (see Figure A1), more powerful frequencies existed than the one that RTLplus received in the late 1980s. In line with our historical documentation (Appendix A), public media programs were preferred, while RTLplus received a remaining but still quite powerful 200 kW frequency. Given the historical relevance of this station, the "Wesel transmitter" ("Sender Wesel") is covered on Wikipedia where additional information can be found.

B4). Fifth, we check our empirical procedure regarding cases when individuals move between regions throughout our investigation period. It turns out that the findings are qualitatively similar if we modify our choices in this regard (Table B5). Sixth, we examine whether the definition of our time-use variable is of importance (Table B6). In fact, there are a few outliers with more than 70 hours of reported TV consumption per week.[32] To avoid arbitrary decisions, we keep those observations in our main data, but for the purpose of robustness, we recode the variable by reducing the reported number to a maximum of either 100 or 70 hours of TV consumption per week. We also employ a monotone logarithmic transformation of TV watching. All those changes do not affect our results, they are not driven by outliers. Seventh, the results do not change significantly, when we expand our model by adding survey factors as control variables, such as the week of the survey and interview mode. In a final check, we also merge the SOEP data with information about weather conditions on the day of the interview and weeks before. Adding such controls also does not change our results (Table B7). All in all, we conclude that the results are robust and confirm our finding: TV makes people happy, not unhappy.

1.5.5 Effect Heterogeneity

We further investigate the main findings based on groups that are expected to be most affected by the treatment of television to understand the plausibility of the main effects. Previous research suggest that females are more susceptible to TV than men. Benesch et al. (2010) therefore conduct separate analyses for the sexes, and they make a distinction by age. Differentiating by age also makes sense in our setting since commercial TV providers focus on younger audiences that are believed to be more susceptible to advertisement. Motivated by recent research on TV, such as Durante et al. (2019), we also examine differences between low and high educated individuals, testing the idea of private TV as light entertainment for the uneducated, and we distinguish between households with children vs. those without, given that private TV programs could be particularly attractive for minors.

Table 6 suggests evidence for gender differences in the effect of television on life satisfaction. A

32. In the sample used for our analyses, the share of reported TV hours per week above 70 hours is 0.5 percent.

direct test via interaction terms shows no statistically significant difference, but the point estimates as well as statistical significance levels indicate that the happiness effect due to having access to private TV is driven by females. This finding could be seen as informative in light of research on the empowerment of women via TV (Jensen and Oster 2009), since role models of successful women may be particularly effective in changing behavior if women enjoy watching even more than men do. As a second observation, it appears that our main finding is not driven by one specific age-group. The effect is highly significant for individuals below 50. Again, we do not observe a statistically significant interaction term in a separate regression analysis, which could be due to lack of statistical power. Nevertheless, there is a visible difference when we compare the results in Table 6 after conducting subgroup splits, since the effect is highly significant for the young and insignificant for older individuals.

Table 1.6: Heterogeneous Intent-to-Treat Effects of TV Consumption on Life Satisfaction

	Female	**Male**	**Young**	**Old**
Private TV	0.231*** (0.084)	0.158* (0.082)	0.264*** (0.071)	0.134 (0.117)
N	10,204	10,084	13,301	6,987
	High Education	**Low Education**	**No Children**	**Having Children**
Private TV	0.211** (0.088)	0.176* (0.093)	0.143 (0.105)	0.271*** (0.092)
N	7,517	12,771	11,726	8,562

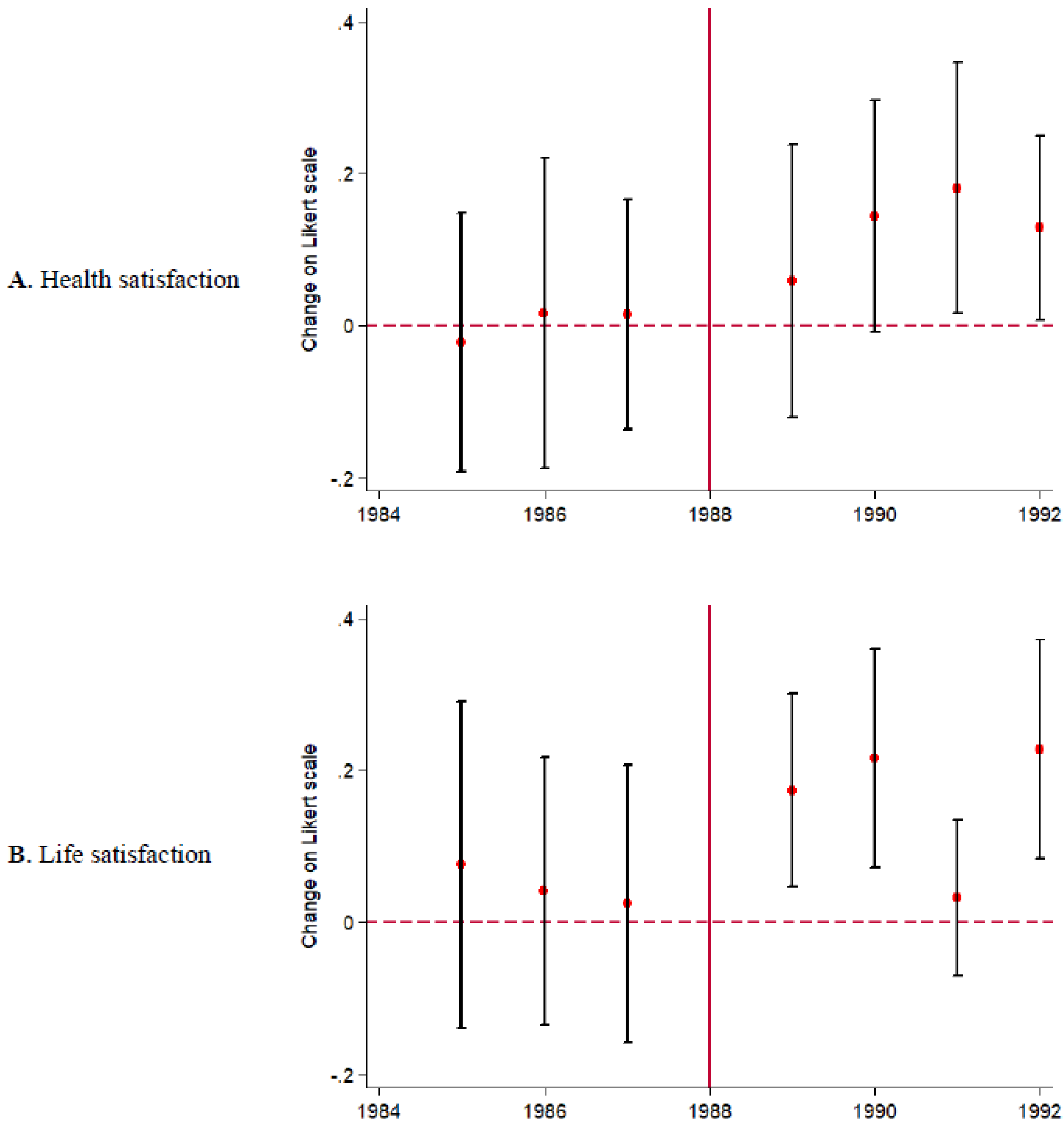
Notes: The independent variable is the 200 kW private TV signal. The dependent variable is life satisfaction on a 0 to 10 scale. High Education is defined as more than 11 years of education and young is defined as being below the age of 50. The baseline model is an individual fixed effects specification with year-fixed effects showing heterogeneous reduced form estimates. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

For education, our finding is somewhat surprising at first, given Table 6 reveals that higher-educated individuals respond more positively to private TV. This seems to counter the idea of TV as light entertainment. However, recent research on the cognitive development of children in Norway finds similar effect heterogeneity when comparing the impact of private TV on educational outcomes in high vs. low educated families (Hernæs et al. 2019). Finally, Table 6 shows that the happiness effect seems to be driven by individuals living in a household with children, whereas the effect is insignificant for households without children. We interpret this finding in such a way that families with children benefit the most from television since they can substitute other forms of spending time with children by watching TV together. At this point, it would be interesting to know more about health and life satisfaction implications of television for minors, but due to the SOEP focus on adults at the time of the natural experiment, we cannot pursue this avenue of further research.

1.5.6 Extension of Time Period

An interesting question is, whether the happiness effect is short-term or long-term in nature. Recall that we focus on the main phase of 1987 to 1989, as the SOEP time-use battery does not include information on TV consumption beyond 1989. Therefore, we are restricted in our efforts to inspect the long-term effects of exposure to private TV via terrestrial frequencies, but we can expand the data for supplementary intent-to-treat analyses, in which we continue to employ our main outcomes as dependent variables and private TV access as the independent variable without consideration of any manipulation variable. By expanding the investigation until 1992, we add several years of treatment via terrestrial TV signals from the time before private TV became universally accessible in Germany. Simultaneously, we expand the number of treatment-free years without private TV signals for both control and treatment regions by adding data from 1985 and 1986, allowing for a balanced number of treatment and control years. We present results from the expanded data based on a dynamic treatment analysis for both outcome variables that are continuously available in the SOEP, i.e. life satisfaction and health satisfaction, in order to find out whether we can confirm the previous findings from our short-run analysis.

Figure 1.3: Dynamic Perspective of Satisfaction Measures



Notes: The illustrations are based on a dynamic effects reduced form model showing intent-to-treat coefficients within a 95 percent confidence interval. Life and health satisfaction are both measured on a scale ranging from 0 ("completely dissatisfied") to 10 ("completely satisfied").
Source: SOEP data are from 1985 to 1992.

Figure 3 displays the dynamic evolution of our reduced form results for individual well-being in two panels. In Panel A, we confirm that across all treatment years, the effect of receiving private

TV on health satisfaction is not negative. Instead, the panel displays minor positive effects that are not instantaneous but seem to grow slowly, reaching even statistical significance in two years. Panel B shows that the life-satisfaction effect from TV consumption is immediately positive in 1989, in line with our short-run analysis, and also continuously positive. Statistical significance is reached in three out of four post-treatment years without any clear evidence for a fading-out of the happiness effect, despite increasing availability of private TV via cable and satellite in control regions without terrestrial TV signals.

The results in Figure 3 indicate that the happiness effect of TV is not just of short-term nature. It appears that individuals do not just watch private TV because it is a novel and exciting experience for them at first. For health, the results are clear evidence against the hypothesis that TV viewing increases individual sickness. This does not seem to be the case, despite a year-long TV treatment effect in our setting. Quite the contrary, our evidence for health satisfaction rather raises the question if television might even improve individual health. One interpretation could be that increases in life satisfaction have spill-overs on health, as happier individuals might be less immune towards psychological problems like depression.[33] This interpretation is amplified by the visual evidence in Figure 3 showing that the positive life-satisfaction effect happens first, and positive health-satisfaction effects seem to trail behind.

We use the other health measures in the SOEP to conduct further analyses of possible long-run effects. Table D5 (Panel A) reveals that the measures for doctoral visits (i.e. visited a doctor, number of doctor visits) are unaffected when we exploit two additional years of TV treatment in a reduced-form analysis. By using our long panel, we can now also analyze information on hospital stays, which are available for two treatment years and may reflect strong differences in individual health. Table D5 (Panel B) shows no significant effects of television in this indicator for ill-health, independent of the chosen definition (i.e. incidence of a hospital visit, number of hospital visits, number of nights in hospital, log number of nights in hospital). While these findings do not provide additional support for the idea of positive health effects, the evidence against

33. See Argyle (1997) who discusses the idea of happiness as a cause of health by highlighting the role of positive moods.

negative long-run health effects is now well-grounded on a set of three different indicators. We discuss the possibility of positive (or negative) health effects due to television further in the following subsection by turning to a different dataset.

1.5.7 Health-related Expenditures

We proceed by using the EVS data to investigate different effects of exposure to private TV on consumer activities. Due to the restriction of our data on two waves, i.e. 1988 and 1993, we compare the changes in outcomes over time for municipalities receiving private TV on a powerful terrestrial frequency since 1988 as the treatment regions against municipalities not receiving a signal. Our main outcomes are informative about health-related behaviors and are analyzed using the following difference-in-difference model:

$$Expenditures_{it} = \delta_0 + \delta_1 PrivateTV_{it} + \delta_2 \mathbb{1}(1993)_{it} + \delta_3 PrivateTV \times \mathbb{1}(1993)_{it} + \zeta_{it} \quad (1.5)$$

We deviate from models used for our SOEP analysis, as we cannot consider individual fixed-effects when analysing two waves of the EVS with its repeated cross-sectional nature. However, we do examine possible pre-treatment differences between regions and consider those in the regression analysis. Furthermore, we shed light on the robustness of results by checking our findings for a different definition of control regions, i.e. by excluding municipalities that receive signals for a shorter period of time (starting after 1988) as well as signals from transmitters on less powerful frequencies.[34] To check the definition of the TV signal regarding signal strength and power, we show the results for the "number of own TV sets" in Appendix C. Based on this alternative indicator for changes in TV consumption, we find evidence in favor of successful manipulation via terrestrial TV signals similar to the TV/Video time-use item in the SOEP data.

To understand possible effects of TV on individual health, we focus on expenditures for health-

34. Excluding low-power frequency regions could play a particular role in the analysis of the EVS due to increased geographical precision of TV signal identification. In some cases, a low-power signal might be insufficient to affect outcomes county-wide, as analyzed in the SOEP, but within counties, at the municipality level, such local frequencies could have an impact.

related products and services. Table 7 shows expenditures on doctoral services as a first outcome. Assuming that sick individuals pay more for doctoral services to become healthy again, higher expenditures could indicate bad health. However, higher expenditures can also indicate higher health awareness. A second variable is expenditures on medical products, which may serve as another health proxy, assuming that sick individuals pay more for products that promise to tackle their sickness.

Table 1.7: Health-related Expenditures

	Doctoral services	Doctoral services	Medical products	Medical products
$\mathbb{1}(1993)$	158.359**	147.208**	-0.514	0.341
	(11.562)	(13.870)	(0.590)	(0.791)
Private TV	-4.155	7.966	-0.404	0.141
	(23.837)	(24.617)	(0.975)	(1.024)
$\mathbb{1}(1993) \times \mathit{Private\ TV}$	63.798*	74.950**	-1.590	-2.445*
	(33.851)	(34.709)	(1.355)	(1.454)
N	65,584	41,763	65,584	41,763
Clean Control Regions		YES		YES

Notes: The dependent variable is expenditures on medical products and doctoral services. Private TV is defined as living in regions with 200kW powered TV signals starting in 1988. The baseline model is a difference-in-difference specification showing reduced form estimates. Municipality-level clustered standard errors are in parentheses. Levels of significance are * p < 0.10, ** p < 0.05, *** p < 0.01.
Source: EVS data are from 1988 to 1993.

Column one, row one of Table 7 shows that in 1993 the annual expenditures for doctoral services were about 160 DM higher compared to 5 years earlier.[35] In 1988, there were no visible differences in such expenditures between treatment and control regions of private TV (row two). For the main effect of private TV on doctoral services, we observe an increase of approximately 60

35. At the time, the currency "Deutsche Mark" (DM) was used in West Germany.

DM. While the effect is weakly significant for the main data, it becomes somewhat stronger and significant at the 5% level for the smaller dataset with only clean control regions in column two.

Given the zero result for the number of doctor visits in the SOEP, a possible interpretation is that individuals in the treatment regions went to the doctor with more severe health issues and thus had to pay more. However, several points contradict such a conclusion in favor of negative health effect from watching TV. First, the effect is not robust. Adding control variables to our regression model based on the full dataset renders the effect insignificant.[36] The effect is also insignificant when we add region fixed effects using dummy variables for all of the several thousand municipalities in Germany. Second, since most Germans are part of the social healthcare system and do not have to pay for doctoral services in general, there could be a change in the total amount of expenditures on doctoral services due to more individuals being privately insured in the treatment regions. In fact, further evidence from employing the same approach on an EVS variable for private health insurance supports this idea, suggesting that private TV induces more individuals to become privately insured (either as main insurance or as an add-on to public healthcare). This could be due to more advertisement for such services on private TV compared to public TV. Third, if treated individuals went to the doctor because of more severe health issues, we would expect to see similar evidence for a positive TV effect in medical products, but the remainder of Table 7 does not support this. If at all, there seems to be a reduction in monthly expenditures on medical products when clean control regions are used. However, effect sizes of roughly 2 DM reductions in expenditures are rather small and adding control variables to our regression model based on the full data shows an insignificant effect, which we also find when adding region fixed effects.

For interpretation of the results on medical products, one has to keep in mind that our starting hypothesis was that TV reduces health. In line with our findings based on the SOEP data, the result for the EVS data does not support this hypothesis. Given our results, another interpretation is that TV actually increases individual health, for example, through better mood. Indeed, if we assume that independent of health status, the expenditures on medical products increase separately

36. The control variables are similar to those used in our SOEP-based analysis and include gender, age, quadratic age, household size, having children, house ownership, renter, municipality size and month of expenditure elicitation.

because of more advertisement for pharmaceutical drugs on private TV, then this could mean that we underestimate the positive effects on individual health. In the absence of further evidence, we conclude that positive health effects of TV are a possibility.

1.6 Discussion

To learn more about the generalizability of our results we discuss the content of the TV programs which were watched by millions of Germans in the late 1980s. One might argue that commercial TV in West Germany differs from other types of television, such as television in the US, and could be a very specific form of TV. However, a strong similarity to US television exists for entertainment content. TV viewers of RTLplus and other commercial channels often watched movies and series that originated in the US (e.g. the hit series “A-Team” or the top movie ”E.T.”). A more difficult question is whether commercial TV in West Germany was substantially different from public TV, which could be helpful to understand our positive result for life satisfaction.

Research on potential differences in content between commercial and public TV finds some differences in content analyses for the 1980s programs. According to Krüger (1989), information-related programs on the two major public TV channels had a higher share of the total content (ARD: 33,5%, ZDF: 39,8%) compared to their main competitors in commercial TV (Sat.1: 26%, RTLplus: 22,4%). Instead, entertainment offers based on fiction were more prevalent in private TV (Sat.1: 50,6%, RTLplus: 49,3%) compared to public TV (ARD: 30,8%, ZDF: 30,0%). This seemed to be what German TV consumers particularly liked about commercial TV, according to research based on early cable-TV projects in the 1980s, which shows how viewers focused on entertainment programs, albeit not solely (Hasebrink 1989).[37] While public broadcasts in Germany follow a mandate to educate the public as justification to collect fees, private TV channels also

37. Indirect evidence for the claim of very similar program offers between public and private TV, both focusing on light entertainment, can be derived from the work on individual’s willingness to protest in East Germany (Kern and Hainmueller 2009, Kern 2011, Crabtree et al. 2015). This research shows that there was no anti-communist propaganda effect due to receiving Western public TV in regions close to the Western border, since any ideological effect was over-compensated by a general reduction of political activity induced by television with its emphasis on entertainment.

agreed to have some political content with an educational purpose.[38] Therefore, private TV in West Germany has some characteristics of public TV, and differs from it mainly in the sense that it is even closer to the television offer that individuals in the United States watch.

This narrative of private TV as light entertainment with some additional sophisticated content, in ways not very dissimilar to public TV in Germany, is further supported empirically. First, our subgroup analysis shows that not just individuals with low education were attracted by private TV in West Germany. Second, in our 2015 survey we asked questions about the role of commercial TV and its program quality in comparison to public broadcasts. A minority of 40.5% believe that public broadcasts fulfil their educational mandate. As that is the justification for the compulsory charge of TV license fees in Germany, it is no surprise that, in a question about the amount of fees that are justified, many respondents deviate from the actual level of pay. Four out of five respondents stated amounts below the actual fees and a fifth of the respondents considered zero fees to be justified, thereby supporting an abolishment of publicly financed TV in Germany. In a third survey question on the differences of the two forms of TV, public and private, half of the survey participants stated either "yes" the program quality was similar or that this "more or less" was the case (29.4% respectively 21.3%). While we cannot say which direction the other half of the respondents perceived differences, it provides further credence that many viewers of TV in Germany do not perceive a quality premium of public broadcasts over commercial channels.

In summary, our evidence suggests that private TV and public TV in Germany do not differ substantially in individuals' perception, which leaves the question open whether there were any specifics in our setting that can explain the positive life satisfaction effect. A novelty effect of commercial TV seems plausible, given low expectations and negative views (see Appendix A). However, our long-run analysis in Section 5.6 rejects this idea, leaving us with the simple explanation that television in general can facilitate increases in individual happiness.

38. Commercial TV providers dedicated parts of their program to sophisticated political and cultural topics. Apart from image concerns, broadcasts like RTLplus had agreed to air shows like Spiegel TV as a major step to convince skeptics in the political sphere who opposed private TV and its proliferation in Germany.

1.7 Conclusion

Despite the large amount of evidence on the economic and social impact of television, no study so far has provided a sufficient answer to the question why individuals do watch so intensely. We align the observation that watching TV is one of the most important single activities of humans with the basic idea that individuals make rational decisions that are not to the detriment of their well-being. In contrast, we find that by watching TV, individuals do not suffer health impairments, and they certainly do not experience unhappiness as a result. Quite the contrary, based on our evidence from a natural experiment in West Germany, we can answer the question "Does watching TV make us happy?" (Frey et al. 2007) with a simple "Yes."

Our findings seem to tell a very positive story. First, we can reject claims that watching TV is necessarily bad for health and, hence, a major public-policy concern. While we want to be clear that our study does not inform us about the consequences of heavy TV viewing in a lifelong context, we argue that increases in TV consumption of one or two hours per week, even for longer periods, do not threaten individual health. Second, our study is the first to provide evidence on a happiness-increasing effect of television, which is good news for those who want to promote the maximization of national happiness as a public policy goal. Television seems to be a very cheap and effective policy instrument, if one follows that notion. The attractiveness of TV, as demonstrated in our study, allows using it as a means to affect individual views and behavior, as it has been implicitly or explicitly suggested in research on edutainment offers (Kearney and Levine 2019) and development policies (La Ferrara 2016).

Having said this, one could also draw very different policy conclusions. In fact, given the manipulative nature of TV, as demonstrated in research on political bias (DellaVigna and Kaplan 2007, Enikolopov et al. 2011, Durante et al. 2019), our findings explain why individuals have difficulties in resisting the TV set, given that it makes them happy without inducing any apparent and immediate costs in terms of ill-health. Our findings also help us to understand the paradox finding on the natural experiment of TV in East Germany where viewers of anti-communist West TV in East Germany were not less supportive of their own regime. The term "opium for the masses" chosen by political scientists Kern and Hainmueller (2009) describes well how television, according to our results, increases individual happiness by so much that it could help stabilize a political system in decline even if it is under ideological attack by the very same TV program. In consequence, there is reason to concur with skeptics of TV such as Germany's former Chancellor Helmut Schmidt describing television as "dangerous" (Appendix A). The other interpretation of our finding on life satisfaction is therefore that individuals maximize their own welfare while TV consumption leads to negative externalities. This narrative would be supported by the findings on TV affecting social outcomes, as listed in the introduction. In addition, the effects of TV for the family in form of more divorces and lower fertility (Boenisch and Hyll 2015) might be of particular importance in developed countries like Germany, which faces major demographic and economic challenges as a result of historically low birth rates. Intriguingly, all the examples of possible negative externalities were pointed out as drawbacks of television by those who tried to defend the national ban on private TV in West Germany. While these attempts were ultimately unsuccessful, they have allowed us as researchers to analyze a unique and fascinating natural experiment, but looking into the past, we have to wonder whether allowing television to flourish has made individuals happier at the expense of exactly the consequences that the skeptics of TV warned about.

2. VACCINES AT WORK

2.1 Introduction

Seasonal influenza causes substantial morbidity and mortality around the world. The World Health Organization (WHO) estimates that the flu is associated with three to five million cases of severe respiratory illnesses and between 290,000 to 600,000 deaths per year worldwide (WHO 2018). The flu is associated with an economic burden of approximately \$34.7 billion in the United States, most of it due to lives lost and foregone work (Rothman 2017), and 16 million days of productivity lost (Molinari et al. 2007). Flu vaccination has the potential to be a cost-effective way to reduce the incidence of the disease and its costs. From an immunological perspective, the flu vaccine increases the level of individual immunity by generating antibodies, which promises to reduce the transmission rate of the disease (Gross et al. 1989; Cox et al. 2004).

However, individual behavior can counter the potential benefits of vaccination in two ways. First, according to the World Bank, the Center for Disease Control (CDC), and other public health institutions, vaccination rates in most countries of the world are substantially below recommended levels.[1] Therefore, it is essential to understand the factors that affect vaccination take-up, particularly of working adults who are the least likely to get the vaccine. Second, economic theory and empirical evidence suggest that the adoption of protective technologies may induce individuals to behave riskier. Vaccinated individuals may overestimate the protection that the vaccine grants and engage in risky behaviors like waiting longer before going to the doctor when feeling sick and taking fewer protective measures to prevent illnesses. Thus, moral hazard could counter the benefits of adopting a preventive medical technology like the flu vaccine.

In this paper, we exploit the background of a company's vaccination campaign to comprehensively study for the first time both determinants and consequences of influenza vaccination. In

1. Public health institutions recommend that everybody over six months should vaccinate against the flu. However, flu vaccination rates in European countries ranges from 2% to 70% (Mereckiene 2015), and only 38.5% of adults 18 and older were immunized in the United States during the 2017-2018 flu season (Srivastav et al. 2018).

cooperation with a bank in Ecuador that provides annual vaccination campaigns to improve its employees' health, we implement a natural field experiment by randomizing incentives to get a flu shot.[2] Our design introduced three modifications to the bank's 2017 vaccination campaign to create exogenous variation in vaccination take-up. First, implementing a company policy of income-dependent subsidies, we selected an income threshold at which the vaccine's price for employees changed. Second, due to capacity constraints, employees had to be assigned to vaccinate during the workweek or on Saturday. By randomizing the assignment of employees for vaccination, we could manipulate the opportunity costs of vaccination because employees would need to incur additional transportation costs and arrange their weekend schedules to get vaccinated on a Saturday. In contrast, assigning employees to the workweek minimizes their opportunity costs because the bank allows them to take time off their duties to get vaccinated at the firm's location. Third, we varied the content of the invitation emails by appealing to altruistic or selfish motives.

Exogenous variation in vaccination generated by these modifications of the campaign allows us to study the consequences of getting a flu shot. First, we analyze the effects of peer vaccination on the propensity for a co-worker to also get vaccinated. Second, we study the impact of individual vaccination as well as peer vaccination on employees' health and sickness-related absence. Third, we analyze how getting vaccinated affects the behavior of employees in order to discuss the possibility of moral hazard when adopting medical technology.

Our design overcomes several challenges that arise when studying the causal effects of vaccination on health-related outcomes. The first challenge is to identify the causal effect of getting vaccinated. While the medical literature documents modest positive health effects of flu vaccination, many of the studies could be affected by selection and other biases (Jefferson et al. 2010; Osterholm et al. 2012; Demicheli et al. 2014; Østerhus 2015 and Demicheli et al. 2018). For instance, researchers describe the problem of a "healthy vaccine recipient effect" that could bias observational studies. If healthier individuals are more likely to get vaccinated, such a positive se-

2. We follow the definition of a natural field experiment by studying individual behavior in an environment where subjects naturally make their decisions without knowing that they are participants in an experiment (Harrison and List 2004).

lection bias could lead to an overestimation of the health effects. Nevertheless, observational studies without randomization of vaccination are often preferred because of ethical concerns regarding randomized controlled trials with placebos in the context of health (Sanson-Fisher et al. 2007; Baxter et al. 2010). For the same reason, RCTs are often conducted using other types of vaccines instead of clean placebos to provide potential health benefits for experimental participants in the control group (Loeb et al. 2010). We present a methodological alternative that addresses ethical concerns by using the exogenous variation in vaccination generated through the manipulation of incentives to take part in the campaign. To study the impact of vaccination on health-related outcomes, we thus employ a random encouragement design (Bjorvatn et al. 2015; List et al. 2017). This is an innovative idea in the context of preventive medical technologies circumventing the ethical dilemma of withholding a potentially effective medical treatment while allowing for causal evidence.

The second challenge we overcome is capturing the total effect of vaccination. Public health institutions and companies are interested in the total effect of health interventions that includes medical and behavioral responses. However, medical research on vaccines focuses solely on the medical effects, without considering changes in behavior that may affect health. In randomized controlled trials, participants know that they are in an experiment but do not know if they received a specific type of vaccine or not. This eliminates the possibility of identifying changes in behavior when comparing experimental conditions. In contrast, our random encouragement design introduces no uncertainty in treatment, capturing both the behavioral and medical effects of getting vaccinated. This allows us to explore if vaccination induces individuals to adopt riskier behaviors.

The bank's data allows us to address a third challenge. We may underestimate the medical effectiveness of the vaccine because of positive health-spillovers from the vaccinated to the unvaccinated (White 2019). The empirical setting attenuates this concern as flu vaccination rates in Ecuador fluctuate around 2% (ENSANUT 2012). To assess the role of spillovers directly, we estimate the effect of peer vaccination on health outcomes. For a comprehensive analysis of health-related outcomes, we have access to the bank's administrative data that we merge with information on treatment assignment at the individual level. The data include detailed medical diagnoses for each employee so that we can identify illnesses, including flu diagnoses, and the resulting sick days. We can also distinguish flu-related sickness from non-flu-related sickness, which allows us to study the behavioral effects of vaccination. Finally, employee surveys before and after the vaccination campaign complement the administrative data and allow inspection of mechanisms for the effects on employee health and behavior.

Regarding the incentives to vaccination we exogenously introduced, we find the following first-stage results. A change of $2.48 in the vaccine's price did not affect take-up. Conversely, decreasing opportunity costs by assigning employees to get vaccinated during the workweek increased take-up by slightly more than ten percentage points, which constitutes an increase compared to Saturday of roughly 100 percent. Thus, reducing opportunity costs has a remarkably strong effect on take-up for working adults. Finally, we find no effect from providing information on altruistic or personal benefits of vaccination. The coefficients are close to zero, negative, and statistically insignificant.

Next, we exploit the exogenous variation in vaccination created by randomly assigning employees to get vaccinated in the workweek to study the consequences of vaccination. First, we study the effect of peer vaccination on individual take-up. For this purpose, we exploit exogenous variation in the proportion of peers who get vaccinated. In our setup, coworkers that work directly together every day define the relevant social groups. Given randomization at the employee level, by chance, some units have more employees assigned to the workweek than other units, and hence, are encouraged to get the vaccine more than in other units. We find that when the proportion of peers that get vaccinated increases by ten percentage points, take-up increases by 7.9 percentage points. More in-depth analyses of the mechanisms behind these peer effects suggest that peers are not changing information or beliefs about vaccination, but instead, employees follow behaviors that they deem socially acceptable by conforming to the peer prescriptions of their working group.

Second, as a question of high relevance for policymakers and firms that run vaccination campaigns, we investigate the consequences of vaccine take-up by examining if flu vaccination is effective in improving working adults' health, thereby potentially lowering sickness-related absence. If flu vaccination decreases flu cases, we expect that offering employees the opportunity to get vaccinated during the workweek would reduce the number of flu cases and absence from work. However, the estimates show no evidence that exogenously triggered vaccination decreased sickness in general or sickness-related absence. By using the data from the medical records, we also cannot find that the probability of getting the flu changed due to participation in the vaccination campaign. The confidence intervals rule out effects that correspond to meaningful thresholds of an effective vaccine based on CDC figures.

There are several potential explanations why we cannot find evidence for health improvements due to vaccination. It could be that positive health-spillovers from vaccinated to unvaccinated individuals explain why we underestimate health benefits. However, the results show that peer vaccination does not affect the probability of being diagnosed sick or having a sick day, which is consistent with unit vaccination rates being below herd immunity levels. It could also be that the flu vaccine was medically ineffective, which our design cannot rule out. Finally, independent of the vaccine's medical effectiveness, employees could adopt riskier and thus health-threatening behavior that could mitigate the vaccine's immunity benefit.

We provide evidence from several behavioral tests consistent with the notion of individuals adopting riskier behaviors when they get vaccinated. Vaccinated individuals could overestimate the protection of the vaccine and avoid going to the doctor when they have flu-like symptoms. We test this hypothesis by investigating the effects of vaccination on non-flu respiratory illness during a national health emergency. The flu vaccine does not provide immunity against non-flu respiratory illnesses, so flu vaccination should not affect the probability of being diagnosed with these diseases. However, flu and non-flu respiratory diseases share symptoms, so a person cannot distinguish them unless she goes to the doctor. In January 2018, as a result of a significant increment of flu cases nationwide, the Ecuadorian government launched a massive media campaign asking the population to go to the doctor if they felt any flu-related symptom. If vaccinated individuals felt protected, they would have been less likely to follow the government's calling when feeling flu-like symptoms, resulting in fewer visits to the doctor and fewer diagnoses of non-flu respiratory diseases that share symptoms with the flu in that month compared to unvaccinated employees.

First, we check if vaccination affects the likelihood of being diagnosed with a non-flu respiratory disease. If vaccinated individuals feel more protected, they might think flu-like symptoms correspond to a minor respiratory illness, and not heed the government's calling, which would reduce non-flu diagnoses. Consistent with this hypothesis, the results show that assigning individuals to the workweek decreased the likelihood of being diagnosed with a non-flu respiratory disease by 7.2 percentage points during January, with no effect in other months.

Second, by the same logic, we check if vaccination affects the likelihood of going to the bank's doctor at the on-site health center. This health center is a convenient feature for the employees because they can visit the doctor during work hours without asking for permission and without charge. Before our intervention, on-site doctors accounted for 77 percent of all diagnosed sickness cases. Vaccinated individuals may have been less likely to visit these doctors when the government launched its media campaign because they felt more protected. In January 2018, we find that being assigned to the workweek for vaccination decreased the probability of going to the onsite doctor by 8.6 percentage points, with no effect in other months.

As the final test of moral hazard, we check if individuals report abandoning practices believed to be effective in improving health, regardless of their actual medical effectiveness. We find that assigning employees for vaccination on the workweek decreased the frequency of individuals reporting to engage in practices culturally believed to help prevent the flu and other respiratory diseases. Moreover, these effects are driven by individuals who believe the vaccine is beneficial to prevent the flu, which supports the idea of vaccinated individuals feeling protected and engaging in riskier practices.

Our findings contribute to two strands of literature. The first is the literature on the determinants of take-up of vaccines and other medical technologies. Previous studies mainly discuss how vaccination take-up is affected by laws, information, education, age, health status, health behavior, and lifestyle (Maurer 2009; Schmitz and Wübker 2011; Godinho et al. 2016; Bradford and

Mandich 2015; Chang 2018; Oster 2018). Few studies have considered how compensating the opportunity costs of vaccination affects vaccine take-up of children and vulnerable groups in rural areas in developing countries (Banerjee et al. 2010; Sato and Takasaki 2018b) or populations with limited income (Bronchetti et al. 2015) by providing in-kind transfers.[3] For the effect of peers on the adoption of medical technologies, the theoretical literature predicts free-riding on vaccination benefits due to herd immunity, but empirical research based on non-hierarchical peer networks such as friends or neighbors finds mixed results (Geoffard and Philipson 1997; Kremer and Miguel 2007; Chen and Toxvaerd 2014; Rao et al. 2017; Sato and Takasaki 2018a). Rao et al. (2017) find that providing information and changing beliefs is the mechanism through which non-hierarchical peers affect the adoption of medical technologies.

Our study contributes to this literature in several ways. First, we employ a unique setup that allows for variation in different types of costs. We manipulate the opportunity costs of vaccination directly by changing the day of vaccination, which changes the next best alternative participants face. Thus, we directly test the effect of changing these costs, which can be different from compensating through reward incentives.[4] We find that reducing opportunity costs has a substantial effect on vaccination of working-age adults who are not constrained by income and who live in locations where access to vaccines is not an issue, as in most major cities in both developing and developed countries. The estimates are of similar magnitude as previous studies that focused only on vulnerable populations, which implies that opportunity costs are an important factor for any population. In contrast, a small change in the vaccine's price did not raise take-up suggesting that financial incentives must be substantial in order to be effective. Also, information nudges were ineffective, like those in previous studies (Bronchetti et al. 2015; Godinho et al. 2016). Second, we study how the adoption of medical technologies can be affected by a peer group that has received

3. Economic theory identifies both monetary and opportunity costs as a relevant component in the decision to adopt medical technologies like vaccination (Brito et al. 1991; Geoffard and Philipson 1997; Kremer and Miguel 2007; Chen and Toxvaerd 2014).

4. Behavioral economics studies have found that losses are treated differently than gains since the seminal theory of Kahneman and Tversky (1979).

little attention in this research area so far: co-workers.[5] Most working adults share at least half of their awake time with their coworkers. Unlike friends and other non-hierarchical peers, employees cannot choose the individuals they are going to work with after they are hired. We document that this peer group can have a significant positive influence on the adoption of preventive medical technologies like vaccines, which is inconsistent with the theoretical concept of free-riding in this context. Our evidence indicates that the primary mechanism for peer effects in vaccine take-up is the employee conforming to the norms of their workgroup. This result provides a new policy lever to influence the adoption of preventive medical technologies in working adults.

Our study contributes to the literature on the consequences of medical technologies as well as the literature of on-site health interventions (Just and Price 2013; List and Samek 2015; Belot et al. 2016), and the broader literature on public health interventions (Cawley 2010; Butikofer and Salvanes 2018). Our findings on the health effects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with a few recent exceptions in the economics literature (Ager et al. 2017, Lawler 2017, Carpenter and Lawler 2019). Regarding studies on flu vaccines, Ward (2014) finds that flu vaccination increased sickness absences in years when the flu vaccine had a bad match with the prevalent flu viruses, and it had no effect in years when the vaccine had a good match. The difference between these two results, which would control for moral hazard, points to the medical benefits of the vaccine. In another vaccination study, Anderson et al. (2020) found no effect of vaccination on hospitalization and mortality on the elderly using a regression discontinuity design. Very few medical studies consider the possibility of medical technologies unintentionally causing moral hazard (Richens et al. 2000; Prasad and Jena 2014) while the few papers in economics that study moral hazard in the context of medical interventions find mixed results (Klick and Stratmann 2007; Margolis et al. 2014; Moghtaderi and Dor 2016; Doleac and Mukherjee 2018).[6]

5. In contrast, there is a large body of research about the effects of co-workers on the productivity of their peers at the workplace (Mas and Moretti 2009; Herbst and Mas 2015).

6. There is a large literature studying whether the adoption of safety devices lead individuals to adopt riskier practices (Peltzman 1975; Richens et al. 2000; Auld 2003; Cohen and Einav 2003; Klick and Stratmann 2007; Peltzman 2011; Prasad and Jena 2014; and Talamàs and Vohra 2018) and also a large literature that studies moral hazard in insurance, e.g. see Einav et al. (2013) and Einav and Finkelstein (2018).

We contribute to this literature in three ways. First, we employ a novel design for public health and medical interventions, allowing us to circumvent measurement and ethical problems. We hope to encourage other researchers to use the same methodology in the field of health to obtain causal estimates. Second, with our evidence on the consequences of flu vaccination for sickness-related absence, we contribute to the research on the determinants of this workplace outcome (Ziebarth and Karlsson 2010; Bütikofer and Skira 2018). Third, we provide behavioral evidence based on experimental variation that getting vaccinated induces individuals to feel protected and to forgo preventive practices like going to the doctor in the presence of illness symptoms. This result is consistent with preventive medical technologies causing moral hazard and with the theoretical model of Talamàs and Vohra (2018). Finally, by showing that preventive medical technologies can unintentionally cause moral hazard, we offer an explanation of why health interventions may not always be as successful in improving health. This finding implies that firms and policymakers should consider moral hazard when promoting the adoption of preventive medical technologies.

2.2 Experimental Design

We ran the field experiment in cooperation with a bank in Ecuador. This bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters are in Quito, Ecuador's capital, and it has six branches across the country with over 1,300 employees, distributed in 31 divisions with 142 working units. The bank had run small vaccination campaigns in the past. These campaigns included only some employees in crowded areas and ran during the workweek in the bank's offices.[7] In 2017, the bank decided to extend its annual campaign to all its employees and allowed us to experimentally modify it to investigate how to increase take-up and the effects of vaccination. We implemented three interventions: we changed the vaccine's price for some employees using income-dependent subsidies, we randomized assignments for on-site vaccinations across weekdays, and we implemented information nudges by varying the content of the emails used to invite employees to vaccinate.

7. These areas included the call center and the collections departments, which only have few employees. We exclude the call center from our analysis of the 2017 campaign, as we have evidence that the call center supervisors pushed their employees into taking the vaccine leading to a take-up of almost 100%.

The bank decided to provide the vaccine for free to areas that participated in campaigns in previous years and to partially subsidize it for new participants. Since the company opposed the randomizing subsidies, we used information on employees' income to allocate this subsidy. Employees who earned less than \$750 per month would pay \$4.95 to get vaccinated, while those who earned more than \$750 would pay \$7.43. Note that the vaccine's full price is \$9.99. Employees were informed about the vaccine's price in their invitation email. This email included basic information about the campaign and informed employees that the payment for the vaccine was directly deducted from their paycheck if they opted to get vaccinated. The email also contained information on the assigned day and time. Appendix Figure A1 shows an example of an invitation to a low-price flu shot on Thursday morning.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[8] First, employees assigned to the control group (Control) were invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday) and were allowed to take time off their duties to get vaccinated. The specific day was selected randomly for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on Saturday. The employees usually do not work during the weekend, so they would incur extra transportation costs and have to arrange their schedules to go to the bank and get vaccinated.[9] Otherwise, this group received the same information as the Control (see Figure A2). This treatment was only applied in Quito because all the other branches are substantially smaller (82% of the employees work in Quito), and their employees could get vaccinated in a single day, which was not possible in Quito.[10]

We also implemented two information nudges. We kept the additional messages as unobtrusive as possible to prevent confounding the effect of information with salience or other behavioral fac-

8. The bank requested that we exclude the CEO and another high executive from the intervention. We also excluded our contact in the Human Resources department and four employees who work in the local branches and did not have a company email address to deliver the treatments.

9. Based on data from the employees' magnetic swipe cards to enter the bank, only 0.4% of the employees work regularly on Saturdays.

10. Branches in the coastlands were randomly assigned to get vaccinated on Wednesday, and branches in the highlands were assigned to Thursday.

tors. The first nudge highlights the social benefits of flu immunization (Altruistic Treatment). In addition to the information provided to the control group, the email included the phrase: "Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly and people with serious health conditions that cannot get vaccinated" (see Figure A3). The second nudge highlights the individual benefits of flu immunization (Selfish Treatment). In addition to the information provided to the control group, the email included the phrase: "Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies" (see Figure A4). Employees in these two treatments were assigned to get vaccinated during the workweek, while the specific day was selected randomly.

Our intervention targeted the Ecuadorian flu season, which usually covers the period from November to the end of February (Ropero 2011). The bank ran a pre-intervention survey from October 25 to October 29, 2017. Human Resources sent the intervention emails on November 1, 2017, using its official email account. Employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the Human Resources department. Employees are used to receiving emails from Human Resources, and according to the Human Resources manager, they typically read these emails carefully. The bank sent out a reminder using the same email account a week later. The vaccination campaign ran from November 8 to November 11, 2017, at locations within the bank's offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-survey during March and April 2018.[11]

2.3 Data

This section describes the data used in our analyses for assessing how monetary and non-monetary determinants can affect take-up and the effects of flu vaccination. First, we have access to the firm's administrative records about its employees: gender, age, education level, and de-

11. The geographic locations of the banks' branches are displayed in Figure A5 and a depiction of the timeline is shown in Figure A6. Figure A7 provides information about the flu vaccine used and Figure A8 shows an individual getting vaccinated during the campaign.

pendents; job and its position within the bank's organizational structure; tenure and income; and medical diagnoses and sick days. Second, we collected vaccination take-up data from the bank's campaign records. Third, we use data from pre- and post-intervention surveys. These surveys asked employees about previous illnesses and general health, knowledge, and beliefs about vaccination and the flu vaccine, habits related to health, relations with coworkers, opinions about the campaign, motivation, organizational attachment and work satisfaction, and risk and time preferences.

Table 2.1: Summary Statistics

	Full Sample	Control	Altruistic	Selfish	Saturday	F-test (p-value)
Monthly Income ($)	1,766	1,860	1,701	1,681	1,827	0.316
Company Tenure (years)	7.9	8.3	7.7	8.1	7.5	0.761
Prop. Women	0.49	0.51	0.52	0.46	0.47	0.497
Age (year)	36.6	37.2	36.4	36.6	35.7	0.553
Prop. College Education	0.91	0.92	0.91	0.90	0.93	0.759
Pre Survey Participation	0.48	0.50	0.50	0.47	0.40	0.171
Post Survey Participation	0.36	0.36	0.38	0.33	0.35	0.519
Diagnosed Sick	0.66	0.67	0.67	0.64	0.67	0.835
Granted a Sick Day	0.37	0.37	0.40	0.37	0.34	0.797
Diagnosed Flu Sick	0.11	0.09	0.13	0.13	0.10	0.348
Vaccination Take-up	0.17	0.22	0.17	0.19	0.08	0.070
N	1,164	344	294	310	216	

Notes: This table characterizes the mean employee of the bank, where we implemented our intervention. We present statistics for the full sample and the four treatment groups. The last column presents the p-value of a joint significance test to check whether there are significant differences across the treatment groups. The proportion of employees diagnosed sick or granted a sick day corresponds to the period between January 1 and November 7, 2017, before the vaccination campaign.

Table 1 presents the mean characteristics of the bank's employees (Column 1). On average, employees earn a total monthly income of $1,760. As a reference, in 2017, the average total income in Ecuador was $479, which implies that the bank's employees are in the three highest deciles of the Ecuadorian income distribution (ENEMDU 2017). The average employee has been in the company for more than seven years and is around 36 years old. The company employs roughly the same number of men and women, and more than 90% of its employees have at least some college education, close to education levels in developed countries. Almost 50% of the employees completed the pre-intervention survey, a high completion rate compared to previous surveys from Human Resources. The completion rate decreased to 36% for the post-intervention survey.

The administrative data include two measures of health: medical diagnoses and sick days. These measures come from two sources: onsite doctors and medical certificates from outside doctors (72 different physicians in total). It is important to note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[12] Consequently, the onsite doctors report every visit they receive to Human Resources. The doctors report the diagnosis (the type of disease), whether they granted a sick day or not, and the number of sick days granted. Also, by law, if an employee takes time off work to go to an outside doctor, then she has to present a medical certificate to Human Resources that indicates the diagnosis and number of sick days granted, if any. Hence, in addition to sick days, we can also observe employees being diagnosed sick with no sick days granted for cases where a doctor did not consider the illness severe enough. Thus, sick days are a measure of more severe illness. From January to early November 2017, before the intervention, two out of three employees were sick from any disease, and 37% had at least one sick day (see Table 1).

Doctors diagnose their patients using a combination of a physical examination, blood tests and culture tests. The specific procedure is part of individual medical records to which we do not have access. Diagnoses that name the 'flu' as the reason for being diagnosed sick provide us with the narrowest definition of flu-related sickness. If flu cases presented complications, then the data

12. By law employees in Ecuador also have up to one year of paid leave due to sickness. Employers are not allowed to terminate employment during sick leave.

reports the complication as the diagnosis and does not mention the flu explicitly. To address this issue, we have an extended definition of flu-related sickness, which includes diagnoses that could likely be complications due to the flu, according to a third-party physician. We focus on this measure in our empirical analysis and check the robustness of the results by employing the narrowest definition and an even broader definition, also provided by this physician. Any other respiratory disease that was not classified by this doctor as flu is by definition listed as a non-flu respiratory disease. A second physician verified these measures to be sure about the distinction between flu-related and non-flu-related health problems.

Table 1 also shows evidence on the balance of treatment assignment. Columns 2 to 5 present the mean employee characteristics across the four groups. All variables have almost identical means across groups. For each characteristic, Column 6 shows the p-value of a joint significance test of differences of means. We cannot reject the null hypothesis that the means are the same across the four treatments, which suggests that our randomization was successful. The Kruskal-Wallis rank test shows the same result. Finally, we test whether participating in the pre and post surveys is different across treatments. We find no statistically significant difference.

2.4 Analysis of Vaccination Take-Up

In this section, we study how monetary and non-monetary determinants affect working adults' decision to vaccinate. Specifically, we consider the effect of opportunity costs, information nudges, and peers on take-up in detail. We do not find any effect of the $2.48 price difference on vaccination take-up from the income-dependent vaccine subsidy.[13] We conclude that such price change may be too small to induce changes in take-up behavior.

The last row in Table 1 presents the flu immunization take-up rates for the different treatments during the campaign. The Control group has a take-up rate of 22%, the Altruistic treatment has a take-up of 17%, and the Selfish treatment has a take-up of 19%. Comparing across the three groups suggests that the information treatments were not sufficient to increase take-up. In contrast,

13. Figure A9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also do not indicate any significant change in take-up at the cutoff which is robust to different bandwidths (see Table A1).

being assigned to get vaccinated during the workweek increases take-up by 14 percentage points in contrast to Saturday (112%).[14] We extend the analysis of these effects in the next section.

2.4.1 Effects of Opportunity Costs and Information on Individual Take-up

We model the effect of opportunity costs, altruistic information, and selfish information on vaccination take-up for employee i in city c using the following equation:

$$Takeup_{ic} = \alpha + \gamma_c + \pi_1 Saturday_{ic} + \pi_2 Altruism_{ic} + \pi_3 Selfish_{ic} + u_{ic} \tag{2.1}$$

where $Takeup_{ic}$ is an indicator of getting vaccinated. We include Quito fixed effects γ_c to account for differences in implementation of the vaccination day assignment across branches as discussed in Section 2. $Saturday_{ic}$, $Altruism_{ic}$, and $Selfish_{ic}$ are dummy variables that indicate treatment assignment. Thus, we estimate the effect of the different treatments relative to those individuals who were assigned to vaccination on the workweek and did not receive any information nudge.

Table 2 presents the effects of the different treatments on take-up. Column 1 shows the baseline results of the effect of opportunity costs and information on vaccination take-up. The estimates indicate that assigning employees to Saturday decreased take-up by 7.9 percentage points compared to the Control. This effect is approximately 46% of the take-up in Quito for the Control and is statistically significant at the 1% level. Hence, minimizing the opportunity costs associated with vaccination is a useful measure to increase take-up.

14. In the post-intervention survey 59 employees report that they got vaccinated outside the campaign. Vaccination outside the campaign is not significantly different by treatment status. All employees who stated they got vaccinated outside, did not get vaccinated during the campaign. One individual stated not getting vaccinated, even though the person did get vaccinated according to our records. 18 individuals stated they got vaccinated during the campaign but did not. If we exclude those 19 individuals, which misremember vaccinations, the estimates do not change. Also note that between November 2017 and February 2018, 20 treated employees quit the bank. Attrition is not affected by treatment assignment.

Table 2.2: Effects of Treatments on Vaccination Take-Up

	Baseline	With Controls	Quito Sample	Non-Compliance	Day of Week Effects
Altruistic Information	-0.0260	-0.0209	-0.0493	-0.0262	
	(0.0310)	(0.0303)	(0.0332)	(0.0306)	
Selfish Information	-0.0032	-0.0011	-0.013	-0.0103	
	(0.0314)	(0.0316)	(0.0339)	(0.0308)	
Thursday					0.0002
					(0.0346)
Friday					-0.0356
					(0.0331)
Saturday	-0.0789***	-0.0791***	-0.0898***	-0.0671**	-0.0818***
	(0.0301)	(0.0304)	(0.0313)	(0.0298)	(0.0315)
Average take-up base group in Quito		0.1732		0.1623	0.1651
N	1164	1164	929	1152	929

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

Notes: Robust standard errors in parentheses. This table presents OLS estimates of the effect of the different treatments on vaccination take-up. All specifications control for Quito fixed effects. Column 1 presents our main estimates from equation (1) without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4, we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. In Column 5, we test for different effects across the different days of the week using only data from Quito that has all the treatments. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance. For Columns 1-3, we define the base group as the Control group in Quito. For Column 4, it is the same group but adjusting the sample for non-compliance, and for Column 5, it is the take-up rate on Wednesday in Quito.

Conversely, we find that emphasizing either the altruistic or the selfish benefits of vaccination does not affect take-up. The coefficients are close to zero, negative, and statistically insignificant. It is plausible that supplying a sentence of additional information is not enough to further increase take-up, given that reducing opportunity costs has a substantial effect on it.[15] One interpretation of these results is that information would have to be very salient to accrue an effect on vaccine take-up in a company context such as this.

Columns 2-4 of Table 2 show the robustness of the results to the inclusion of controls, to the use of a restricted sample, and to controlling for non-compliance. Specifically, Column 2 shows that controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level does not affect the estimates. Column 3 addresses the fact that only employees who work in the bank's headquarters in Quito were assigned to vaccinate on Saturday. In this subsample, assigning employees to Saturday decreased take-up by almost nine percentage points (51% of the control group take-up), significant at the 1% level. This result is slightly larger than the main result, but we cannot reject that they are statistically the same. Both information treatments have small, negative, and statistically insignificant effects. Column 4 shows the effect of controlling for non-compliance.[16] In this subsample, assigning employees to Saturday decreased take-up by 6.7 percentage points, significant at the 5% level. We cannot reject that this estimate is statistically the same as the baseline result. The estimates of the effect of the information treatments are practically the same as the main estimates.

Lastly, in Column 5, we check whether assignment to different days in the week affects take-up differentially. We exploit the fact that vaccination days are randomly assigned, and we regress our indicator of vaccination take-up on dummies for the assigned day (Wednesday, Thursday, Friday, or Saturday), using Quito's subsample.[17] These estimates show that take-up on Thursday and Friday

15. The post intervention survey asks if the employee recalls the altruistic and selfish information statements. Appendix Table A2 shows that neither employees assigned to the Altruistic treatment nor those assigned to the Selfish treatment remember their respective statements better than the control. Another issue could be spillovers of information, but this is unlikely given that our design provides information directly to the treated individuals via email.

16. We identified in the campaign records 12 employees assigned to the workweek who vaccinated on Saturday. The bank asked the medical team in charge of the vaccination campaign to enforce the day assigned to each employee, but they failed to enforce this requirement on Saturday and were unable to send employees back home if they showed up that day. In contrast, nobody of those assigned to Saturday got vaccinated during the workweek.

17. Of the bank's employees in Quito, after excluding the call center, 23.4% where assigned to vaccinate on Wednesday, 26.7% to Thursday, 26.5% to Friday, and 23.4% to Saturday.

is not statistically different from take-up on Wednesday, while the effect of Saturday is substantially larger in magnitude and very close to the baseline estimate in Column 1.[18] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the Saturday effect and are consistent with increasing opportunity costs.[19]

2.4.2 Further Evidence on Opportunity Costs

We analyze heterogeneous treatment effects across different subgroups of our study population, which may yield further evidence that opportunity costs are driving the difference in take-up between being assigned to vaccinate on the workweek and Saturday.[20] We focus on differences across gender, distance to work, and employees with and without children.[21] Figure 1 shows that assignment to Saturday has more substantial effects for men than for women, although the difference is not statistically significant.

Distance to work reflects the transportation costs that an individual regularly incurs. The median employee lives 6.5 km away from work. Figure 1 shows that those who live further away than the median are slightly less likely to get vaccinated when they were assigned to Saturday than those who live closer to the bank, but this difference is not statistically significant. This result is consistent with increasing travel costs, but the magnitude suggests that travel costs are not the main factor driving the difference in take-up between employees assigned to the workweek and Saturday.

Finally, we consider differences in the effect between employees with and without children. Having children may imply higher opportunity costs at the weekend by increased family obligations. Figure 1 shows that assignment to Saturday decreased take-up by 10.6 percentage points for employees with children, while the effect is smaller (5.3 percentage points) and insignificant for

18. While the effect of assignment to Friday is not significant, it is 44% of the effect of Saturday and two orders of magnitude larger than the effect of Thursday. Being assigned to Friday can slightly increase the opportunity cost of vaccination because it is only a 6-hour workday instead of an 8-hour workday.

19. Also, the Control includes employees assigned to Wednesday, Thursday and Friday, so any effect of procrastination is included in the comparison made in the baseline estimates.

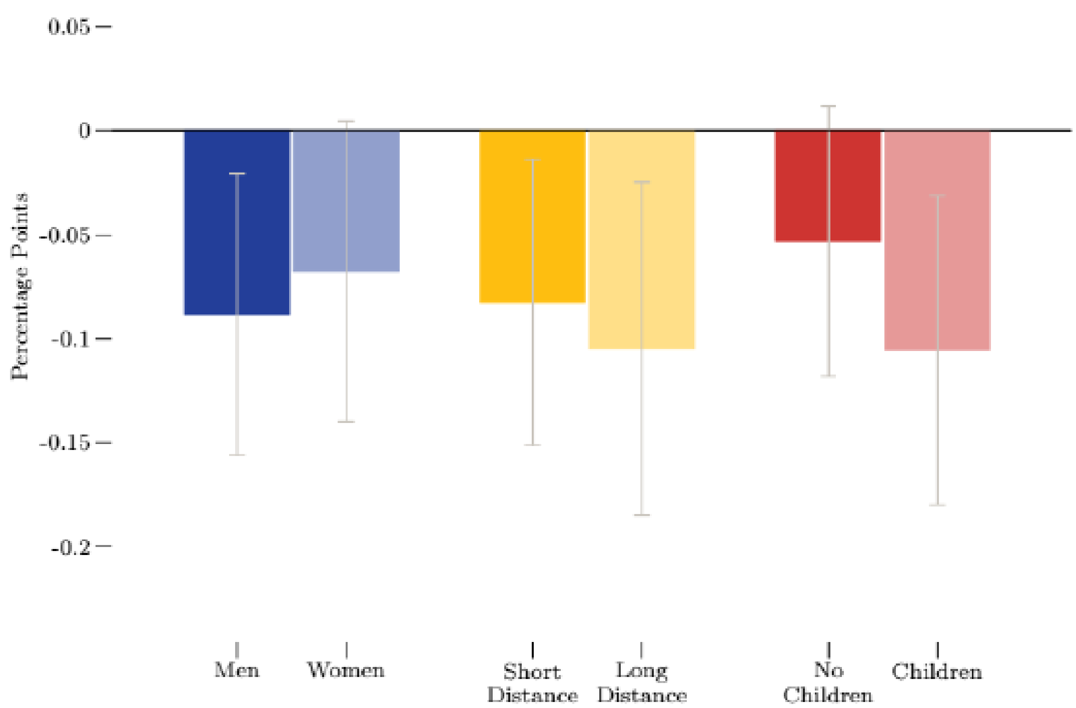
20. We find that the information treatments have no differential effect across subgroups. These estimates are small and statistically insignificant. See Appendix Table A3.

21. Distance to work was calculated with a geo-location service using employees' home addresses.

employees without children. Although the difference between these two effects is not significant, its magnitude is consistent with the idea that opportunity costs increase for individuals assigned to Saturday.

In conclusion, these results suggest that the difference in take-up between employees assigned to the workweek and Saturday corresponds to a change in the opportunity costs of vaccination. We use only this variation in take-up created by lowering opportunity costs as an instrument in the rest of our analyses.

Figure 2.1: Heterogeneous Effects of Assignment to Vaccination on Saturday on Take-up



Notes: This figure presents the intent-to-treat effect of assignment to Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents the point estimate and the 90% heteroscedastic robust confidence interval for each subgroup.

2.4.3 Peer Effects on Vaccination Take-up

Peer effects may play an important role in vaccination behavior by either increasing or decreasing take-up. When a person gets vaccinated, the prevalence of the disease may decrease, making it less likely for others to get sick. Thus, if getting vaccinated has costs, then it may be optimal for some people not to do so if their peers got vaccinated. Theoretically, this free-rider problem can result in a Nash equilibrium, where nobody takes the vaccine (Chen and Toxvaerd 2014). Conversely, peers may increase take-up by exchanging information that affects individual beliefs about the flu and the vaccine. Also, individuals may imitate the health care behavior of their peers to conform to social norms (Kremer and Miguel 2007).

The exogenous variation in take-up created by assigning individuals to get vaccinated in the workweek allows us to estimate peer effects in vaccination. The bank's units define the social groups of employees that work directly together. Thus, we can identify the effect of social groups with whom adults share a large portion of their daily time on vaccine take-up. We will also use this approach to analyze peer effects in health caused by vaccinated peers below (Section 5).

We model the effect of the proportion of peers in unit j who take the vaccine on employee i's decision as

$$Takeup_{ijc} = \gamma_c + \beta_1 Prop.Takeup_{jc} + \beta_2 X_{ic} + \beta_3 \bar{X}_{jc} + \pi_3 Workweek_{ic} + u_{ijc} \tag{2.2}$$

where $Prop.Takeup_{jc}$ is the proportion of peers in unit j who get vaccinated and $\bar{X}_{jc}$ are the average observable characteristics of peers j. Manski (1993) shows that if we estimate equation (2) by OLS, self-selection, common environmental factors and reflection confound the true peer effects β_1 and β_3. However, in our design, employees are randomly assigned to vaccinate on the workweek independently of their unit. This creates an exogenous variation that affects the proportion of peers who get vaccinated independently of employee i's decision to get vaccinated because, by chance, some units have more employees assigned to the workweek than other units.

We can average equation (2) across unit j to obtain the first stage equation:

$$Prop.Takeup_{jc} = \frac{\gamma_c}{1-\beta_1} + \frac{\beta_2+\beta_3}{1-\beta_1}\bar{X}_{jc} + \frac{\pi_3}{1-\beta_1}Prop.Workweek_{jc} + \frac{\bar{u}_{ijc}}{1-\beta_1} \tag{2.3}$$

where the proportion of peers in unit j who get vaccinated is a function of the proportion of peers who were randomly assigned to the workweek ($Prop.Workweek_{jc}$). Random assignment implies that $Prop.Workweek_{jc}$ is uncorrelated with both $\bar{X}_{jc}$ and $\bar{u}_{ijc}$. Hence, the reduced form equation is

$$Takeup_{ijc} = \frac{\gamma_c}{1-\beta_1} + \frac{\beta_1\beta_2+\beta_3}{1-\beta_1}\bar{X}_{jc} + \beta_2 X_{ic} + \frac{\beta_1\pi_3}{1-\beta_1}Prop.Workweek_{jc} + \pi_3 Workweek_{ic} + \tilde{u}_{ijc} \tag{2.4}$$

In our design, the exclusion restriction holds because the proportion of peers that got vaccinated is the only channel through which the proportion of peers assigned to the workweek can affect the individual's vaccination decision. Hence, we can combine the estimates from equations (3) and (4) to obtain an IV estimate of the effect of the proportion vaccinated peers on the employee's take-up. The error term in equation (4) includes both the individual error from equation (2) and the average error from equation (3), so we cluster the standard errors at the unit level.

Panel A in Table 3 presents the main results. The first stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased the proportion of peers that get vaccinated by 3.1 percentage points. The effective F-statistic of Montiel Oleas and Pflueger is 16.48, so we can reject the null of weak instruments for a threshold of 20%, which suggests that the instrument is relevant. The estimates in columns 2-4 show that peer vaccination has a positive effect on individual take-up and that not accounting for endogeneity biases the effect downwards. The IV estimate in Column 4 indicates that a ten percentage points increase in the proportion of peers that get vaccinated increased take-up by 7.9 percentage points. The results are robust to controlling for the total number of employees in the unit and mean age and gender of the peers (Appendix Table A4).[22]

22. Mechanically, smaller units may have larger proportions. We also control for the proportion of peers in the unit

Table 2.3: Effect of Peer Vaccination on Individual Take-up

	First Stage	Reduced Form	OLS	2SLS
Proportion of Peers:				
Assigned to the Workweek	0.3106***	0.2454***		
	(0.0765)	(0.0844)		
Vaccinated			0.5116***	0.7900***
			(0.0748)	(0.1777)
F-value	16.481			
N	1138	1138	1138	1138

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

Notes: Standard errors clustered at the unit level in parentheses. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers, and the reported coefficients measure percentage point changes. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents the results for the first stage. Column 2 displays the results of the reduced form. Column 3 presents OLS estimates of the effect of a change in the proportion of peers that get vaccinated. Column 4 presents 2SLS estimates of the effect of a change in the proportion of peers that get vaccinated.

Two potential mechanisms behind the positive peer effect on individual take-up are peers changing personal beliefs about vaccination or individuals following behavior that they deem socially acceptable. To disentangle these potential channels, we first explore if peers might be changing personal beliefs. We exploit the post-intervention survey questions on beliefs and knowledge of flu vaccines and interactions with coworkers related to vaccination. Even though answering the post-intervention survey is not correlated with treatment assignment (Table 1), the first stage loses precision due to the smaller sample size in the survey. We focus on reduced-form analyses to prevent issues with finite sample bias in the IV estimate.

who have some managerial position. The point estimates are not affected by including this control variable.

Table 2.4: Potential Mechanisms for Peer Effects

	Effect of Prop. of Peers Assigned to the Workweek on	Baseline	N
a. Beliefs about the Flu, its Vaccine, and Interactions with Coworkers			
Vaccines Effective to Improve Health (1-5)	-0.0015	3.74	378
	(0.0049)		
Talked with coworkers about getting vaccinated (pp)	-0.0065***	0.56	359
	(0.0021)		
Went with coworkers to get vaccinated (pp)	0.0010	0.13	359
	(0.0014)		
Probability of Getting Healthy Without the Vaccine (0-100)	-0.0054	44.25	366
	(0.0712)		
Probability of Getting Healthy With the Vaccine (0-100)	0.0300	56.48	366
	(0.0911)		
Informed about the Flu (0-100)	0.0104	69.80	371
	(0.0721)		
Informed about the Flu Vaccine (0-100)	0.0112	63.70	371
	(0.0970)		
Afraid of the Flu (0-100)	0.0457	37.20	371
	(0.1231)		
Afraid of the Flu Vaccine (0-100)	0.0935	24.66	371
	(0.1178)		
Would Get Vaccinated out of the Workplace (pp)	-0.0024	0.61	366
	(0.0020)		
Coworkers Convinced me to get Vaccinated (0-100)	0.0399	20.60	359
	(0.1221)		
I Convinced my Coworkers to get Vaccinated (0-100)	-0.0537	28.37	359
	(0.1329)		
b. Heterogeneous Effects for Extrinsically and Intrinsically Motivated Individuals			
Vaccination of Extrinsically Motivated Individuals (pp)	0.4961***	0.13	247
	(0.1271)		
Vaccination of Intrinsically Motivated Individuals (pp)	0.2240	0.16	262
	(0.1672)		

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the reduced form effect of peers assigned to the workweek on a series of outcomes identified by the row headers. The measurement unit of each outcome is in parentheses next to the outcome's name. We measure the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents estimates, Column 2, the baseline value for each outcome, and Column 3, the sample size.

Panel A in Table 4 shows the results on a set of 12 outcomes. Of these outcomes, the proportion of peers assigned to the workweek had a negative and significant effect only on talking with coworkers about vaccination.[23] Employees assigned to the workweek are less likely to mention this to their coworkers because usually, events organized by the bank take place during the workweek.[24] There is no significant effect on any of the questions regarding information or beliefs about the vaccine or on questions that measure how much coworkers influenced the vaccination decision. Moreover, the point estimates are small compared to the baselines, which suggests that peer behavior did neither affect beliefs nor supplied new information about the vaccine.

As another mechanism of the positive peer effects on vaccination take-up, we test if employees following behavior that they deem socially acceptable. Akerlof and Kranton (2000) show that identity-related behavior, i.e., conforming with the prescriptions or norms of a group, can rationalize a series of behaviors by bolstering a sense of belonging or preventing a loss of image in the group. This creates a channel through which an individual's actions can cause responses from others. To test this mechanism, we estimate how the behavior of different subsets of peers affects individual vaccination, following the idea that groups create feelings of belonging that could affect behavior (Akerlof and Kranton 2000; Hoffman et al. 1996; Perkins 2002). For instance, Akerlof and Kranton (2000) argue that as every person is assigned a gender, individuals have an incentive to follow the prescriptions of their gender to reaffirm their own identity. A similar behavioral response can appear for other groups to which the individual has a sense of belonging. However, to which group individuals react is an empirical question that depends on the context. Table 5 presents these results.

23. This effect is robust to adjusting for the false discovery rate as in Anderson (2008).

24. Additionally, an employee who learns she is in a unit with a large proportion of employees assigned to Saturday might feel lucky that she was assigned to the workweek and get vaccinated. This would bias downwards the estimate of the effect of the proportion of vaccinated peers on take-up in Table 3.

Table 2.5: Heterogeneous Peer Effects on Individual Take-up

	Proportion of Peers Vaccinated:					
	In Managerial Positions	Not in Managerial Positions	Similar Age	Different Age	Same Gender	Different Gender
First stage	0.1710*	0.3058***	0.0859***	0.2504***	0.2222***	0.2136***
	(0.0885)	(0.0789)	(0.0225)	(0.0472)	(0.0570)	(0.0696)
	[3.7302]	[15.0272]	[14.6127]	[28.1757]	[15.1879]	[9.4195]
Reduced form	0.0184	0.2746***	-0.0084	0.1336**	0.1700***	0.1026*
	(0.0525)	(0.0749)	(0.0606)	(0.0611)	(0.0635)	(0.0607)
2SLS	0.1075	0.8928***	-0.0974	0.5298***	0.7603***	0.4780*
	(0.2904)	(0.1552)	(0.7153)	(0.2030)	(0.1906)	(0.2473)
N	982	1082	1138	1138	1101	1030

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

Notes: Standard errors clustered at the unit level in parentheses, first stage F-values in brackets. The bank has 116 units with more than one employee. This table presents the heterogeneous effects of different types of peer vaccination take-up on the individual's vaccination decision. The column headers identify the type of peer, for example, in the first column, we present the effect of the proportion of peers of the same gender who got vaccinated on individual vaccination. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. The first row presents the first stage results; for example, the fifth cell indicates that if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then the proportion of peers of the same gender who got vaccinated increases by 0.22 percentage points. The second row presents reduced form results. For example, for the fifth column, if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then individual take-up increases by 0.17 percentage points. The third row presents 2SLS estimates. For example, for the fifth column, if the proportion of peers of the same gender assigned who got vaccinated increases by one percentage point then individual take-up increases by 0.76 percentage points. All specifications control for Quito fixed effects and individual assignments to the workweek. Sample size changes because not everybody has a peer in the same category. For example, if a manager has no manager peers in her unit, then that observation is dropped.

First, we study if managers influence an individual's vaccination decision. If individuals react to their managers' behavior, this could be in response to an order and not because of the unit's norms. Columns 1 and 2 in Table 5 show that individuals do not follow the behavior of peers in managerial positions, but follow the behavior of peers not in those positions, those who constitute the majority of the working unit.[25] A ten percentage point increase in the proportion of vaccinated peers not in managerial positions increased take-up by 8.9 percentage points.

Second, Columns 3 and 4 in Table 5 show that individuals do not react to the behavior of peers of similar age (within a three-year bandwidth) but react to peers of different ages. A ten-percentage point increase in the proportion of peers of different ages who get vaccinated increased take-up by 5.3 percentage points. Given the large age dispersion within the units (Appendix Figure A10), it is unlikely that age groups can create feelings of belonging. Hence, these results are in line with the idea that individuals follow the behavior of the working unit.

Third, we test whether employees follow behavior they deem socially acceptable from their gender. Column 5 shows that a ten-percentage point increase in the proportion of peers of the same gender who get vaccinated increased take-up by 7.6 percentage points. This effect is almost identical to the main estimate and is driven by men. Column 6 shows that the effect of peers of a different gender is 37% smaller and is not significant. These results indicate that the behavior of gender groups influence individual actions, which is consistent with previous research.

Finally, we check if extrinsically motivated employees are more likely to be affected by peer behavior. Intuitively, intrinsically motivated individuals like doing their job and are more likely to work hard. These employees do not need extrinsic incentives from their employers to be motivated to comply. In contrast, the extrinsically motivated employees respond to external stimuli from their surrounding environment, which implies that they should be more likely to follow peer behavior. The pre-intervention survey has questions to determine if employees are intrinsically or extrinsically motivated.[26] Panel B in Table 4 shows the reduced form effect of the proportion of peers assigned to the workweek on these subgroups. For extrinsically motivated employees, a ten-percentage point increase in the proportion of peers assigned to the workweek would increase take-up by five percentage points, while intrinsically motivated employees' take-up would increase

25. Managerial positions include supervisors, line bosses, assistant managers, and managers.

26. The intrinsic motivation measure is a dummy variable based on a median split of a summation of four measures of motivation in the workplace where employees state how important it is that they (i) learn something interesting, (ii) get motivated to think about things, (iii) gain a thorough understanding of content and (iv) feel that their opinions are considered.

by only 2.2 percentage points. Together, these pieces of evidence indicate that the estimated peer effects are a consequence of individuals conforming to the norms of their workgroup.

2.5 Analysis of the Effects of Vaccination on Health and Risky Behavior

In this section, we exploit random assignment to get vaccinated in the workweek as an instrument to study if flu vaccination improves health and thereby reduced sickness absence in our intervention. In order to shed light on one of the potential mechanisms underlying these results, we use the same approach to explore if getting vaccinated can induce health-threatening behaviors, i.e., moral hazard.

2.5.1 Effects of Flu Vaccination on Health and Absence

Flu vaccines may affect health through multiple avenues, direct and indirect. First and foremost, getting vaccinated could have a direct effect on health by increasing immunity against four strands of the flu virus. Besides, the results in the previous section show that if a person gets vaccinated, the likelihood that her peers get vaccinated increases. This effect would imply that an employee's peers are more protected against the flu, which may decrease the transmission rate of the disease. Thus, positive peer effects on vaccination take-up could create an indirect channel through which getting vaccinated might have a positive effect on health. The proportion of vaccinated peers within the 142 units in the firm varies substantially between 0 and 67%, which indirectly could play a role in health outcomes.[27] Ideally, we could estimate the effect of flu immunization on health-related outcomes (Y_{ijc}), such as medical diagnoses and sick days, through these two channels as:

$$Y_{ijc} = \alpha + \gamma_c + \theta Takeup_{ijc} + \delta Prop.Takeup_{ic} + \nu_{ijc} \tag{2.5}$$

However, vaccination take-up and the proportion of peers who get vaccinated are potentially endogenous. For example, individuals with healthier lifestyles could be more likely to vaccinate

27. Figure A11 displays the number of employees by unit. The CDC and WHO indicate that vaccination rates over 75% grant herd immunity.

and less likely to need a sick day, so the estimates of equation (5) by OLS would be biased downwards. This speaks for instrumenting i) take-up with an indicator of assignment to vaccination during the workweek, and ii) the proportion of vaccinated peers in the unit with the proportion of peers assigned to the workweek. The unadjusted first stage equations have F-statistics of 6.6 and 8.9, respectively, implying that IV estimates of equation (5) may have a problem of finite sample bias.[28] Thus, we focus on the valid reduced form estimates of regressing the health outcomes on the instruments.

Table 2.6: Effects of Vaccination on Overall Sickness

	OLS	Reduced Form
Assigned to the workweek		-0.0166
		(0.0358)
Prop. peers assigned to the workweek		-0.00048
		(0.00110)
Vaccinated	-0.0068	
	(0.0324)	
Prop. peers vaccinated	0.00003	
	(0.00094)	
Average for unvaccinated in Quito (p.p.)	0.47	
N	1120	

* p<0.1,** p<0.05,***p<0.01
Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects on the probability of being diagnosed sick in general. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates.

Table 6 presents the effects of flu vaccination on the probability of being diagnosed sick for any reason between November 2017 and February 2018. The OLS estimate in Column 1 suggests that getting vaccinated decreased the probability of being diagnosed as sick by 0.7 percentage

28. The results of Montiel Olea and Pflueger (2013) only apply for the case of one endogenous variable.

points (1.4% of the baseline), although the effect is insignificant. The reduced form estimates in Column 2 imply that getting vaccinated did not affect the probability of being diagnosed sick. Being randomly assigned to the workweek – which increases vaccination take-up – decreased the probability of sickness by 1.5 percentage points (3.4% of the baseline), which is insignificant at conventional levels. Additionally, the results in columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed sick.

Table 2.7: Effects of Vaccination on Overall Sick Days

	OLS	Reduced Form
Assigned to the workweek		0.0123
		(0.0361)
Prop. peers assigned to the workweek		-0.00006
		(0.00101)
Vaccinated	-0.0407	
	(0.0298)	
Prop. peers vaccinated	0.00042	
	(0.00094)	
Average for unvaccinated in Quito (p.p.)	0.2808	
N	1120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effect on the probability of having a sick day. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates.

Table 7 shows the effects of flu vaccination on the probability of having a sick day. The OLS correlation suggests that vaccination decreased the probability of having a sick day by 4.1 percentage points, but this effect is not significant. Conversely, the reduced form estimates in Column 2 imply that getting vaccinated did not affect the probability of having a sick day. Being randomly assigned to the workweek, which increases vaccination take-up, increased the probability of having a sick day by 1.3 percentage points (5% of the baseline), which is insignificant at conventional levels. From an overall perspective of the firm, the results suggest that the investment in the health campaign was not worthwhile.[29]

Table 2.8: Effects of Vaccination on Flu Diagnoses

	OLS	Reduced Form
Assigned to the workweek		0.0045
		(0.0155)
Prop. peers assigned to the workweek		-0.0003
		(0.0006)
Vaccinated	-0.0254*	
	(0.0151)	
Prop. peers vaccinated	-0.0001	
	(0.0004)	
Average for unvaccinated in Quito (p.p.)	0.0457	
N	1120	

* p<0.1,** p<0.05,***p<0.01
Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects of flu vaccination on the probability of being diagnosed sick because of the flu. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates. The sample includes only units with two or more employees.

There are many diseases over which the flu vaccine has no immunity benefit. Hence, we exploit the data on medical diagnoses and estimate the effect of vaccination on the probability of being diagnosed with the flu (Table 8). The OLS estimates in Column 1 suggest that getting vaccinated decreases the probability of being diagnosed with the flu. However, the reduced form estimate in Column 2 indicates that being assigned to the workweek increased the probability of being

29. We reach the same conclusion based on findings for the number of sick days. Note that sick diagnoses include severe illnesses, such as cancer, which leads to large numbers of sick days not related to the flu. If we exclude outliers with more than 100 sick days, the coefficient of the reduced-form is insignificantly positive, in line with our finding in Table 7on the probability of having a sick day or not. Note also that our results for sickness and sick days do not change if we take out the proportion of peers and estimate only the individual effect of vaccination.

diagnosed with the flu by 0.4 percentage points (9% of the baseline), not significant at conventional levels. This result further suggests that getting vaccinated was ineffective in decreasing the probability of having the flu. Also, the estimates in columns 1 and 2 show that the proportion of vaccinated peers do not affect the probability of being diagnosed with the flu, which suggests that vaccination rates are too low to provide herd immunity. Thus, we drop the proportion of vaccinated peers in the following analyses.[30]

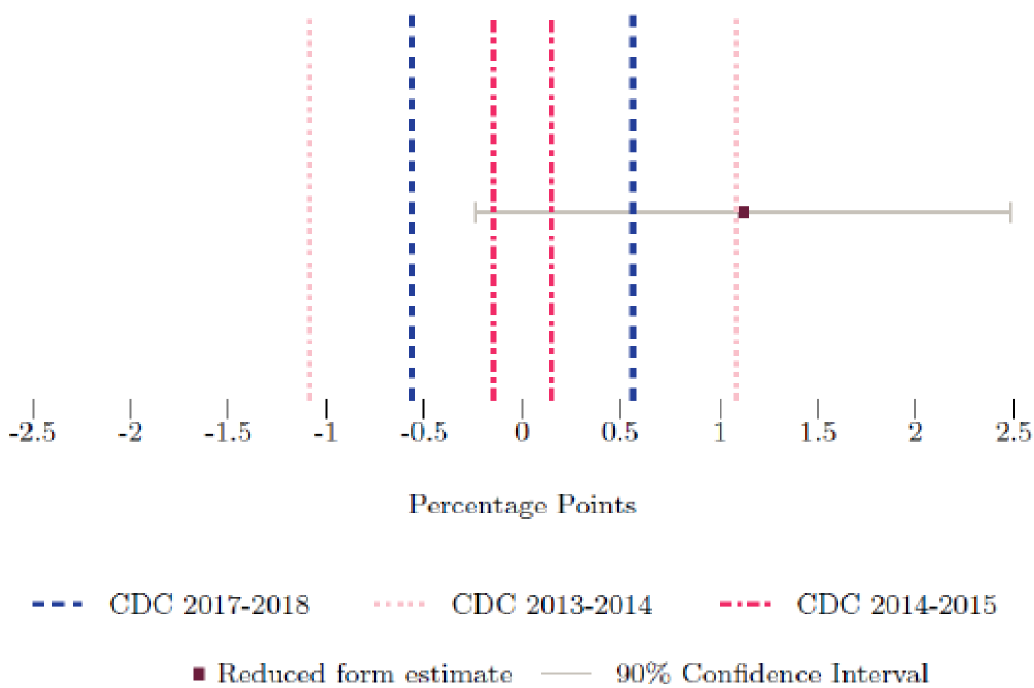
To evaluate if the estimates rule out meaningful effects of vaccination, we implement an equivalence test based on two one-sided hypothesis tests (King et al. 2000; Rainey 2014; Lakens 2017; Hartman and Hidalgo 2018). The equivalence test has two parts. First, we have to define what constitutes a meaningful effect of vaccination. This value defines two thresholds to evaluate if the estimates rule out meaningful effects. To define this value, we use flu vaccine effectiveness estimates from CDC data. While these estimates come from observational studies on flu hospitalizations and might be biased, they constitute the criteria policymakers use to evaluate the vaccine's effectiveness. Since our experimental design guarantees that getting vaccinated is the only channel through which assignment to the workweek in the campaign affects health outcomes, we can use reduced-form estimates to evaluate if vaccination has a meaningful effect on the probability of being granted a sick day because of the flu.[31] According to the CDC, the 2013-2014 vaccine had the highest effectiveness (it reduces hospitalizations by 16 percentage points), the 2014-2015 vaccine had the lowest effectiveness (it reduces hospitalizations by 2.2 percentage points), and the 2017-2018 vaccine's effectiveness during time of the campaign was in between those estimates (it reduces hospitalizations by 8.4 percentage points) for working adults. To compare these values with the reduced form estimates, we multiply the CDC effectiveness estimates by the smallest ef-

30. As can be seen in Appendix Table A5, the main result is robust to the inclusion of controls (gender, age, tenure and income) and to using a broader and narrower definition of flu-related illness. Also note that we check the main result by performing a bounding exercise (Appendix Table A6), in which we consider a possible role of vaccinations outside of the firm for our results (see Section 5.3 for details).

31. The CDC provides the percentage effectiveness of the vaccine. However, we require percentage point changes for the equivalence test. These percentage changes come from CDC cross-tabulations on the number of individuals vaccinated and not vaccinated and the number of individuals who got sick with the flu or not. The CDC further adjusts these estimates controlling for demographic characteristics that affect natural immunity to the flu resulting in larger estimates, so the reported percentages are a conservative lower bound of the CDC estimates.

fect of assignment to the workweek on take-up reported in Table 2, the most conservative estimate of the first stage (6.7 percentage points). This calculation yields reduced form reference values of -1.1 percentage points, -0.1 percentage points, and -0.6 percentage points, respectively.

Figure 2.2: Equivalence Test for the Effectiveness of Vaccination



Notes: This figure presents the reduced-form estimate of the effect of assignment to the workweek to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.

In a second step, we test if the reduced form effect is smaller than each reference value (-1.1, -0.1, -0.6) and higher than the absolute value of the reference values (1.1, 0.1, 0.6). This is equivalent to comparing both the reference values and their respective absolute values to the 90 percent confidence interval of the estimated effect (Rainey 2014; Lakens 2017). If the 90 percent confidence interval lies between the reference and its absolute value, then the estimated effects are consistent only with meaningless effects. If the confidence interval goes over one of these boundaries, it means we cannot rule out meaningful effects in the direction in which the confidence interval overlaps the boundary.

Figure 2 presents the comparisons. We can reject the CDC's effectiveness estimate for the best season (2013-2014) and the CDC's estimate for our campaign season (2017-2018). The estimated effect is consistent with the effectiveness of 2014-2015, the worst season for which there are data available. These results imply that we can safely rule out meaningful health benefits of the flu vaccination based on public health figures provided to policymakers from this intervention. However, the confidence interval in Figure 2 does not rule out potentially large positive values, which would suggest that getting vaccinated might increase illness. We study this potential issue in the next section.

2.5.2 Can Getting Vaccinated Cause Moral Hazard?

The previous results imply that vaccinating employees against the flu appears to be ineffective. A simple explanation could be that the 2017-2018 vaccine did not match the flu strains in that particular flu season. The flu vaccine grants protection against four strands of the flu virus. If the vaccine does not match the prevailing strands of the flu virus, then vaccination would be ineffective in improving health. Taking into account that the quality of the flu vaccine could vary by year and by country, the bank and its employees may have had just bad luck. While our setup does not allow us to test this, we can study if getting vaccinated induces individuals to engage in riskier practices, which may separately contribute to decreasing the effectiveness of flu vaccination and increase illness.

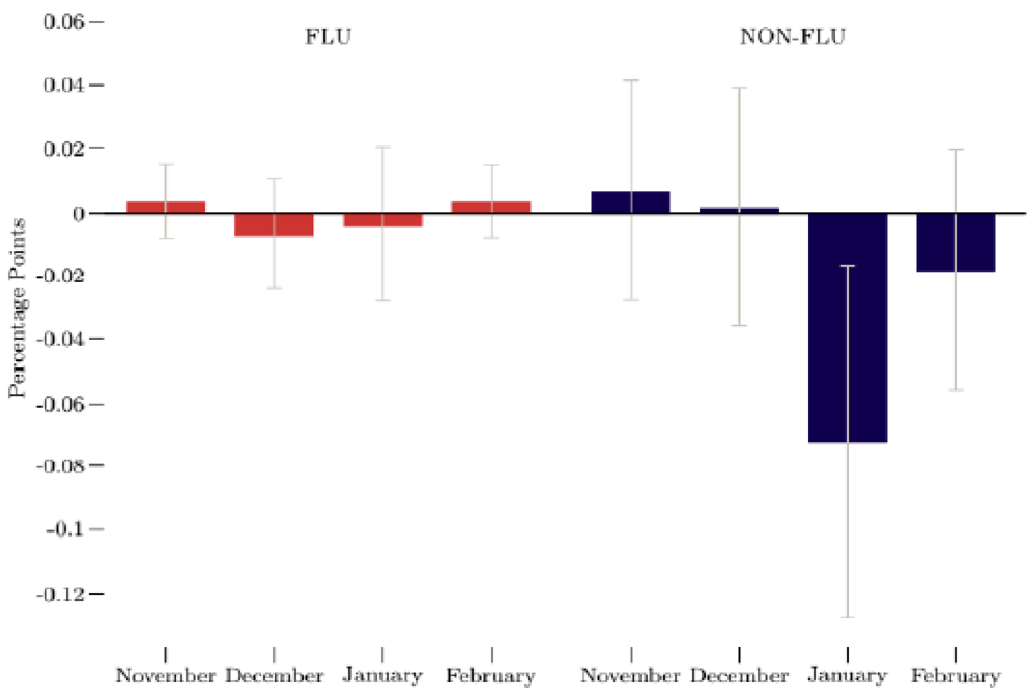
Vaccinated individuals could overestimate the protection of the vaccine and engage in riskier behaviors. As a consequence, vaccinated individuals may avoid going to the doctor or wait longer than the unvaccinated to do it when they feel flu-like symptoms. Also, vaccinated individuals could take fewer protective measures, such as washing hands less frequently. These changes in behavior would expose individuals more to strands of the flu virus that the vaccine does not cover and other diseases that share transmission mechanisms with the flu.

To explore if flu vaccination may cause moral hazard, we test if getting vaccinated induces different reactions than being unvaccinated when flu-like symptoms appear. The idea is that non-flu respiratory diseases have symptoms like the flu, but the vaccine does not provide any immunity benefit to prevent them. Thus, flu vaccination should not affect the probability of being diagnosed with a non-flu disease, so any effect on this probability would imply a change in how individuals react when being diagnosed sick with respiratory disease. In particular, if vaccinated employees felt more protected, they might have been less likely to go to the doctor when they felt flu-like symptoms, decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern cases of mild illnesses where it is up to the individual to decide to go to a doctor or not.

To implement this test, we exploit the richness of the data on medical diagnoses that allows us to identify cases of non-flu respiratory illnesses. We exploit a policy intervention of the Ecuadorian government that happened in our investigation period. In January 2018, Ecuador experienced a significant increment of flu cases nationwide (Epidemiologica 2018). As a result, the Ecuadorian government launched a massive media campaign asking the population to go to the doctor if they felt any flu symptoms. If vaccinated individuals felt protected, we argue that they may not have followed the government's recommendation, resulting in fewer visits to the doctor and fewer non-flu respiratory diagnoses in that month.

We estimate the reduced-form effects of vaccination by month during our investigation period. Figure 3 presents the effects of being assigned to the workweek on flu and non-flu respiratory diagnoses. As with the cross-section estimates in Table 8, assigning employees to the workweek does not affect the probability of being diagnosed with the flu in any month. The point estimates are smaller than 0.7 percentage points in magnitude and insignificant at conventional levels. These results further confirm that the vaccination campaign was ineffective. Regarding non-flu diagnoses, if vaccination did not induce individuals to feel more protected, we would expect to find no effect on the probability of being diagnosed with a non-flu respiratory disease. This is true in November, December, and February. However, in January, when the government asked individuals to go to the doctor, being assigned to the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 percentage points.

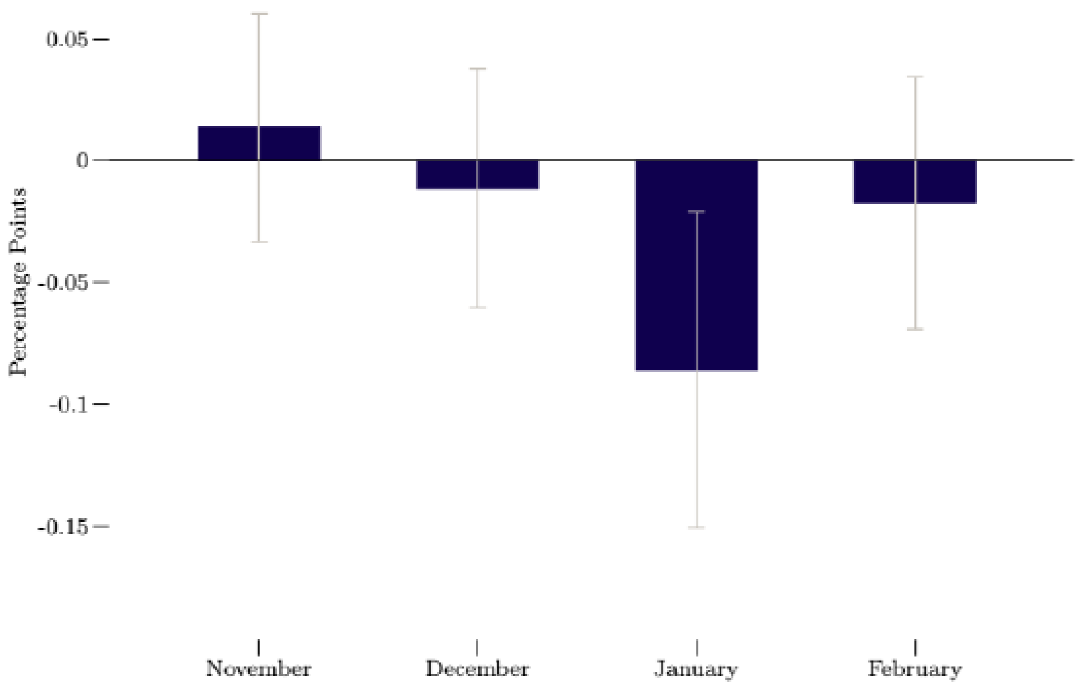
Figure 2.3: Reduced Form Estimates of the Effect of Vaccination on Diagnosed Sickness



Notes: This figure presents the reduced form effect of being assigned to the workweek on the probability of being diagnosed sick by month. The left panel presents the effect of assignment to vaccination on the workweek on flu diagnoses, and the right panel presents the effect of assignment to vaccination on the workweek on non-flu respiratory diagnoses. The figure presents the point estimate and the 9% heteroscedastic robust confidence interval. November includes cases of diagnosed sickness detected since November 12, after the vaccination campaign.

We also estimate the effect of assignment to the workweek on non-flu diagnoses collapsing the data of the four months to a cross-section. In this specification, being assigned to the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.7 percentage points (Appendix Table A5), almost identical to the effect in January. This result suggests that employees assigned to the workweek, who were more likely to get vaccinated, felt protected, and went less to the doctor when they felt flu-like symptoms. These estimates are consistent with the hypothesis of riskier behavior among vaccinated individuals, as they appeared to think that they are protected against the flu.

Figure 2.4: Reduced Form Estimates on the Probability of Going to the Onsite Doctor



Notes: This figure presents the reduced form effect of being assigned to the workweek on the probability of going to the onsite doctor. The figure presents the point estimate and the 95% heteroscedastic robust confidence interval. November includes sick days granted since November 12, after the vaccination campaign.

We can also investigate if vaccination affects the likelihood of going to the doctor at the on-site health center. The bank's on-site health center is a convenient feature for its employees because they do not have to ask for time off to go to the doctor as they can take a few minutes of their work time to go to the health center. Before the intervention, the on-site doctors account for 77 percent of all cases of diagnosed sickness. If vaccinated individuals felt more protected, they may have been less likely to visit these doctors when the government launched its media campaign. Figure 4 presents the effects of assigning employees to the workweek on the probability of going to the on-site doctor by month. There was no significant effect in November, December, and February. In January, being assigned to the workweek for vaccination decreased the probability of going to the onsite doctor by 8.6 percentage points (21% of the baseline). This finding is consistent with moral hazard.

In an additional test for moral hazard, we look at self-reported habits and cultural beliefs related to preventing the flu. In the post-intervention survey, the bank asked its employees how often they: (i) exercise, (ii) take nutritional supplements, (iii) use an umbrella when it rains, and (iv) wash their hands. Washing hands is a proven measure against the flu; exercising and taking nutritional supplements may improve overall health, and many people, including Ecuadorians, believe that carrying an umbrella helps to prevent the flu or other respiratory illnesses. Psychology research show that cultures across the world associate the fact that the flu virus survives longer on a cold and wet environment with the belief that individuals catch the flu by getting wet or cold (Au et al. 2008; Sigelman et al. 1993; Baer et al. 1999; Helman 1978).[32]

Table 9 shows the effects of assigning employees to the workweek on these outcomes. Assigning employees to the workweek did not affect how often employees wash their hands (1.2% of the baseline), which is not surprising since almost all employees report that they wash their hands regularly. Assigning employees to the workweek had a negative but statistically insignificant effect on how often employees exercise (4.9% of the baseline) and how often they take nutritional supplements (19.5% of the baseline). The effect on how often employees carry an umbrella is statistically significant. Being assigned to the workweek decreases the frequency of carrying an umbrella by 1.22 points (17.6% of the baseline) on a Likert scale where one means "never" and ten "all the time."[33] We can also investigate heterogeneous effects across individuals' beliefs on

32. Also, since Quito is on the Equator Line, there are no marked seasons in the year. In Quito, temperatures in a day can fluctuate between the upper forties (°F) and the lower eighties (°F). There are no accurate forecasts for rain.

33. This effect is significant at the 5% level (p-value=0.012) and robust to adjusting for multiple comparisons following Anderson (2008).

the effectiveness of the vaccine using the pre-intervention survey. We find that the effect is driven by individuals who believe the vaccine is very useful in preventing the flu (Appendix Table A7). Thus, this result suggests that vaccinated individuals feel protected, so they neglect other measures that they believe to be helpful in order to prevent respiratory illnesses.

Table 2.9: Reduced Form Estimates on Health-Related Habits

	Baseline	Coefficient	N
Responses on a scale from 1 (“never”) to 10 (“all the time”)			
How often do you exercise	5.93	-0.3145	358
		(0.4026)	
How often do you take dietary supplements	3.18	-0.6212	358
		(0.4376)	
How often do you carry an umbrella when it rains	6.85	-1.2070**	358
		(0.4861)	
How often do you wash your hands	9.25	0.1086	358
		(0.1835)	

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

Notes: Robust standard errors in parentheses. This table presents the reduced form effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. All specifications control for Quito fixed effects. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey question.

2.5.3 Other Interpretations of the Results on Moral Hazard

In the previous section, we provide several pieces of evidence supporting the idea that flu vaccination caused a moral hazard problem. In the following, we discuss whether other interpretations of these findings not related to moral hazard could explain the results. Misdiagnoses could be a competing explanation. If doctors are not able to distinguish the flu from other non-flu respiratory diseases, then some of the non-flu cases could have been flu cases. However, as we observe diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that doctors systematically misdiagnosed the flu. Also, the results are robust to using a broader definition of flu-related illness. Finally, misdiagnoses do not explain why vaccinated individuals report being less likely to carry an umbrella as a cultural protective measure against the flu.

We could also think that doctors misdiagnose conditional on whether a person got vaccinated or not. When a doctor learns that a person who shows flu-like symptoms got vaccinated, the doctor might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 3 show that employees assigned to the workweek, who are more likely to get vaccinated, were diagnosed less with non-flu respiratory diseases.

Another potential concern is the fact that the data on medical diagnoses correspond to employees who went to the onsite doctor or an external doctor during working hours, while employees who went to an external doctor outside working hours, who were diagnosed sick but were not granted a sick day, are coded as healthy. This measurement error will not bias the flu and non-flu estimates as long as it is uncorrelated with the assignment to the workweek. However, if employees assigned to get vaccinated during the workweek are more likely to go to an external doctor after work, then this would overestimate the effect on non-flu respiratory diagnoses. We bound the effect to address this potential concern (Lee 2009). First, we calculate the treatment-control difference in the proportion of healthy individuals. Then, we trim this difference from the control group (assigned to vaccination on Saturday) to obtain an upper bound, and we trim this difference from the treatment group (assigned to vaccination on the workweek) to obtain a lower bound. The effect of being assigned to the workweek on the probability of being diagnosed with a non-flu respiratory is always negative and bounded between 5.4 and 9.8 percentage points (Appendix Table A6).

Finally, an alternative to moral hazard is the idea of adverse selection: employees with higher risk tolerance regarding health are more likely to get vaccinated and to engage in risky health behavior. However, adverse selection cannot be a driver of our results because we use an exogenous source of variation on take-up. The marginal individual who gets vaccinated is a person who would not have gotten vaccinated if assigned to Saturday. This variation is uncorrelated with the underlying risk preferences or other traits of employees that could determine adverse selection.

2.6 Conclusions

Individual behavior may threaten the success of health interventions in multiple ways. First and foremost, individuals can decide not to participate. In this paper, we find that a small price change, as well as information nudges that appeal to the selfish or altruistic benefits of vaccination, does not induce a change in behavior. In contrast, reducing opportunity costs has a substantial effect on participation in a vaccination campaign for working-age employees. Additionally, peers are an important factor that influences vaccination in the workplace. Regarding the health benefits of the intervention, flu vaccination did not have a significant effect on any of our outcomes. While we cannot rule out that the flu vaccine was medically ineffective, we find evidence consistent with moral hazard, i.e., individuals adopting riskier behaviors after getting vaccinated. Moral hazard constitutes a second way through which individual behavior can limit the effectiveness of health interventions.

To answer the question of whether the vaccination campaign was economically beneficial for the company carrying out this health intervention, we can perform a back-of-the-envelope calculation of the net benefit of this campaign. This analysis has the limitation that we are not able to fully quantify all of the possible effects that vaccination may have on outcomes relevant to the bank, like morale and productivity.[34] Our calculation suggests that the net benefit of the campaign was negative regarding sick days. In the best-case scenario, the treatment may result in a net gain of $0.17 regarding gains in work attendance during the flu season, which is not enough to compensate the bank for its costs that include vaccine subsidies of $2.57, $5.05 and $9.99 per vaccine.[35]

34. A channel pertaining to company morale is the perception of individuals that the company cares more about their health when assigned to the work-week which leads them to behave differently. However, we cannot find evidence for that channel using data on organizational perceptions from our post-survey. Appendix Table A8 presents imprecise estimates on self-reported productivity and the duration of the workday measured by the employees' magnetic cards swipes to enter and exit the bank. Albeit statistically insignificant, the point estimates suggest that assigning employees to get vaccinated in the workweek increased their perception on their productivity, while decreased the duration of their workday by about a third of an hour. Given that the bank pays a fixed salary, this could suggest an increase in productivity. However, in the absence of more precise measures of productivity, we cautiously conclude from this analysis that there is no sizable productivity premium. One could argue that from the perspective of a company, sick days have higher economic relevance, given that they often go along with re-assignment of tasks, compared to when some employees are able to finish tasks and leave earlier than others.

35. The estimate's confidence interval implies that at most assigning employees to the workweek could decrease the likelihood of having a flu sick day by 0.5 percentage points. We take the median wage of the bank ($750), divide it by

Our study allows us to draw multiple practical implications for health interventions. From a research perspective, it is useful to employ a randomized encouragement design to circumvent any ethical dilemma when studying the consequences of interventions relevant to people's health. It allows not only the study of potential health benefits and but also of behavioral changes in an unbiased way. The presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in the design of interventions like vaccination campaigns. A promising mechanism to mitigate it could be to increase awareness that the proposed measure, such as flu vaccination, does not guarantee a 100% protection against illnesses. It might be necessary to remind individuals to continue making use of other protective measures against respiratory viruses and bacteria, instead of letting them rely only on the protection potentially provided by medical technology.

Another lesson learned from our investigation is how to raise participation in health interventions. In this paper, we could find two cost-effective measures that increase vaccination take-up in a workplace context where monetary aspects do not seem to play a significant role in the individual willingness to participate in a health campaign. Decreasing opportunity costs is one option to increase participation drastically, which suggests using mobile campaigns in days and locations where people usually congregate. Also, since we find that peer behavior has an important effect on vaccination take-up, and that following social norm could be the potential mechanism, employers can increase participation in health campaigns by using mechanisms to incentivize groups of employees. Small rewards for the entire unit when the unit takes part could have significant effects on participation rates. Evaluating the role of such peer incentives in health-related contexts is a promising area for future research.

the average number of workdays in a month (22), and we multiply this value by 0.005.

3. THE UNINTENDED CONSEQUENCES OF HEALTH INSURANCE

3.1 Introduction

Universal health care allows every citizen to have equal access to health care that is not financially punitive. It is generally seen as beneficial to society: universal health care is associated with lower infant mortality, longer life-expectancy as well as positive labor and income effects (Moreno-Serra and Smith 2012; Dizioli and Pinheiro 2016; Finkelstein et al. 2012). However, more than fifty percent of the world's population does not live in a universal health care system. The World Health Organization has declared health a basic human right and stated its mission to achieve universal health coverage world-wide by 2030 (Prince 2017). Institutions like the Organisation for Economic Co-operation and Development and the United Nations support such a mission. While support of universal health care has increased over time (Pew 2017), what might comprise the most efficient and equitable system requires a better understanding of the underlying economic considerations such as the cost-benefit trade-off.

Under universal health care, supplementary private insurance often provides added individual benefits, such as faster and personalized health services through additional purchases of benefits that include reduced wait times, the choice of doctors, and private hospital rooms. It also provides spill-over social benefits by reducing demand and cost on the public health sector. While universal health care systems in the developed world are correlated with reduced access cost and lower overall health costs, they are associated with longer wait-times, which generally leads to systems that offer both public insurance as well as private insurance due to the tradeoff between equity and efficiency of health care provision.[1] Currently, the exact welfare implications of universal health care with supplementary private insurance remain obscure. While the benefits of being privately insured over being uninsured have been studied extensively (see e.g. McWilliams et al. 2004; Wilper et al. 2009), there is a gap in knowledge for systems implementing public and private and

1. See Figure A1, A2, A3.

insurance simultaneously. Existing research does not address whether the existence and coverage of private health insurance under universal health care increases individual and societal welfare. There also remains scant overall evidence on the long-run consequences of private health insurance coverage. This study initiates to fill in these gaps.

A common problem with studies of insurance is adverse or advantageous selection, i.e. individuals with specific characteristics select into insurance. We solve this problem by employing data from the health care system of Australia. By international standards, Australia represents a low-cost system marked by good health outcomes (Schneider et al. 2017). Administrative tax data spanning multiple years across a large ten-percent panel sample of individuals in Australia allows us to identify the potential benefits and hidden costs arising from private health insurance under universal health care using a policy that creates an unconventional natural experiment.

The Australian policy that introduces price-discrimination by age called the Lifetime Health Cover (LTHC) is exploited for this study. It rewards (punishes) early (delayed) purchase of private health insurance. A regression kink design (Card et al. 2017) is utilized to investigate demand responses of private health insurance where high-quality public insurance is the baseline. With this large administrative sample of observations, precise estimates can be obtained and heterogeneity and implications for policies are explored. The empirical effect of private health insurance on health expenditures, mortality, work expenses, gross salary, and student debt is evaluated via a reduced-form regression kink approach to obtain local average treatment effects at the policy threshold.

This study contributes to numerous strains of literature. First, the studies which investigated three major Australian policies incentivizing private health insurance coverage either used highly aggregated time-series (Butler 2002; Frech et al. 2003; Ellis and Savage 2008) or cross-sectional data (Palangkaraya and Yong 2005, 2007; Stavrunova and Yerokhin 2014). With a few exceptions (Kettlewell 2018), these studies were based on self-reporting from surveys (Cameron et al. 1988, Buchmueller et al. 2013; Cheng 2014), which have potential reporting problems due to social desirability and memory issues. Those problems can be reduced with tax data due to government sanctions from misreporting. While previous investigations mainly focus on explaining one historic jump in private health insurance coverage around the millennium, the coverage is investigated more broadly in the current study. Since the data spans more than a decade, an examination of responses is possible for the same individual directly after an event of interest, thus allowing for a direct look at the effectiveness of the policies in the short- and long-run.

Second, impact evaluation is at the core of health economics and has been done in domains such as incentivized exercises (Charness and Gneezy 2009), wearables (Handel and Kolstad 2017), and TV consumption (Chadi and Hoffmann 2018). Yet, impact studies of private health insurance under universal health care have thus far been neglected. The current study provides the first large-scale investigation of unbiased private health insurance estimates on mortality and income while also assessing long-run moral hazard via health expenditures. (Hall et al. 2005) provides one of the few studies on mortality in a universal health care system, finding that private health insurance is negatively associated with three-year mortality for prostate cancer patients from Western Australia. The robustness of this finding is unclear since both, sorting based on low risk health characteristics (advantageous selection) and high risk health characteristics (adverse selection) was reported in Australia (Buchmueller et al. 2013; Kettlewell 2018), and the direction of the true bias is unknown. The present study can account for sorting and provide unbiased estimates by exploiting the exogenous policy variation.

While adverse selection can be observed here, no evidence for moral hazard based on the administrative tax health expenditure measure is found. No effect of private health insurance on mortality is detected either, likely due to the relatively young age of individuals in the study sample. This study contributes to the scarce literature of health insurance coverage on financial obligations such as debt and work expenditures as well. We investigate whether there are any potential unintended consequences when obtaining health insurance. Surprisingly, gross salary is reduced for individuals that are more likely to be covered privately. One likely partial mechanism is ex post income manipulation of the reported gross income since expenses are higher for the insured. Also, paying the premiums could change the repayment rates of student debt and reduce other expenditures. No evidence of a reduction in work-expenditures reported to the Australian Taxation Office in detected, yet an increase in student debt for those covered by private health insurance is apparent, indicating delay of repayments.

This paper is organized as follows. In the next section, the background of Australian health policies as well as the specific policy used for identification is introduced. Section 2 briefly discusses the underlying theoretical framework. Section 3 describes the data and Section 4 presents the empirical strategy. Section 5 tests the identification assumption. Section 6 presents the impact of the exogenous penalty policy on private health insurance coverage in Australia. Section 7 discusses adverse and advantageous selection and shows the results for the long-run consequences of private health insurance. Section 8 concludes.

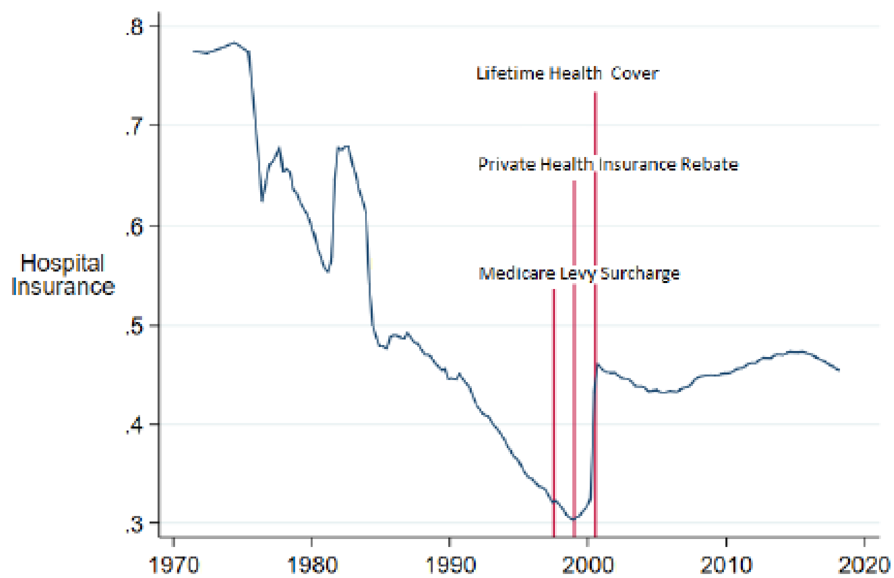
3.2 Background

Universal health care was first introduced in Australia in 1975 and has existed in its current form since 1984. While private health insurance (PHI) did not disappear, it experienced a decline until approximately 1999. Figure 1 illustrates the drop in PHI through the proxy called hospital cover which covers insurance benefits for stays in the hospital.[2] The reduction in coverage raised the concern that healthy low-cost individuals would drop out over time while unhealthy high cost individuals would remain in the insurance market (known as adverse selection death spiral; Cutler and Zeckhauser 1998; Einav and Finkelstein 2011) making insurance increasingly unprofitable and leading to the collapse of the private health insurance system. Subsequently, Australia established three tax-related health policies to increase insurance coverage: the Medicare Levy Surcharge, the Private Health Insurance Rebate and the Lifetime Health Cover loading.[3] After the introduction of the three policies at the turn of the millennium, coverage indeed stabilized.

2. Another part of insurance is called extras cover which covers services such as dental, physiotherapy or chiropractic treatment which are commonly not covered by the hospital cover.

3. A question raised in the Australian private health insurance literature is by how much private health insurance take-up was increased by each individual policy as well as an advertisement campaign by the government promoting private health insurance take-up at the time. Due to the relatively close introduction of the policies, it is difficult to disentangle the effects. This paper will not try to answer this question. It will specifically focus on the Lifetime Health cover that introduces mandatory discrimination by age to evaluate demand responses and the consequences of private health insurance when public insurance is the baseline.

Figure 3.1: Australian Private Health Insurance Coverage



Notes: This figure displays Australian private health insurance (hospital insurance) coverage over time. The percentage of individuals which are privately insured drastically decreased until the change of the millennium when the Australian government introduced three private health insurance policies, among others the Lifetime Health Cover.

The Medicare Levy Surcharge was an additional 1% tax on taxable income on affluent singles (families) with taxable income above $50,000 ($100,000) who failed to purchase PHI. Both, the income threshold as well as the tax penalty increased over time. The Private Health Insurance Rebate initially covered 30% of the private health insurance premiums. Age-related thresholds were introduced over time, yet benefits were also reduced to phase out this costly policy. Neither of these two policies interfere with the third policy of interest: the Lifetime Health Cover.

With the introduction of the Lifetime Health Cover (LTHC) on 1 July 2000, individuals who delay initial sign-up for PHI are penalized with a higher premium than those that purchased PHI while younger. The policy is completely independent of income and applies to every Australian.

Figure 3.2: Lifetime Health Cover – Policy Schedule

Notes: The exogenous policy leads to a price increase of a certain percentage (called Loading) on top of the premium costs based on the age of first sign-up of private health insurance purchase and a growth-rate difference from the left to the right hand side of the threshold of 2 percentage points.

The introduction of the LTHC policy did not result in any increase in premiums for those individuals below the age of 31. Figure 2 illustrates that premiums increase by 2% with each additional year after age 30 for which an agent decided to delay sign-up. An individual purchasing private health insurance for the first time at age 40 has to pay 20% more than what he would have paid, had he signed up at age 30. This price surge based on initial purchase – called a loading – increases up to 70% for individuals signing up for the first time at age 65. At the time of the policy introduction, the loading was locked in at an infinite horizon. The law was changed on April 2007, requiring the loading to be dropped for agents that were continuously covered through private health insurance over ten years.[4] Until 2007, the National Health Act from

4. The Lifetime Health cover policy is also known as an unfunded lifetime community rating. Gale and Brown (2003) claim that the policy is actuarial fair, i.e. the premium is equal to the expected claims.

1953 prohibited premium discrimination based on age, health-status, or other circumstances which effectively establishes a community rating for private health insurance. Crucially, the Lifetime Health Cover (LTHC) was the exception to the community rating rule which requires uniform pricing within the same territory. Those rules changed with the introduction of the Private Health Insurance Act in 2007 which specifically prohibits discrimination by age (except for the LTHC), health status, gender, sexuality, race, religious beliefs, and claims history. While price variation by state of residence was explicitly allowed, it is difficult for companies to price discriminate by state to offset the mandatory price penalty.[5]

3.3 Theoretical Framework

The Lifetime Health Cover policy introduces price discrimination by age. It changes the price of private health insurance each year that purchase is delayed past the age of thirty, leading to an increase in private health insurance premiums when initial sign-up happens later rather than earlier in life. Given that the policy asks health funds to price-discriminate by age but price discrimination by age is not feasible in any other way, it seems impossible for firms to respond to this price change to offset the law optimally to attract higher revenue. Therefore, the theoretical framework and analysis focuses on the consumer side.

Imagine an agent i that maximizes utility with respect to health consumption x_s and non-health, residual consumption y_s over a finite time horizon until $s = t, t+1, ..., T$ until death (T) subject to the available budget w_s.

$$\max_{x,y} U^s(u_s, u_{s+1}, ..., , u_T) = \sum_{s=t}^{T} \delta^s u_s(x_s, y_s) \tag{3.1}$$

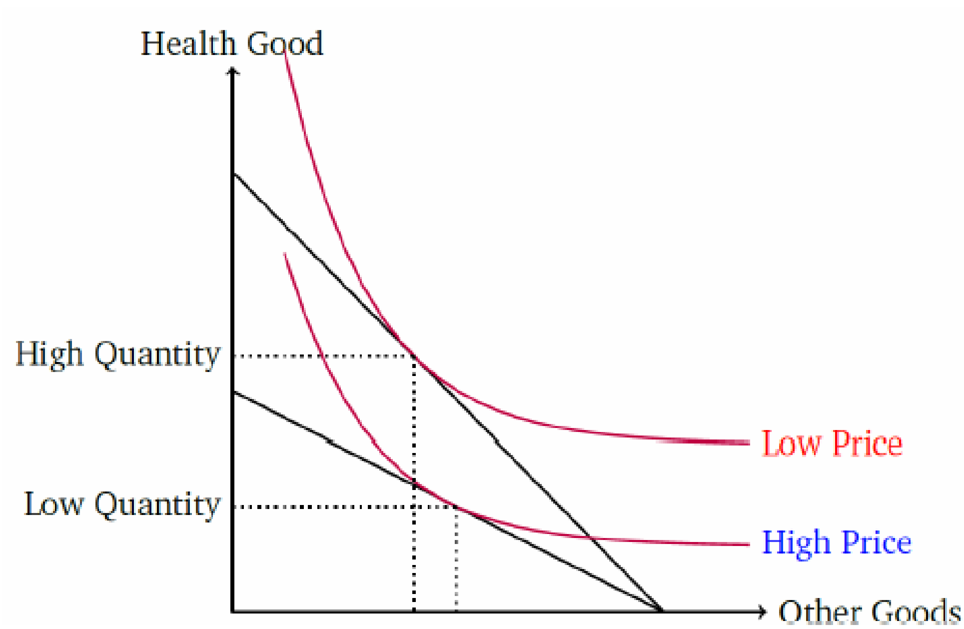
s.t.

$$\forall s : p_x x_s + p_y y_s = w_s \tag{3.2}$$

5. In a binary way, firms could reduce premiums minimally by 2% for states with individuals that are on average above 30 while they could increase prices by 2% in states where individuals are on average below 30. This would not off-set the whole mandatory price schedule but just a tiny fraction. Increasing the difference beyond 2% will introduce adverse selection given that old individuals have higher health risks. It further seems infeasible since no state has tax-payers that are on average below 30 over our period of interest. The overall average age is 42.

In this simple model, the premium change represents an impact on the price of health services p_s, where insurance is a subset of health services. The Lifetime Health Cover changes the price for health services from the vector $p_s = p_t, p_{t+1}, ..., p_T$ to $p_s^{'} = p_t^{'}, p_{t+1}^{'}, ..., p_T^{'}$ with an exogenous price increase for individuals above the age of 30, s.t. $p_s < p_s^{'}$.

Figure 3.3: Lifetime Health Cover – Price Change



Notes: The exogenous Lifetime Health Cover policy leads to a price change on health for agents above the age of 31 and reduces health demand and increases the demand for residual consumption (i.e. the other goods).

Figure 3 displays that change in price for a 35-year-old person who suddenly must pay a higher price for insurance given that the agent is relatively myopic. It implies that the agent did not sign up for cover before the introduction of the policy but is only considering to sign-up after the introduction of the policy which leaves the agent to face the price change at that point in time. The price increase leads to a reduction in demand in the health good x_s and an increase in demand in the residual good y_s. Given this framework where health is an investment good, demand for the health good is expected to be downward sloping.

3.4 Data

A novel data set called A-Life was provided by the Australian Taxation Office. It consists of a panel of ten percent of Australian taxpayers from 1999 to 2012 and is utilized for this study.

Table 3.1: Descriptive Statistics

	1999-2012			2000			2000, H=6		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Running Variable									
Age	42	16	17,246,604	41	16	1,095,225	30	2.8	243,298
Characteristics									
$\mathbb{1}$(Male)	0.52	0.50	17,246,604	0.53	0.50	1,095,225	0.54	0.50	243,298
$\mathbb{1}$(Children)	0.94	0.24	17,246,604	0.98	0.14	1,095,225	0.98	0.14	243,298
$\mathbb{1}$(Partner)	0.99	0.11	17,246,604	0.99	0.10	1,095,225	0.98	0.13	243,298
$\mathbb{1}$(Self-Employed)	0.09	0.29	17,246,604	0.08	0.27	1,095,225	0.09	0.28	243,298
$\mathbb{1}$(Resident)	0.99	0.08	17,246,604	0.99	0.08	1,095,225	0.99	0.08	243,298
Remoteness Index	0.42	0.75	17,083,568	0.43	0.76	1,088,860	0.39	0.76	241,927
$\mathbb{1}$(Occupation 1)	0.00	0.04	13,047,808	0.00	0.00	810,380	0.00	0.00	195,785
$\mathbb{1}$(Occupation 2)	0.11	0.31	13,047,808	0.11	0.31	810,380	0.11	0.31	195,785
$\mathbb{1}$(Occupation 3)	0.19	0.39	13,047,808	0.14	0.35	810,380	0.17	0.38	195,785
$\mathbb{1}$(Occupation 4)	0.09	0.29	13,047,808	0.06	0.24	810,380	0.07	0.25	195,785
$\mathbb{1}$(Occupation 5)	0.10	0.30	13,047,808	0.10	0.31	810,380	0.12	0.33	195,785
$\mathbb{1}$(Occupation 6)	0.10	0.30	13,047,808	0.13	0.34	810,380	0.14	0.34	195,785
$\mathbb{1}$(Occupation 7)	0.13	0.34	13,047,808	0.13	0.34	810,380	0.14	0.35	195,785
$\mathbb{1}$(Occupation 8)	0.06	0.24	13,047,808	0.04	0.21	810,380	0.05	0.21	195,785
$\mathbb{1}$(Occupation 9)	0.11	0.31	13,047,808	0.13	0.33	810,380	0.13	0.34	195,785
$\mathbb{1}$(Occupation 10)									
Outcomes									
$\mathbb{1}$(PHI)	0.44	0.50	17,246,604	0.41	0.49	1,095,225	0.33	0.47	243,298
Medical Expenses	30	281	17,246,604	16	188	1,095,225	7.9	125	243,298
$\mathbb{1}$(Mortality)	0.00	0.05	17,246,604	0.00	0.05	1,095,225	0.00	0.02	243,298
Gross Salary	32,651	45,373	17,246,604	25,031	30,162	1,095,225	30,440	26,454	243,298
Work Expenses	1,183	3,008	17,246,604	850	2,232	1,095,225	1,130	2,458	243,298
Student Debt Due	80	551	17,246,604	55	340	1,095,225	132	539	243,298

Notes: H is the bandwidth of age. The remoteness index has values from 0 to 4.

These data will be used to estimate the causal effects of private health insurance within the universal health care system of Australia. The country has a population of 24.21 million citizens (World Bank 2018), 1.8 million of which are available in the panel, which implies a tax base of 18 million individuals. I restrict the set of included individuals to those between 18 and 60 years of age to avoid any potential confounding effect of retirement. The main sample includes 243,298 unique taxpayers which are followed throughout a decade to investigate the effect of the policy on the following outcomes: medical expenses, mortality, gross salary, work expenses, and outstanding student debt.

Table 1 shows descriptive statistics of the tax data. The average Australian taxpayer is 41 years old, has a dependent child, a partner, is an Australian resident, and lives in a densely populated area. The average gross salary over the full period of interest is $32,651 with 44% of individuals having private health insurance as measured by hospital cover. The main sample includes individuals above and below the age of 30 with a bandwidth of H=6. The average age of the main sample is 30 by construction with otherwise relatively similar characteristics to the full data.

3.5 Empirical Strategy

Ideally, we would like to randomly assign agents to have private health insurance vs. not to obtain the average treatment effect on health, labor, and budget outcomes, which would be identified via the following clean ordinary least square regression for agent i at time t:

$$Y_{it} = \beta_0 + \beta_1 PHI_{it} + \epsilon_{it} \tag{3.3}$$

Such an approach, however, is ethically and practically infeasible. Alternatively, imagine a world where one randomly assigns different prices of private health insurance contracts in a field experiment to the general population or a subset thereof. This would allow us to use an instrumental variable approach where individual compliance is expected to decrease with increasing exogenous premiums. By instrumenting private health insurance coverage through the price, one would obtain the effect of insurance on the outcomes for the set of compliers. This can be achieved by scaling up the reduced form (5) by the first stage (4), as shown in the two equations below.

$$PHI_{it} = \gamma_0 + \gamma_1 price_{it} + \nu_{it} \tag{3.4}$$

$$Y_{it} = \delta_0 + \delta_1 price_{it} + \zeta_{it} \tag{3.5}$$

Closely related, the instrument employed in this setting is using a kink in the policy and with it the regression kink design. While random assignment is not used in its purest form, the quasi-random assignment of individuals to the policy schedule is sufficient to identify effects from differences in slopes generated through the premium increase at the threshold. The following first stage and reduced form equations are used to identify the local causal estimate from the policy kink at the age of thirty.

$$PHI_{it} = \alpha_0 + \sum_{k=1}^{2} \alpha_k age_{i2000}^k + \alpha_3 age_{i2000} \times \mathbb{1}(age_{i2000} \leq 30) + \nu_{it} \tag{3.6}$$

$$Y_{it} = \rho_0 + \sum_{k=1}^{2} \rho_k age_{i2000}^k + \rho_3 age_{i2000} \times \mathbb{1}(age_{i2000} \leq 30) + \zeta_{it} \tag{3.7}$$

The regressions are shown in a bandwidth $|age_{i2000}|| < H$ with the $H = 6$ as the main bandwidth. Robustness of the results is tested with respect to the bandwidth. The reduced form (6) can be scaled up by the first stage to obtain the regression kink estimator:

$$\tau_{SS} = \frac{\lim_{\nu \downarrow 30} \frac{E(Y_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}} - \lim_{\nu \uparrow 30} \frac{E(Y_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}}}{\lim_{\nu \downarrow 30} \frac{E(PHI_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}} - \lim_{\nu \uparrow 30} \frac{E(PHI_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}}} \tag{3.8}$$

The most powerful first stage is a version of percentage changes using $log(PHI_{it})$ and $log(Price_{it})$. However, such a first stage can only be used when aggregating by age which results in a substantial loss of power since age is only available at a yearly level. Therefore, intent-to-treat (reduced form) estimates for the impact of private health insurance are shown instead. To demonstrate that the first stage is reasonable and that intent-to-treat effects are based on the mechanism of the loading policy, the identification is employed without the logarithm for a conservative estimate. The Lifetime

Health cover policy can be translated into the following policy function $B = b(age)$ such that:

$$\frac{\partial b(age)}{\partial age} = \begin{cases} 0 & \text{if age} \leq 30 \\ 0.02 & \text{if age} > 30 \end{cases} \tag{3.9}$$

Intent to treat percentage premium increases are employed based on the policy-schedule, i.e. the loading of the Lifetime Health Cover and we can observe the change in the growth rate of insurance coverage due to a change in the growth rate of the price.

$$\tau_{SS} = \frac{\lim_{\nu \downarrow 30} \frac{E(PHI_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}} - \lim_{\nu \uparrow 30} \frac{E(PHI_{it}|R=age_{i2000})}{\mathrm{d}age_{i2000}}}{\lim_{\nu \downarrow 30} \frac{\mathrm{d}b(age)}{\mathrm{d}age_{i2000}} - \lim_{\nu \uparrow 30} \frac{\mathrm{d}b(age)}{\mathrm{d}age_{i2000}}} \tag{3.10}$$

Since the loading increases by 2 pp. every year past age 30, the most conservative first stage slope change is 0.02 and can be fixed by construction.

3.6 Testing the Identification Assumption

For a regression kink design to provide unbiased estimates, it is necessary that only the growth-rate of the outcome changes due to the policy, i.e. there is no other factor that induces the change in the growth-rate. I test this identifying assumption in three ways: I check for manipulation of the running variable, smoothness of covariates and I run placebo-tests.

3.6.1 Manipulation of the Running Variable

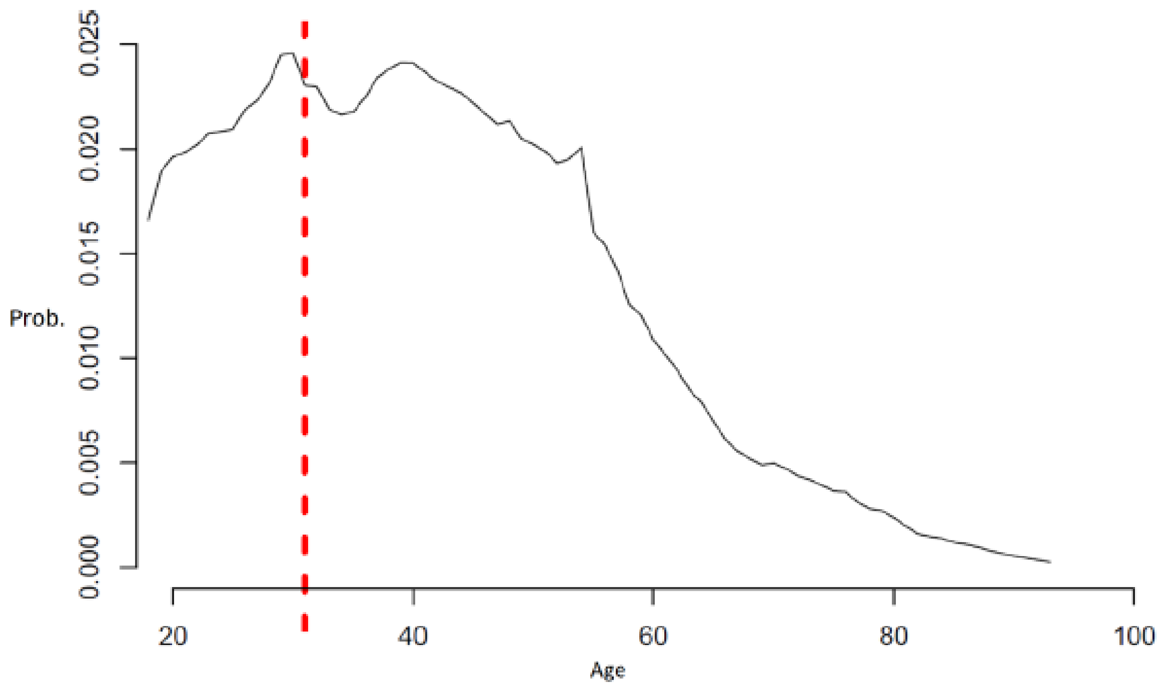
A necessary first check regards any possible manipulation of the reported running variable age. Based on the policy incentives one might suspect that agents could systematically report being below the age of 31 to receive no price punishment. However, such manipulation is extremely difficult since age must be reported the first time a person enters the taxpayer database and for every tax filing thereafter to a powerful government agency, Australian Taxation Office (ATO).

The ATO can and does cross validate information per individual over time. It is further possible for the ATO to cross-check the stated age with official birth certificates. In recent years, the ATO alleviated the tax-filling procedure by receiving information directly from the employers to pre-fill tax forms for submission which are another source to cross-check to ensure consistency in tax filing statements. It implies that any misreporting would have to be done in at least two official registries and with the employer. Beyond those large hurdles, the ATO is able to punish individuals not only in monetary terms but also with criminal charges when individuals do not comply with the agency.[6]

Figure 4 shows the probability density distribution over age. It is relatively smooth across the cut-off. However, there is a minor mass peak right before the kink. The related McCray test shows a significant relationship at the five percent level. Now, if one would not know the institutional setting, this peak might be worrisome. However, the peak can be traced back to an increase in birth rates in the 1970s (Figure A4). Unless the parents of those individuals systematically decided to give birth more often while forecasting a policy that is implemented thirty years later and was only debated in 1997, manipulation seems impossible. One might still worry that pure statistical association could confound the estimates. Yet, it can be demonstrated that the coefficient of private health insurance coverage obtained is not an outlier in the distribution of estimates, but rather the mean of the distribution. This was accomplished through 10,000 jackknife Monte Carlo simulation cycles wherein 1% of the population at the left hand-side of the cutoff was randomly dropped at each iteration (Figure A5). Therefore, neither manipulation is detected nor do any potential associations of the unlucky birth-peak coinciding with the policy influence the estimates.

6. The Australian Taxation Office has three types of penalties: i) failure to lodge penalty up to $10,502, ii) After years a default assessment based on income is conducted and iii) a maximum penalty $8,500 or imprisonment for up to 12months is the most extreme type.

Figure 3.4: Probability Density Distribution of Private Health Insurance over Age



Notes: The figure shows the likelihood of having private health insurance coverage as a probability density function for each age group in the year 2000, i.e. at the time of the introduction of the policy.

3.6.2 Smoothness of Covariates

Table 2 shows the effect of the slope-change on the covariates. Almost all variables are statistically insignificant which means that the covariates cannot explain the underlying treatment. It is reasonable to conclude that private health insurance demand only changes through the policy; therefore, any intent-to-treat effects of insurance on health, labor, and budget outcomes can only be induced due to the change in the growth-rate of the price from the left to the right hand side at the age of 30 in the year 2000.

Table 3.2: Smoothness of Covariates

	Coefficient	Standard Error	p-value	Mean at Kink	Standard Deviation
Characteristics					
$\mathbb{1}$(Male)	0.01	0.01	0.37	0.54	0.50
$\mathbb{1}$(Children)	0.00	0.00	0.36	0.98	0.14
$\mathbb{1}$(Partner)	-0.00	0.00	0.64	0.98	0.13
$\mathbb{1}$(Self-Employed)	-0.00	0.00	0.21	0.09	0.28
$\mathbb{1}$(Resident)	0.00	0.00	0.85	0.99	0.08
Remoteness Index	0.01	0.01	0.38	0.39	0.76
$\mathbb{1}$(Occupation 1)	0.00	0.00	1.00	0.00	0.00
$\mathbb{1}$(Occupation 2)	0.00	0.00	0.76	0.11	0.31
$\mathbb{1}$(Occupation 3)	0.01	0.01	0.02	0.17	0.38
$\mathbb{1}$(Occupation 4)	0.00	0.00	0.30	0.07	0.25
$\mathbb{1}$(Occupation 5)	0.00	0.00	0.57	0.12	0.33
$\mathbb{1}$(Occupation 6)	-0.01	0.00	0.02	0.14	0.34
$\mathbb{1}$(Occupation 7)	0.00	0.01	0.84	0.14	0.35
$\mathbb{1}$(Occupation 8)	0.00	0.00	0.84	0.05	0.21
$\mathbb{1}$(Occupation 9)	0.00	0.00	0.84	0.13	0.34
$\mathbb{1}$(Occupation 10)	0.00	0.00	0.68	0.08	0.27

Notes: The bandwidth H=6. The remoteness index has values from 0 to 4.

3.6.3 Placebo-Tests across Age and Time

Finally, we carry out two placebo checks to make sure that the assignment of the kink happens at the correct age and time. Placebo-kinks are placed at different values of the age distribution with subsequent checks on whether the regression kink design retains predictive power to change private health insurance demand at these kinks. The adjusted R2 values for the age-based placebo-test is very high at the policy-kink and lower at the placebo kinks (not shown). It means the predictive power of the kink is large and we conclude the age of 30 is the correct kink.

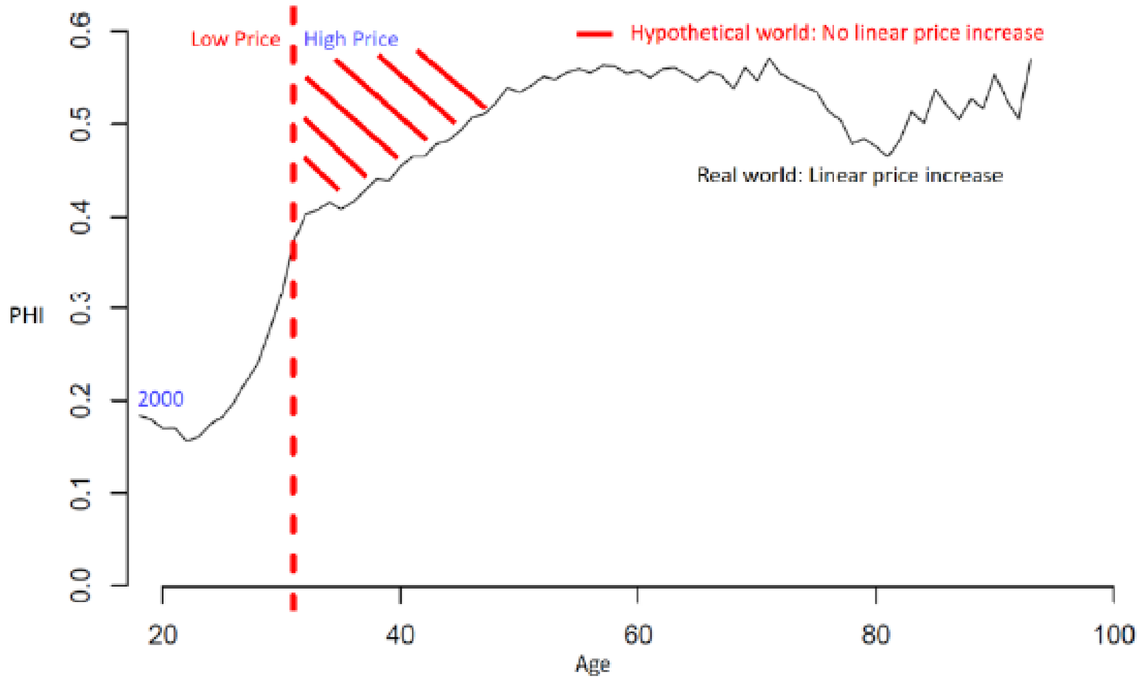
The time-period prior to the introduction of the LHC policy was examined for any potential issues since the policy was initially publicly discussed in 1997. Gale and Brown (2003) have previously implied that responses to price increase may have started one month prior to policy implementation. Due to data limitation, no year prior to 1999 is available. However, there is no significant change in the slope during the year 1999. The main demand responses occurred during the year 2000 and is driven – as expected – by new purchases. No kink is observed in the demand of private health insurance for individuals that already had coverage in 1999; however, a kink is apparent at the year of policy implementation for those that were not covered in 1999 and incentivized by the policy (not shown).

3.7 Private Health Insurance Coverage

Given the change in the growth-rate of the premiums for initial purchase (see Figure 2), a change in the growth-rate of private health insurance coverage is expected, i.e. the quantity demand.

Figure 5 shows the private health insurance coverage propensities over the distribution of age. Below the age of 30, the growth rate of private health insurance coverage is relatively steep and it drops after the kink point. This is consistent with a reduction in the quantity demanded due to an increase in the price of health goods, i.e. in particular private health insurance. This change in slope is used as a conservative first stage to estimate the impact of private health insurance. To estimate the change in coverage, the loadings assigned to each age-cohort are utilized to obtain a first stage estimate of 0.02 with a very high F-value of 713,247 for the main bandwidth ($H = 6$).

Figure 3.5: Private Health Insurance Coverage at Time of the Policy Introduction



Notes: The figure shows mean private health insurance coverage for each age group in the year 2000, i.e. at the time of the introduction of the policy. In a hypothetical scenario without the linear price increase schedule, private health insurance growth would be higher than in the current scenario where private health insurance is suppressed by schedule that yearly increases the price of private health insurance. The growth rate difference of the left and right hand side is between 2 and 4 pp.

Table 3 displays the reduced form and second stage estimates. All reduced form estimates have F-values beyond 10 which means that there is a strong first stage. However, a different first stage could theoretically be obtained using the logarithm of insurance coverage. Since this is not possible without aggregation and loss of power, only the intent-to-treat estimates for the later consequences of private health insurance are shown. For the main bandwidth of individuals between the ages of 25 and 36 (H=6), the private health insurance growth rate drops by 2.1 pp. when moving from below (and including) the age of 30 to above the age of 30 using the main specification without controls.

Table 3.3: Policy-Induced Demand for Private Health Insurance

	$Age \in [25,36]$		$Age \in [24,37]$		$Age \in [23,38]$	
First Stage						
Beta			-0.020			
SE			(0.000)			
F			713,247			
Reduced Form						
OLS	0.021***	0.016***	0.029***	0.026***	0.031***	0.030***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Probit	0.026***	0.021***	0.034***	0.031***	0.036***	0.034***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Second Stage						
2SLS	-1.06***	-0.80***	-1.45***	-1.29***	-1.57***	-1.50***
	(0.21)	(0.23)	(0.16)	(0.18)	(0.13)	(0.14)
IV Probit	-3.38***	-2.70***	-4.54***	-4.11***	-4.85***	-4.63
	(0.60)	(0.68)	(0.47)	(0.54)	(0.38)	(0.44)
Controls		x		x		x
F	27.56	10.24	93.44	42.25	141.37	107.95
N	243,298	231,466	336,892	268,362	384,518	305,506

Notes: In parenthesis are heteroskedastic standard-errors, generated via the delta method. All estimates are calculated for the year 2000, the year of the policy introduction. The first stage is the intent-to-treat loading schedule applied resulting in a first stage of 0.020*** with an F-value of 713,247 for H=6; A polynomial of degree P=2 was used for the running variable age.

The reduced form probit estimate is slightly larger but within the confidence interval of the ordinary least square coefficient while the coefficient with controls is smaller and remains within the confidence interval. The coefficients are marginally larger when we increase the bandwidth up to 3.1 (3.6) pp. reductions of the growth rate for OLS (Probit) but, overall, the results are robust. The second stage results for ages between 25 and 36 show that an increase in the price by 1 pp.

reduces demand by more than 1 pp. The coefficient with controls is within the confidence interval of the main specification. Coverage is negatively affected by the change in price: demand is downward sloping. We conclude that there is a robust first stage of the penalty policy on coverage which allows us to proceed to estimate the intent-to-treat estimates of being insured.

3.8 Effects of Private Health Insurance

Several questions regarding the purchase of private health insurance are discussed in this section.

Table 3.4: From Correlation to Causality – Health, Labor, and Budget Considerations

	Medical Expenses	Mortality	Gross Salary	Work Expenses	Student Debt Due
OLS					
Coeff.	38.45***	-0.00***	23,175.35***	570.72***	4.90***
(SE)	(0.142)	(0.000)	(56.427)	(4.061)	(0.582)
Baseline	14.15	0.00	32,778.34	1,353.56	82.92
(%)	170%	-49.23%	170%	142%	4%
Reduced Form					
Coeff.	-0.44	0.00	-1,084.76***	10.69	8.90***
(SE)	(0.66)	(0.00)	(139.62)	(9.23)	(1.42)
Baseline	28.01	0.00	43,149.49	1,631.71	76.32
(%)	-2%	18%	-3%	1%	12%

Notes: In parenthesis are heteroskedastic standard-errors, generated via the delta method. All estimates are calculated for the years 2000-2012. A polynomial of degree P=2 was used for the running variable age based on the year 2000 and a bandwidth H=6. The mortality coefficients are very small (low incidence), percentage changes are based on mean comparisons of those with and without private health insurance using the fourth digit after the coma (not shown).

What is the role of information asymmetries for Australian insurance contracts? Does the Australian insurance market exhibit adverse selection, advantageous selection, or moral hazard? What are the impacts of being privately insured on health, labor market outcomes, and personal budgets? If there would be no behavioral response after being exogenously insured, the benefits of private health insurance can manifest themselves in reduced health expenditures, lower mortality, an increase in gross salary, and no budget responses. However, moral hazard might lead to an increase in health expenditures.

Given longer stays in hospitals due to more convenient amenities, private health insurance can also lead to reduction in salaries. From a budget perspective, individuals who are exogenously pushed into private health insurance coverage might experience increased budget constraints resulting in reduced work expenses and an increase in outstanding student debt due. Agents might also be inclined to manipulate their reported gross salary to compensate for their premium expenses. The results are displayed in Table 4 and they are discussed in the next two sections.

3.8.1 Adverse Selection, Advantageous Selection and Moral Hazard

Acting upon information asymmetries is a commonly observed human behavior for health insurance, either in the form of adverse (advantageous) selection, i.e. the attraction of high (low) risk health types, or in the form of moral hazard, i.e. the change in risk behavior after obtaining insurance. A crucial question is whether the Lifetime Health Cover policy which discriminates by age (as well as the other policies at the time) was necessary to reduce adverse selection. Are unhealthy individuals more likely to select into private health insurance? Standard selection tests for insurance markets compare expected costs of the insured vs. expected costs of the uninsured. If the expected costs are larger for the insured than the uninsured, one might conclude that adverse selection exists. However, as Einav and Finkelstein (2011) point out, the positive correlation test is a joint test of moral hazard and adverse or advantageous selection, at least in the absence of differentiating the two problems of information asymmetry through any other means. Fortunately, the exogenous variation in PHI data provided through the current design allows for a differentiation.

The first row in Table 4 shows the correlations of private health insurance with the outcomes of interest. There is evidence of positive selection on medical expenses, mortality, gross salary and work expenses and student debt due. Medical expenditure and private health insurance are positively correlated by AU$ 38.45 for those individuals between 25 and 36 that grew older by 13 years in the time-horizon of interest. The reduced form estimation reveals that there is no moral hazard in the classical sense since having private health insurance does not visibly increase medical expenditures. It implies that all the variation from the OLS correlation with medical expenditure can be attributed to adverse selection. While it is a small amount in absolute terms, the relative impact is a large 170% increase in comparison to not being privately insured. A simple back of the envelope calculation reveals a total adverse selection cost of AU$ 9,354,808 for this young subgroup. This finding also confirms the intuition that exogenous variation of private health insurance coverage is crucial to obtain unbiased estimate of the effects.[7]

3.8.2 Health, Labor, and Budget Considerations

From Table 4 it is evident that private health insurance does not lead to changes in health in the reduced form (intent-to-treat). This strongly implies that relatively young individuals do not obtain any health benefits over the long horizon of 13 years when purchasing insurance, at least for the medical expenditure and mortality measures inspected in this study.

The main labor market outcome is gross salary from the Australian tax data. Intent-to-treat reductions of gross salaries around AU$1,000, or 3% of the baseline, are observed. While, one might expect increases in salary due to a faster return to the labor force when privately insured, it appears the opposite is the case. The private health insurance industry states the benefits of having coverage in addition to public insurance are the free choice of doctors, private rooms, and reduced wait-times for health services. Potentially, the convenience of a private room over a shared room might induce individuals to stay longer in the hospital when they are sick. This results in them foregoing the next best alternative, i.e. working and obtaining a higher residual income. Hence, those individuals with private health insurance forgo opportunity costs from the variable pay component of their income. Unfortunately, no direct tests of this hypothesis are currently possible with the data at hand, as individual-level data on the length of hospital stay is lacking. However, it

7. We take the average correlation and multiply the number of individuals that are the main bandwidth, i.e. AU$ 38.45*243,298 = AU$ 9,354,808.

appears that the effect is driven by a sub-group of individuals with the highest residual income: the self-employed. Self-employed individuals forgo AU$1,500 of residual income when insured. This finding is robust (not shown).[8] External aggregate data from the Australian Health and Welfare Institute shows a positive correlation of the length in the hospital and being privately insured (AIHW 2017).[9] Those two pieces of evidence together reinforce the idea that individuals forgo income by staying in the hospital for a longer period of time. However, since there is no effect on medical expenditures, this explanation is unlikely.

Another explanation is ex post income manipulation. Agents who get privately insured, have higher expenses but the same income. To optimize, they will report a lower income which leads to a lower tax burden if the ATO does not detect misreporting. The explanation of income manipulation is consistent with the observation that the self-employed drive the gross salary effect. However, since there is also an effect from the employed – albeit at a lower level. Therefore, ex post income manipulation can only partially explain the results.

Finally, the availability of budget restriction measures, such as work-related expenses and outstanding student debt, allowed us to test the effect of private health insurance on personal budgets. While suffering the consequences of having to pay the premium, individuals may substitute away from non-health expenditures. There are no effects of private health insurance coverage on work-related expenses. However, there is some evidence for the substitution hypothesis which displays the unintended consequence of policies that push people into private health insurance. Private health insurance increases the intent-to-treat amount of student debt due by AU$ 8.90. This is a 12% increase with respect to the baseline. It indicates that individuals with private health insurance delay their student debt repayments that they owe the government. If everyone would have student debt and coverage in this sample, it would imply a burden of AU$ 2,165,352.[10] According to the Australian Taxation Office, the number of people with outstanding student debt was 1,188,000 in

8. All results are robust. Among others, robustness checks for my intent-to-treat results entail going beyond a polynomial of degree 2, checking for potentially spurious life-cycle effects by adding age and quadratic age or year fixed effects as well as adding the covariate set displayed in Table 1.

9. Private patients stay 3.5 (2.2) days on average vs 3.1 (1.9) days for publicly insured patients in public (private) hospitals.

10. AU$ 8.90*243,298 = AU$ 2,165,352.

2006 and it is steadily increasing. A back of the envelope calculation implies that extending coverage to those individuals could result in a delay of student debt payments of an amount of AU$ 10,573,200.[11]

3.9 Conclusion

We document that the change in the growth rate of the price, changes the growth rate of insurance coverage. The Australian market seem to exhibit adverse selection which implies that the concern of an adverse selection death spiral that motivated the government interventions incentivizing private health insurance take-up at the millennium was justified. We do not find positive effects of private health insurance but some unintended consequences. In particular, we do not find any effects of private health insurance on the measures of health and even negative effects on gross salary which are likely driven by ex post income manipulation to reduce the premium burden from private health insurance. We also find evidence for substitution due to increased student debt indicating repayments slowing down for those being insured.

For the relatively young individuals of the ages between 25 and 48 private health insurance coverage does not seem to pay off. However, the largest benefits might not be observable yet in this study. For example, non-emergency surgery wait-times for privately insured individuals are significantly lower than for publicly insured individuals and the likelihood to have non-emergency surgery increases with age. This study underlines that we must understand health insurance more holistically to be aware not only of the benefits but also of the costs that can arise from increasing supplementary insurance in a universal health care system.

11. AU$ 8.90*1,188,000 = AU$ 10,573,200.

4. SUMMARY AND CONCLUSIONS

My three essays on the Demand and Welfare in Health Economics employ quasi-experimental and experimental methods to investigate causes, intended and unintended consequences from technologies and institutions in the domain of individual health, well being and the labor market.

In Section 2, we ask whether television consumption is an economic good or bad and whether it can be seen as a prime example of irrational behavior. Consistent with the literature on negative social effects of television consumption is a wide-held belief that watching TV is generally harmful. However, it is unclear whether television consumption is individually detrimental and if so: why do individuals then watch so much television in the first place? We study the causal effects from television consumption using a large-scale natural experiment in West Germany where households in a few geographically restricted areas received commercial television via terrestrial frequencies. Contrary to previous research, we find no health impact when television consumption increases. For life satisfaction, we even find positive effects. Additional data support the notion that television is not an economic bad and that non-experimental evidence seems to be driven by negative selection.

In Section 3, we study how individual behavior may affect the effectiveness of medical technologies, in the context of flu vaccination. We find that reducing opportunity costs increases vaccination take-up substantially and that peer behavior can influence participation in vaccination campaigns. These results suggest that decreasing opportunity costs is one option to increase participation drastically and that employers and policymakers can increase participation in health campaigns by using mechanisms to incentivize groups of people. We also find evidence consistent with vaccination causing a moral hazard problem. Our study provides several pieces of evidence that speak to the idea of riskier behaviors regarding health among vaccinated individuals. Forgoing other protective measures and increasing risky behavior could partially explain the ineffectiveness of vaccination and can improve our understanding why health interventions may sometimes fail.

In section 4, I study the unintended consequences of being privately insured in the universal health care system of Australia. A novel regression kink design in conjunction with a policy which punishes individuals who delay the purchase of private health insurance allows me to circumvent the adverse selection problem, i.e. that unhealthy individuals might be more likely to have insurance. After being exogenously insured, just having insurance can lead to individuals using more health services which can be captured through medical expenditures. However, it appears that private health insurance does not cause moral hazard despite evidence of adverse selection. Supplementary insurance does not change mortality or work expenses but it changes the budget. We observe an increase in student debt which is consistent with premium payments crowding out debt repayments to the government. There is a loss of gross income from private health insurance which is consistent with income under-reporting to reduce expenses from premium payments.

Overall, the chapters of my dissertation emphasize how technologies and institutions can shape behavior and that technologies can lead to unintended consequences which need to be carefully considered when implementing policies to improve individual health and labor market outcomes. The first study on the consequences of television consumption highlights that the common perception of TV being an economic bad seems to be wrong. Our finding is consistent with agents maximizing utility while imposing negative externalities on others. This knowledge is important for policy-makers to pick the most effective policy which takes this reality into account. Similarly, vaccination causing risky behavior needs to be carefully considered by policymakers so that the effectiveness of vaccination is not diminished. Finally, when the government incentives individuals to take up private health insurance, individuals might under-report their income to compensate for the expenses that they incur when insured. Those offsetting behaviors could be detrimental to the success of policies and the overall budget of the government, and, hence, policies designed to circumvent such actions are important to improve individual and societal welfare.

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APPENDIX I

TELEVISION, HEALTH, AND HAPPINESS

I.1 Appendix A – Historical details

In the following, we provide information on various aspects related to television in Germany, for which we have screened available historic news coverage. Aspects that these reports shed light on are a) the German people's (negative) views on television, b) positions of the German Supreme Court and interpretations of it, and c) the (limited) proliferation of private TV in Germany in the late 1980s and early 1990s. Figure A1 shows a timeline of events and illustrates important phases of the historical development. We provide references and links to sources, if we find those in the internet. Checks of all links were conducted on September 14, 2019. Note that original source documents are all in German.

a) *Perception of (private) television in Germany and political debate*

To illustrate what Germans think about television, we refer to the president of the European Commission, Ursula von der Leyen, in our introductory quote. This quote was taken from the ZDF TV talkshow "Berlin Mitte" with host Maybrit Illner and is from October 12, 2006, when von der Leyen participated as Germany's former Federal Minister of Family Affairs. Her statement was well-received by the audience and can be watched, e.g. here: `www.youtube.com/watch?v=z0LMjPHSoNs`.

Evidence for Germans' negative views on television are ubiquitous and go back to the middle of the 20th century. An early example of media coverage on TV and its potential implications for society is an article in DER SPIEGEL from April 13, 1950 (`https://www.spiegel.de/spiegel/print/d-44448169.html`) with the title "TV makes stupid" ("Fernsehen macht dumm"). This article, in Germany's most-read news magazine, informs about a severe decline in educational standards in Californian schools, which the article linked directly to the proliferation of television in the US at that time.

DER SPIEGEL also reported on the situation in Europe in the late 1970s when citizens in countries like Italy initially experienced private TV. An article from December 17, 1979 (`https://www.spiegel.de/spiegel/print/d-39685909.html`) had the title: “Private TV – more stultification of the masses?” (German: “Privatfernsehen: Nur noch Volksverdummung?”). The report refers to “porn shows with the beauties of the night” on Italian TV and concludes that television in Italy has degenerated into a family peep show since the public-media monopoly was lifted. This reminds the SPIEGEL reporter of how “American TV chains anesthetize their audiences around the clock.” The article then continues to cover the efforts of the federal government under the social-democratic Chancellor Helmut Schmidt to prevent private TV in Germany and cites, among others, leading social-democrat Egon Bahr who warned of a “convenient end of democracy.” Representatives of the other political camp included conservative minister-president of Lower-Saxony Ernst Albrecht (and father of Ursula von der Leyen) who argued for freedom of choice and appeared to disagree with the notion of stultification through proliferation of private TV.

Another article in DER SPIEGEL from October 10, 1979 (`https://www.spiegel.de/spiegel/print/d-39868784.html`) covers in more detail how the federal government in Germany thought to keep the public-media monopoly intact and cited chancellor Helmut Schmidt with his statement that “we must not stumble into dangers that are more acute and dangerous than nuclear energy” when talking about private TV. Schmidt also was convinced that private TV “could change the structures of the democratic society.” Instead of promoting the proliferation of television, the Chancellor suggested that it would be good to have a television-free day of the week. Ministers of the federal government under Schmidt followed his lead, including the minister of the interior, Gerhart Baum, who saw a need to “protect a humane democratic society against harmful influences of information overload and manipulation of public opinion” for which changes to the

German constitution were considered as an option. Justice Minister Hans-Jochen Vogel, concurring with this idea, argued that the freedom of information guaranteed under Article 5 of the Basic Law (Germany's de-facto constitution) should be ”restricted” by its Article 6, which protects the family. Vogel stated: “We cannot allow information overload to destroy the privacy of the family.”

Given all this, it is no surprise that the German public was not enthusiastic about private TV when it was started in the 1980s. DER SPIEGEL reported on August 1, 1983 (`https://www.spiegel.de/spiegel/print/d-14018801.html`) that a clear majority of 70% considered the current public TV offer as “sufficient” and only 5% of Germans considered private TV as “necessary” in a survey. However, over the years the media coverage of private TV changed. One example is a report in DER SPIEGEL on July 17, 1989 (`https://www.spiegel.de/spiegel/print/d-13494730.html`), in which the success story of RTLplus is described. The channel celebrated record ratings of 7.2 million viewers when covering a major sports event in Wimbledon with German tennis player Boris Becker. The same article also reported that German media corporation Bertelsmann, a partial owner of DER SPIEGEL, invested heavily into RTLplus (owning 38.9% of the shares). The rising popularity of RTLplus is also documented in actual market shares, which climbed steadily from almost zero in the mid-1980s to a historical peak of 18.9% in 1993. This can be seen in an illustration on the Wikipedia page of RTL (`https://de.wikipedia.org/wiki/RTL_Television`), which also points out that limited access explains the development of market shares in the channel's early years.

b) *Supreme Court decisions and interpretation*

The 1981 Supreme Court Rule (BVerfGE 57, 295), also labelled the “3rd broadcasting decision” (“3. Rundfunkentscheidung”) enabled the proliferation of private TV in Germany. According to the original text (`http://www.servat.unibe.ch/dfr/bv057295.html`), there was a “special situation of broadcasting caused by the scarcity of transmission frequencies” which in the eyes of the court justified the public-media monopoly until then. During the introduction of private TV, the newspaper DIE ZEIT described lifting the ban in an article on March 30, 1983 (https://www.zeit.de/1984/01/kabel-frei/komplettansicht) as follows: "Cable and satellite are nevertheless capable of revolutionizing electronic media. Both end the decade-long lack of television transmission. Just this technical bottleneck has always served as a final justification for the public service broadcasting system and for the political imperative to deny private access to this medium."

With its 1981 decision, the Supreme Court expanded on an earlier landmark decision from 1961 (BVerfGE 12, 205), i.e. the 1st broadcasting decision ("1. Rundfunkentscheidung"). In this early decision (http://www.servat.unibe.ch/dfr/bv012205.html), Germany's Supreme Court confirmed the existence of a public-media monopoly. It also required an independent organization of frequencies for new public media outlets. The rule arose due to a heated conflict in the late 1950s about Germany's second public TV broadcast ZDF, adding to the first public TV channel ARD (see Figure A1). One of Germany's major political parties, the social-democratic party, was the initiator of this legal case in an effort to fight what was called "Adenauer-TV" (in reference to the conservative chancellor) and meant television controlled by the federal government. The Supreme Court shared those concerns of social-democrats and called for political independence of the organization of Germany's media. As a direct consequence of the Court rule, a sub-organization of the Deutsche Bundespost was in charge of the construction of new transmitter stations with the distinct purpose to provide frequencies for public media outlets, while state governments were in charge of the frequency usage. After the Supreme Court decision of 1961, stations were built for the new public TV channel ZDF. During that time, there was also a realistic prospect of additional public broadcasts, such as regional public TV programs at the state level and further radio programs. Given that those plans for future public broadcasts were uncertain, new transmitter stations built after 1961 had varying capacities for possible additional broadcasts, all of which however were expected to be for public media during the construction phase.

When the Court re-formulated its position in 1981, it was clear from the actual text about the restrictions on broadcasting and how those could be "eliminated in the course of modern development" that new technologies like cable and satellite were seen as the way for private TV to reach German households. However, it was not explicitly stated by the Court, that still available terrestrial frequencies could not be used by private TV channels. Since the Court saw a scarcity of those terrestrial frequencies as the reason for the public monopoly, there was no expectation of many open frequencies. Therefore, for the few frequencies available in the 1980s, it was legally possible to broadcast private TV on them, as long as i) they were established for this particular purpose (of broadcasting public TV) and ii) the state government, in which the station was located, agreed to have private TV being put on the air.

c) *Limited reach of private TV: cable, satellite and terrestrial frequencies*

As a result of the legal framework established by Germany's Supreme Court, the federal government under Helmut Kohl decided to roll out private TV in Germany via cable. Several pieces of evidence however document the delays in expansion of Germany's cable net. Already in 1984, few days after private TV started in Germany, DER SPIEGEL reported on January 9 (`https://www.spiegel.de/spiegel/print/d-13508379.html`) that "Post Minister Schwarz-Schilling has lost track of his cable projects." The article described miscalculations and unexpected costs in the rollout of cable. On January 27, 1984, newspaper DIE ZEIT (`https://www.zeit.de/1984/05/im-kabel-verfangen/komplettansicht`) headlined a report asking whether Schwarz-Schilling could become a "Minister of Crisis" for the Kohl government. In this report, a media expert described cable TV in Germany as a "billion-dollar grave" that the Deutsche Bundespost cannot cope with in the long-term (which turned out to be a good prediction since this public institution was dismantled ten years later). On September 3, 1984, DER SPIEGEL had the headline the "Cable TV: The debacle is here" (`https://www.spiegel.de/spiegel/print/d-13509973.html`) and described "miscalculation with billions, chaotic charges policy, gadgets with outdated technology: Christian Schwarz-Schilling brings the state-owned company Bundespost with his favorite project - cable global, television total - in financial difficulties.“ In consequence, there was a very slow spread of cable

TV across Germany from the early 1980s to the mid-1990s. As reported by the federal government in 1988 due to a request in the German parliament, cable TV was unavailable in 85.2% of West German households. This figure included data from West-Berlin, which had the highest state-level access rate with 31.6%. The state of NRW had a cable access rate of 11.8%. Detailed figures for all federal states are on the German Wikipedia page (`https://de.wikipedia.org/wiki/Kabelfernsehen`). Because German households had to pay a non-negligible amount of money for cable TV, actual use of cable TV was even lower. On April 6, 1987, DER SPIEGEL (`https://www.spiegel.de/spiegel/print/d-13521242.html`) reported that less than half of households who technically could have ordered cable TV did not order it because of "high connection fees". More than a third of German households still had no access to cable at the end of 1995, according to the above Wikipedia page, in reference to the Deutsche Telekom (a successor of the Bundespost).

The other option for private TV was satellite. Watching private TV via their own satellite dishes became the norm for Germans throughout the 1990s. Thereby satellite TV emerged as an important transmission channel since millions of German households still had no access to cable TV. For satellite TV, we can safely rule out that this affects our results for our main investigation period in the late 1980s. There is news coverage from our main period of investigation, in form of a report in DIE ZEIT of July 1, 1988 (`https://www.zeit.de/1988/27/mami-hol-pudding/komplettansicht`), illustrating why satellite TV was practically irrelevant in Germany in the 1980s due to the activities of the Deutsche Bundespost. The article reports on a new direct-transmitting satellite that was announced as a possible supplement for cable but turned out "to be a total failure" which after being launched "floats as a mummy in space" due to technical problems. According to DIE ZEIT, a successor satellite was planned for the next year but this "will hardly bring more viewers to private programs" because of "a new transmission standard" that required a special decoder, a technology that was not even on the market. Clearly, due to large investments into cable, the Bundespost had strong incentives to not foster satellite

TV as an alternative (similar to providing terrestrial frequencies to private TV). In this context, the above-cited article in DER SPIEGEL from September 3, 1984 is illuminating, as it describes the concern of the Bundespost that satellite could render all the investments into cable useless if it becomes the norm. The article reports on efforts by post minister Schwarz-Schilling to prevent "satellite reception by anyone" and to thereby ensure that Germans need cable to watch private TV.

Due to satellite TV not being an option before the 1990s and delays in the expansion of cable TV, a time window of several years was created in which still available terrestrial frequencies were enormously important for the private TV channels. In consequence, there was a limited number of regions in which German households could watch private TV via regular antennas. The limitation of these terrestrial frequencies was a direct result of the legal framework established by Germany's Supreme Court. How those limitations worked in practice is demonstrated in the news coverage from the late 1980s, such as in the above-referenced DIE ZEIT report from July 1988. The article describes how private TV channels received the rights to broadcast on the terrestrial frequencies in the state of NRW. Tele5 owner Silvio Berlusconi "jumped on the bandwagon at the last minute" to apply for the attractive terrestrial frequencies in this state. He realized that the cable net in Germany was "tight" and that the terrestrial frequencies were thus absolutely essential for the TV providers. NRW with its history as a large coal-producer and highly populated was the "key region" to determine the winner in the competition for market shares in the new TV landscape. To assuage the social-democrat run state government of NRW, Berlusconi promised a "quality program" and that he would "support the left" when he was given the "juicy" frequencies in NRW. Despite all the efforts by Berlusconi, however, the state of NRW preferred the channel RTLplus due to several "concessions" agreed upon by the owners, e.g. the Bertelsmann corporation. The channel started as a broadcaster from Luxembourg, but the decision was made to move to Cologne, the largest city of NRW. DIE ZEIT stated that the deal "was worth it" since RTLplus could expect to reach "up to six and a half million viewers" thanks to being preferred in the competition for transmission frequencies. DER SPIEGEL also reported on this NRW deal in an article from March 27, 1989 (`http://www.spiegel.de/spiegel/print/d-13495757.html`), in which the term “juicy” was used to describe the powerful terrestrial frequencies in NRW (“die leckeren terrestrischen Frequenzen in Nordrhein-Westfalen“). The report emphasizes the importance of these powerful frequencies to reach large numbers of households and sheds light on other facets of the secret deal that was referred to as a “crooked number” by an insider.

An article by DER SPIEGEL from January 9, 1989 (`https://www.spiegel.de/spiegel/print/d-13493795.html`) describes the efforts to overcome the restrictions on terrestrial frequencies by Silvio Berlusconi. While his channel Tele5 had difficulties to obtain frequencies, it was “beyond question” for Berlusconi that an expansion of terrestrial frequencies was technically possible. Based on studies by his own technicians, Berlusconi was confident that new transmitter stations “covering the entire territory” of Germany would allow exceeding the preexisting networks. DER SPIEGEL speculated about Tele5 expansions which “could suddenly bring the distressed commercial channel the economic breakthrough.” However, it is no surprise, given the legal framework, that the German regulatory bureaucracy (“Aufsichtsbürokratie”) stopped such efforts. The Bundespost even tried to prevent Tele5 from getting the available low-power frequencies, which were already greenlighted by state governments. In consequence, Berlusconi sent his team of technical experts from Italy “on a journey across the Alps” to disprove false claims of the Bundespost, according to which there were no available frequencies. While those local frequencies were then given to Tele5, the article further describes Berlusconi’s failure to get one of the powerful frequencies in the West or in the North. This was explained with a lack of support for Berlusconi in German politics, especially among conservatives who thought that the Italian could sympathize with social-democrats. All this happened as part of what was called a “battle for the frequencies” by DER SPIEGEL, a battle in which the Italian media tycoon had to recognize defeat.

Figure I.A1: Timeline for Proliferation of Commercial Television in West Germany

1952	Start of Germany's 1st public TV channel (*ARD*)
	One network of transmitter stations for terrestrial frequencies, no private TV allowed on stations
1961	Supreme Court confirms public-media monopoly, requires state-free organization of broadcasting
	Construction phase of network of new transmitter stations for 2nd public TV channel (ZDF)
1974	Social-democrat Helmut Schmidt becomes chancellor
	Federal Government defends monopoly of public TV and blocks cable TV projects
1981	Supreme Court paves the way for private TV by referring to new technological developments
	Crisis of the Federal Government: coalition between social-democrats and liberal party falls apart
1982	Conservative Helmut Kohl becomes chancellor
	Federal Government pushes private TV and assigns Deutsche Bundespost to roll out private TV
1984	Start of commercial TV in Germany
	Deutsche Bundespost fails to provide private TV to German households according to time schedule
1988	Private TV channels receive powerful terrestrial frequencies on public-media transmitters
	Divided country: reception of private TV via antenna in some regions of Germany, not in others
1993	*RTL* (formerly *RTLplus*) becomes Germany's No.1 TV channel with record market share of 18.9%
	Due to lacking cable access, many Germans prefer using satellite dishes to watch private TV
1994	Deutsche Bundespost dismantled, cable TV still unavailable in many German households

Notes: The following abbreviations for TV channels are used in this table: ARD (*Arbeitsgemeinschaft der öffentlich-rechtlichen Rundfunkanstalten der Bundesrepublik Deutschland*), ZDF (*Zweite Deutsche Fernsehen*), and RTL (*Radio Télévision Luxembourg*). Note that the organizers of the latter channel dropped the „plus“ from the channel's name on November 1, 1992.

I.2 Appendix B – Sensitivity analyses

In the following, we discuss the results from a plethora of sensitivity analyses, which we conduct for our main analysis based on SOEP data, as reported in the paper. Aspects that we shed light on are a) determination of TV signals (i.e. calculation method, signal strength thresholds, linear signal strength instrument), b) sample restrictions (i.e. regions considered in the analysis, movers exclusion), c) alternative definitions of TV consumption, and d) additional covariates (i.e. further survey control variables, weather influences).

a) *TV signals*

We start our sensitivity analyses by inspecting whether the method of TV signal calculation could affect the results. In Table B1 we provide a check for the aggregation method of regional signal strength. In our main analysis, we used the most precise method, i.e. the one square kilometer raster calculation and then averaging all raster values within a county using the mean. For comparison, this is shown in Column (1). Column (2) shows that switching from mean to median, does not alter the results at all. We also calculated the signal strength on the municipality and county level directly, without raster calculation. For the county level, we determine the signal strength either at the geographical center of the region or at the population-center. This information comes from a geo service and reflects the inner city of a county's largest town. We use the latter to define signal strength at the municipality level. To use the information on signal strength at the municipality level for our SOEP analysis at the county level, we aggregate municipality signals either as the mean (i.e. average signal strength across all municipalities within a county) or as the median. The first stage coefficients are slightly smaller in comparison to the more precise raster calculation, but still highly significant and robust in all cases. Comparing the results for mean and median aggregation in Columns (3) and (4) reveals no difference. For the least precise calculations based on one signal value per county in Columns (5) and (6), we find that the coefficients of the TV signals using the population-center are remarkably robust, while using the geographic mean within a county leads to a weaker first stage. This underlines the importance of considering the location of the population when determining TV signal reach at the regional level.

Second, we discuss the role of the threshold level of the signal strength which is used to define treatment and control regions. By changing the threshold, we are more (or less) optimistic regarding the reach of TV signals for German households when we use lower (or higher) threshold values in dBuV/m. Similar checks are conducted in Bursztyn and Cantoni (2016) whose default threshold is slightly higher than ours, while for radio signals analysed by Yanagizawa-Drott (2014) the default threshold is lower than ours. By varying the minimum signal strength from 50 dBuV/m to 65 dBuV/m stepwise, column after column, we consider a range of thresholds that involve those used in the literature. Table B2 shows that the results do not change when we vary the signal threshold. In all the columns, the instrument is still highly significant for each dBuV/m value allowing always for instrumentation with an F-value above 10. Consistently, the results on the second stage do not change qualitatively. Television increases life satisfaction for any of the threshold values, while individual health is not impaired in any case, in line with the findings in the main text.

Third, we check our decision to focus on signal strength thresholds for a binary distinction between regions with sufficient signal strength and those without, in contrast to interpreting signal strength in linear fashion. One reason for doing so is that TV signal quality does not increase linearly in signal strength but discontinuously, as pointed out in related research (Bursztyn and Cantoni 2016). One could argue that either a household receives a program or not, with little happening in between. To nevertheless assess our decision empirically, Table B3 shows the results when we employ a linear variable of signal strength in dBuV/m. We also vary the use of our standard control variable set. The results in Columns (3) and (4) conform qualitatively to the results established via binary treatment indicator, which are shown in Columns (1) and (2).

b) *Sample restrictions*

In a fourth step, we check the robustness of our main findings regarding changes to the sample. Table B4a presents the first set of results from sensitivity analyses where we exclude regions from the set of non-affected control group regions. We show both the first stage and reduced form results. Column (1) shows the results without any adjustment. In column (2), we show the estimates when we only condition on "clean control regions." For this purpose, we exclude all regions which did not receive TV signals from powerful frequencies but could have received TV signals on frequencies with low power. In column (3), we drop all counties with early cable projects in the mid-1980s (Hasebrink 1989). These cable projects were in Munich, Dortmund, Ludwigshafen, and Berlin. Then, we drop data from megacities with about one million inhabitants, i.e. Hamburg, Munich and Cologne in column (4). This test excludes regions that are not necessarily affected by terrestrial TV signals, given higher chances of cable access. In column (5) we exclude border regions, in which individual TV consumption was arguably not affected by private TV as much, given that foreign TV was potentially available. By doing so, we exclude some counties which received a 200 kW signal from the Wesel transmitter, so that this check results in rather conservative estimates. Finally, in column (6), we apply all restrictions simultaneously. The table shows that the coefficients are all robust.

Table B4b completely cleans the control region by only accounting for control regions that do not have a transmitter. Thereby we drop counties in which there were local frequencies for private TV with very low power (close to zero kW), so that the signal did not even surpass our signal strength threshold in the county where the stations was located. When we move from the main results from column (1) to column (2), the F statistics for the first stage increase slightly and thus the instrument remains strong. Another curious case is the state of Rhineland Palatinate which borders Luxembourg. Luxembourg was the original location of the channel RTLplus and the Minister president Vogel was highly in favor of promoting private television. Therefore, we drop the state of Rhineland Palatinate in column (3), which again leads to a small increase in F statistics. When we restrict the sample to controls without stations and exclude the state of Rhineland Palatinate in column (4), the findings on increased TV consumption and higher life satisfaction due to private TV remain strong. Overall, there are no significant differences in coefficients across specifications which leads us to conclude that the results are robust.

Table B5 provides robustness checks for our exclusion restriction regarding individuals who move between regions during our investigation period. We show the first stage and reduced form effects based on the private TV signals using different restrictions for moving individuals. Column (1) shows the results based on our main restriction, as described in Section 2, where we include observations only if the same person is observed in the same county, in which he or she lived in 1989. The idea behind using 1989 as the reference year is to maximize the number of treatment observations, which implies having a left-skewed distribution of observations across years in our main sample. In Column (2), we shift the reference year and include only individual observation if the person is observed in the same county, in which he or she lived in 1988. Column (3) shows results with the strictest mover restriction where each individual is observed always within the same county throughout the investigation period. This goes along with a substantial loss of sample size. Column (4) shows the results without accounting for any compositional change in the sample due to relocations, thereby maximizing the sample size. In this case, individuals are observed in different counties if they move between regions during the investigation period. This prevents us from clustering standard errors at the county level in our fixed-effects analyses. To allow for a comparison of results, we always employ individual-level clustered standard errors across specifications in Table B5. We observe that the coefficient for the first stage decreases when we compare the strictest mover exclusion restriction in Column (3) to having no mover restriction in Column (4) while the results for our preferred definition as used in the paper lies in between. In any case, the reduced form results for our main outcome variables do not change qualitatively.

c) *Alternative definitions of TV consumption*

In Table B6, we show the results from checks of our manipulation variable television consumption. Using various approaches based on different definitions of this variable, we address the potential role of outlier, i.e. high levels of TV consumption. Column (1) shows the main results for comparisons. We windsorized television consumption to levels below 100 and 70 hours per week to exclude outliers in Columns (2) and (3). The first stage and the reduced form results remains stable. As a different approach, we log-transform television consumption in Column (4) which reduces the sample size only slightly since it excludes individuals that do not watch TV. Receiving the signal increases TV consumption by 7.9% on the first stage.

In Table B6, we further alter our variable reflecting television consumption. In a first step, we estimate the impact of the television signal on different levels of television consumption, i.e. the first, second, third, and fourth quartile of the distribution are used as dependent variables in Columns (7), (8), (9), and (10). Receiving a signal improves the share of individuals in the highest quartile of television consumption, while it reduces the likelihood of being observed in the lowest quartile. In a final step, we use the raw measures of television consumption which are split into television consumption during the workweek, including Saturday (Column 5), and the weekend, i.e. Sunday (Column 6) in the SOEP. We find that TV consumption is increased more on the weekend than during the week, but in both cases, private TV reception yields significantly positive effects in TV consumption.

d) *Further control variables*

Table B7 provides a robustness check of the first stage and reduced form effects based on the private TV signal using different sets of control variables. Column (1) shows the main results. Column (2) adds the standard control set, which slightly increases the first stage coefficient. The standard control set includes gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. Column (3) shows the main results with survey controls, i.e. survey week, day of the interview, number of survey participations in the SOEP and whether the survey was administered orally or filled out by the survey participant. Column (4) shows results when controlling for both standard controls and survey controls. The first stage remains stable in all cases and reduced form results for our main outcome variables are robust.

Similar, in Column (5) we add weather controls, i.e. temperature and sunshine hours on the day of the interview as well as average temperature and average sunshine hours over the last four weeks prior to the interview. We use a weather dataset that was prepared for a different project and allows us to check the role of weather influences for our results. Regional identifiers at the regional policy region level are used, which is one level higher than counties in the German regional hierarchy. Each of the 74 regions in West Germany at the time consists of at least one county (four counties on average) and has a weather station that allow us to merge the SOEP data with weather data for analyses. The data on regional weather conditions for each (interview) day are from the German weather service and are explained in detail in Chadi (2017). We find that adding weather controls does not alter the results. Finally, in Column (6) we include all three control sets. The first stage and reduced form remain unaltered.

Table I.B1: TV signal calculation method

	Raster Aggregation (Mean)	Raster Aggregation (Median)	Municipality Aggregation (Mean)	Municipality Aggregation (Median)	County Population Center	County Geographic Center
First Stage: TV Consumption	(1)	(2)	(3)	(4)	(5)	(6)
Private TV	1.677***	1.677***	1.582***	1.582***	1.672***	1.413***
	(0.332)	(0.332)	(0.344)	(0.344)	(0.333)	(0.394)
F	25.515	25.515	21.150	21.150	25.211	12.862
N	*19,532*	*19,532*	*19,532*	*19,532*	*19,532*	*19,532*

Notes: Dependent variable is weekly TV consumption in hours. Private TV is a dummy variable indicating whether the SOEP respondent lives in a county for which a 200kW TV signal based on terrestrial frequencies was calculated. The columns show results using different signal calculations for the county level. The baseline specification contains year-fixed effects without control variables. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table I.B2: TV Signal strength thresholds

First Stage: TV Consumption	50	55	60	65
Private TV	1.602***	1.602***	1.484***	1.627***
	(0.374)	(0.374)	(0.424)	(0.436)
F	18.348	18.348	12.25	13.925
N	*20,334*	*20,334*	*20,334*	*20,334*
Second Stage: Life Satisfaction	50	55	60	65
Private TV	0.169***	0.169***	0.197***	0.136***
	(0.063)	(0.063)	(0.073)	(0.042)
N	*19,166*	*19,166*	*19,166*	*19,166*
Second Stage: Health Satisfaction	50	55	60	65
Private TV	0.067	0.067	0.007	0.007
	(0.063)	(0.064)	(0.064)	(0.061)
N	*19,190*	*19,190*	*19,190*	*19,190*
Second Stage: Visited A Doctor	50	55	60	65
Private TV	-0.007	-0.007	-0.010	0.006
	(0.011)	(0.011)	(0.013)	(0.012)
N	*19,197*	*19,197*	*19,197*	*19,197*
Second Stage: Doctor Visits	50	55	60	65
Private TV	0.010	0.010	-0.104	-0.064
	(0.171)	(0.171)	(0.170)	(0.165)
N	*14,088*	*14,088*	*14,088*	*14,088*

Notes: Dependent variables reflect time in hours per week that a person spends watching TV (first stage) and life satisfaction, health satisfaction, doctoral visits on the extensive margin – i.e. visited a doctor – and the number of doctor visits (second stage). Private TV is a dummy variable indicating whether the SOEP respondent lives in a county for which a 200kW TV signal based on terrestrial frequencies was calculated. The columns show results with different signal calculations from 50, 55, 60 and 65 dB. The baseline specification contains year-fixed effects. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table I.B3: Binary Signal vs Continuous Signal

First Stage: TV Consumption	(1)	(2)	(3)	(4)
Private TV Signal	1.677***	1.712***	0.022***	0.023***
	(0.332)	(0.332)	(0.005)	(0.005)
F	25.515	26.208	19.360	21.160
N	*19,532*	*19,401*	*19,532*	*19,401*
Reduced Form: Health Satisfaction	**(7)**	**(8)**	**(9)**	**(11)**
Private TV Signal	0.076	0.088	0.001	0.001
	(0.106)	(0.109)	(0.001)	(0.001)
N	*19,510*	*19,379*	*19,510*	*19,379*
Reduced Form: Life Satisfaction	**(13)**	**(14)**	**(15)**	**(17)**
Private TV Signal	0.260***	0.255***	0.004***	0.004***
	(0.082)	(0.084)	(0.001)	(0.001)
N	*19,500*	*19,369*	*19,500*	*19,369*
Reduced Form: Visited A Doctor	**(19)**	**(20)**	**(21)**	**(23)**
Private TV Signal	-0.013	-0.013	-0.000	-0.000
	(0.019)	(0.019)	(0.000)	(0.000)
N	*19,516*	*19,386*	*19,516*	*19,386*
Reduced Form: Doctor Visits	**(25)**	**(26)**	**(27)**	**(29)**
Private TV Signal	0.002	-0.001	-0.000	-0.000
	(0.299)	(0.307)	(0.004)	(0.004)
N	*15,257*	*15,151*	*15,257*	*15,151*
Binary Signal	YES	YES		
Continuous Signal			YES	YES
Standard Controls		YES		YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is TV consumption in hours. The private TV signal is based on 200KW. The baseline specification contains year-fixed effects. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * p < 0.10, ** p < 0.05, *** p < 0.01.
Source: SOEP data are from 1987 to 1989.

Table I.B4a: Sample checks: exclusion of regions I

First Stage: TV Consumption	(1)	(2)	(3)	(4)	(5)	(6)
Private TV Signal	1.677***	1.744***	1.664***	1.619***	1.825***	1.963***
	(0.332)	(0.341)	(0.328)	(0.331)	(0.357)	(0.367)
F	25.515	26.157	25.737	23.924	26.133	28.609
N	*19,532*	*17,291*	*18,830*	*18,277*	*16,682*	*13,907*
Reduced Form: Health Satisfaction	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
TV	0.076	0.078	0.064	0.078	0.087	0.098
	(0.106)	(0.107)	(0.105)	(0.106)	(0.117)	(0.118)
N	*19,510*	*15,661*	*17,798*	*17,277*	*15,815*	*12,874*
Reduced Form: Life Satisfaction	**(13)**	**(14)**	**(15)**	**(16)**	**(17)**	**(18)**
TV	0.260***	0.257***	0.255***	0.259***	0.215**	0.217**
	(0.082)	(0.084)	(0.082)	(0.082)	(0.085)	(0.087)
N	*19,500*	*15,657*	*17,792*	*17,268*	*15,801*	*12,871*
Reduced Form: Visited A Doctor	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
TV	-0.013	-0.007	-0.014	-0.013	-0.029	-0.027
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)
N	*19,516*	*15,674*	*17,807*	*17,288*	*15,823*	*12,886*
Reduced Form: Doctor Visits	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**
TV	0.002	-0.003	0.020	0.016	-0.107	-0.108
	(0.299)	(0.301)	(0.298)	(0.299)	(0.320)	(0.324)
N	*15,257*	*11,022*	*13,024*	*12,669*	*11,548*	*9,083*
Clean Control Regions		YES				YES
No Cable Projects			YES			YES
No Megacities				YES		YES
No Border Regions					YES	YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is TV consumption in hours. The private TV signal is based on 200KW. The baseline specification contains year-fixed effects. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table I.B4b: Sample checks: exclusion of regions II

First Stage: TV Consumption	(1)	(2)	(3)	(4)
Private TV Signal	1.677***	1.715***	1.715***	1.781***
	(0.332)	(0.335)	(0.328)	(0.329)
F	25.515	26.208	27.339	29.305
N	*19,532*	*17,879*	*18,305*	*16,812*
Reduced Form: Health Satisfaction	**(7)**	**(8)**	**(9)**	**(11)**
TV	0.076	0.048	0.061	0.033
	(0.106)	(0.107)	(0.106)	(0.107)
N	*19,510*	*17,857*	*18,286*	*16,793*
Reduced Form: Life Satisfaction	**(13)**	**(14)**	**(15)**	**(17)**
TV	0.260***	0.255***	0.254***	0.241***
	(0.082)	(0.084)	(0.082)	(0.084)
N	*19,500*	*17,850*	*18,279*	*16,788*
Reduced Form: Visited A Doctor	**(19)**	**(20)**	**(21)**	**(23)**
TV	-0.013	-0.009	-0.012	-0.008
	(0.019)	(0.019)	(0.019)	(0.019)
N	*19,516*	*17,867*	*18,290*	*16,801*
Reduced Form: Doctor Visits	**(25)**	**(26)**	**(27)**	**(29)**
TV	0.002	-0.027	-0.018	-0.022
	(0.299)	(0.300)	(0.298)	(0.301)
N	*15,257*	*13,639*	*14,294*	*12,801*
Controls without Stations		YES		YES
No Rhineland Palatinate			YES	YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is TV consumption in hours. The private TV signal is based on 200KW. The baseline specification contains year-fixed effects. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table I.B5: Sample checks: exclusion of movers

First Stage: TV Consumption	(1)	(2)	(3)	(4)
Private TV Signal	1.677***	1.677***	1.832***	1.547***
	(0.377)	(0.377)	(0.439)	(0.355)
F	19.787	19.787	17.415	18.990
N	*20068*	*19532*	*14,619*	*23093*
Reduced Form: Health Satisfaction	**(5)**	**(6)**	**(7)**	**(8)**
TV	0.076	0.076	0.080	0.036
	(0.082)	(0.082)	(0.125)	(0.076)
N	*20044*	*19510*	*14,600*	*23060*
Reduced Form: Life Satisfaction	**(9)**	**(10)**	**(11)**	**(12)**
TV	0.260***	0.260***	0.253***	0.216***
	(0.072)	(0.072)	(0.079)	(0.067)
N	*20035*	*19500*	*14,595*	*23052*
Reduced Form: Visited A Doctor	**(13)**	**(14)**	**(15)**	**(16)**
TV	-0.013	-0.013	-0.018	-0.003
	(0.021)	(0.021)	(0.022)	(0.019)
N	*20052*	*19516*	*14,607*	*23071*
Reduced Form: Doctor Visits	**(17)**	**(18)**	**(19)**	**(20)**
TV	0.002	0.002	-0.128	0.132
	(0.230)	(0.230)	(0.314)	(0.209)
N	*15404*	*15257*	*11,275*	*17709*
No Movers 1989 Baseline	YES			
No Movers 1988 Baseline		YES		
No Movers 1987-1989			YES	
All Movers Included				YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is TV consumption in hours. The private TV signal is based on 200KW. The baseline specification contains year-fixed effects. Individual-level clustered standard errors are in parentheses. Levels of significance are * p < 0.10, ** p < 0.05, *** p < 0.01.

Source: SOEP data are from 1987 to 1989.

Table I.B6: Definition of TV Consumption

First Stage: TV Consumption	TV (1)	TV < 100h (2)	TV < 70h (3)	Log(TV) (4)	TV Workweek (5)
Private TV	1.677***	1.677***	1.576***	0.079***	0.149***
	(0.332)	(0.332)	(0.320)	(0.020)	(0.047)
F	25.515	25.515	24.256	15.603	10.050
N	*19,532*	*19,532*	*19,523*	*18,970*	*19,532*

First Stage: TV Consumption	TV Weekend (6)	$TV \leq Q_1$ (7)	$Q_1 < TV \leq Q_2$ (8)	$Q_2 < TV \leq Q_3$ (9)	$TV > Q_4$ (10)
Private TV	0.255***	-0.051***	-0.023	0.012	0.063***
	(0.052)	(0.015)	(0.019)	(0.019)	(0.021)
F	24.048	11.560	1.465	0.399	9.000
N	*19,532*	*19,532*	*19,532*	*19,532*	*19,532*

Notes: Dependent variable is health and life satisfaction on a 0 to 10 scale and doctoral visits on the intensive (number of doctor visits) and extensive margin (visited a doctor). The instrumented independent variable is TV consumption in hours. Q1,Q2,Q3 are the first, second and third quartile. The baseline specification contains year-fixed effects without control variables. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP data are from 1987 to 1989.

Table I.B7: FE Regressions with different sets of controls

First Stage: TV Consumption	(1)	(2)	(3)	(4)	(5)	(6)
Private TV Signal	1.677***	1.712***	1.661***	1.683***	1.665***	1.649***
	(0.332)	(0.322)	(0.330)	(0.325)	(0.336)	(0.331)
F	25.515	28.268	25.334	26.816	24.556	24.819
N	*19,532*	*19,401*	*19,262*	*19,133*	*19,422*	*19,024*
Reduced Form: Health Satisfaction	**(7)**	**(8)**	**(9)**	**(11)**	**(10)**	**(12)**
TV	0.076	0.088	0.078	0.093	0.078	0.101
	(0.106)	(0.109)	(0.103)	(0.105)	(0.106)	(0.105)
N	*19,510*	*19,379*	*19,240*	*19,111*	*19,400*	*19,002*
Reduced Form: Life Satisfaction	**(13)**	**(14)**	**(15)**	**(17)**	**(16)**	**(18)**
TV	0.260***	0.255***	0.257***	0.248***	0.273***	0.263***
	(0.082)	(0.084)	(0.084)	(0.086)	(0.085)	(0.089)
N	*19,500*	*19,369*	*19,230*	*19,101*	*19,390*	*18,992*
Reduced Form: Visited A Doctor	**(19)**	**(20)**	**(21)**	**(23)**	**(22)**	**(24)**
TV	-0.013	-0.013	-0.014	-0.013	-0.010	-0.011
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)
N	*19,516*	*19,386*	*19,246*	*19,118*	*19,406*	*19,009*
Reduced Form: Doctor Visits	**(25)**	**(26)**	**(27)**	**(29)**	**(28)**	**(30)**
TV	0.002	-0.001	-0.098	-0.108	0.030	-0.056
	(0.299)	(0.307)	(0.287)	(0.295)	(0.293)	(0.292)
N	*15,257*	*15,151*	*15,065*	*14,961*	*15,172*	*14,877*
Standard Controls		YES		YES		YES
Survey Controls			YES	YES		YES
Weather Controls					YES	YES

Notes: The dependent variables are health and life satisfaction on a 0 to 10 scale as well as doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. The instrumented independent variable is TV consumption in hours. The private TV signal is based on 200KW. The baseline specification contains year-fixed effects. The standard set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. The survey controls contains the day of the week of the survey and the interview mode. The weather controls contains temperature and sunshine hours on the day of the interview and over the last four weeks prior. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I.3 Appendix C – Income and Consumption Sample

In this Appendix, we a) describe the income and consumption sample (EVS) data that we use for empirical analyses and b) exploit household ownership information on the number of TV. The latter serves as a complementary test for the plausibility of the calculated TV signals, i.e. whether receiving private TV encourages not only TV watching (as examined in the SOEP) but also TV ownership.

a) *Preparation of data analysis and discussion of its limitations*

To merge EVS data with TV signals for the late 1980s and early 1990s, we use two waves of this repeated cross-sectional sample on consumption and expenditures. The earliest available EVS data wave with regional identifiers of households below the state level is from 1988. For the subsequent EVS wave of 1993, regional identifiers are also available. This information on the regional level of the data is needed for the merger with our TV signals data. Available to us are municipality indicators, which allows using the EVS data at a lower hierarchical level compared to the SOEP (see Figures D3 and D4 for a visual comparison). Apart from potential increases in precision when using TV signal information at the municipality vs. county level, we have to consider special data regularities and other minor caveats for our analysis of the EVS data.

Data protection plays an important role for our analysis of the EVS data at the municipality level. We work with a restricted use version that is only available for research on the premises of the Federal Statistical Office of Germany. As researchers, we can differentiate between all the different municipalities, in which EVS participants reported on their lives, but we cannot uniquely identify any municipality. To maintain the anonymous nature of the municipalities, we searched for possible problem cases when deciding about data restrictions concerning the territory included in the analysis. When we restrict the EVS data to West Germany, we exclude the two municipalities of West Berlin (as in our SOEP-based analysis) and Helgoland prior to the data merger. The island of Helgoland had a separate transmitter station that started broadcasting several private TV channels with low power but exceptionally early in 1987, according to the official records. This would, in principle, allow us to perfectly identify all the data from interviews with individuals living on this island. Therefore, we exclude Helgoland with its low case numbers to ensure data protection.

Representativeness is one of the features of the EVS but the dataset available to us is limited in this respect for the following reasons. First, the German statistical office could obtain municipality information from most of the official interviewer records, but in some areas of Germany, this was not possible for the wave of 1988. In consequence, we condition on West German municipalities that are included in both EVS data waves of 1988 and 1993. Second, we also lose some municipalities that are affected by regional reforms at the municipality level. Fortunately, such cases are rare in our investigation period and mostly took place in unpopulated areas.

Underestimation of effects is another issue in our analysis of the EVS data, given the timespan between 1988 and 1993. First, EVS data collection takes place throughout the year, which for 1988 could slightly conflict with the fact that the most important treatment variation in terrestrial frequencies of private TV started within that year (see Table D1). This could lead to a weakening of the TV effect on individual behavior in our analysis. Second, in 1993, the treatment of private TV exposure has taken place for several years in some regions, compared to non-treated regions. However, cable and satellite are certainly no rarity anymore in this year, which may also contribute to possible underestimation of effects.

b) *Plausibility of TV signal identification in the EVS data*

We exploit information on the reported number of TV sets. Generally, the number of households without any TV set is marginal (roughly 4% in 1988). Most individuals have one TV in their household. However, in our investigation period, more and more households bought a second TV set, which provides some variation. The results for the most powerful instrument (200kW ERP) as well as for a less powerful version (20kW ERP) are shown in Table C1. We also vary the signal strength threshold from low (50 dBuV/m) to high (65 dBuV/m), similar as in Table B2 for the SOEP data.

Table C1 shows increases in the number of TV sets in treatment regions, suggesting that individuals responded to the availability of private TV via terrestrial frequencies. There seem to be some pre-treatment differences in case of the less powerful instrument with 20kW in Panel A. This could be due to endogeneity but also due to early start of TV broadcast in 1988. The effects are stronger in the bottom Panel B for the most powerful instrument based on minimum frequency power of 200kW. This is in line with the analysis of the SOEP data and further substantiates our preference of this particular instrument. In regard of the signal strength threshold, we now observe (weak) differences in the effect between all threshold levels. While the coefficients are overall similar in all four cases, for our main instrument definition of 200kW power the default threshold 55 dBuV/m has the largest coefficient. This speaks for our default choices. Finally, in additional analyses (not shown), we find that the likelihood of having a second TV in the household increases significantly if we use a binary indicator for having two or more TV sets. A possible interpretation is that family members wanted to have the opportunity to watch different TV programs at the same time and thus bought a second TV set, which then explains increases in TV watching that we observe in the SOEP data.

Table I.C1: Number of TV sets and different definitions of TV signals

PANEL A) 20kW ERP

Signal strength:	50	55	60	65
$\mathbb{1}(1993)$	0.045**	0.045**	0.048**	0.050**
	(0.008)	(0.008)	(0.007)	(0.008)
Private TV	0.044**	0.040**	0.037**	0.037
	(0.016)	(0.017)	(0.019)	(0.024)
$\mathbb{1}(1993) \times \textit{Private TV}$	0.043**	0.049**	0.047**	0.053**
	(0.017)	(0.018)	(0.020)	(0.014)
N	65,584	65,584	65,584	65,584

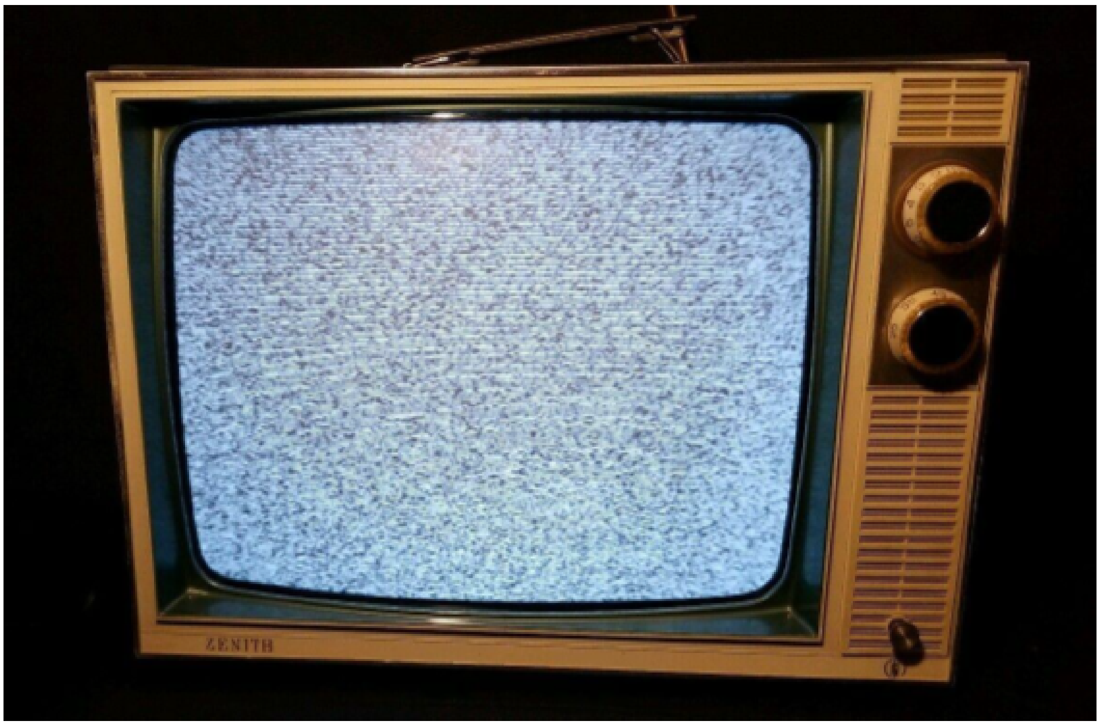
PANEL B) 200kW ERP

Signal strength:	50	55	60	65
$\mathbb{1}(1993)$	0.047**	0.047**	0.048**	0.051**
	(0.007)	(0.007)	(0.007)	(0.007)
Private TV	0.012	0.010	0.006	0.013
	(0.019)	(0.019)	(0.020)	(0.023)
$\mathbb{1}(1993) \times \textit{Private TV}$	0.076**	0.080**	0.079**	0.066**
	(0.015)	(0.015)	(0.016)	(0.014)
N	65,584	65,584	65,584	65,584

Notes: Dependent variable is number of own TV sets. Private TV in Panel A (B) is defined as living in regions with 20kW (200kW) powered TV signals starting in 1988. The baseline model is a difference-in-difference specification showing reduced form estimates. Municipality-level clustered standard errors are in parentheses. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: EVS data are from 1988 and 1993.

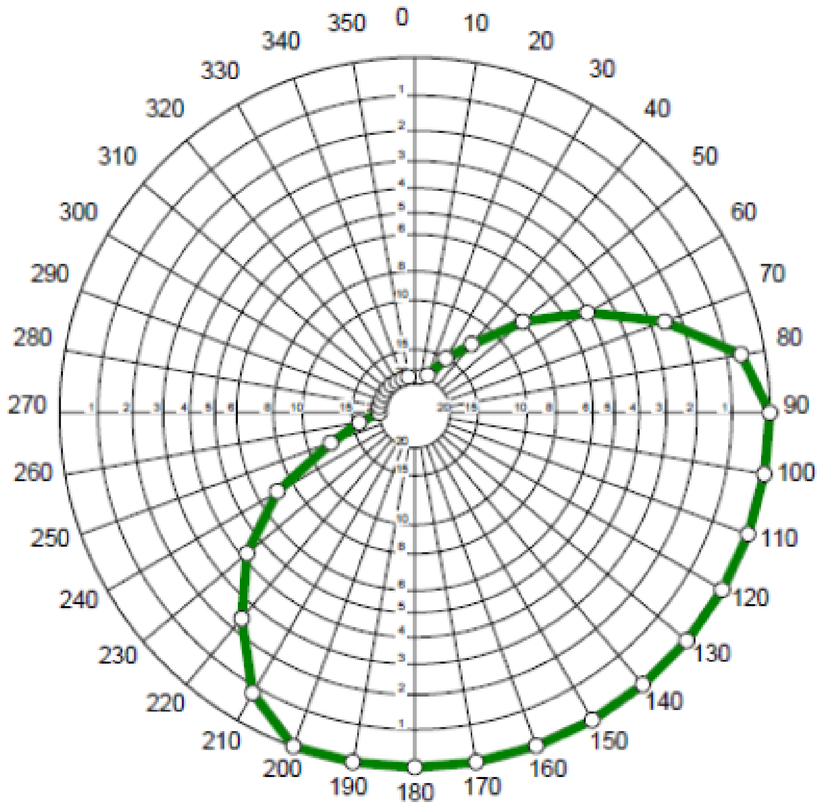
I.4 Appendix D – Additional figures and tables

Figure I.D1: Example TV set from 1988



Notes: This is a 16 inch Zenith TV set. It was one of the options to watch television during the 1980s. The picture is taken from Ebay.com.

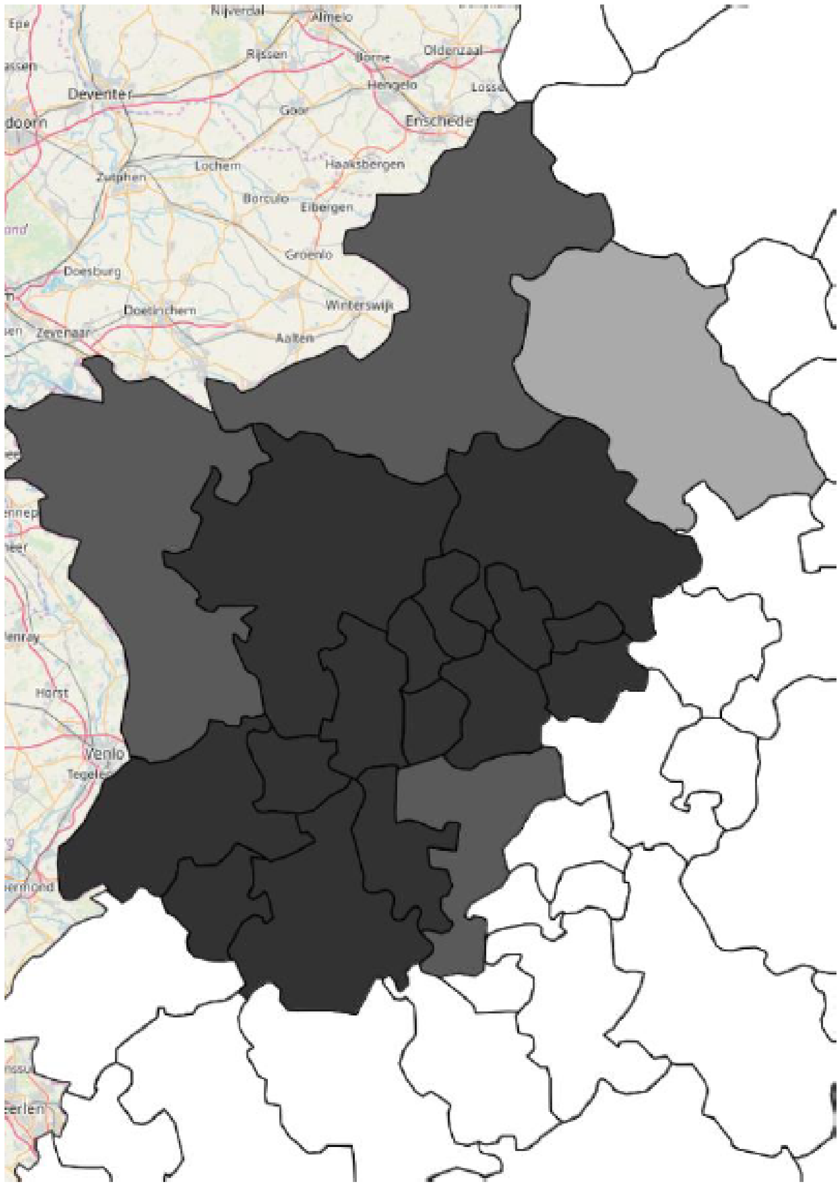
Figure I.D2: Antenna Pattern (Transmitter Wesel)



N ü.O	dBW (A	N ü.O	dBW (A)
0	33.0	180	53.0
10	33.0	190	53.0
20	34.0	200	53.0
30	38.0	210	52.2
40	41.0	220	50.6
50	45.0	230	48.8
60	48.0	240	46.0
70	50.5	250	41.0
80	52.4	260	37.0
90	53.0	270	33.0
100	53.0	280	33.0
110	53.0	290	33.0
120	53.0	300	33.0
130	53.0	310	33.0
140	53.0	320	33.0
150	53.0	330	33.0
160	53.0	340	33.0
170	53.0	350	33.0

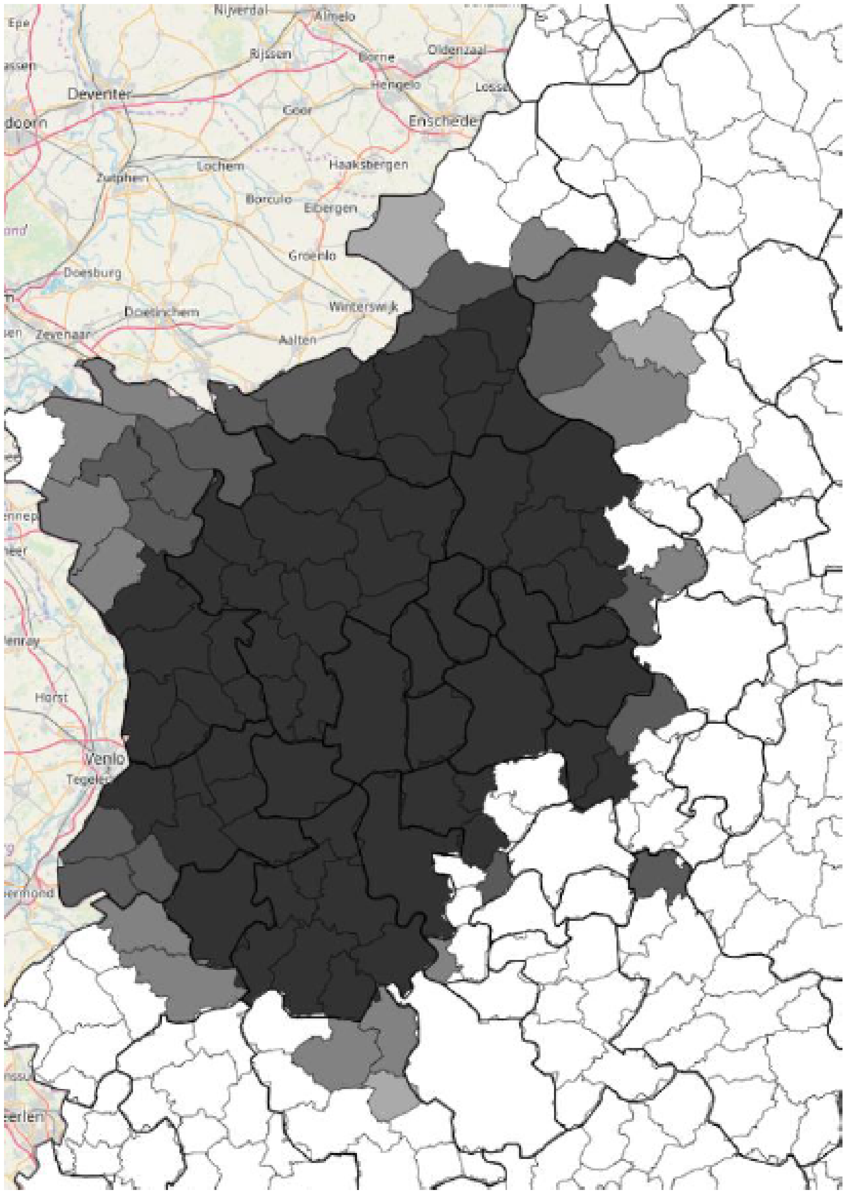
Notes: The illustration shows the antenna pattern for frequency channel 52 (used by *RTLplus* in 1989) from the transmitter station Wesel. The first and third column in table stands for the direction (0 means north, 90 means east, 180 means south, and 270 means west). Second and fourth column information in the table is the power of the signal (aimed at each direction).

Figure I.D3: Terrestrial TV signals of Transmitter Wesel (County Level)



Notes: The map shows the Western regions of NRW and illustrates reception of *RTLplus* via terrestrial frequencies based on the Longley-Rice propagation model.

Figure I.D4: Terrestrial TV signals of Transmitter Wesel (Municipality Level)



Notes: The map shows the Western regions of NRW and illustrates reception of *RTLplus* via terrestrial frequencies based on the Longley-Rice propagation model.

Table I.D1: Transmitter Stations Used by Private TV Channels in 1989 (min. power 10 kW)

Station	Channel	Frequency	Start-Year	Start-Month	Start-Day	Initial SOEP-Year	kW	Initial phase of low power
Wesel	*RTLPlus*	52	1988	7	18	1989	200	
Hennstedt	*RTLPlus*	59	1988	11	7	1989	100	
Hennstedt	*SAT1*	49	1988	11	1	1989	100	
Rosengarten	*SAT1*	52	1988	11	14	1989	80	
Luebeck	*RTLPlus*	36	1988	2	3	1989	34	yes
Dusseldorf	*RTLPlus*	36	1988	6	6	1989	20	
Hamburg	*RTLPlus*	46	1987	10	13	1989	15	yes
Bremen	*SAT1*	29	1989	2	7	1989	10	
Hamburg	*SAT1*	48	1988	3	1	1989	10	yes

Table I.D2: Statistics for main SOEP data sample

	Mean	Std. Dev.	Min	Max
Health satisfaction	6.80	2.37	0	10
Life satisfaction	7.17	1.88	0	10
Female	0.51	0.50	0	1
Age	42.71	16.60	17	95
Household size	3.24	1.51	1	17
Children in the household	0.74	1.04	0	8
Size of dwelling (in m^2)	93.03	37.30	8	340
Owner of dwelling	0.43	0.49	0	1
Main tenant	0.56	0.50	0	1
Other tenant	0.01	0.11	0	1
Years of education	10.60	2.28	7	18
Employment experience	15.65	13.04	0	55.3
Unemployment experience	0.38	1.30	0	38
Married	0.68	0.47	0	1
Care	0.03	0.18	0	1
Log(Income)	7.26	0.46	0	10.14
Time-use variables				
Housework, errands (hours per week)	17.24	16.64	0	130
Child care (hours per week)	8.67	18.61	0	168
Job, commuting (hours per week)	32.32	28.67	0	168
Schooling and training (hours per week)	3.98	12.62	0	135
Repairs, gardening (hours per week)	5.69	7.23	0	92
Watching TV, Video (hours per week)	20.04	10.93	0	112
Hobbies, leisure (hours per week)	11.05	13.25	0	154

Source: SOEP data from 1987 to 1989.

Table I.D3: Statistics for own survey

	Mean	Std. Dev.	Min	Max
Health satisfaction	7.50	2.11	0	10
Life satisfaction	7.91	1.71	0	10
Female	0.56	0.50	0	1
Age	53.89	17.60	18	95
Household size	1.98	0.85	1	5
Living in West Germany (in 1989)	0.67	0.47	0	1
Watching TV (hours per week)	14.07	12.61	0	70
Observations		511		

Source: Own data from 2015.

Table I.D4: Terrestrial TV and long-run health effects (Doctor Visits and Hospital Visits)

Panel A)	Visited A Doctor		Doctor Visits	
	(1)	(2)	(3)	(4)
Private TV	-0.020	-0.017	-0.136	-0.117
	(0.015)	(0.015)	(0.199)	(0.207)
N	52,458	52,458	37,112	37,112

Panel B)	Visited A Hospital		Hospital Visits	
	(1)	(2)	(3)	(4)
Private TV	-0.010	-0.007	-0.035	-0.028
	(0.009)	(0.009)	(0.026)	(0.027)
N	45,536	45,536	45,161	45,161
	Nights in Hospital		**Log(Nights in Hospital)**	
	(5)	(6)	(7)	(8)
Private TV	-0.146	-0.034	-0.030	-0.019
	(0.348)	(0.356)	(0.025)	(0.026)
N	45,476	45,476	45,476	45,476
Control variables	NO	YES	NO	YES

Notes: The independent variable is the 200kW private TV signal. The dependent variables in Panel A are doctoral visits on the extensive margin (visited a doctor) and number of doctor visits in the last three months. No information on doctor visits is available in the SOEP for the year of 1990. The dependent variables in Panel B are hospital visits on the extensive margin (visited a hospital), number of hospital visits, number of hospital nights, and log number of hospital nights in the entire year. No information on doctor visits is available in the SOEP for the years of 1989 and 1992. The baseline model is an individual fixed effects specification with year-fixed effects showing reduced form estimates. The set of control variables contains gender, age, quadratic age, household size, owner of dwelling, main tenant of dwelling, education (in years), and survey month. See Table D2 for descriptive statistics on the control variables used. County-level clustered standard errors are in parentheses. Levels of significance are * p < 0.10, ** p < 0.05, *** p < 0.01.
Source: SOEP data are from 1985 and 1992 are used.

APPENDIX II

VACCINES AT WORK

II.1 Appendix – Tables

Table II.A1: Regression Discontinuity Effects of Higher Price on Vaccination Take-Up

	Baseline	With Controls	Quito Sample	Non-Compliance
Threshold	0.0590	0.1738	0.0655	0.0400
	(0.0730)	(0.1533)	(0.0786)	(0.0722)
N	608	608	461	604

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

Notes: Robust standard errors in parentheses. This table presents the local average treatment effects of a small price change on vaccination take-up. We report the normalized coefficient at a wage of $750 and a bandwidth of $300. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.99. There is no visible discontinuity across the threshold — all specifications control for city fixed effects. Column 1 presents our main estimates without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4, we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. Reducing the bandwidth in steps of $50 to $150 does not change the results.

Table II.A2: Recall Information Statements

	Heard Altruistic Statement	Heard Selfish Statement
Altruistic Information	-1.2079	-8.4337**
	(4.9521)	(4.1692)
Selfish Information	-3.8421	-0.0181
	(4.9557)	(4.0281)
Saturday	-3.5966	-2.5732
	(6.2362)	(5.0237)
Baseline	69.09	76.43
N	377	377

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

Notes: Robust standard errors in parentheses. This table presents the effects of the different treatments on measurements of recalling the altruistic and selfish statements. The post-intervention survey collects these measures on a scale from 0 to 100.

Table II.A3: Heterogeneous Treatment Effects on Vaccination Take-up

	Men	Women	Short Distance	Long Distance	No Children	Children
Altruistic Information	-0.0017	-0.0508	-0.0564	-0.0477	-0.0163	-0.0368
	(0.0452)	(0.0429)	(0.0441)	(0.0521)	(0.0421)	(0.0454)
Selfish Information	0.0098	-0.0166	-0.0074	-0.0291	0.0188	-0.0253
	(0.0439)	(0.0451)	(0.0460)	(0.0527)	(0.0435)	(0.0452)
Saturday	-0.0883**	-0.0677	-0.0825**	-0.1047**	-0.0531	-0.1056**
	(0.0413)	(0.0441)	(0.0420)	(0.0488)	(0.0396)	(0.0453)
N	593	571	446	449	556	608

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

Notes: Robust standard errors in parentheses. This table presents the effect of the different treatments on vaccination take-up for different subgroups in the study's population.

Table II.A4: Robustness Check on Peer Effects Estimates

	Unit Size	Peer Characteristics
A. Main Effect		
Proportion of peers:		
Vaccinated	0.7852***	0.7102***
	(0.1836)	(0.1888)
N	1138	1138
B. Heterogeneous Effects		
Proportion of peers:		
Same Gender Vaccinated	0.7538***	0.7242***
	(0.1979)	(0.2031)
Different Gender Vaccinated	0.4779**	0.4302*
	(0.02408)	(0.2457)

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

Notes: Standard errors clustered at the unit level in parentheses. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 controls for the number of employees in each unit. Column 2 controls for the number of employees in each unit and peers' age and gender.

Table II.A5: Robustness Check on Effects of Vaccination on the Flu

	Narrowest Definition of Flu	Main Definition of Flu	Broadest Definition of Flu
	Reduced Form	Reduced Form	Reduced Form
a. Baseline specification			
Assigned to the workweek	-0.0054	0.0032	-0.0118
	(0.0156)	(0.0160)	(0.0191)
N	1148	1148	1148
b. Additional control variables			
Assigned to the workweek	-0.0051	0.0040	-0.0115
	(0.0157)	(0.0161)	(0.0192)
N	1145	1145	1145

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

Notes: Robust standard errors in parentheses. This table presents robustness checks of the effects of being assigned to the workweek on the probability of being diagnosed sick because of the flu using different definitions of the flu. All specifications control for Quito fixed effects. Panel b additionally considers control variables for the vaccine's price, income, tenure, division in the company, gender, age, and education level, and income.

Table II.A6: Bounds

	Diagnosed with Flu			Diagnosed with Non-flu		
	Main	Lower Bound	Upper Bound	Main	Lower Bound	Upper Bound
Assigned to the workweek	0.0032	0.0050	0.0024	-0.0777**	-0.1028***	-0.0562
	(0.0160)	(0.0161)	(0.0161)	(0.0363)	(0.0379)	(0.0368)
N	913	898	858	913	898	858

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

Notes: Robust standard errors in parentheses. This table presents bounds for the effect of being assigned to the workweek on the probability of being diagnosed with the flu and other non-flu respiratory diseases. All specifications control for Quito fixed effects.

Table II.A7: Heterogeneous Effects on Using an Umbrella

	Baseline	Coefficient	N
Responses on a scale from 1 ("never") to 10 ("all the time")			
	A. Overall		
How often do you carry an umbrella when it rains	6.85	-1.2190**	358
		(0.4856)	
	B. Vaccine Effective		
How often do you carry an umbrella when it rains	7.13	-1.5793***	256
		(0.5651)	
	C. Vaccine Ineffective		
How often do you carry an umbrella when it rains	6.06	-0.2292	102
		(0.9615)	

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

Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effect of being assigned to the workweek on instances of carrying an umbrella and heterogeneity with beliefs of vaccine effectiveness splitting beliefs at the median on a Likert-scale of 8/10. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey.

Table II.A8: Reduced Form Effects on Productivity

	Post-Survey		Swipe-Cards		
	General Productivity	Productivity Post-Intervention	Entry to Work	Exit from Work	Duration at Work
Assigned to the workweek	0.1684	0.1534	-0.1492	-0.4879	-0.3387
	(0.1357)	(0.1718)	(0.1945)	(0.3487)	(0.4004)
N	343	343	403	403	403

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

Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effect of the assignment to the workweek on self-reported measures productivity and duration of the workday. The post-intervention survey collects these self-reported measures on a scale from 0 to 10. The swipe card information corresponds to January and is measured in hours.

II.2 Appendix – Figures

Figure II.A1: Treatment Message: Control

Notes: The above image portrays the email sent to the control group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure II.A2: Treatment Message: Opportunity Cost (Saturday)

Notes: The above image portrays the email sent to the "Saturday" treatment group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Saturday, November 11, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure II.A3: Treatment Message: Altruism

Notes: The above image portrays the email sent to the "Altruistic Treatment" group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. Getting vaccinated yourself also protects people around you, including those who are more vulnerable to severe flu illness, like infants, young children, the elderly and people with dangerous health conditions that cannot get vaccinated If you have questions, please contact _____. Let's get vaccinated!

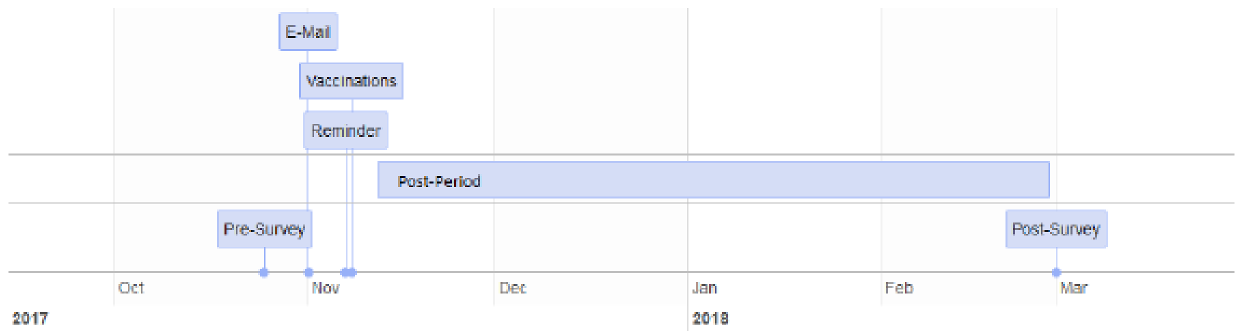
Figure II.A4: Treatment Message: Selfish

Notes: The above image portrays the email sent to the "Selfish Treatment" group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies. If you have questions, please contact _____. Let's get vaccinated!

Figure II.A5: Locations of the Bank in Ecuador

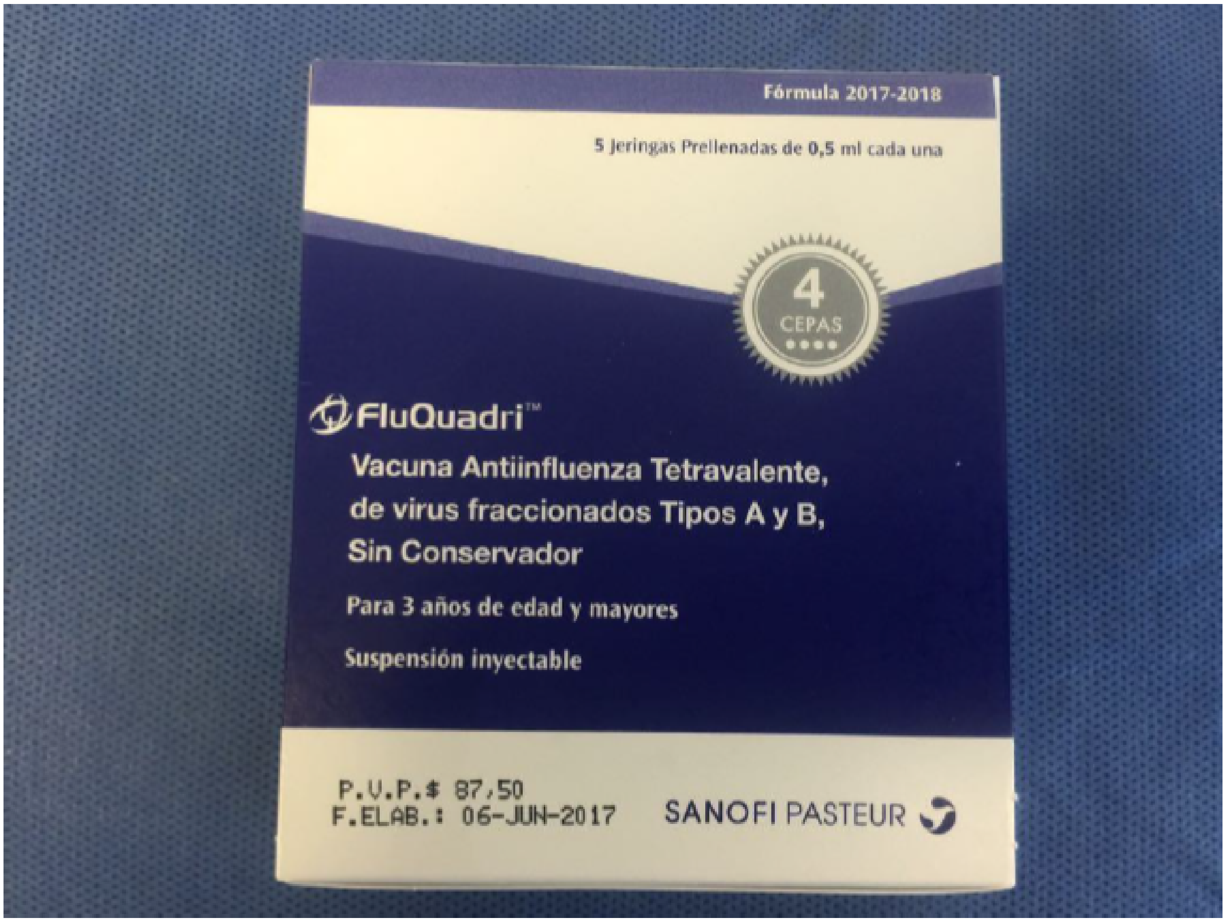
Notes: The map contains the locations of the bank in Ecuador (orange), where we implemented our intervention.

Figure II.A6: Timeline of Experiment Implementation



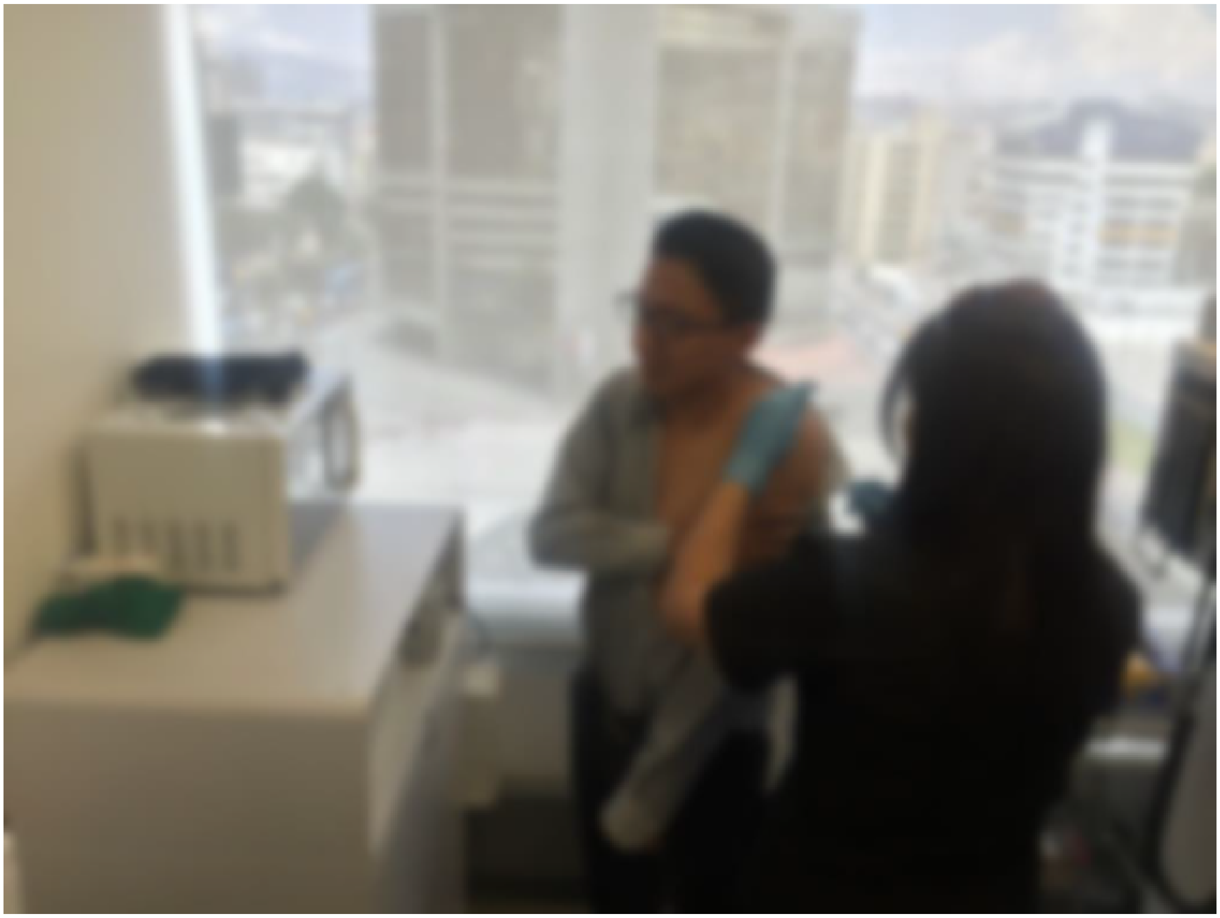
Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources Department mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11. The post-treatment period (Ecuadorian flu season) went from November 13 to March 1. The bank sent the post-intervention survey during March and April 2018.

Figure II.A7: Vaccination Campaign: Influenza Vaccine



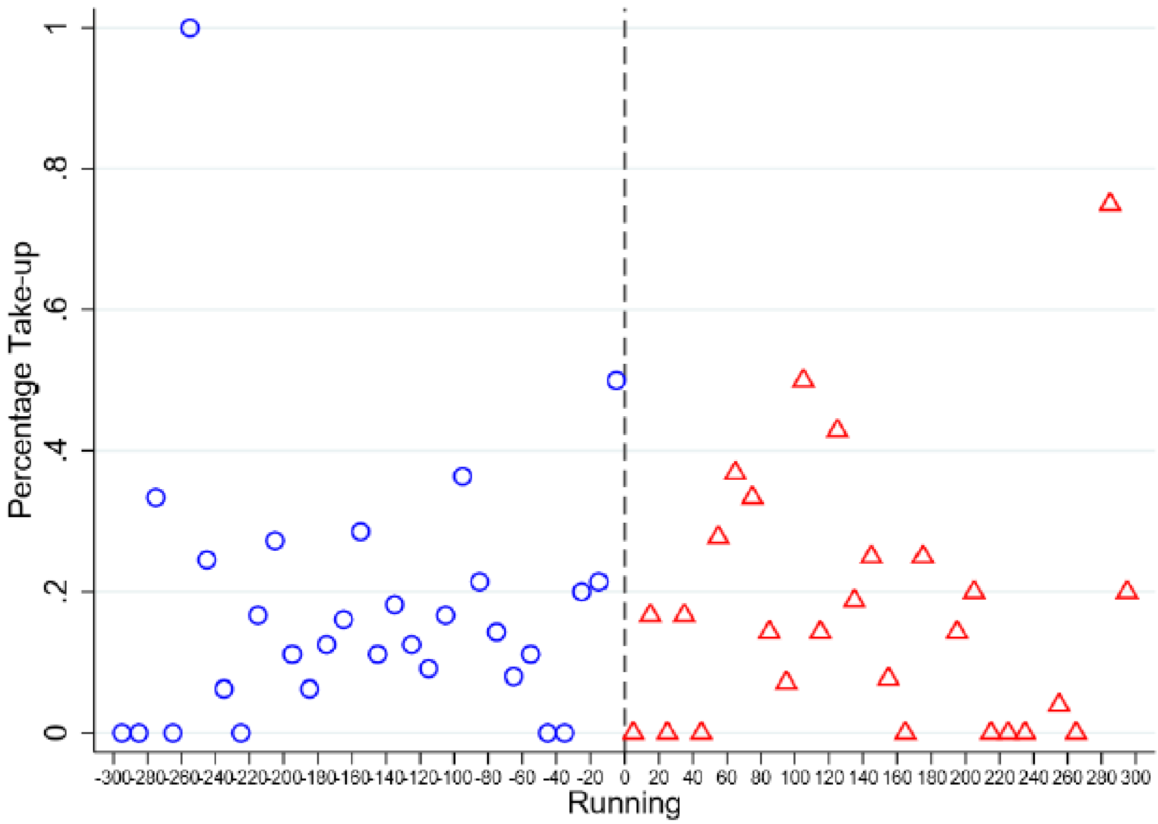
Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure II.A8: Vaccination Campaign: Flu Shot in Action



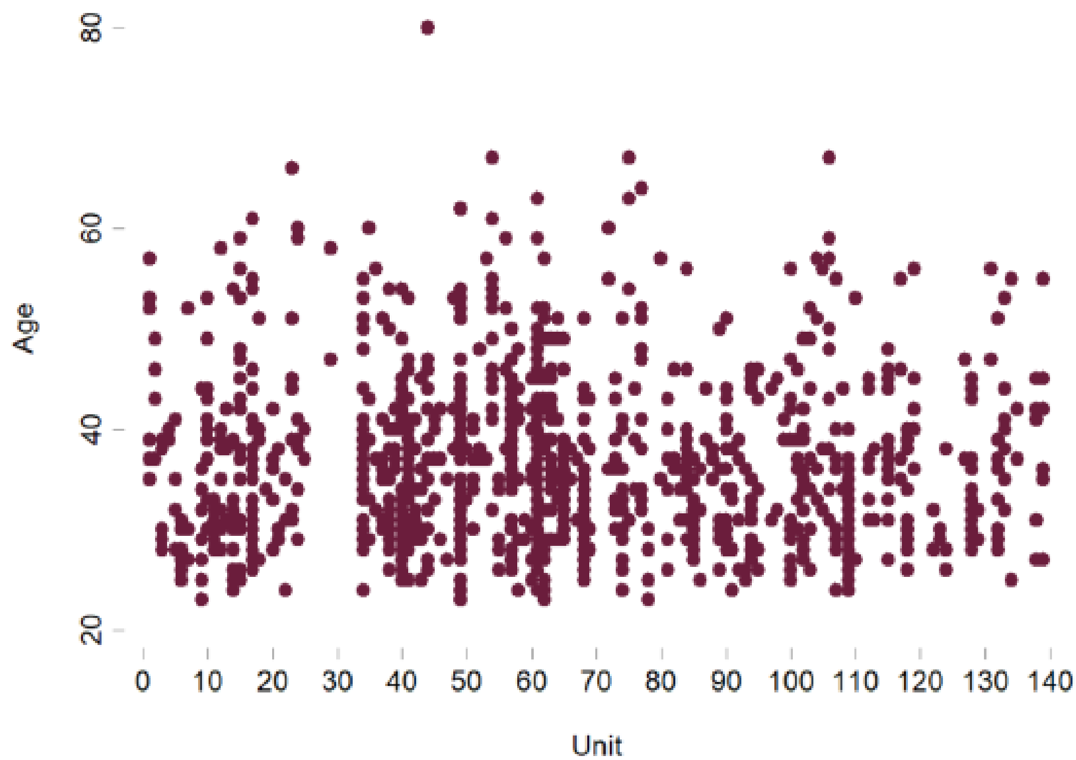
Notes: Immunization at the firm.

Figure II.A9: Vaccination Take-up around $750 Wage Threshold



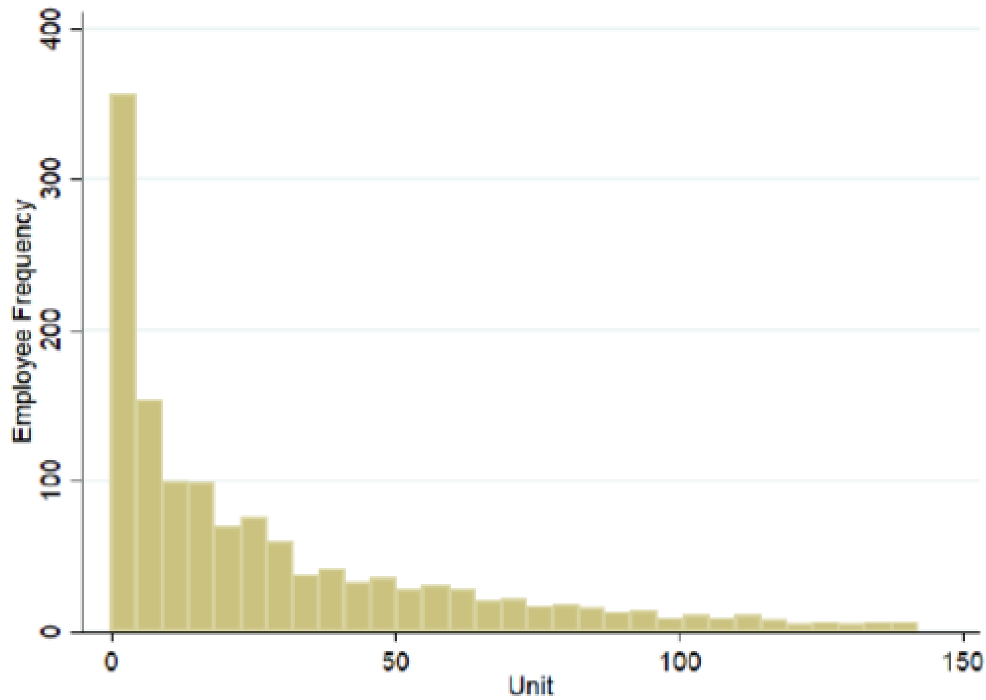
Notes: This figure presents the evolution of vaccine take-up around the $750 threshold with a bin size of $10. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.99.

Figure II.A10: Age Distribution of Employees in Units



Notes: This figure presents the distribution of age within each of the company's working units.

Figure II.A11: Frequency Distribution of Employees in Units

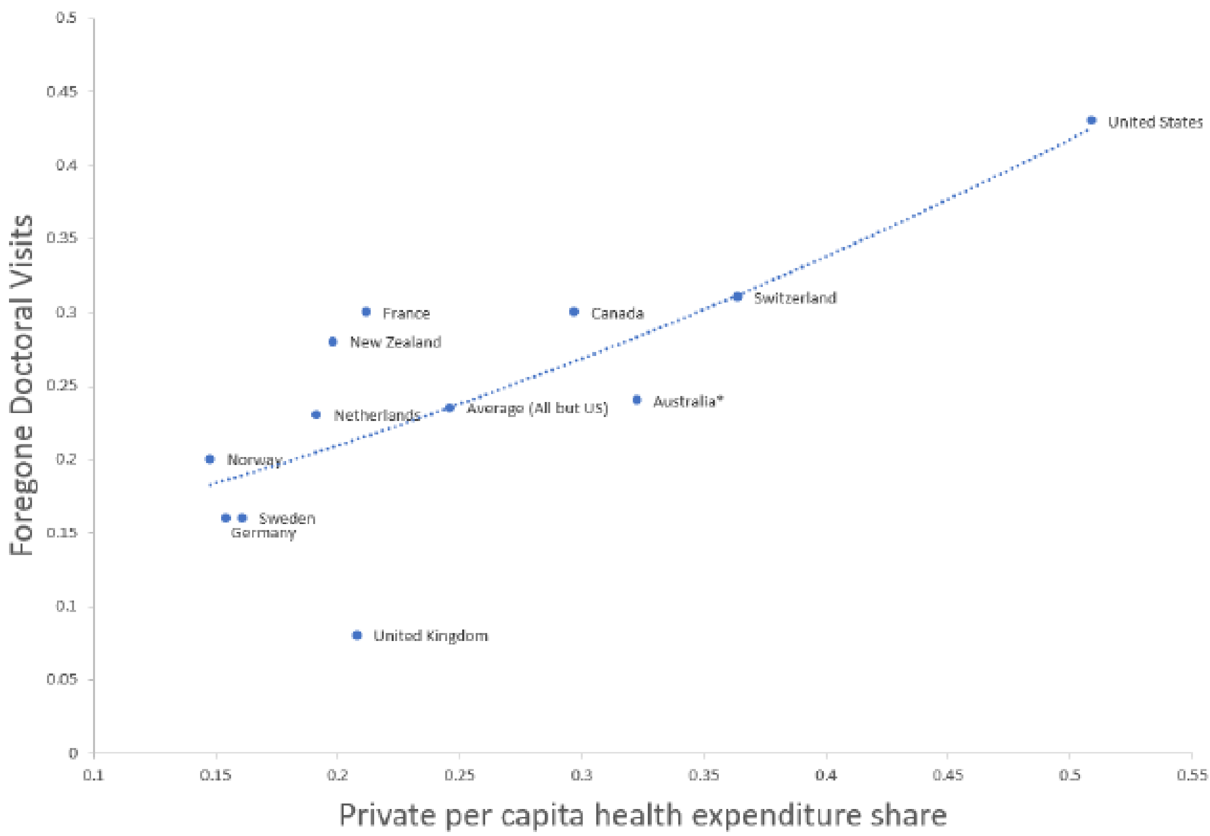


Notes: This figure presents the number of employees in each of the 142 units.

APPENDIX III

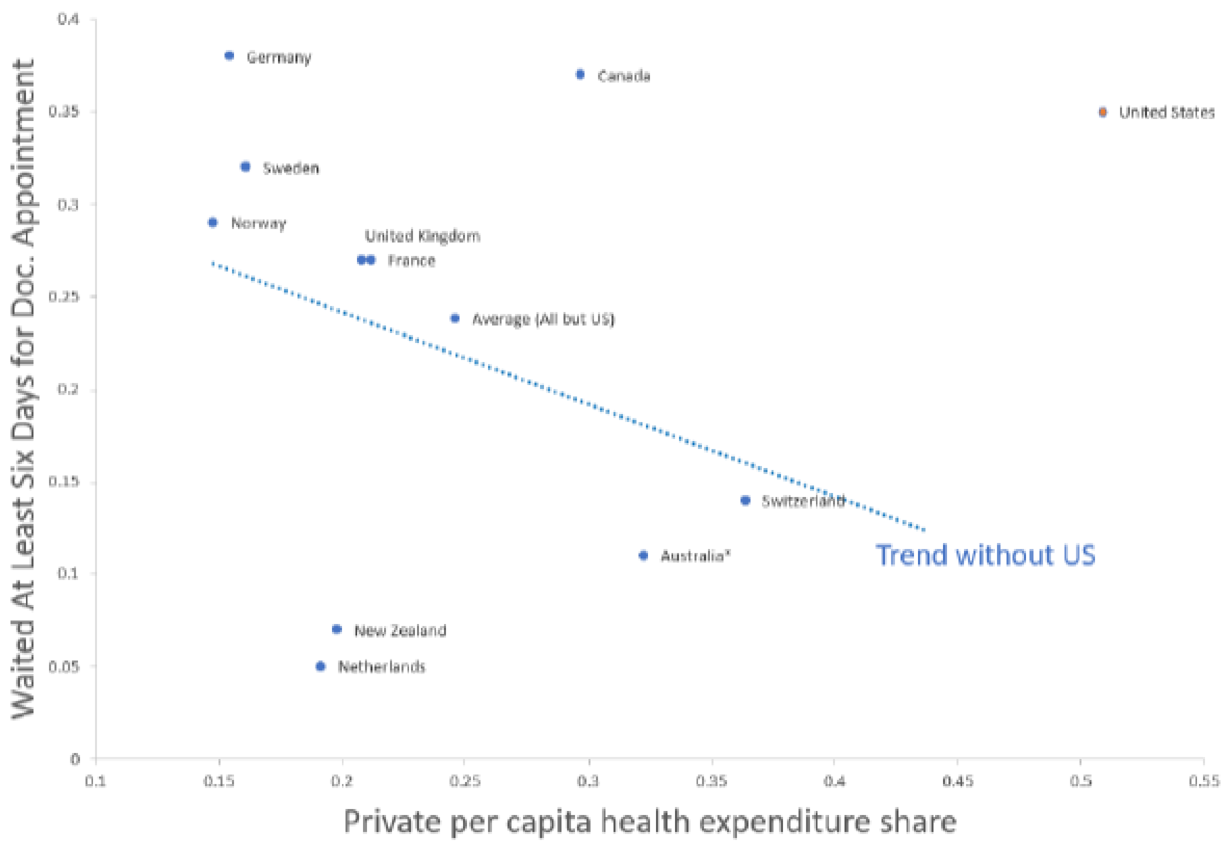
THE UNINTENDED CONSEQUENCES OF HEALTH INSURANCE

Figure III.A1: Foregone Doctoral Visits and Private Provision of Health Care



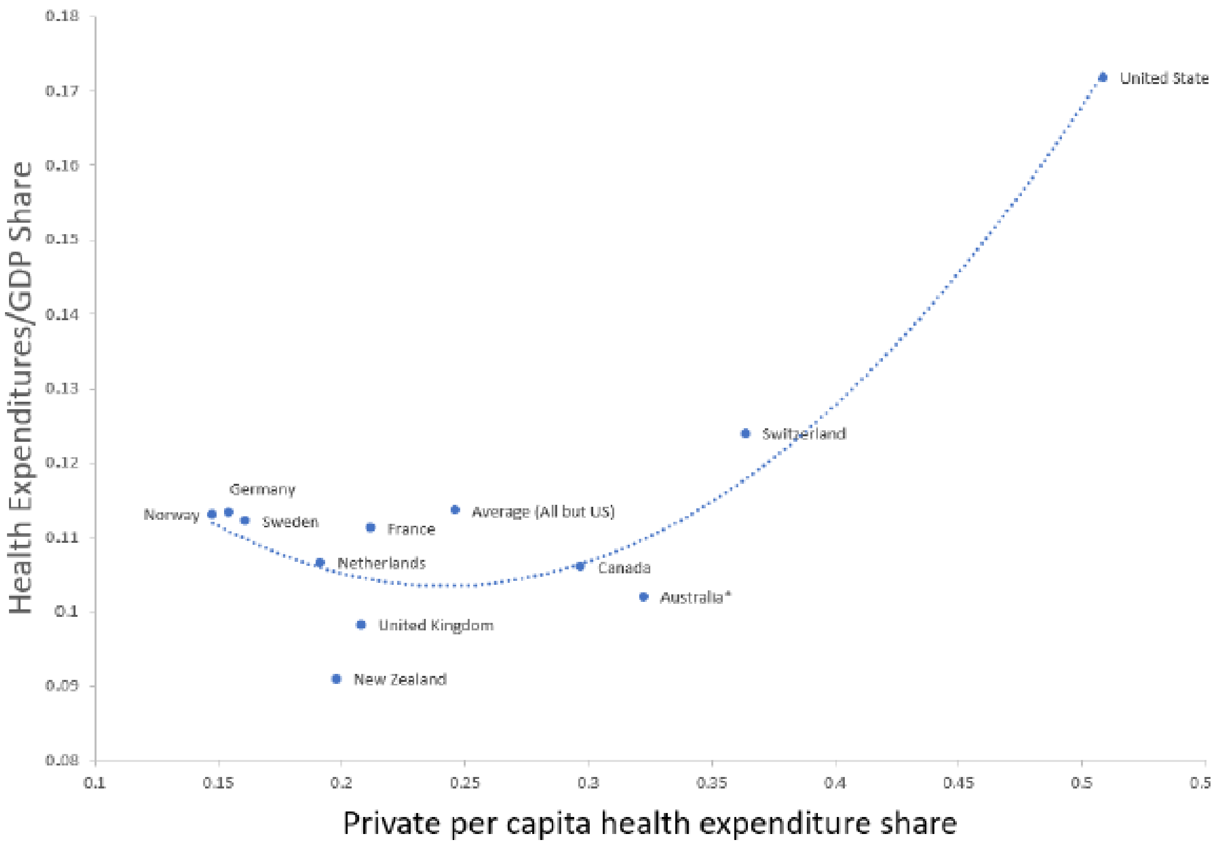
Notes: All countries aside from the United States have systems of universal health care. Overall foregone doctoral visits increase with the share of private health expenditure. Australia has a share of foregone doctoral visits that is below the level that is expected given private expenditures. Source: OECD, WHO, The Commonwealth Fund, 2016.

Figure III.A2: Wait Time for Doctoral Appointments and Private Provision of Health Care



Notes: Wait-times are generally higher with lower private provision of health goods but this trend reverses when private expenditures are too high, as in the case for the United States. Australia has lower wait-times than expected given its share of private expenditures. Source: OECD, WHO, The Commonwealth Fund, 2016.

Figure III.A3: Health Expenditures and Private Provision of Health Care

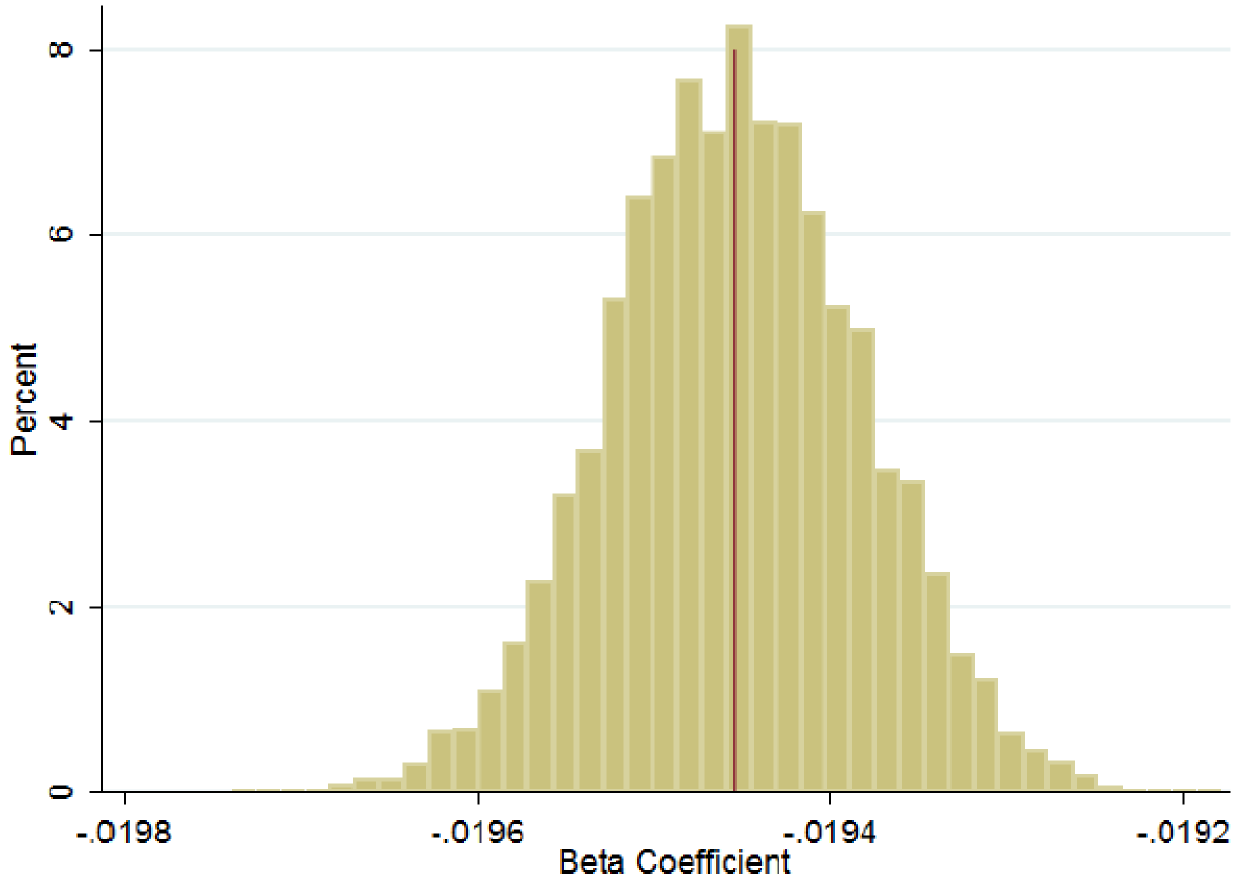


Notes: Health expenditures are relatively high for the only developed nation without universal health care, i.e. the United States. There seems to be an optimal balance between private and public provision of health goods. Australia performs better than predicted. Source: OECD, WHO, The Commonwealth Fund, 2016.

Figure III.A4: Australian Birth Rates over Time

Notes: This figure shows an increasing trend of official birth registered from 1901 to 2017 in Australia. The peak in births around 1970 coincides with the peak visible in the frequency distribution of the Australian Taxation Office. Source: Australian Bureau of Statistics, 2016.

Figure III.A5: Monte Carlo Simulation of Private Health Insurance Beta Coefficients



Notes: This figure displays 10,000 coefficients from jack-knife Monte Carlo simulation where 1% of individuals below the age of 30 are dropped for each run. The coefficient obtained without the simulation is identical to the average coefficient of the distribution which implies that the minor peak in the frequency distribution does not create any association issue for the treatment effects.