Essays in Applied Economics

Inauguraldissertation

zur Erlangung des akademischen Grades
eines Doktors der Wirtschaftswissenschaften
der Universität Mannheim

Vorgelegt von

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Mannheim
2025

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Verteidigung	Mannheim, den 27. Oktober 2025

To Lisa.

Acknowledgements

This thesis would not exist without the tremendous help and guidance from many people. First and foremost, I am indebted to my supervisors, Antonio Ciccone and Markus Frölich. Antonio, your critical thinking, your guidance and the way you ask questions have not only elevated this work to a level it would have never reached without you, but also taught me so much about research and approaching problems in general. I am deeply grateful for the countless discussions that broadened my perspectives on economic research and very often beyond. Markus Frölich, I am deeply grateful for the possibility you gave me to bring up own ideas and develop them in the field together with C4ED, while continuously guiding me whenever it was necessary. Your thoughts and perspectives truly influenced me and the way I work in a very positive way. I am also deeply grateful for the guidance from Alexandra Avdeenko, who provided me so much help in navigating through many difficult situations, believed in me when it all started and has impressed me in so many ways. Moreover, many thanks to Philipp Ager for reviewing this thesis.

I am deeply thankful to all the people at C4ED in Mannheim and Islamabad, who all did more than business as usual to enable the research project discussed in the first chapter to succeed. In particular, without Sharafat Hussain, this research would have never existed. Sharafat, I am impressed by the way you approach and solve problems, by your calmness and the way you lead others. Similarly, I thank Maria Riaz, who has spent incredible efforts on this research and enabled a smooth implementation in the field. I thank Laura Peters, Irene Furlani, Ankhbayar Delgerchuluun, Bibi Asab and Jakob Gärtner for uncountable hours of work and their efforts beyond of what I could have ever expected. In particular Laura and Irene, I am truly grateful for your incredible patience with all my ideas and requests. I thank Mikaela Cochran for all of her help at the start. I thank all other people involved in the project at C4ED, including Muhammad Usama, Ashar Khan, Alexander Prior, Mehwish Hussain and so many more.

I thank all the enumerators who put patience and effort into the project and spent weeks calling and traveling for this project. I am also very thankful for the positive experience in Pakistan, for the warm welcome of the C4ED team, all the help, all the guidance, whether it was related to research or anything else. I will never forget the friendly atmosphere and all the wonderful people in the office and beyond.

I am deeply thankful for the support and work by all people involved from NRSP. In particular, I thank Muhammad Tahir Waqar, without whom this research in Pakistan would have never been possible. Similarly, Muhammad Hussain and Muhammad Saad Iqbal have spent incredible effort on the practical implementation of the project and I am very thankful for that. Mentioning all NRSP employees involved in the fieldwork, including all regional managers, area managers, branch managers, credit officers and so many more would unfortunately go beyond what I can put in these acknowledgments, but I am very thankful to them as well.

For the research project presented in the second chapter, I am deeply indebted to Krzysztof Pytka. Krzysztof, I am truly grateful for the opportunity you gave me to use this data. This project would not exist without your generosity and support, and I will always remember your help.

The research project in the third chapter is based on the support of at least 1,560 employees in German municipalities and counties, from whom I received valuable insights and information. While it would be inappropriate to name each of them individually at this point, I am thankful to every one of them. I also wish to thank Thomas Behles for his advice in setting up the technical communication infrastructure, which was crucial for the project.

I thank all the supportive people I met during my PhD studies. Max M. and Felix K., I am very thankful for all the discussions about research and econometrics, our attempts to start joint projects and everything beyond, both in the office and during dinners. I thank Felix R. and Max R. for discussions on research and politics. I thank Ursula, Ulrich, Max, Marina and Matilde for making the first year enjoyable. I thank Assaf for interesting discussions.

I thank Khadija and Manuel for all the nice memories in the library and beyond, already during the studies. I thank all my friends who brought in aspects beyond economic research while writing this thesis. Thank you all for always allowing me to share my worries and joys with you.

I thank everyone from the university administration who supported me during the PhD. I thank Golareh Khalilpour for always having an open door and being supportive in so many different matters. Similarly, I thank Sylvia Rosenkranz, Ulrich Kehl, Marion Lehnert, Kristina Kadel and Stephanie Lilwall for all of their help and support. I thank Regina Mannsperger who helped me a lot with administrative issues and was always up for a nice chat. I thank Markus Behlau, Yi Liu and Anja Maurer for making my trips to Pakistan possible.

My deepest gratitude goes to my family. To my parents Karin and Jochen, whose strength and love has shaped me. To my siblings, who made this journey so much more enjoyable and in particular to Melissa, for countless coffee-breaks on Friday afternoon.

Most of all, I want to thank my wife and coauthor Lisa. Without your encouragement, your unconditional support and love, this thesis would not exist. Thank you for always being with me, thank you for so many wonderful moments, thank for always reminding me what truly matters.

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Introduction

Global warming will continue to rise in the coming decades under all reasonable scenarios and may even accelerate unless substantial reductions in greenhouse gas emissions occur in the coming decades (IPCC, 2023). The United Nations (2025) has identified climate change as the "defining issue of our time". Encouragingly, mitigating emissions does not require the emergence of new technologies. Numerous low-emission technologies, such as wind and solar energy, electric vehicles and batteries have been available for decades, and their unit costs have steadily declined in recent years (IPCC, 2023). Thus, the central challenge is not technological innovation but the widespread adoption of existing solutions. Even if future inventions prove even more effective, mitigating climate change ultimately hinges on the actions of individuals, firms and policymakers. Their current and future decisions determine whether respective technologies are adopted and behavioral changes occur to decelerate global warming within the 21st century. The three chapters in this thesis investigate these adoption and decision-making processes in three contexts: corporate investment, individual behavior and public policy.

Chapter 1 (co-authored with Alexandra Avdeenko and Markus Förlich) develops, implements, and evaluates a novel encouragement scheme designed to increase investments in renewable energy by small firms and entrepreneurs in rural Pakistan. According to the International Energy Agency (IEA, 2024), investments in renewable energy across emerging and developing economies remain significantly below the levels required to hit national climate and energy targets. A major obstacle to these investments is the risk of volatile electricity production, which poses a particular challenge for liquidity-constrained private actors. One expectedly promising approach to mitigate these risks is to develop "lending models that match more closely to cash flow needs of borrowers" (Banerjee, Karlan and Zinman, 2015). We introduce and evaluate such an innovative lending model that combines a loan for financing solar energy systems with a novel index insurance scheme, designed to protect borrowers against low returns of their solar systems. Specifically, the index insurance scheme reduces the monthly loan repayment amounts when the borrower's location experiences a high number of "cloudy days" – a condition we show to be associated with a significant drop in solar system electricity output. We evaluate this novel insurance scheme through a randomized controlled trial involving 518 entrepreneurs applying for a loan to finance a solar system. We find that the index insurance increases solar system uptake by 27%, reduces monthly electricity spending by 28% and substantially lowers grid electricity usage. Our cost-benefit analysis suggests the insurance to be welfare-improving. Furthermore, we conduct two additional randomized controlled trials with 991 additional entrepreneurs to investigate the mechanisms underlying the observed effects of the index insurance on electricity usage and generation.

Chapter 2 (co-authored with Lisa-Marie Müller) examines individual climate change mitigation behavior following mass protests. We investigate how protests organized by Fridays for Future, a global youth-led movement advocating for climate action since 2018, have influenced meat consumption in US households. Using a panel of consumer data, we estimate the duration of these behavioral shifts. To establish causality, we first demonstrate that the movement spread asymmetrically from Sweden to the US through social networks. Exploiting this asymmetry in a shift-share instrumental variable approach, we assess the impact of US protests on food consumption. Our findings show that up to five weeks following a protest, households with "young" individuals (age 14-25) reduce their meat consumption by 5.45%, whereas households without those young individuals exhibit no significant change in behavior. Meat consumption is chosen as the outcome of interest, because it is a significant contributor to overall greenhouse gas emissions. Furthermore, to estimate the duration of any potential behavioral change caused by protests, one has to consider a decision that is undertaken regularly, which is the case for grocery shopping. Chapter 2 mainly contributes to the literature studying the effects of protests and social movements on behavior (e.g. Madestam et al. (2013), or Branton et al. (2015)). We discuss several potential rationales from the literature around identity and social norms to discuss why it were only the households with young individuals that changed their consumption following a protest.

In Chapter 3, I analyze 270 municipal subsidy programs for plug-in photovoltaic (PV) systems in Germany. Using a novel, hand-collected dataset covering 733 municipalities over more than four years, I causally identify the effects of these programs on the investments in plug-in PV systems. I estimate that the additional installed photovoltaic capacity attributable to the subsidy programs is between 1.20 and 1.41 times as large as the capacity municipalities could have installed directly at the same total cost. Moreover, I also find a robust positive effect of the size of the upfront subsidy payment on the number of additional plug-in PV systems caused by a program, conditional on a fixed subsidy program budget. This suggests that municipalities could have enhanced the effectiveness of their subsidy programs by increasing the upfront subsidy payment without any need for additional budget. I discuss this finding using a simple static theoretical framework to derive potential rationales for the empirical relations.

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Chapter 1

Boosting Climate Mitigation Investments: Weather Insurance for Solar Panels in Pakistan

(joint with Alexandra Avdeenko and Markus Frölich)

1.1 Introduction

According to estimates by the International Energy Agency (IEA, 2024), starting from 2024, investments in renewable energies within emerging and developing economies must increase by more than 220% to fulfill "national climate and energy pledges", and by more than 490% to maintain a "1.5-degree pathway" until the early 2030s. The IEA emphasizes that this "massive scale-up" in investments would primarily need to come from private sources, considering the insufficiency of public funds. However, a significant challenge with renewable energy investments

* This study is registered at AEA RCT registry under AEARCTR-0010648. The necessary ethics approvals have been requested from Research and Development Solutions, Pakistan.
The authors are very grateful for the financial support received from the King Climate Action Initiative (K-CAI) at J-PAL. Moreover, the authors wish to thank www.visualcrossing.com for the provision of weather data for research purposes. David F. Müller gratefully acknowledges financial support by the German Research Foundation (DFG) through CRC TR 224 (project A04).
The authors are particularly grateful for the outstanding work and support from Maria Riaz, Sharafat Hussain, Laura Peters, Bibi Asab, Muhammad Tahir Waqar, Muhammad Hussain, Irene Furlani, Ankhbayar Delgerchuluun, Muhammad Saad Iqbal, Tallat Mahmood, Jakob Gärtner, Mikaela Cochran and the whole team of NRSP Pakistan and C4ED Pakistan.
The authors also thank Antonio Ciccone, Maximilian Mähr, Matthias Meier, Philipp Ager and participants of Seminars in Mannheim for helpful comments.

is the risk of fluctuating electricity production, especially for liquidity-constrained private actors in low- and middle income countries (LMICs). Mitigating these "payment and revenue risks" is crucial for encouraging more private investment in renewable energies (IEA, 2024). One expectedly promising approach to mitigate these risks would be to develop "lending models that match more closely to cash flow needs of borrowers" (Banerjee, Karlan and Zinman 2015). In this chapter, we introduce and evaluate such an innovative lending model that combines a loan for financing solar energy with a novel index insurance scheme aimed at reducing the financial risks associated with solar energy investments for firms and entrepreneurs.

Low uptake rates of solar panels by firms and entrepreneurs in LMICs are both understudied and an essential policy challenge. The limited evidence on solar energy adoption in LMICs indicates that it is difficult to encourage take-up in LMICs. Burgess et al. (2023), for instance, estimate a demand model for electricity in India using data from a randomized controlled trial. They find that the source of electricity does not matter to households. Using an incentivized experiment, Grimm et al. (2020) find that the willingness to pay for off-grid solar systems of households in Rwanda is far below cost-covering prices for theses systems.[1] The findings of these studies indicate that the uptake of solar panels must be incentivized to increase solar energy investments. Furthermore, besides the low willingness to pay for solar energy systems in LMICs at least one additional key challenge exists if the solar investment is financed with a conventional loan, especially for liquidity constrained poor entrepreneurs: While the adopters have to repay a fixed amount every month during the repayment phase, they cannot expect a constant flow of electricity from the panel. This can place them in a difficult position during a cloudy month, when loan repayments are due, solar systems generate little or no electricity, and they must rely on costly grid power or generators to keep their business running.

To address these challenges, we develop and implement an innovative subsidized lending model that reduces the risks associated with solar energy investments financed through a micro-loan. Our key innovation is the automatic reduction of loan repayments in months when the solar panels produce little to no electricity. We implement this subsidized lending model in a randomized controlled trial in rural Pakistan in partnership with NRSP, one of the biggest NGOs and micro-

[1] Similar results are found by Nduka (2023) in Nigeria. Moreover, Meriggi, Bulte and Mobarak (2021) or Rom, Pomeranz and Günther (2024), besides others, find the demand for solar lamps (with a battery) to be strongly decreasing in price.

finance institutions in Pakistan. The study targeted 518 rural entrepreneurs in Punjab. Between February 2023 and January 2024, entrepreneurs could apply for a micro loan to finance a solar energy system, consisting of solar panels and necessary equipment. From the 17 districts in the study area, we randomly selected nine districts in which half of the applicants were randomly offered to receive an index insurance for free, conditional upon taking the loan and installing solar panels. More specifically, under this index insurance, applicants received a payout on their loan account in any month during the loan repayment phase with more than eight cloudy or foggy days.[2] Therewith, the index insurance reduced the loan repayment amount in months in which the solar panels produced little to no electricity due to poor weather.

Up to date, index insurances were applied in the form of rainfall insurances in agricultural settings only, where these insurances aim at insuring crop yields. In the literature, studies find small to modest uptake rates of such rainfall index insurances.[3] Several arguments have been brought up explaining these small to modest uptake rates of index insurances: First, it has been argued that individuals in LMICs have some positive willingness to pay for index insurances, but they cannot and/or do not want to pay the premium upfront (Casaburi and Willis, 2018; Belissa et al., 2019).[4] Second, it has been argued that the concept of insurance itself might be difficult to understand for farmers in LMICs (see, besides others, Cai, De Janvry and Sadoulet (2020)). Third, several authors argue that low uptake rates results from a low correlation between the index on which the insurance rests and the outcome to be insured (see, among others, Giné, Townsend and Vickery (2008)). Besides these arguments, there might be one more reason for the low take-up of index insurances, that has—to the best of our knowledge—not been discussed in the literature yet: In agricultural settings, where these weather insurance schemes were implemented so far, the mapping

[2] This scheme was applied in the first 12 months of the loan repayment period. In the final sample, 78.1% of loan holders chose a loan duration of 12 months.

[3] See experimental evidence on impacts of varying interventions to increase the uptake of agricultural index insurance schemes, including: insurance premium discounts (Cai, De Janvry and Sadoulet, 2020; Cole et al., 2013; Cai, Janvry and Sadoulet, 2015; Hill et al., 2019), social network interventions (Cole et al., 2013; Cai, Janvry and Sadoulet, 2015), financial education programs (Cai, De Janvry and Sadoulet, 2020; Gaurav, Cole and Tobacman, 2011) and informal insurance group bundles (Dercon et al., 2014).

[4] In the studies from Casaburi and Willis (2018) and Belissa et al. (2019), insurance premium payments can be delayed to happen after harvest, i.e., when the risks already materialized. While the delayed repayment possibility in these studies comes with the issue of limited contract enforcement, the higher take-up found in these studies demonstrates that individuals in LMICs have some positive willingness to pay for insurances, but they cannot and/or do not want to pay the premium upfront.

between an index (precipitation amount) and the actual outcome that is aimed to be insured (growth of plants/crops) is rather complex. In fact, the relation between precipitation and crop yields has been shown to be highly non-linear (Schlenker and Roberts 2009; Chmielewski and Potts 1995; Schierhorn et al. 2021), to vary spatially (Propastin, Kappas and Muratova 2008; Kukal and Irmak 2018), and to vary temporarily, because of the impact of precipitation on crop yields depends on the growth phase of the plant (Chen, Wang and Fu, 2020; Teasdale and Cavigelli, 2017)). Moreover, both precipitation frequency and the variance of precipitation amounts within a growth season has been shown to be at least as important for crop yields and vegetation as the total precipitation amount (Fishman 2016; Sloat et al. 2018; Wang et al. 2022).[5] To be clear, it has been argued that the relation between the outcome to be insured and the index on which the insurance rests might be too weak for farmers to take up the index insurance, whereas we argue that the *complexity* of this relation further discourages uptake of index insurances.[6] Given that the complexity between the index and the outcome indeed influences insurance take-up, index insurances may prove more effective for renewable energy investments than for agriculture—despite similar index-outcome correlations—for two reasons: First, for renewable energy investments, the relation between the index (clouds) and the outcome to be insured (solar panel electricity output) is less complex than in agricultural settings. In fact, using data from Punjab, we illustrate that the relation between clouds (the index) and solar panel electricity output (the risk) is almost linear, while it is highly non-linear for agricultural settings (see above).[7] Second, entrepreneurs in our study also perceive the relation of the weather and agricultural yields to be much more complex than the relation of the weather and solar panel electricity output: More specifically, we explicitly asked entrepreneurs in our study to draw the relation between clouds and solar panel output as well as between precipitation and agricultural yields. The descriptive empirical results show that entrepreneurs in our study perceive the relation

[5] This is also true for the province Punjab in Pakistan, our study region: for instance, Abbas and Mayo (2021) study the impact of precipitation on rice production in Punjab and find that variation in precipitation matters as it shifts precipitation to different growth phases of the plant, thereby impacting the yields.

[6] While changing the complexity of the relation between the index and the outcome also directly impact the strength (i.e., the correlation) between the outcome and an index, there can be different levels of complexity for the same level of correlation. To put it differently, for the same level of correlation between the index and the outcome, there can either be a non-complex (e.g. linear) or a complex (e.g. highly non-linear) underlying relation which produces this correlation.

[7] In Appendix 1.D.5, we illustrate the functional form of the relation between the weather and the output of solar energy systems in Pakistan.

between clouds and solar panel electricity output as almost linear, while they perceive the relation between precipitation and agricultural yields to be inversely u-shaped and thereby highly non-linear.[8] Notably, all entrepreneurs included in our study live in rural areas. These rural areas in Punjab are characterized by agriculture, which indicates that entrepreneurs in our study should be well educated about the relation between precipitation and agricultural yields. To summarize, we argue that index insurances can potentially be more successful to insure renewable energy investments than they have been in insuring agricultural outputs, because of different levels of complexities of the index-outcome relation. Besides, an index insurance for renewable energy technologies could also work better than for agricultural technologies, because of its flexibility to insure on a monthly instead of a seasonal basis.

Our results indeed show that the index insurance scheme significantly increases the uptake of solar panels that are financed with a loan. Receiving the insurance against low returns of solar panels throughout the first twelve months of loan repayment period increases the uptake rate by 12 percentage points in our baseline specification, which corresponds to an 27% increase. Moreover, receiving the index insurance decreases monthly electricity spending by 28%, which corresponds to 5.4% of the average profits at time of the endline data collection. Our evidence further shows that this reduction stems from reduced grid electricity usage. Moreover, we find that every USD invested in the intervention generates a present value benefit of USD 3.36 for entrepreneurs, which suggests that the intervention can be self-financing in the long-run. Moreover, we find that for every 100 USD of intervention costs, the intervention causes a reduction of 2.57 tons in CO_2-eq. emissions. Recent estimates about the social cost of carbon (e.g., Barrage and Nordhaus (2024)) show that 2.57 tons of CO_2-eq. emissions would cause a damage of USD 169, thereby also indicating the insurance intervention to be welfare improving.

To understand the mechanisms behind the effects of the insurance on electricity usage and uptake, we conduct two further experiments with 991 additional entrepreneurs taking up loans to finance solar panels. In these additional experiments, we subsidize entrepreneurs with simpler subsidy schemes paying out cash either at the beginning or at the end of the loan repayment period to encourage the uptake of solar panels that are financed with a loan. The findings from these additional experiments suggest that the insurance reduces electricity spending because it nudges *specific* entrepreneurs to take up solar systems. More specifically,

[8] See Appendix 1.A.1 for details.

the insurance encourages those entrepreneurs to take up solar systems who are both able and willing to reduce their grid electricity spending in their business and who would have not taken up solar panels in the absence of an insurance. Using the findings from the additional experiments, we can also exclude alternative explanations for the causal effects of the insurance on electricity spending: First, we demonstrate that the reduction in electricity spending caused by the insurance is neither driven by specific knowledge of the entrepreneurs who receive the insurance, nor by potential differences in the size or the quality of the solar panels.

We also present additional analysis and conduct additional surveys and experiments to highlight the robustness of our findings. As one of these robustness tests, we present evidence rejecting the concern of a low level of understanding of the insurance: First, during the endline data collection, we asked three knowledge questions regarding the details of the insurance scheme. For these three questions, 76%, 87% and 98% of respondents gave the correct answers, respectively. Interestingly, these shares of correct answers includes all respondents, i.e., even those who did not even receive the insurance and for which the education about the insurance was already more than one year ago. Second, after a mistake in the insurance calculation, we received a sequence of complaints from all districts, indicating that those who received the insurance had a very good understanding about how the insurance is calculated. Third, 81.6% of local NRSP staff believe that entrepreneurs understood the insurance well, while none thinks the entrepreneurs lacked understanding entirely.[9] As a further robustness test to our findings, we conduct an additional survey among the solar panel vendors that were contracted by NRSP in this project. This survey provides suggestive evidence that our findings are not biased by potential increase in market prices and are thereby externally valid. Moreover, we also conduct an additional survey experiment to test whether the way we assigned our treatment intervention—which we did by allowing entrepreneurs to take part in a lottery, in which they could "win" the index insurance—biases the treatment effect estimates. Using this survey experiment, we cannot find any evidence for the concern that our treatment assignment process biases the treatment effect estimates. Finally, to test whether our findings are relevant in practice, we conduct an additional survey among NRSP staff, asking them for their educated guesses on the results of our experiment. The descriptive results of this survey among NRSP staff reveal that

[9] See Appendix 1.C for details.

the results of our study are informative to the local staff in the field, rejecting the concern that our findings are trivial and thereby non-relevant to local experts.

Considering these findings, we contribute to the literature studying the relation between index insurances and investments. Studies in this strand of the literature all operate in agricultural settings with farmers, implying that we are the first who study index insurances as an instrument to increase investments in the context of renewable energies. Karlan et al. (2014) and Cole, Giné and Vickery (2017) provide farmers with cash grants and/or vouchers for a rainfall insurance. Both studies find that mitigating investment risks with a rainfall insurance for farmers increases their investments in novel crops. Interestingly, Karlan et al. (2014) find that the cash grant alone did not increase investments, while the insurance increases investments alone as well as in combination with the cash grants.[10] Bulte et al. (2020) show that when farmers receive a rainfall insurance for free conditional upon buying improved seeds, they are more likely to buy these improved seeds compared to a setting without a free insurance. And even if farmers have to pay an insurance premium equal to their willingness to pay for a monsoon insurance in India, Burlig et al. (2024) find that farmers who were randomly offered to buy the insurance increase their overall agricultural investments. Moreover, there is also a small literature studying index insurances that are bundled with loans in agricultural settings: Giné and Yang (2009) conduct an experimental study in Malawi and offer farmers a loan to buy high-yielding crop seeds.[11] Randomly selected farmers were requested to purchase a rainfall insurance if they wanted to take up the loan. The results suggest that those farmers who were required to purchase an insurance when taking up a loan were less likely to take up the loan and invest in the high-yielding technology. The authors rationalize this findings by arguing that farmers are already implicitly insured through the possibility to default on the loan. However, they cannot rule out that farmers might have not sufficient funds to buy an insurance upfront.[12]

[10] Similarly, insuring the unexpected death of sows for farmers in China, Cai et al. (2015) find that the provision of a subsidized insurance increases the production of farmers. The increased production results from farmers being willing to take higher risks by not slaughtering the sow after six months but keeping it fertile to produce more sows.

[11] In theory, bundling index insurances with micro-loans was studied by Farrin and Miranda (2015), Carter, Cheng and Sarris (2016), Shee, Turvey and You (2019) and Syll (2021), besides others. Laboratory experiments in which participants are exposed to investment decisions were index insurances and agricultural loans are bundled are conducted, for instance, by Visser, Jumare and Brick (2020) or Wu and Li (2023).

[12] In fact, Giné and Yang (2009) document a positive correlation between take-up of the insured loan and both income and wealth. They argue that this is in line with the hypothesis that higher loan default costs are associated with larger insurance uptake. They do not find such a positive correlation between take-up and income/wealth for the uninsured loan.

We contribute to this strand of literature by offering an index insurance for free in a bundle with a loan, set in the context of renewable energies with entrepreneurs instead of farmers.[13]

Furthermore, considering our evidence, we contribute to the literature studying cost and benefits of climate policies (Avdeenko and Frölich 2025; Barrage and Nordhaus 2024; Atkinson and Mourato 2015; Bilal and Känzig 2024; Auffhammer 2018; Cai and Lontzek 2019). Using so-called integrated assessment models, studies in this literature aim at evaluating and optimizing climate policies. Naturally, these models must rely on a range of assumptions, one of which is the opportunity costs of the "backstop" technology, the technology that is currently available and under which there would be zero emissions (Barrage and Nordhaus, 2024). As Barrage and Nordhaus (2024) point out, "estimates of the costs of the backstop technology are controversial". As we carefully document costs and benefits of a specific policy for solar panels as well as for solar panels themselves (a backstop technology) in LMICs, our estimates help inform policy makers about expected costs of backstop technologies in LMICs. Thereby, we add to the literature by also quantifying the role of risks of climate mitigation measures on individuals' behavior, which is particularly difficult when it comes to evaluation of climate policies (Atkinson and Mourato 2015; Cai and Lontzek 2019).

Finally, we add new evidence to the literature studying off-grid solar energy adoption in LMICs. Findings from this literature suggest that the willingness to pay for off-grid solar solutions is smaller than the costs (Grimm et al. 2020; Nduka 2023). Moreover, it has been documented that the source of electricity does not matter for individuals (Burgess et al. 2023). Hence, our findings confirm these results by demonstrating that subsidies for solar energy solutions can be used to increase uptake rates of solar energy investments in LMICs.

The rest of this chapter is structured as follows: In Section 1.2, the evaluated program is described. In Section 1.3, the implementation and monitoring activities are discussed. Thereafter, in Section 1.4, we describe the estimation strategy and identification, while in Section 1.5, we present the results. In Section 1.6, we introduce two additional experiments to discuss potential mechanisms for the observed impacts of the index insurance scheme. In Section 1.7, we discuss the

[13] Similar to our bundling strategy, Mishra et al. (2023) conducts a RCT study with farmer groups were some groups of farmers receive a free index insurance once taking up a loan for agricultural inputs. They do not find any effect on secondary outcomes like productive input usage in farming activities.

robustness of our findings presenting additional analyses and evidence. Finally, Section 1.8 concludes.

1.2 Solar-Energy Loan and Index Insurance

The micro-finance loan which builds the basis of our evaluation in this study was initiated by the National Rural Support Programme (NRSP).[14] This specific loan is given to entrepreneurs for the sake of installing solar panels. In practice, the procedure guarantees the loan to be used for solar panels (and their direct necessary equipment) only. More specifically, entrepreneurs apply for a loan for solar panels at NRSP. If the entrepreneur meets the formal criteria for receiving a loan (see below), the entrepreneur contacts a certified solar panel vendor that is under contract with NRSP. The vendor installs the solar panels for the entrepreneur and receives the loan payout directly from NRSP. Loan applicants targeted by NRSP for this loan included self-employed farmers, small businesses, micro-enterprises and home-based small businesses in rural Punjab, a province of Pakistan. To be eligible for a loan, applicants had to be between 25 and 55 years old, meet NRSP's general criteria to receive a loan and go through the general loan approval process of NRSP.[15] For the loans, the minimum loan size was set to PKR 50,000 (corresponds to USD 179.66, in July 2024) and the maximum loan size was set to PKR 400,000 (corresponds to USD 1,437.28, in July 2024). The duration of the repayment phase of the loans evaluated was typically 12 months,

[14] NRSP is a Pakistani Non-Governmental Organization (NGO) that focuses on rural development with a community development approach. The mandate of NRSP is to alleviate poverty by harnessing people's potential and undertake development activities. The NGO is one of the country's largest rural support program in terms of outreach, staff, and development activities. As a reputable NGO in Pakistan involved in micro-finance, NRSP already serves more than 2.5 million poor households across the country. Under its core program, it has granted more than 2 million loans.

[15] More specifically, the conditions for receiving a loan from NRSP in general are: Depending on the loan size, applicants must provide a collateral and two personal guarantees; loan applicants must not be tenants; loan applicants, their spouse and their guarantors must not be in the proscribed lists (AML: Anti-Money Laundering/CFT: Counter Financing of Terrorism); loan applicants must not be a defaulter of any MFI (Microinance Insitution)/MFB (Microfinance Bank) in the last five years as per CIB (Credit Information Bureau) report; the loan installment must not exceed 50% of the disposable monthly income. NRSP processes applications through the following steps: First, there is a social appraisal of client residence, behavior, reputation, income, expenses, liabilities and living conditions. Second, there is a technical appraisal of the house and the enterprise of the applicant.

but could be chosen to be up to 24 months.[16] All loans evaluated in this study were repaid monthly.

Table 1.1: Risk-Cover Intervention Evaluated

Period	Intervention Details	#Applications
Period 1 (02/23-09/23)	In the first 12 months of loan repayment, if there are at least 8 *cloudy days* in a given month, clients receive **33%** of their monthly installment (but at most PKR 7,800) as a payout in that month. Clients can receive at most 4 payouts.	102
Period 2 (10/23-01/24)	In the first 12 months of loan repayment, if there are at least 8 *cloudy days* in a given month, clients receive **66%** of their monthly installment (but at most PKR 7,800) as a payout in that month. Clients can receive at most 4 payouts.	416

Definition Cloudy Day :
A *cloudy day* is a day where at least 50% of the sky is covered in clouds (over the period of 24 hours) and/or the average visibility on the ground over the day is less than 2 miles.

Note: Summary for the Risk-Coverage intervention evaluated in this study. The scheme changed in October 2023. The column (#Applications) shows the number of applications in each period, respectively.

The intervention we evaluate is the index insurance intervention, which is granted to some loan applicants for the described solar energy loan. We call this index insurance scheme the Risk-Coverage scheme, as it covers some of the risks of the solar energy investments for loan applicants. Under the Risk-Coverage scheme, loan holders receive insurance payments in months with many clouds and/or heavy fog. Note that these insurance payments are made directly on the loan account of loan holders, thereby reducing their repayment duty for the loan. In Table 1.1, we describe the details of the Risk-Coverage scheme that changed once during the evaluation period, such that we consider two periods of the intervention. The insurance payout rule is defined by first defining a “Cloudy Day”, which we refer to as a day where at least 50% of the sky is covered in clouds throughout the day and/or the visibility on the ground over the day is less than 2 miles. Hence, a “Cloudy Day” is understood in practice as a day which was either cloudy, or

[16] In the final sample, 78.1% of loan holders chose a loan duration of 12 months. At the start of the project, the maximum repayment duration was 12 months. The extension of the duration was introduced in June 2023, i.e., four months after start of the program. More details are provided in Section 1.3.2.

on which there was heavy fog, or on which both occurred. Loan applicants who were insurance receivers that applied during Period 1 (02/23-09/23) received an insurance payout equal to 33% of their monthly installment (but at most PKR 7,800) directly on their loan account during the first twelve months of loan repayment when within that month, there were at least eight cloudy days. In Period 2 (10/23-01/24) loan applicants who were insurance receivers received 66% (but at most PKR 7,800) during the first twelve months of the loan repayment period in months in which there were at least eight cloudy days.

The Risk-Coverage index insurance scheme was designed jointly with NRSP. In the following, we refer to the "design team" as the team of NRSP headquarter staff and researchers who jointly designed the insurance scheme. The idea was to design a scheme that (1) is relatively easy to understand for loan applicants, and (2) makes insurance payouts in months with low electricity production because of limited sunshine. To design the index insurance scheme, weather data from Visual Crossing (Visual Crossing Corporation, 2025) was used, a weather data provider with global outreach. The weather data contains many different variables, including a *solar energy* measure, which is a measure for the energy emitted from the sun that reaches the earth's surface on a particular day on a particular place.[17] The solar energy that reaches the earth surface is mainly varying due to the season and atmospheric conditions, which include clouds and fog. The insurance rule described in Table 1.1 was determined in two steps: First, the design team used weather data for Germany from Visual Crossing (Visual Crossing Corporation, 2025)—i.e., the same weather data provider as for Pakistan—and merged it to monthly electricity output data from German solar panels. The resulting data was used to verify that the solar energy measure from the weather data provider is indeed a very good approximation for the amount of electricity that a solar panel can produce. Second, the design team constructed a weather dataset for 30 evenly chosen locations in Punjab (the study area), reporting the daily weather for the period 2010 to 2021, also using data from Visual Crossing (Visual Crossing

[17] More specifically, the measure *solar energy* is a measure of the total energy from the sun that builds up over the day on the ground. This measure is constructed in two steps: first, the weather data provider measures the solar radiation (usually once per hour) in a weather station. Solar radiation is a form of electromagnetic radiation including many different wavelengths, covering visible and non-visible parts of the spectrum, such as ultraviolet and near infrared parts. Solar radiation is "the general term for the energy emitted from the sun", it "is measured as the amount of solar radiation per unit area per second" and is reported by the data provider in Watts per meter squared (W/m^2). Second, solar energy is calculated by aggregating the solar radiation that builds up over the day. Source: `https://www.visualcrossing.com/resources/documentation/weather-data/how-to-obtain-solar-radiation-data/` (last access: 18/10/2022).

Corporation, 2025). This data was used (1) to show that the number of cloudy days (as defined in Table 1.1) has a strong impact on the solar energy and is a unpredictable non-seasonal risk that can be insured, as well as (2) to calculate the expected insurance payouts for a prospective loan holder for different insurance rules.[18] The design team then chose the final insurance rule such that the expected payouts from the insurance gave a benefit of approximately USD 60 at the time of the start of the program. The choice of USD 60 was made in accordance with results from a previous cash experiment (Section 1.6) in which we found that a benefit of around USD 80 had very promising encouragement effects on uptake.

1.3 Randomization and Implementation

To describe the implementation of the Risk-Coverage scheme for the experimental evaluation, we will first explain the randomization procedure. Following this, we will discuss the practical implementation of the Risk-Coverage scheme in the field.

1.3.1 Randomization and Baseline Data Collection

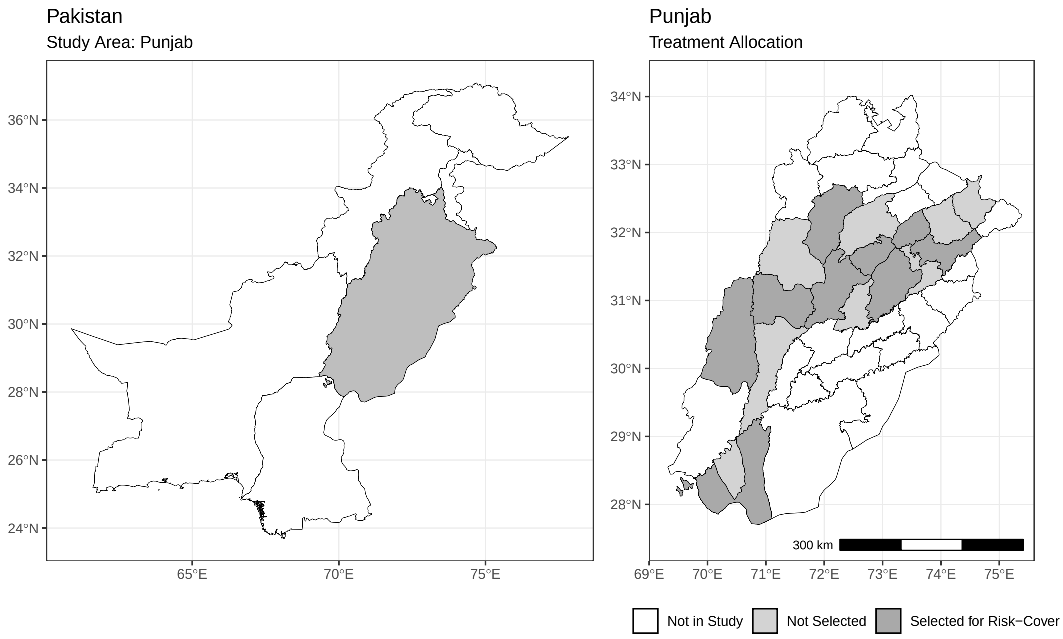
The randomization happened on two levels. We randomized the program at the district level, then at the applicant level.

District-Level Randomization. There are 17 districts in the study area, which were selected by NRSP headquarter to be in principle suitable for the study.[19] According to the population count from 2023, the population across the study area amounts to 57.6 million people, living together in 8.7 million households.[20] From these 17 districts, we randomly choose 9 districts in which the Risk-Coverage intervention was implemented. In the remaining 8 districts, the Risk-Coverage intervention was *not* implemented. The reason for this selection is that we conducted an additional experiment in the non-selected districts in order to identify mechanisms behind the effects of the Risk-Coverage intervention. We

[18] See more details on the steps in Appendix 1.D.
[19] The practical limitation to 17 districts was mainly due to: (1) Security concerns in other districts; (2) smaller numbers of potential clients in other districts; and (3) high costs of training NRSP staff in more than the selected districts about the program.
[20] Source: Pakistan Bureau of Statistics (`https://www.pbs.gov.pk/sites/default/files/population/2023/Punjab.pdf` last access: 25/06/2024).

will come back to this additional experiment in Section 1.6. The randomization at the district level was conducted under the condition that the intervention is maximally dispersed in terms of geography, i.e., such that districts selected for the Risk-Coverage intervention are ideally *no* neighboring districts.[21] In Figure 1.1, the Risk-Coverage study districts are displayed. The impact of Risk-Coverage intervention on loan uptake are tested on the applicant level, where a further experimental variation was introduced, as explained in the following.

Figure 1.1: Implementation Area for Risk-Coverage Intervention



Note: Punjab (grey) in Pakistan (left hand side) and selected districts for Risk-Coverage intervention (right hand side).

Applicant-Level Randomization. The second level of randomization was conducted during the loan application process among loan applicants. When applying for a solar-energy loan in the selected districts for the Risk-Coverage intervention, applicants could decide to take part in a lottery in which they could win the described index insurance scheme. Even though the participation in the lottery was voluntarily, all solar energy loan applicants during the study period in the selected districts decided to take part in this lottery, which is not surprising given the offered benefit in case of winning. The probability of winning

[21] The implementer (i.e., NRSP) was practically limited to 17 districts and also recommended to select the intervention areas at the district level instead of the branch level to avoid implementation mistakes. Different branch managers within one district work very closely together and district managers coordinate these branches. A coordination of different branches within one district seemed very challenging for NRSP, which resulted in the decision of the researchers to randomize on the larger district level. See Appendix 1.G.1 for more details.

this lottery, i.e., to be eligible to receive the insurance, was 50%, which was clearly and transparently explained to loan applicants. The randomization of loan applicants within the selected Risk-Coverage districts into the treatment group (lottery winners) and into the control group (lottery losers) was conducted in weekly batches. More specifically, within each week, i.e., within each batch of applicants, half of the applicants were randomly chosen to “win the lottery”. The randomization of each loan applicant, respectively, was conducted after NRSP confirmed the eligibility of the respective loan applicant for the loan, after the baseline survey was conducted with the respective loan applicant, and before the respective loan applicant knew about their eligibility for the loan.[22] Hence, in practice, after applying, a loan applicant was—usually one week after his/her application—told (1) whether he/she was eligible for the loan and/or (2) whether he/she won the lottery. After this information about loan eligibility and the lottery outcome, each respective applicant could then decide whether to take up the loan or not.[23] It is important to notice that the randomization of loan applicants was conducted such that the likelihood of balance in the covariates between treatment and control applicants was maximized.[24] After we reached the target sample size of 518 eligible entrepreneurs who applied for this loan and took part in the lottery, the characteristics measured at baseline of the 255 winners (treated entrepreneurs) are on average comparable to the characteristics of the 263 losers (control entrepreneurs).[25]

[22] The baseline survey was conducted via phone on a rolling basis, whenever there was a new loan applicant.

[23] More specifically, credit officers entered a new application into the system and conducted the social and technical appraisal. Then, the respective branch manager reviewed the application and decided whether a loan applicant was eligible or not. After that, NRSP transmitted eligible applications to the system of the research team. The research team forwarded the necessary details of the loan applicants to the enumerators on the same day, who then immediately started with the baseline survey. Every Wednesday during the program implementation, the research team randomly assigned the treatment status to all applicants that applied since the last round of randomization and that finished their baseline survey. The baseline survey for a respective applicant was also considered as “finished” when this respective applicant refused to take part in the baseline survey. Even if an applicant refused to take part in the survey, he/she was still considered eligible for the lottery and therefore the treatment intervention. The results of the randomization was then entered through a virtual interface into the management information system of NRSP. Afterwards, the area manager finally decided about the eligibility of the loan applicant. This decision of the area manager is usually a formality. If the area manager decided that the applicant was eligible for the loan, the loan applicant received an automatic SMS message including her eligibility for the loan and the result of the lottery for her. Then, the loan applicant could decide to take up the loan or not, irrespective of the lottery result.

[24] See Appendix 1.G.2 for details.

[25] See Appendix 1.E.2 for the balance tables.

Table 1.2: Descriptive Statistics for Loan Applicants in Risk-Coverage Districts

Variables	Mean	SD	Min	Max	N
Electricity spending typical month (USD)	36.41	46.92	2.16	251.52	469
Indicator: business connected to elect grid	0.90	0.29	0.00	1.00	479
Indicator: respondent owns generator	0.06	0.24	0.00	1.00	479
Number of electric machines/tools in business	4.57	3.12	0.00	17.00	443
Profit from business last month (USD)	218.56	181.90	39.52	988.12	472
Total typical monthly household income (USD)	524.94	362.93	125.76	1796.58	479
Savings typical month (USD)	104.16	91.02	0.00	431.18	473
Number of power-cuts per week	6.87	6.22	0.00	28.00	476
Total hours devoted to business per week	67.22	25.73	14.00	112.00	475
Number of people in household	7.92	3.76	3.00	20.00	479

Note: Descriptive statistics for each entrepreneur who is part of the study in the Risk-Coverage districts. The variables (except for the indicator variables) were winsorized at the 2.- and 98.-percentiles, respectively for this table. Columns show averages (mean), standard-deviations (sd), minimum (min) and maximum (max) values as well as the number of observations for each variable. Monetary values are shown in USD (converted at exchange rate from July 2024).

In Table 1.2, we present descriptive statistics on the characteristics and business activities of all entrepreneurs who applied for the Risk-Coverage lottery in our sample. Note that 90% of all entrepreneurs are connected to the grid and 6% own a generator. On average, the entrepreneurs own 4.57 tools/machines for their business that require electricity and experience on average 6.87 power cuts per week, indicating potential demand for electricity sources besides the grid to run the business. While they make USD 218 profits per month on average with their business activities by working 67 hours per week for their business on average, their total household income is much larger on average, which is natural considering that, on average, 7.92 individuals live together in one household, where not everyone is involved in the business. Moreover, besides these characteristics discussed in Table 1.2, it should be noted that entrepreneurs operate their business in various different fields: 46.72% of entrepreneurs have their primary business in the trade of manufactured goods and/or wholesale, 14.86% conduct daily services such as transportation or laundry services, 11.58% are engaged in agriculture (i.e., farming) and the rest are engaged in various different activities, which are mainly part of the service sector (Appendix 1.A.2). Note that only 5.98% of entrepreneurs engage in manufacturing, such as the production of clothes.

1.3.2 Implementation, Monitoring and Adjustments

After the design of the insurance we started to conduct training activities with the field staff of NRSP in collaboration with the headquarter of NRSP. The nine districts in which the Risk-Coverage intervention was implemented consist of 43 smaller organizational units of NRSP, which are called branches and where each of these units is managed by a branch manager. Within each branch, several credit officers are responsible for loan applications. Training sessions were conducted with all 43 branch managers in one-day workshops in February 2023 at three different locations across the 9 study districts. The field staff of NRSP was trained about the details of the loan and about the Risk-Coverage scheme.[26] Moreover, we also conducted enumerator training sessions for the baseline interviews in mid February. In March 2023, the first applications for the loan were registered. Printed leaflets were used to advertise the loan. In addition to these leaflets, NRSP staff also used their local community meetings to advertise the loan and the Risk-Coverage scheme. Figure 1.2 illustrates the implementation activities on a timeline.

In the first months of the loan application period, we registered only very few and in some weeks no applications. As a response to the low number of applications, we conducted two field visits. Each of these visits was followed by adjustments to the loan and the Risk-Coverage intervention: During the *first* visit, in May and June 2023, we retrained branch managers and credit officers about the loan and the Risk-Coverage intervention and provided them with additional advertisement material. Moreover, we talked to all branch managers about problems and potential improvements of the intervention and the loan, and attended sales conversations with clients. The key result of this visit was that in all but one district, the field staff of NRSP mentioned that the initial maximum loan size of PKR 200,000 and the initial maximum loan duration of 12 months was a major reason for the missing number of applications, considering the high inflation at that time.[27] As a response, NRSP increased the maximum loan size to PKR 400,000 and the maximum duration to 24 months.[28] We refer to these adjustments of the loan conditions as "loan adjustments" in the following. After we did still not see a substantial increase in the number of loan applications after these implemented changes, we did the *second* field visit starting at the end of August 2023 until mid September 2023. During this visit, we conducted eight focus

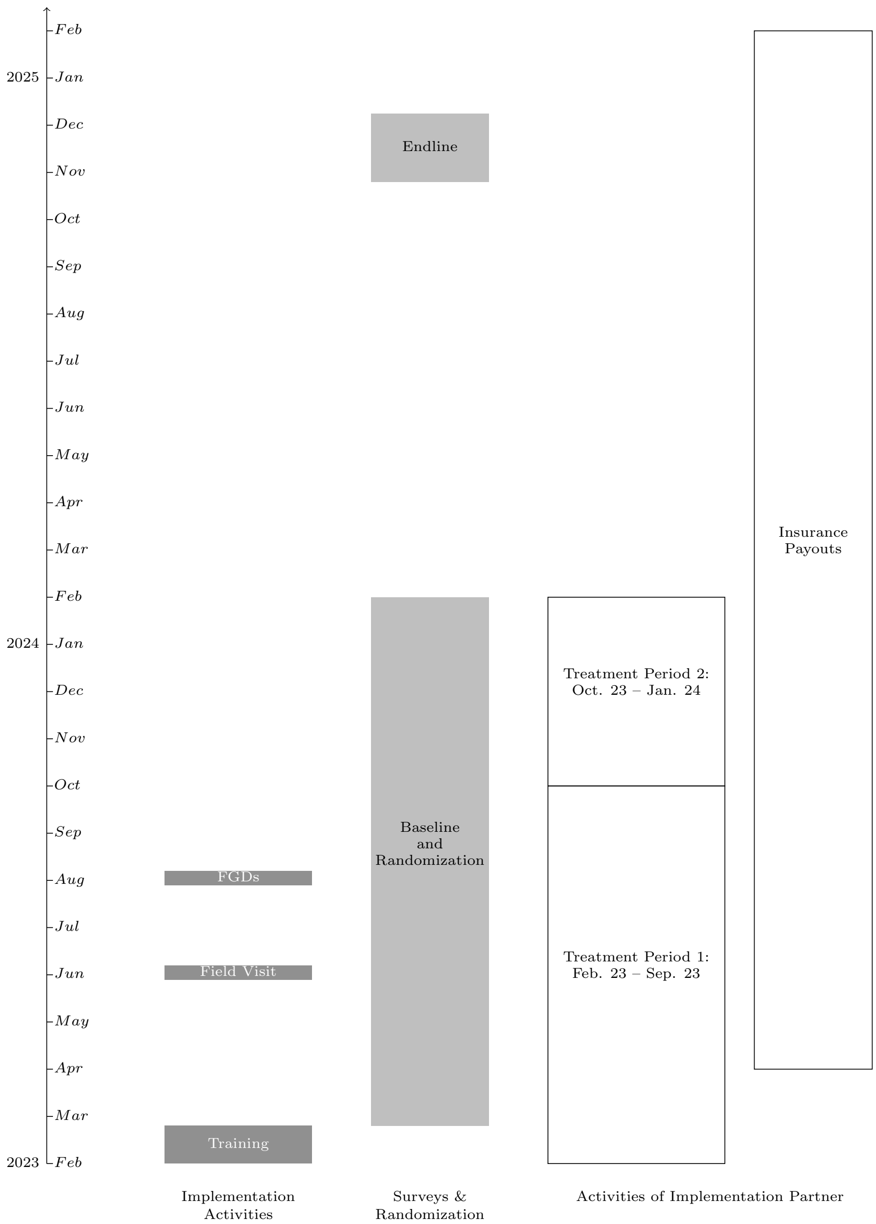
[26] In total, there were three training sessions in three different locations. The training sessions consisted of workshops about the details of the new loan product and the Risk-Coverage intervention, group discussions about the product and the Risk-Coverage intervention, marketing strategies for the new product and written tests about the content of the training. The training sessions were conducted in Urdu. Training materials are available upon request.

[27] More results of the field visit are provided in Appendix 1.H.1.

[28] Moreover, branch managers also expressed the need of further advertisement material. To meet these needs, the research team printed advertisement banners and sent one banner to each branch office and also sent a digital version of it to conduct advertisement on social media.

group discussions (FGDs) across the whole study area, each conducted with approximately 30 clients of NRSP and additional community members.[29] The main result of this FGDs was that the economic environment including the high inflation—which resulted in high prices for solar panels and a high interest rates for the loan—were very challenging and a major obstacle for investments.[30]

Figure 1.2: Activities Over Time

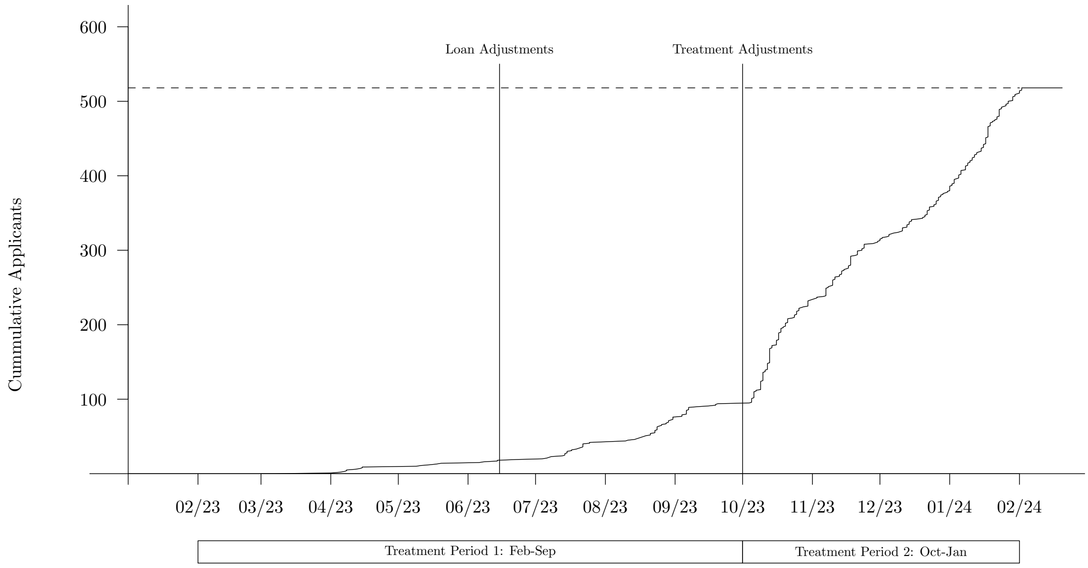


Note: The figure shows the different activities: "Implementation Activities" and "Surveys & Randomization" were conducted by the research team and "Activities of the Implementing Partner", were conducted by NRSP. FGDs stands for Focus Group Discussions.

[29] The 17 districts are each assigned to a region. There are four regions in the study area and we conducted two FGDs in each region.
[30] See more details on further results of the focus group discussions in Appendix 1.H.2.

Considering the results of the second field visit and after careful discussions with NRSP staff both in the headquarter and in the field, we decided to double the Risk-Coverage insurance payouts for all individuals who applied after 1st of October (Table 1.1). We call this adjustment the "treatment adjustments". The treatment adjustments divide the study period into two periods: For applicants applying in Period 1, one insurance payout was equal to 33% of a monthly installment, but at most PKR 7,800. In contrast, for applicants applying in Period 2, one insurance payout was equal to 66% of a monthly installment, but at most PKR 7,800. Thus, given the constant maximum insurance payout of PKR 7,800, the treatment adjustments were specifically beneficial for smaller loan sizes. After the treatment adjustments, we were able to attract more loan applications until the end of January 2024. Figure 1.3 illustrates the cumulative number of applications over time. As Figure 1.3 illustrates, after the treatment adjustments, we experienced a significant boost in the number of applications per week.

Figure 1.3: Cumulative Number of Risk-Coverage Applications



Note: Loan adjustments refer to the doubling of the maximum loan size and duration. Treatment adjustments refer to the difference between the periods: In Period 1, each insurance payout was equal to 33% of a monthly installments, but at most PKR 7,800. In Period 2, the intervention details changed such that each insurance payout was equal to 66% of a monthly installment, but at most PKR 7,800 (Table 1.1 for details).

1.4 Estimation Strategy and Identification

In order to measure the impact of the treatment, i.e., of receiving the insurance, on outcomes of interest, we consider the following regression for entrepreneur i, being randomized in batch b, located in district d:

$$Y_{ibd} = \beta_0 + \beta_1 D_i + \mathbf{X}_i \boldsymbol{\beta}_3 + \eta_b + \delta_d + \varepsilon_i \tag{1.1}$$

We denote Y_{ibd} as the outcome of interest and D_i takes the value one if i won the lottery, i.e., was entitled to received the insurance conditional upon taking up the loan, and zero otherwise. $\mathbf{X}_i$ includes all baseline controls, i.e., all variables that are listed in Appendix 1.E.2. To avoid a drop in sample size, we replace missing values in control variables included in $\mathbf{X}_i$ by the average value within each district.[31] The term η_b corresponds to randomization batch fixed effects, where randomization batches refer to the groups in which individuals were jointly randomized (Section 1.3.1). Thus, the randomization batch fixed effects also account for differences between treatment periods (Table 1.1 for details). Lastly, δ_d corresponds to district fixed effects, accounting for potential differences across districts in the practical advertisement and implementation strategies of NRSP.

To identify treatment effect heterogeneity, we implement the algorithm of Chernozhukov et al. (2025). The algorithm of Chernozhukov et al. (2025) works with any machine learning method and estimates features of the conditional average treatment effects (CATEs). These features include, besides others: (1) the average treatment effect (ATE); and (2) a heterogeneity parameter (HET), which can be used to test if there is any treatment effect heterogeneity.[32]

1.5 Results

In the following, we will first discuss the estimated effects of the Risk-Coverage index insurance scheme on the primary outcome (Section 1.5.1). The primary outcome of the Risk-Coverage intervention is the uptake of the loan. Second, we discuss the estimated effects of the Risk-Coverage index insurance scheme on further secondary outcomes (Section 1.5.2). Third, we discuss and present additional results regarding the practical relevance of our findings (Section 1.5.3).

[31] See Appendix 1.A.3 for details.

[32] Other estimated features are the sorted group average treatment effects (GATES), which are obtained by sorting the sample by the size of the estimated individual treatment effects, splitting the sample into quintiles and then calculating the average treatment effects within each quintile. These GATES can be used to: compare the treatment effects of the 20% most and 20% least affected individuals, and to conduct a classification analysis (CLAN), in which the average characteristics of the most and least affected individuals are compared. Moreover, the algorithm also estimates goodness-of-fit measures (denoted as Λ and $\bar{\Lambda}$) to identify the best performing machine learning algorithm to estimate the respective features.

1.5.1 Primary Outcome

Remember that upon taking the loan, the disbursement process ensures the loan is used to purchase a solar system, as the loan is paid out directly to a contracted solar panel vendor. Thus, loan uptake is equivalent to solar panel uptake.[33] Table 1.3 presents the results for the estimated effects of the insurance eligibility on uptake. In the specification including fixed effects for batches and districts, winning the lottery and thereby being entitled to receive the index insurance increases the take-up rate from 44.5% to 56.5%, i.e. by 27% (12 percentage points). Thus, winning the lottery and thereby being entitled to receive the Risk-Coverage insurance has a significant and sizable effect on the uptake of solar panels.

Table 1.3: Effects of Risk-Coverage Intervention on Uptake of the Micro-Loan

	(1) b/t	(2) b/t	(3) b/t
Entitled for insurance	0.09**	0.11***	0.12***
	2.00	2.76	2.83
N	497	497	497
Batch FE	✓	✓	✓
District FE		✓	✓
Baseline Controls			✓
Mean Control	0.445	0.445	0.445
SD Control	0.498	0.498	0.498

Note: Results for specification 1.1, where the dependent variable is a dummy variable being one if the individual took up the loan and thereby financed a solar panel and zero otherwise. SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Moreover, we ran the algorithm by Chernozhukov et al. (2025) to detect heterogeneities in the effect of the insurance on the uptake behavior. The HET values are not significantly different from zero, which makes us conclude that we cannot identify significant treatment effect heterogeneity in loan uptake behavior among entrepreneurs (Appendix 1.B.5).

[33] At time of endline, 98% of entrepreneurs who received a solar energy loan in the Risk-Coverage districts were able to show the solar energy system to enumerators. The remaining entrepreneurs reported that they sold the solar panels to someone else in the meantime.

1.5.2 Secondary Outcomes

After considering the uptake of solar panels, we now turn to the secondary outcomes following their adoption, specifically considering outcomes measuring the business activities of entrepreneurs.

Electricity Usage. The most direct outcome following the adoption of solar panels concern the usage of electricity. Table 1.4 presents the results of specification (1.1), i.e., it presents the intention-to-treat effects of the Risk-Coverage index insurance scheme on three different outcomes related to electricity usage. First, in Columns (1) to (3), we examine the impact of the index insurance on overall monthly electricity spending. The estimated effects are similar across these specifications. In Column (2), which includes all fixed effects, being assigned to receive the insurance reduces monthly electricity spending by PKR 2532.6—an approximate 28% decrease relative to the control group's average spending. This reduction is economically relevant, as it corresponds to 5.4% of average monthly profits as recorded in the endline survey. Second, Columns (4) to (6) focus on grid electricity usage, measured in kilowatt-hours (kWh) over the last month. Note that the sample size for grid electricity usage is smaller than that for overall electricity spending, which may partly explain the lower significance levels. This drop in sample size emerges from missing values for the grid electricity usage outcome, which is not reported by all entrepreneurs. In Column (5), which incorporates all fixed effects, the assignment to the insurance leads to a decrease of 22.75 kWh, amounting to a 14.4% reduction compared to the control group average. It is important to note that overall electricity spending can include additional costs, such as fuel for generators, potentially accounting for the differences in relative effects between overall electricity spending and grid electricity usage. Moreover, both the pricing and taxing scheme of grid electricity is non-linear in Pakistan, which can also explain this difference in relative effects among the two outcomes. Third, in Columns (7) to (9), we assess the impact of the insurance on the frequency of intentional grid shutdowns conducted to reduce electricity spending. To measure this intentional grid shutdowns, entrepreneurs were asked whether they had ever turned off grid electricity to save money and, if so, how frequently this occurred over the past six months, which we then consider as the number of intentional grid shutdowns. The results in Column (9) suggest that solar panels are likely being used to replace expensive grid electricity with cheaper solar energy. Overall, the results in Table 1.4 indicate that the insurance scheme significantly reduces conventional energy usage among entrepreneurs, consistent with their initial motivations at time of the baseline survey: In the baseline survey, 97.3% of entrepreneurs cited the cost savings associated with solar panels as a key reason for their loan application, while 64.5% noted that solar panels provide more stable electricity than the grid (Appendix 1.A.2). Moreover, we ran the algorithm by Chernozhukov et al. (2025) to detect heterogeneities in the effect of the index insurance on the on the three outcomes related to electricity usage analyzed in Table 1.4. The HET values are not significantly different from zero, which makes us conclude that we cannot identify significant treatment effect heterogeneity in electricity usage outcomes among entrepreneurs (Appendix 1.B.5).

Further Secondary Outcomes We examine several additional secondary outcomes related to the business activities, attitudes, and household conditions of the entrepreneurs. The results of the effects of the insurance scheme on these outcomes are presented in Appendix 1.B.2. First, we find no significant effects of the insurance on business profits, profit variability, business revenue, or business expenses. Similarly, we detect no significant impacts of the insurance on downstream outcomes such as business investments, household income, or household savings. Although it might appear puzzling that the insurance affects electricity spending but not other business indicators, it is important to note that the statistical power to detect changes in total expenses, revenues, and profits

Table 1.4: Effects of Risk-Coverage Intervention on Electricity Outcomes

	Electricity Spending (PKR)			Grid Usage (kWh)			Num. of Intentional Shutdowns		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-2276.77**	-2475.51**	-2424.96**	-22.35*	-20.58	-19.03	4.99	5.32	5.78*
	-2.36	-2.40	-2.38	-1.65	-1.50	-1.48	1.44	1.64	1.74
N	422	422	422	356	356	356	480	480	480
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	9003.6	9003.6	9003.6	158.1	158.1	158.1	16.48	16.48	16.48
SD Control	11127.4	11127.4	11127.4	132.0	132.0	132.0	36.67	36.67	36.67

Note: Results for specification (1.1), where the respective dependent variables are: Electricity spending in the last month measured in PKR (Columns (1) to (3); electricity usage from the grid in the last month measured in kWh (Columns (4) to (6)); and the number of intentional grid shutdowns in the last six months that are conducted with the aim to reduce grid electricity spending (Columns (7) to (9)). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

is inherently lower, given the larger variance in these measures.[34] Second, we also consider outcomes related to the attitudes and knowledge of entrepreneurs regarding solar panels and climate change. Our findings indicate no significant effect of the insurance on the proportion of entrepreneurs who believe that solar panels can replace a generator, nor on their overall knowledge about climate change. However, we do observe a statistically significant, albeit small, effect (less than one percentage point) on the share of individuals who have ever heard the term "climate change" and/or "climate crisis".[35]

1.5.3 Results on Practical Relevance

After discussing the secondary outcomes, in this subsection, we investigate the practical relevance of our findings through: (1) a cost-benefit calculation indicating the returns of investments for the intervention from the perspective of a social planner; and (2) an additional survey among NRSP field staff to assess the practical implications of the findings in the field.

Cost-Benefit Calculations Given the previous findings, the benefits of the Risk-Coverage intervention materialize along three distinct dimensions. First, additional solar systems are installed that would not have been installed in the absence of the Risk-Coverage intervention. Second, entrepreneurs use less grid electricity. Third, entrepreneurs spend less on electricity overall. These outcomes correspond to three distinct benefit-cost ratios, which we discuss in the following and which are calculated in Table 1.5. First, from the perspective of entrepreneurs' financial gains, we relate the total reduction in electricity spending over the average expected lifetime of the solar panels to the cost of the intervention. This yields a benefit-cost ratio of 3.36, meaning that every USD invested in the intervention generates a present value benefit of USD 3.36 for participating entrepreneurs over the panels' average expected lifespan. This implies the insurance to be an efficient policy, as the overall benefits created are larger than the costs. It further implies that a well-designed financing mechanism could potentially recover the intervention's costs, making it self-financing in the long run. Second, from the perspective of climate change mitigation, we relate the reduction in grid electricity usage—and the implied reduction in greenhouse

[34] Since we do not have a direct measure of household consumption, we are unable to test whether the reduction in electricity spending is directly allocated to consumption.
[35] Due to the absence of significant effects on the secondary outcomes, we do not explore heterogeneity in these effects as we do for the primary outcomes.

Table 1.5: Cost-Benefit Calculation for Risk-Coverage Intervention

COSTS		
Total insurance payments		PKR 2,795,454
Training costs of staff about insurance		PKR 217,250
Overhead costs (10% of total payments)		PKR 279,545
	Sum:	PKR 3,292,249 (USD 12,623)
BENEFITS		
	Per Unit:	*Whole Sample:*
Reduction in electricity spending		
Per month (Estimates see Table 1.4, Column 2)	PKR 2532.58	PKR 645,808
Per lifetime of solar panel (11.22 years, present value consideration)	PKR 43,346	11,053,230 PKR
Reduction in grid electricity		
Per month (Estimates see Table 1.4, Column 5)	22.75 kWh	5801 kWh
⇒ Implied greenhouse gas reduction:	9.4kg CO_2-eq.	2.4t CO_2-eq.
Per lifetime of solar panel (11.22 years)	3063 kWh	781,047 kWh
⇒ Implied greenhouse gas reduction:	1.27t CO_2-eq.	324.1t CO_2-eq.
Additional solar systems		
Number of additional systems		28
Additional capacity installed	2.1kWp	61.1kWp
BENEFIT-COST RATIOS		
Lifetime reduction in electricity spending divided by total program costs		3.36
Reduction in tons of CO_2-eq. emissions per 100 USD		2.57
Additional kWp capacity installed per 100 USD		0.48

Note: The conversion from PKR to USD is done with a conversion rate of 0.00372, which is the conversion rate as of February 1st, 2023, when the insurance started. The number of additional solar systems is calculated by considering the uptake rate of solar systems in the control group, which is 44.5%. As there are 255 entrepreneurs who received the Risk-Coverage treatment, the number of installed systems in the absence of the treatment would have been 44.5% of 255, which is 113 systems. As 141 solar systems were installed in the treatment group, it follows that 28 (141-113) additional systems were installed through the intervention. The additional capacity is estimated by considering the average capacity (measured in kilo-watt peak, abbreviated as kWp) for a solar system among the Risk-Coverage entrepreneurs who have installed such a system in our sample, which is 2.183 kWp. The expected lifetime of a solar system corresponds to the average expectation of entrepreneurs in the Risk-Coverage intervention about the lifetime of a solar panel at time of endline, which is 11.22 years. The average greenhouse emissions for one kWh of electricity from the electricity grid in Pakistan is 415 gram CO_2 equivalent, (which is abbreviated as CO_2-eq.). This average of 415 CO_2-eq. is derived from Figure 8 in the paper from Umer et al. (2024), who estimate the CO_2-eq. emissions for electricity from the grid in Pakistan in 2023. To calculate the present value of the overall electricity spending reduction for the entrepreneurs over the lifetime of a solar panel, we take several steps. First, we use the average expected lifetime of a solar panel of 11.22 years. Second, during the baseline interview, we used a questionnaire module developed by Falk et al. (2023) to estimate the discount rate of entrepreneurs. Using this module, we find a average patience parameter of 5.45 of entrepreneurs in the Risk-Coverage districts. This patience parameter implies that a person is approximately indifferent between receiving PKR 400 today and PKR 790 in 12 months. Thus, the *monthly* discount rate of an average entrepreneur, denoted as r_{monthly}, is defined through the equivalence $400(1 + r_{\text{monthly}})^{12} = 790$, which implies that $r_{\text{monthly}} \approx 5.84\%$. Third, using this discount rate of 5.84%, an average lifetime expectancy of 135 months (11.22 years) and the monthly reduction in electricity spending, we calculate the net present value of this overall reduction.

gas emissions—to the program cost. Our estimates show that for every 100 USD spent, the Risk-Coverage intervention reduces emissions across the average expected lifespan of the installed solar systems by 2.57 tons of CO_2-eq. emissions. For comparison, Barrage and Nordhaus (2024) estimate the social cost of carbon in their baseline scenario at USD 66 per ton of CO_2-eq. emissions. This implies that emissions of 2.57 tons of CO_2-eq. are equivalent to a economic damage of USD 169 in terms of discounted consumption. Thus, as our program reduces

CO_2-eq. emissions by 2.57 for USD 100, it is beneficial from an economic point of view. Third, we calculate the cost-efficiency in terms of solar capacity installed. For every 100 USD spent, the program leads to the installation of 0.48 kWp of additional solar capacity.

Practical Relevance of Findings for Implementing Agency. To assess whether the key findings of our evaluation constitute novel and relevant information for the implementing agency, we conduct an additional survey. If the results were already known or could have been accurately anticipated by field staff, the experiment would be of limited additional value—rendering the experimental approach redundant. To examine this concern, we conducted a targeted survey among the branch managers, who received direct training on the Risk-Coverage intervention from our field team and were tasked with promoting the loan and training the credit managers. Prior to presenting the branch managers with any evaluation results, we surveyed them using an online tool. Within the districts selected for the Risk-Coverage intervention, we successfully surveyed 41 out of 43 branch managers, achieving a response rate of 95.5%. Two key sets of results emerge from the branch manager survey. First, branch managers overestimate the effectiveness of the insurance.[36] While our evaluation finds that the insurance increases solar panel uptake by 12 percentage points, branch managers expect an average increase of 29.7 percentage points—more than double the actual effect. Specifically, they slightly underestimate the baseline uptake rate without insurance (39.1% estimated by branch managers vs. 44.5% actual uptake rate), but substantially overestimate uptake with insurance (68.9% estimated by branch managers vs. 56.5% actual uptake rate). Second, branch managers appear to misjudge the motivations of entrepreneurs applying for solar panel loans. While 97.3% of surveyed entrepreneurs cited reducing electricity spending as a motivation, branch managers estimated this share to be just 22.7%. Similarly, 69.7% of entrepreneurs reported that reducing outages was a reason for their application, while branch managers estimated this at only 17.8%. These findings demonstrate that key insights from the evaluation—particularly regarding program effectiveness and client motivations—are novel to local staff.

[36] Detailed results of the branch manager survey are presented in Appendix 1.C.

1.6 Mechanisms

In this section, we ask why the insurance "works" in increasing uptake of solar panels and reducing electricity spending and usage and consider several potential mechanisms. To address this question, we implemented two additional experiments, which will be described in more detail first. We then summarize the insights gained from these additional experiments and discuss how they relate to the results of the main Risk-Coverage intervention. In particular, we will focus on whether the observed effects are driven by the characteristics and set up of the Risk-Coverage insurance itself or by the specific characteristics of the entrepreneurs who applied for the loan to finance the solar panel.

1.6.1 Experimental Settings

The first additional experiment, referred to as the "pilot experiment" was conducted between December 2021 and June 2022.[37] Hence, this experiment was both started and ended several months before the Risk-Coverage intervention started. The second additional experiment, referred to as the "subsidy experiment", was implemented in parallel to the Risk-Coverage intervention (i.e., at the same time) in the eight districts that were *not* selected for the Risk-Coverage scheme (Section 1.3.1). Both experiments were conducted in collaboration with our implementing partner NRSP.[38]

Pilot Experiment. The pilot experiment took place in the district of Sargodha, which is one of the districts within the study area (Section 1.3.1) that was not selected for the Risk-Coverage intervention. Within this district, we randomly selected nine (out of 18 available) branches for our pilot experiment. Between December 2021 and June 2022, within these selected branches, NRSP offered a loan product that could exclusively be used for the installation of solar systems by small- and medium sized businesses, i.e., by the same type of entrepreneurs who are the target group in the Risk-Coverage experiment. The conditions of the loan product in this pilot experiment were the same as the conditions of the loan evaluated in the main intervention (Section 1.2 for details). Within the nine

[37] Registered at the AEA RCT registry under AEARCTR-0010648. Ethical approvals were obtained from Research and Development Solutions, Pakistan.
[38] This experiment was jointly registered at the AEA RCT registry with the Risk-Coverage intervention. Ethical approvals were obtained from Research and Development Solutions, Pakistan.

selected branches, we received 509 applications that were eligible for receiving this loan. We randomly selected 250 of them to be eligible to receive a so-called "cash treatment". This cash treatment was worth USD 86 and entitled the treated entrepreneur to receive USD 86 at the end of the loan repayment period directly on her loan account. Thus, if the entrepreneur was assigned to be treated and decided to take up the loan, NRSP repaid the last USD 86 that she would have to repay herself in the absence of the treatment. Regardless of their treatment status, eligible loan applicants could decide whether to take up the loan after they learned their treatment status. NRSP carefully explained to the applicants that they are put in a lottery in which they are either assigned to treatment or control after their loan eligibility is confirmed. No baseline survey was conducted in the pilot experiment, but the same endline survey used in the Risk-Coverage intervention was administered.

Subsidy Experiment. The Subsidy experiment was conducted between February 2023 and January 2024 alongside the Risk-Coverage intervention. The basis for the Subsidy experiment was the same solar energy loan that was also the basis for the Risk-Coverage intervention.[39] For this loan, we introduced a further intervention called "Subsidy" intervention scheme in the eight districts that were not selected for the Risk-Coverage intervention within the study area (Section 1.3.1). Here, when applicants were selected for the Subsidy scheme, the entrepreneurs received a one-time upfront payment directly on their loan account at the beginning of the loan repayment period. In Table 1.6, we describe the details of the Subsidy scheme that changed once during the evaluation period. During Period 1 (02/23-09/23), if selected for the Subsidy intervention, clients received 50% of their total interest payments (at most PKR 20,500) in the first month of the loan repayment directly on their loan account. During Period 2, this payment was increased to 100% of the total interest payment, while the maximum amount of PKR 20,500 was kept the same. We used exactly the same applicant-level randomization procedure as for the Risk-Coverage intervention which was described in Section 1.3.1.[40]

[39] See Section 1.2 for details.
[40] Thus, not every applicant for the loan in the districts in which the Subsidy intervention was introduced received the Subsidy scheme. When applying for the loan, NRSP first checked the loan eligibility of the applicant, who was then—if he/she was eligible—called for a baseline interview. After the interview, we conducted the applicant-level randomization once per week. As in the Risk-Coverage scheme, we called this randomization "lottery" and explained to each applicant carefully that the chance to "win" this lottery was 50%. After the randomization, the applicant learned her loan eligibility status and the outcome of the lottery. Regardless of the outcome of the lottery, if eligible, entrepreneurs could decide whether to take up the loan.

Table 1.6: Subsidy Intervention Evaluated

Period	Intervention Details	#Applications
Period 1 (02/23-09/23)	Clients receive **50%** of their total interest payments (sum of interest payments that accumulate over 12 months) but can receive at most PKR 20,500 in the first month of the loan repayment.	133
Period 2 (10/23-01/24)	Clients receive **100%** of their total interest payments (sum of interest payments that accumulate over 12 months) but can receive at most PKR 20,500 in the first month of the loan repayment.	349

Note: Summary for the Subsidy intervention evaluated in this study. The scheme changed in October 2023. The column (#Applications) shows the number of applications in each period, respectively.

Comparability of the Experiments. Before turning to the results, it is important to highlight that estimated effects on outcomes of interest across the three interventions—the Risk-Coverage intervention, the Cash intervention, and the Subsidy intervention—are not directly comparable for two reasons. First, given the distinct nature of each scheme, it is likely that applicants for the different schemes differ in both observable and unobservable characteristics. Indeed, baseline comparisons confirm statistically significant differences in observable characteristics between applicants in the Risk-Coverage and Subsidy districts (Appendix 1.F). However, we do not find any differences in average characteristics across the districts that were selected for the Risk-Coverage intervention to those that weren't selected for the Risk-Coverage intervention (Appendix 1.E.1). This indicates that different types of applicants selected themselves to apply for the different schemes. Second, the present value of the financial benefits across intervention schemes likely differs. Although the expected value of the insurance payouts in the Risk-Coverage scheme was set to match the upfront payment in the Subsidy scheme for any given loan size, the present value for each entrepreneur is likely to be different for the different schemes due to individual risk preferences and discount rates. However, even though we cannot directly compare the effects of the different intervention schemes, we argue that one still learns something about the mechanisms of the effects of the insurance from the results in the other experiments, as we will explain in the following.

1.6.2 Results

To evaluate the effects of the interventions of both the Subsidy experiment and the Pilot experiment, we will use the same regression framework as for the evaluation of the Risk-Coverage intervention. Hence, we will estimate specification (1.1) for the sample of randomized entrepreneurs in the Subsidy experiment and Pilot experiment, respectively. The results of these regressions on all outcomes discussed for the Risk-Coverage intervention are presented in Appendix 1.B.3 for the Subsidy experiment and in Appendix 1.B.4 for the Pilot experiment.

Take-up. Both the Pilot intervention scheme as well as the Subsidy scheme show substantially larger treatment effects on solar panel uptake than the Risk-Coverage intervention. While winning the Risk-Coverage scheme increased uptake rates by 12 percentage points, winning the Subsidy scheme increases uptake rates by 28 percentage points and winning the Pilot experiment cash scheme increases uptake rates by even 44 percentage points. However, uptake rates among treated entrepreneurs are relatively similar across all three interventions: 56.5% under Risk-Coverage scheme, 58.4% under the Subsidy scheme, and 46% under the Pilot cash scheme. This suggests that, conditional on receiving the benefit, the insurance is as effective as other incentive mechanisms in encouraging uptake, when effectiveness is measured in uptake rates among those who receive some benefit under such a scheme.

Several factors may explain the smaller treatment effect observed in the Risk-Coverage intervention. First, one might suspect lower comprehension of the insurance scheme than in the other schemes, but this appears unlikely, as the evidence discussed in Section 1.1 suggests the insurance scheme was well understood. Second, it could be argued that entrepreneurs are already implicitly insured via the option to default on the loan. However, this too seems implausible: Default carries severe reputational costs in Pakistan, including formal listing on governmental lists that blocks future access to loans. Moreover, across all interventions involving over 1,500 entrepreneurs, not a single default occurred. Third, while the insurance product was well understood, entrepreneurs were largely unfamiliar with weather insurance as a concept. At baseline, only 17.75% of entrepreneurs in Risk-Coverage districts had heard of weather insurance, which may have limited its perceived value. Fourth, as we already argued in the previous subsections, there are observable differences in the applicant pools (Appendix 1.F), which might also contribute to the differences in treatment effects.

Electricity Usage. While we find sizable effects of the Risk-Coverage intervention on grid electricity usage and electricity spending, we do not find any effect of both the Subsidy intervention (Table 1.7) and of the intervention in the pilot experiment (Table 1.8) on any of these outcomes. The most plausible explanation for these different findings lies in differences in the applicant pools across the schemes. At baseline, Risk-Coverage applicants reported, on average, 11% lower business profits, 14% lower electricity spending for their business, and owned 0.51 fewer machines that require electricity for their business in comparison to the entrepreneurs applying for the Subsidy scheme (Appendix 1.F). Notably, at baseline, the entrepreneurs did not significantly differ in their monthly household income on average. Hence, in comparison to applicants for the Subsidy scheme, electricity is less critical to the businesses of Risk-Coverage applicants and the business itself contributes less to the overall household income for Risk-Coverage applicants. Both factors indicate that it is easier for Risk-Coverage applicants to substitute grid electricity with potentially less stable solar energy, because their businesses require less electricity and they require their business less for the household income.

Table 1.7: Effects of the Subsidy Intervention on Electricity Usage

	Electricity Spending (PKR)			Grid Electricity Usage (kWh)			Num. of Intentional Shutdowns		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	-287.40	-250.93	-81.38	-3.70	0.57	-0.20	0.07	2.66	2.91
	-0.31	-0.27	-0.09	-0.26	0.04	-0.01	0.02	0.93	0.97
N	430	430	430	331	331	331	440	440	440
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	8145.1	8145.1	8145.1	161.7	161.7	161.7	21.70	21.70	21.70
SD Control	9549.1	9549.1	9549.1	134.2	134.2	134.2	41.73	41.73	41.73

Note: Results for specification (1.1), where the respective dependent variables are: Electricity spending in the last month measured in PKR (Columns (1) to (3)); electricity usage from the grid in the last month measured in kWh (Columns (4) to (6)); and the number of intentional grid shutdowns in the last six months that are conducted with the aim to reduce grid electricity spending (Columns (7) to (9)). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Three alternative explanations for the differences in the effects on electricity spending and usage across the schemes appear less plausible: First, one might suspect that Risk-Coverage applicants invested in higher-quality or larger solar systems. However, system size and price do not differ significantly between

Risk-Coverage and Subsidy applicants (Appendix 1.F).[41] Second, differences in ability to use solar systems efficiently seem unlikely: Risk-Coverage applicants are on average even less formally educated, and using solar systems typically requires no specialized skills, as systems are professionally installed by the vendors. Third, differential access to solar systems outside the NRSP loan program could potentially bias estimates—if more control group entrepreneurs in the Subsidy experiment obtained solar panels independently than in the Risk-Coverage experiment, the treatment-control gap in electricity use would shrink. Yet, Appendix 1.F shows this is not the case.

Table 1.8: Effects of the Pilot Experiment Intervention on Electricity Usage

	Electricity Spending (PKR)		Grid Electricity Usage (kWh)		Num. of Intentional Shutdowns	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	-304.64	-246.61	-7.72	4.54	-1.53	-5.36
	-0.37	-0.30	-0.51	0.32	-0.41	-1.56
N	446	446	258	258	479	479
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓		✓		✓
Mean Control	7041.1	7041.1	133.4	133.4	15.56	15.56
SD Control	8528.7	8528.7	116.1	116.1	42.02	42.02

Note: Results for specification (1.1), where the respective dependent variables are: Electricity spending in the last month measured in PKR (Columns (1) to (2); electricity usage from the grid in the last month measured in kWh (Columns (3) to (4)); and the number of intentional grid shutdowns in the last six months that are conducted with the aim to reduce grid electricity spending (Columns (5) to (6)). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Further Outcomes. We neither find effects of the Subsidy intervention nor of the Cash intervention on further business-performance related outcomes.

1.6.3 Implications for Risk-Coverage Intervention

Using these results from the additional experiments, let us come back to our initial question, of why the Risk-Coverage scheme, i.e., the insurance, enables

[41] The size and prize of solar systems of applicants in the Pilot experiment was different to the applicants in the other schemes. However, the systems in the pilot experiment were also bought much earlier, when the market offered different systems.

entrepreneurs to reduce their electricity spending and install additional solar systems. Considering the different baseline characteristics between the entrepreneurs who apply for the different intervention schemes, it seems plausible that the effect of the insurance on electricity usage are specific to the entrepreneurs who applied for the loan and the insurance scheme and not the insurance scheme itself. At the same time, the uptake rate among those who receive a benefit is the same across the different experiments, indicating that the Risk-Coverage scheme does not nudge more or less entrepreneurs to install a solar system, but other type of entrepreneurs. Hence, on the one hand, the same entrepreneurs who received the Risk-Coverage scheme would have probably also reduced their grid electricity usage and electricity spending if they would have been successfully nudged by other schemes to install the solar system. On the other hand, the results do also suggest that at least the other intervention schemes in the Subsidy and Pilot experiment do not successfully nudge those entrepreneurs who reduce their grid electricity spending to install a solar system. Thus, we can conclude that the insurance reduces electricity spending because it nudges specific entrepreneurs to take up solar systems, who are able and willing to reduce their grid electricity spending and who would have not taken up solar systems with direct payment schemes. In that sense, it would in fact be the insurance that causes the electricity spending and usage to decrease. Moreover, the presented results did also indicate that other mechanisms which could explain the effects of the Risk-Coverage scheme on electricity usage appear implausible: First, it is likely not the case that Risk-Coverage entrepreneurs have specific knowledge and/or skills which allows them to use the solar systems better than other entrepreneurs, because Risk-Coverage entrepreneurs are less educated than other clients. Second, it is also not the case that the insurance encourages entrepreneurs to buy larger or qualitatively better panels. Third, the results are also not driven by a systematic error which would bias the estimates if entrepreneurs in the Risk-Coverage districts have different outside options to finance a solar system if they decide to not take up the loan with NRSP than entrepreneurs in the other districts.

1.7 Robustness

In this section, we discuss threats to our results and identification strategy, as well as our approaches to demonstrate the robustness of our findings to these threats.

The Effects of Winning a Lottery. Recall that those who apply for the loan are told that they are put in a lottery in which they can either “win” and receive the insurance or “lose” and not receive the insurance. Thus, the control group experiences a form of “treatment” by being told they “lost” the lottery, which could discourage loan uptake. Conversely, “winning” the lottery might provide additional encouragement to take up the loan and solar system, beyond the insurance’s direct effect. These encouragement and discouragement effects could inflate the estimated treatment effect of the insurance on uptake, because both effects—if they exist—increase the gap in uptake between those who receive access to the insurance and those who do not. We cannot directly separate these potential effects from the insurance’s direct impact in our setting. However, we run an additional survey experiment with 460 individuals to investigate whether such encouragement and discouragement are likely to exist or not. We conducted this additional survey experiment in the district of Sargodha, in which we did not offer the insurance lottery. In this survey experiment, participants were offered to buy a solar-powered fan with a battery at varying conditions. We bought ten of these fans at a price of PKR 11,000 each. We randomly assigned each participant in the survey experiment to one of four groups: In the first group, respondents were offered to buy the fan at a “full price of PKR 10,000”. In the second group, respondents were offered to buy the fan “with a discount for PKR 8,000” and were told that the usual price was actually PKR 10,000. In the third and fourth group, respondents were offered to take part in a lottery which would give them, in case they win, a voucher worth PKR 2,000 that can be used to buy the fan which costs PKR 10,000 without the voucher. In both the third and fourth group, respondents were told that they can decide to buy the fan after they learn their lottery outcome. In the third group, respondents were then told that they “lost the lottery” but that they can still buy the fan at “a full price of PKR 10,000”. In the fourth group, respondents were told that they “won the lottery” and they can still decide whether to buy the fan at a “discounted price of PKR 8,000” using their voucher. In all four groups, respondents were told that if they decide to buy the fan, they are put in a pool of people from which we randomly select ten to indeed execute the deal, i.e., deliver the fan and collect the agreed price from them. After the survey experiment, we randomly selected ten respondents who decided to “buy” the fan and sent one fan to each of them for free as a gift. Using the respondents in this survey experiment, we consider the following regression specification:

$$\text{Uptake}_{je} = \alpha_0 + \alpha_1 \text{Discount}_j + \beta_2 \Big(\text{Discount}_i * \text{Lottery}_i \Big) + \eta_e + \varepsilon_i \qquad (1.2)$$

where Uptake_{je} is a dummy variable indicating whether respondent j, interviewed by enumerator e, decided to buy the solar fan; Discount_j is a dummy variable indicating whether respondent j was eligible for a discount of PKR 2,000, i.e., whether a respondent was either in group 2 or in group 4; Lottery_j is a dummy variable indicating whether respondent j received the discount through the lottery, i.e., whether respondent j was in group 4 or not; and η_e are enumerator fixed-effects which account for differences across enumerators, who might influence both, how the lottery and the discount is perceived by respondents and whether they decide to buy (take up) the fan. Reassuringly, we do find that the discount significantly increases uptake, whereas we do not find an additional significant effect when the discount is allocated through the lottery (Appendix 1.B.1). Hence, in our additional survey experiment, we do not find evidence for the existence of an additional encouragement and/or discouragement effect through the lottery mechanism used to distribute the insurance.

Potential Price Distortions. One might further be concerned that our estimates are “biased” by potential price distortions indirectly caused by our intervention, because our intervention could have potentially increased the demand of solar panels significantly in the study area, thereby increasing both the demand and the price for solar systems and their components in the study area. If such price effects would exist, our estimated effects on uptake would be smaller than the direct effect of the intervention, putting a threat to the external validity of the findings to contexts in which such price effects would be different or non-existing. To address this concern, we conducted an additional phone survey with all contracted solar panel vendors of NRSP in the study area. The survey, conducted in December 2024, was voluntary and achieved a response rate of 78%, with 48 vendors participating across the districts included in the Risk-Coverage intervention. On average, each vendor reports monthly sales of 1,285 solar panels, with a median of 128 panels. Even based on the median figure, the 258 solar panels installed among entrepreneurs in our study over the course of one year represent a negligible share of local market activity. It is therefore unlikely that our intervention had any impact on solar panel prices or installation fees. Hence, we are confident that our estimates are not “biased” by price distortions caused by the intervention in our the experiment. Whether a scaled-up version of the insurance would affect market prices remains uncertain. However, several factors mitigate this concern. The solar panel market is international in scope, and vendor responses suggest a high level of competition. Although all interviewed vendors in the survey reported sourcing panels from China, they each sell panels from an average of 3.75 different brands. This variety supports the conclusion that the market is competitive and unlikely to be easily influenced by moderate increases in local demand.

Migration. A threat to the external validity of our findings could be that individuals move to the study districts in order to participate in the lottery. However, we consider this to be very unlikely for two reasons. First, the benefit of the treatment is around USD 60 to USD 80, which means that receiving this benefit with only 50% probability should under no circumstance weight out the cost of moving to another district. Second, in order to receive a loan from NRSP, NRSP conducts a social appraisal. This social appraisal includes conversations with the neighbors and other individuals in the community to determine the trustworthiness of a loan applicant. Considering the case of a loan applicant who moves to a new village, according to NRSP, it is very unlikely that the social appraisal for such a loan application would be successful.

1.8 Conclusion

In this study, we develop and implement a novel index insurance that aims at insuring low returns of solar panels. We study low-income entrepreneurs in Pakistan who take up a loan to finance a solar panel. During the repayment phase, the insurance makes a payout on the loan account of an entrepreneur in months in which there are more than eight cloudy or foggy days. Thus, the scheme is paying parts of the loan installment for the entrepreneur, when the panel produces little to no electricity. Essentially, the insurance scheme that we develop reduces the risks associated with solar energy investments. The results show that providing this insurance for free increases the uptake rate of solar panels by 12 percentage points (an increase of 27%). Moreover, those entrepreneurs who are entitled to receive the insurance spend 28% less on electricity at the time of the endline data collection on average, which corresponds to 5.4% of average profits. Similarly, they are using 14.4% less grid electricity and use intentional grid shutoffs more regularly to reduce their electricity spending when the prices for grid electricity are high. The cost-benefit ratio calculations show that per 100 USD of program costs, 0.48 kWp additional solar panel capacity is installed and there is an overall reduction of 2.57 tons of CO_2-eq. emissions. The latter finding implies that our intervention is economically efficient, considering recent estimates which suggests that the social costs of carbon for 2.57 tons of CO_2-eq. emissions equals USD 169 Barrage and Nordhaus (2024). Furthermore, the ratio between the overall lifetime reduction in electricity spending by all entrepreneurs and the total program costs is 3.36. Hence, the long-term benefits exceed the long-term costs of the index insurance, thereby suggesting that the insurance scheme can be self-financing and is also welfare improving. Considering these results, we also conduct two further experiments with two further intervention schemes to understand the mechanisms behind the effects of the insurance scheme. The results of these two experiments suggest that the insurance scheme is as effective as other schemes to encourage the uptake of solar systems, when effectiveness is measured in uptake rates among those who receive some benefit under such schemes. Moreover, the results from these additional experiments do also provide evidence for the hypothesis that the insurance reduces electricity spending because it nudges specific entrepreneurs to take up solar systems, who are able and willing to reduce their grid electricity spending and who would have not taken up solar systems with direct payment schemes in the absence of an insurance.

Considering the results of our study, we demonstrate that entrepreneurs in low- and middle-income countries can be encouraged to invest in renewable energies with relatively small incentive schemes. Moreover, the very similar uptake rates among treated individuals in different experiments in our study suggest to policy makers that reducing the risk of an investment can be effective to encourage more investments. Furthermore, the results also suggest that reducing risks of renewable energy investments encourages types of entrepreneurs who are not encouraged by direct payments to install a solar system. Therefore, even though further research is required, we potentially open policy makers a new possibility to encourage renewable energy investments that encourages those who would have not been encouraged with simpler direct payments.

For clear policy recommendations, however, further research is required. First, it would be very interesting to test a treatment interaction between an upfront subsidy payment and an index insurance. Results from Jack et al. (2025) suggest that this combination might be very effective, as the upfront payment establishes trust in the institution. This, in turn, also increases trust that future payments of a potential insurance are indeed made, which in turn increases uptake of insurance. In our case, one would then expect an additional interaction effect of the insurance scheme and a simpler upfront payment. Lastly, it would also be interesting to test schemes that are self-financing in the sense of delaying the insurance premium payment to some later time, when the benefits of the solar system already materialized to the entrepreneurs.

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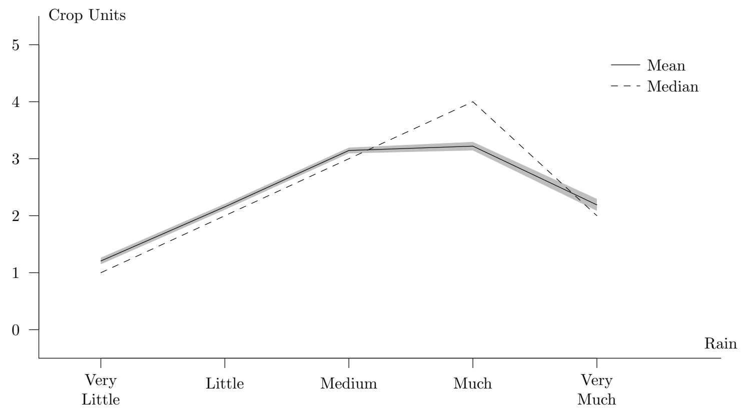
Appendices

1.A Descriptives

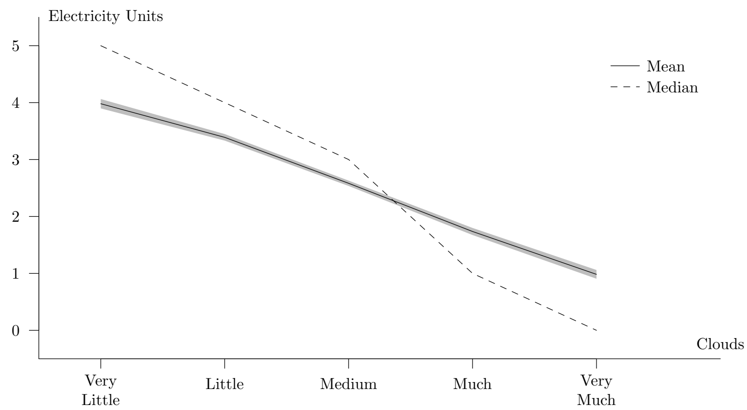
1.A.1 Index-Outcome Relation

Figure 1.A.1: Perceived Relation between Rain and Agricultural Yields



Note: The basis of this figure is a question in which we asked respondents to tell us what they think how many "units of crop" a farmer can earn given "very little", "little", "medium", "much" or "very much" rain, respectively. For each level, they were able to choose between 0 and 5 crop units, where we told respondents that the farmer can get at most 5 crop units under optimal conditions from his plot. The Figure shows the both the median response (dashed line) for each level of rain as well as the mean response (including the 95% confidence interval marked in gray) for each level of rain.

Figure 1.A.2: Perceived Relation between Clouds and Solar Panel Output



Note: The basis of this figure is a question in which we asked respondents to tell us what they think how many "batteries" a farmer can fully charge given "very little", "little", "medium", "much" or "very much" clouds, respectively. For each level, they were able to choose between 0 and 5 batteries, where we told respondents that the farmer can charge at most 5 batteries under optimal conditions from his solar panels. The Figure shows the both the median response (dashed line) for each level of clouds as well as the mean response (including the 95% confidence interval marked in gray) for each level of clouds.

1.A.2 Descriptives for Entrepreneurs

Table 1.A.1: Primary Business of Entrepreneurs

	Share of Entrepreneurs	
Sectors	Risk-Districts	Other Districts
Trade of manufactured goods or wholesale/retail (trade and/or selling of non-agricultrual goods and non-livestock goods, e.g. trade of mobile phones, clothing goods, etc.)	46.72%	49.79%
Daily services (transportation, laundry, hairdressing, tailoring, other houshold services, etc.)	14.86%	10.58%
Agricultural Production (farming, growing crops)	11.58%	10.58%
Manufacturing (clothing production, gadget production)	5.98%	7.68%
Hospitality service (hotels, resturaunts, etc.)	4.63%	3.32%
Trades service (electrician, construction, builder, etc.)	4.25%	3.11%
Livestock Trade (trade of livestock & livestock production like milk)	3.47%	5.60%
Agricultural Trade (trade of plant goods)	0.19%	0.62%
Livestock Caretaking (herdsmen, stockyard, pastoralists)	0.19%	0.41%
Other	0.19%	0.41%
Trade and selling of land/real estate	0.19%	0.00%

Note: This is data from the baseline. We asked entrepreneurs about the primary business in which they are active. For each sector, the table shows the share of entrepreneurs that are present in that sector for each type of districts, respectively. Note that the percentages do not sum up to 100, as the sector is not known for some entrepreneurs.

Table 1.A.2: Application Reasons of Entrepreneurs

Reason for applying:	Mentioned by (in Percentage):
Solar panels provide cheap(er) electricity	97.3%
Solar panels have less outages than the grid	64.5%
Solar panels come with little maintenance cost	6.5%
Solar panels are better for health than generator	2.5%
Respondent wants to win the lottery (i.e., treatment)	0.8%
Solar panels can be used to sell electricity	0.8%
Respondent wants to help the environment	0.1%
Respondent wants to do something against climate change	0.1%

Note: Entrepreneurs were asked after their application in the baseline, why they applied for the loan to install solar panels. We did not give answer options and only reported all reasons that were mentioned by each respondent.

1.A.3 Control Variable Missing Values Imputation

Table 1.A.3: Descriptive Statistics Before and After Imputation

	Before Imputation			After Imputation		
Variable	N	Mean	SD	N	Mean	SD
b_spend	912	12,653.03	26,374.91	986	12,591.93	25,415.03
b_spend_usually	912	12,072.86	23,518.47	986	12,045.42	22,686.70
b_elect_connect	926	0.91	0.28	1000	0.92	0.27
b_gen_elect_generator	926	0.07	0.25	1000	0.07	0.24
b_bus_elect_tools_2	859	5.15	5.52	1000	5.10	5.15
b_profit	908	66,981.83	68,706.48	982	67,153.01	66,279.33
b_income_hh_total	926	162,070.52	286,398.78	1000	162,142.20	275,878.68
b_profit_var	900	56,945.00	91,800.85	1000	57,070.90	87,250.01
b_business_seasonal	925	0.10	0.30	1000	0.10	0.29
b_bisp	926	0.22	0.42	1000	0.22	0.40
b_income_hh_agriculture	926	0.87	1.21	1000	0.87	1.17
b_income_hh_business	926	2.03	1.21	1000	2.03	1.17
b_saving_lastmo	910	31,905.60	38,662.70	984	31,947.42	37,273.27
b_savings_avg	906	32,026.60	35,938.37	980	32,058.67	34,674.73
b_shock_coping	925	0.96	0.19	999	0.96	0.19
b_shock_cover_poss	926	1.27	0.57	1000	1.27	0.55
b_risk_avg	767	22.57	11.21	1000	22.57	9.89
b_time_avg	783	5.62	10.04	1000	5.57	8.90
b_powercut	925	0.85	0.35	1000	0.85	0.34
b_typcmonth_elect	922	8.04	7.41	996	8.05	7.15
b_solar_reducecost	926	1.04	0.20	1000	1.04	0.19
b_solar_health	926	1.02	0.16	1000	1.02	0.15
b_time_business	921	68.19	25.71	1000	68.20	24.72
b_invest_future	925	0.41	0.49	1000	0.41	0.47

Note: The Table shows the number of observations (N), the Mean and the standard deviation (SD) separately for all control variables before and after the imputation took place for the entrepreneurs in the Risk-Coverage intervention. The variables in the table are: spend: electricity spendings last month; spend_usually: electricity spendings typical month; elect_connect: indicator: business connected to electricity grid; gen_elect_generator: indicator: respondent owns a generator; bus_elect_tools_2: number of electric machines/tools in business; profit: profit from business last month; income_hh_total: total monthly household income; profit_var: profit variability: best profit in last six months minus worst profit in last six month; business_seasonal: indicator: business is seasonal; bisp: indicator: respondent is Bisp beneficiary (poverty program of Pakistan government); income_hh_agriculture: number of people in household receiving income from agriculture; income_hh_business: number of people in household receiving income from employment/business; saving_lastmo: savings last month; savings_avg: Savings typical month; shock_coping: indicator: Respondent has shock coping mechanisms; shock_cover_poss: number of shock cover possibilities that respondent has; risk_avg: risk-aversion measure: average risk-aversion (we used a questionnaire module developed by Falk et al. (2023) to estimate the risk-aversion of entrepreneurs); time_avg: discounting factor: average value (we used a questionnaire module developed by Falk et al. (2023) to estimate the discount rate of entrepreneurs); powercut: indicator: power-cuts happened in last week; typcmonth_elect: number of power-cuts per week; solar_reducecost: respondent agrees to: solar panels reduce costs; solar_health: respondent agrees to: solar panels are healthier; time_business: total hours devoted to business per week; invest_future: indicator: respondent has serious plans to make investments.

1.B Results

1.B.1 Survey Experiment

Table 1.B.1: Results for Survey Experiment

	(1)	(2)	(3)	(4)
	b/t	b/t	b/t	b/t
Discount	0.03125	0.04315*	0.04208	0.05624*
	1.30	1.82	1.34	1.85
Discount * Lottery			-0.02226	-0.02695
			-0.60	-0.75
N	448	448	448	448
Enumerator Fixed Effects		✓		✓

Note: Regression results for specification (1.2) for the respondents taking part in the survey experiment described in Section 1.5.1. Standard errors were calculated robust to heteroskedasticity. Significance is indicated by: $*p < 0.1, **p < 0.05, ***p < 0.01$.

1.B.2 Secondary Outcomes Risk-Coverage Intervention

Table 1.B.2: Effects of Risk-Coverage Intervention on Profits

	Profits Last Month (PKR)			Profits Typical Month (PKR)			Variation in Profits (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-4848.79	-5662.64*	-1819.33	-4134.47	-4444.45	-950.53	-6147.88	-7462.20*	-5255.71
	-1.49	-1.81	-0.62	-1.25	-1.42	-0.32	-1.32	-1.70	-1.20
N	467	467	467	465	465	465	462	462	462
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	37618.5	37618.5	37618.5	40764.1	40764.1	40764.1	45515.9	45515.9	45515.9
SD Control	40680.6	40680.6	40680.6	40197.4	40197.4	40197.4	54770.5	54770.5	54770.5

Note: Results for specification (1.1), where the respective dependent variables are: profits in the last month measured in PKR in Columns (1) to (3); profits in a typical month (we ask entrepreneurs to report the profit per month they usually or typically make) measured in PKR in Columns (4) to (6); and the variation in profits, which is measured by the difference between the highest monthly profit in the last six months (in PKR) and the smallest monthly profits in the last six months (in PKR) in Columns (7) to (9). Standard errors are robust to heteroscedasticity. SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.3: Effects of Risk-Coverage Intervention on Revenue and Expenses

	Revenue Last Month (PKR)			Total Expenses Last Month (PKR)			Employment Expenses Last Month (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-4459.70	-8697.90	3085.49	-1279.06	-5243.71	2951.95	-9599.71	-6170.05	-3751.12
	-0.35	-0.70	0.25	-0.13	-0.55	0.31	-1.12	-0.87	-0.45
N	463	463	463	465	465	465	103	103	103
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	97552.6	97552.6	97552.6	61706.0	61706.0	61706.0	38992.2	38992.2	38992.2
SD Control	140192.4	140192.4	140192.4	112430.7	112430.7	112430.7	46271.2	46271.2	46271.2

Note: Results for specification (1.1), where the respective dependent variables are: total business revenue in the last month measured in PKR in Columns (1) to (3); total business expenses in the last month measured in PKR in Columns (4) to (6); and the expenses for employment in the business in the last month measured in PKR in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.4: Effects of Risk-Coverage Intervention on Investments

	Planned Investment Realized (0/1)			Total Yearly Investments (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-0.08	-0.06	-0.05	-6382.72	-7580.99	-6073.34
	-0.92	-0.70	-0.47	-0.69	-0.85	-0.70
N	135	135	135	494	494	494
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓
Baseline Controls			✓			✓
Mean Control	0.349	0.349	0.349	45862.8	45862.8	45862.8
SD Control	0.476	0.476	0.476	111242.1	111242.1	111242.1

Note: Results for specification (1.1), where the respective dependent variables are: the share of realized investments at time of the endline data collection that were planned at time of baseline om Columns (1) to (3); and total yearly investments in the last 12 months at time of the endline data collection measured in PKR in Columns (4) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.5: Effects of Risk-Coverage Intervention on Income and Savings

	Income Last Month (PKR)			Income Typical Month (PKR)			Savings Last Month (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-17784.69*	-15749.73	-10409.37	-6279.80	-4272.20	1414.42	-3331.65	-3445.02*	-2205.84
	-1.69	-1.53	-0.98	-0.61	-0.43	0.14	-1.54	-1.75	-1.18
N	482	482	482	457	457	457	466	466	466
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	102849.6	102849.6	102849.6	90556.9	90556.9	90556.9	22162.7	22162.7	22162.7
SD Control	129606.2	129606.2	129606.2	115763.9	115763.9	115763.9	28042.5	28042.5	28042.5

Note: Results for specification (1.1), where the respective dependent variables are: total household income of entrepreneurs in the last month measured in PKR in Columns (1) to (3); total household income of entrepreneurs in typical month (we ask entrepreneurs to report the income per month they usually or typically have) measured in PKR in Columns (4) to (6); and total household savings of entrepreneurs in the last month measured in PKR in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.6: Effects of Risk-Coverage Intervention on Satisfaction and Attitudes

	Satisfaction with Product			Heard about Climate Change (0/1)			Knowledge about Climate Change (1-7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for insurance	-0.02	0.00	0.00	0.08*	0.07*	0.09**	0.03	0.08	0.10
	-0.52	0.03	0.11	1.77	1.77	2.08	0.19	0.64	0.73
N	479	479	479	482	482	482	257	257	257
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	0.655	0.655	0.655	0.508	0.508	0.508	2.290	2.290	2.290
SD Control	0.476	0.476	0.476	0.501	0.501	0.501	1.106	1.106	1.106

Note: Results for specification (1.1), where the respective dependent variables are: the satisfaction with the solar product, which is measured by the share of respondents who believe that solar panels can fully replace a generator in Columns (1) to (3); the share of individuals who mentioned that they have heard the term climate change and/or climate crisis in Columns (4) to (6); and the knowledge about climate change which is measured by asking individuals to mention all facts they know about climate change and where every correct mentioned fact gives one point (the maximum reachable points were 7) in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for insurance. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

1.B.3 Outcomes Subsidy Intervention

Table 1.B.7: Effects of Subsidy Intervention on Uptake

	(1)	(2)	(3)
	b/t	b/t	b/t
Entitled for subsidy	0.28***	0.26***	0.30***
	6.75	6.74	7.25
N	455	455	455
Batch FE	✓	✓	✓
District FE		✓	✓
Baseline Controls			✓
Mean Control	0.304	0.304	0.304
SD Control	0.461	0.461	0.461

Note: Results for specification 1.1, where the dependent variable is a dummy variable being one if the individual took up the loan and thereby financed a solar panel and zero otherwise. SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 1.B.8: Effects of Subsidy Intervention on Profits

	Profits Last Month (PKR)			Profits Typical Month (PKR)			Variation in Profits (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	3164.35	1977.90	1710.78	4043.72	3331.30	3231.29	-1000.86	-171.38	-2819.30
	0.79	0.50	0.42	1.11	0.93	0.90	-0.20	-0.03	-0.56
N	439	439	439	436	436	436	410	410	410
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	43691.2	43691.2	43691.2	45798.7	45798.7	45798.7	52143.5	52143.5	52143.5
SD Control	41931.2	41931.2	41931.2	38297.4	38297.4	38297.4	52297.5	52297.5	52297.5

Note: Results for specification (1.1), where the respective dependent variables are: profits in the last month measured in PKR in Columns (1) to (3); profits in a typical month (we ask entrepreneurs to report the profit per month they usually or typically make) measured in PKR in Columns (4) to (6); and the variation in profits, which is measured by the difference between the highest monthly profit in the last six months (in PKR) and the smallest monthly profits in the last six months (in PKR) in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.9: Effects of Subsidy Intervention on Revenue and Expenses

	Revenue Last Month (PKR)			Total Expenses Last Month (PKR)			Employment Expenses Last Month (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	-1432.86	-2450.59	-3860.00	-11915.82	-9412.84	-8515.75	-3634.96	-3679.39	-4006.63
	-0.10	-0.17	-0.26	-1.14	-0.92	-0.78	-0.87	-0.88	-0.95
N	436	436	436	434	434	434	150	150	150
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	125533.6	125533.6	125533.6	74267.3	74267.3	74267.3	31011.4	31011.4	31011.4
SD Control	153519.8	153519.8	153519.8	109720.5	109720.5	109720.5	27666.7	27666.7	27666.7

Note: Results for specification (1.1), where the respective dependent variables are: total business revenue in the last month measured in PKR in Columns (1) to (3); total business expenses in the last month measured in PKR in Columns (4) to (6); and the expenses for employment in the business in the last month measured in PKR in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.10: Effects of Subsidy Intervention on Investments

	Planned Investment Realized (0/1)			Total Yearly Investments (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	0.07	0.07	0.05	12840.60	8555.33	9748.85
	0.80	0.82	0.57	1.47	1.10	1.22
N	132	132	132	450	450	450
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓
Baseline Controls			✓			✓
Mean Control	0.261	0.261	0.261	29823.0	29823.0	29823.0
SD Control	0.442	0.442	0.442	90280.5	90280.5	90280.5

Note: Results for specification (1.1), where the respective dependent variables are: the share of realized investments at time of the endline data collection that were planned at time of baseline in Columns (1) to (3); and total yearly investments in the last 12 months at time of the endline data collection measured in PKR in Columns (4) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.11: Effects of Subsidy Intervention on Income and Savings

	Income Last Month (PKR)			Income Typical Month (PKR)			Savings Last Month (PKR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	-15550.56	-13726.99	-16520.78	-10885.23	-9412.38	-8534.95	209.51	-388.49	-1029.73
	-1.26	-1.12	-1.34	-0.94	-0.81	-0.72	0.08	-0.16	-0.41
N	450	450	450	449	449	449	427	427	427
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	128074.1	128074.1	128074.1	122566.1	122566.1	122566.1	29816.0	29816.0	29816.0
SD Control	140014.3	140014.3	140014.3	130834.7	130834.7	130834.7	30557.7	30557.7	30557.7

Note: Results for specification (1.1), where the respective dependent variables are: total household income of entrepreneurs in the last month measured in PKR in Columns (1) to (3); total household income of entrepreneurs in typical month (we ask entrepreneurs to report the income per month they usually or typically have) measured in PKR in Columns (4) to (6); and total household savings of entrepreneurs in the last month measured in PKR in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.12: Effects of Subsidy Intervention on Satisfaction & Attitudes

	Satisfaction with Product			Heard about Climate Change (0/1)			Knowledge about Climate Change (1-7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	0.06	0.07**	0.06**	0.00	-0.01	-0.02	0.05	-0.05	-0.04
	1.48	2.30	2.08	0.03	-0.27	-0.41	0.31	-0.36	-0.24
N	447	447	447	446	446	446	186	186	186
Batch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓		✓	✓
Baseline Controls			✓			✓			✓
Mean Control	0.689	0.689	0.689	0.436	0.436	0.436	2.067	2.067	2.067
SD Control	0.464	0.464	0.464	0.497	0.497	0.497	0.993	0.993	0.993

Note: Results for specification (1.1), where the respective dependent variables are: the satisfaction with the solar product, which is measured by the share of respondents who believe that solar panels can fully replace a generator in Columns (1) to (3); the share of individuals who mentioned that they have heard the term climate change and/or climate crisis in Columns (4) to (6); and the knowledge about climate change which is measured by asking individuals to mention all facts they know about climate change and where every correct mentioned fact gives one point (the maximum reachable points were 7) in Columns (7) to (9). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

1.B.4 Outcomes Pilot Experiment Intervention

Table 1.B.13: Effects of Pilot Intervention on Uptake

	(1)	(2)
	b/t	b/t
Entitled for subsidy	0.45***	0.44***
	13.74	13.41
N	502	502
Batch FE	✓	✓
District FE		✓
Mean Control	0.00781	0.00781
SD Control	0.0882	0.0882

Note: Results for specification 1.1, where the dependent variable is a dummy variable being one if the individual took up the loan thereby financed a solar panel and zero otherwise. SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 1.B.14: Effects of Pilot Intervention on Profits

	Profits Last Month (PKR)		Profits Typical Month (PKR)		Variation in Profits (PKR)	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	1744.35	910.30	1012.25	-1733.27	-1291.91	-733.77
	0.42	0.22	0.20	-0.38	-0.22	-0.13
N	465	465	466	466	434	434
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓		✓		✓
Mean Control	47868.6	47868.6	57008.5	57008.5	60252.3	60252.3
SD Control	40784.7	40784.7	48595.0	48595.0	58996.7	58996.7

Note: Results for specification (1.1), where the respective dependent variables are: profits in the last month measured in PKR in Columns (1) to (2); profits in a typical month (we ask entrepreneurs to report the profit per month they usually or typically make) measured in PKR in Columns (3) to (4); and the variation in profits, which is measured by the difference between the highest monthly profit in the last six months (in PKR) and the smallest monthly profits in the last six months (in PKR) in Columns (5) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.15: Effects of Pilot Intervention on Revenue and Expenses

	Revenue Last Month (PKR)		Total Expenses Last Month (PKR)		Employment Expenses Last Month (PKR)	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	19750.95	7425.01	11350.36	5465.01	-2222.79	-5072.45
	1.16	0.49	0.79	0.42	-0.29	-0.71
N	466	466	465	465	146	146
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓		✓		✓
Mean Control	134484.2	134484.2	88522.3	88522.3	32172	32172
SD Control	155810.9	155810.9	133775.9	133775.9	34629.0	34629.0

Note: Results for specification (1.1), where the respective dependent variables are: total business revenue in the last month measured in PKR in Columns (1) to (2); total business expenses in the last month measured in PKR in Columns (3) to (4); and the expenses for employment in the business in the last month measured in PKR in Columns (5) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.16: Effects of Pilot Intervention on Investments

	(1)	(2)
	b/t	b/t
Entitled for subsidy	-717.13	6469.40
	-0.13	1.34
N	500	500
Batch FE	✓	✓
District FE		✓
Mean Control	25085.5	25085.5
SD Control	64279.6	64279.6

Note: Results for specification (1.1), where the respective dependent variable is the total yearly investments in the last 12 months at time of the endline data collection measured in PKR. SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.17: Effects of Pilot Intervention on Income and Savings

	Income Last Month (PKR)		Income Typical Month (PKR)		Savings Last Month (PKR)	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	20747.96	8420.26	25217.92	2909.91	-2386.95	-556.68
	1.19	0.56	1.41	0.19	-1.20	-0.30
N	475	475	476	476	451	451
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓		✓		✓
Mean Control	131610.4	131610.4	130361.7	130361.7	24808.4	24808.4
SD Control	161992.5	161992.5	173763.7	173763.7	21032.2	21032.2

Note: Results for specification (1.1), where the respective dependent variables are: total household income of entrepreneurs in the last month measured in PKR in Columns (1) to (2); total household income of entrepreneurs in typical month (we ask entrepreneurs to report the income per month they usually or typically have) measured in PKR in Columns (3) to (4); and total household savings of entrepreneurs in the last month measured in PKR in Columns (5) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

Table 1.B.18: Effects of Pilot Intervention on Satisfaction and Attitudes

	Satisfaction with Product		Heard about Climate Change (0/1)		Knowledge about Climate Change (1-7)	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
Entitled for subsidy	-0.05	-0.00	0.04	0.01	0.03	-0.02
	-1.23	-0.07	0.94	0.14	0.17	-0.17
N	470	470	476	476	176	176
Batch FE	✓	✓	✓	✓	✓	✓
District FE		✓		✓		✓
Mean Control	0.795	0.795	0.360	0.360	2.609	2.609
SD Control	0.405	0.405	0.481	0.481	1.082	1.082

Note: Results for specification (1.1), where the respective dependent variables are: the satisfaction with the solar product, which is measured by the share of respondents who believe that solar panels can fully replace a generator in Columns (1) to (2); the share of individuals who mentioned that they have heard the term climate change and/or climate crisis in Columns (3) to (4); and the knowledge about climate change which is measured by asking individuals to mention all facts they know about climate change and where every correct mentioned fact gives one point (the maximum reachable points were 7) in Columns (5) to (6). SD stands for standard deviation, while "Mean Control" and "SD Control" report the mean and standard deviation of the respective outcome variable for those who are not entitled for subsidy. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. FE stands for fixed effects.

1.B.5 Treatment Effect Heterogeneity

Table 1.B.19: Treatment Heterogeneity Estimation for Risk-Coverage Intervention

	Uptake		**Electricity Spending**		**Electricity Usage**	
Comparison of Learners:						
	Λ (BLP)	$\bar{\Lambda}$ (GATES)	Λ (BLP)	$\bar{\Lambda}$ (GATES)	Λ (BLP)	$\bar{\Lambda}$ (GATES)
Random Forest	0.00095	0.03576	284904	12985609	853.21	3913.87
Elastic Net	0.00317	0.03274	4909943	14150609	656.44	4886.82
Boosting	0.00091	0.03315	1920197	11524331	833.38	4510.74
Support-Vector-Machine	0.00442	0.03669	530754	13335347	592.30	4807.24
Lasso	0.00288	0.02570	5313833	12614852	1032.22	6741.33
Neural Network	0.00269	0.03932	4489642	9923386	346.65	3463.02
Best Linear Predictor:						
	ATE (p-value)	HET (p-value)	ATE (p-value)	HET (p-value)	ATE (p-value)	HET (p-value)
Best Learner:	0.13052	0.21796	-1920.92	0.30011	-11.7925	-0.01536
	(0.06303)	(0.53371)	(0.42261)	(0.28115)	(1.00000)	(1.00000)

Note: The algorithm of Chernozhukov et al. (2025) was run with the given learners, following the procedure described in Section 1.4. BLP is the abbreviation for best linear predictor; ATE is the abbreviation for average treatment effect, HET is the abbreviation for the heterogeneity parameter; and GATES is the abbreviation for Sorted Group Average Treatment Effects. To calculate the ATE and HET value, we use the algorithm with the largest Λ, respectively. The “Uptake” columns refer to the results of the algorithm with the dependent variable being an indicator variable equal to one if the entrepreneur took up the loan and installed a solar system and zero otherwise. The “Electricity Spending” columns refer to the results of the algorithm with the dependent variable being equal to the total electricity spending in the last month, measured in PKR. The “Electricity Usage” columns refer to the results of the algorithm with the dependent variable being equal to the grid electricity usage in the last month, measured in kWh.

1.C Branch Manager Survey

1.C.1 Background

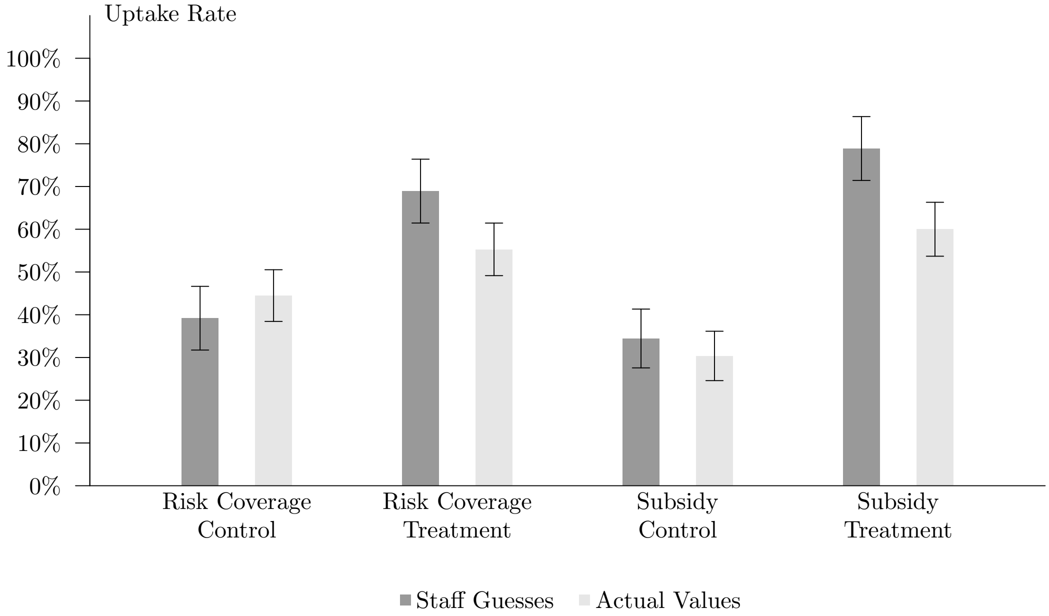
We conducted a survey among all 88 branch managers in the study districts. Branch managers are part of field staff and managers of the smallest administrative units of the implementing partner NRSP. The aim of this survey was to estimate branch managers' priors on key results of the baseline and inform them about corresponding baseline findings. Out of 88 branch managers, 84 participated in the survey. For all results, we first asked branch managers about their priors on results and then showed them these results. Furthermore, we also asked them to what extend they were surprised by the results and how certain they were when making their guesses. Each branch manager was solely asked about the intervention that was implemented in his/her branch, respectively, i.e., either about the Subsidy intervention or about the Risk-Coverage intervention.

1.C.2 Compare Priors to Actual Values

First, consider Figure 1.C.1. Figure 1.C.1 compares the average guesses of branch managers on the uptake rates to the actual uptake rates. This comparison is done for those who received the treatment as well as for those those who did not, for each intervention type, respectively. As the Figure shows, on average, branch managers slightly overestimate both the effectiveness of treatments for both interventions as well as the uptake of renewable energy loans in general. However, on average, branch managers' priors are not far away from the actual values.

Next, consider Figure 1.C.2 corresponding to one question in the baseline data collection in which we asked respondents about their reasons for their loan application. Respondents were able to mention more than one reason. The percentages of the light-grey bars shown in Figure 1.C.2 correspond to the share of individuals who actually mentioned a respective reason. Similarly, we asked branch managers to guess for how many individuals each reason was important when applying for the loan, respectively. The Figure shows that branch managers' priors differ substantially from actual values: On average, they believe a variety of reasons to be important for a loan application. However, for loan applicants, there are mainly two reasons inducing their application: First, solar panels provide cheaper electricity, which is important to almost everyone; Second, solar panels

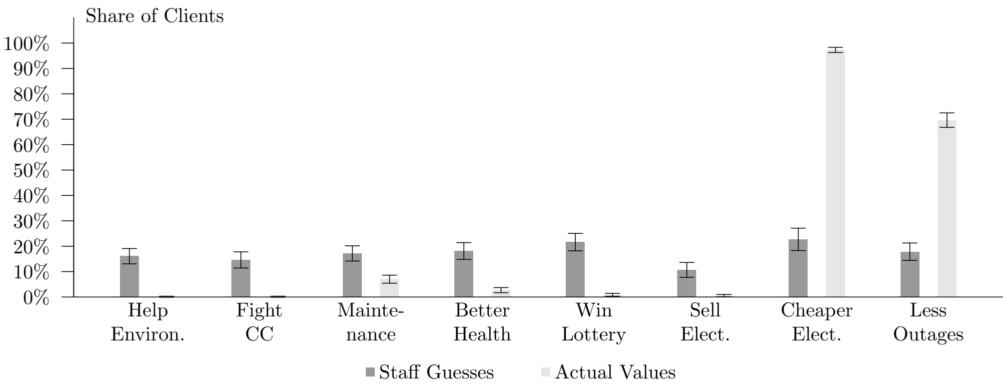
Figure 1.C.1: Loan Uptake: Staff Priors and Actual Values



Note: The figure shows the average guesses of branch managers on uptake rates for both interventions and for treated and control individuals, respectively. Also actual values are shown. Confidence intervals correspond to a 5% significance level.

reduce the impact of power cuts (outages) from the grid, which is important to more than 70% of loan applicants.

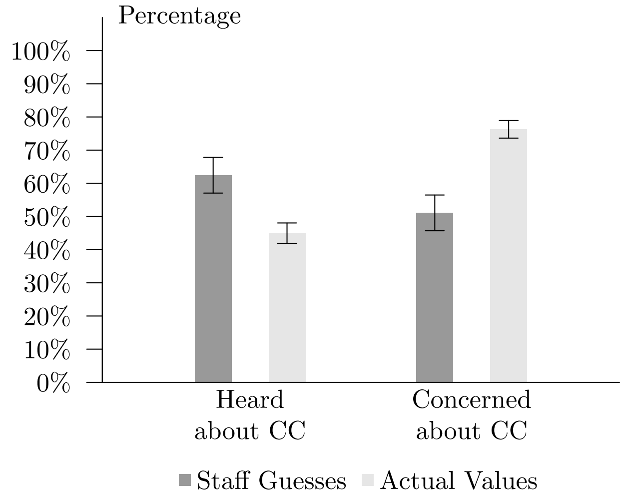
Figure 1.C.2: Application Reasons: Staff Priors and Actual Values



Note: The figure shows the averages of branch managers guesses on the share of applicants for whom this reason is an important reason to apply for a renewable energy loan. Moreover, the actual shares of individuals is displayed for whom each application reason was important when applying for the loan. Confidence intervals correspond to a 5% significance level.

Finally, consider Figure 1.C.3, which corresponds to two questions in the baseline survey in which we first asked individuals whether they have already heard about the terms climate change or climate crisis. Furthermore, we asked those who said they have already heard something about at least one of these two terms, whether they are concerned about climate change. The Figure shows that branch managers overestimate the share of those who have already heard something about climate change, but largely underestimate the share of those who are concerned about climate change. In fact, 75% of loan applicants who know the term climate change were concerned about it at time of baseline.

Figure 1.C.3: Climate Change: Staff Priors and Actual Values

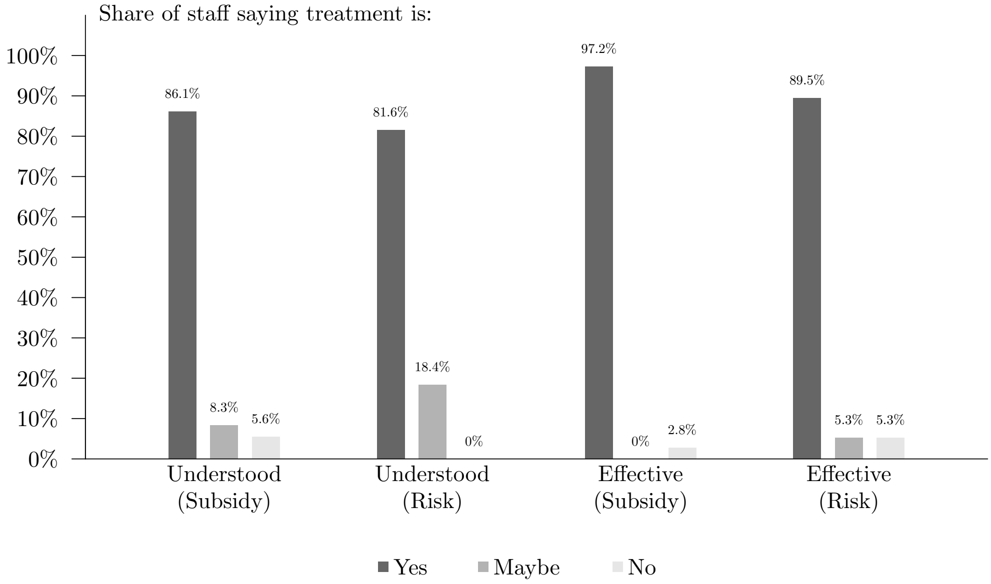


Note: The Figure shows the share of loan applicants who have (1) heard about the terms climate change (CC) before and (2) who are concerned about climate change. For each of these shares, we asked branch managers to guess the shares for both values, and show the averages of these guesses here. Confidence intervals correspond to a 5% significance level.

1.C.3 Perceptions on Understanding and Effectiveness

Besides asking branch managers about their priors on treatment effects, we also asked them about their perception of whether clients (1) understood the treatment interventions, and (2) whether they believe the treatment interventions were effective in increasing the uptake rates of renewable energy loans. The results are presented in Figure 1.C.4 and show the share of branch managers who said “yes”, “maybe” or “no” when asked whether they think the intervention was understood (and effective) for each treatment intervention, respectively. Interestingly, for both interventions, more than 80% branch managers believe that the intervention was understood and there is not a single manager in the Risk-Coverage intervention who said that clients did not understand the intervention. Moreover, 97.2% of branch managers believe the Subsidy intervention to be effective in increasing the uptake rates and 89.5% believe the Risk-Coverage intervention to be effective in increasing uptake rates.

Figure 1.C.4: Staff: Intervention Understanding and Effectiveness



Note: The figure shows the share of branch managers for each of the two treatment interventions who says “yes”, “maybe” or “no” when asked whether the treatment intervention is understood by clients and whether it is effective in increasing uptake rates, for each of the two interventions, respectively.

1.D Construction of Index Insurance

In this section, we outline the construction of the index insurance scheme using weather data. We obtain a variable called *solar energy* from the weather data provider, which measures the energy emitted by the sun that reaches the Earth's surface at a specific location over a given time period. First, we use data from German solar power plants and their corresponding weather data to demonstrate that the solar energy variable is a reliable proxy for the actual return of a solar panel. Second, we utilize the solar energy variable along with other weather data from Pakistan to: (i) quantify the impact of seasonal and weather factors on the return of solar panels in Pakistan; (ii) analyze the risks associated with the return of solar panels in Pakistan; and (iii) develop an insurance scheme.

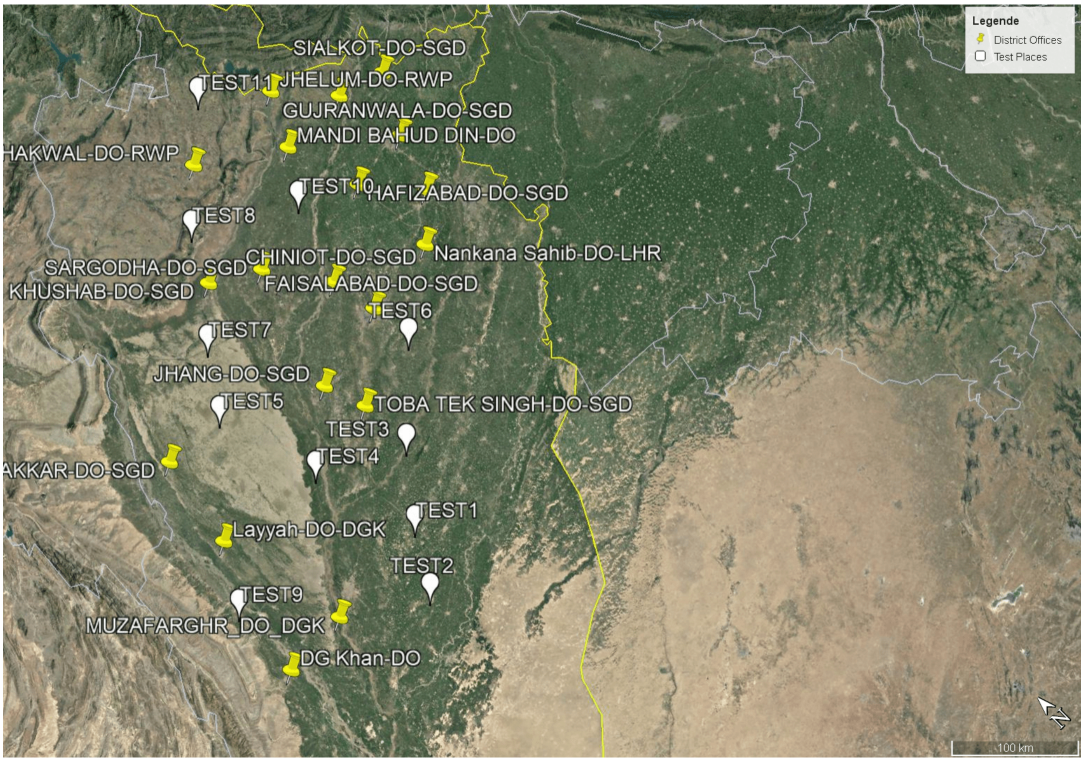
1.D.1 Data and Descriptive Statistics

The main dataset, referred to as "Panel A", consists of daily historical weather observations for 30 locations in Pakistan, sourced from the weather data provider.[42] To construct this dataset, we selected 30 locations within the study area. We used data from the entire study area because the selection of districts for the Risk-Coverage scheme was made after calculating the insurance scheme. Figure 1.D.1 illustrates these locations. The 30 locations include all district offices within the study area and additional sites to ensure wide geographic coverage of Punjab. The selection was made in close consultation with NRSP. For each of the 30 locations, we downloaded daily weather information from January 1, 2010, to December 31, 2021, resulting in 131,490 observations. The weather on any given day is described by several characteristics, as detailed in Panel A of Table 1.D.1.

The second dataset, referred to as "Panel B", is a merge of several datasets and comprises a panel of German solar power plants. In Germany, data about "larger" solar power plants (see details below) is provided on a public platform (`www.netztransparenz.de`) to ensure transparency regarding the subsidies these power plants receive. To obtain a valid sample from the panels documented on the German transparency platform for subsequent analysis, we apply several restrictions: First, we restrict the analysis to power plants with a total capacity above 30 kWp (kilowatt peak), as the exact addresses of these plants are known. This allows us to determine the precise weather conditions for each power plant at any given time. Second, we only consider power plants that cannot be turned off.

[42] The weather data provider is `www.visualcrossing.com`.

Figure 1.D.1: Map of Punjab with Weather Sample Locations



Map showing the 30 places in Punjab which are used to construct the daily weather dataset.

This restriction is made for technical reasons: Besides the non-regulable power plants, there are other plants that can be remotely turned off by electric-network operators if they produce excess electricity.[43] Including these plants in the analysis could bias the estimation of the relationship between solar energy and actual electricity production. Third, we restrict the analysis to the year 2021, as this is the only year for which monthly data on electricity production was available at time the insurance was constructed. In previous years, data was only available on an annual basis due to new transparency rules that came into effect in 2021. We complement this selected sample of solar power plants with further datasets. Address information for the selected sample of solar power plants from 2021 was downloaded from the "EEG-Anlagenstammregister"(`https://www.netztransparenz.de/EEG/Anlagenstammdaten`), and data on monthly energy production for each solar power plant was downloaded from the "EEG-Jahresabrechnungen "(`https://www.netztransparenz.de/EEG/Jahresabrechnungen`). Using the addresses, we manually collected the exact locations with Google Maps and verified them with satellite imagery from Google Maps and Google Earth.

[43] This occurs when the electricity supply exceeds demand, and operators turn off solar power plants to balance supply and demand.

Table 1.D.1: Summary Statistics for Construction of Insurance Scheme

Variable Definition	Variable Name	Mean	sd	Min	Median	Max	N
***Panel A**: Weather in Pakistan (Daily Observations)*							
Total energy emitted from the sun reaching Earth's surface (measured in Mega-Joule per square meter)	solar_energy	19.54	5.92	2.00	20.40	31.80	131,490
Share of hours with non-zero precipitation	precipcover	3.48	9.56	0.00	0.00	100.00	131,490
Share of sky covered in clouds throughout the day	cloudcover	23.07	23.23	0.00	16.00	99.60	131,490
Time between sunrise and sunset	daylight	12.17	1.42	9.95	12.19	14.35	131,490
Distance (in miles) at which distant objects are visible	visibility	2.53	1.45	0.00	2.5	151.20	104,600
***Panel B**: German Solar Power Plants and Weather (Monthly Observations)*							
Panel capacity (kWp)	panel_max _kwp	358.00	740.37	30.00	142.80	7,870.28	6,485
Electricity produced (kWh, in thousand)	electricity _produced	23.56	57.18	0.00	6.82	1,316.27	6,485
Utilized capacity throughout the month	utilized_capac	0.10	0.06	0.00	0.10	0.27	6,485
Total energy emitted from the sun reaching Earth's surface (measured in Mega-Joule per square meter)	solar _energy	316.34	209.99	0.00	315.50	865.40	6,485
Share of hours with non-zero precipitation	precipcover	10.04	4.80	1.67	9.27	31.45	6,485
Share of sky covered in clouds throughout the day	cloudcover	51.61	16.16	0.00	51.03	83.65	6,485
Distance (in miles) at which distant objects are visible	visibility	17.10	4.06	5.08	17.96	26.96	6,443

Note: Descriptive statistics for the two datasets used to construct the index insurance scheme. Panel A is the panel dataset containing daily weather information for 30 locations in Punjab. Panel B is the panel dataset about German solar power plants which is a merged dataset from return data for these plants as well as weather information for the locations of these plants.

We then used these locations to download daily weather data for each solar power plant in the selected sample for 2021 from our weather data provider (`www.visualcrossing.com`) and aggregated it to a monthly level. The resulting dataset is an unbalanced panel following 573 German solar power plants for up to 12 months in 2021, totaling 6,485 observations at the month-power-plant level. Descriptive statistics for this dataset are provided in Panel B of Table 1.D.1. In addition to the weather data, two variables related to the solar power plants require specific attention: First, *panel_max_kwp* reports the kilowatt peak (kWp), of a plant, which is the maximum kilowatt output the plant can produce in one hour under optimal conditions. Second, *utilized_capacity* is measured monthly for each power plant. This variable represents the kilowatt hours (kWh) produced in a month divided by the maximum kWh the plant could have produced under optimal conditions (assuming 24 hours of sunlight per day). While it is possible to calculate the maximum kWh using the exact hours of daylight in a month, this is not necessary as we include month-fixed effects and control for daylight hours in the regressions.

1.D.2 Solar Energy Variable as Proxy

In this subsection, we conduct regression analyses using the Panel B data on German solar power plants to verify that the solar energy variable is a reliable proxy for the actual electricity production of a solar panel at a specific location and time. We consider the following specification:

$$\text{utilized\_capacity}_{im} = \alpha + \beta \text{solar\_energy}_{im} + \gamma X_{im} + \eta_i + \zeta_m + \varepsilon_{im} \tag{1.3}$$

where we regress *utilized_capacity* of solar power plant i in month m on: a set of plant- and time-varying control variables X_{im}; on solar_energy$_{im}$, which is the solar energy measure as explained above at the geographical position of power plant i in month m; and on month- as well as plant- fixed effects, ζ_m and η_i. The main coefficient of interest is β, which reports the strength of the relation between the solar energy as reported by the weather data provider and the actual energy produced by the power plants.

The estimation results for specification (1.3) are presented in Table 1.D.2. Solar energy has a positive and significant effect on utilized capacity across all specifications. Notably, in Column (1), solar energy alone, without additional control variables or fixed effects, yields an adjusted R^2 of 0.713. Including month-fixed effects substantially reduces the coefficient for solar energy. This reduction can be attributed to the significant variation in solar energy across calendar months. The coefficients for both precipitation and cloudcover are negative and stable across all specifications. However, these variables have minimal explanatory power, as indicated by the negligible change in R^2 when they are added. Initially, this may seem puzzling, as the weather data provider reports that the solar energy variable already accounts for cloud cover in determining the energy reaching the Earth's surface. This would suggest that the coefficients for cloudcover and should not be significantly different from zero once solar energy is included. The significant effects observed in the regressions are due to the differing granularity of the variables. The solar energy variable is constructed from data recorded over an area, which averages out localized effects such as clouds[44]. In contrast, cloudcover, precipitation and visibility have finer granularity, providing additional explanatory power in the regressions.

Table 1.D.2: Determinants of Utilized Capacity of German Solar Power Plants

	(1)	(2)	(3)	(4)
	b/se	b/se	b/se	b/se
solarenergy	0.025***	0.028***	0.006***	0.003***
	(0.000)	(0.000)	(0.001)	(0.000)
precipcover				-0.113***
				(0.010)
cloudcover				-0.066***
				(0.006)
visibility				0.136***
				(0.015)
N	6484	6476	6476	6435
adj. R^2	0.713	0.873	0.924	0.934
Location FE		✓	✓	✓
Month FE			✓	✓

Note: Results for specification (1.3), using the panel of German solar power plants. The dependent variable is utilized capacity, which is defined in the main text. The further variables are defined in Table 1.D.1. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$. FE stands for fixed effects.

Next, we need to verify that the solar energy data is comparable between Pakistan and Germany, ensuring that the variable has consistent properties in both countries. One potential issue is that weather data measurement methods might differ between the two regions. Both German and Pakistani data are gathered using remote sensing techniques and weather stations. However, Pakistan has fewer stations per square kilometer, relying more on remote sensing. The weather data provider has confirmed that observations from stations and remote sensing data are directly comparable, with multiple investigations supporting this conclusion.[45]

Given these findings, the solar energy variable accurately measures the potential return of a solar panel at a specific location over a day or month. It serves as a reliable approximation for solar panel returns in Germany and is comparable

[44] Source: communication via mail with support service of weather data provider. Available upon request.

[45] The weather provider has explained to us that "the observation based solar data [i.e., from the stations] and the remote (satellite) data is directly comparable [...]. Multiple investigations have concluded that the two are comparable". Source: communication via mail with support service of weather data provider. Available upon request.

across countries, indicating it can also approximate actual electricity production of solar power plants in Pakistan.

1.D.3 Solar Energy in Pakistan

In this subsection, we aim to identify weather characteristics that can be used as indices for insuring the returns of solar panels in Pakistan with an index insurance scheme. These characteristics should meet three criteria: first, the weather characteristics must have a sizable relation with the solar energy measure to ensure the index insurance covers meaningful variations in solar panel returns. Second, the weather characteristics must be easily observable by entrepreneurs to make the insurance calculations transparent. Third, for the insurance to be non-deterministic, the weather characteristics should vary sufficiently well in order to insure an "unpredictable" risk of solar panel returns. If, for instance, there would only be clouds in January in Pakistan, an index insurance relating to clouds would essentially be a deterministic payment in January.

As a first step, to identify variables that have a strong impact on the solar energy measure, we consider the following specification:

$$solar\_energy_{lt} = \alpha + \beta X_{lt} + \eta_l + \gamma_t^y + \gamma_t^m + \varepsilon_{lt} \tag{1.4}$$

where $solar\_energy_{lt}$ is equal to the solar energy measure as discussed above at location l on day t, X_{lt} denotes a set of time-varying characteristics at location l on time t, and η_l, γ_t^y and γ_t^m are location-year and calender-month fixed effects.

Table 1.D.3 presents the estimation results for specification 1.4. The explanatory power of the variables is very high, with an adjusted R^2 well above 0.8 in all specifications, indicating that seasonality effects as well as varying weather conditions explain the returns of solar panels very well. The variable cloudcover has a negative stable coefficient across all specifications. The estimated results suggest that moving from a cloud-free day to a day with complete cloud cover reduces solar energy by 7-8 units, which is more than one standard deviation of the solar energy variable (around 5.92) in the sample. The coefficient size does not change once including fixed effects and controlling for visibility. Visibility also affects the solar energy measure, though to a lesser extent. Decreasing visibility by one standard deviation (1.45 units) reduces solar energy by 0.19 to 0.23 units when controlling for fixed effects. Notably, including fixed effects significantly changes the visibility coefficient but not the cloud cover coefficient. This suggests

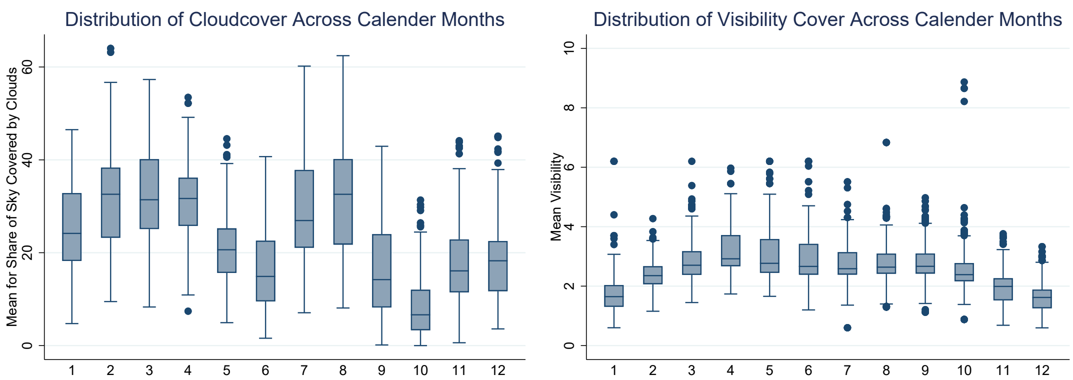
Table 1.D.3: Determinants of Solar Energy at Pakistan Sample Places

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
cloudcover	-0.072***	-0.075***	-0.079***	-0.080***	-0.082***	-0.081***	-0.084***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
daylight	3.730***	3.693***	3.770***	3.770***	3.774***	3.779***	3.774***
	(0.004)	(0.005)	(0.026)	(0.026)	(0.028)	(0.025)	(0.028)
visibility		0.243***			0.132***		0.159***
		(0.005)			(0.005)		(0.005)
N	131490	104600	131490	131490	104600	131490	104600
adj. R^2	0.849	0.856	0.879	0.881	0.884	0.885	0.888
Month FE			✓	✓	✓	✓	✓
Year FE				✓	✓	✓	✓
Location FE						✓	✓

Note: Results for specification (1.4), using Panel A of weather data from Punjab. The dependent variable is solar energy, which is defined in the main text. The further variables are defined in Table 1.D.1. Standard errors are robust to heteroscedasticity. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$. FE stands for fixed effects. In each cell, pcor denotes the squared semipartial correlation between the outcome and the respective variable, i.e. the partial R^2.

that cloud cover varies less seasonally than visibility, as the latter's effect is partly captured by seasonal fixed effects. In fact, Figure 1.D.2, shows that clouds can occur in any month in Pakistan, with high monthly variance. In contrast, visibility varies less within months and has a stronger seasonal component.

Figure 1.D.2: Variations of Weather Across Calender Months.



Note: The figures illustrate variables from Panel A from the data used to construct the index insurance scheme. Variables are defined in Table 1.D.1. For the illustration, the data was first aggregated from the day-level to the month-level. The figures show boxplots for each variable, respectively, for different calender month (January = 1, ..., December=12) across the period of analysis.

To summarize, both cloudcover and visibility significantly impact the solar energy measure, vary across months and are easily observable, making them suitable for index insurance. The cloudcover variable has a greater impact and varies more than visibility. However, from qualitative consultations of local NRSP staff during the insurance design phase, we learned that the visibility variable is important for entrepreneurs in Punjab, because heavy fog affects people in Punjab regularly. The weather data provider does not provide direct data on fog, but only on visibility on the ground, which is low when there is fog. For instance, on a day with heavy fog, it is usually impossible to travel across cities in Punjab. Also during the insurance piloting phase, stakeholders frequently asked if the insurance would also cover variations in solar panel reduction due to heavy fog. Therefore, we decided to relate the index insurance not only to cloudcover but also to visibility.

1.D.4 From Data to Insurance Scheme

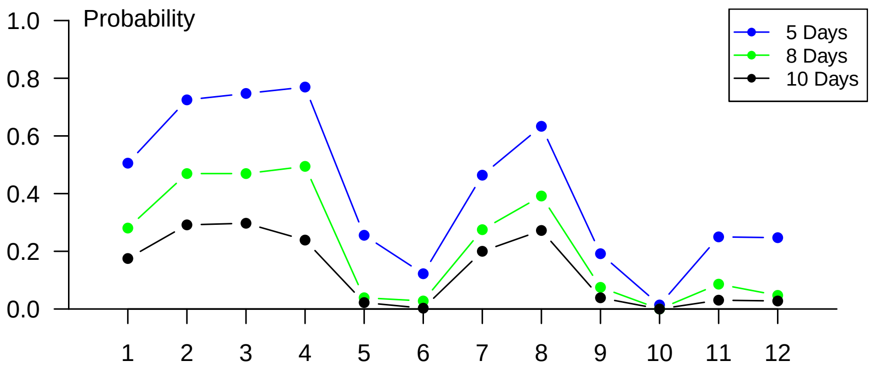
As identified in the previous subsection, both cloud cover and visibility are suitable as the indices for the insurance scheme. To make the insurance scheme easy to understand, we base the insurance payouts on "cloudy days" and choose the following insurance scheme:

> ***Insurance Scheme:*** A day is considered to be a "cloudy day" if more than half of the sky is covered by clouds and/or if the visibility on the ground is less than two miles. If within one month, there are more than 8 cloudy days, the insurance taker receives a payout equal to k% of her monthly loan repayment. Insurance receivers can receive at most four payouts.

A key benefit of the suggested insurance policy is the simplicity of the conditions under which a payout happens: First, "one half" is a straightforward concept, even for those with little education. Second, a "cloudy day" is no abstract concept and is easy to observe. As indicated earlier, the strong and clear relationship between cloudy days and solar panel returns makes the benefits of this index insurance easy to grasp.

Figure 1.D.3 shows the share of locations that would have received a payout during the analyzed years for each calender month for different thresholds of cloudy days in the suggested insurance policy. For instance, the first point for the threshold of 5 cloudy days (blue line) indicates that approximately 50% of locations would have received a payout in January each year during the period of analysis. Hence, Figure 1.D.3 can be interpreted as showing the probability of a

Figure 1.D.3: Insurance Payout Share Across Calender Months

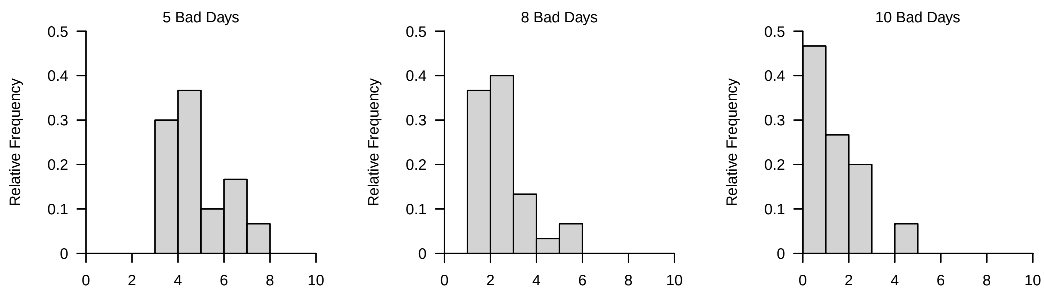


Note: The Figure shows the share of locations that would have received a payout in each calender month (January = 1, ..., December=12) across the period of analysis. This share is interpreted as the payout probability in each month for different insurance rules, where the number of days which need to be cloudy per month are varied.

insurance receiver to receive a payout in a certain calender month under different thresholds. While there is some seasonality, no month guarantees a payout for everyone. This confirms that the suggested payout scheme is not deterministic ex ante but rather insures against an unpredictable risk.

One might worry that payouts are concentrated in just a few locations, but Figure 1.D.4 shows this is not the case. Figure 1.D.4 displays the distribution of annual payouts per location for each threshold. For example, with an 8-day threshold, 40% of locations could expect to receive 2 to 3 payouts per year.

Figure 1.D.4: Distribution of Number of Payouts per Year

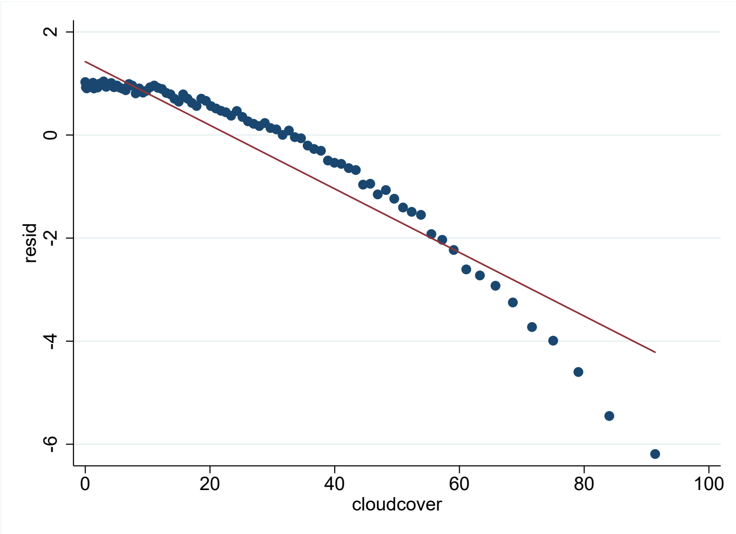


Note: Distribution of the number of payouts that a place receives per year depending on the required number of cloudy days (called “Bad Days” in the header) stipulated in the insurance contract.

The 8-day threshold was chosen in collaboration with NRSP to cover a significant portion of the risk. We also set a maximum of four payouts per insurance receiver to avoid excessively high payouts that could strain NRSP. Lastly, the choice of k was conducted such that for each given loan size, the expected insurance payouts equal the upfront Subsidy payment in the Subsidy intervention scheme, to make the schemes roughly comparable in terms of their financial benefits.

1.D.5 Functional Form Solar Output and Cloudcover

Figure 1.D.5: Functional Form Solar Output and Cloudcover



Note: This Figure shows the binned scatter plot between the residuals from a regression of the solar energy variable (daily weather observations, Panel A, compare Table 1.D.1) on day-of-the-year fixed effects throughout the period of analysis (vertical axis) to the cloud-cover variable (horizontal axis). The red line shows the linear regression fit of the binned scatter points.

1.E Balance of Covariates

1.E.1 Balance of Districts

Table 1.E.1: Balance Table Across Districts

Variable	Risk-Cover Districts	Other Districts	Difference
Agriculture Loans (Number)	2570.89	1598.12	-972.76
	(4769.05)	(2402.85)	(1870.89)
Agriculture Loans (Amount)	1.62e+08	8.90e+07	-7.26e+07
	(3.07e+08)	(1.34e+08)	(1.18e+08)
Livestock Loans (Number)	4203.22	4072.88	-130.35
	(4911.36)	(5884.04)	(2617.70)
Livestock Loans (Amount)	1.29e+08	1.26e+08	-2.88e+06
	(1.30e+08)	(1.71e+08)	(7.30e+07)
Enterprise Loans (Number)	5857.89	11888.00	6030.11
	(4732.47)	(12278.36)	(4408.12)
Enterprise Loans (Amount)	1.72e+08	3.45e+08	1.73e+08
	(1.33e+08)	(3.56e+08)	(1.27e+08)
Observations	9	8	17

Note: These numbers are constructed from the aggregated loan data from NRSP on the district level. The randomization of districts was done before the baseline data collection. In each cell, the table shows the average across districts as well as the standard deviations. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

1.E.2 Balance of Applicants

Table 1.E.2: Balance Table of Applicants

	Applicants for Subsidy Scheme			Applicants for Risk-Cover Scheme		
Variable	Control	Treatment	Difference	Control	Treatment	Difference
spend	12799.69	14696.89	1897.20	12773.06	10480.94	-2292.11
	(18006.69)	(42748.82)	(3124.17)	(22250.87)	(13733.47)	(1708.33)
spend_usually	12398.65	14977.50	2578.85	11416.24	9698.30	-1717.94
	(16324.21)	(38678.09)	(2827.50)	(18039.64)	(12739.83)	(1442.69)
elect_connect	0.95	0.90	-0.05**	0.90	0.90	0.00
	(0.22)	(0.30)	(0.02)	(0.30)	(0.29)	(0.03)
gen_elect_generator	0.08	0.08	-0.00	0.08	0.05	-0.03
	(0.27)	(0.27)	(0.03)	(0.26)	(0.21)	(0.02)
bus_elect_tools_2	5.63	5.30	-0.34	4.71	4.99	0.28
	(5.87)	(5.73)	(0.57)	(4.07)	(6.18)	(0.50)
profit	73443.18	72611.11	-832.07	64651.71	58191.18	-6460.53
	(73565.98)	(88997.92)	(7827.44)	(55183.11)	(52297.27)	(4950.27)
income_hh_total	164553.33	187087.08	22533.75	153324.92	145344.95	-7979.98
	(253175.30)	(498036.12)	(37445.30)	(116001.48)	(116951.25)	(10643.91)
profit_var	66244.29	57702.32	-8541.97	52049.79	52397.87	348.09
	(91161.91)	(95588.23)	(8969.14)	(66059.81)	(108919.46)	(8329.18)
business_seasonal	0.08	0.11	0.03	0.11	0.11	0.00
	(0.27)	(0.31)	(0.03)	(0.31)	(0.32)	(0.03)
bisp	0.17	0.19	0.02	0.23	0.29	0.06
	(0.38)	(0.39)	(0.04)	(0.42)	(0.45)	(0.04)
income_hh_agriculture	0.80	0.93	0.14	0.87	0.90	0.03
	(1.09)	(1.21)	(0.11)	(1.21)	(1.32)	(0.12)
income_hh_business	1.98	1.91	-0.07	2.03	2.18	0.15
	(1.18)	(1.16)	(0.11)	(1.18)	(1.31)	(0.11)
saving_lastmo	32456.82	34859.45	2402.63	32700.00	27918.49	-4781.51
	(33774.05)	(50975.71)	(4142.40)	(35015.66)	(32794.40)	(3120.29)
savings_avg	33894.50	34213.95	319.46	32480.85	27891.18	-4589.67
	(31772.85)	(47173.08)	(3870.51)	(33963.54)	(28886.25)	(2900.78)
shock_coping	0.95	0.96	0.01	0.98	0.95	-0.02
	(0.22)	(0.20)	(0.02)	(0.14)	(0.21)	(0.02)
shock_cover_poss	1.25	1.24	-0.01	1.32	1.27	-0.06
	(0.60)	(0.53)	(0.05)	(0.57)	(0.57)	(0.05)
risk_avg	23.56	23.53	-0.04	21.31	22.04	0.74
	(11.06)	(10.48)	(1.13)	(11.59)	(11.54)	(1.16)
time_avg	5.92	5.70	-0.21	5.26	5.63	0.37
	(10.04)	(10.28)	(1.05)	(9.66)	(10.23)	(0.98)
powercut	0.92	0.84	-0.08**	0.85	0.81	-0.04
	(0.28)	(0.37)	(0.03)	(0.36)	(0.39)	(0.03)
typcmonth_elect	9.03	9.55	0.52	7.04	6.72	-0.32
	(8.09)	(8.51)	(0.79)	(6.39)	(6.14)	(0.57)
solar_reducecost	1.04	1.05	0.01	1.03	1.02	-0.01
	(0.23)	(0.23)	(0.02)	(0.18)	(0.16)	(0.02)
solar_health	1.03	1.05	0.01	1.01	1.01	-0.00
	(0.17)	(0.21)	(0.02)	(0.11)	(0.11)	(0.01)
time_business	68.02	70.37	2.35	67.39	67.13	-0.26
	(24.83)	(25.50)	(2.38)	(26.22)	(26.24)	(2.41)
invest_future	0.42	0.39	-0.03	0.41	0.43	0.02
	(0.50)	(0.49)	(0.05)	(0.49)	(0.50)	(0.05)
Observations	247	235	482	263	255	518

The variables are defined as: spend: electricity spendings last month; spend_usually: electricity spendings typical month; elect_connect: indicator: business connected to electricity grid; gen_elect_generator: indicator: respondent owns a generator; bus_elect_tools_2: number of electric machines/tools in business; profit: profit from business last month; income_hh_total: total monthly household income; profit_var: profit variability: best profit in last six months minus worst profit in last six month; business_seasonal: indicator: business is seasonal; bisp: indicator: respondent is Bisp beneficiary (poverty program of Pakistan government); income_hh_agriculture: number of people in household receiving income from agriculture; income_hh_business: number of people in household receiving income from employment/business; saving_lastmo: savings last month; savings_avg: Savings typical month; shock_coping: indicator: Respondent has shock coping mechanisms; shock_cover_poss: number of shock cover possibilities that respondent has; risk_avg: risk-aversion measure: average value of risk-aversion (we used a questionnaire module developed by Falk et al. (2023) to estimate the risk-aversion of entrepreneurs); time_avg: discounting factor: average value of discount factor (we used a questionnaire module developed by Falk et al. (2023) to estimate the time-discounting preference of entrepreneurs); powercut: indicator: power-cuts happened in last week; typcmonth_elect: number of power-cuts per week; solar_reducecost: respondent agrees to: solar panels reduce costs; solar_health: respondent agrees to: solar panels are healthier; time_business: total hours devoted to business per week; invest_future: indicator: respondent has serious plans to make investments. The table shows the mean value as well as the respective standard deviation in parenthesis for each variable and each group of entrepreneurs. The difference columns show the differences in means of each respective group, as well as the standard deviation of this difference. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

1.F Comparison of Studies

Table 1.F.1: Differences Between Applicants Across Studies

	Applicants			Loan-Takers		
Variable	Subsidy	Risk-Cover	Difference	Subsidy	Risk-Cover	Difference
Overall education level (1-6)	3.50	3.24	-0.26***	3.40	3.13	-0.27*
	(1.45)	(1.51)	(0.09)	(1.56)	(1.58)	(0.14)
Knowledge on math test (percentage)	0.92	0.91	-0.01	0.94	0.89	-0.05**
	(0.21)	(0.23)	(0.01)	(0.19)	(0.25)	(0.02)
Trust in NRSP	3.33	3.65	0.32***	3.57	3.81	0.24***
	(0.76)	(0.58)	(0.04)	(0.69)	(0.40)	(0.05)
Monthly household income at baseline	147704.33	146094.92	-1609.41	136334.16	141890.25	5556.09
	(104138.01)	(101006.70)	(6749.82)	(94436.02)	(101259.91)	(9264.21)
Electric tools at baseline (number)	5.08	4.57	-0.51**	4.95	4.34	-0.61*
	(3.52)	(3.12)	(0.23)	(3.80)	(3.09)	(0.35)
Time in business per week at baseline	69.02	67.22	-1.80	67.40	63.87	-3.53
	(24.51)	(25.73)	(1.66)	(27.06)	(25.71)	(2.52)
Monthly profit at baseline	68592.89	60826.27	-7766.62**	62966.67	61679.59	-1287.07
	(58083.57)	(50624.21)	(3628.68)	(53174.73)	(56915.15)	(5264.95)
Monthly electricity spending at baseline	11956.55	10131.77	-1824.78**	11836.75	10324.48	-1512.27
	(14062.67)	(13057.77)	(899.97)	(14181.24)	(13292.98)	(1318.55)
Risk aversion at baseline	23.55	21.68	-1.86**	22.15	22.37	0.22
	(10.76)	(11.56)	(0.81)	(11.42)	(11.33)	(1.20)
Time discounting at baseline	5.81	5.45	-0.36	5.45	6.12	0.67
	(10.15)	(9.95)	(0.72)	(9.76)	(10.75)	(1.06)
Observations	482	518	1,000	216	258	474

Note: Applicants refer to all entrepreneurs who applied for the loan under the Subsidy study and the Risk-Coverage study and were eligible for the loan. Loan-takers refer to all entrepreneurs who were eligible for the loan and also took up the loan, irrespective of their treatment status, i.e., irrespective of whether they were entitled to receive any benefit from the scheme or not. The table shows the mean value as well as the respective standard deviation in parenthesis for each variable and each group of entrepreneurs. The difference columns show the differences in means of each respective group, as well as the standard deviation of this difference. Significance stars refer to the significance of a t-test on the differences in means between applicants and loan-takers in the Subsidy study and those in the Risk-Coverage study. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 1.F.2: Solar System Owner By Treatment Status and Study

	Subsidy		Risk-Cover		Pilot	
Variable	Control	Treatment	Control	Treatment	Control	Treatment
Entrepreneur owns solar system at endline (0/1)	0.42	0.68	0.58	0.66	0.26	0.50
	(0.49)	(0.47)	(0.49)	(0.48)	(0.44)	(0.50)
Observations	247	235	263	255	256	246

Note: The table shows the mean value as well as the respective standard deviation in parenthesis for each variable and each group of entrepreneurs.

Table 1.F.3: Descriptives for System Owners by Treatment Status and Study

	Studies 2023/2024			Study 2022
Variable	Subsidy	Risk-Cover	Difference	Pilot
Solar panel capacity (Watt peak)	2492.74	2325.46	-167.28	1612.18
	(3127.89)	(3219.85)	(276.15)	(2622.58)
Solar panel price	183788.30	179442.20	-4346.10	133385.38
	(195227.38)	(206764.48)	(17001.18)	(170833.25)
Repair incidents of solar panel (number)	0.06	0.12	0.06**	0.18
	(0.24)	(0.33)	(0.02)	(0.39)
Difficulty in finding solar vendor (1-5)	1.89	1.75	-0.13**	1.97
	(0.68)	(0.79)	(0.06)	(0.64)
Own battery for solar electricity (0/1)	0.48	0.43	-0.05	0.51
	(0.50)	(0.50)	(0.04)	(0.50)
Battery's capacity for solar electricity	4.28	3.88	-0.40	4.37
	(2.23)	(2.28)	(0.29)	(2.60)
Expected lifetime expectancy of solar panel	14.02	11.26	-2.77***	11.28
	(5.43)	(5.71)	(0.37)	(5.93)
Observations	482	518	1,000	502

Note: The table shows the mean value as well as the respective standard deviation in parenthesis for each variable and each group of entrepreneurs. The difference columns show the differences in means of each respective group, as well as the standard deviation of this difference. Significance stars refer to the significance of a t-test on the differences in means between applicants and loan-takers in the Subsidy experiment and those in the Risk-Coverage experiment. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

1.G Randomization Details

1.G.1 District Level Randomization

The randomization of districts was implemented in three steps: First, we created a set of all possible treatment allocations such that 9 out of 17 offices are selected for the Risk-Coverage treatment. These are 24310 possible allocations, indexed by $a = 1, \cdots, 24310$. Second, for each allocation a, we calculated, for each district d respectively, the number of neighboring districts that have the same treatment status under allocation a and denoted this number Z_d^a. Thus, Z_d^a is the number of neighboring districts of district d that have the same treatment status as d under the potential allocation a. Then, we defined $Z^a = \sum_d Z_d^a$ and defined the set of possible allocations A as $A = \{a|Z^a \leq \min_a(Z^a) + M\}$, where M is a positive integer. We chose $M = 6$ to have a reasonable number of possible allocations. For $M = 6$ we then end up with $|A| = 1185$. Third, from all 1185 *possible* allocations A, we chose one of them at random which was then our final allocation.

1.G.2 Applicant Level Randomization

Within each weekly batch of applicants, the randomization was conducted in a procedure that took at maximum four steps: Within one batch, we randomly created allocations for which half of the applicants are treated and the other half are assigned to be in the control group. For each of these allocations, we did t-tests on the mean difference for all covariates reported in the balance table in Appendix 1.E.2 between treated and control applicants. The first allocation that was found for which all t-test values are smaller than 1.64 was kept as the randomized allocation. If we did not find such an allocation after two hours of creating random allocations, we continued to Step 2. In Step 2, we did the same procedure as in Step 1, but we implemented the first allocation for which *all but one* t-test was smaller than 1.64. If we did not find such an allocation after twp hours, we continued to Step 3. In Step 3, we did the same procedure as in Step 1, but we implemented the first allocation for which *all but two* t-test was smaller than 1.64. If we did not find such an allocation after two hours, we continued to Step 4. If we reached Step 4, we created one allocation for which half of the applicants in each group are treated and the other half are assigned to be in the control group, which was then implemented.

1.H Details from Fieldwork

1.H.1 Field Visits in June 2023

Further problems that were reported in the field visits were: (1) Some lack in understanding from both the credit officers and the clients of the product. More specifically, the understanding in the field of the product offered mixed results. In five out of eight Risk-Coverage districts, branch managers reported that clients have “difficulties” in understanding the product and its conditions. For instance, these branch managers reported that uneducated clients had difficulties in understanding the exact details and conditions of the Risk-Coverage scheme. Some credit officers reported that there are no difficulties (even within the same branch in which the branch managers reported problems) while others reported that there are “some” problems in understanding, which ”can usually be solved through explanations”. However, one of the credit officers reported that clients do not understand the scheme at all. As a response to these concerns, the research team conducted the retraining of all staff members involved and was confident that this retraining solved the difficulties in understanding from NRSP staff. (2) A further problem that appeared during the field visit was some confusion regarding the solar vendor registration process. The training and additional material regarding the vendor registration process that was distributed among NRSP staff clarified this confusion. (3) There were some further problems mentioned by NRSP field staff, which were all related to the economic situation including a very high inflation at that time.

1.H.2 FGDs in September 2023

Furthermore, the other results of the FGDs were: (1) Surging electricity prices posed a significant challenge for entrepreneurs, driving them to seek alternative solutions. Among these options, solar energy stood out as an appealing choice for entrepreneurs looking to evade the burden of soaring electricity costs. (2) Individuals seem to have a good knowledge of the benefits and the value of solar panels in general. (3) In general, there seemed to be a good understanding of the benefits of the treatments, even though it appeared to be non-perfect. (4) Moreover, individuals in the FGDs did not like the process of the lottery, i.e., the process of randomization, which put some of them in uncomfortable circumstances.

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Chapter 2

Climate Protest and Food Purchases: Fridays for Future's Impact on US Meat Consumption

(joint with Lisa-Marie Müller)

2.1 Introduction

Over the past decades, protests have been a ubiquitous tool employed by diverse groups worldwide to draw attention to a wide array of issues (Cantoni et al., 2024). While research has causally linked protests to shifts in societal attitudes, voting patterns, and behaviors[1], the longevity of these changes, particularly in behavior, remains unclear.[2] This uncertainty extends to protests advocating for climate action, which saw a global surge in 2019 with the "Fridays for Future"

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* Disclaimer for the analysis: Researchers' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.
The authors gratefully acknowledge the support and help of Krzysztof Pytka.
The authors especially wish to thank Antonio Ciccone, Thomas Gschwend and Maximilian Mähr. The author also wishes to thank Felix Köhler, Philipp Ager and Felix Rusche.

[1] See, for example, Branton et al. (2015), Valentim (2024), Madestam et al. (2013), Brehm and Gruhl (2024), or Acemoglu, Hassan and Tahoun (2018), among others.

[2] Although some studies indicate that attitudinal shifts can be long-lasting (see, e.g., Mazumder (2018) or Hungerman and Moorthy (2023)), to the best of our knowledge, there is no existing research on the duration of changes in consumption patterns after protests.

movement, initiated in Sweden in 2018 and followed by thousands of protests across more than 125 countries. The protests initiated by Fridays for Future urged both governments and societies at large to reduce greenhouse gas emissions. In this chapter, we ask whether the Fridays for Future protests indeed led to a reduction in greenhouse gas emissions through changes in individual grocery shopping behavior and, if so, how long these changes persisted. Specifically, we investigate whether these protests impacted meat consumption, how their impact varied across groups, and how long this impact lasted. As with all protests, a key challenge is determining whether observed behavioral changes were caused by the protests or merely coincided with them (Madestam et al., 2013). To address this challenge, we employ a novel identification strategy by demonstrating how the global Fridays for Future protest movement disseminated asymmetrically through social networks and by leveraging that asymmetric dissemination to identify the causal effects of protests on behavior.

The focus on meat consumption in this study is driven by two primary reasons. First, meat consumption is a significant contributor to greenhouse gas emissions: approximately 35% of global emissions stem from food production, with 57% of these food-related emissions attributable to animal-based foods (Xu et al., 2021). Notably, beef production alone accounts for 12% of all food-related emissions (Xu et al., 2021). The environmental impact of meat consumption is well-recognized by activists within the Fridays for Future movement. Greta Thunberg, the movement's initiator and figurehead, has abstained from eating meat since the age of eleven to reduce her carbon footprint (Simonsson, 2019). Additionally, representatives of Fridays for Future have highlighted in various interviews, as early as 2019, that they and their families consume little to no meat to minimize their greenhouse gas emissions.[3] Thus, a reduction of meat consumption can be considered as one direct demand of protesters on the society, making it an appropriate choice to check whether theses protests had any impact on their formulated targets. Second, to assess the duration of behavioral changes following a protest, it is essential to examine a regular decision of individuals. Grocery shopping, which is analyzed in this chapter, is a weekly routine for most households, making it an ideal focus for this study.

In our analysis, we integrate several datasets, including consumption panel data from NielsenIQ, social connectedness data from Meta (Facebook), and protest data from various sources. Employing a novel shift-share instrumental variable approach, we causally identify the impact of climate protests on meat consumption

[3] See, for example, Simonsson (2019), Washington Post (2019) and Washington Post (2020).

in the weeks following a protest, across different households. Specifically, we utilize the timing of protests in Sweden as exogenous shifts and the social connectedness between US counties and Swedish regions as shares. In the first stage of the instrumental variable regression, we demonstrate that the occurrence of protests in a US county during a specific week can be predicted by the interaction between protests in Swedish regions in previous weeks (shifts) and the strength of social connections between the US county and these Swedish regions (shares). In the second stage, we leverage the temporal variation of predicted protests from the first stage within each county to analyze household-level data, thereby identifying the effects of US protests on household consumption decisions. The underlying rationale is that while the timing of Swedish protests has no direct impact on US household consumption decisions, it asymmetrically influences the timing of protests in US counties based on their social connections to Swedish regions.

Our baseline specification reveals that in the week following a protest in the US, households with at least one member aged between 14 and 25—which we call "Young" households—reduce their meat consumption by up to 23% after large protest events and by up to 5.45% for any protest event. Conversely, households without young adults—which we call "Old" households—do not alter their consumption patterns. These asymmetric changes in consumption persist for up to five weeks post-protest. Several robustness checks validate the instrument and the effects in the IV regression. In the first stage, our findings remain robust across different specifications and protests from countries other than Sweden, reinforcing that protests elsewhere do not influence US protests. For the second stage, robustness tests include, among others, null findings in reduced-form regressions for not-yet treated households, null findings in placebo tests for different types of consumption, and various clustering methods, including spatial dependencies, which do not affect the results. The findings are also robust to weak-instrument inference. Thus, this study demonstrates that meat consumption changes in the short and medium term following climate protests.

Considering both our methodology as well as our findings, we are contributing to the literature studying the impact of social movements and protests on attitudes and behavior. While it was previously established in various settings that protests can affect attitudes and behavior (see, e.g. Branton et al. (2015), Carey Jr, Branton and Martinez-Ebers (2014) or Madestam et al. (2013)) and that these impacts can be long-lasting (see e.g., Hungerman and Moorthy (2023) and Mazumder (2018)), to the best of our knowledge, there is no study investing the short-term consumption impacts of protests. Considering not only protests but social movements in general, however, there is some evidence that consumption can change through these movements (see, e.g. Levy and Mattsson (2024)). Moreover, it is also well established that societal or political events can influence consumption choices of individuals: For instance, Pandya and Venkatesan (2016) show that a US-French political dispute led to less consumption of french-sounding brands in the US and Nardotto and Sequeira (2025) show that the Brexit influenced the preferences of British consumers. However, also these works did not—to the best of our knowledge—determine the duration of these impacts. Besides the more general literature on the impacts of protests, there is also a small but growing literature studying the impacts of Fridays for Future protests. Some authors show that the Fridays for Future protests changed voting outcomes in the short and medium-run (Valentim, 2024; Fabel et al., 2025; Böken, 2023). Furthermore, it is also documented that the Fridays for Future protests changed political attitudes (Brehm and Gruhl, 2024; Flörchinger et al., 2025) and influenced the decision of consumers to buy cars which are more environmentally friendly in Italy (Marini and Nocito, 2025). Our major contribution to the whole literature studying the impact of protests is the introduction of our novel identification strategy. While previous papers have relied on the usage of the weather to instrument for protest participation to establish causality (see, for instance, Böken (2023), Fabel et al. (2025), Marini and Nocito (2025), Hungerman and Moorthy (2023), Madestam et al. (2013) or Klein Teeselink and Melios (2025)), we use the asymmetric spread of protests across social networks to establish causality. To the best of our knowledge, we are the first who use a shift-share instrumental variable with asymmetric protest dissemination to find the causal effect of protests on some outcome. This potentially opens the door for other research projects that could use this idea to study the effects of protests in other settings. It should also be noted that other papers using the weather were investigating the intensive margin of protests by predicting the number of *protesters* with the weather.[4] In our setting, however, we are searching for an instrument for the extensive margin of protests, i.e., for the question of whether there is any protest or not. Even though the weather might be a very good predictor about how many individuals will show up at a protest, it might be less appropriate when asking whether there is a protest or not, because even when the weather is bad, it is probably unlikely that no one will show up. Moreover, recent research has cast doubt on the validity of the weather as an instrument in general (Mellon, 2021). Finally, by considering the first stage of our instrumental variable regression, we establish

[4] To the best of our knowledge, there is no paper that uses the weather as an instrument to predict the extensive margin of protests, i.e., to predict whether there is any protest or not.

that the protest activity spread through social networks across the globe. Hence, with our results, we contribute to the literature studying the spread of protests through social networks (see, e.g., Qin, Strömberg and Wu (2024), Enikolopov, Makarin and Petrova (2020) or Fergusson and Molina (2019)).

It is possible to identify different—even though very closely related—rationales for the empirical findings in this chapter. These rationales relate to the literature around social norms and to the literature around identity. Considering the literature around social norms first, it is well established that individuals are so-called "conditional cooperators" (Fischbacher, Gächter and Fehr, 2001), willing to sacrifice utility for a public good when others are also sacrificing some of their utility. A finding that is confirmed in several experimental settings studying so-called "social-norm interventions", interventions which are typically information treatments about existing norms (see, e.g. Krupka and Weber (2013), and Farrow, Grolleau and Ibanez (2017) or Constantino et al. (2022) for summary articles). To explain why Fridays for Future protests impact meat consumption of Young households with the literature around social norms, one must consider protests as a "social-norm intervention" and the reduction of meat consumption as a social norm. In fact, it might be that young individuals were just not aware that reducing personal greenhouse gas emissions is a social norm that other individuals share, before they experienced a protests. Misperceptions about social norms are very widespread (see Bursztyn and Yang (2022) for a review). This is especially true for social norms around climate policies. Andre et al. (2024) show that people in the US "heavily underestimate" the willingness of others to act against climate change and Mildenberger and Tingley (2019) document that politicians in the US hold "wrong" beliefs resulting in a "underestimation of pro-climate positions". Thus, environmental protests might in fact be considered as delivering information that individuals did not have, i.e., that reducing greenhouse gas emissions is a social norm. Furthermore, it is also well-established in several settings that the correction of misperceptions about social norms can change the behavior of individuals (see, e.g. Bursztyn, González and Yanagizawa-Drott (2020), Mildenberger and Tingley (2019), Geiger and Swim (2016) and Bursztyn and Yang (2022) for a summary on these findings). Andre et al. (2024) show that correcting the misperceptions of those who thought others are not willing to fight climate change in the US increases their donations to a climate fund. Thus, one explanation for the empirical results in this chapter is that protests informed individuals about a "climate norm" that already existed and made individuals adjust their behavior. Another explanation for the findings is that the protests did not inform individuals about an *existing* norm but rather *established* a new norm around meat consumption that did not exist before. Across different disciplines, it is documented that there can be "tipping points" in a society at which social norms abruptly change (Nyborg et al., 2016). For instance, Bursztyn, Egorov and Fiorin (2020) use experiments to show that Donald Trump's local popularity made individuals with xenophobic views more comfortable in expressing these views to the public and conclude that "social norms can quickly shift as a result of private opinions being aggregated and diffused [...]". Similarly, Gulino and Masera (2023) show that local corruption scandals by politicians in Italy lead to more dishonest behavior of consumers in supermarkets. Environmental protests might be a form of "opinion aggregation" that shift social norms and thus let individuals adjust their consumption behavior. However, these two explanations around social norms might not fully explain the asymmetric response of Young and Old households. If the protests are "information treatments" about a *universal* social norm, it is not clear why the there should be no effect on older people. The zero finding for the Old households could then either be explained by (1) arguing that there are different norms among young people than there are among old people,[5] or by (2) arguing that the older generation missed the information about the protests, or by (3) arguing that the older generations have already been aware of that norm—and followed that norm—before, or by (4) arguing that the older generation does not care about the norm, in contrast to the younger generations. The last two explanations seem implausible, considering the results of Andre et al. (2024), who do not find such asymmetric effects by different age groups in the US. Moreover, it also seems implausible that the older generation missed the information about the protests, as the protests had a very strong global media coverage (see Section 2). Moreover, usage of Facebook—through which the protest spread across the world (see below)—is as prevalent among old people as it is among younger people.[6] Thus, the only plausible explanation remaining is that old and young generations follow different social norms. A very closely

[5] In fact, in the literature, social norms are not considered as rules that are always universal among all members of a society: Nyborg et al. (2016) define "a social norm as a predominant behavioral pattern within a *group*, supported by a shared understanding of acceptable actions and sustained through social interactions within that group." Similarly, in a review article, Postlewaite (2011) defines "the term social norm to describe the behavior of a *group* if the behavior differs from that of other groups in similar environments.

[6] In 2019, the share of US adults who ever used Facebook was around 70 percent for all age groups, except for the +65 year old people, where the share was still 46 percent. Source: PEW Research Center `https://www.pewresearch.org/short-reads/2019/04/10/share-of-u-s-adults-using-social-media-including-facebook-is-mostly-unchanged-since-2018/ft_19-04-10_socialmedia2019_snapchatandinstagram_2-png/` (Last Access: 27/04/2024)

related explanation following this conclusion from the social norms literature for the empirical findings in this chapter can be found in the literature on identity.[7] In contrast to the literature considering social norms, authors in the identity literature argue that individuals belong to different identity groups and adjust their consumption behavior in accordance to the "ideal" or "typical" consumption bundles of the groups they belong to (Shayo, 2020). In the US, one could think of these groups to be, for instance, "Whites", "Women", "Young People" or "Academics". Moreover, as individuals belong to several different groups with different typical consumption bundles, it is ex ante unclear to which of these groups an individual adjusts her own consumption bundle the most. Shayo (2020) and others argue that individuals adjust their behavior towards the ideal of a certain group they belong to if the membership in that specific group is becoming more salient. The membership in a group can become more salient if that specific group is in increasing conflict with another group. The existence of such a group adjustment effect has been documented in several empirical examples. Atkin, Colson-Sihra and Shayo (2021) show that increasing conflicts between different religious groups in India leads members of the religious group to adjust their consumption behavior to be "more religious"; meaning that, for instance, Muslims eat more halal food in case there is an increasing conflict between Muslims and Hindus. Interestingly, Atkin, Colson-Sihra and Shayo (2021) can document these adjustment effects even for behavior that is not publicly observable. Hence, the literature around identity goes beyond the argument that individuals signal their group membership to others (see, for instance, Kuhn et al. (2011)) and argues that individuals adjust their behavior to the group norm in order to also signal to *themselves* that they are part of a specific group. Similarly, Pandya and Venkatesan (2016) show that during a major diplomatic conflict between France and the US (when France did not follow the US in Iraq), consumers in the US bought fewer products that are appearing to be French, even when these products are not made in France. Nardotto and Sequeira (2025) showed that after the Brexit in UK, which increased the salience of the national identity in the UK, consumers bought more products that are marked to be British with flags and other salient signals on the packaging. Moreover, the existence of the identity effect, i.e., the idea that individuals adjust their consumption behavior to group norms if the salience of group membership increases has also been shown in various experiments (see, e.g., Oh (2023), Charness and Chen (2020), or Schneider (2022)). Coming back to the findings in this study, young people protesting on

[7] See Akerlof and Kranton (2000) as a starting point in this literature and Shayo (2020) and Kranton (2016) for summaries of the identity literature.

the street for more action against climate change and less meat consumption, might in fact be both: first, a sign that the ideal consumption bundle of young people consists of little to no meat; and second, a sign that there is an increasing conflict between the young and old generations, expressed through protests on the streets done by the "young" against the climate policy that is mainly done by the "old". Thus, protests might be considered as a sign of increasing conflict, which let young individuals adjust their behavior closer to the ideal consumption bundle in their group.

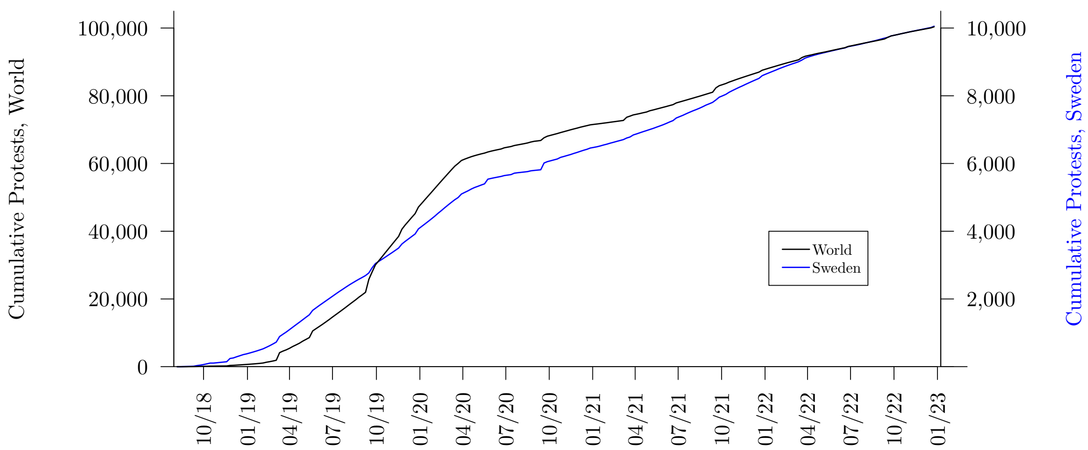
The remainder of the chapter is structured as follows. In Section 2.2, we will give more background information on Fridays for Future and how the movement spread across the world. In Section 2.3, we will explain the different datasets that we are using. Section 2.4 lays out the identification strategy and Section 2.5 presents the results. In Section 2.6, we present our robustness tests. Finally, Section 2.7 concludes.

2.2 Background

In August 2018, the 15 year old Greta Thunberg started to protest in front of the Swedish parliament. She held a sign stating "School Strike for Climate", and asked politicians to—as she put it—"prioritize the climate question, focus on the climate and treat it like a crisis" (Crouch, 2018). She continued her absenteeism from school until the general elections in Sweden on September 9th, 2018, and reduced her school strikes to happen every Friday after the election (Marquardt, 2023). During these strikes, Greta Thunberg used the Hashtag "FridaysForFuture" on her social media accounts, which later became the name of the movement she initiated. In beginning of September, the international media started to pay attention to Greta Thunberg (Crouch, 2018). In the subsequent weeks, more and more pupils joined her strikes, first in Sweden and then in the whole world (Marquardt, 2023). It were mainly pupils (age 14 to 19) who protested until spring 2019; during the year 2019, also older cohorts joined the protests (Soßdorf and Pollex, 2023). However, the Fridays for Future protests are and were mainly conduced by younger individuals. In fact, the protests were also called "Youth Strike for Climate" or "School Strike for Climate".

The extend of the spread across the world and the speed of this spread of the Fridays for Future movement was remarkable. In the literature, the Fridays for Future movement is mentioned as a "seldom" example of a global protest movement that consists of many local actors (Haunss, Sommer and Daphi, 2023). Similarly, in a summary article, Soßdorf and Pollex (2023) conclude that Fridays for Future had a "remarkably high media coverage" across the world and a "very strong mobilization impact" on younger generations. On March 15th, 2019, six months after Greta Thunberg was sitting alone in front of the Swedish parliament, there were Fridays for Future protests already on all five continents: in total, it is estimated that there were about 2,000 protests in 125 countries on a single day in March 2019 (Glenza et al., 2019).

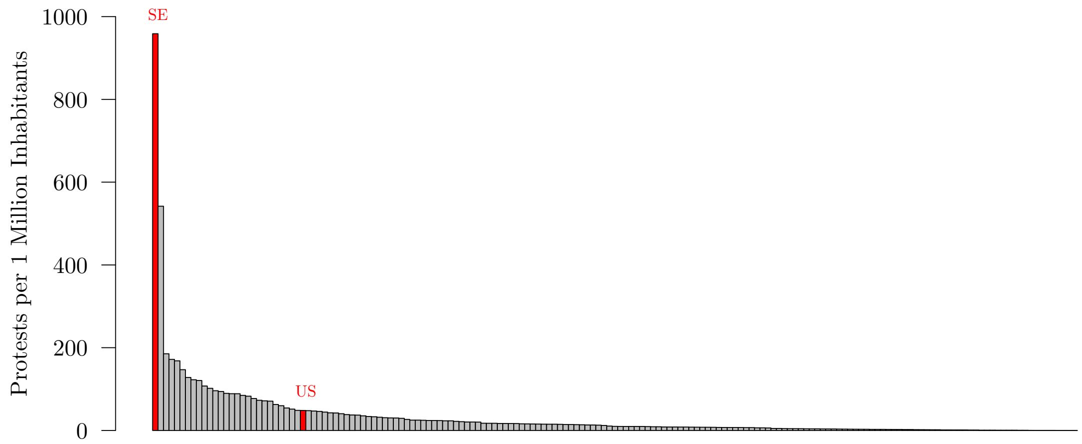
Figure 2.1: Fridays for Future Protests Over Time



Note: Cumulative number of Fridays for Future protests in Sweden (blue line) and in the whole world (black line). The axis for the blue line (Sweden) is on the right side, while the axis for the black line (World) is on the left. Source: Fridays for Future (Section 3.2).

Figure 2.1 visualizes the spread of the Fridays for Future movement in both Sweden and the whole world. The Figure shows that the movement began in Sweden and had a strong rise in 2019. Moreover, it should be noted that the number of protests continued to grow during the pandemic, even though there was a slowdown in the growth rates in spring 2020. The protests during the Covid pandemic were often so-called "shoe protests", in which the activists collected pairs of shoes from all those willing to strike and filled the floor on public places with those shoes (Belotti, Bussoletti and Donato, 2023). Finally, considering the total number of protests per 1 million inhabitants displayed in Figure 2.2, it becomes clear that Sweden can be considered to be the "center" of the protest activity.

Figure 2.2: Total Protests Across Countries



Note: Total number of protests (since the start of the protests until June 2023) per one million inhabitants for each country in which a protest took place. Each bar represents the protests per one million inhabitants for one country in which protests took place. Source: own calculations from the Fridays for Future database (Section 3.2) and population counts from World Bank (2024).

2.3 Data and Descriptives

The compiled dataset for the analysis is constructed using consumer panel data, protests data and further datasets from various sources. In the compiled dataset, the level of observation in the baseline sample is a household observed for one week. Even though the granularity of the data allows a daily analysis, the baseline specifications will be conducted on the week level, because the protests always happen on Fridays and the interpretation is convenient when considering the events on that level. Our compiled dataset starts in the week starting at August 6th, 2018, which we label as week number 1, as a matter of convenience.

2.3.1 NielsenIQ Panel Data

The main data used in this chapter is the Consumer Panel Dataset from NielsenIQ (2022). This panel dataset reports purchasing decisions of households in the US. More specifically, by using in-home scanning devices, panelists record all their purchases intended for their own usage. The data contains demographic and geographic characteristics for the panelists and their household members,[8] as well as detailed information on their purchases, like the date of the purchase and

[8] This includes, besides others: household income (in brackets), presence and age of children, marital status, age of household head(s), employment characteristics, occupation and education, as well as geographic data about the residence of the panelists, such as zip code and FIPS county codes.

product characteristics for the products purchased.[9] NielsenIQ (2022) samples the panelists at random from the US population and ensures the quality of the data by various validation and quality checks. For the analysis in this chapter, we use the NielsenIQ data from 2018 until 2021, as the Fridays for Future protests started in late 2018 and because the main activity stopped after the year 2021. The main variable of interest in the analysis is the consumption expenditure for meat products per week. To construct this main outcome variable, we sum the expenditures for each panelist for one week for all products that are classified as fresh or frozen meat, sausages, ham or bacon according to NielsenIQ. In the following, we will refer to each panelist to be a "household", as one panelist reports the purchases for her entire household. Ultimately, we end up with 9,030,426 week-household-level observations, following 86,380 households for 176 weeks. In Appendix 2.A.1, descriptive statistics for the households are reported.

2.3.2 Protests

In the empirical analysis both, Fridays for Future protests in the US as well as Fridays for Future protest in Sweden are used. To construct a dataset for protests in both Sweden and the US, we use reports by Fridays for Future about their global protest activity, which they publish on their website.[10] More specifically, Fridays for Future publishes tables that report—for each month since August 2018—all protests that happened globally. For each of these protests, the tables on their website include the date of the protest, the number of protesters and the city in which the protest took place. Using this global protest database, we extract two databases, one for the US and one for Sweden. To construct a database for the US that reports the number of protests for each week and for each county (and city), a few more steps are required. First, we added the state information for each city to the data from Fridays for Future.[11] Second, we used a

[9] This includes product description, price, brand, size and packaging of products, besides many others.

[10] In Appendix 2.E, there is a detailed list for the sources for all tables that were used to construct this protest database.

[11] In the US, from the city name, it is ambiguous in which state and county the city is located, as it often happens that two different cities in different states have the same name. However for each city in the protest tables on the website, the state in which the city is located is available through a link to a background database provided by Fridays for Future. We used web-scraping to add the state information to each city in the protest tables from the background website. The background website is `https://www.gamechanger.eco/action/locations_list` and has itself many sub-pages that contain for each city the state, from which the scraping algorithm takes the state information. For a few protests, the location information was missing. These are dropped from the analysis.

city-database from the US[12] and merged the city-state observations from Fridays for Future to this city-database to obtain the county for each city in which a protest took place. For 448 cities in which protests took place, this automatic merging did not work, as these cities were not recorded in the city database.[13] For these 448 cases, we added the county information by hand. In the following, we call this dataset from Fridays for Future that contains protests in the US "FFF". The main specifications are run on the county-week level, meaning that we summarize the protest activity within all cities within each county, respectively. The main reason for this is that the identifying variation of the instrument is on the county level. Similarly, to construct a protest database for Sweden, we hand-collected the geographic coordinates for each city in Sweden, in which a protest took place.[14] Then, we used shape-files from the European Union[15] to assign each city to one of the 21 regions (which is something like a county in the US) in Sweden.

Besides the global protest database from Fridays for Future, we use a second data source for Fridays for Future protests in the US. This data comes from the Crowd Counting Consortium (2023) (henceforth, CCC). The CCC aims at creating a database on all protests that happen in the US with several web-scraping techniques, collecting any protest information from online print and broadcast sources, social media accounts, Google Searchers and other websites. The data is updated on a daily basis. Any event that is detected through their algorithm is hand-coded by CCC, which ensures that protests in the CCC-database are indeed real protests. As the CCC-database contains all kinds of protests on all topics, we selected the Fridays-for-Future-protests by hand from the database.[16]

[12] We used the basic version of this databse: `https://simplemaps.com/data/us-cities` (last access: 23/06/2023).
[13] The city database does not contain all cities from the US, but only a sample of them.
[14] We did so by searching for the city on Google Maps and then reporting the longitude and latitude from the city center.
[15] These shape-files are available on `https://gisco-services.ec.europa.eu/distribution/v2/nuts/nuts-2021-files.html` (last access: 03.07.2023).
[16] We select the Fridays-for-Future-Protests from the CCC database in several steps: First, we select all protests in which the actors were one of the following: "Fridays for Future", "General Protesters", "Environmental Activists", "Students", "Parents Strike 4 Climate", "Climate activists", full names of individuals who are—according to Google Search—activists engaged for Fridays for Future, and full names of schools or colleges. Second, we only keep protests that asked for more measures or actions against climate change and not for less. Third, we only keep protests that were asking for more action against climate change in general and dropped all protests that asked for specific local measures. For instance, we dropped protests that asked for the restriction of a single local oil extraction place. Fourth, we also kept protests when the actor and/or the organization of the protest was missing but the aim of the protest was "urgent action on climate change". Given this selection through this four-step procedure, we

In Table 2.1, some descriptive statistics for the protests are summarized: the number of protests, the number of protests for which the number of protesters is known, as well as some descriptive statistics on the number of protesters separately for both Sweden and the US, as well as for the different data sources. Note that the number of protesters is *not* known for almost 80% of the documented protests.

Table 2.1: Protests in US and Sweden

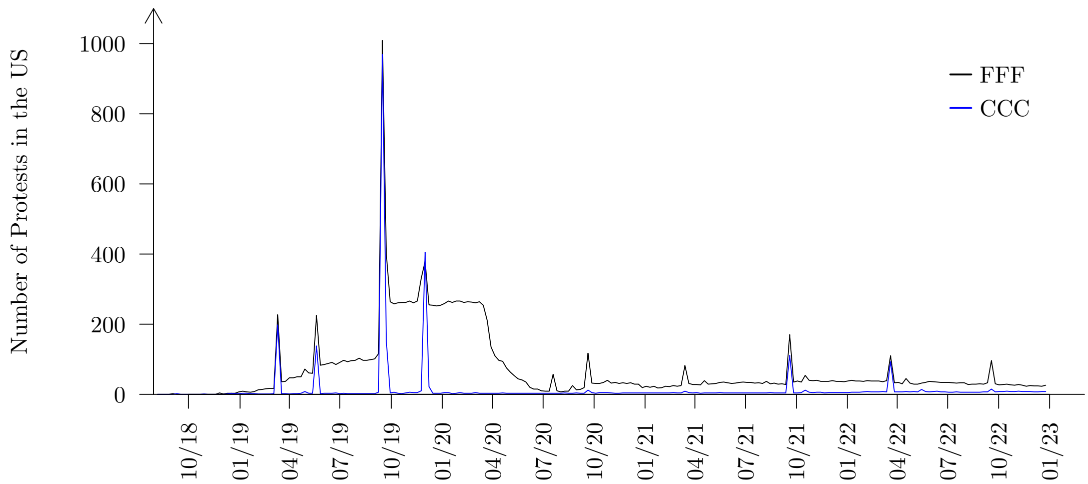
	Number of Protests		Number of Protesters			
	Overall	Number of Protesters known	Min	Average	Median	Max
Protests in the US						
Source: CCC	3,049	775	1	634.65	70	250,000
Source: FFF	16,433	3,242	1	445.28	12	400,000
Protests in Sweden						
Source: FFF	9,364	2,002	1	186.92	12	70,000

Note: Number of protests and protesters by different sources (FFF and CCC) for the US and Sweden. Source: own dataset construction as described in the main text.

The dynamics of protests over time in the US is illustrated in Figure 2.3. Figure 2.3 shows that for the major peaks in the number of protests, both data sources (CCC and FFF) report a very similar number of protests. However, in the time between the peaks, the number of protests reported by CCC is much lower than the number of protests reported by FFF. This is not surprising, as the CCC algorithm is designed in a way that it overlooks very small protests in between the major events. To put it differently, the FFF data—in contrast to the CCC data—probably records many protests that were no major events and can thus also only have a very limited effect on the public. Hence, for a protest being recorded in the CCC database, it can be seen as a sign for this protest to be a large event, whereas the FFF protests also include very minor protests that are not reported on the traditional media or not widely on social media accounts. Thus, when estimating the effects of *any* Fridays for Future protest on households' behavior in the next sections, one should consider the effects of CCC protests on households' behavior as upper-bound estimates (showing the effects of large protests on households' behavior) and the effects of FFF protests on households' behavior as lower-bound estimates.

also added all protests that were organized as a "Youth Climate Strike" or a "Climate Strike" if not already added through the four-step procedure.

Figure 2.3: Protests Over Time in the US



Note: The figure shows the total number of protests per week over time for the US for the two different data sources: CCC and FFF.

In Appendix 2.A.2, further descriptive statistics on the protests in both Sweden and the US are provided: First, the geographic dispersion of protests in the US in Figures 2.A.2 and 2.A.3 shows that the Fridays for Future protests were not concentrated in a single area in the US, but happened in nearly all states. Furthermore, also within states, the protests took place in both, more rural and more urban counties. Second, the dynamics of protests over time in Sweden displayed in Figure 2.A.4 exhibits a strong increase of protests throughout the year 2019. Moreover, in Sweden, there are also sharp peaks in the number of protests. However, in contrast to the US, the protest peaks in Sweden are less pronounced than in the US. Similarly to the US, the geographic dispersion of Protests in Sweden among the 21 regions is quite balanced, as displayed in Figure 2.A.5.

2.3.3 Social Connectedness

To measure the social connectedness between US counties and the 21 regions in Sweden, we use the "Social Connectedness Index" which was introduced by Bailey et al. (2018).[17] As described by Bailey et al. (2018), the Social Connectedness Index is computed by relating the number of Facebook connections between two regions—in this case a county i in the US and a region j in Sweden—to the

[17] The data that is provided by "Data for Good at Meta" was downloaded from `https://data.humdata.org/dataset/social-connectedness-index?` on 30.06.2023. The US-County to Country dataset as well as the US-County to US-County dataset are used for this analysis.

number of Facebook users in both regions:

$$\text{Social Connectedness Index}_{i,j} = \frac{\text{Facebook Connections}_{i,j}}{\text{Facebook Users}_i * \text{Facebook-Users}_j}$$

Bailey et al. (2018), calls the resulting scaled Social Connectedness Index a measure for the "relative probability of friendship". As explained by the authors, "[i]f this measure is twice as large, this means that a given Facebook user in county i is about twice as likely to be connected with a given Facebook user in [...] j" (Bailey et al., 2018, p. 262). To make the coefficients easier to interpret in the latter analysis, we linearly re-scale the index such that the values of the index for the social connectedness between a US county and a Swedish region range between zero and one in our sample.[18] For the construction of a control-variable, we also use the US-County to US-County social connectedness index.[19]

2.4 Identification

To identify the effect of a Fridays for Future protest in the US on consumption behavior in the week(s) after the protest took place, we use a shift-share instrumental variable approach. More specifically, consider county c, located in state s in the US. Consider region $r = 1, \cdots, 21$ in Sweden. Moreover, consider household i, residing in county c in the US, observed in week t. The following specification defines the first-stage specification for the instrumental variable approach:

$$\mathbb{1}\left[P^{\mathrm{US}}_{c,t} > 0\right] = \beta_0 + \beta_1 \underbrace{\left(\sum_{r=1}^{21} SCI_{r,c} * P^{\mathrm{SE}}_{r,t-1}\right)}_{=:\mathrm{IV}_{c,t}} + \beta_2 \underbrace{\left(\sum_{r=1}^{21} SCI_{r,c} * \Delta P^{\mathrm{SE}}_{r,t}\right)}_{=:\Delta\mathrm{IV}_{c,t}} + \mathbf{X}_{c,t}\beta_3 + \gamma_c + \eta_{t(s)} + v_{ct} \quad (2.1)$$

[18] In the dataset, not the direct values, but a scaled version of the index is provided, such that the index ranges between 1 and 1,000,000,000. However, the total dataset that is used does contain the connectedness of each US county to each region on earth. Considering only the Swedish regions and the US counties, the values of the index in the data range from 1 to 5839. To make the values of the index ranging between 0 and 1, we subtract 1 from each index value and divide each value by 5838.

[19] For the construction of the control variable, and the county-to-county social connectedness index, we apply the same linear adjustments as described for the county to region analysis. Then, the social connectedness index for the county-to-county analysis ranges between zero and one.

Where $P^{\mathrm{US}}_{c,t}$ denotes the number of Fridays for Future protests in county c in the US at time t, $P^{\mathrm{SE}}_{r,t}$ denotes the number of Fridays for Future protests in region r in Sweden at time t, $SCI_{r,c}$ denotes the social connectedness index between US county c and the Swedish region r, $X_{c,t}$ is a vector of controls, γ_c and $\eta_{t(s)}$ are county- and state-specific time fixed effects, respectively and v_{ct} is the random error term. Note that $\Delta P^{\mathrm{SE}}_{r,t} = P^{\mathrm{SE}}_{r,t} - P^{\mathrm{SE}}_{r,t-1}$. Moreover, to increase the readability in tables, note that we define $\mathrm{IV}_{c,t}$ and $\Delta\mathrm{IV}_{c,t}$ as short notations for the two instruments used. In some regressions, we will also use a modified first-stage specification, which does not include the growth component in Sweden, i.e., does not contain $\sum_{r=1}^{21} SCI_{r,c} * \Delta P^{\mathrm{SE}}_{r,t}$ and which we call the “simple” first-stage specification. In all baseline specifications, the vector of controls is defined as $\mathbf{X}_{c,t} = \left(P^{\mathrm{US}}_{c,t-1},\ \ \sum_{c' \neq c} SCI_{c',c} * P^{\mathrm{US}}_{c',t-1}\right)$. The main specification of interest (i.e., the second-stage specification) is given by

$$\operatorname{arcsinh} C_{i(c),t+\tau} = \alpha_0 + \alpha_1 \widehat{\mathbb{I}[P^{\mathrm{US}}_{c,t} > 0]} + \psi_i + \phi_{t(s)} + u_{it}, \tag{2.2}$$

where the main variable of interest is the predicted indicator variable from the estimated Specification (2.1), ψ_i and $\phi_{t(s)}$ are household- and state-specific time-fixed effects, respectively, $C_{i(c),t+\tau}$ is the meat consumption of household i (living in county c in week $t+\tau$) and u_{it} is the random error term. Furthermore, we also run the corresponding reduced form regressions, given by

$$\operatorname{arcsinh} C_{i(c),t+\tau} = \omega_0 + \omega_1 \mathrm{IV}_{c,t} + \omega_2 \Delta\mathrm{IV}_{c,t} + \mathbf{X}_{c,t}\boldsymbol{\omega}_3 + \xi_i + \theta_{t(s)} + w_{it}, \tag{2.3}$$

where ξ_i and $\theta_{t(s)}$ are household- and state-specific time-fixed effects and w_{it} is the random error term.[20]

As shown by Borusyak, Hull and Jaravel (2022), for a shift-share instrumental variable approach to identify causal effects, the shares (in this case the social connectedness, $SCI_{c,r}$) are allowed to be endogenous, when the shocks can be considered to be exogenous. Thus, for the proposed instrumental variable regression to be valid, the shocks, which are (1) protests in Sweden in region r one week prior to the protests in county c and (2) the growth in protest activity between week t and the last week $(t-1)$ in Swedish region r, must have no *direct* impact on the consumption decisions of households in the US in weeks $t+\tau$. Moreover, for the instrumental variable strategy to be valid, the protest activity in the US must—at least partly—be driven by the shocks, i.e., the

[20] To calculate all IV regressions, we use the software provided by Correia (2018). For all linear specifications, we use the software provided by Correia (2016).

instrument must be relevant. The relevance assumption of the instrument seems plausible for several reasons: First, as Böken (2023) shows, the Fridays for Future protests indeed spread through Facebook to other regions. Böken (2023) causally establishes that the protest activity was transmitted through Facebook, using the same social connectedness data that is used in this study. As Böken (2023) argues, the Fridays for Future protests were mainly organized through social networks and Greta Thunberg used Facebook to document her protest activities. Hence, it seems plausible that the protests did not only spread across Europe through Facebook (as established by Böken (2023)), but also from Europe to the US through Facebook. Second, it is a well established fact that protests can spread through social networks (see, for instance, Qin, Strömberg and Wu (2024), Enikolopov, Makarin and Petrova (2020) or Fergusson and Molina (2019)). Third, as argued in Section 2.2, the protests started in Sweden and spread from there to the entire world, highlighting the role of Sweden as a key driver of the protests. The key role of Sweden becomes even clearer once considering the relative number of protests in the entire world and in Sweden. As it is highlighted in Figure 2.1, from 100,000 Fridays for Future protests that took place on earth until the beginning of 2023 in more than 125 countries on all five continents, 10,000 took place in Sweden. Considering the number of protests per one million inhabitants presented in Figure 2.2, it is evident that Sweden can be considered as the "center" of the protests. Hence, it seems reasonable that protests in the US are driven by both, the level of past protests in Sweden and the change in protest activity in Sweden. If protesters in the US are instigated by their Swedish counterparts, they plausibly protest more when there are more protests in Sweden and also react on changes to the activity in Sweden. It remains to discuss the exogeneity of the instruments. There are some threads to the exogeneity assumption for which we directly account for in our specifications: First, it might well be that protests activity in Sweden is correlated with the global protest activity in week t, which might in turn directly impact consumption behavior of households. By including state-specific time fixed effects, we account for this possibility, taking out the global protest activity which might influence each state differently through state-specific news. Second, *past* protests in Sweden $(t-1)$ might not only directly affect *current* protest activity in each US county (t), but also past protest activity in the US $(t-1)$, when protests are announced on social media early and protesters in the US react very fast on upcoming protests. Past protests in the US, in turn, might directly affect consumption decisions in the future, which would then lead to a violation of the exogeneity assumption. We account for this violation by controlling for the *past* protest activity in each US county ($P^{\text{US}}_{c,t-1}$). Third, and closely related to the second point, *past* protests in Sweden ($t-1$) might directly affect *past* protests in any county $c' \neq c$ in the US. This protest activities in other counties might directly affect both, current protest activities in county c (which would be a protest spillover within the US) and consumption decisions in periods $t+\tau$ in county c. However, by controlling for $\sum_{c' \neq c} SCI_{c',c} * P^{\text{US}}_{c',t-1}$, i.e., the spillovers from other US counties, we account for these indirect effects. Fourth, we consider the protest activity in Sweden as an "information" treatment that triggers protests in the US, which then, in turn, influence consumption decisions. However, it might also be the case that this information treatment itself does also directly affect consumption decisions. In other words, it might not be the protests in the US that cause consumption to change, but protests in Sweden that cause both, protest activity in the US and changes in consumption patterns in the US. We cannot directly account for this fourth concern in the specifications above. However, we will present the results of a reduced form regression of the instrument on the outcome for "not-yet takers" (i.e., those counties who will experience a protest but have not yet experienced a protest). This approach follows the tests of the exclusion restrictions applied by Caprettini and Voth (2023) or Acemoglu et al. (2022).[21] The intuition behind this robustness test is that if there is a direct influence of Swedish protest activity on consumption, this direct influence should already be measurable in the weeks before the first protests take place, as consumption adjustments can be made much quicker than the organization of a protest. To put it differently, if we find any effect of Swedish protests on consumption in a certain county only *after* the protests in this county start, then it is likely the protests in the US and not in Sweden that changed consumption behavior.

Lastly, note that in all baseline specifications, we cluster standard errors at the county-level, as the treatment assignment is conducted on the county level. More specifically, for a household being "treated", there must be at least one Fridays for Future protest in the county in which the household lives. According to Abadie et al. (2023), clustering of standard errors should be conducted on the assignment

[21] These tests were initially developed by Angrist and Krueger (1994) and Bound and Jaeger (2000) and theoretically justified by D'Haultfœuille, Hoderlein and Sasaki (2024) and others. Note that the robustness test conducted by Acemoglu et al. (2022) and Caprettini and Voth (2023) conduct a "never-taker" analysis, i.e., a reduced form regression of the instrument on those that never receive a treatment. In this case, we conduct this test by regressing the instrument on the outcome of the not-yet takers. In this case, not-yet taker observations seem the more natural comparison group than the never-takers, as counties in which there is never any protest are probably systematically different to counties in which there is at least one protest in the entire sample period.

level, even in case of random sampling (as it is the case here). However, as a robustness check, we will also use spatial clustering methods.

2.5 Results

2.5.1 First-Stage Regressions

In Table 2.2 the results for the first stage, defined by Equation 2.1 are presented. Columns (3) and (6) show the regression results including state-specific time fixed effects and a sample restriction to the time period August 2018 until December 2021 (which is the sample period for the outcome data used in the full instrumental variable regression) that are considered as the baseline first-stage specification. The results for these baseline first-stage specifications show that one more protest in Sweden in region r in $t-1$ significantly increases the probability that there is at least one protest at time t documented in the FFF database by 3.37 percentage points on average in a county c that is most connected to this Swedish region r (i.e., for which $SCI_{r,c} = 1$). For the CCC database, the effect is 1.91 percentage points, which makes sense given the lower incidence of protests in the CCC database. Moreover, if the protests in the most connected region r in Sweden increase by one from period t compared to period $t-1$, the probability that there is at least one protest in a county documented in the FFF database significantly increases by 4.14 percentage points on average. For the CCC database, this effect is equal to 2.47 percentage points.

In all these specifications in Table 2.2, the F-statistic for the joint significance of the two instruments is well above 10, indicating that the instruments are quite strong. However, we will still use weak instrument robust asymptotics in the subsequent IV regressions. Reassuringly, the effects in the unrestricted time period (including the time periods beyond 2021) that are presented in Columns (2) and (5) of Table 2.2 are smaller than those in Columns (3) and (6), respectively. This makes sense, considering the possibility that the longer the protests are already conducted in the US, the smaller should be the dependence on the Swedish counterparts, as own protest organization probably become more independent from Sweden.

In Appendix 2.B.1, we consider variations in the first-stage specification. First, running the regressions without the vector of control variables $\mathbf{X}_{c,t}$ delivers very similar results. If anything, the effects are even larger and the F-tests are more pronounced when running (2.1) without control variables. Second, we also run the first-stage regression including only one of the instruments, i.e., the past protests in Sweden, leaving out the growth-component of Swedish protests. Technically, this means we run specification (2.1) without ΔIV. We call this specification the "simple" first-stage specification. For the simple specification, we do not find significant effects of IV for the FFF data and significant, but smaller effects for the CCC data. Thus, the simple specification does not fully capture the relation between Swedish protests and protests in the US, which seems to be best described by US protesters who are impacted by (1) the number of protests in the past (IV) and (2) the growth of Swedish protests (ΔIV).

Table 2.2: First-Stage Regression - Full Specification - Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.03098***	0.02984**	0.03368**	0.01795***	0.01559***	0.01912***
	3.29	2.92	3.14	5.91	5.38	6.02
ΔIV	0.04377***	0.04029***	0.04144***	0.02796***	0.02328***	0.02466***
	6.07	5.23	5.25	7.05	6.12	6.25
N	719060	718831	555603	719060	718831	555603
adj. R^2	0.535	0.551	0.545	0.321	0.395	0.360
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21
F-Test IVs	22.51	16.42	16.43	25.24	18.75	19.62

Note: Results of estimates for specification (2.1). The dependent variable equals the indicator variable whether there was at least one protest either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables IV and ΔIV display the estimates of coefficients β_1 and β_2, respectively, defined in Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "F-Test IVs" is the test statistics of a F-test on the joint significance of IV and ΔIV. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

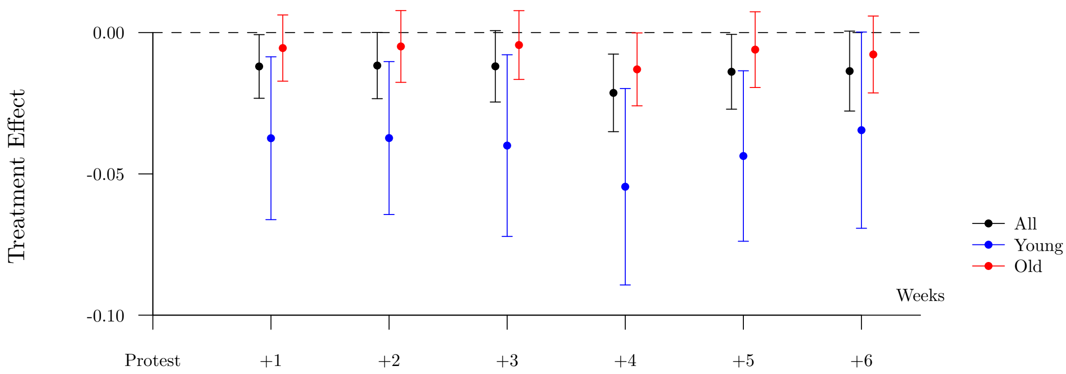
So far, the dependent variable in the first-stage regression was an indicator variable equal to one if there was at least one protest in a certain county in a certain week. As an addition, we also consider a different dependent variable in the first stage. This alternative dependent variable is a dummy variable indicating the *very first* protest in a US county. More specifically, we define this indicator variable for the first protests to be equal to zero if there was not yet a protest in county c at time t; equal to one in the week of the first protest in county c; and not defined otherwise. Hence, for each county, we consider only the observations until the first protest takes place, respectively. In Appendix 2.B.2, we present the results for first-stage specifications with this alternative dependent variable. The results for the full specification including both instruments IV and ΔIV are the same in signs compared to the baseline results for all protests. However, the coefficients are much smaller in size and the F-Tests for the joint significance of the instruments is also smaller. Running specification (2.B.1) with the indicator for the first protest excluding the growth-component in the instrument (ΔIV) delivers different results than in the case where the dependent variable is any protest. In contrast to the case of all protests presented above, the simple specification for the first protests shows significant estimates for the instrument and shows the correct sign for all specifications. Moreover, the F-tests the simple specification is well above 10. Hence, one can conclude that the simple first-stage specification describes the relation between Swedish protests and the *first* protests in the US "better" than the specification that includes the growth component of Swedish protests as an additional instrument. However, it should be noted that the F-tests are smaller than for the baseline specification predicting the occurrence of any protest in the US shown in Table 2.2 that were discussed above. To summarize, the first-stage results indicate that IV and ΔIV are valid instruments to predict the occurrence of any protests in the US. For the first protests in each county, from these results, it seems that only the IV instrument is valid.

2.5.2 Second-Stage Regressions

The results of the full instrumental variable regressions are presented separately for the protests documented in the FFF database and in the CCC database. For all specifications we do sample splits: First, we estimate the effects of protests on meat consumption for all households in the sample. Second, we consider only the subsample of "Young" households. We call a household a "Young" household if at least one person between the ages of 14 and 25 lives in that household. Third, we estimate the effects for "Old" households. We call all households "Old" that are not "Young" according to the previous definition. The split according to the ages is motivated by the fact that the Fridays for Future protests were mainly organized and conducted by "Young" people, as discussed in Section 2.2. Finally, to check whether the protests only change consumption patterns of households who are as old as those people who protested, we also estimate the effects on households in which at least one person lives aged between 0 and 13.

In Figure 2.4, the estimated effects from the instrumental variable regression of at least one protest recorded in the FFF database at time t on meat consumption in periods $t + \tau$ are presented. The Figure documents a reduction in meat

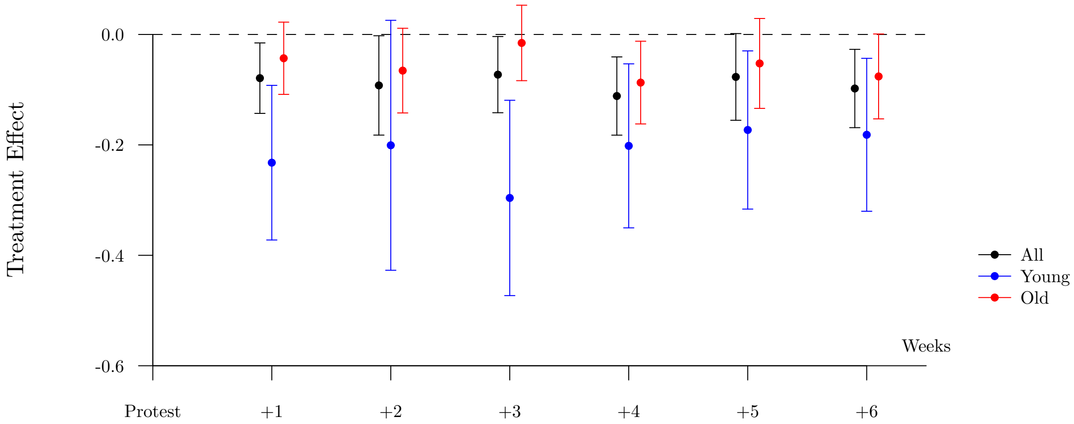
Figure 2.4: Effect of Protests (FFF) on Meat Consumption



Note: Estimation results of coefficient α_1 from specification (2.2) for $\tau = 1, \cdots, 6$ for the effects of a protest recorded in the FFF database on meat consumption for the whole sample (All), the sub-sample of Young households and the sub-sample of Old households.

consumption for Young households only until five weeks after protests happen. For Old households, there is no significant effect on meat consumption. In Figure 2.5, the estimated effects from the instrumental variable regression of at least one protest recorded in the CCC database at time t on meat consumption in periods $t + \tau$ are presented. The coefficients are very similar in their sign and significance to the ones from the FFF database. In contrast to the effects after a protest documented in the FFF database, the effects for protests documented in the CCC database are much larger. The point estimates suggest a reduction in meat consumption by Young households of 3.74% in the week after a protest documented in the FFF database took place; in contrast, after a protest documented in the CCC database took place, Young households reduce their meat consumption by 23.22%. Moreover, in contrast to the effects for protests in the FFF database, which seem to vanish after 5 weeks, the effects for the CCC database become a little smaller but are still significant even six weeks after a protest took place. As it was explained in Section 2.3.2, the estimates from the effect of a protest recorded in the CCC database should be considered as an upper bound estimate of the effect of *any* Fridays for Future protest on meat consumption, because the CCC database only contains the protests that gained large attention on social media and other public news sources. In contrast, the estimated effects from the FFF database can be considered as lower bound estimates, because the protests in this database are self-reported protests from Fridays for Future and probably contain some protests that were smaller events. Hence, while estimates suggest that Young households reduce their meat consumption after large protest events (see Figure 2.5) by more than 20% on average in the first six weeks after the protest took place, this should be seen as an upper bound estimate. When asking for the effect of any of these protests on the meat consumption of Young households, the results for the protests documented in the FFF database should be considered.

Figure 2.5: Effect of Protests (CCC) on Meat Consumption



Note: Estimation results of coefficient α_1 from specification (2.2) for $\tau = 1, \cdots, 6$ for the effects of a protest recorded in the CCC database on meat consumption for the whole sample (All), the sub-sample of Young households and the sub-sample of Old households.

In Appendix 2.C.1 three additional results related to Figures 2.4 and 2.5 are presented. First, we report the results for the reduced form estimates of the instruments on the outcome for each subgroup, as defined in specification (2.3). The reduced form estimates show significant negative effects of the instruments on meat consumption for both, protests documented in the CCC database as well as for protests documented in the FFF database, thereby confirming the results from the main IV regressions. Second, we also show the IV estimates as well as the reduced form estimates for households in which at least one person aged 0 to 13 lives. For both, the estimates stemming from the CCC protests as well as for the estimates stemming from the FFF protests, we do not find significant negative effects of these protests on meat consumption.[22] Third, to account for the concern of weak instruments, we calculate adjusted p-values following Anderson and Rubin (1949), henceforth called AR p-values.[23] The AR p-values confirm the significance of all estimates for Young households for both the protests from the CCC database as well as for protests from the FFF

[22] Even though we find significant effects for $\tau = 4$ for the household in which at least one person aged 0 to 13 lives, the significance of these effects does not "pass" the weak instrument asymptotic (i.e., AR-p-values are not smaller 0.05) and are therefore not considered to be significant.

[23] The p-values calculated with the method of Anderson and Rubin (1949) correspond to inference that delivers correct coverage of confidence intervals under arbitrary weak instruments.

database. Hence, the instrumental variable regressions "pass" the test for weak instruments.

So far, the results of the effects of *any* Fridays for Future protest on meat consumption were presented. Similarly to the analysis for any protest, the same analysis can be conducted for the effects of the *first* protest in each database. As described for the results of the first-stage specifications (and as it will be confirmed in Section 2.6.1), the first-stage specification for the occurrence of the first protests is better described by a "simple" version of the first-stage regression, leaving out the growth component (ΔIV) of Swedish protests and using only one instrument (i.e., using only IV as defined above). The results for the effects of the first protests documented in both the CCC as well as the FFF data, including the corresponding reduced form results and the weak instrument inference are presented in Appendix 2.C.2. Considering the AR p-values for all effects of first protests presented in Appendix 2.C.2, all effects for the first protests on consumption are *not* significant considering a 5% significance level for Young households. Hence, the instrument is too weak to identify the effects of the first protests on consumption. Intuitively, the weakness of the instrument for the first protests makes sense in comparison to the results of any protest, as it is much harder to predict the occurrence of the *first* protest than to predict the occurrence of *any* protest at a certain place.

Summarizing these results, in the weeks after a Fridays for Future protest took place, Young households in the US reduce their meat consumption on average by 4.23% for five weeks.[24] This is presumably a lower-bound estimate, as the FFF database contains many events which were probably no large protests events, thereby not gaining a lot of attention. The reduction in meat consumption is much stronger for larger protest events for Young households. For households in which no young individual lives, we cannot find any significant reduction in meat consumption. For the effect of the first protest on meat consumption, the instruments used in our analysis are not strong enough to identify an effect on meat consumption. In other words, Young households seem to follow the example of Greta Thunberg, the founder and figurehead of the Fridays for Future movement who does not eat meat. In contrast, older households or households with children of age 0 to 13 do not change their consumption habits.

[24] This number is obtained by taking the average of the effects of the occurrence of a protest documented in the FFF database across five weeks for Young households.

2.6 Robustness Checks

2.6.1 First-Stage Regressions

As a first robustness check, we summarize all Swedish regions ($r = 1, \cdots, 21$) to one region (i.e., Sweden) and estimate the following adjusted first-stage regression:

$$\mathbb{1}\left[P^{\mathrm{US}}_{c,t} > 0\right] = \beta_0 + \beta_1 \Big(\underbrace{SCI_{\mathrm{SE},c} P^{\mathrm{SE}}_{t-1}}_{=:\mathrm{IV}'_{c,t}} \Big) + \beta_2 \Big(\underbrace{SCI_{\mathrm{SE},c} * \Delta P^{\mathrm{SE}}_{t}}_{=:\Delta\mathrm{IV}'_{c,t}} \Big) + \mathbf{X}_{c,t}\boldsymbol{\beta}_3 + \gamma_c + \eta_{t(s)} + v_{ct} \tag{2.4}$$

where $SCI_{\mathrm{SE},c}$ denotes the social connectedness index between county c and Sweden and P^{SE}_t denotes the number of protests in Sweden at time t. We call the adjusted instruments IV′ and ΔIV′, respectively. The results of this regression are presented in Appendix 2.D.1. Comparing these results to the baseline first-stage specification, the estimates are qualitatively the same. There are only minor changes in the size of the coefficients. The F-tests do not vary much, they do even increase in some cases. Similar to the main first-stage regressions, we also conduct regressions of specification (2.4) without the growth component, i.e., excluding ΔIV′. The corresponding results are also presented in Appendix 2.D.1 and show the same patterns, signs and significance levels than the corresponding baseline specification.

Similarly, in Appendix 2.D.1, we run the modified first-stage specifications of specification (2.4) in which we replace the dependent variable by a dummy variable indicating the very first protest within each county. The results look almost the same to the results of the corresponding modified baseline specifications.

Following specification (2.4), we construct a test suggesting that it is indeed the variation of protests in Sweden that drive the protests in the US and not the protests in other countries in which protests took place. To construct this test, we take three steps: First, we estimate a modified version of specification (2.4) for each country in which Fridays for Future protests took place. More specifically, for each country C in which at least one protest took place, we estimate (2.4), replacing P^{SE}_{t-1} by P^{C}_{t-1} and $\Delta P^{\mathrm{SE}}_{t}$ by $\Delta P^{\mathrm{C}}_{t}$, being defined as the protest measures in country C in accordance to the definitions for Sweden. Second, we multiply the estimated coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ of each country regression from the first step with the variance of protests across weeks within each country, respectively.[25] This

[25] The results look very similar when we scale with the total number of protests instead.

scaling is necessary to make the estimates from different countries comparable: To see whether Sweden indeed has a higher *overall* impact on the protest activity in the US than, for instance, Germany, it is necessary to multiply the effect of one more protest in both Sweden and Germany by the identifying variation of the actual protests conducted in both countries. Third, we calculate the relative position of the scaled estimates for Sweden in comparison to the scaled coefficients of all other countries. If the relative position of the scaled Swedish estimate is above the 95-percentile, we reject the null hypothesis that the protests are driven by any other country than Sweden. Conducting this test, we find that for the protests in the FFF (CCC) database, the scaled $\hat{\beta}_1$ coefficient for Sweden is in the 98.12 (95.62) percentile of all scaled $\hat{\beta}_1$ coefficients and the scaled $\hat{\beta}_2$ coefficient for Sweden is in the 98.12 (96.88) percentile of all scaled $\hat{\beta}_2$ coefficients. In Appendix 2.D.3, we show the distribution of scaled estimates and the relative position of Sweden.[26] Thus, we reject the null hypothesis that any country other than Sweden had a similar impact on US protests than Sweden. Lastly, we conduct the same tests for the specification in which the dependent variable is the occurrence of the *first* protest. For these specifications involving the *first* protests, we can reject the null hypothesis with the constructed test for the simple specification, but not for the growth component in the full first-stage specification.[27] Thus, the tests conducted suggest that it is indeed the protest activity in Sweden and not in other countries that drive the protest activity in the US.

Finally, we set up a further adjusted first-stage regression specification to show that the first-stage results are not driven by outliers in Swedish protests. More specifically, we run the following regression:

$$\mathbb{1}\left[P^{\text{US}}_{c,t} > 0\right] = \beta_0 + \beta_1 \Big(\underbrace{SCI_{\text{SE},c} \tilde{P}^{\text{SE}}_{t-1}}_{=:\text{IV}''_{c,t}} \Big) + \beta_2 \Big(\underbrace{SCI_{\text{SE},c} * \Delta \tilde{P}^{\text{SE}}_{t}}_{=:\Delta\text{IV}''_{c,t}} \Big) + \mathbf{X}_{c,t}\beta_3 + \gamma_c + \eta_{t(s)} + v_{ct} \tag{2.5}$$

where we replace the protests in Sweden P^{SE}_{t-1} by the trend of protests in Sweden, denoted by $\tilde{P}^{\text{SE}}_{t-1}$. The trend of protests in Sweden is estimated using locally esti-

[26] We also conducted this test for the simple first-stage specification, finding the same results: the scaled $\hat{\beta}_1$ coefficient in the simple first-stage specification for Sweden is in the 98.12 percentile of all scaled $\hat{\beta}_1$ coefficients across all countries for the FFF database and in the 96.88 percentile for the CCC database.

[27] Similarly, we also conducted the same test for the first-stage specifications for which the dependent variable is the occurrence of the first protest: For the full first-stage specification, we find the percentiles to be 93.75 (FFF) and 93.75 (CCC) for the scaled $\hat{\beta}_1$ coefficients and 11.88 (FFF) and 16.25 (CCC) for the scaled $\hat{\beta}_2$ coefficients. For the simple first-stage specification, we find the percentiles to be 96.25 (FFF) and 96.88 (CCC).

mated scatterplot smoothing (for details, see Appendix 2.D.2). The corresponding results of specification (2.5) as well as for a modified version of specification (2.5), in which we leave out $\Delta\text{IV}''$, are presented in Appendix 2.D.2. The results for these first-stage estimates with Swedish trends are qualitatively the same as the baseline estimates, thereby suggesting that the results from the first-stage specification are not driven by outliers.

2.6.2 Second-Stage Regressions

As a first robustness check, we conduct a reduced form regression of the instruments (IV and ΔIV) on the outcome for "not-yet takers", i.e., households that were not yet experiencing a protest but will experience a protest in their county in later weeks during the period of observations. Technically, we run specification (2.3) for the restricted sample of Young households living in county c observed at time t for which it holds that there will be at least one protest in county c during the period of observation, but there was no protest in this county c until (and including) week t. As described above, this test can be understood as a test for the exclusion restriction of the instrument to hold (see, Acemoglu et al. (2022); Caprettini and Voth (2023); Angrist and Krueger (1994); Bound and Jaeger (2000); D'Haultfœuille, Hoderlein and Sasaki (2024)). The basic idea of this test is that when for a subset of households, the instrument does not change the treatment (protests), then this subset can be used to test whether there is a direct effect of the instrument on the outcome (D'Haultfœuille, Hoderlein and Sasaki, 2024) for this group. If we would find any effects, then it would be incredible that the exclusion restriction holds in other cases. The results of this test are presented in Appendix 2.D.4. Reassuringly, we do not find any effect of the instrument on the meat consumption for $\tau = 1, ..., 6$ (see specification (2.3) for the definition of τ) for the not-yet taking household, suggesting that the exclusion restriction holds for the not-yet takers, thereby serving as a piece of evidence for the exclusion restriction to hold for our instruments.

Moreover, another thread to identification could be that households living in counties that are more socially connected to Sweden exhibit a different trend in meat consumption. In Appendix 2.A.1, we plot the average meat consumption as a percentage of total grocery consumption expenditures over time for the whole sample, as well as separately for counties which exhibit an above-median social connection to Sweden and those who exhibit a below-median social connection to Sweden. Even though there are neither different trends nor different levels of the average meat consumption between counties with above- versus below-median social connection, we still control for the possibility that different trends in social connection to Sweden might influence the results. To do so, we include a linear trend component in the second-stage regressions, i.e., we estimate the following adjusted second-stage specification:

$$\text{arcsinh } C_{i(c),t+\tau} = \beta_0 + \beta_1 \widehat{\mathbb{I}[P^{\text{US}}_{c,t} > 0]} + \beta_2 ABSCI_i * t + \psi_i + \phi_{t(s)} + u_{it}, \quad (2.6)$$

where $ABSCI_i$ is an indicator function being one if household i is living in a county with above-median social connectedness to Sweden and zero otherwise. The results for specification (2.6) are presented in Appendix 2.D.5. In comparison to the baseline estimates, the estimation results for specification (2.6) are very similar in terms of effect sizes and the same in terms of significance levels and sign. Hence, we conclude that households in different counties are neither on different trends regarding their meat consumption nor do any potential differences in these trends influence our results.

Similarly, to account for the possibility that the effects are driven by an increased meat consumption right before the protests, we include the meat consumption of the past two weeks in the second-stage regression as an additional control and estimate the following adjusted specification:

$$\text{arcsinh } C_{i(c),t+\tau} = \beta_0 + \beta_1 \widehat{\mathbb{I}[P^{\text{US}}_{c,t} > 0]} + \beta_2 (C_{i(c),t-1} + C_{i(c),t}) + \psi_i + \phi_{t(s)} + u_{it}, \quad (2.7)$$

The results for specification (2.7) are presented in Appendix 2.D.6. In comparison to the baseline estimates, the estimation results for specification (2.7) are very similar in terms of effect sizes and the same in terms of significance levels and sign. Hence, we conclude that the results in the baseline specification are not driven by an increased meat consumption right before the protests take place.

Another thread to our identification might stem from the large product base from which consumers choose every week. More specifically, note that we operate in a setting in which we want to detect differences in choices between groups (or within groups across weeks) and in which those groups choose a comparably small number of items (grocery shopping within one week) from a very large number of possible items (all items across all supermarkets in the US). Intuitively, in such a setting, estimated choice differences between groups (or within groups across weeks) might be biased, because differences are more likely to occur by coincidence when the number of items chosen are relatively small in comparison the the number of possible choices (Gentzkow, Shapiro and Taddy, 2019). To test whether the effects that we find might be driven by such coincidence effects, we construct a specific test. Remember that in the raw data, we observe the choices of households on the level of each product that a household buys for each day in the years of observation. In our baseline specifications, to construct our outcome variable, we take the total sum of money that a household spend on meat products within each week. In total, the raw data that we are using contains 244,799,387 individual purchases, from which 5.46% are purchases of fresh or frozen meat, sausages, bacon or ham. In our test, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for all households, Old households and Young households, separately for the CCC protests and the FFF protests, respectively. Then, we calculate the relative position (i.e., the percentile) of the baseline second-stage effects within the distribution of the placebo estimates, separately for each specification, respectively. The results are presented in several Figures in Appendix 2.D.7. For Young households, the effects of FFF and CCC protests on meat consumption in the first six weeks after a protest took place that were significant in the baseline specifications are all well below the 5% percentile of each respective distribution of placebo estimates. Hence, this is another confirmation for our findings. Even more reassuring, for all estimates for Old households or all households, the baseline estimate is not below the 5% percentile of each respective placebo estimates, thus confirming the null-findings for Old households and all households.

Lastly, we recalculate the standard errors of the IV estimates using spatial clustering methods. So far, in all specifications, we have computed clustered standard errors by clustering all observations on the county level. Hence, in the baseline specifications, we account for any correlation within each county across all time periods. This type of clustering also directly controls for serial correlation. However, clustering on the county level does not account for potential correlation for households that live close together but are located in two different counties. To account for this, we calculate standard errors accounting for spatial correlation following the approach of Conley (1999, 2016). More specifically, by using the US ZIP-code of each household, we calculate the spatial proximity of all households to each other and calculate the standard errors of the baseline specifications accounting for spatial correlations of observations within 100, 300 and 500 miles, respectively.[28] Note that for the spatial clustering, we automatically control for any form of serial correlation, even between different households that are located closer than the specified cutoff values. The results are reported in Appendix 2.D.8. The results show that for all cutoff values, the conclusions from the significance tests under the county-level clustering under a 5-percent significance level remain the same under spatial clustering of standard errors, except for the CCC-estimates in week 5 for a cutoff of 500 miles. Thus, we can conclude that the results are robust after recalculating the standard errors.

2.7 Conclusion

Protests that asked for more action against climate change in the US by Fridays for Future changed meat consumption patterns by different age groups asymmetrically. In weeks in which a Fridays for Future protest occurs Young households (which are households in which at least one person aged 14 to 25 lives) reduce their meat consumption by up to 23.22% on average in the first week afterward, while Old households (which are all those households that are not Young) show no change in meat consumption. Moreover, considering the average effect of any type of Fridays for Future protest as recorded by Fridays for Future themselves on meat consumption, we find that Young households reduce their meat consumption by 4.23% on average across 5 weeks after a protest took place.

Our study also comes with limitations. First, as with all studies using instrumental variables in just-identified cases, we cannot directly test our exclusion restriction. Even though we provide evidence suggesting that the exclusion restriction holds for the subgroup of not-yet treated households, we can of course not rule out that the exclusion restriction does not hold for the treated observations. However, even if the exclusion restriction would not hold, our findings would still suggest that protests changed consumption behavior. In this case, it would not have been the protests in the US, but the protests in Sweden that directly caused consumption to change. Second, as individuals are observed on the household level, we cannot identify which individuals within that household change their

[28] See Appendix 2.D.8 for details.

consumption habits, i.e., whether it is the whole household consuming less meat or whether it is only the young person living in that household.

Considering our identification strategy, we open some new pathways for future research to measure the effect of other protest movements that might have spread through social networks on various outcomes. In fact, one might use our novel identification strategy to identify the effects of other global protest movements, like the Arab Spring, the Occupy Movement or the Black Lives Matter movement on any type of behavior, attitudes or believes. Our identification strategy could work in all settings in which protests spread from a specific origin asymmetrically around the globe through social networks.

Regarding the effects that we found in the context of climate protests, it would be interesting to document the potential effects of protests also for long-term decisions, such as buying an electric vehicle, or investments in “green” investment goods, which have a much larger on long-lasting impact on greenhouse gas emissions. Furthermore, it would be interesting to see whether the same effects of consumption changes can be seen in other countries in which the climate protests happened or whether this is specific to the US society. Even more broadly, it might be interesting to test whether protests impact only those individuals that identify themselves with the protesters (in the case of Fridays for Future, these were young individuals), or whether there are also cases in which all individuals in a society change their behavior as a consequence of protests. And lastly, especially in Europe, where the climate protests became much more radical in the last years through protests by the “Last Generation”, it would be interesting to check whether one would still find the same effects for these more “radical” protests.

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Appendices

2.A Descriptive Statistics

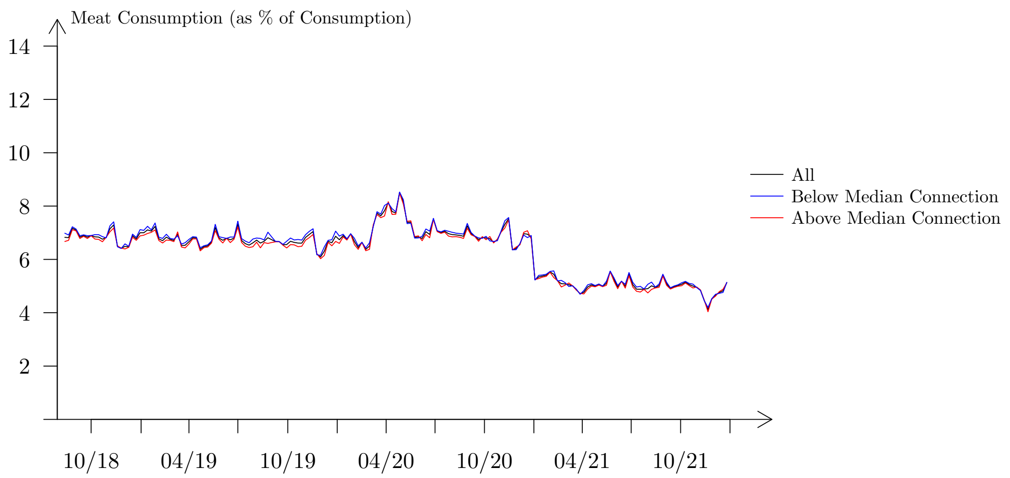
2.A.1 Household Descriptives

Table 2.A.1: Descriptive Statistics, Final Data Used in Analysis

	Never Treated				Treated			
	Old Household		*Young Household*		*Old Household*		*Young Household*	
	Mean	SD	Mean	SD	Mean	SD	Mean	Std.Dev.
# Weeks observed	106.76	55.66	95.67	55.30	106.40	55.78	97.79	55.77
Meat spending	7.93	6.78	9.88	7.91	7.43	6.80	9.53	8.20
% Weeks with protest (CCC)	0.00	0.00	0.00	0.00	0.05	0.12	0.04	0.12
% Weeks with protest (FFF)	0.00	0.00	0.00	0.00	0.33	0.75	0.31	0.71
Household income	58042.46	34882.37	67280.96	36018.17	66440.93	37349.94	75945.21	37485.86
Household size	2.20	1.10	3.96	1.30	2.12	1.12	3.92	1.32
Single family house	0.81	0.40	0.83	0.37	0.71	0.45	0.79	0.40
Household heads married	0.64	0.48	0.76	0.43	0.56	0.50	0.71	0.45
Children under 18 present	0.19	0.39	0.62	0.49	0.19	0.39	0.59	0.49

Note: The descriptive statistics in this table are calculated for different groups. The "Never Treated" households are all those households living in counties in which there is never any protest during the whole sample period. The "Treated" households are those that experience at least one protest during in the sample. Young households are all those where at least one individual aged between 14 and 25 lives, as defined in the main text. Old households are all those households which are no Young households. For some variables in the table note that: The Household size refers to the number of individuals living in a household. The variables "Single Family House", "Household Heads Married" and "Children under 18 present" are all indicator variables. SD stands for standard deviation.

Figure 2.A.1: Meat Consumption Over Time



Note: The figure shows the average meat consumption as a percentage of the average total consumption over time, for all households, as well as separately for households living in counties with above median social connectedness and those living in counties with below median social connectedness.

2.A.2 Protest Descriptives

Figure 2.A.2: Protests Across US Counties (FFF Database)

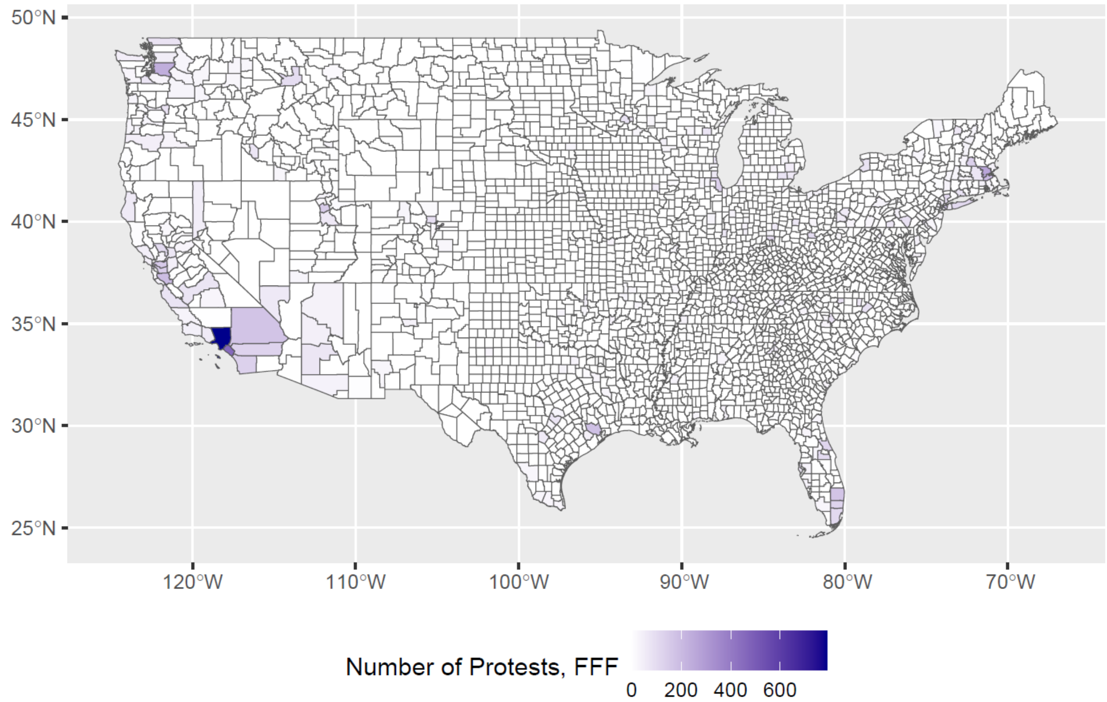


Figure 2.A.3: Protests Across US Counties (CCC Database)

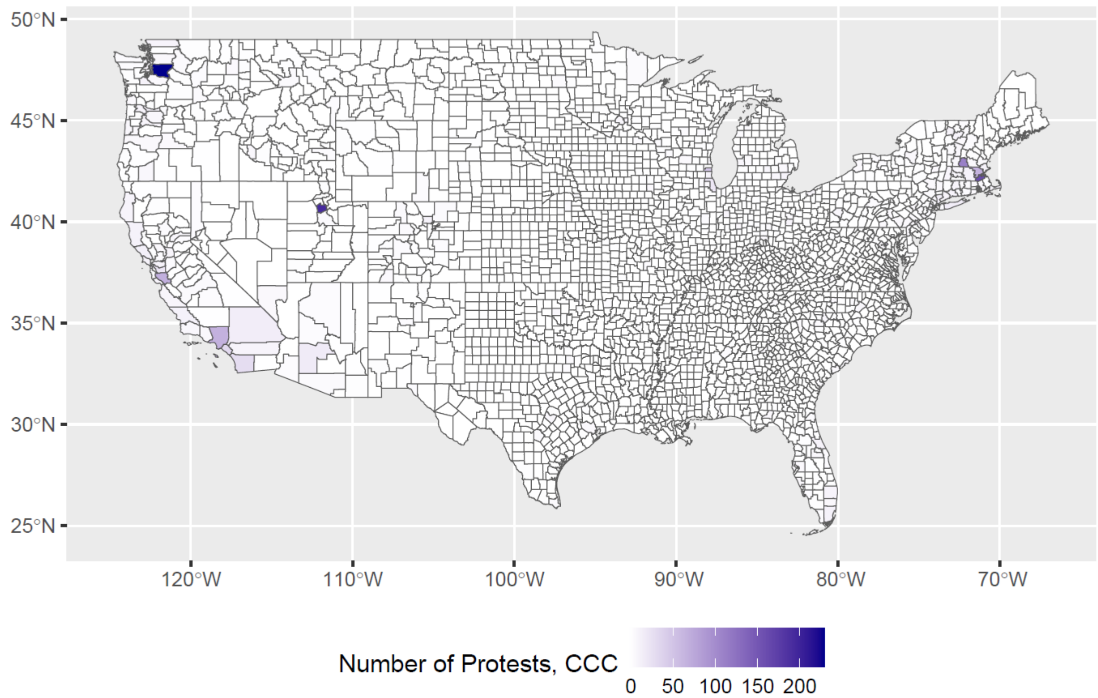
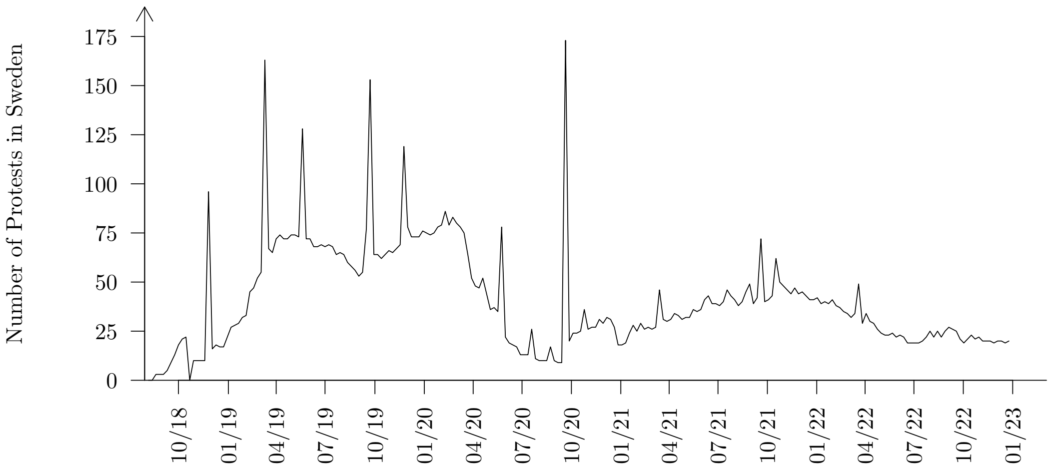
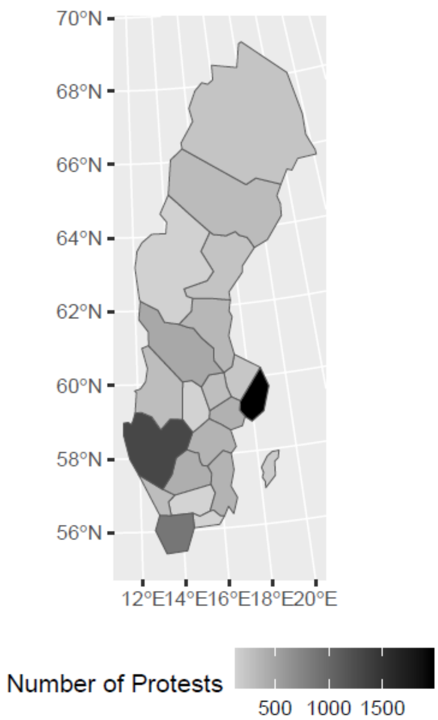


Figure 2.A.4: Protests Over Time in Sweden



Note: Total number of protests over time in Sweden. The data was first aggregated to the week-level.

Figure 2.A.5: Protests Across Swedish Regions



Note: Total number of protests aggregated over time in Swedish regions.

2.B First-Stage Results

2.B.1 Outcome: Any Protest

Table 2.B.1: First-Stage Regression, No Controls, Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.10106***	0.08861***	0.09303***	0.02237***	0.01956***	0.02211***
	7.07	5.87	5.96	6.89	5.95	6.13
ΔIV	0.07418***	0.06464***	0.06661***	0.03020***	0.02520***	0.02616***
	7.48	6.24	6.30	7.37	6.31	6.35
N	719060	718831	555603	719060	718831	555603
adj. R^2	0.293	0.322	0.330	0.304	0.379	0.352
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21
F-Test IVs	28.98	20.13	20.36	27.22	19.90	20.21

Note: Results of estimates for specification (2.1), excluding the vector of controls $\mathbf{X}_{c,t}$. The dependent variable equals the indicator variable whether there was at least one protest either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables: IV and ΔIV display the estimates of coefficients β_1 and β_2, respectively, defined in Equation (2.1). The "F-Test IVs" is the test statistics of a F-test on the joint significance of IV and ΔIV. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 2.B.2: First-Stage Regression, Simple Specification, Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.01345	0.01383	0.01540	0.00689***	0.00640***	0.00832***
	1.87	1.76	1.93	3.96	3.93	5.23
N	719060	718831	555603	719060	718831	555603
adj. R^2	0.533	0.550	0.543	0.316	0.393	0.357
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21

Note: Results of estimates for specification (2.1), excluding variable ΔIV. The dependent variable equals the indicator variable whether there was at least one protest either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variable IV displays the estimates of coefficients β_1 defined in Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "F-Test IVs" is the test statistics of a F-test on the joint significance of IV and ΔIV. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

2.B.2 Outcome: First Protest

Table 2.B.3: First-Stage Regression, Full Specification, Outcome: First Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.01649***	0.01648***	0.01437***	0.01589***	0.01590***	0.01337***
	5.03	5.01	4.17	5.17	5.17	4.33
ΔIV	0.02572***	0.02572***	0.02192***	0.02527***	0.02528***	0.02057***
	5.10	5.09	4.22	5.24	5.24	4.38
N	585103	585103	585051	594456	594456	594397
adj. R^2	0.090	0.090	0.159	0.117	0.117	0.201
Type of FE	Week	Week	State#Week	Week	Week	State#Week
Time Period	All	All	All	All	All	All
F-Test IVs	13.02	12.98	8.890	13.75	13.76	9.590

Note: Results of estimates for the modified specification (2.1), changing the dependent variable. The dependent variable equals the indicator variable equal to one in the week of the first protest, zero before the first protest and is set to “missing” after the first protest happened either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables: IV and ΔIV display the estimates of coefficients β_1 and β_2, respectively, defined in the modified Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The “F-Test IVs” is the test statistics of a F-test on the joint significance of IV and ΔIV. The “Time Period” indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 2.B.4: First-Stage Regression, Simple Specification, Outcome: First Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.00672***	0.00663***	0.00600***	0.00633***	0.00633***	0.00554***
	4.83	4.78	4.05	4.93	4.93	4.15
N	585103	585103	585051	594456	594456	594397
adj. R^2	0.082	0.082	0.155	0.109	0.109	0.197
Type of FE	Week	Week	State#Week	Week	Week	State#Week
Time Period	All	All	All	All	All	All

Note: Results of estimates for the modified specification (2.1), changing the dependent variable and excluding variable ΔIV. The dependent variable equals the indicator variable equal to one in the week of the first protest, zero before the first protest and is set to “missing” after the first protest happened either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variable IV displays the estimates of coefficients β_1 as defined in the modified Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The “Time Period” indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

2.C IV-Results

2.C.1 Effect of All Protests

Table 2.C.1: IV and Reduced Form (Baseline Specification, Effect of Any Protest)

	IV Estimates (b/se/AR p-value)						**RF Estimates** (b for IV/b for ΔIV)					
	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CCC												
All	-0.0791*	-0.0922*	-0.0727*	-0.1115**	-0.0769	-0.0978**	-0.0116***	-0.0113***	-0.0106***	-0.0119***	-0.0146***	-0.0133***
	0.0011	0.0021	0.0012	0.0013	0.0016	0.0013	-0.0066*	-0.0104***	-0.0066*	-0.0040	-0.0082**	-0.0109***
	0.0046	0.0047	0.0180	0.0000	0.0001	0.0019						
Young	-0.2322**	-0.2007	-0.2960**	-0.2017**	-0.1730*	-0.1817*	-0.0241**	-0.0328***	-0.0298***	-0.0251***	-0.0288***	-0.0174*
	0.0051	0.0133	0.0081	0.0057	0.0053	0.0050	-0.0122	-0.0196*	-0.0286***	-0.0087	-0.0119	-0.0228**
	0.0017	0.0010	0.0005	0.0006	0.0002	0.0323						
Old	-0.0431	-0.0655	-0.0153	-0.0872*	-0.0525	-0.0760	-0.0085*	-0.0062	-0.0059	-0.0085*	-0.0110**	-0.0118**
	0.0011	0.0015	0.0012	0.0015	0.0017	0.0015	-0.0051	-0.0084**	-0.0011	-0.0027	-0.0072*	-0.0080*
	0.0282	0.0715	0.2608	0.0248	0.0043	0.0340						
0-13	-0.1455	-0.1256	-0.1461	-0.2112*	-0.0850	-0.0886	-0.0102	-0.0147	-0.0194*	-0.0194*	-0.0117	-0.0158
	0.0070	0.0108	0.0084	0.0076	0.0064	0.0092	-0.0058	-0.0070	-0.0096	-0.0144	-0.0048	-0.0111
	0.2304	0.4429	0.1716	0.0752	0.6055	0.4000						
FFF												
All	-0.0120*	-0.0117	-0.0119	-0.0213**	-0.0139*	-0.0136	-0.0110***	-0.0108**	-0.0097**	-0.0111**	-0.0137***	-0.0123***
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0001	-0.0064*	-0.0102***	-0.0062*	-0.0036	-0.0079**	-0.0105***
	0.0048	0.0046	0.0034	0.0000	0.0007	0.0006						
Young	-0.0374*	-0.0373**	-0.0400*	-0.0545**	-0.0437**	-0.0345	-0.0228**	-0.0304***	-0.0273***	-0.0220**	-0.0264***	-0.0132
	0.0002	0.0002	0.0003	0.0003	0.0002	0.0003	-0.0116	-0.0186*	-0.0277***	-0.0074	-0.0107	-0.0209**
	0.0013	0.0003	0.0002	0.0000	0.0001	0.0006						
Old	-0.0055	-0.0049	-0.0044	-0.0130*	-0.0060	-0.0077	-0.0081*	-0.0061	-0.0053	-0.0082*	-0.0104**	-0.0115**
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	-0.0050	-0.0083**	-0.0008	-0.0025	-0.0070*	-0.0079*
	0.1764	0.0865	0.2317	0.0395	0.0720	0.0424						
0-13	-0.0311	-0.0301	-0.0272	-0.0367*	-0.0345	-0.0136	-0.0096	-0.0139	-0.0180*	-0.0174*	-0.0091	-0.0140
	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	-0.0055	-0.0068	-0.0093	-0.0138	-0.0037	-0.0103
	0.4269	0.3207	0.1885	0.0897	0.3193	0.5148						

Note: Columns (1) to (6) show, in each cell, coefficient estimates α_1 defined in specification (2.2) in the first row, the corresponding standard error (estimated with cluster-robust variance estimation where observations are clustered on the county level) of each coefficient in the second row, as well as the AR (Anderson-Rubin) p-value for this coefficient in the third row. Significance stars are reported according to the cluster-robust variance estimation (significance stars are corresponding to: $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$). In Columns (7) to (12) the coefficients of the reduced form regressions as defined in specification (2.3) are presented. Within each cell, the first row shows the estimated coefficient ω_1 and in the second row the estimated coefficient ω_2 as defined in specification (2.3). For the reduced form regressions, standard errors were calculated using cluster-robust standard error estimates, where observations are clustered on the county level. Standard errors are not reported in the table, however, significance stars are reported for each coefficient and are corresponding to $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.
In the upper part (CCC), the estimates for the CCC data are shown, i.e., where the indicator whether there is at least one protest within each county and week is defined using the protests occurring in the CCC data. For these definition, the estimates are done for different samples: all household (All), young households in which there lives at least one person aged between 14 and 25 (Young), old household which are all those households that are not young (Old) and all households in which there lives at least one person aged between 0 and 13 (0-13). In the bottom part, the same estimations are conducted using the protests occurring in the FFF data.

2.C.2 Effect of First Protests

Table 2.C.2: IV and Reduced Form (Baseline Specification, Effect of First Protest)

	IV Estimates (b/se/AR p-value)						**RF Estimates** (b for IV/b for ΔIV)					
	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CCC												
All	-0.9007***	-0.6373**	-0.6476**	-0.8715**	-0.9748***	-0.7759**	-0.0073**	-0.0046*	-0.0063**	-0.0093***	-0.0093***	-0.0063**
	0.0557	0.0548	0.0471	0.0734	0.0578	0.0647	NA	NA	NA	NA	NA	NA
	0.0004	0.0138	0.0044	0.0003	0.0001	0.0018						
Young	-1.3019*	-1.0797	-0.6947	-1.0751	-1.7610**	-1.3341*	-0.0160**	-0.0198***	-0.0110*	-0.0194***	-0.0210***	-0.0024
	0.3520	0.3225	0.2762	0.3184	0.4447	0.3143	NA	NA	NA	NA	NA	NA
	0.1084	0.1519	0.2585	0.1621	0.0026	0.0842						
Old	-0.7439**	-0.4727	-0.6016**	-0.7754**	-0.7501**	-0.5887*	-0.0052*	-0.0008	-0.0052*	-0.0068*	-0.0063*	-0.0067*
	0.0573	0.0587	0.0527	0.0879	0.0585	0.0850	NA	NA	NA	NA	NA	NA
	0.0045	0.1226	0.0175	0.0043	0.0022	0.0724						
0-13	-1.5923*	-2.5697***	-1.5033*	-1.6390*	-1.2287	-2.1209**	-0.0063	-0.0100	-0.0130*	-0.0098	-0.0084	-0.0084
	0.4830	0.5953	0.4847	0.5075	0.6186	0.4723	NA	NA	NA	NA	NA	NA
	0.0414	0.0014	0.0378	0.0427	0.1868	0.0005						
FFF												
All	-0.7030**	-0.4895*	-0.6094**	-0.7828**	-0.9347***	-0.7357**	-0.0068**	-0.0041	-0.0056*	-0.0087***	-0.0085***	-0.0054*
	0.0529	0.0405	0.0449	0.0844	0.0753	0.0559	NA	NA	NA	NA	NA	NA
	0.0029	0.0250	0.0012	0.0004	0.0000	0.0003						
Young	-1.4321*	-1.0995*	-0.9535*	-1.1587	-1.7731**	-1.1929*	-0.0150**	-0.0179**	-0.0087	-0.0170***	-0.0193***	0.0007
	0.3246	0.2641	0.2236	0.3865	0.3729	0.2484	NA	NA	NA	NA	NA	NA
	0.0710	0.1684	0.1278	0.0497	0.0033	0.0889						
Old	-0.4989*	-0.3212	-0.5119*	-0.6537*	-0.7323**	-0.6019*	-0.0048*	-0.0006	-0.0048	-0.0065*	-0.0058*	-0.0063*
	0.0482	0.0462	0.0461	0.0709	0.0684	0.0695	NA	NA	NA	NA	NA	NA
	0.0454	0.2590	0.0111	0.0077	0.0015	0.0176						
0-13	-0.9766	-1.6409**	-1.1509	-0.9476	-0.9991	-1.9177**	-0.0057	-0.0092	-0.0116	-0.0079	-0.0066	-0.0070
	0.3371	0.3780	0.3906	0.4107	0.3832	0.3921	NA	NA	NA	NA	NA	NA
	0.1994	0.0122	0.0627	0.1144	0.2208	0.0003						

Note: Columns (1) to (6) show, in each cell, coefficient estimates α_1 defined in specification (2.2) (where the corresponding first stage is the modified specification (2.1), where we leave out ΔIV and replace the first-stage left hand side variable by an indicator variable indicating the very first protest in each counties), the corresponding standard error (estimated with cluster-robust variance estimation where observations are clustered on the county level) of each coefficient in the second row, as well as the AR (Anderson-Rubin) p-value for this coefficient in the third row. Significance stars are reported according to the cluster-robust variance estimation (significance stars are corresponding to: $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$). In Columns (7) to (12) the coefficients of the modified reduced form regressions as defined in specification (2.3) (where the effects are only estimated for the weeks after the very first protest in each county), are presented and we leave out ΔIV. Within each cell, the estimated coefficient ω_1, as defined in the modified specification (2.3), is displayed and a “NA” indicates that coefficient ω_2 was not estimated, as ΔIV is not included. For the reduced form regressions, standard errors were calculated using cluster-robust standard error estimates, where observations are clustered on the county level. Standard errors are not reported in the table, however, significance stars are reported for each coefficient and are corresponding to $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.
In the upper part (CCC), the estimates for the CCC data are shown, i.e., where the indicator whether there is at least one protest within each county and week is defined using the protests occurring in the CCC data. For these definition, the estimates are done for different samples: all household (All), young households in which there lives at least one person aged between 14 and 25 (Young), old household which are all those households that are not young (Old) and all households in which there lives at least one person aged between 0 and 13 (0-13). In the bottom part, the same estimations are conducted using the protests occurring in the FFF data.

2.D Robustness Checks

2.D.1 First-Stage: Summarize all Regions in Sweden

Table 2.D.1: First-Stage Regression, Full Specification: Summarize Protests in Sweden to One Measure, Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV′	0.00317**	0.00317**	0.00367**	0.00177***	0.00156***	0.00197***
	3.13	2.81	3.09	5.81	5.70	7.06
ΔIV′	0.00371***	0.00350***	0.00375***	0.00238***	0.00203***	0.00224***
	5.64	4.88	5.05	7.42	6.73	7.16
N	712780	712553	549325	712780	712553	549325
adj. R^2	0.535	0.551	0.546	0.321	0.396	0.362
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21
F-Test IVs	25.50	18.44	18.69	29.05	22.78	26.19

Note: Results of estimates for specification (2.4). The dependent variable equals the indicator variable whether there was at least one protest either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables: IV′ and ΔIV′ display the estimates of coefficients β_1 and β_2, respectively, defined in Equation (2.4). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The “F-Test IVs” is the test statistics of a F-test on the joint significance of IV′ and ΔIV′. The “Time Period” indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 2.D.2: First-Stage Regression, Simple Specification: Summarize Protests in Sweden to One Measure, Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV′	0.00141	0.00152	0.00173*	0.00065***	0.00061***	0.00083***
	1.89	1.82	2.02	3.57	3.78	5.86
N	712780	712553	549325	712780	712553	549325
adj. R^2	0.533	0.550	0.544	0.318	0.394	0.359
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21

Note: Results of estimates for specification (2.4), excluding variable ΔIV′. The dependent variable equals the indicator variable whether there was at least one protest – either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variable IV′ displays the estimates of coefficients β_1 defined in Equation (2.4). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The “Time Period” indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 2.D.3: First-Stage Regression, Full Specification: Summarize Protests in Sweden to One Measure, Outcome: First Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV′	0.00198***	0.00177***	0.00198***	0.00187***	0.00161***	0.00181***
	5.57	4.72	4.90	5.72	4.90	5.05
ΔIV′	0.00274***	0.00239***	0.00252***	0.00263***	0.00218***	0.00231***
	5.51	4.61	4.70	5.64	4.79	4.87
N	578823	578773	454924	588176	588119	461539
adj. R^2	0.092	0.161	0.162	0.119	0.203	0.202
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21
F-Test IVs	15.51	11.28	12.69	16.41	12.11	13.27

Note: Results of estimates for the modified specification (2.4), changing the dependent variable. The dependent variable equals the indicator variable equal to one in the week of the first protest, zero before the first protest and is set to "missing" after the first protest happened either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables: IV′ and ΔIV′ display the estimates of coefficients β_1 and β_2, respectively, defined in the modified Equation (2.4). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "F-Test IVs" is the test statistics of a F-test on the joint significance of IV′ and ΔIV′. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

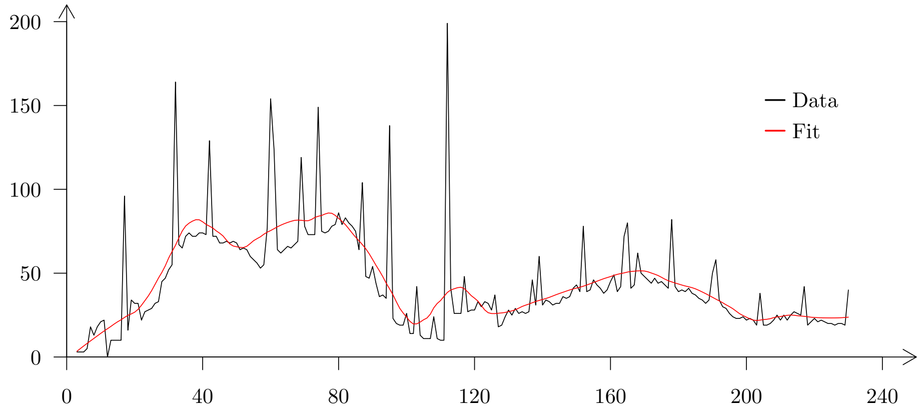
Table 2.D.4: First-Stage Regression, Simple Specification: Summarize Protests in Sweden to One Measure, Outcome: First Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV′	0.00075***	0.00069***	0.00076***	0.00070***	0.00070***	0.00069***
	5.26	4.55	4.68	5.42	5.42	4.80
N	578823	578773	454924	588176	588176	461539
adj. R^2	0.084	0.157	0.157	0.110	0.110	0.197
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21

Note: Results of estimates for the modified specification (2.4), changing the dependent variable and excluding variable ΔIV′. The dependent variable equals the indicator variable equal to one in the week of the first protest, zero before the first protest and is set to "missing" after the first protest happened either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variable IV′ displays the estimates of coefficients β_1 as defined in the modified Equation (2.4). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

2.D.2 First-Stage: Trend of Protests in Sweden

Figure 2.D.1: Trend Estimate for Protests in Sweden



Note: The Figure shows the actual actual number of protests over time in Sweden (black) and the fitted trend (red). The fitted trend is estimated using Locally Estimated Scatterplot Smoothing (LOESS). More specifically, for each point along the horizontal axis, a first-degree polynomial is fit to a subset of the data defined by a neighborhood around that point, with observations weighted by their distance to the target point. The smoothing parameter (span) is set to 0.15, meaning that 15% of the data points are used in each local fit. Tricubic weighting is applied, where weights are proportional to $(1 - (d/d_{\max})^3)^3$, with d representing the distance from the target point and $d_{\max}$ the maximum distance within the neighborhood.

Table 2.D.5: First-Stage Regression, Full Specification: Trend in Sweden as IV, Outcome: Any Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV$''$	0.00508***	0.00487***	0.00580***	0.00176***	0.00157***	0.00213***
	4.07	3.48	3.79	5.43	5.48	6.92
ΔIV$''$	-0.01024*	-0.00968	-0.00732	0.00695**	0.00722**	0.01126***
	-2.34	-1.90	-1.53	2.62	3.26	5.15
N	712780	712553	549325	712780	712553	549325
adj. R^2	0.535	0.552	0.546	0.319	0.395	0.360
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21
F-Test IVs	9.850	7.250	7.920	14.94	15.56	26.94

Note: Results of estimates for specification (2.5). The dependent variable equals the indicator variable whether there was at least one protest either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variables: IV$''$ and ΔIV$''$ display the estimates of coefficients β_1 and β_2, respectively, defined in Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "F-Test IVs" is the test statistics of a F-test on the joint significance of IV$''$ and ΔIV$''$. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

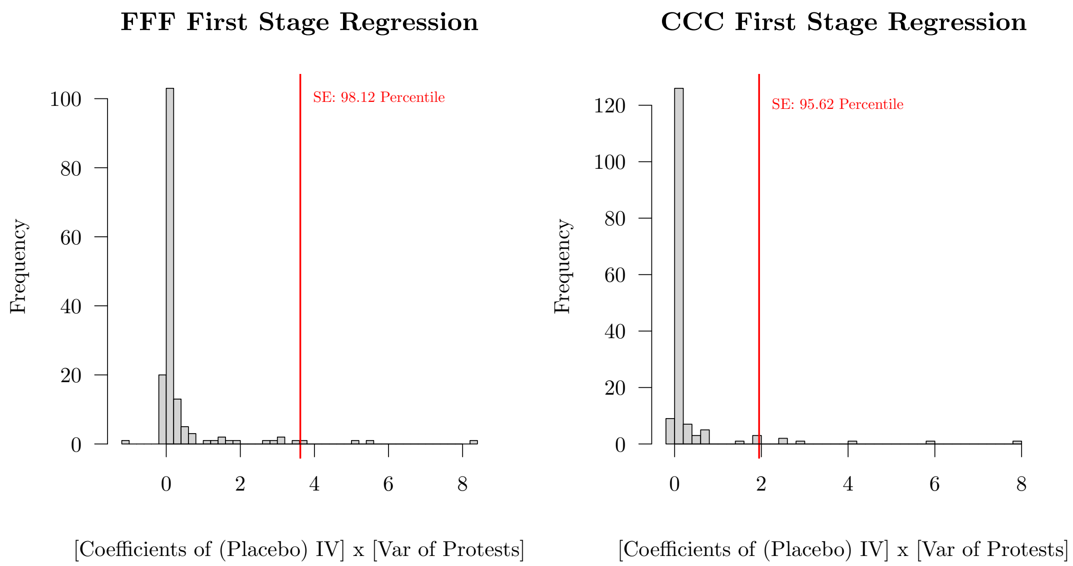
Table 2.D.6: First-Stage Regression, Simple Specification: Trend in Sweden as IV, Outcome: First Protest

	FFF			CCC		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/t	b/t	b/t	b/t	b/t	b/t
IV″	0.00188***	0.00170***	0.00197***	0.00173***	0.00173***	0.00171***
	5.74	4.80	5.07	5.87	5.87	5.24
N	578823	578773	454924	588176	588176	461539
adj. R^2	0.086	0.158	0.159	0.112	0.112	0.198
Type of FE	Week	State#Week	State#Week	Week	State#Week	State#Week
Time Period	All	All	08/18-12/21	All	All	08/18-12/21

Note: Results of estimates for the modified specification (2.5), changing the dependent variable and excluding variable ΔIV″. The dependent variable equals the indicator variable equal to one in the week of the first protest, zero before the first protest and is set to "missing" after the first protest happened either in the CCC data (Columns (1),(2) and (3)) or in the FFF data (Columns (4),(5) and (6)) in a certain county in a certain week. All regressions are conducted using clustered standard errors on the county-level. In all regressions, county fixed effects are included and the type of time fixed effect is indicated in each column, respectively. The variable IV″ displays the estimates of coefficients β_1 as defined in the modified Equation (2.1). In all specifications, the vector of controls $\mathbf{X}_{c,t}$, as defined in the main text, are included. The "Time Period" indicates whether there is any sample restriction. FE stands for fixed effects. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

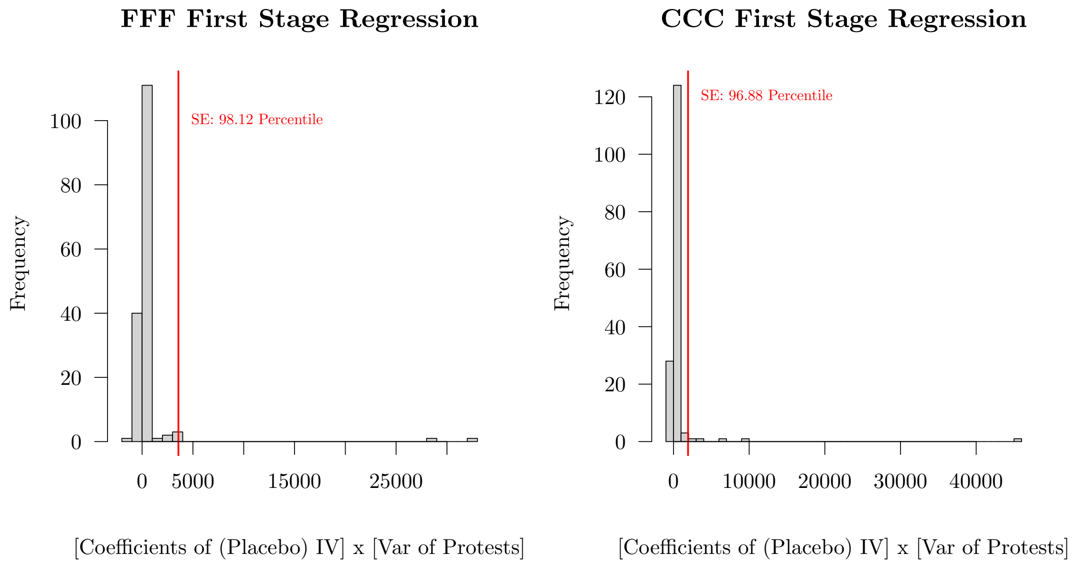
2.D.3 First-Stage: Relative Position of Sweden

Figure 2.D.2: Distribution of Scaled Coefficients for IV



Note: Distribution of scaled $\hat{\beta}_1$ coefficients of test described in Section 2.6.1. The position of the coefficient of Sweden (SE) is indicated by the red line, including the percentile of the coefficient of Sweden in the distribution of coefficients.

Figure 2.D.3: Distribution of Scaled Coefficients for ΔIV



Note: Distribution of scaled $\hat{\beta}_2$ coefficients of test described in Section 2.6.1. The position of the coefficient of Sweden (SE) is indicated by the red line, including the percentile of the coefficient of Sweden in the distribution of coefficients.

2.D.4 Second-Stage: Not-Yet Taker Analysis

Table 2.D.7: Not-Yet Taker Analysis, Young Households, CCC Protests

	(1)	(2)	(3)	(4)	(5)	(6)
	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$
	b/t	b/t	b/t	b/t	b/t	b/t
IV	0.01385	-0.00865	-0.00252	-0.00845	0.02457	0.00979
	0.67	-0.45	-0.16	-0.42	1.30	0.57
ΔIV	-0.00587	-0.01615	-0.01967	-0.01113	0.01251	0.00972
	-0.28	-0.86	-1.21	-0.59	0.69	0.49
N	292536	288660	284791	280972	277172	273257
adj. R^2	0.233	0.232	0.234	0.233	0.234	0.232

Note: Results for estimates of specification (2.3) for the sample of Young households who experienced at least one protest documented in the CCC database in their county, but have not yet experienced a protest in their county at time of observation. Each column refers to different vales of τ, thereby changing the outcome. The vector of controls is adjusted, leaving out the protest lag within each county. Standard errors are calculated using cluster-robust variance estimations, where the observations are clustered on the county level. Significance stars are corresponding to: *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Table 2.D.8: Not-Yet Taker Analysis, Young Households, FFF Protests

	(1)	(2)	(3)	(4)	(5)	(6)
	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$
	b/t	b/t	b/t	b/t	b/t	b/t
IV	-0.00788	-0.02093	0.00646	-0.01634	0.00050	0.02313
	-0.36	-0.95	0.30	-0.70	0.02	1.13
ΔIV	-0.01203	-0.02166	-0.00049	-0.02208	0.00238	0.00811
	-0.54	-1.07	-0.02	-1.07	0.13	0.37
N	285700	281833	278006	274177	270412	266502
adj. R^2	0.235	0.235	0.237	0.234	0.236	0.235

Note: Results for estimates of specification (2.3) for the sample of Young households who experienced at least one protest documented in the FFF database in their county, but have not yet experienced a protest in their county at time of observation. Each column refers to different vales of τ, thereby changing the outcome. The vector of controls is adjusted, leaving out the protest lag within each county. Standard errors are calculated using cluster-robust variance estimations, where the observations are clustered on the county level. Significance stars are corresponding to: *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

2.D.5 Second-Stage: Include Linear Trend

Table 2.D.9: Robustness Specification with Linear Trend, Effect of Any Protest

	IV Estimates (b/se/AR p-value)						**RF Estimates** (b for IV/b for ΔIV)					
	$\tau = 1$ (1)	$\tau = 2$ (2)	$\tau = 3$ (3)	$\tau = 4$ (4)	$\tau = 5$ (5)	$\tau = 6$ (6)	$\tau = 1$ (7)	$\tau = 2$ (8)	$\tau = 3$ (9)	$\tau = 4$ (10)	$\tau = 5$ (11)	$\tau = 6$ (12)
CCC												
All	-0.0699*	-0.0829	-0.0627	-0.1012**	-0.0659	-0.0862*	-0.0109***	-0.0106***	-0.0098**	-0.0110**	-0.0136***	-0.0123***
	0.0010	0.0020	0.0012	0.0013	0.0015	0.0012	-0.0059*	-0.0097***	-0.0058*	-0.0032	-0.0074**	-0.0100***
	0.0096	0.0084	0.0345	0.0001	0.0003	0.0058						
Young	-0.2208**	-0.1897	-0.2842**	-0.1892*	-0.1597*	-0.1678*	-0.0230**	-0.0317***	-0.0287***	-0.0238***	-0.0275***	-0.0159*
	0.0049	0.0129	0.0079	0.0056	0.0051	0.0048	-0.0112	-0.0187*	-0.0276***	-0.0076	-0.0108	-0.0216**
	0.0024	0.0014	0.0010	0.0008	0.0003	0.0462						
Old	-0.0344	-0.0566	-0.0057	-0.0773*	-0.0419	-0.0648	-0.0079*	-0.0056	-0.0051	-0.0077*	-0.0101**	-0.0108**
	0.0011	0.0015	0.0012	0.0014	0.0016	0.0015	-0.0044	-0.0077**	-0.0004	-0.0020	-0.0064	-0.0072*
	0.0469	0.1050	0.2553	0.0350	0.0078	0.0685						
0-13	-0.1397	-0.1195	-0.1393	-0.2038*	-0.0768	-0.0798	-0.0097	-0.0142	-0.0188*	-0.0187*	-0.0109	-0.0149
	0.0069	0.0106	0.0082	0.0075	0.0064	0.0091	-0.0053	-0.0065	-0.0091	-0.0137	-0.0041	-0.0103
	0.2397	0.4556	0.1898	0.0903	0.6333	0.4476						
FFF												
All	-0.0101	-0.0098	-0.0100	-0.0195**	-0.0119	-0.0117	-0.0105**	-0.0102**	-0.0090**	-0.0103**	-0.0128***	-0.0114**
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	-0.0058*	-0.0095***	-0.0055	-0.0029	-0.0071*	-0.0096**
	0.0111	0.0105	0.0105	0.0001	0.0019	0.0025						
Young	-0.0353*	-0.0354**	-0.0380*	-0.0526**	-0.0416**	-0.0325	-0.0220**	-0.0295***	-0.0263**	-0.0209**	-0.0252***	-0.0119
	0.0002	0.0002	0.0003	0.0003	0.0002	0.0003	-0.0107	-0.0177*	-0.0268***	-0.0065	-0.0097	-0.0198**
	0.0023	0.0005	0.0005	0.0000	0.0002	0.0012						
Old	-0.0036	-0.0031	-0.0025	-0.0112	-0.0041	-0.0058	-0.0076*	-0.0055	-0.0047	-0.0075	-0.0096**	-0.0106**
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	-0.0044	-0.0077**	-0.0002	-0.0018	-0.0062	-0.0071*
	0.2406	0.1264	0.3179	0.0662	0.1182	0.0860						
0-13	-0.0301	-0.0291	-0.0261	-0.0356*	-0.0333	-0.0123	-0.0091	-0.0134	-0.0174*	-0.0167*	-0.0084	-0.0132
	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	-0.0051	-0.0063	-0.0087	-0.0132	-0.0030	-0.0095
	0.4544	0.3397	0.2066	0.1060	0.3538	0.5685						

Note: Columns (1) to (6) show, in each cell, coefficient estimates α_1 defined in specification (2.6) in the first row, the corresponding standard error (estimated with cluster-robust variance estimation where observations are clustered on the county level) of each coefficient in the second row, as well as the AR (Anderson-Rubin) p-value for this coefficient in the third row. Significance stars are reported according to the cluster-robust variance estimation (significance stars are corresponding to: $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$). In Columns (7) to (12) the coefficients of the modified reduced form regressions as defined in specification (2.3) are presented. Specification (2.3) is modified by including $ABSCI_i$ as defined in the main text as an additional control. Within each cell, the first row shows the estimated coefficient ω_1 and in the second row the estimated coefficient ω_2 as defined in specification (2.3). For the reduced form regressions, standard errors were calculated using cluster-robust standard error estimates, where observations are clustered on the county level. Standard errors are not reported in the table, however, significance stars are reported for each coefficient and are corresponding to $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

In the upper part (CCC), the estimates for the CCC data are shown, i.e., where the indicator whether there is at least one protest within each county and week is defined using the protests occurring in the CCC data. For these definition, the estimates are done for different samples: all household (All), young households in which there lives at least one person aged between 14 and 25 (Young), old household which are all those households that are not young (Old) and all households in which there lives at least one person aged between 0 and 13 (0-13). In the bottom part, the same estimations are conducted using the protests occurring in the FFF data.

2.D.6 Second-Stage: Include Past Meat Consumption

Table 2.D.10: Robustness Specification with Past Meat Consumption, Effect of Any Protest

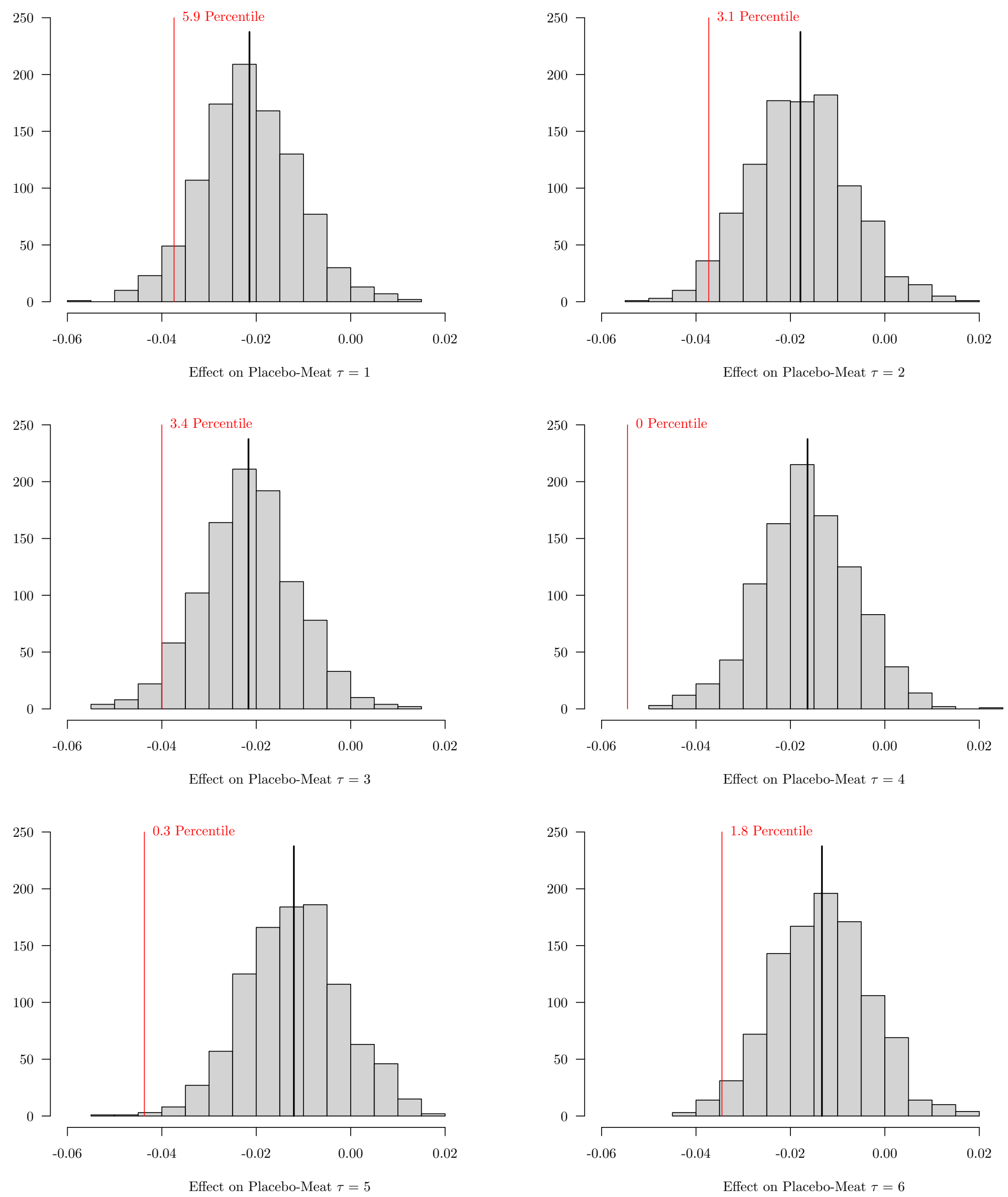
	IV Estimates (b/se/AR p-value)						**RF Estimates** (b for IV/b for ΔIV)					
	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CCC												
All	-0.0785*	-0.0898*	-0.0698*	-0.1083**	-0.0742	-0.0953**	-0.0114***	-0.0106***	-0.0097**	-0.0110**	-0.0138***	-0.0126***
	0.0010	0.0019	0.0011	0.0012	0.0015	0.0012	-0.0065*	-0.0102***	-0.0063*	-0.0038	-0.0080**	-0.0107***
	0.0048	0.0050	0.0283	0.0001	0.0002	0.0022						
Young	-0.2307**	-0.1957	-0.2899***	-0.1953**	-0.1678*	-0.1774*	-0.0238**	-0.0319***	-0.0288***	-0.0241***	-0.0280***	-0.0166*
	0.0050	0.0129	0.0077	0.0054	0.0051	0.0049	-0.0121	-0.0192*	-0.0282***	-0.0082	-0.0116	-0.0225**
	0.0017	0.0007	0.0004	0.0006	0.0002	0.0372						
Old	-0.0427	-0.0637	-0.0132	-0.0848*	-0.0505	-0.0740	-0.0084*	-0.0056	-0.0051	-0.0077*	-0.0103**	-0.0112**
	0.0011	0.0014	0.0011	0.0014	0.0016	0.0015	-0.0051	-0.0082**	-0.0010	-0.0026	-0.0070*	-0.0079*
	0.0286	0.0622	0.4028	0.0326	0.0062	0.0404						
0-13	-0.1439	-0.1194	-0.1382	-0.2031*	-0.0783	-0.0826	-0.0098	-0.0129	-0.0173*	-0.0172*	-0.0099	-0.0142
	0.0069	0.0100	0.0076	0.0070	0.0061	0.0089	-0.0056	-0.0065	-0.0090	-0.0137	-0.0043	-0.0106
	0.2304	0.4890	0.2334	0.0951	0.6575	0.4890						
FFF												
All	-0.0118*	-0.0111	-0.0113	-0.0207**	-0.0134*	-0.0132	-0.0109***	-0.0100**	-0.0088**	-0.0102**	-0.0129***	-0.0116**
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0001	-0.0064*	-0.0100***	-0.0059*	-0.0034	-0.0077**	-0.0103***
	0.0051	0.0043	0.0040	0.0000	0.0009	0.0006						
Young	-0.0368*	-0.0356**	-0.0379*	-0.0524**	-0.0421**	-0.0332	-0.0226**	-0.0296***	-0.0264***	-0.0210**	-0.0257***	-0.0125
	0.0002	0.0002	0.0003	0.0003	0.0002	0.0003	-0.0115	-0.0182*	-0.0273***	-0.0070	-0.0104	-0.0206**
	0.0012	0.0002	0.0002	0.0000	0.0001	0.0006						
Old	-0.0054	-0.0046	-0.0041	-0.0127*	-0.0058	-0.0075	-0.0080*	-0.0054	-0.0045	-0.0073	-0.0096**	-0.0108**
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	-0.0050	-0.0082**	-0.0006	-0.0023	-0.0068*	-0.0077*
	0.1824	0.0736	0.2780	0.0424	0.0856	0.0475						
0-13	-0.0308	-0.0288	-0.0256	-0.0351*	-0.0332	-0.0125	-0.0092	-0.0121	-0.0159	-0.0152	-0.0074	-0.0124
	0.0003	0.0003	0.0002	0.0003	0.0003	0.0003	-0.0054	-0.0063	-0.0087	-0.0131	-0.0031	-0.0098
	0.4360	0.3658	0.2389	0.0941	0.3239	0.5642						

Note: Columns (1) to (6) show, in each cell, coefficient estimates α_1 defined in specification (2.7) in the first row, the corresponding standard error (estimated with cluster-robust variance estimation where observations are clustered on the county level) of each coefficient in the second row, as well as the AR (Anderson-Rubin) p-value for this coefficient in the third row. Significance stars are reported according to the cluster-robust variance estimation (significance stars are corresponding to: $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$). In Columns (7) to (12) the coefficients of the modified reduced form regressions as defined in specification (2.3) are presented. Specification (2.3) is modified by including $C_{i(c),t-1} + C_{i(c),t}$ as defined in the main text as an additional control. Within each cell, the first row shows the estimated coefficient ω_1 and in the second row the estimated coefficient ω_2 as defined in specification (2.3). For the reduced form regressions, standard errors were calculated using cluster-robust standard error estimates, where observations are clustered on the county level. Standard errors are not reported in the table, however, significance stars are reported for each coefficient and are corresponding to $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$.

In the upper part (CCC), the estimates for the CCC data are shown, i.e., where the indicator whether there is at least one protest within each county and week is defined using the protests occurring in the CCC data. For these definition, the estimates are done for different samples: all household (All), young households in which there lives at least one person aged between 14 and 25 (Young), old household which are all those households that are not young (Old) and all households in which there lives at least one person aged between 0 and 13 (0-13). In the bottom part, the same estimations are conducted using the protests occurring in the FFF data.

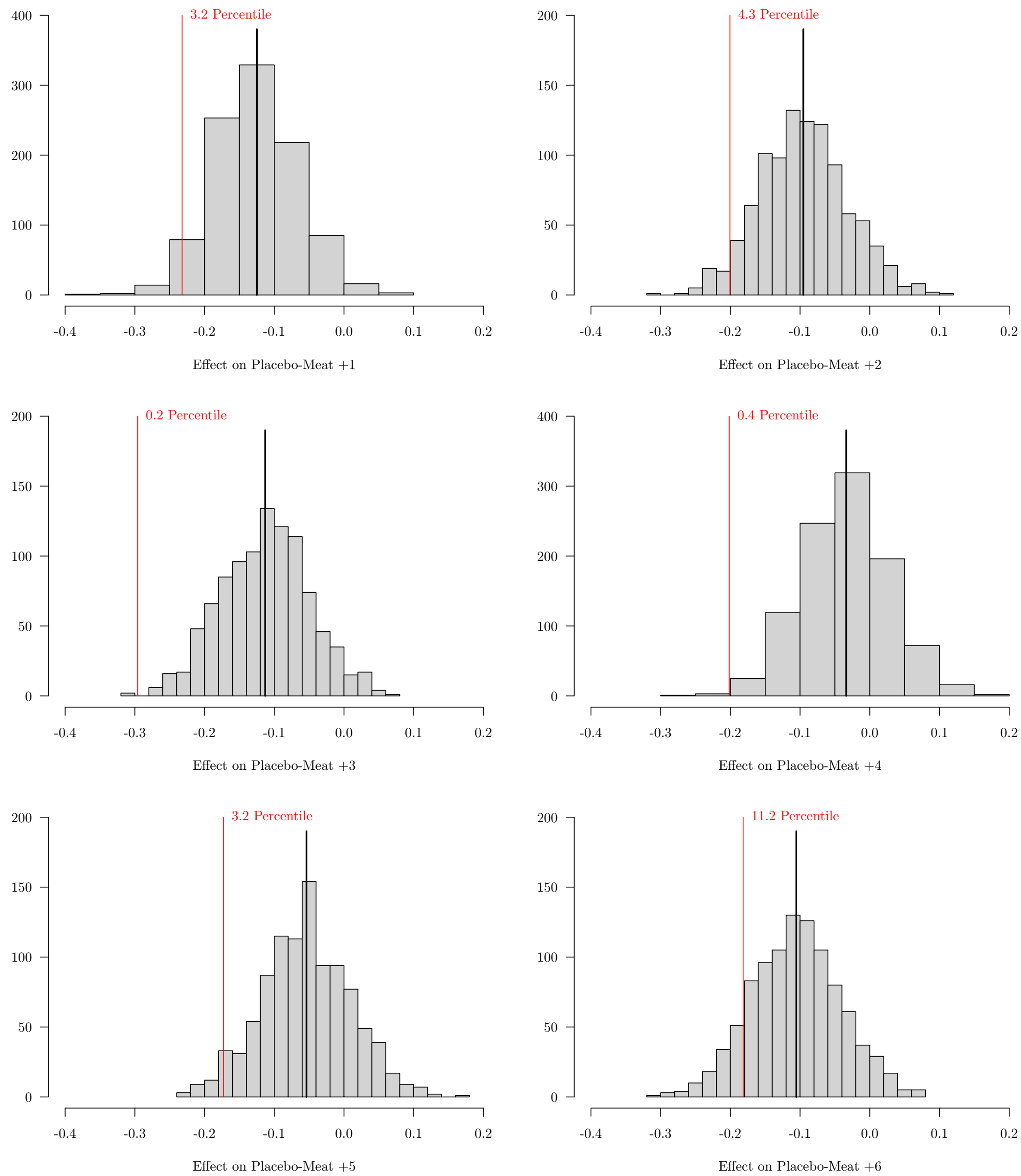
2.D.7 Second-Stage: Placebo Permutation Test

Figure 2.D.4: Placebo Meat Consumption, Young Households, FFF Data



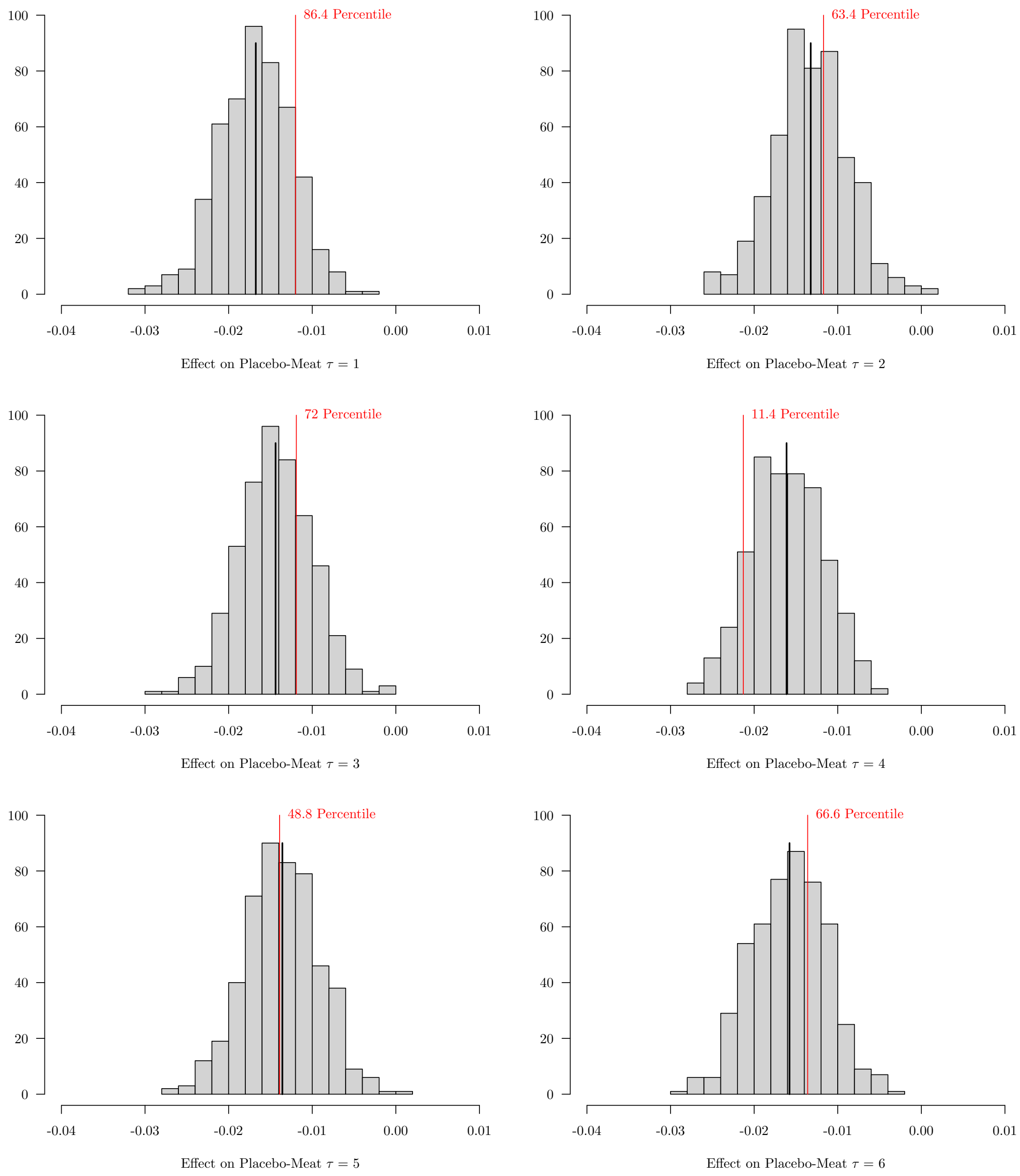
Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for Young households for the FFF protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

Figure 2.D.5: Placebo Meat Consumption, Young Households, CCC Data



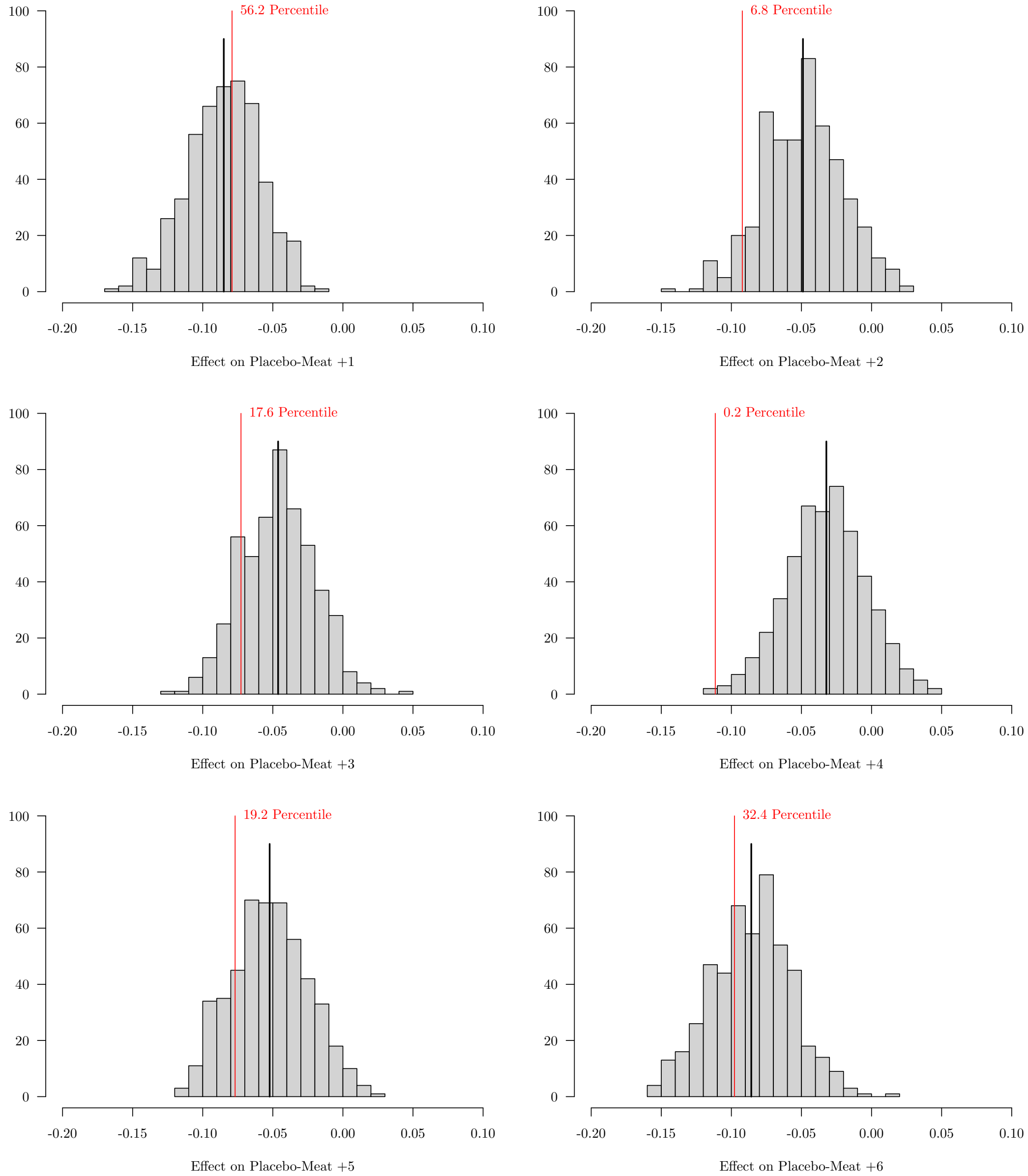
Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for Young households for the CCC protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

Figure 2.D.6: Placebo Meat Consumption, All Households, FFF Data



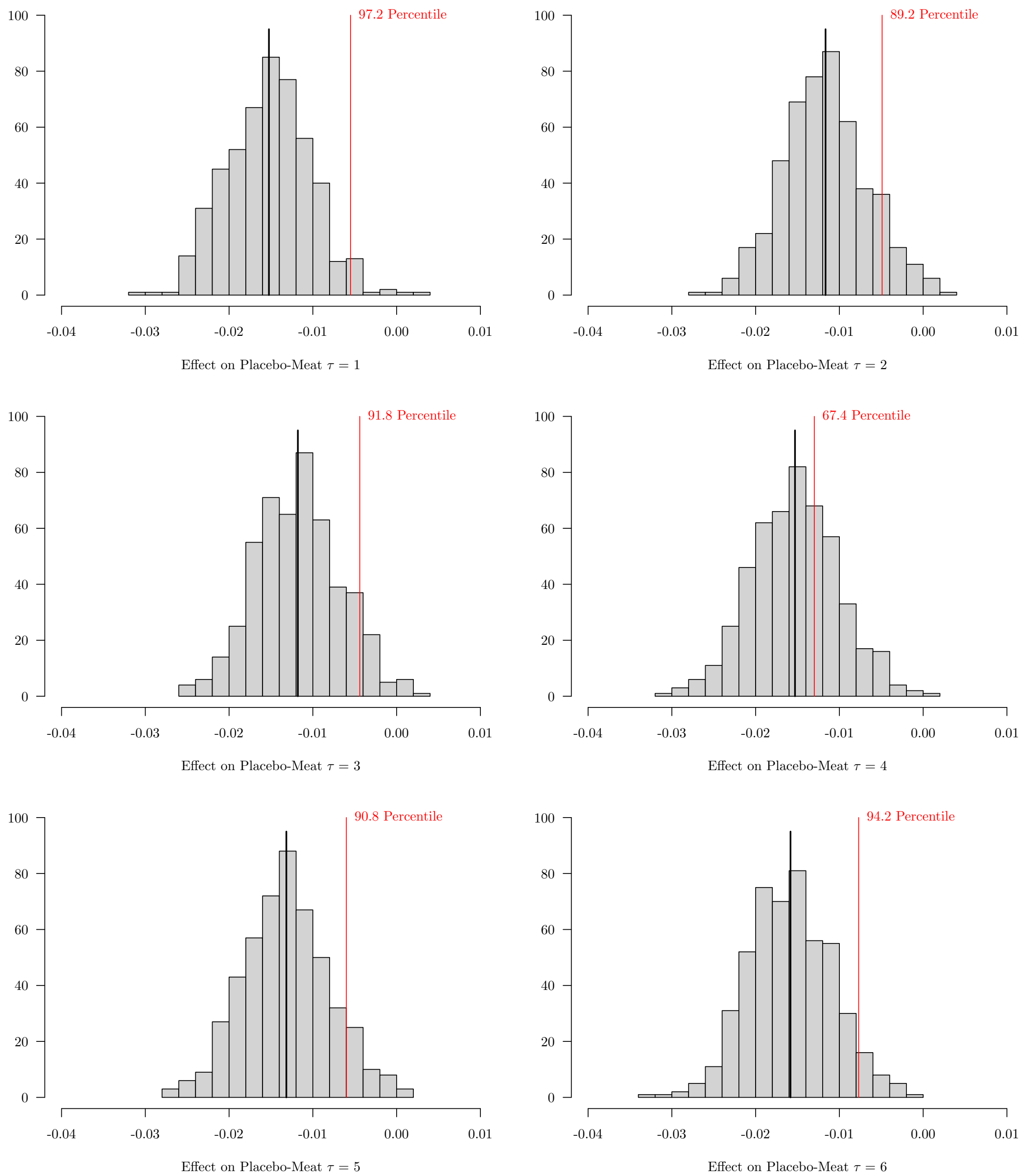
Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for all households for the FFF protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

Figure 2.D.7: Placebo Meat Consumption, All Households, CCC Data



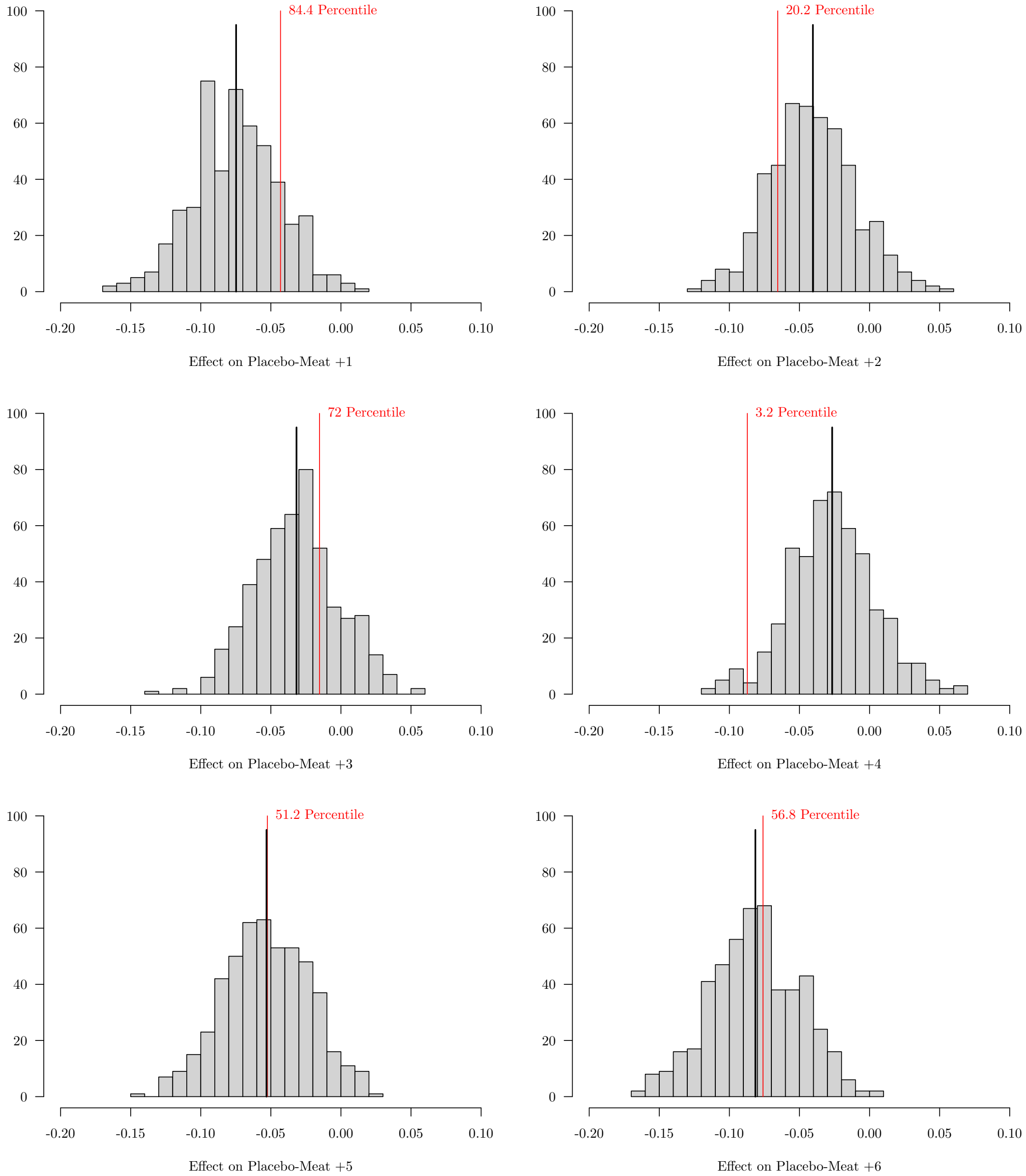
Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for all households for the CCC protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

Figure 2.D.8: Placebo Meat Consumption, Old Households, FFF Data



Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for Old households for the FFF protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

Figure 2.D.9: Placebo Meat Consumption, Old Households, CCC Data



Note: For each histogram, we construct alternative outcome variables in two steps. First, from all purchases that were not classified as meat purchases (i.e., these are 94.54% of the 244,799,387 purchases), we randomly select 5.46% (this was the fraction of purchases that was classified as meat) and label them as placebo meat. Second, for each household and each week, the alternative outcome variable equals the sum of purchases that were randomly classified as placebo meat for this household in that week. We repeat both steps 1000 times to generate 1000 random alternative placebo outcome variables. For each of these 1000 placebo outcome variables, we run the IV regression for Old households for the CCC protests, respectively. The histogram shows the distribution of these placebo effects. Then, we calculate the relative position (i.e., the percentile) of the respective baseline second-stage effects (indicated by the red line) within the distribution of the placebo estimates, separately for each specification. The black line indicates the average placebo effect.

2.D.8 Second-Stage: Spatial Clustering

To calculate standard errors accounting for spatial dependence of observations, consider the variance-covariance matrix of IV regressions required to introduce spatial clustering of standard errors: (Colella et al., 2020):

$$VCV(b) = \left(\hat{X}'\hat{X}\right)^{-1}\hat{X}'\Big(S \times (uu')\Big)\hat{X}\left(\hat{X}'\hat{X}\right)^{-1}$$

where $\hat{X} = \left(Z'Z\right)^{-1}Z'XZ$ is obtained from the first-stage regression with instruments Z, u are the residuals from the second stage and S is the pattern matrix (also see Cameron, Gelbach and Miller (2011)). In a spatial cross-sectional setting, the element ij in S equals $K(\cdot,\cdot)$, where $K : \mathbb{R}^2 \rightarrow [0,1]$ is a kernel function mapping positions in space to a weight measuring the dependence between two observations (Conley, 1999). In our case, the aim is to control for (1) correlation of error terms for households that live closer together than some cutoff in the cross-section (while households living closer together within that cutoff will receive a higher weight) and (2) serial correlation of error terms within households over time.

To calculate the error terms of coefficients under these considerations, we first need to estimate the proximity of each household to every other household. To do so, we use the 5-digit postal zip-code in which households live. The center of the area covered by one postal code is assumed to be the location of a household living in that zip-code area. We obtain the coordinates of the geographic center of each of the 5-digit zip code areas through two steps. First, we download the a dataset from `https://gist.github.com/erichurst/7882666` (last access, 23.09.2024) which contains the spatial center point for almost all US zip code areas. Second, for those zip code areas which are missing, we scrape the geographic center from `https://greatdata.com/` (last access 23.09.2024).

To execute the calculation spatially adjusted standard errors, we denote two arbitrary households by i and j and consider two arbitrary time periods (weeks) t and t'. We calculate for each observation pair i t and j s the distance in space (that does not vary over time) and assign a weight in $[0,1]$ to that pair. This means that: First, for a given i, because the distance of the observations i t and i t' is always zero for any pair t, t', the cell in row i t and column i t' is equal to one for any pair t, t'. Thus, instead of controlling "only" for serial correlation, we do control for any possible form of correlation between all error terms across time within each household. Second, for any two households i and j, the employed procedure controls for the correlation in error terms not only in the cross-section but also between any two time periods t and t'. In other words, we allow all error terms of household i to be correlated in any form with all error terms of household j, when their spatial proximity is below a certain cutoff value. We implemented the uniform kernel function and used different cutoff values (in miles). The cutoff of 100 means, for instance, that the correlation between the error terms between two households is assumed to be zero if these households are living at least 100 miles apart.

Table 2.D.11: Second-Stage with Alternative Clustering

		Clustering on County-Level		Spatial Clustering Cutoff=100		Spatial Clustering Cutoff=300		Spatial Clustering Cutoff=500	
	Coefficient	Std. Err.	p-Value	Std. Err.	p-Value	Std. Err.	p-Value	Std. Err.	p-Value
FFF Estimates:									
$\tau = 1$	-0.03738	0.0147	0.011	0.0141	0.008	0.0145	0.010	0.0120	0.002
$\tau = 2$	-0.03732	0.0138	0.007	0.0133	0.005	0.0143	0.009	0.0125	0.003
$\tau = 3$	-0.03998	0.0164	0.015	0.0156	0.010	0.0161	0.013	0.0141	0.005
$\tau = 4$	-0.05455	0.0177	0.002	0.0170	0.001	0.0179	0.002	0.0187	0.004
$\tau = 5$	-0.04367	0.0154	0.005	0.0132	0.001	0.0138	0.001	0.0115	0.000
$\tau = 6$	-0.03453	0.0177	0.051	0.0175	0.049	0.0193	0.074	0.0190	0.069
CCC Estimates:									
$\tau = 1$	-0.23218	0.0714	0.001	0.0734	0.002	0.0536	0.000	0.0520	0.000
$\tau = 2$	-0.20072	0.1155	0.082	0.0883	0.023	0.1044	0.054	0.1144	0.079
$\tau = 3$	-0.29604	0.0902	0.001	0.0968	0.002	0.1066	0.005	0.1182	0.012
$\tau = 4$	-0.20172	0.0758	0.008	0.0696	0.004	0.0693	0.004	0.0485	0.000
$\tau = 5$	-0.17297	0.0731	0.018	0.0731	0.018	0.0880	0.049	0.0905	0.056
$\tau = 6$	-0.18167	0.0707	0.010	0.0776	0.019	0.0553	0.001	0.0769	0.018

Note: The table shows the instrumental variable results: The coefficient refers to the second-stage coefficient of the baseline specifications discussed in Appendix 2.C.1 of a protest in week $t = 0$ on the meat consumption of Young households in week $t = \tau$. The spatial clustering was calculated as described in Appendix 2.D.8 for three different cutoff values: 100 miles, 300 miles and 500 miles. For each cutoff, the table shows the standard error of the coefficient and the corresponding p-value.

2.E Data Sources Fridays for Future Database

Table 2.E.1: Data Sources for Fridays for Future Protests

Table	Source	Download Date
2018, SS4C	https://map.fridaysforfuture.org/lists?list=SS4C_Global_Trends_Record_2018_08_09&show=allevents#	17.05.2023
2018, 4	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2018_09_10_11_12&show=allevents#	15.05.2023
2019, 1	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2019_01_02_03&show=allevents#	15.05.2023
2019, 2	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2019_04_05_06&show=allevents#	15.05.2023
2019, 3	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2019_07_08_09&show=allevents#	15.05.2023
2019, 4	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2019_10_11_12&show=allevents#	15.05.2023
2020, 1	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2020_01_02_03&show=allevents#	15.05.2023
2020, 1 Shoe	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_Monthly_ShoeProtest_2020_04_05_06_07_08&show=allevents#	17.05.2023
2020, 2	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2020_04_05_06&show=allevents#	15.05.2023
2020, 2 Shoe	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_Monthly_ShoeProtest_2020_09_10_11_12&show=allevents#	17.05.2023
2020, 3	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2020_07_08_09&show=allevents#	15.05.2023
2020, 4	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2020_10_11_12&show=allevents#	17.05.2023
2021, 1	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2021_01_02_03&show=allevents#	17.05.2023
2021, 2	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2021_04_05_06&show=allevents#	17.05.2023
2021, 3	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2021_07_08_09&show=allevents#	17.05.2023
2021, 4	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2021_10_11_12&show=allevents#	17.05.2023
2022, 1	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2022_01_02_03&show=allevents#	17.05.2023
2022, 2	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2022_04_05_06&show=allevents#	17.05.2023
2022, 3	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2022_07_08_09&show=allevents#	17.05.2023
2022, 4	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2022_10_11_12&show=allevents#	17.05.2023
2023, 1	https://map.fridaysforfuture.org/lists?list=FFF_Global_Trends_Record_2023_01_02_03&show=allevents#	17.05.2023

Note: The Table shows all sources used to construct the FFF database. The protests in the FFF database is reported in several tables, reporting all protests worldwide on a quarterly basis. In addition, there are some special tables reporting "Shoe Protests" (see Section 2.3.2). The hyperlinks directly show the tables reporting the protests.

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Chapter 3

Beyond Free Riders. Assessing the Additionality of Municipal Plug-PV Subsidy Programs

3.1 Introduction

Globally, governments have established programs to support clean energy investments. Examples include Germany's Special Climate and Transformation Fund, China's subsidies for renewable electricity generation and the U.S. Inflation Reduction Act.[1] Between April 2020 and April 2023, total public spending on all clean energy investment support across the world reached USD 1,343 billion, averaging USD 448 billion per year (IEA, 2023). This indicates that public clean energy investment support amounted to at least 0.45 % of global GDP annually.[2] Since many of these government programs are typically applied uniformly across a country over a specific period, constructing a credible counterfactual to assess whether these programs cause *additional* investments is challenging. In this

* The author especially wishes to thank Antonio Ciccone, Markus Frölich and Maximilian Mähr. The author gratefully acknowledges the support of the staff from various German municipalities and counties, whose willingness to answer numerous questions and share valuable insights greatly contributed to this research.
The author gratefully acknowledges financial support by the German Research Foundation (DFG) through CRC TR 224 (project A04).

[1] See Appendix 3.F.3 for details.

[2] Own calculations based on `https://data.worldbank.org/indicator/NY.GDP.MKTP.CD` (last access: 22.07.2025). The number is a lower bound, as the database by IEA (2023) might not cover all support programs. For instance, it does not cover the municipal subsidy programs evaluated in this study.

chapter, I address this challenge for a specific type of clean energy investment support. I exploit variations in both the existence and the staggered roll-out of subsidy programs for so-called plug-in photovoltaic systems (hereafter referred to as plug-PV systems) across German municipalities. Using large variations in subsidy characteristics across different programs, I estimate the effects of these characteristics on the number of additional plug-PV systems caused by these programs.

Indeed, designing investment subsidy schemes that maximize private investments while minimizing public spending has long been of interest to both policymakers and researchers. Although researchers have identified subsidy schemes that achieve this trade-off, i.e. minimizing public spending for a given investment target (e.g., De Groote and Verboven (2019); Burr (2016); Feger, Pavanini and Radulescu (2022); Langer and Lemoine (2022); Kiso (2022)), they have, to the best of my knowledge, overlooked a key practical constraint faced by policymakers: fiscal rules and budgetary limits. For instance, De Groote and Verboven (2019) find that if Belgium had replaced its electricity-output subsidy for photovoltaic systems with upfront investment subsidies, it could have reduced total subsidy spending by 51% for the same level of private investments in photovoltaic systems. However, it remains unclear whether the Belgian government could have legally and practically financed the required upfront subsidy payments in the respective fiscal year(s), rather than smoothing the output subsidy payments across several fiscal years. In fact, fiscal constraints are tightening globally, pressuring governments to consolidate fiscal budgets (IMF, 2025). Thus, the question of how upfront subsidy programs operating under tight subsidy budgets should be designed to achieve a maximum of additional investments is both understudied and increasingly important. Using both the results of the empirical analysis in this chapter and a theoretical static framework, I derive implications for the design of optimal subsidy programs when the policymaker is operating under tight budget constraints.

Since 2018, plug-PV systems have been legally permitted for installation in Germany. These systems are small photovoltaic setups typically comprising two photovoltaic panels equipped with a converter. Notably, plug-PV systems function like conventional photovoltaic systems, but their installation is significantly simpler. They are referred to as *plug*-PV systems because they can be installed by placing the panels in a suitable location and plugging the converter's cable directly into a power socket. This technical feature simplifies the analysis of the subsidy programs for plug-PV systems conducted in this chapter, as local mechanics are not required for installation. This results in a uniform price for the systems across municipalities, enabling the comparison of both varying upfront subsidy payments and the varying effects of subsidy programs across different municipalities. Moreover, another distinct feature of plug-PV systems is the fact that they are not required to be installed on the roof. Most users place the panels on their balcony. Thus, also renters can use them, implying that subsidy programs are targeting a large proportion of the population. Given these technical features, policymakers in municipalities and state governments have implemented subsidy programs for plug-PV systems to encourage their adoption, based on the reasoning that a financial incentive could motivate a broad segment of the population to invest in a simple form of clean energy. In fact, from the nearly 11,000 municipalities in Germany, at least 400 implemented their own subsidy programs for plug-PV systems between 2018 and March 2024.[3] Municipalities in Germany operate under very tight budget constraints. According to anecdotal evidence from communications with these municipalities, policymakers designed these upfront subsidy programs with a predetermined limited budget. In all analyzed programs, policymakers set an upfront subsidy payment and used either a lottery or a first-come-first-served approach to determine subsidy recipients within the budget limits. In fact, for 81.8% of all subsidy programs analyzed in this chapter that ended before the final observation period, official sources confirmed that the allocated budget was fully used by the time the program ended. This indicates that the budgetary limit was binding in these cases.

To identify the causal effects of these subsidy programs on investments in plug-PV systems, I construct a novel hand-collected dataset. The panel dataset from Bertelsmann Stiftung (2024), which reports indicators for all German municipalities with more than 5,000 inhabitants, covering approximately 90% of the German population, serves as a basis for the analysis.[4] For the municipalities contained in the Bertelsmann dataset, I used web scraping to obtain their email addresses and sent an individual email to each in March 2024. The email inquired whether they had, ever had, or planned to have a subsidy program for plug-PV systems. I received 1,352 responses, which corresponds to a response rate of 45.6%. In addition, I sent individual emails to all German counties ("Landkreise") requesting the same information and checked state government websites for any such subsidy programs on the state ("Bundesländer") level. I restrict the analysis to municipalities where neither the respective county nor state had or planned to have a subsidy program for plug-PV systems as of March 2024. This leaves

[3] Source: Own data collection, see Section 3.3.3.

[4] The dataset does not contain "kreisfreie Städte", which are cities independent from the counties.

me with a sample of 733 municipalities, of which 223 had implemented their own subsidy programs and 510 had not in March 2024. For the 223 municipalities with subsidy programs, I hand-collected detailed information on each program. The outcome analyzed in this study is derived from the "Marktstammdatenregister" (MaStR), an official registry operated by the German Federal Network Agency. This registry includes all electricity-generating facilities connected to the grid, including plug-PV systems.[5] I complement this data with hand-collected price data for plug-PV systems from two German websites and data on the potential yearly return of solar systems across municipalities from the Germany's National Meteorological Service (Deutscher Wetterdienst, 2025).

Using the hand-collected panel dataset, I employ three distinct methods to estimate the number of additional plug-PV systems caused by a municipal subsidy program for each municipality with a program and each month following the program's initiation. The three methods used are the Synthetic Control method (Abadie, 2021), a parametric event-study method (Borusyak, Jaravel and Spiess, 2024) and the Synthetic Difference-in-Differences approach (Arkhangelsky et al., 2021). Although these methods rely on different identifying assumptions, they all depend on some form of a (weighted) parallel trends assumption. Intuitively, each method constructs a counterfactual trend of plug-PV systems after the start of a subsidy program for the respective subsidizing municipality using the (weighted) trend of plug-PV system installations from those municipalities that never (or not yet) implemented their own program. This allows for the estimation of the number of additional plug-PV systems caused by each individual program separately. With the set of estimates for the number of additional plug-PV systems caused by each single subsidy program, I use two different approaches to identify the effects of subsidy program characteristics and municipality characteristics on these estimates. First, as an exploratory approach, I use the Least Absolute Shrinkage and Selection Operator (LASSO) to determine which characteristics are relevant in predicting the number of additional plug-PV systems caused by a subsidy program. Second, motivated by theoretical considerations on the relevance of the subsidy value and the subsidy program budget on the additional plug-PV systems caused by the programs, I employ the Double Debiased Machine Learning (DDML) algorithm (Chernozhukov et al., 2018). Intuitively, the DDML algorithm estimates the partial linear effect of both the subsidy value and the subsidy program budget on the number of additional plug-PV systems, while

[5] Since 2014, all electricity-generating facilities are required to register with the MaStR registry in Germany.

remaining exploratory regarding both the relevant control characteristics and the way how the relevant control characteristics should be accounted for to identify the respective partial linear effects.

Across all subsidy programs I find, on average, a positive number of additional plug-PV systems caused by the subsidy programs. This result holds true regardless of the estimation method used or the month considered after the program's initiation. Back-of-the-envelope calculations indicate that these subsidy programs can be considered beneficial from the perspective of causing additional photovoltaic capacity within municipalities. Specifically, on average, the additional installed photovoltaic capacity resulting from the subsidy programs is estimated to be 1.20 to 1.41 times larger than the photovoltaic capacity that could have been installed directly by the municipalities at the same total cost.

Moreover, I find a robust positive, and significant partial linear effect of both the subsidy value and the subsidy program budget on the estimated number of additional plug-PV systems. This result holds true for both the LASSO estimator and the DDML approach, across a varying set of machine learning implementations in the DDML algorithm, and for all three methods used to estimate the number of additional plug-PV systems. Using a tractable static framework, I demonstrate that the positive estimated relationship between the subsidy value and the number of additional plug-PV systems can arise from two possibilities only in theory: Either the subsidy program budget is *not* fully used, meaning no over-subscription should be observed; *or* both the subsidy program budget is fully used *and* the demand function is locally "sufficiently convex". An example of a demand function that fulfills the condition of being "sufficiently convex" on the whole domain is a constant elasticity demand function with a price elasticity between zero and one. Surprisingly, the theory suggests that for a fixed budget and under a "sufficiently convex" demand function, increasing the upfront subsidy value can lead to an increase in the additional investment in plug-PV systems caused by a subsidy program for a large range of subsidy values when the subsidy program budget is already oversubscribed. In other words, even when already more citizens request the subsidy than the municipality can finance with the limited program budget given some upfront subsidy payment, increasing the subsidy value further can still increase additional investments caused by the subsidy program. Intuitively, this occurs because when the municipality announces the subsidy and distributes it via a lottery among all interested citizens, two types of individuals apply: those who would have installed the system even without the subsidy, called "Always Buyers", and those who buy only if they receive the subsidy, called

"Conditional Buyers". Hence, the number of additional plug-PV systems caused by a subsidy program is equal to the number of Conditional Buyers who indeed receive the subsidy. Coming back to the intuition, suppose the program is already oversubscribed and the municipality increases the subsidy value further. Then, the group of Conditional Buyers increases, given that the demand is increasing when the subsidized price is falling due to an increase in the subsidy. Then, there are two effects at play: *First*, as the subsidy value increases, the fixed budget allows for fewer subsidies to be distributed, which should decrease the number of additional plug-PV systems because fewer Conditional Buyers receive the subsidy. *Second*, the proportion of Conditional Buyers relative to Always Buyers increases. This is, because the mass of Always Buyers remains constant, regardless of the subsidy amount, while the group of Conditional Buyers increases with the increase in the subsidy value, as argued above. Thus, a fair lottery will grant the subsidy more often to Conditional Buyers than to Always Buyers, thereby increasing the number of additional plug-PV systems. If the demand function is sufficiently convex, meaning it increases strongly with an decrease in price for smaller prices, small increases in the subsidy value lead to large increases in the mass of Conditional Buyers. Then, with a small increase in the subsidy value, the number of subsidies to be distributed decreases only slightly, while the number of Conditional Buyers relative to Always Buyers increases strongly. This means the second effect outweighs the first, causing the number of additional plug-PV systems to increase as the subsidy value increases, even when the subsidy program is already over-subscribed. In any case, regardless of whether the budget is not fully used, or fully used and the demand function for plug-PV systems within German municipalities is sufficiently convex, the empirical findings—if correctly identified—imply that, on average, municipalities could have increased the number of additional plug-PV systems caused by the subsidy program by increasing their subsidy value and using a lottery to determine who receives the subsidy without changing the overall subsidy program budget.

This study contributes to the literature studying the "additionality problem", a concept introduced by Joskow and Matron (1992). The additionality problem describes the problem of disentangling the number of purchases or the investment amount made *because* of a subsidy from those that would have occurred also in its absence. Several studies have addressed this problem in the context of photovoltaic systems. For instance, Kattenberg et al. (2024) use a temporary randomized household-level lottery in the Netherlands to assess the impact of subsidy eligibility on uptake of photovoltaic systems. They find that households receiving the subsidy were more likely to adopt these systems. Other papers have leveraged regional and temporal variations in subsidy availability for identification. Hughes and Podolefsky (2015) use the variation among three regions in California for the identification, Crago and Chernyakhovskiy (2017) consider state-level variation in the US, and Bollinger and Gillingham (2012) use variation at the border between two areas in California with different subsidy schemes. Beyond photovoltaic systems, a growing body of literature explores the additionality of subsidies for other durable goods, particularly electric vehicles. Studies have used regional variations across countries (Haan, Santonja di Fonzo and Zaklan, 2024), US states (Clinton and Steinberg, 2019), two regions in Canada (Fournel, 2023) and communities in California (Muehlegger and Rapson, 2022). Additional approaches have considered variation in the subsidy levels or subsidy eligibility for similar products (Chen, Hu and Knittel, 2021; Houde and Aldy, 2017) or between eligible and ineligible households at the margin (Boomhower and Davis, 2014). In contrast to these studies, this study examines regional variation in subsidy programs at a much more granular level by comparing the development of 233 municipalities that implemented a program with 510 that did not. This granularity facilitates the credible construction of counterfactual developments of the outcome variable. Furthermore, unlike previous evaluations, this study assesses a large number of different subsidy programs for the same homogeneous product, with significant heterogeneity among these programs. For instance, the upfront subsidy value in the evaluated subsidy program ranges from EUR 38 to EUR 569. This allows for an analysis of the effects of subsidy program characteristics on the additionality of the subsidies.

Moreover, this study contributes to the literature on optimal subsidy designs aimed at encouraging private investments. De Groote and Verboven (2019) find large implicit discount rates for future subsidy payments of Belgian households. Their findings imply that upfront subsidies can cut government spending substantially by allowing smaller overall subsidy payments. Burr (2016), by studying a photovoltaic subsidy program in California, confirms this finding by demonstrating that upfront subsidies generate larger photovoltaic uptake than production-based subsidies. Extending the subsidy optimization problem to households that can consume the electricity they produce with their photovoltaic investments, Feger, Pavanini and Radulescu (2022) find that upfront subsidies result in smaller welfare losses than other subsidy schemes. In a more general context of adoption decisions, Langer and Lemoine (2022) and Kiso (2022) find that in a dynamic setting, upfront subsidies should not remain constant over time. In this study, I contribute to this literature by considering the design of an optimal upfront subsidy policy when the policymaker's temporary budget for the subsidy is limited, which has significant practical relevance. To the best of my knowledge, this is the first theoretical attempt to design an optimal upfront subsidy scheme for durable goods under a limited budget for the policymaker. By accounting for the limited budget, I demonstrate that the optimal upfront subsidy rate crucially depends on the degree of convexity of the demand function and, consequently, on the distribution of reservation prices.

The rest of this chapter is structured as follows. In Section 3.2, I discuss some background information on plug-PV systems and German municipalities. In Section 3.3, I discuss the construction of the dataset and provide some descriptive statistics. Section 3.4 introduces the identification and estimation strategy. Empirical results are provided in Section 3.5, while Section 3.6 discuss the results by using the static theoretical framework. Finally, Section 3.7 concludes.

3.2 Background

In this section, I first explain the technical features of plug-PV systems and their legal status in Germany. Second, I describe the tasks, responsibilities and scope of action of German municipalities and counties.

3.2.1 Plug-PV Systems

Plug-PV systems are bundles of (one or more) photovoltaic panel(s) equipped with a converter. They function similarly to conventional photovoltaic systems but possess distinct features. First, their installation and registration is significantly simpler compared to conventional photovoltaic systems. They are referred to as *plug*-PV systems because they can be installed by placing the panels in a suitable location—such as a balcony—and plugging the converter's cable directly into a power socket.[6] The German Association for Electrical, Electronic and Information Technologies (VDE)[7] is responsible for setting binding legal standards

[6] See: `https://www.enbw.com/blog/energiewende/solarenergie/balkonkraftwerk-mini-solaranlagen-fuer-die-steckdose/`, last accessed April 14, 2025.
[7] VDE stands for "Verband der Elektrotechnik Elektronik Informationstechnik", see `https://www.vde.com/de/ueber-uns` (last accessed April 14, 2025).

for electronic installations in Germany.[8] In one of its publications, the VDE states that even non-professionals can safely install plug-PV systems (VDE, 2025). Moreover, in Germany, all electricity-generating facilities are legally obliged to register with the national digital register called "Markstammdatenregister" (MaStR). For plug-PV systems, however this registration is also—in comparison to conventional photovoltaic systems—relatively straightforward and, throughout the study period (i.e., until March 2024), required only a simplified registration with the MaStR and the respective local grid operator (BMKW, 2024). Second, plug-PV systems are considerably smaller than conventional photovoltaic systems. The size of a photovoltaic system is typically measured by its system capacity, which refers to the maximum power output under optimal conditions and is measured in Watt-peak (Wp).[9] For example, a system with a capacity of 1000 Wp would produce 1000 watts of electricity under standard test conditions.[10] Under German law during the study period, the maximum capacity for plug-PV systems was limited to 600 Wp (BMKW, 2024). By comparison, as of September 30, 2024, the average capacity of all photovoltaic systems in Germany was 19,427 Wp, while the median was 7,600 Wp.[11] Third, unlike conventional systems, plug-PV users in Germany do not receive any feed-in tariff for surplus electricity fed into the grid.[12]

Despite their lower system capacity and the absence of feed-in tariffs, plug-PV systems generally achieve cost recovery within a few years. Appendix 3.A.4 presents amortization time estimates for a standard plug-PV system (600 Wp) across 16 usage and installation scenarios from 2019 to 2024. These calculations use the median plug-PV system prices and average residential electricity prices for each year. In 2023 and 2024, amortization times ranged from five to eight years. In contrast, in 2019, amortization ranged from seven to thirteen years. On average, across scenarios, the amortization period decreased by 3.5 years between

[8] The VDE hosts the DKE, the organization responsible for developing these technical norms; see `https://de.wikipedia.org/wiki/Deutsche_Kommission_Elektrotechnik_Elektronik_Informationstechnik`, last accessed April 14, 2025.
[9] I refer to system capacity as the lesser of the Watt-peak rating of all panels in the system—which is the maximum power output that the photovoltaic panels can achieve in total under optimal conditions—and the power rating of the converter (measured in Watts) connecting the panels with the electricity user.
[10] While not legally defined, "standard test conditions" usually refer to an irradiance of 1000 Watts per square meter and a panel temperature of 25 degrees Celsius.
[11] Author's own calculations based on data described in Section 3.3.4.
[12] See: `https://www.netze-bw.de/dsc/faq?id=11abb37a-d5ae-4d0a-bb37-401ad2815ba0`, last accessed April 14, 2025.

2019 and 2024. This improvement results from a simultaneous rise in electricity prices and a fall in plug-PV system prices—especially pronounced in 2023.

This increase in financial viability has coincided with a rapid rise in plug-PV installations. Although plug-PV systems became legal in 2018,[13] adoption remained limited through the end of 2021. As shown in Appendix 3.A.1, around 100,000 plug-PV systems had been installed by the end of 2022 in Germany. However, beginning in 2023, the number of installations surged, reaching nearly 600,000 by July 2024.[14]

Lastly, it remains to discuss the characteristics of plug-PV systems that are indeed installed in Germany. As explained above, during the period analyzed in this study, the maximum allowed system capacity for plug-PV systems was 600 Wp. In fact, as of September 30, 2024, 85.1% of the installed plug-PV systems in Germany have a system capacity of exactly 600 Wp. Moreover, 82.4% of the installed plug-PV systems have two panels.[15] Therefore, in the following, I will consider a plug-PV system with two panels (each of which has a capacity of usually 300 Wp) and total system capacity of 600 Wp as the "standard" system in Germany.

3.2.2 German Municipalities and Counties

In Germany, there are nearly 11,000 municipalities ("Gemeinden").[16] Municipalities constitute the smallest administrative unit in the German federal system. The next higher tier consists of counties ("Landkreise"), although some larger cities are classified as independent of counties ("kreisfreie Städte"). These independent cities are excluded from the analysis in this study, because the basic dataset used for this study described in Section 3.3.1 does not contain these independent cities.

[13] See: https://www.enbw.com/blog/energiewende/solarenergie/balkonkraftwerk-mini-solaranlagen-fuer-die-steckdose/ (last accessed April 11, 2025) and https://www.dgs.de/projekte/pvlotse/steckersolar-geraete/ (last accessed April 11, 2025).
[14] Note that there were some further simplifications for the installation of plug-PV systems as well as an increase in the maximum allowed system capacity after March 2024 (see BMKW (2024)), which are not of interest for this study, as they became effective after March 2024, which marks the end of the period analyzed in this study (see Section 3.3.3).
[15] Author's own calculations based on data described in Section 3.3.4.
[16] The number of municipalities varies slightly over time due to occasional municipal mergers. As of December 31, 2021, Germany had 10,944 municipalities (see: https://www.bbsr.bund.de/BBSR/DE/forschung/raumbeobachtung/Raumabgrenzungen/deutschland/gemeinden/gemeinden-gemeindeverbaende/gemeinden.html, last accessed: April 15, 2025).

Both municipalities and counties have defined mandatory responsibilities but also enjoy a degree of fiscal and administrative autonomy. Within their respective competencies and given they have sufficient financial resources, they may independently allocate public funds as decided by their elected councils. Both counties and municipalities have their county councils and municipal councils, respectively, in which elected counselors decide on all matters of the respective unit. Both municipalities as well as counties manage their own budgets and financial planning. This institutional autonomy has allowed both counties and municipalities in Germany to establish their own subsidy programs for plug-PV systems. In all subsidy programs analyzed in this study, the programs limit eligibility to residents of the respective administrative unit. Section 3.3.3 provides a more detailed discussion of the structure and content of these programs.

Besides the autonomy of municipalities in managing their own budgets, it should be noted that this autonomy is limited in practice. In fact, in the past years, municipalities had to cut voluntary programs in order to finance legally mandated ones.[17] Municipal associations warn that budgets are often not even sufficient for mandatory duties.[18] This strain is evident in the numbers: The budgets of all municipalities (excluding the city-states Hamburg, Berlin and Bremen) ran a record financing gap of EUR 24.8 billion in 2024.[19] Moreover, in Germany, the autonomy of municipalities to decide on their own budget is limited by the approval of the respective municipal supervisory authority which can restrict the overall volume of the budget when municipal spending exceeds municipal revenues (Brüning and Söbbeke, 2024, p. 163). Therefore, municipalities can use some funds for subsidy programs, but these funds are limited and thereby required the policymakers in municipalities to design subsidy programs given a tight budget.

3.3 Data and Descriptives

The dataset used for the analysis combines several sources: a hand-collected dataset on subsidy programs in German municipalities, a panel dataset on characteristics of German municipalities from Bertelsmann Stiftung (2024), a

[17] `https://www.staedtetag.de/positionen/beschluesse/2025/241-hauptausschuss-kommunale-finanzkrise` (last access: 24.06.2025)
[18] `https://www.dstgb.de/publikationen/pressemitteilungen/kurswechsel-einleiten-starke-kommunen-moeglich-machen/` (last access: 24.06.2025)
[19] `https://www.destatis.de/DE/Presse/Pressemitteilungen/2025/04/PD25_126_71137.html` (last access: 24.06.2025)

panel dataset covering the universe of installed plug-PV systems in German municipalities and a panel of web-scraped pricing data for plug-PV systems in Germany. The unit of observation in the compiled dataset is a municipality observed in one month. While the outcome data is available at a daily level, all specifications are estimated at the monthly level. This choice reflects the fact that the treatment variable—the existence of a subsidy program—changes at the monthly level in nearly all cases, making a more granular resolution not beneficial in comparison to a monthly analysis.

3.3.1 Bertelsmann Data

The municipality dataset provided by Bertelsmann Stiftung (2024) forms the basis for the analysis. It is a panel dataset covering all German municipalities with at least 5,000 inhabitants over the period from 2006 to 2021. The dataset accounts for approximately 90% of the German population.[20] The Bertelsmann dataset does not cover cities independent from counties, so called "kreisfreie Städte". The Bertelsmann dataset contains detailed information on tax revenues, municipal debt, population, demographics, and additional indicators. In total, the dataset used in this study comprises 2,967 municipalities.[21]

3.3.2 Prices for Plug-PV Systems

To gather information on the mean and median prices of plug-PV systems in Germany over time, I draw on two main sources: the German Section of the International Solar Energy Society (referred to as DGS[22] in the following) and the company "PV Magazine" (referred to as PVM in the following). DGS maintains a website[23] that lists all plug-PV systems currently available on the German market, including details on system capacity and price.[24] A similar market overview, independent from the overview by DGS, is provided by PVM via its

[20] See: `https://www.wegweiser-kommune.de/methodik`, last accessed April 7, 2025.
[21] The original dataset contains 3053 municipalities (see `https://www.bertelsmann-stiftung.de/de/ueber-uns/was-wir-erreicht-haben/wegweiser-kommune`, last access April 7, 2025) from which I only keep those that have non-missing information on the core variables including tax revenues, debt and sustainability indicators.
[22] The German name of this society is "Deutsche Gesellschaft für Sonnenenergie" (DGS)
[23] Available at: `https://www.pvplug.de/marktuebersicht/`
[24] As of late 2024, price information is no longer displayed on their webiste.

own website.[25] Historical data from both DGS and PVM market overviews is not directly accessible. However, I use the Internet Archive[26], which periodically stores snapshots of websites. For the DGS market overview, the Internet Archive contains 46 snapshots taken at irregular intervals between July 2018 and March 2024. Similarly, 64 snapshots are available for the PVM market overview over the same period. I scrape all available snapshots for both market overviews and estimate the mean and median price of listed plug-PV systems at each available time point. Using these mean and median prices for the available time points, I apply local interpolation to estimate monthly mean and median prices for plug-PV systems on the German market for each month between July 2018 and March 2024 (see Appendix 3.A.2 for details).

In Appendix 3.A.2, the estimated mean and median prices of a standard plug-PV system are presented over time.[27] The estimated median price steadily declined from approximately EUR 950 in July 2018 to around EUR 750 in early 2023, after which it remained relatively stable. Similarly, the mean price fell from around EUR 1,100 in July 2018 to just below EUR 900 by early 2023 and has also remained roughly constant since then.

3.3.3 Subsidy Programs in Municipalities

For all municipalities in the Bertelsmann dataset, I used web scraping to obtain the email address of the main contact point for each municipal administration. Specifically, I scraped contact information from the website of the organization "Frag den Staat", which hosts an online database containing administrative contacts details for nearly all public authorities in Germany.[28] In cases where contact information was missing or outdated, I manually retrieved the relevant details from the official website of the respective municipality. In a similar manner, I used web scraping to collect the general contact email address for all county administrations in Germany. First, I scraped the list of German counties and

[25] Available at: `https://www.pv-magazine.de/marktuebersichten/produktdatenbank-stecker-solar-geraete/`
[26] Accessible at: `https://web.archive.org/`
[27] A "standard" plug-PV system is defined here as a system with a capacity of 600 Wp (Section 3.2.1).
[28] The online database from "Frag den Staat" can be accessed through `https://fragdenstaat.de/behoerden/`. I scraped all information in January 2024.

their corresponding official web addresses from Wikipedia.[29] Second, I scraped all 294 county websites to obtain the respective general contact email addresses.

After obtaining the email addresses, I sent an email to all 2967 municipalities in the Bertelsmann dataset and asked them whether they currently have and/or whether they ever had any subsidy program for plug-PV systems.[30] I received 1352 responses within a few weeks, which corresponds to a response rate of 45.6%. Moreover, I sent an individualized email to all 294 German counties, also asking them whether they currently have and/or whether they ever had any subsidy program for plug-PV systems.[31] I received 208 responses, which corresponds to a response rate of 70.7%. All emails both to municipalities and counties were sent in the first week of March 2024. Hence, the period of analysis is restricted to end in March 2024, as there is no information on whether municipalities that reported having no subsidy program at that time introduced one thereafter. As it was explained in Section 3.2.1, since 2018, it is allowed to install plug-PV systems in Germany. Hence, to capture potential previous "illegal" installations in pre-trends in the analysis, I chose the start of the analysis to be in January 2017. Hence, the period of analysis is January 2017 until March 2024.

Based on all collected email responses, I construct the sample of municipalities used in this study. The sample includes all municipalities that responded via email and that (i) are located in counties that also sent an answer via email whether they have and/or had their own subsidy program and answered that they have and/or had no own county subsidy program during the period of analysis (i.e., before March 2024), and (ii) are situated in federal states that also did not implement their own subsidy programs during the period of analysis (i.e., before March 2024).[32] Municipalities located in counties or states with their

[29] More specifically, I first scraped the list of counties from `https://de.wikipedia.org/wiki/Liste_der_Landkreise_in_Deutschland` (last Access: April 7, 2025) and obtained the Wikipedia page of each county. Second, I scraped the Wikipedia page of each county, respectively, to obtain the web address of each county.

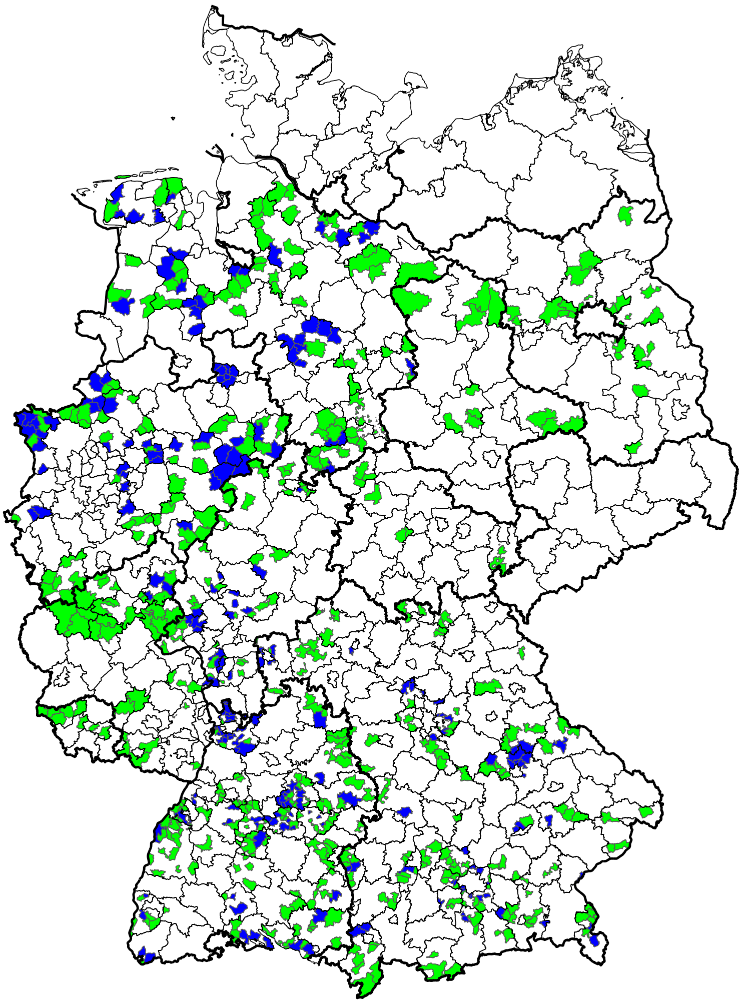
[30] The message sent is provided in Appendix 3.F.1 in German language (as it was sent to municipalities) as well as in a translated English version.

[31] The message sent is provided in Appendix 3.F.2 in German language (as it was sent to counties) as well as in a translated English version.

[32] I exclude municipalities from all states that maintained their own subsidy programs throughout the period of analysis. These include Mecklenburg–Western Pomerania (see `https://www.lfi-mv.de/foerderfinder/mini-solaranlagen/`, last accessed April 7, 2025), Saxony (see `https://www.sab.sachsen.de/balkonkraftwerke-stecker-pv-anlagen`, last accessed April 7, 2025), and Schleswig-Holstein (see `https://zufish.schleswig-holstein.de/detail?areaId=&pstId=283094855&ouId=`, last accessed April 7, 2025). In addition, I exclude the three city-states – Berlin, Hamburg, and Bremen – due to their distinct administrative structures, which are not directly comparable to those of other German states.

own funding programs are excluded to avoid complicating the analysis and the interpretation of relative subsidy effects. After applying these restrictions, the final sample comprises 733 municipalities, of which 223 implemented their own subsidy program for plug-PV systems. Figure 3.1 provides a visual overview of the municipalities included in the final sample.

Figure 3.1: Municipalities Included in Sample



Note: Municipalities included in the sample are color-coded based on the presence of a local subsidy program: Municipalities with their own subsidy program are shown in blue, while those without are shown in green. Municipalities not included in the sample are displayed in white, with white municipality borders to de-emphasize them visually. County borders are marked in black, and state borders are shown in bold black lines. The map is constructed using the map from GeoBasis-DE (2025) and coloring the respective areas and borders.

As a next step, I hand-collected detailed information on the subsidy programs implemented by the 223 municipalities that offered their own subsidy programs for plug-PV systems. This information was gathered from four different sources. First, some municipalities provided information directly via email—either in response to my initial inquiry (as described above) or following clarification requests I sent via individual follow-up emails in case I had concrete questions to the respective subsidy program that I could not answer with the other available sources described in the following.[33] Second, the primary source of information for most municipalities was the respective municipality's "council information system" ("Ratsinformationssystem"). These systems host official documents related to the topics discussed in the municipal councils, including meeting minutes, background documents on the discussed issues and council decisions.[34] For nearly all municipalities with a subsidy program, the council information system contained both the minutes of the session in which the subsidy program was approved and the official funding guidelines governing the respective subsidy program. Third, I gathered additional details from the official websites of the municipalities and, when necessary, from archived versions of those websites available through the Internet Archive.[35] Fourth, in a few cases, I supplemented the information gathered through the first three sources with information from local newspaper articles that reported on the respective subsidy programs. Importantly, for all 223 municipalities, I was able to obtain a document titled funding guidelines (or a similarly named document), which outlines the legal basis, conditions, and operational details of the respective subsidy program.[36]

In fact, the subsidy programs differ widely in the way they grant subsidies for the plug-PV systems. The main difference lies in the type of payment schemes that are used to subsidize the plug-PV systems. Even though all subsidy programs make an upfront subsidy payment, they make use different types of payment schemes, including lump-sum subsidies, relative subsidies covering a percentage of system costs, subsidies paid per solar module, and subsidies calculated per installed Watt-peak (Wp). To make the programs comparable across these different schemes, I compute the "subsidy value". This subsidy value is defined as the amount in euros that one receives by installing a *standard* plug-PV system under the respective subsidy program. I define a standard plug-PV system as the most installed system in Germany during the period of analysis, which is a system with two panels and a capacity of 600 Wp (Section 3.2.1). The method of calculation for the subsidy value depends on the subsidy type. For lump-sum

[33] Access to the full email correspondence with all municipalities is available upon request.
[34] In a few cases, these documents were hosted directly on the municipality's website rather than in a dedicated council information system.
[35] In some cases, information about the subsidy programs was not available on the current municipal website. In these instances, I used the Internet Archive, accessible at `https://web.archive.org/`, to retrieve earlier versions—if they were available—of the relevant webpages.
[36] All funding guidelines, along with the full set of supporting documents and sources for each of the 223 municipalities, are available upon request.

programs, the subsidy value is equal to the fixed amount granted. For relative subsidies, where municipalities cover a share $\rho \in (0, 1)$ of the total system cost, I multiply ρ by the monthly median plug-PV system price, which was described in Section 3.3.2. I then average this monthly value over the active period of the program to obtain the program's average subsidy value. For per-module subsidies, I multiply the per module subsidy value by two, as a standard 600 Wp plug-PV solar system consists of two panels (Section 3.2.1). For subsidies granted per 100 Wp, I multiply the subsidy per 100 Watt-peak by six to obtain the subsidy value for a standard 600 Wp plug-PV solar system.

Table 3.1: Details about Subsidy Programs

Characteristics	N	Mean	Sd.	Min	Median	Max
Subsidy value:						
Calculated absolute value of subsidy	270	175.51	84.54	37.92	189.62	568.88
Subsidy according to funding guidelines:						
Lump-sum subsidy	161	156.43	68.42	50	150	400
Relative subsidy (percentage of total cost)	66	0.31	0.17	0.05	0.3	0.9
Per module subsidy	32	95.7	27.79	37.5	100	150
Per Watt-peak subsidy (per 100 Wp)	12	41.72	12.73	25	42	66.66
Further Characteristics:						
Subsidy can only be received by a certain group (0/1)	270	0.07	0.25	0	0	1
Subsidy value differs by income	270	0.05	0.21	0	0	1
Budget is combined with other funding program (0/1)	254	0.3	0.46	0	0	1
Subsidy program budget	142	24604.08	28119.10	400	18625	150000

Note: N reports the number of programs for which the respective information is available. The "Calculated absolute value of subsidy" refers to the subsidy value as explained in the main text. The "Subsidy according to funding guidelines" are the direct subsidy information provided in the guidelines. The "(0/1)" indicates that the characteristic can either take the value 0, if it does not have that characteristic, or 1, if it has that characteristic.

In total, the 223 municipalities that had a subsidy program at any point up to March 2024 implemented 270 individual subsidy programs. This number exceeds the number of municipalities because some offered more than one program.[37] Table 3.1 provides descriptive statistics for the 270 subsidy programs. The programs vary considerably in design. Approximately 30% of the programs funded additional items beyond plug-PV systems. Furthermore, on average, the programs provided EUR 175.51 per standard plug-PV system, though the amount varied widely—from EUR 37.92 to EUR 568.88. Similarly, the subsidy program budget ranged from EUR 400 to EUR 150,000, with an average of EUR 24,604.08. As illustrated in Appendix 3.A.3, 30 of the 270 subsidy programs

[37] I define a subsidy program as a continuous period during which a municipality funded plug-PV systems under unchanged funding conditions. Consequently, multiple programs within the same municipality occur either when there is a break between two funding phases, or when the funding conditions are modified during an ongoing phase or when both happens at the same time.

started before January 1, 2022, while 93 started before January 1, 2023. The rest of the programs in the period of analysis started in 2023 or until March 1, 2024.

3.3.4 Plug-PV Systems

For the outcome of interest—the number of installed plug-PV systems in each municipality included in the sample—I use data from the Marktstammdatenregister (MaStR), the official digital registry operated by the German Federal Network Agency (Bundesnetzagentur). Since 2014, all electricity-generating facilities connected to the electricity grid in Germany have been legally required to register in the MaStR, including all privately owned plug-PV systems.[38] The MaStR allows to download a full dataset of all registered photovoltaic systems, including those that were registered but are not active any more.[39] The dataset includes, for each (both active and inactive) photovoltaic system in Germany, the installation date, the postal code, the official municipality identification code, the capacity of the system and additional characteristics, which are not relevant for the analysis.[40] Using this raw data from MaStR, I generate a panel dataset containing, for each municipality in the sample, the cumulative number of installed plug-PV systems in each month in the period of analysis.[41] One might worry that the cumulative number of installed plug-PV systems does not reflect the number of active plug-PV systems in each municipality. However, as of September 30, 2024 only 0.32% of all plug-PV systems in the MaStR that were registered since 2017 are marked as "temporarily out of order", whereas none of these systems is marked to be permanently out of order.

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[38] See: `https://www.marktstammdatenregister.de/MaStRHilfe/subpages/GrundlagenZielsetzung.html`, last accessed April 11, 2025.

[39] I downloaded the data on September 30, 2024 at 3pm from: `https://www.marktstammdatenregister.de/MaStR/Datendownload`

[40] The municipality identification code refers to the "Amtliche Gemeindeschlüssel", which is a eight-digit code official statistical code that identifies a municipality in Germany.

[41] To identify plug-PV systems in the registry data, I filter the photovoltaic systems by their system capacity. More specifically, as explained in Section 3.2.1, plug-PV systems until April 2024 are characterized by a maximum system capacity of 600 Wp. Thus, all photovoltaic systems in the registry with a system capacity of smaller or equal to 600 Wp are considered as plug-PV systems. In theory, one could also install "conventional" non-plug-PV systems with a capacity of less or equal to 600 Wp, which my approach would wrongly consider to be plug-PV systems. However, as the illustration in Appendix 3.A.1 shows, there have not been a measurable number of any photovoltaic system with a system capacity of smaller or equal to 600 Wp before 2018 in Germany, i.e., before plug-PV systems existed. Thus, I consider it to be very unlikely that the approach used to identify plug-PV systems in the MaStR data counts a sizable number of conventional systems, which would cause a measurement error in the outcome variable.

3.3.5 Solar Potential

In some specifications, I control for the potential annual return of photovoltaic panels across municipalities. This potential return is measured by the variable *global radiation*, which refers to the total energy emitted by the sun that reaches the earth's surface in a given area over the course of a year. Global radiation is measured in kilowatt-hours per square meter (kWh/m^2) and varies with atmospheric conditions and the geographical location of each municipality. To calculate the average global radiation for each municipality, I use data from Germany's National Meteorological Service (Deutscher Wetterdienst, 2025), which reports annual average global radiation for the period from 1991 until 2020.[42] The resulting variable captures, for each municipality, the average potential annual output of a PV system in kWh/m^2.

3.3.6 Municipality Characteristics

It remains to discuss the characteristics of the sample of municipalities for this study prior to the first subsidy program implementation in 2017. Table 3.2 presents the average characteristics in 2017 for the municipalities that had at least one subsidy program for plug-PV systems during the period of analysis ("Treatment") and for the municipalities that never had a subsidy program for plug-PV systems during the period of analysis ("Control"), as well as the difference in the average characteristics between these two groups of municipalities. Municipalities implementing a subsidy program had a significantly higher population, tax income per capita, population density and population increase in comparison to the control municipalities in 2017. Moreover, the treated municipalities also have a significantly lower average age compared to the control municipalities. However, the other variables, including municipal debt level per capita, the number of photovoltaic systems in operation and the potential yearly return of photovoltaic systems is not significantly different between the two groups of municipalities. Note that these differences are not threatening the identifying assumption for the identification strategy discussed in the next Section.

[42] The data from Deutscher Wetterdienst (2025) is provided on a raster grid with a resolution of one kilometer by one kilometer. To compute the average global radiation for each municipality, I overlay this grid with the municipal boundaries provided by GeoBasis-DE (2025). The average value is then calculated based on all raster cells intersecting each municipality's area.

Table 3.2: Descriptive Municipality Characteristics for Sample in 2017

Variable	Control	Treatment	Difference
Population	15277.28	20950.68	5673.40***
	(26107.31)	(16528.33)	(1630.51)
Change in population, 2012-2017 (percent)	2.62	3.50	0.88**
	(3.52)	(4.86)	(0.37)
Average age	44.85	44.20	-0.64***
	(1.98)	(1.48)	(0.13)
Population density (population per hectare)	3.05	5.67	2.62***
	(3.23)	(4.56)	(0.34)
Income tax per capita (EUR)	501.12	562.22	61.10***
	(139.54)	(136.19)	(11.14)
Corporate income tax per capita (EUR)	384.17	598.09	213.93***
	(261.99)	(1050.24)	(71.44)
Tax per capita (EUR)	1072.09	1372.32	300.23***
	(447.89)	(1412.85)	(96.98)
Municipal debt per capita (EUR)	822.91	778.60	-44.30
	(880.54)	(941.80)	(74.89)
Photovoltaic systems in operation	408.84	443.48	34.64
	(282.43)	(311.38)	(24.49)
Average yearly global radiation (kWh/m^2)	1109.40	1107.47	-1.92
	(62.07)	(62.83)	(5.07)
Observations	482	223	705

Note: The Table shows the average characteristics for municipalities that had at least one subsidy program for plug-PV systems during the period of analysis ("Treatment") and for the municipalities that never had a subsidy program for plug-PV systems during the period of analysis ("Control"). The Difference column refers to the average differences between these two groups. "Photovoltaic systems in operation" refer to conventional PV systems installed within a municipality as of January 1st, 2017. Standard errors of each respective estimate are presented in brackets. Significance levels are indicated by * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

3.4 Estimation

In the following, I first describe the identification strategy for estimating the municipal-level causal effects of municipal subsidy programs. These municipal-level causal effects refer to the number of additional plug-PV systems installed in a certain municipality due to the subsidy program of this specific municipality. Second, after quantifying the effect of each subsidy program and each month after the start of each subsidy program on the number of additional plug-PV systems separately, I outline the estimation strategy for the relation between subsidy program and municipality characteristics and the number of additional plug-PV systems.

3.4.1 Identifying Number of Additional Plug-PV Systems

Consider the market for plug-PV subsidy systems in municipality i at time t. Municipalities are observed in periods $t = 1, \cdots, T$, where period T is March 2024. The set of observations in the analyzed sample is given by $it \in \Omega$. The stock of installed plug-PV systems at time t in municipality i is denoted as Q_{it}.

Municipality i implements a subsidy program after month t_i by effectively reducing the price of plug-PV systems by an upfront subsidy payment within municipality i. The upfront subsidy payment can only be given to a limited number of individuals, because there is a fixed limited budget for the subsidy program. The municipalities either used a lottery or a first-come-first-served approach to allocate the subsidies. Given the implementation of the subsidy program, t_i denotes the last month in which there has not been any subsidy program so far in municipality i. In practice, I chose t_i to be the last month in which the municipality did not have some plans to implement a subsidy program to account for potential anticipation effects. For municipalities that do not implement a subsidy program during the study period, it follows that $t_i = \infty$.

The counterfactual number of installed plug-PV systems is denoted as Q^*_{it} for $t > t_i$. Thus, Q^*_{it} for $t > t_i$ equals the stock of plug-PV systems that would have been installed in municipality i at time period $t > t_i$ if no subsidy program would have been started in period t_i. Given this counterfactual stock of plug-PV systems, the number of additional plug-PV systems caused by the subsidy program in municipality i at time $t > t_i$ is given by $\tau_{it} = Q_{it} - Q^*_{it}$.

I define the treatment indicator variable D_{it} to be equal to one if $t > t_i$ and to be equal to zero otherwise. Thus, D_{it} is equal to one in all periods after the first subsidy program was implemented in municipality i and is always zero for municipalities that never implemented any subsidy program. Hence, the treatment is considered to be an absorbing state, which allows the usage of recent theoretical panel identification methods (Abadie, 2021; Borusyak, Jaravel and Spiess, 2024; Arkhangelsky et al., 2021). In practice, the subsidy programs end after the budgets are fully used or after some pre-defined ending date, which means that the subsidy programs themselves cannot be considered to be an absorbing state. However, it makes sense to consider the time periods after the first subsidy program has been implemented in a specific municipality as "treated", because I consider the stock of additionally installed plug-PV systems as the outcome, where each installation is an effectively irreversible outcome in the period of analysis. Once installed, any potentially positive stock of additional plug-PV systems is not reduced back to zero in the medium run after the subsidy program ends.[43] Considering a subsidy program to be an absorbing treatment captures the effect of the subsidy program on the permanent stock of plug-PV systems even after the program ends, which is precisely what I aim to estimate. Moreover, the estimation aims at estimating τ_{it} for each it separately and not any average across municipalities or time periods. For the estimation methods considered in the following that aim at estimating the set of $\{\tau_{it}\}_{(i,t)\in\Omega:D_{it}=1}$, it does not make any difference for the estimation of a single τ_{it} whether the program already ended or not, because the counterfactual Q^*_{it} in all methods is constructed using data from municipalities that have not been treated until period t.

As Imbens and Rubin (2015, p. 15) point out, estimation of causal effects requires to "predict, or impute, the missing potential outcome". Hence, the identification of the causal effects of the subsidy programs, i.e., of τ_{it}, requires to predict Q^*_{it}. For this prediction, I employ three methods, which are described in the following.[44]

Parametric Approach: The method developed by Borusyak, Jaravel and Spiess (2024) considers the estimation of treatment effects in event-study designs, when treatment timing is varying across treated units and treatment effects are heterogeneous across time.[45] The method is therefore well suited for the estimation of τ_{it} with heterogeneity in t_i. The basic idea of the method developed by Borusyak, Jaravel and Spiess (2024) is to estimate a parametric model for Q_{it}

[43] This medium-run time period refers to 3-4 years after the installation at maximum, which is the maximum time horizon considered in the study, and which is much lower than the average life-expectancy of a plug-PV solar system, which is more than 20 years (Frauenhofer ISE, 2025).

[44] Besides these three methods, there are also other methods for event-study settings, which are, however, not well suited for the case of the studied subsidy programs: First, conventional event-study estimators (often referred to as two-way fixed-effects estimators) implicitly embed the assumption of homogeneous treatment effects and can lead to estimates of the average treatment effects involving negative weights on the long-run treatment effects (Borusyak, Jaravel and Spiess, 2024). Moreover, other recent methods acknowledging the problem of conventional event-study designs and developing event-study methods robust to heterogeneous treatment effects and treatment timing, i.e., the methods developed by Sun and Abraham (2021), Callaway and Sant'Anna (2021) and De Chaisemartin and d'Haultfoeuille (2020), are also not suited for the case of the studied subsidy programs because these methods directly target the estimation of cohort-specific average treatment effects. A cohort defined by these authors is a group of units which have their start of treatment in the same month, i.e. a group of potentially completely different subsidy programs. Hence, these methods do not allow to estimate individual treatment effects or any average treatment effect useful for the scope of this study.

[45] The key principle of the method is the estimation of individual treatment effects for each treated unit and each period after the treatment took place. Using this set of estimated treatment effects, Borusyak, Jaravel and Spiess (2024) then define estimation targets, which are weighted averages of the estimated treatment effects.

only with observations for which $D_{it} = 0$. Then, this estimated parametric model is used to predict counterfactual outcomes for Q_{it} for observations for which $D_{it} = 1$. Formally, following the definitions by Borusyak, Jaravel and Spiess (2024), denote $\Omega_0 = \{it \in \Omega : D_{it} = 0\}$ and $\Omega_1 = \{it \in \Omega : D_{it} = 1\}$. Moreover, define $Q'_{it} = Q_{it}/\text{Pop}_i$, where Pop_i denotes the population in thousands in 2017, i.e., before the study period starts. The parametric method by Borusyak, Jaravel and Spiess (2024) is implemented in three steps: First, the following specification is estimated only with observations belonging to the set Ω_0:

$$Q'_{it} = \alpha_i + \beta_t + \gamma_i t + \varepsilon_{it}, \quad \forall it \in \Omega_0 \tag{3.1}$$

Where α_i denotes municipality fixed effects, β_t denotes time fixed effects, γ_i accounts for municipality-specific linear time trends and ε_{it} denotes the random error term. The parametric model in this first step is estimated for Q'_{it} as a dependent variable and not for Q_{it}, because the total stock of plug-PV systems not adjusted for population is not directly comparable between different municipalities, which would give β_t an unreasonable meaning. Second, the estimated coefficients from specification (3.1) are used to calculate, for all $it \in \Omega_1$, $\hat{Q}'_{it} = \hat{\alpha}_i + \hat{\beta}_t + \hat{\gamma}_i t$. Third, the final estimates are computed by setting $\hat{Q}^{\text{Parametric}}_{it} = \text{Pop}_i * \hat{Q}'_{it}$, where $\hat{Q}^{\text{Parametric}}_{it}$ denotes the parametric estimator for Q^*_{it}.

Synthetic Control (SC): The second method to predict the counterfactual outcome is the Synthetic Control (SC) method, going back to Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010, 2015). The SC method was initially developed for the case of a single treated unit and multiple potential control units. However, as Abadie (2021) points out, there are "no additional conceptual challenges" when the method is applied in the case of multiple treated units. Moreover, Abadie (2021) states that "[t]reatment effects can be estimated for each treated unit separately and aggregated in a second step if desired".

The idea of the SC method is to select—separately for each treated municipality—a suitable group of control municipalities such that their weighted average matches the pre-treatment trend of the outcome variable for the respective treated municipality as closely as possible. Thus, the SC method implicitly controls for all factors which determine the development of plug-PV systems before the subsidy programs start. Formally, define $I_0 = \{i : \max_{0<t<T} D_{it} = 0\}$ and $I_1 = \{i : \max_{0<t<T} D_{it} = 1\}$. Moreover, define $N_1 = |I_1|$ and $N_0 = |I_0|$. For each pair of municipalities $j \in I_1$ and $i \in I_0$, define a weight ω^{SC}_{ji}. These weights are col-

lected in vectors $\omega_j^{SC} = \left(\omega_{j1}^{SC}, \cdots \omega_{jN_0}^{SC}\right)$ for all $j \in I_1$. Defining $w = (w_1, \cdots, w_{N_0})$ and following Arkhangelsky et al. (2021), the weight vectors ω_j^{SC} are estimated through the following minimization problem:

$$\hat{\omega}_j^{SC} = \underset{w \in W}{\arg\min} \sum_{t=1}^{t_j} \left(Q_{jt} - \sum_{i \in I_0} w_i Q_{it} \right)^2 + \zeta^2 t_j \|w\|_2^2$$

Where ζ denotes a regularization parameter.[46] Moreover, W denotes the set of weights such that all weights are weakly positive and sum up to one.[47] The regularization is conducted to increase the dispersion of the weights and to achieve uniqueness of weights in practice (Arkhangelsky et al., 2021). Given a vector of estimated weights ω_j^{SC} for each treated unit j, the SC estimator of Q_{jt}^*, denoted as $\hat{Q}_{jt}^{\text{SC}}$ is given by:

$$\hat{Q}_{jt}^{\text{SC}} = \sum_{i \in I_0} \hat{\omega}_{ji}^{SC} Q_{it}, \qquad \forall t > t_j$$

Synthetic Difference-in-Differences (SDID): The third method to predict the counterfactual outcome is the Synthetic Difference-in-Differences (SDID) method going back to Arkhangelsky et al. (2021). In principle, the SDID method is a combination of the first two methods, i.e., of the parametric approach following Borusyak, Jaravel and Spiess (2024) and of the SC approach. More specifically, the idea of the SDID method is to model the conditional expectation of the number of plug-PV systems for the observations in Ω_0 with a two-way fixed effects model—like in the parametric approach—and to weight the observations such that there is a balance between treated and non-treated municipalities—like in the SC approach—which allows to weaken the parallel trends like assumption in the fully parametric model. To obtain treatment effect estimates for each

[46] I follow the proposition of Arkhangelsky et al. (2021) for the regularization parameter. The parameter is defined by $\zeta = (10^6 N_0(t_j - 1))^{-1} \sum_{i \in I_0} \sum_{t=0}^{t_j} (\Delta_{it} - \bar{\Delta})^2$, where $\Delta_{it} = Q_{i(t+1)} - Q_{it}$ and $\bar{\Delta} = (N_0^{-1} t_j)^{-1} \sum_{i \in I_0} \sum_{t=0}^{t_j} \Delta_{it}$. Thus, the regularization parameter is larger, i.e., the shrinkage of weights towards zero is stronger, when there is a larger variance in changes in Q_{it} pre-treatment across never-treated municipalities. Intuitively, it is reasonable that the approach leads to smaller weights and thus to a smaller dependency on single municipalities, when the number of plug-PV systems changes a lot within municipalities before treatment, because the dependency on a few municipalities seems to be worse when there is a lot of variance across these municipalities. On the contrary, if there would be no variance across municipalities at all, a dependency on a single municipality as a synthetic control would not be a problem, meaning that the shrinkage would be allowed to be less strong.

[47] Formally, $W = \left\{ w : w_i \geq 0, \sum_{i=1}^{N_0} w_i = 1 \right\}$

municipality separately, I follow the approach chosen in the SC approach and apply the SDID method separately for each municipality $j \in I_1$. Formally, denote $I_j = (I_0 \cup (j \in I_1))$, for $j = 1, \cdots, N_1$ as the set of municipalities that includes all municipalities that never implement a subsidy program and the j'th municipality that implements a subsidy program during the study period. Note that $|I_j| = N_0 + 1$. Without loss of generality, I order the units in I_j such that the first N_0 units are the never-treated municipalities while the j'th unit is the municipality j that implemented a subsidy program during the study period. To estimate the treatment effect for municipality j, I use the following specification:

$$\left(\hat{\tau}_j^{sdid}, \hat{\alpha}_j, \hat{\beta}_j\right) = \underset{\tau_j, \alpha_j, \beta_j}{\arg\min} \left\{ \sum_{i \in I_j} \sum_{t=1}^{T} \left(Q_{it} - \alpha_{ij} - \beta_{tj} - D_{it}\tau_j\right)^2 \hat{\omega}_i^j \hat{\lambda}_t^j \right\} \tag{3.2}$$

Where $\alpha_j = (\alpha_{1j}, \cdots, \alpha_{N_0+1,j})$ denotes the vector of unit fixed effects, $\beta_j = (\beta_{1j}, \cdots, \beta_{Tj})$ denotes the vector of time fixed effects and τ_j denotes the average treatment effect across periods after t_j for municipality j. There are unit-weights $\hat{\omega}_i^j$ and time-weights $\hat{\lambda}_t^j$ which are determined *before* solving the minimization problem (3.2). Both unit- and time-weights are chosen following the procedure of Arkhangelsky et al. (2021). Intuitively, the unit weights are chosen such that in every time period before the subsidy program starts, the weighted average of plug-PV systems in never-treated municipalities roughly equals the number of plug-PV systems in municipality j, i.e., such that for all $t = 1, \cdots, t_j$ it holds that $\sum_{i \in I_0} \hat{\omega}_i^j Q_{it} \approx Q_{jt}$. More specifically, defining the vector $\hat{\omega}^j = (\hat{\omega}_1^j, \cdots, \hat{\omega}_{N_0+1}^j)$, the unit weights are determined by solving the following optimization problem:

$$(\hat{\omega}_0^j, \hat{\omega}^j) = \underset{\omega_0^j \in \mathbb{R}, \omega^j \in W}{\arg\min} \sum_{t=1}^{t_j} \left(Q_{jt} - \omega_0^j - \sum_{i=1}^{N_0} \omega_i^j Q_{it} \right)^2 + \zeta^2 t_j \|w\|_2^2 \tag{3.3}$$

where ζ denotes the regularization parameter[48] and W denotes the set of ω_i^j such that all ω_i^j are weakly positive, $\omega_{N_0+1}^j$ is equal to one and all remaining weights sum up to one.[49] The regularization is conducted to increase the dispersion of the weights and to achieve uniqueness of weights in practice (Arkhangelsky et al., 2021). Note that setting ω_0^j equal to zero would lead to the same unit weights as chosen in the SC case. Hence, while the unit weights in the SC

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[48] I follow Arkhangelsky et al. (2021) and set the regularization parameter as $\zeta = (T - t_j)^{1/4} (N_0(t_j - 1))^{-1} \sum_{i=1}^{N_0} \sum_{t=1}^{t_j - 1} \left(\Delta_{it} - \bar{\Delta}\right)^2$, where $\Delta_{it} = Q_{i(t+1)} - Q_{it}$ and $\bar{\Delta} = (N_0(t_j - 1))^{-1} \sum_{i=1}^{N_0} \sum_{t=1}^{t_j - 1} \Delta_{it}$.

[49] Formally $W = \left\{\omega^j \in \mathbb{R}_+^{N_0+1} : \sum_{i=1}^{N_0} \omega_i^j = 1, w_{N_0+1}^j = 1\right\}$, where $\mathbb{R}_+$ denotes the positive real line.

method where chosen such that the weighted average of plug-PV systems in the never-treated units matches the pre-treatment plug-PV systems in the treated unit j, in the SDID, the unit weights are chosen such that the number of plug-PV systems in municipality j is parallel to the weighted average of the never-treated municipalities until period t_j and is therefore allowed to differ by a constant. The two-way fixed effects structure with individual intercepts in (3.2) accounts for this level difference in the estimation. It remains to discuss the determination of the time-weights. Intuitively, the time weights are chosen such that for each never-treated municipality, the weighted average of the number of plug-PV systems before t_j across time periods differs by a constant from the average number of plug-PV systems after t_j. More specifically, defining the vector $\hat{\lambda}^j = \left(\hat{\lambda}_1^j, \cdots, \hat{\lambda}_T^j\right)$, the time weights are determined by solving the following optimization problem:

$$\left(\hat{\lambda}_0^j, \hat{\lambda}^j\right) = \underset{\lambda_0^j \in \mathbb{R}, \lambda^j \in L}{\arg\min} \sum_{i=1}^{N_0} \left(\lambda_0 + \sum_{t=1}^{t_j} \lambda_t Q_{it} - \frac{1}{T - t_j} \sum_{t=t_j+1}^{T} Q_{it}\right)^2 \tag{3.4}$$

where L denotes the set of $\hat{\lambda}^j$ such that all $\hat{\lambda}_t^j$ are weakly positive, $\hat{\lambda}_1^j, \cdots, \hat{\lambda}_{t_j}^j$ sum up to one and all remaining $\hat{\lambda}_t^j$ are equal to a constant (i.e., to $(T - t_j)^{-1}$).[50] Finally, the SDID estimator for Q_{it}^*, denoted as $\hat{Q}_{it}^{\text{SDID}}$, is defined by using $\hat{\alpha}_{(N_0+1)j}$ and $\hat{\beta}_j$ determined in the minimization problem (3.2) and computing $\hat{Q}_{it}^{\text{SDID}} = \hat{\alpha}_{(N_0+1)j} + \hat{\beta}_{tj}$.

Note that all three methods rely on different forms of identifying assumptions. Moreover, because none of these identifying assumptions can directly be tested, it is unclear which of the three methods is superior for the estimation of the number of additional plug-PV systems. However, in the subsequent Sections, I will present the estimates of the SC method as the default method and present the results for the other two methods in the Appendix for two reasons: First, in contrast to the SC approach, the ultimate goal of the parametric approach as reported by Borusyak, Jaravel and Spiess (2024) is *not* the estimation of unit-level treatment effects but of weighted averages of these. The identifying assumption in the parametric approach is set to identify an average treatment effect and not unit-level treatment effects, which is not necessarily problematic, but which can cast doubt on the identification of individual-level estimates. Second, as Arkhangelsky et al. (2021) point out, the time weighting of units in the SDID method can

[50] Formally, $L = \left\{\lambda \in \mathbb{R}_+^T : \sum_{t=1}^{t_j} \lambda_t = 1, \lambda_t = (T - t_j)^{-1} \forall t = t_j + 1, \cdots, T\right\}$, where $\mathbb{R}_+$ denotes the positive real line.

increase estimation bias in comparison to SC methods. In fact, the underlying outcome Q_{it} is on a constantly increasing trend. Hence, in the estimations, it turns out that the time weighting in the SDID approach puts the entire weight on the last time period before the subsidy program starts. Intuitively, this happens because the last period before t_i is most similar to the time periods after the treatment starts, when Q_{it} is constantly increasing. This time-weighting in the SDID approach could indeed increase the bias in the estimates in comparison to the SC approach, as the SDID estimates rely on only one time period as a reference point.

3.4.2 Effect of Program Characteristics

Following the estimation of the counterfactual outcome Q^*_{it} for all $it \in \Omega_1$ using any of the three discussed estimators (SC, SDID or Parametric), the estimated number of additional plug-PV systems is given by $\hat{\tau}^E_{it} = Q_{it} - \hat{Q}^E_{it}$, where $E \in \{\text{SC}, \text{SDID}, \text{Parametric}\}$. Remember that any effect of program characteristics on the number of additional plug-PV systems in the following is solely estimated on observations in the set Ω_1.

Potential characteristics of interest that might determine the number of additional plug-PV systems by a subsidy program are: First, the subsidy value of municipality i at time t, i.e. the amount in Euro that municipality i grants some of its citizens when buying a plug-PV system in month t, which is denoted as S_{it}. Second, the overall budget in Euro that municipality i provides for the subsidy payments throughout the whole period of analysis is denoted as B_i. Third, the set of further program characteristics besides S_{it} and B_i of the specific subsidy program in municipality i are summarized in the vector $\mathbf{X}_i^P$ (Appendix 3.E). Fourth, the set of municipality characteristics of municipality i are summarized in the vector $\mathbf{X}_i^C$ (Appendix 3.E). To simplify notation, define the vector $\mathbf{X}_{it} = \left(S_{it}, B_i, \mathbf{X}_i^P, \mathbf{X}_i^C\right)$, which has K elements and where the k'th element is denoted as X_{itk}.

To identify which subsidy program characteristics and municipality characteristics determine the number of additional plug-PV systems caused by these programs, I employ two different methods discussed in the following:

Least Absolute Shrinkage and Selection Operator (LASSO): As an exploratory approach, I conduct a LASSO estimation to identify the program and municipality characteristics which determine relevant variation in the additional number of plug-PV systems caused by the subsidy programs. The LASSO approach does not require any specific prior about which of the characteristics drives the impact of the subsidy programs. To the best of my knowledge, there is no prior research on subsidy programs for plug-PV systems in German municipalities, which makes any formation of priors particularly difficult and thereby the LASSO method useful. Following Belloni, Chernozhukov and Hansen (2014), the LASSO estimator for the determinants of the number of additional plug-PV systems estimated with estimator E is given by:

$$\hat{\beta}^E = \arg\min_{\beta} \sum_{it \in \Omega_1} \left(\hat{\tau}_{it}^E - \eta_{\tilde{t}(t)} - \psi_{W(i)} - \gamma_{A(i)} - \phi_{L(i)} - \sum_{k=1}^{K} b_k X_{itk} \right)^2 + \lambda \sum_{k=1}^{K} |b_k|$$

where $\eta_{\tilde{t}(t)}$ accounts for month after treatment start fixed effects, i.e. $\tilde{t}(t) = t - t_i$; $\psi_{W(i)}$ accounts for treatment period fixed effects, where two municipalities have the same treatment period when both their first subsidy program started in the same quarter and their last subsidy program ended in the same quarter; $\gamma_{A(i)}$ accounts for the type of subsidy fixed effects, where the type of subsidy might either be a lump-sum, relative, per-module, per Watt-peak, or a mixed subsidy approach; and $\phi_{L(i)}$ accounts for state-fixed effects (Bundesländer); and where $\beta = \left(b, \eta_{\tilde{t}(t)}, \alpha_{W(i)}, \gamma_{A(i)}, \phi_{L(i)}\right)$. Prior to estimation, I standardize all characteristics and fixed effects dummy variables such that all have a zero mean and a variance equal to one. Following, Wüthrich and Zhu (2023), the shrinkage parameter λ is chosen using cross-validation.[51] Note that the shrinkage is only applied to the program and municipality characteristics but not to the fixed effects.

Double Debiased Machine Learning (DDML): Second, I implement the DDML approach following Chernozhukov et al. (2018).[52] In contrast to the LASSO approach, the DDML approach is less exploratory in the sense of having some prior on the relevance of specific characteristics to be determinants of $\hat{\tau}_{it}^E$. Specifically, the prior is built by the theoretical framework (Appendix 3.C), indicating that both S_{it} and B_i should affect the number of additional plug-PV systems. Intuitively, the DDML approach aims at identifying the partial linear effect of both S_{it} and B_i on $\hat{\tau}_{it}^E$, while being exploratory on both the relevant

[51] I use five-fold cross-validation and compute the lambda that minimizes the root-mean-squared error of the prediction. Following this, I select $\lambda_{1\text{se}}$, because the lambda that minimizes the root-mean-squared-error would lead to a "substantial over-selection" of control variables Wüthrich and Zhu (2023).

[52] I implement this approach in R using the package provided by Bach et al. (2024).

control characteristics and the way these control characteristics enter the equation in order to identify the unbiased effect of both S_{it} and B_i on $\hat{\tau}_{it}^E$. Following Chernozhukov et al. (2018), I consider the following partially linear regression model:

$$\begin{aligned}\hat{\tau}_{it}^E &= \alpha_1 S_{it} + \alpha_2 B_i + g_0\left(\mathbf{Z}_{it}\right) + \zeta_{it}\\ S_{it} &= m_0^S\left(\mathbf{Z}_{it}\right) + \nu_{it}^S\\ B_i &= m_0^B\left(\mathbf{Z}_{it}\right) + \nu_i^B\end{aligned}$$

where $\mathbf{Z}_{it} = \left(\mathbf{X}_i^P, \mathbf{X}_i^C, \eta_{\tilde{t}(t)}, \alpha_{W(i)}, \gamma_{A(i)}, \phi_{L(i)}\right)$, and where ζ_{it}, ν_{it}^S and ν_{it}^B are random error terms. The DDML algorithm works by randomly splitting the sample of observations, estimating the function g_0 with half of the sample and the functions m_0^S and m_0^B with the other half of the sample, using machine learning estimators, respectively. I implement the algorithm with five alternative estimators: LASSO, Neural Network, Random Forest, Support Vector Machine and Gradient Boosting.[53] The estimated functions $\hat{g}_0$, $\hat{m}_0^S$ and $\hat{m}_0^B$ are then used to compute the orthogonalized components of both S_{it} and B_i, i.e. $\hat{\nu}_{it}^S = S_{it} - \hat{m}_0^S\left(\mathbf{Z}_{it}\right)$ and $\hat{\nu}_{it}^B = B_i - \hat{m}_0^B\left(\mathbf{Z}_{it}\right)$, and similarly the orthogonalized component of $\hat{\tau}_{it}^E$, which is given by $\hat{\tau}_{it}^E - \hat{g}_0\left(\mathbf{Z}_{it}\right)$. Then, the parameters α_1 and α_2 are estimated in a ordinary least squares regression using the orthogonalized components. Finally, the sample split is inverted, generating a second estimate for both α_1 and α_2. The final estimates are then the averages of the estimated parameters α_1 and α_2. As Chernozhukov et al. (2018) show, this estimator for both α_1 and α_2 “concentrate in an $N^{-1/2}$-neighborhood of the true parameter values and are approximately unbiased and normally distributed”. Intuitively, the DDML algorithm accounts for potential omitted variable biases by implicitly “controlling” for the union of (1) characteristics that predict the outcome variable ($\hat{\tau}_{it}^E$) and (2 characteristics that predict the explanatory variables of interest (S_{it} and B_i).

[53] The respective methods are implemented using the R-packages provided by: Lang et al. (2019) for LASSO (where the month-after-treatment dummies are not penalized); Wright and Ziegler (2017) for random forest (with default settings); Venables and Ripley (2002) for the Neural Network (with one hidden layer and 5 units); Meyer et al. (2024) for Support Vector Machines (with default settings); and Chen and Guestrin (2016) for Gradient Boosting (with default settings).

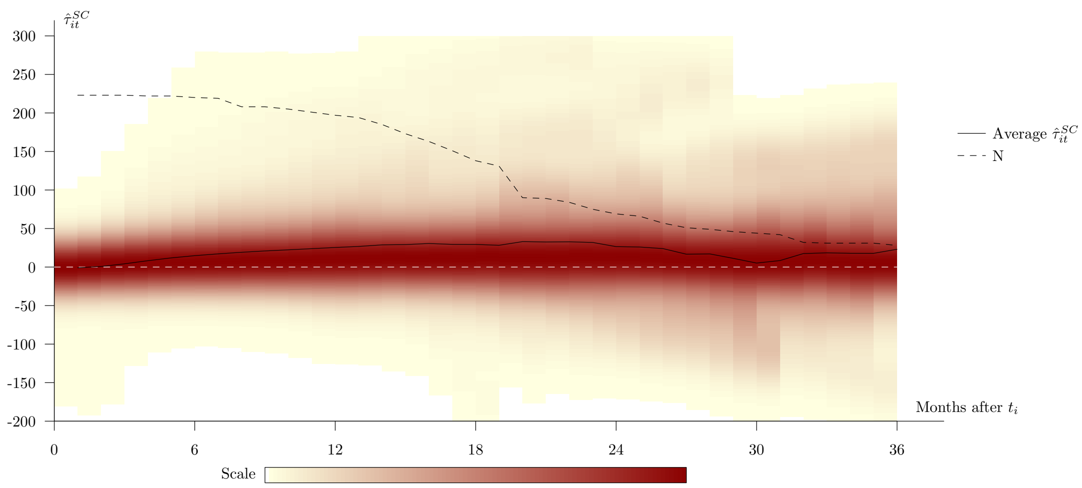
3.5 Results

In the following, I will first provide an overview on the estimated number of additional plug-PV systems caused by the subsidy programs. In the second Subsection, I will discuss the exploratory results on the determinants of the numbers of additional plug-PV systems. In the last Subsection, I will discuss the estimates aiming at identifying the effect of the subsidy value and the subsidy program budget on the number of additional plug-PV systems.

3.5.1 Number of Additional Plug-PV Systems

In Figure 3.2, the distribution of the estimated number of additional plug-PV systems using the SC method, i.e., of $\hat{\tau}_{it}^{\mathrm{SC}}$ are illustrated. Both the average $\hat{\tau}_{it}^{\mathrm{SC}}$, as well as the bulk of the distribution of estimated $\hat{\tau}_{it}^{\mathrm{SC}}$ is positive throughout the period of analysis, indicating that the majority of subsidy programs are found to generate additional plug-PV systems that would have not been installed in the absence of these subsidy programs. This is especially pronounced in the first 12 to 18 months after the start of the subsidy program. The distribution of $\hat{\tau}_{it}^{\mathrm{SC}}$ becomes more dispersed after 18 months, which might be caused by the substantial drop in the number of municipalities for which $\hat{\tau}_{it}^{\mathrm{SC}}$ can be estimated for more than 18 months.

Figure 3.2: Additional Plug-PV Systems Estimated (SC)



Note: Heat-map illustrating the distribution of $\hat{\tau}_{it}^{\mathrm{SC}}$ for each month t after t_i. The color intensity on the provided Scale illustrates the empirical distribution of estimated effects. The dashed line shows the number of observations (i.e., municipalities) for which an estimate τ_{it} is available for each month after treatment start. The solid line shows the average estimated $\hat{\tau}_{it}^{\mathrm{SC}}$.

In Appendix 3.B.1, the distributions of the estimated number of additional plug-PV systems are illustrated for both the Parametric estimation method as well as the SDID method. The Figures provided in Appendix 3.B.1 show very similar qualitative and quantitative patterns as for the SC method. Moreover, to further evaluate the similarity in the estimates between the different estimators, I compute, for each municipality i with a subsidy program, the correlation between the vectors $\left(\tau^{SC}_{it_i+1}, \cdots, \tau^{SC}_{iT}\right)$ and $\left(\tau^{E'}_{it_i+1}, \cdots, \tau^{E'}_{iT}\right)$ for $E' \in \{\text{Parametric}, \text{SDID}\}$, respectively. For 81.2% (67.9%) of municipalities, the correlation between the estimated effects between the SC method and the SDID (Parametric) method is above 0.8, indicating a substantial correlation between the results produced by different estimators. In Appendix 3.B.2, the full distribution of correlations is illustrated.

In addition to the estimated additional number of plug-PV systems, I calculate whether the additional photovoltaic capacity installed due to the subsidy programs exceeds the capacity of a large photovoltaic system that the municipality could have built instead of implementing the subsidy program at the same total cost. Naturally, this analysis has to rely on a set of assumptions and should therefore be considered with caution. Twenty months after program start, the number of municipalities for which I can estimate the number of additional plug-PV systems caused by the subsidy program, decreases sharply (Figure 3.2). Thus, it seems reasonable to base the effectiveness estimate on the average number of plug-PV systems caused by the subsidy programs twenty months after program start. The average estimated number of additional plug-PV systems twenty months after program start is given by 32.99. As a first simplifying assumption, assume that these additional plug-PV systems persist for the life-time of a plug-PV system. In fact, as Figure 3.1 shows, 36 months after program start, the average number of additional plug-PV systems is not substantially lower. Moreover, for five subsidy programs, the number of additional plug-PV systems can be estimated for 49 months after program start. For these five programs, the average number of additional plug-PV systems 49 months after program start is equal to 33.43, which provides some justification to this simplifying assumption.[54] As a next step, I assume that the additionally installed plug-PV systems are standard systems (as defined in Section 3.2.1) of 600 Wp. This assumption seems reasonable, considering that 82.4% of the plug-PV systems in Germany have a capacity of exactly 600 Wp (Section 3.2.1). Thus, considering the average budget across all subsidy programs, the average additional kWp

[54] The estimates after 36 months are not displayed in Figure 3.2.

installed due to the subsidy program per EUR 1000 subsidy program budget is equal to 0.704.[55] As an alternative to implement a subsidy program for plug-PV systems, the municipalities could have decided to use the budget to simply install a photovoltaic system itself. Full prices for photovoltaic systems depend on both the capacity of the system and on regional factors, influencing installation costs. As of March 2024, the average price for a photovoltaic system between 6 kWp and 10 kWp, including installation costs, ranged from EUR 1,700 to EUR 2,000 per kWp.[56] Thus, per EUR 1,000, the municipality could have installed 0.5 to 0.59 kWp of photovoltaic capacity in March 2024. Considering that prices of photovoltaic systems are in general on a decreasing long-term trend (Frauenhofer ISE, 2025), it seems reasonable to assume that during the whole period of analysis the installation prices were the same as of March 2024, which is probably even an overestimation of the actual price of photovoltaic capacity for municipalities and thereby implicitly biases the effectiveness estimates of subsidy programs downwards. To benchmark the subsidy program with a simple installation of a photovoltaic system, two further assumptions are necessary. First, I assume that there are zero overhead costs for the subsidy program. Second, to obtain a fair comparison between the two alternatives, I also assume zero overhead cost for the planing and installation of the alternative photovoltaic system. Given these assumptions, note that the comparison can be conducted by relating the kWp that are caused to be installed per EUR 1,000 by the subsidy program (0.704) with the kWp that can be installed per EUR 1,000 with the installation of a conventional photovoltaic system (0.5-0.59). Hence, the additional photovoltaic capacity installed by the subsidy program is 1.20-1.41 as large as the photovoltaic capacity that could be installed directly by the municipality at the same total cost, suggesting the subsidy program to be preferable over a direct installation of a photovoltaic system from a welfare perspective.

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[55] For the average subsidy program, there are 32.99 additional plug-PV systems. Assuming that each of these systems has a capacity of 600Wp, the overall additional capacity installed due by an average subsidy program is equal to 19.794 kWp. For an average subsidy program budget of 28119.1, this means per EUR 1000 of subsidy program budget, there is an additional capacity of 0.7039 kWp installed.

[56] See `https://web.archive.org/web/20240524235020/https://www.pv-magazine.de/themen/photovoltaik-preisindizes/indikative-systempreise-fuer-photovoltaik-anlagen-auf-einfamilienhaeusern/` (last access: 24.07.2025).

3.5.2 Exploratory Results

The variation in the estimated number of additional plug-PV systems resulting from the subsidy programs, as illustrated in the previous subsection, motivates an investigation into the determinants of this dispersion. Table 3.3 presents the results of the LASSO estimator, which serves as an exploratory approach to identify the most relevant factors influencing this variation. Conceptually, the estimates presented in Table 3.3 across the different columns differ with respect to the set of fixed effects included in the estimation equation (Section 3.4.2). A blank cell in Table 3.3 indicates that the LASSO estimator shrunk this specific variable in this specification to zero, meaning it was not "selected" to be a relevant determinant of the number of additional plug-PV systems. Note that in contrast to an ordinary least squares regression, the coefficients estimated by the LASSO estimator do not have a clear cardinal interpretation, considering the shrinkage of coefficients towards zero. However, positive (negative) coefficients indicate a partial positive (negative) association of an explanatory variable and the number of additional plug-PV systems.

Table 3.3 indicates a positive association between both the subsidy value and the number of additional plug-PV systems as well as between the budget and the number of additional plug-PV systems throughout all specifications. For subsidy programs in which the group of potential subsidy receivers are restricted, i.e., by giving the subsidy only to people below a certain income threshold (receivers restricted), there is, on average, a larger number of additional plug-PV systems. To understand the intuition, recall that those individuals with a reservation price above the market price are denoted as "Always Buyers", while those with a reservation price below the market price but above the market price less the subsidy payment are denoted as "Conditional Buyers". Intuitively, the positive association between a restriction of subsidy receivers and the number of additional plug-PV systems caused by the subsidy programs makes sense, when high-income individuals are excluded from the subsidy and when these high-income individuals are more likely belonging to the group of Always Buyers rather than Conditional Buyers, which seems plausible. For municipalities with a larger tax revenue per capita (tax per capita), the number of additional plug-PV systems is found to be larger throughout all specifications. Similarly, this might be caused by a larger group of Always Buyers relative to the group of Conditional Buyers, when higher tax per capita correlates with the income of individuals within a municipality. Larger municipalities (population) are also found to have larger

Table 3.3: LASSO Estimator Results for SC Estimates

	(1)	(2)	(3)	(4)	(5)
Subsidy value	0.0479	0.0300	0.0325	0.0113	0.0111
Budget	0.0003	0.0003	0.0005	0.0006	0.0006
Program Duration	-0.0030	-0.0286			
Combined budget			-9.7882	-7.9889	
Council vote share		-2.4246			
MaStR obligation		0.0488			
Receivers restricted	0.1957		16.8806	19.0142	17.3461
Income differences					
Tax per capita	-0.0165	-0.0163	-0.0154	-0.0281	-0.0224
Income tax per capita					
Population density					
Population	0.0004	0.0004	0.0002	0.0004	0.0003
Average age			0.7954		
Solar potential	-0.0012		0.0250	0.0263	
N	1922	1922	1922	1922	1922
λ	3.891	0.883	0.247	0.199	0.321
R^2	0.173	0.305	0.520	0.549	0.569
FE Post-treatment month		✓	✓	✓	✓
FE Program period			✓	✓	✓
FE Subsidy type				✓	✓
FE State					✓

Note: Results for the LASSO estimator (Section 3.4.2) for varying set of fixed effects (FE) where the outcome analyzed is τ_{it}^{SC} and where all variables were standardized prior to the estimation. Blank cells indicate that the LASSO estimator shrinks the coefficient of the respective variable to zero, meaning these variables were not selected by the estimator. For variable definitions, refer to Appendix 3.E. The parameter λ is chosen using five-fold cross-validation as described in the main text.

number of additional plug-PV systems. Note that this is association is not driven by a mechanical effect resulting from a larger group of potential buyers in larger municipalities, as the SC method already implicitly controls for the population in the municipality. Thus, the positive association between the size of a municipality and the additional number of plug-PV systems indicates that in larger municipalities, more Conditional Buyers rather than Always Buyers receive the subsidy than in smaller municipalities. For all other characteristics shown in Table 3.3, the LASSO estimates do not show consistent relations across the different specifications.

In Appendix 3.B.3, the LASSO estimates for the determinants of the number of additional plug-PV systems, estimated with the SDID and the Parametric approach, are shown. The SDID estimates confirm the positive associations between both the subsidy value as well as the budget and the number of additional plug-PV systems throughout all specifications. For the Parametric approach, no negative associations between both the subsidy value and the budget and the number of additional plug-PV systems are found, while there are positive relations once including all fixed effects. Thus, the positive association between the subsidy value and the estimated number of additional plug-PV systems is also found when using the additionality estimates stemming from the SDID and the parametric method.

3.5.3 Effects of Subsidy Value and Budget

To get more insights into the associations of both the subsidy value as well as the budget and the number of additional plug-PV systems, consider the results of the DDML algorithm presented in Table 3.4. The results presented in Table 3.4 refer to the estimates stemming from the SC method. Regardless of the method used in the implementation of the DDML algorithm, the effects of both the subsidy value as well as the budget on the number of additional plug-PV systems are significantly positive. The estimates suggest that for an increase of EUR 100 in the subsidy rate, the number of additional plug-PV systems caused by the subsidy programs increase by 12 to 19 (depending on the method applied in the DDML approach). As a comparison, note that the average number of additional plug-PV systems across all subsidy programs and all months after treatment start estimated with the SC method is equal to 26.11, while the standard deviation in the subsidy value across programs is equal to EUR 68. Increasing the budget by EUR 10,000 increases the number of additional plug-PV systems, on average, by 2 to 5 (depending on the method applied in the DDML approach).

In Appendix 3.B.4, the DDML estimation results for the estimates stemming from both the SDID method as well as the Parametric method are provided. The results confirm the findings from the analysis using the estimates stemming from the SC method provided in Table 3.4. In all specifications and for both types of estimates (SDID and Parametric), the budget has a positive significant effect on the number of additional plug-PV systems. The effects of the subsidy value on the the number of plug-PV systems are also all positive and almost all are significantly different from zero, thereby underlining the robustness of the discussed findings.

Table 3.4: DDML Results for SC Estimates

	Lasso b/se	Neural Network b/se	Random Forest b/se	Support Vector Machine b/se	Gradient Boosting b/se
Subsidy value	0.1903**	0.1694**	0.1471**	0.1525**	0.1219*
	(0.0785)	(0.0697)	(0.0744)	(0.0698)	(0.0672)
Budget	0.0005**	0.0005**	0.0005***	0.0005***	0.0002*
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
N	1922	1922	1922	1922	1922

Note: Estimation results for the DDML algorithm (Section 3.4.2) for $\hat{\tau}_{it}^{SC}$. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. All approaches are implemented following the descriptions in Section 3.4.2. The columns differ with respect to the method used to estimate g_0, m_0^S and m_0^B.

3.6 Discussion

To simplify the discussion on the empirical results, I develop a simple theoretical static framework (Appendix 3.C). The setup of the theoretical framework aims at closely mimicking the market for plug-PV systems and the municipal subsidy programs in Germany. In the framework's setup, I consider the demand function for plug-PV systems within each municipality to be strictly decreasing; the supply of photovoltaic systems to be fully elastic at a world market price; and a municipality that operates under a given budget and grants an upfront subsidy to its citizens when buying a plug-PV system. More specifically, I assume that the budget for the subsidy program is exogenously given to the policy maker in the municipality who then decides on the upfront subsidy with the aim to maximize additional investments in plug-PV systems. The additional investments are defined as the investments caused by the subsidy program which would have not been conducted in the absence of the program. The municipality first announces the upfront subsidy which is smaller than the market price for plug-PV systems. In a second step, all citizens who are willing to buy a plug-PV system at the subsidized price register with the municipality. Third, if the registered subsidy requests exceed the municipality's budget for the subsidies, the municipality uses a fair lottery to determine who will receive the subsidy payment.

To derive implications of the positive partial effects of the subsidy value and the subsidy program budget on the number of additional plug-PV systems, it is necessary to assume that these effects are correctly identified and estimated.

In other words, to enable any discussion of the positive relations, I assume that: First, the estimates for the number of additional plug-PV systems are correctly identified and are thus unbiased. Second, I also assume that there are no missing control variables or incorrect functional forms used, when estimating the effects of the subsidy value and the subsidy budget on the number of additional plug-PV systems caused by the subsidy programs. This second assumption implies that the subsidy value and the subsidy budget are both conditionally exogenous given the included control variables.[57] Given these assumptions, there are two implications following from the empirical results:

First, the empirical findings imply that there is sufficient variation in the subsidy values under various different flexible controls to identify significant positive effects of the subsidy value given a fixed budget on the number of additional plug-PV systems. This implies that comparable municipalities do *not* set the same subsidy value given a fixed budget. Hence, municipalities do, on average, not set a theoretically *optimal* subsidy value given a fixed budget, where an optimal subsidy value refers to a subsidy value that maximizes the number of additional plug-PV systems given a fixed budget. If they were to set optimal subsidy values, I would not be able to use any identifying variation in the subsidy value for comparable municipalities. Note again that this does of course only follow under the assumption that all control variables included in the estimations already account for all relevant differences across municipalities and subsidy programs. Then, the theoretical framework suggests that two municipalities that face the same demand function and have also otherwise similar characteristics should set exactly the same subsidy value if they have the same subsidy program budget and if they are interested in maximizing the additional investments in plug-PV systems. However, because there is variation in the subsidy values for comparable municipalities and programs, municipalities do not set optimal subsidy values.

Second, given that municipalities did not choose an optimal subsidy value to maximize the additional investments, the empirical results together with the theoretical framework imply that if municipalities would have increased their subsidies—even without changing the subsidy program budget—they could have increased the additional investments in plug-PV systems. In principle, this conclusion could already be seen without any theoretical consideration by the empirical results directly: Higher subsidy rates—conditional on the subsidy program budget—imply higher additional numbers of plug-PV systems caused

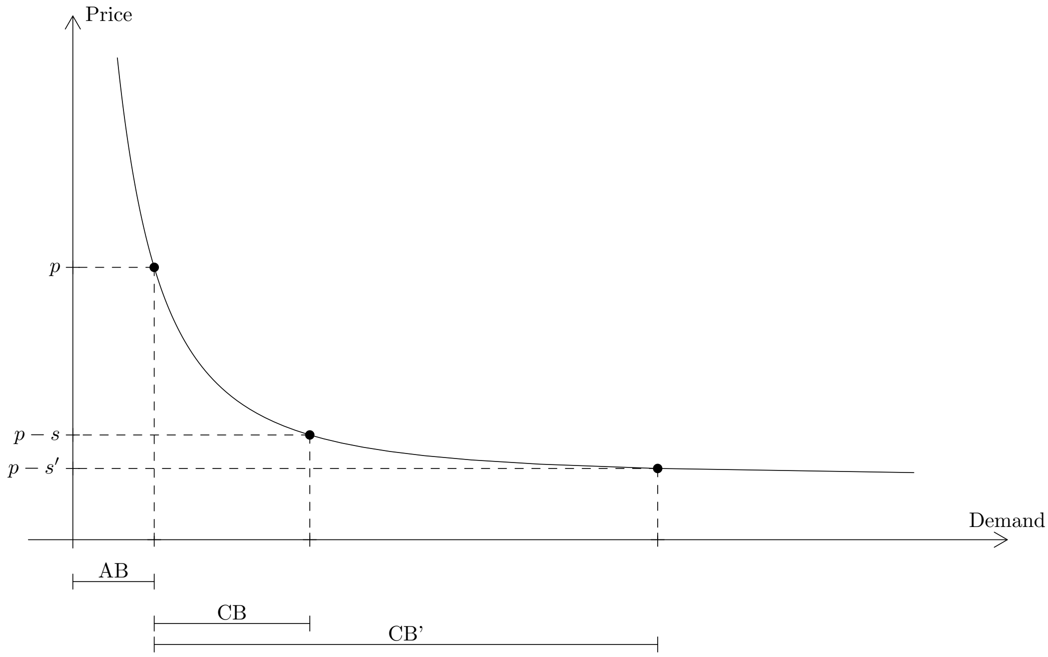
[57] This second assumption is also known as the Conditional Independence Assumption (see, e.g. Hansen (2000)).

by the subsidy programs. The theoretical static framework implies that there are exactly two potential explanations for the this positive relation between the subsidy value and the number of additional plug-PV systems given a fixed program budget:

As a first explanation for the positive relation between the subsidy value and the number of additional plug-PV systems given a fixed budget, it might be that, on average, the subsidy is set “too small”, such that too few interested citizens apply for the subsidy and the subsidy program budget is not fully used. In this case, increasing the subsidy increases the number of citizens who apply for the subsidy program. Because there is still unused budget, all additional requests for subsidies coming with the increase in the subsidy rate can be financed and consequently, the number of additional plug-PV systems caused by the subsidy program increases. In other words, the empirical findings might be explained by subsidy rates that do, on average, not attract enough citizens to apply and by subsidy program budgets that are larger than the budget that is required to meet all subsidy requests induced by the demand for plug-PV systems with the given subsidized price. This theoretical possibility of too small subsidy rates and resulting unused budgets exists as long as the demand function is strictly decreasing in price. For this explanation to make sense, it must follow that, on average, subsidy program budgets are not fully used in practice. However, for 81.8% of all subsidy programs analyzed in this study that ended before March 2024 (which marks the end of the period of analysis), it could be confirmed from official sources that the budget was fully used with the end of the subsidy program. Thus, it makes sense to consider the second potential explanation.

Second, there is a further potential explanation for the positive relation between the subsidy value and the number of additional plug-PV systems, which is explained together with the illustration in Figure 3.3. This second explanation provides a rationale for why an increase in the subsidy value can increase the number of additional plug-PV systems, even when the subsidy program is already oversubscribed, meaning that the number of subsidy requests given the subsidy value exceeds the subsidy program’s budget. To understand the intuition for this second potential explanation, remember that in the theoretical model, the municipality announces a subsidy rate and then uses a lottery to determine who will receive that subsidy in case more citizens apply to receive the subsidy than the municipality can give out given the limited subsidy program budget. Moreover, to understand the intuition, it is useful to distinguish between two types of citizens: First, “Always Buyers” are citizens whose reservation price for the plug-PV

Figure 3.3: Effects of Subsidy Increase



Note: The Figure shows an exemplary demand function for plug-PV systems. The market price p and the subsidy values s and s' with $s' > s$ are considered. AB denotes the "Always Buyers" who buy the plug-PV system irrespective of whether they receive a subsidy or not. The reservation price of an AB is weakly above the market price. CB denotes the "Conditional Buyers", who only buy the plug-PV system if they receive the subsidy. The reservation price of an CB is weakly above the subsidized market price (i.e., above $p - s$ or $p - s'$, respectively) but strictly below the market price p.

system is above the market price. Always Buyers will buy the plug-PV systems regardless of whether they are granted a subsidy or not. However, they will delay their purchase to happen after the lottery has been conducted and register for the subsidy program as well, because they want to spent as little money as possible for their plug-PV systems. Second, "Conditional Buyers" are citizens whose reservation price for the plug-PV system is strictly below the market price, but above the subsidized market price, i.e., above the market price minus the lump-sum subsidy. Conditional Buyers will buy the plug-PV systems only if they are granted the subsidy. Hence, the number of additional plug-PV systems caused by the subsidy program is equal to the number of Conditional Buyers who are granted the subsidy. The municipality cannot distinguish between Always Buyers and Conditional Buyers who both register to receive the subsidy. In Figure 3.3, I have plotted and exemplary demand function. Figure 3.3 shows, for the subsidy s and the market price p, the demand by Always Buyers (AB in the Figure) and the demand by Conditional Buyers (CB in the Figure). Now, suppose the program is already oversubscribed and the municipality increases the subsidy further, from s to s', as illustrated in Figure 3.3, which causes the the number of Conditional Buyers to increase, such that the over-subscription becomes even stronger. On the one hand, because the subsidy increases, given a fixed budget, the municipality can now distribute less subsidies, which should *decrease* the number of additional plug-PV systems with a fair lottery. I call this the *Direct Effect*. On the other hand, however, the mass of Conditional Buyers relative to Always Buyers increases. Thus, a fair lottery will grant the subsidy more often to a Conditional Buyer than to an Always Buyer, therefore increasing the number of additional plug-PV systems. I call this second effect the *Composition Effect*. If the demand function is locally "sufficiently convex" at the considered price-subsidy combination, such that at subsidized prices the demand increases strongly with a further decrease in price (from $p - s$ to $p - s'$, as illustrated in Figure 3.3), small increases in the subsidy rate lead to a large increase in the number of Conditional Buyers. This can also be seen in Figure 3.3, where the difference between s and s' is small, but the difference in CB to CB' is large. Then, a small increase in the subsidy leads to a small Direct Effect but a large Composition Effect, therefore causing the number of additional plug-PV systems to increase when the subsidy is increasing, even when the subsidy program is already oversubscribed. A demand function that is "sufficiently convex" on its whole domain is—for instance—the constant elasticity demand function with a price elasticity between zero and one. Please refer to Appendix 3.C for a full discussion including a formal derivation of these effects.

It remains to discuss the meaning of the positive association between the budget and the number of additional plug-PV systems. According to the theoretical framework, a positive association between the subsidy program budget and the number of additional plug-PV systems conditional on a fixed subsidy rate can only emerge when the budget is fully used. This is true for any type of decreasing demand function and does not depend on the curvature of demand. Intuitively, given a fixed subsidy rate, the budget can either be sufficient to meet all subsidy requests or not. First, suppose the budget is sufficient to meet all requests. Then, increasing the budget further does *not* increase the number of additional plug-PV systems, because already under the old budget, all existing subsidy requests could be financed. In this case, a larger budget does not help to generate additional investments by Conditional Buyers because all Conditional Buyers already received the subsidy under the old budget. Second, suppose the budget is *not* sufficient to meet all requests. Then, increasing the budget always allows the municipality to distribute more subsidies. Since the budget was binding before, with a larger budget, more Conditional Buyers can receive a subsidy which consequently increases the number of additional plug-PV systems. As outlined above, for 81.8% of all subsidy programs analyzed in this study that ended before March 2024 (which marks the end of the period of analysis), it could be confirmed from official sources that the budget was fully used with the end of the subsidy program. Thus, it also makes sense that the empirical results show a positive association between the subsidy program budget and the number of additional plug-PV systems, given that budgets were fully used for the overwhelming majority of programs.

To summarize, if municipalities would have increased the subsidy rates, they would have achieved, on average, a higher number of additional plug-PV systems caused by the subsidy program without changing their overall subsidy budget. This might either be caused by subsidy rates that are too small to attract a sufficient number of citizens applying for the subsidy, resulting in unused budget; or by a demand function that is locally "sufficiently convex", which can cause an increasing number of additional plug-PV systems following from an increase in the subsidy rate, even when the subsidy program is already oversubscribed.

3.7 Conclusion

In this chapter, I analyzed 270 subsidy programs for plug-PV systems over a period of more than four years across 733 municipalities in Germany. I estimated the causal effect of these programs on the investment in plug-PV systems. Using the estimated causal effects, I analyzed the impact of both subsidy program characteristics and municipality characteristics on the additional investment caused by the subsidy programs. I find that the subsidy programs are, on average, contributing positively to more investments. Back of the envelope calculations suggest that the additional capacity caused by the subsidy programs is, on average, 1.20 to 1.41 times as large as the photovoltaic capacity that the municipality could have installed at the same total costs itself. Moreover, I also find a positive relation between the subsidy value and the additional investments caused by the subsidy programs given a fixed budget, implying that if municipalities would have increased the subsidy rates and if they have all used a fair lottery scheme to determine who receives the subsidy, they would have achieved, on average, a higher number of additional plug-PV systems caused by the subsidy program without changing their overall subsidy budget.

I contribute to the literature by explicitly accounting for budget constraints in the evaluation of subsidy policies. Previous research has not accounted for such budget constraints. Accounting for these budget constraints leads to novel insights on the potential effects of the subsidy value on the additional investment caused by a subsidy program. In particular, the theoretical results discussed show that when a subsidy program is already oversubscribed, increasing the subsidy rate and thereby increasing the over-subscription can increase the additional investments caused by the subsidy program. Moreover, another contribution of this study is the analysis of a set of geographically very granular subsidy programs, which makes the construction of a credible counterfactual particularly easy.

With the explicit inclusion of budget constraints in the design of optimal subsidy policies, several directions of future work are possible. For instance, one might extend the theoretical framework to a dynamic version, accounting for temporary budget constraints. Furthermore, within such a dynamic framework, it would be interesting to study the behavior of citizens who account for potential future encouragement programs. Further directions of future research concern other municipal investment support programs. While plug-PV subsidy programs are arguable among the most prevalent investment support programs of German municipalities, at least for German municipalities, there is a range of further local investment support programs that could be exploited to study the additionality of investment support programs.

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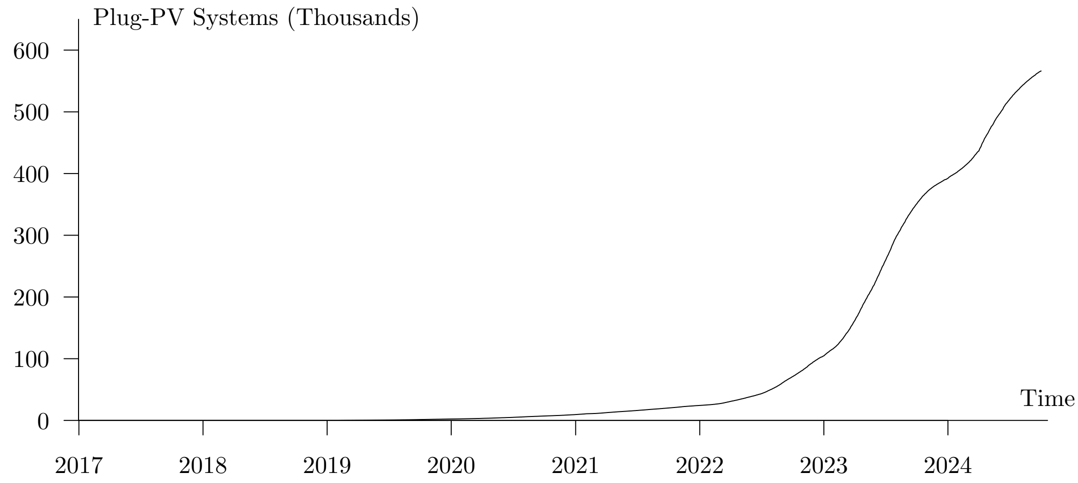
Appendices

3.A Descriptives

3.A.1 Number of Plug-PV Systems in Germany

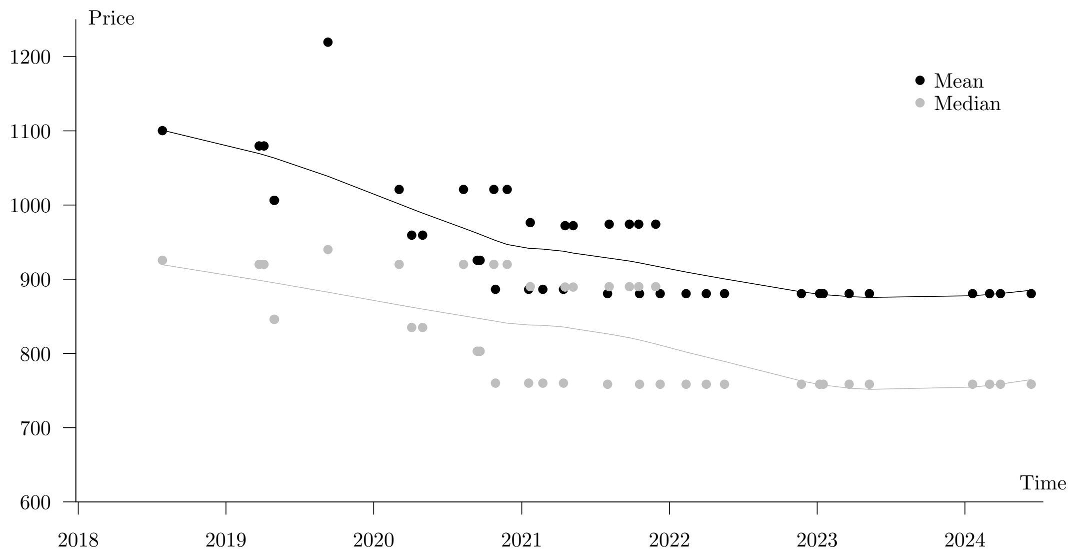
Figure 3.A.1: Number of Plug-PV Systems in Germany Over Time



Note: The number of installed plug-PV systems in Germany over time according to the MaStR data. The number of plug-PV Systems refers to whole Germany, also including municipalities not included in the study sample. Plug-PV Systems in the MaStR registry data are counted as described in in Section 3.3.4.

3.A.2 Plug-PV System Prices Over Time in Germany

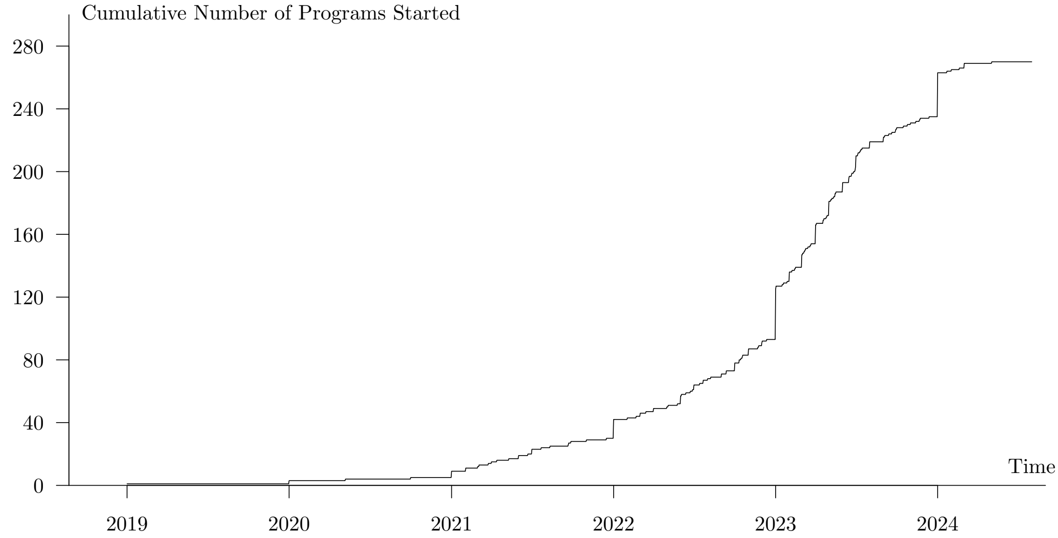
Figure 3.A.2: Prices of Plug-PV Systems in Germany Over Time



Note: Mean (black) and median (gray) price of plug-PV Systems available on the German market (according to PVM and DGS, see Section 3.3.2) over time. The dots refer to mean and median prices of available plug-PV systems in different snapshots from the database, i.e., for different points in time for both PVM and DGS. To calculate mean and median prices for one snapshot across all available plug-PV systems, I first scale the price for each available plug-PV system. More specifically, for plug-PV system j observed in snapshot s, the database reports both the price P_{js} for this panel on the market as well as its capacity measured in watt peak (Wp) and denoted as C_{js}. The scaled price $\tilde{P}_{js}$ is given by $\tilde{P}_{js} = P_{js} * (600/C_{js})$. The scaling is conducted to make the prices comparable across different systems with different capacities. In the Figure, one black (gray) dot is the average (median) $\tilde{P}_{js}$ across all systems j available in snapshot s. The lines are calculated by using local polynomial regression fitting (LOESS). More specifically, for each point along the horizontal axis, a second-degree polynomial is fit to a subset of the data defined by a neighborhood around that point, with observations weighted by their distance to the target point. The smoothing parameter (span) is set to 0.75, meaning that 75% of the data points are used in each local fit. Tricubic weighting is applied, where weights are proportional to $(1-(d/d_{\max})^3)^3$, with d representing the distance from the target point and $d_{\max}$ the maximum distance within the neighborhood.

3.A.3 Plug-PV Subsidy Programs over Time

Figure 3.A.3: Subsidy Programs for Plug-PV Systems Over Time in Sample



Note: Cumulative number of subsidy programs started until each point in time on the horizontal axis analyzed in the used sample of this study.

3.A.4 Amortization Time of Plug-PV Systems

Table 3.A.1: Amortization Time of Plug-PV Systems

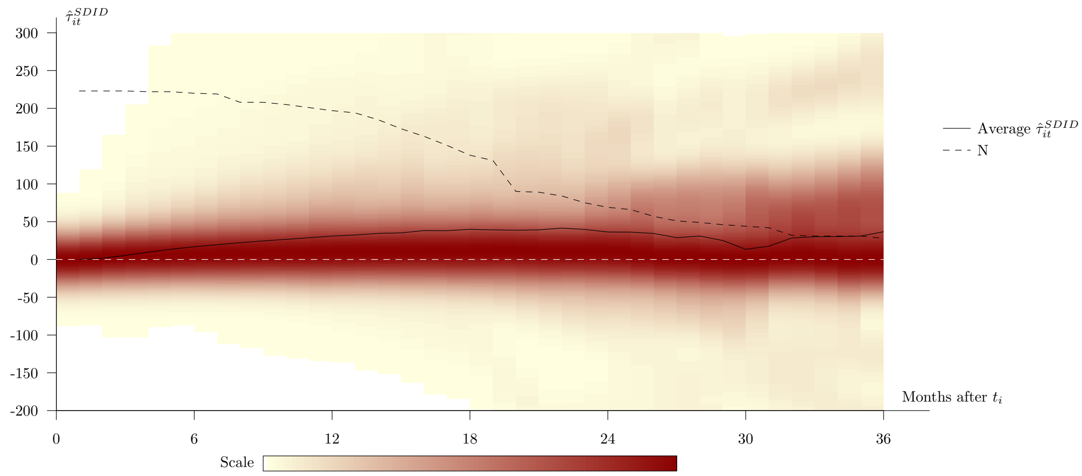
	2019	2020	2021	2022	2023	2024
Amortization time in years for investment						
Apartment						
1 Person (1500 kWh/Year)						
Balcony installation	13	12	12	10	8	8
Roof installation	11	10	10	8	7	7
2 Persons (2100 kWh/Year)						
Balcony installation	11	10	10	8	7	7
Roof installation	9	8	8	7	6	6
3 Persons (2600 kWh/Year)						
Balcony installation	11	10	10	8	7	7
Roof installation	9	8	8	6	6	6
4 Persons (3000 kWh/Year)						
Balcony installation	10	9	9	8	6	6
Roof installation	8	7	7	6	5	5
Single Family House						
1 Person (2500 kWh/Year)						
Balcony installation	11	10	10	8	7	7
Roof installation	9	8	8	7	6	6
2 Persons (3000 kWh/Year)						
Balcony installation	10	9	9	8	6	6
Roof installation	8	7	7	6	5	5
3 Persons (3700 kWh/Year)						
Balcony installation	10	8	8	7	6	6
Roof installation	8	7	7	5	5	5
4 Persons (4000 kWh/Year)						
Balcony installation	9	8	8	7	6	6
Roof installation	7	6	6	5	5	5
Data used to calculate amortization time						
Median Price of Plug-PV Systems	920	835	889.5	758.5	758.5	758.5
Average price of one kWh (incl. taxes) in cents						
For yearly usage in [1000 kWh, 2500 kWh)	32.44	33.43	35.88	37.38	44.58	44.23
For yearly usage in [2500 kWh, 5000 kWh)	28.78	30.06	32.34	33.57	40.20	39.51

Note: The amortization time was calculated with the online amortization time calculator for investment in plug-PV systems from the University of Applied Sciences Berlin, which can be accessed on `https://solar.htw-berlin.de/rechner/stecker-solar-simulator/`. The amortization time is calculated using the respective parameters given for each cell. Balcony installations refer to installations of the panels in southern direction 90 degrees. Roof installations refer to installations in southern direction in 45 degrees. The online calculator automatically uses the average electricity usage per year in Germany for different number of people in the household and for apartments and single family houses, respectively. I used a standard 600 Wp plug-PV system with two panels for the calculation and used its median price per year, which is shown in the table and which is calculated following the procedure and using the data laid out in Section 3.3.2. Moreover, average prices for electricity (per kWh, including taxes) are taken from the German federal statistical office (i.e., from Statistisches Bundesamt (Destatis)). The calculated scenarios do assume a constant price of electricity in the year of the installation.

3.B Results

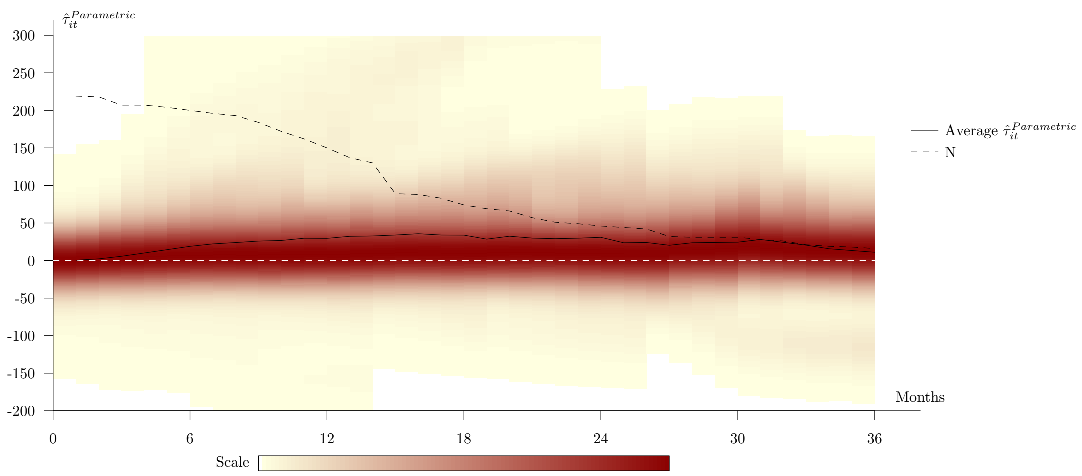
3.B.1 Estimated Number of Additional Plug-PV Systems

Figure 3.B.1: Additional Plug-PV Systems Estimated (SDID)



Note: Heat-map illustrating the distribution of $\hat{\tau}_{it}^{\text{SDID}}$ for each month t after t_i. The color intensity on the provided Scale illustrates the empirical distribution of estimated effects. The dashed line shows the number of observations (i.e., municipalities) for which an estimate τ_{it} is available for each month after program start. The solid line shows the average estimated $\hat{\tau}_{it}^{\text{SDID}}$.

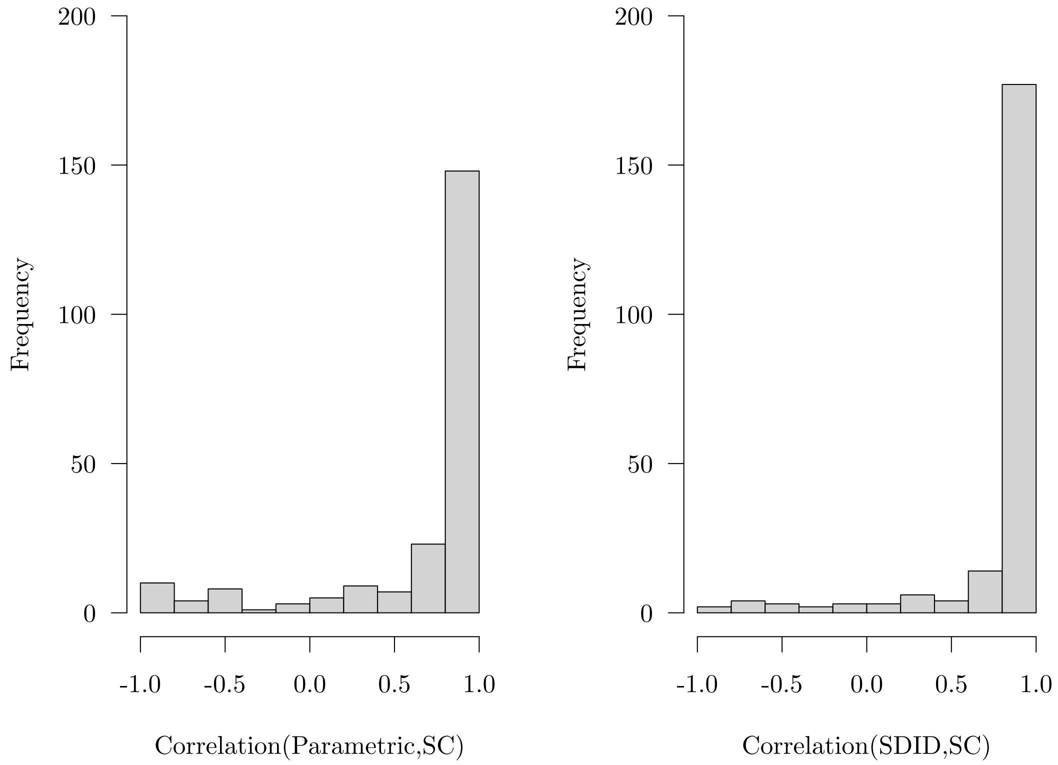
Figure 3.B.2: Additional Plug-PV Systems Estimated (Parametric Method)



Note: Heat-map illustrating the distribution of $\hat{\tau}_{it}^{\text{Parametric}}$ for each month t after t_i. The color intensity on the provided Scale illustrates the empirical distribution of estimated effects. The dashed line shows the number of observations (i.e., municipalities) for which an estimate τ_{it} is available for each month after program start. The solid line shows the average estimated $\hat{\tau}_{it}^{\text{Parametric}}$.

3.B.2 Correlation of Estimates by Different Methods

Figure 3.B.3: Correlation of Estimates Between Different Methods



Note: For each municipality i with a subsidy program, the correlation between the vectors $\left(\tau^{SC}_{it_i+1}, \cdots, \tau^{SC}_{iT}\right)$ and $\left(\tau^{E'}_{it_i+1}, \cdots, \tau^{E'}_{iT}\right)$ for $E' \in \{\text{Parametric}, \text{SDID}\}$, respectively, is computed. The Figure shows the histograms of the respective correlations.

3.B.3 Exploratory Results

Table 3.B.1: LASSO Estimator Results for SDID Estimates

	(1)	(2)	(3)	(4)	(5)
Subsidy value	0.0827	0.0487	0.0437	0.0453	0.0378
Budget	0.0005	0.0006	0.0009	0.0009	0.0010
Program Duration	-0.0083	-0.0468			
Combined budget					
Council vote share					
MaStR obligation	9.2467	7.6728	0.3875	0.2284	
Receivers restricted			-6.1868		
Income differences	-14.0880	-8.4510	-6.6522	-4.8566	
Tax per capita	-0.0222	-0.0233	-0.0412	-0.0427	-0.0440
Income tax per capita			0.0168	0.0054	
Population density					
Population	0.0008	0.0007	0.0011	0.0012	0.0011
Average age	2.0311	0.4726	0.4985		
Solar potential	-0.1701	-0.1311			
N	1922	1922	1922	1922	1922
λ	3.002	0.812	0.252	0.285	0.355
R^2	0.286	0.424	0.576	0.591	0.609
FE Post-treatment month		✓	✓	✓	✓
FE Program period			✓	✓	✓
FE Subsidy type				✓	✓
FE State					✓

Note: Results for the LASSO estimator (Section 3.4.2) for varying set of fixed effects (FE) where the outcome analyzed is τ_{it}^{SDID} and where all variables were standardized prior to the estimation. Blank cells indicate that the LASSO estimator shrinks the coefficient of the respective variable to zero, meaning these variables were not selected by the estimator. For variable definitions, refer to Appendix 3.E. The parameter λ is chosen using five-fold cross-validation as described in the main text.

Table 3.B.2: LASSO Estimator Results for Parametric Estimates

	(1)	(2)	(3)	(4)	(5)
Subsidy value					0.0002
Budget					0.0001
Program Duration	-0.0002	-0.0017			
Combined budget				0.2151	0.0721
Council vote share			1.2215	1.9850	2.6436
MaStR obligation	0.5547	0.5460	0.2710	0.0916	0.2455
Receivers restricted					
Income differences					
Tax per capita	-0.0009	-0.0009	-0.0015	-0.0022	-0.0023
Income tax per capita	0.0006	0.0008	0.0015	0.0011	
Population density			-0.0126	-0.0120	
Population	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
Average age	0.3687	0.3184	0.4333	0.3628	0.3453
Solar potential	-0.0054	-0.0043			
N	1427	1427	1427	1427	1427
λ	0.131	0.035	0.013	0.013	0.011
R^2	0.301	0.425	0.567	0.600	0.623
FE Post-treatment month		✓	✓	✓	✓
FE Program period			✓	✓	✓
FE Subsidy type				✓	✓
FE State					✓

Note: Results for the LASSO estimator (Section 3.4.2) for varying set of fixed effects (FE) where the outcome analyzed is $\tau_{it}^{\text{Parametric}}$ and where all variables were standardized prior to the estimation. Blank cells indicate that the LASSO estimator shrinks the coefficient of the respective variable to zero, meaning these variables were not selected by the estimator. For variable definitions, refer to Appendix 3.E. The parameter λ is chosen using five-fold cross-validation as described in the main text.

3.B.4 Effects of Subsidy Value and Budget

Table 3.B.3: DDML Results for SDID Estimates

	Lasso b/se	Neural Network b/se	Random Forest b/se	Support Vector Machine b/se	Gradient Boosting b/se
Subsidy value	0.1504*	0.2493***	0.2104***	0.2153**	0.0879
	(0.0871)	(0.0841)	(0.0796)	(0.0887)	(0.0683)
Budget	0.0009***	0.0007***	0.0007***	0.0008***	0.0004**
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0002)
N	1922	1922	1922	1922	1922

Note: Estimation results for the DDML algorithm (Section 3.4.2) for $\hat{\tau}_{it}^{SDID}$. Subsidy Value refers to S_{it} and Budget refers to B_i. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. The columns differ with respect to the method used to estimate g_0, m_0^S and m_0^B.

Table 3.B.4: DDML Results for Parametric Estimates

	Lasso b/se	Neural Network b/se	Random Forest b/se	Support Vector Machine b/se	Gradient Boosting b/se
Subsidy value	0.0012	0.0044	0.0092**	0.0049	0.0096**
	(0.0016)	(0.0038)	(0.0046)	(0.0044)	(0.0044)
Budget	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
N	1427	1427	1427	1427	1427

Note: Estimation results for the DDML algorithm (Section 3.4.2) for $\hat{\tau}_{it}^{Parametric}$. Subsidy Value refers to S_{it} and Budget refers to B_i. Subsidy Value refers to S_{it} and Budget refers to B_i. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. The columns differ with respect to the method used to estimate g_0, m_0^S and m_0^B.

3.C Conceptual Framework

In this Appendix, I present a tractable static framework analyzing the effect of municipal subsidies for plug-PV systems, when the municipal budget for subsidies is limited.

3.C.1 Setup

The structure of the theoretical framework includes a characterization of the demand and supply of plug-PV systems in the first subsection as well as a characterization of the theoretical municipal subsidy program in the second subsection. The theoretical municipal subsidy program aims at closely mimicking the subsidy programs by German municipalities analyzed empirically in this paper.

Demand and Supply

Consider a municipality with a unit mass of citizens. Within the municipality, the demand for plug-PV systems is characterized by the demand function $Q(\cdot)$ and the supply of plug-PV systems is characterized by the supply function $S(\cdot)$. Throughout the analysis, I consider the demand to be decreasing in price and a fully elastic supply:

Assumption 1. $Q : \mathbb{R}_+ \to \mathbb{R}_+$ *is twice continuously differentiable with* $Q(x)' < 0$ *for all* $x \in \mathbb{R}_+$*. For* $S : \mathbb{R}_+ \to \mathbb{R}_+$*, it holds that* $S(x) = 0$ *for all* $x < p$ *and* $S(x) = \infty$ *for all* $x \geq p > 0$*.*

The fully elastic supply of plug-PV systems at price $p > 0$ aims to reflect the relatively small market of plug-PV systems within a considered municipality in comparison to the international market on which these systems are traded. In some parts of the subsequent analysis, I make the additional Assumption 2 on the curvature of the demand function:

Assumption 2. *The demand function* $Q(\cdot)$ *is log-concave, i.e. the function* $ln(Q(\cdot))$ *is concave.*

Assumption 2 further restricts the space of potential demand functions. However, as Kang and Vasserman (2025) point out, "many common demand curves are log-concave". Indeed, Bagnoli and Bergstrom (2005) document various applications of log-concave demand characterizations.[58] To get some better understanding of Assumption 2, it is useful to consider the demand function as the result of summation a mass of individuals who differ according to their willingness to pay for plug-PV systems. More specifically, following Bagnoli and Bergstrom (2005), suppose $f(\cdot)$ is the density function of individuals' reservation prices for plug-PV systems and $F(\cdot)$ the corresponding distribution function. Then, the demand at price p, i.e., $Q(p)$, is proportional to $\bar{F}(p) = 1 - F(p)$ (Bagnoli and Bergstrom, 2005). As Bagnoli and Bergstrom (2005) show, when the density function $f(\cdot)$ is continuously differentiable and log-concave, then $\bar{F}(p)$ and thereby $Q(p)$ is also log-concave. In fact, various commonly known distributions exhibit log-concave density functions including the uniform, normal, exponential, logistic, extreme value, gamma, chi-squared and chi-distribution (Quint, 2014). Thus, Assumption 2 would allow for all these type of reservation price distributions.

Municipal Subsidy Program

Taking the supply and demand as given, the municipality introduces a subsidy program for plug-PV systems in order to increase the number of installed plug-PV systems. To do so, the municipality grants its citizens a lump-sum subsidy $s \in (0, p]$, thereby effectively reducing the price for plug-PV systems to $p - s$ for citizens within the municipality.[59] The municipality provides a fixed budget $B > 0$ for the subsidies. In the following, a "subsidy program" is characterized by the tuple (s, B).

Given B, either the provided budget is large enough to satisfy all requested subsidies or not. In the first case, if the budget is large enough, i.e., $B \geq sQ(p-s)$, then the number of installed plug-PV systems is equal to $Q(p - s)$ and the municipality spends $sQ(p - s)$ of its budget. In the second case, the municipality cannot satisfy all requests of its citizens to receive the lump-sum subsidy, i.e., $B < sQ(p-s)$. Then, congruent with the considered subsidy programs of German municipalities analyzed in this paper, individuals who are willing to buy a plug-PV system at price $p - s$ first register with the municipality. Then, the municipality

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[58] For instance, more recently, Weyl and Fabinger (2013), Tan and Wright (2018), Condorelli (2022) or Miravete, Seim and Thurk (2020) derive results showing that log-concave demand functions can have a meaningful economic interpretations in different settings.

[59] I assume $s \leq p$, because in practice, all documented subsidy rates are found to be strictly smaller than the price p (see Table 3.1 for the subsidy height and Appendix 3.A.2 for the prices of plug-PV systems over time).

uses a fair lottery among all these registered citizens who applied to determine who will receive the subsidy to buy a plug-PV system.[60]

Definition 1. *For each subsidy program* (s, B) *denote individual* i*'s reservation price as* θ_i*. Then:*

1. *All* i *with* $\theta_i \geq p$ *are called "Always Buyers".*
2. *All* i *with* $\theta_i \in [p - s, p)$ *are called "Conditional Buyers".*
3. *All* i *with* $\theta_i < p - s$ *are called "Never Buyers".*

It is useful for the latter analysis to distinguish three types of individuals within the municipality given the subsidy s. These are (1) "Always Buyers" who register with the municipality and buy the plug-PV system irrespectively of whether they receive a subsidy or not; (2) "Conditional Buyers" who register with the municipality and buy the plug-PV system *only* if they receive the subsidy s; and (3) "Never Buyers" who do not register with the municipality and *never* buy the plug-PV system irrespectively of whether they receive the subsidy s or not.

To characterize the optimal behavior of the municipality, I assume that the municipality is aiming at maximizing the number of installed plug-PV systems with its subsidy program. Formally, for each subsidy program (s, B), I define $\tau(s, B)$ as the number of *additional* plug-PV systems that are installed due to the subsidy program. Thus, $\tau(s, B)$ is equal to the difference between the number of installed plug-PV systems under the subsidy program and $Q(p)$. Also note that $\tau(s, B)$ is equal to the number of Conditional Buyers who indeed receive a subsidy s and thus buy the plug-PV system. The aim of the municipality is to maximize $\tau(s, B)$ by the choice of s and B. However, congruent with the considered subsidy programs by German municipalities, even though I allow B to vary across municipalities, I focus on the optimal choice of s given B in the subsequent analysis. In practice, even though municipalities have some degree of autonomy on the allocation of their overall financial budget, they face tight budget constraints and constraints on the autonomy to use their budget freely, which motivates this approach.[61]

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[60] In practice, in the analyzed municipal subsidy programs, municipalities either use a first-come-first-serve approach or a lottery mechanism to determine who receives the subsidy in case the applications for subsidies cannot be met with the available budget.

[61] See Section 3.2.2 for a discussion of the financial autonomy and budgets of German municipalities.

3.C.2 Theoretical Results

In order to derive theoretical implications in the following, I will first derive the relation between subsidy program characteristics and the number of installed plug-PV systems in the municipality. Using this characterization, I will characterize the optimal behavior of the municipality.

Additional Plug-PV Systems

To derive testable theoretical implications, first consider the characterization of $\tau(s, B)$, which requires a case distinction, depending on the question of whether the municipal budget B is sufficient to pay out a subsidy to all individuals who are interest to buy at price $p - s$:[62]

$$\tau(s,B) = \begin{cases} \left(\frac{Q(p-s)-Q(p)}{Q(p-s)}\right)\frac{B}{s} & \text{if } \; B < Q(p-s)s \\ Q(p-s) - Q(p) & \text{if } \; B \geq Q(p-s)s \end{cases}$$

Considering this characterization of $\tau(s, B)$, the comparative statics are as follows:

Lemma 1. *If Assumption 1 holds, then:*

$$\frac{\partial \tau(s,B)}{\partial B} \begin{cases} > 0 & \textit{if } B < Q(p-s)s \\ = 0 & \textit{if } B \geq Q(p-s)s \end{cases} \qquad \textit{and} \quad \left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B \geq Q(p-s)s} > 0$$

If Assumptions 1 and 2 hold, then:

$$\left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B < Q(p-s)s} < 0$$

Proof. See Appendix 3.D.2.

Lemma 1 shows that an increase in the budget B for the subsidy program strictly increases the number of additional plug-PV systems if the given budget cannot fully satisfy the subsidy requests given the subsidy s. If the given budget exceeds the total sum of subsidy requests given the subsidy s, then a further increase does not change the number of additional plug-PV systems. Intuitively, given s, more budget leads to an increase in the additional panels only if there are some Conditional Buyers who are willing to buy a plug-PV system at price $p - s$, but who are not granted the subsidy due to the municipal budget limitations given B.

Lemma 1 further shows that if the budget is sufficient to cover all subsidy requests given s, an increase in the subsidy rate s increases the number of

[62] For details, see Appendix 3.D.1.

additional plug-PV systems. Intuitively, increasing the subsidy increases the mass of Conditional Buyers and leaves the mass of Always Buyers unchanged. Since there is enough budget at the margin, these additional requests for plug-PV systems from Conditional Buyers can be financed and thus lead to an increase in the number of additional plug-PV systems.

Lemma 1 also characterizes the effect of an increase in s in case the budget is already fully used given a subsidy s. In this case, an increase in s increases the requests for subsidies by Conditional Buyers, because $Q(p-s)$ increases in s. Then, the limited budget leads to two opposing effects. First, there is a *Direct Effect*: Because the mass of individuals who apply for the subsidy program increases, the probability to receive the subsidy after applying decreases for both Always Buyers and Conditional Buyers. Second, there is a *Composition Effect*: With an increase in s, the group of Conditional Buyers increases relative to the group of Always Buyers. Thus, the chance that a Conditional Buyer rather than an Always Buyer will receive one of the randomly granted subsidies increases. In other words, when the municipality randomly selects individuals from all registered individuals, there is a higher chance that one of these selected individuals will be a Conditional Buyer rather than a Always Buyer when the mass of Conditional Buyers increases. Hence, while the Direct Effect describes a decrease in the probability for *all* registered individuals to receive the subsidy, the Composition Effect describes a increase in the probability *only* for the group of Conditional Buyers to receive the subsidy. For a log-linear demand function which follows from Assumption 2, the Direct Effect always dominates the Composition Effect, which means that the mass of Conditional Buyers who receive the subsidy decreases, while the mass of Always Buyers is constant, which consequently decreases the number of additional plug-PV systems.

Optimal Subsidy Program for Log-Concave Demand

Using Lemma 1, I can also characterize the behavior of a municipality that aims at maximizing the additional number of plug-PV systems given a fixed budget B for a log-concave demand function. Denote the maximizing subsidy rates as s^*, i.e., $s^* = \arg\max_s \tau(s, B)$.

Proposition 1. *Suppose Assumptions 1 and 2 hold and the municipality takes B as given. Then, if $\lim_{s \to p} sQ(p-s) > B$ it follows $s^* = \{s : B = Q(p-s)s\}$. Otherwise, if $\lim_{s \to p} sQ(p-s) \leq B$, $s^* = p$.*

Proof. If Assumptions 1 and 2 hold, then:

$$\frac{\partial \tau(s,B)}{\partial s}\Big|_{B<Q(p-s)s} < 0 \quad \text{and} \quad \frac{\partial \tau(s,B)}{\partial s}\Big|_{B\geq Q(p-s)s} > 0$$

by Lemma 1. Thus, since $\tau(s,B)$ given a fixed B is increasing on $B < Q(p-s)s$ and decreasing on $B \geq Q(p-s)s$, it attains its maximum on $s^* = \{B = Q(p-s)s\}$ if $\lim_{s\to p} sQ(p-s) > B$. If $\lim_{s\to p} sQ(p-s) \leq B$, there is a corner solution with $s^* = p$. □

Proposition 1 shows that given a fixed budget that is insufficient to finance a maximum subsidy of $s = p$ and given a strictly decreasing log-concave demand, the number of additional plug-PV systems is maximized when a subsidy is chosen such that the budget is fully used. Proposition 2 shows that this subsidy rate s^* is always unique.

Proposition 2. *Suppose Assumptions 1 and 2 hold and the municipality takes B as given. Then there always exists a unique $s^* > 0$.*

Proof. See Appendix 3.D.3.

Lastly, it remains to discuss the impact of variables considered as exogenous on the choice of s^* of the municipality. The market price p is assumed to be fully exogenous and the budget B can reasonably considered to be exogenous (Section 3.C.1).

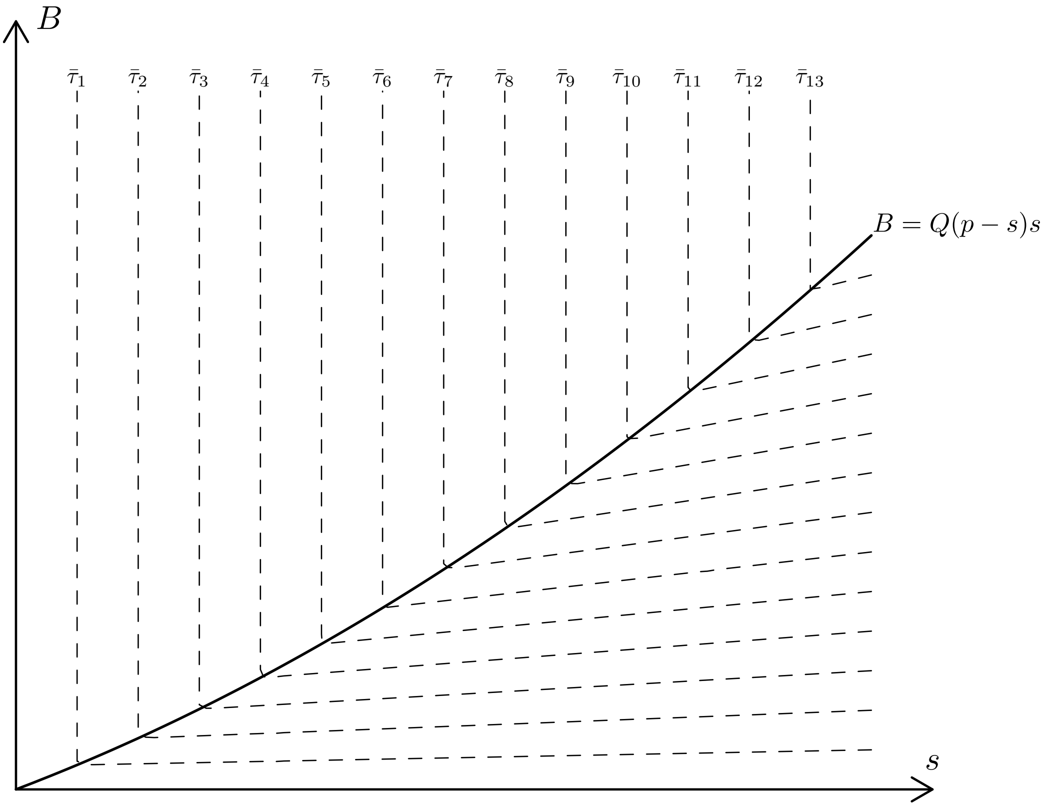
Lemma 2. *Suppose Assumptions 1 and 2 hold. If $\lim_{s\to p} sQ(p-s) > B$, then $\frac{ds^*}{dB} > 0$ and $\frac{ds^*}{dp} \in (0,1]$. If $\lim_{s\to p} sQ(p-s) > B$ and $Q(p-s) > 0$, then $\frac{ds^*}{dp} \in (0,1)$. If $\lim_{s\to p} sQ(p-s) \leq B$, then $\frac{ds^*}{dB} = 0$ and $\frac{ds^*}{dp} = 1$.*

Proof. See Appendix 3.D.4.

Lemma 2 shows that in case the budget is increasing, the municipality only increases the subsidy rate in case the budget is binding, because a larger budget allows an increase in the subsidy to create more additional plug-PV systems. If the budget was *not* binding and the subsidy was already chosen such that the maximum number of additional plug-PV systems is installed, a further increase in the budget does not influence this optimal subsidy rate. Similarly, in case of a price increase, the municipality increases s^* to compensate the increase. While the compensation is equal to the increase in case the budget is not binding, it is strictly smaller than the increase when the budget is binding and the municipality attracts at least some positive demand with its subsidy program.

To illustrate the results presented so far for log-concave demand functions, consider a exemplary linear demand function of the form $Q(p) = \frac{\alpha - p}{\beta}$, which is log-concave.[63] Figure 3.C.1 illustrates the number of additional plug-PV systems for different combinations of s and B for the exemplary linear demand function.[64] Figure 3.C.1 shows contour lines (dashed lines) for different levels of additional plug-PV systems. More specifically, $\bar{\tau}_k$ denotes a fixed level of additional plug-PV systems such that $\bar{\tau}_k < \bar{\tau}_l$ for any $k < l$. The condition $B = Q(p - s)s$ illustrates the budget such that given s, the budget of the municipality is fully used and all subsidy requests of citizens can be met.

Figure 3.C.1: Contour-Plot: Additional Plug-PV Systems for Linear Demand



Note: Contour lines (dashed lines) illustrating different levels of additional panels $\bar{\tau}_j = \tau(s, B)$ for different combinations of (s, B), where $\bar{\tau}_k < \bar{\tau}_l$ for any $k < l$ for a linear demand function of the form $Q(p) = \frac{\alpha - p}{\beta}$ with $\alpha - p = 1$ and $\beta = 0.5$. The line $B = Q(p - s)s$ shows for each s the budget B such that the budget is fully used. The Figure considers the case $\lim_{s \to p} Q(p-s)s > B$.

[63] For the illustrations, I set $\alpha - p = 1$ and $\beta = 0.5$. The illustrations all relate to the case of $\lim_{s \to p} Q(p - s)s > B$ for all levels of B considered, which practically means that the municipality has not enough budget to set $s = p$ and finance all subsidy requests.

[64] For the exemplary linear demand function it follows: $\tau(s, B) = B/(\alpha - p + s)$ if $Q(p - s)s \leq B$ and $\tau(s, B) = s/\beta$ if $Q(p - s)s > B$.

As Figure 3.C.1 shows, if $B > Q(p-s)s$, any additional budget B given s does *not* increase the number of additional plug-PV systems because all subsidy requests given s can already be financed with the smaller budget $B = Q(p-s)s$. Furthermore, if $B > Q(p-s)s$, any increase in s given a fixed B does unambiguously increase the number of additional plug-PV systems, because the budget allows to increase s in order to attract more Conditional Buyers whose requests can all be met given the unused budget.

As Figure 3.C.1 further illustrates, if $B < Q(p-s)s$, any increase in B does unambiguously increase the number of additional plug-PV systems because given a certain subsidy s, the budget is insufficient to finance all subsidy requests. Therefore, a increase in the budget allows to finance more requests by Conditional Buyers, thereby increasing the number of additional plug-PV systems. Moreover, increasing the subsidy s decreases the number of additional plug-PV systems. This decrease is stronger for larger combinations of both s and B, illustrated by a larger slope for larger contour lines below the condition $B = Q(p-s)s$. Intuitively, the larger the subsidy already is, the larger the required increase in the budget if the subsidy is increased in order to keep the number of financed systems constant.

Optimal Subsidy Program for Other Demand Functions

While the results in the previous subsections relied on Assumption 2, i.e., the log-concavity of demand, it remains to discuss cases in which Assumption 2 does not hold, i.e., when the demand is not log-concave. The critical point where Assumption 2 enters the analysis is in the characterization of the partial effect of an increase in s on $\tau(s, B)$ in case the budget of the municipality is fully used, which was shown to be negative given Assumption 2.

If the effect of an increase in s on $\tau(s, B)$ in case the budget of the municipality is fully used—i.e., when $B < Q(p-s)s$—would be *positive*, we would in fact be in a situation in which an increase in s always increases $\tau(s, B)$. In this case, an increase in s is *always* optimal for the municipality which aims at maximizing $\tau(s, B)$. Lemma 3 shows the rather technical condition under which increasing s would lead to an *increase* in $\tau(s, B)$ in case the budget of the municipality is fully used.

Lemma 3. *Suppose Assumption 1 holds, then:*

$$\left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B<Q(p-s)s} > 0 \Longleftrightarrow \frac{-Q'(p-s)s}{Q(p-s)} > \frac{Q(p-s)-Q(p)}{Q(p)} \tag{3.5}$$

Proof. See Appendix 3.D.5.

Condition (3.5) given in Lemma 3 ensuring an increase in τ given an increase in s is hard to interpret directly. A potentially more accessible and sufficient condition which allows a more reasonable interpretation is provided in Corollary 1.

Corollary 1. *Suppose Assumption 1 holds. If for all $x \in [p-s,p]$ it holds that*

$$Q''(x) > \frac{-Q'(p-s)2}{s}\left(\frac{Q(p-s)-Q(p)}{Q(p-s)}\right) > 0,$$

Condition (3.5) holds.

Proof. See Appendix 3.D.6.

Corollary 1 shows that Condition (3.5) holds once the demand function is "strongly" convex, which follows from Q'' being larger than some positive number. In other words, the rate at which the demand drops in the price must be slowing down for larger prices in order for Condition (3.5) to hold. In practice, this can occur when there is a relatively small group of buyers who are relatively insensitive to price changes and are also wiling to buy at larger prices, while a *lot* more buyers are willing to buy once the price drops sufficiently. In such a case, an increase in the subsidy rate s—even when there is not enough budget to satisfy all subsidy requests—can increase the number of additional plug-PV systems. Intuitively, this occurs because there is a relatively strong increase in the number of Conditional Buyers while the number of Always Buyers is relatively small. In such a case, the Composition Effect of an increase in the subsidy outweighs the Direct Effect of such an increase (Section 3.C.2): The Direct Effect describes the *decrease* in the likelihood for both the Always Buyers and the Conditional Buyers to receive a subsidy when the number of Conditional Buyers increases with an increase in s. The Composition Effect describes the *increase* in the likelihood that a Conditional Buyer rather than a Always Buyer receives a given subsidy. This increase in the likelihood described by the Composition Effect is particularly large when the number of Conditional Buyers in relation to the number of Always

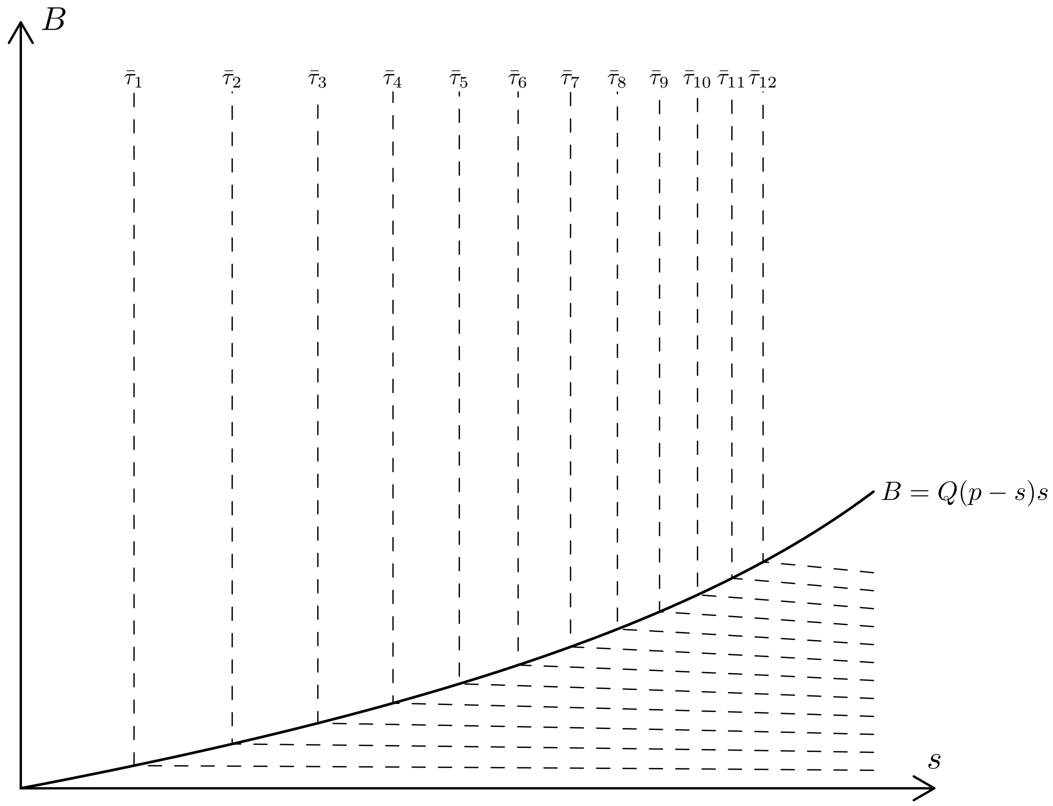
Buyers grows very fast, which is the case when there are only few Always Buyers who are insensitive to price changes and a lot more Conditional Buyers when the price decreases.

Corollary 2 shows that for a commonly known convex demand function with constant elasticity, Condition (3.5) provided in Lemma 3 holds.

Corollary 2. *For the demand function* $Q(p) = p^{-b}$ *with* $b \in (0, 1)$, *Assumption 1 and Condition (3.5) hold.*

Proof. See Appendix 3.D.7.

Figure 3.C.2: Contour-Plot: Additional Plug-PV Systems for Constant-Elasticity Demand



Note: Contour lines (dashed lines) illustrating different levels of additional panels $\bar{\tau}_j = \tau(s, B)$ for different combinations of (s, B), where $\bar{\tau}_k < \bar{\tau}_l$ for any $k < l$ for the demand function $Q(p) = p^{-1/2}$. The line $B = Q(p - s)s$ shows for each s the budget B such that the budget is fully used.

To illustrate the findings for demand functions that fulfill Condition (3.5) laid out in Lemma 3, I compute the number of additional panels for different (s, B) for the exemplary constant-elasticity demand function $Q(p) = p^{-1/2}$ following Corollary

2. Figure 3.C.2 illustrates the number of additional plug-PV systems for different combinations of s and B for the exemplary constant-elasticity demand function. Figure 3.C.2 follows in its structure Figure 3.C.1 (see Section 3.C.2).

As Figure 3.C.2 shows, the effects of an increase of B on the number of additional plug-PV systems for the exemplary constant-elasticity demand function are the same as for log-concave demand functions discussed in Section 3.C.2. The main difference to the log-concave demand lies in the effect of an increase in s in case the budget is already fully used (i.e., illustrated in Figure 3.C.2 by the area below the line $B = Q(p - s)s$). In this case, an increase in s leads to an increase in $\tau(s, B)$. Moreover, the slope of the contour lines becomes stepper for larger combinations of (s, B), meaning that increasing s leads to a stronger increase in $\tau(s, B)$ given a fixed B, when there is already a higher subsidy. Intuitively, the origin for this increase in the steepness lies in the strong convexity of the demand function with an increasing slope coefficient for larger prices (see Corollary 1). In other words, for smaller effective prices, i.e., for larger subsidies, a decrease in the price leads to a much stronger increase in demand than for larger prices, i.e., for smaller subsidies. The strong increase of demand for larger subsidies in case the subsidy is marginally increased leads to a strong Composition Effect, thereby leading ultimately to a strong increase in the number of additional panels for larger subsidy levels.

3.D Proofs and Derivations

3.D.1 Characterization of Additional Plug-PV Systems

To characterize $\tau(s, B)$, we need to distinguish between two cases:
Case 1: Assume the budget is not sufficient to satisfy all requested subsidies for the subsidy rate s, i.e. $B < sQ(p-s)$. In total, given the subsidy s, $Q(p-s)$ citizens register with the municipality to receive the subsidy. The number of subsidies available is equal to B/s. Assuming a fair lottery allocation by the municipality, the probability to receive the subsidy after registering with the municipality, denoted as π, is given by:

$$\pi = \frac{B/s}{Q(p-s)} = \frac{B}{sQ(p-s)}$$

Given this probability, the number of additional plug-PV systems is given by:

$$\begin{aligned}\tau(s,B) &= \underbrace{Q(p)}_{\text{Demand from Always Buyers}} + \pi\Big[\underbrace{Q(p-s) - Q(p)}_{\text{Demand from Conditional Buyers}}\Big] - Q(p) \\ &= \frac{B}{s}\left(\frac{Q(p-s) - Q(p)}{Q(p-s)}\right)\end{aligned}$$

Case 2: Assume the budget is sufficient to satisfy all requested subsidies for the subsidy rate s, i.e. $B \geq sQ(p-s)$. In total, given the subsidy s, $Q(p-s)$ citizens register with the municipality, receive the subsidy and install a plug-PV system. Thus, for $B \geq sQ(p-s)$:

$$\tau(S, B) = Q(p-s) - Q(p)$$

3.D.2 Proof of Lemma 1

Proof. To characterize $\partial\tau(s, B)/\partial B$, we need to distinguish two cases:
Case 1: For $B \geq sQ(p-s)$, it readily follows that:

$$\left.\frac{\partial\tau(s,B)}{\partial B}\right|_{B\geq sQ(p-s)} = 0$$

Case 2: For $B < sQ(p-s)$ it readily follows that:

$$\left.\frac{\partial\tau(s,B)}{\partial B}\right|_{B<sQ(p-s)} = \frac{1}{s}\left(\frac{Q(p-s)-Q(p)}{Q(p-s)}\right) > 0$$

where the inequality follows from $p-s < p$ and Assumption 1. Thus, $\partial\tau(s,B)/\partial B \geq 0$.

Similarly, to characterize $\partial\tau(s,B)/\partial s$, given Assumptions 1 and 2, we need to distinguish two cases:
Case 1: For $B \geq sQ(p-s)$, it readily follows that:

$$\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B\leq sQ(p-s)} = -Q'(p-s) > 0$$

The inequality directly follows from Assumption 1.

Case 2: To establish the sign of the partial derivative given $B < sQ(p-s)$, first note that by the Mean Value Theorem[65] (De la Fuente, 2000, p. 159) there exists some $x \in (p-s, p)$ such that:

$$Q'(x) = \frac{Q(p)-Q(p-s)}{p-(p-s)} = \frac{Q(p)-Q(p-s)}{s} \tag{3.6}$$

Second, for $B < sQ(p-s)$ it follows:

$$\begin{aligned}
\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B<sQ(p-s)} &= B\frac{-Q'(p-s)sQ(p-s) - \big[Q(p-s) - sQ'(p-s)\big]\big[Q(p-s)-Q(p)\big]}{\big(sQ(p-s)\big)^2} \\
&= B\frac{-Q(p-s)\big[Q(p-s)-Q(p)\big] - sQ'(p-s)\big[Q(p-s) - \big(Q(p-s)-Q(p)\big)\big]}{\big(sQ(p-s)\big)^2} \\
&= B\frac{sQ(p)\big[-Q'(p-s)\big] - Q(p-s)\big[Q(p-s)-Q(p)\big]}{\big(sQ(p-s)\big)^2}
\end{aligned}$$

[65] The Mean Value Theorem can be applied because $Q(\cdot)$ is continuously differentiable and $p-s < p$.

Thus, as the denominator of the last fraction is always positive as well as $B > 0$, it follows that there exists some $x \in (p-s, p)$:

$$\begin{aligned}
\left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B<sQ(p-s)} < 0 &\iff -Q'(p-s)sQ(p) - Q(p-s)\Big[Q(p-s) - Q(p)\Big] < 0 \\
&\iff Q(p-s)\Big[Q(p) - Q(p-s)\Big] < Q'(p-s)sQ(p) \\
&\iff \frac{Q(p-s)\Big[Q(p) - Q(p-s)\Big]}{sQ(p-s)} < \frac{Q'(p-s)sQ(p)}{sQ(p-s)} \\
&\iff \underbrace{\frac{Q(p) - Q(p-s)}{s}}_{=Q'(x) \text{ by } (3.6)} < Q(p)\frac{Q'(p-s)}{Q(p-s)} \\
&\iff \frac{Q'(x)}{Q(p)} < \frac{Q'(p-s)}{Q(p-s)}
\end{aligned}$$

Now note that since $Q'(z) < 0$ for all $z \in \mathbb{R}_+$ by Assumption 1, it follows that $Q'(x) < 0$ and also $Q(p) < Q(x)$. Therefore, $\frac{Q'(x)}{Q(p)} < \frac{Q'(x)}{Q(x)}$. Thus, $\frac{Q'(x)}{Q(x)} < \frac{Q'(p-s)}{Q(p-s)}$ is a sufficient condition for $\frac{Q'(x)}{Q(p)} < \frac{Q'(p-s)}{Q(p-s)}$. Moreover note that, since $x > p-s$, the condition $\frac{Q'(x)}{Q(x)} < \frac{Q'(p-s)}{Q(p-s)}$ for any $p \in \mathbb{R}_+$ and any $0 < s < p$ and any $x \in (p-s, p)$ is equivalent to $\frac{Q'(z)}{Q(z)}$ being decreasing in z for $z \in \mathbb{R}_+$. To summarize, we have that:

$$\begin{aligned}
\frac{Q'(z)}{Q(z)} \text{ decreasing in } z \text{ for any } z \in \mathbb{R}_+ &\iff \frac{Q'(x)}{Q(x)} < \frac{Q'(p-s)}{Q(p-s)} \\
&\implies \frac{Q'(x)}{Q(p)} < \frac{Q'(p-s)}{Q(p-s)} \\
&\iff \left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B<sQ(p-s)} < 0
\end{aligned}$$

By Assumption 2, $Q(\cdot)$ is log-concave. As Kang and Vasserman (2025) show, log-concavity of $Q(\cdot)$ is equivalent to $\frac{Q'(z)}{Q(z)}$ to be decreasing in z for any $z \in \mathbb{R}_+$. Thus, Assumption 2 is sufficient for $\left.\frac{\partial \tau(s,B)}{\partial s}\right|_{B<sQ(p-s)} < 0$ □

3.D.3 Proof of Proposition 2

Proof. To establish the existence and uniqueness of s^*, one needs to distinguish two cases regarding $\lim_{s\to p} g(s)$:

Case 1: If $\lim_{s\to p} sQ(p-s) \leq B$ note that $s^* = p > 0$ if by Proposition 1, which exists, is unique and strictly positive.

Case 2: Suppose $\lim_{s \to p} sQ(p-s) > B$. Define $g(s) = Q(p-s)s$. Note that $g(0) = 0$ and $\lim_{s \to p} g(s) > B$. Also since $Q(\cdot)$ is a continuous function, $g(\cdot)$ is a continuous function. Thus by the Intermediate Value Theorem (De la Fuente, 2000, p. 219), there exists an s^* such that $B = Q(p-s^*)s^*$. Also, since $B > 0$ and $g(0) = 0$, $s^* > 0$.
Furthermore, because $Q > 0$ and $Q' < 0$, it follows that $g'(s) = Q(p-s) - Q'(p-s)s > 0$. Thus, because $g(\cdot)$ is strictly increasing on $(0,p)$, s^* is unique. □

3.D.4 Proof of Lemma 2

Proof. The proof is made separately for two cases:
Case 1: Suppose $\lim_{s \to p} sQ(p-s) \leq B$. Then, given Assumptions 1 and 2, $s^* = p$ by Proposition 2. Thus, it immediately follows that $\frac{ds^*}{dp} = 1$ and $\frac{ds^*}{dB} = 0$.
Case 2: Suppose $\lim_{s \to p} sQ(p-s) > B$. Define $H := B - Q(p-s)s$. Observe that given B, $H(s^*, B) = 0$ by Propositions 1 and 2 given Assumptions 1 and 2. Also, note that H is continuously differentiable because Q is continuously differentiable by Assumption 1. Then by the Implicit Function Theorem (De la Fuente, 2000, p. 207), it follows:

$$\frac{ds^*}{dB} = -\frac{\partial H / \partial B}{\partial H / \partial s} = \frac{1}{Q(p-s) - Q'(p-s)s} > 0$$

Where the last inequality follows from $Q > 0$ and $Q' < 0$, following Assumption 1. Moreover, by the Implicit Function Theorem (De la Fuente, 2000, p. 207), it further follows:

$$\begin{aligned}
\frac{ds^*}{dp} = -\frac{\partial H / \partial p}{\partial H / \partial s} &= -\frac{-Q'(p-s)s}{-\big(-Q'(p-s)s + Q(p-s)\big)} \\
&= \frac{\big[Q'(p-s)s\big](-1)}{\big[Q'(p-s) - Q(p-s)\big](-1)} \\
&= \frac{-Q'(p-s)s}{Q(p-s) - Q'(p-s)s} > 0
\end{aligned}$$

Also note that $\frac{ds^*}{dp} \leq 1$, because:

$$\frac{-Q'(p-s)s}{Q(p-s) - Q'(p-s)s} \leq 1 \Longleftrightarrow 0 \leq Q(p-s)$$

Finally, when $Q(p-s) > 0$, it follows $\frac{-Q'(p-s)s}{Q(p-s)-Q'(p-s)s} < 1$. □

3.D.5 Proof of Lemma 3

Proof. By the rearrangements in Appendix 3.D.2 it readily follows that for $B < sQ(p-s)$:

$$\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B<sQ(p-s)} > 0 \iff -Q'(p-s)sQ(p) - Q(p-s)\Big[Q(p-s) - Q(p)\Big] > 0$$
$$\iff \frac{-Q'(p-s)s}{Q(p-s)} > \frac{Q(p-s)-Q(p)}{Q(p)}$$

□

3.D.6 Proof of Corollary 1

Proof. By Assumption 1, $Q(\cdot)$ is twice continuously differentiable. Then, following the Taylor Polynomial (Simon and Blume, 1994, p. 828), there exists a $x \in (p-s, p)$ such that:

$$Q(p) = Q(p-s) + Q'(p-s)s + \frac{1}{2}Q''(x)s^2$$
$$\Leftrightarrow Q(p-s) - Q(p) = -Q'(p-s)s - \frac{1}{2}Q''(x)s^2$$

Using the equality in the last line to replace $Q(p-s) - Q(p)$ in Condition (3.5), it follows:

$$\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B<Q(p-s)s} > 0 \iff \frac{-Q'(p-s)s}{Q(p-s)} > \frac{-Q'(p-s)s - \frac{1}{2}Q''(x)s^2}{Q(p)}$$
$$\iff \frac{1}{2}Q''(x)s > -Q'(p-s)\left[1 - \frac{Q(p)}{Q(p-s)}\right]$$
$$\iff Q''(x) > \frac{-Q'(p-s)2}{s}\left(\frac{Q(p-s)-Q(p)}{Q(p-s)}\right)$$

Note that the term on the right of the inequality sign is strictly positive, because $Q(p-s)-Q(p)$ is strictly positive by Assumption 1 and $p > s$; and by Assumption 1, also $-Q'(\cdot) > 0$. Moreover, because there exists an $x \in (p-s, p)$ such that the last inequality is equivalent for $\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B<Q(p-s)s} > 0$, it follows that if the last inequality holds for any $x \in [p-s, p]$, it follows $\left.\frac{\partial\tau(s,B)}{\partial s}\right|_{B<Q(p-s)s} > 0$. □

3.D.7 Proof of Corollary 2

Proof. For the function $Q(p) = p^{-b}$ it follows $Q'(p) = -bp^{-b-1}$. Thus, Condition (3.5) provided in Lemma 3 for $Q(p) = p^{-b}$ reads as:

$$\begin{aligned} \frac{-\left(-b\,(p-s)^{-b-1}\right)s}{(p-s)^{-b}} &> \frac{(p-s)^{-b} - p^{-b}}{p^{-b}} \\ \iff \frac{bs}{p-s} &> \left(\frac{p}{p-s}\right)^{b} - 1 \end{aligned}$$

By defining $x := \frac{p}{p-s}$ it follows that $p - s = \frac{p}{x}$ and $s = p\frac{x-1}{x}$. Observe that $x > 1$ because $p > s > 0$. Using these expressions for $p - s$ and s in terms of x in the last line of the previous inequality, it follows that Condition (3.5) provided in Lemma 3 for $Q(p) = p^{-b}$ reads as:

$$\begin{aligned} \frac{bp\frac{x-1}{x}}{\frac{p}{x}} &> x^{b} - 1 \\ \iff \underbrace{b(x-1) - x^{b} + 1}_{=:g(x)} &> 0 \end{aligned}$$

Thus, Condition (3.5) provided in Lemma 3 for $Q(p) = p^{-b}$ holds if and only if $g(x) > 0$ for $x \in (1, \infty)$. In fact, $g(1) = 1 - 1^{b} = 0$. Moreover, $g'(x) = b(1 - x^{b-1})$ and because $x > 1$ and $b \in (0, 1)$ it readily follows that $x^{b-1} < 1$ and thus $g'(x) > 0$ for $x \in (1, \infty)$. Thus, by $g(1) = 0$ and $g(\cdot)$ being strictly increasing on $(1, \infty)$ it follows that $g(x) > 0$ for $b \in (0, 1)$ and $x > 1$ which means that Condition (3.5) provided in Lemma 3 for $Q(p) = p^{-b}$ holds. □

3.D.8 Differentiability of Additional Systems

Observe that for $\lim_{s\to p} sQ(p-s) \leq B$ it follows from Appendix 3.D.4:

$$\begin{aligned} \lim_{s\to(s^*)^-} \frac{\partial\tau(s,B)}{\partial s} &= -Q'(p-s) \\ &\neq \frac{sQ(p)\big[-Q'(p-s)\big] - Q(p-s)\big[Q(p-s) - Q(p)\big]}{\big(sQ(p-s)\big)^{2}} = \lim_{s\to(s^*)^+} \frac{\partial\tau(s,B)}{\partial s} \end{aligned}$$

Thus $\tau(s, B)$ is not continuously differentiable.

3.E Variable Definitions

The characteristics of the subsidy programs, included in vector $\mathbf{X}_i^P$ are given by:

- *Subsidy value*: Calculated subsidy value following the procedure described in Section 3.3.3.
- *Budget*: Budget in Euro the municipality provided throughout the whole duration of the subsidy program for the subsidies.
- *Program duration*: Duration of the program in days, where the start day is the day on which the program was first discussed in the council and the end day is the effective end day of the program, which is either the legally set end of the subsidy program or the day on which the budget was fully used.
- *Combined budget*: Indicator variable (0/1), which is equal to one if the subsidy program was implemented with other subsidy programs for other products at the same time, and shared a combined budget. Note that even if there is a combined budget, the *Budget* variable accounts for that by solely accounting for the budget that was used for the plug-PV subsidy program.
- *Council vote share*: Share of members of the municipal council who voted for the subsidy program, where abstentions are not counted.
- *MaStR obligation*: Indicator variable (0/1), which is equal to one if the funding guidelines of the respective subsidy program specified that the payout for the subsidy can only be made once the citizen provides a document confirming that the plug-PV system was registered within the MaStR registry.
- *Receivers restricted*: Indicator variable (0/1), which is equal to one if the subsidy program was not available to all citizens within a municipality, but only to individuals that had an income below some specified threshold.
- *Income differences*: Indicator variable (0/1), which is equal to one if the subsidy value differs depending on the income of individuals.

The characteristics of the municipalities, included in vector $\mathbf{X}_i^C$ are given by:

- *Tax per capita*: Total tax revenue of the municipality per capita in Euro. Taxes of municipalities include: property tax for land, business tax, municipal share of income tax, and municipal share of sales tax.
- *Income tax per capita*: Income tax revenue per capita in Euro (municipal share of total income taxation).
- *Population density*: Population per hectare.
- *Population*: Population (count).
- *Average age*: Average age of the population.
- *Solar potential*: Average yearly global radiation measured in kWh per square meter.

3.F Miscellaneous

3.F.1 Message Sent to Municipalities

German Original Email:

Sehr geehrte Damen und Herren,
ich bin Doktorand an der Universität Mannheim im Fachbereich Volkswirtschaftslehre und schreibe Ihnen, weil wir im Rahmen eines Forschungsprojektes untersuchen möchten, welche Förderungen für Photovoltaikanlagen in Deutschland auf kommunaler Ebene bestehen und bestanden.
Einige Kommunen in Deutschland haben eigene Förderprogramme für sogenannte "Balkonkraftwerke" bzw. "Balkonsolaranlagen" (manchmal auch "Steckersolaranlage" oder "Steckerfertige PV-Anlage" genannt). Dabei handelt es sich um Programme, bei denen Mieter oder Hausbesitzer eine finanzielle Förderung von ihrer Kommune erhalten, wenn sie eine kleine (maximal 800 Wp) Photovoltaik-Anlage kaufen und installieren.
Unsere Frage an Sie ist deshalb: Haben Sie oder hatten Sie in Ihrer Kommune ein eigenes Förderprogramm für kleine Photovoltaikanlagen (sogenannte Balkonsolaranlagen oder auch Stecker-PV-Anlagen)? Falls Sie ein solches Förderprogramm nicht haben und bisher nicht hatten, würde eine sehr kurze Antwort auf diese Mail für unsere Forschungszwecke ausreichen. Falls Sie in Ihrer Kommune eine solche Förderung haben oder hatten, würden wir uns sehr freuen, wenn Sie uns weitere Informationen zu dem Förderprogramm in Ihrer Kommune senden könnten.
Wir fragen diese Informationen von mehreren Kommunen in Deutschland ab, um die Wirkung dieser Förderung in einem bundesweiten Forschungsprojekt evaluieren zu können. Sollten Sie Rückfragen zu dem Forschungsprojekt oder dieser Mail haben, können Sie sich jederzeit unter [MAIL] bzw. telefonisch unter [NUMMER] oder [NUMMER] an mich wenden.
Herzlichen Dank für Ihre Antwort, die für die Forschung sehr wichtig ist!
Mit freundlichen Grüßen, David Müller

Translation of Email:

Dear Sir or Madam,
I am a PhD student at the University of Mannheim at the Department of Economics and I am writing to you because, as part of a research project, we would like to investigate which subsidies exist and have existed for photovoltaic systems in Germany at municipal level.
Some municipalities in Germany have their own funding programs for so-called “balcony power plants” or “balcony solar systems” (sometimes also called “plug-in solar systems” or “plug-in PV systems”). These are programs in which tenants or homeowners receive financial support from their municipality if they purchase and install a small (maximum 800 Wp) photovoltaic system.
Our question to you is therefore: Do you or did you ever have own funding program for small photovoltaic systems (so-called balcony solar systems or plug-in PV systems) in your municipality? If you do not have and have not had such a funding program, a very short answer to this email would be sufficient for our research purposes. If you have or have had such a funding program in your municipality, we would be very pleased if you could send us more information about the funding program in your municipality.
We are requesting this information from several municipalities in Germany in order to be able to evaluate the impact of this funding in a nationwide research project. If you have any questions about the research project or this email, please feel free to contact me at [MAIL] or by phone at [NUMBER] or [NUMBER].
Thank you very much for your response, which is very important for the research!
Yours sincerely, David Müller

3.F.2 Message Sent to Counties

German Original Email:

Sehr geehrte Damen und Herren,
ich bin Doktorand an der Universität Mannheim im Fachbereich Volkswirtschaftslehre. Derzeit erforschen wir in einem Projekt, welche Förderungen für Mini-Photovoltaikanlagen (manchmal auch Balkonkraftwerke/Balkonsolaranlagen oder Steckersolaranlagen genannt) in Deutschland bestehen und welche Wirkung diese Programme haben.
Unsere Frage an Sie ist deshalb: Haben Sie oder hatten Sie im Landkreis [LANDKREISNAME] ein eigenes finanzielles Förderprogramm für kleine Photovoltaikanlagen (sog. Balkonkraftwerke/Balkonsolaranlagen oder Steckersolaranlagen)?
Falls Sie ein solches Förderprogramm nicht haben und bisher nicht hatten, würde eine sehr kurze Antwort ("Nein") auf diese Mail für unsere Forschungszwecke ausreichen. Falls Sie im Landkreis [LANDKREISNAME] eine solche Förderung haben oder hatten, würden wir uns sehr freuen, wenn Sie uns – falls vorhanden – die dazugehörige Förderrichtlinie und/oder Antragsformulare für die entsprechenden Förderprogramme per Mail zukommen lassen könnten.
Wir fragen diese Informationen von mehreren Kreisen und Kommunen in Deutschland ab, um die Wirkung dieser Förderung in einem bundesweiten Forschungsprojekt evaluieren zu können. Sollten Sie Rückfragen zu dem Forschungsprojekt haben, können Sie sich jederzeit unter [MAIL] bzw. telefonisch unter [NUMMER] an mich wenden.
Herzlichen Dank für Ihre Antwort, die für die Forschung sehr wichtig ist!
Mit freundlichen Grüßen, David Müller

Translation of Email:

Dear Sir or Madam,
I am a PhD student at the University of Mannheim at the Department of Economics. We are currently researching in a project which subsidies exist for mini-photovoltaic systems (sometimes also called balcony power plants/balcony solar systems or plug-in solar systems) in Germany and what effect these programs have.
Our question to you is therefore: Do you or did you have your own financial support program for small photovoltaic systems (so-called balcony power plants/balcony solar systems or plug-in solar systems) in the county [LANDKREISNAME]?
If you do not have and have not had such a funding program, a very short answer ("no") to this mail would be sufficient for our research purposes. If you have or have had such funding in the county [COUNTY NAME], we would be very grateful if you could email us the associated funding guidelines and/or application forms for the relevant funding programs, if available.
We are requesting this information from several counties and municipalities in Germany in order to be able to evaluate the impact of this funding in a nationwide research project. If you have any questions about the research project, you can contact me at any time at [MAIL] or by phone at [NUMBER].
Thank you very much for your response, which is very important for the research!
Yours sincerely, David Müller

3.F.3 Clean Energy Investment Support Programs

Details and sources about clean energy investment programs:

- United States: Section 13701 of the Inflation Reduction Act:

  > "The U.S. Inflation Reduction Act of 2022 creates a tax credit for domestic production of clean electricity with a greenhouse gas emission rate of zero (or lower). Eligible technologies include solar PV and wind."
  > Source:
  > `https://www.iea.org/policies/16283-inflation-reduction-act-2022-sec-13701-clean-electricity-production-credit`
  > (last access: 22.07.2025)

- China: Renewable Energy Electricity Subsidy:

  > "The Chinese government earmarked subsidies for renewable electricity generation (wind, solar and biomass) provided to local public utilities and power generation companies in 2024."
  > Source: `https://www.iea.org/policies/21175-renewable-energy-electricity-subsidy-for-2024` (last access: 22.07.2025)

- Germany: Germany's Special Climate and Transformation Fund

  > "In the framework of the Germany's Special Climate and Transformation Fund (KTF), the German government earmarked financial support for energy transition programmes and measures in the areas of renewable energies, electricity and grids, digitalization and energy infrastructure."
  > Source: `https://www.iea.org/policies/21194-germanys-special-climate-and-transformation-fund-energy-transition-programmes-and-measures-in-the-areas-of-renewable-energies-electricity-and-grids-digitalization-and-energy-infrastructure` (last access: 22.07.2025)

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