Beyond Unemployment: An Investigation of Social Policies to Empower Workers in a Changing World of Work

Lukas Lehner

April 2024

Dissertation

DPhil in Social Policy

Department of Social Policy and Intervention

St Edmund Hall

University of Oxford

To the workers of Marienthal

and to my family

Karin and Thomas

Helma and Herbert

Johann † and Agnes †

Anna †

Thesis Abstract

This dissertation analyzes social policies aimed at supporting unemployed workers, scrutinizing consequences of such policies on facilitating access to training, employment, and in improving wages, health, and well-being amidst a changing world of work. Central to the research are two field experiments at scale (chapters 2 and 3), designed in collaboration with implementation partners, uniquely leveraging high-quality administrative records and extensive survey data collected over three years. Chapter two tests the idea of guaranteed employment in a natural context by carefully evaluating a job guarantee scheme, which highlights the psychosocial value of employment. Social programs of this kind often suffer from incomplete take-up, which is investigated in the third chapter. It demonstrates that low-cost interventions can meaningfully decrease non-participation in job training by reducing associated social stigma. Both evaluations are based on combinations of experimental and quasi-experimental methods to separate out direct effects, anticipation effects, and spillover effects, and to understand underlying mechanisms. The field experiments are implemented at scale in a natural context in Austria and involve a €7.4 million expensive program for the second chapter and an intervention involving over 10,000 job seekers for the third chapter. The fourth chapter takes a step further to examine the consequences of temporary jobs, which often constitute job seekers' only option for re-employment. The analysis explores the implications of temporary employment on workers' wages and scrutinizes the interplay with labor market institutions across 30 high-income countries, revealing negative spillover effect on permanent workers' wages. The fifth chapter goes full circle by uncovering unemployed workers' preferences between guaranteed employment and guaranteed income, revealing a strong correlation in support for both concepts with higher support for guaranteed employment. The dissertation is further complemented with a comparative viewpoint published in several complimentary papers and founded on the Oxford Supertracker—a global directory of policy trackers and surveys to document responses to the Covid-19 pandemic across different areas, countries, and data types. At the methodological level, the dissertation propels the empirical turn in social research by widening access to novel data sources and enhancing the application of empirical methods for causal and comparative social policy analysis. At the substantive level, the dissertation contributes by creating robust evidence on innovative social policy ideas to foster a comprehensive understanding of social policies devised for unemployed workers in a changing world of work.

Contents overview

Contents

List of Tables

List of Figures

Preface

"Everyone has the right to work, to free choice of employment, to just and favourable conditions of work and to protection against unemployment."

—Article 23, Universal Declaration of Human Rights
United Nations General Assembly, Paris, 1948

This principle, although universally acknowledged, echoed through my childhood home in a manner most personal and profound. Growing up in a household that spanned four generations under a single roof, I enjoyed the privilege of learning from oral history and lived experience. My paternal great-grandmother, born in 1918 in an Austrian village, was a living testament. Her experiences of poverty and political repression inspired the lunch table conversations. Her life's journey was marked by her father's tragic fate. Her father—a mining worker—was tortured to death in 1934 as a political opponent of the totalitarian regime.[1] As a consequence, she could not afford to continue attending school but instead had to work in the field, picking potatoes—at the same age I was when I sat next to her as a teenager.

My maternal grandparents lived just down the road, in the house just next door where these tragic events had unfolded. My grandpa's earliest memory, at five years old, was rushing to the coal mine outside our village to see his father's lifeless body—electrocuted at a work accident in the local coal mine. Occupational safety was still in its infancy at the time. From that day, my grandpa was raised in deep poverty by his grandmother, alongside his siblings and cousins. His mother, overwhelmed by the inability to provide for her family, suffered from depression. In 1942, she was

[1]The very men who had beaten him to death with a bull pizzle, a kind of whip made out of a bull's penis, were never prosecuted but carried the Canopy of Heaven, a religious artifact, at Corpus Christi processions.

deported by the Nazis to a euthanasia center, masked as a medical facility. Seven months later, her children received a letter claiming their mother had succumbed to pneumonia, leaving them orphaned. Meanwhile, my grandpa, oblivious to his mother's murder, found what he said felt like a real "family" in the Hitler Youth (HJ), which prepared him to fight in war. Having gone through dreadful experiences of mass unemployment and poverty that resulted in the horrors of fascism and WWII, he carried with him a lifelong conviction: fighting unemployment (but not people) must be the foremost priority of every politician.[2]

Eradicating unemployment emerged not merely as my grandpa's personal takeaway from that era but as a collective lesson that shaped policies in the decades to follow. The right to employment, quoted above, was even enshrined in the Universal Declaration of Human Rights after WWII. Despite adoption at the UN General Assembly in 1948 by countries around the world, it remains a non-enforceable right.

As a steady reminder, the Universal Declaration of Human Rights is always on my desk, a practice adopted from the idol of my adolescence, Jean Ziegler, at the time United Nations Special Rapporteur on the Right to Food. His assertion "the more conservative your appearance, the more radical your ideas" taken seriously has inspired my pathway. This dissertation seeks to honor that principle by employing advanced causal inference methods to meticulously examine radical concepts in social policy.

A decade later, my path fortuitously crossed with Ziegler's when I worked at the ILO in Geneva, an experience that shaped my understanding of labor rights.[3]

[2] While what followed my family's trajectory is emblematic of post-WWII social upward mobility, these events have left a lasting mark. Social upward mobility, achieved collectively yet not universally, was propelled by educational attainment and public sector employment, underpinned by progressive welfare state expansion. And yet, the early experiences persist in guiding my thoughts and moral compass to this day, serving as the foundation this dissertation is built upon.

[3] Inspiring encounters with UN Special Rapporteurs should continue. I was fortunate to interact with Olivier de Schutter, UN Special Rapporteur for Extreme Poverty and Human Rights, when he

Together with my work for the OECD Chief Economist, the experience prepared me for this PhD. The ILO's foundational ethos, encapsulated in its 1944 Declaration of Philadelphia, echoed my family's experiences. Its foremost principles declare:

- *(a) Labour is not a commodity;*
- *(b) Freedom of expression and of association are essential to sustained progress;*
- *(c) Poverty anywhere constitutes a danger to prosperity everywhere;*
- *(d) The war against want requires to be carried on with unrelenting vigour... to the promotion of the common welfare.*

The devastating consequences of disregarding these principles have been studied long before. During my time at MIT, I had the fortune to sit next to Lotte Baylin's office. Lotte, Professor Emerita, was born in 1931 in Vienna as the child of Paul Lazarsfeld and Marie Jahoda shortly before they wrote "Marienthal: The Sociography of an Unemployed Community". Today a classic in social research, the study documents the devastating economic and social consequences of mass unemployment. It is the inspiration for Chapter 2 of this dissertation, which returns to the same site to investigate the opposite: what happens when unemployed workers receive guaranteed employment?[4] Lotte recounted how her father once referred to himself during a lecture as a "socialist on leave." This prompted the response from someone in the audience: "Who gave you leave?" The principle of no leave equally applies to the creation of knowledge through academic work—for better or worse.[5]

decided to write his 2023 annual report to the UN Human Rights Council on the Job Guarantee.

[4]While my family members, poor workers, were stuck with fascism, Lotte, of a Jewish family, had to flee. A refugee child, she was able to pursue an academic career in the United States, being appointed the first woman faculty member at MIT Sloan.

[5]Another lesson learned through my PhD journey is individuals possess far greater resilience than they think they do.

Acknowledgements

This dissertation was written in five countries, at five universities, and at ten residences. Living such a transient life gave me the opportunity to meet numerous people from whom this dissertation has benefited.

First and foremost, I would like to thank Bernhard Ebbinghaus and Brian Nolan, who have superbly supervised my dissertation and provided mentorship far beyond. Bernhard saw the potential of a single tweet and made us launch the Oxford Supertracker: The Global Directory for COVID Policy Trackers and Surveys, which he supported enthusiastically to develop into a departmental project. This resulted in co-authoring three papers on top of my dissertation, which was an invaluable educational experience for developing my academic skills and knowledge. Brian included me right from the start in his team at the Institute of New Economic Thinking (INET) at the Oxford Martin School, which should become an important source of support for the job guarantee research project. He was always readily available to offer detailed and insightful comments on my draft papers and projects as they progressed. Both also supported me in pursuing the entrepreneurial, and at times risky, endeavors that allowed me to work on the Supertracker, the job guarantee study, and the reframing active Labor market policy field experiment.

I would also like to thank Simon Jäger, Aaron Reeves, and Tim Vlandas, who did me the honor of serving as examiners on my dissertation committee. Their insightful comments and perspectives on the papers included in this dissertation have been invaluable in helping me navigate among multiple traditional academic disciplines. They have provided new perspectives for shaping and refining my research. Beyond what can be expected, they have offered their mentorship navigating strategic choices and career paths throughout my doctoral journey, which was invaluable for navigating the early stages of academia.

I would also like to thank the other members of the department, who have not only provided important advice and guidance, but have acted as my role models at various stages. I am grateful to Marek Naczyk and Mary Daly, who worked with me on the Supertracker; to Rossella Ciccia, who nurtured me to acquire my first teaching experience, which I was able to develop further by teaching quantitative methods with Mark Fransham, whose pedagogical approaches inspired some of my own teachings. I would also like to thank Selçuk Bedük, Jane Gingrich, and Kenneth Nelson, who were full of support for my research after having joined the department while I was abroad wrapping up my dissertation.

In my journey, I have been lucky to encounter mentors whose influence on my intellectual and professional path has been immeasurable. Since my first PhD year, Maximilian Kasy took me under his wing regardless of whether we met in Oxford, Vienna, or Boston. His mentorship through our collaboration on the job guarantee study was formative for my journey. I am grateful for the invaluable lessons offered both on an academic and personal level. Zachary Parolin has provided unwavering support since I met him during my PhD. His approach to academic research has been nothing short of inspirational, and I have benefitted immensely in my approach to research by collaborating with him at Bocconi University, where we have worked on co-authoring several articles. Nathan Wilmers played a pivotal role in shaping my doctoral journey. He welcomed me to MIT, where he devoted considerable time to providing insightful advice on my research and trajectory. I consider myself fortunate to have had the opportunity to co-author with him recently. My time at MIT also allowed me to benefit from the advice of Anna Stansbury, who has long been a source of inspiration for my work. At UC Berkeley, my gratitude extends to Emmanuel Saez, who not only permitted me to study for a portion of my PhD there but also offered invaluable guidance and advice. Learning from him and the broader academic community there has been an enriching experience. In particular, I owe a great deal of appreciation to Jesse Rothstein for his support, advice, and constructive feedback. Lawrence Kahn, in his role as editor of the ILR Review, provided exceptionally insightful comments that significantly contributed to the advancement of Chapter 4 on wage growth. A source of strength came from the always warm welcome at WU Vienna, where I benefited from the unbound support of Wilfried Altzinger, Jürgen Essletzbichler, Karin Heitzmann, and Mathias Moser. Thanks also go to the researchers and policymakers from other institutions who advised and inspired me during my formative years, including Markus Marterbauer, Pirmin Fessler, Matthias Schnetzer, Martin Schürz, Tobias Schweitzer, and Sepp Zuckerstätter. An unforeseen yet profoundly impactful experience late in my dissertation journey was the opportunity to meet Chris Featherman at MIT, who taught me invaluable strategies for refining and articulating my ideas effectively and acted as an important sparring partner. Looking ahead, I am thrilled about the opportunity to join the University of Edinburgh and its Social Policy faculty including, Daniel Clegg, Elke Heins, Philip Rathgeb, and many others.

In the realm of co-authors and collaborators, my gratitude is vast. Anna Schwarz, who at the time was a PhD candidate at the Vienna University of Economics and Business (WU Vienna), deserves immense thanks for her exceptional efforts in advancing our “Reframing Active Labor Market Policy” field experiment. Her dedication has enabled us to reach over 50,000 job seekers to date with the follow-up research project. Paul Ramskolger was instrumental in starting our collaboration with Aleksandra Riedl at the Central Bank of Austria. Working alongside both has been incredibly educational and enriching for me. Beyond the confines of this dissertation, I have had the fortune of working and learning from Ishaana Talesara and Arthur Wickard at MIT through our ongoing collaboration on employment risk misperceptions. Elias Naumann at GESIS and the University of Mannheim, whom I met through our collaboration on the Supertracker, has been a supportive mentor and source of ideas from which I have greatly benefited, a collaboration that has led to a co-authored article on attitudes toward social policies. Emanuel List's unwavering support has been key in starting our joint trade union research project and providing a home for me at WU Vienna and its Research Institute Economics of Inequality (INEQ), which has offered a nurturing environment for our collaborative efforts. Through the INEQ community, I have had the opportunity to collaborate with Franziska Disslbacher and Severin Rapp, returning to our roots from over a decade ago when we began our studies together. INET Oxford played a pivotal role in launching the job guarantee project thanks to the outstanding efforts of Jessica Kaplan and the immediate support provided by Eric Beinhocker. Cindy Cheng and Luca Messerschmidt have led the COVID-19 PHSM Data Coverage Conferences, which resulted in a global network and a co-authored article. Lastly, Maximilian Trenkmann has worked closely with me to build the bridge between academic research and computer science. Our work together started with the foundation of the Supertracker and has led to co-teaching a course to strengthen the transparency and reproducibility of academic research.

The research in this dissertation has a real-world focus and would not have been possible without several collaborators and supporters in non-academic institutions. First and foremost, I would like to thank Sven Hergovich, who, in his former position as the managing director of the public employment service (Arbeitsmarktservice *AMS* Niederösterreich), has become an archetype of a policy entrepreneur in taking bold and innovative steps to build the capacity to strengthen knowledge exchange between academia and government institutions to promote evidence-based policymaking. Daniel Riegler had the challenge of aligning ideas and reality to coordinate our research support, which he excelled at working under Karmen Frena's supportive leadership to make this research possible. I would also like to recognize the contribution of the staff of the public employment service and it.works who have worked hard to support the collaboration.

My gratitude is also directed toward Barbara Blaha and Leonhard Dobusch at Momentum Institut, whose initiation of a large-scale survey laid the groundwork for my collaboration with Daniel Schönherr at SORA. This partnership was instrumental in conducting the research that eventually culminated in Chapter 5. From the outset of my doctoral studies, I had the privilege of being invited to join the editorial team of Momentum Quarterly, which allowed me to learn and collaborate closely with Leonhard as well as Astrid Mager, Stefanie Wöhl, and Quirin Dammerer. In particular, Dennis Tamesberger has been helpful, diligently guiding me through the nuances of editorial work. His early and consistent support of my research endeavors has been invaluable, and I am delighted that our collaboration has extended to a co-authored handbook chapter on labor market policy.

At the five institutions where this Ph.D. was written, I have been fortunate to work alongside inspiring colleagues, many of whom have transitioned from peers to friends. Oxford's Social Policy community is especially noteworthy for its unparalleled cheerfulness. My heartfelt appreciation goes out to Emefa Agawu, Leo Azzollini, Ali Bargu, Enrico Benassi, Bastian Betthäuser, Alexandra Blackwell, Rafael Carranza, Jakob Dirksen, Benjamin Ferschli, Michael Ganslmeier, Tim Goedemé, Benjamin Goodair, Alexander Haas, Matthias Haslberger, Solhee Han, Joe Hasell, Christoph Henking, Leonie Hoffmann, Caspar Kaiser, Kun Lee, Alistair Leitch, Nontokozo Langwenya, Linus Mattauch, Adrienne McManus, Kathleen Murphy, Lea Pfeffermann-Goehl, Hannah Phillips, Ertuğrul Polat, Marii Paskov, Steffen Redeker, Cherise Regier, Moa Schafer, François Schoenberger, Bård Smedsvik, Nhat An Trinh, Roland Tusz, David Weisstanner, and Rocco Zizzamia. At MIT, the Institute for Work and Employment Research (IWER) community has been another source of inspiration and cheerfulness, including Per Engzell, Arrow Minster, Dylan Nelson, Patrick Nüß, Soohyun Roh, MacKenzie Scott, Di Tong, George Ward, and Victoria Zhang, soon to be complemented by Alexander Busch. The inspiring environment at Berkeley introduced me to remarkable colleagues, including Lukas Delgado-Prieto, Amory Gethin, Sebastian Hager, Ségal Le Guern Herry, Toni Juuti, Alice Lapeyre, Wouter Leenders, Jakob Miethe, Petra Orešković, Jon Piqueras, and Justin Wiltshire. These places are interconnected, as exemplified by Jan Gromadzki, whom I had the fortune of meeting first at Berkeley before his transition to WU Vienna's INEQ, where we have started to collaborate. My time in Vienna has greatly benefited from the cheerful community including the interactions and exchanges with Sarah Beran, Lorenz Bodner, Judith Derndorfer, Michael Ertl, Moritz Hörl, Alexander Huber, Vanessa Lechinger, Klaudia Marschalek, Patrick Mokre, Petra Sauer, Eva Six, Matthias Stehle, Jakob Sturn, Lea Steininger, Christoph Walch, Philipp Wimmer, and Daniel Witzani-Haim. Joining Bocconi University during the final stretches of my PhD, I further benefited from collaborations and interactions with Maximilian Böck, Stefano Filauro, Hannah Massenbauer, Clemente Pignatti, Rafael Pintro Schmitt, and Felix Poege. The list would not be complete without mentioning colleagues from various institutions who have become friends over the years, including Valentina Consiglio, Robert Dorschel, Lisa Hanzl, Roberto Iacono, Patrick Kaczmarczyk, Gustav Kalm, Frieder Mitsch, Atanas Pekanov, Manuel Schechtl, Paulus Wagner, and Peter Wyckoff.

Reflecting on these interactions, I realized that the journey through academia has brought me much more than knowledge; it has provided me with bonds that I consider invaluable. Tremendously important are also the friendships formed outside the academy, which offer balance and broader perspectives. My heartfelt appreciation goes particularly to Francesca Bertolino, Martin Giefing, Benedikt Göhmann, Julia Herr, Florian Luckinger, Daniel Posch, Matthias Punz, Christina Tschürtz, and Philipp Tzaferis.

In the preface, I have already reflected upon the significance of my family as a source of identity and orientation. More than that, they have provided me with unconditional love and support. I am deeply grateful to my mom, who has provided me with the aspirations and mindset to achieve anything, and who visits me wherever I go; to my dad, who has provided me with the intellectual power to think critically and who continues to be a sparring partner; and to my grandparents, whose ever cheerful support has provided a constant and stabilizing anchor at home.

Above all, my deepest congratulations go to the workers of Marienthal. Unemployed at the outset of this dissertation, they have since found employment through the job guarantee. Through their work in renovation, public gardening, and social support, they have created public goods that benefit the community at large. But they have achieved even more: They have set an example of international relevance, one that has already inspired social policies across Europe and will continue to serve as inspiration for the future.

Chapter 1

General Introduction

This dissertation examines social policies to support unemployed workers in a changing world of work. I investigate how social policies support unemployed workers to get into training and employment to improve their wages, health, and well-being. I employ diverse empirical strategies, supported by experimental designs for causal inference and comparative approaches for contextual analysis.

Understanding unemployment and the pathways out of it is crucial for contemporary societies, as it directly influences economic prosperity, individual well-being, and social cohesion. The persistence of long-term unemployment remains a significant challenge, with numerous policy responses attempted, albeit with varying degrees of success. The effectiveness of these policies often remains uncertain, providing a fertile ground for different actors to claim disparate facts based on predetermined views rather than solid knowledge. This uncertainty underscores the need for rigorous analysis to identify truly effective social policies. An evidence-based approach to policy analysis, integrating both experimental and comparative methods, is invaluable in this context. Field experiments provide causal insights into the effects of policy interventions in real-world settings, revealing the efficacy of specific measures. Combined with quasi-experimental approaches, external validity can be enhanced and spillover effects detected. Complementary surveys can be employed to further understand the underlying mechanisms at play. Concurrently, comparative approaches deepen our understanding by examining variations across different contexts and systems, thereby offering a broader perspective on contextual influences. Employing a collage approach that amalgamates these methodologies, this dissertation endeavors to advance knowledge creation about the effectiveness, trade-offs, and complementarities of social policies across various welfare state configurations. These insights are crucial for formulating evidence-based recommendations that are both contextually informed and empirically grounded—essential for devising policies that effectively tackle unemployment challenges and facilitate smoother transitions into employment.

Four pillars of Decent Work The Decent Work Agenda by the International Labour Organization (ILO), with its four pillars, provides an overarching framework that can be used to structure the four empirical chapters of this dissertation.

- 1. **Employment promotion** is central to Chapter 2 on the right to work implemented through guaranteed employment.
- 2. **Social protection** relates to Chapter 3, where I study non-take up of job training as an archetypical social program and active labor market policy.
- 3. **Rights at work** include the job contract, which can be permanent or temporary. The distinction has major ramifications, not only for workers but even for entire economies and macroeconomic policymaking, as shown in Chapter 4 on the wage growth slowdown in Europe.
- 4. **Social dialogue** between worker and employer representatives is key to mediating industrial and employment relations. As a precondition, preferences of relevant stakeholder groups must be aggregated, for instance, unemployed workers' attitudes toward proposed social policy innovations as documented in Chapter 5.

With regard to the theoretical framework, this dissertation explores the impact of social policies on individual behavior and the drivers of social policies across several dimensions. Chapter 2 delves into the behavioral consequences of social policies from an individual perspective. Its analysis extends to spillover effects at the aggregate level, namely local labor markets. Chapter 3 expands the scope by tracing how individual intentions translate into actual behaviors, using training participation as an example to explore the resulting impacts of these actions. Chapter 4 expands the focus to the macro level, examining interactions between different types of workers and how these interactions are shaped by labor market institutions at the national level. This analysis shows how individual-level characteristics, such as the type of job contract, matter for the macroecnomic environment. Chapter 5 returns to the individual level and complements the analysis of individual behavior by examining people's attitudes. This approach allows for a comparison between stated preferences and revealed preferences, offering insights into the alignment and discrepancy between what people believe they will do and what they actually do. Complementary research expands the investigation by exploring the role of cross-national contexts and welfare state configurations in shaping variations in labor market policies (Ebbinghaus and Lehner, 2022). Such a multifaceted approach ensures a comprehensive understanding of both the individual and systemic impacts of social and labor market policies.

The studies in this dissertation examine both the causes and the consequences of social policies. Social policies are thus employed as dependent and as independent variables across the studies. Chapters 2 to 4 assess the economic and social consequences of social policies. By contrast, Chapter 5 and the complementary studies reverse the focus, analyzing policy preferences and policy implementation as outcomes of interest. This dual approach allows the dissertation to thoroughly investigate a critical, often demanded, but rarely investigated goal of social policy research. By exploring the causes and consequences of social policies in tandem, the dissertation provides a comprehensive understanding of why social policies occur and what they achieve.

Dissertation papers At the heart of the dissertation are two field experiments at scale (Chapters 2 and 3) for which I designed the experiments, co-created the interventions with the implementation partners, and evaluated the programs. The analyses are based on high-quality administrative data from multiple registers linked with comprehensive survey data that I collected over several years. Job guarantee schemes have been much-debated as innovative employment policies, but little evidence exists of their consequences for the world of work. Chapter 2 (Kasy and Lehner, 2023a)[1] evaluates a scheme that guarantees employment to unemployed workers by leveraging the first of the two field experiments. Combining randomized assignment, synthetic control, and matching methods, the results underscore the psychosocial value of employment. Social programs of this kind are, however, often associated with stigma, which I investigate in Chapter 3 (Lehner and Schwarz, 2024).[2] We demonstrate the potential of low-cost interventions to reduce non-take-up of job training with randomized controlled trials involving 50,000 job seekers. Chapter 4 investigates the consequences of temporary employment, which is often the only way for unemployed workers to find a job (Lehner et al., 2024). We study the consequences of temporary employment as an important driver of dualization for workers' wages and the moderating impact of labor market institutions across 30 high-income countries.[3] Complementing the analysis of revealed preferences with stated preferences in Chapter 2, I examine in Chapter 5 why unemployed workers prefer guaranteed income versus guaranteed employment (Lehner, 2024). A survey experiment shows a strong correlation in support for either concept, contrasting with the public debate, which often presents polarized views on each concept.

[1] Summarized in Kasy and Lehner (2023b), Lehner et al. (2023), and Kasy and Lehner (2024).

[2] The analysis led to the large-scale follow-up field experiment registered as Lehner and Schwarz (2022) and resulted in a permanent implementation of the most effective intervention by the public employment service.

[3] forthcoming at the ILR Review.

Complementary research This is a paper-based dissertation that consists of four empirical stand-alone papers. Complementing the four papers with a comparative perspective, I founded the Oxford Supertracker (Daly et al., 2020a) during the Covid-19 pandemic, which provided analysis of new data sources (Daly et al., 2020b). The project resulted in several publications, including on short-time work schemes (Ebbinghaus and Lehner, 2022) and preferences for social policies (Ebbinghaus et al., 2022). It also led me to engage in debates about how to improve global data collection for comparative research (Lehner, 2020; Cheng et al., 2022). Furthermore, I provided a synthesized discussion of labor market institutions and comparative research avenues (Lehner and Tamesberger, 2024).

In the remainder of this introductory chapter, I introduce each of the four empirical chapters and complementary research in greater detail and discuss their linkages.

1.1 Hearing the unheard

Although it appears at the end of the dissertation, the 5th and final Chapter serves as the basic motivation for this dissertation. I examine the attitudes of unemployed workers toward two frequently debated innovative social policy proposals that are often thought of as rivals. I study under what conditions unemployed workers support guaranteed employment or guaranteed income by surveying a nationally representative sample of unemployed workers as its main beneficiary group and conducting an experiment within the survey ("What Do Unemployed Workers Want: Guaranteed Jobs or Guaranteed Income?"). In the experiment, I directly compare—for the first time—preferences for basic income with those for a job guarantee and study underlying mechanisms. In contrast to other research, which studies popular support among the general population, I focus on unemployed workers as the main beneficiaries of these policy options. Overall, support for both guaranteed jobs and guaranteed income is strong, though support is consistently higher for the former. Support for both is strongly correlated, which contrasts with the divisiveness suggested by public discourse. For either policy, increasing the pay level yields strong increases in support. Crossing a critical threshold, at the low-pay level just above average unemployment benefits, notably raises willingness to accept guaranteed jobs, indicating a strong willingness to work for little monetary benefit. Support is more prevalent among disadvantaged people, whereas opposition to both policies is unaffected by the pay level among a small, steadfast minority in more favorable socio-economic circumstances. The survey experiment was carried out during the Covid-19 pandemic, but my complimentary paper shows that social policy attitudes remained largely stable during this period despite the shock caused by the pandemic (Ebbinghaus et al., 2022). Overall, the experimental results suggest a shift from viewing the policies as competing to complementary strategies for strengthening the social safety net.

1.2 Implementing the right to employment

> *"After more than 600 job applications over three years, my wish for employment proved hopeless. Too old, too expensive, over-qualified, without long-term prospects due to my age, with multiple university degrees seemingly over-qualified for service jobs, many obstacles seemed to exist."*
>
> —Marienthal Job Guarantee worker

Motivating Chapter 2 is the disillusionment of many long-term unemployed workers with their chances of re-entering employment combined with large, stated support of unemployed workers for guaranteed jobs. Do unemployed workers follow their stated preferences and participate in a job guarantee if one is implemented in a real-world context? And what are the consequences for their economic and social well-being? Traditional approaches to supporting unemployed workers rely on transfer payments and training. Interest in job guarantee programs, as an innovation in the social policy toolkit, has recently surged, though little evidence of the impact of such programs exists. In Chapter 2, I conducted a field experiment on guaranteed employment, "Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program" (co-authored with Maximilian Kasy). We matched workers to employers and provided formal work. We evaluated the program using a combination of pairwise matched randomization, pre-registered synthetic control, and additional control groups to separate out direct causal effects, spillover effects, and anticipation effects. The results establish causal evidence that formal employment provides benefits far beyond pay: participants experience improved well-being, social recognition, and inclusion. The effects are not only short-term consequences of transitioning from unemployment to employment but stay over time as participants remain employed. Already the anticipation of starting a job yields improvements in subjective well-being, social status, and inclusion. The results also show no negative employment spillovers but the creation of additional social jobs that benefit the community. While using state-of-the-art evaluation methods for causal inference, the project builds on fundamental scholarship in the sociology of work and social psychology by Paul Lazarsfeld and Marie Jahoda. It highlights the psychosocial value of employment, which is exemplified by the continuation of the quote presented above:

> *"After more than 600 job applications over three years, my wish for employment proved hopeless. Too old, too expensive, over-qualified, without long-term prospects due to my age, with multiple university degrees seemingly over-qualified for service jobs many obstacles seemed to exist. The*

> *job guarantee proved extremely valuable and useful for me. In cooperation with the municipality and the local museum, I am archiving and documenting the cultural, scientific, and economic value of the historical site of Marienthal."*
>
> —Marienthal Job Guarantee worker

Employment means more than having a job While often used interchangeably, employment and jobs are not identical. This dissertation primarily uses the term of a job guarantee, but examines a scheme that–so the argument goes—provides employment that goes beyond the wage-paying job. Employment is often seen as providing identity and offering a system that includes, in many cases, job training, career opportunities, and collective purpose. A mere job is seen as more unstable compared to offering secure employment. While *job guarantee* has become the prevalent term, the distinction suggests that *employment guarantee* might carry a more comprehensive commitment than providing a mere job.

Revealed and stated preferences The importance of revealed preference versus stated preferences has been subject of intense debate. Economists tend to prefer studying revealed preferences while political scientists typically focus on stated preferences (with notable exceptions). Social policy research, inherently at the intersection of multiple disciplines, focuses on both. Comparing the results of Chapter 2 with those of Chapter 5, my dissertation sheds light on the relationship between both concepts.

Exploring the distinctions between stated and revealed preferences can uncover intriguing insights. Figure 1.1 compares results derived from two chapters within this dissertation. This comparison illustrates the differences between stated support and willingness to accept employment under a job guarantee alongside the actual acceptance of employment within a pilot job guarantee program.

Approximately 9 out of 10 unemployed workers support a job guarantee scheme that offers at least the minimum wage, allows for voluntary participation, and creates positions within social, non-profit enterprises (as shown in the left-hand side plot). However, among these same respondents, fewer than 8 out of 10 expressed their willingness to accept such a guaranteed job (as depicted in the center plot). Chapter 5 reveals that the segment who endorse guaranteed jobs but are reluctant to accept them diminishes as the pay levels of guaranteed jobs increase, indicating these are likely job seekers who currently receive higher benefits than the pay level offered under the job guarantee scheme. When examining revealed preferences, we observe that about 6 out of 10 unemployed workers actually accept a job when offered one in a real-world setting. The findings underscore an incomplete translation from stated support and willingness to engage in guaranteed jobs to the actual acceptance of jobs under a genuine scheme.

The comparison, however, is not clean and comes with several important caveats. First, the populations differ: a representative sample of unemployed workers across Austria forms the basis for the left-hand side and center plots, whereas the right-hand side plot draws from the population of long-term unemployed workers in the municipality of Gramatneusiedl. Second, the initial expressions of support were solicited based on a hypothetical scheme concept, meaning elicited intentions in a real-world program might differ. Third, the Marienthal Job Guarantee scheme incorporated an arm that subsidized hiring in the private sector, enabling employment in regular enterprises—a feature absent in the proposed job guarantee scheme. The result for revealed preferences in the right-hand side plot includes approximately one-third of participants who had taken up employment under this hiring subsidy arm (in early 2022, which later rose as the scheme continued). Lastly, the data depicted on the right-hand side plot capture job take-up a year after the scheme's inception and initial job offers.

Fig. 1.1: Support and acceptance of guaranteed jobs

Note: The left-hand side and center plots are based on results from Chapter 5. The right-hand side plot is based on results from Chapter 2.

Outlook With the pilot project Marienthal Job Guarantee (in German: "Model Projekt Arbeitsplatzgarantie Marienthal (MAGMA)"), Austria has taken on an international pioneering role. The project was awarded prizes by the UK Economic and Social Research Council (ESRC) and the European Innovation in Politics Award. Based on the evidence generated by its evaluation, broad international support emerged: First, in November 2023, the European Parliament adopted a resolution, with reference to the Marienthal Job Guarantee, that calls for the introduction of job guarantees in Europe to facilitate the Just Transition. The resolution was supported by all political groups, with the exception of the far-right parliamentary groups. Second, Olivier De Schutter, the United Nations Special Rapporteur on extreme poverty and human rights, dedicated his 2023 annual report to the Job Guarantee, which was discussed in the United Nations Human Rights Council. Third, international organisations such as the Organisation for Economic

Co-operation and Development (OECD) and the International Labour Organization (ILO) have repeatedly highlighted in their reports the Marienthal Job Guarantee as a showcase pilot for innovative labor market policy. At the same time, other job guarantee initiatives have emerged in European countries, such as in Belgium, the Netherlands, Germany (Berlin), and, recently, in Italy. France has the largest of its kind with a similar project being implemented on a large scale since 2017 with the approval of the president and parliament. Based on the evidence from Austria and other countries, the European Commission, on the initiative of Commissioner Nicolas Schmit, has decided to make €23 million available to finance job guarantee pilot projects in Europe in 2024.

1.3 Assistance unclaimed

As my work shows, social programs can be effective tools to improve economic and social well-being. However, many social programs suffer from incomplete take-up—a problem I soon realized when planning the research for the previous chapter. At a global scale, the issue is of such magnitude that UN Special Rapporteur De Schutter devoted his 2022 annual report to addressing the non-take-up of rights for social protection. Job training programs are an archetypical social program for which governments often struggle to voluntarily enroll unemployed workers. Information frictions and psychological frictions may pose barriers that prevent job seekers from training. In Chapter 3 "Reframing Active Labor Market Policy: Field Experiments on Barriers to Program Participation" (co-authored with Anna Schwarz), we designed a randomized controlled trial involving 50,000 job seekers to investigate frictions that may prevent take-up of job training as an archetypical social program. We randomly provide varying information about training programs to unemployed workers to separate out direct effects of raising awareness, signaling the program's monetary value, and providing information on labor demand by occupation. Further, we combine an instrumental variable strategy with the randomized experiment to evaluate the causal effect of job training on employment. As part of the project, we have placed great emphasis on tracking long-term consequences and fielding our own surveys to discover underlying mechanisms.

The study demonstrates the potential of low-cost information interventions to reduce barriers to enrollment in social programs caused by information and psychological frictions. First, raising awareness about job training to reduce information frictions increases program enrolment by 20% compared to baseline. Second, signaling the monetary value of job training to reduce internalized stigma as a psychological friction increases training completion by 26%. Women and low-income job seekers benefit disproportionally from increased take-up. Third, providing information on labor demand offsets the positive effects by discouraging job seekers from training. We do not find any positive effects of the training programs on employment or wages.

1.4 Not every job glitters

Well-designed labor market policies support unemployed workers as my research shows. Many workers manage to re-enter employment on a temporary work contract. Such temporary employment contracts have become a pervasive feature of modern labor markets. In Chapter 4 “Beggaring Thy Coworker: Labor Market Dualization and the Slow-down of Wage Growth in Europe” (co-authored with Paul Ramskogler and Aleksandra Riedl, forthcoming at the ILR Review), we study the consequences of temporary employment for wages across European countries. We investigate whether the structure of labor markets and the possibility to employ temporary workers affect aggregate wage growth. This is particularly relevant as slow wage growth was an important challenge during the decade following the Global Financial Crisis (GFC). We propose a structural explanation by incorporating labor market dualization into the standard Phillips curve model to explain wage growth in 30 European countries in the period 2004-2017.

We find that the presence of workers with temporary contracts slows down the wage growth of permanent workers. We hypothesize that the underlying reason for this slowdown is the competition between temporary and permanent workers. This has consequences for the aggregate economy, but strong bargaining institutions help mitigate the competition effect. By contrast, countries with low union density show more pronounced competition effects. The competition effect's magnitude is substantial: labor market dualization has been at least as important in slowing wage growth since the GFC as unemployment, i.e., the observed flattening of the Phillips curve. The analysis shows that concepts such as labor market dualization, widely studied in labor relations and social policy, have implications for macroeconomic policy. Social policies and labor market institutions affect not only workers when they are unemployed but also when they are in employment.

1.5 Lessons from around the world

Complementing my four empirical dissertation papers is another set of papers in which I have adopted a comparative perspective that highlights contextual factors. While job training is intended to support unemployed people, short-time work prevents workers from losing their jobs in the first place. In "Cui bono – business or labour? Job retention policies during the COVID-19 pandemic in Europe" (co-authored with Bernhard Ebbinghaus), we analyze cross-national variations in job retention that was massively scaled up during the COVID-19 pandemic. Surprisingly, Liberal welfare states fostered more labor hoarding than Nordic countries. To advance comparative research, I founded the Oxford Supertracker (accessible at https://supertracker.spi.ox.ac.uk/), a global directory of over several hundred policy trackers and surveys related to COVID-19, designed to search and identify relevant information resources across different areas, countries, and data types. The directory has provided access to over 5,000 researchers from over 140 countries to date and resulted in several publications ("Tracking Policy Responses to COVID-19: Opportunities, Challenges and Solutions", 2020, co-authored with Mary Daly, Bernhard Ebbinghaus, Marek Naczyk, and Tim Vlandas; "The Lockdown Tweet That Launched a COVID-19 'Supertracker"' Nature Career Column, 2020; "Capturing the COVID-19 Crisis through Public Health and Social Measures Data Science." Scientific Data, co-authored with Cheng Cindy et al.).

Overview At a methodological level, the dissertation advances empirical social research by expanding access to new data sources and improving the use of empirical methods for causal and comparative social policy analysis. Substantively, the dissertation contributes by producing reliable evidence on innovative social policy concepts, which enhances the understanding of social policies aimed at empowering unemployed workers in a changing world of work. Table 1.1 provides an overview of the papers included in Chapters 2 to 5. Each of the papers includes an appendix, which are collected at the end of the dissertation. The references at the end of this chapter provide an overview of the research that I have completed as part of the work on my dissertation during my DPhil studies.

Table 1.1: Overview of the dissertation papers

#	Title	Research question	Outcomes	Identification	Method	Analysis level	Data	Region	Period
1	Employing the unemployed of Marienthal	What are the consequences of a job guarantee?	Economic and social well-being; labor market spillovers	Randomized assignment	Mean comparison (OLS), Synthetic control comparison	Person; Municipality	Admin; Survey	Gramatneusiedl (AUT)	2020-2023
2	Reframing active labor market policy	What prevents job seekers from training?	Training; employment	Randomized assignment	Mean comparison (OLS)	Person	Admin; Survey	Lower Austria	2021-2023
3	Beggaring thy co-worker	Why was wage growth so slow?	Wage growth	Reweighting and internal instruments	IPW + FE panel regression	Country	EU-SILC	Europe	2003-2017
4	What do unemployed workers want	How do primary beneficiaries view JG and BI?	Support for a JG and BI	Randomized assignment	Mean comparison (Logit)	Person	Survey	Austria	2021

Note: JG refers to job guarantee and BI refers to basic income. IPW refers to inverse probability weighting and FE refers to fixed effects.

Bibliography

Cheng, C., Desvars-Larrive, A., Ebbinghaus, B., Hale, T., Howes, A., Lehner, L., Messerschmidt, L., Nika, A., Penson, S., Petherick, A., Xu, H., Zapf, A. J., Zhang, Y., and Zweig, S. A. (2022). Capturing the COVID-19 Crisis through Public Health and Social Measures Data Science. *Scientific Data*, 9(1):520.

Daly, M., Ebbinghaus, B., Lehner, L., Naczyk, M., and Vlandas, T. (2020a). Oxford Supertracker: The Global Directory for COVID Policy Trackers and Surveys. Technical report, Department of Social Policy and Intervention, University of Oxford, https://supertracker.spi.ox.ac.uk/, Oxford.

Daly, M., Ebbinghaus, B., Lehner, L., Naczyk, M., and Vlandas, T. (2020b). Tracking Policy Responses to COVID-19: Opportunities, Challenges and Solutions. Technical report, Department of Social Policy and Intervention, University of Oxford, Oxford.

Ebbinghaus, B. and Lehner, L. (2022). Cui bono – business or labour? Job retention policies during the COVID-19 pandemic in Europe. *Transfer: European Review of Labour and Research*, 28(1):47–64.

Ebbinghaus, B., Lehner, L., and Naumann, E. (2022). Welfare state support during the COVID-19 pandemic: Change and continuity in public attitudes towards social policies in Germany. *European Policy Analysis*, 8(3):297–311.

Kasy, M. and Lehner, L. (2023a). Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program.

Kasy, M. and Lehner, L. (2023b). Wirtschaftliche und soziale Auswirkungen einer Jobgarantie. In *Elisabeth Kaiser, Marcus Schober (Eds.): Beschäftigung Für Alle. Die Zukunft Der Arbeit*, volume 4 of *Wiener Perspektiven*, pages 92–100. ÖGB Verlag, Vienna.

Kasy, M. and Lehner, L. (2024). Die Marienthal Jobgarantie 2020-2024: Wirtschaftliche und soziale Auswirkungen im Rahmen einer kontrollierten Feldstudie. In *Daniel Haim,* *Clara Moder, Hannah Quinz, Dennis Tamesberger, Simon Theurl (Eds.): Vom Arbeitszwang Zum Recht Auf Gute Arbeit. Beiträge Zu Einer Österreichischen Jobgarantie.* ÖGB Verlag, Vienna.

Lehner, L. (2020). The lockdown tweet that launched a COVID-19 ‘supertracker’. *Nature Career Column.*

Lehner, L. (2024). What do unemployed workers want: Guaranteed work or guaranteed income? *Working Paper.*

Lehner, L., Kasy, M., and Kaplan, J. (2023). Universal Job Guarantee boosts Wellbeing & eliminates long-term Unemployment. Technical report, Institute of New Economic Thinking at the Martin School, University of Oxford, Oxford, UK.

Lehner, L., Ramskogler, P., and Riedl, A. (2024). Beggaring thy coworker: Labor market dualization and the slow-down of wage growth in Europe. *ILR Review.*

Lehner, L. and Schwarz, A. (2022). What prevents job-seekers from training? Reframing training access to reduce non-take up. Pre-Analysis Plan, American Economic Association.

Lehner, L. and Schwarz, A. (2024). Reframing active labor market policy: Field Experiments on Barriers to Program Participation. *Working Paper.*

Lehner, L. and Tamesberger, D. (2024). Unemployment and Labor Market Policy. In *Handbook of Social Infrastructure.* Edward Elgar Publishing.

Chapter **2**

Employing the unemployed of Marienthal: Evaluation of a guaranteed job program

Co-authored with Maximilian Kasy (University of Oxford)

We evaluate a guaranteed job program launched in 2020 in Austria. Our evaluation is based on three approaches, pairwise matched randomization, a pre-registered synthetic control at the municipality level, and a comparison to individuals in control municipalities. This allows us to estimate direct effects, anticipation effects, and spillover effects.

We find positive impacts of program participation on economic and non-economic well-being, but not on physical health or preferences. At the municipality level, we find a large reduction of long-term unemployment, and no negative employment spillovers. There are positive anticipation effects on subjective well-being, status, and social inclusion for future participants.

JEL codes: I38, J08, J45

We thank Sven Hergovich, who initiated the MAGMA job guarantee program, and the employees of the *AMS Niederösterreich*, including Martina Fischlmayr, Elisabeth Reiter and Daniel Riegler, the *AMS Österreich*, including Christian Bliem and Nicole Nemecek-Tomschy and of *it.works*, including Daniela Scholl, Beata Strosin and Michaela Windisch for their support, without which this study would not have been possible.

We thank David Autor, Lotte Bailyn, David Card, Bart Cockx, Adam Coutts, Stefano DellaVigna, Bernhard Ebbinghaus, Pirmin Fessler, Helmut Hofer, Hilary Hoynes, Simon Jäger, Erin Kelly, Adam Leive, Jackson Lu, Sanaz Mobasseri, Claire Montialoux, Mathilde Muñoz, Brian Nolan, Soohyun Roh, Jesse Rothstein, Emmanuel Saez, Benjamin Schoefer, Anna Stansbury, Andreas Steinmayr, Nathan Wilmers and Gabriel Zucman for valuable feedback and comments, and Klaudia Marschalek and Carlos Gonzalez Perez for their research assistance. Lukas Lehner acknowledges financial support from the Economic and Social Research Council [grant number ES/P000649/1], the Scatcherd European fund, the Saven European fund, the Horowitz Foundation for Social Policy, and the Austrian Marshall Plan Foundation as part of his PhD funding.

This study was registered as AEARCTR-0006706 (Kasy and Lehner, 2020).

2.1 Introduction

Employment, with appropriate wages and working conditions, can have have numerous benefits. This includes both economic benefits such as income and economic security, and non-economic benefits, such as social inclusion, recognition, and sense of purpose. Consideration of such benefits informs a recent resurgence of interest in job guarantee programs as part of the social policy toolkit. For discussions of job guarantee programs by the media, international organizations, and think tanks see for instance Lowrey (2017); The Guardian (2020); Porter (2021); OECD (2021); ILO (2021); EU CoR (2023); UN Special Rapporteur (2023); Tanden et al. (2017); Nunn et al. (2018); Paul et al. (2018); Tcherneva (2020). Despite this widespread interest in job guarantee programs in the recent policy debate, there exists little evidence on the impact of such programs, in particular for rich countries. In the present paper, we evaluate a pilot program which aims to address this lack of evidence – the MAGMA job guarantee program, which launched in 2020 in Lower Austria. We study the impact of this program both on the participants themselves, and on other residents of the same municipality.

In doing so, we contribute to the literature in three ways. First, we provide the first rigorous evidence, in a rich country context, on the impact of a policy that has received much attention in the recent public debate. Second, we provide causal evidence on the non-monetary benefits of employment, which a large correlational literature has documented – mostly outside economics, with the notable exception of a recent experiment by Hussam et al. (2022) among Rohingya refugees in Bangladesh. Third, on a methodological level, our study provides a template for the evaluation of small local policy pilots, where we leverage a range of experimental and observational methods to obtain precise estimates of the effects of this policy, including anticipation and spillover effects.

The MAGMA job guarantee program The MAGMA job guarantee[1] is a pilot program launched in the municipality of Gramatneusiedl by the Public Employment Service (Arbeitsmarktservice, *AMS*) of Lower Austria in October 2020, and is scheduled to last until 2024. We co-designed this policy experiment with the AMS, using pairwise matched randomization for program enrollment. MAGMA provides a guaranteed job to all residents of this municipality who were long-term unemployed (12 months or more) or at risk of long-term unemployment (9 to 12 months). Participation in the program is voluntary, but no person who was offered a job has declined the opportunity. If someone were to decline, their income would be about 30% lower, corresponding to about 390 Euro less per month. This provides a monetary incentive for participation, but the near-universal takeup is nonetheless remarkable. A small number of eligible individuals could not be offered employment for reasons including illness, a prison sentence, or because they found regular employment before the start of the program.

The guaranteed job was preceded by individually tailored preparatory training of about 8 weeks. The jobs themselves could either be subsidized jobs in the regular labor market, or (for the majority of participants) employment in a social enterprise, implementing projects for the municipality. Salaries for all participants were at least equal to the minimum wage set by collective bargaining. Jobs were created to fit the individual needs and constraints of participants, and to provide meaningful activity. Expenditures of the *AMS* per participant and year were about EUR 29,841. We discuss this number further in Section 2.2.

The MAGMA program differs from typical active labor market policies, and might alternatively be compared to pure income support and welfare programs. The intervention is quite big and long-lasting, and the objective is different from more conventional active labor market policies (Card et al., 2010), which aim at re-integration of participants

[1]MAGMA is short for "Modellprojekt Arbeitsplatzgarantie Marienthal," which translates as "model project job guarantee Marienthal." Marienthal is one part of the municipality of Gramatneusiedl.
MAGMA has received considerable attention from international organizations (OECD, 2021; ILO, 2021; EU CoR, 2023; UN Special Rapporteur, 2023) and news media; see for instance ZDF (2022); ARTE (2021); Romeo (2022); Henderson (2021); Pausackl (2021); Horowitz (2020); Bendix (2020); Stone (2020). The latter were published in ZDF, ARTE, The New Yorker, Forbes, Die Zeit, CNN, Business Insider, and The Independent, respectively.

into the regular labor market. While participants of the MAGMA program are certainly encouraged to take up employment in the regular labor market, and such employment is subsidized by the program, this is not a likely outcome for many participants. Instead, the stated policy goal of the MAGMA program is to directly eradicate long-term unemployment in the municipality, and thereby to improve participants' economic and social situation. Correspondingly, our evaluation focuses on the impact of the program on the well-being of participants along various economic and non-economic dimensions, and on the impact on the municipality-level labor market overall.

Evaluation strategy We draw on several administrative data sources, including the *AMS* internal registry, the "occupational-career monitoring", and data obtained from the national statistical agency, as well as several surveys that we administered ourselves. Our evaluation of the job guarantee program is based on three complementary approaches.[2]

Our first approach uses pairwise randomization within pairs of participants who were matched using baseline covariates; cf. Athey and Imbens (2017). Participants are assigned by us to one of two groups, where the second group starts the program 4 months after the first one. This allows us to estimate the short-term effects of the program, by comparing participants across the two groups, around 3-4 months after the start of employment for the first group.

Our second approach uses the synthetic control method; cf. Abadie et al. (2010). We construct a synthetic control town for Gramatneusiedl, based on other towns in the province of Lower Austria.[3] The synthetic control town is a convex combination of similar towns. This method allows us to estimate effects of the program at the town level, including potential spillovers on non-eligible residents, in particular effects on short-term unemployment.

[2]We registered a pre-analysis plan for evaluation strategy 1 and 2 for this study before the start of the MAGMA program, at https://www.socialscienceregistry.org/trials/6706. Evaluation strategy 3 was added later.

[3]Throughout this paper, we use "town" and "municipality" interchangeably.

Our third approach compares program participants to observationally similar individuals in control towns. We conducted interviews with individuals who are residents of the three main towns that are part of our synthetic control (Ebreichsdorf, Zeillern, Rußbach), and who satisfy the participation criterion of at least 9 months of unemployment. We additionally adjust for a rich set of baseline covariates in our regressions.

The size of the initial cohort of MAGMA participants was fairly small, with 62 participants in the initial treatment group. This is compensated, however, by the magnitude of the intervention, and by the fact that it was geographically concentrated. For these two reasons, and given our design which aims to minimize sampling variability, our study is adequately powered to estimate both individual-level and municipality level effects. In particular, our standard errors for individual-level outcomes with range $[0, 1]$ are on the order of .02 to .03, while the estimated treatment effects for our headline outcomes range from about .1 to .65.

Similarly, for the synthetic control approach, recall that all long-term unemployed in Gramatneusiedl are eligible to participate. If each person employed in the program were to displace a job on the regular labor market, this would imply an increase of short term unemployment by almost 50% from 3 to 4.5 percentage points, or about 60 persons out of a labor force of around 4,000, as of January 2022. Such an increase would be significant at the 5% level when performing permutation inference for the synthetic control approach.

Anticipation effects, equilibrium effects, and long-term effects The combination of our three evaluation strategies is attractive not only because it lends robustness to our empirical findings, but also because it allows us to separate out direct program effects on participants from anticipation effects and equilibrium (spillover) effects.

Regarding anticipation effects, consider the simultaneous comparison of current participants to both future participants in Gramatneusiedl, and to observationally similar individuals in control towns. While current participants experience the direct effect of the program, future participants anticipate employment by the program in about a month.

Comparison of future participants to control town individuals allows us to identify such anticipation effects.

Regarding equilibrium effects, there are various channels through which non-eligible residents might be impacted by the program. Possible channels include (i) demand spillovers through increased consumption of participants, (ii) crowd-out of regular employment by guaranteed employment, (iii) anticipation effects, where the short-term unemployed know they will become eligible for program participation at a certain point, thus reducing their search effort, and (iv) a shift of resources of the labor market service agency away from other programs. Our synthetic control estimates at the municipality level capture any such equilibrium or spillover effects.

An additional benefit of the comparison to individuals in control towns is that this comparison allows us to estimate the longer-term effects of program participation. While all individuals in the experimental control group eventually become eligible to participate, individuals in control-towns never become eligible. We follow up on these longer term effects by conducting surveys in subsequent years.

Main findings Our main empirical findings can be summarized as follows. For the **individual-level** experimental comparison of current to future participants, three sets of findings are noteworthy. First we find large positive effects of participation on economic well-being (employment, income, and economic security). This is as expected, but it is not mechanical since (i) program participation is voluntary, and (ii) those individuals who decline participation are still eligible to receive unemployment benefits.

Second, we find large effects on a number of measures of well-being that have been emphasized in the sociology of work, social psychology, and organizational behaviour (Jahoda, 1982), and which have been summarized as the "latent and manifest benefits" of work, (Kovacs et al., 2019). This includes measures of time structure, activity, social contacts, a sense of collective purpose, and social recognition. Our experimental findings thus corroborate descriptive work in sociology and social psychology on the importance of these non-economic benefits of employment, including the "need to belong" (Baumeister and Leary, 1995), and the "desire for status," (Anderson et al., 2015); see also Strandh (2001). Such measures of well-being have received less attention in labor economics thus far, with notable exceptions such as Clark (2003, 2006); Kassenboehmer and Haisken-DeNew (2009).

Third, we estimate the effect of program participation on a number of measures where no short-term movement was expected, including physical health and economic preferences (time and risk preferences, reciprocity, altruism, trust). As we had anticipated, we find precisely estimated zero effects on these outcomes, with the possible exception of a small effect on physical health. We view this as a validation (placebo test) of our approach, which increases our confidence that the estimated program effects are not driven by "interviewer demand effects."

Turning to **municipality-level** effects, which we estimate using the synthetic-control approach, our headline finding is a large reduction of municipality-level unemployment due to the program. This in turn is driven by a near-elimination of long-term unemployment in Gramatneusiedl – which, again, is not mechanical, given the voluntary nature of the program. We do not find any systematic increase of short-term unemployment, and thus no evidence of negative spillovers. Correspondingly, we find that the reduction of total unemployment is of the same magnitude as the reduction of long-term unemployment.

Lastly, when we compare long-term unemployed **individuals in control towns** to program participants, we find effects that are similar to those that we found in our experimental comparison. The point estimates are almost identical for our headline outcomes (income and economic security, employment and unemployment, and the latent and manifest benefits of work). The estimates from this comparison are slightly larger than the experimental estimates for some other dimensions, however, including (subjective) well-being and social status. This suggests the presence of some anticipation effects, but most of the program benefits only manifest after the start of employment.

Considering outcomes in subsequent years, we find that the effects estimated initially largely persist, with little attenuation over time. This suggests that the benefits of a guaranteed job are sustained beyond the initial period.

The historical arc from "Die Arbeitslosen von Marienthal" (1933) to MAGMA The location chosen for the job guarantee pilot is no coincidence. Ninety years prior to this experiment, Marienthal was the location of a pathbreaking study on the impact of long-term mass unemployment (Jahoda et al. 2017, "Die Arbeitslosen von Marienthal," originally published in 1933). At the time, Marienthal was a factory town dominated by a single factory. When this factory shut down in the Great Depression, most residents lost their employment, with devastating consequences. Jahoda et al. (2017), in a large multi-method study, documented the impact of this situation. This study proved to be of lasting influence on the sociology and social psychology of work.

90 years later, the MAGMA experiment provides a mirror image of the original situation, by offering employment to all the long-term unemployed residents of Marienthal and of the municipality of Gramatneusiedl. Strikingly, as noted above, some of the most pronounced effects of program participation that we find are on the "latent and manifest benefits of work" – a measure which operationalizes concepts developed by Marie Jahoda, building on the original Marienthal study. Marie Jahoda continued to work as a sociologist in exile in the United Kingdom, following the rise of fascism in Austria. In section A.4 we offer some reflections on the contrast between the original Marienthal study and the present paper, taking the opportunity to discuss 90 years of methodological developments in the social sciences.

Job guarantee versus unconditional income support The direct individual-level treatment effects that we estimate compare program participants to non-participants who remain in the regular unemployment benefit system. It would be interesting to also compare participants to recipients of the same level of income in the form of an unconditional transfer, without the employment guarantee, in order to separate the effects of the employment guarantee from the effects of the income support. We were not able to directly make such a comparison, but we can provide some indirect evidence.

First, note that non-participants continue to receive unemployment benefits. For our experimental control group, these are on average equal to EUR 890 per month, compared to the average monthly income of program participants of EUR 1280. The monthly income of the control group is thus lower by EUR 390, or 30%, relative to participants. This is not negligible, but unlikely to explain the large effects that we find.

Second, a number of existing studies consider the effect of unconditional cash transfers in rich countries. cf. the review by Marinescu (2018). Most of the studies that they review find no or very little impact of unconditional cash transfers on labor supply. There is some evidence that an unconditional cash transfer can improve health and educational outcomes and decrease criminality, and drug and alcohol use among the most disadvantaged youths. Relatedly, McGuire et al. (2022) review the impact of cash transfers on subjective well-being and mental health in low- and middle-income countries. They find that cash transfers have a small but statistically significant positive effect on both subjective well-being and mental health among recipients. Jaroszewicz et al. (2022), in a recent study of unconditional cash transfers in the US, find no evidence that these transfers had positive impacts on pre-specified survey outcomes, including financial well-being, psychological well-being, cognitive capacity, and physical health.

Literature There is a large literature studying the effectiveness of active labor market policies (ALMPs); see in particular the meta-analyses Card et al. (2010, 2018), and the earlier reviews Heckman et al. (1999); Kluve (2010), as well as Crépon and van den Berg (2016). The existing evaluations of ALMPs in German-speaking countries are mostly observational (recent exceptions are Altmann et al. 2018; van den Berg et al. 2021; Böheim et al. 2022); by contrast, there are numerous experimental studies from the US, e.g. Card and Hyslop (2005); Schochet et al. (2008); Gelber et al. (2016), and France, e.g. Crépon et al. (2013); Behaghel et al. (2014). Cummings and Bloom (2020) discuss a number of recent RCTs in the US evaluating subsidized employment programs, focusing on the effects on employment after the subsidies expire. They find some evidence of positive effects on employment, in particular among the most disadvantaged participants.

This literature also includes some recent evaluations of public employment schemes for India (Khera, 2011; Muralidharan et al., 2023; Banerjee et al., 2020), Ivory Coast (Bertrand et al., 2017), and Malawi (Beegle et al., 2017), and an evaluation of the psychosocial value of employment in Rohingya refugee camps (Hussam et al., 2022). By contrast, we provide the first experimental evaluation of a job guarantee program in a rich country.

A common conclusion of evaluations of ALMPs appears to be that job search programs are somewhat effective in improving participants' future employment prospects, as are (sectoral) training programs (Katz et al., 2022), whereas public employment programs are not. Two points are worth emphasizing in this context. First, most of this literature considers different outcomes and policy objectives than we do, focusing in particular on (market) employment, in German-speaking countries, and (market) earnings, in English-speaking countries. By contrast, we are interested in the impact on the community and on participant welfare, without an expectation that participants will enter market employment.

Second, much of this literature focuses on individual-level effects, neglecting spillovers; important exceptions are Crépon et al. (2013), who study the negative displacement effect of job counseling using a large-scale clustered randomized controlled trial in France, and Lalive et al. (2015); Huber and Steinmayr (2021), who consider spillovers of unemployment insurance in the Austrian context. Plausibly, the spillovers of search assistance (redistributing existing vacancies without impacting overall employment) are more pronounced than those of a job guarantee (creating additional jobs); we study the latter spillovers in the present paper. Relatedly, Muralidharan et al. (2023) study genereral equilibrium effects of a reform of India's National Rural Employment Guarantee Scheme (NREGS). They find large positive spillovers of the reform, and no crowd-out of private sector employment.

The present paper also speaks to the large literature on the (negative) consequences of (un)employment. A correlational association between health and employment is widely documented in social epidemiology and neighboring fields, cf. Brand (2015); Avendano and Berkman (2014), though the causal link between the two is contested. Similarly, there is a strong association between employment and (subjective) well-being, cf. Clark and Oswald (1994); Korpi (1997); Clark (2003, 2006); Kassenboehmer and Haisken-DeNew (2009); Young (2012); Pohlan (2019); see also Haushofer and Fehr (2014). In economic theory, Basu et al. (2009) discuss the implications of an employment guarantee scheme on efficiency and social welfare. The negative psychological consequences of unemployment have also been studied in a much older psychological literature; Eisenberg and Lazarsfeld (1938), for instance, review over 100 such studies conducted during the Great Depression. A general conclusion of this older literature was that unemployment leads to loss of purpose, confidence, and time structure, and to apathy, rather than political radicalization. (As an aside, Lazarsfeld, one of the authors of this review, was a co-author of the original Marienthal study, and later became president of the American Sociological Association.) In contrast to both the older and most of the more recent correlational literature, we estimate causal effects of employment well-being.

Methodologically, we build on the large literature on experimental and observational program evaluation. For the experimental component of our study, using pairwise randomization within pairs of participants matched using baseline covariates, we draw on the review by Athey and Imbens (2017). For the synthetic control approach for estimating municipality-level effects, we draw on Abadie et al. (2010) and Abadie (2019). For the causal interpretation of direct effects, anticipation effects, equilibrium effects, and total program effects, we discuss a formal framework that loosely builds on Graham et al. (2010).

Roadmap The rest of this paper is structured as follows. Section 2.2 provides further context and details regarding the MAGMA job guarantee program. Section 2.3, building on our pre-analysis plan, details our experimental design and analysis, as well as the construction of the synthetic control municipality, and discusses the formal interpretation of our causal estimands. Section 2.4 discusses our empirical findings, for each of the three approaches. Section 2.5 concludes.

Appendix A.1 presents additional details on our evaluation strategies, additional empirical findings, and robustness checks. Appendix A.2 lists all the survey questions that were used to construct the indices for our empirical analysis, as well as the sources on which these survey questions were based. Appendix A.3 provides a detailed list of all the jobs that were created in both the market and non-market sector, reports views from program participants, describes some of the jobs that were created in greater detail, and shows photos of participants at work. Appendix A.4 contrasts (Jahoda et al., 2017) and our study to discuss changes in the methodology of empirical social science over the last 90 years.

2.2 Background and program details

Starting in October 2020, the Public Employment Service of Lower Austria (*Arbeitsmarktservice Niederösterreich, AMS NÖ*) has piloted an intervention that aims to eradicate long-term unemployment and improve social, health and well-being outcomes for people in long-term unemployment, by bringing them back into employment. The intervention has provided a guaranteed job to people in long-term unemployment. The intervention took place in one town in Lower Austria, Gramatneusiedl. Gramatneusiedl encompasses the settlement of Marienthal, where the historic "Marienthal study" on the consequences of unemployment took place in the early 1930s (Jahoda et al., 2017).

All residents who were "at risk of long-term unemployment" (unemployed for 9 to 12 months) or "long-term unemployed" (unemployed for 12 months or more) were eligible to participate. The experimental sample includes all residents unemployed for more than 9 months in September 2020. Residents who reached the eligibility threshold later were eligible to participate in the program, but are not part of our experimental comparison. The initial duration for the project was set until 2024 and budgeted with EUR 7.4 million.

Preparatory training The program was implemented by the private service-provider *it.works*, which specializes in implementing active labour market programs for the *AMS*. *it.works* provided preparatory training for participants, and continued counseling and training after participants had taken up employment. The preparatory training phase was scheduled for a maximum of 8 weeks, but durations were allowed to vary depending on individual conditions and progress. Each participant received a tailored curriculum according to her individual needs. This could include individual and group counseling, skills development, support for initiatives proposed by participants, and assistance with applications for health-related benefits. Participants continued to be encouraged to take up regular employment outside of the program, if available.

Guaranteed jobs After completion of the preparatory training phase, participants joined the job guarantee program for up to 3 years. Participants were supported to find a job on the regular labor market. The *AMS* subsidized wages for such jobs, paying 100% of labor costs for the first 3 months, and 66% of labor costs for the subsequent 9 months. Employers were legally allowed to fire subsidized workers at any point during or after the subsidy. However, they could reasonably expect to face difficulties in obtaining future referrals of job seekers by the *AMS* if they did so repeatedly. This provided an incentive to continue to employ these subsidized workers.

Those participants who remained without job placement received an employment offer with a newly established social enterprise operated by *it.works*. All participants were paid the occupation- and experience-specific minimum wage, as set by collective bargaining in Austria. This includes both those employed at *it.works*, and those working for private employers. This minimum wage of around EUR 1,500 per month, in 2020 compares to an average monthly wage of EUR 3,308 in the municipality.[4]

The social enterprise implemented projects at the municipal and regional level. This involved activities such as childcare, gardening, renovation, and carpentry, depending on orders acquired by the enterprise. In addition, participants were supported to develop and propose their own ideas for projects of the social enterprise, based on their expertise and local knowledge of community needs. Examples of projects proposed by participants included a workshop to renovate furniture, maintenance of public gardens, support for elderly residents in their day-to-day activities, planning and construction of a bike trail, and refurbishment of the local museum. section A.3 provides a detailed list of all the jobs that were created, in both the market and non-market sector, describes some of the jobs that were created in greater detail, and reports views from some of the participants in the program. Figure A.8 in section A.3 shows photos of program participants at work, in carpentry, bee keeping, and tailoring.

A specific effort was made to create productive and meaningful employment that is adequate to the participants' previous jobs and interests. The jobs created were furthermore tailored to the needs of the recipients: Participants who were only available to work part-time, given their other obligations, received a corresponding part-time offer. Participants who could carry out only a limited number of tasks for health reasons similarly received a corresponding offer. Social workers and instructors continued to provide support to employees of the social enterprise as needed. Participants had access to occupational physicians. Those participants that felt ready to work for third-party employers received targeted support and additional counseling to apply and find employment outside of the program.

Voluntary participation Work conditionality was eased for this pilot program. Under current law (*Arbeitslosenversicherungsgesetz AlVG §9*), recipients of unemployment

[4] By 2023, the minimum wage had increased to around EUR 1,700.

benefits are assigned to labour market programs by the *AMS*. They have the obligation to participate and they have to accept any employment offer that conforms to their skill-set, otherwise they might lose their unemployment benefits.

By contrast, within the job guarantee program only participation at the information event and during the preparatory training phase were subject to this conditionality, while take-up of employment offered as part of the job guarantee was voluntary; there were no sanctions in case a job offer was declined by participants.

Out of the 62 experimental participants, 45 were employed as of July 2022, 37 of those via *MAGMA* and 8 through a job outside of *MAGMA*. The remainder could not participate, mostly due to illness or because they had moved.

Timeline for the intervention The program was rolled out in two waves, and launched in October 2020. At that time the tailored curriculum and coaching started for the first group of 31 participants. In December 2020, this first group of participants were scheduled to start their employment. In February 2021, the tailored curriculum and coaching started for the second group of 31 participants. We conducted our first round of surveys just after the start of training for this second group. In April 2021, the participants in this second group were scheduled to start their employment. The program was set to continue for (at least) 3 years, up to March 2024.

In addition to obtaining administrative data, we collected detailed survey data from both participants and similar individuals in control towns. Our first survey was conducted in February 2021, when the first group of participants was in employment, but the second group was not yet. Our second survey was conducted in February 2022, when both groups were in employment. In both years, some participants were allowed to complete the survey in March, to minimize attrition.

Impact of the Covid-19 pandemic The implementation and timeline of the job guarantee pilot were not affected by the Covid-19 pandemic, and the pilot continued as planned. The Covid pandemic did not affect the internal validity of any of our three estimation approaches. It might affect the external validity of our findings, however, for extrapolation to contexts with tighter labor markets.

Due to the pandemic, labor market conditions worsened in Lower Austria, including Gramatneusiedl. The trajectory of economic conditions in Gramatneusiedl during the pandemic was similar to that of control municipalities. All individuals included in our treatment and control groups, for the experimental approach, had become unemployed before the pandemic, but their opportunities to find employment might have been impacted by the pandemic. The same is true for the individuals surveyed in control municipalities.

Entrants into the job guarantee scheme at a later stage included those who became unemployed during the pandemic. These late entrants are not part of our experimental comparison, or the individual-level comparison across municipalities. They do figure in municipality level comparisons using the synthetic control approach, however. As of July 2022, there were 112 eligible individuals, including 62 experimental participants and 50 late entrants. Out of those, 80 had found a job, including 45 at the social enterprise founded by MAGMA, 22 on the regular labor market with a wage subsidy, and 13 on the regular labor market without subsidy.

We took precautionary measures during the fieldwork and data collection to guarantee the safety of both the participants and the researchers involved. We have detailed those in the ethics application for our study that was approved by the Departmental Research Ethics Committee at the Department of Economics, University of Oxford.

Program costs We were able to obtain the following (partial) information on program costs from the *AMS*. The annual expenditures for the intervention by the *AMS* are EUR 29,841 per eligible participant. Of this sum, EUR 19,155 are "labor costs" for participants, which includes both (net) wages, as well as social insurance contributions and some income taxes. Social insurance contributions and income taxes flow back to the state, and would need to be subtracted for a full cost-benefit analysis. The social enterprise also generated revenues of around EUR 1,500 per participant, which again need to be subtracted for a full cost-benefit analysis. Of the sum of EUR 29,841, the remainder are wages of non-participants (trainers and work supervisors, etc.), and expenditures for materials (building rent, etc.).

Comparable numbers for long-term unemployed people outside the program were not available at the time of writing. The total expenditure per unemployed person of the state (including the *AMS*, social insurance, etc.) in 2018 was reported to equal EUR 20,328 in 2018 by the Ministry of Labor. This number is not fully comparable to the program expenditures reported above, however. The reported expenditures per unemployed person do not involve social insurance contributions or income taxes flowing back to the state. This biases the difference upwards, relative to the true difference in costs for the state. Furthermore, this number does not apply to the same population, with ambiguous implications for the difference. Of the expenditures per unemployed person, EUR 11,083 were direct payments to the unemployed.

Parallel qualitative evaluation A complementary study (Quinz and Flecker, 2022), conducted by researchers at the Department of Sociology at the University of Vienna, is based on a mixed-methods design and qualitative in-depth interviews. Based on their interviews, they classify program participants into three groups or "ideal-types." Group A consists of long-term unemployed participants with underlying health conditions or discontinuous employment trajectories, who had given up the hope to find stable employment outside the program before they participated. Members of Group A are grateful for the opportunity to participate. Group B is eager to find re-employment outside of the program and therefore focused on enhancing their skills. By contrast, Group C had already given up any hope to find re-employment as a consequence of a negative shock in their life, and views the guaranteed job as a form of individual fulfillment before retirement.

Moreover, their study identifies the 8 week preparatory training program as essential to prepare job seekers for their jobs under the guaranteed jobs scheme. They conclude that positive consequences of the program are contingent on offering purposeful work to participants that takes their individual health and life situation into account.

2.3 Study design

Sample selection The set of participants who were eligible for the job guarantee program included all current residents of Gramatneusiedl registered with the *AMS* who are "at risk" of long-term unemployment (i.e., had been unemployed for between 9 and 12 months) or in long-term unemployment (unemployment spell exceeding 12 months).[5] The definition of unemployment used here is the *AMS* definition of "beschäftigungslos." This definition implies that the duration of unemployment is measured regardless of whether individuals have participated in active labor market programs of the *AMS* during their unemployment spell. It also includes those who have registered sick leave for less than 62 consecutive days, or have attempted to take up employment but were employed for less than 62 consecutive days since the start of the unemployment spell. The count of the unemployment spell duration starts again from zero if a formerly unemployed person returns to unemployment from sick leave or employment that lasted longer than 62 days.

Outcomes of interest We estimate the effect of program participation on a range of economic and social outcomes. These outcomes are listed and defined in Table 2.1. The first set of individual-level outcomes are based on administrative data sources. These include employment status and duration of unemployment, from the "AMDB Erwerbskarrieremonitoring."

The second set of individual-level outcomes are based on surveys that we conducted in February 2021 and in February 2022. The complete list of survey questions corresponding

[5] The description in this section follows our pre-analysis plan.

Table 2.1: Variable definitions

Variable	Definition	Source
Individual level, economic		
Unemployment (-)	Share of days not employed since Oct 1, 2020.	Admin
Employment	Share of days employed since Oct 1, 2020.	Admin
Income	Current monthly income, divided by 2000.	Survey
Economic security	Normalized index of five item scales of income, financial situation and material deprivation.	Survey
Individual level, other	**Normalized index of**:	
Depression symptoms (-)	A five item depression scale.	Survey
Covid stress (-)	A seven item scale on the impact of the Covid-19 pandemic on stress, mental health, employment and income.	Survey
Social inclusion	Two item social inclusion scale, including the number of new people met in the past month, divided by 10, and the current relationship status.	Survey
Preferences	Twenty-two items for economic preferences, including time preferences, risk preferences, reciprocity, altruism and trust.	Survey
Latent and manifest benefits	A twelve item scale on the latent and manifest benefits of employment that include activity, social interaction, collective purpose, time structure, social recognition, and financial strain.	Survey
Physical health	A fifteen item physical health scale.	Survey
Anxiety symptoms (-)	A seven item anxiety scale.	Survey
Social network	A six item social network scale.	Survey
Well-being scale	A five item mental well-being scale.	Survey
Well-being change	Subjective well-being compared to six months ago.	Survey
Social status	Three item scale on current social status, status compared to the past, and expected future status.	Survey
Number of contacts	The number of meaningful social contacts with respect to work-related and job-search issues in the six past month, divided by 5.	Survey
Subjective health	Two questions on overall health situation and recent changes.	Survey
Municipality level		
Unemployment	Number of unemployed as a share of working age population.	Admin
Long-term unemployment	Number of long-term unemployed ($>$ 1 year) as a share of working age pop.	Admin
Short-term unemployment	Number of short-term unemployed ($\leq$ 1 year) as a share of working age pop.	Admin
Employment	Number of employed as a share of working age pop.	Admin
Inactivity rate	Number of inactive persons of working age as a share of working age pop.	Admin

to each of these outcomes is listed in section A.2. We collected information on a rich set of economic outcomes (in particular income and economic security), as well as non-economic outcomes. For non-economic outcomes, we construct a range of indices, on the "latent and manifest benefits" of work, measures of mental and physical health, subjective well-being, social inclusion and recognition, etc. Our construction of these indices follows established practice in survey design, sociology, psychology, and public health; cf. again section A.2 for references and details.

To enable a compact presention of our results in Section 2.4, we normalize all individual-level outcomes, such that higher values correspond to "better" outcomes (variables where the sign is flipped are marked by *(-)* in the table and subsequent figures), and such that the range of these variabes is the interval $[0, 1]$; cf. Table 2.1.

The third set of outcomes, defined at the municipality level, is again based on administrative data from the "AMDB Erwerbskarrieremonitoring." We observe, in particular, the share of the population in each municipality that is in short- and long-term unemployment, employment, and out of the labor force ("inactive").

2.3.1 Three identification approaches

In order to assess the impact of the guaranteed job program, we consider three contrasts. First, we compare the outcomes of participants in two groups, where Group 2 starts the program later than Group 1. Assignment to these groups is based on pairwise randomization, where pairs are matched on baseline covariates. The pairwise randomization approach reduces sampling variability, relative to full randomization. The comparison of the two groups delivers credibly identified treatment effects. It is restricted, however, to short-term individual-level outcomes measured in February 2021, before the second group of participants starts their jobs. Furthermore, the control group might be impacted by the anticipation of future program receipt.

Second, we estimate municipality-level treatment effects by comparing Gramatneusiedl to a synthetic control. This comparison allows us to estimate equilibrium effects and spillovers at the municipality level, which might, for instance, be driven by the crowd-out of jobs, by consumer demand effects of those participating in the program, or by a re-allocation of resources of the labor market service agency. This synthetic control comparison includes effects on residents who were not eligible to participate in the program because they were not long-term unemployed.

Third, we construct a control group of long-term unemployed residents of the synthetic control municipalities, who would have been eligible to participate in the program had they been residents of Gramatneusiedl. This comparison allows us to estimate treatment effects which are not affected by anticipated program participation, and to estimate longer-term effects of program receipt.

Approach 1: Pairwise randomization We assigned program participants to one of two groups using pairwise randomization. We matched pairs using a number of covariates,[6] including gender, age, "migration background" (i.e., being a migrant or child of migrants), education (i.e., more than "Pflichtschule," the legally required minimum), presence of a disability or medical condition recorded by the *AMS*, the level of benefits most recently received (which is closely correlated with prior income), and the number of days recorded as unemployed and looking for a job within the last 10 years. We constructed these variables from raw data for the eligible participants using the *AMS* internal registry (*AMS Data Warehouse*). All of these variables were used as available to the *AMS* in September 2020. These data were recorded at the last prior interaction between each of the participants and the *AMS*.

We calculated pairwise distances between all 62 program participants using the Mahalanobis distance, based on these covariates. The Mahalanobis distance of two covariate

[6]The code implementing the following designs has been uploaded to GitHub, at https://github.com/maxkasy/Marienthal, prior to the start of the MAGMA program. For the matched pair design, we used the package *nbpMatching* in R, for the synthetic control design we used the package *Synth*.

vectors x_1 and x_2 that are realizations of a random vector X is given by $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot Var(X)^{-1} \cdot (x_1 - x_2)}$. We matched participants into pairs such that the total sum of distances between the members of each matched pair is minimized. We then randomly assigned one of the participants in each pair to Group 1, starting the program earlier, while the other participant was assigned to Group 2, starting the program later. Summarizing the resulting assignment, Table 2.2 shows the differences in covariate means between groups, and the corresponding (naive) t-statistics. Confirming that our procedure worked as intended, all available covariates are balanced across groups.

Table 2.2: Covariate balance for our matched pair design

Covariate	Mean wave 1	Mean wave 2	Difference	t-statistic	p-value
Male	0.581	0.581	0.000	0.000	1.000
Age	44.452	44.935	-0.484	-0.165	0.869
Migration Background	0.323	0.355	-0.032	-0.264	0.793
Education	0.452	0.452	0.000	0.000	1.000
Health condition	0.290	0.323	-0.032	-0.271	0.787
Benefit level	29.839	29.839	0.000	0.000	1.000
Days unemployed	1721.871	1600.839	121.032	0.483	0.631

Approach 2: Synthetic control Our second approach is based on the construction of a synthetic control municipality for Gramatneusiedl. For this construction we draw on data from various sources, including (i) the *AMS* internal registry for administrative data on the unemployed, (ii) the "occupational-career monitoring" (*Erwerbskarrierenmonitoring, EWKM*), accessed via the *AMS* internal registry for social security registry data, and (iii) the national statistical agency (*STATcube - Statistische Datenbank* of *Statistik Austria*) for population and communal tax data. All data were retrieved in September 2020.

We construct a synthetic control municipality in two steps. In the first step, we select a subsample of 5% of the available municipalities in the state of Lower Austria (25 out of 505 municipalities) that are most similar to Gramatneusiedl. None of these municipalities experienced relevant changes of labor market policy or other major economic shocks during the study period. Similarity is again measured in terms of the Mahalanobis distance in covariate space. The covariates used are listed in Table A.1 in section A.1. The averages of these covariates for both Gramatneusiedl and the (synthetic) control municipalities are shown in Table A.2 in section A.1. Most of our covariates are based on observations for the year 2019 (as measured in December). In addition to these covariates, we also include some covariates measured in July of 2020, after the onset of the Covid pandemic, to control for possibly heterogeneous impacts of this pandemic across municipalities. The averages of these covariates are shown in the bottom panel of Table A.2.

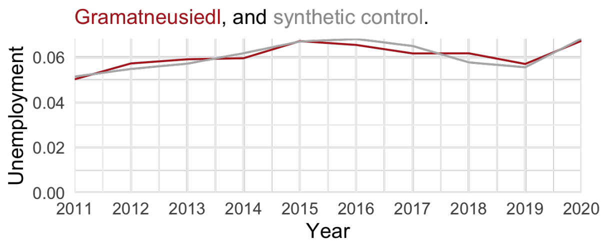
In the second step, we construct a synthetic control based on these 25 municipalities, using the approach described in Abadie et al. (2010) and reviewed in Abadie (2019). This synthetic control is chosen to match the same list of covariates used in the first step (where we selected a subsample of municipalities), as well as additionally the trajectory of unemployment rates (i.e., the number of unemployed as a share of the working age population; monthly unemployment numbers are averaged across the year) in Gramatneusiedl from 2011 to 2020, that is, for the 10 years preceding the intervention. Unemployment is the primary municipality-level outcome of interest in our analysis below. Program effects on unemployment include direct, anticipation, and equilibrium effects.

The resulting weights are shown in the table at the left of Figure 2.1, which lists all municipalities with non-negligible weights. The location of these municipalities is shown in Figure A.1 in section A.1. The right side of Figure 2.1 shows the time series of the predicted unemployment rate using the synthetic control, and the corresponding realized time series of unemployment for Gramatneusiedl in the 10 years preceding the intervention. Table A.2 in section A.1 similarly compares the covariate values for Gramatneusiedl with those for the synthetic control as well as those for each of the municipalities with positive synthetic control weights.

Approach 3: Individual-level comparison to control municipalities Our third approach is based on data for individuals from the three municipalities with the

Fig. 2.1: Synthetic control weights, and unemployment trajectory

Weight	Municipality
0.487	Ebreichsdorf
0.203	Zeillern
0.134	Rußbach
0.079	Leopoldsdorf im Marchfelde
0.046	Strasshof an der Nordbahn
0.024	Sieghartskirchen
0.023	Sollenau



largest weight in the synthetic control (Ebreichsdorf, Zeillern, Rußbach). Taken together, the weights of these three municipalities constitute 82.4% of our synthetic control. We construct a control group for program participants in Gramatneusiedl from the set of long-term unemployed individuals in these three municipalities. We consider all individuals who were unemployed for at least 9 months as of September 2020; this is the eligibility criterion for program participation in Gramatneusiedl.

We conducted two surveys in the control municipalities, in February 2021 and in February 2022. We furthermore have administrative data for all these individuals, including the same set of baseline covariates that was used for the construction of matched pairs in our experimental design. We obtain a sample of 71 individuals who answered all survey questions and satisfy the inclusion criteria. Of these 71 individuals, the majority are from Ebreichsdorf (62 individuals); the remainder are from Rußbach and Zeillern. Our third approach compares the outcomes of these individuals in the control towns to the outcomes of program participants (Group 1 in February 2021, and both Group 1 and 2 in February 2022), as well as future program participants (Group 2 in February 2021) in Gramatneusiedl.

To verify that the sample of control town individuals is similar to the set of participants, we again compare their baseline covariates. Table A.3 in section A.1 shows that there are no significant differences in baseline covariate means across the towns considered, with the exception of benefit levels, which are slightly higher among control individuals, and (marginally) age, which is also higher in the control towns. When estimating treatment effects in Section 2.4, we adjust for baseline covariates to correct for any remaining imbalances between the long-term unemployed in Gramatneusiedl and in the control municipalities.

2.3.2 Causal interpretation of estimands – spillover effects and anticipation effects

Formal framework In order to discuss the interpretation of our estimates in terms of spillover effects and anticipation effects, it is useful to introduce some formalism, where we loosely follow the approach of Graham et al. (2010). Let Y_i denote an outcome for individual i, such as employment status or income. Let D_i denote current eligibility for the job guarantee, and D_i^{+1} future eligibility, at some fixed time horizon. Let $\overline{D}$ be the share of long-term unemployed in the municipality who are currently eligible. Let finally ϵ_i be a vector of unobserved individual characteristics, which are not affected by the program. We can then assume that

$$Y_i = g(D_i, D_i^{+1}, \overline{D}, \epsilon_i), \tag{2.1}$$

where g is a structural function determining counterfactual outcomes. The dependence of g on D captures direct treatment effects, the dependence on D^{+1} captures anticipation effects, and the dependence on $\overline{D}$ captures equilibrium (spillover) effects. Let L_i be an indicator for unemployment longer than 9 months as of September 2020, which determines eligibility for participation in our experiment, and let expectations average over the distribution of unobserved heterogeneity ϵ_i for the treated municipality, Gramatneusiedl.

Identifying contrasts With this notation, we can now describe the identified averages from our three evaluation approaches in structural terms. Table 2.3 provides a mapping from these averages to the structural notation. Correspondingly, Table 2.4 provides a mapping from the contrasts we have been discussing so far to the corresponding average

Table 2.3: Identified averages

Group 1, Feb 21	$E[g(1,1,\frac{1}{2},\epsilon_i)\vert L_i=1]$
Group 2, Feb 21	$E[g(0,1,\frac{1}{2},\epsilon_i)\vert L_i=1]$
Both groups, after April 21	$E[g(1,1,1,\epsilon_i)\vert L_i=1]$
Control town individuals	$E[g(0,0,0,\epsilon_i)\vert L_i=1]$
Short-term unemp, GN, after April 21	$E[g(0,0,1,\epsilon_i)\vert L_i=0]$
Short-term unemp, synthetic control	$E[g(0,0,0,\epsilon_i)\vert L_i=0]$
Total unemp, GN, after April 21	$E[g(L_i,L_i,1,\epsilon_i)]$
Total unemp, synthetic control	$E[g(0,0,0,\epsilon_i)]$

structural effects. For simplicity of notation, we neglect any possible non-stationarity in the distribution of ϵ_i; in principle, everything should be subscripted by time t.

Let us interpret these identified objects, as listed in Table 2.4. The experimental comparison of Group 1 to Group 2, in February 2021, identifies an **average direct effect on the treated**, where both spillover effects and anticipation effects are held constant across the two groups. The comparison of both groups, after April 2021, to control town individuals identifies the **average total effect on the treated**, which incorporates direct effects, anticipation effects, and spillover effects.

The comparison of Group 2 to control town individuals, again in February 2021, identifies a combination of spillover and anticipation effects. Under the plausible additional assumption that these eligible individuals are not impacted by spillover effects, because they anticipate employment outside the market, $E[g(0,1,\frac{1}{2},\epsilon_i)|L_i=1] = E[g(0,1,0,\epsilon_i)|L_i=1]$, this contrast identifies the **average anticipation effect on the treated**, $E[g(0,1,0,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i=1]$.

Turning to our synthetic control comparisons, the identified object depends on the outcome considered. For short-term unemployment, the comparison of Gramatneusiedl to the synthetic control identifies the **average spillover effect on the untreated**. Here we assume that there are no anticipation effects impacting the short-term unemployed, who are not currently eligible for program participation, but might become so after a longer

Table 2.4: Identified effects and roadmap

Contrast	Identified effect	Interpretation	Figures and Tables
February 2021			
Group 1 vs. Group 2	$E[g(1,1,\frac{1}{2},\epsilon_i) - g(0,1,\frac{1}{2},\epsilon_i) \mid L_i = 1]$	Average direct effect on the treated	Figure 2.2, Figure 2.3, Table 2.5 Figure A.3
Group 2 vs. control town	$E[g(0,1,\frac{1}{2},\epsilon_i) - g(0,0,0,\epsilon_i) \mid L_i = 1]$	Average anticipation effect on the treated	Figure 2.6, Figure 2.7, Table 2.6 Table 2.7, Figure A.4
After April 2021			
Group 1 & 2 vs. control town	$E[g(1,1,1,\epsilon_i) - g(0,0,0,\epsilon_i) \mid L_i = 1]$	Average total effect on the treated	Figure 2.6, Figure 2.7 Figure A.5
Gramatneusiedl vs. synth (short-term unemp)	$E[g(0,0,1,\epsilon_i) - g(0,0,0,\epsilon_i) \mid L_i = 0]$	Average spillover effect on the untreated	Figure 2.4, Figure 2.5
Gramatneusiedl vs. synth (total unemp)	$E[g(L_i,L_i,1,\epsilon_i) - g(0,0,0,\epsilon_i)]$	Average total effect	Figure 2.4, Figure 2.5

term.

For total unemployment, the comparison of Gramatneusiedl to the synthetic control identifies the **average total effect** of the program. This effect combines the average total effect on the treated, $E[g(1,1,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 1]$, and the average spillover effect on the untreated, $E[g(0,0,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 0]$, i.e.,

$$
\begin{aligned}
E[g(L_i, L_i, 1, \epsilon_i) - g(0,0,0,\epsilon_i)] =& E[g(1,1,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 1] \cdot P(L_i = 1) + \\
& E[g(0,0,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 0] \cdot P(L_i = 0). \quad (2.2)
\end{aligned}
$$

2.3.3 Inference

Individual-level randomization inference To perform inference for the individual-level treatment effects in the pairwise randomized experiment, we consider permutations of treatments, that is, randomization inference. This approach allows us to test the null hypothesis that the intervention had no effect, that is, $Y_i^1 = Y_i^0$ for all individuals i and potential outcomes Y_i^1, Y_i^0.

We re-assign treatment at random *within* each of the matched pairs of participants. For this counterfactual treatment assignment, we can re-calculate any given test-statistic, such as the difference in means between groups. Repeating this process many times, we calculate the share of re-assignments for which the difference in means is bigger than the realized value of the difference in means. This share is the p-value for the null hypothesis of no effects.

Municipality-level permutation inference for the synthetic control Our inference for the synthetic control method relies on the permutation approach as described in Abadie et al. (2010). This approach is analogous to the randomization inference approach at the individual level. We consider Gramatneusiedl and each of the 25 control municipalities based on which the synthetic control for Gramatneusiedl was constructed. For each of these, we calculate a synthetic control based on the other 25 municipalities and use this synthetic control to predict outcomes in the post-intervention period. The share of these municipalities for which the resulting gap between realized and predicted outcomes is larger than for Gramatneusiedl can then be interpreted as a p-value for the null-hypothesis that the intervention had no effect on these outcomes for Gramatneusiedl.

Attrition and survey non-response We made an effort to keep attrition to a minimum. We could follow all individuals through administrative data. We thus have complete data for employment outcomes, in particular, in both Gramatneusiedl and the control towns.

For the surveys in Gramatneusiedl, we achieved a survey response rate of 73% in 2021 (with complete questionnaires for 69%) and of 77% in 2022 (with complete questionnaires for 73%). Only seven individuals did not participate in either of the surveys. Following up, we documented the reasons for their non-response: Two persons found a regular job before the program started, and two program participants refused to complete the survey out of general privacy concerns. One person moved abroad, one unsubscribed from seeking a job, and one became seriously ill. Others participated only in one of both surveys due to serious illness or because of unavailability due to incarceration or having passed away.

We achieved lower response rates in the control towns, with 34% in 2021 and 30% in 2022. The difference in response rates is likely due to the fact that program participants in Gramatneusiedl were reminded to participate in the online survey by *it.works* and their job counselor, while participants in the control towns were only reminded by the call center of the public employment service. We adjust for baseline covariates (their means are reported in Table A.3, as discussed above) when comparing individual outcomes across towns to mitigate the impact of possibly selective non-response.

2.4 Findings

We are now ready to discuss our empirical findings. We will consider a large number of outcomes and contrasts.[7] Our headline findings are summarized by Figures 2.2 through 2.7 in this section, as well as Figures A.3 through A.5 in section A.1. Individual-level estimates are also shown numerically in Table 2.5 through Table 2.7.

Individual-level outcomes and outcome indices in these figures and tables are normalized as follows: (i) They have a potential range from 0 to 1, and (ii) higher values represent "better" outcomes (e.g., lower unemployment, higher income, lower anxiety, etc.); recall that variables where the sign is flipped are marked by *(-)* in all our figures. Additional figures with results for further outcomes, alternative identification approaches, confidence intervals, and robustness checks can be found in section A.1. Table 2.4 provides a roadmap through the findings presented in this section and in the appendix.

2.4.1 Experimental comparison

We first consider the experimental comparison between program participants in Group 1, who started employment in December 2020, and participants in Group 2, who started employment in April 2021. We estimate the short-term individual effects of the program by comparing Groups 1 and 2 using data from February 2021, from both administrative sources and a survey that we administered.

Figure 2.2, Figure 2.3, and Table 2.5 show estimates for this experimental comparison. The left panels in both figures shows average outcomes for the treatment and control group, adjusting for covariates. The right panels shows p-values for the null of a zero treatment effect. These p-values are based on randomization inference, using 1000 simulation draws, where we permute treatment within pairs. Random permutation within pairs corresponds

[7]The code implementing the following analysis has been uploaded to GitHub, at https://github.com/maxkasy/Marienthal_Analysis.

to our experimental design using pairwise matched randomization.

All of these estimates should be interpreted as “intention to treat” effects. If we make the additional assumption that all effects are mediated by employment, these estimates can be scaled up by the effect of treatment on the probability of employment on a random day, which yields instrumental variable estimates of the local average treatment effect of employment. The effect of assignment on employment is estimated to be around .5, so that the corresponding instrumental variable estimates of all treatment effects would be about double the reported intention to treat effects.

The estimates in Figure 2.2, Figure 2.3, and Table 2.5 control linearly for baseline covariates, to adjust for potential non-random attrition in the survey. Figure A.6 and Figure A.7 in section A.1 display analogous findings without controls, and with controls for pair fixed effects. In both cases, the resulting estimates are close to those in our preferred specification using linear controls. Figure A.3 in section A.1 further shows confidence intervals for treatment effects, based on robust standard errors for the regressions with linear controls.

Findings For economic outcomes (shown in the top panels of Figure 2.2 and Table 2.5), measured using both survey and administrative data, we find highly significant positive effects.[8] Unemployment is strongly reduced in Group 1 through program participation. This is not due to transitions out of the labor force (e.g., to early retirement or disability status). Instead, our estimates show that this effect is fully driven by the increase in employment.

Participants who accept a guaranteed job increase their income. The estimates shown in Figure 2.2 and Table 2.5) imply an average increase of 392 Euro per month, from an average of 888 Euro to an average of 1280 Euro per month. While the control group, Group 2, receives unemployment benefits, the treatment group, Group 1, enters jobs

[8] Recall the normalization of these outcome variables from Table 2.1: Employment and unemployment are defined as the share of days since the program started, and the monthly income is divided by 2000.

that are remunerated according to the floor set by collective bargaining in Austria, for the respective occupation and experience categories. Correspondingly, as shown by our estimates, program participation results in both increased income and economic security.

Turning to non-economic outcomes (bottom panels of Figure 2.2 and middle panel of Table 2.5), we see a more heterogeneous picture. For some outcomes, in particular those related to social status, subjective health, mental health, social network, number of contacts, and preferences, we do not find a significant effect. Disaggregating the preference index into its components in Figure 2.3 and the bottom panel of Table 2.5, we correspondingly find no effects on risk- or time-preferences, or personality traits. These findings provide a placebo test of our experimental design and identification approach. A priori, it would not be plausible to find short-term effects of employment on physical health or preferences. The fact that we indeed do not find such effects increases our confidence that survey answers are not driven by interviewer demand effects, in particular.

By contrast, we do find large and significant effects of the program on Covid stress, subjective well-being and its change over time, and in particular on the index measuring the "latent and manifest benefits" of work. Disaggregating the latter again, Figure 2.3 and the bottom panel of Table 2.5 show significant effects of participation on several components of this index, including activity, social recognition, and financial strain, and positive but marginally insignificant effects on time structure, collective purpose, and social interactions.

These effects are remarkable not only in their own right, but also because of the historical importance of Marienthal, which was the location of the original Jahoda et al. (2017) study, and because of the literature on the sociology of work which connects our study to Jahoda et al. (2017). The LAMB scale[9] was developed to quantify Jahoda's insight (Jahoda, 1982), based on the Marienthal study and subsequent work, that

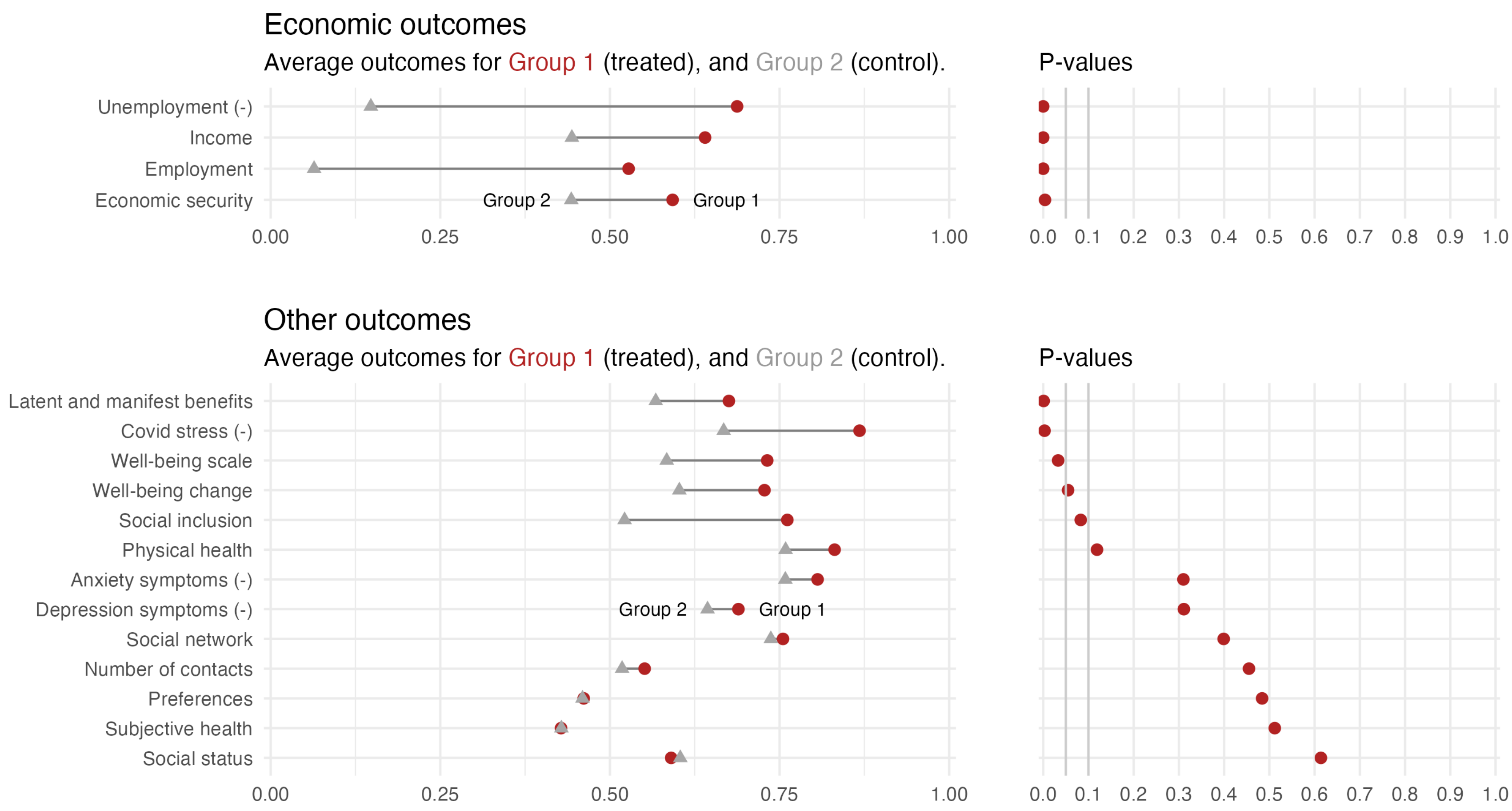
"[individuals] have deep-seated needs for structuring their time use and

[9]We thank Adam Coutts for pointing us to this line of work in sociology (Kovacs et al., 2017, 2019; Knight et al., 2020).

> perspective, for enlarging their social horizon, for participating in collective enterprises where they can feel useful, for knowing they have a recognised place in society, and for being active."

The LAMB scale measures these “latent” benefits (time structure, activity, social contact, collective purpose, and social recognition), in addition to the “manifest” material benefits (income) resulting from employment. Jahoda’s insights regarding the detrimental impact of unemployment, as witnessed in the Great Depression, are thus quantitatively validated by our experimental study a century later, in the same location, in a program where we document the positive impact of employment on the formerly unemployed.

Fig. 2.2: Experimental estimates with linear controls



Notes: The left hand figures show average outcomes for the treated and control group, adjusting for baseline covariates. The outcome variables are defined in Table 2.1. Higher values imply better outcomes. Outcomes are scaled to range from 0 to 1. Income is monthly income divided by 2000, and unemployment is share of days *not* unemployed since Oct 1, 2020. The right hand figures show p-values for tests of the null of a zero or negative effects of treatment. Small values imply positive effects of treatment. These p-values are based on 1000 simulation draws. These estimates are also tabulated in Table 2.5.

Table 2.5: Experimental estimates with linear controls

Economic outcomes

Outcome	Treated	Control	Difference	p-value	SE	n_1	n_2
Employment	0.528	0.064	0.464	0.000	0.070	31	31
Unemployment (-)	0.687	0.148	0.540	0.000	0.067	31	31
Income	0.640	0.444	0.196	0.000	0.072	19	19
Economic security	0.592	0.443	0.149	0.004	0.055	21	22

Other outcomes

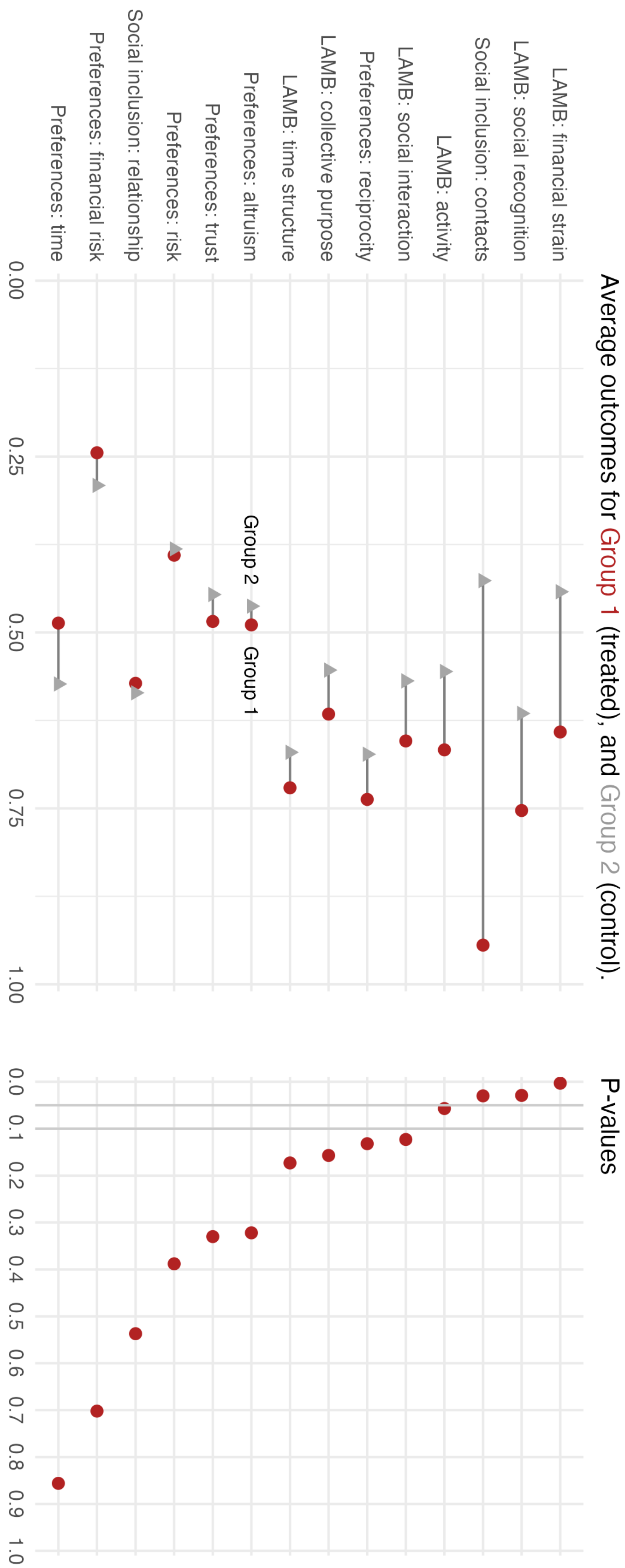
Outcome	Treated	Control	Difference	p-value	SE	n_1	n_2
Latent and manifest benefits	0.675	0.568	0.108	0.001	0.042	21	22
Covid stress (-)	0.868	0.668	0.200	0.003	0.072	20	22
Well-being scale	0.732	0.584	0.148	0.033	0.076	20	22
Well-being change	0.728	0.602	0.125	0.055	0.080	21	22
Social inclusion	0.761	0.522	0.240	0.083	0.198	21	22
Physical health	0.831	0.759	0.072	0.119	0.054	20	22
Anxiety symptoms (-)	0.806	0.759	0.048	0.310	0.082	20	22
Depression symptoms (-)	0.689	0.644	0.045	0.311	0.072	20	22
Social network	0.755	0.737	0.018	0.399	0.064	12	12
Number of contacts	0.551	0.518	0.033	0.455	0.258	21	22
Preferences	0.461	0.460	0.002	0.484	0.032	21	22
Subjective health	0.428	0.428	0.000	0.512	0.065	20	22
Social status	0.590	0.604	-0.013	0.614	0.052	21	22

Disaggregated outcomes

Outcome	Treated	Control	Difference	p-value	SE	n_1	n_2
LAMB: financial strain	0.641	0.442	0.199	0.003	0.073	21	22
LAMB: social recognition	0.753	0.615	0.138	0.029	0.080	21	22
Social inclusion: contacts	0.944	0.426	0.518	0.030	0.347	21	21
LAMB: activity	0.667	0.555	0.111	0.057	0.056	21	22
LAMB: social interaction	0.654	0.569	0.085	0.123	0.068	21	22
Preferences: reciprocity	0.737	0.673	0.064	0.132	0.061	20	22
LAMB: collective purpose	0.616	0.553	0.063	0.157	0.065	21	22
LAMB: time structure	0.721	0.670	0.050	0.173	0.061	21	22
Preferences: altruism	0.489	0.463	0.027	0.322	0.057	20	22
Preferences: trust	0.484	0.446	0.038	0.330	0.087	20	22
Preferences: risk	0.390	0.381	0.009	0.388	0.046	20	22
Social inclusion: relationship	0.572	0.586	-0.014	0.537	0.163	21	21
Preferences: financial risk	0.245	0.291	-0.046	0.702	0.083	21	22
Preferences: time	0.487	0.573	-0.087	0.856	0.080	21	22

Notes: These tables report the same estimates as Figure 2.2 and Figure 2.3. P-values are based on randomization inference, SE are robust standard errors for the treatment effect (difference). n_1 and n_2 are the number of treated and control observations, respectively.

Fig. 2.3: Experimental estimates with linear controls, disaggregated outcomes



2.4.2 Synthetic control municipalities

We next consider the comparison of municipality-level outcomes between Gramatneusiedl and the pre-registered synthetic control. For this comparison, we use municipality-level administrative data on unemployment (total, long-term, and short-term), employment, and inactivity. Our synthetic control estimates are shown in Figure 2.4 and Figure 2.5. The top row of these figures plots the realized trajectory for Gramatneusiedl against the realized trajectory for the synthetic control. The plots show outcomes for both the pre-period and since the start of the program.

The monthly series for unemployment (total, long-term, and short-term) align remarkably well between Gramatneusiedl and the synthetic control in the pre-period. Note that this is not mechanical: The construction of the synthetic control used only *annual* total unemployment for the preceding decade, and was not based on these *monthly* series.

The second row of Figure 2.4 and Figure 2.5 plots the gap between Gramatneusiedl and the synthetic control, and the corresponding gap for 25 permutations.[10] This permutation approach provides a formal analog to randomization inference. For each of the permutations, we consider another municipality as fictitiously treated, construct a synthetic control for this municipality, and plot the corresponding outcome gap. Extreme gaps for Gramatneusiedl, relative to these permutations, indicate program effects that are arguably not just driven by random fluctuations. Correspondingly, the last row of these figures plots the rank of Gramatneusiedl among the permutations.

When interpreting the following findings, it is important to note that program eligibility was determined based on residency in the *municipality* of Gramatneusiedl, while our aggregate data are available at the level of a *zip code*. This zip code is a larger geographic unit than the municipality of Gramatneusiedl. In particular, in September 2020 about 50% of the long-term unemployed individuals residing in the zip code were also residents

[10]Figure A.2 in section A.1 provides an analogous figure for the 10 years prior to the program, where unemployment gaps are close to 0 mechanically, by construction of the synthetic controls.

of the municipality, and thus eligible to participate in MAGMA.

Findings As expected, the program has a large effect on long-term unemployment in the municipality. By the time both groups of eligible participants are enrolled in the program, in April 2021, long-term unemployment has been reduced by about 1.5 percentage points, down to less than 1% as a share of the working age population. This is a larger reduction than for any of the 25 permutation municipalities. Recall that all long-term unemployed residents of Gramatneusiedl are eligible to enroll in the program after April 2021, but participation is voluntary. Our estimates reflect the fact that the program was successfully implemented and take-up was widespread.

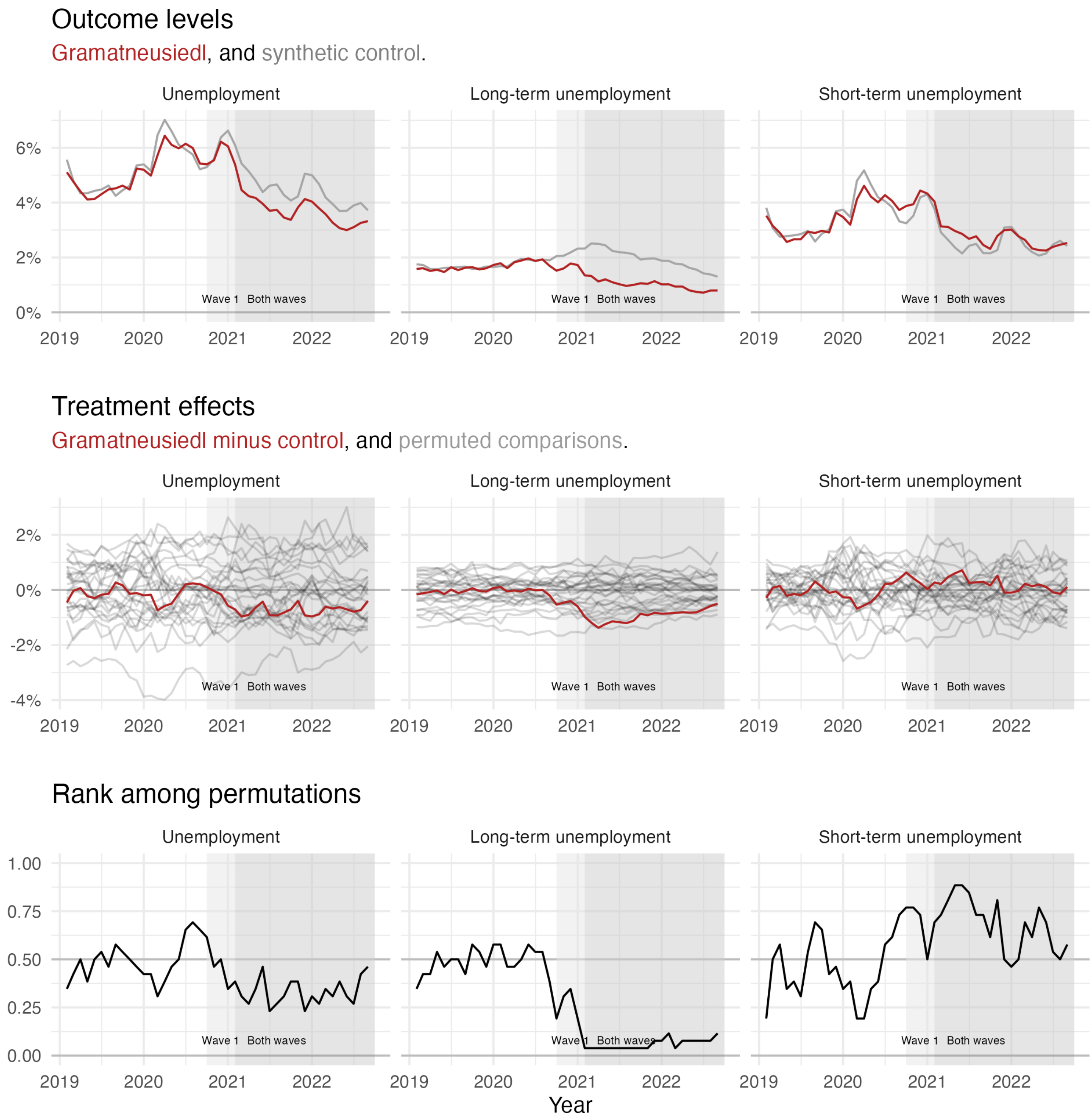
Consider next the impact of the program on total unemployment, which is the sum of long-term and short-term unemployment. This total impact is negative. The synthetic control estimate suggests a reduction of the unemployment rate by about 1 percentage point, from 5% to 4% in 2021, and from 4% to about 3% in 2022. Correspondingly, Gramatneusiedl is around the 30th percentile in terms of the relative reduction of unemployment, compared to the permutation municipalities. This total effect suggests that the program was successful in reducing unemployment in the aggregate, and did not simply lead to crowd-out of other forms of employment.

Any gap between our estimated effects on long-term and total unemployment is the effect on short-term unemployment. There are some fluctuations over time, but it appears that Gramatneusiedl experienced no increase of short-term unemployment relative to the synthetic control. The estimated relative increase fluctuates around the 60th percentile among permutation municipalities. This suggests that there were no systematic negative spillovers of the job guarantee on the short-term unemployed, who are not eligible to participate.

One might conjecture that the reduction of unemployment is driven by a transition of the unemployed out of the labor force, for instance into (early) retirement or into a certified disabled status, in order to avoid work requirements associated with the job guarantee. That this is not the case for the program studied here is verified by Figure 2.5. The left column of this figure shows effects on employment, and the right shows effects on "inactivity" (i.e., the share out of the labor force). As reflected in this figure, the increase of employment in Gramatneusiedl, relative to the synthetic control, was about the same as the reduction of unemployment.[11] Put differently, rather than inducing the unemployed to transition out of the labor force altogether, the program might have had the opposite effect.

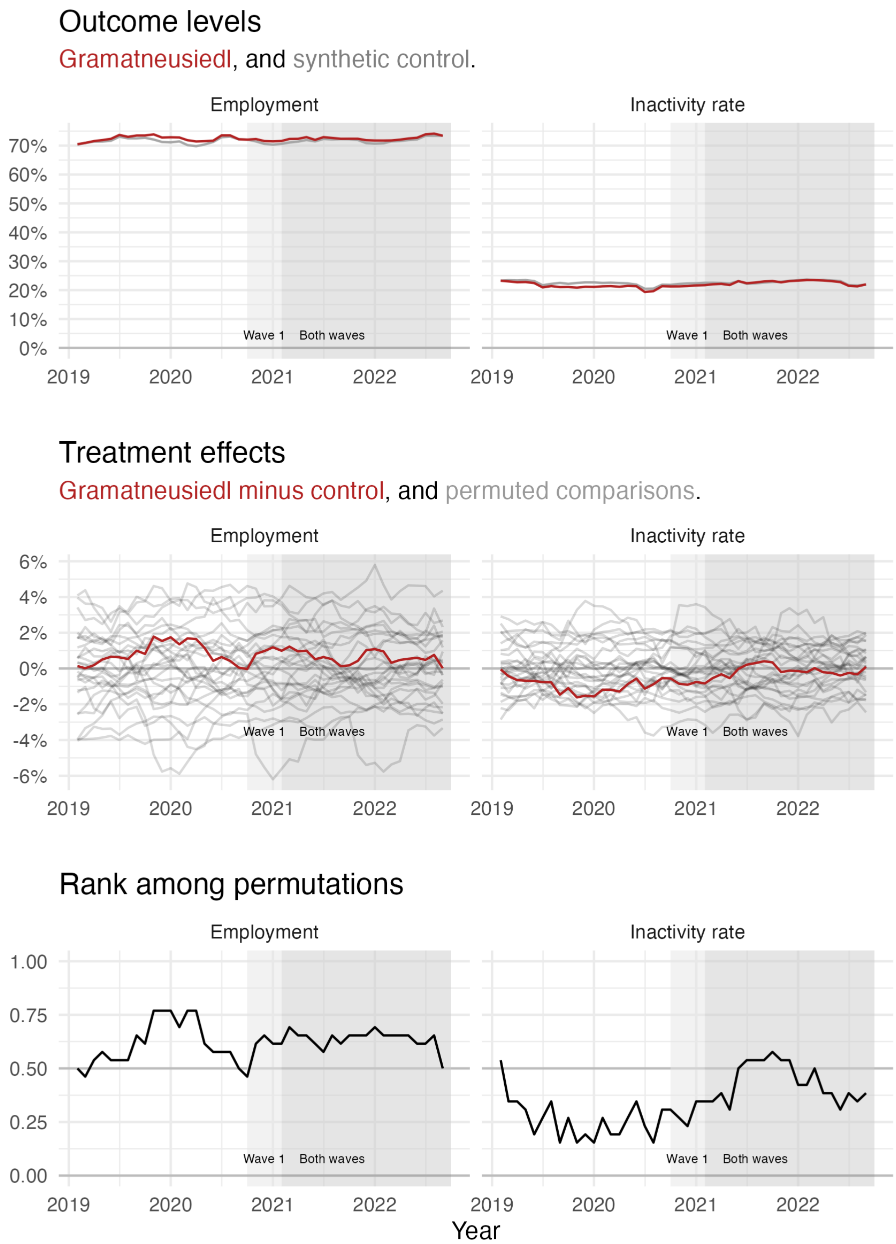
[11] While unemployment, employment, and inactivity sum almost to 1, there is a small residual category of people who are currently in AMS training. This category amounts to about 1-2% of the population, who are not included in either of the three other categories. If anything, there was a small reduction of the rate of "inactivity."

Fig. 2.4: Synthetic control estimates of the program effect on unemployment



Notes: Monthly series of municipality-level outcomes from administrative data. The top row shows outcomes for Gramatneusiedl and for the synthetic control. The absence of a gap in the pre-period is not mechanical, since the synthetic control was constructed based on *annual* data on total unemployment. The middle row shows gaps (estimated treatment effects) relative to the synthetic control where, for each of 25 comparison municipalities, a synthetic control is constructed. The bottom row shows the rank of the gap for Gramatneusiedl relative to these comparison municipalities, providing the analog of a p-value.

Fig. 2.5: Synthetic control estimates of the program effect on employment and inactivity



2.4.3 Comparison to individuals in control towns

We finally turn to our third and last identification approach. For this approach, we compare participants in both Group 1 and Group 2 to similar individuals in three of the towns that are part of our synthetic control. We have surveyed individuals in the towns of Ebreichsdorf, Zeillern, and Rußbach, which are the three towns with the largest synthetic control weights, amounting to 82.4% of our synthetic control. We contacted individuals in these towns who were selected based on the same criteria as program participants in Gramatneusiedl. In particular, these are individuals who had unemployment spells of at least 9 months in September 2020. We observe the same baseline covariates for these individuals as we used for the construction of our matched pairs in the experimental sample. The reported estimates adjust for any differences in these baseline covariates. We observe administrative and survey outcome data in February 2021 (when Group 1 was treated, but Group 2 was not yet treated), and February 2022 (when both groups had been treated for at least 10 months).

The resulting estimates are shown in Figure 2.6 and Table 2.6 for economic outcomes and Figure 2.7 and Table 2.7 for other outcomes. In both figures, we show outcomes for 2021 at the top, where we separate individuals in Group 1, Group 2, and the control towns, and outcomes for 2022, where we compare all eligible individuals in Gramatneusiedl (Group 1 and 2), to individuals in the control towns.

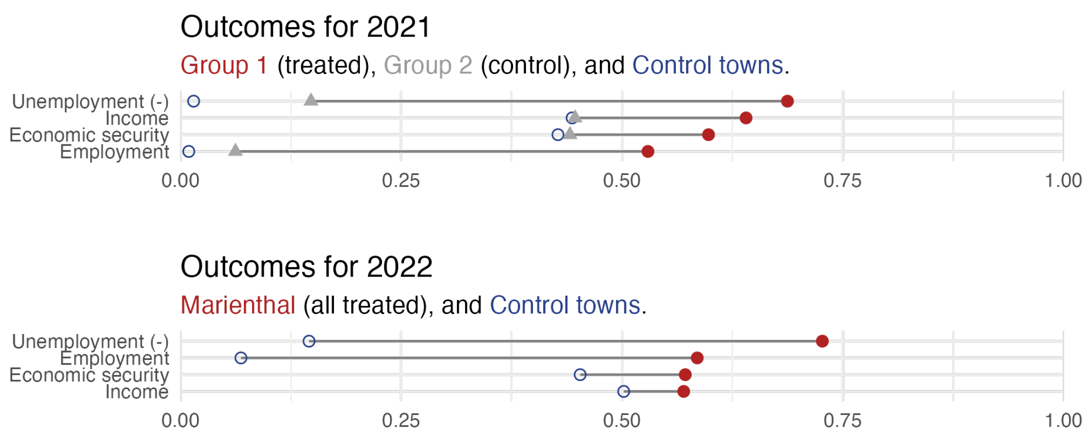
Figure A.4 and Figure A.5 show corresponding confidence intervals. Figure A.4 contrasts Group 2 to control town individuals in 2021, thus providing an estimate of the average anticipation effect on the treated. Figure A.5 contrasts both groups to control town individuals in 2022, thus providing an estimate of the average total effect on the treated.

Findings For income and economic security, the comparison to control town individuals yields estimates that are indistinguishable from the estimates based on the experimental comparison. The same holds for the leading non-economic outcomes, in particular the latent and manifest benefits of work, and Covid stress. Similarly, for the preference index and for subjective health, no effects are found in either comparison.

These findings again corroborate our identification approaches (which rely on alternative identifying assumptions), and increase the confidence in our findings. Furthermore, these effects on income and economic security, latent and manifest benefits, and Covid stress persist into 2022. These are thus not just short-term effects, but are effects maintained over the course of the program.

For unemployment, social status, and subjective well-being, the comparison to control towns yields even stronger effects in 2021 than the experimental comparison. This suggests the presence of some anticipation effects. Both social status and well-being change increase prior to the start of employment. Overall, however, the scope of these anticipation effects, as experienced during the training phase, appears rather limited, and most of the program benefits only manifest after the start of employment.

Fig. 2.6: Control town comparisons with linear controls, economic outcomes



Notes: These estimates are also tabulated in Table 2.6.

Table 2.6: Control town comparisons with linear controls, economic outcomes

2021

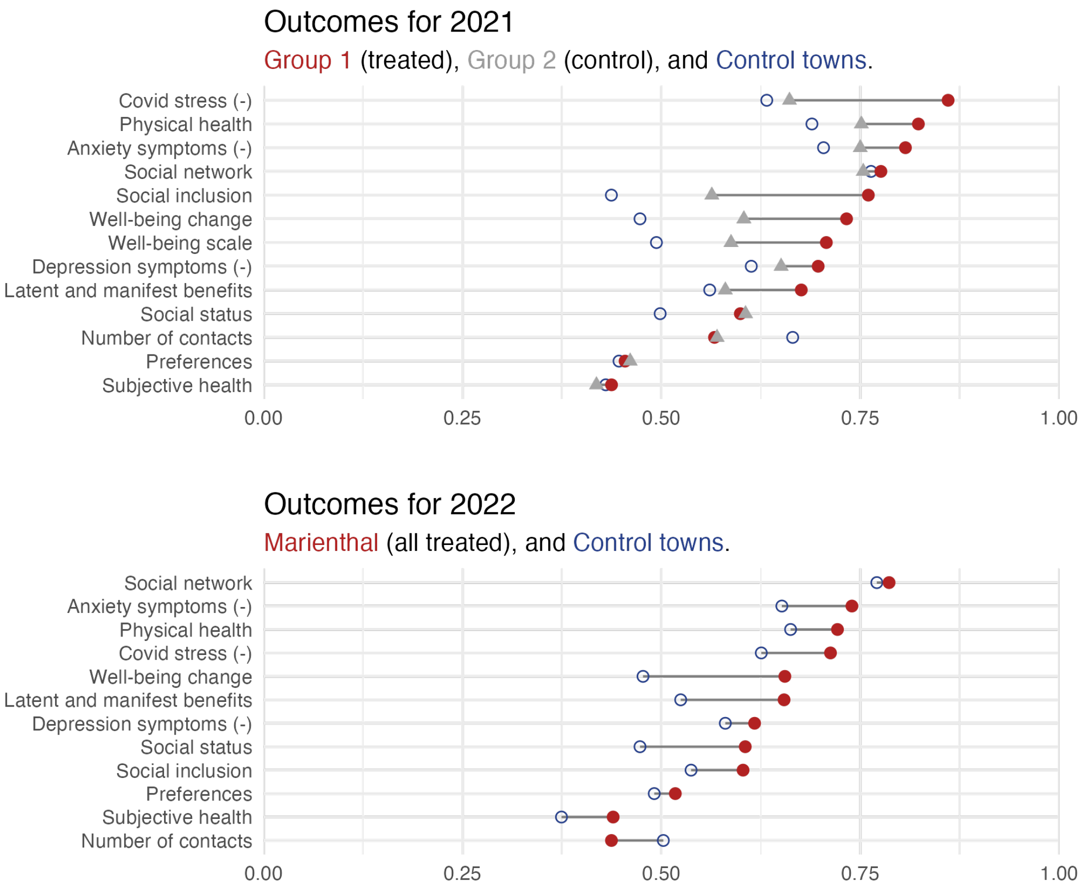
Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	n_1	n_2	n_{ct}
Unemployment (-)	0.687	0.148	0.015	0.132	0.054	31	31	71
Income	0.640	0.447	0.443	0.009	0.016	19	19	59
Economic security	0.598	0.441	0.427	0.012	0.038	21	22	63
Employment	0.529	0.062	0.009	0.060	0.040	31	31	71

2022

Outcome	Marienthal	Control towns	Mt vs. Ct towns	SE	n_{mt}	n_{ct}
Unemployment (-)	0.727	0.146	0.581	0.039	62	64
Employment	0.585	0.068	0.517	0.049	62	64
Economic security	0.572	0.453	0.119	0.037	45	61
Income	0.570	0.502	0.068	0.035	42	56

Notes: These tables report the same estimates as Figure 2.6, Figure A.4, and Figure A.5. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Marienthal and Control town observations.

Fig. 2.7: Control town comparisons with linear controls, other outcomes



Notes: These estimates are also tabulated in Table 2.7.

Table 2.7: Control town comparisons with linear controls, other outcomes

2021

Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	n_1	n_2	n_{ct}
Covid stress (-)	0.860	0.661	0.632	0.027	0.067	20	22	62
Physical health	0.823	0.751	0.689	0.059	0.054	20	22	62
Anxiety symptoms (-)	0.807	0.750	0.704	0.040	0.062	20	22	62
Social network	0.776	0.754	0.764	-0.013	0.033	12	12	45
Social inclusion	0.760	0.563	0.437	0.124	0.100	21	22	66
Well-being change	0.733	0.604	0.473	0.144	0.059	21	22	71
Well-being scale	0.707	0.588	0.494	0.084	0.063	20	22	62
Depression symptoms (-)	0.697	0.651	0.613	0.030	0.065	20	22	62
Latent and manifest benefits	0.676	0.580	0.561	0.018	0.039	21	22	68
Social status	0.599	0.606	0.498	0.115	0.051	21	22	68
Number of contacts	0.567	0.570	0.665	-0.057	0.143	21	22	66
Preferences	0.454	0.461	0.447	0.015	0.027	21	22	63
Subjective health	0.437	0.418	0.430	-0.006	0.057	20	22	61

2022

Outcome	Marienthal	Control towns	Mt vs. Ct towns	SE	n_{mt}	n_{ct}
Social network	0.786	0.771	0.015	0.040	26	39
Anxiety symptoms (-)	0.740	0.651	0.088	0.061	44	58
Physical health	0.721	0.662	0.059	0.040	44	58
Covid stress (-)	0.713	0.626	0.087	0.061	42	53
Well-being change	0.655	0.477	0.178	0.051	45	62
Latent and manifest benefits	0.654	0.524	0.130	0.030	45	60
Depression symptoms (-)	0.617	0.580	0.037	0.051	44	58
Social status	0.605	0.473	0.132	0.034	46	62
Social inclusion	0.603	0.537	0.065	0.100	45	61
Preferences	0.518	0.491	0.026	0.019	44	58
Subjective health	0.439	0.374	0.065	0.052	44	58
Number of contacts	0.437	0.502	-0.065	0.102	47	61

Notes: These tables report the same estimates as Figure 2.7, Figure A.4, and Figure A.5. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Marienthal and Control town observations.

2.5 Conclusion

We conclude by summarizing our evaluation approaches and main findings, before discussing bigger-picture takeaways and avenues for future research. Our evaluation is based on several experimental and non-experimental contrasts, as summarized in Table 2.4. We use an experimental staggered roll-out design, comparing earlier and later entrants into the program, to identify direct effects of the job guarantee on the treated. We use a synthetic control approach at the municipality level to identify spillover effects of the job guarantee on the untreated, as well as the average total effect of the job guarantee on the labor market. And we compare program participants to observationally similar individuals in control towns, to separate out anticipation effects, and to estimate the long-term effects of the job guarantee.

Assignment to the two groups (early and late entrants) in the experimental comparison is based on pairwise matched random assignment. This approach allows us to increase the precision of our estimates by making the two groups observationally as similar as possible. This reduces standard errors relative to conventional random assignment, which is particularly relevant given our small sample size. Both the pairwise matches and the synthetic control weights were pre-registered. This ties our hands and prevents us from cherry-picking results, including for the observational comparisons in our evaluation. Our inference approach is primarily based on randomization inference (permuatation inference). This guarantees finite sample validity without any asymptotic approximations. In section A.1, we also report conventional confidence intervals, using robust standard errors; the conclusions remain unchanged.

Turning to our empirical findings, a first remarkable fact is that everyone offered a job after the 8-week training phase accepted this job. In our experimental comparison, we find large positive effects of the job guarantee on participants' economic and non-economic well-being. This includes effects on employment, income, and income security, which are expected given the nature of the program. This also includes large positive effects on time structure, activity, social contacts, collective purpose, and social status. These non-econonomic effects of employment have been discussed in the sociological literature, mostly in the context of observational studies, but have received less attention in economics. We do not find effects on physical health and economic preferences, including time and risk preferences, reciprocity, altruism, and trust. The estimated effects persist over time.

We further find a large reduction of municipality-level unemployment, which is driven by a near-elimination of long-term unemployment. There appears to be no increase of short-term unemployment. While we were not able to independently verify program costs, it is estimated that the total cost per eligible particpant and year was around EUR 30,000, of which around EUR 20,000 were wages, taxes, and social insurance contributions for participants.

These findings have implications for both policy and future research. First, our findings suggest that the job guarantee is a promising policy instrument to reduce long-term unemployment, and to improve the well-being of the unemployed. Crucial for this conclusion was our focus on participant well-being. This contrasts with a focus on market employment as the primary outcome for most existing evaluations of active labor market programs.

Our study is based on a small-scale pilot program in a single municipality. It would be desirable to see evaluations at a larger scale, and in different contexts. Several international organizations have cited the Marienthal pilot as a promising example of a job guarantee, and have called for further pilots and evaluations, see for instance OECD (2021); ILO (2021); EU CoR (2023); UN Special Rapporteur (2023).

Turning to implications for future research in labor economics, our study points toward the importance of non-economic dimensions of employment. Labor economists conventionally model labor supply decisions as resulting from a trade-off between monetary returns and the disutility of work. Sociologists, however, have long recognized that employment also has non-economic benefits. While much of the existing evidence on these benefits is correlational, our study provides causal evidence for the importance of these non-economic benefits of employment. Explicit consideration of these non-economic benefits of employment might lead to a refined understanding in economics of labor supply and labor market dynamics more generally.

Bibliography

Abadie, A. (2019). Using synthetic controls: Feasibility, data requirements, and methodological aspects. *Journal of Economic Literature*, 59(2).

Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105(490):493–505.

Altmann, S., Falk, A., Jäger, S., and Zimmermann, F. (2018). Learning about job search: A field experiment with job seekers in Germany. *Journal of Public Economics*, 164:33–49.

Anderson, C., Hildreth, J. A. D., and Howland, L. (2015). Is the desire for status a fundamental human motive? A review of the empirical literature. *Psychological Bulletin*, 141(3):574–601.

ARTE (2021). Helping the Long-term Unemployed. RE: European Stories. Documentary film.

Athey, S. and Imbens, G. W. (2017). The Econometrics of Randomized Experiments. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevir.

Avendano, M. and Berkman, L. F. (2014). Labor Markets, Employment Policies, and Health. In Berkman, L. F., Kawachi, I., and Glymour, M. M., editors, *Social Epidemiology*, pages 182–233. Oxford University Press.

Banerjee, A., Duflo, E., Imbert, C., Mathew, S., and Pande, R. (2020). E-governance, Accountability, and Leakage in Public Programs: Experimental Evidence from a Financial Management Reform in India. *American Economic Journal: Applied Economics*, 12(4):39–72.

Basu, A. K., Chau, N. H., and Kanbur, R. (2009). A theory of employment guarantees: Contestability, credibility and distributional concerns. *Journal of Public Economics*, 93(3-4):482–497.

Baumeister, R. F. and Leary, M. R. (1995). The need to belong: Desire for interpersonal attachments as a fundamental human motivation. *Psychological Bulletin*, 117(3):497–529.

Beegle, K., Galasso, E., and Goldberg, J. (2017). Direct and indirect effects of Malawi's public works program on food security. *Journal of Development Economics*, 128:1–23.

Behaghel, L., Crépon, B., and Gurgand, M. (2014). Private and Public Provision of Counseling to Job Seekers: Evidence from a Large Controlled Experiment. *American Economic Journal: Applied Economics*, 6(4):142–174.

Bendix, A. (2020). Residents of a small Austrian town are being promised work for 3 years in the world's first universal jobs guarantee experiment. *Business Insider*.

Bertrand, M., Crépon, B., Marguerie, A., and Premand, P. (2017). Contemporaneous and Post-Program Impacts of a Public Works Program. *Working Paper*.

Böheim, R., Eppel, R., and Mahringer, H. (2022). More Caseworkers Shorten Unemployment Durations and Save Costs. Results from a Field Experiment in an Austrian Public Employment Office. *Working Paper*.

Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, 41(1):359–375.

Card, D. and Hyslop, D. R. (2005). Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers. *Econometrica*, 73(6):1723–1770.

Card, D., Kluve, J., and Weber, A. (2010). Active labour market policy evaluations: A meta-analysis. *The Economic Journal*, 120(548):F452–F477.

Card, D., Kluve, J., and Weber, A. (2018). What works? A meta analysis of recent active labor market program evaluations. *Journal of the European Economic Association*, 16(3):894–931.

Clark, A. E. (2003). Unemployment as a Social Norm: Psychological Evidence from Panel Data. *Journal of Labor Economics*, 21(2):323–351.

Clark, A. E. (2006). A Note on Unhappiness and Unemployment Duration. *IZA Discussion Paper*, 1(2406).

Clark, A. E. and Oswald, A. J. (1994). Unhappiness and Unemployment. *The Economic Journal*, 104(424):648.

Crépon, B., Duflo, E., Gurgand, M., Rathelot, R., and Zamora, P. (2013). Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment *. *The Quarterly Journal of Economics*, 128(2):531–580.

Crépon, B. and van den Berg, G. J. (2016). Active Labor Market Policies. *Annual Review of Economics*, 8(1):521–546.

Cummings, D. and Bloom, D. (2020). Can Subsidized Employment Programs Help Disadvantaged Job Seekers? A Synthesis of Findings from Evaluations of 13 Programs. OPRE Report 23, Office of Planning, Research, and Evaluation, Administration for Children and Families, U.S. Department of Health and Human Services, Washington, DC.

Eisenberg, P. and Lazarsfeld, P. F. (1938). The psychological effects of unemployment. *Psychological Bulletin*, 35(6):358–390.

EU CoR (2023). Zero long-term unemployment: The local and regional perspective. Rapporteur: Yonnec Polet. Technical report, European Union Committee of the Regions. Opinion of the Commission for Social Policy, Education, Employment, Research and Culture, Brussels.

Gelber, A., Isen, A., and Kessler, J. B. (2016). The Effects of Youth Employment: Evidence from New York City Lotteries. *The Quarterly Journal of Economics*, 131(1):423–460.

Graham, B., Imbens, G., and Ridder, G. (2010). Measuring the Effects of Segregation in the Presence of Social Spillovers: A Nonparametric Approach.

Haushofer, J. and Fehr, E. (2014). On the psychology of poverty. *Science*, 344(6186):862–867.

Heckman, J. J., Lalonde, R. J., and Smith, J. A. (1999). The Economics and Econometrics of Active Labor Market Programs. In *Handbook of Labor Economics*, volume 3, pages 1865–2097. Elsevier.

Henderson, R. (2021). The New World Of Work Needs A New Social Contract. *Forbes*.

Horowitz, J. (2020). Job guarantees and free money: 'Utopian' ideas tested in Europe as the pandemic gives governments a new role. *CNN*.

Huber, M. and Steinmayr, A. (2021). A Framework for Separating Individual-Level Treatment Effects From Spillover Effects. *Journal of Business & Economic Statistics*, 39(2):422–436.

Hussam, R., Kelley, E. M., Lane, G., and Zahra, F. (2022). The Psychosocial Value of Employment: Evidence from a Refugee Camp. *American Economic Review*, 112(11):3694–3724.

ILO (2021). Public Employment Initiatives and the COVID-19 crisis. Technical report, International Labour Organization (ILO), Geneva.

Jahoda, M. (1982). *Employment and Unemployment : A Social-Psychological Analysis*. Cambridge University Press, Cambridge.

Jahoda, M., Lazarsfeld, P. F., and Zeisel, H. (2017). *Marienthal: The Sociography of an Unemployed Community (Original Work Published 1933)*. Routledge.

Jaroszewicz, A., Jachimowicz, J., Hauser, O., and Jamison, J. (2022). How Effective Is (More) Money? Randomizing Unconditional Cash Transfer Amounts in the US. *SSRN Electronic Journal*.

Kassenboehmer, S. C. and Haisken-DeNew, J. P. (2009). You're Fired! the Causal Negative Effect of Entry Unemployment on Life Satisfaction. *The Economic Journal*, 119(536):448–462.

Katz, L. F., Roth, J., Hendra, R., and Schaberg, K. (2022). Why Do Sectoral Employment Programs Work? Lessons from WorkAdvance. *Journal of Labor Economics*, 40(S1):S249–S291.

Khera, R., editor (2011). *Battle for Employment Guarantee.* Oxford University Press, Delhi Oxford.

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics*, 17(6):904–918.

Knight, T., Lloyd, R., Downing, C., Svanaes, S., and Coutts, A. (2020). Group Work/JOBS II Project: Process Evaluation Technical Report. Technical report, Department for Work and Pensions, London.

Korpi, T. (1997). Is utility related to employment status? Employment, unemployment, labor market policies and subjective well-being among Swedish youth. *Labour Economics*, 4(2):125–147.

Kovacs, C., Batinic, B., Stiglbauer, B., and Gnambs, T. (2019). Development of a Shortened Version of the Latent and Manifest Benefits of Work (LAMB) Scale. *European journal of psychological assessment : official organ of the European Association of Psychological Assessment*, 35(5):685–697.

Kovacs, K., Batinic, B., Muller, J., Coutts, A., and Wang, S. (2017). Jahoda's Latent and Manifest Benefits scale, 12 item version.

Lalive, R., Landais, C., and Zweimüller, J. (2015). Market Externalities of Large Unemployment Insurance Extension Programs. *American Economic Review*, 105(12):3564–3596.

Lowrey, A. (2017). Should the Government Guarantee Everyone a Job? - The Atlantic. *The Atlantic.*

Marinescu, I. (2018). No Strings Attached: The Behavioral Effects of U.S. Unconditional Cash Transfer Programs.

McGuire, J., link will open in a new window Link to external site, t., Kaiser, C., link will open in a new window Link to external site, t., Bach-Mortensen, A. M., and link will open in a new window Link to external site, t. (2022). A systematic review and meta-analysis of the impact of cash transfers on subjective well-being and mental health in low- and middle-income countries. *Nature Human Behaviour*, 6(3):359–370.

Muralidharan, K., Niehaus, P., and Sukhtankar, S. (2023). General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India. *Econometrica*, 91(4):1261–1295.

Nunn, R., O'Donnell, J., and Shambaugh, J. (2018). Labor Market Considerations for a National Job Guarantee. *Brookings. The Hamilton Project*, Framing Paper:37.

OECD (2021). *Building Inclusive Labour Markets: Active Labour Market Policies for the Most Vulnerable Groups*. OECD Policy Responses to Coronavirus (COVID-19). OECD, Paris.

OECD (2021). *OECD Economic Surveys: Austria 2021*. OECD Economic Surveys: Austria. OECD, Paris.

Paul, M., Darity, W., and Hamilton, D. (2018). The Federal Job Guarantee—A Policy to Achieve Permanent Full Employment. *Center on Budget and Policy Priorities (CBPP)*.

Pausackl, C. (2021). Jobgarantie für Langzeitarbeitslose: Nie wieder arbeitslos. *Die Zeit*.

Pohlan, L. (2019). Unemployment and social exclusion. *Journal of Economic Behavior & Organization*, 164:273–299.

Porter, E. (2021). Should the Feds Guarantee You a Job? *The New York Times*, page 2.

Quinz, H. and Flecker, J. (2022). „Marienthal. reversed". The effects of a job guarantee in an Austrian town. *ILPC Padova, April 21, 2022*.

Romeo, N. (2022). What Happens When Jobs Are Guaranteed? *The New Yorker*.

Schochet, P. Z., Burghardt, J., and McConnell, S. (2008). Does Job Corps Work? Impact Findings from the National Job Corps Study. *American Economic Review*, 98(5):1864–1886.

Stone, J. (2020). Unconditional job guarantee to be trialled in Austria, in world first. Pilot designed by Oxford University economists. *The Independent*.

Strandh, M. (2001). State Intervention and Mental Well-being Among the Unemployed. *Journal of Social Policy*, 30(1):57–80.

Tanden, N., Martin, C., Jarsulic, M., Duke, B., Olinsky, B., Boteach, M., Halpin, J., and Teixeira, R. (2017). Toward a Marshall Plan for America. *Center for American Progress*.

Tcherneva, P. R. (2020). *The Case for a Job Guarantee*. Polity.

The Guardian (2020). The Guardian view on a job guarantee: A policy whose time has come. Editorial. *Editorial*.

UN Special Rapporteur, D. S. O. (2023). The employment guarantee as a tool in the fight against poverty. Report of the Special Rapporteur on extreme poverty and human rights, Olivier De Schutter. United Nations General Assembly. Human Rights Council, Fifty-third session. Technical report, United Nations General Assembly. Human Rights Council, Fifty-third session.

van den Berg, G. J., Hofmann, B., Stephan, G., and Uhlendorff, A. (2021). Mandatory Integration Agreements for Unemployed Job Seekers: A Randomized Controlled Field Experiment in Germany. *Working Paper (SSRN Electronic Journal)*.

Young, C. (2012). Losing a Job: The Nonpecuniary Cost of Unemployment in the United States. *Social Forces*, 91(2):609–634.

ZDF (2022). Zurück in den Job: Wege aus der Arbeitslosigkeit. plan b. Documentary film.

Chapter 3

Reframing active labor market policy: Field experiments on barriers to program participation

Co-authored with Anna Schwarz (Central European University)

Governments struggle to attract unemployed workers to their widely offered job training programs. In a randomized field experiments with 11,000 job seekers, we investigate the barriers to participation in job training programs using information interventions designed to encourage participation. Raising awareness about the availability of job training increased program enrollment by 18%. Signaling program cost with a voucher on top to reduce internalized stigma increased completion by 28%. Effects were sizable and concentrated among women and low-income job seekers. Notably, increased job training did not result in higher employment or wages. These findings indicate that while low-cost informational interventions effectively boost participation, the overall success of job training programs in enhancing employment prospects hinges on their fundamental design.

Keywords: job training, program participation, information friction, social stigma, field experiment

JEL codes: J64, J68, C93, D04, D83

We thank Sven Hergovich former managing director of the *AMS NÖ* and the employees of the *AMS*, including Christian Bliem, Mario Danler, Petra Eisner, Gabriele Fälbl, Martina Fischlmayr, Karmen Frena, Andreas Kreiner, Günter Martinsich, Paul Reisenauer and Daniel Riegler, without whose support this study would not have been possible.

We thank Daron Acemoglu, David Autor, Sydnee Caldwell, David Card, Bart Cockx, Stefano DellaVigna, Lydia Dimitrakopoulou, Bernhard Ebbinghaus, Per Engzell, Chris Featherman, Simone Häckl, Christoph Henking, Simon Jäger, Casper Kaiser, Maximilian Kasy, Erin Kelly, Elisabeth Leduc, Rudolf Moser, Elias Naumann, Brian Nolan, Dylan Nelson, Patrick Nüß, Amanda Pallais, Aaron Reeves, Jesse Rothstein, Georg Rilinger, Emmanuel Saez, Rupert Sausgruber, Susan Silby, Dennis Tamesberger, Simon Theurl, Tim Vlandas, Nathan Wilmers, Janet Xu, Victoria

3.1 Introduction

"No one cares if a course fits your profile or not. The main focus is that you are no longer adding to the unemployed numbers."

– Job seeker in our survey, Austria (2021).

"I am not a blip on a computer screen or a national insurance number, I am a man."

– Job seeker in the Ken Loach's movie "I, Daniel Blake" (2016).

Modern welfare states provide comprehensive social support to disadvantaged people including to unemployed workers. However, take-up of benefits, public services, and social programs by eligible populations is incomplete (UN Special Rapporteur, 2022). As a main pillar of active labor market policies (ALMP), public employment services (PES) provide training to job seekers to improve their re-employment prospects. While governments spend large amounts of public budgets on these programs, many job seekers are hesitant to participate.[1] Information frictions from a lack of awareness and psychological frictions from social stigma attached to public training programs constitute potential barriers faced by job seekers to engage in training (Heckman and Smith, 2004) that recently received renewed attention (Dhia and Mbih, 2020; Leduc and Tojerow, 2023; Muller et al., 2023). Following one explanation, individuals eligible for benefits or social programs are simply

Zhang, and Erza Zuckerman for valuable feedback and comments, and seminar participants at IHS, LSE, MIT, UC Berkeley, University of Chicago, University of Oxford, and WU Vienna. We acknowledge financial support from the AMS. Lukas Lehner acknowledges financial support from the Economic and Social Research Council [grant number ES/P000649/1], the Scatcherd European fund and the Saven European fund at the University of Oxford as part of his PhD funding, and the Austrian Economic Association through its Dissertation Fellowship.

The experiment was pre-registered as AEARCTR-0007141 Lehner and Schwarz (2021). The experiment was approved by the Departmental Research Ethics Committee at the University of Oxford and by the Competence Center for Experimental Research at the Vienna University of Economics and Business.

[1] Governments spend on average 0.5% of GDP across OECD countries with up to 2% in European countries with the most developed active labor market programs (OECD, 2023). On average, less than 1% of the labor force participates annually in job training programs.

not aware of their eligibility and face administrative burdens in accessing their benefits and services (Altmann et al., 2018; Barr and Turner, 2018; Belot et al., 2019; Haaland et al., 2023). Following another explanation, psychological frictions discourage eligible groups from accessing their entitlements even if they know about the programs (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Goldin et al., 2022; Linos et al., 2022a). Such reasons are particularly likely to explain why unemployed people are often seen as failing to contribute, which results in shame and social stigma attributed to reliance on the welfare state (Goffman, 1963; Moffitt, 1983; Walker, 2014; Bursztyn and Jensen, 2017). It raises the question whether eligible individuals are simply not aware of the welfare support available to them, or whether psychological frictions associated with shame and stigma discourage them from accessing benefits, services, and programs including training for job seekers.

This paper investigates why unemployed workers are hesitant to participate in training programs. Job seekers can choose among a wide variety of training programs at no financial cost as long as their choices are deemed reasonable by the PES. However, caseworkers struggle to fill their training programs with unemployed job seekers who are hesitant to participate. We examine barriers for job seekers' hesitancy to engage in training including the role of information frictions and psychological frictions. Answering this question sheds light on how to overcome barriers to increase training to improve human capital and skills in the labor force. Further, it helps to understand whether and under what conditions training helps job seekers to get re-employed. In a broader context, it adds to the understanding of possible barriers that discourage disadvantaged groups from accessing public services resulting in non-take-up.

Experimental Design We answer this question with a multi-armed field experiment at scale with 11,000 job seekers. The experiment consists of three treatment arms in which e-mails with varying content on job training were sent to unemployed job seekers. The intervention was implemented in the first quarter of 2021 by the PES of Lower Austria (*Arbeitsmarktservice Niederösterreich (AMS NÖ)*). The goal was to increase enrollment in training with the aim of increasing re-employment of job seekers.

We randomly allocate 11,000 unemployed job seekers in Lower Austria to three treatment groups and one control group. The first treatment group receives an e-mail with information on training programs offered by the PES; the second treatment group additionally receives a training voucher to be redeemed with the PES up to a value of €15,000; the third treatment group additionally receives information on which occupations have the most open vacancies. The intervention consists only of the variation in the information provided with all options and obligations kept constant for individuals in all four groups. The treatments are stacked and designed to separate out interacted effects of raising awareness (treatment 1), combined with signalling the training program's monetary value (treatment 2), and combined with providing information on the labor market (treatment 3). We observe training and employment from administrative records as our main outcomes. We link those to our own survey data of participants' training intentions, beliefs, and experiences to uncover mechanisms for assigning job seekers to job training. The current paper version includes results up to two years after the intervention.

Main findings Our main empirical findings can be divided in two areas: training and employment among unemployed workers. For average **training outcomes**, three sets of findings are noteworthy. First, **raising awareness** has a large positive effect on training enrollment; **signaling the monetary value** on top helps to improve program completion. The magnitude of an 18%–21% increase in training enrollment compared to baseline is striking given that the intervention consists of only one e-mail. The increase is sustained over a two-year period with recipients still 10% more likely to have participated in job training, which shows that the intervention does not only encourage job seekers to train earlier but increases overall training. The effect is stronger on training completion than enrollment indicating a positive effect on completion even among always-takers. This implies that unemployed workers who would have participated in training without the intervention are more likely to complete training programs due to the intervention. Signalling the monetary value of job training—highly stigmatized programs—increases program completion beyond its increase in participation. The increase in program completion amounts to 28% compared to baseline, and compares to a 19% increase for raising awareness.

Second, job training increases unevenly across programs and results in **spillovers** on other active labor market programs. The increase in job training is driven by a relative shift towards more rigorous programs. Both the e-mail and voucher increase enrollment in programs with longer duration, which are typically oriented toward acquiring job-related skills and human capital formation. Both treatments increase participation in examined programs, which are more rigorous and provide a certificate for successful completion. Signalling the monetary value leads to a larger increase in completion of ambitious programs beyond the increase in participation. Spillovers on other active labor market programs are not negligible. Increasing training drives a substitution of enrollment in other ALMPs, in particular for application courses and subsidized employment. Application course enrollment decreases by about half of the increase in job training. Subsidized employment also shows signs of decline. Raising awareness, thus, spurs substitution of job search and hiring subsidies with training programs.

The average results are driven by substantial **heterogeneity** across sub-groups. Effects are concentrated among disadvantaged groups: women and job seekers with lower income. Both groups are more likely to enroll in training programs at baseline and drive the training increase by a strong response to the information intervention.

Third, reducing information frictions on labor markets can have **unintended consequences**. Informing job seekers which occupations have the highest numbers of open job vacancies results in null effects, cancelling out any positive effects from raising awareness and signaling the monetary value of training programs (treatment 3: e-mail + voucher + occupation information). Those occupations with the highest number of open vacancies are viewed as unattractive, in particular to job seekers with better prospects, as the heterogeneity analysis and surveys reveal.

Turning to average **employment outcomes**, we find no positive effects of training programs on labor market outcomes. Using intention-to-treat (ITT) and instrumental variable (IV) estimation approaches, we find that training programs do not increase re-employment rates or wages of unemployed job seekers. The findings are robust to a number of variable definitions with no signs of meaningful heterogeneity across types of training programs or sub-groups of unemployed workers.

Implications The results demonstrate the potential of information provision in overcoming barriers for disadvantaged populations. Raising awareness to reduce information frictions (treatment 1: e-mail) and framing information to reduce psychological frictions (treatment 2: e-mail + voucher) increase training to foster human capital formation. Providing information does not always work in the same way. It can also have unintended consequences, such as discouraging unemployed workers from training (treatment 3: e-mail + voucher + occupation information).

Literature Job training is a key pillar of active labor market policies, widely studied in labor economics. Heckman and Smith (2004) suggested in a descriptive analysis that the **lack of awareness** of program eligibility is a major determinant of job training participation. Experimental studies have shifted attention to studying the effect of messages to reduce information and psychological frictions as summarised by Haaland et al. (2023). Each of our treatment arms resembles interventions tested contemporaneously in separate experiments in different countries. Our study allows us to separate the interacted effects from addressing information frictions from a lack of awareness of training (compare treatment 1 to Leduc and Tojerow (2023) in Belgium), psychological frictions associated with training programs (compare treatment 2 to Dhia and Mbih (2020) in France), and information frictions on labor demand (compare treatment 3 to Muller et al. (2023) in the Netherlands). We compare results in Section 3.6. Contrary to our study, the shift in training intentions through information provision did not translate into training enrollment in Dhia and Mbih (2020) and Leduc and Tojerow (2023). Previously, Barr and

Turner (2018) used quasi-experimental variation to show for the U.S. that letters sent from the PES informing job seekers about benefits and costs of training substantially increase training participation. Treatment 3 in our intervention contains information on labor demand by occupation, which parallels the experiment by Muller et al. (2023). In line with our study, they find no impact on received benefits and aggregate earnings. By separating out the interacted effects, our experiment further contributes to studies on the provision of job search information (Altmann et al., 2018; Belot et al., 2019; Briscese et al., 2020; Barbanchon Le et al., 2023).

We study job training as an archetypical social program thereby contributing to the public finance literature on barriers to social program take-up (Moffitt, 1983; Bertrand et al., 2000; Currie et al., 2001; Dahl et al., 2014; Finkelstein and Notowidigdo, 2019; Anders and Rafkin, 2022). **Psychological frictions** such as social stigma are suggested as important reasons for non-take-up of benefits (Bursztyn and Jensen, 2017; Friedrichsen et al., 2018; Celhay et al., 2022). A number of field experiments study social benefit take-up in the U.S. They find that provision of information to raise awareness, corresponding to our treatment 1, increases take-up (Bhargava and Manoli, 2015; Goldin et al., 2022), while framing interventions to overcome psychological frictions by reducing stigma, corresponding to our treatment 2, do not have an added benefit (Bhargava and Manoli, 2015; Linos et al., 2022a). The framing of messages, however, does matter in other contexts (Linos et al., 2020; Lasky-Fink and Linos, 2022; Linos et al., 2022b; Osman and Speer, 2023)[2] What differs is that most studied programs are entitlement programs in which participation primarily depends on the decisions of eligible individuals to apply. By contrast, participation in job training depends on the choices of both eligible individuals and caseworkers (Zweimüller and Winter-Ebmer, 1996; Heckman and Smith, 2004). This assignment mechanism is key for social stigma creation: a qualitative study shows that voluntary participation in ALMPs is positively evaluated by employers whereas mandatory assignment by the caseworker is negatively evaluated by employers (Fossati et al., 2021). We contribute by opening the blackbox of program assignment and uncovering

[2]There is no guarantee that framing interventions would always increase program participation as they depend on a behaviorally well-informed design (Hervelin, 2021).

mechanisms around autonomy of choice in program assignment. We trace the steps from job seekers' intention to train, the role of caseworkers, training enrollment, and completion for the first time in an experimental study.

The **heterogeneous effects** in job training participation suggest a "Matthew Effect". Groups with higher enrollment at baseline disproportionally increase their training due to the intervention. This may be the result of "access bias" (Bonoli and Liechti, 2018) from training programs disproportionally targeting disadvantaged groups, such as unemployed women who return to the labor force after childbirth. Information interventions in other contexts find that heterogeneity in responses is driven disproportionately by disadvantaged groups, especially by income (Heffetz et al., 2022; Lasky-Fink and Linos, 2022) and education (Barbanchon Le et al., 2023), which corresponds to our results.

On the employment side, our study contributes to the rich body of **active labor market policy** evaluations. Overall, training programs for job seekers are found to have modest positive effects on re-employment and wages as summarized by the meta-analyses by Card et al. (2010, 2018) as well as by extensive reviews (Heckman et al., 1999; Kluve, 2010; Crépon and van den Berg, 2016). However, large differences between program types, context, and across sub-groups exist. Positive employment effects are more pronounced for disadvantaged groups in the labor market including women (Card et al., 2018) and low-wage workers (Katz et al., 2022). Explanations for why we do not find positive employment effects are discussed in Section 3.6, where we also compare our results to other studies, which, in Europe, are mostly non-experimental.

Our findings contribute to the understanding of **unintended consequences** of active labor market policies (Black et al., 2003; Crépon et al., 2013; Gautier et al., 2018). Unintended consequences may be understood by connecting labor market evaluations with insights from behavioral theory that shape our understanding of job search. Related to the results for our treatment 3, Bandiera et al. (2021) find in a different context that combining training and job search elements leads to worse outcomes than standalone job training. **Discouragement** emerges as the main mechanism behind the result: lower than expected call-back rates lead to negative effects of job search assistance. This form of discouragement stemming from overoptimism is in line with Spinnewijn (2015) and the burgeoning literature on duration dependence that has documented job seekers overoptimism about their employment prospects (Mueller et al., 2021; Maibom et al., 2023; Abebe et al., 2021; Miano, 2023; Adams-Prassl et al., 2023). Overoptimism has also been documented for job seekers in Austria (Böheim et al., 2011). Our results extend current understandings of discouragement as an unintended consequence of labor market interventions by showing that workers can become discouraged when learning about labor demand being concentrated in occupations below their skill level.

Roadmap The rest of this paper is structured as follows. Section 3.2 provides an overview of active labor market policies and the context of the study. Section 3.3, building on our pre-analysis plan, details our experimental design and analysis. Section 3.4 presents our empirical results, which include training, and employment. Section 3.5 investigates mechanisms behind the treatment effects, including training intentions, caseworker effects, and the relationship between job seekers and caseworkers. Section 3.6 discusses the results and Section 3.7 concludes.

Appendix B.1 presents additional details on the design and Appendix B.2 additional results of intervention 1. Appendix B.3 provides details on the complementary survey including the questionnaire and additional results.

3.2 Background

This section provides an overview of the objectives, history, and types of of active labor market policies. It also discusses training programs in the Austrian context, and their assignment and eligibility criteria. Lastly, the impact of the Covid pandemic on the labor market is reviewed.

Objectives Active labor market policy has an economic policy and a social policy function with its dual objective of raising efficiency in labor markets while promoting equity among unemployed workers. Efficiency concerns have primarily centred around raising employment, improving job-worker matching, and increasing human capital, while equity concerns aim at levelling the risk distribution between unemployed job seekers and providing employment opportunities for disadvantaged groups (Clasen et al., 2016; Boeri and van Ours, 2021; Lehner and Tamesberger, 2024). Thereby, ALMPs complement passive labor market policies such as unemployment benefits and early retirement schemes (Ebbinghaus, 2020).

History of ALMP Active labor market policy has a long history. In the 1950s, Sweden pioneered modern ALMP manpower programs in its notorious "Rehn-Meidner Plan" combining expansive macroeconomic policies with ALMPs with the objective of facilitating rapid labor reallocation and up-scaling to raise productivity while sustaining full employment (Weishaupt, 2011). In the late 1960s, Austria followed the Nordic examples and became one of the first countries to introduce far reaching training programs for unemployed workers (Hofer et al., 2013). The sustained increase in unemployment during the 1980s and 1990s resulted in a large up-scaling and convergence of ALMPs across high-income countries (Clasen and Clegg, 2011). Under the "activation" turn in the 1990s (see OECD (1994) for the landmark study at the time), PES introduced increasingly strict benefit conditionality that oblige job seekers to participate in ALMPs once assigned to be eligible for benefits (Bonoli, 2010; Knotz, 2020). Since the 2008 Great Recession, ALMPs continuously expanded the range of programs (OECD, 2018; Boeri and van Ours, 2021) with increasing convergence of activation requirements across high-income countries (Immervoll and Knotz, 2018).

ALMP types Programs can be divided into at least four categories: Job search assistance, training, employment subsidies, and public employment creation (Card et al., 2018). [3] Job search assistance includes one-on-one counseling as well as courses in which job seekers learn job search skills and apply for jobs. These typically focus on job search strategies and CV preparation. Training refers to programs focused on sustaining, deepening, and acquiring skills to build human capital, facilitate re-employment, and spur occupational mobility. Employment subsidies incorporate hiring subsidies for employers as well as a smaller subset of funding support to job seekers who found a start up business. Public employment is typically targeted at the group of most disadvantaged job seekers, which includes those with long unemployment spells and health conditions (Kasy and Lehner, 2023). Our intervention is targeted at training programs, but we are able to observe spillovers on other ALMP types.

Job training programs in Austria Training programs in Austria are recognized as among the most developed in the world, and the Austrian PES has served as a role model for other countries. Expenditures for ALMP in Austria are among the highest as a share of GDP across high-income countries (OECD, 2023). Training programs constitute the largest pillar and receive 2/3 of the annual ALMP budget (Hofer et al., 2013). Training offered by the PES includes over 1,000 programs that cover advancing skills within an occupation as well as acquiring new skills to foster occupational mobility (Zweimüller and Winter-Ebmer, 1996; Eppel et al., 2022). Common programs include mechatronics, plumbing, ICT, programming, restaurant management, hotel and catering assistance, and nursing. Program duration varies from a few days to 1.5 years with longer programs offering high quality training for job specific skills. Among training program participants, about 40% graduate with a certificate after successfully passing an exam. Programs with an exam are typically more rigorous. During training enrollment, individuals continue to receive the same amount as their unemployment benefits, which is topped up with a small amount of €4 per day to account for an increase in expenditures during training participation.

[3] For alternative classifications, see Vlandas (2013) and Ebbinghaus (2020).

The role of caseworkers Caseworkers are street-level bureaucrats employed by the PES as job counselors with several responsibilities. They provide job search assistance and monitor job search effort. They administer benefits and decide on program assignment. Job seekers meet their caseworkers regularly for consultations, where they discuss training opportunities as well as benefits and job search progress. The dual role of caseworkers reflects a deeper ideological divide about emphasizing welfare provision to unemployed workers versus making welfare contingent on demanding active job search and work availability.

Every unemployed job seeker is eligible to participate in training programs. While program participation comes at no financial cost to job seekers, attendance is mandatory and repeated no shows risk sanctions such as benefit cuts.

Unemployed workers can express interest in a large number of ALMPs, but caseworkers have the final say for program assignment. Unlike application courses to which caseworkers occasionally assign job seekers with the aim of "restoring work morale", assignment to training programs is intended to follow job seekers' interest. In practice, 87% of caseworkers in Austria report that they assign job seekers to training programs of their choice, while only 6% report "exercising pressure" when assigning training programs (Schönherr and Glaser, 2023). By contrast, application courses serve more frequently as a disciplining device, with 20% of caseworkers reporting they make assignments to "exercise pressure". Another motive for how caseworkers assign programs is "meeting their target", which is equally the case both for application courses and training programs.

Covid pandemic Our intervention took place in February 2021 as part of a broader PES campaign *Corona Joboffensive* to promote job training programs. The intention was to prepare job seekers for the recovery phase post-lockdown, given the low likelihood of immediate re-employment during the lockdown period. This lockdown extended from November 2020 to May 2021, with temporary easing occurring between February and

March 2021. The PES received additional funding and increased training capacity massively from February 2021, which led to a virtually unlimited supply of training programs only constrained by the demand of job seekers (Leitner and Tverdostup, 2023). The majority of training programs took place in person with safety measures in place while some programs moved online. The type of training programs offered was not affected by the pandemic.

3.3 Study design

We designed a field experiment at scale in a natural context (Harrison and List, 2004) to test whether information provision increases training and employment of job seekers. Job seekers receive an e-mail from the PES with varying content by treatment group to inform them about training opportunities. In this section, we provide an overview of the data and sample selection in Section 3.3.1, experimental design in Section 3.3.2, identifying assumptions in Section 3.3.3, and our approach to estimation and inference in Section 3.3.4. Tables and figures to describe the treatment assignment are shown in section B.1.

The study design was pre-registered and is documented in the pre-analysis plan (AEARCTR-0007141).[4] The experiment was approved by the Departmental Research Ethics Committee at the University of Oxford and by the Competence Center for Experimental Research at the Vienna University of Economics and Business.

[4]The code implementing the study design was uploaded prior to the implementation of the intervention to GitHub at https://github.com/lukaslehner/Vouchers.

3.3.1 Data

Administrative records We leverage a wide range of demographic, benefit, and job characteristics from administrative data including (i) the PES internal registry for administrative data on unemployed workers (AMS Data Warehouse); and (ii) the "occupational-career data" (Erwerbskarrierenmonitoring, EWKM), accessed via the AMS internal registry for social security registry data. Due to our reliance on administrative data, we face virtually zero attrition.

Surveys Additionally, we survey participants and link the data with the administrative records at the individual level. We collect detailed data on training intentions, experiences and perceptions, interactions with caseworkers as well as job search behavior and reservation wages. The surveys are distributed via e-mail to all individuals in our sample. We send the e-mails as researchers, ensure respondents' anonymity, and communicate our independence from the PES. We design the questionnaire using Qualtrics following Stantcheva (2023). Section B.3.2 provides a sample of our survey questionnaire.[5] We achieve a response rate of 30%, which is relatively high compared to related studies (Dhia and Mbih, 2020; Leduc and Tojerow, 2023; Muller et al., 2023).

Sample The sample of our first experiment includes the population of unemployed workers in Lower Austria with an unemployment spell of either 2–3 or 6–12 months at the time of treatment.[6] Unemployed job seekers who are already enrolled in a training program or who have a job offer accepted at the time of the intervention are excluded from the sample. The sample is further restricted to people who are at least 25 years old.[7]

This leaves us with 11,050 unemployed workers (Table 3.1 column (3)).[8] Among

[5]The full questionnaire was pre-registered on the AEA RCT Registry at https://www.socialscienceregistry.org/versions/87136/docs/version/file.

[6]All unemployed workers with a spell of 3 to 6 months received the information treatment 1 without control group two weeks prior to the experimental intervention and thus could not be included in the randomized experiment.

[7]The PES runs specific programs for younger job seekers.

[8]The sample for the analysis is reduced to 10,714 since observations with missing values are

them, 52% are women, 30% are younger than 35, and about 32% are older than 50. A third has no more educational attainment than compulsory schooling. Just over 1/5 has a foreign citizenship and an equally large share has a health restriction preventing them from working in certain occupations. With respect to language, 14.5% speaks only limited or no German.

Overall, our sample is very similar to job seekers across Austria (Table 3.1 column (4)), despite Lower Austrian job seekers being more likely to have Austrian citizenship. We also compare our sample to the population of job seekers before the pandemic (Table 3.1 columns (1–2)). A high share of lay-offs took place at the start of the pandemic in March 2020, which explains the higher share of unemployed workers with a duration of 9–12 months in our sample. Among them, a higher share had minimum educational attainment and non-Austrian citizenship. A smaller share of job seekers in the sample had a health restriction compared to unemployed job seekers before the pandemic. With regard to gender and age, the composition remained broadly the same. In comparison, the sample of survey respondents are disproportionately female, older, and has a higher educational attainment, while there are no differences in their level of German or health conditions (Table C1).

excluded. Missing values include mainly citizenship and occupation as well as in few instances education and pre-unemployment income.

Table 3.1: Sample representativeness across time and states

	Lower Austria			Austria
	Feb.19	Feb.20	**Feb.21**	Feb.21
Total	5551	6540	**11050**	71487
Gender				
Women	53.4%	51.7%	**51.9%**	49.4%
Men	46.6%	48.3%	**48.1%**	50.6%
Age				
Below 35	30.3%	29.7%	**29.9%**	33.4%
35-50	37.0%	37.1%	**38.5%**	39.4%
Above 50	32.6%	33.1%	**31.5%**	27.1%
Education				
Compulsory education	29.5%	29.0%	**32.5%**	36.3%
Higher than compulsory	70.5%	71.0%	**67.5%**	63.7%
Citizenship				
Austrian	82.8%	82.0%	**77.9%**	65.7%
Non-Austrian	17.2%	18.0%	**22.1%**	34.3%
Health				
Health restriction	24.0%	25.8%	**21.3%**	17.5%
No health restriction	76.0%	74.2%	**78.7%**	82.5%
Unemployment duration				
3-4 months	28.5%	30.9%	**24.3%**	28.8%
6-9 months	43.0%	40.0%	**33.9%**	28.9%
9-12 months	28.6%	29.1%	**41.8%**	42.3%
Language skills				
German speaking	89.0%	88.2%	**88.6%**	85.5%
Non-German speaking	11.0%	11.8%	**11.4%**	14.5%
Summary indicators				
Unemployment rate	8.9%	8.7%	**10.0%**	10.7%
In training	16.2%	15.3%	**13.5%**	16.5%

Note: All selection criteria as explained in the text are the same for our sample and the comparison samples.

Outcomes of interest We categorize our outcomes of interest into two main groups: training and employment outcomes. In our main specifications, training outcomes are measured within 12 months after the intervention, whereas employment responses are expected to materialize later, and we thus measure them within 24 months after the intervention.[9] We report descriptive statistics for these outcomes in Table 3.2. We measure training by enrollment and completion of respective training programs. Our training outcomes in the upper part of the table are all binary and take the value of 1 if the unemployed participated in the specific type of ALMP within 12 months after the intervention. The same holds for employment in the lower part of the table. Participation in job training counts as unemployed. We also measure days in employment and unemployment as well as the average daily wage when the person was employed and construct an index for job quality. This index can take values between 0 and 1 and is an equally weighted combination of standardized average wage quality and employment continuity, measured as days in employment. We test a range of alternative definitions for robustness presented in Table B11.

Baseline data At baseline, 11% of job seekers enroll in a training program within 12 months after the intervention (column 1), while almost 10% also complete these programs (column 2). Among all the programs, 8% last for 40 days, which is the median duration, or longer (column 3). Longer programs have a stronger focus on equipping job seekers with new skills and human capital formation, while shorter programs often focus on refreshing existing knowledge or adding complementary skills. Close to 5% of job seekers participate in training programs that finish with an exam, which is another indicator for more demanding training programs (column 4). Besides training, the PES provides a range of active labor market programs discussed in Section 3.2. We present results for enrollment in application courses and subsidized employment to account for spillover effects on other ALMPs.[10] At baseline, 4.5% of job seekers participate in application courses (column 5), while 1 in 4 job seekers finds a job supported by employment subsidies (column 6) within 12 months of starting their unemployment spell.

[9] Naturally, we also report training outcomes 24 months after the intervention as well as employment outcomes 12 months after the intervention in the respective appendix sections.

[10] Public employment programs are targeted at a different sub-group: the most disadvantaged job seekers with very long unemployment spells and health conditions.

Concerning employment outcomes in the lower part of the table of Table 3.2, 75% of job seekers in our sample have been in employment for at least one day within 24 months after the intervention (column 1). During that period, a job seeker is on average 350 days in employment (column 2) and 361 days in unemployment (column 3). Once in employment, their average wage amounts to 51 Euros gross per day (column 4).

Table 3.2: Outcome variables descriptives

	Training outcomes (within 12 months after intervention)					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
Mean	0.112	0.094	0.078	0.047	0.045	0.257
SD	0.316	0.292	0.268	0.211	0.208	0.437
Range	0/1	0/1	0/1	0/1	0/1	0/1
Valid N.	11,050	11,050	11,050	11,050	11,050	11,050
	Employment outcomes (within 24 months after intervention)					
	Employment	Days in employment	Days in unemployment	Avg. daily wage	Job quality	
Mean	0.754	350.103	361.954	50.814	0.382	
SD	0.431	310.971	259.588	29.494	0.144	
Range	0/1	0-928	0-934	1.238-217.479	0-1	
Valid N.	11,050	11,050	11,050	7,938	7,527	

Note: The table shows mean, SD, and range of all outcome variables for the control group. Valid N. refers to all non-missing values in the whole sample (i.e., including the three treatment groups).

3.3.2 Experimental design

Treatment assignment We assigned study participants to one of three treatment groups and one control group using stratified randomization. We used the following covariates to construct the strata: gender, age, educational attainment, region, and unemployment duration. We constructed these variables from raw data for job seekers using the PES internal registry and the social secruity administrative data described above. All of these variables were used as they were available to the PES in February 2021.

For the stratified randomization, we first divided individuals into strata based on the variables described above. We constructed 145 strata for every possible combination of the values of the 5 strata variables ranging from 10 to 270 individuals per stratum as shown in Figure A1. We then assigned individuals randomly within the strata to one of the three treatment groups or the control group. The randomization procedure resulted in four equally-sized, balanced groups as shown in Appendix A1. The pre-analysis plan contains further details on the treatment assignment(AEARCTR-0007141) (Lehner and Schwarz, 2021). For comparison, survey respondents are distributed relatively evenly across groups, showing balance in all aspects except for educational attainment (Table C1).

Intervention The intervention consists of e-mails sent by the PES with varying information on job training aimed at encouraging job seekers to participate in job training. Participants are not aware of the experiment as characteristic for a natural field experiment (List, 2022). The treatments are stacked on top of each other, i.e., treatment group 2 receives the same e-mail as treatment 1 complemented with a voucher; treatment group 3 receives the e-mail and voucher of groups 1 and 2 complemented with information regarding which occupations have open vacancies. The stacked treatment design allows us to interpret the outcomes as interacted treatment effects. The control group is not contacted but continues to have access to training and regular PES consultations. The formal training assignment mechanism remains the same for individuals of all four groups. The intervention was implemented in February 2021.

Treatment group 1 receives an e-mail with information on PES-provided training programs as shown in Figure A2. The intention is to raise job seekers' awareness of training programs to overcome information frictions that discourage them from participation.

Treatment group 2 includes a voucher for job training programs added to the e-mail as shown in Figure A3.[11] Although training program enrollment is costless to job seekers irrespective of which treatment group they are assigned to, the voucher indicates a value of €15,000.[12] The value was chosen as an upper bound for training program costs as it corresponds to the cost incurred to the PES by their most expensive training programs on offer. By signalling the monetary value of the programs, the treatment is intended to reduce psychological frictions that can discourage job seekers from program participation. These frictions may include internalized stigma about participating in job training (Fossati et al., 2021). The voucher is, thus, solely a way of framing access to training programs that are already available to job seekers.

Treatment group 3 receives a list of occupations with the highest number of open vacancies in addition to the e-mail and voucher as shown in Figure A4. This information is intended to encourage job seekers for training in occupations with high labor demand and broaden their job search beyond their previous occupation. As job seekers are found to search in occupations with relatively few vacancies (Sahin et al., 2014), improving access to information has been shown to broaden job seekers' search and increase the number of job interviews they are invited to (Belot et al., 2019).

E-mail clicks For intervention 2 and 3, we collect data on whether an e-mail was received and opened, and on clicks on hyperlinks in the e-mail to assess whether the intervention was successfully implemented. Figure A5 shows a graphic of the e-mail and hyperlink clicks observed.

[11] The stacked design is necessary since providing a voucher to signal the monetary value inherently raises awareness as well. While we cannot rule out interaction effects between the treatments, the stacked design allows us to keep the effect of raising awareness about training programs constant across the treatment groups to separate out the interacted effect of signaling the monetary value.

[12] The voucher also includes €3,000 for any training not provided via the PES.

3.3.3 Identifying assumptions

Training outcomes Due to the clean randomization of participants into control and treatment groups, it is possible to compare the relevant outcome variables directly between the 4 groups. This provides us with an unbiased estimate of the treatment effect that does not hinge on any assumptions other than the random assignment into groups. The results for training can thus be interpreted as intention-to-treat (ITT) generalizable to the entire population of unemployed job seekers in our sample (Imbens and Angrist, 1994).

With the additional assumption that all effects are mediated by opening the e-mail, these estimates can be scaled up by the effect of treatment on the probability of opening the e-mail. This yields instrumental variable estimates of the local average treatment effect (LATE) of having received the treatment. The effect of assignment on opening the e-mail is estimated to be around .91, so that the corresponding instrumental variable estimates of all treatment effects on training outcomes would be about 10% higher of the reported ITT effects.

Employment outcomes We rely on the same ITT approach to estimate employment outcomes and additionally use an instrumental variable (IV) approach. Training is driven by those job seekers who enroll in training programs because of the treatment. While this is a small share of 2 percentage points who are shifted at the margin, we report our baseline estimations as ITT, which are generalizable to the entire population.

For the IV approach, we use the information intervention to instrument training. This gives us the LATE, which is representative for compliers, i.e., those job seekers at the margin of enrolling in training (Angrist et al., 1996). Our instrument, the information intervention, is correlated with the endogenous variable, training. Our IV estimation has an F statistic above 10, which is conventionally used as a threshold to qualify strong instruments. Our instrument is as good as random since we randomly assigned it. Our identification rests on the exclusion restriction: our instrument affects the dependent variable, employment outcomes, only through training. In other words, the information intervention itself does not affect employment.

3.3.4 Estimation and inference

First, we compare the simple means between the treatment and control groups. To increase precision, we estimate parametric regressions for the treatment effects using the following estimation regression:

$$Y_i = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 T_3 + \mathbf{X_i} + s_i + \epsilon_i \tag{3.1}$$

where Y_i refers to the outcome variables for individual i. Depending on the scale of the outcome variable, an OLS (continuous) or a Logit (binary) regression is used. Our outcome variables are measured at different time periods and for each time period a separate regression is estimated to measure time-varying treatment effects. T_1 to T_3 refer to the treatment groups as described above. Further, as we used stratified randomization, we include strata dummies, following Athey and Imbens (2017). We additionally control for all socio-demographic variables as recorded before treatment $\mathbf{X_i}$ that were not used for stratification. This includes language skills, citizenship, occupation, marginal employment, previous wage, and within the past 10 years the days in employment and number of employment spells. Finally, we include caseworker fixed effects.

For employment outcomes, we maximize statistical power by pooling individuals in the treatment groups that increased training (treatment groups 1 and 2). Table B13 presents the employment results for the three treatment groups separately for robustness.

The heterogeneity analysis is conducted via sub-group regressions of the equation above for the variables specified in the pre-analysis plan.

3.4 Results

Our results are structured around two groups of outcomes: training, in Section 3.4.1 and employment, in Section 3.4.2. The training analyses focus primarily on one year after the intervention, while the employment analyses apply to a two-year time frame.

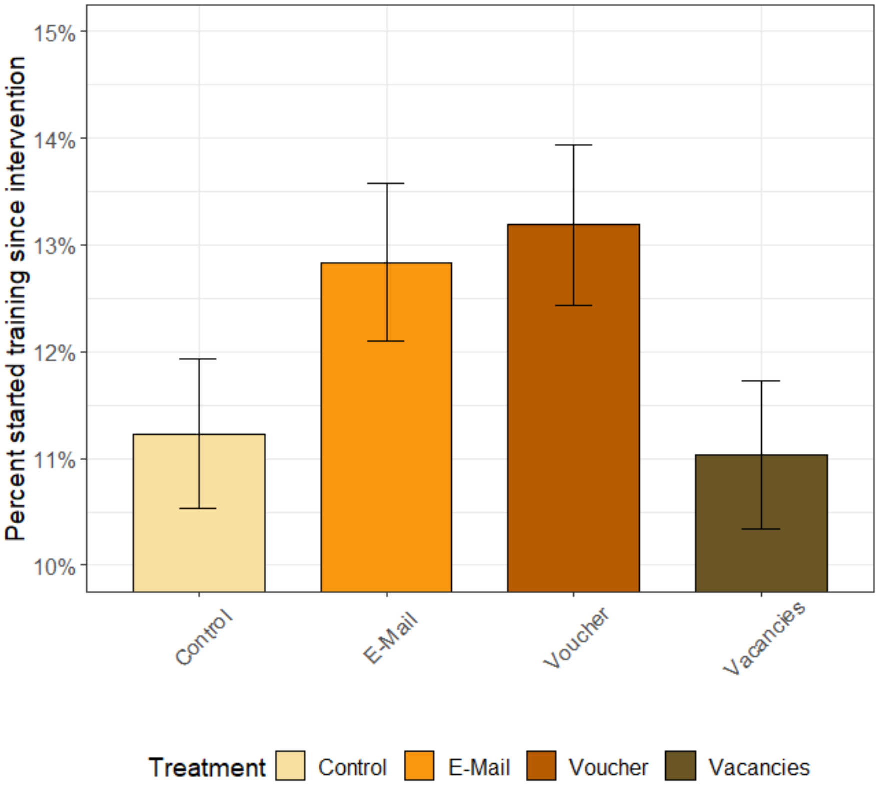
Figures 3.1–3.3 and Tables 3.3–3.5 present our main results. section B.2 documents additional figures with results for robustness using alternative estimation approaches and variable definitions, timing patterns, and heterogeneity.

3.4.1 Training

To analyze treatment effects on training, we first present baseline results on training behavior before proceeding to timing patterns and sub-group results.

Main findings The e-mail and voucher treatments both lead to a significant increase in training enrollment. The increase is substantial in magnitude with 20% and 24%, respectively, from baseline (Table 3.3 column 1), which results in around 13% of treated job seekers participating in training compared to 11% of untreated job seekers (Figure 3.1). The information on vacancies, by contrast, does not increase training. It is important to keep in mind that the information on vacancies is added to the e-mail and voucher as provided to treatment groups 1 and 2. We can interpret the null effect of treatment group 3, thus, as the vacancy information having a negative effect on aggregate training, which offsets the gains from treatment 1 and 2 in magnitude.

Fig. 3.1: Training enrollment



Note: Confidence intervals are reported at the 90%-level.

The e-mail and the voucher treatments both also increase completion of training programs (column 2). The increase in completion is about the same magnitude as for participation (18% for the e-mail and 26% for the voucher), indicating that all those induced to take up training by the intervention also completed it. Table B1 shows that this also holds for the different types of training. Additionally, the difference between the voucher and the e-mail is statistically significant for training completion, which indicates that the voucher has an additional positive effect on training.

The treatments also affect the type of training undertaken. Job seekers shift participation to more demanding training programs defined as longer in duration (column 3) and courses with an exam (column 4). At the same time, the increase in training for the e-mail and voucher treatments seems to have a spillover effect on enrollment in other active labor market programs. Around half of the increase in training of job seekers who receive the e-mail can be attributed to a decline in application course enrollment (column 5), which equals a 20% drop. Job seekers who receive the voucher tend find less subsidized employment, which equals the magnitude of the increase in training enrollment (column 6). The results demonstrate that reducing information frictions substantially increases training take-up. The voucher has an additional effect especially on the completion of training programs, suggesting added benefits from reducing psychological frictions.

Table 3.3: Average treatment effects on active labor market programs

	Dependent variable:					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.020**	0.018**	0.015**	0.011*	−0.009*	−0.005
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.012)
Voucher	0.024***	0.026***	0.014**	0.008	−0.007	−0.019
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.012)
Vacancies	0.0005	0.006	−0.003	0.003	−0.005	−0.019
	(0.008)	(0.008)	(0.007)	(0.005)	(0.005)	(0.012)
Control Mean	0.112	0.094	0.078	0.047	0.045	0.257
Control SD	0.316	0.292	0.268	0.211	0.208	0.437
Observations	10,714	10,714	10,714	10,714	10,714	10,714

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

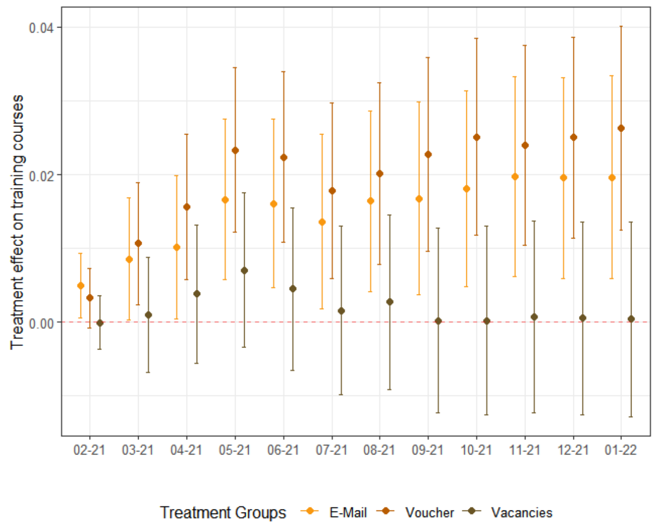
Timing patterns To analyze timing patterns in the treatment effects of active labor market programs as suggested by Card et al. (2018), we investigate the temporal dimension of treatment effects on a monthly basis for 12 months following the treatment. Regarding outcomes, we consider whether job seekers have participated in a training program since the intervention took place. Figure 3.2 shows the treatment effect on training program enrollment per month.

Findings Within the first 4 months, the voucher increases training enrollment by around 2.5 percentage points and the newsletter by around 1.5 percentage points compared to the control group. The treatment effect plateaus afterwards, as many job seekers who started training remain enrolled in their programs. The two treatments consequently lead to sustained higher training enrollment with no catch-up effect of the control group for the first 12 months after treatment, as can be seen in (Figure B1) on cumulative training enrollment within the first year. By contrast, the vacancies information shows no signs of a significant nor substantial increase in training enrollment.

The treatment effects on training remain substantial for two years after the intervention (Table B2 and Figure B2). After two years, treated recipients still exhibit a 10% higher likelihood of having participated in job training. The intervention's impact, thus, extends beyond merely prompting earlier training among job seekers; it also leads to a sustained increase in training enrollment.

The sustained increase in training goes hand-in-hand with a lasting reduction in other ALMPs' participation. The reduction in application course enrollment starts right after the intervention, reaches its strongest magnitude about 4 months after the intervention, and remains constant thereafter (Figure B3). Reductions in subsidized employment start to emerge only about 5 months after the intervention and intensify over time (Figure B4).

Fig. 3.2: Average treatment effects on training enrollment over time



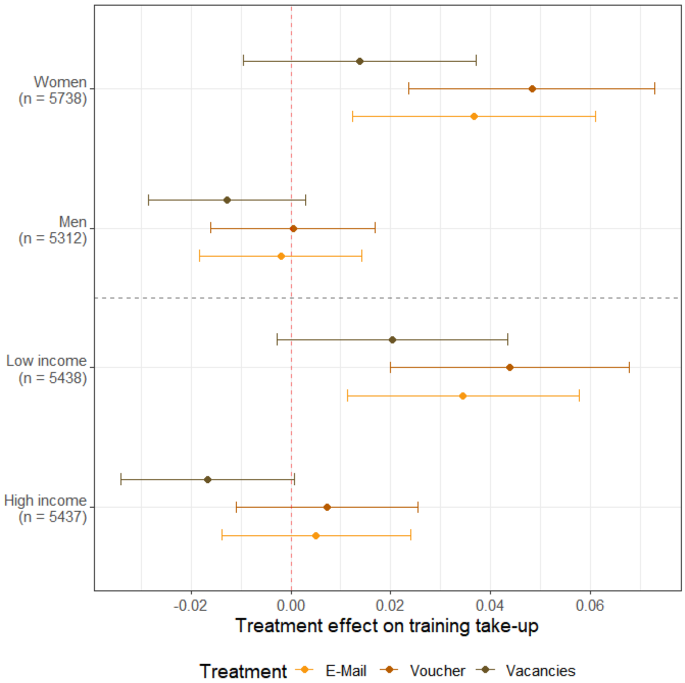
Note: Confidence intervals are reported at the 90%-level.

Heterogeneity To account for heterogeneity, we conducted sub-group regressions of the baseline equation for the main outcome variable. Additional analyses are shown in Section B.2.2.1.

The overall positive treatment effect is mostly driven by women and unemployed workers with lower income in their previous job (Figure 3.3 and Table 3.4). Further, unemployed people older than 35, with Austrian citizenship, or white-collar occupations seem to contribute more to the effect. There are no clear patterns by education or language skills. Heterogeneous effects are similar between providing information (e-mail) and additionally signalling the monetary value (voucher).

Treatment 3 (e-mail + voucher + information) results in interesting diverging outcomes for different sub-groups (Table B5). Contrary to treatments 1 and 2, job seekers in blue-collar occupations react more positively than those in white-collar occupations. The same holds for low-skilled compared to high-skilled occupations. The estimates point in a negative direction for comparatively advantaged groups, such as men, higher income, and core age groups, albeit not significantly. In Section 3.5, we discuss the interpretation of these patterns more in-depth.

Fig. 3.3: Heterogeneity in average treatment effects on training enrollment by gender and income



Note: Confidence intervals are reported at the 90%-level.

Table 3.4: Heterogeneity in training enrollment by gender and income

	Dependent variable:			
	Training take-up			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail	0.034***	−0.002	0.032***	0.005
	(0.013)	(0.010)	(0.012)	(0.011)
Voucher	0.046***	0.0004	0.040***	0.007
	(0.013)	(0.010)	(0.012)	(0.011)
Vacancies	0.012	−0.013	0.018	−0.016
	(0.013)	(0.010)	(0.012)	(0.011)
Control Group Mean	0.137	0.086	0.113	0.102
Control Group SD	0.344	0.28	0.317	0.302
Observations	5,523	5,191	5,363	5,351
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.			

3.4.2 Employment

To analyze treatment effects on employment, we first present baseline results on labor market outcomes and then discuss sub-group results. Table 3.5 reports the results of the 3 treatment arms on employment 24 months after the intervention. To maximize statistical power, we pool individuals in the treatment groups that increase training (treatment groups 1 and 2).

Main Findings Our intervention fails to improve employment status of job seekers within the 24-month period observed. We do not find statistically significant effects for any of the outcomes. However, the coefficients point in a negative direction across a range of outcomes and estimation approaches (Table 3.5). This pattern suggests negative consequences of training on employment status and wages. The short-term employment effects 1 year after the intervention show the same pattern (Table B10). The coefficient for being in employment at any point after the intervention is negative but not statistically significant. Instrumenting training program participation with the information intervention results also in a negative but non-significant coefficient for employment status (column 2). On average, job seekers in the treatment group spent 6 days less in employment. Days in unemployment also decreased marginally (column 4).[13] Neither wages nor job quality increases with training (columns 5 and 6). The findings are robust across different outcome definitions including income (Table B11) and estimation strategies including IV (Table B12) as well as when observing treatment groups separately (Table B13). Signs of negative employment effects start appearing from 4 months after the intervention and solidify, especially for the voucher group, over a two year period (Figure B5).

Table 3.5: Average treatment effects on employment

	Dependent variable:					
	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Job quality
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail + Voucher	−0.008		−6.159	−3.263	−0.086	0.001
	(0.009)		(6.794)	(5.864)	(0.769)	(0.004)
Training		−0.314				
		(0.496)				
Control Group Mean	0.754	0.754	350.103	361.954	50.814	0.382
Control Group SD	0.431	0.431	310.971	259.588	29.494	0.144
Observations	10,714	10,714	10,714	10,714	7,723	7,323

Note: Long-term refers to 2 years after the intervention. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

[13]The categories employment, unemployment and out of labor force sum up to one.

Heterogeneity We do not find significant heterogeneity in employment effects (Appendix B.2.4). Employment effects tend to be more negative for groups with the stronger increase in training, which suggests that lock-in effects drive the employment effects. This includes women (Table B14), those aged 35 to 50 (Table B15), those with Austrian citizenship (Table B16), and those who previously worked in medium-skilled occupations (Table B17). However, the heterogeneous effects are not statistically significant and thus have to be viewed with caution.

3.5 Mechanisms

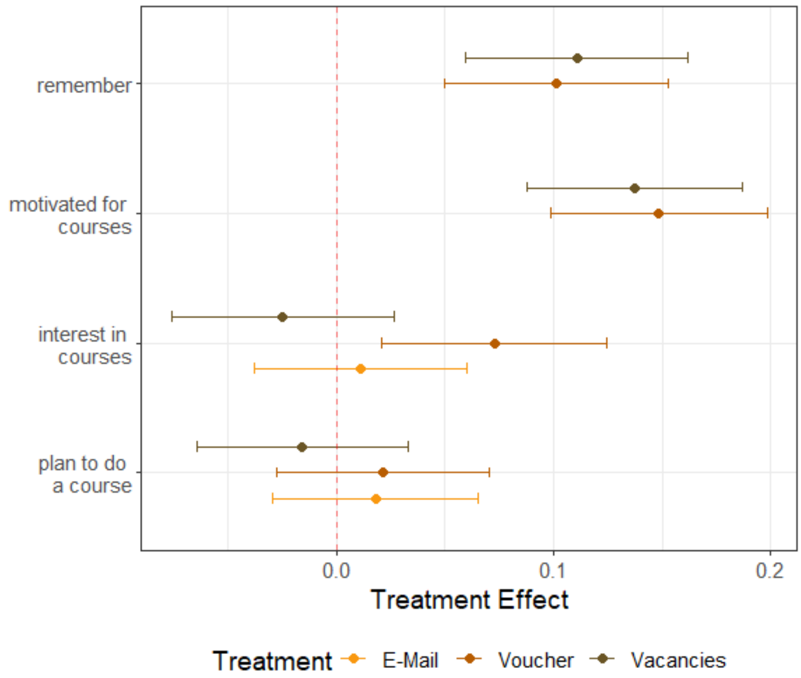
We investigate mechanisms behind job seekers' training participation, the role of caseworkers, and the unintended consequences of the vacancy treatment.

3.5.1 Training intentions

First, we assess the treatments' effectiveness in shifting job seekers' intentions to train. We collect data on intentions with the survey detailed in Section 3.3 and Section B.3.2. We compare whether the treatments affect job seekers' intentions, whether intentions translate into enrollment, and whether treatments affect perceptions of job training. We do so to better understand the role information and psychological frictions play in preventing job seekers from participating in training.

Intentions The treatments are successful in shifting job seekers intentions to participate in training (Figure 3.4). Interest in courses offered by the PES increases after receiving the voucher. Plans to enroll in a program show signs of elevation for e-mail and voucher recipients but the effects are not statistically significant. By contrast, interest and plans to enroll in a program seem to decrease for recipients of the vacancies treatment though not statistically significant. Among those who were treated, job seekers who received the voucher and vacancies information showed greater intentions compared to those who received only the e-mail. More specifically, they were more likely to remember the information received. They also showed higher motivation to participate in courses. Overall, these results demonstrate that the treatments are successful in shifting job seekers' stated preferences for training participation.

Fig. 3.4: Average treatment effect on intentions to train



Note: Outcomes for the intention types *Recollection of treatment* and *Motivation for courses* are relative to treatment group 1, while the other outcomes are compared to the control group. Confidence intervals are reported at the 90%-level.

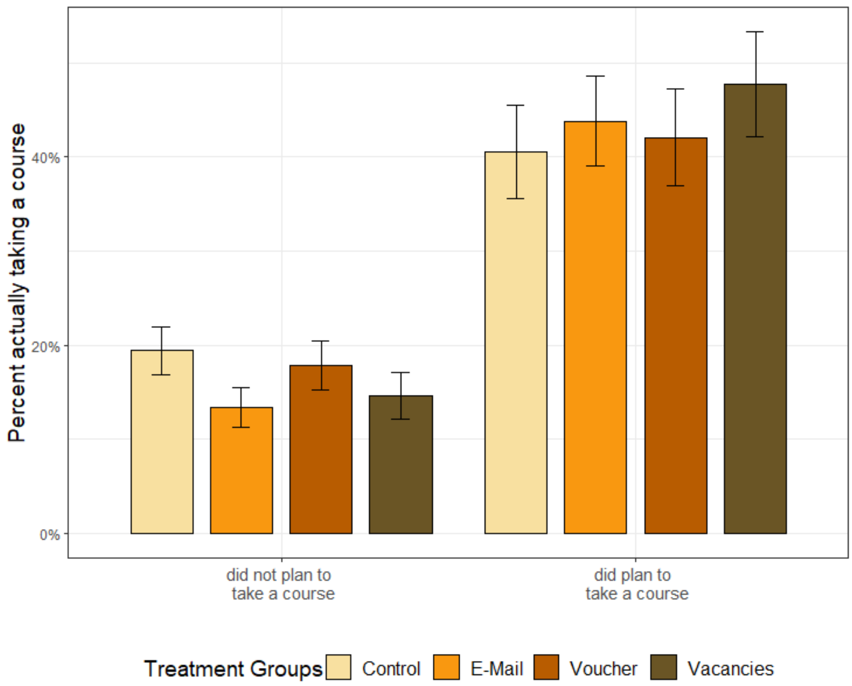
Intentions and enrollment Intentions for training translate into program enrollment (Figure 3.5). Among job seekers who planned to take a course, 40%–50% eventually enroll in a program. By contrast, among those who did not plan to take a course, only 10%–20% eventually enroll in a training program[14]. Among treatment groups no sizeable differences in the correlation of intentions and actual training enrollment are detected. Overall, job

[14]This compares to 40% who stated to have enrolled in a program because they were assigned to it (Figure C5)

seekers intentions are found to matter for training enrollment, which underscores job seekers' discretion in deciding whether to enroll in a program.

For the control group, a smaller share of those who planned to enroll follow through and enroll compared to the treatment groups. Conversely, the share of those who did not plan to enroll but eventually enroll is higher in the control group compared to the treatment groups. The comparison suggests that among survey respondents, about 5% of job seekers would have enrolled in job training regardless of whether their intention was shifted by the treatments.

Fig. 3.5: Training enrollment by intentions



Note: Confidence intervals are reported at the 90%-level.

Perceptions The intervention shifts perceptions of job training reducing information and psychological frictions. In particular, the e-mail and voucher treatments raise awareness and signal the monetary value of training (Table 3.6). Recipients of the e-mail and voucher report less often that they lack information on courses (column 1), which indicates the effectiveness of the treatment in raising awareness and informing job seekers about their training options. In parallel, recipients of the vacancies information tend to report more often that they lack information, which could indicate that the information on occupations with job openings may have provided insufficient content to inform job seekers about their options. Job seekers who receive the voucher tend to report more often that courses are expensive (column 2), which indicates that the voucher is effective in signalling the monetary value of training programs. While the intervention seems to have shifted perceptions of job training in the way intended, the coefficients are not statistically significant, which is likely related to the lower sample size in the survey data.

Table 3.6: Perceptions of courses

	Dependent variable:	
	Lack information	Courses are expensive
	(1)	(2)
E-mail	−0.030	0.017
	(0.038)	(0.030)
Voucher	−0.015	0.030
	(0.040)	(0.031)
Vacancies	0.054	0.035
	(0.040)	(0.031)
Reference Mean	0.425	0.64
Reference SD	0.495	0.48
Caseworker Fixed Effects	0	1
Observations	1,145	1,722

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

3.5.2 Caseworkers

The intervention increases job seekers' interest in training and their intention to take up training. However, for job seekers to be assigned to a training program, they need caseworkers' approval. Increased training intentions from job seekers can, thus, either result in increased enrollment or increased rejection of job seekers' training intentions by caseworkers. While the importance of caseworker discretion has been documented for job search requirements (Arni and Schiprowski, 2019), we are, to our knowledge, the first to provide evidence on their role in the context of training assignment in a quantitative study.

Assignment The treatments strengthen job seekers self-assessed autonomy over program assignment, which has the side effect of increasing rejections of training intentions by caseworkers (Table 3.7). Recipients of any treatment feel more in control over which course to choose (column 1). However, treated job seekers report less often that caseworkers consider their wishes for training program assignment (column 2), which suggests increased disagreement between job seekers and caseworkers about course choice. Consequently, caseworkers more often reject job seekers' training wishes (column 3). These outcomes suggest that while job seekers feel some autonomy over program assignment, that autonomy is constrained by the required approval by caseworkers. Although this indicates the boundaries of increasing perceived autonomy without changing the formal assignment rules, the treatments nevertheless affect program assignment. While training intentions of some job seekers are turned down, others cannot find suitable courses despite increased interest (column 5). Moreover, treated job seekers tend to report less often that assignment to a course by a caseworker is the reason for program enrollment (column 4). Uncovering these mechanisms helps to understand the potential of reducing information and psychological frictions while it shows the remaining limitations set by caseworkers and assignment rules.

Table 3.7: Training program assignment

	Dependent variable:				
	Choose own courses	My wishes are considered	Course was turned down	Assigned to course	Could not find suitable course
	(1)	(2)	(3)	(4)	(5)
E-mail	0.068**	−0.054*	0.051	−0.161	0.235
	(0.031)	(0.029)	(0.033)	(0.054)	(0.039)
Voucher	0.069**	−0.068**	0.105***	−0.573	0.442**
	(0.032)	(0.030)	(0.036)	(0.055)	(0.041)
Vacancies	0.091***	−0.052*	0.036	−0.316	0.368*
	(0.032)	(0.030)	(0.034)	(0.059)	(0.041)
Reference Mean	0.362	0.741	0.225	0.465	0.454
Reference SD	0.481	0.439	0.419	0.501	0.499
Caseworker Fixed Effects	1	1	0	0	0
Observations	1,722	1,722	1,145	480	1,145

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

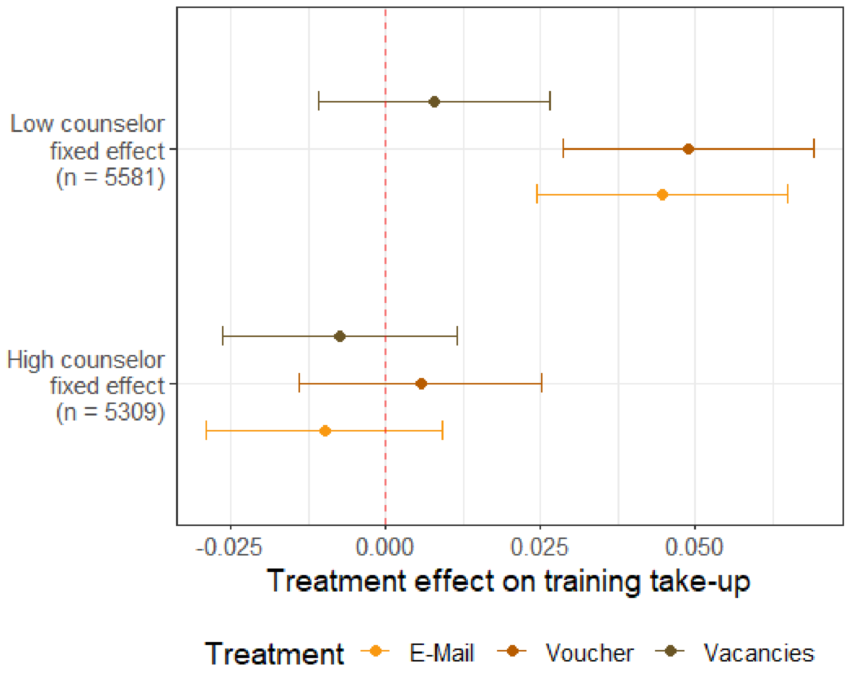
Heterogeneity by caseworkers To further unravel the role of caseworkers, we analyze the interaction of the treatment effects with caseworkers by categorizing caseworkers as high and low productive. We use the fitted values of caseworker fixed effects from a regression on employment duration, as a desired outcome of PES counselling.[15] We control for all baseline covariates and the treatment group. We then construct a dummy that takes the value 1 if the fixed effect of the job seeker's caseworker is higher than the median (high productive); otherwise the value is 0 (low productive). Finally, we re-estimate our main analysis separately for the two sub-groups.

The treatment effect on training enrollment is strongly driven by job seekers assigned

[15] A longer employment duration after the period of unemployment indicates a good match.

to low productive caseworkers (Figure 3.6). The result is robust to alternative dependent variables in the fixed effects estimation (training completion and unemployment duration) (Table B18). We conclude that information interventions affect job seekers counselled by low productive caseworkers. We further discuss the interpretation of caseworker fixed effects in Section 3.6.

Fig. 3.6: Average treatment effect on training enrollment by caseworker type



Note: Confidence intervals are reported at the 90%-level.

3.5.3 Unintended consequences

Vacancies While the vacancies information does not increase training enrollment, it increases perceived autonomy and the disconnect between training intentions and behavior. Sub-group analyses show that the vacancies information may have discouraged from training those job seekers who are overqualified for the jobs with high labor demand. Our survey provides further suggestive evidence (Table B19). Among low educated survey participants, 55% find the information on job openings helpful and nearly 50% are willing to take a job in one of the included occupations. Among those educated above the minimum, only 35% find the information helpful and only 30% are willing to take a job in the listed occupations.

Given that the vacancies information does not affect aggregate training enrollment, one may wonder whether it changes which programs job seekers enroll in with respect to the vacancies. Our analysis, however, does not support this claim (Table B20).

3.6 Discussion

In this section, we compare the magnitude of our effects to related studies and discuss potential mechanisms and implications that could be drawn from our findings. We do this for training (Section 3.6.1) and employment (Section 3.6.2).

3.6.1 Training

The findings are remarkable in three aspects: their large magnitude given a one-off information intervention, the insights we provide into the job seeker-caseworker relationship, and unintended consequences caused by the vacancy information. We further discuss explanations for the spillovers and heterogeneity observed.

Magnitude An increase of 18% to 21% from baseline is substantial for a one-off information intervention that consists only of an e-mail. The closest related studies have found null effects of providing and framing information on training enrollment (Dhia and Mbih, 2020; Leduc and Tojerow, 2023) Like ours, both experiments took place as part of broader PES campaigns to promote job training. Our results are in line with information interventions outside the labor market, which have found larger effects of providing information in mailings. This includes a 35-60% increase in filing applications for social benefits (Bhargava and Manoli, 2015), an increase of up to 15% in compliance with municipal housing codes (Linos et al., 2020), an increase of up to 11% in registrations of high school students for state scholarships (Linos et al., 2022b), and an 11% increase in rental assistance program applications (Lasky-Fink and Linos, 2022). In an observational study, Barr and Turner (2018) find that information letters increase college enrollment of job seekers in the U.S. by 40%, particularly among vulnerable job seekers.

Of the various reasons that may explain, why our experiment was the first to be successful in shifting training enrollment of job seekers, differences in the approach of caseworkers seem most convincing. While the design of the e-mail may be more accessible and appealing to job seekers, our e-mail (treatment 1) is similar in design and content to Dhia and Mbih (2020); Leduc and Tojerow (2023). Similarly, contextual factors may have amplified the large effect on training. Indeed, the intervention was implemented during a large-scale expansion of training programs, which may have lowered the bar for enrollment for job seekers. However, the experiments in Dhia and Mbih (2020); Leduc and Tojerow (2023) took place during similar periods of training expansion-a time suitable for PES to collaborate on information campaigns. Therefore, it seems likely that differences in the approach of PES caseworkers could play a role.

The role of caseworkers We open the black box of caseworker relationships with job seekers. Job seekers subject to the intervention report an increase in rejections of their expressed wishes to enroll in job training after an increase in conversations about job training with their caseworkers. Treatment effects are concentrated among job seekers assigned to caseworkers, whose job seekers have shorter employment durations. Such low fixed effects could be interpreted as capturing less productive or more lenient caseworkers who exert less pressure for re-employment on job seekers. Leniency may translate into job training assignment, which is increased for job seekers assigned to caseworkers who are willing to follow job seekers' expressions of interest. None of the previous studies collect data on rejection rates of job seekers' training intentions. While they do report increases in call-back rates (Dhia and Mbih, 2020) and intentions to enroll in trainings (Leduc and

Tojerow, 2023), it did not translate into training enrollment. Activation requirements in France and Belgium are overall not more stringent than in Austria and even more lenient with regard to ALMP participation (OECD, 2023). The dynamic between job seekers and caseworkers could still play a role. Job seekers, for instance, typically express interest in training informally during repeated interactions with their caseworkers. As discussed in Section 3.2, 87% of caseworkers in Austria report that they assign job seekers to training programs of their choice (Schönherr and Glaser, 2023), which may explain why the intervention in Austria was successful.

Unintended consequences The negative effect of the vacancy information (treatment 3) on training enrollment indicates the importance of targeting information to specific sub-groups, which we test in a follow-up experiment. The purpose of the vacancy information was to provide additional information on the labor market to broaden job seekers' search and training choices towards occupations with high labor demand. As most of the advertised jobs are in low skill occupations, job seekers with educational attainment above the minimum became discouraged from training, while job seekers with minimum educational attainment increased training.

Spillovers Encouraging job seekers to engage in training shifts their participation away from other ALMPs, such as application courses and subsidized employment. The shift may be driven by job seekers' preferences. Our survey documents that job seekers' attitudes differ by ALMP type reflecting fundamental differences in underlying logics (cf. Vlandas, 2013). Application courses are more frequently perceived as a disciplining measure while training programs, in particular longer ones, usually involve an active choice of job seekers. Indeed, our findings are consistent with studies that have found stigma effects to be more severe for application courses and subsidized employment than for job training (Baert, 2016; Van Belle et al., 2019; Kübler et al., 2019; Gatta, 2023). Others have found mandatory assignment, not ALMP participation itself, causes stigma as it may lead employers to interpret ALMP participation as a sign of negative assessment by a caseworker (Liechti et al., 2017).

Heterogeneity Disadvantaged groups, in particular women and job seekers on lower income, drive the aggregate increase in training, which reveals an interesting finding with regard to other studies on widening access to educational programs. Typically, such studies identify a Matthew Effect, first established for higher education, which documents that expanding access to education benefits disproportionally those more likely to enroll in the first place, which widens inequalities. Our study follows this pattern, however, with a different result. Women and job seekers on lower income, who enroll disproportionally in job training, increase their enrollment disproportionally. Yet, by contrast to settings that have documented the Matthew Effect, these groups are disadvantaged. As such, their increased enrollment has the potential to reduce inequalities. Indeed, studies have found women and lower income job seekers to benefit disproportionally from job training (Zweimüller and Winter-Ebmer, 1996; Card et al., 2018). Further, our finding might stem from an "access bias" that emerges through particular target groups for trainings (Bonoli and Liechti, 2018). For instance, a subset of training programs are specifically aimed at unemployed women re-entering the workforce post-childbirth. A contextual factor may have contributed as well. Women experienced a sharper increase in unemployment than men during the pandemic (Leitner and Tverdostup, 2023).

3.6.2 Employment

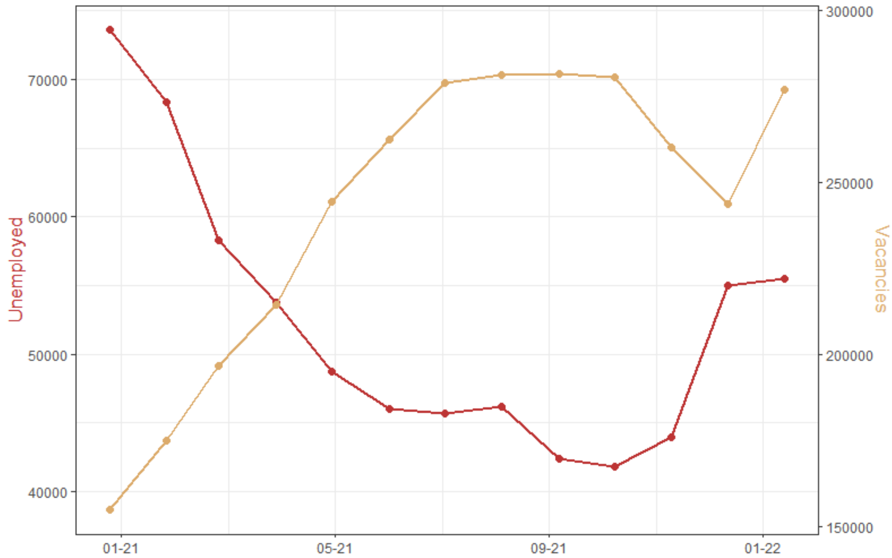
The absence of positive employment effects of training is surprising. We compare our results to related studies and discuss reasons that help understand our findings including possible lock-in effects and the macroeconomic context.

Comparison We do not have directly comparable estimates for employment from similar experiments on training take-up, since those did not shift program enrollment as described in Section 3.6.1. However, we can compare our employment effects to those in Barr and Turner (2018), given their strong increase of enrollment in post-secondary education following an information letter. In line with our findings, they do not find any effects on earnings after three years, suggesting that the returns to increased education offset the negative immediate earnings effects of pausing active job search to enroll in an educational program (lock-in effects). Experiments that provide information treatments to improve job search have delivered mixed results on employment. Providing access to a website targeted to broaden the set of jobs considered delivers null results (Belot et al., 2019). Providing a brochure with job search advice increases employment by 1-4% (Altmann et al., 2018). Providing access to a website with resume and cover letter templates increases employment by 8% (Briscese et al., 2020) and instructing job seekers on how to use the career network website LinkedIn by 10% (Wheeler et al., 2022). Magnitudes measured in days remain small where reported similar to our results. During the year after the intervention, job seekers who receive the job search brochure, on average, increase their employment for about 1.2 days (Altmann et al., 2018). Observational evaluations of job training tend to find small but positive employment effects, though only in the medium- or long-run (Card et al., 2010, 2018). However, our estimates are not directly comparable since employment effects in our study are driven by the subset of participants responsive to the information intervention. Some have shifted to job training from enrolling in application courses, which may lower the employment effect.

Lock-in effects Training program participation can divert job seekers' time and attention temporarily from job search and thereby lengthen unemployment spells. Such lock-in effects of job training programs are widely documented (Lechner and Wunsch, 2009; Lechner et al., 2011). Indeed, we find signs of negative employment effects in the short-run (Figure B5), which dissipate after a year. Lock-in effects are found to be smaller during recessions (Lechner and Wunsch, 2009), which corresponds to our case.

Macroeconomic context Job training participants may have missed out on job opportunities during the rapid labor market recovery in Spring 2021 prioritizing training over job search. While the timing of the intervention coincided with the Covid pandemic to minimize lock-in effects, a strong labor market recovery followed soon after (Figure 3.7). The increase in training enrollment was concentrated in Spring 2021 (Figure 3.1), a recovery period which saw sharply unemployment fall sharply and the number of vacancies double. Following the intervention, participants in job training may have missed out on job opportunities during the recovery prioritizing training instead of job search.

Fig. 3.7: Labor market context



Note: Number of unemployed and posted vacancies in Lower Austria in 2021.

Source: AMS DataWarehouse.

To compare interactions with contextual factors, we investigate the effects of training over an entire year after the Covid-induced lockdowns in our follow-up experiment Lehner and Schwarz (2022). The treatment period (2022-2023) covers times of high and low unemployment–to the best of our knowledge, the first time in an experimental setting.

3.7 Conclusion

Public employment services (PES) across high income countries struggle to attract unemployed workers to voluntary enroll in job training. Many job seekers are hesitant due to barriers from information frictions and psychological frictions. Our multi-armed field experiment at scale demonstrates the benefits of raising awareness and signaling the monetary value. Reducing information frictions by raising awareness increases program enrollment by 18%. Reducing psychological frictions, for instance from internalized stigma, by signaling the monetary value of job training increases training enrollment by 21% and completion even by 28%. Effects are sizable and concentrated among women and low-income job seekers. However, providing information on labor demand can discourage job seekers from enrolling in training programs, in particular those who feel overqualified for jobs with open vacancies. Overall, our findings suggest that information interventions can be effective in reducing barriers to training. However, we do not find positive effects of job training on employment or wages.

Outlook Based on the positive effects on training enrollment, the PES in Lower Austria has implemented the most effective treatment on a permanent basis. Further evaluations should be carried out in other countries and time periods to investigate the surprising absence of positive effects of training on employment. As part of the permanent implementation, we continue to use random assignment of the most effective intervention (treatment 2, voucher) and targeted information on job vacancies by education (modified version of treatment 3) to investigate the effects of targeted information on training. This follow up field experiment spans an entire year post-pandemic to examine whether job training has varying consequences during times of low and high unemployment, and to account for possible distortions due to seasonality and the Covid pandemic.

Implications Our study contributes to the literature on information frictions and psychological frictions as barriers to incomplete take-up of social programs. Disadvantaged people often lack awareness of social programs and experience social stigma related to participation. The results provide evidence on the effectiveness of information interventions in reducing such barriers to increase program take-up. The study also contributes to the active labor market policy evaluation literature. The employment results raise questions about the rationale of encouraging job seekers to participate in job training. The findings strengthen the evidence base to design and implement effective training programs for unemployed workers. Overall, our study shows that information provision can help overcome barriers to program participation but governments should prioritize making social programs effectively work for disadvantaged people.

Bibliography

Abebe, G., Caria, S., Fafchamps, M., Falco, P., Franklin, S., Quinn, S., and Shilpi, F. (2021). Matching Frictions and Distorted Beliefs: Evidence from a Job Fair Experiment. page 83.

Adams-Prassl, A., Boneva, T., Golin, M., and Rauh, C. (2023). Perceived returns to job search. *Labour Economics*, 80:102307.

Altmann, S., Falk, A., Jäger, S., and Zimmermann, F. (2018). Learning about job search: A field experiment with job seekers in Germany. *Journal of Public Economics*, 164:33–49.

Anders, J. and Rafkin, C. (2022). The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association*, 91(434):444–455.

Arni, P. and Schiprowski, A. (2019). Job search requirements, effort provision and labor market outcomes. *Journal of Public Economics*, 169:65–88.

Athey, S. and Imbens, G. W. (2017). The Econometrics of Randomized Experiments. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevir.

Baert, S. (2016). Wage subsidies and hiring chances for the disabled: Some causal evidence. *The European Journal of Health Economics*, 17(1):71–86.

Bandiera, O., Bassi, V., Burgess, R., Rasul, I., Sulaiman, M., and Vitali, A. (2021). The Search for Good Jobs: Evidence from a Six-year Field Experiment in Uganda. *SSRN Electronic Journal*.

Barbanchon Le, T., Hensvik, L., and Rathelot, R. (2023). How can AI improve search and matching? Evidence from 59 million personalized job recommendations.

Barr, A. and Turner, S. (2018). A letter and encouragement: Does information increase postsecondary enrollment of UI recipients? *American Economic Journal: Economic Policy*, 10(3):42–68.

Belot, M., Kircher, P., and Muller, P. (2019). Providing advice to jobseekers at low cost: An experimental study on online advice. *The Review of Economic Studies*, 86(4):1411–1447.

Bertrand, M., Luttmer, E. F. P., and Mullainathan, S. (2000). Network Effects and Welfare Cultures. *The Quarterly Journal of Economics*, 115(3):1019–1055.

Bhargava, S. and Manoli, D. (2015). Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment. *American Economic Review*, 105(11):3489–3529.

Black, D. A., Smith, J. A., Berger, M. C., and Noel, B. J. (2003). Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System. *The American Economic Review*, 93(4):1313–1327.

Boeri, T. and van Ours, J. C. (2021). Active Labor Market Policy. In *The Economics of Imperfect Labor Markets*. Princeton University Press, Princeton, third edition edition.

Böheim, R., Horvath, G. T., and Winter-Ebmer, R. (2011). Great expectations: Past wages and unemployment durations. *Labour Economics*, 18(6):778–785.

Bonoli, G. (2010). The Political Economy of Active Labour Market Policy. *SSRN Electronic Journal*.

Bonoli, G. and Liechti, F. (2018). Good intentions and Matthew effects: Access biases in participation in active labour market policies. *Journal of European Public Policy*, 25(6):894–911.

Briscese, G., Zanella, G., and Quinn, V. (2020). Improving Job Search Skills: A Field Experiment on Online Employment Assistance.

Bursztyn, L. and Jensen, R. (2017). Social Image and Economic Behavior in the Field: Identifying, Understanding, and Shaping Social Pressure. *Annual Review of Economics*, 9(1):131–153.

Card, D., Kluve, J., and Weber, A. (2010). Active labour market policy evaluations: A meta-analysis. *The Economic Journal*, 120(548):F452–F477.

Card, D., Kluve, J., and Weber, A. (2018). What works? A meta analysis of recent active labor market program evaluations. *Journal of the European Economic Association*, 16(3):894–931.

Celhay, P. A., Meyer, B. D., and Mittag, N. (2022). Stigma in Welfare Programs.

Clasen, J. and Clegg, D. (2011). *Regulating the Risk of UnemploymentNational Adaptations to Post-Industrial Labour Markets in Europe*. Oxford University Press.

Clasen, J., Clegg, D., and Goerne, A. (2016). Comparative Social Policy Analysis and Active Labour Market Policy: Putting Quality before Quantity. *Journal of Social Policy*, 45(1):21–38.

Crépon, B., Duflo, E., Gurgand, M., Rathelot, R., and Zamora, P. (2013). Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment *. *The Quarterly Journal of Economics*, 128(2):531–580.

Crépon, B. and van den Berg, G. J. (2016). Active Labor Market Policies. *Annual Review of Economics*, 8(1):521–546.

Currie, J., Grogger, J., Burtless, G., and Schoeni, R. F. (2001). Explaining Recent Declines in Food Stamp Program Participation [with Comments]. *Brookings-Wharton Papers on Urban Affairs*, pages 203–244.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer Effects in Program Participation. *American Economic Review*, 104(7):2049–2074.

Dhia, A. B. and Mbih, E. (2020). Do informational frictions affect enrollment in public-sponsored training? Results from an online experiment.

Ebbinghaus, B. (2020). Changing work and welfare: Unemployment and labour market policies. In *Handbook on Society and Social Policy*, chapter Handbook on Society and Social Policy, pages 291–305. Edward Elgar Publishing.

Eppel, R., Huemer, U., Mahringer, H., and Schmoigl, L. (2022). Evaluierung der Effektivität und Effizienz von Qualifizierungsförderungen des Arbeitsmarktservice Österreich.

Finkelstein, A. and Notowidigdo, M. J. (2019). Take-Up and Targeting: Experimental Evidence from SNAP. *Quarterly Journal of Economics*, 134(3):1505–1556.

Fossati, F., Liechti, F., and Wilson, A. (2021). Participation in labour market programmes: A positive or negative signal of employability? *Acta Sociologica*, 64(1):70–85.

Friedrichsen, J., König, T., and Schmacker, R. (2018). Social image concerns and welfare take-up. *Journal of Public Economics*, 168:174–192.

Gatta, A. (2023). Do Employers Discriminate Participants in Active Labour Market Policies? A field experiment during the Covid-19 pandemic. *Working Paper*.

Gautier, P., Muller, P., van der Klaauw, B., Rosholm, M., and Svarer, M. (2018). Estimating Equilibrium Effects of Job Search Assistance. *Journal of Labor Economics*, 36(4):1073–1125.

Goffman, E. (1963). *Stigma: Notes on the Management of Spoiled Identity*. Spectrum Book. Prentice-Hall, Englewood Cliffs.

Goldin, J., Homonoff, T., Javaid, R., and Schafer, B. (2022). Tax filing and take-up: Experimental evidence on tax preparation outreach and benefit claiming. *Journal of Public Economics*, 206:104550.

Haaland, I., Roth, C., and Wohlfart, J. (2023). Designing Information Provision Experiments. *Journal of Economic Literature*, 61(1):3–40.

Harrison, G. W. and List, J. A. (2004). Field Experiments. *Journal of Economic Literature*, 42(4):1009–1055.

Heckman, J. J., Lalonde, R. J., and Smith, J. A. (1999). The Economics and Econometrics of Active Labor Market Programs. In *Handbook of Labor Economics*, volume 3, pages 1865–2097. Elsevier.

Heckman, J. J. and Smith, J. A. (2004). The Determinants of Participation in a Social Program: Evidence from a Prototypical Job Training Program. *Journal of Labor Economics*, 22(2):243–298.

Heffetz, O., O'Donoghue, T., and Schneider, H. S. (2022). Reminders Work, but for Whom? Evidence from New York City Parking Ticket Recipients. *American Economic Journal: Economic Policy*, 14(4):343–370.

Hervelin, J. (2021). Directing young dropouts via SMS: Evidence from a field experiment. *IZA Journal of Labor Policy*, 12(1).

Hofer, H., Weber, A., and Winter-Ebmer, R. (2013). Labor market policy in Austria during the Crises. Technical report.

Imbens, G. W. and Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 62(2):467.

Immervoll, H. and Knotz, C. (2018). How Demanding Are Activation Requirements for Jobseekers? Technical report.

Kasy, M. and Lehner, L. (2023). Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program. *Working Paper*.

Katz, L. F., Roth, J., Hendra, R., and Schaberg, K. (2022). Why Do Sectoral Employment Programs Work? Lessons from WorkAdvance. *Journal of Labor Economics*, 40(S1):S249–S291.

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics*, 17(6):904–918.

Knotz, C. M. (2020). Does Demanding Activation Work? A Comparative Analysis of the Effects of Unemployment Benefit Conditionality on Employment in 21 Advanced Economies, 1980-2012. *European Sociological Review*, 36(1):121–135.

Kübler, D., Stüber, R., and Schmid, J. (2019). Take Your Time to Grow: A Field Experiment on the Hiring of Youths. *German Economic Review*, 20(4):e706–e729.

Lasky-Fink, J. and Linos, E. (2022). It's Not Your Fault: Reducing Stigma Increases Take-up of Government Programs.

Lechner, M., Miquel, R., and Wunsch, C. (2011). LONG-RUN EFFECTS OF PUBLIC SECTOR SPONSORED TRAINING IN WEST GERMANY. *Journal of the European Economic Association*, 9(4):742–784.

Lechner, M. and Wunsch, C. (2009). Are Training Programs More Effective When Unemployment Is High? *Journal of Labor Economics*, 27(4):653–692.

Leduc, E. and Tojerow, I. (2023). Training Jobseekers to Address Labour Shortages: An Experimental Study on Information Barriers.

Lehner, L. and Schwarz, A. (2021). Reframing active labor market policy: Experimental evidence of training vouchers for unemployed. Technical report, American Economic Association.

Lehner, L. and Schwarz, A. (2022). What prevents job-seekers from training? Reframing training access to reduce non-take up. Technical report, American Economic Association.

Lehner, L. and Tamesberger, D. (2024). Unemployment and Labor Market Policy. In *Handbook of Social Infrastructure*. Edward Elgar Publishing.

Leitner, S. and Tverdostup, M. (2023). Auswirkungen der Corona- Pandemie auf den niederösterreichischen Arbeitsmarkt. *The Vienna Institute for International Economic Studies (wiiw) report.*

Liechti, F., Fossati, F., Bonoli, G., and Auer, D. (2017). The Signalling Value of Labour Market Programmes. *European Sociological Review*, page jcw061.

Linos, E., Prohofsky, A., Ramesh, A., Rothstein, J., and Unrath, M. (2022a). Can Nudges Increase Take-Up of the EITC? Evidence from Multiple Field Experiments. *American Economic Journal: Economic Policy*, 14(4):432–452.

Linos, E., Quan, L. T., and Kirkman, E. (2020). Nudging Early Reduces Administrative Burden: Three Field Experiments to Improve Code Enforcement. *Journal of Policy Analysis and Management*, 39(1):243–265.

Linos, E., Reddy, V., and Rothstein, J. (2022b). Demystifying college costs: How nudges can and can't help. *Behavioural Public Policy*, pages 1–22.

List, J. (2022). Some Tips for Doing Better Field Experiments and Getting Your Work Published. *Artefactual Field Experiments*, (00755).

Maibom, J., Harmon, N., Glenny, A., and Fluchtmann, J. (2023). Unemployed Job Search across People and over Time: Evidence from Applied-for Jobs. *Journal of Labor Economics*, page 725165.

Miano, A. (2023). Search Costs, Outside Options, and On-the-Job Search.

Moffitt, R. (1983). An Economic Model of Welfare Stigma. *American Economic Review*, 73(5):1023.

Mueller, A. I., Spinnewijn, J., and Topa, G. (2021). Job Seekers' Perceptions and Employment Prospects: Heterogeneity, Duration Dependence, and Bias. *American Economic Review*, 111(1):324–363.

Muller, P., Kircher, P., Belot, M., and De Koning, B. (2023). Occupational advice for job seekers in the Netherlands.

OECD (1994). The OECD Jobs Study: Facts, Analysis, Strategies. Technical report, Organisation for Economic Co-operation and Development, Paris.

OECD (2018). Good Jobs for All in a Changing World of Work. The OECD Jobs Strategy. Technical report, Organisation for Economic Co-operation and Development, Paris.

OECD (2023). OECD Employment Database. Public expenditure and participant stocks on LMP. https://stats.oecd.org//Index.aspx?QueryId=123308.

OECD (2023). OECD Strictness of activation requirements database: Item 1. Availability requirements: ALMP participation. https://stats.oecd.org//Index.aspx?QueryId=125823.

Osman, A. and Speer, J. D. (2023). Stigma and take-up of labour market assistance: Evidence from two field experiments. *Economica*, n/a(n/a).

Sahin, A., Song, J., Topa, G., and Violante, G. L. (2014). Mismatch Unemployment. *The American Economic Review*, 104(11):3529–3564.

Schönherr, D. and Glaser, H. (2023). Zwischen Fördern und Fordern: Auswirkungen individueller Beratungsund Vermittlungsstrategien auf die Beschäftigungschancen arbeitsuchender Menschen. Technical report, SORA Institute for Social Research & Consulting., Wien.

Spinnewijn, J. (2015). Unemployed but Optimistic: Optimal Insurance Design with Biased Beliefs. *Journal of the European Economic Association*, 13(1):130–167.

Stantcheva, S. (2023). How to Run Surveys: A Guide to Creating Your Own Identifying Variation and Revealing the Invisible.

UN Special Rapporteur, D. S. O. (2022). Non-take-up of rights in the context of social protection. Report of the Special Rapporteur on extreme poverty and human rights, Olivier De Schutter. United Nations General Assembly. Human Rights Council, Fiftieth session. Technical report.

Van Belle, E., Caers, R., De Couck, M., Di Stasio, V., and Baert, S. (2019). The Signal of Applying for a Job Under a Vacancy Referral Scheme. *Industrial Relations: A Journal of Economy and Society*, 58(2):251–274.

Vlandas, T. (2013). Mixing apples with oranges? Partisanship and active labour market policies in Europe. *Journal of European Social Policy*, 23(1):3–20.

Walker, R. (2014). *The Shame of Poverty.* Oxford University Press.

Weishaupt, J. T. (2011). *From the Manpower Revolution to the Activation Paradigm: Explaining Institutional Continuity and Change in an Integrating Europe.* Amsterdam University Press.

Wheeler, L., Garlick, R., Johnson, E., Shaw, P., and Gargano, M. (2022). LinkedIn(to) Job Opportunities: Experimental Evidence from Job Readiness Training. *American Economic Journal: Applied Economics*, 14(2):101–125.

Zweimüller, J. and Winter-Ebmer, R. (1996). Manpower Training Programmes and Employment Stability. *Economica*, 63(249):113–130.

Chapter 4

Beggaring thy co-worker: Labor market dualization and the wage growth slowdown in Europe

Co-authored with Paul Ramskogler and Aleksandra Riedl (Oesterreichische Nationalbank)

As temporary employment has become a pervasive feature of modern labor markets, reasons for wage growth have become less well understood. To determine whether these two phenomena are related, we investigate whether the dualized structure of labor markets affects macroeconomic developments. Specifically, we incorporate involuntary temporary workers into the standard wage Phillips curve to examine wage growth in 30 European countries for the period 2004-2017. Relying on individual-level data to adjust for a changing employment composition, we show, for the first time, that the incidence of involuntary temporary workers has strong negative effects on aggregate wage growth. This effect, which we name the competition effect, is particularly pronounced in countries where wage bargaining institutions are weak. Our findings shed further light on the reasons for the secular slowdown of wage growth after the global financial crisis.

Keywords: wages, segmentation, unemployment, labor market institutions, Europe

JEL classification: J31 Wage Level and Structure; Wage Differentials, J42 Monopsony; Segmented Labor Markets, J82 Labor Force Composition

We are grateful for feedback and suggestions in particular to Brian Nolan and Bernhard Ebbinghaus as well as Bastian Betthäuser, Joshua Cova, Tony Dobbins, Chris Featherman, Pirmin Fessler, Michael Ganslmeier, Jakob Kapeller, Andrew Messamore, and Tim Vlandas as well as the participants of the SASE EC workshop and annual conference 2022, the BUIRA doctoral workshop and annual conference 2022, and the Social Policy and Politics Research Group at the Department of Social Policy and Intervention, University of Oxford.

This work was supported by the Economic and Social Research Council [grant number ES/P000649/1]; the Scatcherd European fund; and the Saven European fund.

4.1 Introduction

Temporary employment has become a pervasive feature in European labor markets. The reason is simple: Temporary employment is more flexible in terms of labor cost adjustments than permanent employment. Further, it is well established that temporary employment comes with a wage penalty (Kahn, 2016; Pavlopoulos, 2013). For many employers, temporary employment is, thus, a cheaper source of labor. Yet, the flexibility of temporary contracts often disproportionately benefits employers instead of workers (Hyman, 2018), resulting in substantial levels of *involuntary* temporary workers. Involuntary temporary employment has behavioral implications central to our analysis. Notably, *involuntary* temporary employment, by definition, forms part of the labor supply for permanent employment and thus may foster competition between different segments of workers.

We propose a mechanism through which the presence of involuntary temporary employees dampens the bargaining power and wage growth of permanent workers. This "competition effect" is not captured by the unemployment rate. To empirically identify a potential competition effect, we draw on a sample of 30 European countries in the period 2004-2017. Given the large country heterogeneity with respect to the incidence of involuntary temporary employment in Europe, the effect is expected to be mainly driven by differences *across countries.* To what extent temporary employment can influence the bargaining power of workers likely depends on a country's institutional framework. Therefore, we expect the institutional framework to reinforce or dampen the impact of temporary employment on wage growth.

While cross-country variation of temporary employment might predominate, the within-country variation linked to business cycle dynamics should not be neglected. The incidence of temporary workers increases in the early stages of a recovery (i.e., when the unemployment rate starts to decline) and falls swiftly in the downturn.

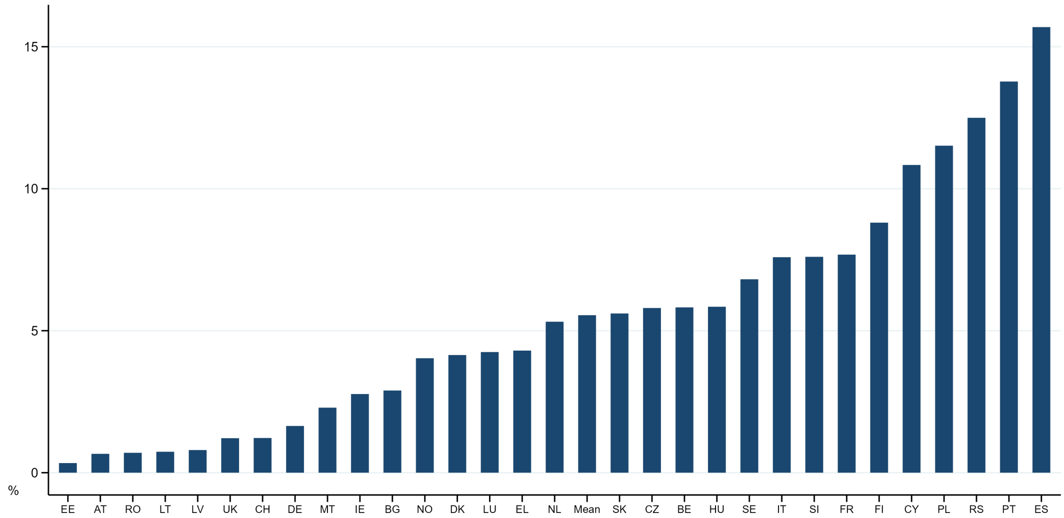
Hence, temporary employees can create an additional source of slack in labor markets, which necessitates examining potential competition between temporary and permanent workers that occur *over the time dimension.* At the same time, fluctuations in temporary employment also affect aggregate wages by changing the share of workers who incur a wage penalty due to their temporary contract. For instance, temporary workers are typically the first to be laid off during a recession, which mechanically increases average wages through a pure *composition effect.* To adjust for a changing composition of employment, we use worker-level data to construct an adjusted wage growth variable that allows us to focus on competition effects only.

Building on the insights of a relatively recent strand of literature (Bellani and Bosio, 2019; Damiani et al., 2018), we thus explore how labor market dualities have affected wage growth in Europe since 2004. We do so by investigating the impact of involuntary temporary employment on wage growth in a wage Phillips curve model and by exploring the role of institutions. We then examine whether involuntary temporary employment helps to understand the Phillips curve flattening in the recovery period after the 2008 Global Financial Crisis (GFC).

4.2 Labor market dualization and the competition effect

Involuntary temporary employment is pervasive in European economies (ILO, 2016) with an increasing share of workers experiencing temporary employment (Latner, 2022). Driving this trend of labor market dualization are structural changes, such as the service sector's growth (Marx, 2011) and deregulation policies (Polavieja, 2006; Emmenegger et al., 2012; Thelen, 2014; Biegert, 2014). Shares vary across countries (Figure 4.1), averaging 5.5% in 2017 compared to a 7.4% average unemployment rate. Does its prevalence impact wage-setting?

Fig. 4.1: Involuntary temporary employment in Europe, 2017



Note: Mean is an unweighted average of all countries shown. Involuntary temporary workers are shown as a share of the labor force aged 15 to 74.
Source: Eurostat/EU-LFS: lfsa_etgar and lfsa_agan.

4.2.1 The relationship between temporary and permanent workers

We introduce the *competition effect* to understand the relationship between temporary and permanent workers and investigate its macroeconomic consequences. Drawing on industrial relations scholarship, we hypothesize that the presence of involuntary temporary workers restrains wage growth due to elevated job insecurity for permanent workers, which weakens their bargaining position. Indeed, temporary employment contributes to a rise in perceived job insecurity (Kuroki, 2012, p. 564), which has been suggested to explain wage restraint (Katz et al., 1999). Our hypothesis is supported by Damiani et al. (2018), who show that reductions in employment protection for temporary workers can reduce overall wage shares. Bellani and Bosio (2019) find that, at the occupational level, wages of permanent employees are negatively affected by the incidence of overall temporary employees (i.e., voluntary and involuntary). Ramskogler (2021) indicates that overall temporary employment has a negative effect on aggregate (unadjusted) wage growth in Europe.

Exacerbating the *competition effect*, employers may foster discord between workers to prevent the emergence of a unified labor bloc (Bellani and Bosio, 2019). For instance, temporary agency workers are used to mitigate wage pressures (Houseman et al., 2003; Drenik et al., 2023), and reforms in temporary employment have worsened conditions for permanent employees (Dolado et al., 2002), in particular for those with lower and middle incomes (Weisstanner, 2020). Empirically, evidence of competition between permanent and temporary employees has been found (Voinea, 2018). In line with earlier work (Piore, 1979; Western and Healy, 1999), we suggest that different segments of workers and their interaction with labor market institutions affect the wage-setting process over and above standard macroeconomic factors.

The competition between temporary and permanent workers could invert the established insider-outsider logic (Lindbeck and Snower, 1988, 2002), which in its early work on dual labor markets challenged human capital theory (Doeringer and Piore, 1971; Rosen, 1972; Reich et al., 1973; Piore, 1983; Dickens and Lang, 1985). Applied to temporary work contracts, the insider-outsider theory suggests that larger hiring and firing transaction costs for insiders create two labor market segments. Insiders enjoy relatively higher economic security than outsiders and can extract rents to the detriment of outsiders by securing higher wages. This results in the wage penalty that is well established: under equal conditions, temporary workers receive smaller paychecks than permanent workers (Kahn, 2016; Pavlopoulos, 2013). Employers have the incentive to replace permanent employees with temporary ones if the transaction costs associated with hiring and firing permanent employees are lower than the wage penalty (Koutentakis, 2008).

As such, the wage penalty can obscure the empirical analysis of the *competition effect*. Changes in the share of temporary workers who suffer from the wage penalty mechanically affect aggregate wages. This results in a *composition effect* on wage growth, which we correct for.

4.2.2 The role of labor market institutions

The competition effect hypothesized interacts with labor institutions. Olson (1971) proposed that significant but non-encompassing collective interests are detrimental at the societal level. The implications for wage determination are widely discussed (Calmfors et al., 1988; Soskice, 1990). In the insider-outsider model, it is easier for insiders to protect their rents at moderate levels of worker organization. At high levels, there are fewer outsiders to bear externalization costs, while at low levels, insiders face stronger competitive pressures.

The simplest measure of the inclusiveness of trade unions is their membership density (Lange, 1984). According to Olson's theory, we expect a negligible competition effect in countries with moderately encompassing membership. We expect a large effect in countries with low membership, as insiders lack sufficient power to protect themselves. In contrast, countries with encompassing membership are expected to experience no competition effect, as outsider interests are likely to be internalized.[1] Only the Nordics, Belgium, Malta, and Cyprus still have high membership rates above 40% of the labor force (Appendix Figure C.1.1). Determining the cut-off between low and medium trade union density (TUD) is less obvious. The distribution of membership rates suggests a cut-off at 20%, which we follow but test for robustness using different cut-off values.

[1] We demonstrate in the empirical analysis that the competition effect is stronger in countries with weak institutions and that this relationship does not result from weak trade unions causing higher shares of temporary employment.

While TUD seems a suitable measure to capture how encompassing unions are, we also use collective bargaining coverage (CBC) for robustness. We assign countries again into three groups: The high group with above 85% coverage comprises mainly countries with an automatic extension of CBC including France, Spain, Belgium, Austria, Finland, and Sweden, while the low group with less than 35% coverage comprises exclusively of Central Eastern European countries and the UK.

Another measure of robustness is wage bargaining coordination (OECD and AIAS, 2021), which describes the vertical and horizontal relations between bargaining units, such as national confederations, sectoral associations, and at the firm-level employers and worker representatives (Traxler and Brandl, 2012). The high group comprises countries with established norms about wage bargaining, predominantly Nordic and Continental European countries. The middle group includes countries that rely on procedural guidelines for coordination with no regularized patterns, such as Spain, Ireland, and Switzerland. The lowest group again contains traditionally Central Eastern European countries, the UK, and some Mediterranean countries.

We also analyze the impact of employment protection legislation (EPL), which refers to the level of protection provided to permanent workers against individual and collective dismissals. We expect the effect of EPL to be ambiguous: Stricter employment protection may enhance workers' bargaining power but also incentivize hiring temporary workers to avoid dismissal costs, thus intensifying competition between temporary and permanent workers. Furthermore, workers subject to more stringent EPL may find themselves in a *golden cage*, where they have more to lose if they were to be replaced with temporary employees and, thus, accept lower starting wages (Lazear, 1990). We categorize countries into high and low EPL groups. Appendix Table C.5.2 displays the descriptive statistics and country groupings.

4.2.3 The post-crisis Phillips curve flattening

After the GFC, wage growth fell short of expectations based on the established relationship between unemployment and wages (Kahn, 1980; Blanchflower and Oswald, 1994). Early signs of a weakened relationship between wages and inflation emerged around 2012 (Anderton and Boele, 2015; Ciccarelli and Osbat, 2017; Moretti et al., 2019). To explain the wage growth slowdown, conventional measures of labor market slack were adjusted for hidden slack, including discouraged workers and involuntary working time reductions, which had increased during the recession (Hurley and Partini, 2017; IMF, 2017; Hong et al., 2018; Nickel et al., 2019). Bell and Blanchflower (2019) construct a labor under-utilization index, which enhances the fit of the Phillips curve for Europe (Bell and Blanchflower, 2021) and the U.S. (Blanchflower et al., 2022).

Involuntary temporary employment constitutes a specific form of labor under-utilization, commonly used by firms to absorb labor market fluctuations (Draeger and Marx, 2017; Hijzen et al., 2017). Temporary workers have lower job stability (Hirsch, 2016; Autor and Houseman, 2010; Gebel and Giesecke, 2011), which suggests that they are more likely to be laid off during unfavorable economic conditions (Costain et al., 2010). Consequently, temporary employment declines faster than permanent employment during economic downturns. An increase in temporary jobs does not provide the same employment opportunities for job seekers and may result in elevated uncertainty among permanent workers as no functional equivalent jobs, and thus outside options for job switchers, open up. If newly created jobs predominantly offer temporary contracts, the reduction in the unemployment rate may thus have a smaller effect on wage growth.

Given the negative correlation between temporary employment and unemployment, fluctuations in temporary employment may balance out the effect of unemployment changes (Appendix Figure C.1.2). Notably, the incidence of involuntary temporary employment reached a historical peak during the 2013-2017 recovery. This warrants investigating whether the high prevalence of temporary workers has contributed to the Phillips curve flattening. Involuntary temporary workers, desiring permanent contracts, represent part of the labor supply for permanent employment and constitute a form of hidden slack. Therefore, any job created based on temporary contracts should reduce the impact of unemployment rate reductions on wage growth, resulting in a flatter Phillips curve.

4.3 The empirical approach

Macroeconomic research as discussed in our theory section has relied primarily on country-level data. As available aggregated wage data do not distinguish between permanent and temporary employees, macroeconomic approaches were unable to disentangle competition from composition effects. The distinction is fundamental since variation in our main independent variable – the incidence of involuntary temporary employment – mechanically affects wages as an inherent component of the employment composition. Hence, an observed negative relationship between temporary workers and wage growth may be the result of changes in the composition of workers with different wage levels. We therefore correct for this composition effect to identify the *competition effect*. In contrast to macroeconomic approaches, industrial relations research has distinguished between wages of temporary and permanent employees by using individual-level data to investigate heterogeneous effects on employment and wages at the meso- or micro-level. We contribute by bringing both strands together. This allows us to account for changes in the share of temporary employees to assess whether competition effects influence macroeconomic outcomes.

More specifically, we rely on worker-level data to construct a country-year panel for wage growth of only permanent employees. If a competition effect exists, temporary workers have a negative impact on the wage growth of permanent employees. As permanent workers make up around 90% of Europe's labor force, wage growth of permanent employees is very likely to be close to overall wage developments. Nevertheless, the use of worker-level data allows us to construct a wage growth series for *all* employees in a country, while netting out potential composition effects. Hence, we also estimate the sensitivity of *overall* aggregate wage growth with respect to the prevalence of temporary work in Europe.

4.3.1 Adjusting wage growth for a changing employment composition

To construct our dependent variable, wage growth, we rely on EU-SILC, a representative population survey containing the longest-running cross-national dataset available with annual information on employment and wages.[2] It allows us to distinguish employees on temporary contracts from permanent ones, which is crucial for our research question. Although the primary focus of EU-SILC lies in collecting representative data on income rather than on the labor market status, the share of temporary employees in total employees in EU-SILC (11.7%) is quite comparable to the respective figure in the Labor Force Survey (13.9%). [3] We discuss the data and aggregation of country-level time series in Appendix C.2. To confirm the validity of

[2]The EU-LFS does not contain the wage level. The EU-SES is only conducted every four years.

[3]The reported figures represent the weighted average of the share of temporary employees in total employees of all European countries in our sample in the observation period 2004-2017. Note also that the share of temporary employment according to EU-SILC seems to follow a quite similar pattern over time compared to the respective series from the EU-LFS. This is reflected by a relatively high correlation coefficient of 0.88 between both series.

our aggregation, we compare our time series to Eurostat's officially published EU-SILC country-level data (Appendix Figures C.2.1-C.2.2) and to the OECD's time series on wages based on national accounts as well as on survey and admin data (Appendix Figure C.2.3).

In addition to the *unadjusted* aggregate wage growth variable of all employees, we calculate the wage growth of permanent workers based on the information about employees' contract type. Figure 4.2 illustrates wage dynamics in Europe for both contract groups separately. What stands out immediately is that wage growth has slowed down since the onset of the GFC, a stylized fact discussed in our theory section. Interestingly, this applies to both groups but seems to be more pronounced in the group of temporary workers. It might be related to the strong *relative* demand for temporary employees before the onset of the crisis (Appendix Figure C.1.2), which could have accelerated wage growth for temporary workers compared to permanent workers. Likewise, the weakened relative demand during 2008-2014 might explain the observed slower wage growth of temporary workers, while the economic recovery gaining traction from 2015 has fuelled demand for temporary labor, thereby lifting their wages.

Fig. 4.2: Wage growth permanent and temporary workers

Note: The average annual change in nominal wages is shown as a weighted average for European countries.
Source: EU-SILC.

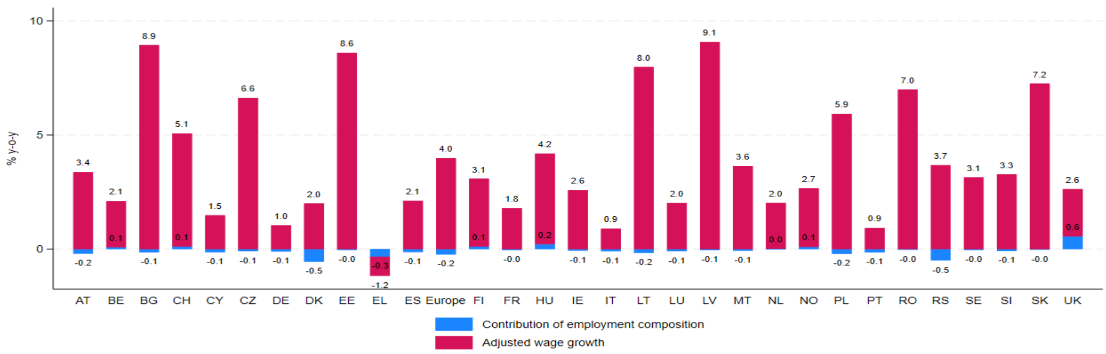
To obtain the wage growth of a pseudo-workforce with a constant employment composition over time, we employ *inverse probability weighting (IPW)* (Rosenbaum and Rubin, 1983; DiNardo et al., 1996; Fortin et al., 2011). First, we use a logit model to predict the probability of each observation of being in temporary employment per year and country, pairing the base year t (2004 or earliest available) with each of the following years $t+n$: $\ln \frac{p}{1-p} = \beta_0 + \sum_{t=1}^{m} \beta_t x_t$ where x_t is employment contract that we control for.[4] We estimate the re-weighting factors for each year and country separately. Second, we adjust the weights for each observation so that the re-weighted sample has the employment composition with regard to the first year available. For the base year, we keep the original weight $g_1 = g$, whereas for control individuals, we use the predicted probability $p(x)$ to receive the adjusted survey weights $g_{1+n} = g\frac{p(x)}{1-p(x)}$. Finally, we aggregate the worker-level data at the country-year level to obtain our adjusted measure for aggregate wage growth that is based on a counterfactual employment composition constant over time with regard to employment contracts.

Figure 4.3 presents the adjusted wage growth variable for each country (averaged over the whole sampling period) and the employment composition effect. The latter purely represents a mechanical effect from the changing share of temporary workers over time. Since temporary workers suffer a wage penalty compared to permanent workers, an increasing share of temporary workers lowers the aggregate average wage given a constant penalty. Adjusted wage growth represents the counterfactual rate of wage growth if the share of temporary workers would have remained constant over time. The size of the composition effect is very heterogeneous at the country-level and sizeable in some countries, in particular Denmark, Serbia and the UK. However, interestingly, it does not play a large role for Europe as a whole. Some countries are characterized by substantial wage differences between temporary and permanent

[4]To adjust for changes in employment shares based on other observable characteristics, we repeat the same procedure using gender, migration background, educational attainment, and work experience as additional controls in the logit model (Figure B.1).

workers and have experienced a strong increase in temporary work. However, even in those cases, temporary workers as a share of all employees have only changed by a few percentage points over several years, resulting in a minor impact of employment composition changes on wages. For robustness, we adjust wage growth additionally for employment composition changes by gender, migration background, educational attainment, and work experience, which warrants slightly larger effects (Appendix Figure C.3.1).

Fig. 4.3: Wage growth 2004-2017 divided in contribution of employment composition and adjusted wage growth



Note: Wage growth is adjusted for a changing employment composition by contract. Wage growth refers to the average annual change in nominal wages 2003-2017. Europe refers to the simple average of all countries shown.
Source: Authors' computations based on EU-SILC.

Although the difference between adjusted and unadjusted wage growth is quite small overall, it must be stressed that only by adjusting can we identify the underlying mechanism that impacts wage growth. Without the adjustment for employment composition, we would not know whether composition or competition is driving our results.

4.3.2 Estimating factors of wage growth

The most widely used empirical model to study the determinants of wage growth is the wage Phillips curve. The traditional wage Phillips curve relates nominal wage growth to labor market slack. Additional determinants typically considered are (expected) inflation and labor productivity growth (Nickel et al., 2019). We use such an augmented Phillips curve model to study the impact of dualization on nominal wage growth in Europe. We estimate a standard reduced form equation in a panel data framework of the form:

$$\dot{W}_{i,t} = \alpha_1 + \alpha_2 \text{U}_{i,t} + \alpha_3 \text{Prod.}_{i,t} + \alpha_4 \text{Infl.}_{i,t} + \alpha_5 \text{Invol. Temp}_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \quad (4.1)$$

As outlined in the section above, our dependent variable is nominal wage growth obtained from EU-SILC. As a benchmark, we first study the dynamics of the unadjusted aggregate wage growth to represent the workhorse Phillips curve model. In the second step, we analyze the nominal wage growth of permanent workers only, and we finally implement our main dependent variable, which is nominal wage growth net of composition effects ($\dot{W}_{i,t}$). While most studies estimating wage Phillips curves use quarterly data[5], we have to stick to an annual frequency (as in the original contribution by Phillips (1958) or more recently by Kiss and Van Herck (2019)) as the computation of our dependent variable is only feasible based on yearly data. Our sample includes 30 European countries (i) and ranges from $t = 2004, ..., 2017$, which leaves us with roughly 340 observations.[6] We intentionally choose a static representation as we do not observe any persistence in wage dynamics (likely due to the annual frequency of our sample). Moreover, as the time-invariant country effects (μ_i) are correlated with the regression variables, we employ the fixed-effects estimator (FE), where unobservable country effects are assumed to be fixed (and

[5]Examples are Bonam et al. (2021), Nickel et al. (2019) and Bulligan and Viviano (2017)

[6]Data for some countries are only available after 2004. We drop 19 observations when there was a break in the time series of wages according to Eurostat (due to a change of source or survey methodology).

not random). We compute standard errors clustered at the country-level in all specifications to control for potential serial correlation in the error term within each country.

As a baseline, we use the conventional labor market slack indicator, which is the headline unemployment rate $\mathrm{U}_{i,t}$, but we also consider several other measures of slack for robustness. Further, we control for the impact of labor productivity ($\mathrm{Prod}_{i,t}$) on wages, which we measure as the growth rate of real output per employment, as well as for inflation ($\pi_{i,t}$). Studies using quarterly wage growth data often employ (one quarter) lagged inflation implying backward-looking expectations (Ramskogler, 2021; Nickel et al., 2019; IMF, 2017). Given the annual frequency of our data, we assume a contemporaneous effect from inflation (measured as the annual change in the harmonized index of consumer prices) on nominal wage growth.[7]

Finally, and most importantly, we add to our Phillips curve specification a variable to identify the competition effect. So far, studies exploring the impact of dualized labor markets on wages have considered *overall* temporary employment (Ramskogler, 2021; Bellani and Bosio, 2019). However, the limitation is that not all temporary workers look for a permanent contract. We identify the competition effect, by focusing on *involuntary* temporary employees as a share of the active working-age population (Invol. $\mathrm{Temp}_{i,t}$). This segment of disadvantaged workers prefers a permanent contract over their temporary one, which we expect to cause the competition effect. A detailed description of the measurement of all variables and their sources is included in Appendix Table C.5.1.

[7]We also consider a survey-based measure capturing forward-looking inflation expectations provided by the European Commission (expected inflation). As this variable is not available for two of our countries (Switzerland and Norway) and does not improve the explanatory power, we stick to realized consumer price inflation.

4.4 Results

In the first part of this section, we present estimation results concerning the identification of the competition effect and its macroeconomic consequences (Tables 4.1 and 4.2). In the second part, we test whether the magnitude of the competition effect depends on labor market institutions (Table 4.3). Finally, we explore whether the rise in involuntary temporary employment can explain the flattening of the Phillips curve in Europe during the post-GFC recovery period (Table 4.4).

4.4.1 Identifying the competition effect at the macroeconomic level

We present the workhorse Phillips curve specification augmented by involuntary temporary employment in column (1) of Table 4.1. The coefficient estimates have the expected signs and are statistically significant. Labor productivity has a positive effect on wages. We also observe that inflation drives up wages with a regression coefficient of around 1.[8] By contrast and as expected, an increase in the unemployment rate reduces nominal wage growth. Yet, $Invol.\,Temp_t$ – our variable of main interest – is also negatively associated with nominal wage growth and is statistically significant. A rise in the share of involuntary temporary employees by 1 percentage point leads to a decrease in nominal wage growth by almost 1 percentage point. As we have considered the *unadjusted* growth rate of wages so far, the coefficient estimate captures both potential composition *and* competition effects. However, before we alter the dependent variable to isolate the competition effect, we include time dummies in our model to control for common shocks that might have affected wage

[8]This finding likely is linked to the annual context of our estimations (Kiss and Van Herck, 2019). While it is not very common in the literature to use quarterly data, Rusinova et al. (2015) show that if four lags of inflation are considered in quarterly estimations, the aggregate effect again accumulates to close to 0.9.

dynamics equally across countries over time, such as the GFC. In fact, a test of joint significance shows that the time dummies have high explanatory power. Their inclusion also reduces coefficient estimates of all variables except $Invol.\,Temp_t$, as we can see in column (2). This is particularly true for inflation, which becomes statistically non-significant.[9] As time dummies are significant in all the following model specifications, we included them to avoid biased estimates (Baltagi, 2005).

Table 4.1: Identifying the competition effect

	all workers, unadjusted		permanent contract workers		
Dep. var.: wage growth	work-horse PC (1)	incl. time dummies (2)	competition effect (3)	excl. Temp (4)	incl. vol. Temp (5)
$Prod_t$	0.57***	0.33**	0.34**	0.33**	0.34**
	(3.43)	(2.21)	(2.46)	(2.24)	(2.46)
$Infl_t$	0.90***	0.55	0.49	0.54	0.46
	(3.00)	(1.18)	(1.06)	(1.20)	(0.97)
U_t	-0.66***	-0.51***	-0.49**	-0.44**	-0.51**
	(-3.91)	(-2.90)	(-2.64)	(-2.09)	(-2.61)
$Invol.\,Temp_t$	-0.96**	-0.92***	-0.98***		-1.04***
	(-2.66)	(-2.82)	(-3.21)		(-3.17)
$Vol.\,Temp_t$					-0.34
					(-1.18)
Cons	11.88***	11.19***	11.35***	5.56**	13.28***
	(5.31)	(3.82)	(3.91)	(2.58)	(3.13)
Model	FE	FE	FE	FE	FE
TimeD	excl.	incl.	incl.	incl.	incl.
N	344	344	344	344	344

Two-tailed significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T-statistics are reported in parenthesis and are based on cluster-robust standard errors by country. Explanatory variables include labor productivity growth ($Prod_t$), inflation ($Infl_t$), unemployment (U_t), involuntary ($Invol.\,Temp_t$.) and voluntary temporary employment ($Vol.\,Temp_t$).

We now alter our dependent variable in column (3) by considering the nominal wage growth of employees with permanent contracts only. This allows us to estimate the competition effect, as we isolate the part of wage growth that cannot be affected

[9]Obviously, price dynamics across countries have followed a very similar pattern over time. Certainly, a common driver of inflation across countries could be oil, which is known to significantly affect consumer price dynamics.

by changes in relative weights between temporary and permanent workers. Compared to column (2), all coefficient estimates remain broadly the same. This result has two main implications. First, it strongly supports our thesis that the incidence of a dualized labor market has negative spillover effects on the dynamics of wages of employees with permanent contracts. This is consistent with Bellani and Bosio (2019) who find that the density of temporary contracts within occupation- and age-specific groups negatively affects average wages for permanent workers belonging to the same group. In addition to their findings, our results show that competition effects are also relevant in a macroeconomic context, where other important wage growth determinants like the unemployment rate are accounted for. The second important implication is that *composition* effects seem to be negligible in Europe in the period 2004-2017. A first indication of these rather small composition effects is the relatively low contribution of employment composition to adjusted wage growth across countries presented in Figure 4.3. In column (4) we show that the sensitivity of wage growth with respect to the unemployment rate decreases when the Phillips curve is specified without controlling for temporary employment.

Unlike in previous studies (Ramskogler, 2021; Bellani and Bosio, 2019), our empirical setting allows us to focus on *involuntary* (rather than on *overall*) temporary employment to measure the degree of labor market dualization. To reveal whether this is indeed the relevant measure in our context, we add the share of *voluntary* temporary employees and report the results in column (5). Comparing the coefficients of both indicators reveals *involuntary* temporary employees drive wage growth of permanent workers, while the impact of workers, who have voluntarily chosen to have a temporary contract ($Vol.\ Temp_t$) is non-significant. Hence, when measuring dualization, it is crucial to quantify those employees who would prefer to be employed on a permanent basis. Note also that the magnitude of the coefficient estimate (and its statistical significance) would drop substantially if we considered overall temporary employment (instead of $Invol.\ Temp_t$). To proxy labor market dualization, our results, thus, strongly suggest that – whenever feasible – *involuntary* rather than *overall* temporary employment should be considered.

To investigate the impact of involuntary temporary employment on overall aggregate wage growth, we re-estimate specification (2) by employing *adjusted* wage growth, i.e., wage growth net of composition effects. The results are depicted in column (1) of Table 4.2 and show almost unchanged coefficient estimates (compared to model (2) in Table 4.1). This is consistent with our previous observation, namely that composition effects are empirically only of minor importance. Further, our results resemble Ramskogler (2021), who finds a significant negative effect from temporary employment on *unadjusted* wage growth in Europe. Additionally, we confirm that the underlying mechanism behind the observed negative relationship arises from a *competition* rather than a *composition* effect.[10]

Before turning to the issue of reverse causality, we provide two interesting extensions of this result. First, we assess the *economic* significance of dualized labor markets by re-estimating specification (1) based on standardized variables. As reported in column (2), involuntary temporary employment turns out to be the most relevant determinant for wage growth followed by unemployment.[11] However, taking into account the uncertainty surrounding the parameter estimates, both variables are equally meaningful in explaining nominal wage growth.[12] Hence, involuntary temporary employment has been at least as important as unemployment in shaping nominal wage dynamics in Europe.

While in specification (1) we control for composition effects with respect to the

[10] As a robustness check, we exclude one country at a time from the sample to show that the result does not depend on one particular country (Appendix Table C.4.1).

[11] An increase in the rate of involuntary temporary employment by one standard deviation leads to a drop in nominal (composition adjusted) wage growth by half a standard deviation.

[12] A test on parameter equality is not rejected.

Table 4.2: The impact of the competition effect on *adjusted* wage growth

Dep. var.: adjusted wage growth	adjusted by contract (1)	standardized coefficients (2)	adjusted by all controls (3)	reverse causality (4)	(5)
$Prod_t$	0.33**	0.14**	0.29	0.35**	0.35***
	(2.26)	(2.26)	(1.58)	(2.51)	(2.61)
$Infl_t$	0.54	0.16	0.53	0.47	0.44
	(1.16)	(1.16)	(1.13)	(1.08)	(0.85)
U_t	-0.50***	-0.33***	-0.58***	-0.57***	-0.65***
	(-2.78)	(-2.78)	(-3.47)	(-3.59)	(-4.18)
$Invol.\ Temp_t$	-0.95***	-0.54***	-0.89**	-1.67**	-1.87**
	(-2.92)	(-2.92)	(-2.74)	(-2.06)	(-2.29)
Cons	11.34***	-0.01	11.68***	15.88***	17.73***
	(3.91)	(-0.09)	(3.94)	(3.24)	(3.59)
Model	FE	FE	FE	GMM	GMM
Ar1				-2.81	-2.78
Ar2				-0.72	-0.71
Hansen				12.83	14.74
Hansen p-val				0.80	0.97
TimeD	incl.	incl.	incl.	incl.	incl.
N	344	344	343	344	344

Two-tailed significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T-statistics are reported in parenthesis and are based on cluster-robust standard errors by country. Explanatory variables include labor productivity growth ($Prod_t$), inflation ($Infl_t$), unemployment (U_t) and involuntary temporary employment ($Invol.\ Temp_t$.). Specification (3) includes wage growth adjusted by contract type, gender, migration, education, and work experience. Specifications (4) and (5) are estimated by first difference GMM (using orthogonal deviations). We use the Stata command xtabond2 and employ the second level lag (up to 11 lags) of the endogenous variables as instruments. As the cross-section dimension is rather small (i.e., 30 countries), we use standard IV instruments rather than GMM-type instruments to limit the instrument count (by using the collapse option (Roodman, 2009)). Specification (4) treats only U_t and $Invol.\ Temp_t$ as endogenous, while specification (5) assumes that all variables are endogenous except $Prod_t$.

type of employment contract, other possible aspects of composition could affect wage growth as well. Some demographic groups may have more bargaining power (e.g., prime-age native-born males) or a higher marginal productivity than others. The composition of who is selected into temporary jobs may change over the business cycle. Hence, in addition to contract type, we adjust wage growth for changes in the employment composition by gender, migration background, educational attainment, and work experience. As highlighted in column (3), this new measure does not alter the observed results with respect to the competition effect. However, the impact of productivity on wage growth becomes non-significant. This is most probably because productivity is largely captured at the worker-level by having netted out changes in the share of skilled and experienced workers.

We now turn to the issue of a potential simultaneity bias arising from reverse causality between nominal wage growth and labor market slack. Usually, reverse causality is approached by inserting the slack variable in its one-period lagged form into the Phillips curve model (Ramskogler, 2021; Byrne and Zekaite, 2020; Nickel et al., 2019). While this is certainly a valid approach when using quarterly data, it is not feasible in our case given the annual frequency of the data.[13] Fortunately, in the case of reverse causality, the fixed-effect estimate of the impact of unemployment on wage growth would be downward biased rather than upward, as higher wage growth should cause higher labor market slack (IMF, 2017; Wooldridge, 2009). The same logic applies to the dualization measure. If wage growth accelerates, it is presumable that employers increasingly demand temporary employees as they are cheaper and associated with lower firing costs. Hence, our findings concerning the importance of temporary employment for wage growth are likely not mistaken even in the presence of reverse causality.

An alternative approach to account for a potential simultaneity bias is to use instrumental variable techniques. As exogenous instruments are not at hand, neither for unemployment nor for temporary employment, we use internal instruments, i.e., time lags of the variables in the model. In particular, we employ the difference GMM estimator[14] (Arellano and Bond, 1991; Blundell and Bond, 1998) and treat both variables as endogenous (by using the lagged levels of the variables as instruments). As displayed in column (4), involuntary temporary employment and unemployment have the expected negative signs and are statistically significant. However, compared

[13]Although lagging unemployment is generally effective in accommodating the, for most periods, relatively minor shifts in unemployment that occur, it is contemporaneous unemployment that affects wage growth (Rusinova et al., 2015). This is because wage demands are negotiated under the constraints of the prevailing bargaining power, despite wage demands in Europe typically being formulated based on the purchasing power losses of the past year.

[14]Our dependent variable is not persistent. For this reason, we choose a static representation and the difference rather than the system GMM estimator.

to the fixed effect estimation in column (1), we observe an increase in the coefficient estimate for both variables. Obviously, controlling for simultaneity has an effect on the estimates in the direction that we expected. A very similar result can be found in Bellani and Bosio (2019). Finally, in column (5) we add inflation to the set of endogenous variables and show that this alteration does not have any significant influence on the estimation outcome.

4.4.2 The effect of institutions

As highlighted in the theory section, the magnitude of the competition effect may depend on a country's labor market institutions. We take account of these considerations in Table 4.3, where we interact involuntary temporary employment with different institutional variables. We consider the wage growth of permanent workers as the dependent variable in all specifications. In addition to interaction effects, we control for the direct effect that institutions might exert on wage growth. This rules out a potential omitted variable bias that might arise if weak trade unions were to cause higher shares of temporary employment due to their inability to prevent the substitution of good jobs with bad jobs.

Following our country grouping based on trade union density, we report group-specific differences in the competition effect in column (1). As we anticipated, the competition effect is significant and large only in countries with low trade union density. Moreover, we can conclude that the competition effect does not arise because weak trade unions cause higher shares of involuntary temporary employees, as we have controlled for the direct impact of institutions. Rather, the observed effect in countries with weak institutions results from the fact that permanent employees cannot use union power to protect themselves from negative wage pressures caused by temporary employment. Trade union density (TUD_t) affects wage growth positively, as expected (Kahn, 1979; Stansbury and Summers, 2020).

Analogously, our findings are confirmed when investigating different proxies for how wage bargaining encompasses workers. These proxies include collective bargaining coverage (CBC) as in specification (2) or wage bargaining coordination (Coord) as in specification (3). In all these cases, the competition effect is most pronounced at the lowest institutional level.[15] Our results, thus, support the hypothesis that competition effects from labor market dualization heavily depend on domestic labor market institutions.

Further, we stress that the competition effect is higher in countries with more stringent employment protection legislation (EPL) as shown in specification (4). This likely is the result of the *golden cage effect*: Permanent workers in countries with more stringent EPL, have more to lose if they were to be laid off. The monetary value of a job can be understood as a function of the wage received per period and the probability of future job loss, which amounts to the discounted income stream expected. In this logic, workers trade job security, corresponding to the likelihood of wage receipt in the future, against higher wages in the present. More stringent EPL increases the chance of continued employment and thus can correspond to lower starting wages (Lazear, 1990).

In the last specification of Table 4.3, we want to highlight that the competition effect, although it varies in magnitude across country groups, is significant in determining *weighted* aggregate wage growth in Europe. In fact, the competition effect remains highly significant and increases in magnitude when we put more weight on countries that are larger, as we demonstrate in column (5).[16] This observation is

[15]Moreover, we find a negative impact from CBC and Coord on wage growth, which corresponds to common expectations (Soskice, 1990; Hancké and Soskice, 2003). This happens as higher levels of centralization and coordination help to prevent wages from taking an inflationary turn (i.e., internalizing negative externalities). Coordination is expected to be the most relevant factor (Soskice, 1990), which is confirmed by our findings.

[16]The relative weight of each country is based on the number of employed persons in 2005.

Table 4.3: The competition effect and the role of institutions (Inst)

Dep. var.: wage growth of perm. workers	Inst: TUD (1)	Inst: CBC (2)	Inst: Coord. (3)	Inst: EPL (4)	weighted sample (5)
$Prod_t$	0.17	0.29	0.28*	0.04	0.04
	(0.89)	(1.46)	(1.83)	(0.25)	(0.15)
$Infl_t$	0.59	0.44	0.50	-0.20	-0.25
	(1.53)	(0.98)	(1.17)	(-0.55)	(-0.38)
U_t	-0.60**	-0.61***	-0.58***	-0.31**	-0.45**
	(-2.44)	(-3.17)	(-3.06)	(-2.09)	(-2.08)
$Invol.\, Temp_t$					-1.13***
					(-3.39)
...low Inst	-1.40***	-1.39**	-1.47***		
	(-4.89)	(-2.44)	(-3.64)		
...med. Inst	0.36	-0.20	-0.76		
	(0.32)	(-0.50)	(-1.25)		
...high Inst	-0.11	-0.81	-0.29		
	(-0.35)	(-1.40)	(-1.05)		
$Invol.\, Temp_t$					
...low EPL				-0.74**	
				(-2.52)	
...high EPL				-1.15**	
				(-2.11)	
TUD_t	0.43***				
	(3.49)				
CBC_t		-0.09*			
		(-1.95)			
$Coord_t$			-1.24*		
			(-1.86)		
EPL_t				-0.75	
				(-0.53)	
Cons	-2.33	15.64***	14.80***	12.99***	14.33***
	(-0.55)	(4.54)	(3.67)	(3.50)	(3.91)
Model	FE	FE	FE	FE	FE
TimeD	incl.	incl.	incl.	incl.	incl.
N	300	302	344	278	344

Two-tailed significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T-statistics are reported in parenthesis and are based on cluster-robust standard errors by country. Explanatory variables include labor productivity growth ($Prod_t$), inflation ($Infl_t$), unemployment (U_t), involuntary temporary employment ($Invol.\, Temp_t$.), employment protection legislation (EPL_t), trade union density (TUD_t), collective bargaining coverage (CBC_t), and coordination of wage setting ($Coord_t$). As the CBC time series has a lot of gaps, we impute missing values with lagged available values. Column (5) represents estimates from a weighted regression. The relative weight of each country is based on its number of employed persons in 2005.

consistent with the fact that low TUD countries expose a particularly strong competition effect, as the countries belonging to this group (comprising 14 countries) make up more than 75% of overall employment in Europe and drive the aggregate weighted effect.

Finally, empirically analyzing differences across country groups always involves choosing the "right" threshold that divides countries into the respective groups. In the case of TUD and Coord, a disproportionately high share of countries are clustered in the first group (i.e., countries with the weakest institutions) as rationalized in the theory section. This categorization might work against finding a statistically significant effect for the medium and high groups. As a robustness check, we consider an alternative clustering procedure that groups the countries more evenly by using terciles of the institutional variables. We report our results in Appendix Table C.4.2. In columns (1) to (4), we show that our results hold when the number of countries does not vary across groups.

In column (5) of Appendix Table C.4.2, we base our country grouping on a joint set of institutional variables to form three equally sized clusters. Concretely, we perform a principal component analysis (PCA) to understand the correlation of the institutional variables, as the PCA reduces their multi-dimensional character by identifying their common grounds (components). We consider the first resulting component, which explains 75% of the overall variation of TUD, Coord and CBC.[17] Using terciles of the constructed index to form the grouping again confirms our main finding. The magnitude of the competition effect decreases with the strength of institutions. Lastly, in column (6) we employ an index for union strength developed by Metten (2021) who uses a more sophisticated theoretically informed PCA to identify determinants of trade union strength. Employing this index again supports

[17] We have not considered EPL in constructing the index, as EPL does not load the first component of the PCA in the same direction as the remaining three institutional variables. Hence, EPL does not seem to capture the inclusiveness of wage bargaining but other underlying institutional factors. Recall that the competition effect is more pronounced for countries with high EPL.

our hypothesis that the competition effect is large in countries where insiders do not have sufficient power to shelter themselves from competitive pressure.[18]

4.4.3 The Phillips curve flattening

So far, we have shown that competition effects play a statistically significant role in explaining aggregate nominal wage growth and that they interact with the institutional dimension. Does this help to understand the observed flattening of the Phillips curve in Europe after the GFC? To tackle this question, we extend the Phillips curve framework by allowing for a different unemployment parameter after the crisis. This allows us to test the Phillips curve flattening and study possible interaction effects with temporary employment. We employ adjusted wage growth in all specifications to rule out possible composition effects and to obtain results that reflect the wage dynamics of both temporary and permanent employees. To investigate post-crisis differences, we first interact unemployment as well as involuntary temporary employment with a post-crisis dummy that equals 1 for the period 2013-2017 and 0 for the preceding period.[19] Second, following the hidden slack literature, we construct a labor market slack measure by summing up unemployed and involuntary temporary employees ($Slack_t$) to study their joint impact on wage growth before and after the crises.[20]

The corresponding results are summarized in Table 4.4. Column (1) shows a model that allows for a crisis interaction term on the unemployment rate without considering temporary employment. The slope parameter of the unemployment

[18]The index is not available for Malta. Moreover, due to several missing values at the beginning and end of the individual time series, we have not imputed missing values and therefore did not include the index as an additional regressor into the model.

[19]We have tested other thresholds as well. It turned out that the break in slope parameters is most pronounced when the post-crisis period is defined from 2013 onward.

[20]Both Unemp_t and Invol. Temp_t, representing $Slack_t$, are measured as a share of the active working-age population.

rate is statistically different across the two time periods and points to a decreased sensitivity of wage dynamics to unemployment of more than 50% since the post-crisis period. While a decrease in the unemployment rate boosted wage growth by 0.56 percentage points before 2013, this sensitivity declined to 0.18 percentage points[21] in the post-crisis period. Our results, thus, support the empirical findings in the literature that indicate a lower explanatory power of labor market slack measures in the post-crisis period (Byrne and Zekaite, 2020).

[21] The slope parameter of unemployment after 2012 is obtained as follows: -0.56 + 0.38 = -0.18.

Table 4.4: Phillips curve flattening: the role of dualization; Dep. variable: adjusted wage growth

	headline U			broad U & invol. part-time			de-trended labor market var.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Prod_t$	0.34	0.34	0.35	0.36	0.34	0.35	0.29	0.26	0.33*
	(1.53)	(1.66)	(1.67)	(1.56)	(1.58)	(1.58)	(1.50)	(1.46)	(1.77)
$Infl_t$	0.99**	0.98**	0.92**	1.06**	1.04**	0.99**	0.81**	0.70*	0.44
	(2.65)	(2.51)	(2.52)	(2.70)	(2.62)	(2.66)	(2.22)	(1.93)	(1.16)
U_t	-0.56**	-0.58**		-0.44*	-0.50***		-1.04***	-1.06***	
	(-2.17)	(-2.63)		(-2.03)	(-2.79)		(-2.81)	(-3.07)	
$U_t * post\text{-}crisis$	0.38*	0.38**		0.30*	0.26		1.06***	1.05***	
	(1.80)	(2.14)		(1.76)	(1.61)		(2.87)	(3.04)	
$Invol.\ Temp_t$		-0.92**			-0.95**			-1.80***	
		(-2.34)			(-2.29)			(-3.51)	
$Invol.\ Temp_t * post\text{-}crisis$		-0.10			-0.08			1.19	
		(-0.90)			(-0.59)			(1.28)	
$Slack_t$			-0.56**			-0.51**			-2.28***
			(-2.46)			(-2.49)			(-4.08)
$Slack_t * post\text{-}crisis$			0.17			0.14			1.49*
			(1.47)			(1.21)			(1.78)
$Invol.\ Part._t$				-0.08	0.40	0.36			
				(-0.19)	(0.78)	(0.72)			
Cons	2.38	8.55***	6.38**	2.76	8.42***	6.17**	1.39	1.72	1.85
	(1.59)	(3.16)	(2.75)	(1.58)	(3.17)	(2.49)	(1.24)	(1.50)	(1.55)
Model	FE	FE	FE	FE	FE	FE	FE	FE	FE
TimeD	incl.	incl.	incl.	incl.	incl.	incl.	incl.	incl.	incl.
N	285	285	285	285	285	285	285	285	285

Two-tailed significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T-statistics are reported in parenthesis and are based on cluster-robust standard errors by country. Explanatory variables include labor productivity growth ($Prod_t$), inflation ($Infl_t$), unemployment (U_t), involuntary temporary employment ($Invol.\ Temp_t$.), involuntary part-time employment ($Invol.\ Part._t$) and labor market slack ($Slack_t$). $Slack_t$ is measured as the sum of the respective unemployment rate and involuntary temporary employment in models (3) and (6). Models (7) to (9) use the cyclical components of the considered labor market variables, i.e., U_t is based on the NAWRU (OECD) and $Invol.\ Temp_t$ and $Slack_t$ are based on an HP filter, where $Slack_t$ is defined as the sum of the headline unemployment rate and involuntary temporary employment. *Post-crisis* is a dummy variable with values of 1 for the years 2013-2017.

In column (2), we add temporary employment into the model and allow for different slope parameters on this variable as well. Two things stand out. First, the sensitivity of nominal wage growth with respect to involuntary temporary employment remains largely unchanged. Even though we observe an increase in the impact of temporary employment after 2012, it is not statistically significant. Second, adding labor market dualization into the model does not help to understand the flattening of the Phillips curve as the slope parameter of $U_t * \textit{post-crisis}$ remains unchanged. Interestingly though, employing the variable that summarizes unemployment and temporary employment (i.e., $Slack_t$) leads to a different conclusion, as can be seen in column (3). The flattening is still observable since the slope parameter on $Slack_t * \textit{post-crisis}$ is positive, but it is smaller and statistically not significant.

Given the thus far inconclusive results concerning the role of temporary employment for the Phillips curve flattening, we follow the literature on hidden slack by considering a broader measure of the unemployment rate as well as involuntary part-time employment (as another source of potential hidden slack) and re-estimate the first three specifications accordingly. We extend the headline unemployment rate by additionally considering discouraged as well as marginally attached workers (U-5). Moreover, we account for employees who work part-time but do so involuntarily. Note however, that unlike by Bell and Blanchflower (2019), we are not able to account for labor under-utilization based on desired hours of work due to data availability. Instead, we have to rely on headcounts to capture the degree of underemployment. The flattening disappears when adding involuntary temporary employment next to the broader unemployment measure (columns (4) to (6)). The same holds true when investigating overall slack as defined above.[22]

Finally, in the remaining three specifications, we employ the cyclical components

[22]That is, summing up the broader unemployment measure U-5 and involuntary temporary employment.

of our independent labor market variables. In specifications (7) and (8), we rely on the concept of the non-accelerating wage rate of unemployment (NAWRU) and consider the unemployment gap arising between the headline unemployment rate (U-3) and the NAWRU. In model (8), we add the cyclical component of involuntary temporary employment, which we compute by applying an HP filter.[23] For the final model, we use the same filtering technique to de-trend the time series of labor market slack ($Slack_t$). The results of the last three models are quite similar to the ones obtained when considering the narrow definition of the unemployment rate (U-3). Adding labor market dualization does not explain the flattening of the Phillips curve as can be seen from a comparison of columns (7) and (8), but dampens the flattening in specification (9), where unemployment is considered jointly with involuntary temporary employment.

Overall, the presented results concerning the interaction between dual labor markets and the flattening of the Phillips curve are not robust. In our view, though, the findings point to a potential role of involuntary temporary employment in the hidden slack debate. One reason for the inconclusive results might be the relatively short time period of our analysis. Adding more observations might eventually result in more robust findings, especially if temporary employment were to increase further. Moreover, becoming more granular concerning the slack variable (e.g., by considering the variable created by Bell and Blanchflower (2019)) could help to improve estimation efficiency for future research.

[23] To avoid the end-point problem of the HP filter (Orphanides and van Norden, 2002), we consider the most recent data, which are available until 2021 for all countries in our sample. Following Ravn and Uhlig (2002), we set the smoothing parameter to 6.25. As the filtering technique does not allow gaps within the time series, we impute four observations in AT (2004,2005), MT (2004) and ES (2005). However, we do not use these observations for estimating our models.

4.5 Conclusion

In this paper, we have demonstrated that competition between involuntary temporary and permanent workers has suppressed wage growth in Europe. This means that the higher the incidence of temporary workers who are involuntarily on a temporary contract, the lower the growth rate of wages. The effect is clearly present when investigating (i) the rate of wage growth of permanent employees alone and when employing (ii) adjusted aggregate wage growth that nets out potential composition effects caused by fluctuations in the share of temporary employment. Moreover, we have illustrated that involuntary temporary employment has been at least as important as the unemployment rate in shaping wage dynamics in Europe. Hence, the competition effect is not only statistically but also economically significant.

On top, the cross-country nature of our analysis also allowed us to investigate the role of institutions. We have shown that the competition effect is more pronounced when wage bargaining institutions are weak, which is consistent with industrial relations scholarship. Crucially, our findings are robust when we put more weight on larger countries, thus ruling out the possibility that only small countries drive the results.

Finally, we have presented some tentative evidence that the competition effect might help to understand the strange flattening of the Phillips curve in Europe during the post-GFC recovery period. In fact, we have shown that accounting for the incidence of involuntary temporary employees in defining labor market slack explains the flattening of the Phillips curve to some extent. However, our findings in this regard are the least (statistically) significant and thus leave ample room for further research.

Overall, our analysis shows the important macroeconomic consequences of the dualized structure of labor markets. Despite the recent uptick in wage growth, the entrenchment of temporary employment calls for macroeconomic policies that are cognizant of the labor market structure.

Bibliography

Anderton, R. and Boele, B. (2015). Downward wage rigidities in the euro area. Discussion Paper 9, University of Nottingham.

Arellano, M. and Bond, S. (1991). Some tests of specification for panel data: Monte carlo evidence and an application to employment equation. *The Review of Economic Studies*, 58(2):277–297.

Autor, D. H. and Houseman, S. N. (2010). Do Temporary-Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from "Work First". *American Economic Journal: Applied Economics*, 2(3):96–128.

Baltagi, B. H. (2005). *Econometric Analysis of Panel Data*, volume 3rd ed. John Wiley & Sons, Ltd.

Bell, D. N. F. and Blanchflower, D. G. (2019). The lack of wage growth and the falling NAIRU. *National Institute Economic Review*, (245):R40–R54.

Bell, D. N. F. and Blanchflower, D. G. (2021). Underemployment in the United States and Europe. *Industrial and Labour Relations Review*, 74(1):56–94.

Bellani, D. and Bosio, G. (2019). Knocking' on heaven's door? Reframing the debate on temporary employment and wages: evidence from Europe. *Socio-Economic Review*, (0):1–39.

Biegert, T. (2014). On the outside looking in? Transitions out of non-employment in the united kingdom and germany. *Journal of European social policy*, 24(1):3–18.

Blanchflower, D. G., Bryson, A., and Spurling, J. (2022). The wage curve after the great recession. *NBER Working Paper Series*.

Blanchflower, D. G. and Oswald, A. J. (1994). *The wage curve*. MIT Press, Cambridge, Mass ; London.

Blundell, R. and Bond, S. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics*, 87(1):115–143.

Bonam, D., de Han, J., and van Limbergen, D. (2021). Time-varying wage Phillips curves in the euro area with a new measure for labour market slack. *Economic Modelling*, (96):157–171.

Bulligan, G. and Viviano, E. (2017). Has the wage Phillips curve changed in the euro area. *IZA Journal of Labor Policy*, (6):R40–R54.

Byrne, D. and Zekaite, Z. (2020). Non-linearity in the wage Phillips curve: Euro area analysis. *Economics Letters*, 186(C).

Calmfors, L., Driffill, J., Honkapohja, S., and Giavazzi, F. (1988). Bargaining structure, corporatism and macroeconomic performance. *Economic policy*, 3(6):14–61.

Ciccarelli, M. and Osbat, C. (2017). Low inflation in the euro area: Causes and consequences. *ECB – Occasional Paper Series*, (181).

Costain, J. S., Jimeno, J. F., and Thomas, C. (2010). Employment fluctuations in a dual labor market. *Banco de Espana Working Paper*, (1013).

Damiani, M., Pompei, F., and Ricci, A. (2018). Labour shares, employment protection and unions in European economies. *Socio-Economic Review*.

Dickens, W. T. and Lang, K. (1985). A Test of Dual Labor Market Theory. *The American Economic Review*, 75(4):792–805.

DiNardo, J., Fortin, N. M., and Lemieux, T. (1996). Labor market institutions and the distribution of wages, 1973-1992: A semiparametric approach. *Econometrica*, 64(5):1001–1044.

Doeringer, P. B. and Piore, M. J. (1971). *Internal Labor Markets and Manpower Analysis*. Routledge, New York.

Dolado, J. J., García-Serrano, C., and Jimeno, J. F. (2002). Drawing lessons from the boom of temporary jobs in Spain. *The Economic journal (London)*, 112(480):F270–F295.

Draeger, V. and Marx, P. (2017). Do firms demand temporary workers when they face workload fluctuation? Cross-country firm-level evidence. *ILR Review*, 70(4):942–975.

Drenik, A., Jäger, S., Plotkin, P., and Schoefer, B. (2023). Paying Outsourced Labor: Direct Evidence from Linked Temp Agency-Worker-Client Data. *The Review of Economics and Statistics*, 105(1):206–216.

Emmenegger, P., Häusermann, S., Palier, B., and Seeleib-Kaiser, M. (2012). *The Age of Dualization: The Changing Face of Inequality in Deindustrializing Societies.* International Policy Exchange Series. Oxford University Press, New York.

Fortin, N., Lemieux, T., and Firpo, S. (2011). Chapter 1 - Decomposition methods in economics. volume 4 of *Handbook of Labor Economics*, pages 1 – 102. Elsevier.

Gebel, M. and Giesecke, J. (2011). Labor market flexibility and inequality: The changing skill-based temporary employment and unemployment risks in Europe. *Social Forces*, (1).

Hancké, B. and Soskice, D. (2003). Wage-setting and inflation targets in emu. *Oxford review of economic policy*, 19(1):149–160.

Hijzen, A., Mondauto, L., and Scarpetta, S. (2017). The impact of employment protection on temporary employment: Evidence from a regression discontinuity design. *Labour Economics*, 46:64–76.

Hirsch, B. (2016). Dual labor markets at work: The impact of employers' use of temporary agency work on regular workers' job stability. *Industrial and Labour Relations Review*, 69.

Hong, G. H., Koczan, Z., Lian, W., and Nabar, M. (2018). More slack than meets the eye? Recent wage dynamics in advanced economies. *IMF - Working Paper Series*, (50).

Houseman, S. N., Kalleberg, A. L., and Erickcek, G. A. (2003). The role of temporary agency employment in tight labor markets. *Industrial & labor relations review*, 57(1):105–127.

Hurley, J. and Partini, V. (2017). Estimating labour market slack in the European Union. *Euorfound - Research Report*.

Hyman, L. (2018). *TEMPS – How American Work, American Business, and the American Dream Became Temporary*. Viking, New York.

ILO (2016). *Non-standard employment around the world - Understanding challenges, shaping prospects*. International Labour Office, Geneva.

IMF (2017). Recent wage dynamics in advnace economies: Drivers and Implitations. *World Economic Outlook - Chapter 2*, (2).

Kahn, L. M. (1979). Union Strength and Wage Inflation. *Industrial Relations: A Journal of Economy and Society*, 18(2):144–155.

Kahn, L. M. (1980). Bargaining power, search theory and the phillips curve. *Cambridge journal of economics*, 4(3):233–244.

Kahn, L. M. (2016). The structure of the permanent job wage premium: Evidence from Europe. *Industrial Relations: A Journal of Economy and Society*, 55(1):149–178.

Katz, L. F., Krueger, A. B., Burtless, G., and Dickens, W. T. (1999). The high-pressure u.s. labor market of the 1990s. *Brookings papers on economic activity*, 1999(1):1–87.

Kiss, A. and Van Herck, K. (2019). Short-term and long-term determinants of moderate wage growth in the EU. *IZA Policy Paper*, (144).

Koutentakis, F. (2008). The effect of temporary contracts on job security of permanent workers. *Economics Letters*, 101(3):220–222.

Kuroki, M. (2012). The deregulation of temporary employment and workers' perceptions of job insecurity. *Industrial & labor relations review*, 65(3):560–577.

Lange, P. (1984). *Union democracy and liberal corporatism : exit, voice and wage regulation in postwar Europe*. Cornell studies in international affairs. Western societies papers. Center for International Studies, Cornell University, Ithaca, N.Y.

Latner, J. P. (2022). Temporary employment in Europe: Stagnating rates and rising risks. *European Societies*, 24(4):383–408.

Lazear, E. P. (1990). Job Security Provisions and Employment*. *The Quarterly Journal of Economics*, 105(3):699–726.

Lindbeck, A. and Snower, D. (2002). The insider-outsider theory: A survey. Discussion Paper 534, IZA, Bonn.

Lindbeck, A. and Snower, D. J. (1988). Long-term unemployment and macroeconomic policy. *The American economic review*, 78(2):38–43.

Marx, P. (2011). The unequal incidence of non-standard employment across occupational groups: An empirical analysis of post-industrial labour markets in Germany and Europe. Discussion Paper 53521, IZA, Bonn.

Metten, A. (2021). Rethinking trade union density: A new index for measuring union strength. *Industrial Relations Journal*, 52(6):528–549.

Moretti, L., Onorante, L., and Saber, S. Z. (2019). Phillips curves in the euro area. *Working Paper Series*, (2295).

Nickel, C., Bobeica, E., Gerrit, K., Lis, E., and Porqueddu, M. (2019). Understanding low wage growth in the euro area and European countries. *ECB – Occasional Paper Series*, (232).

OECD and AIAS (2021). Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts (ICTWSS). OECD Publishing, Paris.

Olson, M. (1971). *The logic of collective action; public goods and the theory of groups.* Harvard economic studies ; v. 124. Harvard University Press, Cambridge, Mass.

Orphanides, A. and van Norden, S. (2002). The unreliability of output-gap estimates in real time. *The Review of Economics and Statistics*, 84(4):569–583.

Pavlopoulos, D. (2013). Starting your career with a fixed-term job: Stepping-stone or "dead end"? *Review of Social Economy*, 71(4):474–501.

Phillips, A. W. (1958). The relation between unemployment and the rate of change of money wage rates in the United Kingdom 1861-1957. *Economica, New Series*, (25):485–503.

Piore, M. J., editor (1979). *Unemployment and Inflation: Institutionalist and Structuralist Views*. M. E. Sharpe, White Plains, N.Y.

Piore, M. J. (1983). Labor Market Segmentation: To What Paradigm Does It Belong? *The American Economic Review*, 73(2):249–253.

Polavieja, J. (2006). The incidence of temporary employment in advanced economies: Why is spain different? *European Sociological Review*, 22(1):61–78.

Ramskogler, P. (2021). Labour market hierarchies and the macro-economy – do labour market dualities affect wage growth in Europe? *Structural Change and Economic Dynamics*, 56:154–165.

Ravn, M. O. and Uhlig, H. (2002). On adjusting the Hodrick-Prescott filter for the frequency of observations. *The Review of Economics and Statistics*, 84(2):371–375.

Reich, M., Gordon, D. M., and Edwards, R. C. (1973). A Theory of Labor Market Segmentation. *The American Economic Review*, 63(2):359–365.

Roodman, D. (2009). How to do xtabond2: An introduction to difference and system GMM in Stata. *Stata Journal*, 9(1):86–136.

Rosen, S. (1972). Learning and Experience in the Labor Market. *The Journal of Human Resources*, 7(3):326–342.

Rosenbaum, P. R. and Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1):41–55.

Rusinova, D., Lipativ, V., and Heinz, F. (2015). How flexible are real wages in EU countries? A panel investigation. *Journal of Macroeconomics*, 43:140–154.

Soskice, D. (1990). Wage determination: The changing role of institutions in advanced industrialized countries. *Oxford review of economic policy*, 6(4):36–61.

Stansbury, A. and Summers, L. H. (2020). The declining worker power hypothesis: An explanation for the recent evolution of the american economy. *Brookings papers on economic activity*, 2020(1):1–77.

Thelen, K. A. (2014). *Varieties of liberalization and the new politics of social solidarity.* Cambridge studies in comparative politics. Cambridge; New York.

Traxler, F. and Brandl, B. (2012). Collective Bargaining, Inter-Sectoral Heterogeneity and Competitiveness: A Cross-National Comparison of Macroeconomic Performance. *British Journal of Industrial Relations*, 50(1):73–98.

Voinea, L. (2018). Explaining the post-crisis Philips curve: Cumulated wage gap matters for inflation. *CEPS Working Document*, (2018/05).

Weisstanner, D. (2020). Insiders under pressure: Flexible employment and wage inequality. *Journal of Social Policy*, pages 1–20.

Western, B. and Healy, K. (1999). Explaining the OECD Wage Slowdown Recession or Labour Decline? *European Sociological Review*, 15(3):233–249.

Wooldridge, J. M. (2009). *Introductory Econometrics: A Modern Approach.* South-Western.

Chapter 5

What do unemployed workers want: Guaranteed jobs or guaranteed income?

This paper studies unemployed workers' support for two much-debated social policy innovations often positioned as rivals: a job guarantee and basic income. Leveraging an experimental design in a representative survey among unemployed workers as its main beneficiary group, the analysis finds broad support for both policies, guaranteed jobs and guaranteed income, though support is consistently higher for guaranteed jobs. For either policy, increasing the pay level yields strong increases in support. Crossing a critical threshold at the low-pay level just above average unemployment benefits, notably raises the willingness to accept guaranteed jobs, indicating a strong willingness to work for little monetary benefit. Support is more prevalent among disadvantaged people, whereas opposition to both policies is unaffected by the pay level among a small, steadfast minority, in more favorable socio-economic circumstances. The results suggest a shift in perspective to view both policies as complementary rather than competing strategies to strengthen the social safety net.

Keywords: job guarantee, universal basic income, unemployed workers, social policy preferences, welfare state attitudes

I thank Daniel Schönherr at SORA who led the work on the Arbeitslosenmonitor survey, and Barbara Blaha and Leonhard Dobusch at Momentum Institut who initiated and funded it, without whose support this study would not have been possible.

I thank Judith Derndorfer, Bernhard Ebbinghaus, Chris Featherman, Karin Heitzmann, Maximilian Kasy, Vanessa Lechinger, Elias Naumann, Brian Nolan, Patrick Nüß, Anna Schwarz, Dennis Tamesberger, Simon Theurl, and Tim Vlandas for valuable feedback and comments.

I acknowledge financial support from the Economic and Social Research Council [grant number

5.1 Introduction

"The proposal of the negative income tax is a proposal to help poor people by giving them money, which is what they need."

—Milton Friedman (1962)

"Man does not live by bread alone. We need more, not just games. We need work under humane conditions to be fully human."

—Marie Jahoda (1984)

Is income support sufficient to alleviate poverty? Do humans need paid employment to strive and participate in our society? The debate about the value of employment is at least as old as capitalism and has generated opposing views since Lafargue [1893](2023). In recent years, guaranteed jobs and guaranteed income have become much-debated social policy innovations with prominent trials taking place across the world (Baird et al., 2011; Haushofer and Shapiro, 2016; Verho et al., 2022; Kasy et al., 2024; Kasy and Lehner, 2023) supported by the European Commission (Markowitsch and Scharle, 2024), European Parliament (Parliament, 2023), and United Nations Special Rapporteur on extreme poverty and human rights (UN Special Rapporteur, 2023). While those trials have revealed important insights into behavioral responses to guaranteed jobs and guaranteed income policies, less is known about people's attitudes, in particular those of primary beneficiary groups. This is where the article contributes turning to a group that would be most affected by such policies to ask: what do unemployed workers actually want: guaranteed jobs or guaranteed income?

Eliciting beliefs to uncover stated preferences complements the growing body of evidence on revealed preferences with regard to guaranteed income and guaranteed jobs.

ES/P000649/1], the Scatcherd European fund, and the Saven European fund as part of my PhD funding. The survey data collection was funded by the Momentum Institut.

A scientific use file of the survey data has been made available publicly in the Austrian Social Science Data Archive at https://data.aussda.at/dataset.xhtml?persistentId=doi:10.11587/XJNNYA (Schönherr and Lehner, 2022).

While numerous studies have explored preferences for basic income support (Roosma and van Oorschot, 2020; Vlandas, 2021; Weisstanner, 2022) and assessed support for its different versions (Chrisp et al., 2020; Stadelmann-Steffen and Dermont, 2020; Rincón et al., 2022; Laenen, 2023; Rincon, 2023), no representative study, to my knowledge, has examined preferences for job guarantee support. There appears to be a gap in comparing support for the two proposed policies, mirroring the ongoing public debate. Documenting stated preferences of beneficiary groups further complements the vibrant field of studying attitudes on social policies usually aimed at uncovering public support of the general population. Studies focusing on the attitudes of beneficiary groups often involve small sample sizes due to capacity constraints. While existing studies typically look at associations of individual characteristics with support for the proposed social policies, this experiment provides causal estimates of the impact of pay level on beneficiaries' support—a crucial design factor that remains often unaddressed. The study contributes by providing the first direct comparison through a survey experiment to assess the causal impact of pay levels on support for either policy. Moreover, it surveys a representative sample of a hard-to-reach subpopulation. Complementing the analysis of revealed preferences in Kasy and Lehner (2023) with stated preferences, I study the conditions under which unemployed workers prefer income- or employment support.

Research design To assess the impact of the monetary payments on scheme participants, I designed a survey experiment for a representative telephone survey of unemployed workers in Austria. Causal inference rests on a randomized assignment: three groups of survey respondents receive varying types of information. The randomized information includes the amount paid by the schemes so that every participant would receive pay either at the average unemployment benefits level (€1,000, Group 1), or a low wage at the 10^{th} percentile of the wage distribution (€1,500, Group 2), or a living wage at the 33^{rd} percentile of the wage distribution (€2,000, Group 3). I can compare the outcome means of participants in three groups with varying pay levels of the proposed schemes to identify the causal impact of the pay level. I also evaluated whether respondents support both or only one of the schemes and how support varies by the schemes' pay level. Further, I used logistic regressions to understand how the likelihood of supporting either of the proposed schemes is influenced by respondents' socio-economic characteristics and personal experiences. I conducted the same analyses for the willingness to accept a guaranteed job.

Main results Three critical insights emerge from the study regarding beneficiaries' support for guaranteed jobs and guaranteed income schemes and the acceptance of guaranteed jobs. First and foremost, there exists robust support for both guaranteed job and guaranteed income policies. A noticeable preference for guaranteed jobs at all pay levels and a high willingness to accept guaranteed jobs indicates a strong willingness to work even under modest pay increases over benefits.

Second, as pay levels increase so does support for both policies, alongside a marked increase in peoples' willingness to accept guaranteed jobs. This trend is particularly pronounced in the shift from the pay level of average unemployment benefits to the low-wage pay level. This indicates a critical pay threshold that highlights the important role of economic incentives in enhancing beneficiaries' acceptance of guaranteed jobs.

Third, contrary to the public debate where job guarantee policies are often pitched against basic income proposals, and vice versa, the analysis reveals a predominantly complementary view among respondents. This suggests that increased pay levels not only bolster support for each policy individually but also foster a supportive attitude toward both. Despite a prevailing pro-work sentiment—evidenced by stronger support for guaranteed jobs over guaranteed income—our findings challenge the notion of mutual exclusivity between support for job and income guarantees.

Mechanisms The exploration of mechanisms indicates that more disadvantaged people, characterized by socio-economic conditions and personal experiences, show stronger support for both policies. Socio-economic factors, such as adverse health conditions and lower unemployment benefits, as well as experienced labor market discrimination are linked to stronger support for either policy.

Following general support, it is important to understand whether potential beneficiaries would participate in the proposed policies. While in the proposed policies, everyone would receive basic income unconditionally, take-up of guaranteed jobs would be voluntary. Socio-economic factors and personal experiences strongly predict an unemployed worker's willingness to accept a guaranteed job. Notably, poorer health status and experienced discrimination consistently go hand-in-hand with a higher likelihood of accepting a guaranteed job, underscoring the appeal of job security among those particularly disadvantaged. Conversely, higher unemployment benefits and greater occupational prestige correspond to a reduced likelihood of accepting a guaranteed job, highlighting a preference for existing social status and benefits over program participation. A small, steadfast minority, better off socio-economically, opposes both policies at any pay level.

Roadmap The remainder of the paper is structured as follows. Section 5.2 sets out the theoretical framework guiding the debate around guaranteed income and guaranteed jobs (Section 5.2.1) and reviews existing evidence on social policy preferences (Section 5.2.2). Section 5.3 introduces the empirical approach including the data and sample selection (Section 5.3.1), outcomes of interest (Section 5.3.2), experimental design (Section 5.3.3), identification approach (Section 5.3.4), and the approach to inference (Section 5.3.5). Section 5.4 presents the empirical findings including respondents' support for guaranteed job and guaranteed income schemes (Section 5.4.1), the complementarity or exclusivity involved in supporting either or both schemes (Section 5.4.2), and respondents' willingness to accept jobs provided under the proposed scheme (Section 5.4.3). Section 5.5 investigates the individual-level determinants of support for guaranteed jobs and guaranteed income (Section 5.5.1), identifies supporters and opponents (Section 5.5.2), and explores predictors of respondents' willingness to accept a guaranteed job (Section 5.5.3). Section 5.6 discusses implications of the findings and concludes. Appendix D.1 provides details on the sample. Appendix D.2 presents details on the survey and questionnaire.

5.2 Background

Let us now explore the debate on job guarantees and basic income (Section 5.2.1) and discuss this study's contribution regarding research on preferences for social policies (Section 5.2.2).

5.2.1 Guaranteed jobs and guaranteed income

The debate around the value of work is at least as old as capitalism. Notably, Karl Marx's son-in-law, Paul Lafargue, made an early contribution to this discourse with his 1883 pamphlet, *Le Droit à la paresse* (*The Right to Be Lazy*), which contrasts markedly with the ethos of the right to jobs (Lafargue, 1883). Already Thomas More (2016) imagined a form of guaranteed income, which is often cited by contemporary advocates, who position basic income as a counterpoint to guaranteed jobs (Standing, 2005, 2018), and vice versa (Tcherneva, 2012). Neither has been part of the conventional toolbox of social and labor market policy (Ebbinghaus, 2020; Lehner and Tamesberger, 2024).

Guaranteed jobs Guaranteed job proposals, commonly referred to as a job guarantee or an employment guarantee, have garnered increasing attention in recent years (Tcherneva, 2020; Romeo, 2023). These proposals are characterized by voluntary participation, ensuring access to anyone within the beneficiary group. Upon opting for a guaranteed job, participants are compensated with a real wage, establishing a formal employment relationship. Notably, existing projects under these proposals emphasize meaningful work and include participatory elements, offering workers the opportunity to design their own jobs and projects.

Several projects have initiated guaranteed jobs in specific regions and municipalities in recent years. These include the Marienthal job guarantee in Austria (Kasy and Lehner, 2023), Basibaan in the Netherlands, and Territoires zéro chômeur de longue durée (*Zero* *long-term unemployed territories*) Markowitsch and Scharle (2024). The latter, as the largest job guarantee initiative in Europe, has already expanded to encompass roughly half of France's territories with the backing of France's parliament and president. These programs have transcended national borders, inspiring similar projects in Belgium and Italy to adopt guaranteed employment strategies. This momentum led the European Commission to allocate €23 million in 2024 for job guarantee pilots and prompted the European Parliament to pass a resolution advocating for the expansion of job guarantees across Europe to facilitate a just transition (Parliament, 2023).

These recent developments in Europe draw inspiration from historical precedents in the United States, most notably Franklin D. Roosevelt's Works Progress Administration (WPA) during the New Deal era. Outside high-income countries, the Indian National Rural Employment Guarantee Act (NREGA) stands as the most substantial project of its kind, providing employment to between 5 and 100 million workers annually since 2006 (Ehmke, 2016; De Venanzi, 2020; Muralidharan et al., 2023a).

Guaranteed income Guaranteed income, commonly referred to as basic income, has emerged as one of the most extensively discussed social policy innovations in recent years (Standing, 2017; Van Parijs and Vanderborght, 2017; Prainsack, 2020). Its key features include a regular payment made by the state, accessible universally regardless of the recipient's other income sources or wealth, and its unconditional nature. This unconditionality marks the primary distinction from guaranteed jobs.

Several trials of guaranteed income have been conducted. At the national level, pilots have taken place in Finland (Verho et al., 2022) and Germany (Kasy et al., 2024). At the state level, the Alaska Permanent Fund Dividend represents the first permanent implementation of such a program, providing an annual payment to state residents (Jones and Marinescu, 2022). Historically, trials experienced a high phase during the 1970s in the United States and Canada (Kershaw et al., 1976; Watts and Rees, 1977; Burtless and

Hausman, 1978; Robins, 1985; Ashenfelter and Plant, 1990; Hum and Simpson, 1993; Calnitsky and Latner, 2017) a period in which guaranteed income was much-debated in the form of a negative income tax (Friedman, 1962).

5.2.2 Existing evidence on social policy preferences

5.2.2.1 Attitudes of beneficiaries

This study enriches the debate around job guarantees and basic income proposals by eliciting the stated preferences of beneficiaries. This approach complements the existing evidence on revealed preferences in response to guaranteed income and guaranteed jobs, thereby offering a more nuanced understanding of recipients' dispositions toward these social policies. While much of the research in this field aims to gauge the support of the general population, documenting the specific preferences of beneficiaries adds a critical dimension often overlooked due to the challenges of engaging with smaller sample sizes. Rare but noteworthy exceptions, such as Ebbinghaus and Naumann (2018), underscore the value of exploring welfare state attitudes from the perspective of its primary recipients. Few studies have examined how individual risk perceptions influence social policy preferences (Gingrich and Ansell, 2012; Rehm et al., 2012). Moreover, scholars in industrial relations have established a tradition of surveying workers about their preferences concerning employment relations and workplace practices (Kochan, 1979; Freeman and Rogers, 2006; Kochan et al., 2019). These cross-disciplinary examples from social policy and industrial relations research underscore the importance of informing both theoretical debates and practical policy developments by directly engaging with those most affected by policies.

Previous research studying public attitudes suggests that social policy support observed among unemployed workers can be considered an upper bound for the general population, as support tends to be stronger among beneficiaries (Kangas, 1997; Svallfors, 1997; Naumann et al., 2016). The gap in support between beneficiaries and the general population is notably significant in the context of unemployment support and has also been documented for basic income (Vlandas, 2021). Compared to other groups, such as families with children, older adults, people requiring healthcare, or persons with disabilities, the jobless people tend to receive less solidarity and are often viewed as less "deserving" by the broader public (van Oorschot, 2006; van Oorschot et al., 2017; Buß et al., 2017; Ebbinghaus et al., 2022). This phenomenon may be further explained by the insider-outsider cleavages that emerge as a result of labor market dualization, which has been documented as affecting support for active labor market policies. Outsiders, which in my study are the beneficiaries themselves, demonstrate a significantly higher inclination toward supporting increased benefits and advocating for public responsibility in social welfare (Fraile and Ferrer, 2005).

5.2.2.2 Preferences by social policy design

The focus of this article extends beyond support for the two social policies, delving into how policy design, in particular pay levels, influences policy support.

Conditionality Policy design critically influences public support (Chrisp et al., 2020). The unconditional nature of guaranteed income, i.e., lacking work requirements, stands as its most contentious aspect (Rincón et al., 2022; Laenen et al., 2023), aligning with the prevalent approval of conditionality in unemployment benefits (Buß et al., 2017). Indeed, unemployment benefits and government-led redistribution receive more backing than guaranteed income (Weisstanner, 2022). Similarly, conditional cash programs, which require active participation, such as *Participation Income* proposed by Atkinson (1996), often see higher support than unconditional ones. Guaranteed jobs, which may be viewed as a specific version of conditional cash programs, do not only condition wages on work requirements, they also provide unemployed workers the opportunity to engage in paid work as a social activity and a source of collective purpose (Jahoda, 1984). Indeed, high willingness to work among long-term unemployed workers has been demonstrated by the

Marienthal job guarantee pilot (Kasy and Lehner, 2023).

Interestingly, generous basic income schemes, above the subsistence level, garner more approval (Laenen, 2023). Support for basic income also increases when funded through progressive taxation rather than by cutting existing benefits. This suggests the observed support in this experiment is a potential lower bound.

By comparing guaranteed jobs to guaranteed income, my vignette survey experiment adds to previous conjoint experiments that focused on design features of guaranteed income (Stadelmann-Steffen and Dermont, 2020; Rincón et al., 2022; Rincon, 2023) This experimental design allows for causal inference of the pay level's effect on beneficiaries' support—a crucial design feature that often remains unaddressed in studies that have documented associations of individual characteristics with support for the proposed social policy.

5.2.2.3 Preferences for basic income

Research on social policy preferences has recently experienced a surge in studies measuring attitudes toward proposed basic income schemes, with little attention given to support for proposed job guarantee schemes.[1] Despite both being central to welfare state reform considerations, no study has yet directly compared attitudes toward these two much-debated social policy innovations.

Overall support Remarkably, the 63% support for guaranteed income found in this study corresponds to the support from unemployed workers in Austria five years earlier, to a similarly phrased question in wave 8 of the European Social Survey (ESS) in 2016 (Vlandas, 2019). For comparison, the same study also reports 56% support from those who experienced unemployment in the past five years and 46% from the general population, which corroborates findings from Roosma and van Oorschot (2020).

[1](Langer et al., 2023) report strong support for publicly funded job programs with a non-representative convenience sample of 400 respondents.

Individual determinants Research on support for basic income has significantly expanded recently (Chrisp et al., 2020; Stadelmann-Steffen and Dermont, 2020; Rincon, 2023; Laenen, 2023) while comparable counterparts for guaranteed jobs remain missing. Several ESS-based studies have identified key factors, such as younger age and lower benefits or incomes, that increase support (Roosma and van Oorschot, 2020; Vlandas, 2021). Results of the ESS show no clear gender preference (Roosma and van Oorschot, 2020) or slightly stronger support among men (Vlandas, 2021; Weisstanner, 2022). The latter studies also indicate greater support among the working class.

5.3 Research design

I designed a randomized survey experiment to elicit support among unemployed workers for guaranteed job and guaranteed income schemes. Specifically, I examined how variation in the proposed pay level influences respondents' support for these schemes and their willingness to take up employment under the guaranteed job scheme. This section provides an overview of the data and sample selection (Section 5.3.1), outcomes of interest (Section 5.3.2), experimental design (Section 5.3.3), identification approach (Section 5.3.4), and the approach to inference (Section 5.3.5).

5.3.1 Data

To reach a representative sample of a hard-to-reach vulnerable sub-population, the survey relied on a probability sampling design together with telephone interviews.[2] Data collection for the survey took place between May and July 2021, a period characterized by a rapid labor market recovery, following several Covid-induced lockdowns during winter 2021. The specific nature of this period due to the pandemic has to be acknowledged, even though studies have found only limited changes in welfare preferences during the pandemic (Ebbinghaus et al., 2022). A scientific use file of the data has been made available publicly in the Austrian Social Science Data Archive (Schönherr and Lehner, 2022) at

https://data.aussda.at/dataset.xhtml?persistentId=doi:10.11587/XJNNYA.

Sample selection First, the sampling relied on probability sampling based on pre-stratified, non-clustered random selection of people by municipality. All telephone network providers were used for sampling. Additionally, secret numbers were called through random digit dialing (RDD). For each random number, at least 3 contact attempts were made

[2]The questionnaire was jointly designed with Daniel Schönherr (SORA). Within the survey, I designed the survey experiment on support for guaranteed jobs and guaranteed income. SORA and IPR Umfrageforschung carried out the survey. Because of these arrangements, I did not seek ethics approval for the data collection. Further details are reported in the survey report (Schönherr, 2021).

up to a maximum of 5. Within the households identified, the interviewee was selected via a screening question on their current employment status, which filtered respondents for being "without work or orders" regardless of their registration with the Public Employment Service (PES) (Appendix D.2.2.1). Thus, the sample relies on a self-reported measure of unemployment, which has the advantage of including registered job seekers as well as those not registered with the PES and those self-employed without work.

Survey mode Second, the survey was conducted via computer-assisted telephone interviewing (CATI). In addition to German, 10% of the interviews were conducted in Turkish, Serbo-Croatian, Hungarian, and Arabic to reach non-German speakers, which are disproportionally represented among unemployed workers (Appendix Table D.2.2. An interview lasted 19.24 minutes on average and 18 minutes in the median.

Response rate The survey achieved a response rate of 17% among eligible participants who answered the phone and agreed to participate in the survey (Appendix Table D.2.3). A utilization rate of 6.8% can be calculated by adding the 10,810 people not reached to the number of conducted interviews and refusals. Comparing eligible persons (conducted interviews and refusals) to non-eligible persons (sample-neutral dropouts) yields a non-employment rate of 3% of the working-age population (>15 years) as a share of total population.

Sample This leaves us with a sample of 1,215 respondents (Appendix Table D.1.1). Of those, around half have a migration background and 30% are women. The average age is around 40 years and a respondent has spent an average of 10-11 weeks in unemployment. Around 25% have been unemployed for 1 year or longer. This compares to 47% women, 1/3 with foreign citizenship, and an average of 26 weeks in unemployment among registered unemployed workers in spring 2021 (AMS, 2021).[3] Most variables have no missing observations and no variable has more than 7% of missing observations.

To adjust for educational attainment and the duration of the unemployment spell,

[3]Note that the sample definitions differ as the survey includes those not registered with the PES and those self-employed without work.

weights were created on the basis of the Microcensus 2020 and administrative Public Employment Service data. Appendix Table D.1.1 shows only marginal differences between the unweighted and weighted samples except accounting for a shortfall in women respondents. I conducted the analysis on the unweighted sample, as the random assignment took place on a simple randomization procedure without pre-stratification.

5.3.2 Outcomes of interest

I estimated the effect of payment level on support for each scheme and willingness to participate in a guaranteed job scheme. These schemes' characteristics are described in Table 5.1 and the outcomes as well as the covariates are defined in Table 5.2. All outcomes and covariates are based on the survey described above. The outcomes were collected after the covariates except for demographic characteristics. The complete list of survey questions corresponding to each of these outcomes and covariates is listed in Appendix Section D.2.2. It follows established practices in survey design, sociology, psychology, and public health.

The three outcomes are support for guaranteed jobs, support for guaranteed income, and a willingness to accept a job under the scheme. The proposed schemes (Table 5.1) echo existing pilot schemes (Verho et al., 2022; Kasy and Lehner, 2023; Kasy et al., 2024) and policy proposals (Standing, 2017; Van Parijs and Vanderborght, 2017; Tcherneva, 2020; UN Special Rapporteur, 2023). The proposed design of the guaranteed income scheme also closely matches the question on support for basic income from the European Social Survey (ESS, 2016). Each of these outcomes is based on a dummy variable that indicates 1 if the respondent supports the scheme or is willing to accept a guaranteed job.

Regarding guaranteed jobs, respondents were introduced to a hypothetical scheme that offered anyone unemployed for longer than 1 year a publicly funded job, which eligible participants could voluntarily decide whether to accept. Jobs would involve local non-profit activities and be paid a collectively bargained minimum wage of €1,000, €1,500, or

€2,000 net monthly per randomized group. Participants were asked whether they would support or oppose such a scheme and whether they would accept a job under the scheme.

Regarding guaranteed income, respondents were introduced to a hypothetical scheme paying a fixed amount of €1,000, €1,500, or €2,000 net monthly from the state per randomized group. The recipient would have to do nothing in return and receive the amount regardless of whether they would work or had any other income or assets. The scheme, however, would replace other social benefits currently in place.

Table 5.1: Scheme characteristics

Dimension	Guaranteed job	Guaranteed income
	Treatment	
Pay level	€1,000, €1,500, or €2,000[a] net per month	€1,000, €1,500, or €2,000 net per month
	Constant features	
Conditionality	Work[b]	None
Eligibility	> 1 year unemployed	Everyone
Existing benefits	Supplementary	Substitute[c]

[a] In the survey, the pay level is defined as "a collectively-bargained minimum wage."
[b] Work is defined as "in non-profit activities in the municipality or district."
[c] The description of guaranteed income in the survey indicates that "there would no longer be any social benefits as there are now."

Accordingly, the key difference between guaranteed jobs and guaranteed income, as proposed in the survey questions, lied in their conditionality, eligibility criteria, and effect on existing benefits: guaranteed jobs would require work in local non-profit activities and would be available to anyone unemployed for more than a year. In contrast, guaranteed income would not require work or would not have any other conditionality attached[4] and would be paid to everyone irrespective of employment status or income. While guaranteed income would replace existing social benefits, guaranteed jobs would add an extra layer to the social safety net. The comparison between guaranteed jobs and guaranteed income reveals several distinctions that likely influence support levels and, in the current design, can only be assessed collectively to document support for the most prominent policy packages.

[4] No conditionality can be linked to automatic enrollment, whereas in the job guarantee context, with a work requirement, voluntary participation is crucial.

For comparison, the pay level of €1,000 amounted close to the average level of monthly unemployment benefits (Statistics Austria (2024a)) well below the minimum wage of any sector. The pay level of €1,500 was at about the level of the 10$^{\text{th}}$ percentile, which amounts to 2/3 of the median wage reflecting a low-wage earner at around the minimum wage. By comparison, the pay level of €2,000 was at about the level of the 33$^{\text{rd}}$ percentile in the wage distribution (Statistics Austria, 2024b). For easier contextualization, I refer to the pay levels as "Unemployment benefits level (UB level)", low wage, and living wage.

Respondents might be impacted by the order of questions on guaranteed jobs and guaranteed income, which is why the order of the questions regarding support for guaranteed jobs and guaranteed income was randomized. The question on willingness to accept a guaranteed job was asked immediately following the question on support for guaranteed jobs.

Covariates Covariates include unemployment duration in weeks, age in years, and dummy indicators for gender and migration background. For unemployment benefits reported in income brackets, I assigned the mean of each bracket to the respective observation to obtain a continuous variable, which I logged to avoid assigning too much weight to outliers in the regression analysis and to interpret the coefficient as percentage changes. For other covariates, I constructed a range of indices, on health status (-), occupational prestige, experienced discrimination, and stigma awareness. These indices are equally weighted combinations of the responses to the respective survey scales. To enable a compact presentation of our results in Section 5.4, I normalized the indices, such that higher values correspond to "better" outcomes (health status with a flipped sign is marked by *(-)* in the subsequent tables and figures), and such that the range of these variables is the interval $[0, 1]$; cf. Table 5.2.

Table 5.2: Variable definitions

Variable	Definition
	Outcomes
Support for guaranteed jobs	Dummy variable (for/against), I don't know, no answer)
Support for guaranteed income	Dummy variable (for/against[a])
Willingness to accept a guaranteed job	Dummy variable (yes/no, I don't know, no answer)
	Covariates
Health status (-)	Normalized index of six item scales of physical and mental health, switched sign so that a lower value means a better health status
Unemployment duration	Nr. of weeks since the start of the unemployment spell
Unemployment benefits	Level of unemployment benefits received, including other benefits, in logs
Occupational prestige	Normalized index of the Standard International Occupational Prestige Scale (SIOPS), which is based on ISCO-08 2-digit occupation categories (Treiman, 1977)
Discrimination	Normalized index of the number of four potential reasons for experienced discrimination in the labor market
Stigma	Normalized index of five item scales of stigma awareness building on the IAB Panel Arbeitsmarkt und soziale Sicherung (PASS, 7th wave, 2013)
Gender	Dummy variable (woman/man)
Age	Years
Migration background	Dummy variable (yes/no)

[a]The responses I don't know/no answer were excluded from the analysis.

5.3.3 Experiment design

I deployed a survey experiment where the varying factor was the level of income. The income difference amounts to €500 per group, where Group 1 was told that the guaranteed jobs and guaranteed income schemes respectively pay €1,000, Group 2 €1,500, and Group 3 €2,000.

Assignment to these groups was randomized for each participant at the time of the survey data collection (Muralidharan et al., 2023b). Summarizing the resulting assignment, Table 5.3 shows the covariate means per group and the corresponding p values for a joint test of significance. Confirming that our procedure worked as intended, the available covariates are balanced across groups with one exception: women are significantly less represented in Group 1 compared to Groups 2 and 3.

Table 5.3: Balance table by group, unweighted

	Unemployment benefits level	Low wage	Living wage	p value
Health status (-)	0.19	0.19	0.20	0.87
Unemployment duration	10.43	10.71	10.14	0.75
Unemployment benefits	918.64	905.83	909.65	0.84
Occupational prestige	0.34	0.34	0.34	0.93
Discrimination	0.11	0.13	0.12	0.40
Stigma	0.36	0.38	0.39	0.20
Women	0.46	0.56	0.53	0.02
Age	40.20	41.31	40.46	0.39
Migration Background	0.25	0.31	0.30	0.19
N	399	400	416	

5.3.4 Identification approach

Due to the clean randomization of participants into three different groups, it is possible to identify treatment effects by comparing the relevant outcome variables directly between the three groups.[5] This yields an unbiased estimate of the treatment effect that does not hinge on any assumptions other than the random assignment into groups. The results for policy support can thus be interpreted as intention-to-treat (ITT) generalizable to the entire population in the sample (Imbens and Angrist, 1994).

Second, individual-level determinants of support can be estimated by using linear regressions. While the experimental part rests on clean causal identification, the exploration of mechanisms has a correlational character.

5.3.5 Inference

Mean comparison Inference relies on random assignment based on the experimental design. To assess treatment effects, I first compared the means between the three groups:

$$\Delta = \bar{X}_1 - \bar{X}_2 \tag{5.1}$$

This design allows me to test the null hypothesis that the treatment had no effect, that is, $Y_i^1 = Y_i^0$ for all respondents i and potential outcomes Y_i^1, Y_i^0. To explore whether support for guaranteed jobs and guaranteed income is mutually exclusive or complementary, and how it relates to pay levels, I cross-tabulated support for both policies and compared mean support across treatment groups.

Logistic regression To increase the precision of the treatment effect and to explore underlying individual-level determinants, I then estimated parametric logistic regressions for the treatment effects, using the following estimation equation:

$$\log\left(\frac{p}{1-p}\right) = \beta_0 + \beta_1 ... \beta_x + \alpha_{\text{group}} + \mathbf{X}_{\text{control}} \tag{5.2}$$

[5] I controlled for the share of women to adjust for the imbalance resulting from the group assignment.

where $\beta_1 ... \beta_x$ include the independent variables health status, unemployment duration, unemployment benefits, occupational prestige, discrimination, and stigma, α treatment group fixed effects, and $\mathbf{X}$ a vector of demographic controls. Following the established conventions in research on welfare preferences, I controlled for respondents' demographic characteristics including gender, age, and migration background.

5.4 Findings

The results highlight the influence of pay levels on respondent support for guaranteed job and guaranteed income schemes (Section 5.4.1), the relationship between support for either (Section 5.4.2), and respondents' willingness to accept jobs provided under a scheme (Section 5.4.3).

5.4.1 Support for guaranteed jobs and guaranteed income

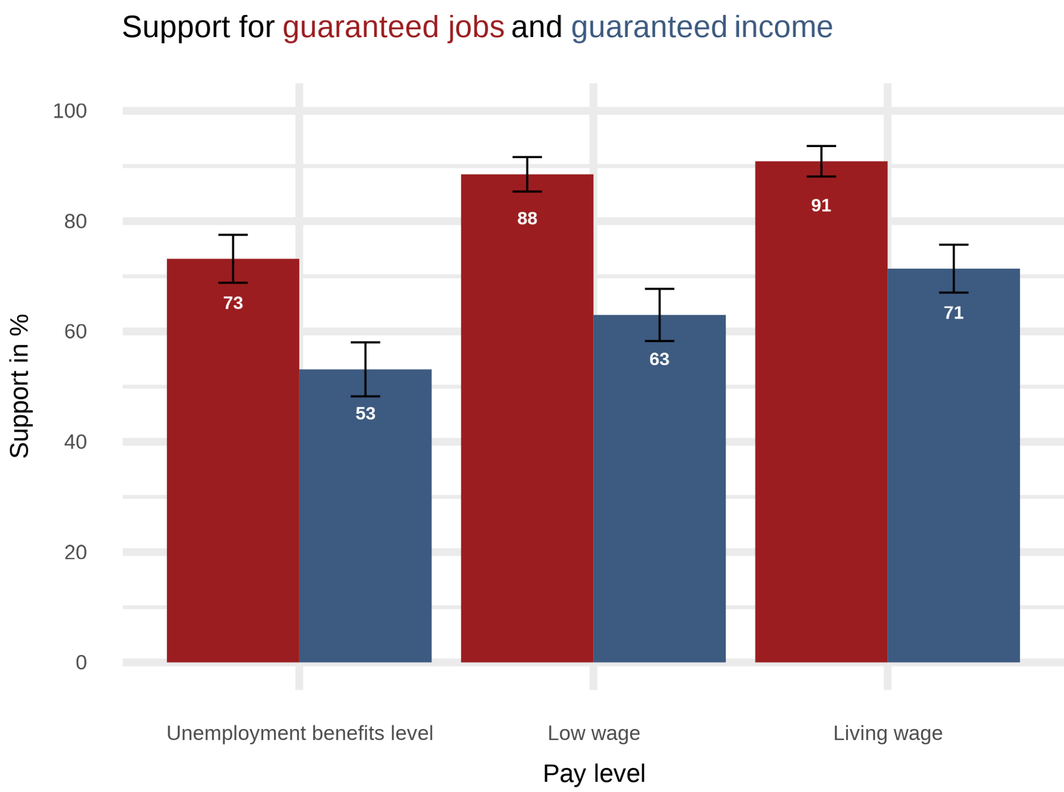
Mean comparison Figure 5.1 shows the support for guaranteed jobs and guaranteed income according to each scheme's pay level. The red bars (left-hand side) represent the percentage of respondents who support guaranteed jobs, and the blue bars (right-hand side) represent the percentage of respondents who support guaranteed income. The black skewers represent the 95% confidence intervals for these percentages.

Findings The results show broad support for both schemes, though it is consistently higher for guaranteed jobs and a significant increase in support for either scheme at higher pay levels.

First, the results indicate widespread support for both guaranteed jobs and guaranteed income across all pay levels. On average across pay levels, support amounts to 84% for guaranteed jobs and 62% for guaranteed income. At any pay level, support for guaranteed jobs is consistently higher than support for guaranteed income with an average support gap of 22%. Only at the living-wage pay level does the level of support for guaranteed income roughly correspond to the level of support for guaranteed jobs at the unemployment benefits pay level.

Second, the results show that support for both guaranteed jobs and guaranteed income increases with pay level. Support for guaranteed jobs starts at 73% of the unemployment benefits level and escalates to 88% at the low-wage pay level, just below its peak at 91%, when the pay matches the living wage. Conversely, support for guaranteed income begins at 53% of the level of unemployment benefits, increases to 63% at the low-wage pay level, and peaks at 71% when the pay is aligned with the living wage. The effects of €500- and €1000-increases in pay yield significant differences across schemes and pay levels. Moving from the unemployment benefits pay level to the low-wage pay level results in a 15 percentage point increase in support for guaranteed jobs and a 10% percentage point increase for guaranteed income. This trend of a stronger increase for guaranteed jobs compared to guaranteed income reverses upon reaching the living-wage pay level, where both schemes see an 18 percentage point rise in support relative to the unemployment benefit pay level.

Fig. 5.1: Support for guaranteed jobs and guaranteed income



Note: The red bars (left-hand side) represent the percentage of respondents who support guaranteed jobs, and the blue bars (right-hand side) represent the percentage of respondents who support guaranteed income. The black skewers represent confidence intervals at the 95% level. Percentages within each group do not sum up to 100 as responses for "I don't know" and "No response" are omitted.

Controlling for observables Table 5.4 shows the effects of the low-wage treatment and the living-wage treatment compared to the unemployment benefits level treatment, which is used as the control group. Coefficients of the logit regressions show log odds, which are transformed into odds ratios in the text below for easier interpretation. Specifications (1) and (4) show the baseline specification for guaranteed jobs and guaranteed income, respectively. Specifications (2) and (5) control for demographic characteristics, further complemented with socio-economic and personal experience controls in specifications (3) and (6). Unemployment benefits, occupational prestige, and unemployment duration are summarized under socio-economic controls, while discrimination and stigma are summarized under personal experience controls.

Findings Higher pay levels for both guaranteed jobs and guaranteed income significantly enhance support across all specifications, indicating a robust and strong treatment effect.

Although the coefficients for guaranteed jobs of both the low-wage and living-wage groups are roughly double those for guaranteed income, the increases from the baseline are of similar magnitudes, considering that baseline support for guaranteed jobs is higher.

Notably, the living-wage group exhibits a stronger treatment effect compared to the low-wage group: about 25–40% larger for guaranteed jobs and twice as large for guaranteed income. Support for guaranteed income rises linearly with every €500 increment in pay, yet the initial boost to the low-wage pay level prompts a comparatively larger surge in support for guaranteed jobs than an additional €500 increase to the living-wage pay level.

Robustness The effects' magnitudes remain unchanged when moving from the baseline specifications (1 and 4) to those including demographic controls (2 and 5). This is reassuring, considering the initial imbalance in women across groups (as discussed in Section 5.3.3). Adding socio-economic and personal experience controls yields slightly larger treatment effects for guaranteed jobs, while their direction and significance remain unchanged.[6] Overall, the estimated treatment effects with linear controls (5.4) yield consistent results with the simple mean comparison (5.1), which confirms the validity of the experimental design.

Table 5.4: Support for guaranteed jobs and guaranteed income

	Guaranteed jobs			Guaranteed income		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-wage	1.04***	1.02***	1.13***	0.41***	0.40***	0.39***
	(0.00)	(0.01)	(0.04)	(0.00)	(0.01)	(0.06)
Living-wage	1.29***	1.27***	1.60***	0.79***	0.79***	0.86***
	(0.00)	(0.01)	(0.02)	(0.00)	(0.01)	(0.05)
N	1215	1215	1061	1215	1215	1061
Demographic controls		x	x		x	x
Socio-economic controls			x			x
Personal experience control			x			x
R2	0.05	0.05	0.14	0.02	0.02	0.06

Note: Robust standard errors are clustered at the treatment group level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Demographic controls include gender, age, and migration background. Socio-economic controls include unemployment benefits, occupational prestige, and unemployment duration. Personal experience controls include experienced discrimination and stigma awareness. Treatment groups refer to a monthly payment of €1,000, €1,500, and €2,000.

5.4.2 Complementary or oppositional public support

Let us now explore whether support for guaranteed jobs or guaranteed income constitutes opposing or complementary attitudes. Table 5.5 presents a cross-tabulation of support for guaranteed jobs or guaranteed income as a share of responses per pay level.

Findings Table 5.5 illustrates that a majority of respondents support both policies, indicating a complementary rather than oppositional sentiment. This trend of mutual support strengthens progressively with higher pay levels, suggesting mutually reinforcing support among respondents.

The second-largest group consists of respondents who show support for guaranteed

[6] The identification based on random assignment, but controlling for observables can yield more precise estimates of the treatment effect and may adjust for any imbalances not accounted for by randomization.

jobs but not for guaranteed income. Their share remains relatively constant at 26% for the unemployment benefits pay level, 29% for the low-wage pay level, and 21% for the living-wage pay level.

Third in size is the group who does not support either policy. While their share amounts to 21% at the unemployment benefit pay level, it drops markedly to around 8% at the relatively higher pay levels. The increase from the low-wage to living-wage pay level barely reduces the share of this group further, indicating a threshold beyond which further increases in pay levels do not substantially diminish the core opposition.

A minimal subset of the cohort exhibits support for guaranteed income while withholding support for guaranteed jobs, with their proportion dwindling from 6% at the lowest pay level to 1% at the highest.

Overall, the results show that increasing the pay level weakens opposition among unemployed workers, with core opposition to any policy remaining small and relatively stable, oscillating around 8%.

Table 5.5: Support across pay levels

	Support for guaranteed income								
	UB level			Low wage			Living wage		
		Yes	No		Yes	No		Yes	No
Support for guaranteed jobs	Yes	46.9	25.3	Yes	60.0	28.2	Yes	70.0	20.2
	No	5.5	19.5	No	2.5	7.5	No	1.2	7.5

Note: Percentages within each group do not sum up to 100 as responses for "I don't know" and "No response" are omitted. UB level refers to the unemployment benefits level.

5.4.3 Willingness to accept a guaranteed job

Figure 5.2 illustrates the willingness to accept a job under a job guarantee scheme at varying pay levels. The bars represent percentages of respondents: green for those who are willing to accept a guaranteed job, red for those who are not willing to accept it, and grey for those who do not know. The black skewers represent the 95% confidence intervals.

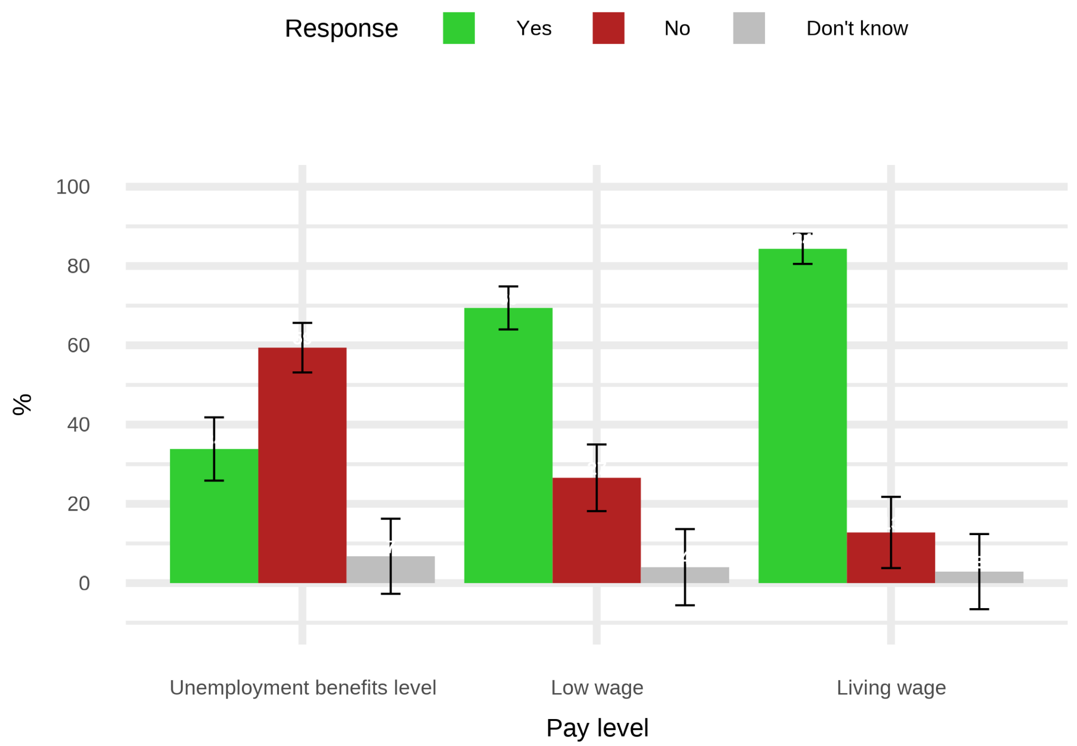
Findings First, the willingness to accept a job under the scheme rises with higher amounts offered. At the lowest pay level, the majority of respondents indicated that they would not accept such a job, compared to about 1/3 who would accept a job. When the pay level increased by €500, the pattern of responses shifted markedly, with affirmative responses surging to just under 3/4 and negative responses diminishing to around 1/4. At the living-wage pay level, the proportion of affirmative responses reached its peak, surpassing 3/4, whereas the negative responses decreased further. Interestingly, the proportion of uncertain responses remained relatively stable across the different pay levels, indicating a consistent level of uncertainty or indecision among a subset of respondents.

Second, the results indicate a massive treatment effect at a critical threshold between the unemployment benefits pay level and the low-wage pay level, beyond which the likelihood of accepting a job under the scheme significantly increases. In particular, the willingness to accept a guaranteed job experiences a much larger increase at the threshold compared to the increase in general support (cf. Section 5.4.1). While raising the pay level from the unemployment benefits pay level to the low-wage pay level increases support for guaranteed jobs by 15%, from 73% to 88%, it increases the willingness to accept a job by around 40%, from 30% to 70%. Overall, the figure conveys a clear effect of the pay level on job acceptance.

Support and willingness To explore to what degree support for and willingness to accept a guaranteed job are conditional on each other, Table 5.6 presents a cross-tabulation of the two outcomes per pay level.

Findings At the lowest pay level, the share of respondents who support guaranteed jobs but would not accept them is about 1/3. The share of respondents who support and would accept a guaranteed job doubles to 2/3 at the low-wage pay level and culminates at 82.5% at the living-wage pay level. As would be expected, hardly anyone would accept a guaranteed job while not supporting the policy, though the share rises from 0.8% at the lowest pay level to 1.7% at the highest pay level. The results illustrate that the vast

Fig. 5.2: Willingness to accept a guaranteed job



Note: The bars represent the percentages of respondents who are willing to accept a guaranteed job (green bars, left-hand side), are not willing to accept a guaranteed job (red bars, middle), and who do not know (grey, right-hand side). Observations with "No response" are omitted. The black skewers represent confidence intervals at the 95% level.

majority of respondents who support guaranteed jobs would also accept a guaranteed job at a sufficient pay level.

Table 5.6: Job guarantee support and acceptance across pay levels

	Willingness to accept a guaranteed job								
	UB level			Low wage			Living wage		
		Yes	No		Yes	No		Yes	No
Support for guaranteed jobs	Yes	33.1	34.6	Yes	68.2	16.2	Yes	82.5	6.2
	No	0.8	24.1	No	1.0	9.0	No	1.7	6.2

Note: Percentages within each group do not sum up to 100 as responses for "I don't know" and "No response" are omitted. UB level refers to the unemployment benefits level.

5.5 Mechanisms

The analysis of mechanisms explores the individual-level determinants of support for each policy (Section 5.5.1), identifies supporters and opponents (Section 5.5.2), and examines the motivation to accept a guaranteed job (Section 5.5.3). Results should be interpreted correlational since I cannot rule out endogeneity and omitted variable bias.

5.5.1 Determinants of support

Table 5.7 presents individual-level determinants of support for guaranteed jobs and guaranteed income policies with treatment group fixed effects. Logistic regressions estimated the likelihood of supporting either policy on a binary variable with a gradually increasing number of covariates from equations 1–3 and 4–6. The effect of each treatment group remains significantly positive with its magnitude unaffected by the set of covariates.

Guaranteed jobs For guaranteed jobs, people with a poorer health status show a higher probability of supporting guaranteed jobs. The duration of unemployment has a consistently negative association, indicating that longer periods of unemployment are linked to lower support for guaranteed jobs. Notably, higher unemployment benefits are connected to lower support for guaranteed jobs. Discrimination experienced by respondents is positively associated with support, indicating that those facing workplace discrimination are more likely to favor guaranteed jobs as a policy solution. Higher occupational prestige is generally associated with lower support though not statistically significant.

Guaranteed income For guaranteed income, people with a poorer health status are more supportive. The duration of unemployment is not significantly associated with support for guaranteed income across the specifications. The level of actual unemployment benefits received and occupational prestige as well as experienced job discrimination show negative but not significant associations.

Among demographic controls, only gender shows significant associations. Women are more likely to support guaranteed jobs and less likely to support guaranteed income. Age tends to go along with lower support for either policy, whereas having a migration background shows no association with support for guaranteed jobs but a positive association with support for guaranteed income.

The results underscore different dimensions of beneficiaries' support for guaranteed jobs versus guaranteed income. Both policies see an uptick in support from people with a poorer health status, suggesting that even among primary beneficiaries, those potentially benefiting most show the strongest support. The duration of unemployment has a nuanced association. It is linked to reduced support for guaranteed jobs while there is no link with the sentiment toward guaranteed income, potentially reflecting a waning attachment to the labor market with prolonged unemployment spells. Remarkably, the level of unemployment benefits is inversely associated with support for guaranteed jobs, indicating that those with lower previous earnings are more inclined toward the program. The analysis also highlights a threshold effect: unemployed workers receiving less than €1,000 per month show a marked preference for guaranteed jobs, while the preference toward guaranteed income becomes noticeable below €450 per month. The result on occupational prestige suggests that those who used to have jobs with higher prestige may lean away from supporting such programs, though at non-significant levels. Furthermore, occupational prestige is partially accounted for by controlling for the benefit level, which is based on previous earnings and correlates strongly with occupational prestige. Discrimination experiences go hand-in-hand with support for guaranteed jobs and guaranteed income. These patterns suggest that overall more disadvantaged unemployed workers are more likely to support either policy. Longer unemployment spells, as an exception, are associated with a lower likelihood of supporting guaranteed jobs, suggesting a waning attachment to the labor market the longer people are unemployed.[7]

[7] While also linked to disadvantage, unemployment duration increases lethargy over time, which results in a waning desire to return to paid employment as people grow more accustomed to their situation (Jahoda, 1984).

Table 5.7: Support for guaranteed jobs and guaranteed income

	Guaranteed jobs			Guaranteed income		
	(1)	(2)	(3)	(4)	(5)	(6)
Health status (-)		2.46*	2.44*		1.36**	1.45**
		(1.12)	(1.06)		(0.51)	(0.47)
Unemployment duration		−0.02+	−0.02+		0.01	0.02
		(0.01)	(0.01)		(0.02)	(0.02)
Unemployment benefits		−0.58***	−0.45***		−0.12	−0.15
		(0.08)	(0.08)		(0.19)	(0.20)
Occupational prestige		−1.26	−1.26		−0.72	−0.64
		(1.51)	(1.50)		(0.89)	(1.00)
Discrimination	2.04***	0.84	1.00+	1.25*	0.37	0.39
	(0.23)	(0.60)	(0.60)	(0.50)	(0.66)	(0.61)
N	1215	1109	1109	1215	1109	1109
Group fixed effects	x	x	x	x	x	x
Demographic controls			x			x
R2	0.069	0.123	0.127	0.03	0.046	0.052

Note: Robust standard errors are clustered at the treatment group level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Demographic controls include gender, age, and migration background. Treatment groups refer to a monthly payment of €1,000 , €1,500, and €2,000.

5.5.2 Identifying supporters and opponents

Who constitutes the approximate quarter supporting guaranteed jobs while opposing guaranteed income? Table 5.8 details the average characteristics of this group compared to those with differing opinions.

Supporters of guaranteed jobs who oppose guaranteed income have the shortest unemployment spell duration compared to others. In other aspects, such as health status, occupational prestige of their previous job, experienced discrimination, and stigma awareness, their characteristics are relatively similar to other groups. Their average unemployment benefits match those who support both policies. They are more likely to identify as women compared to those who oppose guaranteed jobs but align closely with the gender distribution of those who support both policies.

Conversely, the small group opposing guaranteed jobs while favoring guaranteed income experiences the longest spells of unemployment among all participants, yet they face the least stigma from being unemployed. Furthermore, the comparison indicates that support for guaranteed income is less common among women, consistent with results based on the ESS (Weisstanner, 2022).

Table 5.8: Support group characteristics

	For JG & BI	For JG, Against BI	Against JG, For BI	Against JG & BI
Health status (-)	0.22	0.17	0.15	0.11
Unemployment duration	10.88	8.50	16.24	10.49
Unemployment benefits	897.47	897.88	947.64	1012.51
Occupational prestige	0.32	0.35	0.37	0.41
Discrimination	0.15	0.10	0.06	0.06
Stigma	0.42	0.32	0.28	0.33
Women	0.52	0.56	0.32	0.46
Age	40.66	40.24	42.89	40.09
Migration Background	0.31	0.24	0.35	0.24
N	718	298	37	139

Note: Percentages within each group do not sum up to 100 as responses for “I don’t know” and “No response” are omitted. JG refers to the job guarantee scheme and BI refers to the basic income scheme.

To understand the profile of those consistently opposed to both guaranteed jobs and guaranteed income, even at low-wage and living-wage pay levels, we turn to Table 5.9. This table delineates the average characteristics of respondents based on their support for each policy within the low wage and living wage categories.

The people consistently opposing both guaranteed jobs and guaranteed income, even at low-wage and living-wage pay levels, are generally better off. They have a better health status and hold a superior socio-economic position compared to other groups, characterized by shorter durations of unemployment, higher unemployment benefits, and greater occupational prestige in their previous job. Additionally, they report having experienced less discrimination in the labor market and tend to experience unemployment as less stigmatizing. Demographically, they are comparatively similar to others.

Table 5.9: Support group characteristics, low-wage and living-wage groups

	For JG & BI	For JG, Against BI	Against JG, For BI	Against JG & BI
Health status (-)	0.22	0.16	0.12	0.11
Unemployment duration	11.62	7.59	14.80	7.69
Unemployment benefits	899.21	889.91	914.79	1047.79
Occupational prestige	0.32	0.35	0.38	0.53
Discrimination	0.14	0.12	0.02	0.06
Stigma	0.43	0.31	0.26	0.24
Women	0.53	0.60	0.47	0.48
Age	40.69	40.91	45.13	40.26
Migration Background	0.32	0.26	0.27	0.34
N	531	197	15	61

Note: Percentages within each group do not sum up to 100 as responses for "I don't know" and "No response" are omitted. JG refers to the job guarantee scheme and BI refers to the basic income scheme.

5.5.3 Determinants of willingness to accept a guaranteed job

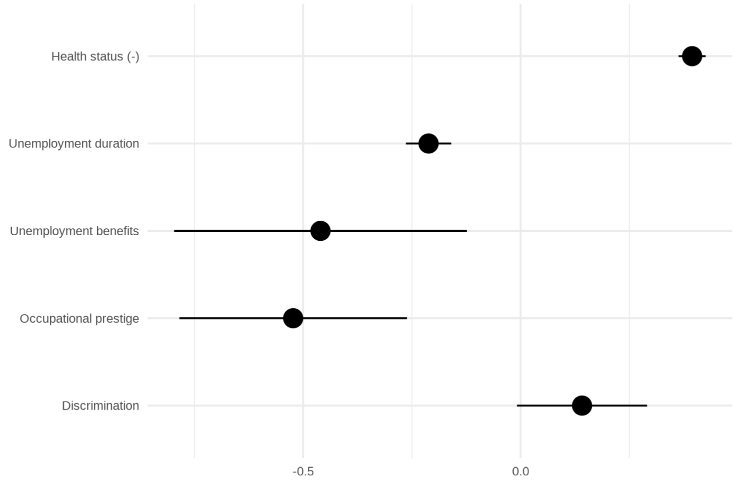
Table 5.10 presents the determinants of willingness to accept a guaranteed job based on socio-economic circumstances and past labor market experiences with treatment group fixed effects. Logistic regressions estimated the association between the willingness to accept a job under the proposed job guaranteed scheme with a gradually increasing number of covariates from equations 1–4. Standardized coefficients are shown in equation (5) and Figure 5.3 . As for support, the effect of each treatment group on willingness to accept a guaranteed job remains significantly positive with its magnitude unaffected by the set of controls.

Findings Disadvantaged people who are less likely to be able to return to a regular job emerge as those with a higher willingness to accept a guaranteed job. The exception is unemployment duration, likely due to a diminishing desire to return to paid employment as workers become more accustomed over time to being without a job. Unemployment duration stands out as the independent variable particular susceptible to reverse causality:

people unwilling to engage in paid employment may consequently remain longer unemployed.

First, health status emerges as a pivotal factor, with a deteriorating condition markedly increasing the likelihood of accepting a guaranteed job. Second, the duration of unemployment exhibits a significant negative correlation, with the willingness to accept a guaranteed job consistent across the specifications. Third, people on higher unemployment benefits show significantly less willingness to accept guaranteed jobs across the specifications. Conversely, people on lower unemployment benefits are more likely to accept guaranteed jobs. Fourth, occupational prestige negatively correlates with the willingness to accept guaranteed jobs, indicating a preference for maintaining social and professional status over the security offered by such programs. Fifth, experienced discrimination shows a positive and statistically significant association that weakens when controlling for socio-economic and demographic characteristics. This reduction in significance suggests that while discrimination experiences might drive unemployed workers toward the stability of guaranteed jobs, the impact may be mediated by other factors. Comparing the relative importance with standardized coefficients reveals occupational prestige and unemployment benefits as the strongest predictors followed by health status, unemployment duration, and experienced discrimination (Figure 5.3 and Table 5.10 equation (5)). The overall consistent pattern of higher willingness to accept guaranteed jobs among persons who are less likely to return to regular employment, with the exception of unemployment duration, underscores the willingness to work among the most vulnerable groups of unemployed workers.

Fig. 5.3: Willingness to accept a guaranteed job



Note: The figure shows standardized coefficients controlled for group fixed effects and demographic characteristics. Confidence intervals are at the 95% level. The figure corresponds to equation (5) in Table 5.10.

Table 5.10: Willingness to accept a guaranteed job

	(1)	(2)	(3)	(4)	(5)
Health status (-)		1.93***	2.02***	2.01***	0.39***
		(0.19)	(0.10)	(0.08)	(0.02)
Unemployment duration			−0.02***	−0.02***	−0.21***
			(0.00)	(0.00)	(0.03)
Unemployment benefits			−1.17**	−1.14**	−0.46**
			(0.40)	(0.43)	(0.17)
Occupational prestige			−2.67***	−2.55***	−0.52***
			(0.55)	(0.65)	(0.13)
Discrimination	1.71***	0.86***	0.60*	0.64+	0.14+
	(0.24)	(0.23)	(0.30)	(0.34)	(0.08)
N	1215	1213	1109	1109	1109
Group fixed effects	x	x	x	x	x
Demographic controls				x	x
R2	0.165	0.178	0.261	0.265	0.265

Note: Robust standard errors are clustered at the treatment group level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Demographic controls include gender, age, and migration background. Treatment groups refer to a monthly payment of €1,000 , €1,500, and €2,000.

5.6 Conclusion

The study presents three key takeaways regarding unemployed workers' perception of guaranteed job and guaranteed income schemes. First, there is strong support for both policies, though a discernible inclination toward guaranteed jobs becomes apparent. Differences in support levels may reflect unemployed workers' desire to work and preferences for participation requirements, which is apparent across different remuneration levels. Second, an increase in pay leads to heightened support for both policies and a greater willingness to partake in guaranteed jobs, especially at a critical threshold just above the average unemployment benefits, suggesting a high willingness to work for little monetary benefit. Third, contrary to a prevalent assumption of mutual exclusivity, both findings reveal a complementary perspective among unemployed workers, with increased pay fostering an attitude that is supportive of both guaranteed jobs and guaranteed income. The investigation into mechanisms suggests that vulnerable socio-economic conditions and negative personal experiences go hand-in-hand with higher support for and willingness to accept guaranteed jobs. Predictors include socio-economic factors such as adverse health conditions and low unemployment benefits as well as experienced labor market discrimination.

Outlook As Europe witnesses an expansion of guaranteed job and guaranteed income pilot programs, bolstered by newfound support from the European Union, the horizon for social policy innovation brightens. This burgeoning interest heralds a promising wave of research to better understand, dissect, and compare the nuanced attitudes toward these transformative policies. Future studies would be well-advised to delve into 1) the impact of different policy designs on support by using factorial survey designs, 2) the variation in support across welfare state models, 3) the consequences of correcting misperceptions and framing policies in various ways, and 4) the perspectives of diverse beneficiary groups and changes in support over their life course. This endeavor warrants improving our theoretical understanding of welfare state support. However, it is not merely academic as it will be crucial for informing public discourse and guiding the development of effective welfare state policies. The choices we make today to better understand these policies will (hopefully) inform the path for their implementation, eventually creating more inclusive and adequate welfare states for the future. This study demonstrates that guaranteed jobs and guaranteed income can be considered complementary, rather than opposing, strategies for enhancing the social safety net.

Bibliography

AMS (2021). Übersicht über den Arbeitsmarkt. Juni 2021. Technical report, Arbeitsmarktservice Österreich, Vienna.

Ashenfelter, O. and Plant, M. W. (1990). Nonparametric Estimates of the Labor-Supply Effects of Negative Income Tax Programs. *Journal of Labor Economics*, 8(1, Part 2):S396–S415.

Atkinson, A. B. (1996). The Case for a Participation Income. *The Political Quarterly*, 67(1):67–70.

Baird, S., McIntosh, C., and Özler, B. (2011). Cash or Condition? Evidence from a Cash Transfer Experiment. *The Quarterly Journal of Economics*, 126(4):1709–1753.

Burtless, G. and Hausman, J. A. (1978). The Effect of Taxation on Labor Supply: Evaluating the Gary Negative Income Tax Experiment. *Journal of Political Economy*, 86(6):1103–1130.

Buß, C., Ebbinghaus, B., and Naumann, E. (2017). Making deservingness of the unemployed conditional: Changes in public support for the conditionality of unemployment benefits. *The social legitimacy of targeted welfare: Attitudes to welfare deservingness*, pages 167–88.

Calnitsky, D. and Latner, J. P. (2017). Basic Income in a Small Town: Understanding the Elusive Effects on Work. *Social Problems*, 64(3):373–397.

Chrisp, J., Pulkka, V.-V., and García, L. R. (2020). Snowballing or wilting? What affects public support for varying models of basic income? *Journal of International and Comparative Social Policy*, 36(3):223–236.

De Venanzi, A. (2020). Can Latin America learn from India's efforts at fighting poverty? The case of the Mahatma Gandhi National Rural Employment Guarantee Act. *Journal of International and Comparative Social Policy*, 36(2):159–174.

Ebbinghaus, B. (2020). Changing work and welfare: Unemployment and labour market policies. In *Handbook on Society and Social Policy*, chapter Handbook on Society and Social Policy, pages 291–305. Edward Elgar Publishing.

Ebbinghaus, B., Lehner, L., and Naumann, E. (2022). Welfare state support during the COVID-19 pandemic: Change and continuity in public attitudes towards social policies in Germany. *European Policy Analysis*, 8(3):297–311.

Ebbinghaus, B. and Naumann, E., editors (2018). *Welfare State Reforms Seen from Below*. Springer International Publishing, Cham.

Ehmke, E. (2016). India's Mahatma Gandhi National Rural Employment Act: Assessing the quality of access and adequacy of benefits in MGNREGS public works. *International Social Security Review*, 69(2):3–27.

ESS (2016). European Social Survey European Research Infrastructure (ESS ERIC). ESS8 Data Documentation. Sikt - Norwegian Agency for Shared Services in Education and Research.

Fraile, M. and Ferrer, M. (2005). Explaining the Determinants of Public Support for Cuts in Unemployment Benefits Spending across OECD Countries. *International Sociology*, 20(4):459–481.

Freeman, R. B. and Rogers, J. (1999 (2006)). *What Workers Want*. ILR Press/Cornell University Press.

Friedman, M. (1962). Alleviation of Poverty. In *Capitalism and Freedom*, pages 7–17. University of Chicago Press.

Gingrich, J. and Ansell, B. (2012). Preferences in Context: Micro Preferences, Macro Contexts, and the Demand for Social Policy. *Comparative Political Studies*, 45(12):1624–1654.

Haushofer, J. and Shapiro, J. (2016). The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *The Quarterly Journal of Economics*, 131(4):1973–2042.

Hum, D. and Simpson, W. (1993). Economic Response to a Guaranteed Annual Income: Experience from Canada and the United States. *Journal of Labor Economics*, 11(1):S263–S296.

Imbens, G. W. and Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 62(2):467.

Jahoda, M. (1984). Braucht der Mensch die Arbeit? In *Frank Niess (ed.) Leben wir, um zu arbeiten? Die Arbeitswelt im Umbruch*, pages 11–17. Bund-Verl, Köln.

Jones, D. and Marinescu, I. (2022). The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund. *American Economic Journal: Economic Policy*, 14(2):315–340.

Kangas, O. E. (1997). Self-interest and the common good: The impact of norms, selfishness and context in social policy opinions. *The Journal of Socio-Economics*, 26(5):475–494.

Kasy, M. and Lehner, L. (2023). Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program. *Working Paper*.

Kasy, M., Schwerter, F., Fiedler, S., Schupp, J., and Bohmann, S. (2024). Experimental evaluation of a Basic Income Pilot in Germany.

Kershaw, D., Fair, J., Watts, H. W., and Rees, A., editors (1976). *The New Jersey Income-Maintenance Experiment, Volume 2*. Institute for Research on Poverty Monograph Series. Academic Press, New York.

Kochan, T. A. (1979). How American workers view labor unions. *Monthly Labor Review*, 102(4):23–31.

Kochan, T. A., Yang, D., Kimball, W. T., and Kelly, E. L. (2019). Worker Voice in America: Is There a Gap between What Workers Expect and What They Experience? *ILR Review*, 72(1):3–38.

Laenen, T. (2023). *The Popularity of Basic Income: Evidence from the Polls*. Exploring the Basic Income Guarantee. Springer International Publishing, Cham.

Laenen, T., Van Hootegem, A., and Rossetti, F. (2023). The multidimensionality of public support for basic income: A vignette experiment in Belgium. *Journal of European Public Policy*, 30(5):849–872.

Lafargue, P. (1883). *The Right to Be Lazy and Other Writings.* New York Review Books Classics. Labour Literature Society (New York Review Books, 2023), New York.

Langer, M., Hafner, A., Urban, J., Schwertgen, D., and Theocharis, I. (2023). European perceptions of public programmes for zero unemployment. Policy Brief, FEPS, Brussels.

Lehner, L. and Tamesberger, D. (2024). Unemployment and Labor Market Policy. In *Handbook of Social Infrastructure.* Edward Elgar Publishing.

Markowitsch, J. and Scharle, Á. (2024). Towards zero long-term unemployment in the EU: Job guarantees and other innovative approaches. ESF+ Community of Practice on Employment, Education and Skills, European Commission.

More, T. (1516 (2016)). *Utopia.* Verso, London New York.

Muralidharan, K., Niehaus, P., and Sukhtankar, S. (2023a). General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India. *Econometrica*, 91(4):1261–1295.

Muralidharan, K., Romero, M., and Wüthrich, K. (2023b). Factorial Designs, Model Selection, and (Incorrect) Inference in Randomized Experiments. *The Review of Economics and Statistics*, pages 1–44.

Naumann, E., Buss, C., and Bähr, J. (2016). How Unemployment Experience Affects Support for the Welfare State: A Real Panel Approach. *ESR*, 32(1):81–92.

Parliament, E. (2023). Job creation – the just transition and impact investments. European Parliament resolution of 23 November 2023 on job creation – the just transition and impact investments (2022/2170(INI)).

Prainsack, B. (2020). *Vom Wert des Menschen: warum wir ein bedingungsloses Grundeinkommen brauchen.* Brandstätter, Wien, 1. auflage edition.

Rehm, P., Hacker, J. S., and Schlesinger, M. (2012). Insecure Alliances: Risk, Inequality, and Support for the Welfare State. *American Political Science Review*, 106(2):386–406.

Rincon, L. (2023). A Robin Hood for all: A conjoint experiment on support for basic income. *Journal of European Public Policy*, 30(2):375–399.

Rincón, L., Vlandas, T., and Hiilamo, H. (2022). What's not to like? Benefit design, funding structure and support for universal basic income. *Journal of European Social Policy*, 32(4):467–483.

Robins, P. K. (1985). A Comparison of the Labor Supply Findings from the Four Negative Income Tax Experiments. *The Journal of Human Resources*, 20(4):567–582.

Romeo, N. (2023). *The Alternative: How to Build a Just Economy.* PublicAffairs, New York, first edition edition.

Roosma, F. and van Oorschot, W. (2020). Public opinion on basic income: Mapping European support for a radical alternative for welfare provision. *Journal of European Social Policy*, 30(2):190–205.

Schönherr, D. (2021). Zur Situation von Arbeitslosen in Österreich 202. Technical report, SORA Institute for Social Research & Consulting., Vienna.

Schönherr, D. and Lehner, L. (2022). Causes and Consequences of Unemployment in the COVID-19 pandemic (SUF edition).

Stadelmann-Steffen, I. and Dermont, C. (2020). Citizens' Opinions About Basic Income Proposals Compared – A Conjoint Analysis of Finland and Switzerland. *Journal of Social Policy*, 49(2):383–403.

Standing, G. (2005). Why Basic Income Is Needed for a Right to Work Symposium: Basic Income Guarantees and the Right to Work. *Rutgers Journal of Law and Urban Policy*, 2(1):91–102.

Standing, G. (2017). *Basic Income: And How We Can Make It Happen.* Number 14 in A Pelican Introduction. Pelican, an imprint of Penguin Books, London.

Standing, G. (2018). Why a Job Guarantee is a bad joke for the precariat – and for freedom.

Statistics Austria (2024a). Arbeitslosengeld und Notstandshilfe. Arbeitsmarktservice Österreich. https://www.statistik.at/statistiken/bevoelkerung-und-soziales/sozialleistungen/arbeitslosengeld-und-notstandshilfe.

Statistics Austria (2024b). Lohnsteuer- und SV-Daten. Jährliche Personeneinkommen. Nettojahreseinkommen der ganzjährig Vollzeitbeschäftigten 2021. https://www.statistik.at/statistiken/bevoelkerung-und-soziales/einkommen-und-soziale-lage/jaehrliche-personeneinkommen.

Svallfors, S. (1997). Worlds of Welfare and Attitudes to Redistribution: A Comparison of Eight Western Nations. *European Sociological Review*, 13(3):283–304.

Tcherneva, P. R. (2012). The Job Guarantee: Delivering the Benefits That Basic Income Only Promises – A Response to Guy Standing. *Basic Income Studies*, 7(2):66–87.

Tcherneva, P. R. (2020). *The Case for a Job Guarantee*. Polity, Cambridge, UK; Medford, MA.

Treiman, D. J. (1977). *Occupational Prestige in Comparative Perspective*. Quantitative Studies in Social Relations. Academic Press, New York.

UN Special Rapporteur, D. S. O. (2023). The employment guarantee as a tool in the fight against poverty. Report of the Special Rapporteur on extreme poverty and human rights, Olivier De Schutter. United Nations General Assembly. Human Rights Council, Fifty-third session. Technical report, United Nations General Assembly. Human Rights Council, Fifty-third session.

van Oorschot, W. (2006). Making the difference in social Europe: Deservingness perceptions among citizens of European welfare states. *Journal of European Social Policy*, 16(1):23–42.

van Oorschot, W., Roosma, F., Meuleman, B., and Reeskens, T. (2017). *The Social Legitimacy of Targeted Welfare: Attitudes to Welfare Deservingness*, volume forthcoming. E. Elgar, Cheltenham.

Van Parijs, P. and Vanderborght, Y. (2017). Basic Income: A Radical Proposal for a Free Society and a Sane Economy. In *Basic Income*. Harvard University Press.

Verho, J., Hämäläinen, K., and Kanninen, O. (2022). Removing Welfare Traps: Employment Responses in the Finnish Basic Income Experiment. *American Economic Journal: Economic Policy*, 14(1):501–522.

Vlandas, T. (2019). The Politics of the Basic Income Guarantee: Analysing Individual Support in Europe. *Basic Income Studies*, 14(1).

Vlandas, T. (2021). The political economy of individual-level support for the basic income in Europe. *Journal of European Social Policy*, 31(1):62–77.

Watts, H. W. and Rees, A., editors (1977). *The New Jersey Income-Maintenance Experiment. Volume 3: Expenditures, Health, and Social Behavior; and the Quality of the Evidence / Ed. by Harold W. Watts*. Acad. Press, New York.

Weisstanner, D. (2022). COVID-19 and welfare state support: The case of universal basic income. *Policy and Society*, 41(1):96–110.

Appendix A

Appendix to "Employing the unemployed of Marienthal: Evaluation of a guaranteed job program"

A.1 Additional tables and figures

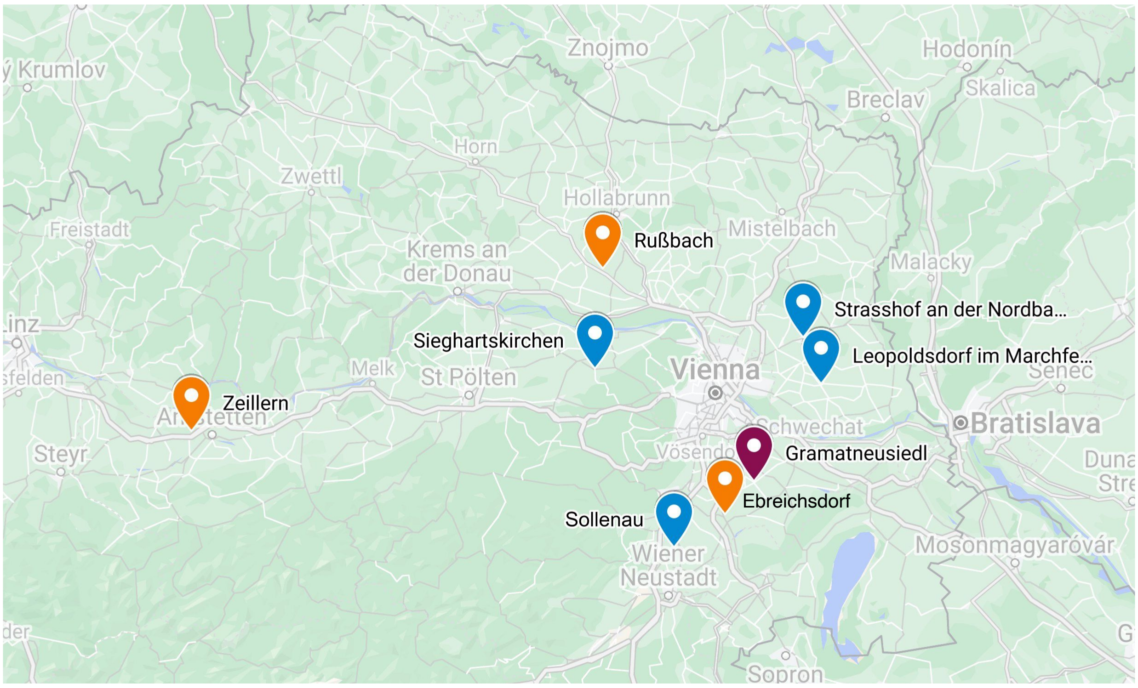
A.1.1 Synthetic control: Further details

Table A.1: Variables used for the construction of the synthetic control

Variable	Definition
Working age pop	Working age population.
Long term unemp/pop	Number of long-term unemployed (> 1 year) as a share of working age pop.
Inactive/pop	Number of inactive persons in working age as a share of working age pop.
Mean age	Mean age in years of the total population.
Share small firms	Small firms (less than 10 employees) as a share of total firms.
Share mid firms	Medium sized firms (10-249 employees) as a share of total firms.
Share low edu	Persons with low education (ISCED 1-2) as a share of total pop.
Share mid edu	Persons with medium education (ISCED 3-4) as a share of total pop.
Share men	Male persons as a share of total pop.
Share migrant	Persons with a migrant background as a share of total pop.
Share care resp	Active persons with care responsibilities as a share of total pop.
Mean wage	Mean wage level.
Mean age unemp	Mean age in years of the unemployed.
Low edu/unemp	Unemployed with low education (ISCED 1-2) as a share of total unemployed.
Mid edu/unemp	Unemployed with medium education (ISCED 3-4) as a share of total unemployed.
Poor German/unemp	Unemployed with low German skills (< A2 CEFR) as a share of total unemployed.
Men/unemp	Male unemployed as a share of total unemployed.
Migrant/unemp	Unemployed with a migrant background as a share of total unemployed.
Health cond/unemp	Unemployed with a medical condition limiting employment opportunities as a share of total unemployed.
Communal tax/pop	Communal tax per working age pop.

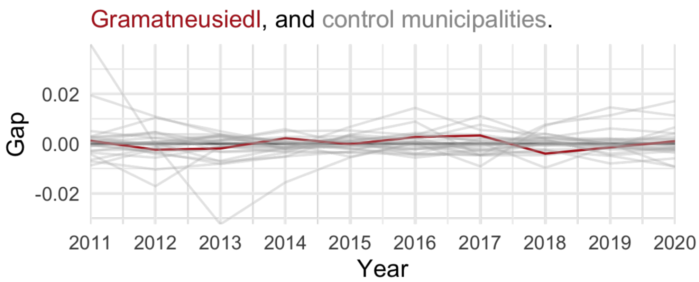
Notes: This table describes the variables used for the construction of the synthetic control municipality; cf. Table A.2.

Fig. A.1: Location of municipalities included in the synthetic control



Notes: Gramatneusiedl, the treated municipality, is marked in red. The 3 municipalities with the largest weights in the synthetic control are marked in orange. Municipalities with smaller weights are marked in blue.

Fig. A.2: Unemployment gap and permutation inference.



Notes: This figure shows the unemployment gap between Gramatneusiedl and its synthetic control (red), and between each of the 25 potential control municipalities and *their* synthetic control (grey). This figure parallels the second row of Figure 2.4, for the 10 years before the MAGMA program.

Table A.2: Gramatneusiedl and control municipality covariates

Variables observed in December 2019

Municipality	Working age pop	Long term unemp/pop	Inactive/pop	Mean age	Share small firms	Share mid firms	Share low edu
Gramatneusiedl	5013	0.007	0.220	50.775	0.115	0.339	0.208
Synthetic control	4830	0.016	0.228	51.074	0.126	0.363	0.225
Zeillern	1263	0.004	0.227	50.229	0.093	0.335	0.199
Ebreichsdorf	7655	0.020	0.228	50.810	0.139	0.381	0.235
Leopoldsdorf im Marchfelde	2035	0.022	0.247	51.304	0.135	0.348	0.242
Strasshof an der Nordbahn	6920	0.024	0.213	51.403	0.115	0.324	0.250
Rußbach	942	0.013	0.219	52.230	0.126	0.369	0.206
Sieghartskirchen	4560	0.010	0.224	52.464	0.135	0.337	0.197
Sollenau	5122	0.017	0.248	54.286	0.129	0.360	0.284

Municipality	Share mid edu	Share men	Share migrant	Share care resp	Mean wage	Mean age unemp	Low edu/unemp
Gramatneusiedl	0.642	0.511	0.242	0.257	3416	42.694	0.530
Synthetic control	0.644	0.503	0.181	0.235	3293	43.422	0.452
Zeillern	0.702	0.509	0.053	0.256	3168	40.462	0.346
Ebreichsdorf	0.620	0.498	0.234	0.235	3379	44.344	0.465
Leopoldsdorf im Marchfelde	0.619	0.498	0.260	0.216	3294	43.627	0.513
Strasshof an der Nordbahn	0.600	0.496	0.276	0.257	3393	42.364	0.465
Rußbach	0.676	0.513	0.088	0.224	3137	45.500	0.525
Sieghartskirchen	0.641	0.510	0.195	0.206	3366	41.257	0.387
Sollenau	0.608	0.496	0.229	0.193	3235	41.819	0.521

Municipality	Mid edu/unemp	Poor German/unemp	Men/unemp	Migrant/unemp	Health cond/unemp	Communal tax/pop	Lt ue/pop 2020
Gramatneusiedl	0.455	0.082	0.627	0.418	0.245	57.281	0.009
Synthetic control	0.516	0.061	0.583	0.312	0.264	217.301	0.018
Zeillern	0.654	0.000	0.692	0.115	0.303	97.822	0.004
Ebreichsdorf	0.480	0.086	0.546	0.374	0.213	282.242	0.022
Leopoldsdorf im Marchfelde	0.473	0.093	0.573	0.467	0.256	284.806	0.023
Strasshof an der Nordbahn	0.496	0.089	0.528	0.472	0.303	160.549	0.027
Rußbach	0.475	0.025	0.575	0.200	0.375	97.079	0.016
Sieghartskirchen	0.552	0.054	0.609	0.360	0.281	329.855	0.012
Sollenau	0.460	0.140	0.558	0.457	0.282	308.998	0.019

Variables observed in July 2020

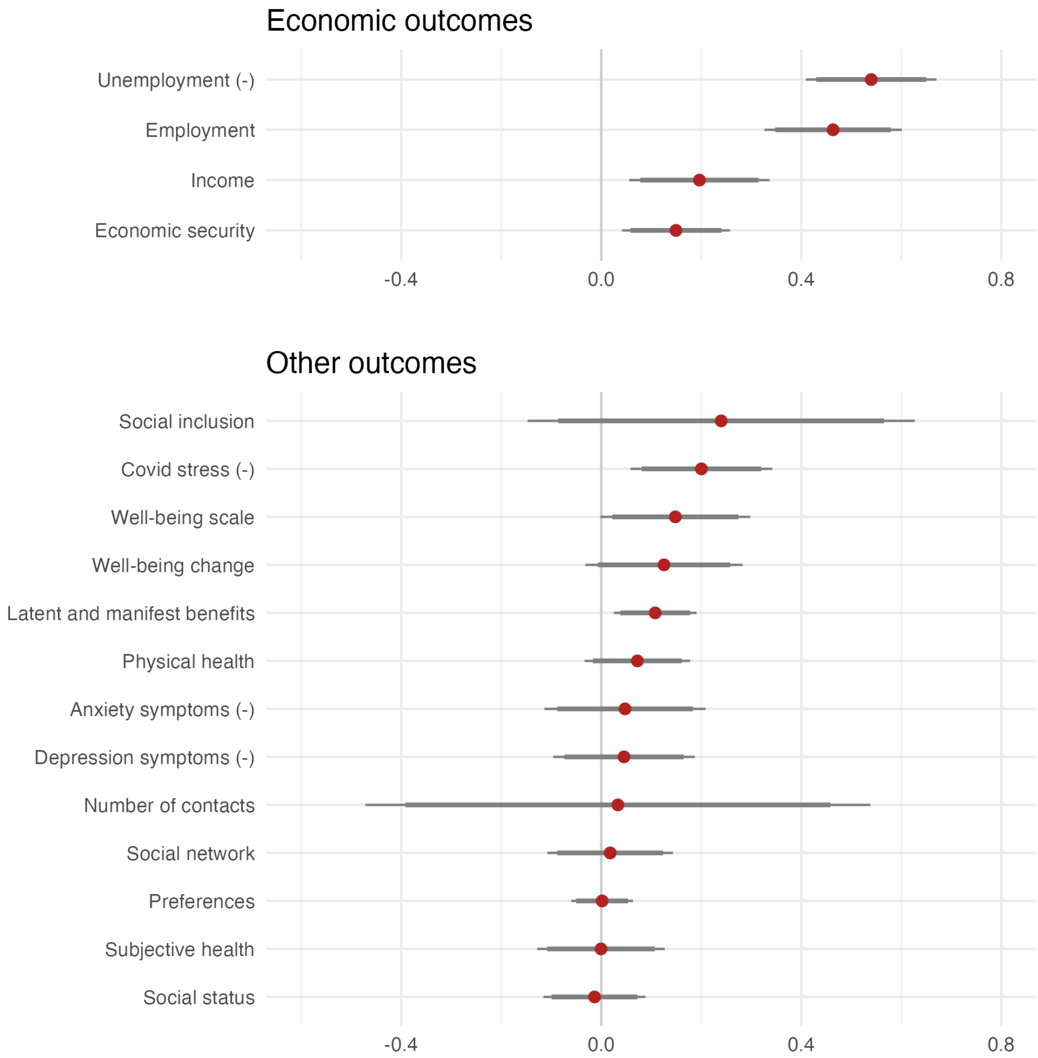
Municipality	Inactive/pop	Mean wage	Mean age ue	Low edu/ue	Mid edu/ue	Poor German/ue	Health cond/ue
Gramatneusiedl	0.209	3308	42.069	0.456	0.481	0.031	0.209
Synthetic control	0.219	3181	42.625	0.389	0.577	0.059	0.212
Zeillern	0.222	3025	41.474	0.289	0.711	0.000	0.193
Ebreichsdorf	0.217	3278	43.101	0.424	0.527	0.082	0.169
Leopoldsdorf im Marchfelde	0.244	3222	44.021	0.472	0.507	0.056	0.225
Strasshof an der Nordbahn	0.202	3264	41.188	0.458	0.493	0.061	0.260
Rußbach	0.208	3022	42.314	0.343	0.629	0.057	0.349
Sieghartskirchen	0.220	3241	43.406	0.319	0.626	0.043	0.278
Sollenau	0.238	3071	41.847	0.460	0.517	0.119	0.274

Table A.3: Covariate balance for the individuals in our control town sample

Covariate	Gramatneusiedl	Control towns	Difference	T-statistic	P-value
Male	0.581	0.535	-0.045	0.523	0.602
Age	44.694	49.634	4.940	-2.496	0.014
Migration Background	0.339	0.310	-0.029	0.352	0.726
Education	0.452	0.535	0.084	-0.958	0.340
Medical condition	0.306	0.338	0.032	-0.386	0.700
Benefit level	29.839	34.535	4.697	-2.600	0.011
Days unemployed	1661.355	1638.521	-22.834	0.136	0.892

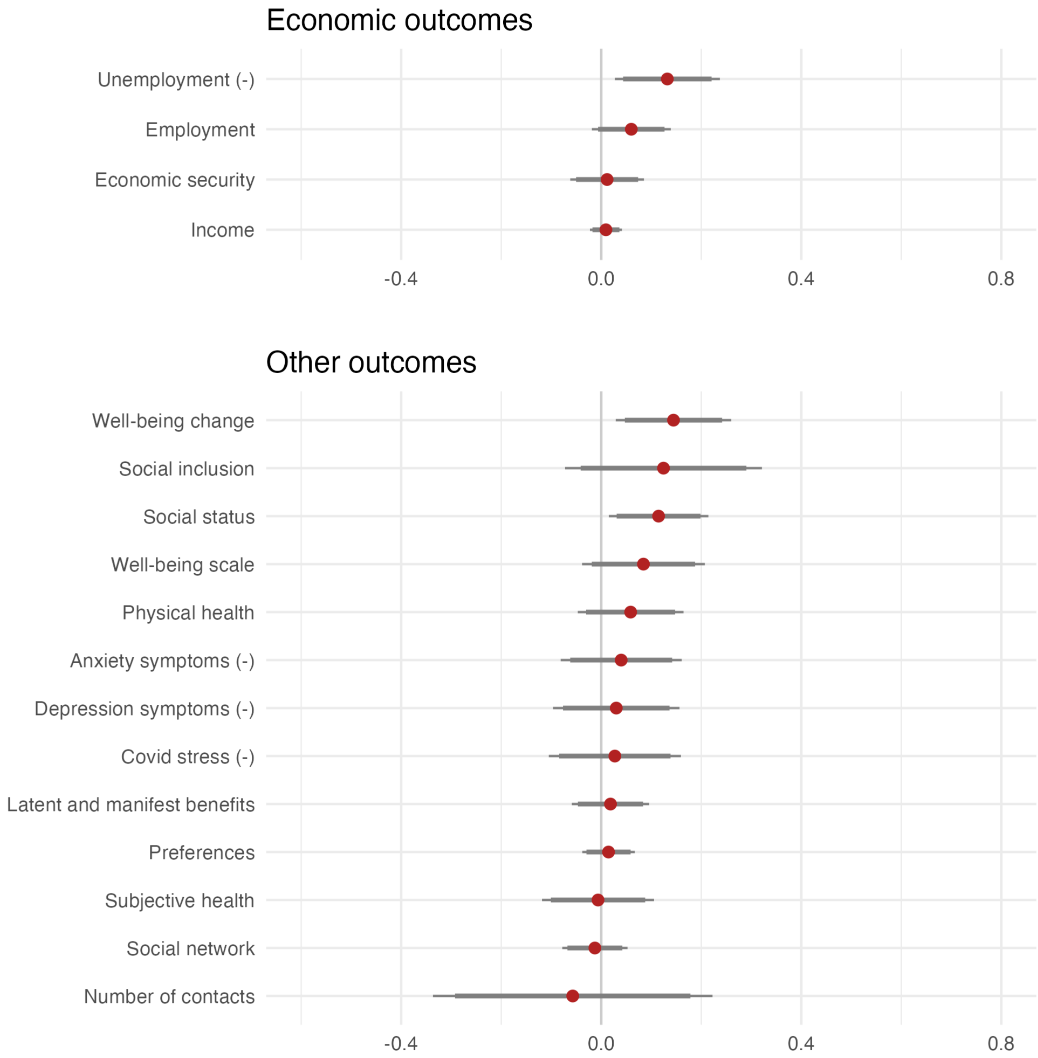
A.1.2 Confidence intervals

Fig. A.3: Confidence intervals for contrast of Group 2 and Group 1 in February 2021



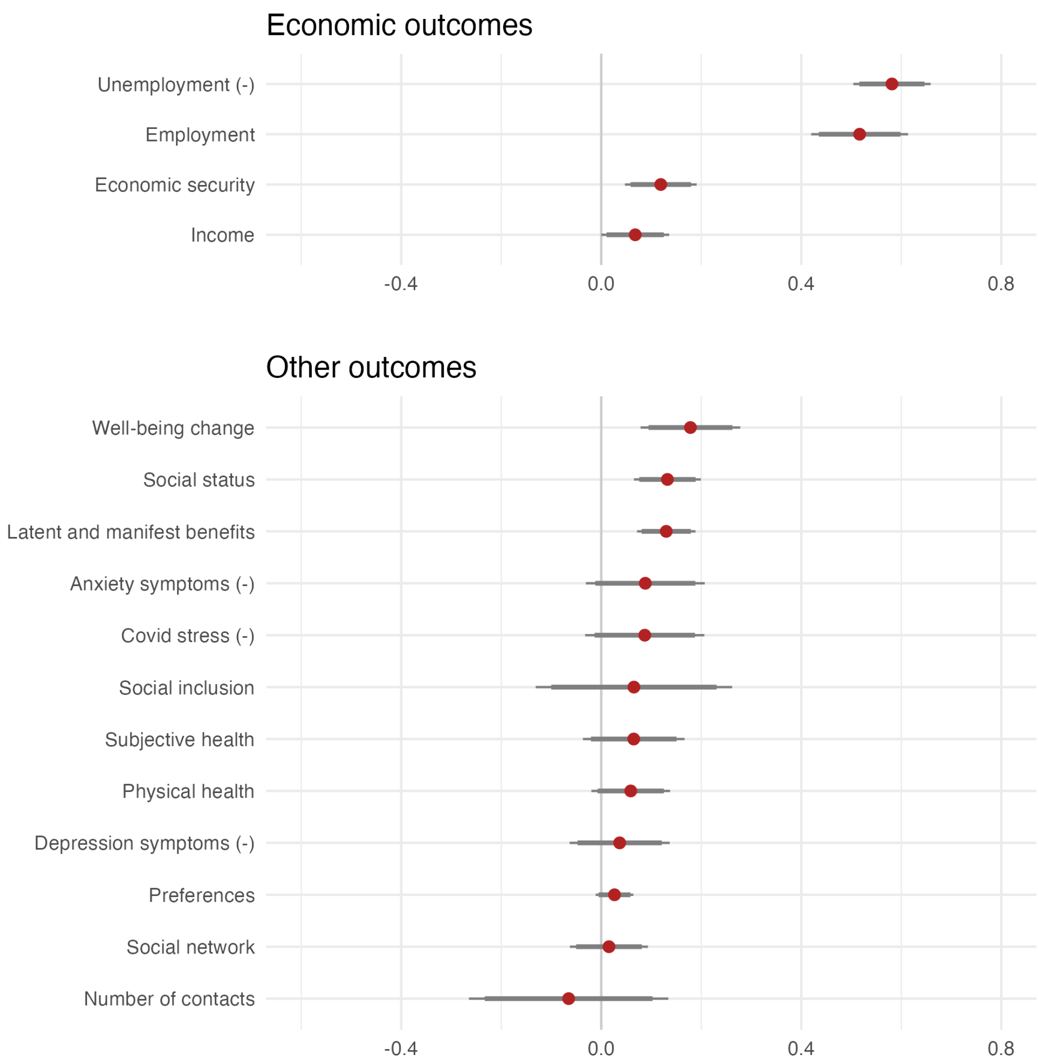
Notes: Confidence intervals for treatment effects, estimated with linear controls for baseline covariates, and with robust standard errors. The thin line shows the 95% confidence interval and the wider line shows the 90% confidence interval. These confidence intervals correspond to the estimates reported in Figure 2.2. These estimates are also tabulated in Table 2.5.

Fig. A.4: Confidence intervals for contrast of Group 2 and control town individuals, February 2021



Notes: These confidence intervals correspond to the estimates reported in Figure 2.6 and Figure 2.7. These estimates are also tabulated in Table 2.6 and Table 2.7.

Fig. A.5: Confidence intervals for contrast of participants in both groups and control town individuals, February 2022



Notes: These confidence intervals correspond to the estimates reported in Figure 2.6 and Figure 2.7. These estimates are also tabulated in Table 2.6 and Table 2.7.

A.1.3 Robustness checks

Fig. A.6: Experimental estimates with pair controls

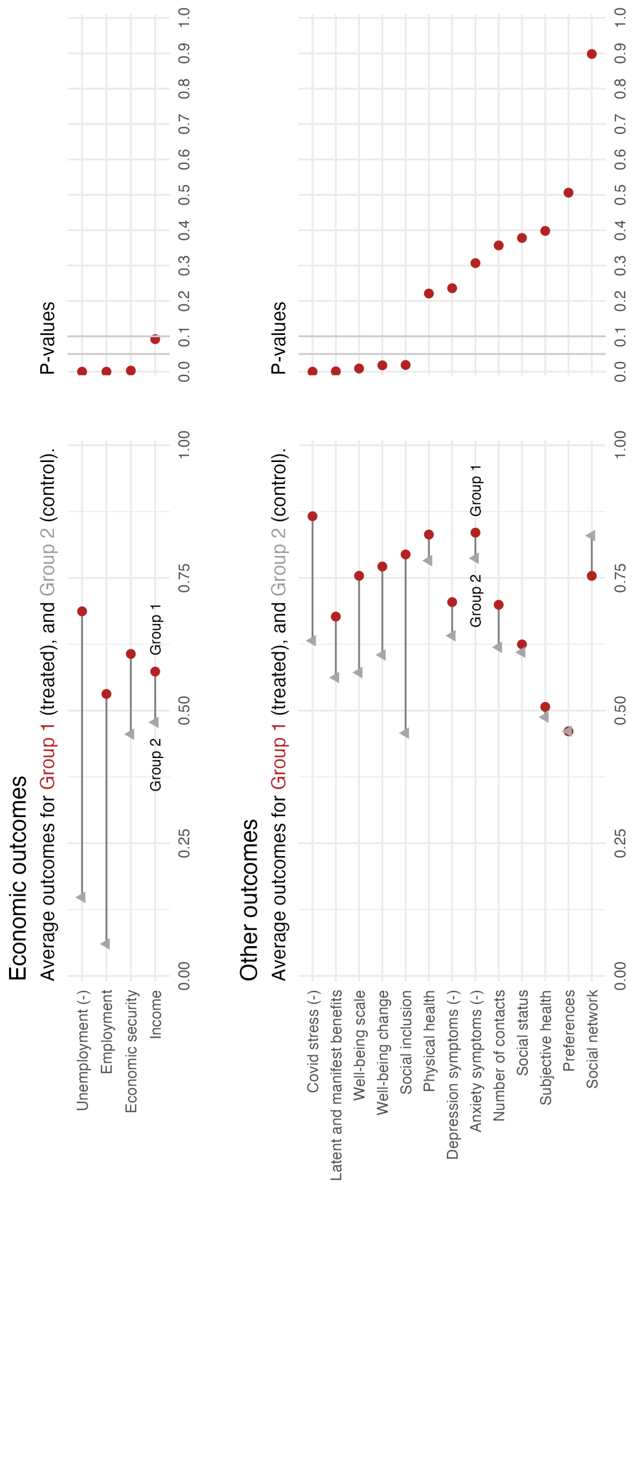
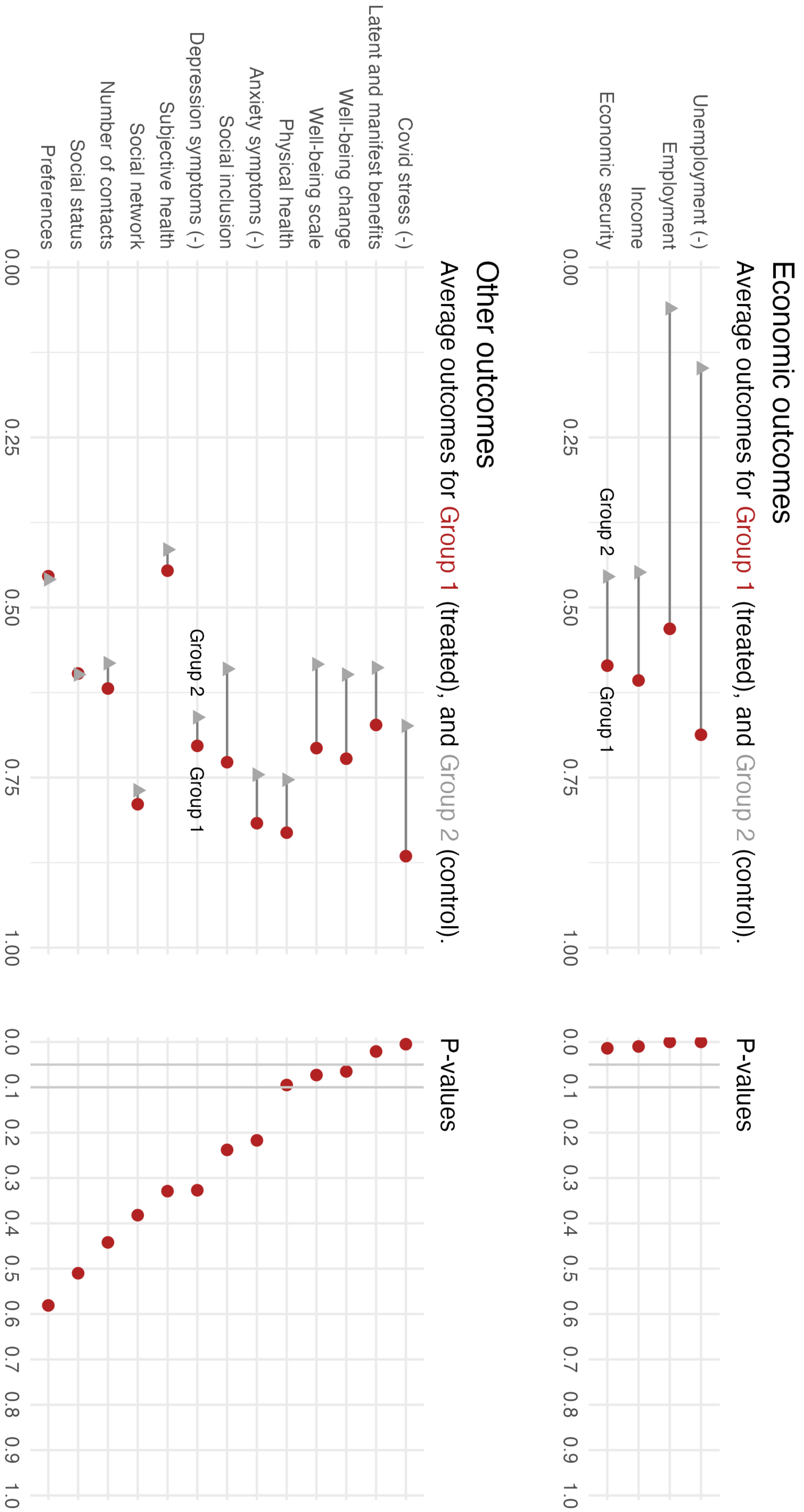


Fig. A.7: Experimental estimates with no controls



A.2 Survey questions

This section includes the questions used to survey participants in the treatment and control groups. The questions are structured by outcomes. First-level numbered bullet points correspond to the questions that constitute the aggregate index for each outcome reported. Each question was used with equal weights for the aggregation. Second-level alphabetically listed bullet points correspond to the answer categories provided in the survey. Some questions (on income and on social networks) are repeated, to clarify that they enter the construction of different outcome measures, as listed in Table 2.1 in the manuscript. The questionnaire for the survey was registered at https://www.socialscienceregistry.org/trials/6706.

Income security

Source of questions: US-SHED (Board of Governors of the Federal Reserve System, 2019), EU-SILC (Eurostat, 2019), and own.

1. Overall, which one of the following best describes how well you are managing financially these days:
    (a) Living comfortably
    (b) Doing okay
    (c) Just getting by
    (d) Finding it difficult to get by
2. Compared to 6 months ago before the start of MAGMA, would you say that you are better off, the same, or worse off financially?
3. How much is your monthly income?
   Subsequent question if no response: Can you try to guess in which category your monthly income falls approximately?

(a) less than 600 €

(b) 600 - 1,000 €

(c) 1,000 - 1,400 €

(d) 1,400 - 1,800 €

(e) 1,800 - 2,200 €

(f) 2,200 - 2,600 €

(g) 2,600 € or more

4. Are you in arrears with a regular payment such as rent, phone bill, loan installment or the like?

5. Are you able to make an unexpected expense such as X for a repair?

Income

Source of questions: US-SHED (Board of Governors of the Federal Reserve System, 2019), EU-SILC (Eurostat, 2019), and own.

1. How much is your monthly income?

   Subsequent question if no response: Can you try to guess in which category your monthly income falls approximately?

   (a) less than 600 €

   (b) 600 - 1,000 €

   (c) 1,000 - 1,400 €

   (d) 1,400 - 1,800 €

   (e) 1,800 - 2,200 €

   (f) 2,200 - 2,600 €

   (g) 2,600 € or more

Depression symptoms

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020).

Over the last 2 weeks, how much does the statement describe your feelings?

1. I feel I cannot shake off the blues, even with help from my family and my friends.
2. I feel sad.
3. I feel happy.
4. I feel life is not worth living.
5. I feel depressed.

Covid stress

Source of questions: Conway et al. (2020)

Please tell us whether the following statements apply to you:

1. Thinking about the coronavirus (COVID-19) makes me feel threatened.
2. I am afraid of the coronavirus (COVID-19).
3. I am stressed around other people because I worry I'll catch the coronavirus (COVID-19).
4. The Coronavirus (COVID-19) has impacted me negatively from a financial point of view.

5. I have lost job-related income due to the Coronavirus (COVID-19).
6. I have become depressed because of the Coronavirus (COVID-19).
7. The Coronavirus (COVID-19) outbreak has impacted my psychological health negatively.

Social inclusion

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020).

1. How many new people have you met in the past month? Please type the approximate number.
2. Which of the following statements best describes your current relationship status?
   (a) I am romantically involved on a steady basis. We live together.
   (b) I am romantically involved on a steady basis. We live separately.
   (c) I am involved in an on-again and off-again relationship.
   (d) I am not involved in a romantic relationship.

Preferences

Source of questions: Falk et al. (2018). Weber and Blais (2006). Mobasseri et al. (2022). Own.

Time preferences

1. Would you prefer to receive 100 € today, or 300 € in 1 month?

2. Would you prefer to receive 100 € today, or 300 € in 6 months?

3. Would you prefer to receive 100 € today, or 300 € in 12 months?

4. Suppose you have some money to do business, and you have a choice between 2 options. Which option would you choose?

   (a) A business that can give you a lot of profit every month, but there is a chance you could lose money.

   (b) A business with less profit every month, but you can't lose your money.

5. Imagine you have saved 10,000 € from working at a job. You receive the following offer from a good bank: If you invest with them there is a chance that you will double the money you invested immediately, or lose half of the money you invested. How much do you want to invest? You only have 10,000 €.

Personality traits

6. In general terms, most people can be trusted.

7. You are willing to give up something that is beneficial for you today in order to benefit more from it in the future.

8. When someone does me a favor I am willing to return it.

9. If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so.

10. I am willing to punish someone who treats me unfairly, even if there may be costs for me.

11. Imagine the following situation: Today you unexpectedly received 1,000 €. How much of this amount would you donate to a good cause?

12. Generally, I am willing to give to a good cause without expecting anything in return.

Risk preferences

We are interested in your risk-taking behavior. Please select how risky you find the respective behavior.

13. Admitting that your tastes are different from those of a friend.
14. Drinking heavily at a social function.
15. Disagreeing with an authority figure on a major issue.
16. Having an affair with a married man/woman.
17. Passing off somebody else's work as your own.
18. Betting a day's income on the outcome of a sporting event.
19. Engaging in unprotected sex.
20. Revealing a friend's secret to someone else.
21. Speaking your mind about an unpopular issue in a meeting at work.
22. Not returning a wallet you found that contains 200 €.

Latent and manifest benefits

Source of questions: Kovacs et al. (2017)

Please select whether you agree or disagree with the following statements:

Activity

1. There is usually not enough spare time in my day.

2. I often have nothing to do.

Social interaction

3. I usually have a lot of opportunities to mix with people.
4. I seldom meet new people.

Collective purpose

5. I rarely feel that I make a meaningful contribution to society.
6. I often feel a valuable part of society.

Time structure

7. My days are usually well organized.
8. I rarely catch up with the things I need to do.

Social recognition

9. I am usually important to my friends.
10. My friends rarely value my company.

Financial strain

11. My income usually allows me to do the things I want.
12. My income usually does not allow me to socialise as often as I like.

Physical health

Source of questions: PHQ-15 somatic symptom scale (Kroenke et al., 1998).

During the past month, how much have you been bothered by any of the following problems?

1. belly
2. back
3. limbs
4. menstruation (asked for women only)
5. sexual intercourse
6. head
7. chest
8. dizziness
9. passed out
10. heart
11. breath
12. intestine
13. digestion
14. sleep
15. energy

Anxiety symptoms

Source of questions: GAD-7 general anxiety disorder (Spitzer et al., 2006).

Over the last 2 weeks, how often have you been bothered by the following problems?

1. Feeling nervous, anxious or on edge.
2. Not being able to stop or control worrying.
3. Worrying too much about different things.
4. Trouble relaxing.
5. Being so restless that it is hard to sit still.
6. Becoming easily annoyed or irritable.
7. Feeling afraid as if something awful might happen.

Social network

Source of questions: Social Network Accuracy Test ("SNAT") from Mobasseri et al. (2022), and own.

1. From time to time, most people discuss work-related and job-search issues with other people. Looking back over the last 6 months, who are the people with whom you discussed work-related and job-search issues with? In the boxes below, please list the FIRST NAME and LAST NAME INITIAL of the people with whom you discuss important matters. E.g., Maria Maier would be recorded as "Maria M." Please list only one name per box. If two people on your list share the same first name and last initial, use numbers to distinguish them (e.g., "Maria M" and "Maria M2"). If you don't discuss important matters with anyone, just leave the fields blank.
2. Below is a list of the names you provided on the prior page. Please answer the questions below about each person you named. How frequently are you in contact with each person?

3. Please select whether you agree or disagree with the following statement. This person is close to you.

4. Please select whether you agree or disagree with the following statement. Compared to other people you know, this person is very valuable to you.

5. Which of the following best describes your relationship to each person?

    (a) Spouse/Significant Other

    (b) Other Family Member

    (c) Friend/Social Contact

    (d) Work/Professional Contact

    (e) Other

6. Please select whether you agree or disagree with the following statements. This contact is someone who looks up to me.

Well-being scale

Source of questions: WHO-5 Well-being Index (WHO, 1998; Topp et al., 2015).

The following statements relate to your well-being in the past two weeks. For each statement, please mark the number that you think best describes how you have felt over the past two weeks. In the last two weeks ...

1. I was happy and in a good mood.
2. I felt calm and relaxed.
3. I felt energetic and active.
4. I felt fresh and rested when I woke up.
5. My everyday life was full of things that interest me.

Well-being change

Source of questions: Own questionnaire.

1. Compared to 6 months ago before the start of MAGMA, would you say that you are doing better, the same, or worse?

Social status

Source of questions: US-SHED (Board of Governors of the Federal Reserve System, 2019), and own.

1. Imagine a ladder showing where people stand in society. At the top are the people who are the best off — those who have the most money, the most education, and the most respected jobs. At the bottom are the people who are the worst off — those who have the least money, the least education, and the least respected jobs or no job. Where would you place yourself on this ladder? *(The questionnaire includes an annotated image of a ladder).*
2. Over the past half year did your status in society...
   (a) improve a lot
   (b) improve
   (c) improve a little
   (d) remain as it was
   (e) worsen a little
   (f) worsen
   (g) worsen a lot

3. Thinking of the future, do you expect your status to...

    (a) improve a lot

    (b) improve

    (c) improve a little

    (d) remain as it was

    (e) worsen a little

    (f) worsen

    (g) worsen a lot

Number of contacts

Source of questions: Social Network Accuracy Test (“SNAT”) from Mobasseri et al. (2022), and own.

1. From time to time, most people discuss work-related and job-search issues with other people. Looking back over the last 6 months, who are the people with whom you discussed work-related and job-search issues with? In the boxes below, please list the FIRST NAME and LAST NAME INITIAL of the people with whom you discuss important matters. E.g., Maria Maier would be recorded as “Maria M.” Please list only one name per box. If two people on your list share the same first name and last initial, use numbers to distinguish them (e.g., “Maria M” and “Maria M2”). If you don’t discuss important matters with anyone, just leave the fields blank.

Subjective health

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020), and own.

1. Would you say your health generally is...
   (a) excellent
   (b) very good
   (c) good
   (d) fair
   (e) poor
2. Over the past 6 months, would you say your health generally has...
   (a) improved a lot
   (b) improved
   (c) improved a little
   (d) remained stable
   (e) worsened a little
   (f) worsened
   (g) worsened a lot

A.3 Description of jobs

A.3.1 Jobs created

This section documents the type and number of jobs created by the Marienthal job guarantee scheme between its start in 2020 until November 2022 both in the market and non-market sectors. This includes jobs for individuals who joined the scheme after treatment was assigned. Jobs of eligible individuals who found a job outside of the program are not included in this section. Figure A.8 shows some of the program participants at work.

Jobs created in the non-market sector

- 13 Carpenters
- 7 Tailors
- 6 Gardeners
- 5 Renovation workers
- 3 Registrars
- 3 Cleaners
- 1 Driver
- 1 Assistant counselor

Jobs created in the market sector

- 6 Office clerks
- 2 Warehouse workers

- 2 Assistant electricians
- 1 Care home assistant
- 1 Technical sales assistant
- 1 Facility manager
- 1 Construction worker
- 1 Salesperson
- 1 Construction foreman
- 1 Taxi driver
- 1 Hospitality assistant
- 1 Carpenter
- 1 Marketing assistant
- 1 Municipal building yard worker
- 1 Farm worker
- 1 Nursery worker
- 1 Call centre agent
- 1 Lift technician
- 1 Assistant cook
- 1 Forklift driver
- 1 Accounting clerk
- 1 HR consultant

A.3.2 Participant views

Werner V., aged 60: "After more than 600 job applications over three years, my wish for employment proved hopeless. Too old, too expensive, over-qualified, without long-term prospects due to my age, with multiple university degrees seemingly over-qualified for service jobs... many obstacles seemed to exist. The job guarantee proved extremely valuable and useful for me. In cooperation with the municipality and the local museum, I am archiving and documenting the cultural, scientific and economic value of the historical site of Marienthal."

Mohamad A., aged 44: "I am from Syria and live here in the village with my family–my wife and my 4 children, some of whom are already at school. I recently had a job offer, the company wanted to hire me full time but due to the current Covid situation they changed their minds and offered only a marginal employment contract. By contrast, the job guarantee scheme provides an opportunity to work [full-time], which suits me because we can work every day and learn something new. I'd also like to use the time to improve my German language skills so that I can later catch up on my general qualification for university entrance and perhaps study at a university of applied sciences. I'm grateful for the help the job guarantee offers; it is important for me."

Johann G., aged 65: "I live in Gramatneusiedl and worked for 38 years at a company in the chemical industry that was located in Gramatneusiedl and closed down some years ago. I am now taking part in the job guarantee since 2020, which makes me feel comfortable. Under the scheme, I have worked in renovation and have been able to apply my skills in many ways. With the help of the job guarantee, I can start as a warehouse worker in a recycling company in October 2022."

A.3.3 Case studies

Public vegetable garden: The local mayor provided $250m^2$ of land which participants cultivate as a sustainable food garden. Herbs and vegetables can be picked free of charge and the garden is open year-round. The first harvest was in summer 2022.

Animal therapy: Two participants are employed with an association providing animal-assisted therapy for children with various conditions (e.g., autism, ADHD, disabilities, learning difficulties). By looking after the association's animals, house, and garden, they have enabled the centre to improve its services and care for more young people.

Funeral urns: During participant Michaela P.'s (paid) internship doing office work at a funeral parlour, her employer noticed her talent for painting. Her internship turned into permanent employment in spring 2022 and, in addition to office work, she now paints urns – a new business venture for the parlour. Before Michaela became unemployed, she worked in a canteen and never thought she would be able to include her hobby in her job.

Fig. A.8: Program participants at work

A.4 From "Die Arbeitslosen von Marienthal" to our study

Ninety years ago, in 1930, a team of researchers (including Marie Jahoda, Paul Lazarsfeld, and Hans Zeisel) wrote the pathbreaking study "Die Arbeitslosen von Marienthal" (Jahoda et al., 2017). Three years ago, in 2020, a pilot of a guaranteed job program for the long-term unemployed was launched in the very same location, which we evaluate in the present paper ("Employing the unemployed of Marienthal," EUM).

In this note, we take the occasion to reflect on the methodological differences between these studies. These two studies can be seen as examples of broader developments in social science methodology over the course of the 20th century. We would like to emphasize that this comparison is intended to be descriptive rather than taking a stance regarding the superiority of different methodological approaches.

The study of Jahoda et al. (2017), while pioneering in many ways, also reflected established approaches to empirical social science at the time. Similarly, our study EUM is fairly typical for policy evaluations in current empirical economics (and social science more generally). The methodological state of the art that we follow is reflected in standard graduate curricula in applied econometrics, and has been canonized by the economics Nobel prizes of 2019 ("for their experimental approach to alleviating global poverty") and 2021 ("for his empirical contributions to labour economics" and "for their methodological contributions to the analysis of causal relationships").

There are some commonalities between Jahoda et al. (2017) and EUM. Both are quantitative, empirical studies drawing on a variety of data sources, including self-collected surveys and administrative data.[1] Both are based on similar sample sizes (a few hundred) and geographic scope (Marienthal and Gramatneusiedl, and nearby communities).

Turning to differences between the two studies, there is first the type of question

[1] Jahoda et al. (2017) also has an important qualitative component.

asked. Beyond its rich description, a primary contribution of Jahoda et al. (2017) is a **classification** of the unemployed of Marienthal into 4 types (ungebrochen / resigniert / verzweifelt / apathisch, which translate as unbroken / resigned / desperate / apathetic). By contrast, our focus is on the estimation of **causal effects** of a job guarantee, on both its beneficiaries and the wider community.

The focus on classification was a primary concern of 19th century empirical social science, from Adolphe Quetelet's "social physics" and its focus on types of "average man" through the "scientific" racism of the 19th century in biology and the humanities and its obsession with classifying humanity into distinct "races," to Max Weber's "ideal types." In an afterword to Jahoda et al. (2017), Hans Zeisel justifies the focus on comprehensive description and classification (or "sociography," as the authors call it) out of the need to understand a complicated and unstable capitalist society, for the purpose of rational policy, a need which he argues did not arise in pre-capitalist feudal times, where the classification of individuals was stable and known to everyone. An important role that Zeisel assigns to classification is to make qualitative data amenable to quantitative analysis.[2]

The focus in statistics on causal effects of interventions, on the other hand, traces back to the work of Neyman and Fisher in the 1920s, and has more recently first entered clinical trials in medicine, and has since the 1990s become dominant in empirical economics as well as other social sciences.

Closely related to this focus on classification versus causality is a distinction in the type of event studied. Jahoda et al. (2017) consider the consequences of a **historical macro event** (the Great Depression) – there is not even an attempt at finding a comparison group for their study sample of unemployed workers and their families. In EUM, by contrast, we focus on the **causal effect of a (micro) policy** intervention; much of the methodological effort goes into finding valid comparisons. The notion of causality is intimately related to the ideas of **interventions** and **comparison groups.**

[2] Classification of course still plays an important role in some social sciences as well as psychology today.

Another related aspect is how these studies deal with **heterogeneity**. Jahoda et al. (2017) engage in an impressive and comprehensive effort to **fully capture** and describe the variability of circumstances and psychological responses of the unemployed of Marienthal. By contrast, no such comprehensive effort is made in EUM. Instead, the methodology of causal inference – pairwise matching, randomization, synthetic controls – is used to ensure that comparison groups for causal inference are the **same on average**.

This different approach to heterogeneity is reflected in another striking difference: In Jahoda et al. (2017), no attempt is made to **quantify statistical uncertainty** – there are no standard errors, confidence intervals, or p-values. The study contains a large number of statistical tables, but there is no sense in which these reported numbers (e.g., shares in the sample belonging to a particular category) are related to an underlying **population object** (e.g., shares in the population belonging to a particular category). There is no distinction between estimate and estimand; the reported numbers are what they are. By contrast, EUM follows modern standard practice in reporting standard errors, confidence intervals, and p-values, and additionally addresses the issue of multiple hypothesis testing. The implicit notion is that there are true causal effects (either in the sample or in a larger population), and that the reported estimates are noisy approximations of these effects.

Again related, a striking feature of Jahoda et al. (2017) is its **methodological open-endedness**, contrasting with the complete **pre-registration** of EUM. Jahoda et al. (2017) use a wide variety of data-sources and personal observations, and enter Marienthal without prespecified questions that they will ask. Instead, they distill abstractions and classifications from the rich empirical material they find. By contrast, recent empirical social science has been greatly impacted by its perceived replication crisis, attributed to selective reporting of findings by authors (p-hacking) and journals (publication bias); cf. Andrews and Kasy (2019). A key remedy that has been promoted in recent years, enshrined in journal policies, and followed by EUM, is the pre-registration of experimental designs and statistical analyses. Such pre-registration prevents selective reporting of findings by publicly tying researchers' hands. The aim is to make findings replicable and independent of researcher identity.

Let us conclude by emphasizing one more arc connecting the two studies over the course of a century. A key contribution of Jahoda et al. (2017) was that they documented the devastating impact of unemployment beyond its material consequences on income – in the form of psychological outlook, attitudes to the future, time structure, social cohesion, etc. This perspective was further developed by Marie Jahoda over the course of her career, and has been operationalized by sociologists of work in the form of survey instruments for the Latent And Manifest Benefits (LAMB) of work. In EUM, these survey instruments were included in our data collection. And, indeed, these are the dimensions where our experimental findings suggest the strongest impact of a job-guarantee on the well-being of beneficiaries, besides the direct economic impacts.

Bibliography

Andrews, I. and Kasy, M. (2019). Identification of and correction for publication bias. *American Economic Review*, 109(8):2766–94.

Bendheim-Thoman Center for Research on Child Wellbeing and Center, C. P. R. (2020). Fragile Families Survey. User's Guide for the Fragile Families and Child Wellbeing Study Public Data, Year 15. Technical report, Princeton University.

Board of Governors of the Federal Reserve System (2019). U.S. Survey of Household Economics and Decisionmaking (SHED). Supplemental Appendixes to the Report on the Economic Well-Being of U.S. Households in 2018. Technical report, Federal Reserve Board, Washington, D.C.

Conway, L. G., Woodard, S. R., and Zubrod, A. (2020). Social Psychological Measurements of COVID-19: Coronavirus Perceived Threat, Government Response, Impacts, and Experiences Questionnaires (Updated September 26, 2020).

Eurostat (2019). European Union Statistics on Income and Living Conditions (EU-SILC). Questionnaire for Austria. Technical report, Publications Office of the European Union, Luxembourg.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D., and Sunde, U. (2018). Global Evidence on Economic Preferences. *The Quarterly journal of economics*, 133(4):1645–1692.

Jahoda, M., Lazarsfeld, P. F., and Zeisel, H. (2017). *Marienthal: The Sociography of an Unemployed Community (Original Work Published 1933)*. Routledge.

Kovacs, K., Batinic, B., Muller, J., Coutts, A., and Wang, S. (2017). Jahoda's Latent and Manifest Benefits scale, 12 item version.

Kroenke, K., Spitzer, R. L., deGruy, F. V., and Swindle, R. (1998). A symptom checklist to screen for somatoform disorders in primary care. *Psychosomatics*, 39(3):263–272.

Mobasseri, S., Stein, D. H., and Carney, D. R. (2022). The accurate judgment of social network characteristics in the lab and field using thin slices of the behavioral stream. *Organizational behavior and human decision processes*, 168:104103.

Spitzer, R. L., Kroenke, K., Williams, J. B. W., and Löwe, B. (2006). A brief measure for assessing generalized anxiety disorder: The GAD-7. *Archives of Internal Medicine*, 166(10):1092–1097.

Topp, C. W., Østergaard, S. D., Søndergaard, S., and Bech, P. (2015). The WHO-5 Well-Being Index: A Systematic Review of the Literature. *Psychotherapy and Psychosomatics*, 84(3):167–176.

Weber, E. U. and Blais, A.-R. (2006). A Domain-Specific Risk-Taking (DOSPERT) Scale for Adult Populations. *Judgment and decision making*, 1(1):33–47.

WHO (1998). Wellbeing measures in primary health care/the DEPCARE project. Technical report, WHO Regional Office for Europe, Copenhagen.

Appendix B

Appendix to "Reframing active labor market policy: Field experiments on barriers to program participation"

B.1 Intervention 1: Design

B.1.1 Background

Data and ALMP evaluations in Austria Austria's PES has access to high-quality data from longitudinal administrative records. Observational evaluations have found training to increase job seekers' re-employment stability (Zweimüller and Winter-Ebmer, 1996). However, no randomized evaluations of training programs have been carried out.[1]

B.1.2 Sample

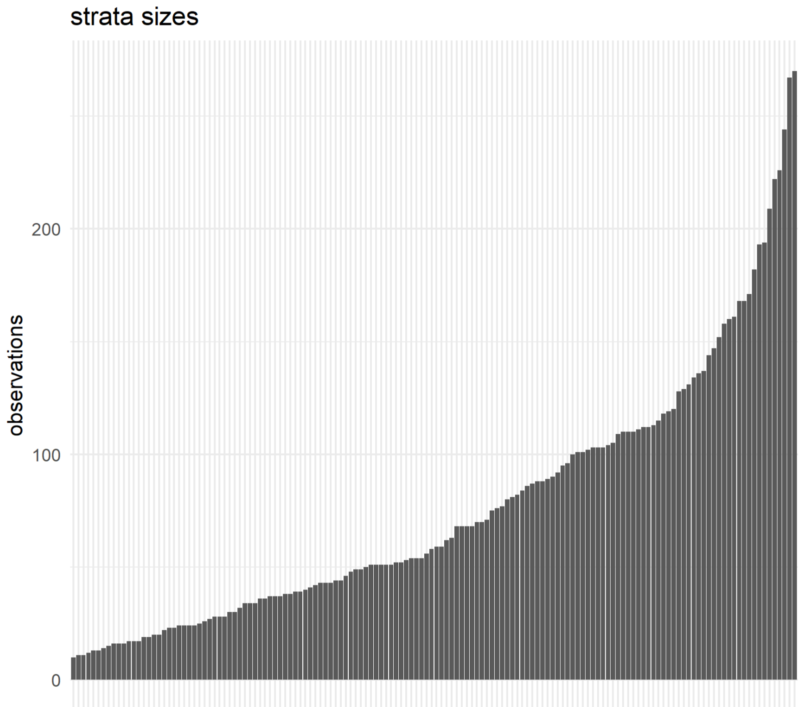
B.1.3 Treatment assignment

[1]The few experimental evaluations of ALMPs in Austria have focused on job search assistance (Mühlböck et al., 2022; Böheim et al., 2022) and public employment programs (Kasy and Lehner, 2023).

Table A1: Balance table

	T1 (N=2769)	T2 (N=2766)	T3 (N=2760)	T4 (N=2755)	Total (N=11050)	p value
Gender						0.999
Women	1437 (51.9%)	1434 (51.8%)	1433 (51.9%)	1434 (52.1%)	5738 (51.9%)	
Men	1332 (48.1%)	1332 (48.2%)	1327 (48.1%)	1321 (47.9%)	5312 (48.1%)	
Age group						1.000
Below 35 years	831 (30.0%)	828 (29.9%)	826 (29.9%)	823 (29.9%)	3308 (29.9%)	
35 - 50 years	1062 (38.4%)	1067 (38.6%)	1064 (38.6%)	1063 (38.6%)	4256 (38.5%)	
Over 50 years	876 (31.6%)	871 (31.5%)	870 (31.5%)	869 (31.5%)	3486 (31.5%)	
Education						1.000
Missing	10	9	8	9	36	
Primary	897 (32.5%)	898 (32.6%)	896 (32.6%)	891 (32.4%)	3582 (32.5%)	
Higher than primary	1862 (67.5%)	1859 (67.4%)	1856 (67.4%)	1855 (67.6%)	7432 (67.5%)	
Region						1.000
Industrieviertel	1222 (44.1%)	1225 (44.3%)	1227 (44.5%)	1219 (44.2%)	4893 (44.3%)	
Mostviertel	741 (26.8%)	731 (26.4%)	732 (26.5%)	732 (26.6%)	2936 (26.6%)	
Waldviertel	243 (8.8%)	245 (8.9%)	239 (8.7%)	241 (8.7%)	968 (8.8%)	
Weinviertel	563 (20.3%)	565 (20.4%)	562 (20.4%)	563 (20.4%)	2253 (20.4%)	
Unemp. dur.						1.000
3 - 4 Months	676 (24.4%)	675 (24.4%)	671 (24.3%)	668 (24.2%)	2690 (24.3%)	
6 - 9 Months	937 (33.8%)	937 (33.9%)	937 (33.9%)	934 (33.9%)	3745 (33.9%)	
9 - 12 Months	1156 (41.7%)	1154 (41.7%)	1152 (41.7%)	1153 (41.9%)	4615 (41.8%)	
Citizenship						0.778
Missing	1	2	3	1	7	
Austria	2147 (77.6%)	2146 (77.6%)	2150 (78.0%)	2165 (78.6%)	8608 (77.9%)	
Other	621 (22.4%)	618 (22.4%)	607 (22.0%)	589 (21.4%)	2435 (22.1%)	
Health						0.991
No health restriction	2185 (78.9%)	2177 (78.7%)	2168 (78.6%)	2169 (78.7%)	8699 (78.7%)	
Health restriction	584 (21.1%)	589 (21.3%)	592 (21.4%)	586 (21.3%)	2351 (21.3%)	
Marg. empl.						0.733
No	2457 (88.7%)	2479 (89.6%)	2467 (89.4%)	2463 (89.4%)	9866 (89.3%)	
Yes	312 (11.3%)	287 (10.4%)	293 (10.6%)	292 (10.6%)	1184 (10.7%)	
German						0.456
Partial or non	404 (14.6%)	403 (14.6%)	377 (13.7%)	418 (15.2%)	1602 (14.5%)	
Proficient or native	2365 (85.4%)	2363 (85.4%)	2383 (86.3%)	2337 (84.8%)	9448 (85.5%)	

Fig. A1: Strata size



B.1.4 Treatment (Intervention 1)

Fig. A2: E-mail for treatment groups 1, 2, and 3

Ihr Weg zum beruflichen Neustart

Sehr geehrte Damen und Herren,

auch jetzt in Zeiten der Krise gibt es nachgefragte Berufe und Qualifikationen mit Zukunft. Die Corona-Joboffensive bietet Ihnen die Möglichkeit, neue Qualifikationen zu erwerben, die Ihnen den Wiedereinstieg ins Berufsleben ermöglichen.

Darum lade ich Sie ganz persönlich ein: Nutzen Sie Ihre Chancen zum beruflichen Neustart mit einer Aus- oder Weiterbildung! Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

Nehmen Sie Ihre berufliche Zukunft in die Hand – und bleiben Sie gesund!

Ihr

Sven Hergovich
Landesgeschäftsführer des AMS Niederösterreich

Aus- und Weiterbildung für den Neustart am Arbeitsmarkt

Aktuelle und nachgefragte Qualifikationen sind der wichtigste Erfolgsfaktor für den beruflichen Neustart.

Ob Auffrischungskurs für Ihre Fachkenntnisse oder eine Ausbildung mit Lehrabschluss - das AMS Niederösterreich hält eine Vielzahl von Aus- und Weiterbildungsmöglichkeiten für Sie bereit.

Einige Beispiele:

- Metall- und elektrotechnische Berufe
- Mechatronik
- Berufskraftfahrer/in, Transportwesen
- Pflegeassistenz / Pflegefachasisstenz

Verschaffen Sie sich einen Startvorteil am Arbeitsmarkt und nutzen Sie unsere Aus- und Weiterbildungsangebote!

So finanzieren wir Sie während Ihrer Ausbildung

© stock.adobe.com/grafikplusfoto

Mit dem Schulungsgeld vom AMS sind Sie während der Ausbildung finanziell abgesichert. Der Betrag entspricht zumindest Ihrem Arbeitslosengeld oder Ihrer Notstandshilfe und wird unter bestimmten Voraussetzungen aufgestockt.

Zusätzlich erhalten Sie einen Bildungsbonus in Höhe von 4€ pro Tag, wenn Sie Arbeitslosengeld oder Notstandshilfe beziehen, Ihre Ausbildung zumindest vier Monate dauert und noch in diesem Jahr startet.

Vorsorge und Sicherheit: Ihre Ausbildung während der COVID-19-Maßnahmen

Das AMS nimmt die Situation um die COVID-19-Pandemie ernst. Deswegen passen wir gemeinsam mit unseren Partnerinstituten den Kursbetrieb laufend den gerade erforderlichen Corona-Schutzmaßnahmen an.

Damit Sie gesund bleiben und dennoch Ihre Ausbildung starten können, richtet sich das AMS dabei nach dem Grundsatz:
Soviel Distance Learning wie möglich – so viel Präsenzunterricht wie notwendig!

Informieren Sie sich jetzt!

Jetzt informieren unter
050 904 343

Sie möchten mehr über Ihre Weiterbildungsmöglichkeiten erfahren oder wünschen sich Unterstützung bei der Wahl Ihrer passenden Ausbildung?

Unsere ExpertInnen der AMS-Weiterbildungshotline stehen Ihnen bei Fragen montags bis donnerstags von 07:30h bis 16:00h und freitags von 07:30h bis 13:00h unter der Nummer **050 904 343** gerne telefonisch zur Verfügung.

Oder Sie schreiben ein E-Mail.

B.1.5 Tracking e-mail responses

Fig. A3: Voucher for treatment groups 2 and 3

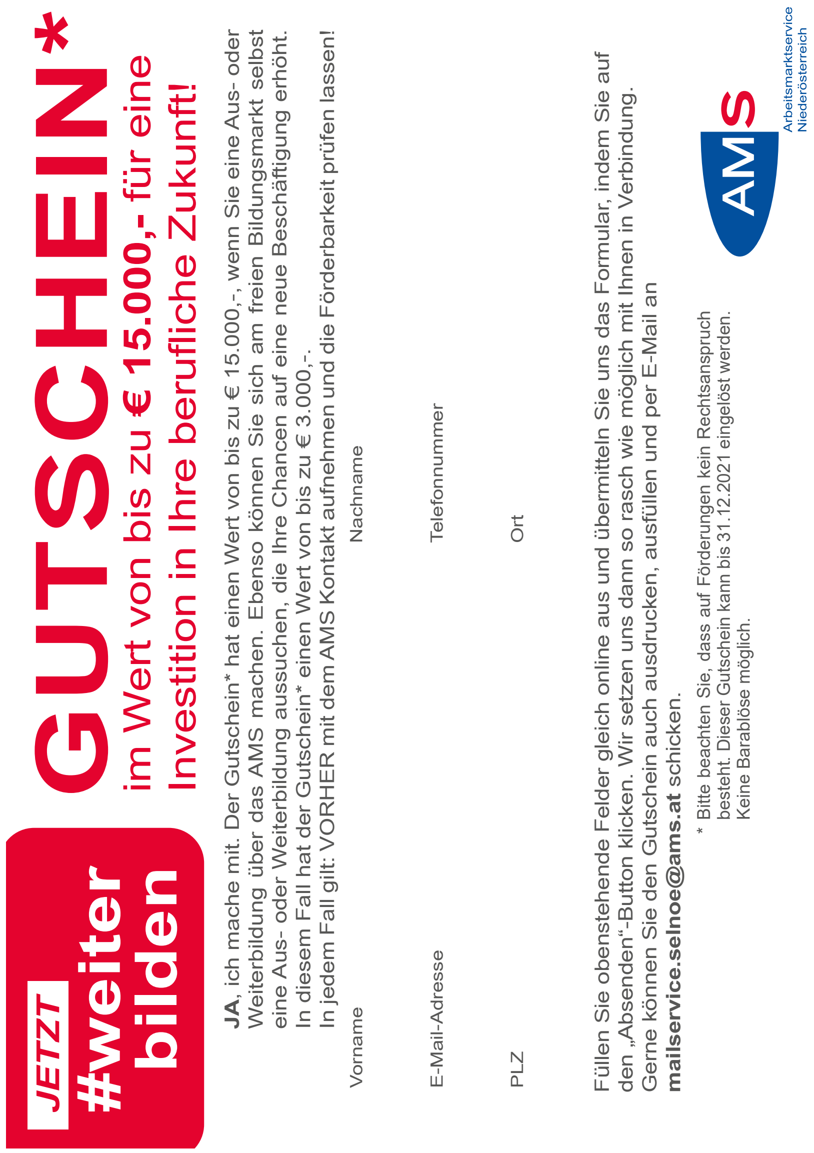


Fig. A4: Occupations with the highest number of open vacancies for treatment group 3

Die aktuellen Top Jobs am niederösterreichischen Arbeitsmarkt

- **Elektroinstallateur(e)innen, -monteur(e)innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **343**
- **Dipl. Krankenpfleger, -schwestern**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **229**
- **Kraftfahrer/innen (alle Bereiche)**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **228**
- **Maurer/innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **170**
- **Techniker/innen für Datenverarbeitung**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **159**
- **Rohrinstallateur(e)innen, -monteur(e)innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **157**
- **Hotel- und Gaststättenberufe**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **132**
- **Techniker/innen für Maschinenbau**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **117**
- **Pflegeassistent/in**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **110**
- **Medizinisch-technische Fachkräfte (m./w.)**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **81**

Fig. A5: Measurement of e-mail openings and clicks

B.2 Intervention 1: Results

B.2.1 Training

Table B1: Training completion

	Completion			
	Long training	Examined training	Application courses	External courses
	(1)	(2)	(3)	(4)
E-Mail	0.018**	0.010**	−0.009*	−0.002
	(0.008)	(0.005)	(0.005)	(0.004)
Voucher	0.026***	0.009*	−0.006	0.005
	(0.008)	(0.005)	(0.005)	(0.005)
Vacancies	0.006	0.004	−0.003	0.0001
	(0.008)	(0.005)	(0.005)	(0.005)
Control Mean	0.094	0.033	0.042	0.029
Control SD	0.292	0.177	0.2	0.169
Observations	10,714	10,714	10,714	10,714

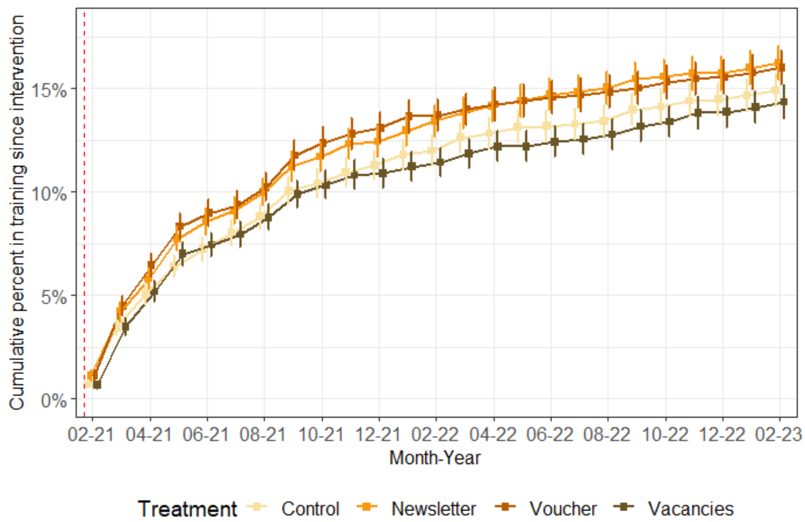
Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B2: Long-term average treatment effects on active labor market programs

	Dependent variable:					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.015*	0.011	0.017**	0.010	−0.013**	0.003
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Voucher	0.013	0.016*	0.013*	0.004	−0.007	−0.012
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Vacancies	−0.005	−0.004	−0.003	0.005	−0.006	−0.018
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Control Mean	0.149	0.13	0.1	0.061	0.062	0.319
Control SD	0.356	0.336	0.301	0.24	0.241	0.466
Observations	10,714	10,714	10,714	10,714	10,714	10,714

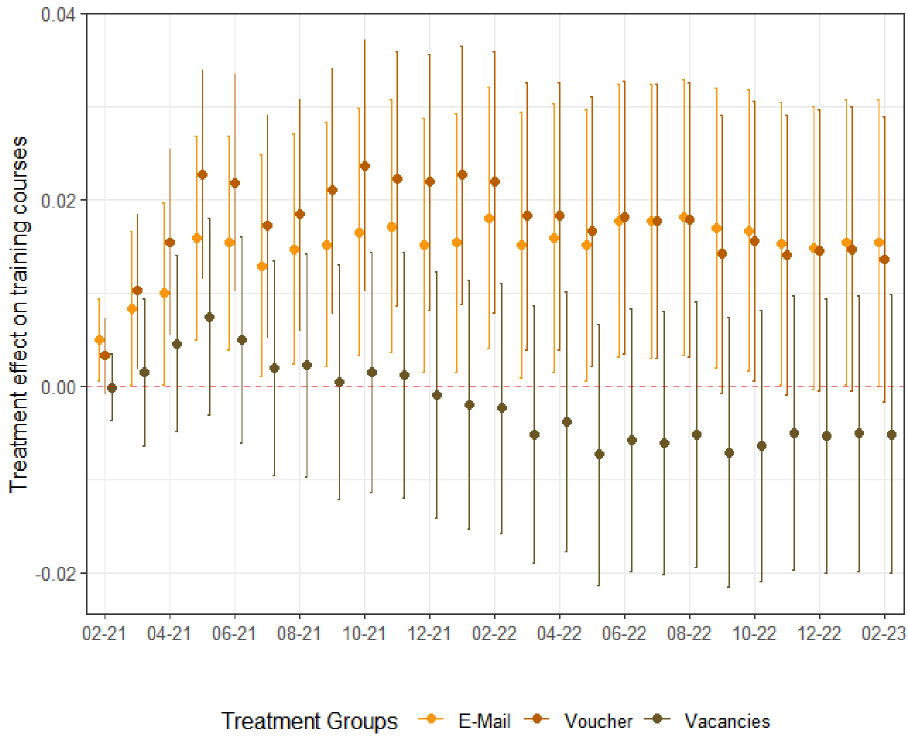
Note: Long-term refers to 2 years after the intervention. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Fig. B1: Average treatment effects on training participation over time (cumulative)



Note: Confidence intervals are reported at the 90%-level.

Fig. B2: Average treatment effects on training participation over time (long-term)



Note: Confidence intervals are reported at the 90%-level.

B.2.2 Heterogeneity in training

B.2.2.1 Training enrollment

Table B3: Heterogeneity in training enrollment by age and education

	Dependent variable: Training Enrollment					
	Below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.003	0.028*	0.021*	0.015	0.026**	0.021**
	(0.017)	(0.015)	(0.011)	(0.014)	(0.013)	(0.013)
Voucher	0.013	0.036**	0.021*	0.027*	0.030**	0.018**
	(0.017)	(0.015)	(0.011)	(0.014)	(0.013)	(0.013)
Vacancies	0.005	−0.013	0.010	0.012	−0.004	−0.001
	(0.017)	(0.014)	(0.011)	(0.014)	(0.012)	(0.012)
Control Group Mean	0.132	0.05	0.153	0.108	0.137	0.086
Control Group SD	0.338	0.219	0.36	0.311	0.344	0.28
Observations	3,169	4,116	3,429	4,350	3,995	2,369

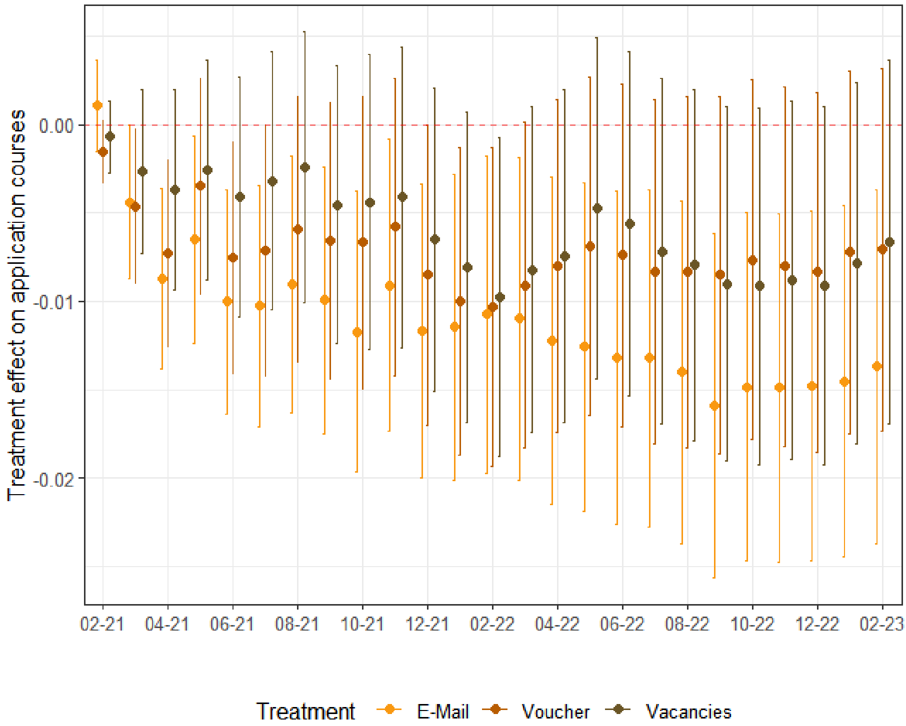
Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B4: Heterogeneity in training enrollment by citizenship and language

	Dependent variable: Training Enrollment			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail	0.031	0.018**	0.058**	0.016*
	(0.022)	(0.009)	(0.029)	(0.008)
Voucher	0.004	0.025***	0.036	0.022***
	(0.022)	(0.009)	(0.030)	(0.008)
Vacancies	0.003	−0.0003	0.029	−0.001
	(0.021)	(0.008)	(0.028)	(0.008)
Control Group Mean	0.196	0.088	0.243	0.09
Control Group SD	0.398	0.283	0.429	0.286
Observations	2,270	8,444	1,460	9,254

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Fig. B3: Average treatment effects on application course enrollment over time



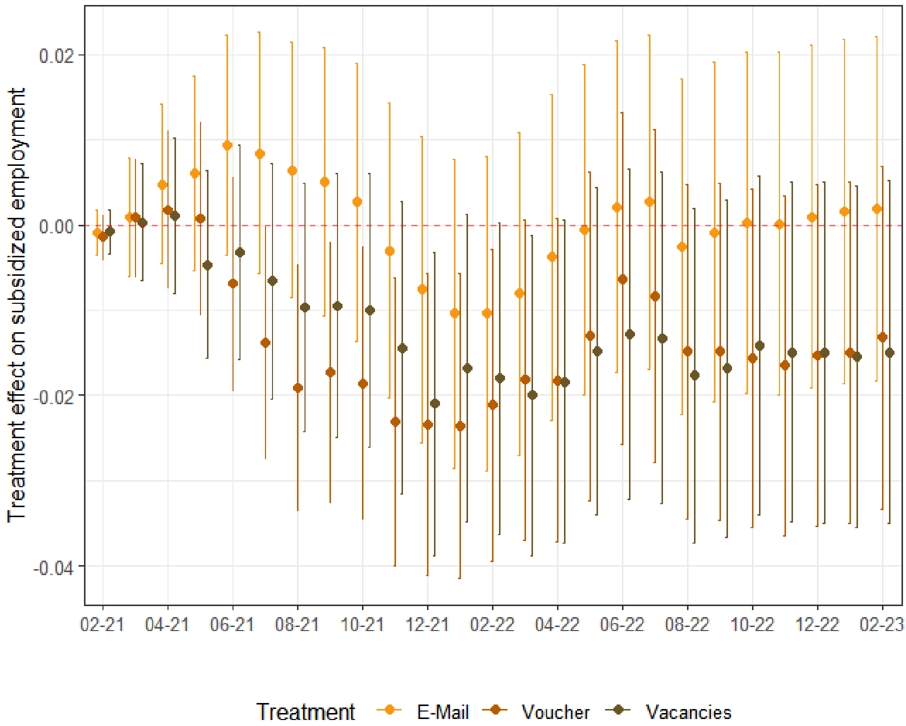
Note: Confidence intervals are reported at the 90%-level.

Table B5: Heterogeneity in training enrollment by occupation

	Dependent variable:				
	Training Enrollment				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail	0.018	0.020**	0.034	0.035***	−0.016
	(0.015)	(0.010)	(0.021)	(0.011)	(0.014)
Voucher	0.010	0.027***	0.005	0.044***	−0.004
	(0.014)	(0.010)	(0.021)	(0.011)	(0.015)
Vacancies	0.024*	−0.012	0.033	0.012	−0.031**
	(0.014)	(0.010)	(0.020)	(0.011)	(0.014)
Control Group Mean	0.121	0.103	0.101	0.155	0.097
Control Group SD	0.326	0.304	0.301	0.362	0.295
Observations	3,775	6,939	2,132	5,694	2,888

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Fig. B4: Average treatment effects on subsidized employment over time



Note: Confidence intervals are reported at the 90%-level.

B.2.2.2 Training completion

Table B6: Heterogeneity in training completion by gender and income

	Dependent variable: Training Completion			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail	0.030**	0.001	0.035***	−0.001
	(0.012)	(0.009)	(0.011)	(0.011)
Voucher	0.040***	0.012	0.042***	0.009
	(0.013)	(0.009)	(0.012)	(0.011)
Vacancies	0.012	−0.0002	0.026**	−0.014
	(0.012)	(0.009)	(0.011)	(0.010)
Control Group Mean	0.123	0.063	0.094	0.084
Control Group SD	0.329	0.243	0.292	0.278
Observations	5,523	5,191	5,363	5,351
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.			

Table B7: Heterogeneity in training completion by age and education

	Dependent variable: Training Completion					
	Below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.014	0.017	0.019*	0.015	0.019	0.026
	(0.016)	(0.013)	(0.011)	(0.013)	(0.012)	(0.012)
Voucher	0.017	0.039***	0.017	0.030**	0.026**	0.027**
	(0.016)	(0.014)	(0.011)	(0.013)	(0.012)	(0.012)
Vacancies	0.010	−0.005	0.011	0.013	−0.001	0.017
	(0.015)	(0.013)	(0.010)	(0.013)	(0.011)	(0.011)
Control Group Mean	0.113	0.045	0.123	0.09	0.116	0.071
Control Group SD	0.317	0.206	0.328	0.287	0.32	0.257
Observations	3,169	4,116	3,429	4,350	3,995	2,369
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.					

Table B8: Heterogeneity in training completion by citizenship and language

	Dependent variable:			
	Training Completion			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail	0.058	0.013*	0.032**	0.016*
	(0.021)	(0.008)	(0.028)	(0.008)
Voucher	0.024	0.026***	0.010	0.027***
	(0.020)	(0.008)	(0.028)	(0.008)
Vacancies	0.041	0.003	0.022	0.002
	(0.020)	(0.008)	(0.027)	(0.007)
Control Group Mean	0.169	0.073	0.213	0.074
Control Group SD	0.375	0.26	0.41	0.262
Observations	2,270	8,444	1,460	9,254

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B9: Heterogeneity in training completion by occupation

	Dependent variable:				
	Training Completion				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail	0.020	0.017*	0.032*	0.031***	−0.016
	(0.013)	(0.010)	(0.019)	(0.011)	(0.014)
Voucher	0.017	0.028***	0.009	0.043***	−0.004
	(0.013)	(0.010)	(0.019)	(0.011)	(0.014)
Vacancies	0.035***	−0.008	0.045**	0.013	−0.027**
	(0.013)	(0.009)	(0.019)	(0.010)	(0.013)
Control Group Mean	0.099	0.088	0.09	0.125	0.08
Control Group SD	0.299	0.283	0.286	0.332	0.272
Observations	3,775	6,939	2,132	5,694	2,888

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.2.3 Employment

Table B10: Employment effects short-term (1 year)

	Dependent variable:					
	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail + Voucher	−0.007		−3.049	−0.317	−0.017	−0.004
	(0.011)		(2.576)	(2.738)	(0.860)	(0.005)
Training		−0.083				
		(0.436)				
Control Group Mean	0.548	0.548	94.625	211.497	48.76	0.348
Control Group SD	0.498	0.498	116.412	119.17	30.172	0.155
Observations	10,714	10,714	10,714	10,714	6,441	5,403

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B11: Income with alternative definitions

	Dependent variable:			
	Daily wage in first job	Cumulative earnings	Higher than median avg. daily wage	Higher than median jobquality
	(1)	(2)	(3)	(4)
E-mail + Voucher	0.405	−553.181	0.002	−0.013
	(0.937)	(531.232)	(0.013)	(0.011)
Control Group Mean	55.91	21729.99	0.447	0.348
Control Group SD	34.933	23900.172	0.497	0.476
Observations	7,544	10,714	7,723	10,714

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

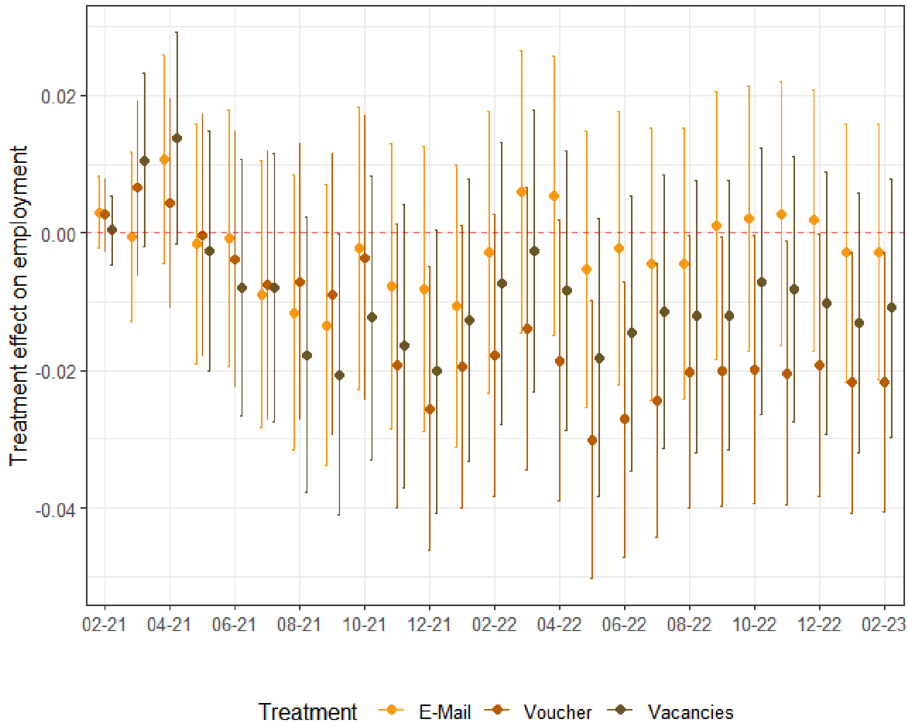
Table B12: Employment outcomes with instrumental variable approach

	Dependent variable:			
	Days in employment IV reg	Days in unemployment IV reg	Avg. daily wage IV reg	Jobquality IV reg
	(1)	(2)	(3)	(4)
Training	−243.832	−245.424	−15.672	−0.010
	(344.317)	(335.887)	(42.283)	
Control Group Mean	350.103	361.954	50.814	0.382
Control Group SD	310.971	259.588	29.494	0.144
Observations	10,714	10,714	7,723	7,323
Note:	Standard errors are in parentheses:*p<0.1; **p<0.05; ***p<0.01.			

Table B13: Employment outcomes with separate treatment groups

	Dependent variable:				
	Any employment	Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)
E-mail	−0.001	−1.937	−3.405	−0.456	−0.0003
	(0.011)	(7.845)	(6.755)	(0.882)	(0.004)
Voucher	−0.014	−10.381	−3.122	0.288	0.001
	(0.011)	(7.905)	(6.796)	(0.903)	(0.005)
Vacancies	−0.004	−4.266	1.590	0.324	0.001
	(0.011)	(7.904)	(6.787)	(0.900)	(0.005)
Control Group Mean	0.754	350.103	361.954	50.814	0.382
Control Group SD	0.431	310.971	259.588	29.494	0.144
Observations	10,714	10,714	10,714	7,723	7,323
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.				

Fig. B5: Average treatment effects on employment over time



Note: Confidence intervals are reported at the 90%-level.

B.2.4 Heterogeneity in employment

Table B14: Heterogeneity in employment by gender and income

	Dependent variable:			
	Days in employment			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail + Voucher	−9.725	−3.420	−4.168	−5.819
	(9.641)	(9.770)	(9.580)	(10.078)
Control Group Mean	87.566	102.242	90.122	100.42
Control Group SD	115.361	117.101	113.099	120.086
Observations	5,523	5,191	5,363	5,351
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.			

Table B15: Heterogeneity in employment by age and education

	Dependent variable:					
	Days in employment					
	Below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-Mail + Voucher	10.039	−14.986	4.714	−1.551	−4.470	−10.781
	(12.669)	(11.367)	(11.634)	(10.761)	(11.270)	(15.464)
Control Group Mean	115.4	110.696	55.436	91.493	97.798	95.066
Control Group SD	123.211	120.787	92.293	113.22	118.382	118.629
Observations	3,169	4,116	3,429	4,350	3,995	2,369
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.					

Table B16: Heterogeneity in employment by citizenship and language

	Dependent variable:			
	Days in employment			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail + Voucher	0.778	−9.885	33.326*	−12.323*
	(15.796)	(7.669)	(20.235)	(7.329)
Control Group Mean	103.229	92.144	99.391	93.811
Control Group SD	114.68	116.843	111.774	117.189
Observations	2,270	8,444	1,460	9,254
Note:	Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.			

Table B17: Heterogeneity in employment by occupation

	Dependent variable:				
	Days in employment				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail + Voucher	−7.006	−6.333	0.290	−18.151*	6.763
	(11.703)	(8.658)	(16.135)	(9.403)	(14.533)
Control Group Mean	94.353	94.935	97.555	90.293	95.004
Control Group SD	114.945	117.265	119.044	112.342	116.676
Observations	3,775	6,939	2,132	5,694	2,888

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.2.5 Mechanisms

Table B18: Average treatment effect on training by caseworker

	Dependent variable:					
	Training enrollment		Training completion		Training enrollment	
	Low caseworker	High caseworker	Low caseworker	High caseworker	Low caseworker	High caseworker
	(1)	(2)	(3)	(4)	(5)	(6)
E-Mail	0.045***	−0.010	0.019*	0.003	0.029***	0.003
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.013)
Voucher	0.049***	0.006	0.035***	0.010	0.036***	0.011
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.013)
Vacancies	0.008	−0.007	0.002	0.002	0.007	−0.008
	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.012)
Fixed effect outcome	empl. duration	empl. duration	empl. duration	empl. duration	unempl. duration	unempl. duration
Control Group Mean	0.113	0.112	0.097	0.092	0.098	0.129
Control Group SD	0.316	0.316	0.296	0.289	0.297	0.335
Observations	5,385	5,176	5,646	5,059	5,489	5,216

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B19: Evaluation of vacancy information by socio-economic characteristics

	Percent at least rather agreeing	
	Information is important for me	Would consider working in one of these jobs
Occupation		
Blue-collar | A	45.21%	38.26%
White-collar | B	38.58%	31.46%
Occupation skill-level		
Low-skilled | A	48.00% C	41.33% C
Medium skilled | B	42.93%	34.03%
High-skilled | C	31.90%	27.59%
Education		
Up to secondary education | A	46.51% C	39.54% C
Vocational education | B	43.26% C	36.17% C
More than secondary education | C	30.97%	23.89%
Age group		
Below 35 years | A	51.14% B	44.32%
35-50 years | B	35.40%	32.30%
Above 50 years | C	40.74%	28.89%
Gender		
Women | A	44.02%	31.20%
Men | B	36.00%	38.00%
Pre-unemployment income		
Below median income|A	45.30%	37.02%
Above median income | B	36.00%	30.00%

Table B20: Treatment effects on specific courses related to vacancy information

	Dependent variable:			
	Training	Training completion	Training	Training completion
	(1)	(2)	(3)	(4)
E-mail	0.004	0.002	0.004	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Voucher	0.005	0.005	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Vacancies	−0.001	0.0001	−0.001	−0.0005
	(0.003)	(0.003)	(0.003)	(0.002)
Control Mean	0.014	0.01	0.013	0.009
Control SD	0.119	0.1	0.115	0.095
Observations	10,714	10,714	10,714	10,714

Note: Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.3 Survey

B.3.1 Survey sample

Table C1: Survey Sample Comparison

	Full	Survey	p value	Control	E-Mail	Voucher	Vacancies	p value
Total	11050	2180		533	555	555	537	
Gender			0.001					0.139
Women	51.9	56.2		55.0	53.7	55.9	60.3	
Men	48.1	43.8		45.0	46.3	44.1	39.7	
Age			0.001					0.295
Below 35	29.9	24.3		24.2	21.8	26.5	24.6	
35-50	38.5	38.3		38.6	40.9	34.1	39.5	
Above 50	31.5	37.5		37.1	37.3	39.5	35.9	
Education			0.001					0.035
Compulsory educ.	32.5	27.3		27.3	23.6	31.5	26.9	
Higher than comp.	67.5	72.7		72.7	76.4	68.5	73.1	
Citizenship			0.115					0.852
Non-Austrian	22.1	23.6		22.3	24.5	23.5	24.0	
Austrian	77.9	76.4		77.7	75.5	76.5	76.0	
Health			0.529					0.818
Health restriction	21.3	21.9		23.3	21.3	21.1	22.0	
No health restriction	78.7	78.1		76.7	78.7	78.9	78.0	
German			0.246					0.776
Partial or non	14.5	15.5		14.4	15.9	15.0	16.6	
Proficient or native	85.5	84.5		85.6	84.1	85.0	83.4	
Marginal Empl.			0.150					0.097
Yes	10.7	9.7		88.4	90.1	92.8	89.9	
No	89.3	90.3		11.6	9.9	7.2	10.1	
Unemployment			0.683					0.928
3-4 months	24.3	25.0		24.6	23.2	25.9	26.1	
6-9 months	33.9	33.0		32.6	33.9	32.1	33.3	
9-12 months	41.8	42.1		42.8	42.9	42.0	40.6	
Region			0.002					0.918
Industrieviertel	44.3	48.8		50.3	48.8	46.8	49.2	
Mostviertel	26.6	24.2		24.8	22.5	24.7	25.0	
Waldviertel	8.8	8.1		7.7	8.3	8.3	8.0	
Weinviertel	20.4	18.9		17.3	20.4	20.2	17.9	

B.3.2 Survey questionnaire

Fig. C1: Survey questionnaire: intro

English (United Kingdom)

Intro

Let us know what you think about AMS courses!

Welcome to this short survey on AMS courses at the Vienna University of Economics and Business on behalf of AMS Niederösterreich. In order to be able to tailor the course offer to your interests, please fill out our short survey. Your opinion counts!

The survey only takes 3 minutes. All answers remain completely anonymous. The answers are evaluated by the Vienna University of Economics and Business on behalf of the AMS Niederösterreich and are incorporated into a research project to improve the AMS offer.

Would you like to participate in the survey?

○ Yes, I have been informed of the purpose of the survey and would like to take part.

If you have any questions or comments about the survey or the research project, you can contact me at any time: anna-magdalena.schwarz@wu.ac.at

Fig. C2: Survey questionnaire: reminder of treatment

About two months ago you received the following newsletter from the AMS on further training: (please scroll down)

Ihr Weiterbildungsgutschein im Wert von bis zu 15.000,- Euro

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu 15.000,- Euro sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

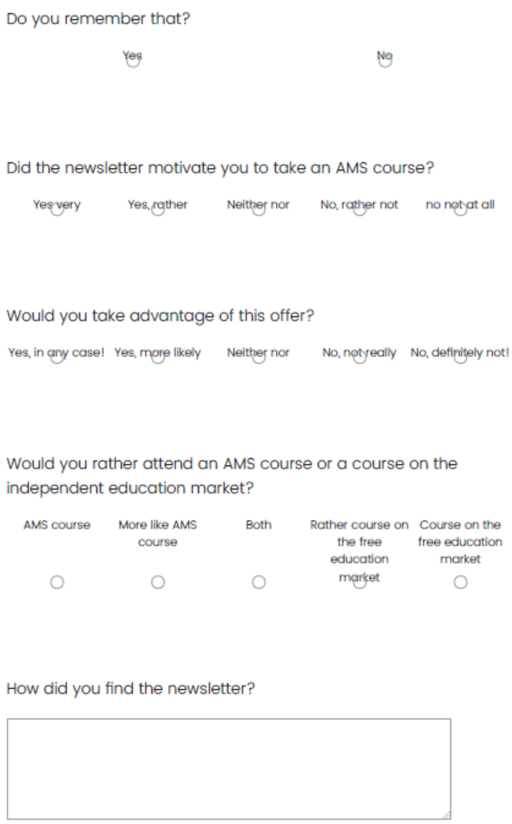
Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben und lösen Sie Ihren Weiterbildungsgutschein ein! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

Nehmen Sie Ihre berufliche Zukunft in die Hand – und bleiben Sie gesund!

Ihr

Sven Hergovich
Landesgeschäftsführer des AMS Niederösterreich

Fig. C3: Survey questionnaire: treatment mechanisms


Do you remember that?

Yes | No

Did the newsletter motivate you to take an AMS course?

Yes very | Yes, rather | Neither nor | No, rather not | no not at all

Would you take advantage of this offer?

Yes, in any case! | Yes, more likely | Neither nor | No, not really | No, definitely not!

Would you rather attend an AMS course or a course on the independent education market?

AMS course | More like AMS course | Both | Rather course on the free education market | Course on the free education market

How did you find the newsletter?

Fig. C4: Survey questionnaire: course participation

How decisive were the following factors for you in your decision not to attend a course?

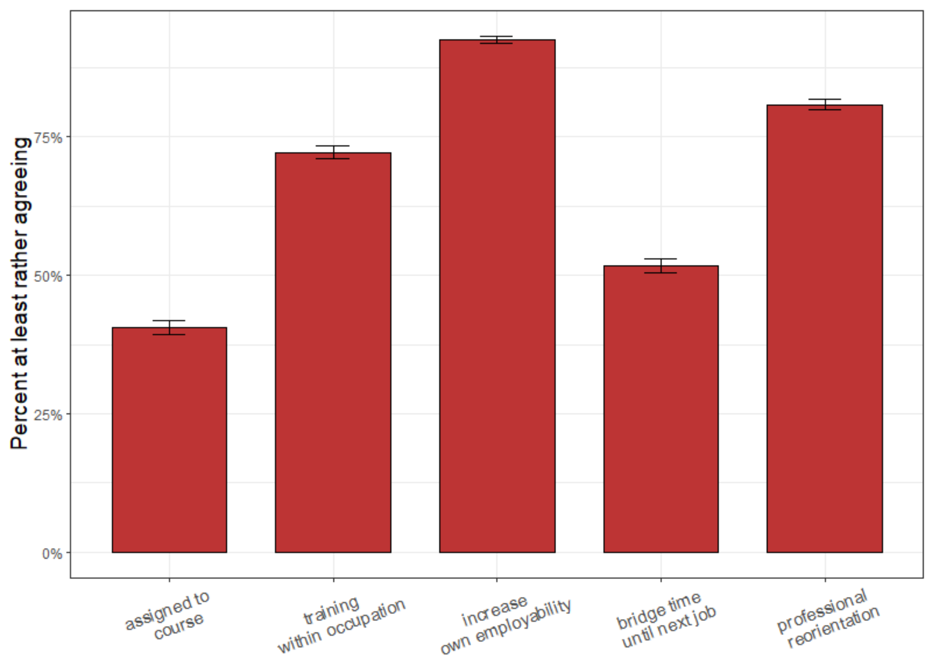
	very important	rather important	Neither nor	not that important	not important at all
The AMS refused my preferred course	○	○	○	○	○
I am too old to do advanced training.	○	○	○	○	○
I haven't found a suitable course for me	○	○	○	○	○

	very important	rather important	Neither nor	not that important	not important at all
I don't have enough information about the AMS courses	○	○	○	○	○
I cannot afford to attend a course for financial reasons	○	○	○	○	○
I am prevented by other obligations (e.g. childcare or caring for relatives)	○	○	○	○	○

B.3.3 Additional survey results

Motivation to train Why do job seekers enroll in training? Desires such as increasing one's employability drive most job seekers enrollment while external constraints such as being assigned to a course drive a sizeable minority. 9 out of 10 job seekers enroll in training to increase their employability (Figure C5). 80% consider professional re-orientation as a motive while for 70% training within their occupation is important. About half of job seekers simply intend to bridge the time until their next job. Assignment by the caseworker as an external factor matters for around 40% of job seekers.

Fig. C5: Motivation for training enrollment



Note: Confidence intervals are reported at the 90%-level.

Training course assignment suffers from perverse incentives.

- *"No one cares if a course fits your profile or not. The main focus is that you are no longer adding to the unemployed numbers."*
- *"All pointless mass processing so that some unemployed fall out of the statistics."*

- *"One should be listened to and not just thrown into a course to make the labor market statistics look better."*

Job seekers demand more autonomy.

- *"It would be nice if people's wishes and needs were taken into account."*
- *"Be more responsive to the needs of the unemployed to provide relevant training."*
- *"The PES should provide us with a targeted offer of courses with self-selection under a certain budget, so that we can make our own choices."*

Bibliography

Böheim, R., Eppel, R., and Mahringer, H. (2022). More Caseworkers Shorten Unemployment Durations and Save Costs. Results from a Field Experiment in an Austrian Public Employment Office. *Working Paper*.

Kasy, M. and Lehner, L. (2023). Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program. *Working Paper*.

Mühlböck, M., Kalleitner, F., Steiber, N., and Kittel, B. (2022). Information, reflection, and successful job search: A labor market policy experiment. *Social Policy & Administration*, 56(1):48–72.

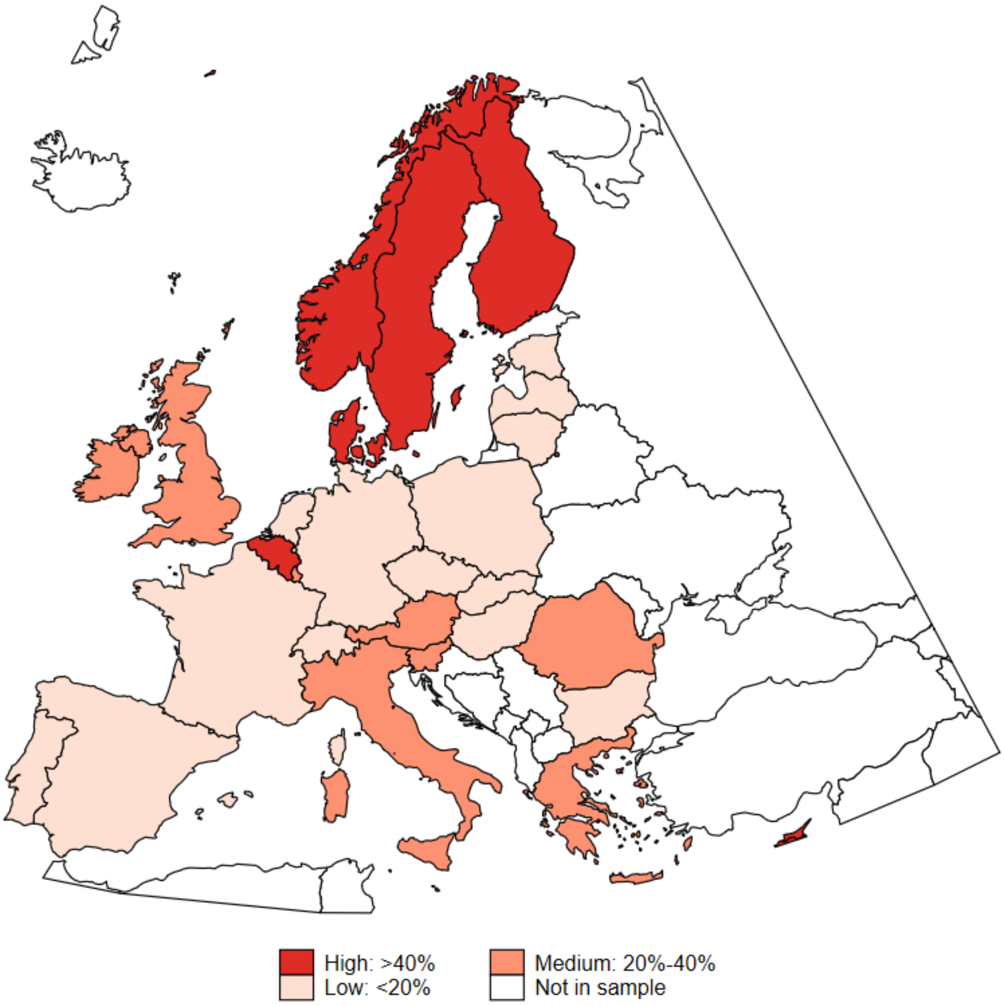
Zweimüller, J. and Winter-Ebmer, R. (1996). Manpower Training Programmes and Employment Stability. *Economica*, 63(249):113–130.

Appendix C

Appendix to "Beggaring thy co-worker: Labor market dualization and the wage growth slowdown in Europe"

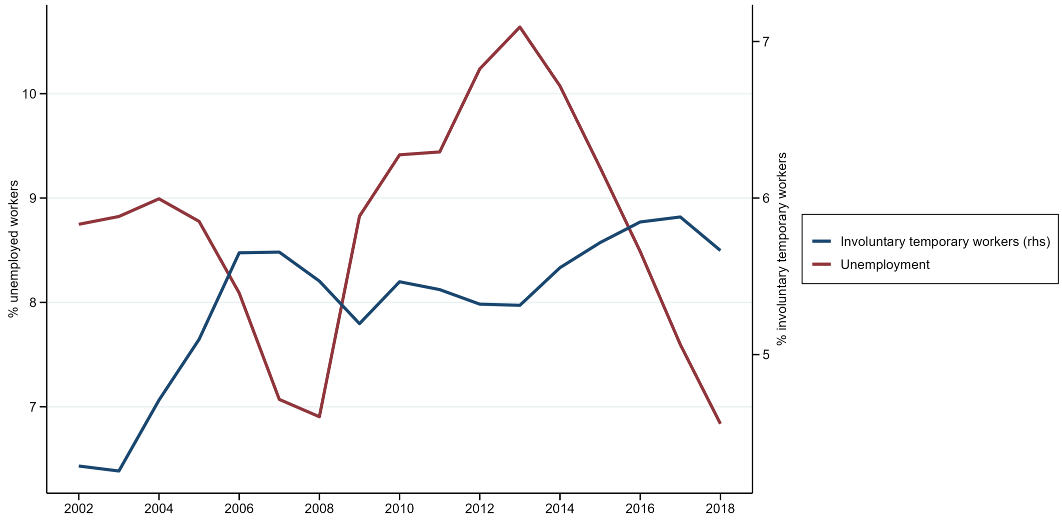
C.1 Appendix: Stylized facts

Fig. C.1.1: Trade union density in Europe



Note: Trade union density refers to dependent employees that are trade union members as a share of the total number of dependent employees. Numbers are reported as an annual average 2003-2018.
Source: OECD/AIAS ICTWSS database.

Fig. C.1.2: Labor market dualization in Europe



Note: Weighted averages of 28 European countries in our sample excluding CH and RS due to data availability for the entire time series. Values for AT 2004, 2005, ES 2005, MT 2004 and FR 2002 are imputed for the graph only. Unemployment and involuntary temporary workers as a share of the labor force aged 15 to 74.
Source: Eurostat/EU-LFS: lfsa_etgar, lfsa_eegais, lfsa_agan.

C.2 Appendix: EU-SILC data

C.2.1 Wage growth aggregated vs. published (net)

The EU-SILC data has been the established standard for cross-country income comparisons in Europe. The survey combines demographic variables from the current year with wages from the previous year (except for Ireland and the UK) (Eurostat, 2018). Since we focus on wage growth, we use the year of the reported wage, i.e., one year prior to the other data collected. We use all waves 2004-2018 and hence yield an (unbalanced) macro-panel of wage data spanning the period 2003-2017.[1] We use *gross employee cash or near cash income (PY010G)* for dependent workers as our main variable for wages since we are interested in their pre-tax wages. We rely on the *number of hours usually worked per week in their main job (PL060)* to compute hourly wages at the individual level. We compute aggregate measures at the country level using the *personal cross-sectional weight (PB040)* and compute the average annual change in nominal hourly wages.

A large effort is put into the harmonization of definitions and variables across countries, although some caveats apply due to national differences in data collection. The income reference period for most countries is the calendar year previous to the survey year with two exceptions: Ireland and the UK. In Ireland, the income reference period is the last twelve months. In the United Kingdom, the current income is annualized and aims to refer to the current calendar year, i.e., weekly estimates are multiplied by 52, monthly by 12 (Eurostat, 2018). Since this data started being collected for 2004, an increasing number of countries have shifted to rely on national registries to construct or correct the wage variables strengthening accuracy and reliability (for a detailed overview see (Goedeme and Trindade, 2020)

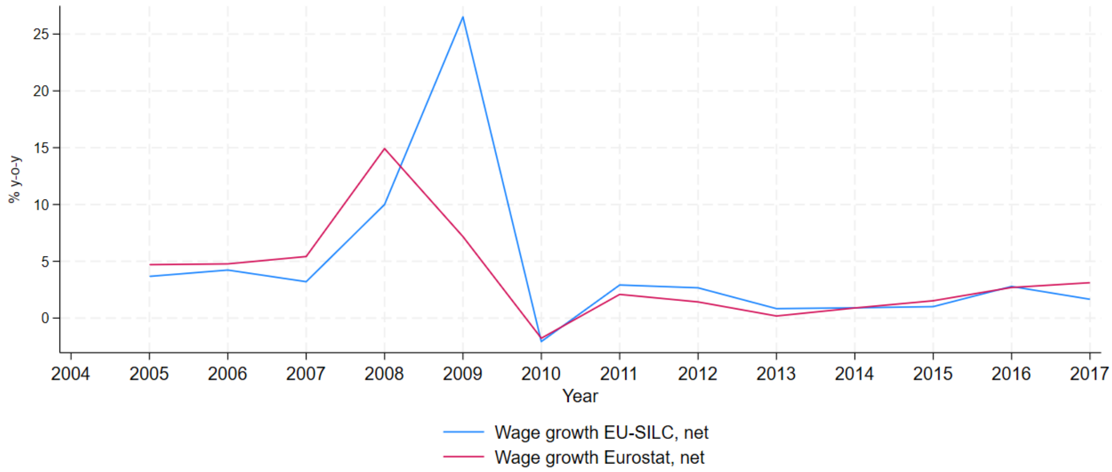
[1]The effective sample starts only in 2004 because our dependent variable is wage growth.

and (Lohmann, 2011)). We carefully examine national particularities in our data cleaning and aggregation procedure following (Trindade and Goedeme, 2019) on the income variables and (GESIS, 2021) in addition to the EU-SILC methodological guidelines and national quality reports to ensure maximum cross-country comparability. However, processing and aggregating individual-level data always entails a series of small decisions that can affect the outcome. To be transparent, we document our aggregation procedure in detail in our annotated Stata code. Eurostat does not publish sufficient details on their procedure for data processing and aggregation that we could follow. For a detailed discussion of EU-SILC representativeness, in particular regarding sampling design, see (Goedeme, 2013; Zardo Trindade and Goedeme, 2016).

To assess the validity of our aggregation, we compare the published aggregate of wages by Eurostat based on EU-SILC to our country aggregation of the individual-level data. Since Eurostat does not publish an aggregate series for *gross* wages from EU-SILC but only for *net* wages, we use *net* wages (*net employee cash or near cash income (PY010N)* in EU-SILC) for comparison. Our aggregated series aligns closely with the officially published time series across Europe (Appendix Figure C.2.1 and Figure C.2.2), although with two limitations. First, in 2009, several countries changed from survey to register data for documenting wages in EU-SILC, resulting in some differences prior to the adjustment, most notably in the year of change (2009). As a result, the alignment of the two series is substantially improved from 2010 onwards. Second, wage growth for Cyprus has an unreliable profile in *net* terms, although our series for *gross* wages in Cyprus is smoother (Figure C.2.2).[2] Excluding both the year 2009 and Cyprus, we obtain a correlation coefficient of 0.91 between our aggregated series and the officially published Eurostat data.

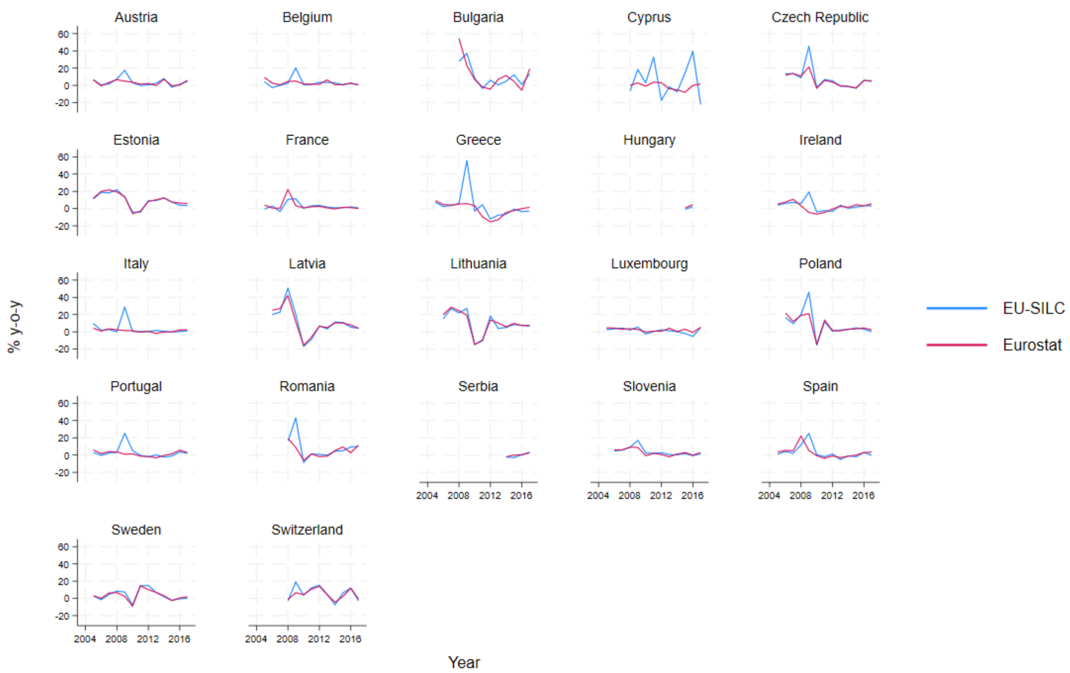
[2](Goedeme and Trindade, 2020) indicate that Cyprus relies on surveys to collect income data but matches it with register data to correct for apparent mistakes and keeps extreme or outlying values in the data if they have been verified.

Fig. C.2.1: Net wage growth EU-SILC aggregated vs published by Eurostat



Note: Wage growth refers to average annual change in nominal wages 2003-2017 and is computed as a weighted average for European countries in our sample with available data for net wages.
Source: Authors' computations based on EU-SILC and Eurostat.

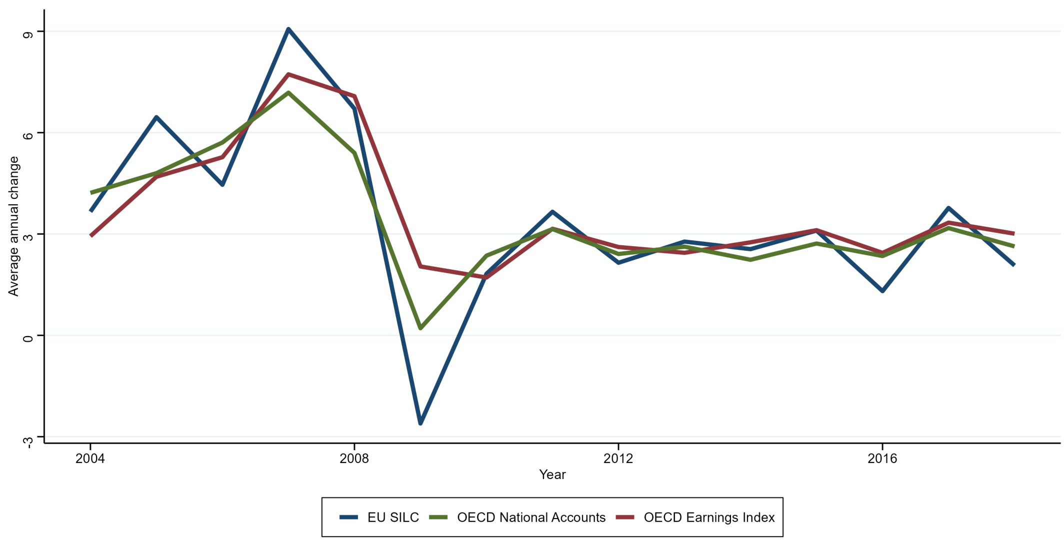
Fig. C.2.2: Net wage growth EU-SILC aggregated vs published by Eurostat for countries with available data for net wages



Note: Wage growth refers to average annual change in nominal wages. Results are shown for all countries with available data for net wages.
Source: Authors' computations based on EU-SILC.

Figure C.2.3 compares our baseline wage growth measure from EU-SILC (PY010G) to established wage data from national accounts as well as surveys and registries. While our measure aligns closely with both series for the 2010-2015 period, some differences occur in the earlier and later years. However, differences between wage measures of different sources are rather common: a correlation analysis reveals that EU-SILC data still aligns closer to each of the two OECD series than the two series align between each other. Our measure aligns closest with the national accounts measure for average annual wages per full-time equivalent dependent employee (CP-NCU). It is computed by dividing the total wage bill by the number of employees multiplied by the ratio of average usual weekly hours per full-time employee to average usually weekly hours for all employees. This approach is rougher compared to ours since we compute hourly wage growth based on respective hours worked on an individual basis. Our measure also conforms the OECD Earnings Index (MEI) that aggregates wage developments (LCEAPR IXOBSA). Differences are likely because the MEI only includes private sector employees based on survey and administrative data.

Fig. C.2.3: Wage growth EU SILC (gross) vs OECD National Accounts vs OECD Earnings Index

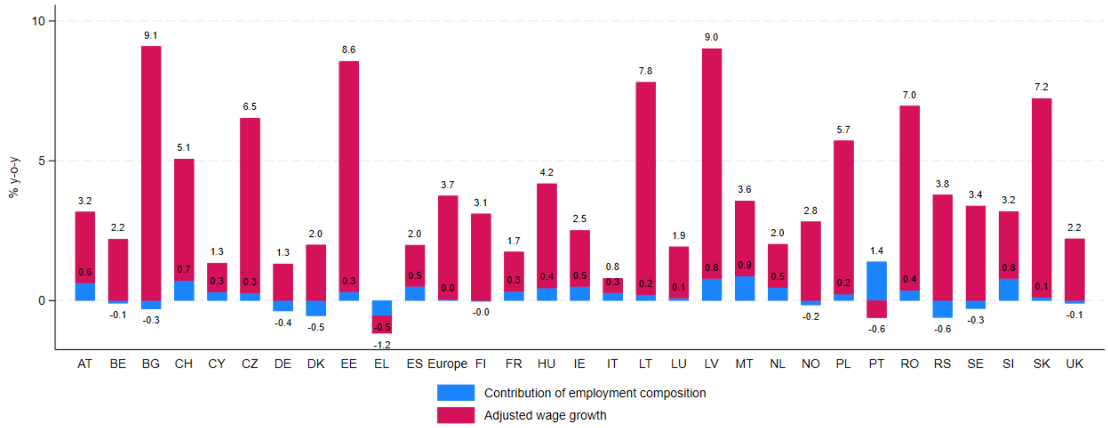


Note: Europe simple average includes all country-year observations with data available for both measures: AUT, BEL, DEU, DNK ESP, FIN, FRA, GBR, ISL, ITA, LTU, LUX, LVA, POL, PRT, SVK.
Source: EU-SILC and OECD.

C.3 Appendix: Micro Adjustment

C.3.1 Adjusted vs unadjusted wage growth

Fig. C.3.1: Wage growth adjusted with all controls for European countries



Note: Wage growth is adjusted for a changing employment composition by contract, gender, migration background, educational attainment, and work experience. Wage growth refers to the average annual change in nominal wages 2003-2017. Europe refers to the simple average of all countries shown. Periods may be limited depending on countries' data availability.

Source: Authors' computations based on EU-SILC.

C.4 Appendix: Robustness

In Table C.4.1 we check the sensitivity of the competition effect when countries are excluded one at a time from the sample. The estimation is based on the model presented in column (1) of Table 2. In Table C.4.2 we report group-specific differences in the competition effect with respect to countries' institutional characteristics based on an alternative clustering procedure. The extensions are based on our baseline results presented in Table 3.

Table C.4.1: Sensitivity of labor market dualization to country exclusion

Country	Invol. Temp.	T-stat.	Country	Invol. Temp.	T-stat.
AT	-0.96***	-2.96	IT	-0.98***	-2.95
BE	-0.97***	-2.84	LT	-1.06***	-3.30
BG	-1.00***	-3.11	LU	-1.00***	-3.10
CH	-0.96***	-3.01	LV	-0.87**	-2.70
CY	-1.04***	-3.09	MT	-0.97***	-2.92
CZ	-0.94***	-2.81	NL	-0.97***	-2.95
DE	-0.91***	-2.80	NO	-1.01***	-3.07
DK	-0.94***	-2.87	PL	-0.81**	-2.26
EE	-1.00***	-3.00	PT	-0.92***	-2.78
EL	-0.86**	-2.43	RO	-0.83**	-2.67
ES	-0.93**	-2.52	RS	-0.91**	-2.55
FI	-0.90**	-2.75	SE	-0.98***	-2.85
FR	-0.94***	-2.85	SI	-0.94***	-2.88
HU	-1.08***	-3.31	SK	-0.81**	-2.62
IE	-1.05***	-3.09	UK	-0.95***	-2.89

Note: Two-tailed significance levels: $*$: 10% $**$: 5% $***$: 1%. T-statistics are based on cluster-robust standard errors by country. Dependent variable: adjusted wage growth, i.e., counterfactual overall aggregate wage growth assuming a constant share of temporary employees in total employees over time (base year: 2004). Independent variable: involuntary temporary employment (Invol. Temp).

Table C.4.2: The competition effect and the role of institutions

Dep. var.: wage growth perm. workers	Inst: TUD (1)	Inst: CBC (2)	Inst: Coord. (3)	Inst: EPL (4)	Inst: PCA (5)	Inst: Metten I. (6)
$Prod_t$	0.21	0.32*	0.29*	0.01	0.25	0.40**
	(1.23)	(1.79)	(1.84)	(0.06)	(1.43)	(2.53)
$Infl_t$	0.62	0.47	0.50	-0.15	0.44	0.46
	(1.61)	(1.03)	(1.15)	(-0.42)	(0.99)	(1.00)
U_t	-0.57**	-0.57***	-0.58***	-0.35**	-0.64***	-0.51**
	(-2.43)	(-2.83)	(-3.09)	(-2.42)	(-3.07)	(-2.71)
$Invol.\,Temp_t$						
...$low\ Inst$	-1.28***	-1.21**	-1.50***	-0.30	-1.42***	-1.72***
	(-3.27)	(-2.30)	(-3.57)	(-0.84)	(-3.04)	(-5.10)
...$med.\ Inst$	-1.12	-0.50	-0.80	-0.75**	-1.51**	-0.60
	(-1.59)	(-1.11)	(-1.41)	(-2.12)	(-2.12)	(-1.42)
...$high\ Inst$	0.05	-0.57	-0.22	-2.07***	-0.04	-0.13
	(0.15)	(-1.09)	(-0.76)	(-3.73)	(-0.17)	(-0.38)
TUD_t	0.40***					
	(3.36)					
CBC_t		-0.08*				
		(-1.89)				
$Coord_t$			-1.26*			
			(-1.90)			
EPL_t				-1.10		
				(-0.92)		
PCA_t					-2.26*	
					(-2.00)	
Cons	-1.41	15.07***	14.34***	14.62***	11.75***	10.24***
	(-0.32)	(4.23)	(3.46)	(4.33)	(5.79)	(3.58)
Model	FE	FE	FE	FE	FE	FE
TimeD	incl.	incl.	incl.	incl.	incl.	incl.
N	300	302	344	278	302	340

Note: Two-tailed significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. T-statistics are reported in parenthesis and are based on cluster-robust standard errors by country. The grouping into low, medium and high countries corresponds to the first, second and third tercile of the respective institutional variable. Explanatory variables include labor productivity growth ($Prod_t$), inflation ($Infl_t$), unemployment (U_t), involuntary temporary employment ($Invol.\ Temp_t$.), trade union density (TUD_t), collective bargaining coverage (CBC_t), coordination of wage setting ($Coord_t$) and employment protection legislation (EPL_t). PCA_t is an index generated by principal component analysis based on TUD, CBC, and Coord (first component). Note, that for running the PCA analysis we imputed some missing observations in TUD and CBC to increase sample size. Metten I. is a composite index for union strength developed by Metten (2021).

C.5 Appendix: Variable measurement and sources

Table C.5.1: Variable measurement and sources

Abbrev.	Variable	Measurement	Source
Wage growth	Annual average change in nominal wages	Gross employee cash or near cash income (PY010G)/months employed (PL073+PL074)/hours worked (PL060) (separated for full time and part time) aggregated with personal cross-sectional weights (PB040)	EU-SILC
Prod.	labor productivity growth	Nominal GDP/employment*100, annual change	Eurostat (naida_10_pe, naida_10_gdp)
Infl.	HICP Inflation	Annual average change of HICP	Eurostat (prc_hicp_aind)
Exp. Infl.	Inflation expectations	Monthly consumer survey asking for price trends over the next months, yearly average over 12 months	European Commission
U	Unemployment rate (U-3)	Unemployed (ILO definition) in % of active working age population (aged 15-74)	Eurostat (lfsa_urgan)
Invol. Temp	Involuntary temporary employment	Employees with a temporary contract who could not find a permanent job, in % of active working age population (aged 15-74)	Eurostat (lfsa_etgar, lfsa_eegais, lfsa_agan)
Temp	Temporary employment	Employees with a temporary contract, in % of active working age population (aged 15-74)	Eurostat (lfsa_etgadc, lfsa_agan)
Vol. Temp	Voluntary temporary employment	Temp − Invol. Temp	
U-5	U-5 Unemployment rate	Unemployed incl. discouraged (not seeking, but available) and marginally attached workers (available, but not seeking)	Eurostat (lfsa_urgan, lfsa_sup_age)
NAWRU	Non-accelarating wage rate of unemployment	Estimates from a model-based approach, European Union, 2017.	European Commission
Invol. Part	Involuntary part-time employment	Share of involuntary part-time employees in labor force, in %	OECD Statistics
EPL	Employment protection legislation	Strictness of employment protection − individual and collective dismissals (regular contracts)	OECD Statistics
TUD	Trade union density	Union members in % of employees (administrative and survey data)	OECD, ICTWSS
CBC	Collective bargaining coverage	Percentage of employees with the right to bargain	OECD Statistics
Coord	Coordination of wage setting	degree of coordination in wage bargaining on an ordinal 5-point scale	OECD Statistics

Table C.5.2: Country grouping according to strength of labor market institutions

	TUD			CBC			Coord			EPL			PCA		Metten I.	
Country	Mean	Base	Robust	Mean	Base	Robust	Mean	Base	Robust	Mean	Base	Robust	Mean	Robust	Mean	Robust
AT	29	2	2	98	3	3	4.0	3	3	2.3	1	2	1.3	3	8.4	3
BE	54	3	3	96	3	3	5.0	3	3	1.8	1	1	2.3	3	9.1	3
BG	18	1	1	.	.	.	1.4	1	1	.	.	.	.	.	5.7	1
CH	17	1	1	52	2	2	3.0	2	3	1.4	1	1	-0.4	2	4.9	1
CY	54	3	3	.	.	.	2.3	2	2	.	.	.	.	.	6.6	2
CZ	15	1	1	30	1	1	1.0	1	1	3.4	2	3	-1.8	1	5.7	1
DE	19	1	2	60	2	2	4.0	3	3	2.6	2	3	0.2	2	7.6	3
DK	68	3	3	79	2	2	4.0	3	3	1.5	1	1	1.9	3	7.3	2
EE	7	1	1	26	1	1	1.8	1	1	2.0	1	1	-1.8	1	5.3	1
EL	22	2	2	67	2	2	1.8	1	1	2.8	2	3	-0.6	2	7.6	2
ES	16	1	1	86	3	3	2.9	2	2	2.2	1	2	0.2	2	7.5	2
FI	68	3	3	87	3	3	3.6	3	3	2.1	1	1	1.9	3	8.8	3
FR	9	1	1	98	3	3	2.0	1	2	2.6	2	2	-0.2	2	7.9	3
HU	11	1	1	25	1	1	1.0	1	1	1.8	1	1	-2.0	1	6.8	2
IE	29	2	2	38	2	1	2.6	2	2	1.2	1	1	-0.6	1	6.4	2
IT	35	2	3	80	2	3	3.2	3	3	2.9	2	3	0.7	3	8.9	3
LT	9	1	1	10	1	1	1.0	1	1	2.5	2	2	-2.4	1	5.4	1
LU	36	2	3	57	2	2	2.2	2	2	2.1	1	2	-0.2	2	7.9	3
LV	14	1	1	17	1	1	1.0	1	1	3.0	2	3	-2.1	1	4.9	1
MT	55	3	3	.	.	.	1.0	1	1	.	.	.	.	.	.	.
NL	19	1	2	81	2	3	4.1	3	3	3.3	2	3	0.7	3	7.3	2
NO	50	3	3	73	2	2	4.0	3	3	2.3	2	2	1.3	3	8.2	3
PL	18	1	2	19	1	1	1.0	1	1	2.3	1	2	-2.0	1	5.6	1
PT	19	1	2	78	2	2	2.0	1	2	3.8	2	3	-0.3	2	7.3	2
RO	32	2	2	.	.	.	2.5	2	2	.	.	.	.	.	6.6	2
RS	.	.	.	.	.	.	1.0	1	1	1.7	1	1	.	.	5.9	1
SE	69	3	3	90	3	3	4.0	3	3	2.5	2	2	2.1	3	9.0	3
SI	29	2	2	75	2	2	2.7	2	2	2.4	2	2	0.2	2	7.3	2
SK	16	1	1	34	1	1	1.8	1	1	2.7	2	3	-1.4	1	5.9	1
UK	26	2	2	30	1	1	2.0	1	2	1.4	1	1	-1.1	1	5.5	1

Note: Institutional variables are trade union density (TUD), collective bargaining coverage (CBC), coordination of wage setting (Coord) and employment protection legislation (EPL). PCA is an index generated by principal component analysis based on TUD, CBC, and Coord and Metten I. is a composite index for union strength developed by Metten (2021). Countries are classified either into two (1 - low, 2 - high) or three groups (1 - low, 2 - medium, 3 - high). Base and Robust refer to the groupings considered in the models of Tables 3 and C.4.2 respectively.

Bibliography

Eurostat (2018). *EU-SILC Eurostat Quality Report*. Luxembourg: Publications Office.

GESIS (2021). Missy - metadata for official statistics. https://www.gesis.org/en/missy/metadata/EU-SILC/, note = Accessed: 2021-04-16,.

Goedeme, T. (2013). How much confidence can we have in EU-SILC? Complex sample designs and the standard error of the Europe 2020 poverty indicators. *Social Indicators Research*, 110(1):89–110.

Goedeme, T. and Trindade, L. Z. (2020). MetaSILC 2015: A database on the contents and comparability of the EU-SILC income variables [Data file].

Lohmann, H. (2011). Comparability of EU-SILC survey and register data: The relationship among employment, earnings and poverty. *Journal of European social policy*, 21(1):37–54.

Trindade, L. Z. and Goedeme, T. (2019). A new database on the contents and comparability of the income variables in EU-SILC: Metasilc 2015. *International Journal of Microsimulation*, 12.

Zardo Trindade, L. and Goedeme, T. (2016). Notes on updating the EU-SILC UDB sample design variables 2012-2014. Working Paper 1602, Herman Deleeck Centre for Social Policy, University of Antwerp.

Appendix D

Appendix to "What do unemployed workers want: Guaranteed jobs or guaranteed income?"

D.1 Research design

D.1.1 Weighting for robustness

Table D.1.1: Sample, continuous variables

Variable	Unweighted Mean	Unweighted SD	Weighted Mean	Weighted SD	valid N
Outcomes					
Support for guaranteed jobs	0.84	0.36	0.86	0.35	1,215
Support for guaranteed income	0.63	0.48	0.65	0.48	1,215
Willingness to accept a guaranteed job	0.63	0.48	0.66	0.47	1,215
Controls					
Health status (-)	0.19	0.20	0.20	0.20	1,213
Unemployment duration	10.43	10.75	11.01	12.94	1,215
Unemployment benefits (log)	6.75	0.40	6.70	0.40	1,177
Occupational prestige	0.34	0.21	0.28	0.23	1,136
Discrimination	0.12	0.22	0.15	0.25	1,215
Stigma	0.38	0.26	0.42	0.27	1,159
Women	0.29	0.45	0.42	0.49	1,215
Age	40.65	12.08	39.81	12.64	1,215
Migration Background	0.52	0.50	0.49	0.50	1,215

D.2 Survey

D.2.1 Survey details

Table D.2.2: Number of Interviews Conducted in Languages other than German

Language	Full Interview	Parts of Interview
Turkish	3	9
Serbo-Croatian	11	57
Arabic	5	8
Romanian	3	11
Hungarian	9	32
Total	31	117

Table D.2.3: Response rate of addresses used

	N	%
Conducted Interviews	1,215	2.4
Refusals	5,952	11.6
Not Reached	10,810	21.2
Sample-neutral Dropouts (no unemployed person)	33,123	64.8
N (Random Addresses)	51,100	100.0

D.2.2 Survey questionnaire

D.2.2.1 Screening question

Are you currently . . . ? [READ TO RESPONDENT]

- without work or orders and registered with the AMS . [→ SURVEYED AND INCLUDED IN SAMPLE]
- without work or orders and not registered with the AMS . . . [→ SURVEYED AND INCLUDED IN SAMPLE]

- employed in short-time work . . . [→ SURVEYED BUT NOT INCLUDED IN SAMPLE]
- employed, not in short-time work . [→ SURVEYED BUT NOT INCLUDED IN SAMPLE]
- all others . . . . [→ NOT SURVEYED]

D.2.2.2 Outcomes

Support for guaranteed jobs

There is currently much discussion about a job guarantee. Anyone who has been unemployed for longer than 1 year is offered a publicly funded job. You can decide voluntarily whether you want to accept it or not. These jobs should involve non-profit activities in the municipality or district. These jobs are paid a collectively-bargained minimum wage of

- €1,000 *(Group 1)*
- €1,500 *(Group 2)*
- €2,000 *(Group 3)*

net per month from the state.

Are you for or against such a job guarantee?

1. For.
2. Against.
3. I don't know, I can't say.
4. No answer.

Willingness to accept a guaranteed job

And would you accept such a job under a job guarantee?

1. Yes.
2. No.
3. I don't know, I can't say.
4. No answer.

Support for guaranteed income

There is much discussion about an unconditional basic income. Every person living in Austria would receive

- €1,000 *(Group 1)*
- €1,500 *(Group 2)*
- €2,000 *(Group 3)*

net per month from the state, with nothing in return and regardless of whether she works, and of her income or assets. At the same time, there would no longer be any social benefits as there are now.

Are you for or against an unconditional basic income?

1. For.
2. Against.
3. I don't know, I can't say.
4. No answer.

D.2.2.3 Controls

Health status

In the last 2 weeks, did you suffer from the following complaints: never, on a few days, on more than half of the days, or on almost every day?

	Never	On few days	On more than half of the days	Almost every day
1) Headache	1	2	3	4
2) Stomach ache	1	2	3	4
3) Problems falling asleep or sleep disturbances	1	2	3	4
4) Depressive thoughts	1	2	3	4
5) Nervousness, anxiety, and tension	1	2	3	4
6) Not being able to stop or control worries	1	2	3	4

Unemployment duration

In which month and year did you become unemployed?

Month _______________

Year _______________

Unemployment benefits

How much money do you have at your disposal each month during unemployment, be it from your unemployment benefit, unemployment assistance, or social benefits such as family allowance? You don't have to tell me the exact amount, an approximate figure will suffice.
(*Interviewer: ask again if necessary*)

1. Below €450

2. €451 - 600
3. €601 - 800
4. €801 - 1.000
5. €1.001 - 1.200
6. €1.201 - 1.400
7. €1.401 - 1.600
8. €1.601 - 1.800
9. €1.801 - 2.000
10. €2.001 - 2.200
11. €2.201 - 2.400
12. €2.401 - 2.600
13. €2.601 - 2.800
14. €2.801 - 3.000
15. €3.001 - 3.200
16. €3.201 - 3.400
17. €3.401 - 3.600
18. €3.601 - 3.800
19. €3.801 - 4.000
20. More than €4,000
21. Don't know
22. Not specified

Occupational prestige

What was your last occupation? Please tell me the exact name of the profession or describe the nature of your work and professional position. (*Interviewer: Assign to Ö-ISCO 08 2-digit: list attached. If not assignable, take note.*)

Discrimination

Did you feel discriminated against in the following areas, i.e., treated less favorably for no objective reason?

	Area	Yes	No	No answer
1)	In your last job?	1	2	99
2)	At the end of your last employment relationship?	1	2	99
3)	When applying for new jobs?	1	2	99
4)	During counseling at the AMS?	1	2	99

Stigma

Do the following statements apply to you very much, quite a bit, a little, or not at all?

	Statement	Very much	Fairly	A little	Not at all
1)	I find it difficult to maintain relationships with people who are employed.	1	2	3	4
2)	I believe that most people are more prejudiced against the unemployed than they openly say.	1	2	3	4
3)	I feel personally affected by prejudice against the unemployed.	1	2	3	4
4)	In certain situations I try to hide the fact that I am unemployed.	1	2	3	4
5)	I am ashamed of being unemployed.	1	2	3	4

Gender

Finally, a few questions about yourself.

Your gender?

- Man 1
- Woman 2
- Diverse 3

Age

How old are you?

Migration background

- In which country were you born?
- In which country was your mother born?
- And your father?

	A)	B)	C)
Austria	1	1	1
Germany	2	2	2
Hungary/Czech Republic/Slovakia	3	3	3
Former Yugoslavia/Albania	4	4	4
Bulgaria/Romania	5	5	5
Other EU countries	6	6	6
Other Europe	7	7	7
Turkey	8	8	8
Arab region	9	9	9
Iran	10	10	10
Africa	11	11	11
China	12	12	12
Philippines	13	13	13
Other Asia	14	14	14
Other country	15	15	15
don't know/no answer	16	16	16