

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

Maximilian Kasy\*      Lukas Lehner†

February 15, 2026

## Abstract

We evaluate a guaranteed job program in Austria in 2020-2024. 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 on economic and non-economic well-being. At the municipality level, we find a reduction of long-term unemployment and no negative employment spillovers. There are positive anticipation effects on well-being, status, and social inclusion. We estimate program costs of 237 Euro per participant and month. Participant income increased by 102 Euro.

*JEL codes:* I38, J08, J45

---

\*Department of Economics, University of Oxford. maximilian.kasy@economics.ox.ac.uk

†Department of Social Policy, University of Edinburgh. lukas.lehner@ed.ac.uk

We thank Sven Hergovich, who initiated the MAGMA job guarantee program, and the employees of the *AMS Niederösterreich*, especially Daniel Riegler and Elisabeth Reiter, 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 whose support 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 Mobasser, Claire Montialoux, Mathilde Muñoz, Brian Nolan, Aaron Reeves, Soohyun Roh, Jesse Rothstein, Emmanuel Saez, Benjamin Schoefer, Anna Stansbury, Andreas Steinmayr, Tim Vlandas, 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.

This study was registered as AEARCTR-0006706 (Kasy and Lehner, 2020). The authors did not receive any financial compensation for this study. Their contract guaranteed that study findings, as described in the pre-analysis plan, would be submitted for publication regardless of the experiment's outcome.

# 1 Introduction

Employment, with appropriate wages and working conditions, can 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. Legislative initiatives proposing job guarantees have been discussed on both sides of the Atlantic (Senate, 2023; Parliament, 2023). 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 this paper, we evaluate a pilot programme that aimed to address this lack of evidence – the MAGMA job guarantee program, which ran from 2020 to 2024 in 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 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 (experimental) evidence on the non-monetary benefits of employment, which have been suggested by a large correlational literature outside economics, by recent willingness-to-pay estimates for job amenities, and by an experiment (Hussam et al., 2022) for 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<sup>1</sup> was a pilot programme implemented by the Public Employment Service (Arbeitsmarktservice, *AMS*) of Lower Austria in the municipality of Gramatneusiedl from October 2020 to March 2024. We co-designed this policy experiment with the AMS, using pairwise matched randomization for program enrollment. MAGMA provided 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 was voluntary, but everyone who was offered a job after the two month preparatory training accepted the opportunity. In total, 107 people participated in MAGMA over the 3.5 year duration of the program. We describe this program in detail in Section 2.

The stated policy goal of the MAGMA program was to directly eradicate long-term

---

<sup>1</sup>MAGMA is short for “Modellprojekt Arbeitsplatzgarantie Marienthal,” which translates as “model project job guarantee Marienthal.” Marienthal is one part of the municipality of Gramatneusiedl. Ninety years prior to our experiment, Marienthal was the location of a pathbreaking study on the impact of long-term mass unemployment in the Great Depression (Jahoda et al. 1933, “Die Arbeitslosen von Marienthal.”). We discuss the historical arc from the 1930s study to our experiment in Appendix E.

unemployment in the municipality, and thereby to improve participants' economic and social situation. In contrast to conventional active labor market policies (Card et al., 2010), regular market employment was not a likely or expected outcome for most participants. 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, 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.<sup>2</sup>

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 direct effect of the program (net of possible anticipation effects), by comparing participants across the two groups, 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.<sup>3</sup> The synthetic control town is a convex combination of similar towns. The weights for this comparison were pre-registered before the start of the program. This allows us to estimate total effects of the program at the town level, on both the treated and untreated, including anticipation effects and potential spillovers on non-eligible residents, in particular effects on short-term unemployment.

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. This allows us to estimate anticipation effects in the short term by contrasting those not-yet-treated to control town individuals, and total effects in the long term, by contrasting those treated to control town individuals.

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. The experimental comparison identifies direct effects. Comparison of future

---

<sup>2</sup>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.socialscisceregistry.org/trials/6706>. Evaluation strategy 3 was added later.

<sup>3</sup>Throughout this paper, we use “town” and “municipality” interchangeably.

participants to control town individuals identifies anticipation effects. The synthetic control comparison identifies equilibrium effects. We discuss this in greater detail in Section 3.2.

**Main findings** Our main empirical findings can be summarized as follows. For the **individual-level** experimental comparison of current to future participants, we find large positive effects of participation on economic well-being (employment, income, and economic security). These effects on employment and income are as expected, but they are not entirely mechanical since (i) program participation is voluntary, and (ii) if individuals had declined participation, they would still be eligible to receive unemployment benefits. We furthermore 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.

Turning to **municipality-level** effects, which we estimate using the synthetic-control approach, our headline finding is a large reduction in municipality-level unemployment due to the program. This in turn is driven by a near-elimination of long-term unemployment in Gramatneusiedl. We do not find any systematic increase of short-term unemployment, and thus no evidence of negative spillovers. Over the three and a half years of the job guarantee, eligible workers spent 555 more days in employment, driven not only by direct job provision but also by a 17 percent rise in unsubsidized employment and a doubling of self-employment.

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; nevertheless, most of the program benefits only manifest after the start of employment. Considering outcomes in subsequent years, we find that the initial effect sizes largely persist, with little attenuation over time. This suggests that the benefits of a guaranteed job are sustained beyond the initial period.

We also evaluate **program costs**, both from the perspective of the AMS and from the perspective of the public sector at large. The program raised direct costs for the AMS in the short run, which were offset over time by increased transitions into non-subsidized employment. Increased expenditures were furthermore partially compensated by both revenues of the social enterprise, and increased tax and social insurance payments from participants. Taking these into account, we obtain an estimated net cost of the program of

237 Euro per participant and month, over the duration of the program, while participant net income increased by 102 Euro over the same duration. The increase in net costs largely reflects increased expenditures in the initial phase of the program. In the second half, the net costs fell to 41 Euro per participant and month, while the increase of participant net income rose to 228 Euro.

**Unintended consequences: Theory versus evidence** To interpret our findings, it is useful to put them in the context of economic theory. We do so in Appendix A, where we discuss two models of the labor market. The first is a model of job search, with endogenous search effort of the unemployed. Eligibility to participate in the MAGMA job guarantee starts after 9 months of unemployment. This might provide incentives to reduce search effort and prolong unemployment. Our search model suggests that this would lead to lower job-finding rates before eligibility, and to rates that decline more steeply over time, relative to the counterfactual of no job guarantee. Empirically, comparing hazard rates out of short-term unemployment between Gramatneusiedl and the synthetic control municipalities, we find the opposite: Gramatneusiedl has *higher* transition rates out of short-term unemployment, which *decline less* over time, relative to the control. There is thus no evidence of reduced search effort.

Our second model is a (static) model of labor demand with different types of workers, some of whom are at risk of long-term unemployment. We assume that wages are (in the short run) fixed institutionally, by sectoral collective bargaining, and adjustments in the local labor market happen via the employment margin. This is realistic in the Austrian context. In this model, depending on the cross-derivative of aggregate output, employment of ineligible workers might increase or decrease when a job-guarantee is introduced. Our synthetic control estimates, discussed above, imply that there is no significant increase or decrease of employment of ineligible workers. This suggests a cross-derivative of aggregate output across types of workers close to zero.

**Literature** A large literature studies the effectiveness of *active labor market policies* (ALMPs) with a common conclusion that job search programs are somewhat effective in improving participants' unsubsidized future employment prospects (Card et al., 2010), as are (sectoral) training programs (Card et al., 2018; Katz et al., 2022) whereas public employment programs are not (Card et al., 2010, 2018). Two points are worth emphasizing in this context. First, most existing studies focus on different outcomes, notably unsubsidized employment and earnings after program participation (Heckman et al., 1999; Card et al., 2010; Crépon and van den Berg, 2016; Card et al., 2018). By contrast, we are primarily interested in the impact on the community and on participant welfare, without an explicit policy objective for participants to enter unsubsidized employment. Our study, thus, differs from evaluations of *transitional* employment programs aimed at improving unsubsidized employment after program participation (Hollister et al., 1984;

Couch, 1992; Uggen, 2000; Cook et al., 2015; Valentine and Redcross, 2015; Cummings and Bloom, 2020).

Second, most evaluations emphasize individual outcomes (though more recently, *spillover* effects have become a more common focus in the labor market policy literature, e.g. Crépon et al. 2013; Ferracci et al. 2014; Gautier et al. 2018; Johnston and Mas 2018; Huber and Steinmayr 2021; Cheung et al. 2025). We examine spillovers from a job guarantee that plausibly creates additional jobs rather than redistributes existing ones, as in job counseling. Our findings contrast with evidence from India’s National Rural Employment Guarantee Scheme, where Muralidharan et al. (2023) document positive wage spillovers. Similarly, Imbert and Papp (2015) and Franklin et al. (2024) find sizeable positive wage spillovers from public work programs on private sector wages in India and Ethiopia, respectively.

Our study relates to recent evaluations of *public employment schemes* in 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). Building on these studies, we provide the first experimental evaluation of a job guarantee program in a *rich country* context.

Our paper also relates to the large literature on *non-economic consequences of (un)employment* (Clark and Oswald, 1994; Korpi, 1997; Strandh, 2001; Clark, 2003, 2006; Knabe et al., 2010; Huber et al., 2011; Young, 2012; Haushofer and Fehr, 2014; Avendano and Berkman, 2014; Brand, 2015). This literature finds strong associations of (un)employment with well-being, and health. A number of papers rely on quasi experimental variation to study non-economic benefits (Kassenboehmer and Haisken-DeNew, 2009; Hetschko et al., 2014; Pohlan, 2019), which extends to participation in active labor market programs (Baekgaard et al., 2024) and employment in direct job creation programs (Ivanov et al., 2020). Some studies focused on sub-groups such as disadvantaged youth or previous offenders have been able to demonstrate the causal effect of employment programs on well-being (Heller, 2014, 2022; Aizer et al., 2024; Bhatt et al., 2024). Relatedly, a series of recent papers in economics estimates willingness to pay for a variety of job-related amenities (Ariely et al., 2008; Mas and Pallais, 2017; Kaplan and Schulhofer-Wohl, 2018; Dube et al., 2022; Maestas et al., 2023; Caldwell et al., 2025), building on the long literature on equalizing differences (see Brown (1980) and Rosen (1986) for early contributions). An older psychological literature also examined the psychological consequences of unemployment, e.g. Eisenberg and Lazarsfeld (1938)<sup>4</sup> Modern work in sociology and social psychology describes non-economic benefits of employment

---

<sup>4</sup>Lazarsfeld, a co-author of this review and pioneer in empirical social research methods led the original Marienthal study (Jahoda et al., 1933) and later became president of the American Sociological Association.

originating from the “need to belong” (Baumeister and Leary, 1995), and the “desire for status,” (Anderson et al., 2015). We build on this tradition by providing causal evidence on the effects of employment on well-being for the population of long-term unemployed, in contrast to both the older and more recent predominantly correlational evidence.

**Roadmap** The rest of this paper is structured as follows. Section 2 provides further context and details regarding the MAGMA job guarantee program. Section 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 4 discusses our empirical findings, for each of the three approaches, and evaluates the program costs. Section 5 concludes.

Appendix A discusses models of job search and of labor demand, in the context of which we interpret our empirical findings. Appendix B presents additional details on our evaluation strategies, additional empirical findings, and robustness checks. Appendix C 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 D provides a detailed list of all the jobs that were created in both the market and non-market sector, reports statements by program participants, and describes some of the jobs that were created in greater detail. Appendix E contrasts Jahoda et al. (1933) and our study to discuss changes in the methodology of empirical social science over the last 90 years.

## 2 Background and program details

From October 2020 to March 2024, the Public Employment Service of Lower Austria (Arbeitsmarktservice Niederösterreich, *AMS NÖ*) piloted an intervention that aimed 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 provided a guaranteed job to people in long-term unemployment. The intervention took place in the municipality of Gramatneusiedl in Lower Austria. 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., 1933).

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) at baseline were eligible to participate. The experimental sample includes all residents unemployed for more than 9 months in September 2020.

Residents who reached the (higher) eligibility threshold of being unemployed for at least 12 months after September 2020 were eligible to participate in the program, but are not part of our experimental comparison. Over the duration of the program, there were 112 eligible individuals, including 62 experimental participants and 50 late entrants.

Out of those, 80 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.

The duration for the project was set until March 2024, and the project had a budget of EUR 7.4 million. A complementary study to ours (Quinz and Flecker, 2022), summarized in Appendix D.5, is based on a mixed-methods design and qualitative interviews. Their evaluation was conducted independently, and does not overlap with any of our surveys. The program implementation overlapped with the Covid-19 pandemic. The program nevertheless took place as planned. We provide details in Appendix D.6.

**Preparatory training** The program was implemented by the private service-provider *it.works*, which specializes in implementing active labor 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 included both those employed at *it.works*, and those working for private employers. This minimum wage of around EUR 1,500 per month in 2020, compared to an average monthly wage of EUR 3,308 in the municipality.<sup>5</sup>

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

---

<sup>5</sup>By 2023, the minimum wage had increased to around EUR 1,700.

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. Appendix D 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 statements by some of the participants in the program. Figure A.9 in Appendix D 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, and tailored to their individual needs. 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.

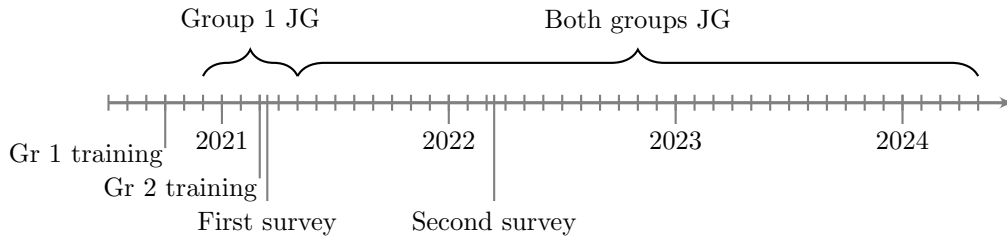
**Voluntary participation** Regular unemployment benefits in Austria replace 55 percent of previous net earnings, with additional supplements for dependents, and are available six to twelve months (depending on work history and age). Thereafter, benefits decline slightly, and are means tested on personal income, but are available indefinitely. Receipt of unemployment benefits requires active job search, which is monitored by the AMS.

Outside the pilot program, according to current law (*Arbeitslosenversicherungsgesetz AIVG §9*), recipients of unemployment benefits are assigned to labor market programs by the AMS, and they have the obligation to participate in these programs and to accept any employment offer that conforms to their skill set. If they do not, 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.

**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

Figure 1: Timeline of the Evaluation



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. Figure 1 summarizes this timeline.

### 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 were “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).<sup>6</sup> 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 A.10 in Appendix C. 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 to each of these outcomes is listed in Appendix C. We collected information on a rich

<sup>6</sup>The description in this section follows our pre-analysis plan.

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 Appendix C for references and details.

To enable a compact presentation of our results in Section 4, we normalize all individual-level outcomes, such that higher values correspond to “better” outcomes, and such that the range of these variables is the interval  $[0, 1]$ ; cf. Table A.10.

The third set of outcomes, defined at the municipality level, is again based on administrative data from the “AMDB Erwerbkarrieremonitoring.” 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”).

**Motivating our focus on non-economic outcomes** A key feature of our evaluation is the focus on non-economic outcomes, beyond employment and income, and in particular on the “latent and manifest benefits of work.” This focus deserves some justification.

We note, first, that transition rates from long-term unemployment to unsubsidized market employment are very low (around .12 per month) for the population of interest. Recognizing this, the policymaker’s declared objective for MAGMA has *not* been to facilitate such transitions, but instead to improve participant well-being: As stated by Sven Hergovich, who initiated MAGMA, “It is better to directly employ people than to manage them in unemployment because work provides support, meaning, structure and dignity.”

Second, there is a growing empirical literature in economics recognizing that workers value employment for dimensions other than earnings, see for instance Ariely et al. (2008) for meaningful work, Mas and Pallais (2017) for scheduling flexibility, Kaplan and Schulhofer-Wohl (2018) for happiness, stress and meaning, Dube et al. (2022) for dignity at work, Maestas et al. (2023) for working conditions more broadly, and Caldwell et al. (2025) using evidence on the willingness to switch employers. This recognition in economics mirrors a long-standing focus on the non-economic dimensions of work in sociology; see e.g., Jahoda (1982), on whose work we build here.

Third, moving beyond the framework of utilitarian welfare economics that is implicit in willingness-to-pay estimates for job amenities such as those referenced above, there are normative reasons to intrinsically care about these non-economic dimensions of well-being. Rawls (1999), for instance, includes not only basic rights and liberties as well as income among his “primary goods,” but also opportunities and the social bases of self-respect. More broadly, Sen (1995) argues that individual well-being should be evaluated in terms of the “capabilities to function” in society, which again depend not only on economic means but also on aspects such as social inclusion, networks, and status.

### 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 started 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 (average direct effect on the treated, net of anticipation effects). It is restricted, however, to short-term individual-level outcomes measured in February 2021, before the second group of participants started 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 captures effects on residents who were not eligible to participate in the program because they were not long-term unemployed. This includes the short term unemployed, who numbered between 82 and 141 individuals in any given month during the program period.

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,<sup>7</sup> 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

---

<sup>7</sup>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*.

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 vectors  $x_1$  and  $x_2$  that are realizations of a random vector  $X$  is given by  $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot \text{Var}(X)^{-1} \cdot (x_1 - x_2)}$ . 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 1 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 1: Covariate balance for our matched pair design

Covariate	Mean Group 1	Mean Group 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” (*Erwerbkarrierenmonitoring, 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 constructed a synthetic control municipality in two steps. In the first step, we selected 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 Appendix B. The averages of these covariates for both Gramatneusiedl and the (synthetic) control municipalities are shown in Table A.2 in Appendix B. Most of our covariates are based on observations for the year 2019 (as measured in December). In addition to these co-

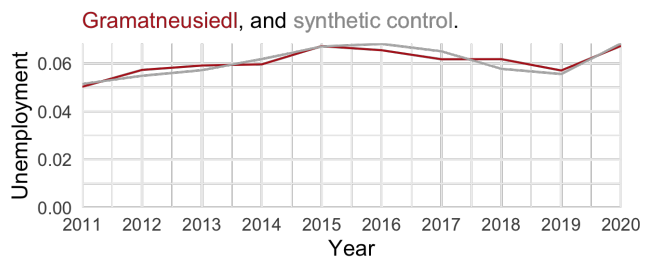
variates, we also included some covariates measured in July of 2020, after the onset of the Covid pandemic, to control for possibly heterogeneous impacts of this pandemic across municipalities. The averages of these covariates are shown in the bottom panel of Table A.2.

In the second step, we constructed 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, which lists all municipalities with non-negligible weights. The location of these municipalities is shown in Figure A.2 in Appendix B. The right side of Figure 2 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 Appendix B 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.

Figure 2: 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



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

While the control municipalities are chosen to be similar to Gramatneusiedl, this does not necessarily imply that the long-term unemployed in these municipalities are similar in terms of their demographics. For suggestive evidence that the sample of control town individuals is in fact similar to the set of participants, we again compare their baseline covariates. Table A.4 in Appendix B 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 (potentially biasing our effect estimates downwards), and (marginally) age, which is also higher in the control towns. When estimating individual-level contrasts to control municipalities, we adjust for baseline covariates. This corrects for any remaining imbalances of covariates between the long-term unemployed in Gramatneusiedl and in the control municipalities; for a causal interpretation of these estimates we thus do *not* require balance of baseline covariates. We do, however, rely on the assumption that unobservables are balanced conditional on observables.

### 3.2 Causal interpretation of estimands

**Spillover and anticipation effects** 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  $\bar{D}$  be the share of long-term unemployed in the municipality who are currently eligible. Let finally  $\epsilon_i$  be a vector of unobserved individual characteristics, which are not affected by the program. We can then assume that

$$Y_i = g(D_i, D_i^{+1}, \bar{D}, \epsilon_i), \quad (1)$$

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,<sup>8</sup> and the dependence on  $\bar{D}$  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  $\epsilon_i$  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 provides a mapping from these averages to the structural notation. Correspondingly, Table 3 provides a mapping from the contrasts we have been discussing so far to the corresponding average structural effects. For simplicity of notation, we neglect any possible non-stationarity in the distribution of  $\epsilon_i$ ; in principle, everything should be subscripted by time  $t$ .

Table 2: Identified averages

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

Table 3: Identified effects and roadmap

Contrast	Identified effect	Interpretation	Figures & Tables
<b>February 2021</b>			
Group 1 vs. Group 2	$E[g(1, 1, \frac{1}{2}, \epsilon_i) - g(0, 1, \frac{1}{2}, \epsilon_i)   L_i = 1]$	Average direct effect on the treated	Figure 3, Figure 4, Table 4
Group 2 vs. control town	$E[g(0, 1, \frac{1}{2}, \epsilon_i) - g(0, 0, 0, \epsilon_i)   L_i = 1]$	Average anticipation effect on the treated	Figure 10, Figure 11, Table 5, Table 6,
<b>After April 2021</b>			
Group 1 & 2 vs. control town	$E[g(1, 1, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)   L_i = 1]$	Average total effect on the treated	Figure 10, Figure 11
Gramatneusiedl vs. synth (short-term unemp)	$E[g(0, 0, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)   L_i = 0]$	Average spillover effect on the untreated	Figure 5, Figure 6
Gramatneusiedl vs. synth (total unemp)	$E[g(L_i, L_i, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)]$	Average total effect	Figure 5, Figure 6

Let us interpret these identified objects, as listed in Table 3. 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

<sup>8</sup>Note that anticipation effects also matter for the currently treated, who might or might not expect to be treated in the future.

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 additional assumption that these eligible individuals are not impacted by spillover effects, so that  $E[g(0, 1, \frac{1}{2}, \epsilon_i)|L_i = 1] = E[g(0, 1, 0, \epsilon_i)|L_i = 1]$ , this contrast identifies the **average anticipation effect on the treated**,  $E[g(0, 1, 0, \epsilon_i) - g(0, 0, 0, \epsilon_i)|L_i = 1]$ . This assumption is plausible, because all Group 2 individuals in February 2021 expected to participate in the job-guarantee shortly, and would not apply to jobs outside the program that could possibly be crowded out by Group 1 participants. If this assumption is violated, then our estimates of anticipation effects are contaminated by spillovers.

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 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.,

$$E[g(L_i, L_i, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)] = E[g(1, 1, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)|L_i = 1] \cdot P(L_i = 1) + E[g(0, 0, 1, \epsilon_i) - g(0, 0, 0, \epsilon_i)|L_i = 0] \cdot P(L_i = 0). \quad (2)$$

### 3.3 Inference

**Individual-level randomization inference** To perform inference for 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.

Following our pre-analysis plan, we report one-sided p-values, testing the null of no

effects against the alternative of positive effects. Because the randomization distribution under the null hypothesis of no effects is symmetric around 0, two-sided p-values can be calculated by doubling the one-sided p-values.

**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.

**Power** 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 .04 to .07, while the estimated treatment effects for our headline outcomes range from about .1 to .55. These standard errors are lower than for a classic RCT because of pairwise randomization.

Regarding power for detecting municipality-level spillover effects, permutation inference for the synthetic control estimates would designate an increase of short-term unemployment of more than 1% as significant at the 10% level. 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.

**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. We achieved lower response rates in the control towns, with 34% in 2021 and 30% in 2022. We adjust for baseline covariates (covariate means are reported in Table A.4) when comparing individual outcomes across towns, to mitigate the impact of possibly selective

non-response. To test for selective non-response, we furthermore perform balance tests. We do not find any significant differences in covariate means, as would be expected in the absence of differentially selective non-response – with the exception of benefit levels, which are slightly higher among control individuals. (Table A.3 - Table A.5). As a further robustness check, we finally estimate bounds on effects in the spirit of Lee (2009) and Semenova (2025), to account for selective non-response. These bounds are reported in Tables A.6 and A.7.

## 4 Findings

We are now ready to discuss our empirical findings.<sup>9</sup> Our headline findings are summarized by Figures 3 through 11 in this section, as well as Figures A.4 through A.6 in Appendix B. Individual-level estimates are also shown numerically in Table 4 through Table 6. 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.).

Table 7 and Figure 12 at the end of this section show our analysis of program costs; Table A.8 and Table A.9 in the appendix decompose these costs across time. Additional figures with results for further outcomes, alternative identification approaches, confidence intervals, and robustness checks can be found in Appendix B. Table 3 provides a roadmap through the findings presented in this section and in the appendix.

### 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 3, Figure 4, and Table 4 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 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

---

<sup>9</sup>The code implementing the following analysis has been uploaded to GitHub, at [https://github.com/maxkasy/Marienthal\\_Analysis](https://github.com/maxkasy/Marienthal_Analysis).

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 3, Figure 4, and Table 4 control linearly for baseline covariates (the same covariates as listed in Table 1), to adjust for potential non-random attrition in the survey. Figure A.7 and Figure A.8 in Appendix B 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.4 in Appendix B further shows confidence intervals for treatment effects, based on robust standard errors for the regressions with linear controls. As an additional robustness check to account for possible selective attrition, we consider bounds in the spirit of Lee (2009), modified to account for selection on observables in the spirit of Semenova (2025). See subsection B.5 in the appendix for details and the resulting bounds.

**Findings** For economic outcomes (shown in the top panels of Figure 3 and Table 4), measured using both survey and administrative data, we find highly significant positive effects.<sup>10</sup> 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 3 and Table 4 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 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 3 and middle panel of Table 4), 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 4 and the bottom panel of Table 4, 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

---

<sup>10</sup>Recall the normalization of these outcome variables from Table A.10: Employment and unemployment are defined as the share of days since the program started, and the monthly income is divided by 2000.

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 4 and the bottom panel of Table 4 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.

P-values in Figure 3 and Table 4 are for single hypotheses. To adjust for multiple-hypothesis testing, we apply the Benjamini-Hochberg (BH) procedure, to control the false discovery rate.<sup>11</sup> Applied to the estimates in the top two panels of Table 4, all test decisions are robust to the BH procedure, with the exception of the effect on the Well-being scale, which becomes marginally insignificant at the 5% level.

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. (1933) study, and because of the literature on the sociology of work which connects our study to Jahoda et al. (1933). The LAMB scale<sup>12</sup> 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 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.”

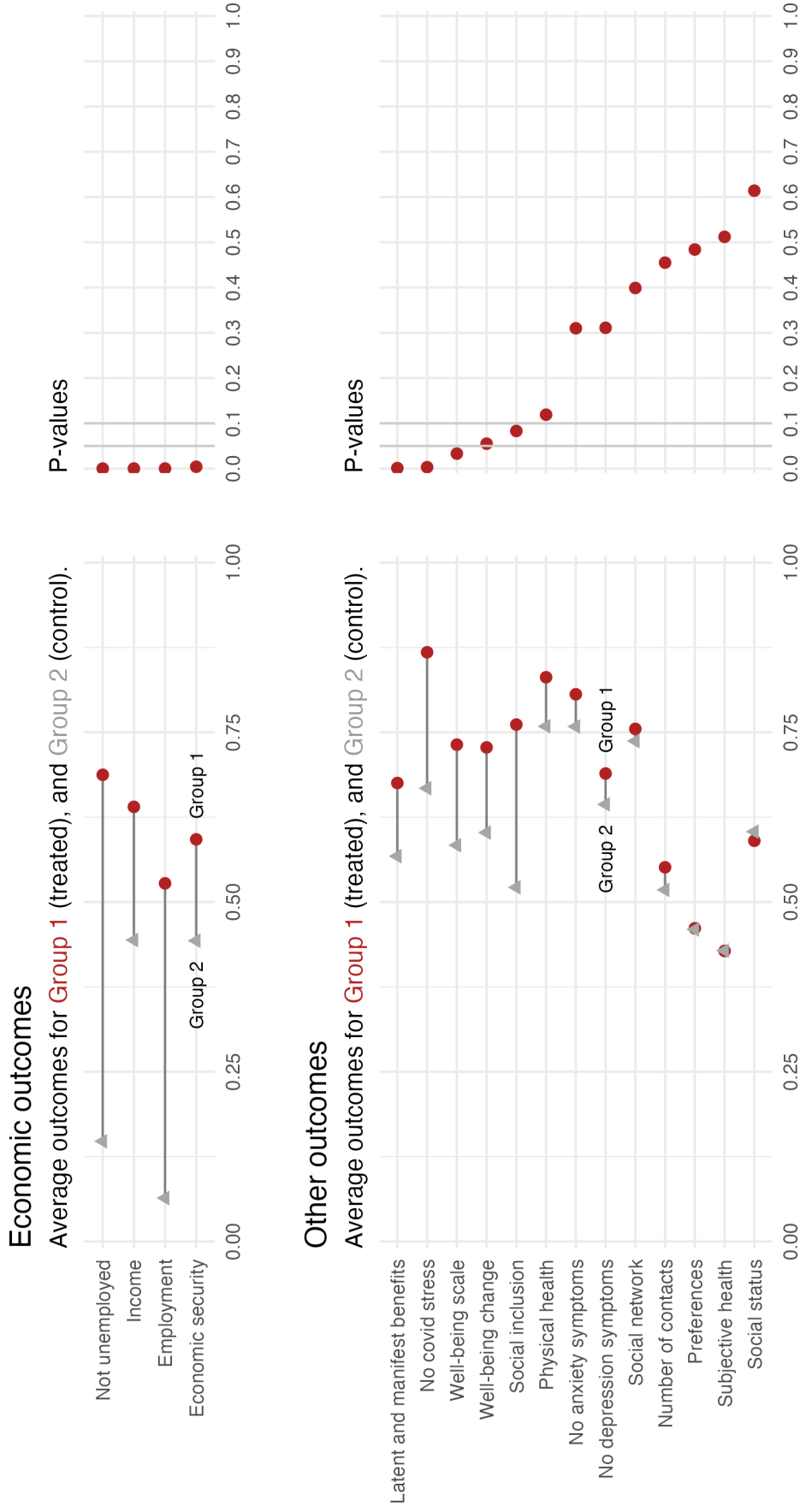
The LAMB scale measures these “latent” benefits (time structure, activity, social contact, collective purpose, and social recognition), in addition to the “manifest” material benefits (income) resulting from employment. Jahoda’s insights regarding the detrimental impact of unemployment, as witnessed 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.

---

<sup>11</sup>This procedure works as follows. Sort the p-values, for each of the  $m$  hypotheses, tested by size, resulting in ordered values  $P_{(j)}$ . For a critical value  $\alpha$ , find the largest value  $k$  such that  $P_{(k)} \leq \frac{k}{m}\alpha$ . Reject the null hypothesis for all  $i = 1, \dots, k$ .

<sup>12</sup>We thank Adam Coutts for pointing us to this line of work in sociology (Kovacs et al., 2017, 2019; Knight et al., 2020).

Figure 3: 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 A.10. 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. All outcomes shown are collected via our surveys, except for employment and unemployment, which are drawn from administrative records. 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 4.

Table 4: 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
Not unemployed	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

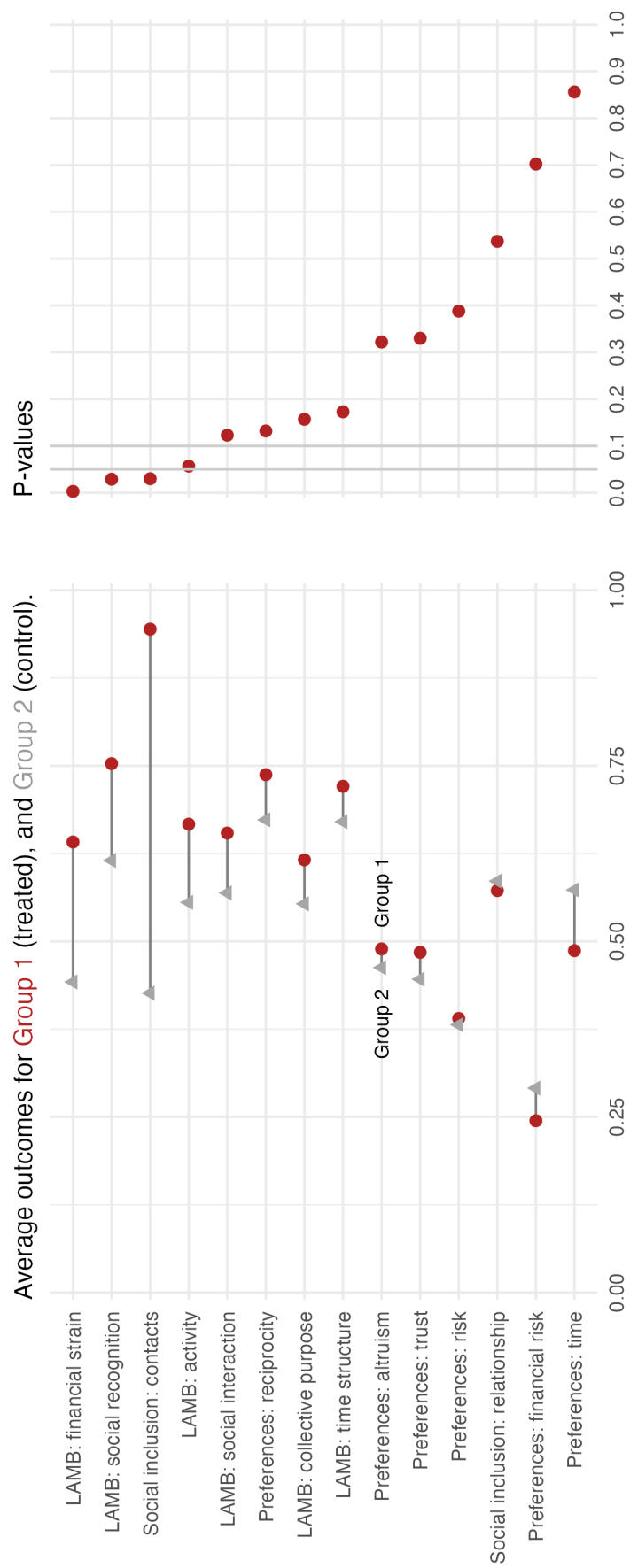
OTHER OUTCOMES							
Outcome	Treated	Control	Difference	p-value	SE	$n_1$	$n_2$
Latent and manifest benefits	0.675	0.568	0.108	0.001	0.042	21	22
No covid stress	0.868	0.668	0.200	0.003	0.072	20	22
Well-being scale	0.732	0.584	0.148	0.033	0.076	20	22
Well-being change	0.728	0.602	0.125	0.055	0.080	21	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
No anxiety symptoms	0.806	0.759	0.048	0.310	0.082	20	22
No 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 3 and Figure 4. All outcomes shown are collected via our surveys, except for employment and unemployment, which are drawn from administrative records. 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.

Figure 4: Experimental estimates with linear controls, disaggregated outcomes



## 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 5 and Figure 6. 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 5 and Figure 6 plots the gap between Gramatneusiedl and the synthetic control, and the corresponding gap for 25 permutations.<sup>13</sup> 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 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 were enrolled in the program, in April 2021, long-term unemployment had been reduced by about 1.5 percentage points, down to less than 1% as a share of the working age population. This was a larger reduction than for any of the 25 permutation municipalities, which can be interpreted as an effect that is significant at the 5% level. Recall that all long-term unemployed residents of Gramatneusiedl were eligible to enroll in the program after April 2021, but participation was 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

---

<sup>13</sup>Figure A.3 in Appendix B 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.

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 (though this effect is not significant, according to permutation inference). 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. There is however also some pre-period difference in employment, and the effect on total employment is less clear-cut than the effect on long-term unemployment.

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 6. 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).<sup>14</sup> The point estimate of the increase of employment in Gramatneusiedl, relative to the synthetic control, is about the same as the reduction of unemployment. The point estimate of the change of inactivity is slightly negative. In light of both pre-treatment differences and statistical uncertainty, these estimates should however be taken with a grain of salt. That said, there is no evidence here that the program *increased* transitions out of the labor force.

**Cumulative effects and composition of jobs** To assess whether the increase in employment came only through direct provision or whether the program also affected transitions into regular and self-employment, we next compare employment status in cumulative days over the program duration per person between participants in Gramatneusiedl and their (initially long-term unemployed) counterparts in the control towns. Figure 7a reports the cumulative number of days spent in each employment status over the full program period. Figure 7b shows the evolution of cumulative days per person in

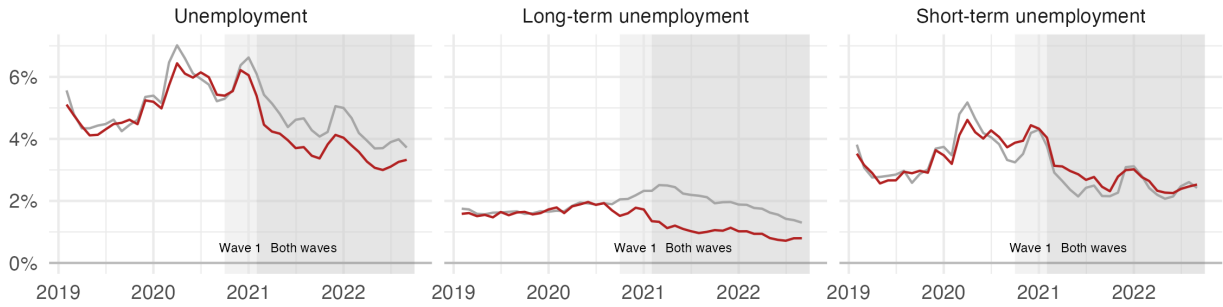
---

<sup>14</sup>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.”

Figure 5: Synthetic control estimates of the program effect on unemployment

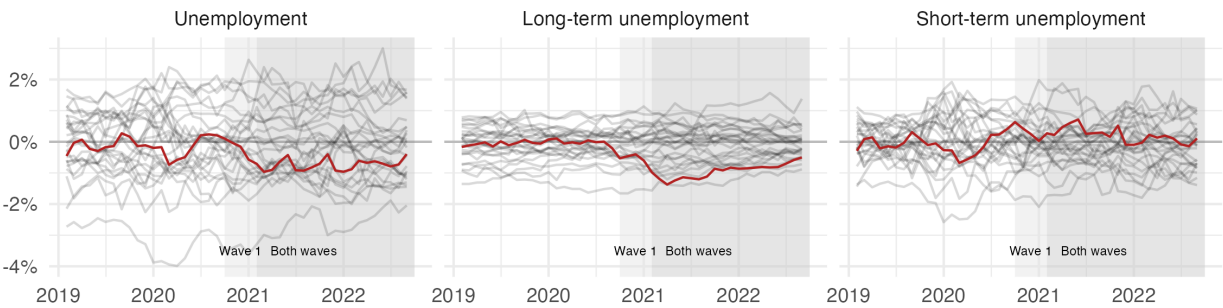
**Outcome levels**

Gramatneusiedl, and synthetic control.



**Treatment effects**

Gramatneusiedl minus control, and permuted comparisons.



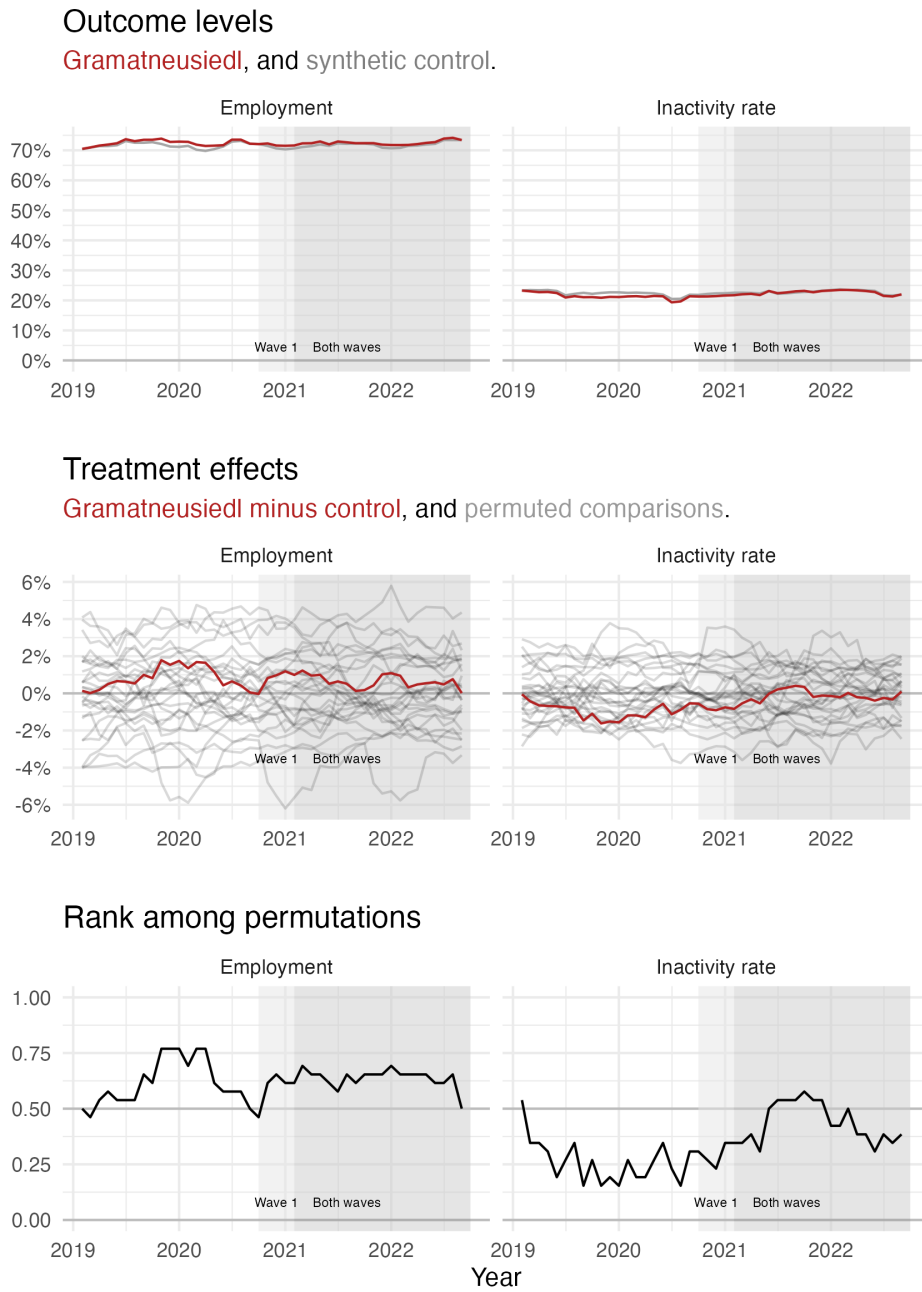
**Rank among permutations**



*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.

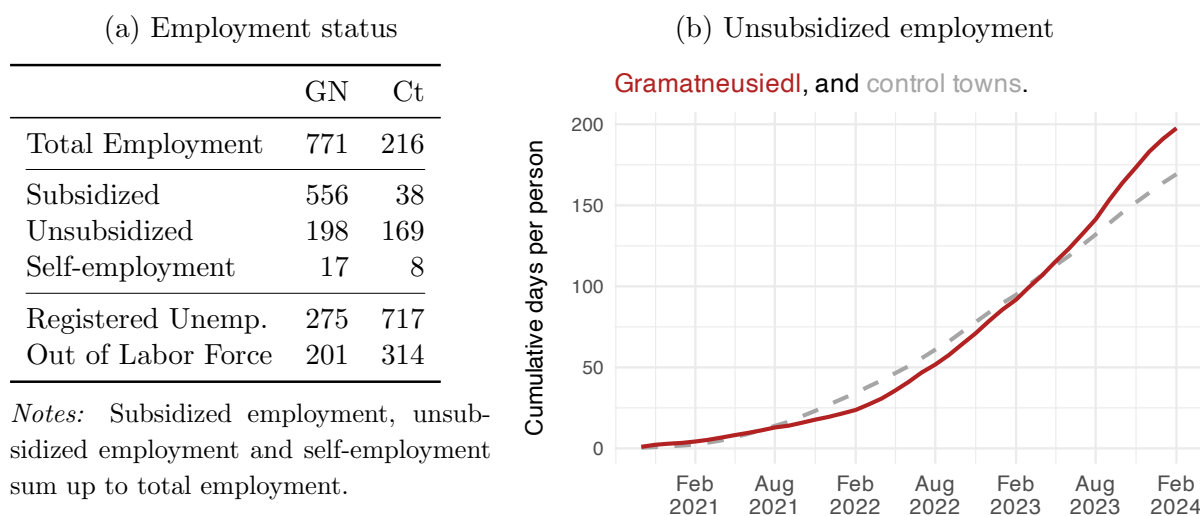
unsubsidized employment over time.

Figure 6: Synthetic control estimates of the program effect on employment and inactivity



Compared to the control towns, participants in Gramatneusiedl spent substantially more time employed: 771 versus 216 days, on average, within 3.5 years; cf. Figure 7. Most of this increase reflects subsidized jobs provided by the program. However, employment days outside the program also rose, producing a cumulative increase in unsubsidized employment of 17 percent, from 169 to 198 days. The job guarantee also doubled the number of days in self-employment from 8 to 17. At the same time, registered unemployment was markedly lower in Gramatneusiedl, by 62 percent or 442 days, and time out of the labor force declined by 36 percent or 113 days.

Figure 7: Cumulative days per person

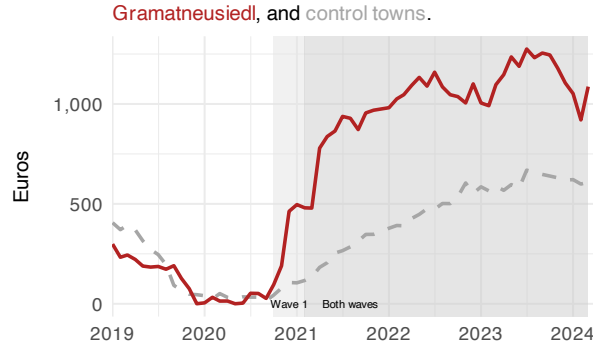


**Earnings of participants** Using administrative data (from the “AMDB Erwerbkarrierenmonitoring”), we can measure earnings for all residents of both Gramatneusiedl and of the control towns, who satisfied the eligibility requirement of 9 months unemployment at baseline. Figure 8 plots these monthly earnings over time.

Participant earnings rise markedly after the program start, as expected. In control towns, earnings rise as well, as some of those unemployed at baseline transition back into employment. Average earnings in control towns however only ever reach about half of those in Gramatneusiedl. Pre-treatment trends are closely aligned.

**Transitions out of unemployment** Figures 5 and 6 show the evolution over time on the *stocks* of unemployment and employment in Gramatneusiedl and the control towns. We next discuss *flows* out of unemployment. To motivate this analysis of flows, we consider a search model of the labor market in Appendix A. Our search model predicts that the incentives for reduced search effort provided by the job guarantee should manifest as both *lower hazard rates out of unemployment*, and a *faster decline* of these hazard rates over time, relative to the counterfactual of no job guarantee. Relatedly, models of labor market spillovers also suggest that negative employment spillovers are strongest for ineligible workers most similar to the long-term unemployed, which are the workers who

Figure 8: Average gross monthly earnings for those eligible at baseline



*Notes:* This figure shows average gross monthly earnings (with no adjustment for controls) in the treated and control municipalities, for those unemployed 9 months or more at baseline. Earnings are based on the assessment basis for social insurance contributions recorded in the “AMDB Erwerbskarrierenmonitoring” database. Data for December 2021 were interpolated to address irregularities in reporting. Bonus payments (13th and 14th salaries) were excluded to smooth seasonal volatility.

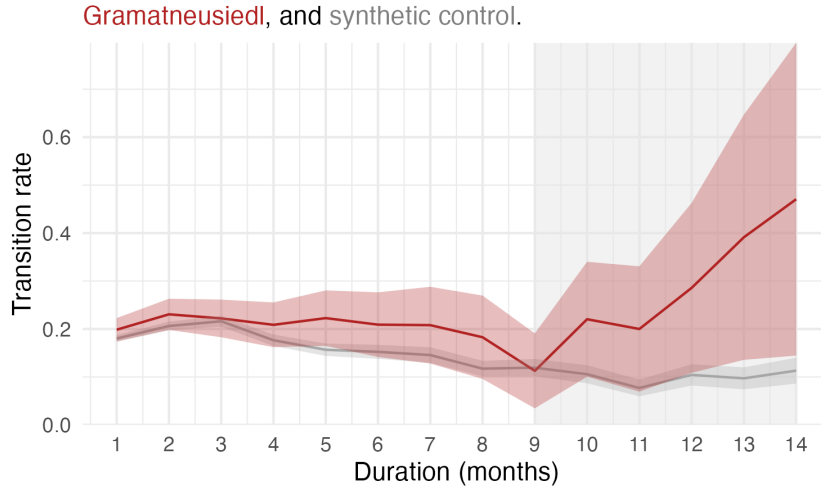
are close to but still below the threshold of eligibility.

Are these predictions of our model borne out in the data? We calculate hazard rates as follows.<sup>15</sup> Using the full sample of all residents registered with the public employment service, and drawing on data from the “AMDB Erwerbs-karrierenmonitoring” database, we create a sample of unemployment spells. Every spell starting after October 2020 and before December 2023 is included. For each duration, rounded up to months, we calculate the number of transitions from unemployment to employment, and divide by the stock of unemployed workers. We do this separately for Gramatneusiedl and for the synthetic control municipalities.

The resulting estimates are shown in Figure 9. We find that the transition rate into employment was *higher* in Gramatneusiedl, for every spell duration below the MAGMA eligibility threshold of 9 months, and the *decline* of hazard rates was *slower*. This is contrary to the predictions of both the search model and of a model of negative spillovers with worker heterogeneity. Regardless of standard errors, we thus cannot reject the null hypothesis that the job guarantee did not decrease search effort. We however *can* reject any economically significant negative impact on transition rates. After 9 months, transitions into employment in Gramatneusiedl increased further. This increase was due to the mechanical effect of the job guarantee program.

<sup>15</sup>This part of our empirical analysis was not pre-registered.

Figure 9: Hazard rates out of unemployment



*Notes:* This figure shows hazard rates from unemployment to employment in the treated and control municipalities, between October 2020 and December 2023. Hazard rates are calculated from the “AMDB Erwerbkarrierenmonitoring” database. Job guarantee eligibility starts after 9 months. The bands show pointwise 95% confidence intervals.

### 4.3 Comparison to individuals in control towns

We finally consider results based on our third 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 (again the same covariates as listed in Table 1). 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 10 and Table 5 for economic outcomes and Figure 11 and Table 6 for other outcomes.<sup>16</sup> 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

<sup>16</sup>The standard errors here are heteroskedasticity robust standard errors, capturing statistical uncertainty for the effect of residing in Gramatneusiedl versus a control municipality. These standard errors condition on the given municipalities, rather than treating municipalities as draws from some super-population.

1 and 2), to individuals in the control towns.

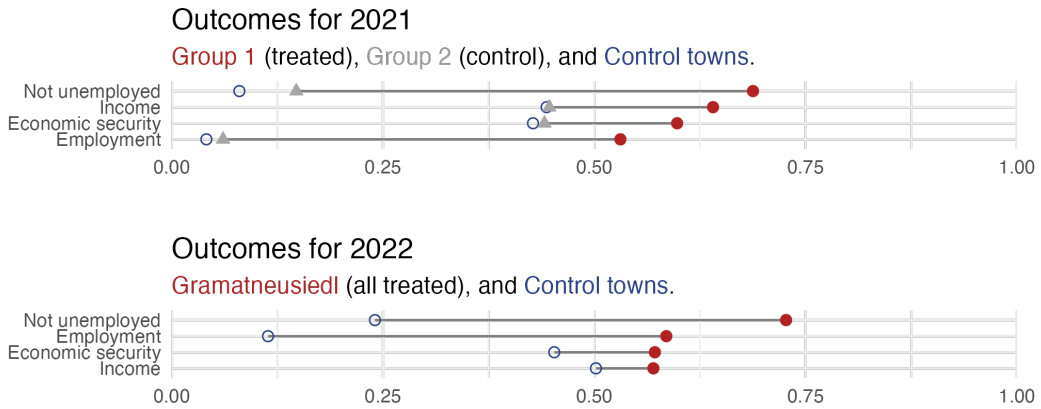
Figure A.5 and Figure A.6 show corresponding confidence intervals. Figure A.5 contrasts Group 2 to control town individuals in 2021, thus providing an estimate of the average anticipation effect on the treated. Figure A.6 contrasts both groups to control town individuals in 2022, thus providing an estimate of the average total effect on the treated.

**Findings** For the economic outcomes, including employment, unemployment, income, and economic security, the comparison of Group 1 to control town individuals in 2021 yields estimates that are indistinguishable from the estimates based on the experimental comparison. This again suggests that there are no anticipation effects for job search, corroborating the finding of Figure 9, and contradicting the implications of the search model of Appendix A.

By contrast, a number of the non-economic outcomes show evidence of positive anticipation effects. Outcomes such as social status and well-being change show significant positive effects, and a number of other outcomes such as social inclusion, anxiety, and depression show positive but insignificant effects.

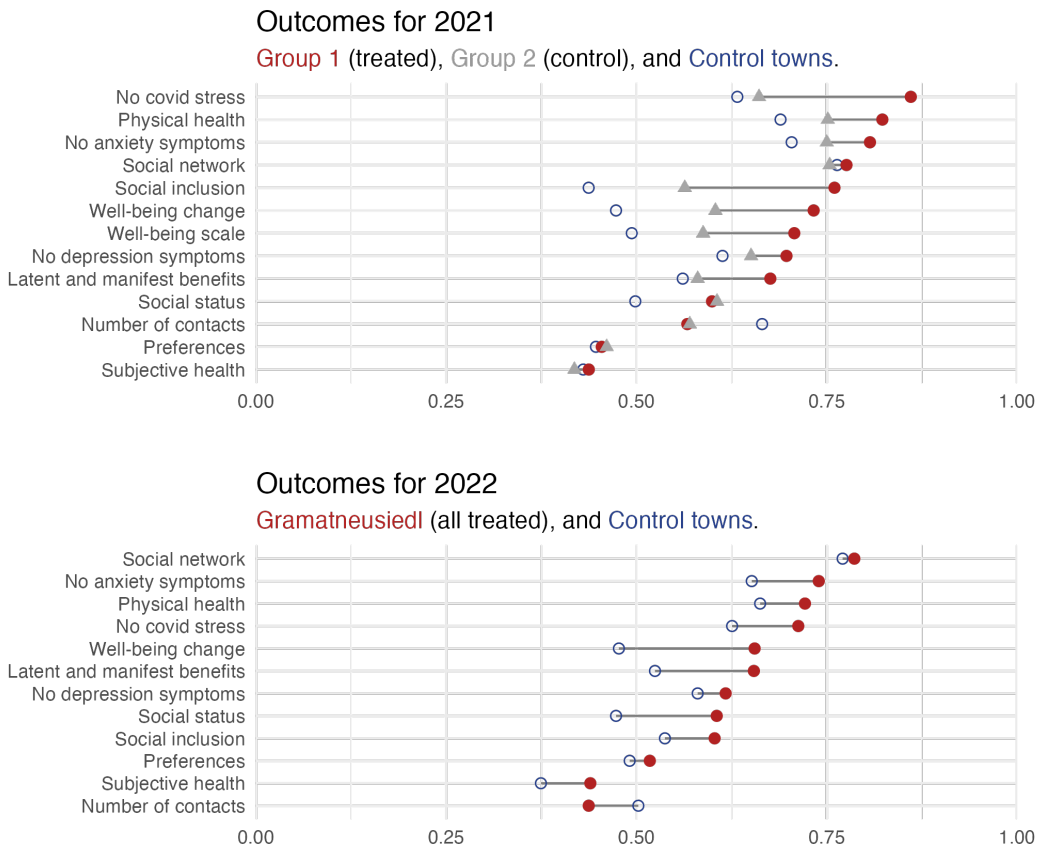
Furthermore, the effects documented using our experimental comparison, on employment, income, and economic security, latent and manifest benefits, well-being change, and Covid stress persisted into 2022. These were thus not just short-term effects; the effects were maintained over the course of the program.

Figure 10: Control town comparisons with linear controls, economic outcomes



Notes: These estimates are also tabulated in Table 5.

Figure 11: Control town comparisons with linear controls, other outcomes



Notes: These estimates are also tabulated in Table 6.

Table 5: Control town comparisons with linear controls, economic outcomes

2021								
Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	$n_1$	$n_2$	$n_{ct}$
Not unemployed	0.688	0.147	0.080	0.065	0.053	31	31	211
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	0.531	0.061	0.041	0.018	0.039	31	31	211

2022							
Outcome	Gramatneusiedl	Control towns	Gn vs. Ct towns	SE	$n_{mt}$	$n_{ct}$	
Not unemployed	0.727	0.240	0.486	0.034	62	211	
Employment	0.585	0.114	0.471	0.046	62	211	
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 10, Figure A.5, and Figure A.6. Employment and unemployment are drawn from administrative records. Income and economic security are collected from our surveys. 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 Gramatneusiedl and Control town observations.

Table 6: Control town comparisons with linear controls, other outcomes

2021									
Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	$n_1$	$n_2$	$n_{ct}$	
No 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	
No 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	
No 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	Gramatneusiedl	Control towns	Gn vs. Ct towns	SE	$n_{mt}$	$n_{ct}$	
Social network	0.786	0.771	0.015	0.040	26	39	
No anxiety symptoms	0.740	0.651	0.088	0.061	44	58	
Physical health	0.721	0.662	0.059	0.040	44	58	
No 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	
No 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 11, Figure A.5, and Figure A.6. All outcomes shown are collected via our surveys. 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 Gramatneusiedl and Control town observations.

## 4.4 Cost comparison

We next turn to an evaluation of program costs. We again compare program participants, in both Group 1 and 2, to comparison individuals in control towns.<sup>17</sup> We obtained daily, individual-level expenditure data from the AMS, covering the entire period of the program up to March 2024. Total expenditures include (i) benefit payments, including unemployment benefits (incurred by the social insurance system and administered by the AMS), and (ii) program costs (incurred directly by the AMS). Benefit payments include both direct transfers to recipients (net benefits) and payments into the pension and health insurance system. Program costs include conventional active labor market policies, such as coaching, job training, and hiring subsidies, but also the costs of the Marienthal job guarantee (including wages, social insurance contributions, payroll taxes, and overhead costs). The social enterprise implementing MAGMA furthermore generated some revenue flowing back to the AMS. We obtained these data from the AMS. There are, lastly, fiscal externalities in the form of tax and social insurance payments flowing back from program participants to the public sector, which we impute.

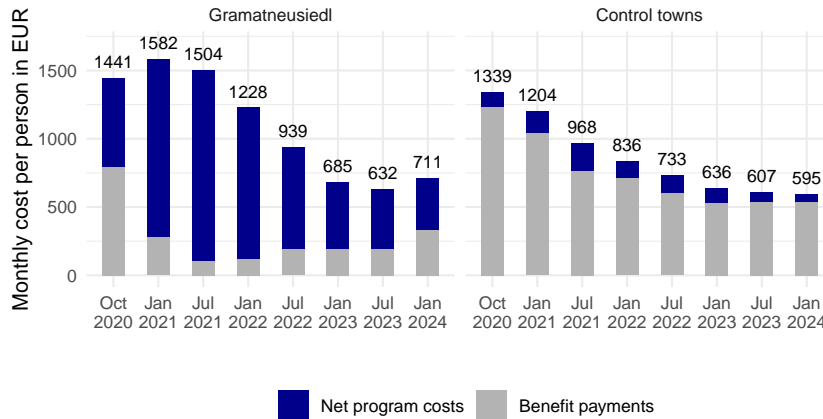
**Program costs for the AMS** The job guarantee increased labor market policy expenditures in the first 18 months; thereafter expenditures declined relative to the initial phase of the program, as shown in Figure 12. This decline over time reflects a reduction in benefit claims due to an increase in unsubsidized employment. The program led to a compositional shift from passive (social insurance) to active (AMS) labor market policy spending. Over the full program period (October 2020 to March 2024), the job guarantee increased monthly labor market policy expenditures per registered long-term unemployed job seeker by 28 percent, from 850 Euro to 1,092 Euro (see Table 7). The bulk of this increase was concentrated in the first half of the program, at 37 percent relative to the control group, whereas it declined to 15 percent thereafter; cf. Tables A.8-A.9 in the appendix.

**Participant income and government revenue** While the program increased costs for the AMS, it also increased participant income, and thereby indirectly increased government revenues from tax and social insurance (SI) payments. Income includes both earnings and benefit receipt. As shown in Table 7, participant gross earnings, as measured using administrative data, increased by 597 Euro per month over the program period. In Austria, the sum of payroll taxes and social insurance contributions equals 37% on average, for workers in the relevant wage bracket. Allowing for the possibility of lower tax or insurance rates that might apply to the program participants, we can conservatively estimate that about 25% of gross earnings directly flow back to the public sector; the implied increase of net earnings is 447 Euro per month. Net benefits for

---

<sup>17</sup>For this comparison, we do not adjust for linear controls, to transparently present the observed monetary costs.

Figure 12: Expenditures per person and month



*Notes:* Total expenditure per participant and month, decomposed into programs, and benefits, calculated from daily, individual-level expenditure data from the AMS. *Net program costs* refer to active labor market policy; *Benefit payments* refer to passive labor market policy. Program costs are net of revenues of the public enterprise. Control individuals are identical to those in our third identification approach (Section 4.3): Individuals unemployed at least nine months in September 2020, from the three towns with the largest synthetic control weights.

participants (i.e., benefit payments net of contributions to the SI system), on the other hand, declined by 345 Euro per month. Summing net earnings and net benefits, their net income thus increased by 102 Euro.

We can furthermore impute tax and SI payments flowing back to the public sector, estimating tax and SI revenues from earnings as  $0.25 \times$  gross earnings. We furthermore impute SI revenues from benefit payments directly from AMS data. On net, these numbers imply that the program increased government revenues by 5 Euro per participant and month. This modest net increase reflects the fact that a nontrivial share of SI revenues accrues through benefit payments flowing back to the state. Subtracting this from the total cost for the AMS yields a net cost for the public sector of 237 Euro per participant and month.

**Marginal value of public funds** To summarize, the net cost of MAGMA for the public sector equals 237 Euro per participant and month, while participant income increased by 102 Euro, and participant non-economic wellbeing increased significantly.

How should the total value of this program be assessed? What is the marginal value of public funds spent on a job guarantee like MAGMA? A complete assessment would require us to assign a monetary value to the non-economic benefits for participants that we documented above (including the significant increases of the latent and manifest benefits of work, the reduction in pandemic-related stress, and the improvement of self-reported well-being and economic security), in addition to the economic benefits (including the increased earnings and income as well as income security).

Table 7: Expenditures and revenues per person and month, October 2020 to March 2024

	Gramatneusiedl	Control towns	Difference
<i>AMS</i>			
Program costs	974	127	847
Benefit payments	234	723	-489
- Revenues of social enterprise	-116	0	-116
Total costs for AMS	1,092	850	<b>242</b>
<i>Public sector</i>			
- Imputed tax and SI revenue	-342	-337	-5
Net costs for public sector	743	510	<b>237</b>
<i>Participant income</i>			
Gross earnings	1,104	507	597
Imputed net earnings	828	381	447
Net benefits	168	513	-345
Imputed net income	996	894	<b>102</b>

*Notes:* The cost comparison covers the full program duration. All numbers are in Euro per person and month without adjustment for linear controls. *Program costs* refer to active labor market policy; *Benefit payments* refer to passive labor market policy. Program costs and net benefits are constructed from daily individual-level expenditure data provided by the AMS. Revenues of social enterprise are provided by the AMS on a monthly basis. Gross earnings are obtained from daily individual-level records in the AMDB register. Tax and social insurance revenues for benefits and earnings are imputed. These imputations yield measures of benefit payments, net earnings, and net income. Control individuals are identical to those in our third identification approach (Section 4.3): Individuals unemployed at least nine months in September 2020, from the three towns with the largest synthetic control weights. See Appendix Tables A.8-A.9 for a split of the program duration into the first and second halves.

Assigning such a value is a fundamentally normative choice, rather than an empirical question, and as such is beyond the scope of the present paper. We can, however, ask the reverse question: How large would such a value need to be in order to rationalize the program? Assuming that participant income is valued as highly as public funds (a reasonable presumption given that participants are relatively disadvantaged) implies that net cost of the program net of participant income equals about  $237 - 102 = 135$  Euro per month, averaged over the duration of the program. The program can thus be rationalized if and only if the societal value assigned to the (significant) improvement of participant well-being exceeds the (modest) net cost of 135 Euro per month.

**Dynamics over time** The fiscal effects of the program display a pronounced temporal pattern (Appendix Tables A.8-A.9). Costs are front-loaded and decline markedly over time, as participants transition into non-subsidized employment. In the first 21 months, imputed tax and SI revenues from benefit payments exceeded those from earnings, re-

sulting in net costs for the public sector of 443 Euro per participant and month.

In the second 21 months, net costs fell substantially, averaging 96 Euro per participant and month for the AMS and 41 Euro for the public sector as a whole. This decline reflects lower program expenditures as participants moved into non-subsidized employment while earnings based tax and SI revenues became more important, relative to benefit related SI payments. Over the same period, the effect on imputed net income rose to 228 Euro per participant and month.

## 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 3. We use an experimental staggered rollout 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 (permutation inference). This guarantees finite sample validity without any asymptotic approximations. In Appendix B, we also report conventional confidence intervals, using robust standard errors; the conclusions remain unchanged.

Turning to our empirical findings, a first remarkable fact is the high take-up of the voluntary program: everyone offered a job after completing 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 recognition.<sup>18</sup> These non-economic effects of

---

<sup>18</sup>In addition to the direct effect of guaranteed work, the easing of conditionality and the changed relationship with the AMS caseworker may have contributed to these effects. We thank an anonymous

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.<sup>19</sup> There appears to be no increase of short-term unemployment. The program raised total employment by 555 days per participant, driven not only by direct job provision, but also by a 17 percent increase in unsubsidized employment and a twofold rise in self-employment. While the program raised direct costs for the AMS in the short run, these were offset over time by increased transitions into non-subsidized employment, resulting in lower net costs in the long term. Net costs to the public sector were further reduced by increased tax and social insurance payments from participant earnings, in particular after the first 18 months. The decline in net costs over time, combined with the sustained gains in earnings, suggest that the long term fiscal and income effects outweigh the initial investment required to implement the program.

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 to inform a possible larger rollout, recently debated in parliaments (Senate, 2023; Parliament, 2023). Some may be possible through the funding for additional job guarantee pilots provided by the European Commission, which was informed by the Marienthal pilot. 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 ILO (2021); OECD (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

---

reviewer for this suggestion.

<sup>19</sup>The program involved employment in a specially created social enterprise. This might have limited the relevance of some displacement or crowd-out mechanisms in the labor market. We again thank an anonymous reviewer for emphasizing this point.

benefits of employment might contribute to a refined understanding in economics of labor supply and labor market dynamics more generally.

## References

- 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.
- Aizer, A., Early, N., Eli, S., Imbens, G., Lee, K., Lleras-Muney, A., and Strand, A. (2024). The Lifetime Impacts of the New Deal’s Youth Employment Program. *The Quarterly Journal of Economics*.
- 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.
- Ariely, D., Kamenica, E., and Prelec, D. (2008). Man’s search for meaning: The case of Legos. *Journal of Economic Behavior & Organization*, 67(3):671–677.
- 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.
- Baekgaard, M., Nielsen, S. A., Rosholm, M., and Svarer, M. (2024). Long-Term Employment and Health Effects of Active Labor Market Programs. *Proceedings of the National Academy of Sciences*, 121(50):e2411439121.
- 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.
- 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.
- Bertrand, M., Crépon, B., Marguerie, A., and Premand, P. (2017). Contemporaneous and Post-Program Impacts of a Public Works Program. *Working Paper*.

- Bhatt, M. P., Heller, S. B., Kapustin, M., Bertrand, M., and Blattman, C. (2024). Predicting and Preventing Gun Violence: An Experimental Evaluation of READI Chicago. *The Quarterly Journal of Economics*, 139(1):1–56.
- Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, 41(1):359–375.
- Brown, C. (1980). Equalizing Differences in the Labor Market\*. *The Quarterly Journal of Economics*, 94(1):113–134.
- Caldwell, S., Haegele, I., and Heining, J. (2025). Why Workers Stay: Pay, Beliefs, and Attachment. *NBER Working Papers*. Number: 33445 Publisher: National Bureau of Economic Research, Inc.
- 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.
- Cheung, M., Egebark, J., Forslund, A., Laun, L., Rödin, M., and Vikström, J. (2025). Does Job Search Assistance Reduce Unemployment? Evidence on Displacement Effects and Mechanisms. *Journal of Labor Economics*, 43(1):47–81.
- Clark, A. (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.
- Cook, P. J., Kang, S., Braga, A. A., Ludwig, J., and O’Brien, M. E. (2015). An Experimental Evaluation of a Comprehensive Employment-Oriented Prisoner Re-entry Program. *Journal of Quantitative Criminology*, 31(3):355–382.
- Couch, K. A. (1992). New Evidence on the Long-Term Effects of Employment Training Programs. *Journal of Labor Economics*, 10(4):380–388.
- 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, U.S. Department of Health and Human Services, Washington, DC.

- Dube, A., Naidu, S., and Reich, A. D. (2022). Power and Dignity in the Low-Wage Labor Market: Theory and Evidence from Wal-Mart Workers. *NBER Working Paper No. 30441*, National Bureau of Economic Research.
- Eisenberg, P. and Lazarsfeld, P. F. (1938). The Psychological Effects of Unemployment. *Psychological Bulletin*, 35(6):358–390.
- Ferracci, M., Jolivet, G., and van den Berg, G. J. (2014). Evidence of Treatment Spillovers Within Markets. *The Review of Economics and Statistics*, 96(5):812–823.
- Franklin, S., Imbert, C., Abebe, G., and Mejia-Mantilla, C. (2024). Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia. *American Economic Review*, 114(5):1382–1414.
- 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.
- Graham, B., Imbens, G., and Ridder, G. (2010). Measuring the Effects of Segregation in the Presence of Social Spillovers: A Nonparametric Approach. Technical Report w16499, National Bureau of Economic Research, Cambridge, MA.
- 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.
- Heller, S. B. (2014). Summer Jobs Reduce Violence Among Disadvantaged Youth. *Science*, 346(6214):1219–1223.
- Heller, S. B. (2022). When Scale and Replication Work: Learning from Summer Youth Employment Experiments. *Journal of Public Economics*, 209:104617.
- Hetschko, C., Knabe, A., and Schöb, R. (2014). Changing Identity: Retiring from Unemployment. *The Economic Journal*, 124(575):149–166.
- Hollister, R., Kemper, P., and Maynard, R. (1984). *The National Supported Work Demonstration*. University of Wisconsin Press.
- Huber, M., Lechner, M., and Wunsch, C. (2011). Does Leaving Welfare Improve Health? Evidence for Germany. *Health Economics*, 20(4):484–504.
- 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.
- Imbert, C. and Papp, J. (2015). Labor Market Effects of Social Programs: Evidence from India’s Employment Guarantee. *American Economic Journal: Applied Economics*, 7(2):233–263.
- Ivanov, B., Pfeiffer, F., and Pohlan, L. (2020). Do Job Creation Schemes Improve the Social Integration and Well-Being of the Long-Term Unemployed? *Labour Economics*, 64:101836.
- Jahoda, M. (1982). *Employment and Unemployment: A Social-Psychological Analysis*. Cambridge University Press, Cambridge.
- Jahoda, M., Lazarsfeld, P. F., and Zeisel, H. (1933). *Marienthal: The Sociography of an Unemployed Community (Original Work Published 1933)*. Routledge.
- Johnston, A. C. and Mas, A. (2018). Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut. *Journal of Political Economy*, 126(6):2480–2522. Publisher: The University of Chicago Press.
- Kaplan, G. and Schulhofer-Wohl, S. (2018). The Changing (Dis-)Utility of Work. *Journal of Economic Perspectives*, 32(3):239–258.
- 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.
- Knabe, A., Rätzl, S., Schöb, R., and Weimann, J. (2010). Dissatisfied with Life but Having a Good Day: Time-use and Well-being of the Unemployed. *The Economic Journal*, 120(547):867–889.
- 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.
- Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *The Review of Economic Studies*, 76(3):1071–1102. Publisher: [Oxford University Press, The Review of Economic Studies, Ltd.].
- Maestas, N., Mullen, K. J., Powell, D., von Wachter, T., and Wenger, J. B. (2023). The Value of Working Conditions in the United States and the Implications for the Structure of Wages. *American Economic Review*, 113(7):2007–2047.
- Mas, A. and Pallais, A. (2017). Valuing Alternative Work Arrangements. *American Economic Review*, 107(12):3722–3759.
- 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.
- OECD (2023). MAGMA – A Job Guarantee Pilot Project. Technical report, OECD, Paris.
- 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)).
- Pohlan, L. (2019). Unemployment and Social Exclusion. *Journal of Economic Behavior & Organization*, 164:273–299.
- Quinz, H. and Flecker, J. (2022). Marienthal. Reversed: The Effects of a Job Guarantee in an Austrian Town. *ILPC Padova, April 21, 2022*.
- Rawls, J. (1999). *A theory of justice*. Belknap Press of Harvard University Press, Cambridge, Mass, rev. ed edition.
- Rosen, S. (1986). Chapter 12 The theory of equalizing differences. In *Handbook of Labor Economics*, volume 1, pages 641–692. Elsevier B.V. ISSN: 1573-4463.
- Semenova, V. (2025). Generalized Lee bounds. *Journal of Econometrics*, 251:106055.
- Sen, A. (1995). *Inequality reexamined*. Oxford University Press, Oxford.
- Senate, U. (2023). Federal Jobs Guarantee Development Act of 2023. Sponsored by Cory Booker.
- Strandh, M. (2001). State Intervention and Mental Well-being Among the Unemployed. *Journal of Social Policy*, 30(1):57–80.
- Uggen, C. (2000). Work as a Turning Point in the Life Course of Criminals: A Duration Model of Age, Employment, and Recidivism. *American Sociological Review*, 65(4):529–546.

- 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.
- Valentine, E. J. and Redcross, C. (2015). Transitional Jobs After Release from Prison: Effects on Employment and Recidivism. *IZA Journal of Labor Policy*, 4(1):16.
- Young, C. (2012). Losing a Job: The Nonpecuniary Cost of Unemployment in the United States. *Social Forces*, 91(2):609–634.