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

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Abstract

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

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

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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 (U.S. Senate, 2023; European 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 several quasi-experimental studies, and by a recent 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[1] 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 no one who was offered a job after the two month preparatory training declined the opportunity.

The guaranteed job was preceded by individually tailored preparatory training of about 8 weeks. The jobs themselves could either be subsidized jobs in the regular labor market, or (for the majority of participants) employment in a social enterprise, implementing projects for the municipality. Salaries for all participants were at least equal to the minimum wage set by collective bargaining. Jobs were created to fit the individual

[1]MAGMA is short for "Modellprojekt Arbeitsplatzgarantie Marienthal," which translates as "model project job guarantee Marienthal." Marienthal is one part of the municipality of Gramatneusiedl.

needs and constraints of participants, and to provide meaningful activity. We discuss a comprehensive evaluation of program costs as part of our findings in Section 4.

The MAGMA program differed from typical active labor market policies, and should instead be compared to pure income support and welfare programs. The intervention was quite big and long-lasting, and the objective was different from more conventional active labor market policies (Card et al., 2010), which aimed at re-integration of participants into the regular labor market. While participants of the MAGMA program were encouraged to take up employment in the regular labor market, and such employment was subsidized by the program, this was not a likely outcome for many participants. Instead, the stated policy goal of the MAGMA program was to directly eradicate long-term unemployment in the municipality, and thereby to improve participants' economic and social situation. Correspondingly, our evaluation focuses on the impact of the program on the well-being of participants along various economic and non-economic dimensions, and on the impact on the municipality-level labor market overall.

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

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

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

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

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

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

additionally adjust for a rich set of baseline covariates in our regressions.

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

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

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

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

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

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

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

Second, we find large effects on a number of measures of well-being that have been emphasized in the sociology of work, social psychology, and organizational behaviour (Jahoda, 1982), and which have been summarized as the "latent and manifest benefits" of work, (Kovacs et al., 2019). This includes measures of time structure, activity, social contacts, a sense of collective purpose, and social recognition. Our experimental findings thus corroborate descriptive work in sociology and social psychology on the importance of these non-economic benefits of employment, including the "need to belong" (Baumeister and Leary, 1995), and the "desire for status," (Anderson et al., 2015); see also Strandh (2001). Such measures of well-being have received less attention in labor economics thus far, with notable exceptions such as Clark (2003, 2006); Kassenboehmer and Haisken-DeNew (2009); Knabe et al. (2010).

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

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

imental estimates for some other dimensions, however, including (subjective) well-being and social status. This suggests the presence of some anticipation effects, but most of the program benefits only manifest after the start of employment. Considering outcomes in subsequent years, we find that the 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**, from the perspective of the AMS. The program raised direct costs for the AMS in the short run, which were offset over time by increased transitions into non subsidized employment, resulting in lower net costs after the first 18 months. The temporary increase of 28% in expenditures was fully matched by the increase of participant income.

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 Section 5, 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. 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.

The historical arc from "Die Arbeitslosen von Marienthal" (1933) to MAGMA The location chosen for the job guarantee pilot is no coincidence. Ninety years prior to this experiment, Marienthal was the location of a pathbreaking study on the impact of long-term mass unemployment (Jahoda et al. 1933, "Die Arbeitslosen von Marienthal," originally published in 1933). At the time, Marienthal was a factory town dominated by a single factory. When this factory shut down in the Great Depression, most residents

lost their employment, with devastating consequences. Jahoda et al. (1933), in a large multi-method study, documented the impact of this situation. This study proved to be of lasting influence on the sociology and social psychology of work.

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

Literature There is a large literature studying the effectiveness of active labor market policies (ALMPs); see in particular the meta-analyses by Card et al. (2010, 2018), and the earlier reviews by Heckman et al. (1999); Kluve (2010), as well as Crépon and van den Berg (2016). The existing evaluations of ALMPs in German-speaking countries are mostly observational (recent exceptions are Altmann et al. 2018; Böheim et al. 2023; van den Berg et al. 2025); by contrast, there are numerous experimental studies from the US, e.g. Card and Hyslop (2005); Schochet et al. (2008); Gelber et al. (2016), and France, e.g. Crépon et al. (2013); Behaghel et al. (2014). Cummings and Bloom (2020) discuss a number of recent RCTs in the US evaluating subsidized employment programs, focusing on the effects on employment after the subsidies expire. They find some evidence of positive effects on employment, in particular among the most disadvantaged participants.

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

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

ployment programs for disadvantaged sub-populations aimed at improving unsupported employment after program participation (Hollister et al., 1984; Couch, 1992; Uggen, 2000; Cook et al., 2015; Valentine and Redcross, 2015).

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

The present paper also speaks to the large literature on the (negative) consequences of (un)employment. A correlational association between health and employment is widely documented in social epidemiology and neighboring fields, cf. Brand (2015); Avendano and Berkman (2014); Huber et al. (2011), though the causal link between the two is contested. Similarly, there is a strong association between employment and (subjective) well-being, cf. Clark and Oswald (1994); Korpi (1997); Clark (2003, 2006); Young (2012); see also Haushofer and Fehr (2014). A number of papers rely on quasi experimental variation to study the relationship (Kassenboehmer and Haisken-DeNew, 2009; Hetschko et al., 2014; Pohlan, 2019). This relationship 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). In economic theory, Acemoglu (1995) argues that in light of duration dependent discrimination against long-term unemployed, positive discrimination for public sector employment is desirable. Basu et al. (2009) discuss the implications of an employment guarantee scheme on efficiency and social welfare The negative psychological consequences of unemployment have also been studied in a much older psychological literature; Eisenberg and Lazarsfeld (1938), for instance, review over 100 such studies conducted during the Great Depression. A general conclusion of this older literature was that unemployment leads to loss of purpose, confidence, and time structure, and to apathy, rather than political radicalization. (Lazarsfeld, one of the authors of this review, was a co-author of the original Marienthal study, and later became president of the American Sociological Association.) In contrast to both the older and most of the more recent *correlational* literature, we estimate *causal* effects of employment on well-being.

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

Roadmap The rest of this paper is structured as follows. Section 2 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 discusses models of job search and of labor demand, in the context of which we interpret our empirical findings. Section 6 concludes.

Appendix A presents additional details on our evaluation strategies, additional empirical findings, and robustness checks. Appendix B 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 C provides a detailed list of all the jobs that were created in both the market and non-market sector, reports views from program participants, describes some of the jobs that were created in greater detail, and includes additional information on the program's policy impact, a parallel qualitative evaluation, the impact of the Covid-19 pandemic, and the program's comparison with unconditional income support. Appendix D 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 (Arbeits-marktservice 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) were eligible

to participate. The experimental sample includes all residents unemployed for more than 9 months in September 2020. Residents who reached the eligibility threshold later were eligible to participate in the program, but are not part of our experimental comparison. Over the duration of the program, there were 112 eligible individuals, including 62 experimental participants and 50 late entrants. Out of those, 80 had found a job, including 45 at the social enterprise founded by MAGMA, 22 on the regular labor market with a wage subsidy, and 13 on the regular labor market without subsidy.

The duration for the project was set until March 2024 and budgeted with EUR 7.4 million. A complementary study to ours (Quinz and Flecker, 2022), summarized in Appendix C.5, is based on a mixed-methods design and qualitative in-depth interviews. The program implementation coincided with the Covid-19 pandemic. Nevertheless the program took place as planned. We provide details in Appendix C.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.[4]

The social enterprise implemented projects at the municipal and regional level. This

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

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

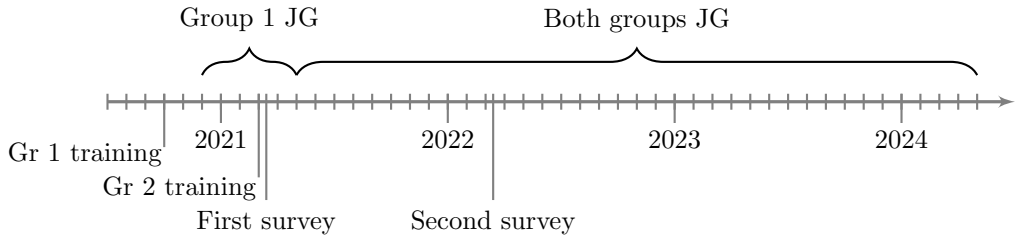
A specific effort was made to create productive and meaningful employment that is adequate to the participants' previous jobs and interests. The jobs created were furthermore tailored to the needs of the recipients: Participants who were only available to work part-time, given their other obligations,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

Voluntary participation Work conditionality was eased for this pilot program. Under law (*Arbeitslosenversicherungsgesetz AlVG §9*), recipients of unemployment benefits were assigned to labor market programs by the *AMS*. They have the obligation to participate and they have to accept any employment offer that conforms to their skill-set, otherwise they might lose their unemployment benefits.

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

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

Figure 1: Timeline of the Evaluation



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

Outcomes of interest We estimate the effect of program participation on a range of economic and social outcomes. These outcomes are listed and defined in Table A.7 in Appendix B. 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 B. We collected information on a rich set of economic outcomes (in particular income and economic security), as well as non-

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

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 B for references and details.

To enable a compact presention of our results in Section 4, we normalize all individual-level outcomes, such that higher values correspond to "better" outcomes (variables where the sign is flipped are marked by *(-)* in the table and subsequent figures), and such that the range of these variabes is the interval $[0, 1]$; cf. Table A.7.

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

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. 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 includes effects on residents who were not eligible to participate in the program because they were not long-term unemployed.

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

Approach 1: Pairwise randomization We assigned program participants to one of two groups using pairwise randomization. We matched pairs using a number of covari-

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

We calculated pairwise distances between all 62 program participants using the Mahalanobis distance, based on these covariates. The Mahalanobis distance of two covariate vectors x_1 and x_2 that are realizations of a random vector X is given by $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot Var(X)^{-1} \cdot (x_1 - x_2)}$. We matched participants into pairs such that the total sum of distances between the members of each matched pair is minimized. We then randomly assigned one of the participants in each pair to Group 1, starting the program earlier, while the other participant was assigned to Group 2, starting the program later. Summarizing the resulting assignment, Table 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" (*Erwerbskarrierenmonitoring, EWKM*), accessed via the *AMS* internal registry for social security registry data, and (iii)

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

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 A. The averages of these covariates for both Gramatneusiedl and the (synthetic) control municipalities are shown in Table A.2 in Appendix A. Most of our covariates are based on observations for the year 2019 (as measured in December). In addition to these covariates, 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.

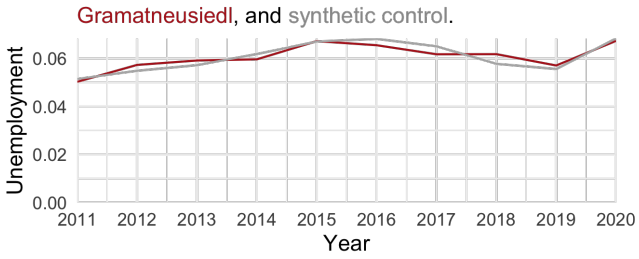
In the second step, we constructed a synthetic control based on these 25 municipalities, using the approach described in Abadie et al. (2010) and reviewed in Abadie (2019). This synthetic control is chosen to match the same list of covariates used in the first step (where we selected a subsample of municipalities), 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.1 in Appendix A. 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 A similarly compares the covariate values for Gramatneusiedl with those for the synthetic control as well as those for each of the municipalities with positive synthetic control weights.

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

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



eligibility criterion for program participation in Gramatneusiedl.

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

To verify that the sample of control town individuals is similar to the set of participants, we again compare their baseline covariates. Table A.3 in Appendix A shows that there are no significant differences in baseline covariate means across the towns considered, with the exception of benefit levels, which are slightly higher among control individuals, and (marginally) age, which is also higher in the control towns. When estimating treatment effects in Section 4, we adjust for baseline covariates to correct for any remaining imbalances between the long-term unemployed in Gramatneusiedl and in the control municipalities.

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 $\overline{D}$ be the share of long-term unemployed in the municipality who are currently eligible. Let finally ϵ_i be a vector of unobserved individual characteristics, which are not

affected by the program. We can then assume that

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

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

Identifying contrasts With this notation, we can now describe the identified averages from our three evaluation approaches in structural terms. Table 2 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 ϵ_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	$E[g(0, 0, 0, \epsilon_i)]$

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 the two groups. The comparison of both groups, after April 2021, to control town individuals identifies the **average total effect on the treated**, which incorporates direct effects, anticipation effects, and spillover effects.

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

Table 3: Identified effects and roadmap

Contrast	Identified effect	Interpretation	Figures & Tables
	February 2021		
Group 1 vs. Group 2	$E[g(1,1,\frac{1}{2},\epsilon_i) - g(0,1,\frac{1}{2},\epsilon_i)\|L_i = 1]$	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 8, Figure 9, Table 5, Table 6,
	After April 2021		
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 8, Figure 9
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

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.,

$$\begin{aligned} E[g(L_i,L_i,1,\epsilon_i) - g(0,0,0,\epsilon_i)] =& E[g(1,1,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 1] \cdot P(L_i = 1) + \\ & E[g(0,0,1,\epsilon_i) - g(0,0,0,\epsilon_i)|L_i = 0] \cdot P(L_i = 0). \quad (2) \end{aligned}$$

3.3 Inference

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

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

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

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

For the surveys in Gramatneusiedl, we achieved a survey response rate of 73% in 2021 (with complete questionnaires for 69%) and of 77% in 2022 (with complete questionnaires for 73%). Only seven individuals did not participate in either of the surveys. 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.3) 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 (Table A.4 - Table A.6).

4 Findings

We are now ready to discuss our empirical findings.[7] Our headline findings are summarized by Figures 3 through 9 in this section, as well as Figures A.3 through A.5 in Appendix A. 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.); variables where the sign is flipped are marked by *(-)* in all our figures. Additional figures with results for further outcomes, alternative identification approaches, confidence intervals, and robustness checks can be found in Appendix A. Table 3 provides a roadmap through the findings presented in this section and in the appendix.

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

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

Findings For economic outcomes (shown in the top panels of Figure 3 and Table 4), measured using both survey and administrative data, we find highly significant positive effects.[8] Unemployment is strongly reduced in Group 1 through program participation. This is not due to transitions out of the labor force (e.g., to early retirement or disability status). Instead, our estimates show that this effect is fully driven by the increase in employment.

Participants who accept a guaranteed job increase their income. The estimates shown in Figure 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

[8]Recall the normalization of these outcome variables from Table A.7: Employment and unemployment are defined as the share of days since the program started, and the monthly income is divided by 2000.

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

Turning to non-economic outcomes (bottom panels of Figure 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 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.

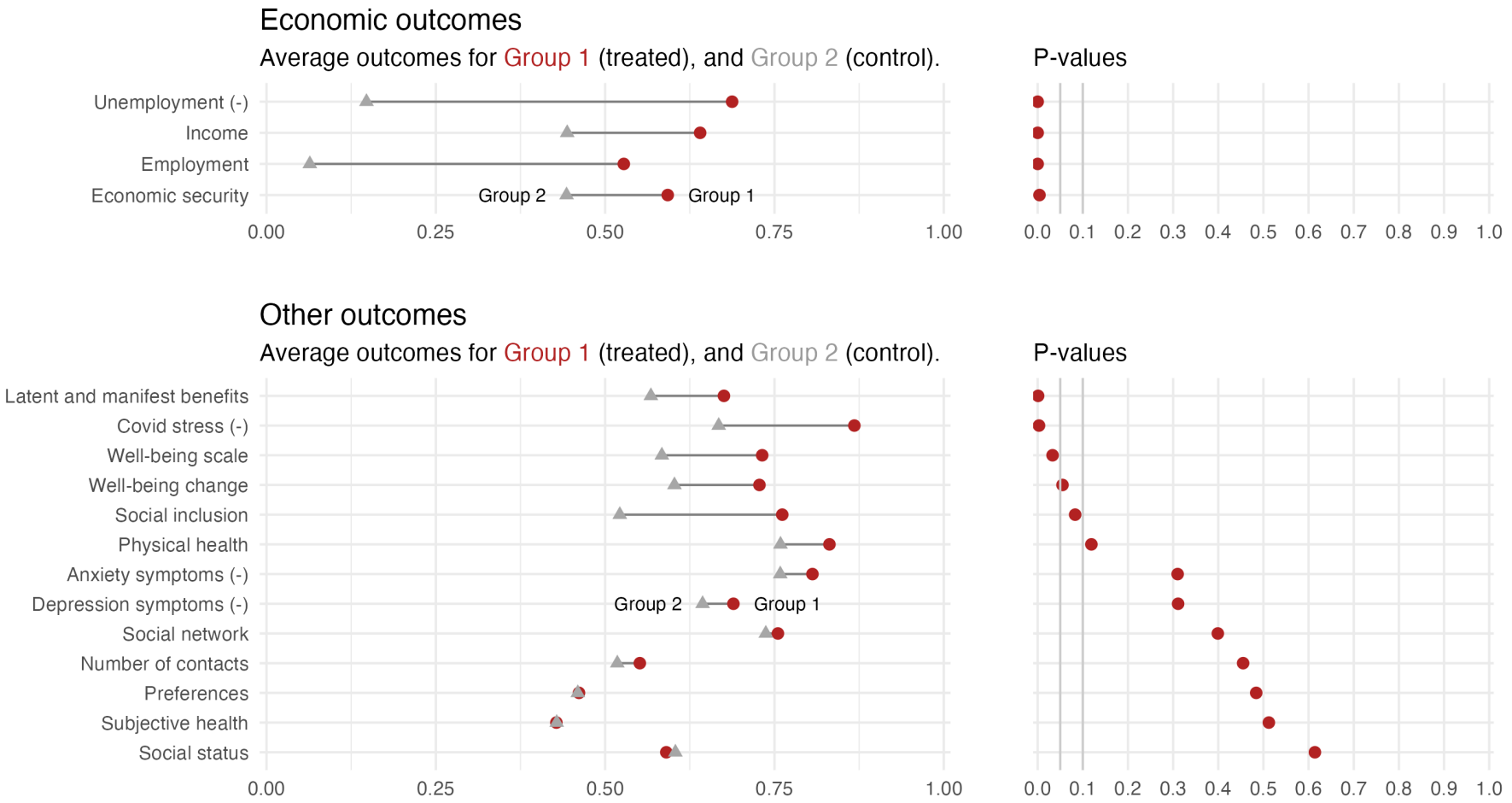
These effects are remarkable not only in their own right, but also because of the historical importance of Marienthal, which was the location of the original Jahoda et al. (1933) study, and because of the literature on the sociology of work which connects our study to Jahoda et al. (1933). The LAMB scale[9] was developed to quantify Jahoda's insight (Jahoda, 1982), based on the Marienthal study and subsequent work, that

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

[9]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.7. Higher values imply better outcomes. Outcomes are scaled to range from 0 to 1. Income is monthly income divided by 2000, and unemployment is share of days *not* unemployed since Oct 1, 2020. The right hand figures show p-values for tests of the null of a zero or negative effects of treatment. Small values imply positive effects of treatment. These p-values are based on 1000 simulation draws. These estimates are also tabulated in Table 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
Unemployment (-)	0.687	0.148	0.540	0.000	0.067	31	31
Income	0.640	0.444	0.196	0.000	0.072	19	19
Economic security	0.592	0.443	0.149	0.004	0.055	21	22

Other outcomes

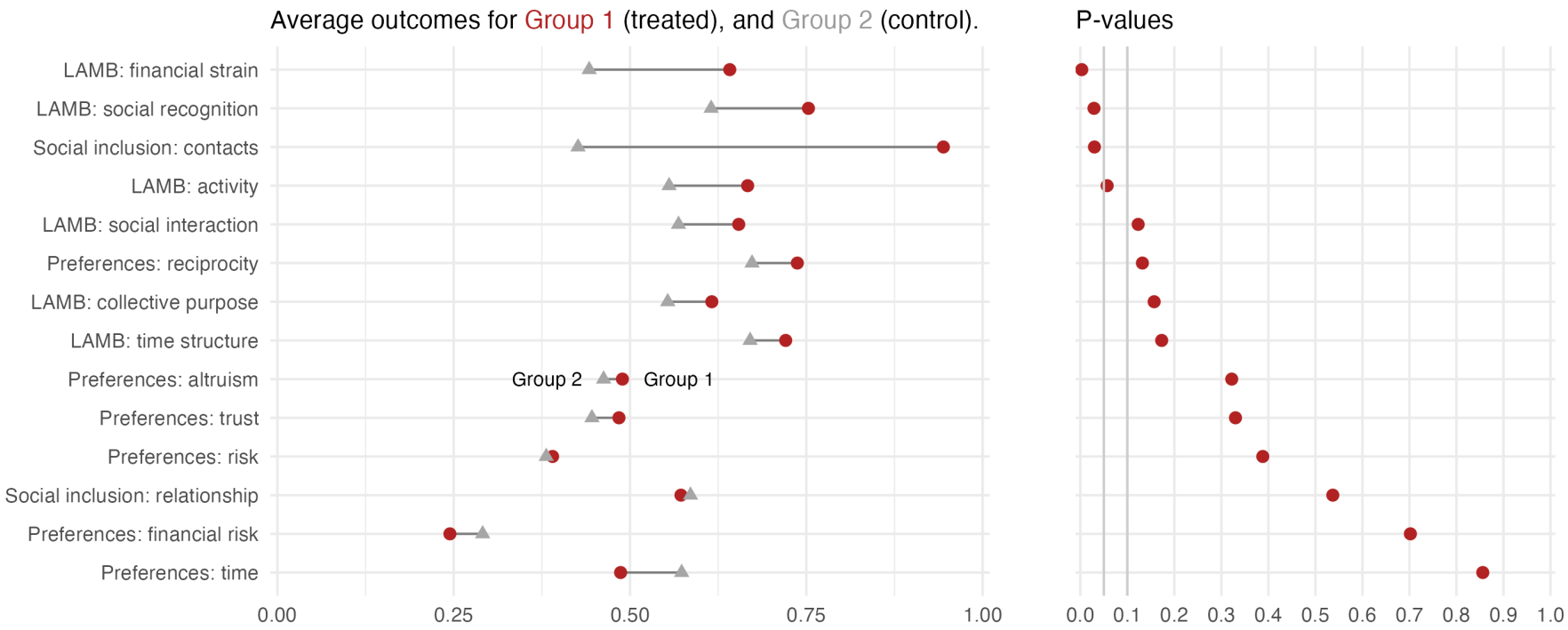
Outcome	Treated	Control	Difference	p-value	SE	n_1	n_2
Latent and manifest benefits	0.675	0.568	0.108	0.001	0.042	21	22
Covid stress (-)	0.868	0.668	0.200	0.003	0.072	20	22
Well-being scale	0.732	0.584	0.148	0.033	0.076	20	22
Well-being change	0.728	0.602	0.125	0.055	0.080	21	22
Social inclusion	0.761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

Disaggregated outcomes

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

Notes: These tables report the same estimates as Figure 3 and Figure 4. 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.[10] This permutation approach provides a formal analog to randomization inference. For each of the permutations, we consider another municipality as fictitiously treated, construct a synthetic control for this municipality, and plot the corresponding outcome gap. Extreme gaps for Gramatneusiedl, relative to these permutations, indicate program effects that are arguably not just driven by random fluctuations. Correspondingly, the last row of these figures plots the rank of Gramatneusiedl among the permutations.

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

[10] Figure A.2 in Appendix A 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.

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

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

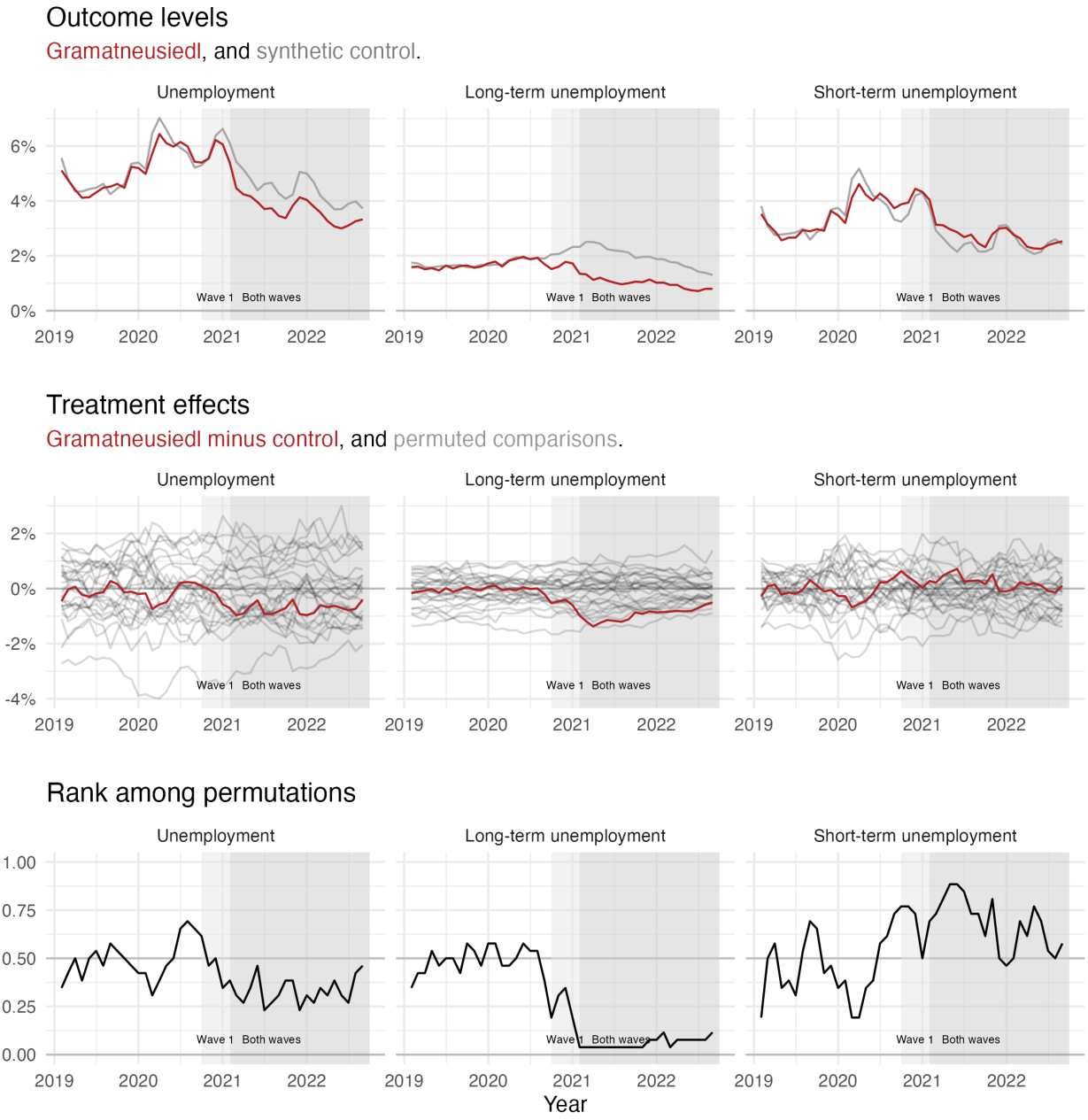
One might conjecture that the reduction of unemployment is driven by a transition of the unemployed out of the labor force, for instance into (early) retirement or into a certified disabled status, in order to avoid work requirements associated with the job guarantee. That this is not the case for the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

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 unsubsidized employment over time.

Compared to the control towns, participants in Gramatneusiedl spent substantially more time employed: 771 versus 216 days, on average, within 3.5 years. 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 employ-

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

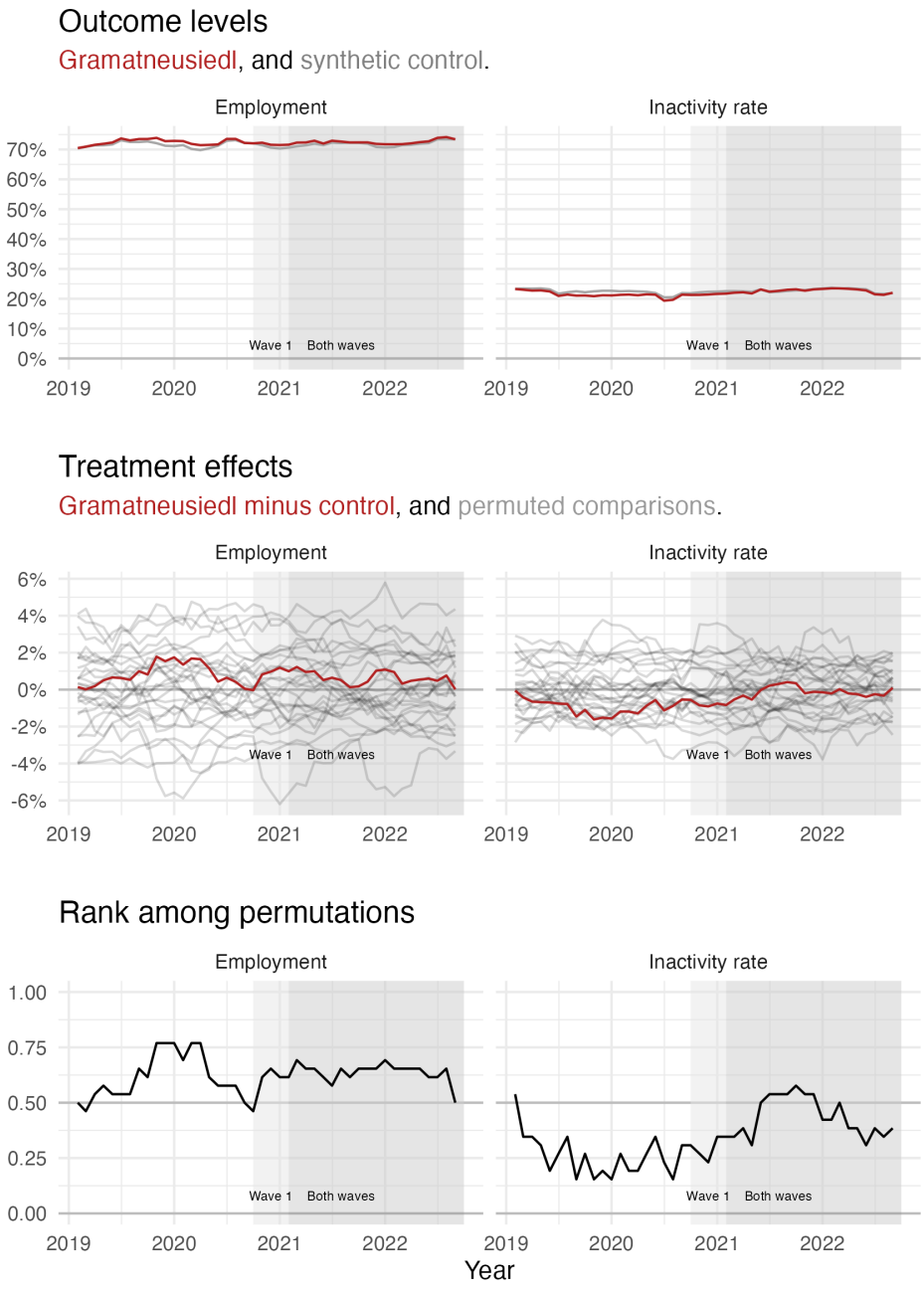
Figure 5: Synthetic control estimates of the program effect on unemployment



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

ment 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

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



force declined by 36 percent or 113 days.

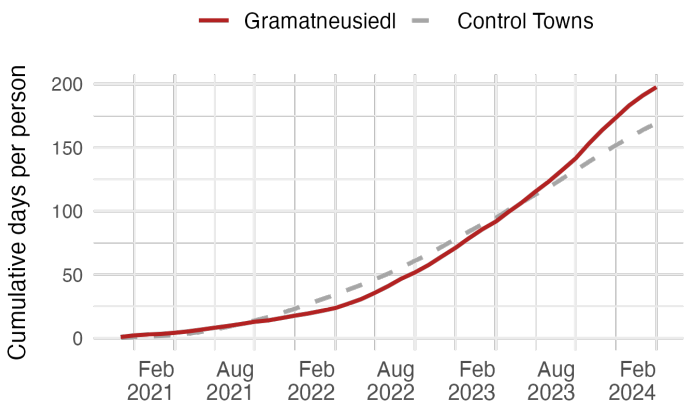
Figure 7: Cumulative days per person

(a) Employment status

	GN	Ct
Total Employment	771	216
Subsidized	556	38
Unsubsidized	198	169
Self-Employment	17	8
Registered Unemp.	275	717
Out of Labor Force	201	314

Notes: Subsidized employment, unsubsidized employment and self-employment sum up to total employment.

(b) Unsubsidized employment



4.3 Comparison to individuals in control towns

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

The resulting estimates are shown in Figure 8 and Table 5 for economic outcomes and Figure 9 and Table 6 for other outcomes. In both figures, we show outcomes for 2021 at the top, where we separate individuals in Group 1, Group 2, and the control towns, and outcomes for 2022, where we compare all eligible individuals in Gramatneusiedl (Group 1 and 2), to individuals in the control towns.

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

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

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

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

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

2021

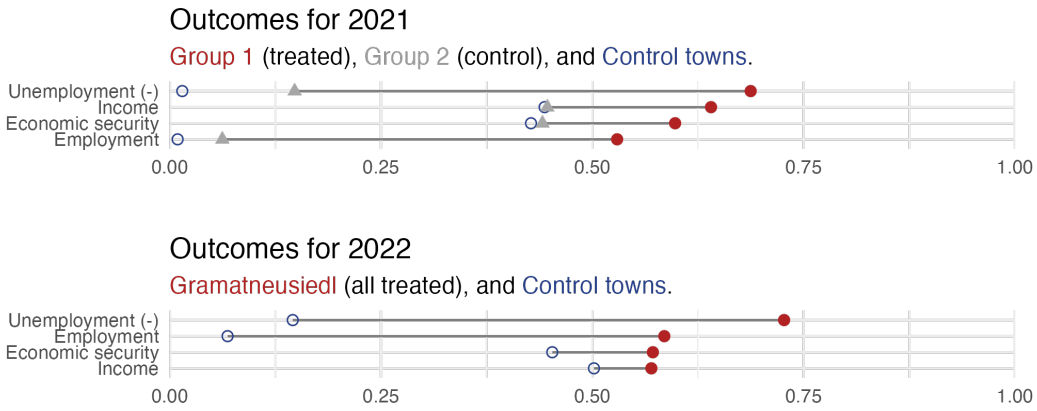
Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	n_1	n_2	n_{ct}
Unemployment (-)	0.687	0.148	0.015	0.132	0.054	31	31	71
Income	0.640	0.447	0.443	0.009	0.016	19	19	59
Economic security	0.598	0.441	0.427	0.012	0.038	21	22	63
Employment	0.529	0.062	0.009	0.060	0.040	31	31	71

2022

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

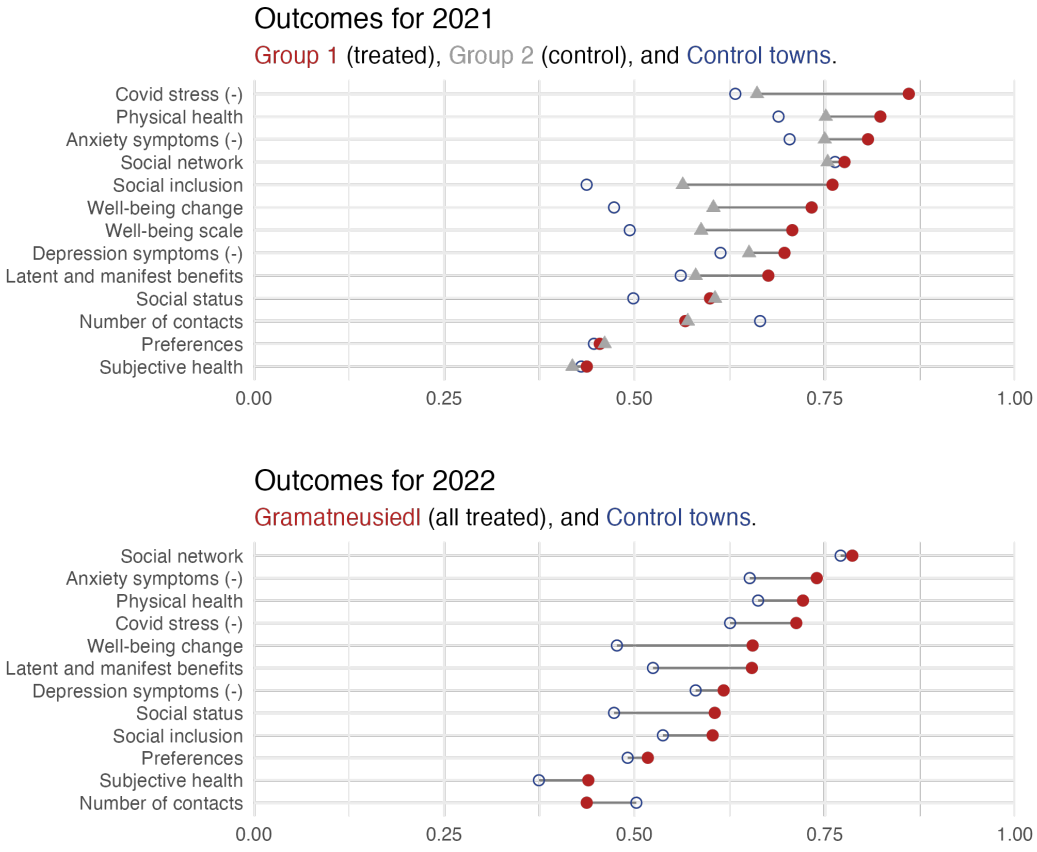
Notes: These tables report the same estimates as Figure 8, Figure A.4, and Figure A.5. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Gramatneusiedl and Control town observations.

Figure 8: Control town comparisons with linear controls, economic outcomes



Notes: These estimates are also tabulated in Table 5.

Figure 9: Control town comparisons with linear controls, other outcomes



Notes: These estimates are also tabulated in Table 6.

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

2022

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

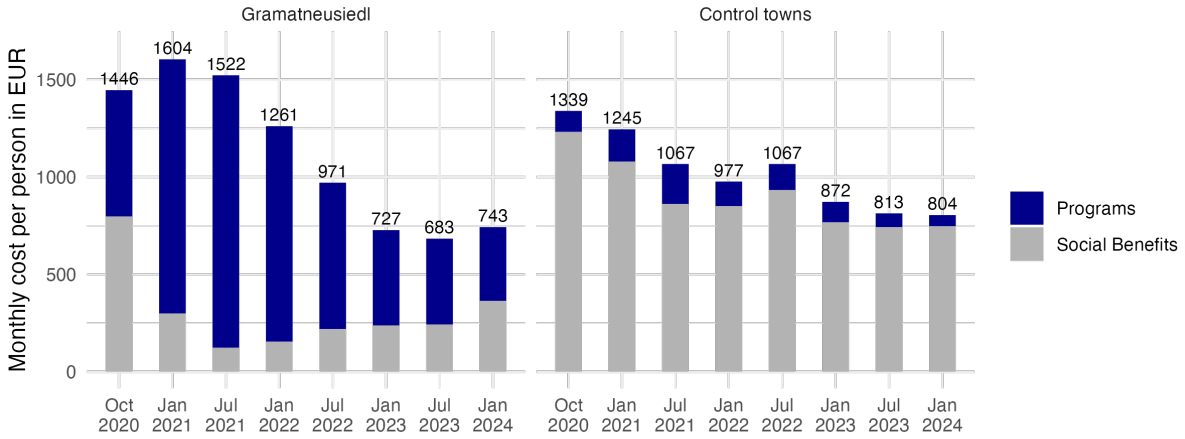
Notes: These tables report the same estimates as Figure 9, Figure A.4, and Figure A.5. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Gramatneusiedl and Control town observations.

4.4 Cost comparison

We next turn to an evaluation of program costs. We again compare participants, in both Group 1 and 2, to comparison individuals in control towns. 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) social benefits, in particular unemployment benefits (incurred by the social insurance system), and (ii) program costs (incurred by the AMS). 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), net of the revenues generated by the public enterprise.

Findings The job guarantee increased labor market policy expenditures in the first 18 months; thereafter expenditures declined, as shown in Figure 10. This decline 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, the job guarantee increased monthly labor market policy expenditures per registered long-term unemployed job seeker by 28 percent, from EUR 850 to EUR 1,092 (see Table 7).

Figure 10: Expenditures per person and month



Notes: Total expenditure per participant and month, decomposed into programs (active measures), and benefits (social insurance).

Participant income and goverment revenue The additional cost of EUR 388 per participant per month is matched by an increase in income of EUR 390 per participant. If a positive value is assigned to the non-monetary benefits of the job-guarantee, this suggests a "marginal value of public funds" greater than 1.

Note furthermore that some of the program costs flowed back to the state, in the form of payroll taxes and social insurance contributions. Program costs therefore overestimate the net costs of the job guarantee. In Austria, the sum of payroll taxes and

Table 7: Comparison of expenditure and benefits between eligible and non-eligible participants

	Programs	Social benefits	Total costs
Gramatneusiedl	EUR 858	EUR 234	EUR 1,092
Control towns	EUR 127	EUR 723	EUR 850
Additional cost per participant			EUR 242
Additional cost per participant until Dec 2022			EUR 388
Additional income per participant until Dec 2022			EUR 390

Notes: Costs are measured per month from October 2020 to March 2024. Additional income refers to the period from October 2020 to December 2022. "Programs" refer to active labor market policy; "Social benefits" refer to passive labor market policy.

social insurance contributions equals 37% on average, for workers in the relevant wage bracket. This suggests that, after taking into account overhead costs, around 30% of nominal program costs directly flow back to the state.

5 Unintended consequences: Theory and evidence

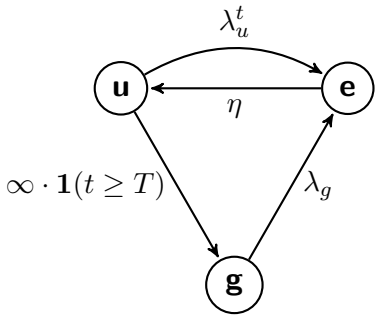
How do our empirical findings compare to the predictions of economic theory? We will discuss two theoretical models of the effect of a job guarantee for the long-term unemployed. The first model captures possible incentive effects of such a job guarantee: Anticipation of a guaranteed job might impact the search effort of the unemployed, and thereby reduce hazard rates out of unemployment. The second model captures possible spillover effects of a job guarantee: Employment in the program might displace market employment, by substituting labor in guaranteed jobs for labor at private employers.

Neither of these predictions are borne out in our data. We find no evidence for either anticipation effects on job finding rates, or for displacement of other jobs. The lack of anticipation effects suggests that there is only limited scope, for those who are at risk of long-term unemployment, to increase their job finding rates via increased search effort. The lack of spillover effects suggests that the type of services provided by program participants do not act as a substitute for services provided via market employment.

5.1 A search model of incentives in a job guarantee program

Our first model is a search model of unemployment in the presence of a job guarantee, in the spirit of Van den Berg (1990); Pissarides (2000). Here we focus on worker search effort, and sidestep questions of equilibrium. Using this model, we analyze the incentives introduced by a job guarantee for the long-term unemployed, derive the implied time dynamics for transition rates out of unemployment, and characterize comparative statics with respect to the parameters of a job guarantee. We then compare these predictions

Figure 11: Flows between employment states



Notes: The rates λ_g and λ_u^t are choice variables, where the latter might be a function of unemployment duration t. Both of these rates might be equal to 0, depending on parameters.

to our estimated time-dynamics of transitions out of unemployment, where we use our synthetic control comparison towns to estimate the effect of the job guarantee on these dynamics.

Assumptions Let $t \geq 0$ denote the time since a worker became unemployed. At each time t, a worker can be in one of three states. They can be unemployed (subscript u), employed in the regular labor market (subscript e), or employed via the job guarantee program (subscript g). The worker's flow utility depends on their income and on job amenities in each state, both of which we assume to be exogenously given and known to the worker. Flow utility in unemployment is v_u, flow utility in market employment is v_e, and flow utility in the job guarantee is v_g. The worker's discount rate equals ρ.

The flows between different states are summarized in Figure 11, and are determined as follows. Jobs on the regular labor market dissolve at an exogenous rate η. Guaranteed jobs do not dissolve. Unemployed workers can search for a job, which they find at a rate λ_u^t. This is a choice variable, which might vary over time. A search effort that yields λ_u^t has a flow cost of $c(\lambda_u^t)$, where the function $c(\cdot)$ is strictly increasing and convex. Similarly, workers with a guaranteed job can search for market employment, which they find at a rate λ_g. A search effort that yields λ_g again has a flow cost of $c(\lambda_g)$. Unemployed workers who have been unemployed for at least T time periods ($t \geq T$) can enter a guaranteed job. If t is measured in months, then $T = 9$ in our context. Assuming that $v_g \geq v_u$ (i.e., the job guarantee is more attractive than unemployment), this implies that unemployed workers with $t \geq T$ will deterministically enter the job guarantee; formally at an infinite rate.

Bellman equations Denote the expected discounted utility of an unemployed worker at time t by V_u^t, of an employed worker by V_e, and of a worker in the job guarantee by V_g. Write $\dot{V}_u^t = \frac{\partial V_u^t}{\partial t}$ for the time derivative of V_u^t Based on our assumptions, we get the following Bellman equations for the expected discounted utility of workers in the different

states:

$$\begin{aligned}
\rho V_u^t &= v_u - c(\lambda_u^t) + \dot{V}_u^t && + \lambda_u^t \cdot (V_e - V_u^t) && \text{if } t < T, \\
\rho V_e &= v_e && + \eta \ \cdot (V_u^0 - V_e), \\
\rho V_g &= v_g - c(\lambda_g) && + \lambda_g \cdot (V_e - V_g). && (3)
\end{aligned}$$

Only V_u^t and λ_u^t are functions of t in this model; utilities and transition rates are time-invariant in all other states, by construction. For $t \geq T$, we get the boundary condition $V_u^t = V_g$.

Optimal search rates Since $c(\cdot)$ is strictly increasing and convex, the first order conditions for optimal search rates imply

$$c'(\lambda_u(t)) = V_e - V_u^t, \qquad c'(\lambda_g) = V_e - V_g. \tag{4}$$

Since $c(\cdot)$ is strictly increasing, the latter equation implies that $\lambda_g = 0$ if $V_g > V_e$. This holds in particular if $v_g > v_e$. Therefore, if the flow utility in the job guarantee program exceeds that of market employment, then the job guarantee is an absorbing state. This simplifies the Bellman equation for V_g to $\rho V_g = v_g$. Define

$$c^*(V) = \sup_{\lambda \geq 0} \left[\lambda \cdot V - c(\lambda) \right].$$

This is known as the Legendre transform, or convex conjugate, of the function $c(\lambda)$. $c^*(\cdot)$ is again monotonically increasing and convex. Using this notation, and rearranging the Bellman equation for V_u^t gives

$$\dot{V}_u^t = -v_u + \rho V_u^t - c^*(V_e - V_u^t). \tag{5}$$

This equation defines a first-order differential equation for the time path of V_u^t.

Discussion and comparative statics The *solution* V_u^t to this differential equation is increasing over time, at an accelerating rate, from its initial value V_u^0, to its maximal value V_g. Correspondingly, $\lambda_u(t) = (c')^{-1}(V_e - V_u^t)$ is decreasing over the duration of the unemployment spell. If $V_g > V_e$, so that the job guarantee is preferred to market employment, then there is a time $T' < T$ after which $\lambda_u^t = 0$, so that no more transitions to market employment occur. If $V_g < V_e$, so that market employment is preferred, then λ_u^t also declines over time, but remains bounded away from 0. In this case, transitions to market employment keep occurring after the start of the job guarantee, at a rate $\lambda_g > 0$.

Two key *policy parameters* characterize the job guarantee in our model: The flow utility v_g, which captures how attractive the guaranteed jobs are, and the time T at which eligibility starts. If we increase v_g, then this increases the expected discounted value V_g

of a guaranteed job, and thus increases the value of $V_u^T = V_g$. This in turn decreases the optimal rate λ_u^t of flows from unemployment to market employment. Similarly, a decrease of the eligibility threshold T shifts the solution V_u^t, and correspondingly λ_u^t, leftward. Since both V_u^t and λ_u^t are monotonic in time, such a shift in time implies an increase of V_u^t, and a decrease of λ_u^t, for any given t.

Our exposition has abstracted from *heterogeneity*. In general, the flow utilities v_u, v_e, and v_g will vary across workers, and the same is true for the cost function c and for the transition rates λ and η. Heterogeneity typically leads to declining job-finding rates over time. Even in the presence of heterogeneity, however, the key empirical prediction of our search model remains the same: 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.

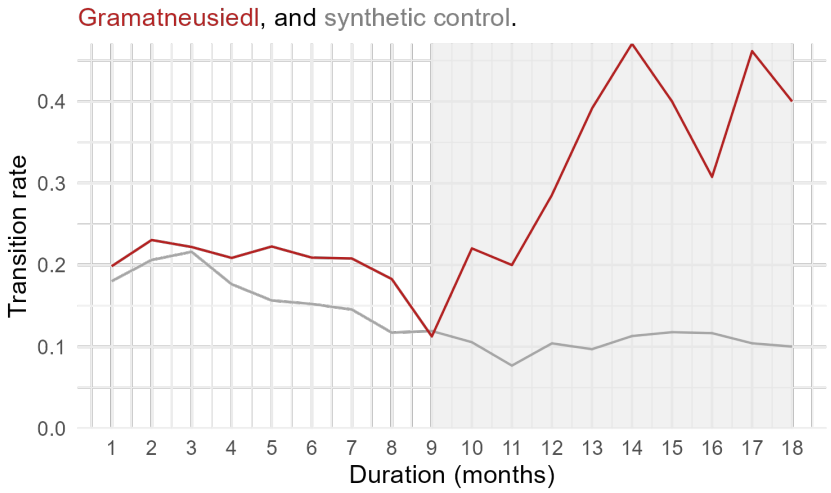
Empirical hazard rates and synthetic control comparison Are these predictions of our model borne out in the data? To estimate the effect of the job guarantee on hazard rates, we compare short term unemployed workers in Gramatneusiedl to those in the synthetic control municipalities.

We calculate hazard rates as follows.[12] Using the full sample of all residents registered with the public employment service, and drawing on data from the "AMDB Erwerbskarrierenmonitoring" 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 divide 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 12. We find that the transition rate into employment was *higher* in Gramatneusiedl, for every spell duration below than the MAGMA eligibility threshold of 9 months, and the *decline* of hazard rates was *slower*. This is contrary to the predictions of the search model. Regardless of standard errors, we cannot reject the null hypothesis that the job guarantee did not decrease search effort. After 9 months, transitions into employment in Gramatneusiedl increased further. This increase was due to the mechanical effect of the job guarantee program.

[12] This part of our empirical analysis was not pre-registered.

Figure 12: 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 Erwerbskarrierenmonitoring" database. Job guarantee eligibility starts after 9 months.

5.2 Spillovers and labor demand

Having considered possible incentive and anticipation effects of a job guarantee on the unemployed, let us now turn to a discussion of possible demand spillovers and substitution effects across workers.

Assumptions Consider the following stylized, static model of labor demand. There are two types of workers, $j = 1, 2$. Type 2 workers are at risk of long-term unemployment, while type 1 workers are not. Type 2 workers are eligible for a guaranteed job, when a job guarantee is introduced. The total output of the local economy is determined by the production function

$$y = f(N_1, N_2), \tag{6}$$

where N_1 and N_2 are the numbers of *employed* workers of type 1 and 2, respectively. Denote the derivatives of f by $f_j = \frac{\partial f}{\partial N_j}$, and $f_{j,j'} = \frac{\partial^2 f}{\partial N_j \partial N_{j'}}$. If employers are profit maximizing and take wages as given, then they will hire workers up to the point where wages w_j are equal to marginal productivity. We get $w_j = f_j$ for $j = 1, 2$, and thus in particular

$$\frac{w_1}{w_2} = \frac{f_1}{f_2}. \tag{7}$$

Relative wages in competitive markets with full employment A large literature has studied the impact of changes in the labor supply of different workers on wage inequality, assuming full employment and competitive wage setting, where wages equal marginal productivity. This includes the literature on the impact of immigration on wages, and the literature on skill biased technical change; cf. Card (2009); Boustan (2009); Autor

et al. (2008).

Consider the elasticity of relative wages with respect to relative labor supply. Assuming constant returns to scale of f, this elasticity can be written as

$$\frac{\partial \log\left(\frac{w_1}{w_2}\right)}{\partial \log\left(\frac{N_1}{N_2}\right)} = -\frac{\partial \log\left(\frac{w_1}{w_2}\right)}{\partial \log\left(N_2\right)} = \frac{\partial \log\left(\frac{w_1}{w_2}\right)}{\partial \log\left(N_1\right)} = (f_{11} - f_{12}) \cdot \frac{N_1}{f_1}. \tag{8}$$

Many papers in this literature estimate regressions of $\log\left(\frac{w_1}{w_2}\right)$ on $\log\left(\frac{N_1}{N_2}\right)$, possibly using instruments or natural experiments, and interpret the slope of such regressions as the inverse of the elasticity of substitution σ.

Employment rates at fixed wages The interpretation of such regressions in terms of a production function f assumes full employment for all types of workers, as well as wage setting by profit maximizing employers in a competitive labor market. In the institutional context of the Austrian labor market, and especially in the short run, it is however more realistic to assume that wages are fixed by collective bargaining at the sectoral level, and that adjustments in local labor markets happen through the employment margin.

Suppose thus in particular that w_1 is fixed. Suppose further that the number N_2 of employed type 2 workers is exogenously increased via the job guarantee program. What is the impact of this increase on labor demand for type 1 workers? If type 1 workers are hired up to the point where their marginal productivity is equal to their wage, then $w_1 = f_1$. Differentiating this condition with respect to N_2 yields $0 = f_{11} \cdot \frac{\partial N_1}{\partial N_2} + f_{12}$, which implies

$$\frac{\partial \log(N_1)}{\partial \log(N_2)} = -\frac{f_{12}}{f_{11}} \cdot \frac{N_2}{N_1}. \tag{9}$$

It is interesting to compare the expressions in Equations (8) and (9). The impact of an increase of the supply of N_2 workers, in the competitive setting and assuming constant returns to scale, depends on the *difference* between f_{12} and f_{11}. If $f_{12} > f_{11}$, then an increase of N_2 leads to a relative increase of the wage of type 1. By contrast, in the setting with fixed wage w_1 and for an exogenous increase of the number N_2 of employed type 2 workers, the impact on the number of employed type 1 workers depends on the *ratio* of f_{12} and f_{11}. It is plausible to assume $f_{11} < 0$ (decreasing returns to scale, when holding other factors fixed). Under this condition, N_1 is increasing in N_2 if $f_{12} > 0$, and decreasing otherwise. Furthermore, N_1 is decreasing less than N_2 is increasing ($\frac{\partial N_1}{\partial N_2} > -1$) whenever $f_{12} > f_{11}$. If this condition holds, then total employment goes up whenever N_2 is increased via the job guarantee. This condition holds if and only if the elasticity of substitution σ is positive, that is, if a relative increase of labor supply leads to a relative decrease of wages.

Comparison to empirical findings This theoretical characterization suggests two empirical questions: (1) As more workers are enrolled in the job guarantee, does total employment $N_1 + N_2$ go up? (2) As more workers are enrolled in the job guarantee, does the employment N_1 of type 1 workers go down? Our empirical estimates using the synthetic control approach in Section 4 speak to these questions. Our point estimates suggest that the introduction of the job guarantee led to a marked decrease of total unemployment. Furthermore, we did not find evidence of crowd-out, that is, our findings are consistent with a model where $\frac{\partial N_1}{\partial N_2} \approx 0$, that is $f_{12} \approx 0$.

6 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 roll-out design, comparing earlier and later entrants into the program, to identify direct effects of the job guarantee on the treated. We use a synthetic control approach at the municipality level to identify spillover effects of the job guarantee on the untreated, as well as the average total effect of the job guarantee on the labor market. And we compare program participants to observationally similar individuals in control towns, to separate out anticipation effects, and to estimate the long-term effects of the job guarantee.

Assignment to the two groups (early and late entrants) in the experimental comparison is based on pairwise matched random assignment. This approach allows us to increase the precision of our estimates by making the two groups observationally as similar as possible. This reduces standard errors relative to conventional random assignment, which is particularly relevant given our small sample size. Both the pairwise matches and the synthetic control weights were pre-registered. This ties our hands and prevents us from cherry-picking results, including for the observational comparisons in our evaluation. Our inference approach is primarily based on randomization inference (permuatation inference). This guarantees finite sample validity without any asymptotic approximations. In Appendix A, 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. These non-econonomic effects of employment have been discussed in the sociological literature, mostly in the context

of observational studies, but have received less attention in economics. We do not find effects on physical health and economic preferences, including time and risk preferences, reciprocity, altruism, and trust. The estimated effects persist over time. We further find a large reduction of municipality-level unemployment, which is driven by a near-elimination of long-term unemployment. There appears to be no increase of short-term unemployment. 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 longer term. The temporary increase of 28% in expenditures is furthermore fully matched by the increase of participant income.

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 roll-out, recently debated in parliaments (U.S. Senate, 2023; European 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 benefits of employment might lead to a refined understanding in economics of labor supply and labor market dynamics more generally.

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

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June 23, 2025

Contents

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A Additional tables and figures

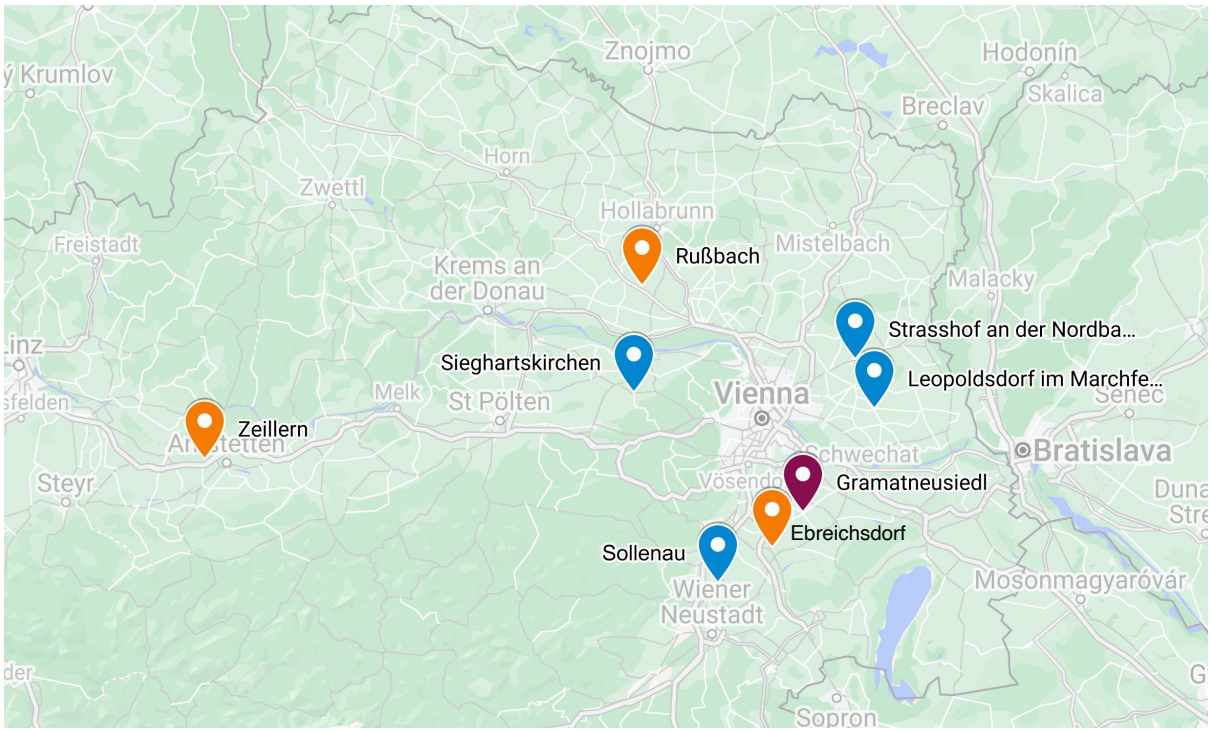
A.1 Synthetic control: Further details

Table A.1: Variables used for the construction of the synthetic control

Variable	Definition
Working age pop	Working age population.
Long term unemp/pop	Number of long-term unemployed (> 1 year) as a share of working age pop.
Inactive/pop	Number of inactive persons in working age as a share of working age pop.
Mean age	Mean age in years of the total population.
Share small firms	Small firms (less than 10 employees) as a share of total firms.
Share mid firms	Medium sized firms (10-249 employees) as a share of total firms.
Share low edu	Persons with low education (ISCED 1-2) as a share of total pop.
Share mid edu	Persons with medium education (ISCED 3-4) as a share of total pop.
Share men	Male persons as a share of total pop.
Share migrant	Persons with a migrant background as a share of total pop.
Share care resp	Active persons with care responsibilities as a share of total pop.
Mean wage	Mean wage level.
Mean age unemp	Mean age in years of the unemployed.
Low edu/unemp	Unemployed with low education (ISCED 1-2) as a share of total unemployed.
Mid edu/unemp	Unemployed with medium education (ISCED 3-4) as a share of total unemployed.
Poor German/unemp	Unemployed with low German skills (< A2 CEFR) as a share of total unemployed.
Men/unemp	Male unemployed as a share of total unemployed.
Migrant/unemp	Unemployed with a migrant background as a share of total unemployed.
Health cond/unemp	Unemployed with a medical condition limiting employment opportunities as a share of total unemployed.
Communal tax/pop	Communal tax per working age pop.

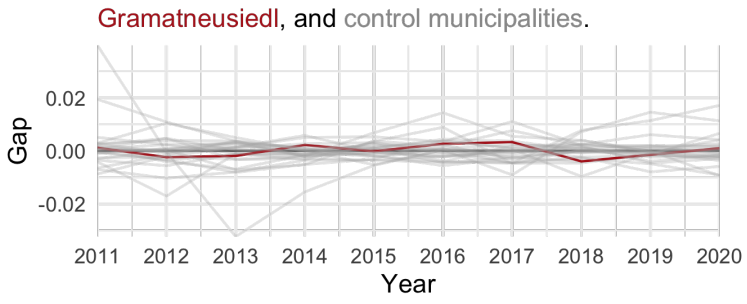
Notes: This table describes the variables used for the construction of the synthetic control municipality; cf. Table A.2.

Figure A.1: Location of municipalities included in the synthetic control



Notes: Gramatneusiedl, the treated municipality, is marked in red. The 3 municipalities with the largest weights in the synthetic control are marked in orange. Municipalities with smaller weights are marked in blue.

Figure A.2: Unemployment gap and permutation inference.



Notes: This figure shows the unemployment gap between Gramatneusiedl and its synthetic control (red), and between each of the 25 potential control municipalities and *their* synthetic control (grey). This figure parallels the second row of Figure 5, for the 10 years before the MAGMA program.

Table A.2: Gramatneusiedl and control municipality covariates

Variables observed in December 2019

Municipality	Working age pop	Long term unemp/pop	Inactive/pop	Mean age	Share small firms	Share mid firms	Share low edu
Gramatneusiedl	5013	0.007	0.220	50.775	0.115	0.339	0.208
Synthetic control	4830	0.016	0.228	51.074	0.126	0.363	0.225
Zeillern	1263	0.004	0.227	50.229	0.093	0.335	0.199
Ebreichsdorf	7655	0.020	0.228	50.810	0.139	0.381	0.235
Leopoldsdorf im Marchfelde	2035	0.022	0.247	51.304	0.135	0.348	0.242
Strasshof an der Nordbahn	6920	0.024	0.213	51.403	0.115	0.324	0.250
Rußbach	942	0.013	0.219	52.230	0.126	0.369	0.206
Sieghartskirchen	4560	0.010	0.224	52.464	0.135	0.337	0.197
Sollenau	5122	0.017	0.248	54.286	0.129	0.360	0.284

Municipality	Share mid edu	Share men	Share migrant	Share care resp	Mean wage	Mean age unemp	Low edu/unemp
Gramatneusiedl	0.642	0.511	0.242	0.257	3416	42.694	0.530
Synthetic control	0.644	0.503	0.181	0.235	3293	43.422	0.452
Zeillern	0.702	0.509	0.053	0.256	3168	40.462	0.346
Ebreichsdorf	0.620	0.498	0.234	0.235	3379	44.344	0.465
Leopoldsdorf im Marchfelde	0.619	0.498	0.260	0.216	3294	43.627	0.513
Strasshof an der Nordbahn	0.600	0.496	0.276	0.257	3393	42.364	0.465
Rußbach	0.676	0.513	0.088	0.224	3137	45.500	0.525
Sieghartskirchen	0.641	0.510	0.195	0.206	3366	41.257	0.387
Sollenau	0.608	0.496	0.229	0.193	3235	41.819	0.521

Municipality	Mid edu/unemp	Poor German/unemp	Men/unemp	Migrant/unemp	Health cond/unemp	Communal tax/pop	Lt ue/pop 2020
Gramatneusiedl	0.455	0.082	0.627	0.418	0.245	57.281	0.009
Synthetic control	0.516	0.061	0.583	0.312	0.264	217.301	0.018
Zeillern	0.654	0.000	0.692	0.115	0.303	97.822	0.004
Ebreichsdorf	0.480	0.086	0.546	0.374	0.213	282.242	0.022
Leopoldsdorf im Marchfelde	0.473	0.093	0.573	0.467	0.256	284.806	0.023
Strasshof an der Nordbahn	0.496	0.089	0.528	0.472	0.303	160.549	0.027
Rußbach	0.475	0.025	0.575	0.200	0.375	97.079	0.016
Sieghartskirchen	0.552	0.054	0.609	0.360	0.281	329.855	0.012
Sollenau	0.460	0.140	0.558	0.457	0.282	308.998	0.019

Variables observed in July 2020

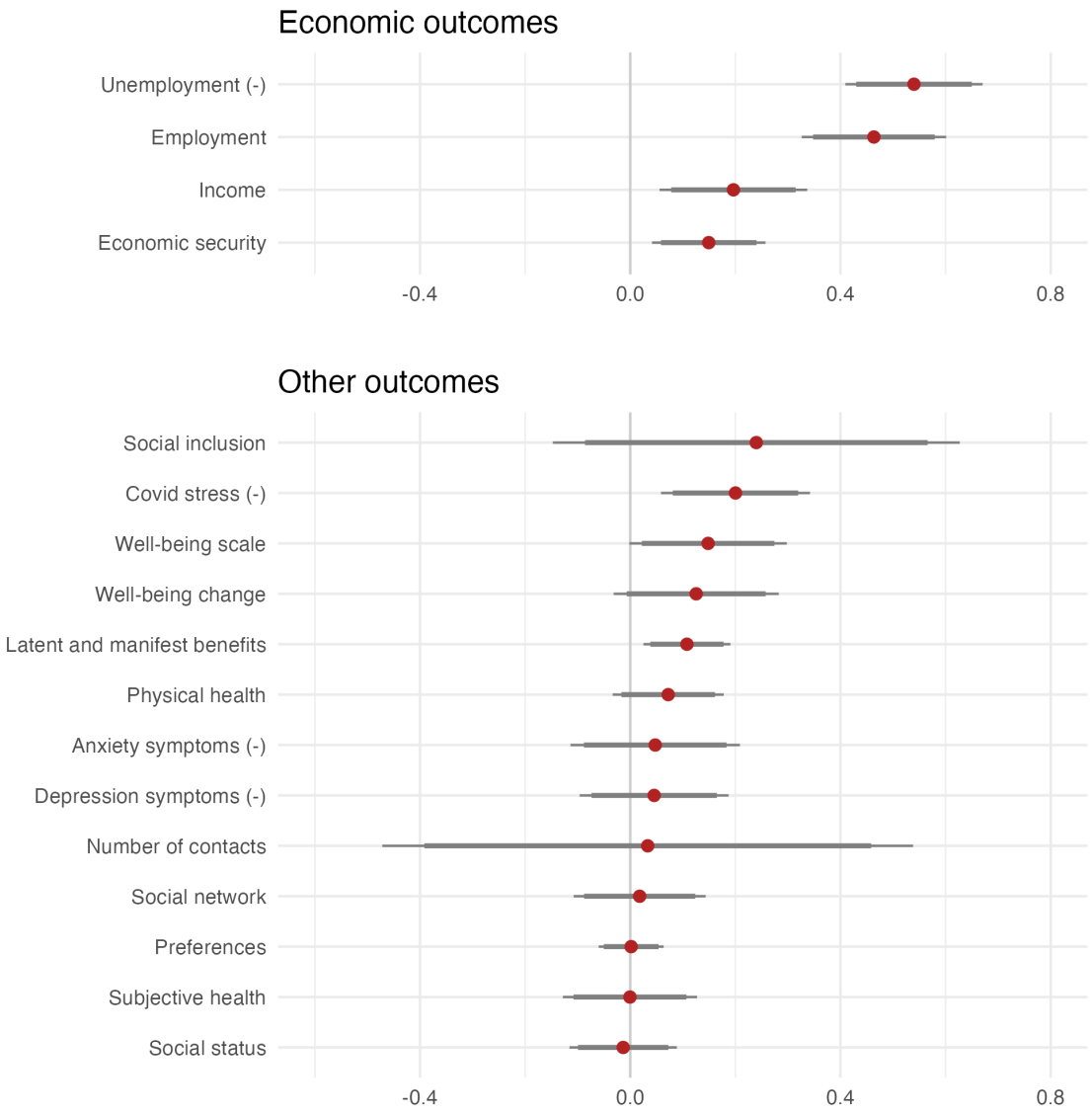
Municipality	Inactive/pop	Mean wage	Mean age ue	Low edu/ue	Mid edu/ue	Poor German/ue	Health cond/ue
Gramatneusiedl	0.209	3308	42.069	0.456	0.481	0.031	0.209
Synthetic control	0.219	3181	42.625	0.389	0.577	0.059	0.212
Zeillern	0.222	3025	41.474	0.289	0.711	0.000	0.193
Ebreichsdorf	0.217	3278	43.101	0.424	0.527	0.082	0.169
Leopoldsdorf im Marchfelde	0.244	3222	44.021	0.472	0.507	0.056	0.225
Strasshof an der Nordbahn	0.202	3264	41.188	0.458	0.493	0.061	0.260
Rußbach	0.208	3022	42.314	0.343	0.629	0.057	0.349
Sieghartskirchen	0.220	3241	43.406	0.319	0.626	0.043	0.278
Sollenau	0.238	3071	41.847	0.460	0.517	0.119	0.274

Table A.3: Covariate balance for the individuals in our control town sample

Covariate	Gramatneusiedl	Control towns	Difference	T-statistic	P-value
Male	0.581	0.535	-0.045	0.523	0.602
Age	44.694	49.634	4.940	-2.496	0.014
Migration Background	0.339	0.310	-0.029	0.352	0.726
Education	0.452	0.535	0.084	-0.958	0.340
Medical condition	0.306	0.338	0.032	-0.386	0.700
Benefit level	29.839	34.535	4.697	-2.600	0.011
Days unemployed	1661.355	1638.521	-22.834	0.136	0.892

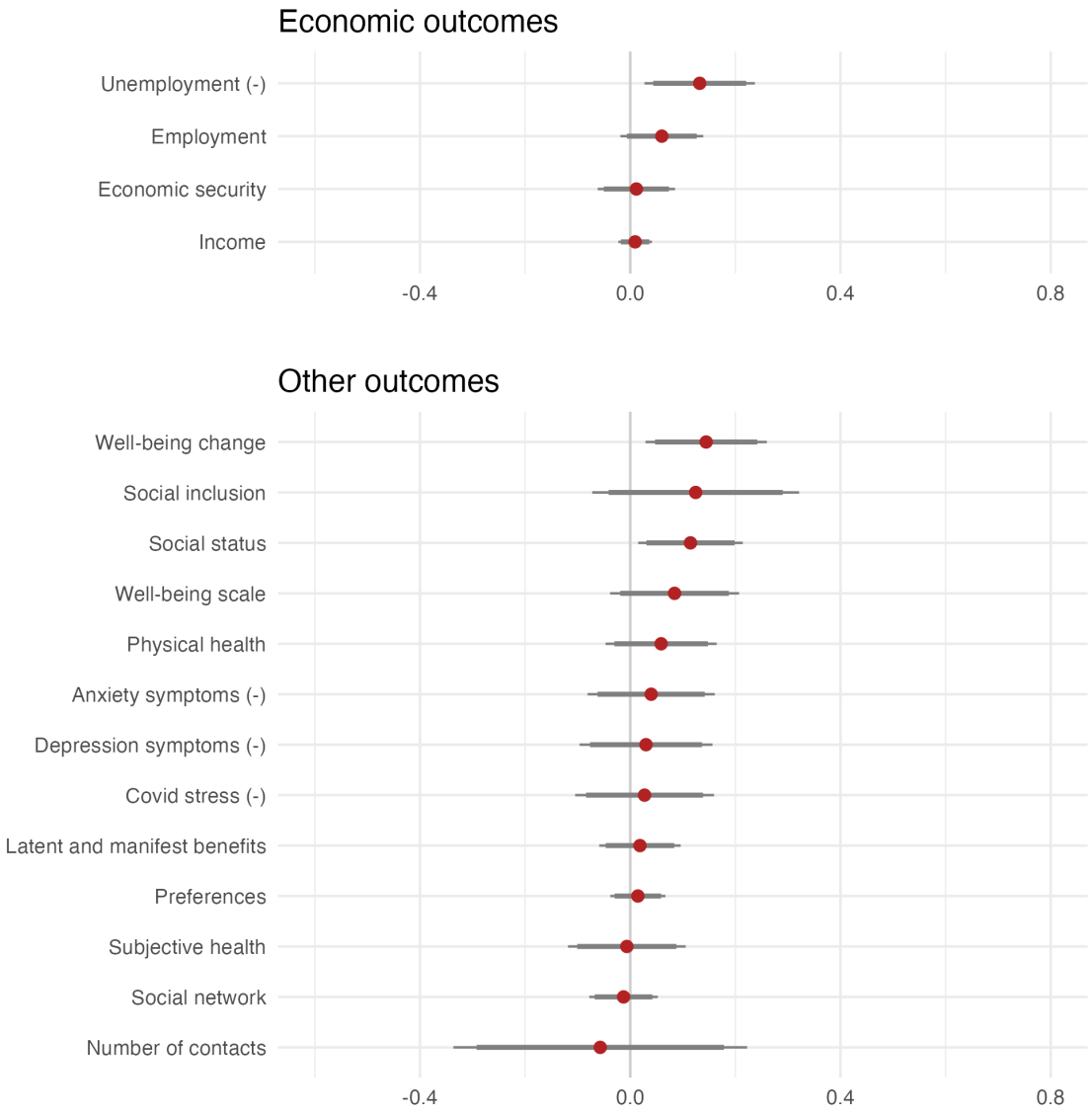
A.2 Confidence intervals

Figure A.3: Confidence intervals for contrast of Group 2 and Group 1 in February 2021



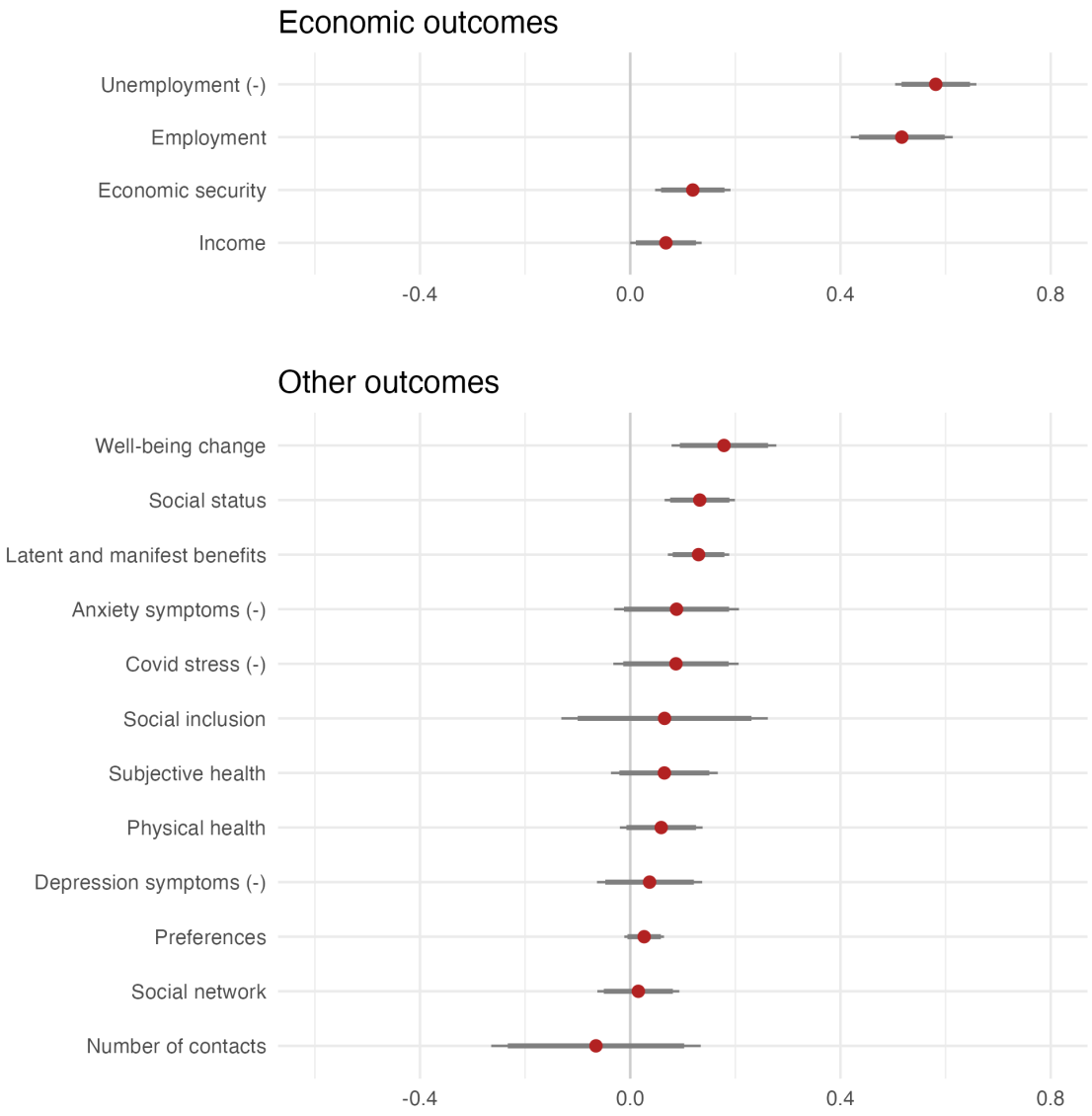
Notes: Confidence intervals for treatment effects, estimated with linear controls for baseline covariates, and with robust standard errors. The thin line shows the 95% confidence interval and the wider line shows the 90% confidence interval. These confidence intervals correspond to the estimates reported in Figure 3. These estimates are also tabulated in Table 4.

Figure A.4: Confidence intervals for contrast of Group 2 and control town individuals, February 2021



Notes: These confidence intervals correspond to the estimates reported in Figure 8 and Figure 9. These estimates are also tabulated in Table 5 and Table 6.

Figure A.5: Confidence intervals for contrast of participants in both groups and control town individuals, February 2022



Notes: These confidence intervals correspond to the estimates reported in Figure 8 and Figure 9. These estimates are also tabulated in Table 5 and Table 6.

A.3 Balance checks

Table A.4: Covariate balance for survey respondents in Gramatneusiedl, 2021

Covariate	Wave 1	Wave 2	Difference	t-statistic	p-value	n_1	n_2
Male	0.571	0.636	-0.065	-0.426	0.673	21	22
Age	42.857	47.727	-4.870	-1.394	0.171	21	22
Migration background	0.238	0.364	-0.126	-0.886	0.381	21	22
Education	0.524	0.545	-0.022	-0.139	0.890	21	22
Medical condition	0.238	0.318	-0.080	-0.575	0.568	21	22

Notes: This table shows the means of pre-determined covariates in the two treatment groups, among 2021 survey respondents, in analogy to Table 4 in the manuscript. The absence of significant differences suggests that differential attrition is not a problem.

Table A.5: Covariate balance for survey respondents in our control town sample, 2021

Covariate	Gramatneusiedl	Control towns	Difference	t-statistic	p-value	n_1	n_2
Male	0.605	0.535	0.069	0.722	0.472	43	71
Age	45.349	49.634	-4.285	-1.933	0.056	43	71
Migration background	0.302	0.310	-0.008	-0.084	0.933	43	71
Education	0.535	0.535	0.000	-0.003	0.997	43	71
Medical condition	0.279	0.338	-0.059	-0.660	0.511	43	71

Notes: This table shows the means of pre-determined covariates in Gramatneusiedl and control towns, among 2021 survey respondents, in analogy to Table 6 in the manuscript. The absence of significant differences again suggests that differential attrition is not a problem.

Table A.6: Covariate balance for survey respondents in our control town sample, 2022

Covariate	Gramatneusiedl	Control towns	Difference	t-statistic	p-value	n_1	n_2
Male	0.600	0.645	-0.045	-0.471	0.639	45	62
Age	46.044	48.726	-2.681	-1.228	0.223	45	62
Migration background	0.400	0.274	0.126	1.347	0.181	45	62
Education	0.444	0.581	-0.136	-1.390	0.168	45	62
Medical condition	0.311	0.355	-0.044	-0.471	0.639	45	62

Notes: This table shows the means of pre-determined covariates in Gramatneusiedl and control towns, among 2022 survey respondents, in analogy to Table 6 in the manuscript. The absence of significant differences again suggests that differential attrition is not a problem.

Figure A.6: Experimental estimates with pair controls

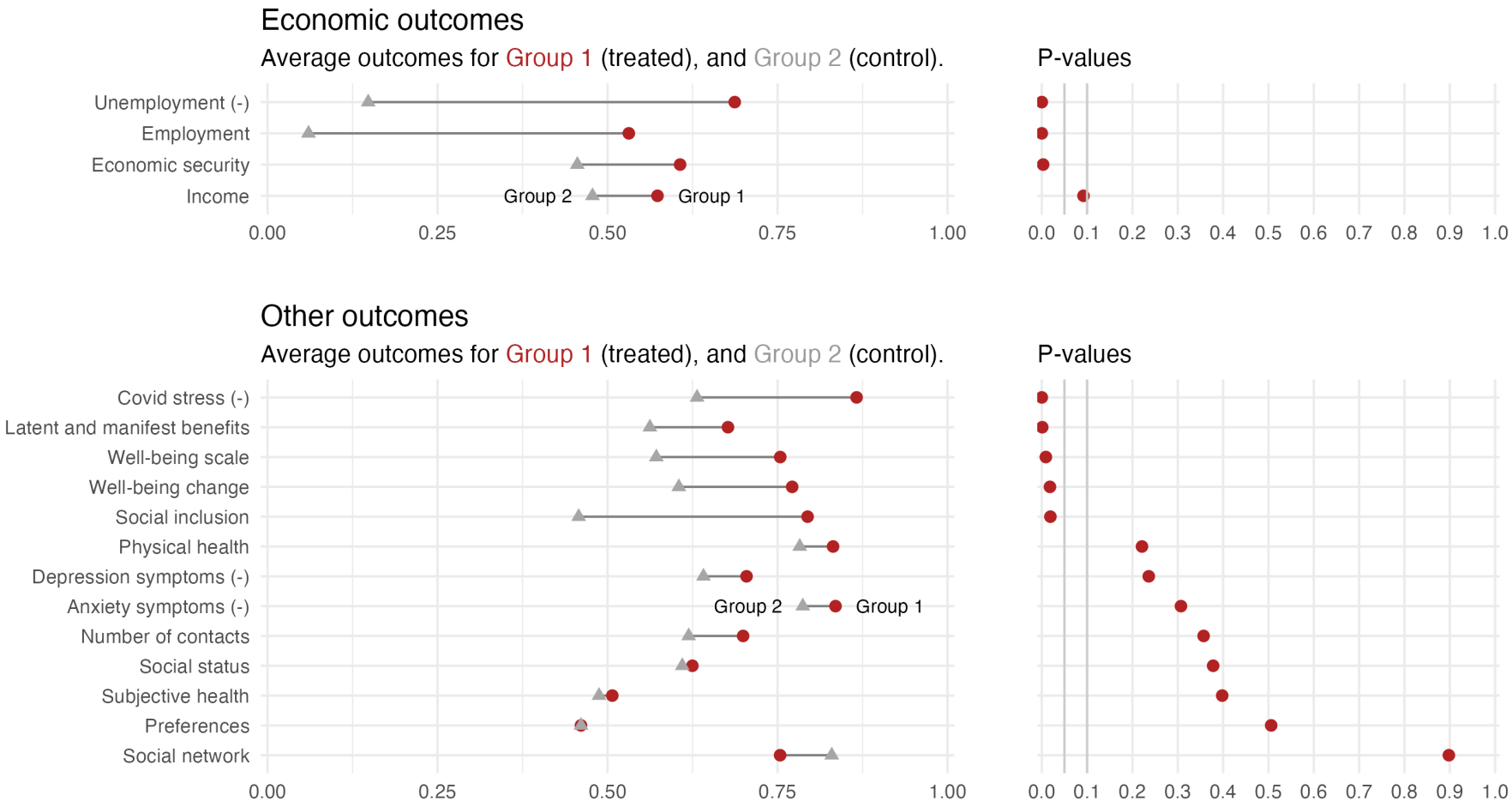
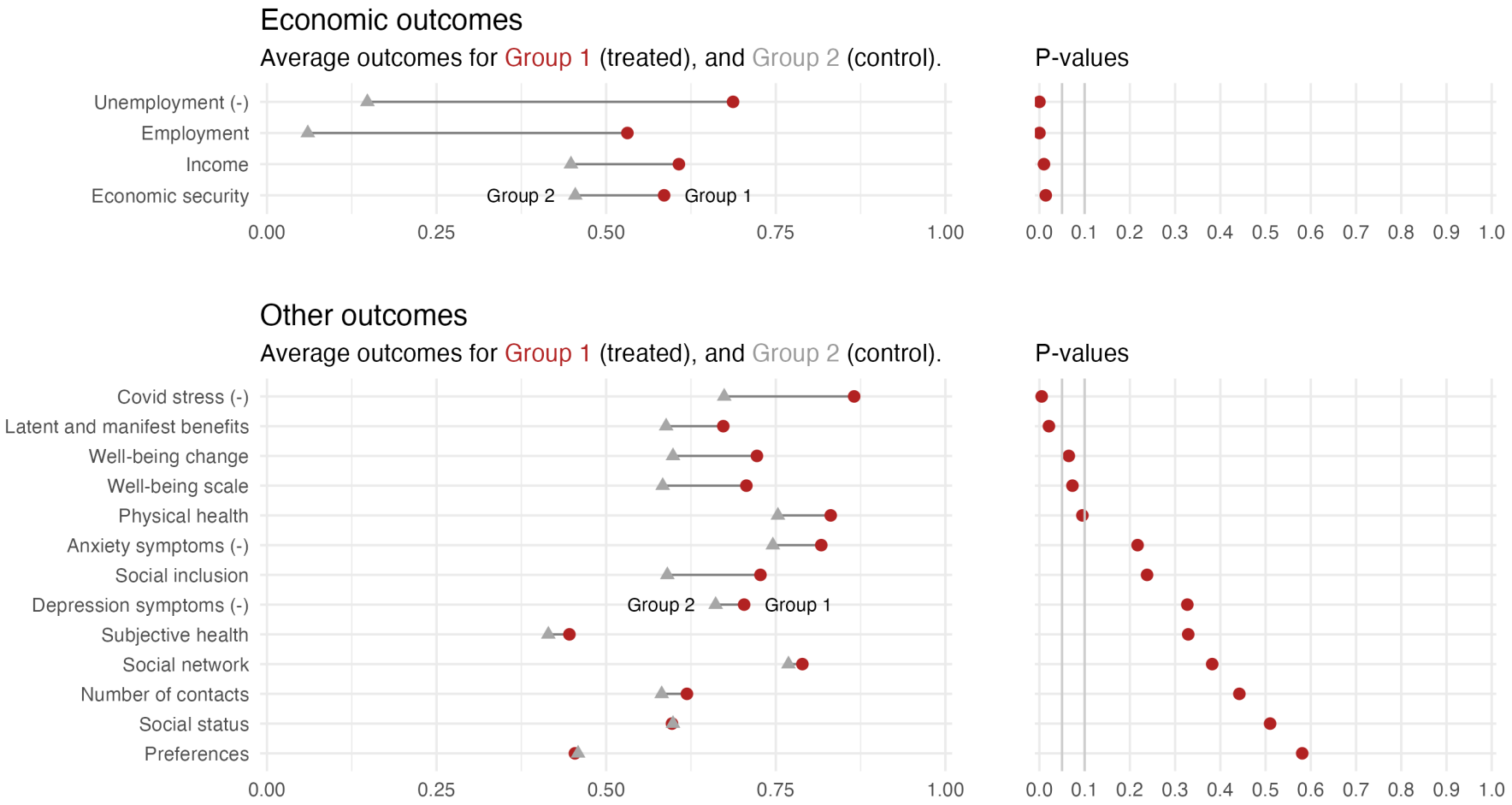


Figure A.7: Experimental estimates with no controls



B Survey questions

This section includes the questions used to survey participants in the treatment and control groups. The questions are structured by outcomes. First-level numbered bullet points correspond to the questions that constitute the aggregate index for each outcome reported. Each question was used with equal weights for the aggregation. Second-level alphabetically listed bullet points correspond to the answer categories provided in the survey. Some questions (on income and on social networks) are repeated, to clarify that they enter the construction of different outcome measures, as listed in Table A.7. The questionnaire for the survey was registered at https://www.socialscienceregistry.org/trials/6706.

Income security

Source of questions: US-SHED (of Governors of the Federal Reserve System, 2019), EU-SILC (Eurostat, 2019), and own.

1. Overall, which one of the following best describes how well you are managing financially these days:
   (a) Living comfortably
   (b) Doing okay
   (c) Just getting by
   (d) Finding it difficult to get by
2. Compared to 6 months ago before the start of MAGMA, would you say that you are better off, the same, or worse off financially?
3. How much is your monthly income?
   Subsequent question if no response: Can you try to guess in which category your monthly income falls approximately?
   (a) less than 600 €
   (b) 600 - 1,000 €
   (c) 1,000 - 1,400 €
   (d) 1,400 - 1,800 €
   (e) 1,800 - 2,200 €
   (f) 2,200 - 2,600 €
   (g) 2,600 € or more
4. Are you in arrears with a regular payment such as rent, phone bill, loan installment or the like?
5. Are you able to make an unexpected expense such as X for a repair?

Income

Source of questions: US-SHED (of Governors of the Federal Reserve System, 2019), EU-SILC (Eurostat, 2019), and own.

Table A.7: Variable definitions

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

1. How much is your monthly income?
   Subsequent question if no response: Can you try to guess in which category your monthly income falls approximately?
   (a) less than 600 €
   (b) 600 - 1,000 €
   (c) 1,000 - 1,400 €
   (d) 1,400 - 1,800 €
   (e) 1,800 - 2,200 €
   (f) 2,200 - 2,600 €
   (g) 2,600 € or more

Depression symptoms

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020).

Over the last 2 weeks, how much does the statement describe your feelings?

1. I feel I cannot shake off the blues, even with help from my family and my friends.
2. I feel sad.
3. I feel happy.
4. I feel life is not worth living.
5. I feel depressed.

Covid stress

Source of questions: Conway et al. (2020)

Please tell us whether the following statements apply to you:

1. Thinking about the coronavirus (COVID-19) makes me feel threatened.
2. I am afraid of the coronavirus (COVID-19).
3. I am stressed around other people because I worry I'll catch the coronavirus (COVID-19).
4. The Coronavirus (COVID-19) has impacted me negatively from a financial point of view.
5. I have lost job-related income due to the Coronavirus (COVID-19).
6. I have become depressed because of the Coronavirus (COVID-19).
7. The Coronavirus (COVID-19) outbreak has impacted my psychological health negatively.

Social inclusion

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020).

1. How many new people have you met in the past month? Please type the approximate number.
2. Which of the following statements best describes your current relationship status?
   (a) I am romantically involved on a steady basis. We live together.
   (b) I am romantically involved on a steady basis. We live separately.
   (c) I am involved in an on-again and off-again relationship.
   (d) I am not involved in a romantic relationship.

Preferences

Source of questions: Falk et al. (2018). Weber and Blais (2006). Mobasseri et al. (2022). Own.

Time preferences

1. Would you prefer to receive 100 € today, or 300 € in 1 month?
2. Would you prefer to receive 100 € today, or 300 € in 6 months?
3. Would you prefer to receive 100 € today, or 300 € in 12 months?
4. Suppose you have some money to do business, and you have a choice between 2 options. Which option would you choose?
   (a) A business that can give you a lot of profit every month, but there is a chance you could lose money.
   (b) A business with less profit every month, but you can't lose your money.
5. Imagine you have saved 10,000 € from working at a job. You receive the following offer from a good bank: If you invest with them there is a chance that you will double the money you invested immediately, or lose half of the money you invested. How much do you want to invest? You only have 10,000 €.

Personality traits

6. In general terms, most people can be trusted.
7. You are willing to give up something that is beneficial for you today in order to benefit more from it in the future.
8. When someone does me a favor I am willing to return it.
9. If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so.
10. I am willing to punish someone who treats me unfairly, even if there may be costs for me.
11. Imagine the following situation: Today you unexpectedly received 1,000 €. How much of this amount would you donate to a good cause?

12. Generally, I am willing to give to a good cause without expecting anything in return.

Risk preferences
We are interested in your risk-taking behavior. Please select how risky you find the respective behavior.

13. Admitting that your tastes are different from those of a friend.
14. Drinking heavily at a social function.
15. Disagreeing with an authority figure on a major issue.
16. Having an affair with a married man/woman.
17. Passing off somebody else's work as your own.
18. Betting a day's income on the outcome of a sporting event.
19. Engaging in unprotected sex.
20. Revealing a friend's secret to someone else.
21. Speaking your mind about an unpopular issue in a meeting at work.
22. Not returning a wallet you found that contains 200 €.

Latent and manifest benefits

Source of questions: Kovacs et al. (2017)

Please select whether you agree or disagree with the following statements:

Activity

1. There is usually not enough spare time in my day.
2. I often have nothing to do.

Social interaction

3. I usually have a lot of opportunities to mix with people.
4. I seldom meet new people.

Collective purpose

5. I rarely feel that I make a meaningful contribution to society.
6. I often feel a valuable part of society.

Time structure

7. My days are usually well organized.
8. I rarely catch up with the things I need to do.

Social recognition

9. I am usually important to my friends.
10. My friends rarely value my company.

Financial strain

11. My income usually allows me to do the things I want.
12. My income usually does not allow me to socialise as often as I like.

Physical health

Source of questions: PHQ-15 somatic symptom scale (Kroenke et al., 1998).

During the past month, how much have you been bothered by any of the following problems?

1. belly
2. back
3. limbs
4. menstruation (asked for women only)
5. sexual intercourse
6. head
7. chest
8. dizziness
9. passed out
10. heart
11. breath
12. intestine
13. digestion
14. sleep
15. energy

Anxiety symptoms

Source of questions: GAD-7 general anxiety disorder (Spitzer et al., 2006).

Over the last 2 weeks, how often have you been bothered by the following problems?

1. Feeling nervous, anxious or on edge.
2. Not being able to stop or control worrying.
3. Worrying too much about different things.
4. Trouble relaxing.

5. Being so restless that it is hard to sit still.
6. Becoming easily annoyed or irritable.
7. Feeling afraid as if something awful might happen.

Social network

Source of questions: Social Network Accuracy Test ("SNAT") from Mobasseri et al. (2022), and own.

1. From time to time, most people discuss work-related and job-search issues with other people. Looking back over the last 6 months, who are the people with whom you discussed work-related and job-search issues with? In the boxes below, please list the FIRST NAME and LAST NAME INITIAL of the people with whom you discuss important matters. E.g., Maria Maier would be recorded as "Maria M." Please list only one name per box. If two people on your list share the same first name and last initial, use numbers to distinguish them (e.g., "Maria M" and "Maria M2"). If you don't discuss important matters with anyone, just leave the fields blank.
2. Below is a list of the names you provided on the prior page. Please answer the questions below about each person you named. How frequently are you in contact with each person?
3. Please select whether you agree or disagree with the following statement. This person is close to you.
4. Please select whether you agree or disagree with the following statement. Compared to other people you know, this person is very valuable to you.
5. Which of the following best describes your relationship to each person?
   (a) Spouse/Significant Other
   (b) Other Family Member
   (c) Friend/Social Contact
   (d) Work/Professional Contact
   (e) Other
6. Please select whether you agree or disagree with the following statements. This contact is someone who looks up to me.

Well-being scale

Source of questions: WHO-5 Well-being Index (WHO, 1998; Topp et al., 2015).

The following statements relate to your well-being in the past two weeks. For each statement, please mark the number that you think best describes how you have felt over the past two weeks. In the last two weeks ...

1. I was happy and in a good mood.
2. I felt calm and relaxed.
3. I felt energetic and active.
4. I felt fresh and rested when I woke up.
5. My everyday life was full of things that interest me.

Well-being change

Source of questions: Own questionnaire.

1. Compared to 6 months ago before the start of MAGMA, would you say that you are doing better, the same, or worse?

Social status

Source of questions: US-SHED (of Governors of the Federal Reserve System, 2019), and own.

1. Imagine a ladder showing where people stand in society. At the top are the people who are the best off — those who have the most money, the most education, and the most respected jobs. At the bottom are the people who are the worst off — those who have the least money, the least education, and the least respected jobs or no job. Where would you place yourself on this ladder? *(The questionnaire includes an annotated image of a ladder).*
2. Over the past half year did your status in society...
   (a) improve a lot
   (b) improve
   (c) improve a little
   (d) remain as it was
   (e) worsen a little
   (f) worsen
   (g) worsen a lot
3. Thinking of the future, do you expect your status to...
   (a) improve a lot
   (b) improve
   (c) improve a little
   (d) remain as it was
   (e) worsen a little
   (f) worsen
   (g) worsen a lot

Number of contacts

Source of questions: Social Network Accuracy Test ("SNAT") from Mobasseri et al. (2022), and own.

1. From time to time, most people discuss work-related and job-search issues with other people. Looking back over the last 6 months, who are the people with whom you discussed work-related and job-search issues with? In the boxes below, please list the FIRST NAME and LAST NAME INITIAL of the people with whom you discuss important matters. E.g., Maria Maier would be recorded as "Maria M." Please list only one name per box. If two people on your list share the same first name and last initial, use numbers to distinguish them (e.g., "Maria M" and "Maria M2"). If you don't discuss important matters with anyone, just leave the fields blank.

Subjective health

Source of questions: Fragile Families Survey (Bendheim-Thoman Center for Research on Child Wellbeing and Center, 2020), and own.

1. Would you say your health generally is...
    (a) excellent
    (b) very good
    (c) good
    (d) fair
    (e) poor
2. Over the past 6 months, would you say your health generally has...
    (a) improved a lot
    (b) improved
    (c) improved a little
    (d) remained stable
    (e) worsened a little
    (f) worsened
    (g) worsened a lot

C Program implementation details

C.1 Jobs created

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

This section documents the type and number of jobs created by the Marienthal job guarantee scheme between its start in 2020 until November 2022 both in the market and non-market sectors. This includes jobs for individuals who joined the scheme after treatment was assigned. Jobs of eligible individuals who found a job outside of the program are not included in this section. Figure A.8 shows some of the program participants at work.

Jobs created in the non-market sector

- 13 Carpenters
- 7 Tailors
- 6 Gardeners
- 5 Renovation workers
- 3 Registrars
- 3 Cleaners
- 1 Driver
- 1 Assistant counselor

Jobs created in the market sector

- 6 Office clerks
- 2 Warehouse workers
- 2 Assistant electricians
- 1 Care home assistant
- 1 Technical sales assistant
- 1 Facility manager

- 1 Construction worker
- 1 Salesperson
- 1 Construction foreman
- 1 Taxi driver
- 1 Hospitality assistant
- 1 Carpenter
- 1 Marketing assistant
- 1 Municipal building yard worker
- 1 Farm worker
- 1 Nursery worker
- 1 Call centre agent
- 1 Lift technician
- 1 Assistant cook
- 1 Forklift driver
- 1 Accounting clerk
- 1 HR consultant

C.2 Participant views

Werner V., aged 60: "After more than 600 job applications over three years, my wish for employment proved hopeless. Too old, too expensive, over-qualified, without long-term prospects due to my age, with multiple university degrees seemingly over-qualified for service jobs... many obstacles seemed to exist. The job guarantee proved extremely valuable and useful for me. In cooperation with the municipality and the local museum, I am archiving and documenting the cultural, scientific and economic value of the historical site of Marienthal."

Mohamad A., aged 44: "I am from Syria and live here in the village with my family–my wife and my 4 children, some of whom are already at school. I recently had a job offer, the company wanted to hire me full time but due to the current Covid situation they changed their minds and offered only a marginal employment contract. By contrast, the job guarantee scheme provides an opportunity to work [full-time], which suits me because we can work every day and learn something new. I'd also like to use the time to improve my German language skills so that I can later catch up on my general qualification for university entrance and perhaps study at a university of applied sciences. I'm grateful for the help the job guarantee offers; it is important for me."

Johann G., aged 65: "I live in Gramatneusiedl and worked for 38 years at a company in the chemical industry that was located in Gramatneusiedl and closed down some years ago. I am now taking part in the job guarantee since 2020, which makes me feel comfortable. Under the scheme, I have worked in renovation and have been able to apply my skills in many ways. With the help of the job guarantee, I can start as a warehouse worker in a recycling company in October 2022."

C.3 Case studies

Public vegetable garden: The local mayor provided $250m^2$ of land which participants cultivate as a sustainable food garden. Herbs and vegetables can be picked free of charge and the garden is open year-round. The first harvest was in summer 2022.

Animal therapy: Two participants are employed with an association providing animal-assisted therapy for children with various conditions (e.g., autism, ADHD, disabilities, learning difficulties). By looking after the association's animals, house, and garden, they have enabled the centre to improve its services and care for more young people.

Funeral urns: During participant Michaela P.'s (paid) internship doing office work at a funeral parlour, her employer noticed her talent for painting. Her internship turned into permanent employment in spring 2022 and, in addition to office work, she now paints urns – a new business venture for the parlour. Before Michaela became unemployed, she worked in a canteen and never thought she would be able to include her hobby in her job.

C.4 Policy impact

MAGMA has received considerable attention. The program has served as the basis for a resolution by the Parliament" (2023) and 23 Million Euros funding for further job guarantee pilots provided by the European Commission. It has received considerable attention from international organizations (ILO, 2021; OECD, 2021, 2023; "UN Special Rapporteur", 2023) and news media; see for instance Romeo (2022); Henderson (2021); Horowitz (2020); ZDF (2022) among others. The latter were published in The New Yorker, Forbes, CNN, ZDF, respectively.

C.5 Parallel qualitative evaluation

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

Figure A.8: Program participants at work

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

C.6 Impact of the Covid-19 pandemic

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

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

same is true for the individuals surveyed in control municipalities.

Entrants into the job guarantee scheme at a later stage included those who became unemployed during the pandemic. These late entrants are not part of our experimental comparison, or the individual-level comparison across municipalities. They do figure in municipality level comparisons using the synthetic control approach, however.

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

C.7 Job guarantee versus unconditional income support

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

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

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

D From "Die Arbeitslosen von Marienthal" to our study

Ninety years ago, in 1930, a team of researchers (including Marie Jahoda, Paul Lazarsfeld, and Hans Zeisel) wrote the pathbreaking study "Die Arbeitslosen von Marienthal" (Jahoda et al., 1933). Three years ago, in 2020, a pilot of a guaranteed job program for the long-term unemployed was launched in the very same location, which we evaluate in the present paper ("Employing the unemployed of Marienthal," EUM).

In this note, we take the occasion to reflect on the methodological differences between

these studies. These two studies can be seen as examples of broader developments in social science methodology over the course of the 20th century. We would like to emphasize that this comparison is intended to be descriptive rather than taking a stance regarding the superiority of different methodological approaches.

The study of Jahoda et al. (1933), while pioneering in many ways, also reflected established approaches to empirical social science at the time. Similarly, our study EUM is fairly typical for policy evaluations in current empirical economics (and social science more generally). The methodological state of the art that we follow is reflected in standard graduate curricula in applied econometrics, and has been canonized by the economics Nobel prizes of 2019 ("for their experimental approach to alleviating global poverty") and 2021 ("for his empirical contributions to labour economics" and "for their methodological contributions to the analysis of causal relationships").

There are some commonalities between Jahoda et al. (1933) and EUM. Both are quantitative, empirical studies drawing on a variety of data sources, including self-collected surveys and administrative data.[1] Both are based on similar sample sizes (a few hundred) and geographic scope (Marienthal and Gramatneusiedl, and nearby communities).

Turning to differences between the two studies, there is first the type of question asked. Beyond its rich description, a primary contribution of Jahoda et al. (1933) is a **classification** of the unemployed of Marienthal into 4 types (ungebrochen / resigniert / verzweifelt / apathisch, which translate as unbroken / resigned / desperate / apathetic). By contrast, our focus is on the estimation of **causal effects** of a job guarantee, on both its beneficiaries and the wider community.

The focus on classification was a primary concern of 19th century empirical social science, from Adolphe Quetelet's "social physics" and its focus on types of "average man" through the "scientific" racism of the 19th century in biology and the humanities and its obsession with classifying humanity into distinct "races," to Max Weber's "ideal types." In an afterword to Jahoda et al. (1933), Hans Zeisel justifies the focus on comprehensive description and classification (or "sociography," as the authors call it) out of the need to understand a complicated and unstable capitalist society, for the purpose of rational policy, a need which he argues did not arise in pre-capitalist feudal times, where the classification of individuals was stable and known to everyone. An important role that Zeisel assigns to classification is to make qualitative data amenable to quantitative analysis.[2]

The focus in statistics on causal effects of interventions, on the other hand, traces back to the work of Neyman and Fisher in the 1920s, and has more recently first entered clinical trials in medicine, and has since the 1990s become dominant in empirical economics as well as other social sciences.

Closely related to this focus on classification versus causality is a distinction in the type of event studied. Jahoda et al. (1933) consider the consequences of a **historical macro event** (the Great Depression) – there is not even an attempt at finding a comparison group for their study sample of unemployed workers and their families. In EUM, by contrast, we focus on

[1] Jahoda et al. (1933) also has an important qualitative component.

[2] Classification of course still plays an important role in some social sciences as well as psychology today.

the **causal effect of a (micro) policy** intervention; much of the methodological effort goes into finding valid comparisons. The notion of causality is intimately related to the ideas of **interventions** and **comparison groups.**

Another related aspect is how these studies deal with **heterogeneity**. Jahoda et al. (1933) engage in an impressive and comprehensive effort to **fully capture** and describe the variability of circumstances and psychological responses of the unemployed of Marienthal. By contrast, no such comprehensive effort is made in EUM. Instead, the methodology of causal inference – pairwise matching, randomization, synthetic controls – is used to ensure that comparison groups for causal inference are the **same on average**.

This different approach to heterogeneity is reflected in another striking difference: In Jahoda et al. (1933), no attempt is made to **quantify statistical uncertainty** – there are no standard errors, confidence intervals, or p-values. The study contains a large number of statistical tables, but there is no sense in which these reported numbers (e.g., shares in the sample belonging to a particular category) are related to an underlying **population object** (e.g., shares in the population belonging to a particular category). There is no distinction between estimate and estimand; the reported numbers are what they are. By contrast, EUM follows modern standard practice in reporting standard errors, confidence intervals, and p-values, and additionally addresses the issue of multiple hypothesis testing. The implicit notion is that there are true causal effects (either in the sample or in a larger population), and that the reported estimates are noisy approximations of these effects.

Again related, a striking feature of Jahoda et al. (1933) is its **methodological open-endedness**, contrasting with the complete **pre-registration** of EUM. Jahoda et al. (1933) use a wide variety of data-sources and personal observations, and enter Marienthal without prespecified questions that they will ask. Instead, they distill abstractions and classifications from the rich empirical material they find. By contrast, recent empirical social science has been greatly impacted by its perceived replication crisis, attributed to selective reporting of findings by authors (p-hacking) and journals (publication bias); cf. Andrews and Kasy (2019). A key remedy that has been promoted in recent years, enshrined in journal policies, and followed by EUM, is the pre-registration of experimental designs and statistical analyses. Such pre-registration prevents selective reporting of findings by publicly tying researchers' hands. The aim is to make findings replicable and independent of researcher identity.

Let us conclude by emphasizing one more arc connecting the two studies over the course of a century. A key contribution of Jahoda et al. (1933) was that they documented the devastating impact of unemployment beyond its material consequences on income – in the form of psychological outlook, attitudes to the future, time structure, social cohesion, etc. This perspective was further developed by Marie Jahoda over the course of her career, and has been operationalized by sociologists of work in the form of survey instruments for the Latent And Manifest Benefits (LAMB) of work. In EUM, these survey instruments were included in our data collection. And, indeed, these are the dimensions where our experimental findings suggest the strongest impact of a job-guarantee on the well-being of beneficiaries, besides the direct economic impacts.

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