Employing the unemployed of Marienthal:
Evaluation of a guaranteed job program
(Working paper)

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Abstract

We evaluate a guaranteed job program that was piloted, starting in October 2020, in the municipality of Gramatneusiedl in Austria. This program provided individually tailored, voluntary jobs to all long-term unemployed residents. Our evaluation is based on three estimation approaches. The first approach uses pairwise matched randomization of participants into waves for program adoption. The second approach uses a pre-registered synthetic control at the municipality level. The third approach compares program participants to observationally similar individuals in control municipalities. These different approaches allow us to separate out direct effects of program participation, anticipation effects of future participation, and municipality-level equilibrium effects.

We find strong positive impacts of program participation on participants' economic (employment, income, security) and non-economic wellbeing (social status, time structure, social interactions, collective purpose). We do not find effects on physical health, or risk- and time-preferences. At the municipality level, we find a large reduction of long-term unemployment, and a slightly attenuated reduction of total unemployment. Comparing participants to similar individuals in control towns, we obtain estimates that are very close to the estimates from the experimental comparison. There is evidence of positive anticipation effects in terms of subjective wellbeing, status and social inclusion for future program participants, relative to ineligible control-town individuals.

1   Introduction

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

The MAGMA job guarantee program   The MAGMA job guarantee[1] is a pilot program launched in the municipality of Gramatneusiedl by the Public Employment Service (Arbeitsmarktservice, AMS) of Lower Austria in October 2020, and is scheduled to last until 2024. This program provides a guaranteed job, complemented by targeted counseling, to all residents of this municipality who were long-term unemployed (12 months or more) or at risk of long-term unemployment (9 months or more). Participation in the program is voluntary, but no candidate who was offered a job has declined the opportunity. A small number of eligible individuals could not be offered employment for reasons including illness, a prison sentence, or because they found regular employment before the start of the program.

The guaranteed job was preceded by individually tailored preparatory training of about 8 weeks, which could include individual and group counseling, skills development, and assistance with applications for health-related benefits. 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 collective-bargaining minimum wage. Jobs were created to fit the individual needs and constraints of participants, and to provide meaningful activity.

The MAGMA program differs from typical active labor market policies. First, because the intervention is quite big and long-lasting. Second, because the objective is different from more conventional active labor market policies (Card et al., 2010), which aim at re-integration of participants into the regular labor market. While participants of the MAGMA program are certainly encouraged to take up employment in the regular labor market, and such employment is subsidized by the program, this is not a likely outcome for many participants. Instead, the stated policy goal of the MAGMA program is to directly eradicate long-term unemployment in the municipality, and thereby to improve participants' 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.

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[1]MAGMA is short for "Modellprojekt Arbeitsplatzgarantie Marienthal," which translates as "model project job guarantee Marienthal." Marienthal is one part of the municipality of Gramatneusiedl.
MAGMA has received considerable attention from international organizations and media; see for instance OECD (2021); ILO (2021); ZDF (2022); ARTE (2021); Henderson (2021); Pausackl (2021); Horowitz (2020); Bendix (2020); Stone (2020).

Evaluation strategy   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 to one of two groups, where the second group starts the program 4 months after the first one. This allows us to estimate the short-term effects of the program, by comparing participants across the two groups, around 3-4 months after the start of employment for the first group.

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

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

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

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. In subsection 3.2 we formally discuss the identification of anticipation and equilibrium effects using the various contrasts in our study design.

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 effects, which combine both direct and indirect (spillover) effects.

A final additional benefit of the comparison to individuals in control towns is that this allows us to estimate the longer-term effects of program participation. While all individuals in

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[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.

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 wellbeing (income, economic security, and employment). 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 wellbeing 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 status. 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), who draws on Jahoda's work, which we discuss below. Such measures have received less attention in labor economics thus far, with notable exceptions such as Clark (2003, 2006).

Third, we estimate the effect of program participation on a number of measures where no short-term movements would be expected, including physical health and economic preferences (time and risk preferences, reciprocity, altruism, trust). As anticipated, we find precisely estimated zero effects on these outcomes. 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. Relative to the reduction of long-term unemployment, the effect on total unemployment is somewhat attenuated. This is due to a small rise in short-term unemployment, relative to control municipalities.

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

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

The historical arc from "Die Arbeitslosen von Marienthal" (1933) to MAGMA The location chosen for the job guarantee pilot is no coincidence. 90 years prior to this experiment, Marienthal was the location of a pathbreaking study on the impact of long-term mass unemployment (Jahoda et al. 2017, "Die Arbeitslosen von Marienthal," originally published in 1933). At the time, Marienthal was a factory town dominated by a single factory. When this factory

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shut down in the Great Depression, most residents lost their employment, with devastating consequences. Jahoda et al. (2017), in a large multi-method study, documented the impact of this situation. This study proved to be of lasting influence on the sociology and social psychology of work.

90 years later, the MAGMA experiment provides a mirror image of the original situation, by providing employment to all the long-term unemployed residents of Marienthal and of the municipality 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 C we offer some reflections on the contrast between the original Marienthal study and the present paper, taking the occasion to discuss 90 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 Card et al. (2010, 2018), and the earlier reviews Heckman et al. (1999); Kluve (2010), as well as Crépon and van den Berg (2016). The existing evaluations of ALMPs in German-speaking countries are mostly observational (recent exceptions are Altmann et al. 2018; van den Berg et al. 2021; Böheim et al. 2022); by contrast, there are numerous experimental studies from the US, e.g. Card and Hyslop (2005); Schochet et al. (2008); Gelber et al. (2016), and France, e.g. Crépon et al. (2013); Behaghel et al. (2014). This literature also includes some recent evaluations of public employment schemes for India (Khera, 2011; Muralidharan et al., 2017; 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).

A common conclusion of evaluations of ALMPs appears to be that job search programs are somewhat effective in improving participants' future employment prospects, whereas public employment programs are not. Two points are worth emphasizing in this context. First, most of this literature considers different outcomes and policy objectives than we do, focusing in particular on (market) employment, in German-speaking countries, and (market) earnings, in English-speaking countries. By contrast, we are interested in the impact on the community and on participant welfare, without an expectation that participants will enter market employment. Second, much of this literature focuses on individual-level effects, neglecting spillovers; important exceptions are Crépon et al. (2013), who study the negative displacement effect of job counseling using a large-scale clustered RCT 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.

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, cf. Avendano and Berkman (2014), though the causal link between the two is contested. Similarly, there is a strong association between employment and subjective wellbeing, cf. Clark and Oswald (1994); Korpi (1997); Clark (2003, 2006); Young (2012); see also Haushofer and Fehr (2014). In economic theory, Basu et al. (2009) discuss the implications of an employment guarantee scheme on efficiency and social welfare. The negative psychological consequences of unemployment have also been studied in a much older psychological literature; Eisenberg and Lazarsfeld (1938), for instance, review over 100 such studies conducted during the Great Depression. A general conclusion of this older literature was that

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unemployment leads to loss of purpose, confidence, and time structure, and to apathy, rather than political radicalization. (As an aside, Lazarsfeld, one of the authors of this review, was a co-author of the original Marienthal study, and later became president of the American Sociological Association.)

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); 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. Appendix A reports views from program participants, and describes some of the jobs that were created in greater detail. Appendix B presents additional details on our evaluation strategies, additional empirical findings, and robustness checks. Appendix C contrasts (Jahoda et al., 2017) and our study to discuss changes in the methodology of empirical social science over the last 90 years.

The Online Appendix 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. The Online Appendix also provides a detailed list of all the jobs that were created, in both the market and non-market sector, and shows photos of participants at work.

2 Background and program details

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

All residents who were "at risk of long-term unemployment" (unemployed for 9 to 12 months) or "long-term unemployed" (unemployed for 12 months or more) were eligible to participate. The experimental sample includes all residents unemployed for more than 9 months in September 2020. Residents who reached the eligibility threshold later were eligible to participate in the program, but are not part of our experimental comparison. The initial duration for the project was set until 2024 and budgeted with EUR 7.4 Million. The *AMS* calculated that the annual cost of the intervention is EUR 29,841 per participant.

Preparatory training The program was implemented by the private service-provider *it.works*, which specializes in implementing active labour market programs for the *AMS*. *it.works* provided preparatory training for participants, and continued counseling and training after participants had taken up employment. The preparatory training phase was scheduled for a maximum of eight 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 wages for the first 3 months, and 66% of wages 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 jobseekers by the *AMS* if they did so repeatedly. This provided an incentive to continue to employ these subsidized workers.

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

The social enterprise implemented projects at the municipal and regional level. This involved activities such as childcare, gardening, renovation, and carpentry, depending on orders acquired by the enterprise. In addition, participants were supported to develop and propose their own ideas for projects of the social enterprise, based on their expertise and local knowledge of community needs. Examples of projects proposed by participants included a workshop to renovate furniture, maintenance of public gardens, support for elderly residents in their day-to-day activities, planning and construction of a bike trail, and refurbishment of the local museum. Appendix A describes some of the jobs that were created in greater detail, and reports views

from some of the participants in the program. Figure 1 in the Online Appendix shows photos of program participants at work, in carpentry, bee keeping, and tailoring. The Online Appendix also provides a detailed list of all the jobs that were created, in both the market and non-market sector.

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

Voluntary participation   Work conditionality was eased for this pilot program. Under current law (*Arbeitslosenversicherungsgesetz AlVG §9*), people in unemployment are assigned to labour market programs by the *AMS*. They have the obligation to participate and they have to accept any employment offer that conforms to their skill-set.

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

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

Timeline for the intervention   The program was rolled out in two waves, and launched in October 2020. At that time the tailored curriculum and coaching started for the first group of 31 participants. In December 2020, this first group of participants were scheduled to start their employment.

In February 2021, the tailored curriculum and coaching started for the second group of 31 participants. We conducted our first round of surveys just after the start of training for this second group. In April 2021, the participants in this second group were scheduled to start their employment. The program was set to continue for (at least) 3 years, up to March 2024.

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

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

Due to the pandemic, labor market conditions worsened in Lower Austria, including Gramatneusiedl. The trajectory of economic conditions in Gramatneusiedl during the pandemic was similar to that of control municipalities. All individuals included in our treatment and control

groups, for the experimental approach, had become unemployed before the pandemic, but their opportunities to find employment might have been impacted by the pandemic. The same is true for the individuals surveyed in control municipalities.

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

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

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, including longer-term follow ups.

Based on their interviews, they classify program participants into three groups or "ideal-types." Group A consists of long-term unemployed participants with underlying health conditions or discontinuous employment trajectories, who had given up the hope to find stable employment outside the program before they participated. Members of Group A are grateful for the opportunity to participate. Group B is eager to find re-employment outside of the program and therefore focused on enhancing their skills. By contrast, Group C had already given up any hope to find re-employment as a consequence of a negative shock in their life, and views the guaranteed job as a form of individual fulfillment before retirement.

Moreover, their study identifies the eight 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.

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).[4] 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 within the last 9 months. Only if a formerly unemployed person returns to unemployment from sick leave or employment that lasted longer than 62 days, the count of the unemployment spell duration starts again from zero.

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 1. The first set of individual-level outcomes are based on administrative data-sources. These include employment status and duration of unemployment, from the "AMDB Erwerbskarrieremonitoring".

The second set of individual-level outcomes are based on surveys that we conducted in February 2021 and in February 2022. We collected information on a rich set of economic outcomes (in particular income and economic security) as well as non-economic outcomes. For non-economic outcomes, we construct a range of indices, on the "latent and manifest benefits" of work, measures of mental and physical health, subjective wellbeing, social inclusion and status, etc. Our construction of these indices follows established practice in sociology and survey design.[5]

To enable a compact presentation of our results in Section 4, we normalize all individual-level outcomes such that higher values correspond to "better" outcomes (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. again Table 1.

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

3.1 Three identification approaches

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

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

[5]For health-related outcomes, we used the PHQ-15, GAD-7, WHO-5 and measures based on Edin and Waldfogel (2020); Conway et al. (2020). For social outcomes we draw on Edin and Waldfogel (2020); Mobasseri et al. (2022); Kovacs et al. (2017, 2019); Knight et al. (2020), and for preferences on Falk et al. (2018); Weber and Blais (2006); Mobasseri et al. (2022).

Table 1: Variable definitions

Variable	Definition	Source
Individual level, economic		
Unemployment (-)	Share of days not employed since Oct 1, 2020.	Admin
Employment	Share of days employed since Oct 1, 2020.	Admin
Income	Current monthly income, divided by 2000.	Survey
Economic security	Normalized index of five item scales of income, financial situation and material deprivation.	Survey
Individual level, other	**Normalized index of:**	
Depression symptoms (-)	A five item depression scale.	Survey
Covid stress (-)	A seven item scale on the impact of the Covid-19 pandemic on stress, mental health, employment and income.	Survey
Social inclusion	Two item social inclusion scale, including the number of new people met in the past month, divided by 10, and the current relationship status.	Survey
Preferences	Twenty-two items for economic preferences, including time preferences, risk preferences, reciprocity, altruism and trust.	Survey
Latent and manifest benefits	A twelve item scale on the latent and manifest benefits of employment that include activity, social interaction, collective purpose, time structure, status, 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
Wellbeing scale	A five item mental wellbeing scale.	Survey
Wellbeing change	Subjective wellbeing 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 (≤ 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 in working age as a share of working age pop.	Admin

Second, we estimate municipality-level treatment effects by comparing Gramatneusiedl to a synthetic control. This comparison includes effects on residents who were not eligible to participate in the program because they were not long-term unemployed. This synthetic control 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.

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

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

We calculated pairwise distances between all 62 program participants using the Mahalanobis distance, based on these covariates. The Mahalanobis distance of two covariate vectors x_1 and x_2 that are realizations of a random vector X is given by $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot Var(X)^{-1} \cdot (x_1 - x_2)}$. We matched participants into pairs such that the total sum of distances between the members of each matched pair is minimized. We then randomly assigned one of the participants in each pair to Group 1, starting the program earlier, while the other participant was assigned to Group 2, starting the program later. Summarizing the resulting assignment, Table 2 shows the differences in covariate means between groups, and the corresponding (naive) t-statistics. Confirming that our procedure worked as intended, all available covariates are balanced across groups.

Table 2: Covariate balance for our matched pair design

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

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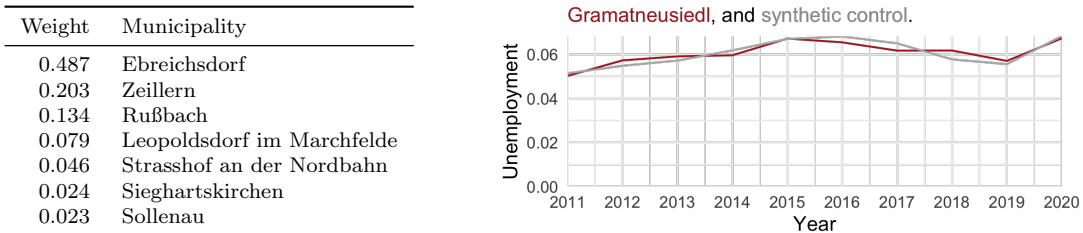
Approach 2: Synthetic control Our second approach is based on the construction of a synthetic control municipality for Gramatneusiedl. For this construction we draw on data from various sources, including (i) the *AMS* internal registry for administrative data on the unemployed, (ii) the "occupational-career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

We construct a synthetic control municipality in two steps. In the first step, we select a subsample of 5% of the available municipalities in the state of Lower Austria (25 out of 505 municipalities) which are most similar (closest) 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 5 in Appendix B. The averages of these covariates for both Gramatneusiedl and the (synthetic) control municipalities are shown in Table 6 in Appendix B. Most of our covariates are based on observations for the year 2019 (as measured in December). In addition to these, we also include some covariates measured in July of 2020, after the onset of the COVID pandemic, to control for possibly heterogeneous impacts of this pandemic across municipalities. The averages of these covariates are shown in the bottom panel of Table 6.

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

The resulting weights are shown in the table at the left of Figure 1, which lists all municipalities with non-negligible weights. The location of these municipalities is shown in Figure 8 in Appendix B. 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, are shown on the right of Figure 1. Table 6 in Appendix B similarly compares the covariate values for Gramatneusiedl with those for the synthetic control as well as those for each of the municipalities with positive synthetic control weights.

Figure 1: Synthetic control weights, and unemployment trajectory

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

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

We conducted two surveys in the control municipalities, in February 2021 and in February 2022. We furthermore have administrative data for all these individuals, including the same set of baseline covariates that was used for the construction of matched pairs in our experimental design. We obtain a sample of 71 individuals who answered all survey questions and satisfy the inclusion criteria. Of these 71 individuals, the majority are from Ebreichsdorf (62 individuals); the remainder are from Rußbach and Zeillern.

Our third approach compares the outcomes of these individuals in the control towns to the outcomes of program participants (Group 1 in February 2021, and both Group 1 and 2 in February 2022), as well as future program participants (Group 2 in February 2021) in Gramatneusiedl.

To verify that the sample of control town individuals is similar to the set of participants, we again compare their baseline covariates. Table 7 in Appendix B shows that there are no significant differences in baseline covariate means across the towns considered, with the exception of benefit levels, which are slightly higher among control individuals, and (marginally) age, which is similarly 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 effects and anticipation effects

Formal framework In order to discuss the interpretation of our estimates in terms of spillover effects and anticipation effects, it is useful to introduce some formalism, where we loosely follow the approach of Graham et al. (2010). Let Y_i denote an outcome for individual i, such as employment status or income. Let D_i denote current eligibility for the job guarantee, and D_i^{+1} future eligibility, at some fixed time horizon. Let $\bar{D}$ 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

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

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 $\bar{D}$ captures equilibrium (spillover) effects. Let L_i be an indicator for unemployment longer than 9 months as of September 2020, which determines eligibility for participation in our experiment, and let expectations average over the distribution of unobserved heterogeneity 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 3 provides a mapping from these averages to the structural notation. Correspondingly, Table 4 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 prin-

Table 3: 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	$E[g(0, 0, 0, \epsilon_i)]$

ciple, everything should be subscripted by time t.

Let us interpret these identified objects, as listed in Table 4. The experimental comparison of Group 1 to Group 2, in February 2021, identifies an **average direct effect on the treated**, where both spillover effects and anticipation effects are held constant across the two groups. The comparison of both groups, after April 21, to control town individuals identifies the **average total effect on the treated**, which incorporates direct effects, anticipation effects, and spillover effects.

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

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

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

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

3.3 Inference

Individual-level randomization inference To perform inference for 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

Table 4: Identified effects and roadmap

Contrast	Identified effect	Interpretation	Figures
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 2, Figure 3 Figure 10 (conf. intervals)
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 6, Figure 7 Figure 11 (conf. intervals)
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 6, Figure 7 Figure 12 (conf. intervals)
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 4, Figure 5
Gramatneusiedl vs. synth (total unemp)	$E[g(L_i, L_i, 1, \epsilon_i) - g(0,0,0,\epsilon_i)]$	Average total effect	Figure 4, Figure 5

the difference in means between groups. Repeating this process many times, we calculate the share of re-assignments for which the difference in means is bigger than the realized value of the difference in means. This share is the p-value for the null hypothesis of no effects.

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

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

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

We achieved lower response rates in the control towns, with 34% in 2021 and 30% in 2022. The difference in response rates is likely due to the fact that program participants in Gramatneusiedl were reminded to participate in the online survey by it.works and their job counsellor, while participants in the control towns were only reminded by the call center of the public employment service. We adjust for baseline covariates (their means are reported in Table 7, as discussed above) when comparing individual outcomes across towns, to mitigate the impact of possibly selective non-response.

4 Findings

We are now ready to discuss our empirical findings. We will consider a large number of outcomes and contrasts, and will present our findings in graphical form.[7] Our headline findings are summarized by Figures 2 through 7 in this section, as well as Figures 10 through 12 in Appendix B. Individual-level outcomes and outcome indices in these figures are normalized as follows: (i) They have a potential range from 0 to 1, and (ii) higher values represent "better" outcomes (e.g., lower unemployment, higher income, lower anxiety, etc.); recall that variables where the sign is flipped are marked by *(-)* in all our figures. Additional figures with results for further outcomes, alternative identification approaches, confidence intervals, and robustness checks can be found in Appendix B. Table 4 provides a roadmap through the findings presented in this section and in the appendix.

4.1 Experimental comparison

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

Figure 2 and Figure 3 show estimates for this experimental comparison. The left panels in either figure shows average outcomes for the treatment and control group, adjusting for covariates. The right panels shows p-values for the null of a 0 or negative 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 matching.

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

The estimates in Figure 2 and Figure 3 control linearly for baseline covariates, to adjust for potential non-random attrition in the survey. Figure 13 and Figure 14 in Appendix B display analogous figures 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 10 in Appendix B furthermore shows confidence intervals for treatment effects, based on robust standard errors for the regressions with linear controls.

Findings For economic outcomes (shown in the top panels of Figure 2), 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.

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

[8]Recall the normalization of these outcome variables from Table 1: Employment and unemployment are defined as the share of days since the program started, whereas the number of spells is divided by 6, and the monthly income is divided by 2000.

Participants who accept a guaranteed job increase their income. While the control group, Group 2, receives unemployment benefits, the treatment group, Group 1, enters jobs that are remunerated according to the floor set by collective bargaining in Austria, for the respective occupation and experience categories. Correspondingly, as shown by our estimates, program participation results in both increased income and economic security.

Turning to non-economic outcomes (bottom panels of Figure 2), we see a more heterogeneous picture. For some outcomes, in particular those related to social status, subjective health and mental health, social network and number of contacts, and preferences, we do not find a significant effect. Disaggregating the preference index into its components, in Figure 3, 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 wellbeing and its change over time, and in particular on the index measuring the "latent and manifest benefits" of work. Disaggregating the latter again, Figure 3 shows significant effects of participation on several components of this index, including activity, status, 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 arc from the original Marienthal study (Jahoda et al., 2017) to our evaluation. 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 status), 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.

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[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 2: Experimental estimates with linear controls
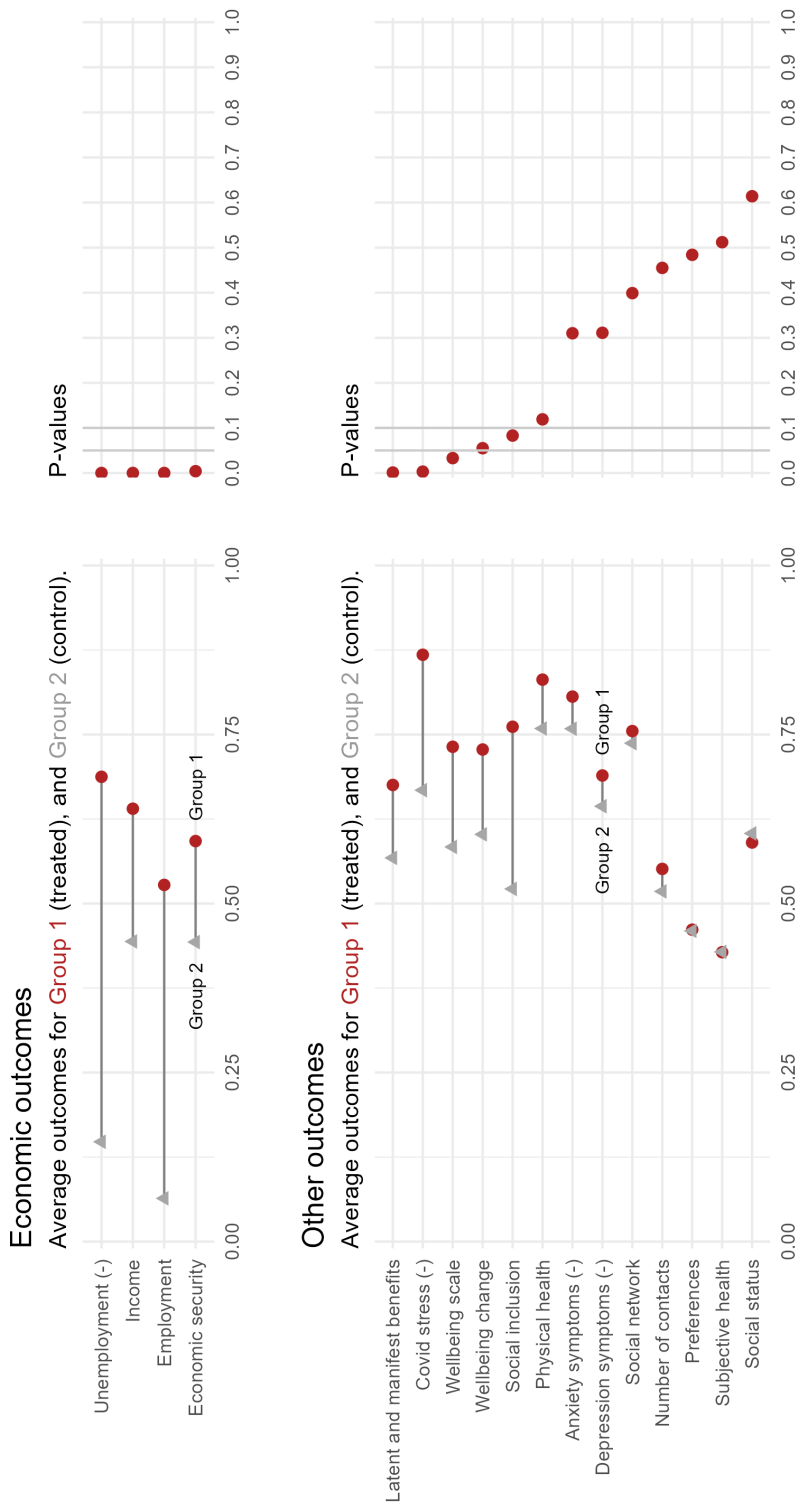
Notes: The left hand figures show average outcomes for the treated and control group, adjusting for baseline covariates. The outcome variables are defined in Table 1. Higher values imply better outcomes. Outcomes are scaled to range from 0 to 1. Income is monthly income divided by 2000, and unemployment is share of days *not* unemployed since Oct 1, 2020.

The right hand figures show p-values for tests of the null of a zero or negative effects of treatment. Small values imply positive effects of treatment. These p-values are based on 1000 simulation draws.

These estimates are also tabulated in Table 8.

Figure 3: Experimental estimates with linear controls, disaggregated outcomes

Average outcomes for Group 1 (treated), and Group 2 (control).

P-values
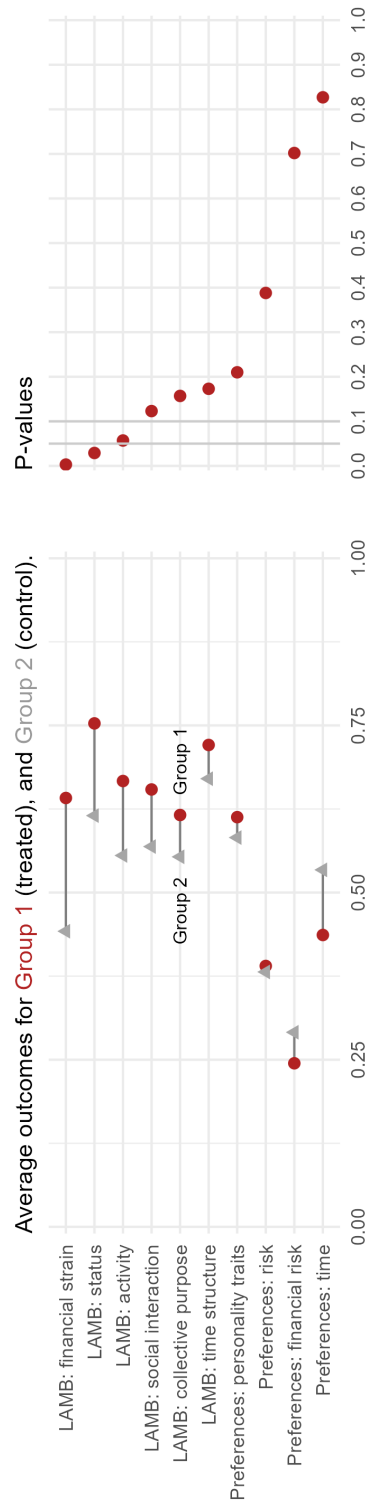
4.2 Synthetic control municipalities

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 4 and Figure 5. The top row of these figures plots the realized trajectory for Gramatneusiedl against the realized trajectory for the synthetic control. The plots show outcomes for both the pre-period and since the start of the program.

Remarkably, 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 4 and Figure 5 plots the gap between Gramatneusiedl and the synthetic control, and the corresponding gap for 25 permutations.[10] This permutation approach provides a formal analog to randomization inference. For each of the permutations, we consider another municipality as fictitiously treated, construct a synthetic control for this municipality, and plot the corresponding outcome gap. Extreme gaps for Gramatneusiedl, relative to these permutations, indicate program effects that are arguably not just driven by random fluctuations. Correspondingly, the last row of these figures plots the rank of Gramatneusiedl among the permutations.

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

Consider next the impact of the program on total unemployment, which is the sum of long-term and short-term unemployment. This total impact is negative. The synthetic control estimate suggests a reduction of the unemployment rate by about 1 percentage point, from 5% to 4%. Correspondingly, Gramatneusiedl is at the 25th percentile in terms of the relative reduction of unemployment, compared to the permutation municipalities. While slightly attenuated relative to the impact on long-term unemployment, 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.

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 5. The left column of this figure shows effects on employment, the right shows effects on "inactivity" (i.e., the share out of the labor force). As can be seen from this figure, the increase of employment in Gramatneusiedl, relative to the synthetic control, was even bigger than the reduction of unemployment.[11] Put

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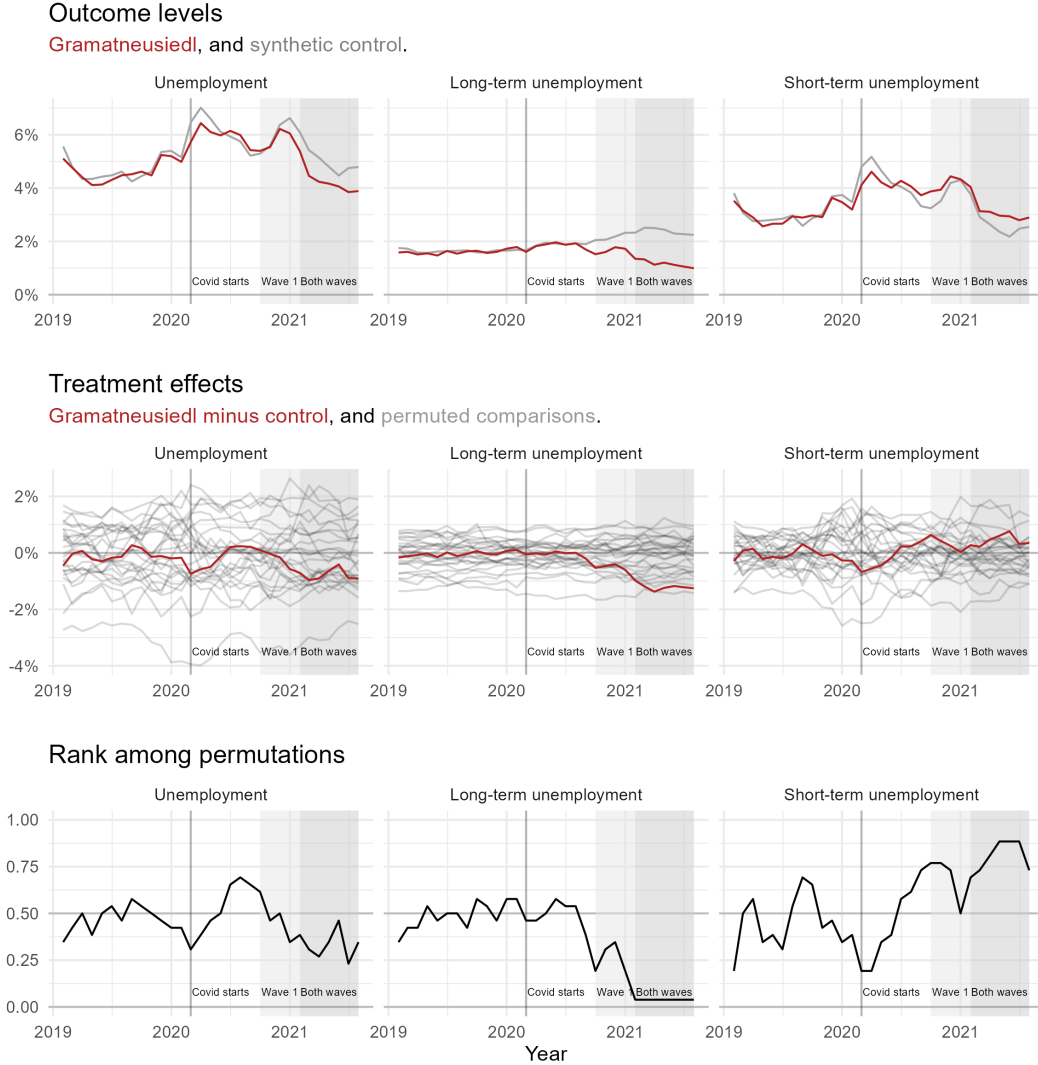[10]Figure 9 in the appendix provides an analogous figure for the 10 years prior to the program, where unemployment gaps are close to 0 mechanically, by construction of the synthetic controls.

[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.

differently, rather than inducing the unemployed to transition out of the labor force altogether, the program might have had the opposite effect.
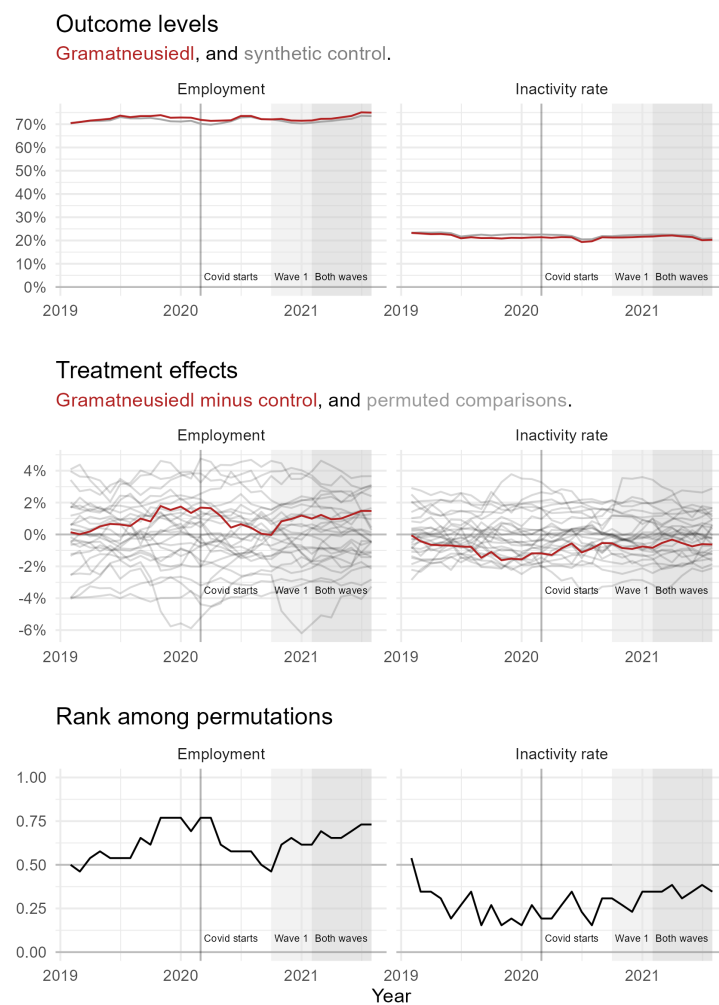
The gap between our estimated effects on long-term and total unemployment is the effect on short-term unemployment. There are some fluctuations over time, but it appears that Gramatneusiedl experienced a slight increase of short-term unemployment relative to the synthetic control – not because short-term unemployment increased in Gramatneusiedl, but because it decreased slightly more in the control towns. This relative increase puts it at the 75th percentile among permutation municipalities. The estimated relative increase in short-term unemployment suggests the possibility of some negative spillovers of the job guarantee on the short-term unemployed, who are not eligible to participate.

Figure 4: Synthetic control estimates of the program effect on unemployment

Outcome levels
Gramatneusiedl, and synthetic control.

Treatment effects
Gramatneusiedl minus control, and permuted comparisons.

Rank among permutations

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

Figure 5: Synthetic control estimates of the program effect on employment and inactivity

Outcome levels
Gramatneusiedl, and synthetic control.

Employment	Inactivity rate

Treatment effects
Gramatneusiedl minus control, and permuted comparisons.

Employment	Inactivity rate

Rank among permutations

Employment	Inactivity rate

Year

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 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 6, for economic outcomes, and Figure 7, for other outcomes. In both figures, we show outcomes for 2021 at the top, where we separate individuals in Group 1, Group 2, and the control towns, and outcomes for 2022, where we compare all eligible individuals in Gramatneusiedl (Group 1 and 2), to individuals in the control towns.
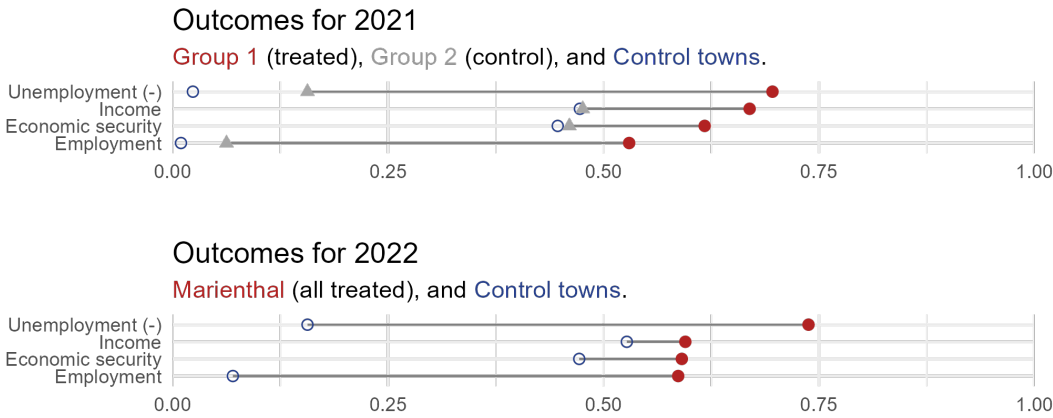
Figure 11 and Figure 12 show corresponding confidence intervals. Figure 11 contrasts Group 2 to control town individuals in 2021, thus providing an estimate of the average anticipation effect on the treated. Figure 12 contrasts both groups to control town individuals in 2022, thus providing an estimate of the average total effect on the treated.

Findings For income and economic security, as well as employment and unemployment, 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 our experimental comparison, in particular the "latent and manifest benefits" of work, and COVID stress. Similarly, for the preference index and for subjective health, no effects are found in either comparison.

These findings again corroborate our identification approaches (which rely on alternative identifying assumptions), and increase the confidence in our findings. Furthermore, these effects on income and economic security, latent and manifest benefits, and COVID stress persist into 2022. These are thus not just short term effects, but are effects maintained over the course of the program.
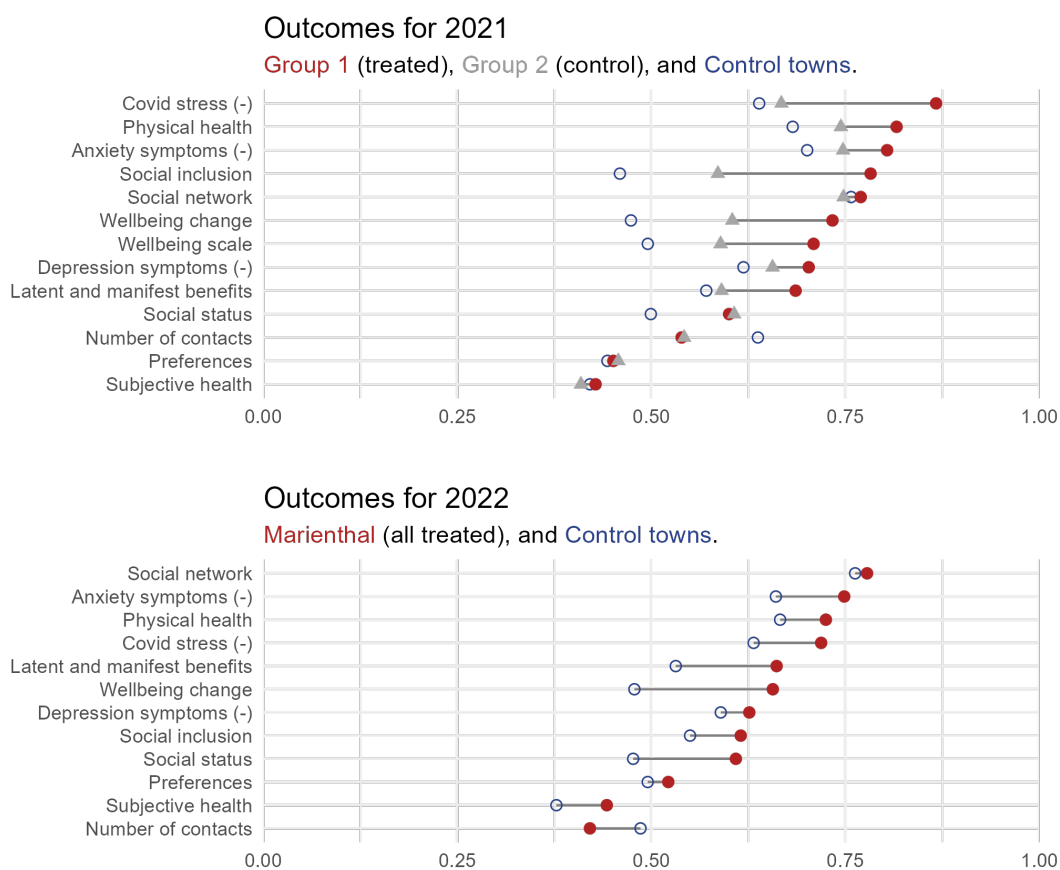
For social status, (subjective) wellbeing and social inclusion, the comparison to control towns yields even stronger effects in 2021 than the experimental comparison, suggesting anticipation effects.

Figure 6: Control town comparisons with linear controls, economic outcomes

Outcomes for 2021

Group 1 (treated), Group 2 (control), and Control towns.

Outcomes for 2022

Marienthal (all treated), and Control towns.

Notes: These estimates are also tabulated in Table 9.

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Figure 7: Control town comparisons with linear controls, other outcomes

Outcomes for 2021
Group 1 (treated), Group 2 (control), and Control towns.

Outcomes for 2022
Marienthal (all treated), and Control towns.

Notes: These estimates are also tabulated in Table 10.

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A Participant views and case studies summary

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 Corona 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 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."

Case studies (summary)

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.

B   Additional tables and figures

B.1   Synthetic control: Further details

Table 5: 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 6.

Figure 8: Location of municipalities included in the synthetic control
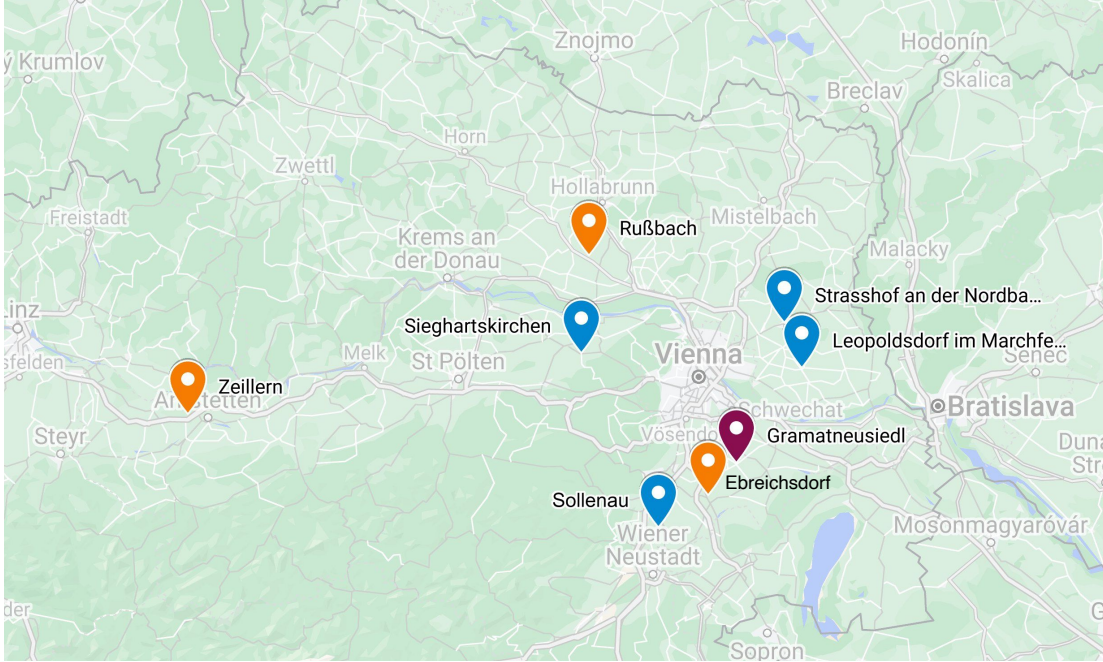
Notes: Gramatneusiedl, the treated municipality, is marked in red. The 3 municipalities with the largest weights in the synthetic control are marked in orange. Municipalities with smaller weights are marked in blue.

Figure 9: Unemployment gap and permutation inference.
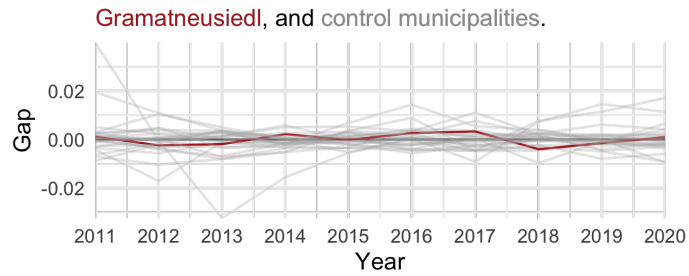
Notes: This figure shows the unemployment gap between Gramatneusiedl and its synthetic control (red), and between each of the 25 potential control municipalities and *their* synthetic control (grey). This figure parallels the second row of Figure 4, for the 10 years before the MAGMA program.

Table 6: 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 age unemp	Mean wage	Low edu/unemp
Gramatneusiedl	0.642	0.511	0.242	0.257	42.694	3416	0.530
Synthetic control	0.644	0.503	0.181	0.235	43.422	3293	0.452
Zeillern	0.702	0.509	0.053	0.256	40.462	3168	0.346
Ebreichsdorf	0.620	0.498	0.234	0.235	44.344	3379	0.465
Leopoldsdorf im Marchfelde	0.619	0.498	0.260	0.216	43.627	3294	0.513
Strasshof an der Nordbahn	0.600	0.496	0.276	0.257	42.364	3393	0.465
Rußbach	0.676	0.513	0.088	0.224	45.500	3137	0.525
Sieghartskirchen	0.641	0.510	0.195	0.206	41.257	3366	0.387
Sollenau	0.608	0.496	0.229	0.193	41.819	3235	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 7: 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

B.2 Confidence intervals, tabulated estimates, and robustness checks

Figure 10: Confidence intervals for contrast of Group 2 and Group 1 in February 2021
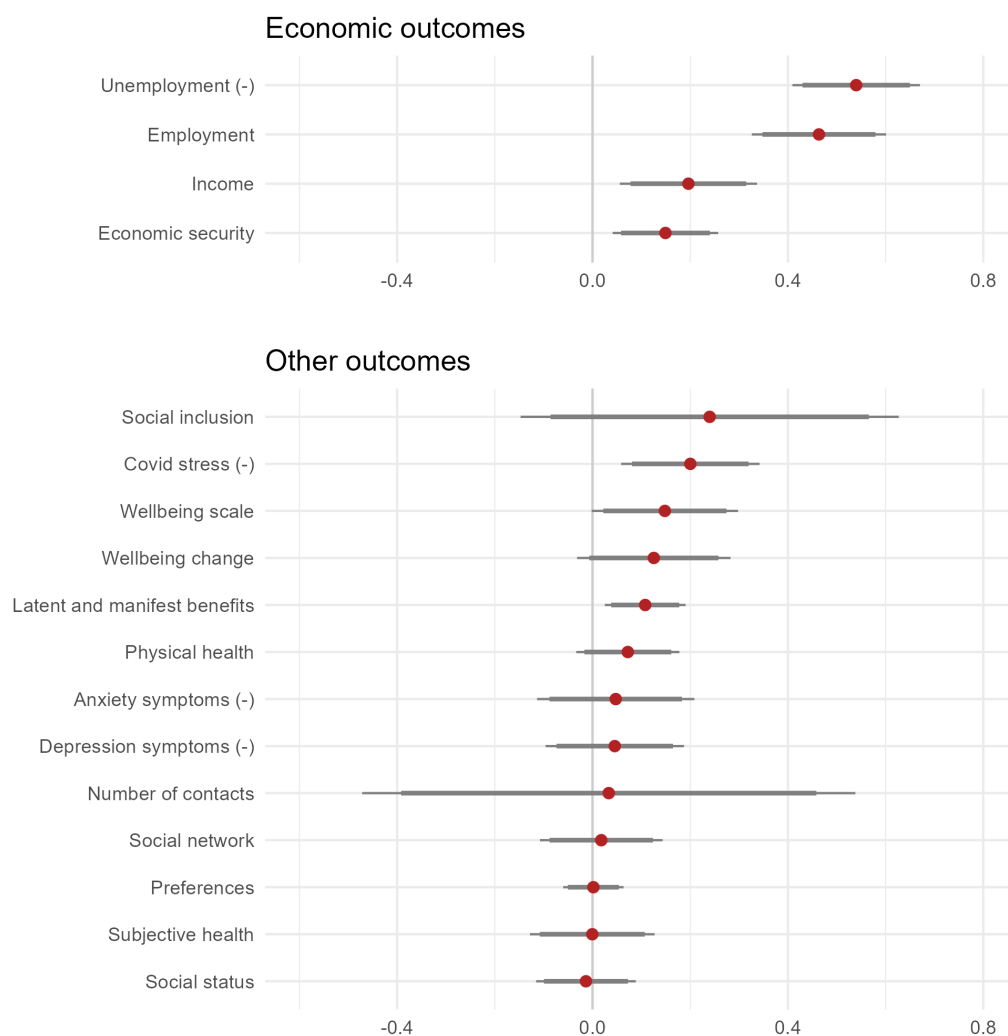
Notes: Confidence intervals for treatment effects, estimated with linear controls for baseline covariates, and with robust standard errors. The thin line shows the 95% confidence interval, the wider line shows the 90% confidence interval. These confidence intervals correspond to the estimates reported in Figure 2. These estimates are also tabulated in Table 8.

Figure 11: Confidence intervals for contrast of Group 2 and control town individuals, February 2021
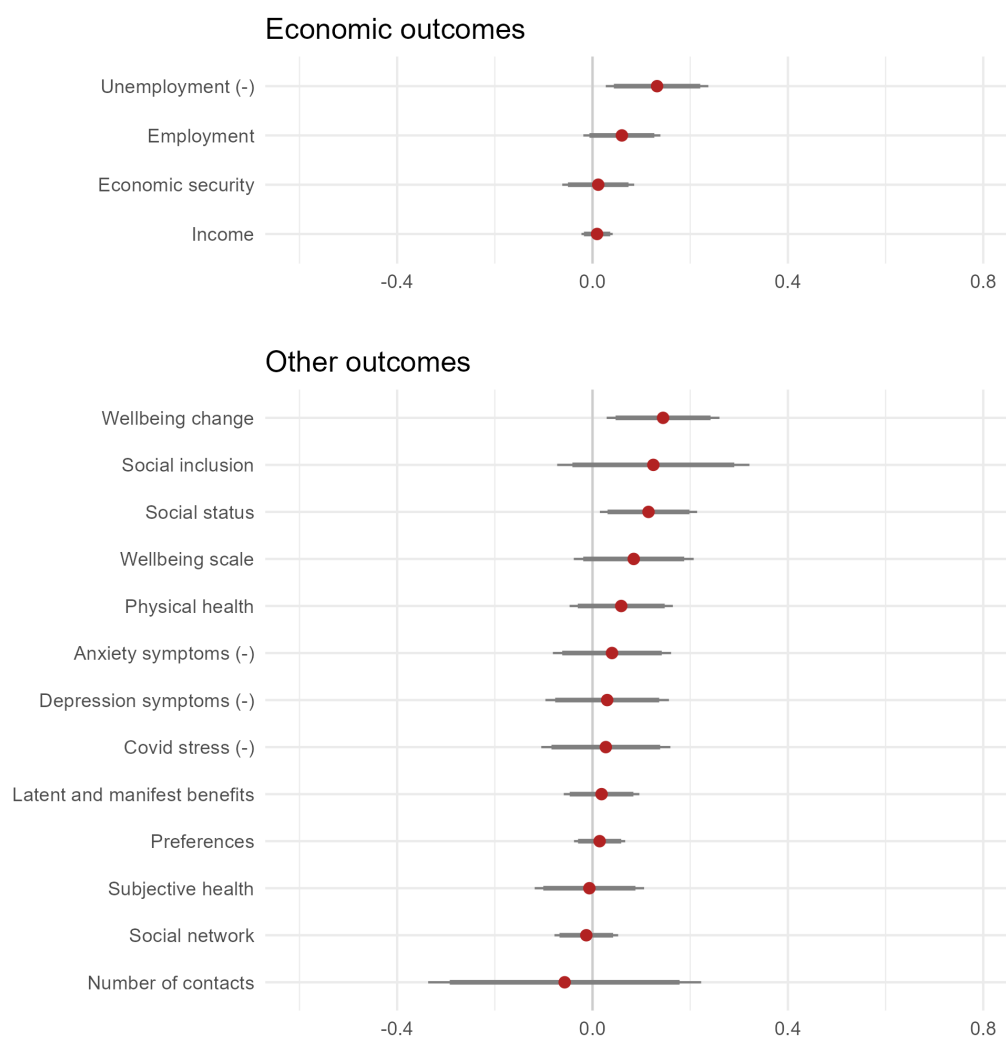
Notes: These confidence intervals correspond to the estimates reported in Figure 6 and Figure 7. These estimates are also tabulated in Table 9 and Table 10.

Figure 12: Confidence intervals for contrast of participants in both groups and control town individuals, February 2022
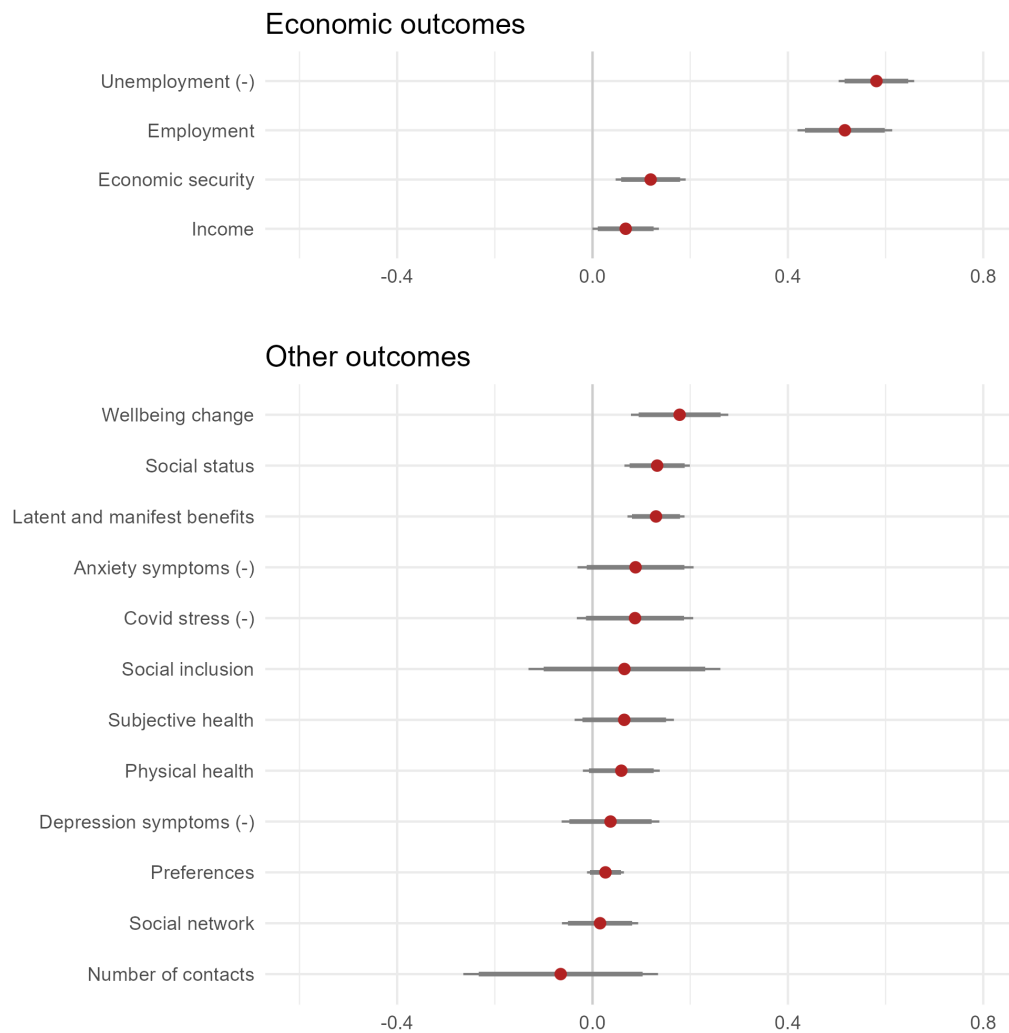
Economic outcomes

Other outcomes

Notes: These confidence intervals correspond to the estimates reported in Figure 6 and Figure 7. These estimates are also tabulated in Table 9 and Table 10.

Table 8: 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
Wellbeing scale	0.732	0.584	0.148	0.033	0.076	20	22
Wellbeing 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: status	0.753	0.615	0.138	0.029	0.080	21	22
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
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: personality traits	0.613	0.582	0.031	0.210	0.038	20	22
Preferences: risk	0.390	0.381	0.009	0.388	0.046	20	22
Preferences: financial risk	0.245	0.291	-0.046	0.702	0.083	21	22
Preferences: time	0.437	0.534	-0.097	0.827	0.111	21	22

Notes: These tables report the same estimates as Figure 2 and Figure 3. P-values are based on randomization inference, SE are robust standard errors for the treatment effect (difference). n_1 and n_2 are the number of treated and control observations, respectively.

Table 9: 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 (-)	0.696	0.157	0.024	0.132	0.054	31	31	71
Income	0.670	0.476	0.473	0.009	0.016	19	19	59
Economic security	0.617	0.460	0.447	0.012	0.038	21	22	63
Employment	0.530	0.063	0.010	0.060	0.040	31	31	71

2022

Outcome	Marienthal	Control towns	Mt vs. Ct towns	SE	n_{mt}	n_{ct}
Unemployment (-)	0.738	0.157	0.581	0.039	62	64
Income	0.595	0.527	0.068	0.035	42	56
Economic security	0.591	0.472	0.119	0.037	45	61
Employment	0.587	0.070	0.517	0.049	62	64

Notes: These tables report the same estimates as Figure 6, Figure 11, and Figure 12. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Marienthal and Control town observations.

Table 10: 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 (-)	0.867	0.668	0.639	0.027	0.067	20	22	62
Physical health	0.816	0.744	0.682	0.059	0.054	20	22	62
Anxiety symptoms (-)	0.804	0.747	0.701	0.040	0.062	20	22	62
Social inclusion	0.782	0.586	0.459	0.124	0.100	21	22	66
Social network	0.770	0.748	0.757	-0.013	0.033	12	12	45
Wellbeing change	0.733	0.604	0.474	0.144	0.059	21	22	71
Wellbeing scale	0.709	0.589	0.495	0.084	0.063	20	22	62
Depression symptoms (-)	0.703	0.656	0.619	0.030	0.065	20	22	62
Latent and manifest benefits	0.686	0.590	0.571	0.018	0.039	21	22	68
Social status	0.600	0.607	0.499	0.115	0.051	21	22	68
Number of contacts	0.539	0.542	0.637	-0.057	0.143	21	22	66
Preferences	0.451	0.457	0.443	0.015	0.027	21	22	63
Subjective health	0.428	0.409	0.421	-0.006	0.057	20	22	61

2022

Outcome	Marienthal	Control towns	Mt vs. Ct towns	SE	n_{mt}	n_{ct}
Social network	0.778	0.762	0.015	0.040	26	39
Anxiety symptoms (-)	0.748	0.660	0.088	0.061	44	58
Physical health	0.725	0.666	0.059	0.040	44	58
Covid stress (-)	0.719	0.632	0.087	0.061	42	53
Latent and manifest benefits	0.661	0.531	0.130	0.030	45	60
Wellbeing change	0.656	0.478	0.178	0.051	45	62
Depression symptoms (-)	0.626	0.589	0.037	0.051	44	58
Social inclusion	0.615	0.550	0.065	0.100	45	61
Social status	0.609	0.477	0.132	0.034	46	62
Preferences	0.522	0.495	0.026	0.019	44	58
Subjective health	0.443	0.378	0.065	0.052	44	58
Number of contacts	0.421	0.486	-0.065	0.102	47	61

Notes: These tables report the same estimates as Figure 7, Figure 11, and Figure 12. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Marienthal and Control town observations.

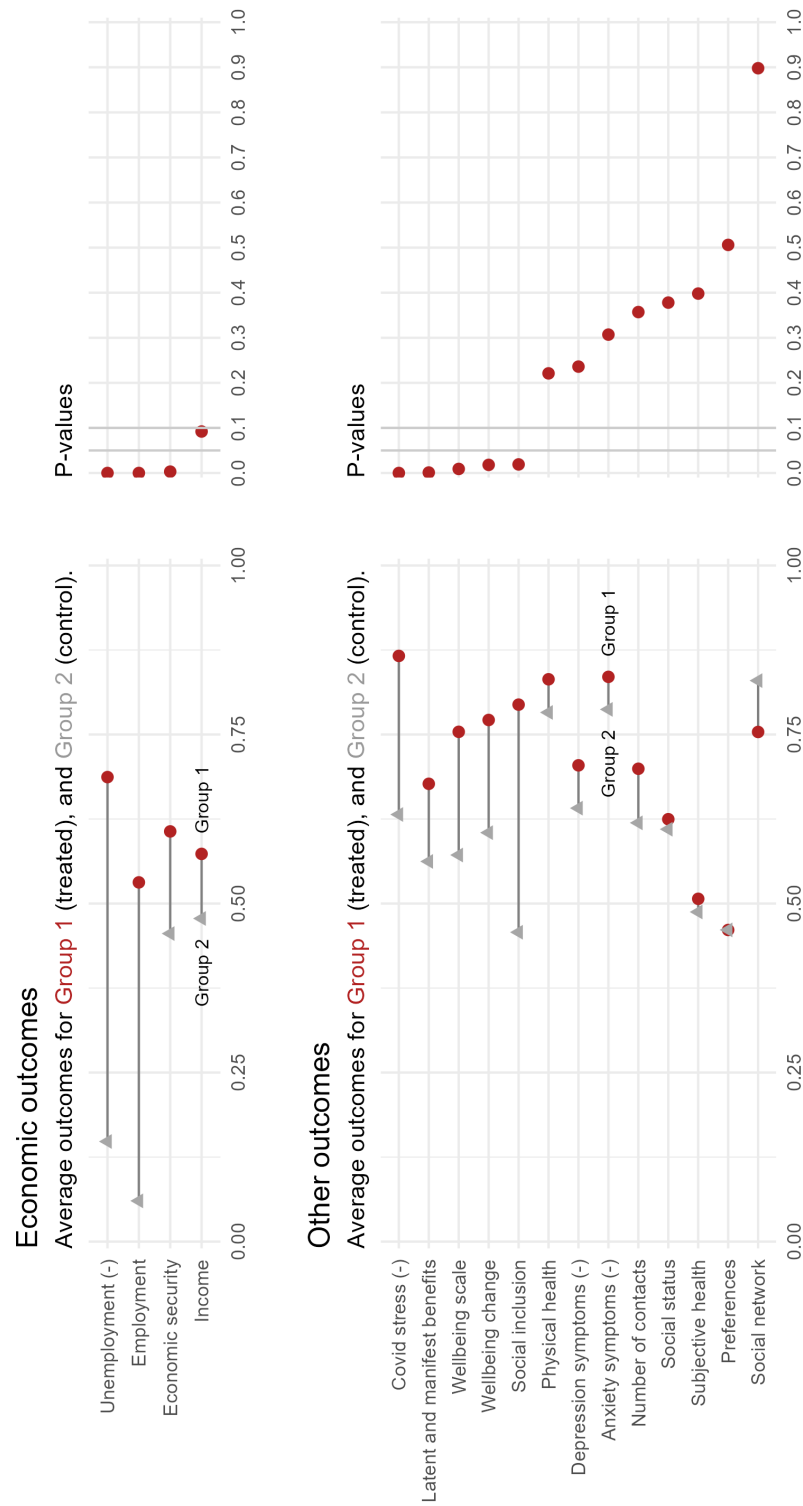Figure 13: Experimental estimates with pair controls

Economic outcomes
Average outcomes for Group 1 (treated), and Group 2 (control).

Other outcomes
Average outcomes for Group 1 (treated), and Group 2 (control).

Figure 14: Experimental estimates with no controls

Economic outcomes

Average outcomes for Group 1 (treated), and Group 2 (control).

P-values

Other outcomes

Average outcomes for Group 1 (treated), and Group 2 (control).

P-values
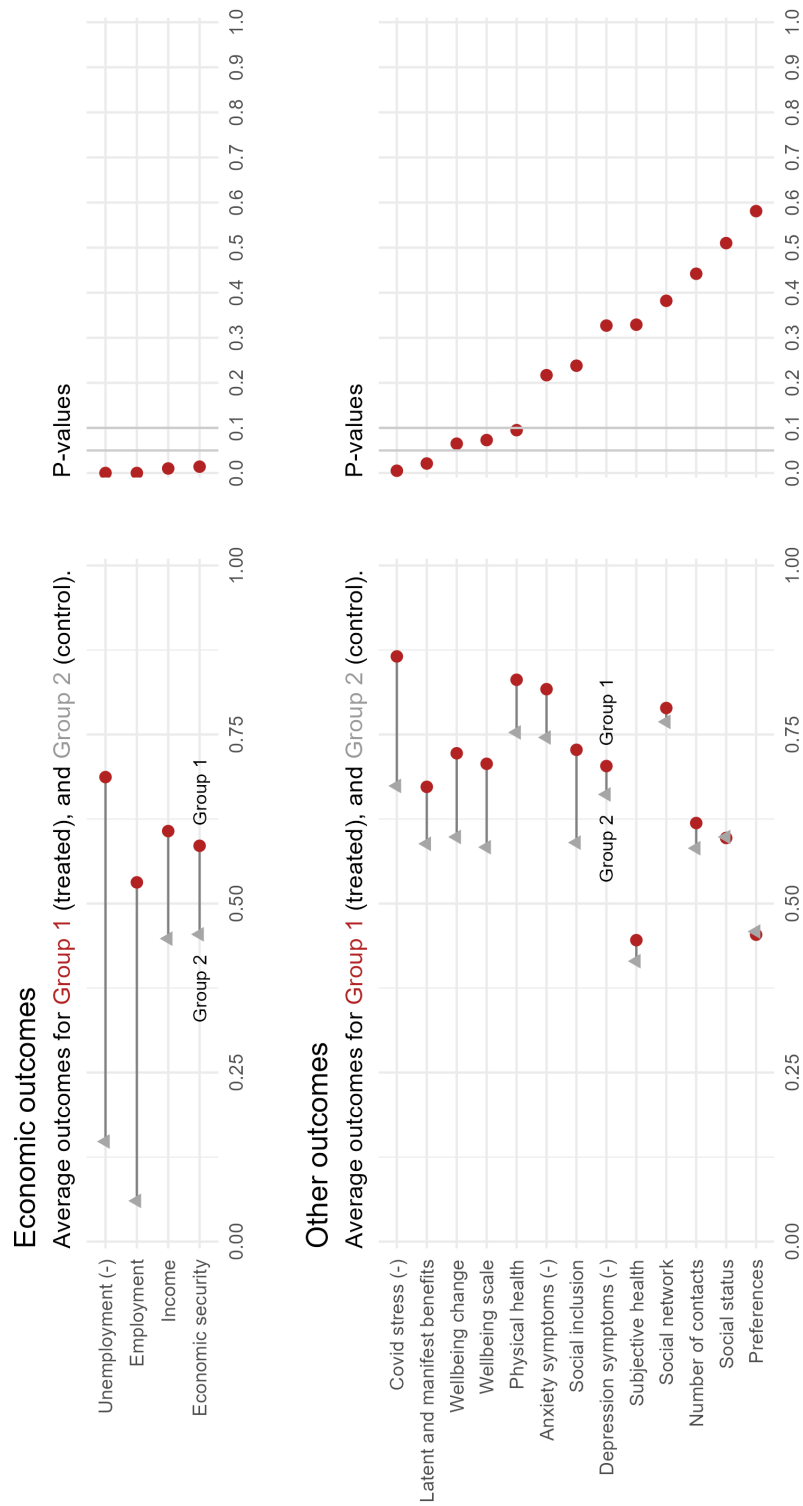
C   From "Die Arbeitslosen von Marienthal" to our study

90 years ago, in 1930, a team of researchers (including Marie Jahoda, Paul Lazarsfeld, and Hans Zeisel) wrote the pathbreaking study "Die Arbeitslosen von Marienthal" (Jahoda et al., 2017). Two years ago, in 2020, an evaluation 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, I take the occasion to reflect on the methodological differences between these two studies. These two studies can be seen as examples of broader developments in social science methodology over the course of the 20th century. I would like to emphasize that this comparison is intended to be descriptive rather than taking a stance regarding the superiority of different methodological approaches.

The study of Jahoda et al. (2017), while pioneering in many ways, also reflected established approaches to empirical social science at its 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 prices 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 the two studies Jahoda et al. (2017) and EUM. Both are quantitative, empirical studies drawing on a variety of data-sources, including self-collected surveys and administrative data.[12] 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. (2017) is a **classification** of the unemployed of Marienthal into 4 types (ungebrochen / resigniert / verzweifelt / apathisch, which translate as unbroken / resigned / desperate / apathetic). By contrast, our focus is on the estimation of **causal effects** of a job guarantee, on both its beneficiaries and the wider community.

The focus on classification was a primary concern of 19th century empirical social science, from Adolphe Quetelet's "social physics" and it's focus on types of "average man" through the "scientific" racists of the 19th century in biology and the humanities and their obsession with classifying humanity into distinct "races," to Max Weber's "ideal types." In an afterword to Jahoda et al. (2017), Hans Zeisel justifies the focus on comprehensive description and classification (or "sociography," as the authors call it) out of the need to understand a complicated and unstable capitalist society, for the purpose of rational policy; a need which he argues did not arise in pre-capitalist feudal times, where the classification of individuals was stable and known to everyone. An important role that Zeisel assigns to classification is to make qualitative data amenable to quantitative analysis.[13]

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

Closely related to this focus on classification versus causality is a distinction in the type of event studied. Jahoda et al. (2017) consider the consequences of a **historical macro event** (the great depression) – there is not even an attempt at finding a comparison group for their study

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[12]Jahoda et al. (2017) also has an important qualitative component.

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

sample of unemployed workers and their families. In EUM, by contrast, we focus on the **causal effect of a (micro) policy** intervention; much of the methodological effort goes into finding valid comparisons. The notion of causality is intimately related to the ideas of **interventions** and **comparison groups.**

Another related aspect is how these studies deal with **heterogeneity**. Jahoda et al. (2017) engage in an impressive and comprehensive effort to **fully capture** and describe the variability of circumstances and psychological responses of the unemployed of Marienthal. By contrast, no such comprehensive effort is made in EUM. Instead, the methodology of causal inference – pairwise matching, randomization, synthetic controls – is used to ensure that comparison groups for causal inference are the **same on average**.

This different approach to heterogeneity is reflected in another striking difference: In Jahoda et al. (2017), no attempt is made to **quantify statistical uncertainty** – there are no standard errors, confidence intervals, or p-values. The study contains a large number of statistical tables, but there is no sense in which these reported numbers (e.g., shares in the sample belonging to a particular category) are related to an underlying **population object** (e.g., shares in the population belonging to a particular category). There is no distinction between estimate and estimand; the reported numbers are what they are. By contrast, EUM follows modern standard practice in reporting standard errors, confidence intervals, and p-values, and additionally addresses the issue of multiple hypothesis testing. The implicit notion is that there are true causal effects (either in the sample or in a larger population), and that the reported estimates are noisy approximations of these effects.

Again related, a striking feature of Jahoda et al. (2017) is its **methodological open-endedness**, contrasting with the complete **pre-registration** of EUM. Jahoda et al. (2017) use a wide variety of data-sources and personal observations, and enter Marienthal without prespecified questions that they will ask. Instead, they distill abstractions and classifications from the rich empirical material they find. By contrast, recent empirical social science has been greatly impacted by its perceived replication crisis, attributed to selective reporting of findings by authors (p-hacking) and journals (publication bias). 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 me conclude by emphasizing one more arc connecting the two studies over the course of a century. A key contribution of Jahoda et al. (2017) was that they documented the devastating impact of unemployment beyond its material consequences on income - in the form of psychological outlook, attitudes to the future, time structure, social cohesion, etc. This perspective was further developed by Marie Jahoda over the course of her career, and has been operationalized by sociologists of work in the form of survey instruments for the "Latent And Manifest Benefits" (LAMB) of work. In EUM, these survey instruments were included in our data collection. And, indeed, these are the dimensions where preliminary findings suggest the strongest impact of a job-guarantee on the wellbeing of beneficiaries.