DISCUSSION PAPER SERIES

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ABSTRACT

Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program*

We evaluate a guaranteed job program launched in 2020 in Austria. Our evaluation is based on three approaches, pairwise matched randomization, a pre-registered synthetic control at the municipality level, and a comparison to individuals in control municipalities. This allows us to estimate direct effects, anticipation effects, and spillover effects. We find positive impacts of program participation on economic and non-economic well-being, but not on physical health or preferences. At the municipality level, we find a large reduction of long-term unemployment, and no negative employment spillovers. There are positive anticipation effects on subjective well-being, status, and social inclusion for future participants.

JEL Classification: I38, J08, J45

Keywords: job guarantee, pairwise matched randomization, synthetic control

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1 Introduction

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

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 to all residents of this municipality who were long-term unemployed (12 months or more) or at risk of long-term unemployment (9 to 12 months). Participation in the program is voluntary, but no person who was offered a job has declined the opportunity. A small number of eligible individuals could not be offered employment for reasons including illness, a prison sentence, or because they found regular employment before the start of the program.

The guaranteed job was preceded by individually tailored preparatory training of about 8 weeks. The jobs themselves could either be subsidized jobs in the regular labor market, or (for the majority of participants) employment in a social enterprise, implementing projects for the municipality. Salaries for all participants were at least equal to the minimum wage set by collective bargaining. Jobs were created to fit the individual needs and constraints of participants, and to provide meaningful activity. Expenditures of the *AMS* per participant were about EUR 29,841. We discuss this number further in Section 2.

The MAGMA program differs from typical active labor market policies, and might al-

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

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

Evaluation strategy 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,

[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.

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.

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

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

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

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

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

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

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

Lastly, when we compare long-term unemployed **individuals in control towns** to program participants, we find effects that are similar to those that we found in our experimental comparison. The point estimates are almost identical for our headline outcomes (income and economic security, employment and unemployment, and the latent and manifest benefits of work). The estimates from this comparison are slightly larger than the experimental estimates for some other dimensions, however, including (subjective) well-being, 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 little attenuation over time. This suggests that the benefits of a guaranteed job are sustained beyond the initial period.

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

90 years later, the MAGMA experiment provides a mirror image of the original situation, by offering employment to all the long-term unemployed residents of Marienthal and of the

municipality of Gramatneusiedl. Strikingly, as noted above, some of the most pronounced effects of program participation that we find are on the "latent and manifest benefits of work" – a measure which operationalizes concepts developed by Marie Jahoda, building on the original Marienthal study. Marie Jahoda continued to work as a sociologist in exile in the United Kingdom, following the rise of fascism in Austria. In Appendix D we offer some reflections on the contrast between the original Marienthal study and the present paper, taking the opportunity to discuss 90 years of methodological developments in the social sciences.

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

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

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

Literature There is a large literature studying the effectiveness of active labor market policies (ALMPs); see in particular the meta-analyses Card et al. (2010, 2018), and the earlier reviews Heckman et al. (1999); Kluve (2010), as well as Crépon and van den Berg (2016). The existing evaluations of ALMPs in German-speaking countries are mostly observational (recent exceptions are Altmann et al. 2018; van den Berg et al. 2021; Böheim et al. 2022);

by contrast, there are numerous experimental studies from the US, e.g. Card and Hyslop (2005); Schochet et al. (2008); Gelber et al. (2016), and France, e.g. Crépon et al. (2013); Behaghel et al. (2014). Cummings and Bloom (2020) discuss a number of recent RCTs in the US evaluating subsidized employment programs, focusing on the effects on employment after the subsidies expire. They find some evidence of positive effects on employment, in particular among the most disadvantaged participants.

This literature also includes some recent evaluations of public employment schemes for India (Khera, 2011; Muralidharan et al., 2023; Banerjee et al., 2020), Ivory Coast (Bertrand et al., 2017), and Malawi (Beegle et al., 2017), and an evaluation of the psychosocial value of employment in Rohingya refugee camps (Hussam et al., 2022).

A common conclusion of evaluations of ALMPs appears to be that job search programs are somewhat effective in improving participants' future employment prospects, as are (sectoral) training programs (Katz et al., 2022), whereas public employment programs are not. Two points are worth emphasizing in this context. First, most of this literature considers different outcomes and policy objectives than we do, focusing in particular on (market) employment, in German-speaking countries, and (market) earnings, in English-speaking countries. By contrast, we are interested in the impact on the community and on participant welfare, without an expectation that participants will enter market employment. Second, much of this literature focuses on individual-level effects, neglecting spillovers; important exceptions are Crépon et al. (2013), who study the negative displacement effect of job counseling using a large-scale clustered randomized controlled trial in France, and Lalive et al. (2015); Huber and Steinmayr (2021), who consider spillovers of unemployment insurance in the Austrian context. Plausibly, the spillovers of search assistance (redistributing existing vacancies without impacting overall employment) are more pronounced than those of a job guarantee (creating additional jobs); we study the latter spillovers in the present paper. Relatedly, Muralidharan et al. (2023) study genereral equilibrium effects of a reform of India's National Rural Employment Guarantee Scheme (NREGS). They find large positive spillovers of the reform, and no crowd-out of private sector employment.

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

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

Roadmap The rest of this paper is structured as follows. Section 2 provides further context and details regarding the MAGMA job guarantee program. Section 3, building on our pre-analysis plan, details our experimental design and analysis, as well as the construction of the synthetic control municipality, and discusses the formal interpretation of our causal estimands. Section 4 discusses our empirical findings, for each of the three approaches. Section 5 concludes.

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

2 Background and program details

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

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

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

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

Those participants who remained without job placement received an employment offer with a newly established social enterprise operated by *it.works*. All participants were paid the occupation- and experience-specific minimum wage, as set by collective bargaining in Austria. This includes both those employed at *it.works*, and those working for private employers. This minimum wage of around EUR 1,500 per month compares to an average monthly wage of EUR 3,308 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 C provides a detailed list of all the jobs that were created, in both the market and non-market sector, describes some of the jobs that were created in greater detail, and reports views from some of the participants in the program. Figure A.8 in Appendix C shows photos of program participants at work, in carpentry, bee keeping, and tailoring.

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

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

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

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

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

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

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

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

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

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

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

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

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

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 since the start of the unemployment spell. The count of the unemployment spell duration starts again from zero if a formerly unemployed person returns to unemployment from sick leave or employment that lasted longer than 62 days.

Outcomes of interest We estimate the effect of program participation on a range of economic and social outcomes. These outcomes are listed and defined in Table 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

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

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, social recognition, and financial strain.	Survey
Physical health	A fifteen item physical health scale.	Survey
Anxiety symptoms (-)	A seven item anxiety scale.	Survey
Social network	A six item social network scale.	Survey
Well-being scale	A five item mental well-being scale.	Survey
Well-being change	Subjective well-being compared to six months ago.	Survey
Social status	Three item scale on current social status, status compared to the past, and expected future status.	Survey
Number of contacts	The number of meaningful social contacts with respect to work-related and job-search issues in the six past month, divided by 5.	Survey
Subjective health	Two questions on overall health situation and recent changes.	Survey
Municipality level		
Unemployment	Number of unemployed as a share of working age population.	Admin
Long-term unemployment	Number of long-term unemployed ($>$ 1 year) as a share of working age pop.	Admin
Short-term unemployment	Number of short-term unemployed ($\leq$ 1 year) as a share of working age pop.	Admin
Employment	Number of employed as a share of working age pop.	Admin
Inactivity rate	Number of inactive persons of working age as a share of working age pop.	Admin

February 2021 and in February 2022. The complete list of survey questions corresponding to each of these outcomes is listed in Appendix B. We collected information on a rich set of economic outcomes (in particular income and economic security), as well as non-economic outcomes. For non-economic outcomes, we construct a range of indices, on the "latent and manifest benefits" of work, measures of mental and physical health, subjective well-being, social inclusion and recognition, etc. Our construction of these indices follows established practice in survey design, sociology, psychology, and public health; cf. again Appendix B for references and details.

To enable a compact presention of our results in Section 4, we normalize all individual-level outcomes, such that higher values correspond to "better" outcomes (variables where the sign is flipped are marked by *(-)* in the table and subsequent figures), and such that the range of these variabes is the interval $[0, 1]$; cf. Table 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 pairwise randomization approach reduces sampling variability, relative to full randomization. The comparison of the two groups delivers credibly identified treatment effects. It is restricted, however, to short-term individual-level outcomes measured in February 2021, before the second group of participants starts their jobs. Furthermore, the control group might be impacted by the anticipation of future program receipt.

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

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

Approach 1: Pairwise randomization We assigned program participants to one of two groups using pairwise randomization. We matched pairs using a number of covariates,[5] 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

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

[5]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*.

data from various sources, including (i) the *AMS* internal registry for administrative data on the unemployed, (ii) the "occupational-career monitoring" (*Erwerbskarrierenmonitoring, EWKM*), accessed via the *AMS* internal registry for social security registry data, and (iii) the national statistical agency (*STATcube - Statistische Datenbank* of *Statistik Austria*) for population and communal tax data. All data were retrieved in September 2020.

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

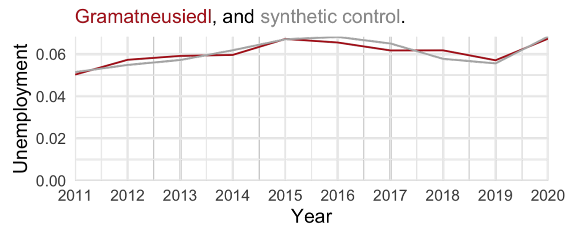
In the second step, we construct a synthetic control based on these 25 municipalities, using the approach described in Abadie et al. (2010) and reviewed in Abadie (2019). This synthetic control is chosen to match the same list of covariates used in the first step (where we selected a subsample of municipalities),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

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

Approach 3: Individual-level comparison to control municipalities Our third approach is based on data for individuals from the three municipalities with the largest weight in the synthetic control (Ebreichsdorf, Zeillern, Rußbach). Taken together, the weights of these three municipalities constitute 82.4% of our synthetic control. We construct a control

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



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

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

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

3.2 Causal interpretation of estimands – spillover 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

Table 3: Identified averages

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

job guarantee, and D_i^{+1} future eligibility, at some fixed time horizon. Let $\overline{D}$ be the share of long-term unemployed in the municipality who are currently eligible. Let finally ϵ_i be a vector of unobserved individual characteristics, which are not affected by the program. We can then assume that

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

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

Identifying contrasts With this notation, we can now describe the identified averages from our three evaluation approaches in structural terms. Table 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 principle, 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 2021, to control town individuals identifies the **average total effect on the treated**, which incorporates direct effects, anticipation effects, and spillover effects.

The comparison of Group 2 to control town individuals, again in February 2021, identifies a combination of spillover and anticipation effects. Under the plausible additional assumption that these eligible individuals are not impacted by spillover effects, because they

Table 4: Identified effects and roadmap

Contrast	Identified effect	Interpretation	Figures and Tables
February 2021			
Group 1 vs. Group 2	$E[g(1,1,\frac{1}{2},\epsilon_i) - g(0,1,\frac{1}{2},\epsilon_i)\|L_i = 1]$	Average direct effect on the treated	Figure 2, Figure 3, Table 5 Figure A.3
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, Table 6 Table 7, Figure A.4
After April 2021			
Group 1 & 2 vs. control town	$E[g(1,1,1,\epsilon_i) - g(0,0,0,\epsilon_i)\|L_i = 1]$	Average total effect on the treated	Figure 6, Figure 7 Figure A.5
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

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.,

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

3.3 Inference

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

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

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

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

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

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

4 Findings

We are now ready to discuss our empirical findings. We will consider a large number of outcomes and contrasts.[6] Our headline findings are summarized by Figures 2 through 7 in this section, as well as Figures A.3 through A.5 in Appendix A. Individual-level estimates are also shown numerically in Table 5 through Table 7.

Individual-level outcomes and outcome indices in these figures and tables are normalized as follows: (i) They have a potential range from 0 to 1, and (ii) higher values represent "better" outcomes (e.g., lower unemployment, higher income, lower anxiety, etc.); recall that variables where the sign is flipped are marked by *(-)* in all our figures. Additional figures with results for further outcomes, alternative identification approaches, confidence intervals, and robustness checks can be found in Appendix A. 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, Figure 3, and Table 5 show estimates for this experimental comparison. The left panels in both figures shows average outcomes for the treatment and control group, adjusting for covariates. The right panels shows p-values for the null of a zero treatment effect. These p-values are based on randomization inference, using 1000 simulation draws, where we permute treatment within pairs. Random permutation within pairs corresponds to our experimental design using pairwise matched randomization.

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

The estimates in Figure 2, Figure 3, and Table 5 control linearly for baseline covariates, to adjust for potential non-random attrition in the survey. Figure A.6 and Figure A.7 in Appendix A display analogous findings without controls, and with controls for pair fixed

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

effects. In both cases, the resulting estimates are close to those in our preferred specification using linear controls. Figure A.3 in Appendix A further shows confidence intervals for treatment effects, based on robust standard errors for the regressions with linear controls.

Findings For economic outcomes (shown in the top panels of Figure 2 and Table 5), measured using both survey and administrative data, we find highly significant positive effects.[7] Unemployment is strongly reduced in Group 1 through program participation. This is not due to transitions out of the labor force (e.g., to early retirement or disability status). Instead, our estimates show that this effect is fully driven by the increase in employment.

Participants who accept a guaranteed job increase their income. 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 and middle panel of Table 5), we see a more heterogeneous picture. For some outcomes, in particular those related to social status, subjective health, mental health, social network, number of contacts, and preferences, we do not find a significant effect. Disaggregating the preference index into its components in Figure 3 and the bottom panel of Table 5, we correspondingly find no effects on risk- or time-preferences, or personality traits. These findings provide a placebo test of our experimental design and identification approach. A priori, it would not be plausible to find short-term effects of employment on physical health or preferences. The fact that we indeed do not find such effects increases our confidence that survey answers are not driven by interviewer demand effects, in particular.

By contrast, we do find large and significant effects of the program on Covid stress, subjective well-being and its change over time, and in particular on the index measuring the "latent and manifest benefits" of work. Disaggregating the latter again, Figure 3 and the bottom panel of Table 5 show significant effects of participation on several components of this index, including activity, social recognition, and financial strain, and positive but marginally insignificant effects on time structure, collective purpose, and social interactions.

These effects are remarkable not only in their own right, but also because of the historical importance of Marienthal, which was the location of the original Jahoda et al. (2017) study, and because of the literature on the sociology of work which connects our study to Jahoda et al. (2017). The LAMB scale[8] was developed to quantify Jahoda's insight (Jahoda, 1982), based on the Marienthal study and subsequent work, that

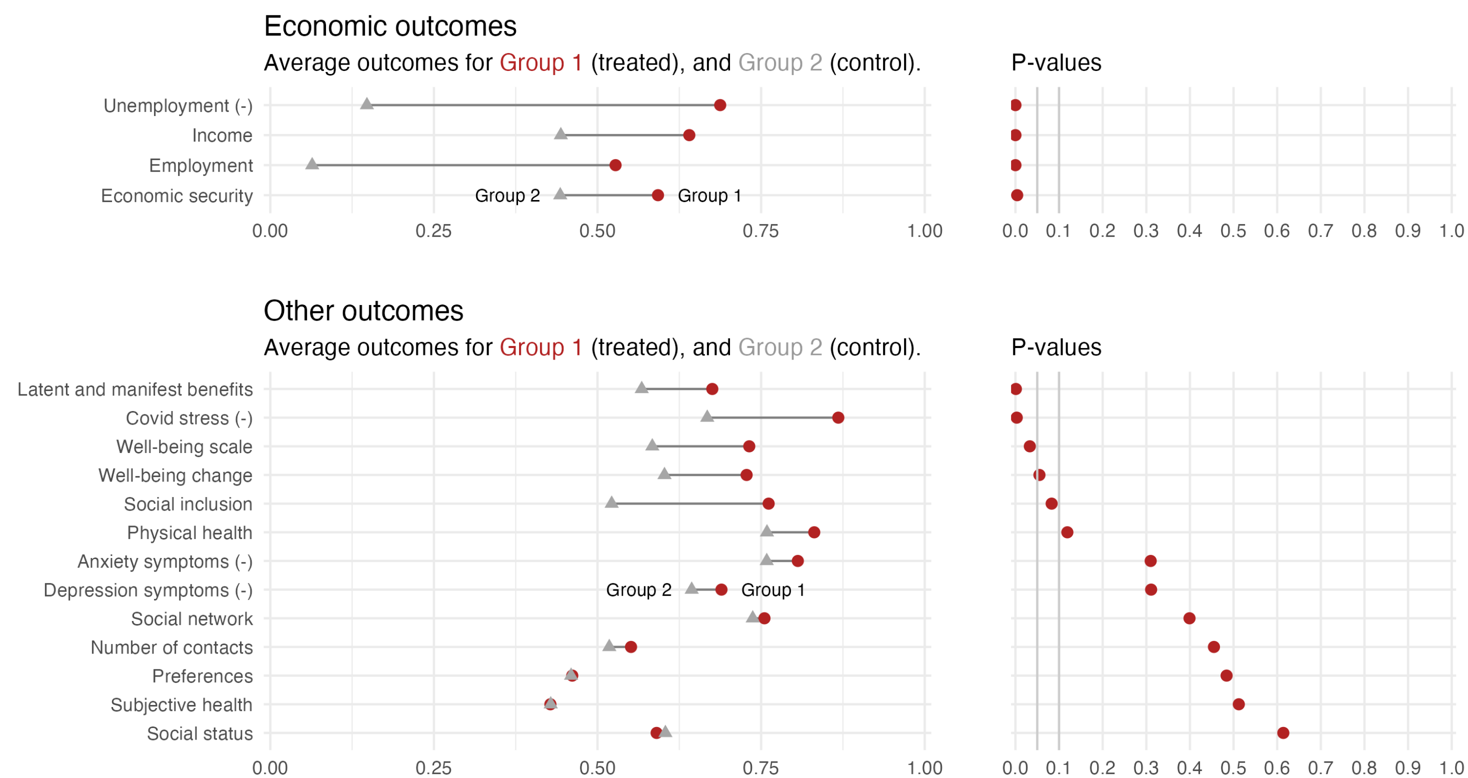
[7] Recall the normalization of these outcome variables from Table 1: Employment and unemployment are defined as the share of days since the program started, and the monthly income is divided by 2000.

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

> "[individuals] have deep-seated needs for structuring their time use and perspective, for enlarging their social horizon, for participating in collective enterprises where they can feel useful, for knowing they have a recognised place in society, and for being active."

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

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 5.

Table 5: Experimental estimates with linear controls

Economic outcomes

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

Other outcomes

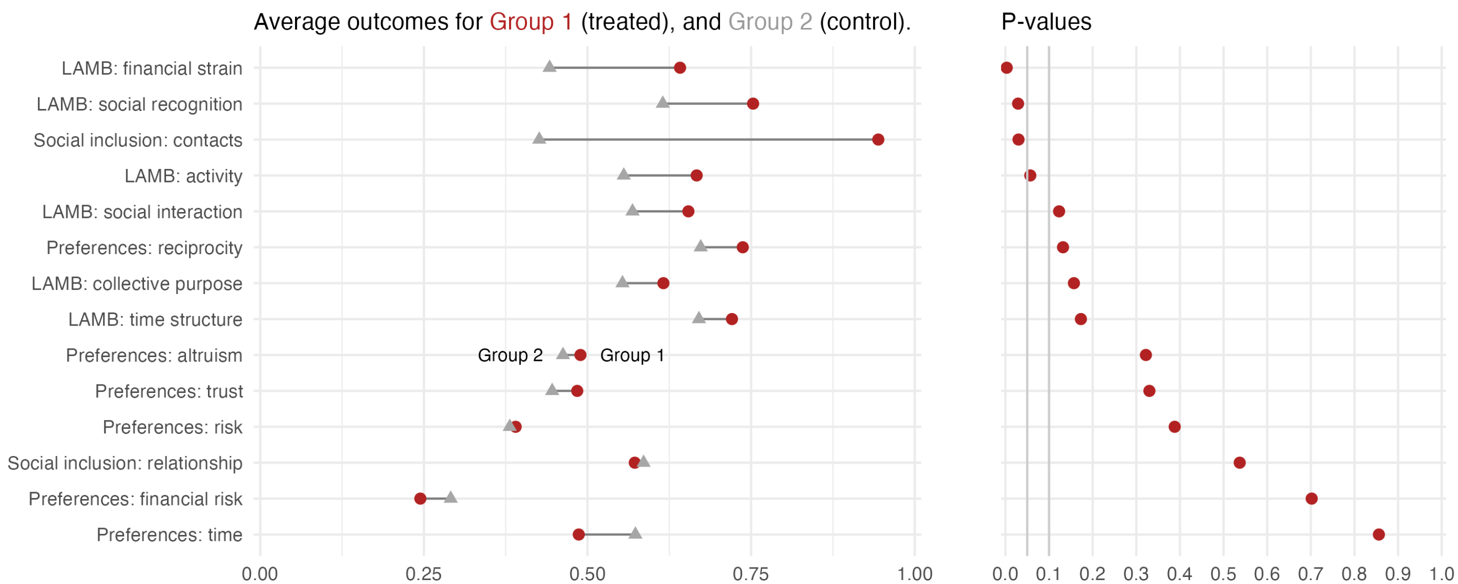
Outcome	Treated	Control	Difference	p-value	SE	n_1	n_2
Latent and manifest benefits	0.675	0.568	0.108	0.001	0.042	21	22
Covid stress (-)	0.868	0.668	0.200	0.003	0.072	20	22
Well-being scale	0.732	0.584	0.148	0.033	0.076	20	22
Well-being change	0.728	0.602	0.125	0.055	0.080	21	22
Social inclusion	0.761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

Disaggregated outcomes

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

Notes: These tables report the same estimates as Figure 2 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.

Figure 3: Experimental estimates with linear controls, disaggregated outcomes



4.2 Synthetic control municipalities

We next consider the comparison of municipality-level outcomes between Gramatneusiedl and the pre-registered synthetic control. For this comparison, we use municipality-level administrative data on unemployment (total, long-term, and short-term), employment, and inactivity. Our synthetic control estimates are shown in Figure 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.

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

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

Findings As expected, the program has a large effect on long-term unemployment in the municipality. By the time both groups of eligible participants 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

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

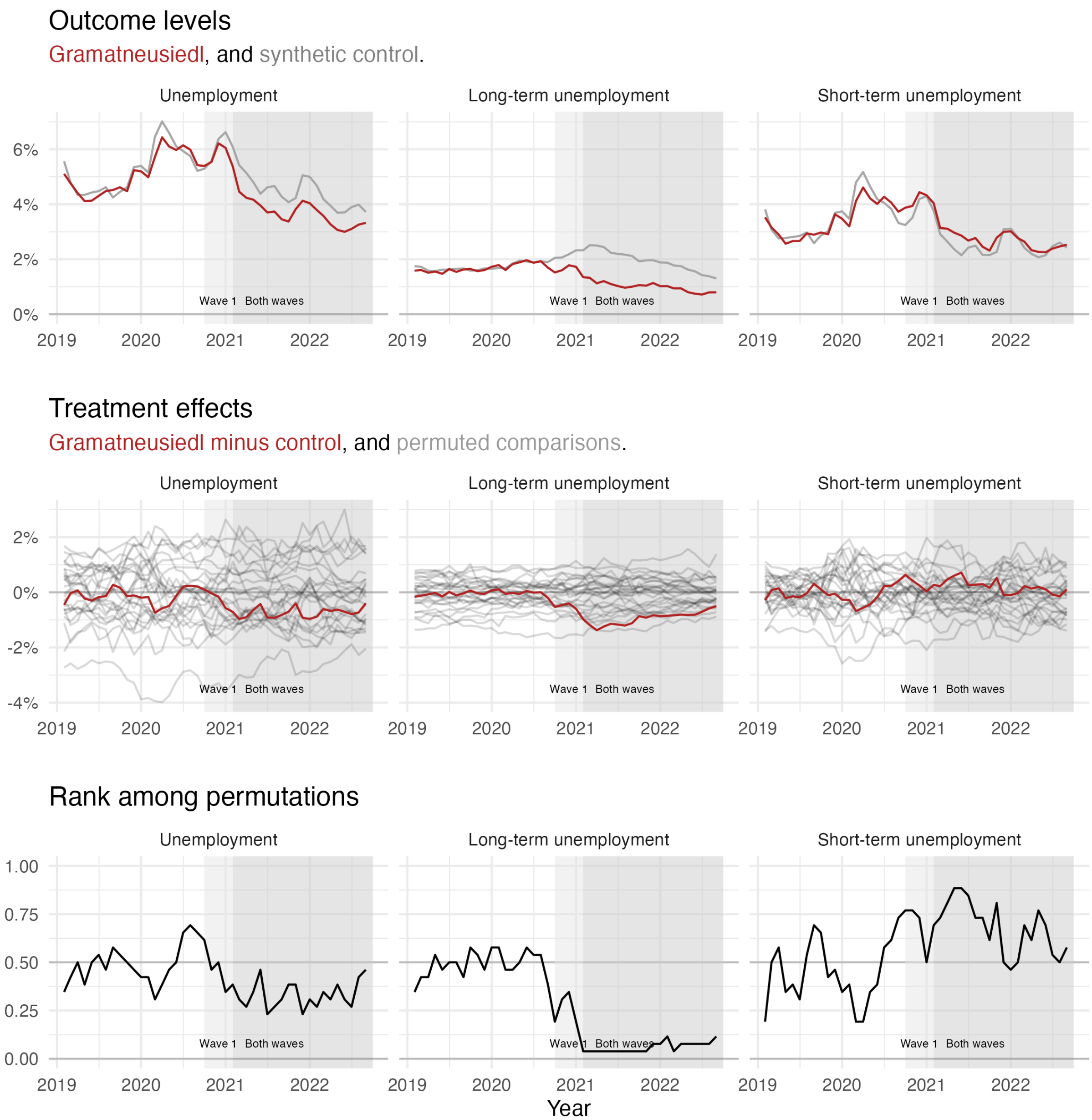
of long-term and short-term unemployment. This total impact is negative. The synthetic control estimate suggests a reduction of the unemployment rate by about 1 percentage point, from 5% to 4% in 2021, and from 4% to about 3% in 2022. Correspondingly, Gramatneusiedl is around the 30th percentile in terms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

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

One might conjecture that the reduction of unemployment is driven by a transition of the unemployed out of the labor force, for instance into (early) retirement or into a certified disabled status, in order to avoid work requirements associated with the job guarantee. That this is not the case for the program studied here is verified by Figure 5. The left column of this figure shows effects on employment, and the right shows effects on "inactivity" (i.e., the share out of the labor force). As 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.[10] Put differently, rather than inducing the unemployed to transition out of the labor force altogether, the program might have had the opposite effect.

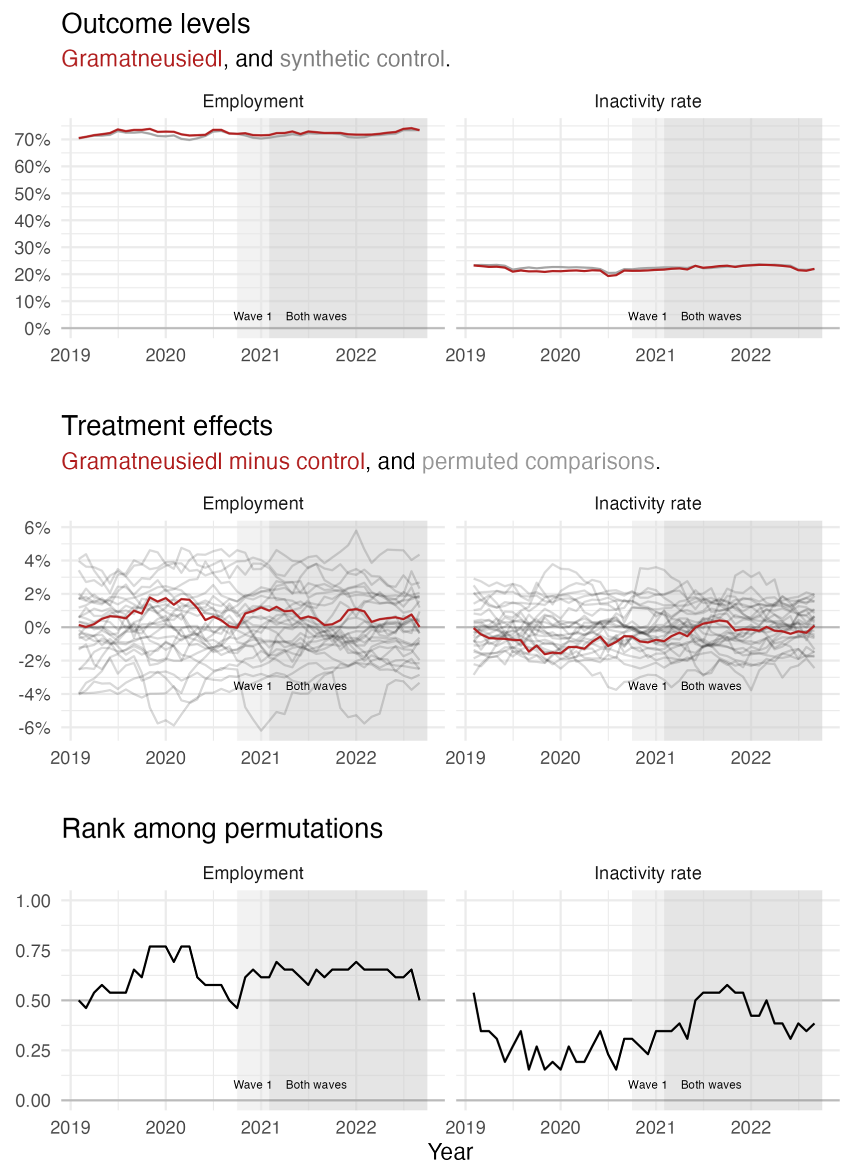
[10]While unemployment, employment, and inactivity sum almost to 1, there is a small residual category of people who are currently in AMS training. This category amounts to about 1-2% of the population, who are not included in either of the three other categories.

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



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

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



4.3 Comparison to individuals in control towns

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

The resulting estimates are shown in Figure 6 and Table 6 for economic outcomes and Figure 7 and Table 7 for other outcomes. In both figures, we show outcomes for 2021 at the top, where we separate individuals in Group 1, Group 2, and the control towns, and outcomes for 2022, where we compare all eligible individuals in Gramatneusiedl (Group 1 and 2), to individuals in the control towns.

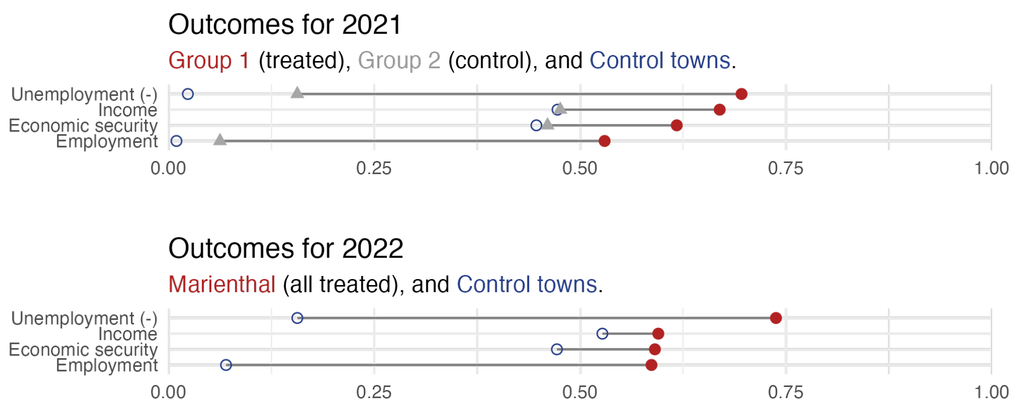
Figure A.4 and Figure A.5 show corresponding confidence intervals. Figure A.4 contrasts Group 2 to control town individuals in 2021, thus providing an estimate of the average anticipation effect on the treated. Figure A.5 contrasts both groups to control town individuals in 2022, thus providing an estimate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

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

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

For employment, social status, (subjective) well-being 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



Notes: These estimates are also tabulated in Table 6.

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

2021

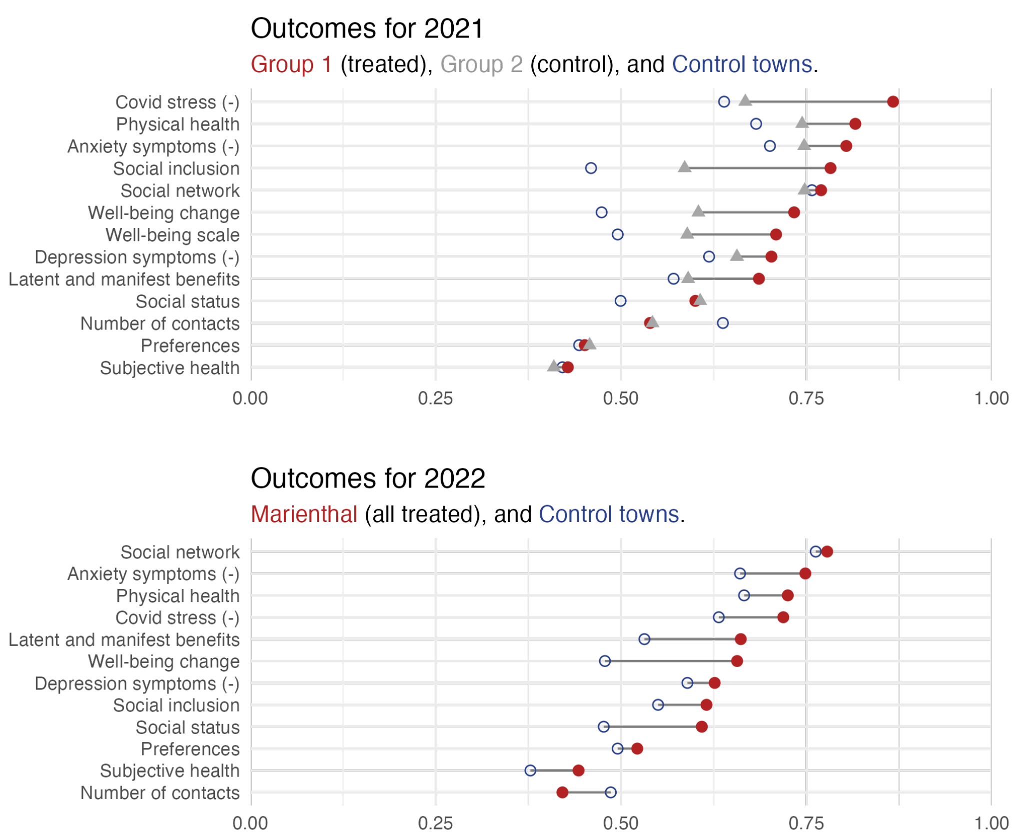
Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	n_1	n_2	n_{ct}
Unemployment (-)	0.696	0.157	0.024	0.132	0.054	31	31	71
Income	0.670	0.476	0.473	0.009	0.016	19	19	59
Economic security	0.617	0.460	0.447	0.012	0.038	21	22	63
Employment	0.530	0.063	0.010	0.060	0.040	31	31	71

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

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



Notes: These estimates are also tabulated in Table 7.

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

2021

Outcome	Treated	Control	Control towns	Ct vs. Ct towns	SE	n_1	n_2	n_{ct}
Covid stress (-)	0.867	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
Well-being change	0.733	0.604	0.474	0.144	0.059	21	22	71
Well-being 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
Well-being 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 A.4, and Figure A.5. SE are robust standard errors for the comparison of the control group (Group 2) and control town individuals (2021), and for the comparison of both groups and control town individuals (2022). n_1 and n_2 are the number of treated and control observations, respectively, and n_{mt} and n_{ct} are the number of Marienthal and Control town observations.

5 Conclusion

We conclude by summarizing our evaluation approaches and main findings, before discussing bigger-picture takeaways and avenues for future research. Our evaluation is based on several experimental and non-experimental contrasts, as summarized in Table 4. We use an experimental staggered roll-out design, comparing earlier and later entrants into the program, to identify direct effects of the job guarantee on the treated. We use a synthetic control approach at the municipality level to identify spillover effects of the job guarantee on the untreated, as well as the average total effect of the job guarantee on the labor market. And we compare program participants to observationally similar individuals in control towns, to separate out anticipation effects, and to estimate the long-term effects of the job guarantee.

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

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

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

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

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

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

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