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The Effect of Subsidized Employment on Happiness

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The Effect of Subsidized Employment on Happiness

Benjamin Crost [*]

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Abstract While a large body of evidence suggests that unemployment and self-reported happiness are negatively correlated, it is not clear whether this reflects a causal effect of unemployment on happiness and whether subsidized employment can increase the happiness of the unemployed. To close this gap, this paper estimates the causal effect of a type of subsidized employment projects - Germany's *Arbeitsbeschaffungsmaßnahmen* - on self-reported happiness. Results from matching and fixed effects estimators suggest that subsidized employment has a large and statistically significant positive effect on the happiness of individuals who would otherwise have been unemployed. Detailed panel data on pre- and post-project happiness suggests that this effect can neither be explained by self-selection of happier individuals into employment nor by the higher incomes of the employed.

Keywords: Happiness, Life Satisfaction, Unemployment, Subsidized Employment
JEL Classification: J28, J68

1   Introduction

A large body of research shows that the unemployed report significantly lower levels of happiness [1] and higher levels of psychological distress than the employed (see McKee-Ryan, 2005, for a review of the psychological literature). The negative correlation between unemployment and happiness, both across individuals and over time, remains significant after controlling for a wide range of observable characteristics, including income (Clark and Oswald 1994, Winkelmann and Winkelmann 1998, Marks and Fleming 1999, Clark 2003, Carroll 2007). A possible explanation for this finding is that, in addition to income, jobs confer social status, respect and a sense of purpose, competence and efficacy, all of which are thought to be important contributors to well-being and job-satisfaction (Izard 1991, Ryan and Deci 2000, Ellingsen and Johannesson 2007, Ariely et al. 2008. Involuntary unemployment [2] may therefore have a psychological cost - a negative effect on well-being that goes beyond its effects on income and consumption (Frey and Stutzer 2002, Carroll 2007). A psychological cost of unemployment would have implications for labor market and welfare policy, implying that the welfare cost of unemployment is greater than the value of lost output and that subsidized employment may be a better way to increase the well-being of the unemployed than direct cash transfers (see, for example, Edlin and Phelps 2009, who cite the psychological benefits of employment as an argument for the introduction of tax credits for employers of low-wage workers).

But the evidence for a negative causal effect of unemployment on happiness is not entirely conclusive. Happiness and unemployment are simultaneously determined, so it is possible that unobserved shocks - for example adverse shocks to (mental) health - simultaneously decrease happiness and increase the probability of becoming or remaining unemployed (e.g. Mastekaasa 1996). There is some evidence that a similar mechanism may explain the negative correlation between self-reported health and unemployment. While the unemployed on

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[1]For the rest of the paper, I use the term "happiness" to denote "self-reported happiness". Following Arrow and Dasgupta (2009), I use happiness as synonymous with life-satisfaction and well-being.

[2]For the rest of the paper, I will use the term "unemployment" to denote "involuntary unemployment".

average report worse subjective health than the employed, individuals who lose their jobs for exogenous reasons, such as the closure of their employer's business, do not experience a decline in subjective health (Salm, 2009). Causality is therefore likely to run from bad health to unemployment and not in the other direction (Bockermann and Ilmakunnas, 2009). If the same is true for happiness, the unemployed may be less happy than the employed even if unemployment has no causal effect on happiness.

But even if unemployment causes unhappiness on average, it is not clear that subsidized employment can increase happiness. It is possible that happiness is only increased by jobs that have certain desirable characteristics, such as being perceived as meaningful or conferring high social status and respect (Ellingsen 2007, Ariely 2008). Since the jobs created by subsidized employment are often poorly paid and confer little social status, it is possible that they do not have the desirable characteristics that cause an increase in happiness. In other words, even if the *average* job increases the happiness of the average employee, the *marginal* job created by an employment subsidy may have no (or even a negative) effect on the happiness of the marginal employee.

This paper contributes to the literature on happiness and unemployment by estimating the effect of subsidized employment on the happiness of the unemployed. To do this I analyze the happiness of participants in a type of public subsidized employment projects (SEPs) - Germany's *Arbeitsbeschaffungsmaßnahmen*. Previous research suggests that these projects have on average had little success in increasing participants' future income and probability of employment (Hujer et al. 2004, Caliendo et al. 2008). But if the goal of public policy is to increase people's happiness, employment subsidies may still be desirable if they prevent the unhappiness of unemployment.

Since participation in the subsidized employment projects is non-random, my identification strategy relies on detailed panel data on happiness before and after the start of the project. The data come from the German Socio-Economic Panel which, among other things, collects information on respondents' happiness and employment status, including partici-

pation in subsidized employment projects. Using this data I show that, for the duration of the subsidized employment project, the happiness of participants is significantly higher than that of unemployed non-participants with similar observable characteristics. The data further show that participants and similar non-participants have virtually identical levels and trends of happiness in the months before the start of the project, which suggests that the observed effect is not driven by self-selection of happier individuals into the projects. Quantitative estimates from fixed-effects and nearest-neighbor matching estimators suggest that, compared to the counterfactual of remaining unemployed, subsidized employment increases happiness by about 0.4 to 0.6 points on a scale from 0-10. This effect corresponds to about 0.4 within-individual standard deviations of happiness, which is large compared to the effects of other observable characteristics like income and marital status.

It should be noted that this estimate does not reflect the total effect of subsidized employment programs on the happiness of their participants, but only their effect against the counterfactual of remaining unemployed. Some participants would have found employment even without the SEP, so that the overall effect of participating in the project (against the counterfactual of not participating) is most likely smaller. Still, the estimated effect can be useful for evaluating the effect of employment subsidies on happiness. Regardless of whether some participants would have found jobs in the absence of the SEP, economic theory suggests that an employment subsidy creates jobs in equilibrium. One could therefore combine this paper's estimate of the effect of subsidized employment on happiness with an estimate of the number of jobs created by the subsidy in equilibrium to derive an estimate of its total effect on happiness.

As an additional contribution, I attempt to determine whether subsidized employment increases happiness by conferring direct psychological benefits - for example by conferring social status and the feeling of being useful - or by increasing income and consumption. In order to draw policy conclusions it is important to disentangle these two channels. A happiness-based argument for publicly subsidized jobs, similar to the one made by Edlin

and Phelps (2009), only holds if employment itself increases happiness. If the employed are merely happier because of their higher incomes, direct income transfers are a more cost-effective way of increasing the happiness of the unemployed. To disentangle the two channels, I exploit the fact that participation in a SEP prolongs individuals' entitlement to public unemployment benefits. Thus the projects' positive effect on income remains even after employment in it has ended. This creates sufficient independent variation in income and employment to allow me to identify the effect of employment while controlling for differences in income. Intuitively, if the positive effect of SEPs on happiness were mainly due to their effect on income, we would expect participants' happiness to remain high as long as the project's effect on income persists, even after employment has ended. But the data show that participants' happiness decreases substantially as employment in the project ends, suggesting that employment has psychological benefits that are independent of its effect on income. To obtain quantitative estimates of the net effect of employment on happiness (excluding the effect of income), I estimate a fixed effects instrumental variables estimator that exploits the fact that participants' probability of employment drops sharply at the end of the subsidized employment project while their incomes remain nearly unchanged.

The next section briefly describes the institutional details of Germany's subsidized employment projects. Section 3 describes the econometric methods used to identify the causal effect of subsidized employment on happiness. Sections 4 and 5 describe the data and present results, Section 6 concludes.

2   Institutional Background: Subsidized Employment Projects in Germany

Subsidized employment projects (*Arbeitsbeschaffungsmaßnahmen*, SEPs) have been an integral part of Germany's active labor market policy for over 30 years (Bernhard et al., 2008). Figure 1 plots the trend of entrants into SEPs over the period of observation, 1994-

2004, both in absolute numbers and as a percentage of the unemployed[3]. At the peak in 1994, approximately 390,000 individuals - slightly over 10% of all unemployed individuals - entered a SEP within a single year. The average annual number of entrants is about 280,000, corresponding to approximately 6% of unemployed individuals.

The institutional rules governing subsidized employment projects have been described in detail by Hujer et al. (2004) and Caliendo et al. (2008) and this section draws on their descriptions. The two main instruments of German active labor market policy are vocational training and subsidized employment projects. Local job-centers have a large amount of autonomy in allocating their budget to different policies, but subsidized employment appears to be the favored instrument in areas with higher unemployment. To create a subsidized employment project, the potential employer applies to the job-center with a description of the proposed jobs. The job-center assesses the proposal according to a number of criteria, most importantly whether the proposed activity is in the public interest and whether the activity is "additional" in nature, meaning that it would not be undertaken in the absence of a SEP. Up to 2002, SEPs were reserved for employers in the non-profit sector, after 2002 exceptions became possible with the approval of the job-center. After approving the SEP, the job-center assigns some of its unemployed clients to the project and pays between 30 and 75 % of their wages, though in exceptional cases the amount of the subsidy can go up to 100%. In the assignment decision, job-centers are mandated to give priority to individuals whose chances of employment outside of SEPs are small.

Before 2002, participants in SEPs had to be unemployed for at least 6 out of the previous 12 months, though exceptions existed for young people without professional training, the short-term unemployed and people with disabilities. In addition, 5 % of the places in SEPs could be allocated to individuals who did not meet any of these conditions. After 2002, all unemployed individuals could be assigned to SEPs, under the condition that the job-center

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[3]The "Hartz-IV" labor market reforms, which came into force in January 2005, introduced substantial changes to the system of subsidized employment in Germany. *Arbeitsbeschaffungsmaßnahmen* lost in importance and were largely replaced with so-called One-Euro-Jobs. In order to keep the results consistent, I therefore only focus on the period until 2004

saw the SEP as their only opportunity for employment. Individuals can refuse to participate in the project, but refusal can be penalized by a reduction in unemployment benefits. The duration of a project is usually 12 months but projects can be extended in special cases.

3 Empirical Strategy

3.1 A simple model of happiness and (un)employment

This section presents a simple empirical model of happiness and (un)employment. It assumes that the happiness of individual i while being unemployed at time t is a function of her characteristics (X_{it}) at the time,

$$h_{it}(0) = f(X_{it})$$

while the individual's happiness while being employed in job k is also a function of the job's characteristics (Z_{kt}),

$$h_{it}(k) = g(X_{it}, Z_{kt})$$

The goal of this paper is to estimate the expected difference in $h_{it}(k)$ and $h_{it}(0)$ for participant/job pairs created by subsidized employment programs (SEPs),

$$\tau = E_{X_{it}, Z_{kt}}[h_{it}(k) - h_{it}(0)], \ (i, k, t) \in S$$

where $(i, k, t) \in S$ implies that at time t individual i participated in a SEP, through which she was employed in job k. The parameter τ is the expected gain in happiness the average participant in a SEP experiences at a given point in time from being employed in the SEP rather than being unemployed.

As mentioned in the introduction, this is of course not the total effect of subsidized

employment programs on the happiness of their participants. First, some participants would have found employment even without the SEP and, second, SEPs may affect future happiness by changing the probability and characteristics of future employment. Still, τ is useful for evaluating the effect of employment subsidies on happiness. Regardless of whether some participants would have found jobs without the SEP, economic theory suggests that an employment subsidy creates jobs in equilibrium. Assuming that the characteristics of the job/employee pairs created by a subsidy in equilibrium are the same as the characteristics of the job/employee pairs in SEPs, τ yields the effect of the average job created by the subsidy. This could be combined with an estimate of the number of jobs created by an employment subsidy to yield an estimate of the subsidy's aggregate effect of happiness.

It should also be noted that τ is an "average effect of treatment on the treated", since it measures the effect of employment in SEPs on individuals who participate in them. Thus τ is the expected effect of employment in SEPs on individuals who are involuntarily unemployed - meaning those who are willing to accept a low-paying job in a SEP - and not the effect on the average person in the population.

3.2 A Matching Estimator for the Effect of Subsidized Employment on Happiness

To estimate τ, I use the nearest neighbor matching estimator described by Abadie and Imbens (2002). Since we can observe individuals' happiness while employed in a SEP, $h_{it}(k)$, the matching estimator only needs to estimate their counterfactual happiness while unemployed, $h_{it}(0)$. This is imputed from the outcomes of matched unemployed non-participants with similar observed characteristics:

$$\widehat{h_{it}}0) = \frac{1}{M} \sum_{(j) \in J_M(i,t)} h_{jt}(0)$$

In this notation $J_M(i,t)$ is the set of matched control observations associated with par-

ticipant i at time t. Matched controls are selected so that their observed characteristics in the pre-treatment period X_{jt-1} are as similar as possible to the observed characteristics of the participant in the pre-tretament period X_{it-1}[4]. More precisely, $J_M(i,t)$ is defined as containing the M observations with the smallest distance between X_{jt-1} and X_{it-1}, using a suitable metric, so that observations are matched to their nearest neighbors in the space of observed characteristics. For this paper, I use the standard distance metric $(X_{jt-1} - X_{it-1})'\Sigma^{-1}(X_{jt-1} - X_{it-1})$, where Σ is the covariance matrix of X.

Subsidized employment projects usually last for 12 months, which is the same as the average interval between two interviews for the German Socio-Economic Panel. The majority of participants is therefore observed only once per employment spell in a SEP [5].

For the baseline estimates, observations are matched on 11 variables: sex, age, years of education, marital status, household size, number of children, unemployment status, household income, income from public unemployment benefits, region[6] and month of interview. In an extended specification, observations are also matched on pre-treatment happiness in order to control for unobserved determinants of happiness.

3.2.1 Testing the conditional independence assumption

The matching estimator's main identifying assumption is that, conditional on the pre-treatment variables used for matching, X_{it-1}, the counterfactual outcome $h_{it}(0)$ is independent of participation in a SEP. This assumption ensures that the actual outcome of the unemployed matched non-participants $h_{jt}(0)$ is a consistent estimator of the counterfactual outcome of the participants under unemployment, $h_{it}(0)$. It implies that participants in subsidized employment projects would have been as (un)happy being unemployed as the

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[4]Observations are matched on characteristics in the pre-treatment period in order to avoid that participants' characteristics are already affected by the treatment

[5]In some cases SEPs are extended beyond 12 months so that we observe particpants more than once during the project. To avoid problems from endogenous duration of employment, the matching estimator only uses participants' first observation during a SEP. Results that use all observations within a SEP are not reported, but are similar to the reported ones.

[6]Western or Eastern Germany

matched non-participants who actually were unemployed.

There are two reasons why this assumption might be violated. First, happier people may be more likely to participate in SEPs, so that participants may have been more happy than non-participants even in the absence of the project. If this were the case, $h_{it}(0)$ would be greater than $h_{jt}(0)$ and the matching estimator of τ would be biased upward. Fortunately, the panel nature of the data allows me to test for this violation by comparing the pre-treatment happiness of participants, h_{it-1}, to the happiness of matched controls in the pre-treatment period, h_{jt-1}. If happier individuals self-select into the project we would expect participants to already be happier than matched controls in the pre-treatment observation, so that h_{it-1} would be greater than h_{jt-1}. On the other hand, observing that h_{it-1} is equal to h_{jt-1}, even for observations close to the the start of the SEP, should increase our confidence that happier individuals do not self-select into the projects.

Second, since participation in SEPs is voluntary [7], people may self-select into the projects according to how much they benefit from them. Participants and matched controls may therefore differ in how strongly their happiness is affected by unemployment, so that $h_{it}(0)$ may be different from $h_{jt}(0)$ even if participants and controls were equally happy when being employed in the pre-treatment observation. To test for this, I compare the pre-treatment happiness of participants and matched controls who were unemployed in the pre-treatment observation - $h_{it-1}(0)$ and $h_{jt-1}(0)$. Finding that participants and matched controls report different levels of happiness when unemployed, or that their happiness during unemployment follows different trends, would indicate that the groups are differently affected by unemployment and that the conditional independenc assumption is violated. Finding no difference in pre-treatment levels and trends of happiness between unemployed participants and matched controls should increase our confidence that both groups are equally affected by unemployment and that the conditional independence assumption holds.

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[7]Though repeated refusal to participate can be lead to sanctions by the job-center.

3.3 Disentangling the effects of income and employment

In principle, there are two ways in which employment in a SEP might affect happiness: by conferring direct psychological benefits - for example feelings of competence and efficacy - and by increasing individuals' incomes. To inform policy, it is useful to disentangle these two channels. A happiness-based argument for publicly subsidized jobs, similar to the one made by Edlin and Phelps (2009), only holds if employment itself increases happiness. If the employed are merely happier because of their higher incomes, direct income transfers are likely to be a more cost-effective way of increasing the happiness of the unemployed.

I therefore present an estimator for the "pure" effect of employment on happiness, net of the effect of increased incomes. Slightly modifying the notation of the previous section, I define $h_{it}(0, k)$ as the happiness that individual i reports at time t if she is unemployed but her income is as high as if she were employed in job k. Using this notation, the net effect of subsidized employment on happiness can be written as

$$\theta = E_{X_{it}, Z_{kt}}[h_{it}(k) - h_{it}(0, k)], \ (i, k) \in S$$

where, as before, S is the set of job/employee pairs created through SEPs. Unfortunately, θ is not easily identified without additional assumptions. Comparing participants and non-participants with similar post-treatment levels of income - either by matching on post-treatment income, or controlling for it in a regression - would not cleanly identify the effect. Since participation in a SEP has a positive effect on wage income, participants and non-participants can only have identical incomes if they differ in unobserved variables. Comparing participants and matched controls with similar incomes therefore risks introducing omitted variable bias (see, for example, Gelman and Hill 2007, pp 188-194).

To allow identification of θ, I assume that the effect of income (Y_{it}) follows a logarithmic functional form and is linearly separable from the effects of individual and job characteristics $(X_{it}$ and $Z_{it})$. Thus, the happiness of employed and unemployed individuals is given by:

$$h_{it}(k) = g(X_{it}, Z_{kt}) + log(Y_{it})\gamma + u_{it}$$

$$h_{it}(0) = f(X_{it}) + log(Y_{it})\gamma + u_{it}$$

so that θ can be written as:

$$\theta = E_{X_{it}, Z_{kt}}[g(X_{it}, Z_{kt}) - f(X_{it})], \ (i, k) \in S$$

where X_{it} and Z_{it} now exclude income.

I estimate θ in two ways. First, I estimate a fixed effects regression of happiness that includes an indicator for being employed in a SEP and controls for income. The estimated equation is

$$h_{it} = \delta_1 D_{it}^{reg} + \delta_2 D_{it}^{SEP} + X_{it}\beta + log(Y_{it})\gamma + \alpha_i + u_{it}$$

where D^{reg} and D^{SEP} are indicators for being employed in a regular job and in a SEP. Under the identifying assumption that u_{it} is uncorrelated with employment in a SEP, $\widehat{\delta_2}$ is an unbiased estimate of θ. However, there are several reasons why this assumption may be violated. First, entry into and exit from SEPs is non-random, so that unobserved shocks may be correlated with employment in a SEP. This concern is similar to the one that was previously discussed in the context of the matching estimator for the aggregate effect of employment in a SEP. A concern that is specific to estimating the net effect of employment - excluding the effect of income and consumption - is that entry into subsidized employment may increase individuals' expectations of future income. This could lead individuals to increase their consumption as they enter a SEP, which may positively affect their happiness (alternatively, expected future income might have a direct effect if individuals receive happiness from anticipating future income). Thus, entry into a SEP may be correlated with unobserved shocks

to expected future income and consumption, which would bias the estimate of θ.

As a robustness test, I graphically examine the happiness of SEP participants at the end of the project. SEPs usually last for one year, and most participants go back into unemployment when they exit the project. Thus, one year after the start of the project, there is a sharp drop in participants' probability of employment. But their incomes do not immediately decrease since participation in a SEP extends their entitlement public unemployment benefits. Also, while participants' expectations of future income may increase as they enter a SEP, it is unlikely that their expectations decrease discontinuously exactly one year after the start of the project (since the duration of the project is known in advance). Thus, if employment affects happiness independently of income, happiness should drop one year after the start of a SEP, as employment ends while current and expected future income remain unchanged (or at least do not change discontinuously). If, on the other hand, the effect of SEPs on happiness is only due to their effect on income, we would not expect a drop in happiness one year after the start of the project.

In addition to the graphical test, I calculate a fixed effects instrumental variables estimator. This estimator exploits the fact that participant's probability of employment drops substantially one year after the start of a SEP while their incomes do not decline immediately. For this regression I use only observations of SEP participants after the start of a SEP in order to avoid bias from endogenous entry and from shocks to expected future income that may affect happiness at the start of a SEP. To avoid bias from endogenous exit from SEPs, I instrument employment by an indicator for an individual's first observation after entering a SEP. The first-stage relationship between this instrument and employment is created by the fact that the usual SEP lasts for one year. This is the same as the average interval between two observations in the German Socio-Economic Panel, so that the probability of employment drops significantly between the first and second observation after entering a SEP. The exclusion restriction rests on the assumption that SEP participants experience no systematic unobserved shocks between their first and second observation after entering a

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SEP. Additional robustness tests for this assumption are discussed in more detail in Section 5, together with the results.

4   Data and Summary Statistics

The empirical analysis in this paper uses data from the German Socio-Economic Panel (SOEP)[8], from the years 1992 to 2004. The sample is restricted to respondents between the ages of 18 and 65. The outcome of interest is respondents' self-reported happiness measured by their answer to the question: "All things considered, on a scale from 0 to 10, how satisfied are you with your life?" [9]. Answers to questions of this type correlate well with more detailed measures of psychological distress (Koivumaa-Honkanen et al., 2004) and physiological indicators of well-being such as blood-pressure (Blanchflower and Oswald, 2008). They also predict suicide risk and mortality (e.g. Koivumaa-Honkanen 2001, Chida 2008). The explanatory variable of interest is participation in subsidized employment projects (SEPs). From 1992 onwards, the SOEP collected information on whether respondents were currently employed in a SEP. Figure 2 shows that the sample estimate of the fraction of unemployed individuals who participate in SEPs closely follows the actual time of participation.

Table 1 reports summary statistics of the whole sample, of the unemployed and of individuals who participate in subsidized employment projects. For SEP participants, the table reports summary statistics in the observation before the project began, so that the results do not measure the effect of participation itself. To be comparable, the summary statistics for the unemployed are based on the lagged observation in which individuals may still have been employed. Columns 4 and 5 show differences in means between participants in SEPs and non-participants as well as between participants and the unemployed. Compared to the population as a whole, SEP participants live in larger households with lower incomes, are

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[8]See Wagner et al. (2007) or Frick et al. (2007) for detailed descriptions of the data

[9]As mentioned in the introduction, I follow Arrow and Dasgupta (2009) in using the term happiness as synonymous with life-satisfaction. I do this to make the text more readable: saying that employment makes people happy is a briefer way of saying that employment makes people more satisfied with their lives.

more likely to be female and have a steady partner and report lower levels of happiness. Compared to the unemployed, SEP participants are also younger and better educated. Clearly participation in SEPs is not random, even conditional on being unemployed, so that we should expect participants and non-participants to differ in observed as well as unobserved characteristics.

5   Results

5.1   The effect of subsidized employment on happiness

Table 2 reports results of matching estimators of τ, the average effect of subsidized employment projects (SEPs) on the happiness of their participants (which is the average effect of treatment on the treated). The matching procedure is described in detail in Section 3.1. Columns 1 and 2 of Table 2 report results from the whole sample of participants, while columns 3 and 4 report results from the sub-sample of participants who were unemployed in the pre-treatment observation.

For the baseline estimates, presented in columns 1 and 3, observations are matched on 11 pre-treatment variables: sex, age, years of education, marital status, household size, number of children, unemployment status, household income, income from public unemployment benefits, region [10] and month of interview. In addition, the estimators presented in columns 2 and 4 match on pre-treatment happiness in order to control for unobserved heterogeneity in factors that affect individuals' happiness. The first row reports the simple nearest neighbor matching estimate, the second row reports the estimate after correcting for potential bias from remaining differences in the control variables.

The results in Table 2 suggest that employment in SEPs has a large and statistically significant effect on participants' happiness falling in the range between 0.39 and 0.62 on the 0 to 10 scale - equivalent to between 0.3 and 0.5 within-individual standard deviations of

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[10]Western or Eastern Germany

self-reported happiness. The simple nearest neighbor estimates do not differ much from the bias adjusted estimates, which suggests that the matching procedure succeeded in selecting controls whose observed characteristics are similar to those of the participants they were matched to.

By looking at the pre-treatment differences in happiness, we can see that the matching procedure appears to work better for the sub-sample of participants that were unemployed in the pre-treatment observation, since their pre-treatment happiness is closer to that of the matched controls. This is most likely because participants who were employed in the pre-treatment observation are unusual in unobserved characteristics. As mentioned in Section 2, one of the formal pre-requisites for entering an SEP is to have been unemployed for 6 out of the preceding 12 months, though there are exceptions for special cases. Participants who were employed in the pre-treatment observation are less likely to fulfill the formal pre-requisite, so they are more likely to be drawn from the special cases that are assigned to SEPs through the discretion of the job-center and therefore more likely to have unusual unobserved characteristics. My preferred specifications are therefore the ones in columns 3 and 4 that are based on participants who were unemployed in the pre-treatment observation. For them, the estimated effect of employment in SEPs is slightly smaller, but still large (at around 0.4) and statistically significant.

5.2   Does the conditional independence assumption hold?

As explained in Section 3.2, the matching estimator's identifying assumption is that individual i's (possibly counterfactual) happiness when unemployed, $h_{it}(0)$, is independent of participation in a subsidized employment project, conditional on the matching variables. Intuitively, since the estimator uses matched obervations to estimate participants' counterfactual outcome if unemployed, the identifying assumption is that participants would have been as (un)happy being unemployed as the matched controls who in fact were unemployed. Since participation in SEPs is non-random, it is not obvious that this assumption holds. I

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therefore conduct the two robustness tests described in Section 3.2.

I first test whether happier individuals self-select into subsidized employment projects, perhaps because they are more motivated to work or because unobserved shocks - for example to health - affect both happiness and the probability of participation. If this were the case, participants would have been happier than matched controls even if they had remained unemployed and the matching estimator would be biased upward. As a robustness test, I compare the happiness of matched controls and participants in the year before they enter the subsidized employment projects. Column 1 in Table 2 shows that the average pre-treatment happiness of participants is slightly lower than that of the matched controls and that the difference is not statistically significant, suggesting that there is no self-selection of happier individuals SEPs[11] But average pre-treatment differences are not the only concern. If unobserved shocks increase both happiness and the probability of entering a project, we would expect the happiness of participants to increase relative to that of non-participants right before the project begins. Thus, despite their slightly lower average happiness in the pre-treatment observation, participants may have been happier than matched controls at the time they entered the project. As a robustness test for this, Figure 3 plots the average happiness of participants and matched controls in the 12 months before and after the start of employment in a SEP. The plots are constructed as follows: For participants, I use information on the start date of employment in a SEP and the interview date to calculate how many months before or after the beginning of the project an interview took place. For the post-treatment observation, matched controls are plotted at the same time-coordinate as the participants they are matched to. I then use the time since the matched individual's previous interview to calculate the time-coordinate at which her pre-treatment observation is plotted. Since the intervals between two interviews are not fixed, participants and their matches are therefore not necessarily plotted at the same time-coordinate in the

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[11]Surprisingly, the only estimator in which pre-treatment happiness of participants and matched controls differs significantly is the one that matched on pre-treatment happiness. However, this is only due to the fact that matching on pre-treatment happiness decreased the standard error of the difference in happiness, so that the estimate is more precise.

pre-treatment period. Still, this procedure makes sure that the pre-treatment time-trend is correctly observed, since the controls' pre-treatment observations are plotted as many months away from the beginning of the project as they would have been if they had entered it at the same time as the participant they are matched to. Consistent with the average difference reported in Table 2, the top panel in Figure 3 shows that the pre-treatment happiness of matched controls is slightly higher than that of the participants. Moreover, participants are less happy than matched controls even right before the start of the project. This observation, as well as the fact that participants' happiness is decreasing in the pre-treatment period but starts to increase right at the start of the project, suggests that the results are not driven by self-selection of happier individuals into the projects.

As a second robustness test, I test whether participants and matched controls differ in how strongly their happiness is affected by unemployment. Since participation is largely voluntary, people are likely to self-select into the projects according to how much they benefit from them. Participants' (counterfactual) happiness when unemployed may therefore be different from that of the unemployed matched controls, which would violate the conditional independence assumption. To test for differences in happiness under unemployment, Column 3 in Table 2 reports differences in the pre-treatment happiness of participants and matched controls who were unemployed in the pre-treatment observation. The point estimate suggests that participants are slightly less happy being unemployed than matched controls, but the difference is very small and not statistically significant. In addition, the bottom panel in Figure 3 shows that the pre-treatment trends in happiness are virtually identical for unemployed future participants and matched controls, giving no evidence that participants adapt more quickly to unemployment than matched controls.

Taken together these results suggest that there are no substantial violations of the conditional independence assumption, so that the matched controls yield a good counterfactual for the happiness participants would have experienced if they had remained unemployed. This is particularly true for participants who were unemployed in the pre-treatment observation,

who are the basis for my preferred specification. The matching estimates therefore suggest a positive causal effect of subsidized employment on happiness.

5.3   Disentangling the effects of employment and income

The matching estimators presented in the previous section measure τ, the effect of SEPs on the happiness of individuals who would otherwise have remained unemployed. But as mentioned above, participation in a subsidized employment project has at least two consequences: participants are employed and receive higher incomes. In order to inform policy, it is important to know through which channel - employment or income - SEPs affect happiness. A happiness-based argument for publicly subsidized jobs, similar to the one made by Edlin and Phelps (2009), only holds if employment *per se* increases happiness. If participants in SEPs are only happier because of their higher incomes, increased income transfers would most likely be a more cost-effective way of increasing the happiness of the unemployed. This section presents graphical evidence and quantitative results from fixed effects and instrumental variables estimators, which all suggest that the effect of SEPs on happiness is due to direct psychological benefits and cannot be explained by the higher incomes of the employed alone. The evidence is based on the fact that participation in a SEP prolongs individuals' entitlement to public unemployment benefits. Thus the projects' positive effect on income remains even after employment in it has ended. This creates sufficient independent variation in income and employment to allow me to identify the effect of employment while controlling for differences in income.

Figure 4 plots the trends of employment, income and happiness around the start of SEPs. In the first year after entering an SEP, participants are employed in the project and are substantially happier than the unemployed matched controls. Since the duration of SEPs is usually limited to 12 months, most participants leave employment in the subsequent year and are as likely to be employed as matched controls. But since participation in a SEP prolongs individuals' entitlement to unemployment benefits, participants' average income

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remains higher than that of matched controls. If income were responsible for the projects' effect, we would expect participants to be significantly happier than matched controls until the difference in incomes disappears. But the plot shows that the projects' positive effect on happiness disappears in the second year after their start, at the same time as employment in the project ends for most participants, suggesting that the effect is due to the psychological benefits of employment *per se* and not due to participants' increased incomes.

To obtain quantitative estimates of θ - the pure effect of subsidized employment net of the effect of increased income - I use the estimators described in Section 3.3. The simple fixed effects estimator reported in Table 3 shows that the correlation between participation in SEPs and happiness remains strong and significant even after controlling for income (both current and future) and unobserved fixed characteristics. The estimated effect is similar in size to the matching estimates reported in Table 2. The estimated effect of employment in SEPs is slightly smaller than that of employment in a regular job, wich is likely due to unobserved heterogeneity in job characteristics.

Tables 4 and 5 report results from the fixed effects instrumental variables estimator described in Section 3.3. As described in that section, the estimator uses an indicator for an individual's first observation in a SEP as an instrument for employment in order to control for endogenous exit from SEPs. The instrument exploits the fact that the usual duration of SEPs is 12 months, so that participants' probability of employment drops significantly between their first and second observation after entering a SEP [12].

However, as shown in Figure 4, participants' expected incomes do not immediately decrease as employment ends, because participation prolongs their entitlement to payments through the public unemployment insurance. This creates independent variation in employment and income, which makes it possible to use the decrease in the probability of employment after 12 months as an instrument for employment while still controlling for income.

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[12]The average interval between observations in the German Socio-Economic Panel is 12 months.

As explained in Section 3.3, I restrict the sample for the fixed effects IV estimator to SEP participants and use only observations made after the start of a SEP. Observations before the start of a SEP are dropped in order to avoid endogeneity bias stemming from unobserved shocks that simultaneously increase happiness and the probability of entering a SEP. To reduce noise from unobserved time-trends, I limit the sample to the first observation after the project's start and the 5 subsequent ones. For participants with multiple spells of employment in a SEP, each spell is treated separately. That is, the first observation in a SEP spell is used as an instrument for employment and the 5 subsequent observations are included in the analysis, regardless of whether the individual enters another SEP during that time. This makes sure that the estimates are not affected by repeated endogenous entry into SEPs. It does, however, have the consequence that some observations are "double-counted", if an individual enters more than one SEP in a 5 year period. To make sure that this double-counting does not lead me to over-state the precision of the estimates, the reported standard errors are clustered at the individual level.

The first-stage results, reported in Table 4 show that the probability of employment drops between 52 and 59 percentage points between the first observation after entering a SEP and later observations, an effect that is large and statistically significant. The 2-stage least squares estimates in Table 5 show that employment in SEPs has a large and statistically significant effect on happiness, even after controlling for income, both current and future. Ranging between 0.39 and 0.50, the estimated effect is large compared to the within-individual standard deviation of 1.32 and compared to the "effects" of the control variables. The next subsection discusses the identifying assumptions of the instrumental variables estimator in more detail and presents robustness tests for them. The subsequent sections discuss whether increased consumption or misreporting of happines can explain the results.

5.3.1 Robustness tests for the fixed effects IV estimator

The identifying asumption for the IV estimator is that the instrument is uncorrelated with the error term. In the present context, this means that there can be no systematic unobserved shocks that affect happiness between individuals' first and second observation after entering a SEP. This assumption is likely to hold, since unobserved shocks that occur after the start of a SEP are likely to be evenly distributed over time, so there is no reason to believe that they would affect happiness in the first year differently than in the following years. One concern is that systematic unobserved shocks occur before the start of the program (perhaps because these shocks increase the probability of participation) whose effect persists in the first year of the project and wear off in later years. If this were the case, the instrumental variables estimate would be biased. Reassuringly, the results in Section 5.2 suggest that there are no systematic unobserved shocks to happiness in the run-up to entering a SEP, since the time trends of happiness of participants and matched controls are almost identical in the year before entering the SEP (shown in Figure 4). To further control for persistent pre-project shocks, the models in columns 3 and 4 in Table 5 include a time-trend that begins with the start of the SEP. If pre-project shocks increase the happiness of participants at the beginning of the project, their effects should wear off over time, so that we expect happiness to decrease after the start of the project. Assuming that these (potential) shocks wear off gradually, and not discontinuously between the first and second year after entering a SEP, their effects can be controlled for by a time-trend. The estimates in columns 3, and 4 of Table 5 should therefore identify the causal effect of employment even in the presence of systematic unobserved shocks to happiness before the start of the program. Additional robustness tests for the exclusion restriction are discussed below.

The exclusion restriction also implies that individuals have to be as happy being employed in SEPs as they are in the jobs they hold in subsequent years. A concern is that regular jobs and jobs in SEPs differ in their effect on happiness due to differences in unobserved characteristics, which would violate the exclusion restriction. As a robustness test, I test

whether the individuals in the sample are as happy when employed in the first year after the start of a SEP as they are when employed in subsequent years. The regression results in Table 6 support the exclusion restriction since there are only small differences between the effect of employment in the first year after the start of a SEP and later years.

The second implication of the exclusion restriction is that individuals would have been as happy being unemployed in the first year after the start of a SEP as they are being unemployed in later periods. Unfortunately, I cannot test this condition in the same way that I tested equality of outcomes under employment since all individuals are employed in the first observation after the start of a SEP. However, it is less likely that this condition is violated since there is less heterogeneity in the situation of the unemployed than in the situation of the employed. One potential violation would occur if individuals adapt to unemployment, so that they are happier being unemployed in later years. As a robustness test, I test for a time trend in the happiness of individuals who are unemployed following a spell in a SEP (t >1). Finding a time-trend would suggest that the effect of unemployment on happiness is changing over time so that the exclusion restriction would be violated. The results in Table 7 show that this is not the case. The interaction of unemployment and time has only a very small and statistically insignificant effect on happiness, which suggests that the effect of unemployment is stable over time, so that the second condition of the exclusion restriction is satisfied.

Taken together, the results of the robustness tests suggest that the instrumental variables estimator is an unbiased estimator of the Local Average Treatment Effect - the effect of subsidized employment (net of the effect of income) on individuals who participate in a subsidized employment project and are unemployed at some point within 5 years after the project's start.

5.4 Can changes in consumption or expected future income explain the results?

The instrumental variables estimators presented in columns 1 through 3 of Table 4 estimate the effect of employment on happiness while controlling for the effect of current income. But this may not be enough to to isolate the pure psychological effect of employment. If individuals rationally maximize lifetime utility, their current consumption is a function of their expected lifetime income (Friedman, 1957). Thus, if employment in SEPs increases expected lifetime income, it may affect individuals' happiness by increasing their consumption. An increase in expected lifetime income might also increase happiness directly if individuals gain happiness from anticipating future income. To rule these channels out, the model in column 4 of Table 4 adds respondents' average income in all future observations as an additional control variable. In addition, the time trend from the start of the SEP should control for the shock to consumption that comes with starting employment in a SEP. As mentioned in the previous sub-section, this trend controls for shocks that occur at (or before) the start of the SEP and wear off gradually. If individuals conform to the Permanent Income Hypothesis, their consumption should increase discontinuosuly as they are offered a job in a SEP, since this constitutes a shock to their expected future income. However, in later periods, their consumption should decline gradually [13], so that the time trend should control for the effect of declining consumption. The results in column 4 of Table 4 show that estimated effect of future income is strongly positive, and that the time trend from the start of a SEP is negative, though neither of them is statistically significant. These results are consistent with the hypothesis that future income affects happiness either through consumption or anticipation and that part of the effect of SEPs operates through this channel. But even after controlling for this channel, the remaining effect of employment is large and statistically significant. This result suggests that employment has psychological benefits that are independent of its

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[13]In fact, if individuals have quadratic utility over consumption, their expected consumption should follow a linear trend (Hall, 1978).

effects on income and consumption.

5.5 Can misreporting of happiness explain the results?

A vital concern when studying self-reported happiness is whether answers to questions like "how satisifed are you with your life?" measure well-being in a meaningful way. One reassuring finding is that self-reported life-satisfaction correlates well with more detailed measures of psychological distress (Koivumaa-Honkanen et al., 2004) and predicts objective outcomes like suicide and mortality (e.g. Koivumaa-Honkanen 2001, Chida 2008). Still, in specific cases there could be systematic misreporting of life-satisfaction due to social norms. In many cultures work is seen as a valuable and central aspect of life, so that respondents may be reluctant to admit being happy while unemployed. It is therefore possible that the unemployed under-report their happiness compared to the employed, which would bias the estimated effect of employment upward.

While I cannot fully rule out that unemployed individuals misreport their happiness relative to those in subsidized employment projects, there are several reasons to believe that the effect of misreporting is small. First, the life-satisfaction question is the last question in a long multi-purpose survey (the German Socio-Economic Panel), while the questions about employment are asked in the first half of the survey. Respondents are therefore not "primed" on their employment status when answering the life-satisfaction question. In addition, respondents are not aware that their answers will be used to study the effect of employment on happines, which should further reduce misreporting due to social norms. Further evidence against misreporting comes from the data. If the unemployed underreport their happiness for reasons of social acceptability, we would expect to see a sharp increase in reported happiness at the start of the SEP. But as shown in Figure 3, happiness initially remains low and increases over the course of the project; a pattern that is not easily explained by misreporting due to socially preferred answers [14].

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[14]The upward trend is more plausibly explained by a gradual and cumulative effect of employment on

6 Conclusion

This paper tries to answer two questions: does unemployment make people unhappy and, if yes, can subsidized employment increase people's happiness? Its findings, based on data from the German Socio-Economic Panel, suggest that the answer to both questions is "yes". A matching estimator suggests that participants in subsidized employment projects (SEPs) are substantially happier than they would have been if they had remained unemployed. Panel data on pre-project happiness suggests that this effect is not due to self-selection of happier individuals into the projects. The data further suggest that the increase in income that comes with subsidized employment does not explain the effect. In the German context, participation in a subsidized employment project prolongs participants' entitlement to public unemployment benefits, so that their average income does not decrease after the project ends, even though 60% of participants become unemployed. Yet happiness sharply decreases after the project ends, suggesting that most of the previous increase in happiness was due to the projects' effect on employment and that only a small fraction, if any, can be explained by their effect on income. Taken together, the results presented in the paper suggest that subsidized employment can have a large positive effect on the happiness of individuals who would otherwise be unemployed.

The paper's results are relevant for two reasons. First, they constitute conclusive evidence for a causal effect of unemployment on happiness. While previous studies (e.g. Clark and Oswald 1994, Winkelmann and Winkelmann 1998, Marks and Fleming 1999, Clark 2003, Carroll 2007) found correlations between (changes in) unemployment and happiness, they were unable to rule out that the correlation was due to reverse causality from happines to unemployment, or caused by unobserved shocks - for example to health - that simultaneously decrease happiness and increase the probability of unemployment. By showing that the effect of subsidized employment projects on happiness is not due to self-selection of happier

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happiness. For example, if part of the psychological benefit of employment comes from the social ties to ones co-workers, we would expect happiness to increase as these social ties strengthen over time.

individuals into the projects, the current paper provides strong evidence that the effect of unemployment on happiness is causal. Second, the results have implications for labor market policy. For some time, economists (e.g. Edlin and Phelps 2009, Phelps 1994, Katz 1996) have argued that subsidies for low-wage jobs should replace traditional transfer-based welfare policy and several countries (most notably France, but also the Netherlands and the UK) have introduced subsidies of this type. Recently, Edlin and Phelps (2009) have cited potential psychological benefits of employment as an additional argument for subsidising low-wage jobs. The main finding of this paper - that subsidized employment can increase people's happiness directly and not just by increasing their incomes - gives empirical support to their argument.

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Table 1: Summary Statistics

	Means (in previous observation)			Differences	
	Whole Sample	Unemployed	SEP Participants	Participants to Non-Participants	Participants to Unemployed
Female	0.510	0.521	0.591	0.082	0.070
	(0.500)	(0.500)	(0.492)	[0.029]**	[0.027]**
Age	40.6	42.1	40.5	-0.08	1.65
	(12.7)	(12.4)	(10.6)	[0.60]*	[0.58]
Steady Partner	0.642	0.602	0.656	0.014	0.055
	(0.479)	(0.491)	(0.476)	[0.027]	[0.025]**
Household Size	2.97	2.87	3.19	0.22	0.312
	(1.20)	(1.23)	(1.26)	[0.07]***	[0.068]***
HH Income (Euros/month)	2762	2112	2012	-753	-99.9
	(1513)	(1092)	(945)	[52]***	[50.5]**
Years of education	11.9	11.2	11.7	-0.17	0.46
	(2.4)	(1.9)	(2.0)	[0.13]	[0.12]***
Self-reported happiness	6.86	5.89	5.44	-1.43	0.44
	(1.75)	(2.00)	(2.06)	[0.11]***	[0.11]***
Observations	90185	6236	413	90185	6649
Individuals	11366	2605	329	11366	2649

Data source: German Socio-Economic Panel, 1992-2004. Values for SEP participants are from the pre-treatment observation. For comparison, values for the whole sample and the unemployed are from the lagged observation. Standard deviations in parentheses. Standard errors of differences in brackets. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels.

Table 2: Effect of Subsidized Employment Projects on Happiness: Matching Estimators

Effects on:	All participants		Unemployed in pre-treatment obs.	
	(1)	(2)	(3)	(4)
Happiness	0.485	0.543	0.389	0.390
	(0.112)***	(0.0101)***	(0.136)***	(0.119)***
Happiness (bias adjusted)	0.531	0.620	0.430	0.434
	(0.112)***	(0.098)***	(0.136)***	(0.113)***
Pre-Treatment Differences:				
Happiness	-0.119	-0.125	-0.031	-0.032
	(0.121)	(0.053)**	(0.141)	(0.061)
Happiness (bias adjusted)	-0.092	-	-0.008	-
	(0.120)		(0.140)	
Matched on pre-treatment happiness	No	Yes	No	Yes
Number of SEP spells	413	413	296	296

Data source: SOEP, 1992-2004. Standard errors in parenthesis. ***,** and * denote statistical significance at the 1, 5 and 10 % level. Estimates are based on individuals' first observation in an employment-spell in a SEP and 3 matched observations. For the baseline matching, observations are matched on: sex, age, years of education, relationship status, household size, number of children, household income, unemployment status, household income from unemployment benefits, region (Western/Eastern Germany) and month of interview. To avoid reverse causality, observations are matched on values in the pre-SEP observation.

Table 3: Employment vs. Income: Fixed Effects Estimates

	Dependent Variable:Happiness		
	(1)	(2)	(3)
Employed in regular job	0.56	0.51	0.49
	(0.03)***	(0.03)***	(0.03)**
Employed in SEP	0.45	0.43	0.40
	(0.08)***	(0.08)***	(0.08)***
Not seeking employment	0.28	0.24	0.26
	(0.04)***	(0.04)***	(0.04)***
Log Household Income		0.23	0.21
		(0.03)***	(0.03)***
Log Inc. from Unemp. Benefits		-0.021	-0.020
		(0.003)***	(0.005)***
Log Avg. Future Income			0.40
			(0.07)***
Age		-0.033	-0.023
		(0.004)***	(0.005)
Education (years)		0.005	0.006
		(0.016)	(0.017)
Lives with partner		0.18	0.20
		(0.05)***	(0.05)
Household Size		-0.061	-0.078
		(0.023)***	(0.025)***
Number of children		0.095	0.090
		(0.027)***	(0.029)***
Eastern Germany		-0.37	-0.29
		(0.11)***	(0.12)**
Constant	6.03	5.78	2.48
	(0.04)	(0.34)	(0.66)
Number of observations	34911	34911	30352
Number of individuals	4892	4892	4462

Data source: SOEP, 1992-2004. *, ** and *** denote statistical significance at the 10, 5 and 1 % levels. All models include individual and year fixed effects. Standard errors are clustered at the individual level. The baseline employment status is unemployed and looking for work.

Table 4: Instrumental Variables Estimates: First Stage

	Dependent Variable: Employed			
	(1)	(2)	(3)	(4)
First observation after start of SEP	0.52	0.54	0.58	0.59
	(0.02)***	(0.02)***	(0.03)***	(0.03)**
Log Household Income		0.070	0.073	0.106
		(0.035)**	(0.036)**	(0.039)
Log Inc. from Unemp. Benefits		-0.024	-0.024	-0.023
		(0.003)***	(0.003)***	(0.004)***
Log Avg. Future Income				0.24
				(0.11)**
Age		-0.0003	-0.0066	0.00042
		(0.0063)	(0.0066)	(0.0066)
Education (years)		0.017	0.015	0.009
		(0.031)	(0.031)	(0.036)**
Lives with partner		0.055	0.056	0.061
		(0.069)	(0.069)	(0.076)
Household Size		-0.063	-0.063	-0.072
		(0.027)**	(0.028)**	(0.030)**
Number of children		0.013	0.008	0.017
		(0.039)	(0.040)	(0.040)
Eastern Germany		-0.50	-0.49	-0.48
		(0.15)***	(0.15)***	(0.17)***
t (years after start of SEP spell)			0.023	0.024
			(0.023)	(0.008)***
Constant	0.41	0.38	0.59	-1.66
	(0.05)	(0.54)	(0.53)	(1.09)
Number of observations	2493	2493	2493	2216
Number of individuals	406	406	406	371

Data source: German Socio-Economic Panel, 1992-2004. *, ** and *** denote statistical significance at the 10, 5 and 1 % levels. All models include individual and year fixed effects. Standard errors are clustered at the individual level (using a clustered bootstrap with 500 replications). The sample only contains SEP participants and is restricted to individuals' first 6 observations after the start of a SEP.

Table 5: Instrumental Variables Estimates: Second Stage

	Dependent Variable: Happiness			
	(1)	(2)	(3)	(4)
Employed	0.44	0.53	0.45	0.40
	(0.15)***	(0.14)***	(0.15)***	(0.16)**
Log Household Income		-0.04	-0.04	-0.04
		(0.15)	(0.15)	(0.18)
Log Inc. from Unemp. Benefits		-0.030	-0.032	-0.032
		(0.011)***	(0.011)***	(0.011)***
Log Avg. Future Income				0.53
				(0.34)
Age		0.002	0.008	0.009
		(0.025)	(0.027)	(0.028)
Education (years)		0.22	0.22	0.23
		(0.10)**	(0.10)**	(0.11)**
Lives with partner		-0.04	-0.04	0.09
		(0.26)	(0.26)	(0.34)
Household Size		0.07	0.06	0.002
		(0.10)	(0.10)	(0.10)
Number of children		0.17	0.17	0.19
		(0.13)	(0.13)	(0.13)
Eastern Germany		-1.44	-1.48	-1.14
		(0.74)*	(0.74)**	(0.71)
t (years after start of SEP spell)			-0.021	-0.023
			(0.023)	0.024
Constant	5.31	4.12	3.95	-0.28
	(0.17)	(2.12)	(2.13)	(3.81)
Number of observations	2493	2493	2493	2216
Number of individuals	406	406	406	371

Data source: German Socio-Economic Panel, 1992-2004. *, ** and *** denote statistical significance at the 10, 5 and 1 % levels. All models include individual and year fixed effects. Standard errors are clustered at the individual level (using a clustered bootstrap with 500 replications). Employment is instrumented by an indicator for the first observation after the start of an employment spell in a subsidized employment project (SEP). The sample only contains SEP participants and is restricted to individuals' first 6 observations after the start of an SEP.

Table 6: Robustness tests: changes in the effect of employment over time

|	Dependent variable: Happiness			
	(1)	(2)	(3)	(4)
Employed	0.45	0.47	0.44	0.40
	(0.09)***	(0.09)***	(0.10)***	(0.11)***
Employed in periods t >1	0.017	-0.061	-0.018	-1.8*10-05
	(0.08)	(0.085)	(0.11)	(0.11)
Log Household Income		-0.033	-0.036	-0.045
		(0.15)	(0.15)	(0.17)
Log Inc. from Unemp. Benefits		-0.032	-0.033	-0.032
		(0.011)***	(0.010)***	(0.011)***
Log Avg. Future Income				0.53
				(0.35)
Age		0.002	0.008	0.009
		(0.025)	(0.027)	(0.029)
Education (years)		0.22	0.22	0.23
		(0.12)*	(0.11)*	(0.10)
Lives with partner		-0.034	-0.035	0.089
		(0.27)	(0.28)	(0.31)
Household Size		0.061	0.062	0.002
		(0.10)	(0.10)	(0.10)
Number of children		0.17	0.17	0.19
		(0.12)	(0.11)	(0.13)
Eastern Germany		-1.49	-1.49	-1.14
		(0.71)**	(0.74)**	(0.63)*
t (years after start of SEP spell)			-0.021	-0.023
			(0.025)	(0.026)
Constant	5.29	4.16	3.97	-0.28
	(0.17)	(2.18)	(2.06)	(3.63)
Number of observations	2493	2493	2493	2216
Number of individuals	406	406	406	371

Data source: German Socio-Economic Panel, 1992-2004. *, ** and *** denote statistical significance at the 10, 5 and 1 % levels. All models include individual and year fixed effects. Standard errors are clustered at the individual level (using a clustered bootstrap with 500 replications). The sample only contains SEP participants and is restricted to individuals' first 6 observations after the start of a SEP.

Table 7: Robustness tests: adaptation to unemployment

	Dependent variable: Happiness			
	(1)	(2)	(3)	(4)
Not employed	-0.42	-0.38	-0.41	-0.38
	(0.11)***	(0.11)***	(0.12)***	(0.12)***
Not employed $\times$ t	-0.016	-0.022	-0.006	-0.009
	(0.033)	(0.032)	(0.039)	(0.040)
Log Household Income		-0.039	-0.038	-0.046
		(0.14)	(0.14)	(0.17)
Log Inc. from Unemp. Benefits		-0.031	-0.032	-0.032
		(0.010)***	(0.010)***	(0.011)***
Log Avg. Future Income				0.53
				(0.32)*
Age		-9.6*10-4	0.007	0.008
		(0.025)	(0.027)	(0.030)
Education (years)		0.22	0.22	0.23
		(0.11)*	(0.12)*	(0.11)**
Lives with partner		-0.032	-0.034	0.090
		(0.27)	(0.27)	(0.32)
Household Size		0.063	0.062	0.002
		(0.097)	(0.10)	(0.11)
Number of children		0.17	0.17	0.19
		(0.12)	(0.12)	(0.12)
Eastern Germany		-1.45	-1.48	-1.14
		(0.76)*	(0.81)*	(0.68)*
t (years after start of SEP spell)			-0.021	-0.021
			(0.024)	(0.025)
Constant	5.78	4.78	4.44	0.15
	(0.17)	(2.18)	(2.16)	(3.50)
Number of observations	2493	2493	2493	2216
Number of individuals	406	406	406	371

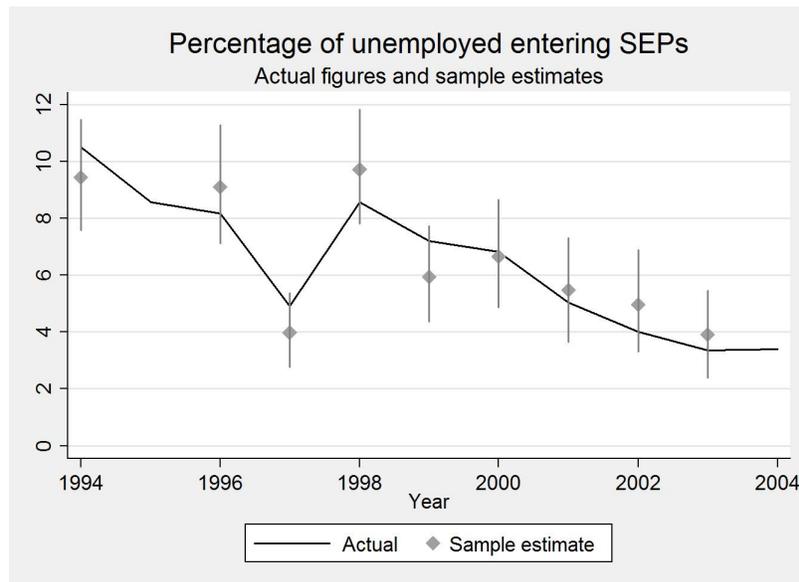Data source: German Socio-Economic Panel, 1992-2004. *, ** and *** denote statistical significance at the 10, 5 and 1 % levels. All models include individual and year fixed effects. Standard errors are clustered at the individual level (using a clustered bootstrap with 500 replications). The sample only contains SEP participants and is restricted to individuals' first 6 observations after the start of a SEP.

Figure 1: Prevalence of Subsidized Employment Projects
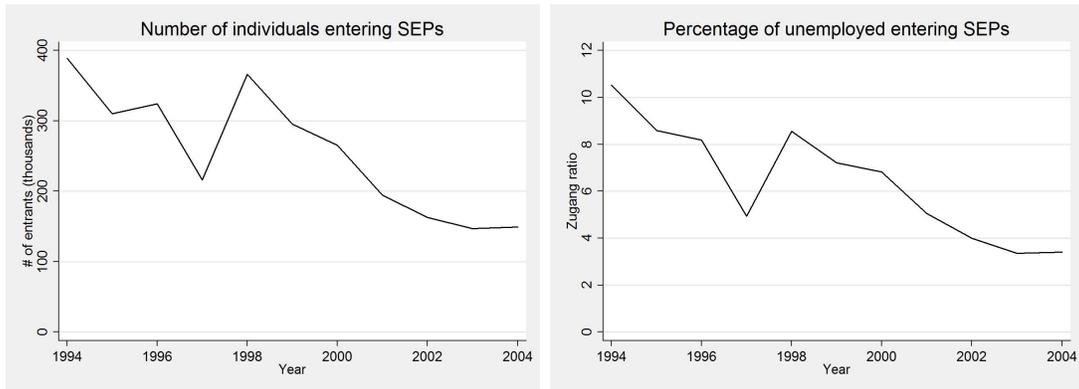
Figure 2: Percentage of Unemployed in Subsidized Employment Projects
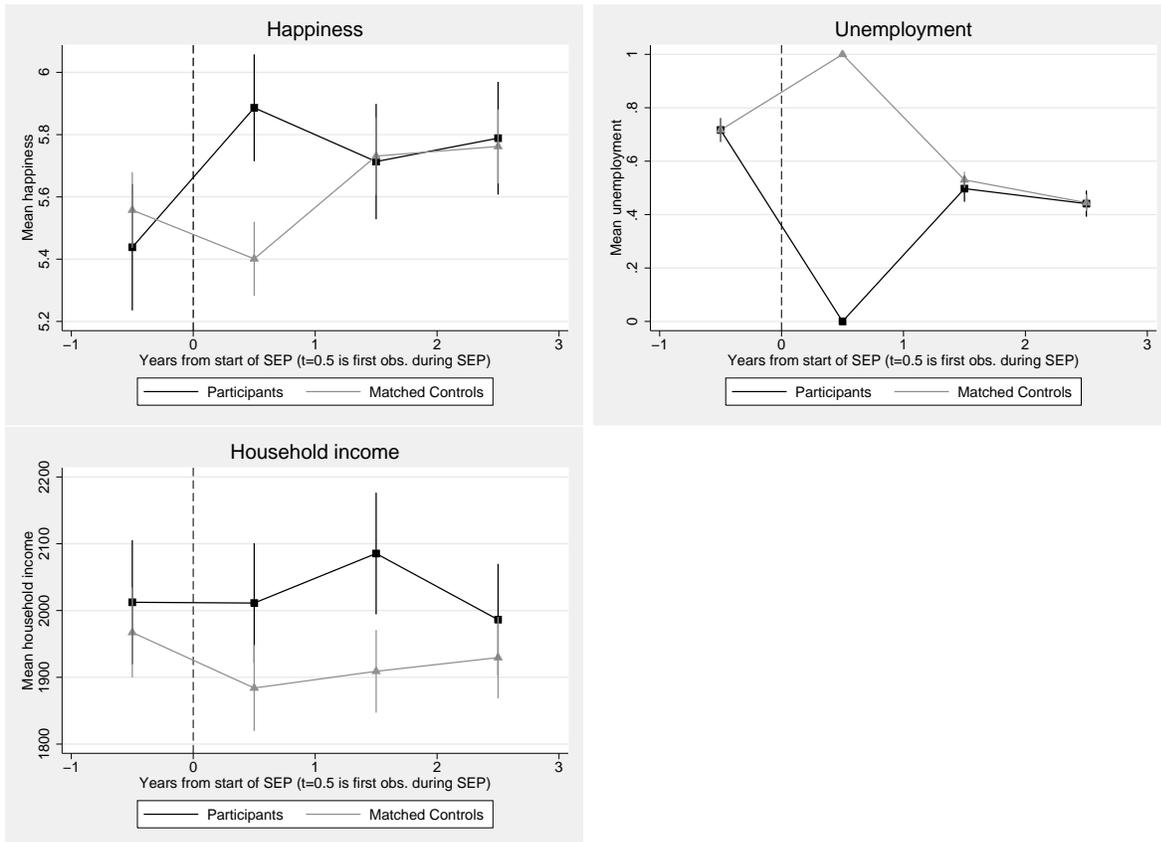

Data source: German Socio-Economic Panel, 1992-2004. Start dates are based on retrospective reports of individuals that have started a SEP since the previous observation. In 1996, respondents were not asked about SEP participation, so that the estimate for the previous year, 1995, is missing.

Figure 3: Trends of Happiness around the Start of Subsidized Employment projects
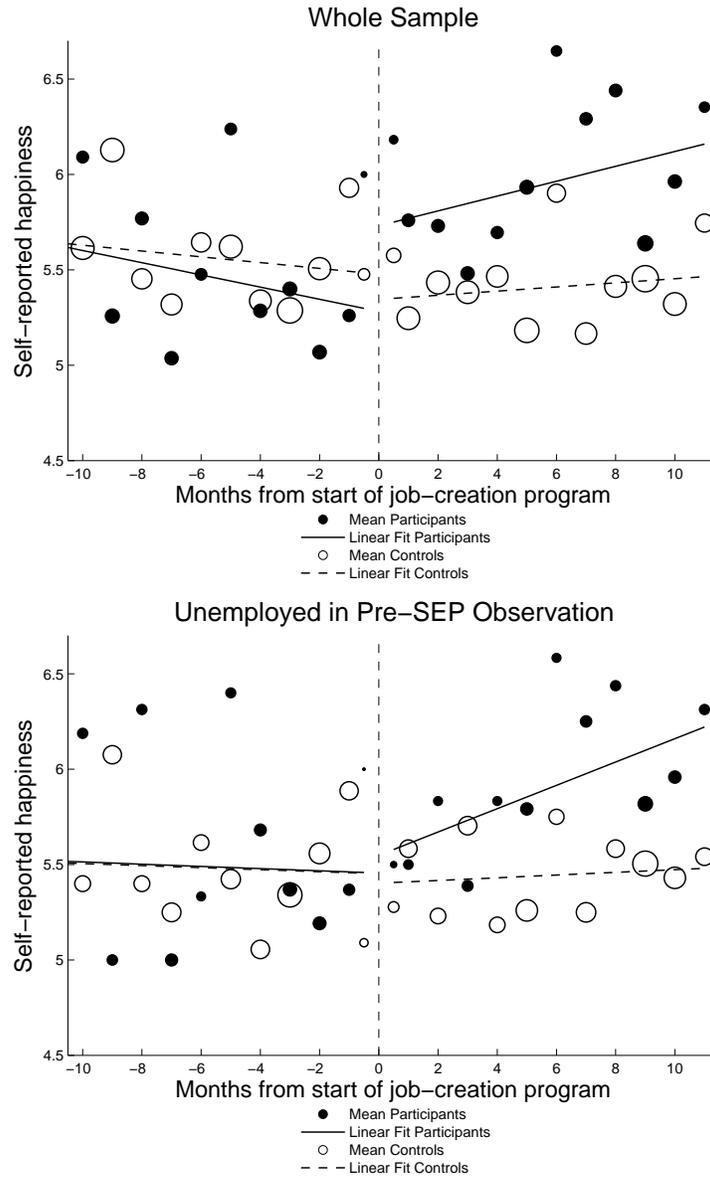
Estimates are based on individuals in the German Socio-Economic Panel (SOEP) that started employment in a SEP in the period 1992-2004 and controls from a nearest neighbor matching procedure. For each participant, the graph plots two observations, one before and one after the start of the project. The horizontal axis plots the time of the interview in months before/after the start of the SEP. Control observations in the "post-treatment" period are plotted at the same time-coordinate as the observation of the matched participant. The time since the control individual's previous interview is then used to calculate the time-coordinate at which the corresponding pre-treatment control observation is plotted.

Figure 4: Happiness, unemployment and income before and after the start of SEPs - all participants

Estimates are based on individuals in the German Socio-Economic Panel (SOEP) that started employment in a SEP in the period 1992-2004. For the time axis, t=0.5 is defined as the first observation after the start of employment in the SEP. The average interval between two observations of the same individual in the SOEP - one year - is used to calculate the other values of t. Happiness is measured on a scale from 0 to 10.