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## Non-Monetary Benefits of Continuous Training

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# Non-Monetary Benefits of Continuous Training

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PRELIMINARY VERSION.

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## Abstract

This paper studies the effects of participating in work-related continuous training on non-monetary outcomes such as satisfaction, worries, and civic participation (measured by social, cultural, and political participation). There is an extensive literature studying the monetary returns to continuous training. Meanwhile, wider benefits of continuous training beyond monetary returns have become a top priority on the European political agenda, while the empirical evidence about these benefits is still scarce. Using rich panel data from the German SOEP, we find evidence that continuous training increases life satisfaction, reduces worries about the own economic and job situation (even after controlling for labor market effects), and increases civic participation in some domains. To mitigate selection bias, we employ a regression-adjusted difference-in-differences matching approach that accounts for selection on observables and for time-invariant unobservables.

JEL-Code: J24, I21, M53

Keywords: non-monetary returns, civic participation, continuous training, difference-in-differences matching approach

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# 1 Introduction

According to the Adult Education Survey (AES) 2011, around 40.3% of adults aged between 25 and 64 years old in Europe took part in learning activities in the 12 months prior to the survey (European Commission/EACEA/Eurydice, 2015). Most of these activities were non-formal (36.8%), meaning that learning has occurred in some form of organized (adult) continuing and further education and training. Other data sources, like the European Labour Force Survey 2014, report that around 10.5% of the European adult population (25–64 years old) have participated in some kind of continuous educational activity four weeks prior to the survey. These activities perceive a high emphasis on the political agenda of European countries. The European Commission strategic framework for European cooperation in education and training (*ET 2020*) formulates the goal that training participation among European adults should increase up to 15% by the year 2020. The European Commission argues that adult learning activities are (among other things, e.g., earnings, employment stability, and economic growth) important for active citizenship and social cohesion (Council of the European Union/European Commission, 2015) and recommends to make the EU an area of lifelong learning (European Commission, 2001). The latter, however, do not translate immediately into directly measurable monetary returns (neither at the individual level, nor at the aggregate level). Related to that, comparatively little is known about the causal effects of continuous education and training on non-monetary outcomes.<sup>1</sup>

Much more is known about the effects of continuous training on labor market outcomes, such as earnings, unemployment, and job promotions.<sup>2</sup> Using OLS, results show that earnings returns to training are rather high, ranging from 3.7% in the Netherlands to 21.6% in Greece (Bassanini et al., 2007, results are from the European Community Household Panel). Using fixed effects, the returns to training are considerably lower and sometimes insignificant in some countries. Fixed effects regression, however, may still not fully control for selection into treatment and thus returns to training might be biased (Pischke, 2001; Schøne, 2004). Studies that try to exploit situations where non-participation was rather random (Leuven and Oosterbeek, 2008) or using randomly distributed training vouchers (Schwerdt et al., 2012; Hidalgo et al., 2014; Görlitz and Tamm, 2016) do not find strong effects of trainings participation on earnings. Overall, results crucially depend on the way researchers are able to control for selection bias (De Grip and Sauermann, 2013).

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<sup>1</sup>Field (2011) provide an overview of the current literature, showing mostly descriptive relationships between participation in continuous training and non-monetary outcomes. See Feinstein and Hammond (2004); Georgellis and Lange (2007); Burgard and Görlitz (2014) for some specific examples.

<sup>2</sup>See Leuven (2005) and Bassanini et al. (2007) for overviews and LaLonde (1986); Lynch (1992); Blundell et al. (1999); Lechner (1999a); Goux and Maurin (2000); Pischke (2001); Leuven and Oosterbeek (2008); Schwerdt et al. (2012) for examples. De Grip and Sauermann (2013) provide a current overview of the main takeaways from the literature.

We contribute to the literature on the effects of continuous training by focusing on non-monetary outcomes. We use rich longitudinal data from the German Socio-Economic Panel Study (SOEP), which has detailed information on participation in continuous training activities, labor market and non-monetary outcomes, and background variables. Our vector of outcome variables center around five broader categories: satisfaction (including life satisfaction and satisfaction with household income), worries (including worries about economic situation and worries about job situation), cultural participation (including attending artistic and music events), social participation (including volunteering in clubs, organizations, and community services and meeting with neighbors, friends, and relatives), and political participation (including participating in local politics and political interest).

To address selection into the treatment, we employ a regression-adjusted matched difference-in-differences approach (Heckman et al., 1997, 1998; Smith and Todd, 2005). Controlling for various pretreatment outcomes simultaneously and controlling for unobserved time-invariant heterogeneity using individual fixed effects, we can eliminate a large part of the (potential) selection bias. The identifying assumption in this context is that there are no unobserved factors that are correlated with participation in training and at the same time correlated with the *change* in the outcome variable. Conditioning on a large set of covariates, we can further mitigate selection bias.

Additional support for the validity of the identifying assumption comes from the fact that we focus in our study on work-related continuous training activities that are mostly taken for fostering skills and competencies relevant at the workplace. Hence, assuming that selection into training activities materializes to a large extent in labor market outcomes (e.g., earnings, unemployment, occupational position, etc.), we can close this selection channel directly by controlling for those outcomes pre- and *posttreatment*. Thus, our estimates on the non-monetary effects of continuous training are net of monetary gains *and* are net of selection bias that is associated with labor market aspects.

Our preliminary results suggest that continuous training affects various outcome variables. In line with evidence from studies evaluating continuous training in Germany, we find that training leads to an increase in earnings by about 6% (Lechner, 1999b; Pischke, 2001; Büchel and Pannenberg, 2004). We are aware that non-experimental data might still conceal correlations of unobserved factors with the treatment and the outcome variables that would violate our identifying assumption. Therefore, we do not argue that this earnings premium reflects causal returns to training, but a combination of causal returns plus selection on unobservables. Regarding non-monetary effects, we find that training increases life satisfaction and satisfaction with household income significantly. As expected, however, the result for satisfaction with household income is driven by an increase in earnings that materialize after the treatment. We also find that participation in continuous training reduces worries about the own economic situation, the own job

situation, and the own health situation. Coefficients also decrease (in absolute terms) after the introduction of labor market control variables, but stay significantly different from zero.

Regarding the effects on cultural, social, and political participation, we find that continuous training has beneficial effects on cultural and social participation and less so on political participation. After controlling for labor market effects, we find that the treatment leads to higher attendance of artistic events (cultural participation) and higher attendance of music events (not significant), but does not increase own artistic or music activities. Continuous training increases significantly volunteering in clubs, organizations, and community services, and it also increases significantly meetings with neighbors, friends, and relatives. But it does not change the assistance of neighbors, friends, and relatives when they need a helping hand. Concerning political participation, we do not find strong positive effects on political interest and participation in local politics. These effects are only marginally significantly positive during treatment, but do not spillover to the posttreatment period.

The paper proceeds as follows. Section 2 describes the empirical strategy. Section 3 introduces the data and explains the construction of the treatment and control groups. Section 4 presents the preliminary results and Section 5 concludes.

## 2 Empirical Strategy

Since the early papers by Ashenfelter (1978); Ashenfelter and Card (1985); LaLonde (1986), economists are interested in the labor market effects of continuous training programs. They acknowledge that selection into training is non-random and leads to false conclusions about the effectiveness of a program. Over time, several papers offered different approaches to solve the evaluation problem. Heckman et al. (1997, 1998); Dehejia and Wahba (2002) proposed matching estimators to construct counterfactual control groups. Smith and Todd (2005) show that matching is not the silver bullet to approach all evaluation problems, however, they conclude that a matching difference-in-differences approach works best among the group of non-experimental estimators.

To identify non-monetary effects of continuous training, we apply a regression-adjusted difference-in-differences (DiD) matching approach (Heckman et al., 1997, 1998; Todd, 2008). The estimator is similar to the traditional difference-in-differences estimator in that it partials out selection on unobservables that is time-invariant. In addition to that, however, it reweights each observation according to matching weights that are obtained from a matching estimator. The identifying assumption is that there are no unobserved variables that determine participation in continuous training on the one hand and simultaneously influence a *change* in the outcome variable of interest. This is similar to the *common trend assumption* that requires that treated individuals would be on the

same trend as individuals in the control group in the absence of treatment. Using only the sample of the matched control group makes it more plausible that this assumption holds. The regression adjustment, including covariates that vary over time and explicitly taking care of the level of outcome variable prior to the treatment, has the advantage that it partials out remaining pretreatment differences that have remained after matching (Caliendo and Kopeinig, 2008).

More formally, Todd (2008) defines the identifying assumption for the matched DiD estimator as follows:

$$E(Y_0^{after} - Y_0^{before} | P, D = 1) = E(Y_0^{after} - Y_0^{before} | P, D = 0) \quad (1)$$

$Y$  is the outcome of interest measured before and after the treatment, indicated by  $D$ .  $P = P(D = 1 | X)$  is the propensity score and gives the conditional probability of participating in continuous training conditional on a vector of background variables  $X$ . The condition says that the expected change in outcome of the treatment group has to be equal to the expected change in outcome of the control group in the absence of treatment.

The matched difference-in-differences estimator is then given by:

$$\hat{\alpha}_{DiD} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} \left[ (Y_{1i}^{after} - Y_{0i}^{before}) - \sum_{j \in I_0 \cap S_P} w(i, j) (Y_{0j}^{after} - Y_{0j}^{before}) \right] \quad (2)$$

In this equation,  $n_1$  is the number of treated individuals. Group membership is indicated by  $I_1$  (treated) and  $I_0$  (control), respectively.  $S_P$  describes the group of individuals that share *common support*. The counterfactual control group is a weighted average of the change in outcome variables, with weights equal to  $w(i, j)$ . Weights depend on the distance between  $P_i$  and  $P_j$ .

We use kernel weighting to construct matching weights. The weights are defined as

$$w(i, j) = \frac{G[(P_j - P_i)/a_n]}{\sum_{k \in I_0} G[(P_k - P_i)/a_n]}, \quad (3)$$

with  $G(\cdot)$  equal to a kernel function and  $a_n$  equal to a bandwidth parameter. We use the Epanechnikov kernel with a bandwidth of  $a_n = 0.06$ , also applied in Heckman et al. (1997).

Longitudinal studies often suffer from panel mortality and (non-random) sample attrition. The survey we are using provides extensive weights to correct for these missing values. However, the use of sampling weights in propensity score matching is still subject to debate (Leuven and Sianesi, 2003). With incorrect or unknown sampling weights, Heckman and Todd (2009) recommend to match on the odds ratio ( $P/(1 - P)$ ) (or of the log odds ratio) because they show that the odds ratio obtained from an estimation with these incorrect or unknown sampling weights is a scalar multiple of the true odds ratio.<sup>3</sup>

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<sup>3</sup>Sampling weights do not affect single nearest neighbor matching (in contrast to kernel matching and local linear matching), because the weights do not affect the ranking of the potential neighbors and thus, the same set of pairs is selected regardless of being matched on the odds ratio or the propensity score (Smith and Todd, 2005; Heckman and Todd, 2009).

Therefore, we match on the odds ratio obtained from a logit regression that predicts participation in continuous training. The construction of sampling weights is explained in section 3.

We implement the difference-in-differences matching estimator in two major steps. First, we estimate a logit model to predict participation in continuous training prior to participation. By doing this, we construct the matching weights  $w(i, j)$ , which we use to weight observations in the control group. In the second step, we apply regression analysis by estimating the following model, including only individuals in the matching set:

$$Y_{it} = \alpha_0 + \alpha_1(T_i \times POST_{te}) + \mathbf{X}'_{it}\beta + \mu_i + \mu_t + \mu_e + \epsilon_{it} \quad (4)$$

$T_i$  is equal to one if individual  $i$  has participated in continuous training and zero otherwise.  $POST_{te}$  is equal to one if the period is a period after treatment and zero otherwise.  $\mathbf{X}_{it}$  is a vector of time-variant background variables. As background variables, we use the same variables as in the matching function.  $\mu_i$ ,  $\mu_t$ , and  $\mu_e$  are individual, treatment-period, and evaluation-period<sup>4</sup> fixed effects, respectively.

We weight individuals according to the matching weights that are provided by the matching algorithm. We incorporate them by multiplying the longitudinal sampling weights with the matching weights.

## 3 Data

To estimate the procedure just outlined above, we need longitudinal panel data that has information on participation in continuous training, information about non-monetary (as well as monetary) outcomes, and a very rich set of background information to control for selection into treatment. Therefore, we are using data from the SOEP (German Socio-Economic Panel Study), one of the world's largest and longest panel studies (Wagner et al., 2007). Representative for the German population, the SOEP has been used for a broad variety of research questions. Started in 1984, the study conducts annually more than 20,000 individual interviews in over 10,000 households in Germany. The respondents provide information about a wide range of topics, including their demographic situation, educational attainment, and labor market outcomes.

### 3.1 Sample Setup

In the years 2000, 2004, and 2008, the SOEP contains special modules with questions about work-related participation in continuous training in the *last three years*.<sup>5</sup> We treat

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<sup>4</sup>Evaluation-period fixed effects identify one of the three evaluation periods that will be explained below in section 3.

<sup>5</sup>In the years 1989 and 1993, there are also modules with information about participation in continuous training. However, we concentrate on the more recent modules because the questionnaires are identical.

each of modules separately. In the analysis, we have three evaluation periods that exploit the information from the different modules. Figure 1 illustrates the setup of the evaluation periods. The survey years that contain questions about continuous training are marked in red. The final dataset stacks all evaluation periods to jointly evaluate the effects of continuous training. This means that some observations are replicated to be used in more than one evaluation period.

For now, the sample is restricted to the years that are indicated in Figure 1. That is, we define the two years after the survey that contains information about continuous training as the posttreatment period. The three years prior the survey with the continuous training information (including the survey year) form the treatment period. Within this period, we assume that treatment could happen at any point in time. We expect that effects could already show up within this period as some people might be treated at the beginning of the period. The two years before the treatment period form the pretreatment period. Later we base the matching procedure and balancing checks on this period.

Detailed information on continuous training, like the start year of the course, course duration, costs, and much more, is available for the last three courses taken. The treatment group contains individuals with at least more than 10 hours exposure to continuous training activities over the three courses within the last three years. The control group consists of individuals who have not participated in any training activity in that evaluation period.

We restrict the sample to individuals who are between 20 and 65 years old, with labor market entry before the first pretreatment period, and who are a white collar worker (49% of all employed individuals) in one of the pretreatment-period years. Hence, we exclude blue collar workers (35%), self-employed individuals (9%), and public servants (7%), whose training are likely to be very different compared to the training white collar workers receive. Blue collar workers have the lowest training incidence (10%). They are followed by self-employed individuals (21%), white collar workers (30%), and public servants (48%). Thus, evaluating white collar workers is interesting because they are largest occupational group with relatively high uptake of continuous training. Finally, we require that each individual has at least one observation in the pretreatment period and at least one observation in either the treatment or the posttreatment period.

Latent concepts such as life satisfaction are more prone to measurement error than labor market outcomes. Kämpfer and Mutz (2013), for example, show that people’s life satisfaction in surveys depend on whether the sun is shining at the interview. This makes it harder to partial out the *true* life satisfaction from one person-year observation. To mitigate measurement error, we use averages for continuous variables of the treatment



periods (Schwerdt et al., 2012).<sup>6</sup> For categorical variables, we use the information from the latest year first. We replace missing information with information from earlier years within the each treatment period.

Pooling all evaluation periods, the baseline sample consists of 8,884 person-year observations (2,392 unique persons) in the treatment group and 21,082 person-year observations (5,585 unique persons) in the (potential) control group (before matching).

The setup of the datasets makes it necessary to adjust observations for sampling weights and standard errors in the estimation. For each evaluation period, we construct longitudinal sampling weights by using the cross-sectional weights of the first year in the pretreatment period multiplied by the inverse staying probabilities (multiplied by each year) (Haisken-DeNew and Frick, 2005). The longitudinal sampling weights are standardized such that they sum up to one within each evaluation period. Hence, each evaluation period has the same weight in the joint analysis. Standard errors are clustered at the individual level throughout to take care of multiple appearances of the same individual.<sup>7</sup>

### 3.2 Outcome Variables

In this paper, we test three basic domains of non-monetary outcome variables: satisfaction, worries, and civic participation. Satisfaction is measured on a 10-point scale from 0 [very low satisfaction] to 10 [very high satisfaction] and is available for *life satisfaction*, *satisfaction with household income*, *satisfaction with personal income*, and *satisfaction with the job*. Worries is measured on a 3-point scale from 1 [no worries], 2 [some worries], to 3 [big worries] and is available for *worries about economic situation in general*, *worries about own economic situation*, *worries about job situation*, and *worries about health situation*.

The measurement of civic participation is guided by the seminal work of Putnam (1995, 2002). We operationalize civic participation by three subdomains: *cultural participation*, *social participation*, and *political participation*. Each of the subdomains is covered by several indicators that are coherently surveyed over the time periods in this study such that we have information for each individual before, during, and after treatment. *Cultural participation* is approximated by three variables: *Active* asks whether the person actively participates in artistic (e.g., painting, photography, acting, dance, etc.) or musical activities. *Attend artistic events* asks whether the person attends artistic events and *Attend musical events* asks whether the person attends musical events. The variables are measured on a 4-point scale from 1 [never], 2 [rarely], 3 [every month], to 4 [every week].

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<sup>6</sup>We always use the latest two years within each treatment period. Hence, for the period of the treatment we calculate averages over the second and third year and use information from the first year only if the information were still missing.

<sup>7</sup>Future version of the paper will check bootstrapped standard errors (Abadie and Imbens, 2008).

*Social participation* is also approximated by three variables: *Volunteering* asks whether the person volunteers in clubs, organizations, or community services. *Socializing* asks whether the person meets with friends, neighbors, and relatives and *Assisting* asks whether the person assists friends, neighbors, and relatives when they need a helping hand. The variables are measured on a 4-point scale from 1 [never], 2 [rarely], 3 [every month], to 4 [every week].

We use two proxies for *political participation*: *Interest* asks whether the person has an interest in politics. The variable is measured on a 4-point scale from 1 [not at all], 2 [not so strongly], 3 [strongly], to 4 [very strongly]. *Participate* asks whether the person participate in local politics. The variable is measured on 3-point scale from 1 [never], 2 [rarely], to 3 [often].

To make the variables comparable, we  $z$ -standardize variables according to Kling et al. (2007); Bauernschuster et al. (2014). We do that by subtracting the mean of each variable and divide the difference by the standard deviation. To calculate means and standard deviations, we jointly use all pretreatment observations from all pretreatment periods and weights observations by their longitudinal sampling weight. Thus, each variable has mean zero and standard deviation one in the pretreatment period. Standardization takes place before matching.

For cultural, social, and political participation, we furthermore construct index variables that contain the  $z$ -scores of its components as an equally weighted average. These index variables are again divided by the standard deviation in the pretreatment period to make the index variables comparable to the component variables (Kling et al., 2007).

### 3.3 Conditioning Variables

The set of conditioning variables should contain variables that affect participation in training and might also have an impact on the change in the outcome variable. The variables can be broadly classified as *demographic characteristics*, *education*, *labor market outcomes*, and *outcome variables prior to the treatment*. Table 1 shows the variables that belong to each of the categories. We select the conditioning variables according to the literature that looks at the determinants of training participation (Arulampalam et al., 2004; Bassanini et al., 2007; Grund and Martin, 2012; Yendell, 2013), according to our own reasoning, and according to data availability. Important for our work is that previous papers have established that more educated worker are more likely to engage in training (Lynch, 1992; Arulampalam and Booth, 1997; Leuven and Oosterbeek, 1999; Bassanini et al., 2007). But the literature has also identified differences in training participation according to age, that is, younger workers are more likely to participate (Oosterbeek, 1996, 1998; Maximiano, 2012). Firm characteristics are also determinants of the probability to participate in training. For example, the probability of receiving training is higher in larger

firms (Oosterbeek, 1996; Lynch and Black, 1998; Grund and Martin, 2012; Maximiano, 2012). More recently, Caliendo et al. (2016) find that behavioral concepts such as locus of control can explain training participation as well.

## 4 Results

### 4.1 Covariate Balancing

Before turning to the results, we discuss the balancing table of covariates and outcome variables prior to treatment (Table 1). The table reveals that there is strong selection into the treatment. For example, training participants are younger, more likely to be male, less likely to be married, much better educated, more likely to be full-time employed, and more likely to work in large firms. This is in line with the literature on the selection into training activities (Grund and Martin, 2012; Yendell, 2013). Interestingly, 57% of the treated group have participated in some kind of training before the current training, whereas only 29% of the unmatched control group have participated before.

There are also significant pretreatment differences regarding outcome variables. For example, treated individuals are more satisfied with their personal income (but interestingly not more satisfied with their life or with their household income). Treated individuals are *less* satisfied with their job, which might be a reason to invest in continuous training. There are no big differences regarding worries between treatment and control group. However, treated individuals are much more likely to participate in cultural events, meaning that they are more likely to attend artistic or musical events and are also more likely to actively participate in such events. This picture of higher civic participation is also true for social participation and political participation. Treated individuals are more likely to volunteer, to socialize with and to assist their neighbors, friends, and relatives. Training participants also report higher political interest and that they participate more often in local politics. Overall, the picture shows that treated individuals are those people who score high on the civic participation domains that we can measure. Comparing them to the average person would definitely result in false conclusions about the effectiveness of continuous training. However, it is also questionable whether additional training can increase these non-monetary outcomes further.

To assess the quality of matching, we use two indicators. First, we calculate the standardized difference in the sample means ( $\tilde{\Delta}$ ) of the treated ( $\bar{X}_1$ ) and control groups ( $\bar{X}_0$ ) (unmatched and matched) as a percentage of the square root of the average of the sample variances in both groups ( $V_1(X)$  and  $V_0(X)$ , respectively) (Rosenbaum and Rubin, 1985). Caliendo and Kopeinig (2008) suggest that one should regard matching as unsuccessful when the standardized difference in means exceeds 5%.

$$\tilde{\Delta} = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{0.5(V_1(X) + V_0(X))}} \quad (5)$$

Second, we use  $t$ -tests to test the equality of means in the treated and the control sample (Caliendo and Kopeinig, 2008). The tests are based on a regression of the specific variable on the treatment, using evaluation-period fixed effects. We use longitudinal sampling weights before matching and longitudinal sampling weights that are multiplied by matching weights after matching. In Table 1, we report the  $p$ -value of the  $t$ -test in columns (4) and (7). Overall, the balancing table reveals that matching was successful in eliminating the large pretreatment gaps. None of the  $p$ -values indicate that a variable predicts training participation, even though the standardized differences show some differences above 5%. However, in most cases, the absolute differences are economically small. We do not expect these small differences affect our results significantly because remaining pretreatment differences are taken care of explicitly by the regression-adjusted difference-in-differences estimation (Heckman et al., 1997, 1998; Caliendo and Kopeinig, 2008).

## 4.2 Earnings, Satisfaction, and Worries

In this section, we present the first set of results regarding earnings, satisfaction, and worries. In the next section, we discuss the results concerning civic participation. As mentioned above, we evaluate the effect of continuous training in the posttreatment period and in the treatment period separately (see Figure 1). To do this, we introduce in the econometric model above (Eq. 4) as second treatment interaction that identifies individuals with continuous training in the treatment period. For earnings in columns (1) and (2) of Table 2, we find that the participation in training is associated with 6% to 8% higher earnings.

The remaining columns of Table 2 test various dimensions of satisfaction. First of all, we find that life satisfaction increases by about 10% of a standard deviation in the posttreatment period. This is only marginally significant. But the effect is stable and robust against the introduction of further control variables, especially labor market controls. Satisfaction with household income (column (5)) increases also by about 10%. However, this increase is fully accounted for by increases in earnings (column (6)). Almost the same pattern can be observed for satisfaction with personal income (columns (7) and (8)) and for satisfaction with the job (columns (9) and (10)), even though the coefficients without control variables are smaller and not significant compared to the coefficient on satisfaction with household income.

Table 3 provides results for various dimensions of individual worries. Overall, it seems that participation in continuous training reduces worries significantly. As expected, coefficients are reduced when labor market controls are included, but the coefficients

stay significant and are economically meaningful with effect sizes around 7% to 16%. Interestingly, worries about the own economic situation and worries about the job situation are mitigated already entirely in the treatment period (while doing the training or shortly after it). The only dimension that is not affected by participation in training is worries about the economic situation in general.

### 4.3 Cultural, Social, and Political Participation

In this section, we provide an overview of the results regarding the civic participation dimensions of cultural, social, and political participation. Figure 2 gives a preview of the findings. There we show mean values of the index variables of the three participation dimensions prior to the treatment ( $t - 1$ ), during treatment ( $t$ ), and posttreatment ( $t + 1$ ) (see Appendix Figures A-1 to A-3 for the same figures of the underlying indicators). Social participation (a) and cultural participation (b) do not show large pretreatment differences, indicating that matching has worked well there. However, political participation (c) shows some differences prior to treatment. But these differences are taken care of in the difference-in-differences estimation discussed later. The figure shows that we can expect increases in the index value especially in cultural and social participation. For political participation, we might find increases during treatment but not in the posttreatment period. To establish significance, we turn now to the regression analysis in Tables 4 to 6.

Cultural participation in Table 4 shows an increase of about 7.8% of a standard deviation in the index value. Even though this effect is comparable to the previous findings, it is not significant (column (1)). Controlling for covariates and labor market outcomes in column (2), increases the coefficient slightly and it turns marginally significant now. It seems that this increase is driven by an increase in attending artistic events (columns (5) and (6)), even though attending musical events (columns (7) and (8)) also increase with coefficient magnitudes comparable to the coefficients for attending artistic events. We do not find any effect on actively taking part in artistic/musical events (columns (3) and (4)).

The next dimension, social participation, shows strong increases in response to participation in training activities (Table 5). The index for social participation is significantly positively related to continuous training and is relatively large with 12.3% (column (2)). The effect is driven by increases in enhanced volunteering in clubs, organizations, and community services (column (4)) and increased socializing and meetings with friends, relatives, and neighbors (column (6)). Assisting friends, relatives, and neighbors if they need a helping hand does not increase (column (8)). But it also does not decrease, which could be expected if participating in continuous training would consume a large amount of leisure time.

As the last dimension, we look at political participation in Table 6. It seems that there are no increase on this dimension in the posttreatment period. We only find marginally

significant positive coefficients in the treatment period. This suggests that continuous training might lead to increased political participation during the treatment (perhaps as a part of the course content) but does not survive into the posttreatment period.

## 5 Conclusions

This paper contributes to the literature on the effects of participating in continuous training. We focus our attention on non-monetary outcomes such as life satisfaction, worries, cultural participation, social participation, and political participation. Until now, little is known about the effect of continuous training on those non-monetary outcomes, even though continuous training and lifelong learning is high on the political agenda.

To mitigate selection bias, we use rich longitudinal panel data from the German Socio-Economic Panel (SOEP) together with a regression-adjusted difference-in-differences matching approach. We match on various covariates and pretreatment outcome variables to obtain a control group that is similar in observable characteristics than the treated group. After that, we use a difference-in-differences estimator on the matched sample to eliminate time-invariant fixed effects and remaining pretreatment differences. In addition, we control for labor market outcomes pre-and posttreatment to net out selection bias that is introduced over this channel.

Our preliminary results suggest that participating in continuous training increases various indicators. We find that continuous training increases life satisfaction and satisfaction with the household income. The latter is entirely driven by real increases in earnings, which is expected. We also document that continuous training decreases worries that are related to the own economic and job situation. That effect persists even after controlling for labor market outcomes.

Regarding social, cultural, and political participation, our results suggest that increases are strongest for social participation, especially volunteering in clubs, organizations, and community services, and socializing with friends, neighbors, and relatives, and followed by increases in cultural participation, especially attending artistic and musical events. For political participation, we cannot document strong changes, despite some increases in the treatment period that, however, fade out quickly in the posttreatment period. Summing up the evidence, we find that continuous training seems to raise civic participation on various domains. However, more detailed analysis (which we are currently working on) is required to check the robustness of the results. The final version also contains subsample and effect heterogeneity analysis.

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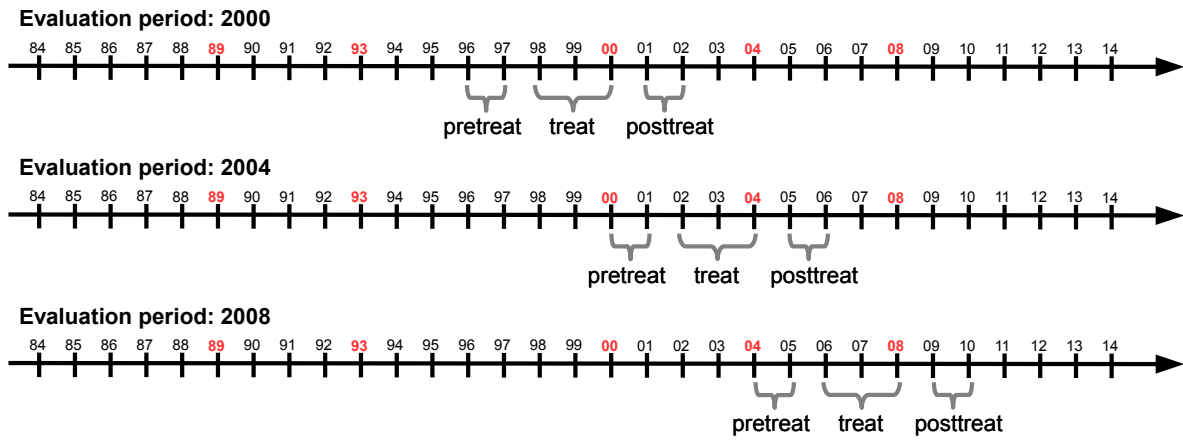


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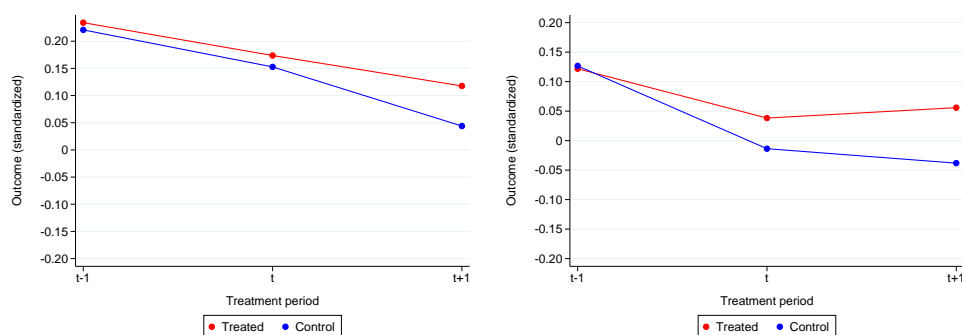
# Figures and Tables

Figure 1: Description of Evaluation Periods



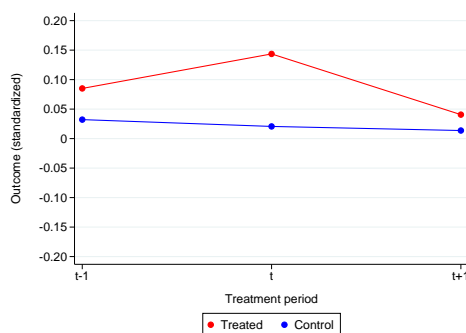
*Notes:* The figure describes the evaluation periods. Years marked in red indicate survey years with qualification survey modules in the GSOEP. We evaluate the years 2000, 2004, and 2008 because survey questions in 1989 and 1993 are slightly different. Treatment periods are centered around most reported treatment years, which in all cases is the year prior to the survey. Matching and standardization of variables is based on information in pretreatment years.

**Figure 2: Continuous Training and Cultural, Social, and Political Participation**



(a) Cultural participation

(b) Social participation



(c) Political participation

*Notes:* The figures shows average group means by treatment status for participation index values prior to treatment ( $t - 1$ ), during treatment ( $t$ ), and posttreatment ( $t + 1$ ). The sample is restricted to the matching sample and observations are weighted according to the longitudinal sampling weights multiplied by the matching weights.

Table 1: Balancing Table – Before Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	Treated	Control					
		Unmatched			Matched		
	Mean	Mean	$\tilde{\Delta}$	<i>t</i> -test <i>p</i> -value	Mean	$\tilde{\Delta}$	<i>t</i> -test <i>p</i> -value
<i>Demographic characteristics</i>							
Age	38.72	41.91	-31.66	0.000	38.73	-0.07	0.977
Female	0.50	0.59	-18.65	0.000	0.52	-4.37	0.356
Migrant	0.13	0.14	-4.59	0.165	0.13	0.12	0.984
German citizen	0.96	0.94	9.48	0.010	0.96	-0.35	0.939
Married	0.58	0.64	-11.96	0.000	0.59	-1.33	0.785
Homeowner	0.46	0.47	-0.87	0.774	0.45	3.03	0.535
Children	0.39	0.35	8.43	0.014	0.38	1.29	0.779
East Germany	0.23	0.19	9.51	0.002	0.22	1.36	0.744
<i>Education</i>							
Vocational	0.73	0.77	-8.55	0.023	0.74	-1.02	0.851
University	0.34	0.21	30.14	0.000	0.34	0.36	0.945
Basic school	0.19	0.32	-30.60	0.000	0.17	3.93	0.426
Intermediate/other school	0.40	0.40	-1.05	0.882	0.42	-4.09	0.409
Technical school	0.09	0.06	8.71	0.004	0.09	-3.00	0.550
Academic school track (Abitur)	0.32	0.20	28.27	0.000	0.31	2.46	0.632
No school degree info	0.01	0.02	-7.61	0.013	0.01	2.53	0.672
Previous continuous training	0.57	0.29	58.33	0.000	0.58	-2.47	0.606
<i>Labor market outcomes</i>							
Earnings	2,637	2,150	32.08	0.000	2,648	-0.73	0.853
Entry age	19.85	18.90	38.56	0.000	19.86	-0.40	0.920
Full-time employed	0.79	0.65	31.95	0.000	0.80	-3.28	0.461
Part-time employed	0.15	0.22	-16.81	0.000	0.14	4.50	0.297
Apprenticeship	0.00	0.00	-1.78	0.451	0.00	3.71	0.231
Marginal/unregular employment	0.02	0.04	-16.23	0.000	0.01	2.50	0.513
Non-working	0.04	0.08	-16.71	0.000	0.05	-3.44	0.461
White collar worker	0.92	0.85	21.22	0.000	0.92	1.86	0.689
Blue collar worker	0.02	0.04	-11.25	0.000	0.02	1.65	0.672
Civil service	0.34	0.23	24.60	0.000	0.32	3.61	0.450
Never unemployed	0.70	0.68	3.12	0.211	0.68	3.69	0.437
Unemployment experience	0.28	0.40	-12.06	0.000	0.30	-2.27	0.598
Tenure	9.00	10.06	-11.53	0.000	8.93	0.91	0.858
Small firms (< 20)	0.16	0.25	-21.17	0.000	0.17	-3.09	0.492
Medium firms (20-200)	0.24	0.26	-3.72	0.276	0.26	-4.09	0.387
Large firms (> 200)	0.55	0.39	32.20	0.000	0.52	6.55	0.175
No firm size info	0.05	0.10	-22.15	0.000	0.05	-1.58	0.725
<i>Outcomes before treatment</i>							
Life satisfaction	-0.01	0.00	-1.22	0.977	-0.04	3.45	0.438
Satisfaction with HH income	0.00	0.00	-0.60	0.983	-0.03	2.29	0.595
Satisfaction with personal income	0.09	-0.04	14.38	0.006	0.07	2.21	0.782
Satisfaction with job	-0.05	0.01	-6.89	0.078	-0.10	5.67	0.210
Worries about economic situation	0.03	-0.01	4.56	0.742	-0.03	6.94	0.175
Worries about own economic situation	-0.03	0.01	-5.29	0.091	-0.04	0.57	0.981
Worries about job	0.02	0.00	2.52	0.384	0.02	0.35	0.995
Worries about health	-0.07	0.03	-11.54	0.001	-0.14	8.46	0.102
Cultural participation (index)	0.23	-0.11	35.02	0.000	0.22	1.35	0.811
Active in artistic/musical events	0.10	-0.03	12.93	0.000	0.13	-3.39	0.525
Attend artistic events	0.13	-0.06	20.44	0.000	0.13	0.35	0.928
Attend musical events	0.18	-0.08	27.38	0.000	0.20	-2.16	0.654
Social participation (index)	0.12	-0.05	17.56	0.000	0.13	-0.51	0.934
Volunteering	0.11	-0.06	17.53	0.000	0.14	-2.08	0.665
Socializing	0.10	-0.02	12.93	0.000	0.11	-2.06	0.675
Assisting	0.04	-0.01	4.79	0.103	0.01	3.43	0.445
Political participation (index)	0.09	-0.05	14.24	0.000	0.03	5.42	0.201
Interest politics	0.11	-0.05	17.86	0.000	0.06	6.24	0.165
Participate local politics	0.02	-0.02	4.34	0.191	0.01	1.55	0.698

*Notes:* Table shows group means before and after propensity score (kernel) matching for treatment and control groups. Sample consists of working-age males and females (20-65 years old), working in white and blue collar occupations, not in school and in full-time, part-time or marginal employment in the two pre-treatment periods.  $\tilde{\Delta}$  is the standardized difference of group means. *p*-value is based on a regression of the specific variable on the treatment indicator and evaluation-period fixed effects. Observations are weighted by longitudinal sampling weights before matching and by longitudinal sampling weights multiplied by matching weights after matching.

**Table 2: Continuous Training, Earnings, and Satisfaction**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Satisfaction									
	Log earnings		Life		HH income		Personal income		Job	
Continuous training $\times$ post	0.054** (0.023)	0.058*** (0.022)	0.100* (0.054)	0.085* (0.052)	0.096** (0.041)	0.036 (0.039)	0.043 (0.075)	0.024 (0.068)	0.046 (0.060)	−0.009 (0.056)
Continuous training $\times$ treat	0.076*** (0.020)	0.078*** (0.019)	−0.004 (0.044)	−0.018 (0.039)	0.043 (0.034)	−0.002 (0.032)	0.020 (0.056)	−0.003 (0.054)	−0.004 (0.048)	−0.031 (0.045)
Control variables		X		X		X		X		X
Individual fixed effects	X	X	X	X	X	X	X	X	X	X
Evaluation-period fixed effects	X	X	X	X	X	X	X	X	X	X
Treatment-period fixed effects	X	X	X	X	X	X	X	X	X	X
R-squared	0.818	0.821	0.626	0.648	0.668	0.689	0.726	0.759	0.527	0.550
Observations	14,440	14,440	14,863	14,405	14,856	14,399	8,942	8,646	14,468	14,369

*Notes:* *Continuous training* equal to one if the person has participated in at least 10 hours of continuous training in the last three years and zero if the person has reported to have not participated in any continuous training activity in the last three years. *Post* is equal to one for the two post-treatment periods and zero otherwise. *Treat* is equal to one for the three treatment periods and zero otherwise. Observations are weighted longitudinal sampling weights times matching weights. *Control variables* contain the variables that are used in the matching procedure (except for the outcome variables). All regressions control for earnings. Standard errors, clustered at the individual level, in parentheses. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 3: Continuous Training and Worries**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Economic situation in general		Own economic situation		Job situation		Health situation	
Continuous training $\times$ post	−0.042 (0.050)	−0.042 (0.052)	−0.094** (0.042)	−0.074* (0.040)	−0.127*** (0.048)	−0.109** (0.045)	−0.143*** (0.055)	−0.161*** (0.055)
Continuous training $\times$ treat	−0.034 (0.049)	−0.048 (0.049)	−0.091** (0.041)	−0.081** (0.040)	−0.089** (0.040)	−0.090** (0.038)	−0.053 (0.045)	−0.052 (0.044)
Control variables		X		X		X		X
Individual fixed effects	X	X	X	X	X	X	X	X
Evaluation-period fixed effects	X	X	X	X	X	X	X	X
Treatment-period fixed effects	X	X	X	X	X	X	X	X
R-squared	0.405	0.411	0.629	0.638	0.579	0.595	0.647	0.648
Observations	14,861	14,403	14,861	14,403	14,435	14,332	13,512	13,054

*Notes:* *Continuous training* equal to one if the person has participated in at least 10 hours of continuous training in the last three years and zero if the person has reported to have not participated in any continuous training activity in the last three years. *Post* is equal to one for the two post-treatment periods and zero otherwise. *Treat* is equal to one for the three treatment periods and zero otherwise. Observations are weighted longitudinal sampling weights times matching weights. *Control variables* contain the variables that are used in the matching procedure (except for the outcome variables). All regressions control for earnings. Standard errors, clustered at the individual level, in parentheses. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 4: Continuous Training and Cultural Participation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Index		Active		Attend artistic events		Attend musical events	
Continuous training $\times$ post	0.078 (0.052)	0.089* (0.051)	0.007 (0.054)	0.023 (0.054)	0.093* (0.053)	0.091* (0.054)	0.077 (0.051)	0.072 (0.052)
Continuous training $\times$ treat	0.022 (0.046)	0.029 (0.043)	0.061 (0.057)	0.057 (0.054)	0.065 (0.044)	0.080* (0.042)	-0.050 (0.046)	-0.043 (0.043)
Control variables		X		X		X		X
Individual fixed effects	X	X	X	X	X	X	X	X
Evaluation-period fixed effects	X	X	X	X	X	X	X	X
Treatment-period fixed effects	X	X	X	X	X	X	X	X
R-squared	0.614	0.636	0.595	0.606	0.534	0.546	0.515	0.538
Observations	13,425	12,970	13,460	13,004	14,846	14,388	14,824	14,367

*Notes:* *Continuous training* equal to one if the person has participated in at least 10 hours of continuous training in the last three years and zero if the person has reported to have not participated in any continuous training activity in the last three years. *Post* is equal to one for the two post-treatment periods and zero otherwise. *Treat* is equal to one for the three treatment periods and zero otherwise. Observations are weighted longitudinal sampling weights times matching weights. *Active* asks whether the person actively participates in artistic (e.g., painting, photography, acting, dance, etc.) or musical activities. *Attend artistic events* asks whether the person attends artistic events and *Attend musical events* asks whether the person attends musical events. *Control variables* contain the variables that are used in the matching procedure (except for the outcome variables). All regressions control for earnings. Standard errors, clustered at the individual level, in parentheses. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



**Table 5: Continuous Training and Social Participation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Index		Volunteering		Socializing		Assisting	
Continuous training $\times$ post	0.097** (0.049)	0.123** (0.051)	0.099** (0.047)	0.100** (0.047)	0.117** (0.055)	0.148*** (0.055)	-0.017 (0.056)	0.001 (0.059)
Continuous training $\times$ treat	0.029 (0.056)	0.030 (0.056)	0.065* (0.038)	0.053 (0.038)	0.018 (0.060)	0.029 (0.057)	0.018 (0.066)	0.020 (0.065)
Control variables		X		X		X		X
Individual fixed effects	X	X	X	X	X	X	X	X
Evaluation-period fixed effects	X	X	X	X	X	X	X	X
Treatment-period fixed effects	X	X	X	X	X	X	X	X
R-squared	0.594	0.603	0.691	0.699	0.531	0.547	0.455	0.469
Observations	12,906	12,500	14,829	14,373	12,943	12,534	12,949	12,541

*Notes:* *Continuous training* equal to one if the person has participated in at least 10 hours of continuous training in the last three years and zero if the person has reported to have not participated in any continuous training activity in the last three years. *Post* is equal to one for the two post-treatment periods and zero otherwise. *Treat* is equal to one for the three treatment periods and zero otherwise. Observations are weighted longitudinal sampling weights times matching weights. *Volunteering* asks whether the person volunteers in clubs, organizations, or community services. *Socializing* asks whether the person meets with friends, neighbors, and relatives and *Assisting* asks whether the person assists friends, neighbors, and relatives when they need a helping hand. *Control variables* contain the variables that are used in the matching procedure (except for the outcome variables). All regressions control for earnings. Standard errors, clustered at the individual level, in parentheses. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

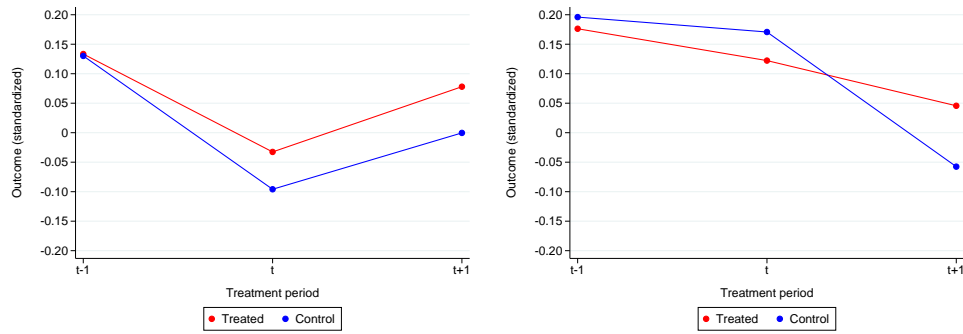
**Table 6: Continuous Training and Political Participation**

	(1)	(2)	(3)	(4)	(5)	(6)
	Index		Interest		Participate	
Continuous training $\times$ post	−0.020 (0.048)	−0.026 (0.051)	0.002 (0.033)	0.011 (0.033)	−0.012 (0.052)	−0.014 (0.055)
Continuous training $\times$ treat	0.061* (0.033)	0.054 (0.034)	0.050 (0.032)	0.040 (0.033)	0.062* (0.037)	0.070* (0.038)
Control variables		X		X		X
Individual fixed effects	X	X	X	X	X	X
Evaluation-period fixed effects	X	X	X	X	X	X
Treatment-period fixed effects	X	X	X	X	X	X
R-squared	0.692	0.695	0.802	0.806	0.578	0.578
Observations	14,816	14,359	14,862	14,404	14,828	14,371

*Notes:* *Continuous training* equal to one if the person has participated in at least 10 hours of continuous training in the last three years and zero if the person has reported to have not participated in any continuous training activity in the last three years. *Post* is equal to one for the two post-treatment periods and zero otherwise. *Treat* is equal to one for the three treatment periods and zero otherwise. Observations are weighted longitudinal sampling weights times matching weights. *Interest* asks whether the person has an interest in politics. *Participate* asks whether the person participate in local politics. *Control variables* contain the variables that are used in the matching procedure (except for the outcome variables). All regressions control for earnings. Standard errors, clustered at the individual level, in parentheses. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

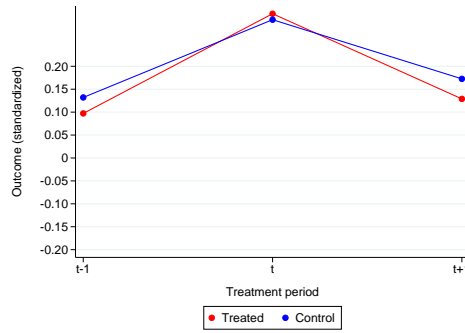
# Appendix

Figure A-1: Cultural Participation



(a) Attend artistic events

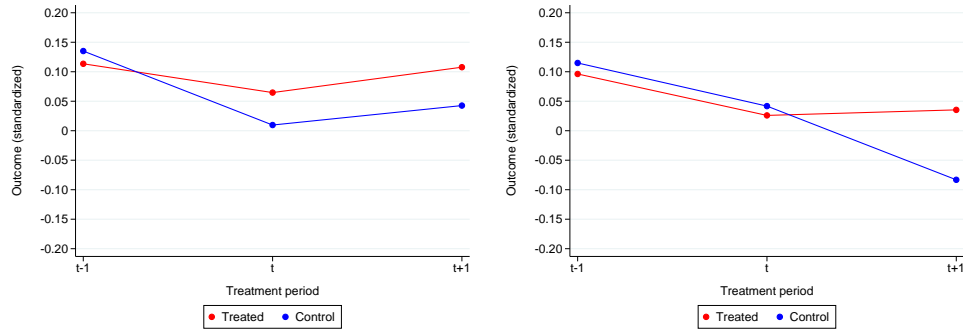
(b) Attend musical events



(c) Active

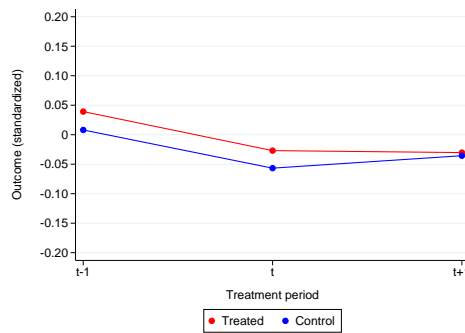
*Notes:* The figures shows average group means by treatment status for the standardized variable prior to treatment ( $t - 1$ ), during treatment ( $t$ ), and posttreatment ( $t + 1$ ). The sample is restricted to the matching sample and observations are weighted according to the longitudinal sampling weights multiplied by the matching weights.

**Figure A-2: Social Participation**



(a) Volunteering

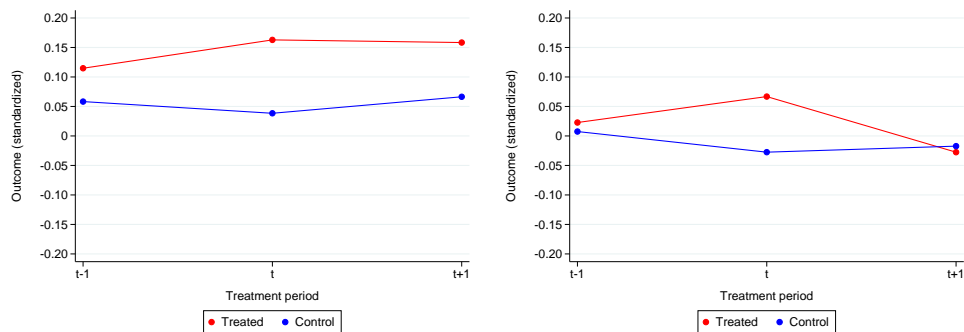
(b) Socializing



(c) Assisting

*Notes:* The figures shows average group means by treatment status for the standardized variable prior to treatment ( $t - 1$ ), during treatment ( $t$ ), and posttreatment ( $t + 1$ ). The sample is restricted to the matching sample and observations are weighted according to the longitudinal sampling weights multiplied by the matching weights.

**Figure A-3: Political Participation**



(a) Political interest

(b) Participate in local politics

*Notes:* The figures shows average group means by treatment status for the standardized variable prior to treatment ( $t - 1$ ), during treatment ( $t$ ), and posttreatment ( $t + 1$ ). The sample is restricted to the matching sample and observations are weighted according to the longitudinal sampling weights multiplied by the matching weights.