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ABSTRACT

Long-Lasting Effects of Socialist Education*

Political regimes influence contents of education and criteria used to select and evaluate students. We study the impact of a socialist education on the likelihood of obtaining a college degree and on several labor market outcomes by exploiting the reorganization of the school system in East Germany after reunification. Our identification strategy utilizes cutoff birth dates for school enrollment that lead to variation in the length of exposure to the socialist education system within the same birth cohort. An additional year of socialist education decreases the probability of obtaining a college degree and affects longer-term male labor market outcomes.

JEL Classification: I25, J24, P36

Keywords: socialist education, non-meritocratic access restrictions, labor market success

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1 Introduction

A vast literature emphasizes the accumulation of human capital (and in particular the level of education of the labor force) as a fundamental factor for economic development.[1]There is also a growing consensus that political and economic institutions are at the root of a significant part of the variation in GDP across countries.[2] However, how interplays between human capital and political institutions contribute to shaping the long term economic prospects of a country has been studied less.

Indeed, education and institutions appear to be highly interconnected. Several studies support the hypothesis that education is a strong predictor of democracy and quality of institutions (see Barro (1999) and Glaeser et al. (2007), among others). Another strand of the literature discusses how political regimes influence the educational system of a country. Bowles and Gintis (1976) argue that norms and values within schools tend to reproduce the internal organization of societies and their labor market structure. Governments may set incentives to affect the educational paths of their citizens (Cantoni and Yuchtman (2013)), determine the identity of the future elites by establishing the criteria used to select and evaluate students, and also shape the ideology of students by directly intervening in the contents of their studies.

We contribute to this debate by focusing on the micro level and evaluating how the transition from a socialist to a democratic regime affects labor market outcomes of individuals through changes in education. Education within socialist economies has often been instrumental to the consolidation and perpetuation of the political regimes and their elites. The curricula systematically aimed at creating a socialist personality, and access to higher education was granted on the basis of political involvement rather than academic credentials alone. We analyze whether both the content and style of education under socialism, as well as non-meritocratic access restrictions to higher education or a desired apprenticeship, had significant long-term effects on the labor market success of individuals in the capitalist labor market.

We study the effects of socialist education on the likelihood of obtaining a college degree and on several labor market outcomes by exploiting the reorganization of the school system in East Germany towards West German standards after reunification. The educational system in the German Democratic Republic (GDR) was transformed

[1]For an overview of the extensive literature, see Krueger and Lindahl (2001).
[2]See, among others, North (1981), and Acemoglu et al. (2001).

very rapidly after the fall of the Berlin Wall. Any elements in the curricula directed towards the creation of a socialist personality were deleted, and restrictions in access to college not based on academic merit were quickly eliminated, as were restrictions in the choice of apprenticeship.

We analyze the labor market success of individuals belonging to the birth cohorts 1971 to 1977, i.e. cohorts that were still in education at reunification, at the age of 31 or older, i.e. at an age when they are already settled in the labor market. Our identification strategy relies on the following consideration: within the same birth cohort, individuals born earlier in the year started school at a younger age and had received one more year of socialist education at reunification. In the GDR, children turning six on or before May 31 of a given year were per decree enrolled in the first grade by September 1 of the same year. We consider as treated individuals born on or after the first of June; individuals born on May 31 or before are instead part of our control group. Within the same birth cohort, treated individuals in East Germany belonging to cohorts 1971 to 1973 were less affected by restrictions in access to college education or a favored apprenticeship than non treated ones, while treated individuals in East Germany belonging to cohorts 1974 to 1977 were exposed to a smaller number of years of socialist teaching. Since the educational system in West Germany did not experience any major changes in the 80s and early 90s, treated respondents born between 1971 and 1977 and educated in the West instead received the same type of education as non treated respondents. By analyzing the difference of treatment effects between East and West we are able to control for any effects that might arise simply due to entering school at a slightly older age.[3] By comparing respondents in the treatment group with those in the control group in both East and West Germany in a standard difference-in-differences specification, we identify the effect of socialist schooling on college attendance and labor market outcomes of respondents educated under the socialist regime and affected by the reorganization at reunification at different stages of their schooling.

We find that an additional year of socialist education substantially decreases the probability of obtaining a college degree. This is true for both males and females, and for respondents belonging to both sets of cohorts. For male respondents belonging to cohorts 1971 to 1973, this effect translates into lower wages and a lower likelihood

[3]See, among others, Angrist and Krueger (1991) for evidence from the United States, Puhani and Weber (2007) from Germany, and Black et al. (2011) from Norway.

of obtaining a managerial or professional job. At the same time, individuals in this cohort group who received an additional year of socialist education have a higher probability of being employed. Thus, we conjecture that the abolishment of non-meritocratic restrictions in access to high school and college as well as choice of apprenticeship allowed able students in the birth cohorts 1971 to 1973 to invest more in their human capital and therefore achieve a better occupational status; yet, for less able individuals the transition into the free labor market at the stage of apprenticeship was a difficult one. For male individuals in the cohorts 1974 to 1977, the lower educational achievements in the non-treated group are accompanied by a decrease in their working hours and by a lower probability of being employed. The elimination of the transmission of socialist values in the school curricula, and the introduction instead of elements that stimulated individual initiative and motivation, seem to have encouraged participation in the labor market and effort in the workplace for the younger cohort group. None of these labor market effects are present for women.

This work contributes also to the literature that studies the long-lasting effects of communism on economic outcomes and individual preferences. Acemoglu et al. (2005) attribute the divergent economic paths experienced by North and South Korea in the second half of the twentieth century to their different institutions. Alesina and Fuchs-Schündeln (2007) find evidence that communism affected not only outcomes but also economic preferences. In this paper, we try to isolate one specific channel through which communist institutions had an impact on outcomes and preferences: the educational system and, in particular, the contents of its curricula, the style of teaching, and the criteria adopted to select which students have access to higher education, as well as the rules of apprenticeship choice. Brunello et al. (2012), Orazem and Vodopivec (1997), and Münich et al. (2005) discuss the distribution of returns to education after the transition from a socialist regime in several post communist countries and compare cohorts who received education under socialism with later born cohorts who did not. Guriev and Zhuravskaya (2009) find that individuals in transition countries who finished their education just before reforms were initiated have lower life satisfaction today than individuals who finished their education after implementation of the reforms. We add to these studies by focussing on a variety of alternative labor market outcomes, showing that educational outcomes, such as the probability of completing a college education, are likely to be affected as well, and discussing the channels through which socialist education may have an impact on the

individual performance in the labor market. Most importantly, we use an identification strategy that relies on a within cohort comparison, therefore eliminating the possibility that results are determined by confounding factors related to unobserved differences between cohorts.

Finally, we relate to the recent research on the effects of quality and contents of teaching.[4] Hoffmann and Oreopoulos (2009) and Chetty et al. (2014) discuss the importance of the quality of instructors in shaping students' performance. The language of instruction also has been proven to be important in determining not only standard labor market outcomes such as wages and likelihood to be employed (Angrist and Lavy (1997)), but also individual identity and political behavior (Clots-Figueras and Masella (2013)). More recently, Cantoni et al. (2015) provide evidence on the effect of changes in school curricula on the social and political attitudes of Chinese students. We try to assess the impact of indoctrination and, more in general, contents of teaching and teaching style within a socialist country on the individual performance in the labor market of a Western economy.

The structure of the paper is as follows: Section 2 provides a brief description of the educational system in East Germany before and after reunification. The data and the empirical strategy employed are discussed in Section 3. Section 4 presents the basic empirical evidence, while Section 5 rules out alternative interpretations of the results. The last section concludes.

2 Schooling and Apprenticeship in the GDR

In this section, we give a short overview of the educational system of the GDR and the socialist teaching in schools and vocational training. We then describe the process of admission into the *Erweiterte Oberschule (EOS)*, the high school that granted the university-entrance diploma, or into a certain apprenticeship. Last, we describe the main reforms related to schooling and apprenticeship after reunification.

[4] Algan et al. (2013) provide evidence that educational systems and teaching practices differ tremendously across countries.

2.1 Structure of Education

Students in the GDR were expected to attend school for 10 years (*Polytechnische Oberschule, POS*). After finishing 10th grade, only a certain fraction of students was allowed to add an additional two years of schooling in high school *(Erweiterte Oberschule, EOS)*, which granted the university-entrance diploma. The majority of students started an apprenticeship**,** which combined schooling with practical training in firms, such that schooling hours in an apprenticeship amounted to only half of schooling hours in POS or EOS. A third, less common, option was to combine a three-year apprenticeship with schooling to attain something resembling a high school equivalent diploma, which also gave permission to attend university. Last, students could attend a *Fachschule*, of which some provided education resembling an apprenticeship, and some resembled applied universities and could only be attended after finishing an apprenticeship. Taking as a base everyone starting at a university, *Fachschule*, or in an apprenticeship in 1987, 12 percent attended university, 18 percent attended a *Fachschule*, and 70 percent started an apprenticeship.

2.2 Socialist Elements of Education

We identify three ways in which teaching in the East differed from teaching in West Germany, namely teaching about socialism, official curricula, and teaching style.

A general socialist education was an official aim of the curriculum in the GDR, with the explicit goal of creating a socialist personality (Block and Fuchs (1993)). This goal found its way into every single school subject. Moreover, there were two subjects devoted explicitly to socialism, both taught from seventh grade on: Social Studies (*Staatsbürgerkunde*), which aimed at providing a deep knowledge of Marxism-Leninism and of the socialist system of the GDR, and Introduction to Socialist Production. Almost 14 percent of the overall teaching hours for grades 7 to 10 (which are the grades of interest in our empirical strategy) were devoted to teaching these socialist subjects. In EOS and vocational schools, the hours devoted to teaching socialist subjects decreased by three quarters compared to grades 7 to 10 in the POS.

Beyond these two subjects, there were certain differences in the curricula between East and West Germany, as shown in Figure 1.[5] We focus here on grades 7 to 10,

[5] Curricula in West Germany are determined by the states, but de facto differ little between the states. The information from the GDR comes from Anweiler (1988), while

which are the relevant grades in our empirical analysis. The teaching of German, mathematics, and social sciences was of similar importance in East and West. However, GDR schools devoted significantly more time to natural sciences, while FRG schools devoted more time to teaching of "softer" subjects like foreign languages, sports, arts and music, and religious education.[6] The main foreign language taught in schools was Russian; it was a compulsory subject in every school in the GDR.

When it comes to teaching styles, there existed important differences between East and West. In GDR schools, critical thinking was not incentivized, and divergent opinions were suppressed (Block and Fuchs (1993)). Beyond the creation of a socialist personality, knowledge transmission was the main official goal of the curricula (Riedel et al. (1994)). Theorems and theories were never the subject of discussion, but rather dogmas to be memorized. The national curricula were extremely specific in what needed to be covered in each week, in part imposing detailed structures of how to cover certain topics, leaving minimal scope for teacher or student initiatives.

Thus, socialist education in our analysis comprises both the education to a socialist personality, as well as an education style that did not encourage individual initiative and independent thinking.[7] In basic subjects like mathematics, German language, and natural sciences, East Germans seem if at all better prepared than West Germans by schooling, and thus any effect of socialist education is likely not due to lack of knowledge (which might only arise in the case of English language skills), but rather due to the education style that minimized critical arguing and individual initiative.

2.3 Allocation of High School Slots and Apprenticeships

Besides academic credentials, political criteria played an important role in the decision of who was allowed to attend the *Erweiterte Oberschule*. Official selection criteria were the grades obtained during the 10th grade, as well as a statement about

for the FRG it relates to the state of Schleswig-Holstein (http://www.schulrecht-sh.de/texte/s/stundentafel.htm). We calculate the average teaching hours of each subject over grades 7 to 10, taking weighted means over the three school forms of Hauptschule, Realschule and Gymnasium for the West.

[6] In grades 1 to 4, GDR pupils had much more teaching time devoted to German than FRG pupils, namely around one half of overall teaching hours, as compared to one quarter to one third in the West.

[7] While the latter is not in itself linked to socialism, it is often associated with non-democratic regimes, which have incentives to suppress independent thinking and initiative.

the personality of the student. This statement was issued by the director of the *POS*; both the class teacher of the student as well as the *Freie Deutsche Jugend*, the de-facto youth group of the government party, were officially involved in the drafting of the statement (Waterkamp (1987)). It was supposed to describe the political and social involvement of the student, his identification with the GDR - documented through words and actions - as well as his social background. Children of workers were more likely to be accepted into the *EOS* than children coming from an academic background. Unofficial selection criteria included the intention of a military career, political position and personal contacts of the parents, as well as a desired career path that was in line with the official planning numbers (Fischer (1992)). Summarizing, there were important criteria in addition to academic merit which affected the acceptance into the *EOS*, and it is thus likely that a significant number of students who possessed the academic merits were not allowed to attend high school. Similar criteria were adopted to select the students who were allowed to attend college among the ones who completed the EOS.

The constitution of the GDR stated that each student had the right (as well as the obligation) to do an apprenticeship (Köhler (2008)). The number of apprenticeships in each firm was determined by central planning. Already in the *POS*, students were brought in contact with firms which were deemed to be a good fit for them in order to influence the students' apprenticeship decision according to the central plan. The application and allocation process was done centrally, but students could express preferences for a certain apprenticeship in their application. Firms were obliged to offer apprentices a permanent position at the end of their vocational training (Wehrmeister (2005)). Similarly to the central planning of apprenticeship positions, the number of students allowed to start studying a certain subject at the university level was centrally determined each year. An official survey from the GDR acknowledged that around one third of all apprentices ended up in an occupation that was not associated at all with their initial wishes (Anweiler (1988)).

In 1994, Germany passed the so-called "Occupational Rehabilitation Law".[8] This law established certain monetary rights for students who for political reasons were not allowed to attend EOS, university, or finish an apprenticeship, and for workers who were not allowed to work in a job they originally trained for or a similar one.

[8] *Gesetz über den Ausgleich beruflicher Benachteiligungen für Opfer politischer Verfolgung im Beitrittsgebiet.*

From 1994 to 2011, 67,400 individuals were granted monetary compensation based on this law (Bundesregierung (2013)). This is an impressive number, as it certainly constitutes a lower bound of affected individuals.

Overall, the non-meritocratic access restrictions led to misallocation of students, and additionally to less incentives for able students to put effort into schooling. The latter fact was exacerbated by the generally low wage inequality in the East.

2.4 The Situation after Reunification

The educational system in the GDR was transformed very rapidly after the fall of the Berlin Wall in November 1989. All school reform acts implemented in the states (*Länder*) in East Germany required the elimination of any elements in the curricula which were directed towards the creation of a socialist personality. Instead, they fostered the development of an educational system which supports students to act independently within the framework of the Western society. Individual initiative, motivation, and creativity became crucial components of the reformed education system. The changes involved going from strict curricula to more open ones, which specified certain topics to be covered within a semester, but did not lay down weekly plans, and thus gave more scope to teachers. Implementation of a student-based approach became a central part of the curricula reforms (Riedel et al. (1994)). A significant share of teachers, namely approximately 20 percent, were dismissed after reunification.[9] These partly targeted dismissals likely accelerated the implementation of the new curricula and teaching styles, which were also fostered in newly established centers for in-service training (Block and Fuchs (1993)). Students were allowed to learn other foreign languages such as English and French. The socialist content of education was abolished almost immediately, and non-meritocratic access restrictions to the EOS (and therefore to college) fell at the beginning of 1990 (Fischer (1992)). The vocational training schools were disassociated from government firms and brought under communal control. The right to freely apply for any apprenticeship was introduced.

Over the period June to September 1990, 18,500 existing apprenticeship contracts were resolved, of which two thirds would have started in the summer of 1990, 3,500 were in the first year of the apprenticeship, and 2,400 in the second apprentice-

[9] Web Appendix Section 1 discusses which implications possible temporary interruptions in schooling would have for our results.

ship year (Bundesministerium für Bildung und Wissenschaft (1991)).[10] This may be partly because of the students' choice to attend EOS and subsequently college, but also because many firms suffered from the economic transition (Wehrmeister (2005)). Private training programs and public active labor market policies were implemented in East Germany in order to offer possibilities for retraining and further education. However, there exists evidence that the success of these programs was limited (see e.g. Lechner and Wunsch (2009)).

3 Methodology and Data

3.1 Exploiting Cut-Off Birth Dates for School Enrollment

We analyze the effects of socialist education on labor market outcomes based on a difference-in-differences approach. In the GDR, children turning six on or before May 31 of a given year were per decree enrolled in the first grade by September 1 of the same year. We define children born June 1 or later as the treated group, and children born May 31 or before as control group. The difference between respondents in East Germany in treatment and control group is that, for any given birth cohort still in school at the time of the fall of the Berlin Wall in November 1989, respondents in the treatment group were one year less advanced in the socialist education system than respondents in the control group, and thus acquired one year less of socialist education. Treated respondents in the West received instead the same type of education as non treated respondents.

By comparing individuals born early and late in the year in the East and still in education at reunification, we compare groups differentially affected by the length of socialist education; by comparing differences between these groups between East and West Germany, we control for any potential general effects of entering school at a slightly older or younger age. The underlying identifying assumption is therefore that the effect of age at school entry on the dependent variables would be the same in East and West Germany in absence of the differential exposure to socialist education

[10] In 1988, 164,000 individuals started an apprenticeship in the East, and in 1989 the corresponding number was 126,000. Thus, around 1.5% of second year apprentices, 2.8% of first year apprentices, and - under the assumption of the same number of starters 1989 and 1990 - 10% of starting apprentices had their contract dissolved in the four months period June to September 1990 alone.

that affects only respondents educated in the East. We show evidence supporting this assumption in Section 5.1.

We run the following difference-in-differences estimation in order to assess whether labor market outcomes are affected by socialist education:

$$Y_{ic} = \beta_0 + \beta_1 East_{ic} + \beta_2 Treat_{ic} + \beta_3 \left(East * Treat\right)_{ic} + \beta_4' \left(X\right)_{ic} + \gamma_c + \varepsilon_{it} \qquad (1)$$

where Y_{ic} is the relevant labor market outcome variable for individual i born in year c, $East$ is a dummy variable indicating whether the individual lives in East Germany, and $Treat$ is a dummy variable being equal to 1 if the individual was born on or after June 1. We do not have panel data; therefore, we pool several survey years (2005 to 2008). X is a vector of control variables including a male dummy, a full set of age dummies, a full set of state of residence dummies, and month of birth, which enters linearly. When we include state dummies, the East dummy drops out. γ_c is a full set of birth year dummies. Standard errors are clustered at the *birthyear-treatment-east* level, i.e. at the group level with groups being built based on all possible interactions of the birth year, treatment, and East dummies.

The coefficient β_1 captures the effect of living in the East on labor market outcomes. The coefficient β_2 controls for any potential effects of being enrolled in school at a slightly older age. The coefficient of main interest is β_3, which captures a differential effect of being treated for East and West Germans. If being treated leads to an additional positive labor market effect in East Germany due to experiencing a shorter GDR education, then β_3 should be positive.[11]

One caveat of the analysis is that we only assign respondents correctly to school cohorts (and therefore to either treatment or control groups) in the absence of grade repetition and early or late enrollment into school. Unfortunately, our data set does not indicate whether an individual ever repeated a grade or whether he/she enrolled in school a year earlier or later than he/she was supposed to. An incorrect assignment

[11] We refrain from performing a difference in regression discontinuity exercise (as in Carneiro et al., 2011) for two reasons. First, the cut-off schooling date in the West was June 30, rather than May 31. We show a robustness check using this cut-off date for the West in Section 5. Secondly, early and late enrollment into school makes measurement error in allocating individuals into treatment and control groups largest for individuals born close to the cut-off date. Results using the difference in regression discontinuity approach by Carneiro et al. (2011), and omitting individuals born in the month of June, are shown in Web Appendix Table W1.

of respondents to school cohorts would induce measurement error that may bias our estimates of β_3 upwards under very specific circumstances. In Section 5.1, we conduct a battery of placebo tests to dismiss the possibility that our results are driven by wrong assignment to treatment of the respondents in our sample.

3.2 Affected Birth Cohorts

Affected birth cohorts are those still in education at reunification, i.e. born 1971 or later. We analyze the birth cohorts 1971 to 1977, splitting them up into two separate cohort groups, namely the cohorts born 1971 to 1973, and those born 1974 to 1977. We would like to analyze even younger cohorts than the one from 1977, but do not observe these cohorts yet at the relevant age to investigate labor market outcomes.

When the Berlin Wall fell, individuals in the cohort group 1974 to 1977 were still in 10th grade or below. For any given cohort in this group, treatment means having received one year less of GDR education than the control group, and thus the length of exposure to socialist contents of education and teaching style is the relevant difference between treatment and control in the cohort group 1974 to 1977. Our hypothesis is that the longer the exposure to socialist education, the stronger was the transmission of values that may not be useful in the unified German labor market and, more generally, in Western societies.

The second relevant cohort group consists of the birth cohorts 1971 to 1973. Consider cohort 1973. While treated individuals were about to complete 10th grade and therefore free to choose an educational path that was propaedeutic to college education or an apprenticeship of their choice in the summer of 1990, individuals in the control group may have been forced to participate in apprenticeship programs instead of attending *EOS* (which granted the university-entrance diploma) in the summer of 1989. Within cohort 1972, individuals in the control group had attended one year more of the apprenticeship program in November 1989, therefore increasing the cost of switching to the educational path needed to be able to attend college, or to switch the apprenticeship subject. On the other hand, as discussed in Section 2.4, they might have been less likely to be dismissed from an apprenticeship. The treated group in the cohort 1971 was either in the last year of apprenticeship or in the last year of EOS and therefore not subject to any restriction in access to college or in the choice of the subject. By contrast, individuals in the control group had already entered the

labor market, had started studying a certain subject at the university, or had started the compulsory military service for males. Given the high economic uncertainty after reunification, individuals with a job or an apprenticeship might have feared giving it up and starting a new career, even if the job was not a good fit for them, affecting their productivity negatively. While changing the subject at university is theoretically possible, it involves reapplying. Being stuck in a subject that does not match the own abilities and interests well might lead to a lower probability of college completion. The cost of changing the apprenticeship subject, going back to high school to earn a university-entrance degree, or switching subjects at a university might also be a purely psychological one based on a resistance to treat sunk costs as such.

Summarizing, for the cohort group 1971 to 1973, the treatment and control groups differ in how far they have been affected by access restrictions to education. We conjecture that it was easier to change schooling or apprenticeship for the treatment group than for the control group, both for practical and psychological reasons.

When we analyze both birth cohort groups separately, we create separate cohort group dummies for both cohort groups and run the following regression:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 East_{ic} + \beta_2 Treat_{ic} \\ &\quad + \beta_3 \left(East * cohort_{71-73}\right)_{ic} + \beta_4 \left(Treat * cohort_{71-73}\right)_{ic} \\ &\quad + \beta_5 \left(East * Treat * cohort_{71-73}\right)_{ic} + \beta_6 \left(East * Treat * cohort_{74-77}\right)_{ic} \\ &\quad + \beta_7' \left(X\right)_{ic} + \gamma_c + \varepsilon_{it} \end{aligned} \tag{2}$$

Now β_5 and β_6 capture any potential additional effect of treatment in the East compared to the West for the birth cohorts 1971 to 1973 and 1974 to 1977, respectively. β_6 should therefore capture the effects of exposure to socialist methods of education, and β_5 the effect of non-meritocratic restrictions in access to high school and college, as well as restricted apprenticeship choice. We conjecture that differential exposure to length of socialist education did not matter for the older cohort group because of the significantly lower teaching time in apprenticeship training, and because less teaching time was devoted to socialism in both apprenticeship and EOS, as documented in Section 2.2.

3.3 Data and Sample Selection

The German Microcensus is a repeated cross-sectional annual survey on a one percent random sample of the German population. The main variable that we need for our analysis is month of birth, which is only available for the years 2005 onwards. Thus, our main analyses are carried out on the samples 2005 to 2008, in which we define treatment correctly as being born on or after June 1. We refer to this as "Definition 1" of treatment. In the survey years prior to 2005, month of birth was only reported as falling either into January to April or May to December,[12] thus not coinciding exactly with our treatment/control definition. We use this less precise definition of treatment, in which individuals born in May are incorrectly assigned to the treatment group, when we study the outcomes of older cohorts and therefore need data from earlier survey rounds in order to observe them at the relevant age.[13] We call this less precise definition of treatment "Definition 2". We focus on individuals aged 31 to 35. We only start at age 31 in order to capture labor market outcomes at an age at which individuals are already settled in the labor market. We stop at age 35 such that the age composition of the different cohorts 1971 to 1977 is not too different in our sample 2005 to 2008.

The Microcensus provides the current state of residence, but unfortunately does not report whether an individual resided in the GDR or FRG before 1989. Thus, we have to work with the current residence as a proxy for residence before 1989, and we drop respondents from the state of Berlin. In Section 5.4, we address the possibility that our results are driven by selection into current residence.

We start by analyzing the impact of reunification on the probability of obtaining a college degree, and then focus on labor market outcomes. The dummy variable *college* takes on the value of 1 if the highest educational degree comes from a university or an applied university, excluding GDR *Fachschulen*. Employment is equal to 1 if the self-reported employment status is given as employed. Working hours are hours in a usual work week, of which we take the logarithm. To construct wages, we have to recur to personal net income, since gross income is not available. Personal net income is reported in approximately 25 brackets, and we set personal income equal to the mean point of each bracket in order to calculate net wages by dividing through

[12] In 2004, the two categories are slightly different: January to March, and April to December.

[13] This is the case for Figure 2 and some of the robustness checks.

working hours (following the methodology by Pischke and von Wachter (2008)). Last, $professional$ is a dummy variable equal to 1 if the respondent is a manager, a professional, or a technician or associate professional according to the ISCO classification (major groups 1, 2, and 3 in ISCO88).

We run linear regressions on all dependent variables to ease the interpretation of the coefficients, but results are robust to running probit specifications if the outcome variables college, employment, or professional are used. Descriptive statistics of all variables, also separated by East and West, as well as treatment and control group, are reported in Table 1.

4 Results

4.1 Results for College Graduation

Figure 2 shows the college completion rate by birth cohort for four different groups of individuals aged 31 to 35, separated by residing in East (thick line) and West (thin line), and by being born between May and December (treated, solid line) or January and April (control, dashed line), thus working with the less precise definition 2 of treatment.[14] In the West, individuals in the treatment group, and thus having been enrolled in school at a later age, exhibit slightly higher college completion rates than individuals in the control group for most birth cohorts. In the East, we observe the same pattern. Yet, differences become quite large, amounting to around one percentage point, starting with birth cohort 1971, which is the oldest cohort that may have been affected by the reorganization of the school and apprenticeship system in East Germany after reunification. This indicates that treatment might have a larger effect on college graduation in the East than in the West for the birth cohorts 1971 to 1977. We test the significance of these results, as well as their robustness to the inclusion of controls, in the following set of regressions.

Panel A of Table 2 shows the results of our main specification, with and without control variables (gender dummy, age dummies, birth year dummies, state dummies, and month of birth). The sample used in the regression analysis consists of respondents born between 1971 and 1977; therefore, we can rely on survey years 2005 to

[14] Since we use birth cohorts from 1965 onwards, we need data from surveys earlier than 2005. In this figure, we work with the less precise definition 2 of treatment for all cohorts.

2008 and use the more precise definition of treatment. The coefficient on East is negative and highly significant, reflecting lower college graduation rates in the East than in the West for the birth cohorts 1971 to 1977, as already visible in Figure 2.[15] The coefficient of main interest on the interaction term between East and Treatment is positive and significant, indicating that being enrolled at an older age in the East and thereby receiving one year less of socialist education increases the probability of attaining a college degree by 2.1 (column (i)) to 2 (column (ii)) percentage points more than being enrolled at an older age in the West. Given a college graduation rate of around 15 percent in the East, this is a very large effect.[16] Columns (iii) and (iv) show that the effect is equally present for both men and women, and is slightly larger for women than for men.

Panel B of Table 2 shows the results of specification (2) and decomposes the cohorts used in the analysis into the two cohort groups 1971 to 1973 and 1974 to 1977, first using the full sample (column ii), and then splitting the sample into males (column iii) and females (column iv). Focusing on the coefficients of interest, the interaction term between East and Treatment has a significantly positive coefficient of very similar size for both cohort groups. When the sample is split into females and males, only the coefficient for females in the older cohort group remains significant. However, the other three coefficients are of the same order of magnitude, yet with larger standard errors.[17] It is interesting that the large effect of socialist education on college graduation rates is present and of similar size for the cohort group 1971 to 1973, which was differentially affected by socialist education in terms of non-meritocratic access restrictions, and the younger cohort group 1974 to 1977, which was differentially affected in terms of length of socialist education. Even for this younger group, the large effects are present, indicating strong effects of socialist indoctrination and socialist teaching styles. The fact that the coefficient is not larger for the older cohort group than for the younger one implies that differential exposure to length

[15] Due to the inclusion of state dummies, the East dummy is omitted in columns (ii) to (iv). The coefficient on treatment is insignificant in the specification without control variables, and only becomes negative and significant when the control variable month of birth is included. This is mostly driven by females, as columns (iii) and (iv) show.

[16] The effect is larger than the one shown in Figure 2. Note that Figure 2 uses the imprecise definition of treatment (Definition 2) since we need to use survey years prior to 2005, while in the regression we use the precise definition of treatment (Definition 1).

[17] p-values vary between 0.11 and 0.19.

of socialist education did not matter for the older cohort group, or alternatively non-meritocratic access restrictions played no role. We conjecture that the former is true, because of the significantly lower teaching time in apprenticeship training, and because less teaching time was devoted to socialism in both apprenticeship and EOS, as documented in Section 2.2. Moreover, the longer-term labor market outcomes show differential results for both cohort groups. In the web appendix, Panel A of Figure W1 shows coefficients on the relevant interaction terms if we run specification (1) cohort by cohort. The treatment effect is declining by cohort for the younger cohort group 1974 to 1977, which might indicate that individuals who were in lower grades than 7th grade at reunification might not experience long-term effects of socialist education on college attainment anymore. Yet, it could also be random variation, and one would need to investigate this issue in the future with data on younger generations.

Summarizing the college results, we find significant negative effects of the length of exposure to socialist contents of education and restricted access to higher education in the GDR on college completion rates. In Table W2 in the web appendix, we show that this result is robust to using years of schooling as outcome variable. Additionally, we report there robust evidence that treated respondents in the East are significantly less likely to have completed vocational education as the highest educational degree; thus, treatment induced respondents to switch from completing a vocational education as highest degree to obtaining a college degree. However, we also find that within cohorts 1971 to 1973 treated respondents are more likely to not complete any official vocational degree (although the effect is not significantly different from zero, with a p-value of .21), an issue we get back to in the next section.

4.2 Results for Longer-Term Labor Market Outcomes

For the four labor market outcomes employment, working hours, wages, and professional occupation, we directly present results of specification (2), each time presenting results on the full sample as well as separately for males and females. We discuss the results for the two cohort groups separately.[18]

Starting with the older cohort group, born 1971 to 1973, Table 3 shows that being treated in the East is associated with a significantly lower likelihood of being employed

[18] Web Appendix Table W3 reports also coefficients on the treatment dummy, and on the interaction term of East and treatment if we do not split the sample into the two cohort groups.

than in the West, but with significantly higher wages, as well as a higher probability of being a professional. The employment effect is present for both men and women, though it is less significant for women than for men, while the effects on wages and professional status are only present for men. They are however quite large for men, indicating a 4.1 percent larger effect of treatment on wages in the East than in the West, and a 5.1 percentage points larger effect on the probability of being employed as a manager or professional.[19] Restrictions in access to education imposed on this cohort group appear to have had significant long-term effects on the labor market success of men in terms of making a career. The elimination of non-meritocratic restrictions in access to college and apprenticeship choice allowed able male students to acquire the human capital they needed to achieve a better occupational status in the labor market. Estimates on college returns in Germany typically lie between 20 percent and 40 percent (see Fuchs-Schündeln et al., 2010, and OECD, 2006). If we accept the estimate of 20 percent coming from a Mincerian regression by the OECD (2006) as the college wage premium in Germany, and take as the mean estimate of Table 2 that the effect of treatment on college is about 2 percentage points for both cohort groups, corresponding to roughly 10 percent of the mean of the college variable, then we would expect an effect of treatment on wage from college alone of 2 percent (0.1*0.2=0.02). So these effects are (roughly) in line with an interpretation that for the older cohort group, there are effects on wages that go beyond college, namely the general abolishment of non-meritocratic access restrictions leading to better matches.

What is very interesting is the contrasting result on employment: being treated leads to a lower probability of being employed in the East than in the West. We conjecture that this effect might come from an increasing variance in labor market success after reunification. Individuals in the control group were more advanced in their apprenticeship at reunification and therefore might have been more likely to finish their vocational education. Individuals who potentially struggle in the labor market might have been better taken care of in the regulated GDR system than in the free labor market of the FRG. Therefore, the overall effect of being treated for this cohort group leads to a larger spread in labor market outcomes: there are more individuals not being employed, but conditional on being employed treatment

[19] In Web Appendix Section W2, we provide more disaggregated evidence. Using the sample of male respondents in the East, we compare the entire occupational distribution of treatment and control groups within birth cohorts 1971-1973, and discuss the results.

leads to higher wages and a higher probability of achieving a professional status in the East than in the West. This is consistent with the results on not obtaining any vocational degree in Web Appendix Table W2, which show weak evidence of a higher proportion of respondents with the lowest possible educational achievement among treated individuals within cohorts 1971 to 1973.[20]

For the younger cohort group born 1974 to 1977, being treated in the East is associated with a higher probability of being employed, as well as longer working hours, than being treated in the West.[21] Both effects are only present for males. This is in line with a longer exposure to socialist teaching being detrimental for individual initiative and motivation. We conjecture that the development of an educational system after reunification that aimed at encouraging independent thinking and individual initiative rather than the creation of a socialist personality may have stimulated individual participation in the labor market, as well as effort in job searches and in the workplace. The sizes of the coefficients are relevant from an economic point of view for both employment and working hours: male respondents born between 1974 and 1977 are 2.1 percentage points more likely to be employed, and also increase their working hours by 1.5 percent when they receive one year less of socialist education, compared to treated individuals in the West.

Panels B to F of Figure W1 in the Web Appendix present results from regressions run cohort by cohort for any significant result in Table 3. There are some oscillations in the size of the coefficient on the interaction term cohort by cohort, but most effects are economically significant for all cohorts.[22] Interestingly, there is no clear decreasing trend of the effect for the younger cohort group as we find for college attainment.

For females, none of the coefficients on the interaction variables of interest in

[20] Selection can also play a role here: if the least abled are not employed, then the treated employed group has on average a higher ability level, which might explain higher wages and higher chances of having a professional job. Web Appendix Section 3 performs a bounding exercise of the potential magnitudes of the possible biases. We find as an upper bound that self-selection biases the treatment effect up by 0.008 in the case of professional status, and 0.012 in the case of wages, i.e. without selection treatment would raise the probability of receiving a professional job by 4.3 percentage points as a lower bound, and would increase wages by 2.9 percent as a lower bound.

[21] For the younger cohort group born 1974 to 1977, being treated in the East is also associated with higher wages. This effect, however, is only marginally significant, and it is not robust to several of the robustness checks we present in Table 4 and in Web Appendix Tables W7 and W8.

[22] The two exceptions are the employment effect for cohort 1971 in panel B, and the employment effect for cohort 1977 in panel E.

Table 3 are significant.[23] We believe that the lack of significant results for females, in contrast to males, can be explained by their generally lower labor market attachment. Women typically experience labor market breaks in their career, and especially at the beginning of their career, through the arrival of children. These breaks often lead to new professional orientations. Therefore, any initial effects of restricted access to education or to the desired apprenticeship might show up less strongly for women over time than for men. It is interesting that for college graduation, which in the majority of cases happens before children are born, we see similar effects for females as for males in terms of the size of the coefficients.

5 Ruling Out Alternative Interpretations of the Results

In this section, we conduct a series of analyses that rule out alternative explanations of the results. We conduct these analyses only on the significant results from Tables 2 and 3. For college attainment, we always show results obtained using the whole sample, given that results were very similar for both cohort groups and for males and females. Concerning labor market outcomes, for the dependent variables wage and professional status we show results only for the older cohort group and the male sample,[24] for the dependent variable working hours only for the younger cohort group and the male sample, while for the employment status we show results for both older and younger cohort group (but in this last case only using the male sample).

5.1 Are West Germans a Valid Control Group?

An underlying assumption of our exercise is that West Germans are a valid control group for East Germans. One might be worried about this for different reasons. Since the school systems in East and West Germany were very different (e.g. due to tracking in the West German school system), the effect of enrollment at an earlier age could differ in East and West. Moreover, as previously discussed, incorrect assignment of treatment due to grade repetition or early/late enrollment into school may bias the

[23] All results for females, as well as for males, are robust to controlling for the number of children. These are shown in Web Appendix Tables W4 to W6.

[24] For wages and professional status, the interaction term of interest is also significant in the full sample, but these results are entirely driven by the male sample.

estimates of our coefficients of interest. In particular, this may be the case if the share of respondents who repeat their grade is systematically different in treatment and control group, and such a difference depends on whether we consider the sample of East or West respondents. Last, one general worry about the results could be that they might capture differences in age trends between East and West residents, since treatment and control individuals are slightly different in terms of age.

Yet, any of these potential biases should show up also for older cohorts born before 1971. Thus, to rule out these alternative interpretations of the results and biases, we run regressions on placebo cohort groups, namely cohorts born between 1961 and 1970.[25] All respondents born 1970 or before had already completed primary and secondary education when the Berlin Wall fell, and were thus not differentially affected by socialist education, but would face the same biases driven by differential age trends, early/late enrollment or grade repetition.[26]

Figure 3 presents the results for different placebo cohort groups, starting from the cohort group 1961 to 1970, 1962 to 1970, up to 1968 to 1970. The figure shows the coefficients on the interaction variable of interest between East and Treat. In Panel A, college attainment is the dependent variable, while Panels B to F focus on the labor market outcomes. For comparison, the panels also show the coefficients for our two relevant cohort groups 1971 to 1973 and 1974 to 1977 from Tables 2 and 3. None of the coefficients of interest on the interaction of East and Treatment are significant for the placebo cohort groups, and indeed most are very close to zero. Thus, we are quite sure that we indeed capture effects of socialist education, rather than general East or West German trends or biases that should also have been present for cohorts 1970 and older.

5.2 General Exposure to Socialist Regime

We next address the possibility that our results are driven by general exposure to a socialist regime and lifestyle, rather than specifically by socialist education. Due

[25] For these robustness checks, we have to use the survey years from 2004 and before in order to observe these cohorts at the same age 31 to 35 as the individuals in our baseline analyses. Thus, we have to recur to the second, less precise definition of treatment (being born on or after May 1). In Web Appendix Table W7, we show that our main results are robust to using this less precise definition of treatment.

[26] An underlying assumption is that East-West differences in regular enrollment and grade repetition did not change significantly for cohorts born before or after 1971.

to their younger age, respondents in the treatment group have been less exposed to socialist culture in general if they come from the East.

The exercises performed in the previous subsection should already be enough to rule out the possibility that length of exposure to socialist culture in general matters, unless we believe that exposure to socialist culture should have a stronger impact at a younger age and in particular at the age when cohorts 1971 to 1977 experienced reunification. Still, we perform an additional robustness check, in which we restrict the sample to respondents born between February and September. Thereby, we are comparing individuals whose age differences are very small, and therefore it is less likely that results are driven by different exposure to socialist culture in general. Results for college graduation are presented in Panel A of column (i) of Table 4, while Panels B and C present results using labor market outcome variables for the older and younger cohort group, respectively. As expected, all coefficients of interest remain of the same sign as in the baseline results and significant.[27]

5.3 Year of Labor Market Entrance

Since individuals in the treatment group enter school one year later, they also enter the labor market one year later than individuals in the control group from the same birth cohort. Given that the East German labor market was in a recession after reunification, this might have mattered. In fact, unemployment rates generally increased in the first years after reunification (see Figure W4 in the web appendix). In order to control for labor market conditions after reunification, we collect information on the unemployment rates of the total and male population in East and West Germany from 1991 onwards from the Federal Employment Agency. We then generate a variable "unemployment rate in the year of labor market entrance", which associates to each individual the unemployment rate observed in East or West Germany (depending on the current residence) 20 years after the individual's birth year for individuals in the control group, and 21 years for individuals in the treatment group, who enter the labor market one summer later. Based on data from the German Socio-Economic Panel, the average age at the first job for individuals fitting our sample criteria is 20.7

[27] Web Appendix Table W8 shows results from a placebo treatment, in which we consider only individuals born between July and December, defining those born between October and December as the placebo treatment group, and those born between July and September as the placebo control group. None of the placebo treatment coefficients is significant.

in the East and 20.6 in the West for individuals in the treatment group, and 20.2 or 20.1, respectively, for individuals in the control group.

We add this newly created variable "unemployment rate in the year of labor market entrance", either referring to the total or the male population, as a control to the baseline specifications in columns (ii) and (iii) of Table 4. Column (iv) instead adds linear trends in the year of labor market entrance; this way, we do not have to make any assumptions on when exactly the individuals enter the labor market, but only that each school cohort enters at the same point in time. All results are robust to adding these additional control variables.[28]

5.4 East-West Migration

One unfortunate feature of the German Microcensus is that it provides information only on the current residence of the respondents, but not on their residence before reunification. Therefore, when generating the variable "East", we are implicitly assuming that respondents currently residing in the East (West) also received their education in the East (West). This assumption needs to be carefully discussed given that migration flows from East to West Germany have been substantial (see e.g. Hunt (2006) and Fuchs-Schündeln and Schündeln (2009)).[29] In particular, this assumption may bias the coefficient of the interaction term between the variables "East" and "Treatment" upwards, if the most talented and hard working respondents in the treatment group were less likely to migrate from the East towards the West than the corresponding most talented and hard working respondents in the control group.

While the Microcensus does not provide a full migration history of each respondent, it reports the state of residence in the current and the previous year (except for

[28] In Table W9 in the Web Appendix, we show a battery of additional robustness checks controlling for unemployment rates in the year of labor market entrance, namely including quadratic terms in the total or male unemployment rate, controlling for the youth unemployment rate (in levels and squared) or state-specific unemployment rates (again in levels and squared). Last, we include state-specific linear trends. The youth unemployment rate is only available from 1993 on, such that we can only include individuals who entered the labor market in 1993 or later, which restricts the sample to the cohort group 1974-1977.

[29] A quick back-of-the-envelope calculation reveals that as an upper bound, East Germans constitute 4.7 percent of the West sample. This calculation is based on cumulative East-West flows from 1989 to 2006, and assumes that 20% of East-West migrants return to the East. The rate of East German return migrants is calculated to be 20% based on German Socio-Economic Panel data by Fuchs-Schündeln and Schündeln (2009), who indicate that this number is a lower bound.

the survey years 2004, 2006, and 2007). Using the sample of all respondents belonging to cohorts 1971 to 1977 who resided in East Germany 12 months prior to the interview and all surveys from 1991 onwards, this allows us to check whether respondents in the treatment group were more likely to migrate in the next year than respondents in the control group. The results are shown in Web Appendix Table W10.[30] Both with and without including control variables, the coefficient on the treatment variable is very close to zero and far from being significantly different from zero, suggesting that it is highly unlikely that patterns of migration have been somehow influenced by the reorganization of the educational system after 1989. Since migration to West Germany was not negligible, however, we need to stress that our findings are only restricted to the fraction of East Germans who did not move to the West after reunification. To achieve a definite answer to the question whether migration explains the results, we would need to be able to analyze the migration pattern not only based on treatment, but also based on the ability of the individuals interacted with treatment.

5.5 Robustness Checks

In Table W7 in the Web Appendix, we also show that results are robust to (i) including data from 2004 and before, working with the less precise Definition 2 of treatment; (ii) defining treatment in the West as being born between July and December, given that the relevant cut-off date for schooling was typically June 30 in the West in the 1980s, not May 31 as in the GDR; (iii) dropping respondents from May, June, or July, for whom wrong assignment to treatment or control is most likely in case of early or late enrollment; and (iv) redefining the college variable by assigning college status also to an individual whose highest educational degree is from a *Fachschule.* All results are robust.

[30] To use the early survey years, we have to rely on the less precise definition 2 of treatment. Web Appendix Table W11 shows the results year by year: in two years, namely 1993 and 1997, the treated were significantly more likely to migrate West, while in one year, namely 1992, they were significantly less likely to do that. Yet, there is no clear trend in migration of treatment vs. control group.

6 Conclusion

The event of German reunification and the rapid transformation of the educational system in East Germany towards a Western model provide a unique setting to assess how political regimes influence individual lives through education. We identify two possible channels: authoritarian, and in particular socialist, forms of government often (i) adopt non-meritocratic criteria to select students and grant access to higher education, and impose restrictions on occupational choice based on central planning, and (ii) shape the contents of curricula to indoctrinate pupils and preserve consensus towards the regime in power, with a teaching style that does not encourage critical discussion. We find that the removal of both these features of the socialist education system increases the likelihood of obtaining a college degree of respondents resident in East Germany, once the transition towards a capitalist society is completed.

Interestingly, the two channels have quite different effects on labor market outcomes. Elimination of restrictions to access to college and restrictions put on occupational choice allows skilled students to acquire higher human capital and therefore better paid jobs. At the same time, we find some suggestive evidence that less able students face a higher likelihood of being non-employed. Thus, the elimination of these restrictions might have increased the variance of labor market outcomes. The abolishment of socialist teaching instead has different consequences. The transition to a system where individual initiative is encouraged leads to higher participation in the labor market and higher effort in the workplace, expressed in a larger number of working hours. The effects that we find in the labor market are, however, limited to the male sample.

One caveat of our analysis is that it is carried out against the background of a depressed labor market in East Germany right after reunification. The high unemployment rates might have made it more difficult for young people to switch education and adjust their occupational choice after the fall of the Berlin Wall, and therefore the effects of restricted occupational choice and non-meritocratic restrictions in access to college might have been larger than in settings with a booming labor market. Yet, this concerns the quantities of the results, not the qualitative results.

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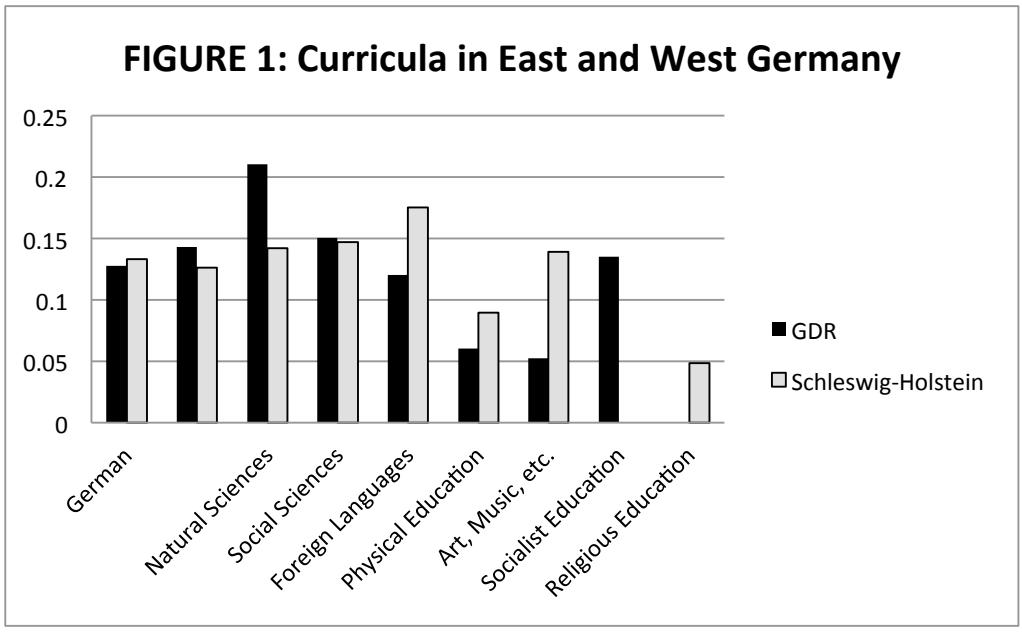
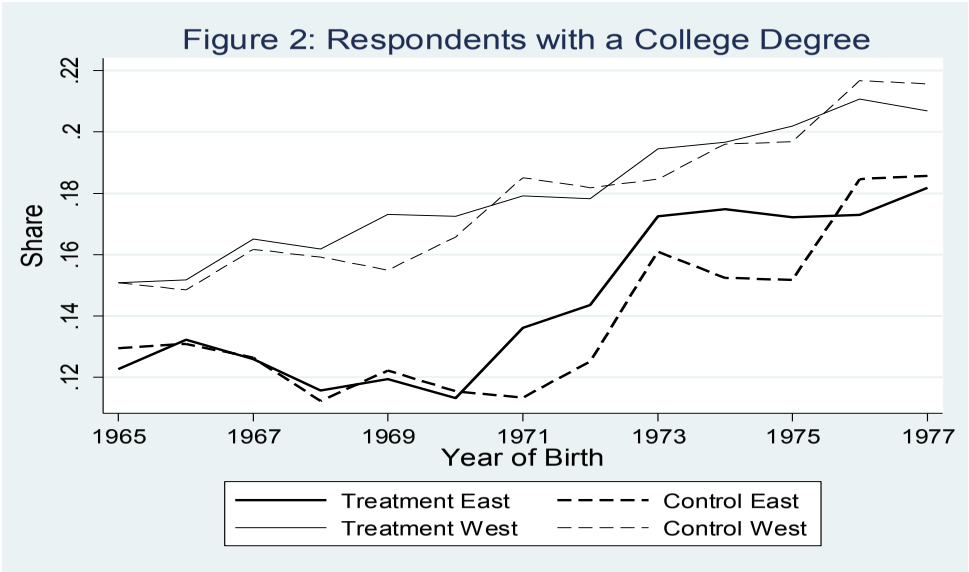
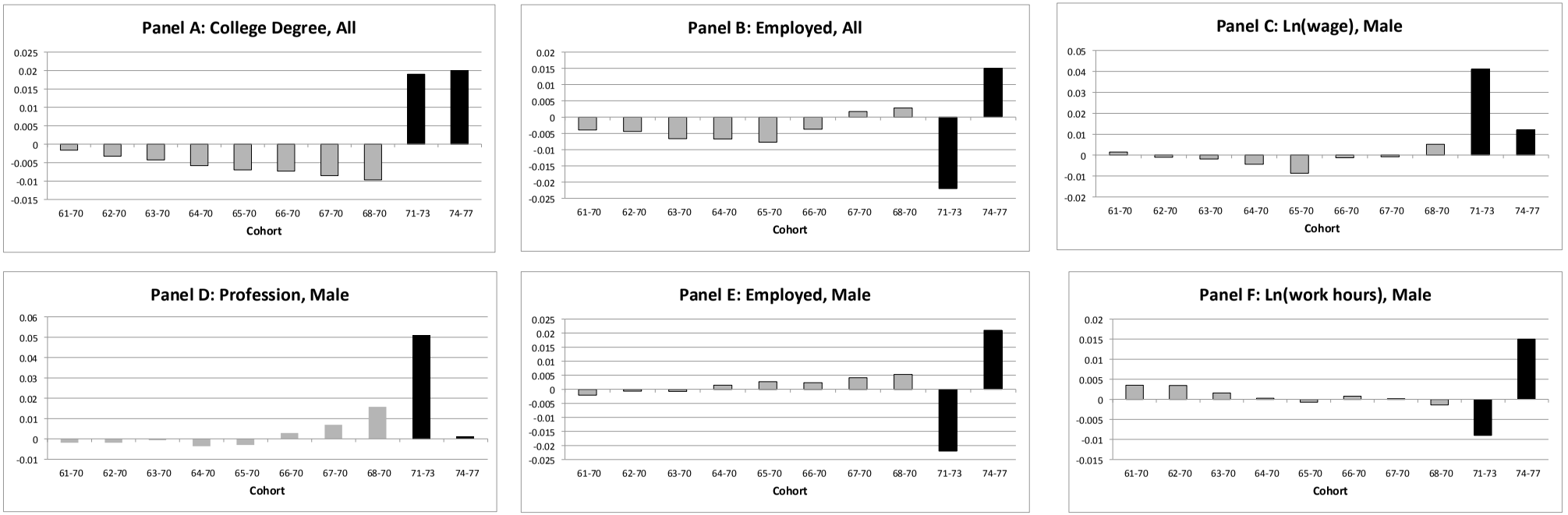


NOTE: This figure shows the share of overall teaching time devoted to certain subject areas in East Germany and the Western State of Schleswig-Holstein on average over the grades 7 to 10.



NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus, own calculations. In this figure, we plot the fraction of respondents aged 31-35 who obtained a college degree by cohort of birth. The Figure uses observations from survey years prior to 2005, and thus we have to work with the less precise definition 2 of treatment, which we do consistently in this Figure. We distinguish 4 groups: (i) group Control East includes all respondents born in the months January to April and residing in the East at the time of the survey; (ii) group Treated East includes all respondents born in the months May to December and residing in the East at the time of the survey; (iii) group Control West includes all respondents born in the months January to April and residing in the West at the time of the survey; (iv) group Treated West includes all respondents born in the months May to December and residing in the West at the time of the survey. Residents in the State of Berlin are dropped from the sample.

FIGURE 3: Regressions for Placebo Cohort Groups, College Attainment and Labor Market Outcomes



NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus, own calculations. This figure shows coefficients of regressions of different labor market outcomes on treatment interacted with East. The sample consists of all respondents aged between 31 and 35 of the respective cohort group indicated in the figure. In Panel A, the dependent variable is a dummy variable equal to one if the respondent obtained a college degree. In Panels B and E, the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Panel C, the dependent variable is the log of wages. In Panel D, the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In Panel F, the dependent variable is the log of the number of weekly working hours. Controls include the treatment dummy, age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Treatment is defined according to Definition 2, i.e. respondents born between May and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. Residents in the state of Berlin are dropped from the sample.

TABLE 1: Sample Characteristics

		East		West	
	mean/percent	Treated mean/percent	Control mean/percent	Treated mean/percent	Control mean/percent
	(i)	(ii)	(iii)	(iv)	(v)
College	0.196	0.169	0.149	0.196	0.196
Employed	0.740	0.740	0.755	0.738	0.749
Ln (work hours)	3.497	3.566	3.568	3.487	3.479
	(0.494)	(0.380)	(0.379)	(0.510)	(0.520)
Ln (wage)	2.164	1.970	1.970	2.190	2.210
	(0.563)	(0.527)	(0.526)	(0.565)	(0.564)
Professional	0.463	0.423	0.404	0.467	0.462
Male	0.495	0.515	0.515	0.488	0.495
Age	33.0	32.9	33.0	32.9	33.0
	(1.381)	(1.398)	(1.385)	(1.378)	(1.380)
East	0.153				
Treated (Definition 1)	0.561				

NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus 2005-2008, own calculations. The sample includes only respondents aged between 31 and 35 and born between 1971 and 1977. When the variable "East" is used, we drop respondents from the state of Berlin. The samples in columns (ii) and (iv) include all respondents born between June and December. The samples in columns (iii) and (v) include all respondents born between January and May. I.e., treatment refers to our "Definition 1" of treatment. Standard deviations are in parentheses.

TABLE 2: Main Results

Dependent variable:	College Degree			
Panel A: cohorts 71-77				
	(i)	(ii)	(iii)	(iv)
			males	females
East	-.047***			
	(.008)			
Treated (Def. 1)	-.0004	-.013**	-.007	-.018**
	(.005)	(0.005)	(0.007)	(.008)
East*Treated (Def. 1)	.021*	.020***	.018*	.022**
	(.011)	(.007)	(.009)	(.010)
Controls		x	x	x
Observations	139605	139605	69090	70515
Panel B: cohorts 71-73 vs. 74-77				
		(ii)	(iii)	(iv)
			males	females
East*Treat*cohort71-73		.019*	.015	.022**
		(.010)	(.011)	(.010)
East*Treat*cohort74-77		.020**	.020	.022
		(.010)	(.012)	(.015)
Controls		x	x	x
Observations		139605	69090	70515

NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus 2005-2008, own calculations. The dependent variable is a dummy variable equal to one if the respondent obtained a college degree. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. The variable East is dropped from the specifications in columns (ii) to (iv) since state of residence dummies are added. Controls in columns (ii) to (iv) include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. In Panel B, we generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, the other that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE 3: Labor Market Outcomes

Dependent variable:	**Employed**			**Ln (work hours)**			**Ln (wage)**			**Professional**		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
	all	males	females	all	males	females	all	males	females	all	males	females
East*Treat*cohort71-73	-.022**	-.022*	-.021	-.019	-.009	-.035	.026***	.041***	.006	.026**	.051***	-.004
	(.009)	(.012)	(0.014)	(.012)	(.008)	(.028)	(.008)	(.011)	(.016)	(.011)	(.009)	(.014)
East*Treat*cohort74-77	0.015	.021***	.009	-.005	.015***	-.027	.018*	.012	.022	-.001	.002	-.002
	(.010)	(.004)	(.019)	(.009)	(.005)	(.019)	(.010)	(.010)	(.014)	(.007)	(.015)	(.022)
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Observations	139605	69090	70515	113086	60838	52248	106496	57487	49009	111995	60140	51855

NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus 2005-2008, own calculations. In Columns (i)-(iii) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Columns (iv)-(vi) the dependent variable is the log of the number of weekly working hours. In Columns (vii)-(ix) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In Columns (x) to (xii) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, the other that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE 4: Further Analyses

Panel A : East*Treat					
	sample	(i) Feb-Sep	(ii) UR	(iii) male UR	(iv) market entr. trends
College	all	.026**	.020***	.020***	.020***
		(.012)	(.007)	(.006)	(.007)
Panel B : East*Treat*cohort71-73					
		(i)	(ii)	(iii)	(iv)
Employed	all	-.016**	-.020**	-.023**	-.021**
		(.007)	(.009)	(.009)	(.010)
Ln (wage)	male	.029**	.036***	.044***	.041***
		(.012)	(.008)	(.011)	(.011)
Professional	male	.045***	.045***	.056***	.051***
		(.012)	(.009)	(.009)	(.009)
Panel B: East*Treat*cohort74-77					
		(i)	(ii)	(iii)	(iv)
Employed	male	.043***	.022***	.023***	.021***
		(.013)	(.004)	(.005)	(.004)
Ln (Working hours)	male	.013*	.015***	.014***	.015***
		(.007)	(.004)	(.004)	(.005)

NOTE: Source: RDC of the Federal Statistical Offices and the statistical offices of the Länder, Microcensus 2005-2008, own calculations. Treatment is defined according to Definition 1 (unless it is differently specified), that is respondents born between June and December are considered treated. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, and a second that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between East and Treat in Panel A, and between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77, in Panels B and C. Panel A reports the coefficient on the variable East*Treat. Panel B reports the coefficient on the variable East*Treat*cohort71-73; Panel C reports the coefficient on the variable East*Treat*cohort74-77. In row (1), the dependent variable is a dummy variable equal to one if the respondent received a college degree. In rows (2) and (5) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In row (3) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (4) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In row (6) the dependent variable is the log of the number of weekly working hours. In Column (i) the sample is restricted to respondents born between February and September. Column (ii) adds the East/West unemployment rate in the year of labor market entrance as a control to the baseline regressions, column (iii) the respective male unemployment rate, and column (iv) East/West linear trends in the year of market entrance. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents (males and females) aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Online Appendix
"Long-Lasting Effects of Socialist Education"

1 Temporary Disruptions in Schooling

On top of the long-term changes that we are focusing on, namely the change in the content of education and teaching style, as well as the abolishment of non-meritocratic access restrictions, the change from a socialist to a Western style schooling system at reunification likely also created some temporary disruptions in teaching; for instance, some teachers might have spontaneously migrated to the West. While the literature discussed in Section 2.4 points to a quick transition to a new school system, these temporary disruptions cannot be ruled out.

Yet, there are three reasons why we do not think that they matter for our analysis. First, and most importantly, these disruptions were likely relatively short term, playing out in the first months after reunification, and thus it is unlikely that they would lead to the large long-run effects that we observe in the data. Secondly, as far as these disruptions were not short-term but rather extended for a longer time period, individuals from the treatment group should have been affected longer by these disruptions, as they were still in education for a longer time period after reunification. This should make it more difficult to detect any negative effects of socialist education, and therefore biases our estimates downwards. Thirdly, since individuals from the treatment group were in a lower school grade at reunification than individuals from the control group of the same birth cohort, they were affected by these temporary disruptions in a lower school grade. The literature (Chetty et al., 2014), however, does not provide evidence in favor of any systematic stronger effects of quality of teaching at higher grades.

2 Disaggregate Evidence on Labor Market and College Outcomes

In Web Appendix Figure W2, we provide more disaggregated evidence on labor market and college outcomes. Using the sample of male respondents in the East, we compare the entire occupational distribution of treatment and control groups within birth cohorts 1971-1973. Figure W2 shows that treated respondents in the East within the cohort group 1971 to 1973 are more likely to occupy managerial occupations and more in general occupations that require a college degree. They are significantly less likely to be a handicrafts person, mechanic, and craftsperson, as well as truck or train driver. Quite interestingly, we observe that treated and control groups within cohorts 71 to 73 seem to differ not only in terms of occupation, but also in terms of field of study. Treated respondents

in the sample of male respondents in the East are more likely to complete an economics degree, which might partly explain the increase in the likelihood of being employed in managerial occupations among treated respondents. This evidence is reported in Web Appendix Figure W3.

3 Bounding Exercise

When we consider wages and type of occupation as dependent variables, the main identification strategy is comparing the difference between the outcomes of (employed) respondents in the East in the treatment and control group with the difference between the outcomes of (employed) respondents in the West in the treatment and control group. If the least able are not employed because of treatment, however, then the treated employed group has on average a higher ability level, which might explain higher wages and higher chances of having a professional job. We perform a bounding exercise in the spirit of Card et al. (2009) and Lee (2009) in order to understand the possible magnitude of the selection bias. As in Card et al. (2009) and Lee (2009), we start by performing the bounding exercise using the specification in which the dependent variable is a dichotomous variable: having a professional job or not. In this case, unlike the case of the wage variable, the worst outcome is naturally defined by the lowest possible value of the variable, 0. We assume that the respondents who are not employed because of treatment would have displayed this worst labor outcome. Let us call T_1 the group of respondents in the East in the treatment group who are employed and T_2 the group of respondents in the East in the treatment group who are not employed becuase of treatment. α and $(1-\alpha)$ are the sizes of T_1 and T_2 respectively. $P_{T_1,E}$, $P_{T_2,E}$ and $P_{C,E}$ are the fraction of respondents with a professional job resident in the East belonging to groups T_1, T_2 and control group respectively; similarly $P_{T,W}$ and $P_{C,W}$ are the fraction of respondents with a professional job resident in the West belonging to the treated and control group respectively.

The coefficient we observe using our current identification strategy is determined by the following expression:

$$\beta^{OBS} = [P_{T_1,E} - P_{C,E}] - [P_{T,W} - P_{C,W}]$$

while the true causal effect of treatment on type of occupation would be:

$$\begin{aligned} \beta^{CAUS} &= [(\alpha P_{T_1,E} + (1-\alpha) P_{T_2,E}) - P_{C,E}] \\ &- [P_{T,W} - P_{C,W}] \end{aligned}$$

It is straightforward to rewrite the two expressions as follows:

$$\beta^{CAUS} = \beta^{OBS} - (1-\alpha)(P_{T_1,E} - P_{T_2,E})$$

where $-(1-\alpha)(P_{T_1,E} - P_{T_2,E})$ is the correction we need to apply to β^{OBS} to obtain an estimate of β^{CAUS}. In the last expression, $(1-\alpha)$ represents the

percent decline in employment experienced by residents in the East because of treatment. Given that we consider only the male sample (as our main findings are restricted to that sample), α corresponds to the ratio between the fraction of employed people in the treatment group in the East (male sample) and the sum of such fraction and the coefficient of East*Treat*cohort74-77 in col (ii) of Table 5, that is $\alpha = .975$. Using the Microcensus data we find that $P_{T_1,E} = .34$. As already argued, we assume the highest possible bias, therefore $P_{T_2,E} = 0$. The size of the correction we need to apply then is $(1-.975)*.34 = .008$ and the bounded estimate of our coefficient of interest is $\beta^{CAUS} = .051 - .008 = .043$.

Obviously, since wage is a continuous variable, it is less easy and rigorous to assume a lower bound of the coefficient obtained in col (viii) of table 5 where the log of the individual wage is the dependent variable. We however propose a complementary exercise to address this issue and assume that respondents who did not get employed as a result of treatment would have received a wage equivalent to the lowest decile of the male wage distribution. The size of the bias in this case would be $(1-\alpha)\left(\ln W_{T_1,E} - \ln W_{T_2,E}\right)$, where $\ln W_{T_1,E}$ would be the average of the log of the wages of (employed) respondents in the male sample of East residents in the treatment group, that is 2.23 and $\ln W_{T_2,E}$ would be set to the lowest decile of the male wage distribution, that is 1.72. The correction bias to be applied would therefore correspond to $(1-.975)*(2.23-1.72) = 0.012$, and the bounded estimate of our coefficient of interest is $\beta^{CAUS} = .041 - .012 = .029$.

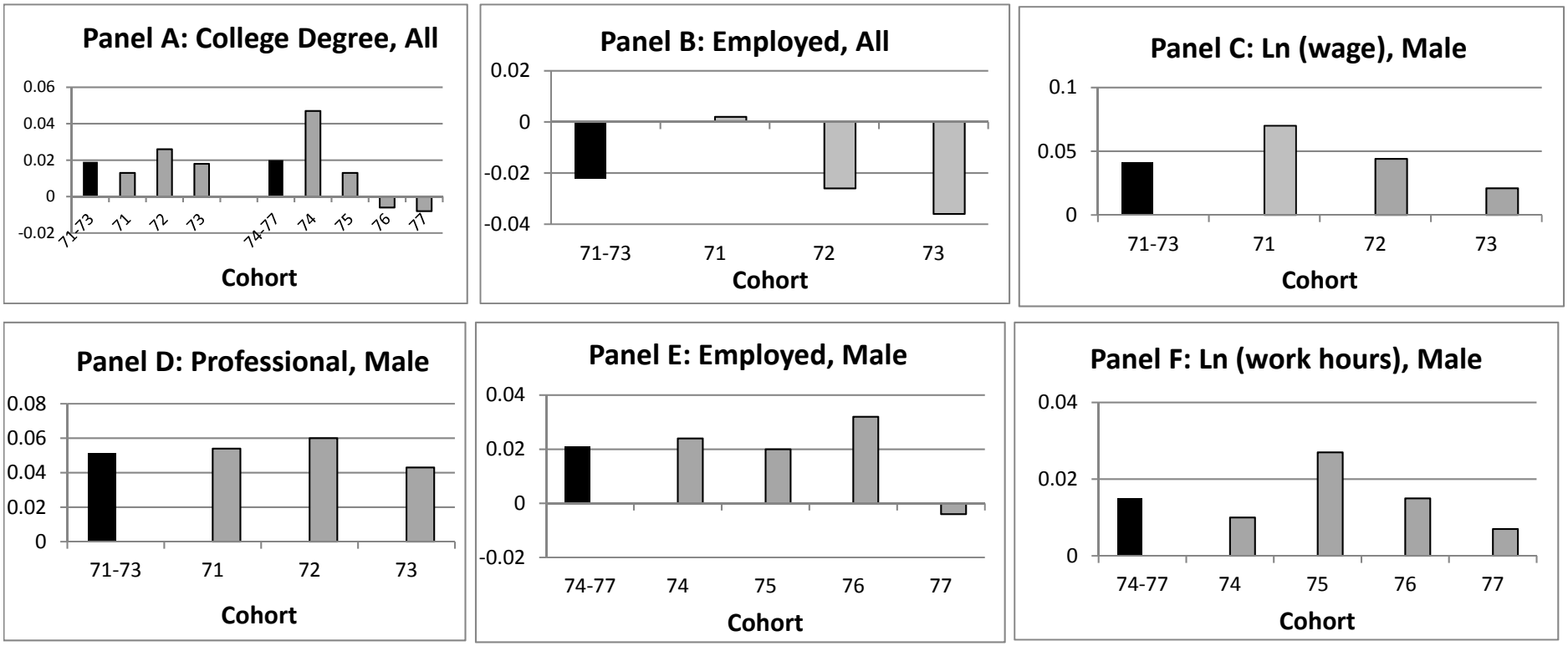
References

[1] Card, David, Carlos Dobkin, and Nicole Maestas (2009). "Does Medicare Save Lives?", Quarterly Journal of Economics, 124(2): 597–636.

[2] Chetty, Raj, John N. Friedman, and Jonah E. Rockoff (2014). "Measuring the Impact of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood", American Economic Review, 104(9): 2633-2679.

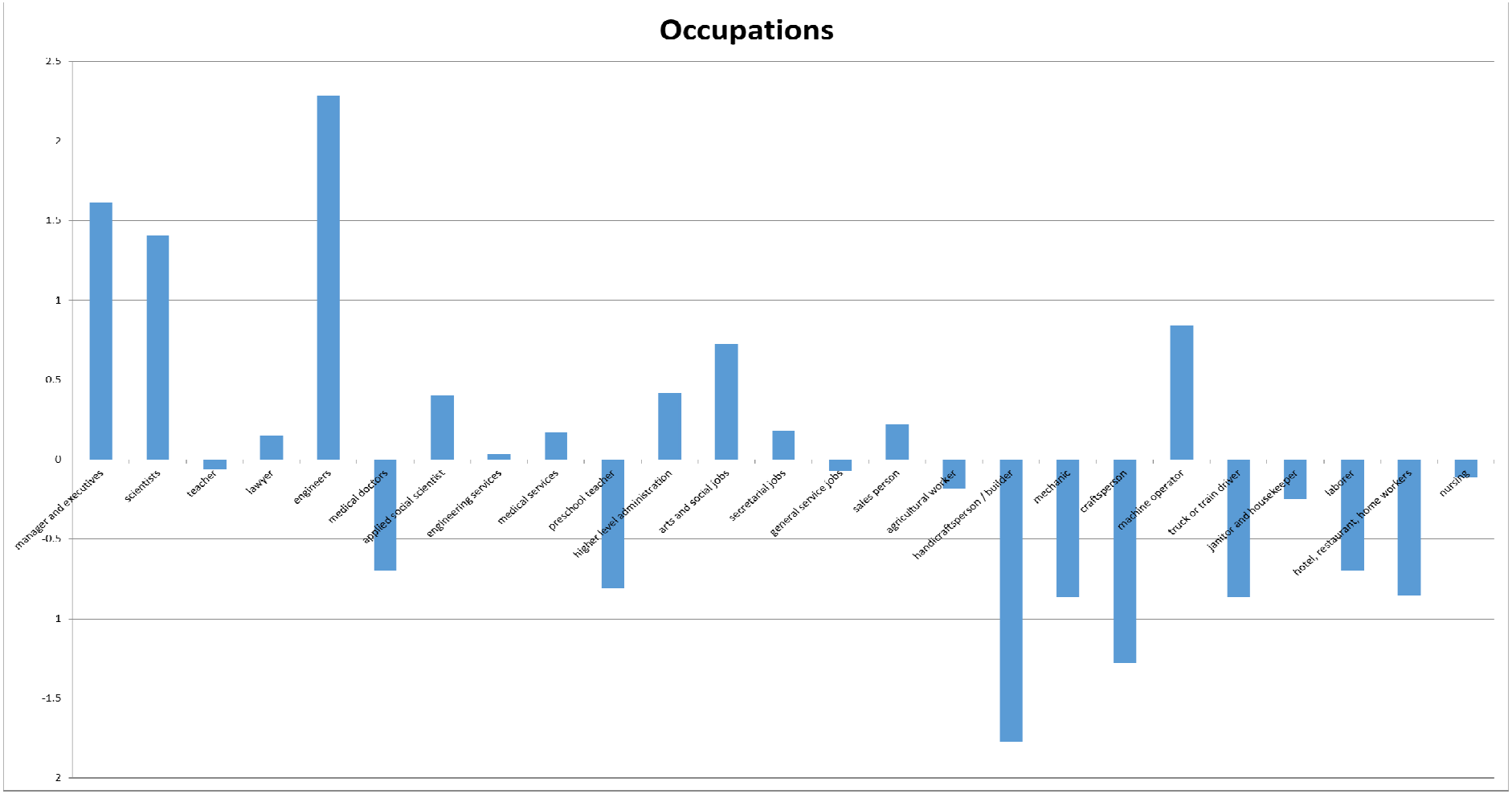
[3] Lee, David S. (2009). "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects", Review of Economic Studies, 76(3): 1071–1102.

FIGURE W1: Long-term College Attainment and Labor Market Outcomes Cohort-by-Cohort



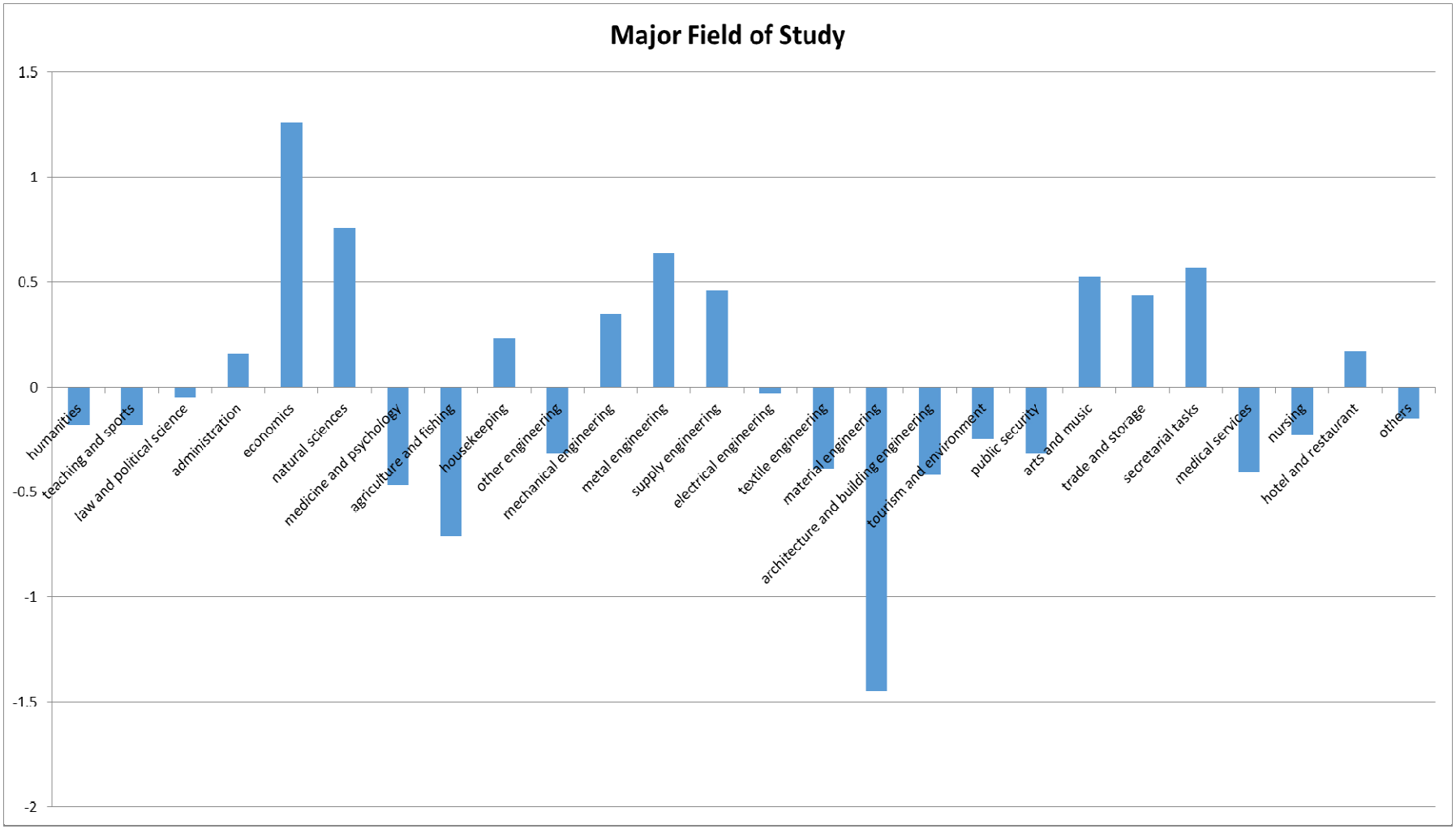
NOTE: The black columns show coefficients reported in Tables 2 (Panel A) and 3 (Panels B to F). The gray columns show coefficients of cohort-by-cohort regressions (i.e. each regression includes only one single cohort) of labor force outcomes on treatment interacted with East. In Panel A, the dependent variable of each regression is a dummy variable equal to one if the respondent obtained a college degree. In Panels B and E, the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Panel C, the dependent variable is the log of wages. In Panel D, the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In Panel F, the dependent variable is the log of the number of weekly working hours. Controls include a treatment dummy, age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. The sample consists of all respondents aged between 31 and 35. Residents in the state of Berlin are dropped from the sample.

Figure W2: Occupational Distribution for East German Cohort 1971-1973, Treat-Control Group (Male Sample)



Note: For each occupational category we plot the difference between the share of workers belonging to that category within the treatment group and the share of workers belonging to that category within the control group. The sample consists of male respondents resident in the East and born between 1971 and 1973. Occupations from the left up to applied social scientist typically require a college degree.

Figure W3: Major Field of Study Distribution for East German Cohort 1971 to 1973, Treat-Control Group (Male Sample)



Note: For each field of study we plot the difference between the share of respondents belonging to that field within the treatment group and the share of respondents belonging to that field within the control group. The sample consists of male respondents resident in the East and born between 1971 and 1973.

FIGURE W4: Labor Market Conditions in East and West Germany

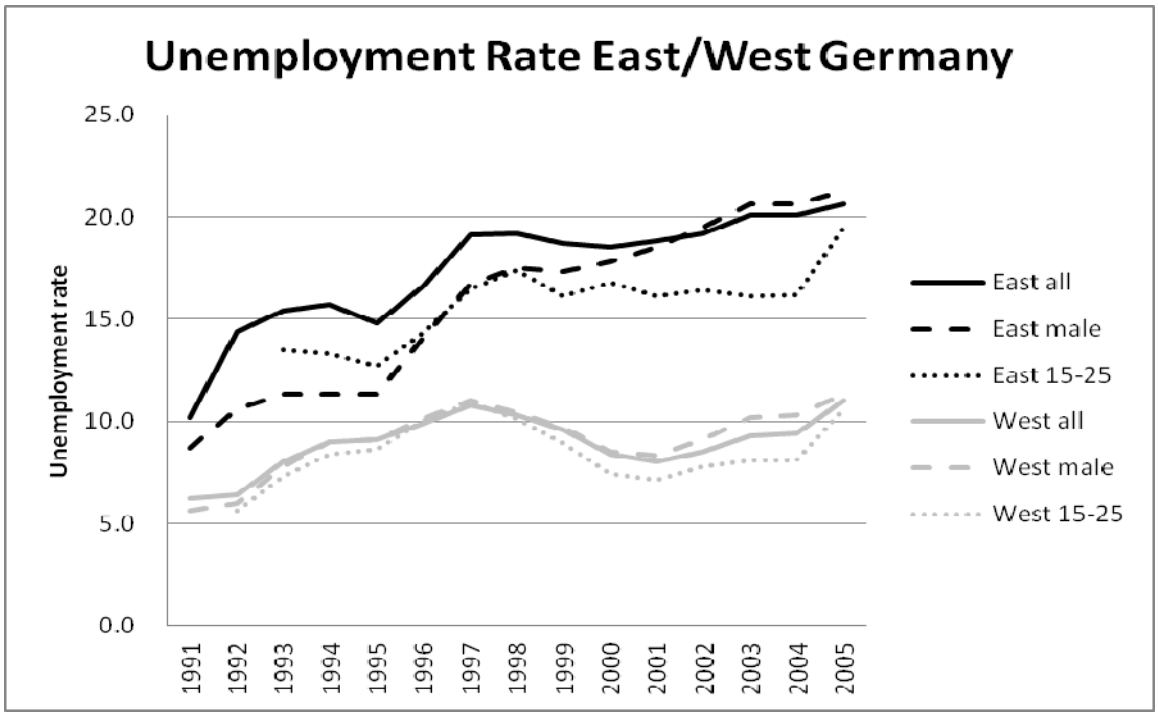


TABLE W1: Difference in RD

	sample	
College	all	.058***
		(3.56)
Ln (wage)	male	.15***
	cohort71-73	(3.19)
Professional	male	.055
	cohort71-73	(1.24)
Employed	all	-.012
	cohort71-73	(0.47)
Employed	male	.045
	cohort74-77	(1.26)
Ln (working hours)	male	-.04
	cohort74-77	(1.418)

Note: This table shows results from a difference in regression discontinuity approach as in Carneiro et al. (2015). Individuals born in the month of June are omitted, due to different assignment in treatment and control group in West and East. A 5 months uniform kernel bandwidth is used for the estimation. Z-statistics are computed based on the assumption that both RD estimates in East and West are independent, as in Carneiro et al. (2015).

TABLE W2: Alternative Measures of Educational Attainment

Dependent variable:	**Years of Schooling**		**Vocational**		**Unlearned**	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
East*Treated (Def. 1)	.139**		-.024***		.006	
	(.058)		(.007)		(.008)	
East*Treat*cohort71-73		0.096		-.025***		.009
		(.081)		(.008)		(.007)
East*Treat*cohort74-77		.182**		-.022*		.002
		(.080)		(.011)		(.007)
Sample	all	all	all	all	all	all
Observations	139605	139605	139605	139605	139605	139605

NOTE: In col. (i) and (ii) the dependent variable is the number of years of schooling (built following the methodology used by the GSOEP) , in col. (iii) and (iv) it is a dummy variable equal to one if the respondent obtained a vocational degree as highest educational attainment, in col (v) and (vi) it is a dummy variable equal to one if the respondent does not have any formal vocational degree (i.e. no college degree, no vocational degree, and no Fachschul-degree). Controls include age dummies, state of residence dummies, birth year dummies, a male dummy and month of birth. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W3: Labor Market Outcomes

Dependent variable:	**Employed**			**Ln (work hours)**			**Ln (wage)**			**Professional**		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
	all	males	females	all	males	females	all	males	females	all	males	females
Treat	-.012**	-.019**	-.006	0.01	-.002	.024*	-.015*	-.016	-.012	-.006	-.019	.011
	(.006)	(.007)	(.008)	(.008)	(.006)	(.013)	(.008)	(.012)	(.014)	(.007)	(.013)	(.009)
East*Treat	-.004	-0.001	-.007	-.013	.003	-.033	.022***	.028**	.013	.013	.027**	-.004
	(.010)	(.008)	(.016)	(.009)	(.006)	(.022)	(.006)	(.012)	(.014)	(.009)	(.013)	(.015)
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Observations	139605	69090	70515	113086	60838	52248	106496	57487	49009	111995	60140	51855

NOTE: In Columns (i)-(iii) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Columns (iv)-(vi) the dependent variable is the log of the number of weekly working hours. In Columns (vii)-(ix) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In Columns (x) to (xii) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W4: Main Results Controlling for the Number of Children

Dependent variable:	College Degree			
	(i)	(ii)	(iii)	(iv)
			males	females
East	-.047***			
	(.007)			
Treated (Def. 1)	-.0004	-.014**	-.008	-.019**
	(.005)	(.005)	(.007)	(.008)
East*Treated (Def. 1)	.021*	.019***	.0171*	.021**
	(.011)	(.007)	(.009)	(.010)
Controls		x	x	x
Observations	139605	139090	68759	70331

NOTE: The dependent variable is a dummy variable equal to one if the respondent obtained a college degree. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, month of birth. and the number of children. The variable East is dropped from the specifications in Columns (ii) to (iv) since state of residence dummies are added. Treatment is defined according to Definition 1, i.e. respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment - birth year- east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W5: Results by Cohort Group Controlling for the Number of Children

Dependent variable:	College Degree		
	(i)	(ii) males	(iii) females
East*Treat*cohort71-73	.017	.013	.021*
	(.010)	(.012)	(.011)
East*Treat*cohort74-77	.021**	.021*	.022
	(.009)	(.011)	(.015)
Controls	x	x	x
Observations	139090	68759	70331

NOTE: The dependent variable is a dummy variable equal to one if the respondent obtained a college degree. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, the other that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, month of birth, and the number of children. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W6: Labor Market Outcomes Controlling for the Number of Children

Dependent variable:	**Employed**			**Ln (work hours)**			**Ln (wage)**			**Professional**		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
	all	males	females	all	males	females	all	males	females	all	males	females
East*Treat*cohort71-73	-.024**	-.025**	-.023	-.021*	-.009	-.037	.028***	.041***	.010	.024**	.05***	-.007
	(.009)	(.011)	(0.015)	(.010)	(.008)	(.025)	(.007)	(.013)	(.016)	(.012)	(.01)	(.014)
East*Treat*cohort74-77	.016	.02***	.009	-.004	.015***	-.031	.013	.006	.017	-.0003	.003	-.003
	(.009)	(.004)	(0.016)	(.009)	(.005)	(.026)	(.011)	(.010)	(.014)	(.006)	(.015)	(.022)
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Observations	139090	68759	70331	112806	60674	52132	106247	57335	48912	111742	59993	51749

NOTE: In Columns (i)-(iii) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Columns (iv)-(vi) the dependent variable is the log of the number of weekly working hours. In Columns (vii)-(ix) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In Columns (x) to (xii) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, the other that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, month of birth, and the number of children. Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W7: Robustness Checks

		Using survey years 2002-2008	Different West cut-off	Dropping respondents born in May	Dropping respondents born in June	Dropping respondents born in July	Fachschule defined as college
Panel A : East*Treat							
	sample	(i)	(ii)	(iii)	(iv)	(v)	(vi)
College	all	.015**	.016**	.018**	.018**	.018***	.019***
		(.007)	(.007)	(.007)	(.007)	(.006)	(.007)
Panel B : East*Treat*cohort71-73							
		(i)	(ii)	(iii)	(iv)	(v)	
Employed	all	-.019*	-.020**	-.024**	-.025**	-.020**	
		(.011)	(.009)	(.010)	(.010)	(.010)	
Ln (wage)	male	.030**	.041***	.029*	.048***	.034***	
		(.012)	(.009)	(.015)	(.012)	(.012)	
Professional	male	.044***	.048***	.057***	.057***	.051***	
		(.011)	(.008)	(.010)	(.013)	(.011)	
Panel B: East*Treat*cohort74-77							
		(i)	(ii)	(iii)	(iv)	(v)	
Employed	male	.016*	.017***	.020**	.013**	.024***	
		(.008)	(.005)	(.007)	(.005)	(.004)	
Ln (working hours)	male	.026***	.013**	.023***	.015***	.018***	
		(.007)	(.006)	(.007)	(.005)	(.005)	

NOTE: Treatment is defined according to Definition 1 (unless it is differently specified), that is respondents born between June and December are considered treated. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, and a second that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between East and Treat in Panel A, and between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77, in Panels B and C. Panel A reports the coefficient on the variable East*Treat. Panel B reports the coefficient on the variable East*Treat*cohort71-73; Panel C reports the coefficient on the variable East*Treat*cohort74-77. In row (1), the dependent variable is a dummy variable equal to one if the respondent received a college degree. In rows (2) and (5) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In row (3) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (4) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In row (6) the dependent variable is the log of the number of weekly working hours. In Column (i) respondents born between May and December are considered treated, respondents born between January and April are considered as part of the control group, and survey years from 2002 on are used. In Column (ii) treatment and control groups are defined according to Definition 1, but respondents born in the West are not considered treated if born in June. In Column (iii), (iv), and (v), respondents born in May, June, and July, respectively, are dropped from the sample. In Column (vi) we redefine the dependent variable in row (1) by assigning one also to individuals whose highest educational degree is from a *Fachschule*. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents (males and females) aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W8: Placebo Exercise

Panel A : East*Treat		
	sample	
College	all	-.006
		(.011)
Panel B : East*Treat*cohort71-73		
Employed	all	.011
		(.012)
Ln (wage)	male	.023
		(.017)
Professional	male	.001
		(.020)
Panel B: East*Treat*cohort74-77		
Employed	male	-.031
		(.021)
Ln (working hours)	male	-.003
		(.008)

NOTE: We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, and a second that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between East and Treat in Panel A, and between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77, in Panels B and C. Panel A reports the coefficient on the variable East*Treat. Panel B reports the coefficient on the variable East*Treat*cohort71-73; Panel C reports the coefficient on the variable East*Treat*cohort74-77. In row (1), the dependent variable is a dummy variable equal to one if the respondent received a college degree. In rows (2) and (5) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In row (3) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (4) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In row (6) the dependent variable is the log of the number of weekly working hours. Respondents born between October and December are considered treated, respondents born between July and September are considered the control group. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents (males and females) aged between 31 and 35 and born between 1971 and 1977. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W9: Robustness Checks-Unemployment in Year of Labor Market Entrance

Panel A : East*Treat

	sample	(i) UR (plus sq)	(ii) male UR (plus sq)	(iii) youth UR	(iv) youth UR (plus sq)	(v) state UR	(vi) state UR (plus sq)	(vii) state male UR	(viii) state male UR (plus sq)	(ix) state youth UR	(x) state youth UR (plus sq)	(xi) market entr. trends*state
College	all	.019**	.019***	.021**	.023**	.020***	.018**	.020***	.018***	.020***	.020***	.016**
		(.007)	(.006)	(.009)	(.010)	(.007)	(.006)	(.006)	(.006)	(.008)	(.008)	(.007)

Panel B : East*Treat*cohort71-73

	sample	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)
Employed	all	-.020*	-.025**			-.020**	-.020*	-.023**	-.024**			-.022**
		(.010)	(.010)			(.009)	(.010)	(.009)	(.009)			(.011)
Ln (wage)	male	.034***	.041***			.035***	.034***	.044***	.044***			.029***
		(.009)	(.012)			(.008)	(.009)	(.011)	(.012)			(.010)
Professional	male	.041***	.050***			.046***	.042***	.055***	.053***			.039***
		(.009)	(.008)			(.008)	(.009)	(.010)	(.010)			(.010)

Panel C: East*Treat*cohort74-77

	sample	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)
Employed	male	.016***	.018***	.021***	.019***	.022***	.020***	.025***	.023***	.021***	.022***	.015**
		(.005)	(.005)	(.005)	(.005)	(.004)	(.004)	(.006)	(.005)	(.004)	(.005)	(.006)
Ln (working hours)	male	.012**	.011**	.015***	.013**	.015***	.014***	.014***	.013***	.015***	.013**	.010**
		(.004)	(.005)	(.005)	(.005)	(.004)	(.004)	(.005)	(.004)	(.005)	(.005)	(.005)

NOTE: Treatment is defined according to Definition 1, that is respondents born between June and December are considered treated. We generate 2 dummies: one that is equal to one if the respondent was born between 1971 and 1973, the other that is equal to one if the respondent was born between 1974 and 1977. We then include in the regression interactions between each of the two dummies and the variables East and Treat, that is East*cohort71-73, East*cohort74-77, Treat*cohort71-73, Treat*cohort74-77. Panel A reports the coefficient on the variable East*Treat; Panel B reports the coefficient on the variable East*Treat*cohort71-73; Panel C reports the coefficient on the variable East*Treat*cohort74-77. In row (1) the dependent variable is a dummy equal to one if the respondent obtained a college degree. In rows (2) and (5) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In row (3) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (4) the dependent variable is a dummy variable equal to one if the respondent is either a manager or a professional according to the ISCO classification. In row (6) the dependent variable is the log of the number of weekly working hours. Controls include age dummies, state of residence dummies, birth year dummies, and gender. Column (i) adds the East/West unemployment rate in the year of labor market entrance as a control in levels and squared, column (ii) the respective male unemployment rate in levels and squared, column (iii) the youth unemployment rate for individuals aged 15-24, column (iv) the respective youth unemployment rate in levels and squared, column (v) adds the state unemployment rate in the year of labor market entrance as a control in levels, column (vi) adds the state unemployment rate in the year of labor market entrance as a control in levels and squared, column (vii) adds the state male unemployment rate in the year of labor market entrance as a control in levels, column (viii) adds the state male unemployment rate in the year of labor market entrance as a control in levels and squared, column (ix) adds the state youth unemployment rate in the year of labor market entrance as a control in levels, column (x) adds the state youth unemployment rate in the year of labor market entrance as a control in levels and squared, column (xi) state-specific linear trends in the year of market entrance. Robust standard errors are in parentheses, and are clustered at the treatment-birth year-east group level. The sample consists of all respondents (males and females) aged between 31 and 35 and born between 1971 and 1977 (in col. (iii), (viii) and (ix) we restrict the sample to cohorts 1974-1977 due to data limitation). Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W10: Migration Decisions

Dependent variable: **Change of residence**		
	(i)	(ii)
Treated (Def. 2)	0.0003	0.0002
	(.0013)	(.0011)
Controls		x
Observations	73713	73713

NOTE: The dependent variable is a dummy variable equal to one if the respondent moved from East to West Germany in the year before the survey. Controls include age dummies, birth year dummies and a male dummy. Treatment is defined according to Definition 2, that is respondents born between May and December are considered treated. Therefore all available surveys (1991-2008) are employed. Robust standard errors are in parentheses, and are clustered at the treatment-birth year level. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977 and resident in East Germany in the year before the survey. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

TABLE W11: Migration Decisions Year by Year

Year	Treated (Def. 2)	
	Coefficient	Standard error
1991	.00028	.00413
1992	-.00736*	.00378
1993	.00738**	.00355
1994	-.00155	.00320
1995	.00523	.00355
1996	-.00281	.00351
1997	.00690*	.00379
1998	-.00259	.00351
1999	.00026	.00395
2000	-.00390	.00439
2001	.00462	.00553
2002	-.00526	.00449
2003	.00052	.00444
2005	.00021	.00265
2008	-.00047	.00186

NOTE: Each row corresponds to a regression using as a sample only the survey year indicated and shows the coefficient and the standard error of the variable "Treated". The dependent variable is a dummy variable equal to one if the respondent moved from East to West Germany in the year before the survey. Controls include age dummies, birth year dummies anda male dummy. Treatment is defined according to Definition 2, that is respondents born between May and December are considered treated. The sample consists of all respondents aged between 31 and 35 and born between 1971 and 1977 and resident in East Germany in the year before the survey. Residents in the state of Berlin are dropped from the sample. *significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.