

# Long-Lasting Effects of Socialist Education\*

Nicola Fuchs-Schündeln

Goethe University Frankfurt and CEPR

Paolo Masella

University of Sussex and CEP

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## Abstract

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

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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.<sup>1</sup> There is also a growing consensus that political and economic institutions are at the root of a significant part of the variation in GDP growth across countries.<sup>2</sup> However, much less attention has been devoted to the study of how interplays between human capital and political institutions contribute to shaping the long term economic prospects of a country.<sup>3</sup>

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 (Barro (1999), Botero et al. (2012), Glaeser et al. (2007), and Papaioannou and Siourounis (2008), among others). Another strand of the literature discusses instead 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. More in general, 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.<sup>4</sup>

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<sup>1</sup>See, among the many, Mankiw et al. (1992), Benhabib and Spiegel (1994) and, more recently, Krueger and Lindahl (2001) and Glaeser et al. (2004).

<sup>2</sup>See, among others, North (1981), Hall and Jones (1999) and Acemoglu, Johnson, and Robinson (2001).

<sup>3</sup>Barro (1996) finds that the positive effect of democracy on growth vanishes once human capital is controlled for.

<sup>4</sup>Using cross country variation, Murin and Wacziarg (2011) and Aghion et al. (2012), however, do not find any evidence supporting the conjecture that democratic institutions improve educational outcomes such as primary enrollment and government expenditure in education. Lott (1999) argues that totalitarian regimes have the greatest returns from indoctrination and therefore are more likely to invest in public education and own television stations, in order to control the information received by citizens.

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 base of political involvement rather than academic credentials alone.<sup>5</sup> We analyze whether both the content 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 GDR was transformed 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 who 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 to some degree.<sup>6</sup> 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,

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<sup>5</sup>For example, the children of the current president of Germany, Joachim Gauck, who was a pastor and civil rights activist in the GDR, were not allowed to attend university in the GDR in the 1980s.

<sup>6</sup>We do not observe individuals born after 1977 at age 31 or older in the data. Therefore, we cannot analyze yet the effects of socialist education on younger birth cohorts, who experienced reunification at a younger age.

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 received instead 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 (see Angrist and Krueger (1991), and Puhani and Weber (2007) or Fertig and Kluve (2005) for evidence from Germany). 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 (1971 to 1973 and 1974 to 1977). For male respondents belonging to cohorts 1971 to 1973, this effect translates into lower wages and a lower likelihood 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 seems to have been a difficult one.<sup>7</sup> 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 weekly number of 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. The performance of women in the labor market is not significantly affected by the reorganization of the school system in East Germany at reunification. This is likely due to their lower attachment to the labor market, but it may also be explained by the transmission of a female role model that is less attached to the work force in the new Western educational system.

This work contributes also to the literature that studies the long lasting effects of communism on economic outcomes and individual preferences. Acemoglu, Johnson, and Robinson (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 preferences: they show that, after reunification, East

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<sup>7</sup>We document that many apprenticeship contracts were resolved at reunification, with a decreasing likelihood the more advanced the apprenticeship was. Students in the birth cohorts 1971 to 1973 and exposed longer to the socialist education system therefore were also more likely to complete their vocational education, if they had started it before reunification. This may have enabled the least able among them to acquire further skills and qualifications that were likely to be useful in the labor market during the transition period and later.

Germans are more in favor of redistribution and state intervention than West Germans. Barro and McCleary (2005) investigate the effect of Communism on religious beliefs.<sup>8</sup> In this paper, we try to isolate one channel through which communist institutions have 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 Munich 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 cohorts who did not.<sup>9</sup> 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 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.<sup>10</sup> Hoffmann and Oreopoulos (2009) and Chetty et al. (2011) discuss the importance of the quality of instructors in shaping students' performance. Malamud and Pop-Eleches (2010) compare the effect of vocational and general education on labor market outcomes using exogenous variation provided by an educational reform in Romania. The language of instruction also has been proved to be important in

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<sup>8</sup>Ockenfels and Weimann (1999) provide experimental evidence that East Germans contribute less to public goods and exhibit less solidarity than West German subjects.

<sup>9</sup>This means that they compare cohorts being born many years apart.

<sup>10</sup>Algan et al. (2011) provide evidence that educational systems and teaching practices differ tremendously across countries.

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)). 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.<sup>11</sup>

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. Section 6 provides several robustness checks. 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 reforms related to schooling and apprenticeship after reunification.

### 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*

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<sup>11</sup> Saint-Paul (2010) provides a theoretical framework to study how indoctrination and transmission of beliefs within schools affect the future occupational choices of students.

*Oberschule, EOS*), which granted the university-entrance diploma. The majority of students started an apprenticeship. A third 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. Of the apprenticeships, 78 percent had a duration of two years, 11 percent of 2.5 years, and a further 11 percent of 3 years.<sup>12</sup> Last, students could attend a *Fachschule*. There existed two types of *Fachschulen*. One type provided education resembling an apprenticeship and could be attended right after finishing 10th grade. The second type might be considered an applied university and could only be attended after finishing an apprenticeship.<sup>13</sup> This second type covered 57 percent of students in *Fachschulen* in 1988 (Köhler, 2008). 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.<sup>14</sup>

## 2.2 Socialist Elements of Education

We identify four ways in which teaching in East Germany differed from the one in West Germany, namely teaching about socialism, official curricula, teaching style, and teaching about the role of women in the workplace.

A general socialist education was an official aim of the curriculum in the GDR, with the explicit goal to create a socialist personality (Block and Fuchs, 1993). This goal found its way into every single school subject. Beyond this general socialist

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<sup>12</sup>The distinction between 2 and 2.5 years depended on the subject matter of the apprenticeship. These percentages were calculated based on information from the Statistical Yearbook GDR (1987), which indicates numbers of students starting a certain apprenticeship, and the Gesetzesblatt der Deutschen Demokratischen Republik (1985), which regulates the duration of apprenticeships in different sectors.

<sup>13</sup>The first type of *Fachschulen* covered mostly the areas of medicine and pedagogy, and the second type mostly different fields of engineering, agriculture, and economics.

<sup>14</sup>Unfortunately, we could not find data giving these percentages for one birth cohort, which would allow a more precise picture.



teaching, 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. An important step in the socialist education was the Jugendweihe (youth oath), which took place in 8th grade. Jugendweihe was a festive act in which students pledged allegiance to the GDR and the socialist idea, and was preceded by a year of intensive preparation in special classes. After the Jugendweihe, students typically joined the official youth organization *Freie Deutsche Jugend* (Free German Youth, FDJ). The FDJ was the only official and subsidized youth organization in the GDR, and thus de facto the youth organization of the official party (*Sozialistische Einheitspartei Deutschlands*, SED).

Beyond the two subjects related directly to socialism, there were certain differences in the curricula between East and West Germany, as shown in Figure 1, where the state of Schleswig-Holstein is taken as exemplary for West Germany.<sup>15</sup> We focus here on grades 7 to 10, 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, amounting to 6.8 percentage points more, while FRG schools devoted more time to the teaching of “softer” subjects like foreign languages, sports, arts and music, and religious education (5.5, 2.9, 8.6, and 4.8 percentage points difference each).<sup>16</sup> The

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<sup>15</sup>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 for the FRG it relates to the state of Schleswig-Holstein (<http://www.schulrecht-sh.de/texte/s/studentafel.htm>). We calculate the average teaching hours of each subject over grades 7-10, taking weighted means over the three school forms of Hauptschule, Realschule and Gymnasium for the West.

<sup>16</sup>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

most important foreign language taught in schools was Russian; it was a compulsory subject in every school in East Germany.

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 which time period, in part imposing detailed structures of how to cover certain topics, leaving minimal scope for teacher or student initiatives.

The socialist ideal was that women as well as men should be contributing members of the workforce. The vast majority of adult females, including mothers, participated in the workforce, contrary to the West German experience. While equality in the work place was not achieved, schools sent clear messages that women, including mothers, were expected to work. Rust and Rust (1995) e.g. report that in analyses of school books used in East and West Germany in 1970, only 10 percent of women depicted in West German school books were in the work force, with few changes in later years, while virtually all depicted women were working in the East German school books.

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.<sup>17</sup> For women, schools taught the desirability of an active involvement in the labor market, in contrast to West Germany.

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in the West.

<sup>17</sup>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.

## 2.3 Allocation of High School Slots and Apprenticeships

Besides academic credentials, political criteria played an important role in the decision who was allowed to attend the *Erweiterte Oberschule*. Official selection criteria were the grades in 10th grade, as well as a statement about 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 *FDJ-Gruppenrat* (the local branch of the FDJ) 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.<sup>18</sup> 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

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<sup>18</sup>Each county school council was supposed to assure that the distribution of *EOS* students across social classes resembled the distribution in the overall population of the county (Baske, 1998).

preferences for a certain apprenticeship in their application.<sup>19</sup> 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.

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 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. Concretely, for many subject areas the changes in curricula 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 left the concrete implementation of the topics to teachers and students. Implementation of a student-based approach became a central part of the curricula reforms after reunification (Riedel et al., 1994). Students were allowed to learn other foreign languages such as English and French.

The socialist content of education was abolished almost immediately, and restric-

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<sup>19</sup>Students who were not allowed to attend the EOS for political reasons sometimes managed to start a three year apprenticeship that led to a high school equivalent diploma. The main criterion for admission into these programs was the intended career in certain desirable sectors (Köhler, 2008).

tions in access to the EOS (and therefore to college) which were not based on academic merit 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 apprenticeship year (Bundesministerium für Bildung und Wissenschaft, 1991).<sup>20</sup> 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). Special funding opportunities and special programs were installed by the government over the summer of 1990 to try to keep current apprenticeships in place (Bundesministerium für Bildung und Wissenschaft, 1991).

The fraction of a birth cohort starting in university increased from around 14 percent in 1988 and 1989 to 19.7 percent in 1990, oscillating between 18 and 20.5 percent in the years to follow.<sup>21</sup>

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<sup>20</sup>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 (Bundesinstitut für Berufsbildung, 1991).

<sup>21</sup>These numbers include universities and applied universities. Some restructuring in the university system in the East led to a decrease in spots at the former and an increase in the latter.

## 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 the children born June 1 or later as the treated group, and the 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 are one year less advanced in the socialist education system than respondents in the control group. Thus, if we compare individuals in East Germany of both treatment and control groups from the same birth cohort, the individual in the treatment group will have one year less of socialist education, and one year more of Western education. On the other hand, 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 identifying assumption is therefore that the effect of age at school entry on the dependent variables we select would be the same in East and West Germany in absence of the differential exposure to socialist education that affects 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:

$$Y_{ic} = \beta_0 + \beta_1 East_{ic} + \beta_2 Treat_{ic} + \beta_3 (East * Treat)_{ic} + \beta'_4 (X)_{ic} + \gamma_c + \varepsilon_{it} \quad (1)$$

where  $Y_{ic}$  is the relevant labor market outcome variable for individual  $i$  born the 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.  $\gamma_c$  is a full set of birth year dummies. Standard errors are clustered at the *birthyear-treatment-east* level, i.e. at the group level where groups are built as all possible interactions of the birth year, treatment, and East dummies.

The coefficient  $\beta_1$  captures the effect of living in the East on labor market outcomes. The coefficient  $\beta_2$  controls for any potential effects of being enrolled in school at a slightly older age.<sup>22</sup> The coefficient of main interest is  $\beta_3$ , 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  $\beta_3$  should be positive.

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

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<sup>22</sup>Puhani and Weber (2007), Bedard and Dhuey (2006), and Fredriksson and Öckert (2013) discuss whether being enrolled at an older age has an impact on test scores, probability of attending college, and also long-run labor market outcomes.

in school a year earlier or later than he/she was supposed to. An incorrect assignment of respondents to school cohorts would induce measurement error that may bias our estimates of  $\beta_3$  upwards under very specific circumstances. In Section 5.1, we discuss the direction of possible biases and 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 in the data to investigate labor market outcomes.

When the Berlin Wall fell on November 9, 1989, the cohort group 1974 to 1977 was 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 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 and knowledge that may not be useful in the unified German labor market and, more generally, in Western societies. The cohort born in 1975 either had already received Jugendweihe in the fall of 1989 (control) or not (treatment), thereby potentially differing most in terms of socialist content of the received education.

The second relevant cohort group consists of the birth cohorts 1971 to 1973. Con-



sider 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.<sup>23</sup> 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. Thus, they might have ended up in jobs in which they were less productive than individuals in the treatment group. 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.<sup>24</sup>

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<sup>23</sup>Males typically served the 18 months compulsory military service either right after finishing *EOS*, or after finishing an apprenticeship (Zimmermann et al., 1985).

<sup>24</sup>Evidence of the existence of the sunk cost fallacy has been provided by Arkes and Blumer (1985)

Summarizing, for the cohort group 1971 to 1973, treatment means to be one year less advanced in terms of apprenticeship or EOS education. The relevant difference in terms of treatment vs. control group lies thus in how far the two groups have been affected by access restrictions to education. We conjecture that for the treatment group it was easier to change schooling or apprenticeship than for the control group, both for practical and psychological reasons.

When we analyze the birth cohort groups 1971 to 1973 and 1974 to 1977 separately, we create separate cohort group dummies for both cohort groups and run the following regression:

$$\begin{aligned}
 Y_{ic} = & \beta_0 + \beta_1 East_{ic} + \beta_2 Treat_{ic} \\
 & + \beta_3 (East * cohort_{71-73})_{ic} + \beta_4 (Treat * cohort_{71-73})_{ic} \\
 & + \beta_5 (East * Treat * cohort_{71-73})_{ic} + \beta_6 (East * Treat * cohort_{74-77})_{ic} \\
 & + \beta'_7 (X)_{ic} + \gamma_c + \varepsilon_{it}
 \end{aligned} \tag{2}$$

Now  $\beta_5$  and  $\beta_6$  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.<sup>25</sup>  $\beta_6$  should therefore capture the effects of exposure to socialist methods of education, and  $\beta_5$  the effect of non-meritocratic restrictions in access to high school and college, as well as restricted apprenticeship choice.

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and more recently by Just and Wansink (2011) and Baliga and Ely (2011). Of course, a year lost in life-time earnings if one changes tracks and the initial education becomes worthless also matters, but it is doubtful that this loss is so large that it is not off-set by potential future gains in the profession more suitable for the individual.

<sup>25</sup>This is a standard triple interaction framework, where the three terms are: *East*, *Treat* and *cohort*<sub>71-73</sub>. Given that we include a full set of birth cohort dummies,  $\gamma_c$ , we do not include the dummy variable *cohort*<sub>71-73</sub> as well.

### 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,<sup>26</sup> 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 rather than the control group, when we study the outcomes of older cohorts and therefore need data from the earlier survey rounds in order to observe them at the relevant age.<sup>27</sup> 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.<sup>28</sup>

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 rule out the possibility

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<sup>26</sup>In 2004, the two categories are slightly different: January to March, and April to December.

<sup>27</sup>This is the case for Figure 2 and some of the robustness checks. 2002 is the first survey round in which we observe the oldest birth cohort of interest, born 1971, at age 31. In robustness checks, we use also cohorts born between 1961 and 1970; therefore in this case 1992 becomes the first survey round in which we observe the oldest birth cohort of interest at age 31.

<sup>28</sup>Results are robust to including all individuals aged 31 and above; these results are available from the authors upon request.

that our results are driven by selection into current residence by analyzing whether the rate of East-West migration is significantly different between our treatment and control groups.<sup>29</sup>

We start by analyzing the impact of reunification on the probability of obtaining a college degree. This is an obvious outcome of interest since, as discussed earlier, access to higher education was restricted based on non-meritocratic criteria.<sup>30</sup> We then focus on standard labor market variables such as employment, working hours, wages, and the type of profession (in particular whether or not the respondent has a managerial or professional job). The dummy variable *college* takes on the value of 1 if the highest educational degree comes from a university or an applied university. We exclude GDR *Fachschulen*, which are a mixture of applied universities and vocational schools.<sup>31</sup> Employment is equal to 1 if the self-reported employment status is given as employed.<sup>32</sup> Working hours are reported working hours in a usual work week; we consider the logarithm of working hours as main dependent variable. To construct wages, we have to recur to personal net income, since gross income is not available. Personal net income is reported in brackets,<sup>33</sup> and we set personal income equal to the mean point of each bracket in order to calculate net wages by dividing through 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

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<sup>29</sup>The only available German data set that allows identifying the residence before 1989, namely the German Socio-Economic Panel, has a sample size that is almost two orders of magnitude smaller than the German Microcensus. Therefore, it does not provide enough observations in order to carry out the analysis on the restricted birth cohorts.

<sup>30</sup>Years of schooling are not available in the Microcensus.

<sup>31</sup>We present results from a robustness check in which we assign *college* = 1 also to an individual with the highest educational degree from a *Fachschule* in Section 6.

<sup>32</sup>Alternatively, we define as employed any individual indicating positive hours worked in a usual work week. Results are robust.

<sup>33</sup>As an example, in the year 2005 there are 24 brackets, ranging from less than 150 Euros to more than 18,000 Euros per month. The exact number of cut-off values for the brackets differ somewhat from year to year.

manager, a professional, or a technician or associate professional according to the ISCO classification.<sup>34</sup>

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.<sup>35</sup>

## 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 black line) and West (thin grey line), and by being born between May and December (treated, solid line) or January and April (control, dashed line). Since we use birth cohorts from 1965 onwards, we need data from surveys earlier than 2005; therefore, we have to use the less precise definition of treatment.<sup>36</sup> 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. However, the differences are generally very small. 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

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<sup>34</sup>These three groups correspond to the major groups 1, 2, and 3 in the ISCO88 classification.

<sup>35</sup>Numbers of observations differ somewhat for each dependent variable. They are therefore not reported in Table 1, but in each regression table.

<sup>36</sup>We use this less precise definition consistently for all cohorts.

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.

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).<sup>37</sup> The sample used in the regression analysis consists of respondents born between 1971 and 1977; therefore, we can rely on survey years 2005 to 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.<sup>38</sup> 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.<sup>39</sup> 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.

Table 3 shows the results of specification (2) and decomposes the cohorts used in

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<sup>37</sup>Due to collinearity, we cannot add year dummies in addition to birth year dummies and age dummies. However, we ran all regressions including the other two combinations of these three variables as controls (i.e. either birth year and year dummies, or age and year dummies), and results are robust. Moreover, all results are robust to excluding the control variables.

<sup>38</sup>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.

<sup>39</sup>The effect is larger than the one shown in Figure 1. Note that Figure 1 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).

the analysis into the two cohort groups 1971 to 1973 and 1974 to 1977, first using the full sample (column i), and then splitting the sample into males (column ii) and females (column iii). 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.<sup>40</sup> 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.

Figure 3 digs deeper into this issue and shows the coefficients on the relevant interaction terms if we run specification (1) cohort by cohort, i.e. always including only one single cohort. Results oscillate a bit cohort by cohort, but all cohorts 1971 to 1975 exhibit a positive coefficient on the relevant interaction term that indicates effects larger than 1 percentage point. However, the effect seems to vanish for the two youngest cohorts 1976 and 1977, with a declining trend for the younger cohort group 1974 to 1977. This 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, we find significant negative effects of the length of exposure to

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<sup>40</sup>p-values vary between 0.103 and 0.19.

socialist contents of education and restricted access to higher education in the GDR on college completion rates.

## 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.

Starting with the older cohort group, born 1971 to 1973, Table 4 shows that being treated in the East is associated with a significantly lower likelihood of being employed 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. 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.

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 and acquire skills and qualifications useful in the labor market. 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 leads to higher wages and a higher probability of achieving a professional status in the East than in the West.<sup>41</sup>

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.<sup>42</sup> 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. Socialist values and role models transmitted by Eastern schools may have been ill suited for the unified German labor market. We conjecture that the development of an educational system after reunification that

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<sup>41</sup>Selection can also play a role here: if the least able 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. However, the magnitudes of the effects are not consistent with a pure selection story: the effect of treatment on the probability of being employed for males is -2.2 percentage points, while the effect on wage is 4.1 percent and on the probability of having a professional job 5.1 percentage points. 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 perform 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 would have displayed this worst labor outcome. Following this procedure, we obtain as an upper bound that self-selection biases the treatment effect up by 0.008, i.e. without selection treatment would raise the probability of receiving a professional status by 4.3 percentage points as a lower bound rather than the estimated 5.1 percentage point.

<sup>42</sup>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 6.

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.<sup>43</sup> 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.<sup>44</sup>

Figure 4 presents results from regressions run cohort by cohort, analogously to Figure 3. For simplicity, we only show results that were significant for the relevant cohort group in Table 4. Overall, the picture is similar to the one for college attainment, namely that there are some oscillations cohort by cohort, but most effects are economically significant for all cohorts.<sup>45</sup> Interestingly, when observing the younger cohort group (panels D and E), results are always smallest for the youngest cohort, i.e. individuals born in 1977, yet without a clear decreasing trend as for college attainment. Again, only the future can show whether this is random variation or it indicates that individuals who were in lower grades at reunification might not experience long-term effects of socialist education on labor market outcomes.

For females, none of the coefficients on the interaction variables of interest in Table 4 are significant.<sup>46</sup> We believe that the lack of significant results for females, in

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<sup>43</sup>Results might also be reflecting a demand effect, where employers have a preference for hiring individuals educated longer in the new system (e.g., for instance because they obtained more market-oriented cognitive skills), and employing them for longer hours.

<sup>44</sup>It is interesting that for the older cohort group the detrimental effect of the length of exposure of socialist contents of education on employment and working hours is missing. This might be due to a decreasing effect of an extra year of socialist education the longer the overall socialist education, or the older the individual.

<sup>45</sup>The two exceptions are the employment effect for cohort 1971 in panel A, and the employment effect for cohort 1977 in panel D.

<sup>46</sup>All results for females, as well as for males, are robust to controlling for the number of children. Results are available from the authors upon request.

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. Moreover, the differential effect of socialist education on labor market outcomes of men and women may also be related to the fact that, as mentioned in Section 2.2, socialist education emphasized the active role of women as members of the work force.

## 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 3 and 4, analogously to Figures 3 and 4. 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,<sup>47</sup> for the dependent variable working hours only for the younger cohort group and the male sample, while for the employment status

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<sup>47</sup>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. Robustness checks on all other cohort or gender groups are omitted for reasons of space and available from the authors upon request.

we show results for both older and younger cohort group (but in this last case only using the male sample).

## 5.1 Can Results Be Explained by Differential Trends?

One worry about the results could be that they might capture differences in trends between East and West residents. Treatment and control individuals are slightly different in terms of age. If age trends in East and West Germany were systematically different, these could therefore show up as differential effects of treatment in East and West. Moreover, as previously discussed, incorrect assignment of treatment due to grade repetition or early/late enrollment to school may bias the 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 difference depends on whether we consider the sample of East or West respondents. Upward estimates of  $\beta_3$  in equation (1) may also be driven by incorrect assignment of treatment due to early or late enrollment in school. This may be the case if late enrollers perform systematically better than early enrollers (in absence of differential exposure to socialist education),<sup>48</sup> and early or late enrollment into school is more frequent in West than East Germany.

Yet, any of these potential biases should show up also for earlier 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.<sup>49</sup> All respondents born 1970 or before had already completed primary

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<sup>48</sup>Descriptive statistics in Table 1 do not seem to support this hypothesis. It is also unclear whether this would be true for the self-selected group that become early or late enrollers due to their parents' choice, rather than following the birth day cut-off rule.

<sup>49</sup>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).

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.<sup>50</sup>

Figures 5 and 6 present the results for different placebo cohort groups, starting from the cohort group 1961 to 1970, 1962 to 1970, up to 1968 to 1970. The figures show the coefficients on the interaction variable of interest between East and Treat. In Figure 5, college attainment is the dependent variable, while Figure 6 focuses on the labor market outcomes. For comparison, the figures also show the coefficients for our two relevant cohort groups 1971 to 1973 and 1974 to 1977 from Tables 3 and 4. 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.<sup>51</sup> 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 life style, rather than specifically by socialist education. Respondents in the treatment group are younger than those in the control group, and therefore they 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

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<sup>50</sup>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, which is supported by suggestive evidence available from the authors upon request.

<sup>51</sup>Appendix Table 1 shows exemplary the coefficients and standard errors for the placebo cohort groups 1961-1970 and 1966-1970.

stronger impact at a younger age and in particular at the age when cohorts 1971 to 1977 experienced reunification.

Still, we perform two additional robustness checks. In the first robustness check we restrict the sample to respondents born between February and September. Thereby, we are comparing individuals whose age difference is very small, and therefore results should be less likely driven by a different exposure to socialist culture in general. These results are presented in column (i) of Tables 5 and 6.<sup>52</sup>

The second check introduces a placebo treatment. Here, 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. In this case, we are considering respondents who received the same exposure to socialist education, but, given the age difference, a different exposure to socialist culture. Results are presented in column (ii) of Tables 5 and 6.

Results for college graduation are presented in Table 5. As expected, the coefficient of interest stays positive and significant at the 5 percent level in column (i), but turns insignificant in the placebo treatment of column (ii). In specification (i), which restricts the sample to individuals born between February and September and thus analyzes a more homogeneous age group, the coefficient even increases to 0.026 from 0.02 in the baseline results of Table 2.

We then perform similar exercises using labor market outcomes as dependent variables. The first two columns of Table 6 report the coefficients on the relevant interaction terms. As in Table 5, all coefficients of interest in specification (i), where the

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<sup>52</sup>We refrain from performing a standard discontinuity exercise 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 6. 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.

sample is restricted to individuals born between February and September, remain of the same sign as in the baseline results and significant. Also, all coefficients of interest in specification (ii), which analyzes the pseudo treatment, are insignificant. Summarizing, these analyses confirm that we are capturing exposure to socialist education, rather than exposure to socialist culture in general.

### 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.<sup>53</sup> Columns (iii) of Tables 5 and 6 address the question whether our results are capturing the impact of the year of entrance into the labor market on later labor market success. In order to control for this effect, we collect information on the unemployment rate in East and West Germany from 1989 onwards.<sup>54</sup> 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) 12 years after the individual enrolled in primary education, that is when he or she is likely to have completed either EOS education or an apprenticeship.

We add this newly created variable “unemployment rate in the year of labor market entrance” as a control to the baseline specifications. As columns (iii) of Tables 5 and 6 show, the results are robust to adding this additional control variable. The coefficient on the unemployment rate (not reported) is mostly insignificant, but

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<sup>53</sup>However, unemployment rates have generally been rising in the first years after reunification, making a later entrance into the labor market potentially more difficult than an earlier entrance.

<sup>54</sup>These data come from the Federal Employment Agency for East Germany from 1991 on and for West Germany throughout. For East Germany in 1989 and 1990, they come from Funken (1996).

always positive. It is significant at the 1% level in the regression with college as the dependent variable, indicating that the likelihood of completing college is increasing in the unemployment rate after 12 years of schooling. This might be a push-effect into college in a labor market in recession.

Results are also robust to including a quadratic term in the unemployment rate. Last, we include the youth unemployment rate (for individuals aged 15 to 25) either linearly or in addition with a quadratic term instead of the overall unemployment rate as a control variable. Unfortunately, this rate is only available from 1993 onwards. Thus, we can only include individuals who entered the labor market in 1993 or later in this regression, which restricts the sample to a subsample of cohort B. All the results are robust to these additional control variables, and available from the authors upon request.

## 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)).<sup>55</sup> 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

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<sup>55</sup>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.



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. In other words, our results would be capturing the effect of the reorganization of the East educational system after reunification on the sample composition if shorter exposure to socialist education would make more able individuals in the East less likely to migrate towards the West.

In order to rule out this possibility we take advantage of the fact that the Micro-census provides information not only on the current residence of the respondent, but also on the residence 12 months before the interview. We have this information for all the survey years starting from 1991, except for the survey years 2004, 2006, and 2007. Therefore, while we cannot capture the full migration history of a respondent, we can capture recent migration by the creation of two variables: (i) a dummy variable that is equal to one if the respondent resided in the East 12 months prior to the survey, and (ii) a dummy variable equal to one if the respondent moved from East to West within the last 12 months. Using the sample of all respondents (belonging to cohorts 1971 to 1977) resident in East Germany 12 months prior to the interview and all surveys from 1991 onwards, we check whether respondents in the treatment group were more likely to migrate in the next year than respondents in the control group. In order to use the early sample years we work with the less precise definition of treatment.<sup>56</sup>

Table 7 provides the results of this exercise. Column 1 reports the specification without control variables, while column 2 adds the usual set of controls. 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

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<sup>56</sup>Results do not change if we use the correct definition of treatment and restrict the sample to the 2005 and 2008 surveys.

have been somehow influenced by the reorganization of the educational system after 1989. Appendix Table 2 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. 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 or not, but also based on the ability of the individuals interacted with treatment.

## 5.5 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., 2011), however, does not provide evidence in favor of any systematic stronger effects of quality of teaching at higher grades.

## 6 Robustness Checks

We also analyze whether our results are robust to different sampling periods, treatment definitions (for the West), and an alternative definition of the dependent variable college. In column (iv) of both Tables 5 and 6 we include data from 2004 and before. To do that, we have to use the less precise Definition 2 of treatment, i.e. being born on or after May 1st, given that this is the only available information in the survey from 2004 and before.<sup>57</sup> In column (v), we redefine 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. In the baseline analysis, we use the same cut-off date for both East and West. This gives us a cleaner control for an age effect, but a less clean control for an early enrollment effect.

Results for college graduation are presented in Table 5. As expected, the coefficient of interest stays positive and significant at the 5 percent level in both specifications.

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<sup>57</sup>To be consistent, in this robustness check we define treatment in this same incorrect way also for the survey rounds 2005 to 2008. We still use only birth cohorts 1971 to 1977, observed at age 31 to 35.

In specification (iv), the coefficient declines to 0.015 from 0.02 in the baseline results, which is expected given the less correct definition of treatment (individuals born in May are incorrectly assigned to the treatment group in specification (iv)). As in Table 5, all coefficients of interest in specifications (iv) and (v) remain significant when we consider labor market outcomes as dependent variables in Table 6.<sup>58</sup>

In column (vi) of Table 5, we redefine the dependent variable by assigning college status also to an individual whose highest educational degree is from a *Fachschule*, given that some *Fachschulen* resembled applied universities. The coefficient on the interaction variable of interest remains positive, significantly different from zero, and also in its size very similar to the one in the main specification.

## 7 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 educational 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 out-

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<sup>58</sup>Results are also robust to excluding individuals born in June from the analysis. These results are available from the authors upon request.

comes. We find that the 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, less able students face a higher likelihood of being non-employed. Thus, the elimination of these restrictions increases 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 a higher participation in the labor market and higher effort in the workplace, expressed in a higher number of working hours. The effects that we find in the labor market are, however, limited to the male sample. This can be explained either by the lower attachment of women to the labor market, or by the emphasis in socialist schools on depicting women as a fundamental component of the workforce.

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 the 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. Moreover, the depressed labor market is not a concern when it comes to analyzing the effects of socialist teaching in schools.

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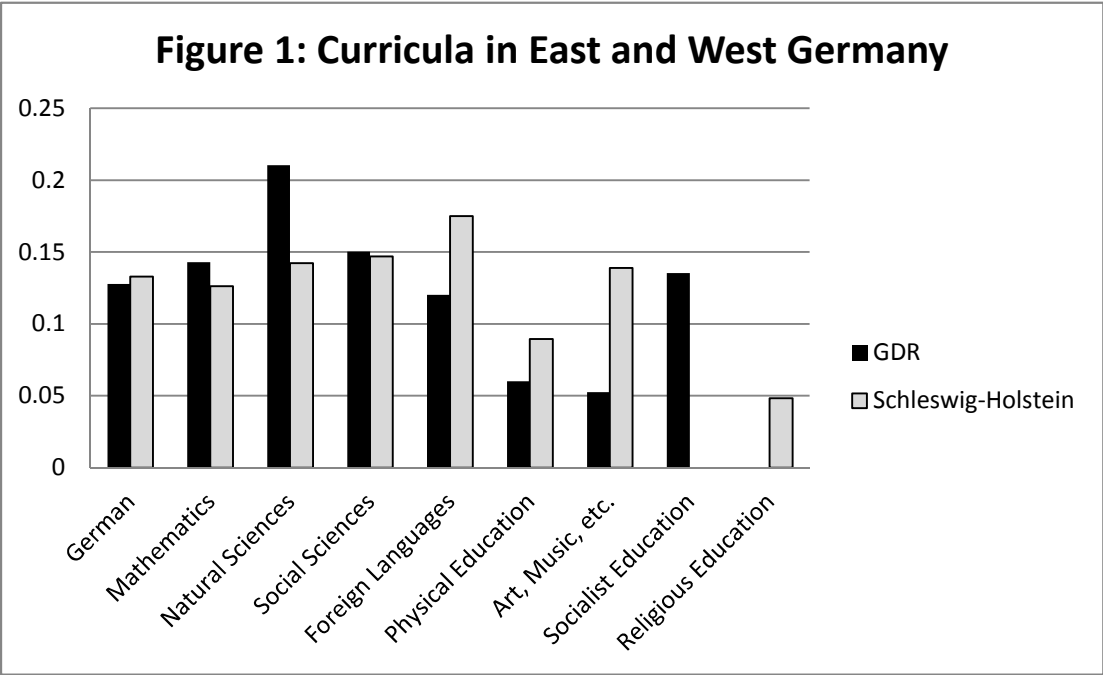


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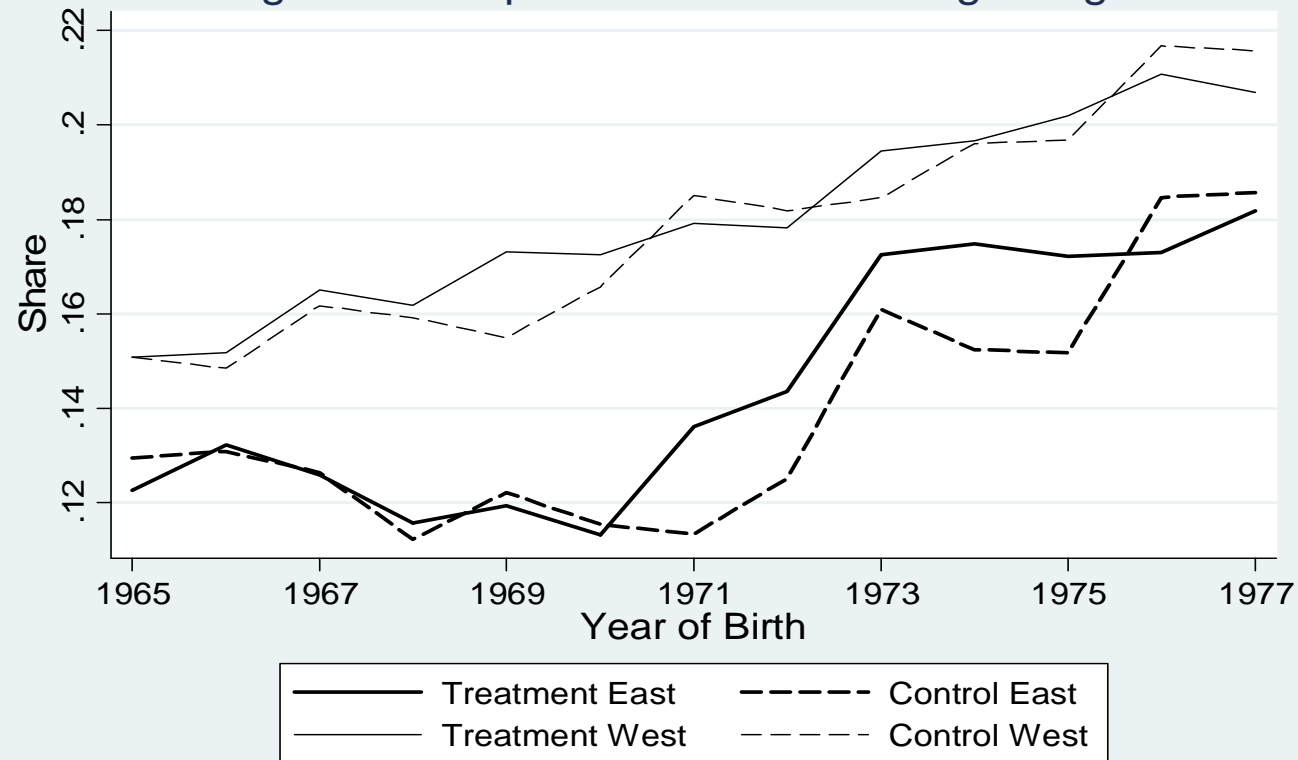
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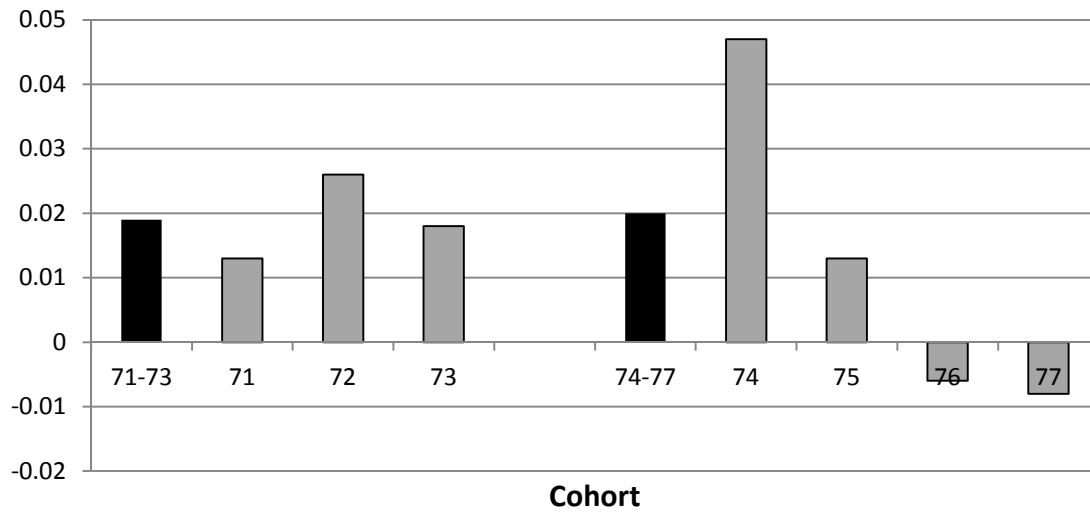
NOTE: This figure shows the share of overall teaching time devoted to certain subject areas in East Germany and the Western State of Schleswig-Holstein on average over the grades 7 to 10.

Figure 2: Respondents with a College Degree



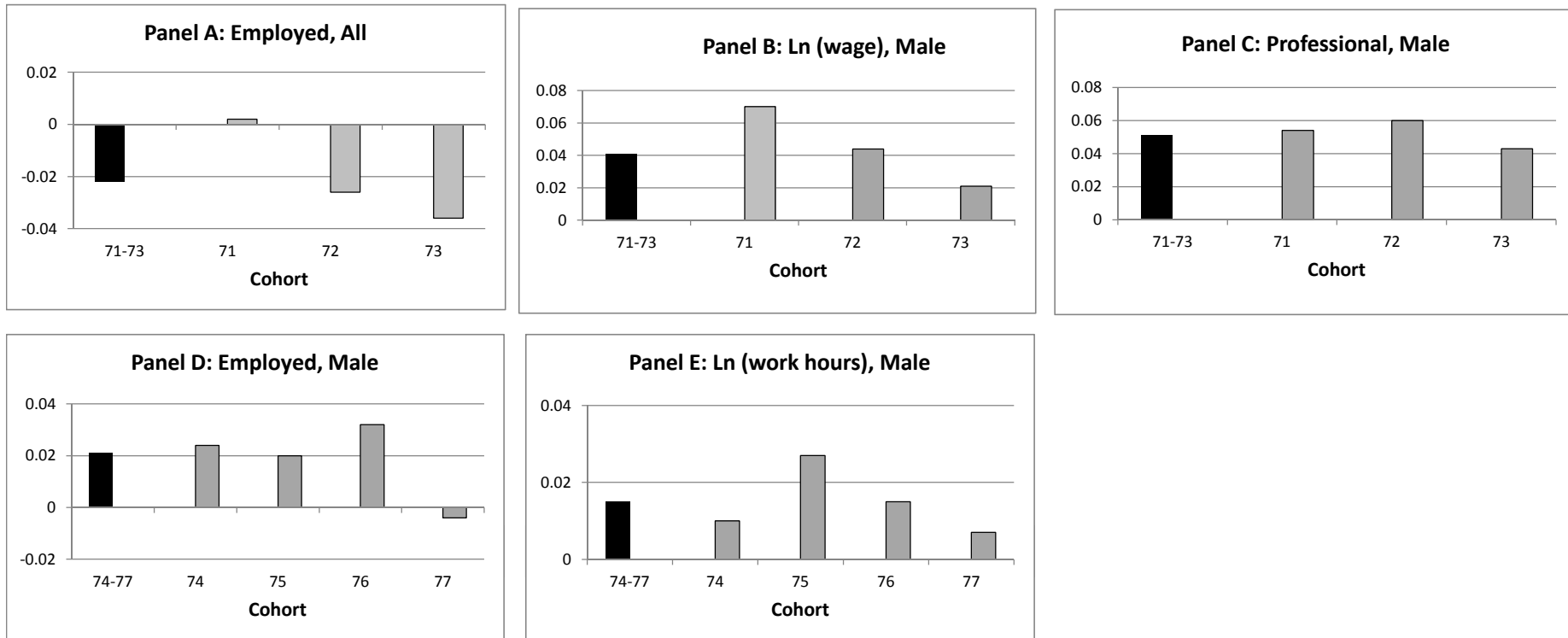
NOTE: Source: German Microcensus. In this Figure we plot the fraction of respondents aged 31-35 who obtained a college degree by cohort of birth. We distinguish 4 groups: (i) group Control East includes all respondents born in the months January to April and resident in the East at the time of the survey; (ii) group Treated East includes all respondents born in the months May to December and resident in the East at the time of the survey; (iii) group Control West includes all respondents born in the months January to April and resident in the West at the time of the survey; (iv) group Treated West includes all respondents born in the months May to December and resident in the West at the time of the survey. Residents in the State of Berlin are dropped from the sample.

**Figure 3: College Degree, All**



NOTE: The black columns show coefficients reported in Table 3. The gray columns show coefficients of cohort-by-cohort regressions (i.e. each regression includes only one single cohort) of college attainment on treatment interacted with East. The dependent variable of each regression is a dummy variable equal to one if the respondent obtained a college degree. 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 1, i.e. 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.

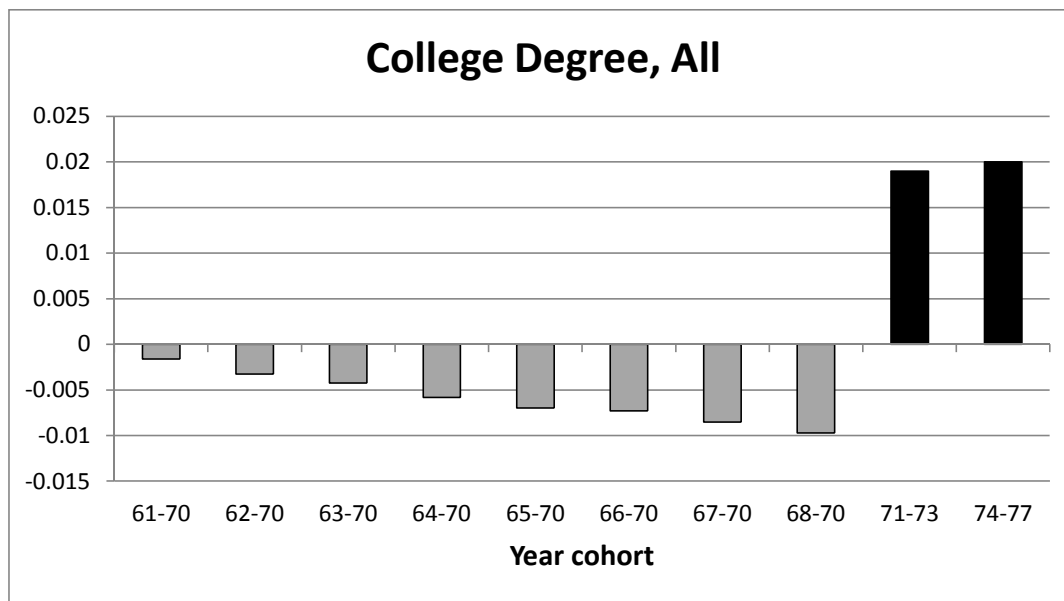
**Figure 4: Long-term Labor Market Outcomes Cohort-by-Cohort**



NOTE: The black columns show coefficients reported in Table 3. 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 Panels A and D, the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Panel B, the dependent variable is the log of wages. In Panel C, 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 E, 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.

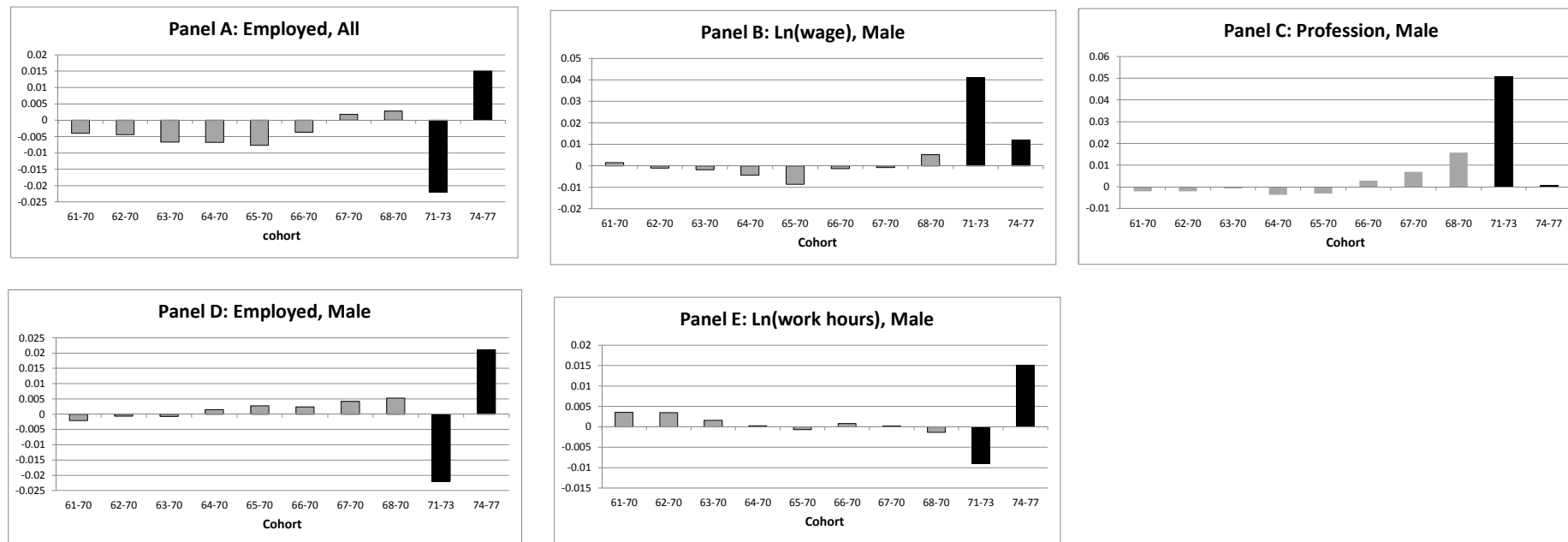


**Figure 5: Regressions for Placebo Cohort Groups, College Attainment**



NOTE: This figure shows coefficients of regressions of college attainment 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. The dependent variable is a dummy variable equal to one if the respondent obtained a college degree. 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.

**Figure 6: Regressions for Placebo Cohort Groups, Labor Market Outcomes**



**Table 1: Sample Characteristics**

	mean/percent	East		West	
		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)	3.496 (0.494)	3.566 (0.380)	3.568 (0.379)	3.487 (0.510)	3.479 (0.520)
Ln (wage)	2.163 (0.563)	1.970 (0.527)	1.970 (0.526)	2.190 (0.565)	2.210 (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 (1.381)	32.9 (1.398)	33.0 (1.385)	32.9 (1.378)	33.0 (1.380)
East	0.153				
Treated (Definition 1)	0.561				

NOTE: Source: German Microcensus, sample years 2005-2008. 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 of 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			
	(i)	(ii)	(iii) males	(iv) females
East	-.047*** (.007)			
Treated (Def. 1)	-.000 (.004)	-.013** (0.005)	-.007 (0.007)	-.018** (.008)
East*Treated (Def. 1)	.021* (.011)	.020*** (.007)	.018* (.009)	.022** (.010)
Controls		x	x	x
Observations	139605	139605	69090	70515

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, and month of birth. 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 3: Results by Cohort Groups**

Dependent variable:	College Degree		
	(i)	(ii) males	(iii) females
East*Treat*cohort71-73	.019* (.010)	.015 (.011)	.022** (.010)
East*Treat*cohort74-77	.020** (.010)	.020 (.012)	.022 (.015)
Controls	x	x	x
Observations	139605	69090	70515

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, 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: 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** (.009)	-.022* (.012)	-.021 (0.014)	-.019 (.012)	-.009 (.008)	-.035 (.028)	.026*** (.008)	.041** (.011)	.006 (.016)	.026** (.011)	.051*** (.009)	-.004 (.014)
East*Treat*cohort74-77	0.015 (.010)	.021*** (.004)	.009 (.019)	-.005 (.009)	.015*** (.005)	-.027 (.019)	.018* (.010)	.012 (.010)	.022 (.014)	-.001 (.007)	.002 (.015)	-.002 (.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: 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 5: Robustness Checks (College)**

Dependent variable:	College Degree (standard definition)			College Degree (Fach. included)		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
East*Treated (Def. 1)	.026** (.012)					
East*Treated (Placebo)		-.006 (.010)				
East*Treated (Def. 1)			.015** (.006)			
East*Treated (Def. 2)				.015** (.007)		
East*Treated (Def. 3)					.016** (.007)	
East*Treated (Def. 1)						.019*** (.007)
Controls	x	x	x	x	x	x
Market entrance dummies			x			
Observations	95279	66938	139605	189285	139605	139605

NOTE: The dependent variable in columns (i) to (v) is a dummy variable equal to one if the respondent obtained a college degree. Treatment (unless it is differently specified) is defined according to Definition 1, that is respondents born between June and December are considered treated. In Column (i) the sample is restricted to respondents born between February and September. In Column (ii) respondents born between October and December are considered treated, respondents born between July and September are considered the control group. In Column (iii) treatment is defined according to Definition 1, and the variable "unemployment rate in the year of labor market entrance" is included as additional control. In Column (iv) 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 (v) 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 (vi) we redefine the dependent variable 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 6: Robustness Checks (Labor Market Outcomes)**

Panel A : East*Treat*cohort71-73						
	sample	(i)	(ii)	(iii)	(iv)	(v)
<b>Employed</b>	all	-.016** (.007)	.011 (.015)	-.024* (.012)	-.019* (.011)	-.020** (.009)
<b>Ln (wage)</b>	male	.029** (.012)	.023 (.018)	.034*** (.009)	.030*** (.012)	.041*** (.009)
<b>Professional</b>	male	.045*** (.012)	.001 (.020)	.049*** (.009)	.044*** (.011)	.048*** (.008)
Panel B: East*Treat*cohort74-77						
		(i)	(ii)	(iii)	(iv)	(v)
<b>Employed</b>	male	.043*** (.013)	-.031 (.023)	.023*** (.005)	.016* (.008)	.017*** (.005)
<b>Ln (Working hours)</b>	male	.013* (.007)	-.003 (.008)	.016*** (.005)	.026*** (.007)	.013** (.006)

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, 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, and gender. Panel A reports the coefficient of the variable East\*Treat\*cohort71-73; Panel B reports the coefficient of the variable East\*Treat\*cohort74-77. In rows (1) and (4) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In row (2) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (3) 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 (5) 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. In Column (ii) respondents born between October and December are considered treated, respondents born between July and September are considered the control group. In Column (iii) treatment is defined according to Definition 1, and the variable "unemployment rate in the year of labor market entrance" is included as additional control. In Column (iv) 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 (v) treatment and control groups are defined according to Definition 1, but respondents born in the West are not considered treated if born in June. 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 7: Migration Decisions**Dependent variable: **Change of residence**

	(i)	(ii)
Treated (Def. 2)	0.0003 (.0013)	0.0002 (.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, a male dummy, and month of birth. 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.

**APPENDIX TABLE 1: Trends**

Panel A						
Dependent variable:	College Degree			Employed		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	(all)	(all)	(all)	(all)	(males)	(males)
East*Treated (Def. 2)	-.001 (.006)	-.007 (.008)	-.004 (.012)	-.004 (.008)	-.002 (.011)	.002 (.006)
Cohorts	1961-1970	1966-1970	1961-1970	1966-1970	1961-1970	1966-1970
Observations	538327	261317	549478	261317	277554	131491

Panel B						
Dependent variable:	Ln (wage)		Professional	Ln (Working hours)		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	(males)	(males)	(males)	(males)	(males)	(males)
East*Treated (Def. 2)	.001 (.013)	-.001 (.007)	-.002 (.008)	.003 (.010)	.004 (.003)	0.001 (.004)
Cohorts	1961-1970	1966-1970	1961-1970	1966-1970	1961-1970	1966-1970
Observations	198705	111077	76322	53878	247158	116880

NOTE: In Panel A columns (i) and (ii) the dependent variable is a dummy variable equal to one if the respondent obtained a college degree; in columns (iii) to (vi) the dependent variable is a dummy variable equal to one if the respondent was employed at the time of the survey. In Panel B columns (i) and (ii) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated); in columns (iii) and (iv) 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 columns (v) to (vi) the dependent variable is the log of the number of weekly working hours. Controls include age dummies, state of residence dummies, birth year dummies, a male dummy, and month of birth. Treatment is defined according to Definition 2, that is 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. In Columns (i), (iii) and (v) the sample consists of respondents aged between 31 and 35 and born between 1961 and 1970. In Columns (ii),(iv) and (vi) the sample consists of respondents aged between 31 and 35 and born between 1966 and 1970. 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.

**Appendix Table 2: Migration Decisions**

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, a male dummy, and month of birth. 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.