

Fuchs-Schündeln, Nicola; Masella, Paolo

Conference Paper

Long-Lasting Effects of Socialist Education

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2013: Wettbewerbspolitik und Regulierung in einer globalen Wirtschaftsordnung - Session: University Enrollment and Student Achievement, No. A07-V3

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Fuchs-Schündeln, Nicola; Masella, Paolo (2013) : Long-Lasting Effects of Socialist Education, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2013: Wettbewerbspolitik und Regulierung in einer globalen Wirtschaftsordnung - Session: University Enrollment and Student Achievement, No. A07-V3, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at:

<https://hdl.handle.net/10419/79865>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Long-Lasting Effects of Socialist Education*

Nicola Fuchs-Schündeln

Goethe University Frankfurt and CEPR

Paolo Masella

University of Sussex

January 18, 2013

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. The effects likely stem from non-meritocratic restrictions in access to high school and college, central planning of vocational training, and curricula directed towards the transmission of socialist values in school.

*We thank Bettina Brüggemann, Martin Butzert, and Leonhard Czerny for excellent research assistantship. Fuchs-Schündeln gratefully acknowledges financial support from the Cluster of Excellence "Formation of Normative Orders" and the European Research Council under Starting Grant No. 262116.

1 Introduction

A vast literature emphasizes the accumulation of human capital (and in particular the level of education of the labor force) as a fundamental factor for economic development.¹ There is also a growing consensus that political and economic institutions are at the root of a significant part of the variation in GDP growth across countries.² 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.³

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 (2005), 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 (2011)), 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.⁴

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

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

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

⁴Using 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

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.⁵ 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.⁶ 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

stations, in order to control the information received by citizens.

⁵For 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.

⁶We 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.

of socialist education at reunification. In the GDR, children turning six on or before May 31 of a given year were per decree enrolled in the first grade by September 1 of the same year. We consider as treated individuals born on or after the first of June; individuals born on May 31 or before are instead part of our control group. Within the same birth cohort, treated individuals in East Germany belonging to cohorts 1971 to 1973 were less affected by restrictions in access to college education or a favored apprenticeship than non treated ones, while treated individuals in East Germany belonging to cohorts 1974 to 1977 were exposed to a smaller number of years of socialist teaching. Since the educational system in West Germany did not experience any major changes in the 80s and early 90s, treated respondents born between 1971 and 1977 and educated in the West 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 Kluge (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 abol-

ishment 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.⁷ 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 Germans are more in favor of redistribution and state intervention than West Germans. Barro and

⁷We 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.

McCleary (2005) investigate the effect of Communism on religious beliefs.⁸ 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 and the criteria adopted to select which students have access to higher education, as well as the rules of apprenticeship choice. Brunello et al. (2010), 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.⁹ We add to those 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.¹⁰ 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 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

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

⁹This means that they compare cohorts being born many years apart.

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

(Clots and Masella (2010)). We try to assess the impact of indoctrination and, more in general, contents of teaching within a socialist country on the individual performance in the labor market of a Western economy.¹¹

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 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 per cent had a

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

duration of two years, 11 per cent of 2.5 years, and a further 11 per cent of 3 years.¹² 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.¹³ This second type covered 57 per cent 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 per cent attended university, 18 per cent attended a *Fachschule*, and 70 per cent started an apprenticeship.¹⁴

2.2 Socialist Content of Education

Socialist teaching was an integral and official part of the GDR curriculum. Social studies was an official school subject from seventh grade on. It aimed at providing a deep knowledge of Marxism-Leninism, and of the socialist system of the GDR. Also beyond this specific subject area, a general socialist education was an official aim of the general curriculum. As discussed by Block and Fuchs (1993), the main goal of socialist education was to create a socialist personality; critical thinking was not incentivized and divergent opinions were suppressed. Theorems and theories were never the subject of discussion, but rather dogmas to be memorized. The most important foreign language taught in schools was Russian; it was a compulsory subject in every school in East Germany.

¹²The 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.

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

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

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 the 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).

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.

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. Chil-

dren of workers were more likely to be accepted into the *EOS* than children coming from an academic background.¹⁵ 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.

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 wishes for a certain apprenticeship in their application.¹⁶ Firms were obliged to offer apprentices a permanent position after 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.

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

¹⁵Each 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).

¹⁶Students 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).

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. Students were allowed to learn other foreign languages such as English and French.

The socialist content of education was abolished almost immediately, and restrictions 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).¹⁷ 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).

¹⁷In 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).

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. 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 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.

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. Exceptions to this rule were rare. According to the official GDR School Statistics, in the school year 1986/87, 2.6 per cent of children deferred school by a year, and 0.14 per cent entered school a year earlier than usual.¹⁸ Thus, more than 97 per cent of a given cohort followed the official rule. 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.

¹⁸The corresponding numbers for the school year 1966/67 are 8.1 per cent and 0 per cent.

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 (2004-2008). X is a vector of control variables including a male dummy, a full set of age dummies, and a full set of state of residence dummies. When we include state dummies, the East dummy drops out. γ_c is a full set of birth year dummies.¹⁹ 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 β_1 captures the effect of living in the East on labor market outcomes. The coefficient β_2 controls for any potential effects of being enrolled in school at a slightly older age.²⁰ The coefficient of main interest is β_3 , which captures a differential effect of being treated for East and West Germans. If being treated leads to an additional positive labor market effect in East Germany due to experiencing a shorter GDR education, then β_3 should be positive.

¹⁹We run all specifications also excluding the control variables, and results are robust.

²⁰Puhani and Weber (2007), Bedard and Dhuey (2006), and Fredriksson and Öckert (2005) indicate that being enrolled at an older age can have positive effects on test scores, probability of attending college, and also long-run labor market outcomes (which would be captured by a positive β_2 coefficient). Since the normal cut-off date for enrollment in school used to be June 30 instead of May 31 in West Germany in the 1980s, we also show a robustness check in which treatment is defined as being born on or after July 1 for individuals in the West (see Section 6).

3.2 Affected Birth Cohorts

Affected birth cohorts are those still in education at reunification. 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.

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 1977.²¹ 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. Consider cohort 1973. While treated individuals were about to complete 10th grade and therefore free to choose an educational path that was propaedeutic to college education or an apprenticeship of their choice in the summer of 1990, individuals in the control group may have been forced to participate in apprenticeship programs instead of attending *EOS* (which granted the university-entrance diploma) in the summer of 1989. Within cohort 1972, individuals in the control group had attended one year more of the apprenticeship program in November 1989, therefore increasing the cost of switching to the educational path needed to be able to attend college, or to switch the apprenticeship subject. On the other hand, as discussed in

²¹We 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.

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. 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.²² 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, and right after reunification many universities in the East could not accept all individuals applying for slots. 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.²³

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

²²Males typically served the 18 months compulsory military service either right after finishing EOS, or after finishing an apprenticeship (Zimmermann et al., 1985).

²³Of 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.

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

$$\begin{aligned}
Y_{ic} = & \beta_0 + \beta_1 East_{ic} + \beta_2 Treat_{ic} & (2) \\
& + \beta_3 (East * cohort_{71-73})_{ic} + \beta_4 (Treat * cohort_{71-73})_{ic} \\
& + \beta_5 (East * Treat * cohort_{71-73})_{ic} + \beta_6 (East * Treat * cohort_{74-77})_{ic} \\
& + \beta'_9 (X)_{ic} + \gamma_c + \varepsilon_{it}
\end{aligned}$$

Now β_5 and β_6 capture any potential additional effect of treatment in the East compared to the West for the birth cohorts 1971 to 1973 and 1974 to 1977, respectively.²⁴ β_5 should therefore capture the effects of exposure to socialist content of education, β_6 the effect of non-meritocratic restrictions in access to high school and college, as well as restricted apprenticeship choice.

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,²⁵ thus not coinciding exactly with our treatment/control definition. We will use this less precise definition of treatment, in which individuals born in May are incorrectly assigned to the

²⁴This is a standard triple interaction framework, where the three terms are: *East*, *Treat* and *cohort*₇₁₋₇₃. Given that we include a full set of birth cohort dummies, γ_c , we do not include the dummy variable *cohort*₇₁₋₇₃ as well.

²⁵In 2004 the two categories are slightly different: January to March, and April to December.

treatment group rather than the control group, when we want to study the outcomes of older cohorts and therefore need data from the earlier survey rounds in order to observe them at the relevant age.²⁶ We call this less precise definition of treatment “Definition 2”. We focus on individuals aged 31 to 35. We only start at age 31 in order to capture labor market outcomes at an age at which individuals are already settled in the labor market. We stop at age 35 such that the age composition of the different cohorts 1971 to 1977 is not too different in our sample 2005 to 2008.

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

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. 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.²⁷ Employment is equal to 1 if the self-reported employment status

²⁶This is the case for Figure 1 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.

²⁷As discussed in Section 2.3, around half of *Fachschule*-students got an education comparable to the one at an applied university. We present results from a robustness

is given as employed.²⁸ 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,²⁹ 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 manager, a professional, or a technician or associate professional according to the ISCO classification.³⁰

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

4 Results

4.1 Results for College Graduation

Figure 1 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 (grey thin 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

check in which we assign *college* = 1 also to an individual with the highest educational degree from a *Fachschule* in Section 6.

²⁸Alternatively, we define as employed any individual indicating positive hours worked in a usual work week. Results are robust.

²⁹As 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.

³⁰These three groups correspond to the major groups 1, 2, and 3 in the ISCO88 classification.

2005; therefore, we have to use the less precise definition of treatment. 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 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 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, and state dummies). 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 treatment is insignificant, indicating that enrollment into school at a later age has an insignificant effect on college completion in the West. 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 1.³¹ Last, 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

³¹Due to the inclusion of state dummies in column (ii), the East dummy is omitted there.

rate of around 15 percent in the East, this is a large effect.³² 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 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.³³ Summarizing, we find significant negative effects of the length of exposure to socialist contents of education and restricted access to higher education in the GDR on college completion rates.

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

³²The 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).

³³p-values vary between 0.103 and 0.187.

are only present for men. They are however quite large for men, indicating a 4.1 per cent 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. For vocational education, we document that indeed a higher percentage of individuals about to start the apprenticeship or being in the first year of apprenticeship had their contracts dissolved than individuals in the second year of apprenticeship (see Section 2.4). 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.³⁴

³⁴Selection also can 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 percent and on the probability of having a professional job 5.1 percentage points.

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.³⁵ 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 aimed at encouraging independent thinking and individual initiative rather than the creation of a “socialist” personality may have stimulated individual participation in the labor market, as well as effort in job searches and in the workplace. The sizes of the coefficients are relevant from an economic point of view for both employment and working hours: male respondents born between 1974 and 1977 are 2.1 percentage points more likely to be employed, and also increase their working hours by 1.5 per cent when they receive one year less of socialist education, compared to treated individuals in the West.³⁶

For females, none of the coefficients on the interaction variables of interest is significant.³⁷ We believe that the lack of significant results for females, in contrast to males, can be explained by their generally lower labor market attachment. Women typically experience labor market breaks in their career, and especially at the beginning of their career, through the arrival of children. These breaks often lead to new professional orientations. Therefore, any initial effects of restricted access to education or to the desired apprenticeship might show up less strongly for women over time than for men. It

³⁵For 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 7.

³⁶It 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.

³⁷All 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.

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 male sample for the dependent variables working hours, wages, and professional status, given that the baseline results are only significant for men.³⁸ For college attendance, we conduct the analyses on the full sample, and for employment on both full and male samples given that the baseline results were significant for all or just for men, depending on the cohort. Last, when we split up by cohort groups, we show results only for the cohort group for which the baseline results are significant.³⁹

5.1 Can Results Be Explained by General 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. To make sure that this is not the case,

³⁸For 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.

³⁹Robustness checks on all other cohort or gender groups are omitted for reasons of space and available from the authors upon request.

we run regressions on two placebo cohort groups, namely those born 1961 to 1970, or those born 1966 to 1970. All respondents born 1970 or before had already completed primary and secondary education when the Berlin Wall fell.

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).

Table 5 presents the results (columns (i), (iii), and (v) for the cohorts 1961 to 1970, columns (ii), (iv), and (vi) for cohorts 1966 to 1970). For simplicity, only the coefficients on the interaction variable of interest are shown. In Panel A, college degree and employment are the dependent variable, and results are shown for the full sample in case of college, and for the full and male samples in case of employment. For the other three labor market outcome variables in Panel B, we show results only for males, as explained above. None of the coefficients of interest on the interaction of East and Treatment are significant, and indeed most are very close to zero. Thus, we are quite sure that we indeed capture effects of socialist education, rather than general East or West German trends.

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 the socialist culture in general matters, unless we believe that exposure to socialist culture should have a stronger impact at a younger age and in particular at the age when cohorts 1971 to 1977

experienced reunification.

Still, we perform 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 6 and 7. 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 6 and 7.

Results for college graduation are presented in Table 6. As expected, the coefficient of interest stays positive and significant at the 5 per cent 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 7 report the coefficients on the relevant interaction terms. As in Table 6, all coefficients of interest in specification (i), where the 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.⁴⁰ Columns (iii) of Tables 6 and 7 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 collected information on the unemployment rate in East and West Germany from 1989 onwards.⁴¹ We then generate a variable "unemployment 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 in the year of labor market entrance" as a control to the baseline specifications. As columns (iii) of Tables 6 and 7 show, the results are robust to adding this additional control variable. The coefficient on the unemployment rate (not reported) is mostly insignificant, but 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 (results available from the authors upon request). Last, we also included

⁴⁰However, 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.

⁴¹These 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).

the youth unemployment rate (for individuals aged 15 to 25) 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. The results that concern cohort B, namely the ones in the lower two panels in Table 7, are robust to including this unemployment rate both in levels or also additionally with a quadratic term (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)). In particular, this assumption may bias the coefficient of the interaction term between the variables “East” and “Treatment” upwards, if the most talented and hard working respondents in the treatment group were less likely to migrate from the East towards the West than the corresponding most talented and hard working respondents in the control group. 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 Microcensus 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 (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 full sample size we use the less precise definition of treatment.⁴²

Table 8 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 have been somehow influenced by the reorganization of the educational system after 1989. Yet, 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, 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

⁴²Results do not change if we use the correct definition of treatment and restrict the sample to the 2005 and 2008 surveys.

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 6 and 7 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.⁴³ In column (v), we redefine treatment in the West as being born between July

⁴³To 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.

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 6. As expected, the coefficient of interest stays positive and significant at the 5 per cent level in both specifications. 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 6, all coefficients of interest in specifications (iv) and (v) remain significant when we consider labor market outcomes as dependent variables in Table 7.

In column (vi) of Table 6, 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 Conclusions

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. 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 outcomes. 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 change in curricula 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 school curricula.

References

- [1] Acemoglu, Daron, Simon Johnson and James Robinson (2001). "The Colonial Origins of Comparative Development: An Empirical Investigation", *American Economic Review*, 91(5): 1369-1401
- [2] Acemoglu, Daron, Simon Johnson, and James Robinson (2005). "Institutions as the Fundamental Cause of Long-Run Growth", in: Aghion, Philippe, and Steven Durlauf (eds.): *Handbook of Economic Growth*, Elsevier, 385-472.
- [3] Aghion Philippe, Torsten Persson, and Dorothee Rouzet (2012). "Education and Military Rivalry", NBER Working Paper 18049.
- [4] Algan, Yann, Pierre Cahuc, and Andrei Shleifer (2011). "Teaching Practices and Social Capital", NBER Working Paper 17527.
- [5] Alesina, Alberto, and Nicola Fuchs-Schündeln (2007). "Goodbye Lenin (or Not?): The Effect of Communism on People's Preferences", *American Economic Review*, 97(4): 1507-1528.
- [6] Angrist, Joshua D., and Alan B. Krueger (1991). "Does Compulsory School Attendance Affect Schooling and Earnings", *Quarterly Journal of Economics*, 106(4): 979-1014.
- [7] Angrist, Joshua D., and Victor Lavy (1997). "The Effect of a Change in Language of Instruction on the Returns to Schooling in Morocco", *Journal of Labor Economics*, 15(1): 48-76.
- [8] Barro, Robert J. (1996). "Democracy and Growth", *Journal of Economic Growth*, 1(1): 1-27.
- [9] Barro, Robert J. (1999). "Determinants of Democracy", *Journal of Political Economy*, 107(S6): S158-S183.

- [10] Barro, Robert J., and Rachel M. McCleary (2005). "Which Countries Have State Religions?", *Quarterly Journal of Economics*, 120(4): 1331-1370.
- [11] Baske, Siegfried (1998). "Das Hochschulwesen", In: Führ, Christoph, and Carl-Ludwig Furck ed. *Handbuch der Deutschen Bildungsgeschichte Band VI, 1945 bis zur Gegenwart, Zweiter Teilband Deutsche Demokratische Republik und neue Bundesländer*, Kapitel 4: Schulen und Hochschulen. München: C.H. Beck.
- [12] Bedard, Kelly, and Elizabeth Dhuey (2006). "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects", *Quarterly Journal of Economics*, 121(4): 1437-1472.
- [13] Benhabib, Jess and Mark M. Spiegel (1994). "The Role of Human Capital in Economic Development: Evidence from Aggregate Cross-Country Data", *Journal of Monetary Economics*, 34(2): 143-173
- [14] Block, Klaus-D., and Hans-W. Fuchs (1993). "The Eastern German Education System in Transition", in: Reuter, Lutz R., and Gerhard Strunk ed. *Universität der Bundeswehr Hamburg Beiträge aus dem Fachbereich Pädagogik*.
- [15] Botero Juan, Ponce Alejandro, and Andrei Shleifer (2012). "Education and the Quality of Government", NBER Working Paper 18119.
- [16] Bowles, Samuel, and Herbert Gintis (1976). "Schooling in Capitalist America: Educational Reform and the Contradictions of Economic Life", New York: Basic Books.
- [17] Brunello, Giorgio, Elena Crivellaro, and Lorenzo Rocco (2010). "Lost in Transition? The Returns to Education Acquired under Communism 15 Years after the Fall of the Berlin Wall," IZA Discussion Paper 5409.

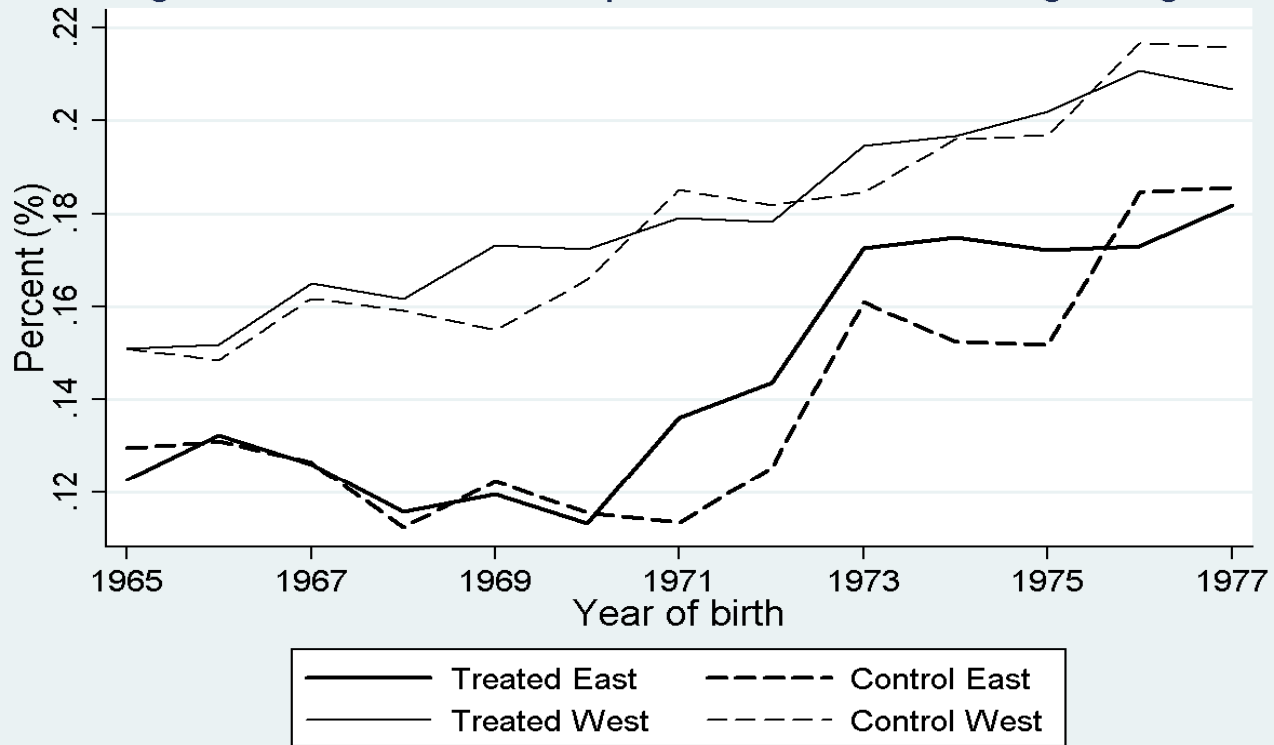
- [18] Bundesministerium für Bildung und Wissenschaft (1991). "Berufsbildungsbericht 1991", Schriftenreihe Grundlagen und Perspektiven für Bildung und Wissenschaft, 28.
- [19] Cantoni, Davide and Noam Yuchtman (2011). "Educational Content, Educational Institutions and Economic Development: Lessons from History", Discussion Papers in Economics 12691, University of Munich, Department of Economics.
- [20] Chetty, Raj, John N. Friedman, and Jonah E. Rockoff (2011): The Long-Term Impacts of Teachers: Teacher Value-Added and Student Outcomes in Adulthood, NBER Working Paper 17699.
- [21] Clots-Figueras, Irma, and Paolo Masella (2010). "Education, Language and Identity", mimeo.
- [22] Fertig, Michael, and Jochen Kluge (2005). "The Effect of Age at School Entry on Educational Attainment in Germany", IZA Discussion Paper 1507.
- [23] Fischer, Andreas (1992). "Das Bildungssystem der DDR: Entwicklung, Umbruch und Neugestaltung seit 1989", Darmstadt: Wissenschaftliche Buchgesellschaft.
- [24] Fredriksson, Peter, and Björn Öckert (2005). "Is Early Learning Really More Productive?: The Effect of School Starting Age on School and Labor Market Performance", IZA Discussion Paper 1659.
- [25] Fuchs-Schündeln, Nicola, and Matthias Schündeln (2009): "Who Stays, Who Goes, Who Returns? East-West Migration within Germany since Reunification", *Economics of Transition*, 17(4): 703-738.
- [26] Funken, Klaus (1996): Keine Wende am Arbeitsmarkt in Ostdeutschland: eine Zwischenbilanz im Jahre 1996, in: *Wirtschaftspolitische Diskurse*, Friedrich Ebert Stiftung, Nr. 89.

- [27] Gesetzesblatt der Deutschen Demokratischen Republik (1985). Teil I: Nr. 4 1985. "Erste Durchführungsbestimmung zur Verordnung über die Facharbeiterberufe - Systematik der Facharbeiterberufe - vom 21. Dezember 1984", veröffentlicht am 20. Februar 1985.
- [28] Glaeser, Edward, Rafael La Porta, Florencio Lopez de Silanes, and Andrei Shleifer (2004). "Do Institutions Cause Growth?", *Journal of Economic Growth*, 9: 271-303.
- [29] Glaeser, Edward, Giacomo Ponzetto, and Andrei Shleifer (2007). "Why Does Democracy Need Education?", *Journal of Economic Growth*, 12(2): 77-99.
- [30] Hall, Robert E. and Charles Jones (1999). "Why Do Some Countries Produce so Much More Output per Worker than Others?", *Quarterly Journal of Economics*, 114(1): 83-116.
- [31] Hunt, Jennifer (2006): "Staunching Emigration from East Germany: Age and the Determinants of Migration", *Journal of the European Economic Association*, 4(5): 1014-1037.
- [32] Hoffmann, Florian, and Philip Oreopoulos (2009). "Professor Qualities and Student Achievement", *Review of Economics and Statistics*, 91(1): 83-92.
- [33] Köhler, Helmut (2008). "Datenhandbuch zur deutschen Bildungsgeschichte Band IX: Schulen und Hochschulen in der Deutschen Demokratischen Republik 1949-1989", Göttingen: Vandenhoeck & Ruprecht.
- [34] Krueger, Alan B. and Mikael Lindhal (2001). "Education for Growth: Why and for Whom?", *Journal of Economic Literature*, December 2001, 39(4): 1101-1136.

- [35] Lochner, Lance, and Enrico Moretti (2004). "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports", *American Economic Review*, 94(1): 155-189.
- [36] Lott, John R. (1999). "Public Schooling, Indoctrination, and Totalitarianism", *Journal of Political Economy*, 107(6): S127-S157.
- [37] Malamud, Ofer, and Cristian Pop-Eleches (2010). "General Education versus Vocational Training: Evidence from an Economy in Transition", *Review of Economics and Statistics*, 92(1): 43-60.
- [38] Mankiw, N. Gregory, David Romer, and David N. Weil (1992). "A Contribution to the Empirics of Economic Growth", *Quarterly Journal of Economics*, 107(2): 407-437.
- [39] Murtin, Fabrice and Romain Wacziarg (2011). "The Democratic Transition", NBER Working Paper 17432.
- [40] Munich, Daniel, Jan Svejnar, and Katherine Terrell (2005). "Returns to Human Capital under the Communist Wage Grid and during the Transition to a Market Economy", *Review of Economics and Statistics*, 87(1): 100-123.
- [41] North, Douglass C. (1981). "Structure and Change in Economic History". New York: Norton & Co.
- [42] Ockenfels, Axel, and Joachim Weimann (1999). "Types and Patterns: An Experimental East-West-German Comparison of Cooperation and Solidarity", *Journal of Public Economics*, 71(2): 275-287.
- [43] Orazem, Peter and Milan Vodopivec (1997). "Value of Human Capital in Transition to Market: Evidence from Slovenia", *European Economic Review*, 41(3-5): 893-903.

- [44] Papaioannou, Elias, and Gregorios Siourounis (2005). "Economic and Social Factors Driving the Third Wave of Democratization", London Business School, mimeo.
- [45] Pischke, Jörn-Steffen, and Till von Wachter (2008). "Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation", *Review of Economics and Statistics*, 90(3): 592-598.
- [46] Puhani, Patrick A., and Andrea M. Weber (2007). "Does the Early Bird Catch the Worm?: Instrumental Variable Estimates of Educational Effects of Age of School Entry in Germany", *Empirical Economics*, 32: 359-386.
- [47] Rust, Val Dean, and Diane Rust (1995). "The Unification of German Education", *Garland Reference Library of Social Science*, 960.
- [48] Saint-Paul, Gilles (2010). "Endogenous Indoctrination: Occupational Choices, the Evolution of Beliefs and the Political Economy of Reforms," *The Economic Journal*, 120: 325–353.
- [49] Waterkamp, Dietmar (1987). "Handbuch zum Bildungswesen der DDR", Berlin: Spitz.
- [50] Wehrmeister, Frank (2005). "Betriebsberufsschulen in der ehemaligen DDR", In: Illerhaus, K. ed. *Koordinierung der Berufsausbildung in der Kultusministerkonferenz. Festschrift anlässlich der 250. Sitzung des Unterausschusses für Berufliche Bildung der Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik.*, 70-85, Bonn.
- [51] Zimmermann, Hartmut, Horst Ulrich, and Michael Fehlauer (1985). "DDR-Handbuch", Köln: Verlag Wissenschaft und Politik.

Figure 1: Fraction of 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.

Table 1: Sample Characteristics

	East			West	
	mean/percent	Treated mean/percent	Control mean/percent	Treated mean/percent	Control mean/percent
	(i)	(ii)	(iii)	(iv)	(v)
College	0.196	0.169	0.149	0.196	0.196
Employed	0.740	0.740	0.755	0.738	0.749
Ln (work hours)	3.496 (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)	-.000 (.002)	.001 (.004)	-.001 (.003)
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, and a male dummy. The variable East is dropped from the specifications in Columns (ii) to (v) 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, and a male dummy. 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) all	(ii) males	(iii) females	(iv) all	(v) males	(vi) females	(vii) all	(viii) males	(ix) females	(x) all	(xi) males	(xii) 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*** (.004)	-.027 (.019)	.018* (.010)	.013 (.010)	.022 (.014)	-.001 (.007)	.001 (.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, and a male dummy. 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: 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)	.004 (.006)	-.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, and a male dummy. 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.

TABLE 6: 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. 2)			.015** (.006)			
East*Treated (Def. 3)				.015** (.007)		
East*Treated (Def. 1)					.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 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, and a male dummy. 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: Robustness Checks (Labor Market Outcomes)

Panel A : East*Treat*cohort71-73						
	sample	(i)	(ii)	(iii)	(iv)	(v)
Ln (wage)	male	.029** (.012)	.022 (.019)	.034*** (.009)	.030*** (.012)	.041*** (.009)
Professional	male	.045*** (.012)	.001 (.020)	.05*** (.009)	.044*** (.011)	.048*** (.008)
Employed	all	-.016** (.007)	.010 (.016)	-.024* (.012)	-.019* (.011)	-.020** (.009)

Panel B: East*Treat*cohort74-77						
		(i)	(ii)	(iii)	(iv)	(v)
Employed	male	.043*** (.013)	-.031 (.023)	.023*** (.004)	.016* (.008)	.017*** (.005)
Ln (Working hours)	male	.013* (.007)	-.003 (.009)	.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 row (1) the dependent variable is the log of wages (see the text for a detailed explanation of how wages are calculated). In row (2) 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 rows (3) 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 (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 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, and a male dummy. 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 8: Migration Decisions

	(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, and a male dummy. 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 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.