Self-selection and the returns to geographic mobility: what can be learned from the German reunification "experiment" *

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Abstract

This paper investigates the causal effect of geographic mobility on income. The returns to German East-West migration and commuting are estimated exploiting the structure of centrally planned economies and a "natural experiment" of German reunification for identification. I find that migration premium is insignificantly different from zero, the returns for commuters equal to 40 percent, and the local average treatment effects for compliers are insignificant. In addition, estimation results suggest no positive self-selection on unobservables for migrants, and some evidence of positive self-selection on unobservables for commuters.

JEL Classification: F22, J61, J62, R23.

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‡The data used in this paper is publicly accessible and available from the DIW, Berlin.

1 Introduction

With cumulative net migration of 7.5% of the original population over the period 1989-2001, East Germany has the second highest emigration rate (after Albania) among the countries formerly behind the Iron Curtain (Brücker and Trübswetter, 2004, Heiland, 2004). The emigration rates have tended to increase again since 1997, and there seems to be no sign of income convergence from 1995 onwards (Figure 1 and OECD, 2001). Moreover, due to the particular geography of Germany, commuting to the West is a popular option for those who do not want to incur fixed costs of moving, and it may substitute for emigration. Since geographic mobility constitutes an investment in human capital, these phenomena have raised concerns that individuals with high abilities move to the West ("brain drain") and contribute to sluggish economic growth in the East, as well as raising the question of how large the mobility premium is in the West. Such issues are also gaining general importance in light of the eastern enlargement of the European Union in May 2004 and resulting European East-West migration.

In attempting to answer these questions, it is important, however, to separate the pure effect of geographic mobility from the effect of confounding factors. The reason why doing this is difficult is often attributable to the unavailability of the relevant data and credible exclusion restrictions. As a result, there exists no study up do date that estimates the *causal* effect of geographic mobility on income. This chapter attempts to fill the gap. In its main contribution to the literature it exploits the structure of centrally planned economy of the former German Democratic Republic (GDR) together with the unique event of German reunification in order to make causal statements about the returns to geographic mobility from East to West Germany, controlling for the potential self-selection on unobservables.

Migration theory (Roy, 1951, Borjas, 1987) postulates that migrants will be positively selected if the distribution of earnings is more unequal in the destination region than in the origin.[1] There exists a vast empirical literature on migration, in which authors have investigated the selectivity issue, using standard Heckman‘s procedure, or have documented the association between migration and income. The majority of the existing empirical studies on East-West German migration address the question of self-selection indirectly.[2] The first study that explicitly deals with this issue is a recent paper by Brücker and Trübswetter (2004), in which the authors find no robust evidence of positive self-selection on unobservables for migrants during 1994-1997. As for the mobility premium, Hunt (2001) shows that those who took a job in the West between 1990-1991 enjoyed large wage gains, but that the correlation between income and migration is small and insignificant for the subsequent movers. She concludes that an economy undergoing a successful transition would initially have high returns to moving, which would fall as the transition progressed. It is not clear, however, what kind of effect is estimated and how the selection on unobservables is dealt with.

This chapter exploits programme evaluation techniques, and attempts to identify the effect of treatment (geographic mobility) on the treated (mover), as well as the effect for

[1] Chiswick (1999) shows that Roy‘s model is a special case of the human capital model of migration (Sjaastad, 1962, Harris and Todaro, 1970).

[2] Burda (1993), Burda et al (1998) analyze individuals‘ intentions to move West. Hunt (2006) estimates the reduced form multinomial logit of the decisions to move, to commute or to stay.

compliers (a subpopulation of movers whose status changes with the instrument). I investigate these questions using both parametric and nonparametric econometric methodologies. Home ownership and geographic residence before unification are argued to provide the exogenous sources of variation in migration and commuting, respectively, since in ex-GDR housing decisions and voluntary geographic labour mobility were usually restricted. Moreover, German reunification was not anticipated by anybody until shortly before the event. Although one may still argue that the allocation of housing, jobs and residence of individuals in the Communist economy was not random, it was largely based on the factors that are not relevant for the market economy and the post-unification individual incomes, which are thus ignorable.

The main findings of this chapter are as follows. First, no evidence of positive selection on unobservables for migrants and some evidence of positive self-selection for commuters is found. Second, the returns in terms of long-run income are insignificant for both migrants and compliers. The returns for commuters are high and equal to 40 percent, but the local average treatment effect for compliers is insignificant.

The chapter is organized as follows. Section 2.2 provides the description of the data and section 2.3 justifies the instruments. Section 2.4 outlines estimation strategy. Estimation results are discussed in section 2.5, and section 2.6 provides a sensitivity analysis. Section 2.7 concludes.

2 Data, definitions and sample selection

The data used in this chapter are extracted from the public use file of the representative German panel household survey (GSOEP)[3], and are merged with the confidential geographical coding of individual places of residence. Due to the GSOEP's longitudinal structure, it is possible to identify and trace movers (and their incomes). Another advantage of this dataset is that the first wave of the eastern sample was drawn in June 1990, i.e. before the monetary union and formal unification took place, and, thus provides a unique opportunity to use pre-unification data to construct the exogenous source of variation in mobility. The main disadvantage of the dataset is the small number of observations for movers.

An individual is defined as a migrant if he has changed his residence from East to West Germany at least once during 1990-2001; otherwise he is a stayer. An individual is a commuter if he lives in the East and his region of work is West Germany in any of the years 1990-2001.[4] A definition of income is not trivial in such a study. Theory suggests that while making a decision to move, an individual takes into account his total lifetime income, and empirical studies find that the assimilation period matters.[5] In order to be consistent with the theoretical definition of lifetime "permanent" income, as well as willing to avoid the problem of transitory income drop right after move and to save observations,

[3] see SOEP Group (2001).

[4] Note that when defining migrants in this way I have to include commuters within "stayers", and when defining commuters - actual and potential migrants within "stayers". I also experiment with excluding actual and potential movers from the respective comparison groups (section 6).

[5] It is argued that estimates based on earnings data with limited time horizons will not capture life-cycle wage growth, tending to downward bias in the estimated returns (Greenwood, 1997).

I have used the mean of annual incomes as a dependent variable. I thus average over the available years for stayers, over the available years after an individual migrates for migrants, and over the years during which an individual commutes for commuters. The total annual income is defined as a sum of labour income (wages, second-job and self-employment earnings) and various social security benefits (such as unemployment benefits, maternity benefits etc.). The mean income is set to missing only if information on all the components is missing.[6] All incomes are inflated to 2001 by regional CPIs and are expressed in DM.

The instruments used in this study are as follows. For migration, I construct a dummy which equals one if an individual was a home-owner in 1990, and is zero otherwise. 32 percent of the respondents in 1990 in East Germany reported owning a house / flat. For commuting, an instrument equals one if an individual resided in a county (*"kreise"*) that had a common border with West Germany or West Berlin before unification. Approximately 30 percent of persons lived in such counties in 1990. Both instruments approximate theoretical costs of moving: the former captures the well-established negative relation between home ownership and the propensity to migrate, while the later captures the costs of commuting West that increase with distance from the border.

I restrict the sample to easterners who were living in East Germany in 1990, exclude pensioners and students, and use the incomes of individuals who are at least 18 years old in each year.[7] Final sample sizes in the most restricted specifications are 3,043 observations

[6] I also exclude the obvious outliers from the sample, i.e. individuals whose average annual income is less than 1,000 DM (19 observations) or greater than 130,000 DM (5 observations). I have experimented with different thresholds and kept all individuals in the sample, and the results were not affected.

[7] I also drop the return and multiple migrants here (around 20%), but retain them in the robustness

for migration (of whom around 6% are migrants[8]), and 2,953 observations for commuting (of whom around 15% are commuters).

Kernel densities of average total annual incomes for movers and stayers are shown in Figure 2. As expected, the distribution of incomes for stayers is more compressed, and there are more migrants and commuters in the upper tail of income distribution. Descriptive statistics for the key variables is given in Table 1. All potential movers have on average a higher total annual income than stayers. Compared to stayers, migrants tend not to own a house in 1990, and commuters tend to live in the border regions in 1990. As expected, potential movers are younger, single and better educated than stayers. There are more males among commuters, however, more females among migrants. Table 1 presents some systematic differences in observable characteristics between movers and stayers; thus, there is a reason to suspect, a priori, that selection on unobservables will be an issue. To cope with this, I rely in the remainder of the chapter on the instrumental variables, which are justified in the next section.

3 Are the instruments legitimate?

In order to make causal statements about the returns to geographic mobility, it is important to justify the validity of the instruments. Unfortunately, this assumption cannot be tested, and one has to rely on the available general facts. To be a valid instrument, pre-unification home ownership and residence dummies must affect income only through

checks (section 6).

[8] this number is consistent with the aggregate figures.

migration or commuting, i.e. they must be uncorrelated with any non-ignorable confounding factors that affect ex-post income in the market economy, such as ability or motivation. This can be justified by referring to the structure of centrally planned economies.

In GDR, as in any Communist societies, there was a high degree of centralization in the labour and product markets: all firms were owned by the state and an elaborate plan directed the allocation of inputs, the distribution of outputs, wage levels and prices (Krueger and Pischke, 1995). To secure constant prices for inhabitants, the state bore 80% of costs of basic supplies, from bread to housing. Shortages were a norm. The distribution of income was compressed, and wage inequality was very low.[9] Official unemployment was absent, since workers were kept inefficiently in companies even if they were unproductive. Political tolerance was important: the system functioned smoothly only when its component parts were staffed with individuals whose values coincided with those of the regime. In general, the communist ideology stressed uniformity of outcomes, irrespective of individual differences in ability or effort.

Housing and occupational choices, and thus voluntary geographic labour mobility, were restricted. In principle, everyone had a right to a house; however, due to rationing by the state (the so-called System of Material Balances), long queues were a norm.[10] Access to housing was regulated largely through informal (and often politically mediated) networks. In many ways access to material and social activities in ex-GDR was mediated through

[9] Fuchs-Schündeln and Schündeln (2005) report that in 1988, the average net income of individuals with a university degree was only 15% higher than that of blue-collar workers.

[10] The "waiting list" could be very long. For example, the wait for an apartment in the Soviet Union during the 1980s was typically 10 to 15 years; as a result, families had to plan and buy housing for their children to live in in advance (www.wikipedia.com).

the sphere of work, and, in particular, the FDGB unions acted as the prime political link between the working population and the Socialist power elite, and as key agents in the distribution of housing. In general, flats were allocated to individuals due to urgent need or merit, personal connections or corruption, or by inheritance. Those who paid a nominal rent for a state-owned flat enjoyed considerable consumer surplus (Kornai, 1980). As for the occupational choice, job offers were usually made to individuals immediately after completion of their education and according to the Socialist plan. Even admissions to the various fields were regulated by the plan.[11]

Overall, the Communist system operated like a large internal labour market, with rules and party membership playing an important role in the allocation of jobs and wages (Krueger and Pischke, 1995). As a result, little was left to individual abilities and motivation. Finally, the fall of the Berlin Wall in 1989 could not been foreseen. Therefore, to the extent that individuals had not been self-selecting into home ownership statuses or into the regions on the basis of their unobservable characteristics relevant for the market economy, the instruments provide the exogenous source of variation in mobility, and the assignment to treatment is strongly ignorable.

However, the exclusion restriction assumption is violated if, for example, more able persons were also more successful in gaining access to their own housing, leading to an upward bias in the estimates. Moreover, in the former GDR, only those who supported the regime (i.e. party members and the so-called "nomenklatura") were likely to be

[11] Only a certain quota of students was allowed to complete the last two years of high school, necessary to attend university. Additional criteria were membership in the official youth organization, political tolerance, and family background (Fuchs-Schündeln and Schündeln, 2005).

allowed to live close to the western border. If these people were also more motivated, the validity of the instrument will be violated unless one controls for the "nomenklatura effect". Fortunately, Bird, Frick and Wagner (1998) provide a proxy for party membership and nomenklatura status - telephone availability before unification, which I also use in the robustness checks (see section 6).

Finally, an informal exercise can be undertaken to further justify the instruments. If they approximate a randomized experiment, the characteristics of those for whom the instrument equals one must be equal to those for whom it equals zero, meaning that persons are randomly assigned across the two groups. Table 2 shows that for migration, the home ownership dummy is indeed orthogonal to some covariates, however there exist differences (at 5%) in some of them. Contrary to expectations, however, the more educated and those having a higher pre-treatment income are *less* likely to own a house before unification. Thus, it is likely that housing was not randomly allocated to individuals in the Communist economy, however such allocation was probably based on some political factors and personal connections (or corruption) and not on the unobservables that are relevant for the market economy, such as individual ability. Moreover, differences in most characteristics, although statistically significant, are not economically pronounced[12]. For commuting, the border dummy is orthogonal to all covariates with the exception of telephone availability in 1990, which actually confirms the existence of the "nomenklatura effect".

Therefore, although one still may argue that the allocation of housing and residence of

[12] Differences in all characteristics range from 9 to 20% of the respective standard deviations.

individuals in the Communist economy was not random, it was largely based on factors that are not relevant for the market economy and the post-unification individual incomes. Overall, I believe that the evidence presented in this section allows to make a valid causal inference, at least for commuters.

4 Econometric methodology

In order to estimate the causal effect of geographic mobility on the income of movers, potential outcomes model is used. Let Y_{1i} and Y_{0i} denote individual i's potential income with and without movement. Then:

$$Y_{1i} = X_{1i}\beta_1 + \varepsilon_{1i} \tag{1}$$

$$Y_{0i} = X_{0i}\beta_0 + \varepsilon_{0i} \tag{2}$$

where X_{ki} are individual socio-economic characteristics, $\beta' s$ are unknown parameters, $E(\varepsilon_{ki}) = 0$, and $k = \{0, 1\}$. Let $D_i = 1$ if individual is a mover, and $D_i = 0$ otherwise. The outcome is observed only in one state, i.e. $Y_i(D_i) = D_i Y_{1i} + (1 - D_i)Y_{0i}$. After some manipulations one can derive the following model:

$$Y_i = \alpha_0 + X_i\beta + \Delta_i D_i + \eta_i \tag{3}$$

where Y_i is the observed outcome, and the "unconditional" error term η_i has a zero mean, i.e. $E(\eta_i|X_i) = 0$, but $E(\eta_i|D_i, X_i) \neq 0$.

Assuming further that there exist costs of moving C_i, the following selection rule applies:

$$D_i = I(Y_{1i} - Y_{0i} - C_i > 0) = I(Z_i\gamma + u_i > 0) \tag{4}$$

where Z_i is a vector of exogenous variables, γ are the reduced form parameters and $E(u_i) = 0$. The errors $(\varepsilon_{1i}, \varepsilon_{0i}, u_i)$ are assumed to be correlated with covariances σ_{ki}, $k = \{0, 1\}$. The self-selection works through this correlation in the errors.

The effect of interest in this study is an average effect of treatment on the treated (ATT). Formally it can be written as follows:

$$\begin{aligned} ATT &= E(\Delta_i|Z_i, D_i = 1) = E(Y_{1i} - Y_{0i}|Z_i, D_i = 1) = \\ &= E(Y_{1i}|Z_i, D_i = 1) - E(Y_{0i}|Z_i, D_i = 1) = \\ &= E(\Delta_i) + E(\eta_i|Z_i, D_i = 1) \end{aligned} \tag{5}$$

where the effect is the difference between actual outcome for movers and a counterfactual outcome for movers had they stayed. It equals to the average effect for a random person in the population *plus* the idiosyncratic gain from treatment (the returns to unobservables), and there is no a priori reason to expect $E(\eta_i|Z_i, D_i = 1) = 0$. Thus, the OLS estimation of (5) provides biased and inconsistent estimates.

To calculate the effect of moving West on income I, first, estimate parametric sample selection model of Heckman (1976, 1979). Note that this procedure requires exclusion restrictions. In addition, if the joint normality assumption does not hold, it produces inconsistent estimates. Then, I also estimate the nonparametric sample selection model of Das, Newey and Vella (2003) that does not impose any distributional assumptions and does not restrict the form of the correction function. The identification requires exclusion restrictions, and the model is identified up to an additive constant. The approach amounts to estimating in the first step a conditional probability of selection (propensity score) without making any distributional assumptions, and, in the second step, to approximating the correction function with polynomial series. The order of the correction term is chosen using a leave-one-out cross-validation criterion. I also use two semiparametric techniques to consistently estimate the intercept (Heckman, 1990 and Andrews and Schafgans, 1998). The ATT is then calculated as the difference between the actual outcome for movers and the counterfactual outcome for movers had they stayed. Finally, making no restrictions on unobserved heterogeneity and no distributional assumptions, I also estimate the local average treatment effect (LATE) for compliers (Angrist, Imbens and Rubin, 1996).[13]

5 Estimating the effect of mobility on income

I use the standard Mincerian specification of the income functions. Variables such as experience, education and marital status in 2001 are endogenous; thus, only exogenous variables, such as sex, age and its square (as a proxy for experience), the predetermined

[13] Note that the Random Assignment, Exclusion Restrictions and a Non-zero Effect of the Instrument on the Treatment assumptions are satisfied based on the evidence presented in Sections 3 and 5. SUTVA assumption seems plausible, since movers constitute only a small fraction of the population, thus ruling out general equilibrium effects. Finally, the assumption of Monotonicity (no defiers) also seems plausible, since both owing a house and living far from the border constitute costs for mobility.

marital status (as a proxy for "psychic" migration costs) and human capital variables in 1990 are used.

5.1 Returns to migration

The first stage estimates (available upon request) confirm that, on average, home owners are less likely to migrate and that the instrument is strong (see also Table 4). Probit marginal effects indicate that the probability of moving West decreases with age, males are less likely to migrate, and both university degree and marital status have expected signs, but neither these variables nor occupation variables and state‘s unemployment rate are significant. Heckman‘s second stage estimates (Table 3 column 1) suggest that males have a higher total income than females, experience as proxied by age and its square has the traditional concave profile, and university graduates earn more. However, neither vocational education nor occupational dummies are significant for movers, suggesting that partly human capital acquired in the centrally planned economy is not transferable to the West. The coefficient on the inverse Mills ratio is positive, but insignificant, indicating no evidence of significant positive self-selection on unobservables for movers after having controlled for human capital and demographics. Estimates for stayers (Table 3 column 2) suggest that, on average, male stayers have a higher total income than females, university graduates earn more, experience has the expected sign, those who had a vocational degree and were employed in the government sector in 1990 earn more, and those in blue-collar occupations in 1990 earn less. The Mills ratio for stayers is also insignificant.

To test the normality assumption I use the conditional moment test (Newey, 1985, Pagan and Vella, 1989), which indicates that normality cannot be rejected, implying that Heckman's estimates are consistent. Nevertheless, I also experiment with the nonparametric sample selection model and do not restrict the form of the correction function. In the first stage, I estimate linear probability model and construct predicted probabilities. The cross-validation criterion suggested the linear correction function for movers and a polynomial of order 3 for stayers. Table 3 (columns 5-6) shows the nonparametric second stage estimates. The coefficients on covariates for both stayers and movers are similar to the parametric ones. When normality is not imposed, there is again no evidence of positive self-selection for movers.

Finally, I estimate the model by IV-LATE framework. Table 4 (panel A) summarizes the so-called intention-to-treat effects (reduced form migration and income equations, columns 1-2), structural IV (column 3) and OLS estimates (column 4). The IV point estimate is not statistically significant. The local average treatment effect for compliers shows that those individuals who migrate if they did not own a house in 1990, and would have not migrated if they had owned a house, have no significant returns to their ex-post long-run income from migration.

Table 5 (Panel A, columns 1-3) summarizes treatment effects for migrants in the different econometric models used. The effects of migration for both migrants and compliers are not statistically different from zero. One should bear in mind, however, that the results for migration have to be interpreted with caution: there might still exist some doubts on the validity of the instrument, the standard errors in IV are traditionally very large and the coefficients flip from large negative to positive.

Overall, several interesting findings occur from the estimates. First, no evidence of positive self-selection on unobservables for East-West German migrants during 1990-2001 is found. Such a result is partly in line with Brücker and Trübswetter (2004), and is also consistent with the theoretical predictions of the human capital model (Chiswick, 1999), when direct out-of-pocket costs of migration are small. Given that the inequality of earnings in East Germany has approached West German levels in the late 1990s, the standard Roy's model would also predict that a positive selection bias should disappear.[14]

Second, both treatment effect for migrants and the LATE for compliers are insignificant. This result might be a consequence of high unemployment in the East, when people move West not in search of a higher income, but to escape from unemployment, and it may also be the cause of return migration to the East. Together with no positive selection for migrants, it may also reflect attitudes towards risk, or non-transferable human capital. Finally, the exclusion of earlier migration (1989-1990) from the analysis due to the unavailability of data may bias the effects downward, since high initial migration most probably left behind those with the highest migration costs. These results, however, are not entirely surprising. Hunt (2001) finds that the correlation between the wage in the West and migration is insignificant for the post-1991 movers. Burda and Hunt (2001) argue that increased by western unions wages, indeed, "kept the East Germans at home".

[14] Ideally, however, one should estimate year by year regressions in order to document the evolution of the selection bias over years, since the cohort quality effect might be at work here, the first migrants being of better quality than the subsequent movers. Unfortunately, small number of observations preclude me from doing this.

5.2 Returns to commuting

Reduced form estimates for commuters (available upon request) suggest that on average males, young and university graduates are more likely to commute West. The West border dummy has a large positive impact on the probability of commuting (i.e. the instrument is strong, see Table 4) and indicates that the costs of commuting, indeed increase with the distance. Second-stage Heckman‘s estimates (Table 3 column 3) suggest that males and university graduates earn more, and experience has a traditional profile. For stayers, in addition, being employed in the government sector and having a vocational degree in 1990 affect their ex-post incomes positively, while being a blue-collar employee in 1990 affects it negatively. The selection correction terms are insignificant for both commuters and stayers. However, the conditional moment test rejects the normality assumption, implying that parametric estimates are inconsistent.

In the nonparametric model, leave-one-out cross-validation criterion suggested a polynomial of order 2 for commuters and no correction polynomial for stayers. The estimated coefficients for both commuters and stayers (Table 3 columns 7-8) are again similar to those in the parametric model, apart from the correction terms. In addition, the marginal effects of the correction functions for commuters are positive, thus suggesting positive self-selection for commuters.

Panel B of Table 4 shows the intentions-to-treat effects, IV and OLS estimates. Again, IV point estimates are not statistically significant. Hence, the local average treatment effect for individuals who commute if they were living in the border regions in 1990 and who would not have commuted otherwise, is not statistically different from zero.

Table 5 (panel A, columns 4-6) summarizes all the effects. Overall, for commuters, positive self-selection seems to be present. The LATE for compliers is again insignificant. However, the treatment effect for commuters equals to 0.4, suggesting a large 42 percent effect on the average long-run income.

6 Robustness checks

The following sensitivity analysis was undertaken. First, I check how robust the results are to the inclusion of additional controls. I include a dummy which equals one if a person was unemployed in 1990 to check how the lagged employment status influences both decision to move and ex-post incomes. I then add the household monthly income in 1990 in order to capture additional household-level characteristics. Second, I improve on the validity of the instruments controlling for the "nomenklatura" effect mentioned above. One may argue that it is also important to control for the ideology, thus I also include a variable that ranks political interests of a person before unification. Finally, I control for the lagged hours worked per week. Third, I exclude self-employed from the sample, since there might be self-selection into this group. Fourth, I retain all return and multiple movers in the sample. Fifth, I improve the definition of the control group: I drop commuters from the control group for migrants, and migrants - from the control group for commuters. Finally, I also control for the years from which the income is taken in order to take further account of wage convergence. Table 5 Panel B shows these sensitivity checks.

In general, the effects are similar to those reported in Panel A.[15]

One could still argue that the income *growth* and not income per se is a relevant dependent variable as it differences away any fixed effects in income levels. However, it still leaves the selection bias associated with the non-random selection of movers, thus it is still necessary to rely on valid exclusion restrictions in order to get rid of the bias. In panel C of Table 5 all models have been reestimated using income growth as a dependent variable.[16] The results have not changed much. The resulting treatment effects for migrants are again insignificant across all the models. For commuters, consistent nonparametric model suggests ATT equal to 29 percent.

7 Conclusions

The question of the returns to geographic mobility, especially in the context of transition economies, remains difficult to deal with, mainly due to data availability and identification problems. This chapter exploited a structure of the centrally planned economy of ex-GDR and a "natural experiment" of German reunification, and attempted to make a causal inference for the returns to East-West German migration and commuting. Preunification home ownership was argued to provide an exogenous source of variation in migration, and

[15] In addition, all models have been re-estimated without human capital covariates and have generated qualitatively identical results (available upon request). Also, I have used labour income as a dependent variable. The results for migration were qualitatively the same. For commuters, nonparametric estimates were slightly higher (0.46) and LATE for compliers was marginally significant and equal to 0.4. These results seem to suggest that commuting particularly pays-off with respect to the labour income, which is, in fact, true by definition of commuters. Finally, I have reestimated the models for two periods, 1990-1995 (convergence) and 1996-2001 (no convergence), and the results are available upon request.

[16] The growth variable is constructed as follows. First, for migrants, I average over the available years before and after an individual move, and for commuters - over the years before the first commuting and after it, and construct $income_i^b$ and $income_i^a$, respectively. I then identify the so-called "average" year weighted by the number of individuals who move before and after it. Then, for stayers I average the incomes before and after that year. Finally, I construct $income\ groowth_i = \ln(income_i^a) - \ln(income_i^b)$.

proximity to the West German border before unification - in commuting.

The main findings are as follows. First, no evidence of positive selection on unobservables for migrants and positive self-selection for commuters was found. Second, no significant returns to migration in terms of long-run income seem to exist. One should bear in mind, however, that the findings for migration have to be interpreted with caution. The returns for commuters are high and equal approximately to 40 percent, however, they are also insignificant for compliers. A higher overall gain for commuters is in line with expectations, taking into account the higher costs of migration and lower unemployment rate for commuters than for migrants. This may also suggest that commuting might indeed be a substitute for migration. Third, the results are robust to different changes in specification and in the sample.

Overall, migrating West does not appear to be a significantly rewarded option for eastern Germans in the long run. This fact, although subject to the assumptions and definitions used in this study, could constitute an important part of the explanation of the East-West migration in Germany.

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Table 1: Descriptive statistics

	Migration		Commuting	
	Migrants	Stayers	Commuters	Stayers
mean_income	39754	31125	43128	30009
	(26828)	(16937)	(22084)	(16739)
home_owner90	0.16	0.33		
border_West90			0.48	0.27
sex	0.42	0.52	0.65	0.49
age90	26.08	31.93	28.59	32.05
	(11.36)	(11.53)	(11.07)	(11.67)
spouse90	0.61	0.74	0.69	0.74
university90	0.16	0.09	0.13	0.09
vocational_education90	0.78	0.88	0.83	0.88
government_sector90	0.44	0.33	0.31	0.34
blue-collar90	0.26	0.35	0.40	0.33
telephone90	0.23	0.23	0.28	0.22
income 1990	26358	24164	29378	23276
	(21708)	(12320)	(18130)	(11354)
state_unempl_rate92	10.51	10.49	10.70	10.45
	(1.02)	(0.93)	(1.02)	(0.91)

Note: standard deviations in parentheses. Incomes are annual, inflated by regional CPIs to 2001 and expressed in DM. Minimum sample sizes are 3043 observations for migration, and 2953 observations for commuting. Mean_income is the average of annual labour income (sum of wages, second job and self-employment income) and annual social security benefits (such as unemployment benefits, maternity benefits etc). Owner 1990 is a dummy which equals one if a person owned a house before unification; border_West 1990 - a dummy which equals one if a person lived in the county that had a common border with West Germany before unification.

Table 2: Means of the variables by instruments

	Migration		Commuting	
	owner90=1	owner90=0	border_West90=1	border_West90=0
sex	0.53	0.50	0.52	0.51
age90	32.29*	31.21*	31.11	31.82
spouse90	0.76	0.73	0.75	0.73
years_schooling90	11.86*	12.26*	12.27	12.06
university90	0.05*	0.11*	0.09	0.09
vocational_education90	0.89	0.87	0.88	0.87
government_sector90	0.28*	0.37*	0.36	0.33
blue-collar90	0.31*	0.36*	0.33	0.35
telephone90	0.24	0.23	0.31*	0.20*
individual_income90	22758*	24973*	24576	23849

Notes: * difference in means significant at 5%. See footnote of Table 1.

Table A3: Second stage estimates

	Heckman's model				Nonparametric model			
	Migration		Commuting		Migration		Commuting	
	Migrants	Stayers	Commuters	Stayers	Migrants	Stayers	Commuters	Stayers
constant	6.02	6.61	8.70	6.45	7.95	6.63	8.14	6.45
	(1.286)	(0.231)	(0.810)	(0.252)	(1.399)	(0.266)	(0.592)	(0.252)
sex	0.74	0.38	0.44	0.38	0.72	0.37	0.46	0.37
	(0.125)	(0.022)	(0.064)	(0.027)	(0.125)	(0.024)	(0.060)	(0.022)
age	0.11	0.14	0.06	0.15	0.10	0.14	0.06	0.15
	(0.049)	(0.009)	(0.025)	(0.010)	(0.054)	(0.010)	(0.025)	(0.010)
age^2	-0.001	-0.001	-0.0006	-0.001	-0.001	-0.001	-0.001	-0.002
	(0.0006)	(0.0001)	(0.0003)	(0.0001)	(0.0006)	(0.0001)	(0.0003)	(0.0001)
spouse90	-0.35	-0.08	-0.06	-0.08	-0.35	-0.07	-0.07	-0.07
	(0.157)	(0.028)	(0.068)	(0.029)	(0.151)	(0.029)	(0.072)	(0.028)
state_unempl_rate92	0.07	0.01	0.04	0.01	0.07	0.01	0.04	0.01
	(0.055)	(0.010)	(0.030)	(0.013)	(0.056)	(0.011)	(0.026)	(0.012)
university90	0.57	0.49	0.47	0.49	0.53	0.47	0.48	0.46
	(0.221)	(0.046)	(0.098)	(0.048)	(0.240)	(0.045)	(0.082)	(0.044)
vocational_education90	-0.13	0.13	0.06	0.15	-0.21	0.13	0.07	0.13
	(0.197)	(0.038)	(0.091)	(0.041)	(0.192)	(0.048)	(0.079)	(0.043)
government_sector90	0.12	0.19	-0.01	0.21	0.12	0.18	0.003	0.22
	(0.141)	(0.024)	(0.061)	(0.025)	(0.142)	(0.023)	(0.061)	(0.023)
blue-collar90	-0.07	-0.10	0.01	-0.10	-0.06	-0.09	0.002	-0.09
	(0.158)	(0.023)	(0.064)	(0.025)	(0.124)	(0.024)	(0.058)	(0.023)
λ	0.67	0.25	-0.02	0.08				
	(0.413)	(0.259)	(0.134)	(0.130)				
pscore					-5.86	-0.87	3.88	
					(3.668)	(3.018)	(1.997)	
pscore^2						52.05	-9.54	
						(56.98)	(4.814)	
pscore^3						-378.62		
						(305.96)		
No. observations	178	2865	430	2523	177	2663	428	2431
CM test 3rd moment	-0.00004		-0.0040					
	(0.0008)		(0.0020)					
CM test 4th moment	0.0005		0.0115					
	(0.0039)		(0.0057)					

Note: standard errors, corrected for heteroskedasticity and for the first step generated regressors for Heckman's model and calculated as in Das et al (2003) for the nonparametrics model, are in parentheses. Depended variable is log of the total annual average income. λ is the inverse Mills ratio. Covariates also include dummies for missing 1990 information. CM test refers to the conditional moment test for normality of Newey (1985), Pagan and Vella (1989). In the reported nonparametric model the intercept is estimated according to Andrews and Schafgans (1998). Pscore is the estimated in the first stage propensity to move West.

Table 4: Intentions to treat effects, IV (LATE) and OLS estimates

	Intentions to treat: Move	Income	IV	OLS
	(1)	(2)	(3)	(4)
		A: Migration		
owner90	-0.039	0.011		
	(0.008)	(0.020)		
migrate			-0.273	0.305
			(0.538)	(0.055)
F-test on instrument in 1st stage		30.23		
		B: Commuting		
border_West90	0.111	0.022		
	(0.015)	(0.022)		
commute			0.199	0.396
			(0.194)	(0.028)
F-test on instrument in 1st stage		62.52		

Note: robust standard errors are in parentheses. Panel A shows the estimates for migration, panel B - for commuting. Dependent variable in column 1 is migration or commuting dummy respectively, dependent variable in columns 2, 3, 4 is the log of average total annual income. Covariates include gender, age and its square, spouse indicator in 1990, educational and occupational dummies in 1990, state's unemployment rate in 1992 and dummies for missing 1990 information.

Table 5: Treatment effects for movers

Migration			Commuting		
Parametric	Nonparametric	LATE	Parametric	Nonparametric	LATE
A: Baseline model					
-0.19	0.40	-0.27	0.27	0.42	0.20
(0.531)	(0.233)	(0.538)	(0.230)	(0.029)	(0.194)
B: Robustness checks					
including unemployment in 1990					
-0.13	0.47	-0.25	0.31	0.42	0.23
(0.525)	(0.269)	(0.535)	(0.227)	(0.029)	(0.191)
including household income in 1990					
-0.04	0.32	-0.03	0.27	0.40	0.23
(0.521)	(2.461)	(0.524)	(0.228)	(0.029)	(0.192)
including telephone in 1990					
-0.16	-0.14	-0.24	0.17	0.39	0.10
(0.529)	(1.167)	(0.535)	(0.236)	(0.072)	(0.202)
including political interests in 1990					
-0.39	0.41	-0.38	0.27	0.41	0.21
(0.534)	(0.224)	(0.541)	(0.228)	(0.029)	(0.193)
including hours worked per week in 1990					
-0.16	0.37	0.09	0.31	0.40	0.32
(0.500)	(0.250)	(0.493)	(0.239)	(0.074)	(0.200)
excluding self-employed					
-0.26	0.07	-0.32	0.31	0.45	0.22
(0.598)	(0.104)	(0.608)	(0.235)	(0.030)	(0.197)
retaining return and multiple migrants					
-0.22	0.07	-0.31	0.30	0.40	0.25
(0.482)	(0.090)	(0.497)	(0.229)	(0.027)	(0.192)
excluding "movers" from the control groups					
0.14	-0.52	0.03	0.24	0.43	0.19
(0.523)	(0.612)	(0.524)	(0.227)	(0.065)	(0.192)
including years for which the incomes are taken					
0.72	1.22	-0.54	0.44	0.32	0.22
(0.434)	(0.847)	(0.553)	(0.184)	(0.027)	(0.187)
C: Income growth as a dependent variable					
0.22	0.04	-0.11	0.13	0.29	0.26
(0.425)	(0.082)	(0.455)	(0.189)	(0.094)	(0.151)

Note: standard errors are in parentheses. Standard errors of the effects for sample selection models are calculated as for the Oaxaca decomposition. Treatment effects are calculated as shown in Section 4. Dependent variable in the regressions is average annual total income in Panels A and B, and is income growth in Panel C . In the reported nonparametric model the intercept is estimated according to Andrews and Schafgans (1998).

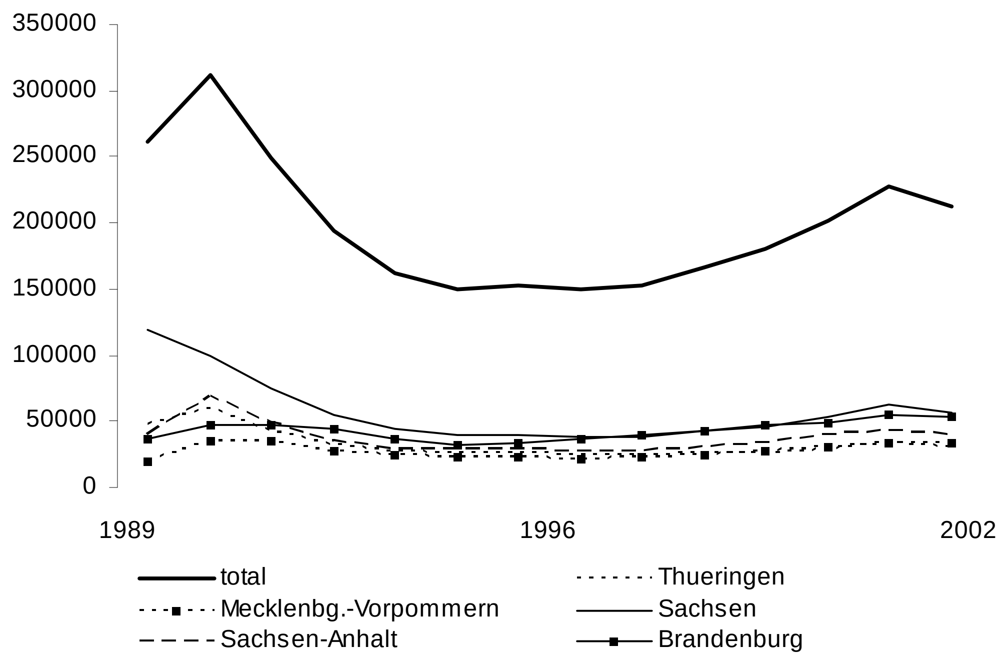

Figure 1: Emigration from East German länder to West Germany after the fall of the Berlin Wall. Source: numbers are from Heiland (2004). Note: East Berlin is omitted due to data unavailability.

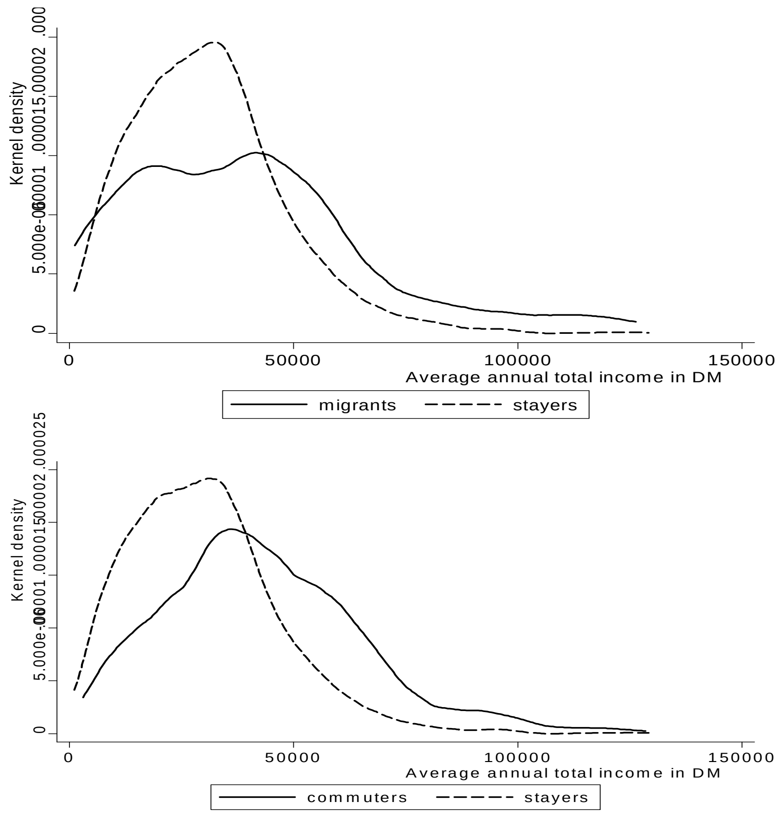

Figure 2: Kernel densities of the average annual total income for movers and stayers in Germany after unification. Source: GSOEP. Notes: see Section 2 for definitions.