Workplace Connections and Labor Migration*

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Abstract

We examine how former coworkers influence migration decisions following major labor market shocks, using the quasi-experimental setting of German reunification. Displaced East German workers are more likely to move to West Germany if they have former coworkers from the German Democratic Republic already there. Migration is strategic: workers move when their labor market prospects align with those of their contacts already in the West, and those contacts have positive labor market experiences. An extended Roy model rationalizes these findings, suggesting that migration is driven by relevant, job-specific information rather than social support from contacts.

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1 Introduction

Regionally concentrated job losses represent one of the most pressing challenges currently facing industrialized countries worldwide. The causes are manifold; among the most widely discussed by economists, policymakers, and the public is rising import competition from low-wage countries. Irrespective of the specific cause, however, a striking and well-documented pattern emerges: the effects of such shocks on local labor markets are more severe than many economists initially anticipated.[1] A key reason for this is that individuals affected by adverse labor market shocks exhibit surprisingly limited geographic mobility.[2]

This study contributes to a deeper understanding of the underlying reasons for individuals' mobility decisions within a single country, that is, in settings with no legal or political restrictions on mobility. We situate our analysis within the context of German reunification, thereby examining a period in German history during which the states of the former German Democratic Republic (GDR) – which we refer to as "East Germany" in the post-unification era – experienced a period of profound job loss.[3] Some displaced workers in East Germany – owing to pre-existing coworker networks – have peers in the more prosperous regions in West Germany, while others do not. Using this design, we provide causal evidence of the impact of former coworkers on mobility decisions, highlighting the potential role of various mechanisms, including the coworker network's provision of social support, as well as the provision of information about potential work locations and workplace conditions.

Our main contribution to the literature is the examination of the role that social connections play in workers' relocation decisions within the context of large-scale structural changes in local labor markets. If a region is undergoing structural transformation, and if finding employment involves considering relocation, social networks may be particularly important. This hypothesis is in line with prior studies showing that social connections are important drivers of international migration decisions (e.g., Munshi, 2003; McKenzie and Rapoport, 2010, for Mexicans in the U.S. labor market). More generally, underinvestment in migration is a widespread phenomenon, and social networks play a crucial role in many contexts, as shown, for example, by Blumenstock et al. (2025) regarding internal migration in a developing country setting.

Social networks can take on many forms. This study focuses on coworker networks, while also discussing the potential role of neighborhood networks. Studies that investigate coworker networks in the labor markets of industrialized countries often highlight the role they play in job finding; however, these studies tend to overlook relocation decisions and assume the economy to be structurally stable. For example, Cingano and Rosolia (2012) focus on the employee side, showing that displaced workers whose networks of former coworkers display higher employment rates experience shorter unemployment duration. In line with this finding, Glitz (2017) and Saygin et al. (2021) find that being embedded in a network of former coworkers with higher employment rates among its members has a positive effect on the employment probability of displaced workers. Caldwell and Harmon (2019) use networks comprised of former coworkers to assess which new positions at other firms (the potential outside options) an individual

[1] The seminal paper on U.S. local labor market effects of import competition from China is Autor et al. (2013). A comprehensive coverage is available in the forthcoming Handbook of Labor Economics (Autor et al., 2025). The latter, as well as Dorn and Levell (2024), also comprehensively reviews the literature on the impact of trade on local labor markets for other countries, including Germany, France, UK, and Norway. For the effects of automation on local labor markets, see Autor and Dorn (2013) and Acemoglu and Restrepo (2020), among others; for a study on the local effects of the energy transition, see Hanson (2023).

[2] See Autor et al. (2025) and Greenland et al. (2019) for the trade context. For earlier studies and other types of shocks, see, for example, Topel (1986), Blanchard et al. (1992), Bound and Holzer (2000), Glaeser and Gyourko (2005), and Notowidigdo (2020). Persistent underinvestment in mobility is a widespread phenomenon observed in many contexts, see, e.g., the seminal paper by Bryan et al. (2014) on Bangladesh.

[3] While each shock has specific features, they often also have similarities. As we discuss in detail below, the collapse of the GDR had a profound and irreversible impact on the entire territory of the former GDR. Similar to the China shock, the direct effects of the shock were largest in the manufacturing sector.

is likely to have heard about. They find that changes in the information about outside opportunities lead to job-to-job mobility and wage growth. Employers also gain information by leveraging their employees' networks to hire new workers (i.e., they rely on referrals when hiring, as shown, for example, by Dustmann et al., 2016, and Hensvik and Skans, 2016). Taken together, these studies support the notion that coworker networks play a crucial role in providing information relevant to labor market outcomes, as postulated in the seminal theoretical studies by Granovetter (1973), Montgomery (1991), and Calvó-Armengol and Jackson (2004). However, we have a very limited understanding of their role in times of large-scale structural changes.

Our empirical analysis, which benefits from the quasi-experimental setting of our study, enables us to address some of the empirical challenges commonly encountered in this area of the literature. One key difficulty is the fact that social ties are rarely formed randomly. In workplace settings, for example, coworkers tend to be selected through sorting processes by which individuals with similar characteristics or abilities cluster together. This homophily makes it difficult to isolate the causal impact of peers from the influence of shared, often unobserved traits (Manski, 1993). We overcome this challenge by leveraging the historical context of the GDR, a country in which occupational and workplace assignments were governed by a centrally planned allocation system. Under this regime, workers were assigned jobs based on political and administrative considerations rather than individual preferences or market-based sorting. While this feature of the GDR labor market has been extensively documented in qualitative research and is discussed in more detail below, we are the first to empirically test it.

In addition, even if GDR workers were initially randomly assigned by the system, for the sake of quality in our identification strategy, it is also important to consider whether these workers became more equal as a result of working together. Based on the empirical tests we provide, we argue that, conditional on some observed characteristics, coworker relationships formed in the GDR prior to reunification can be regarded as quasi-random with respect to subsequent labor market outcomes in a reunified Germany. Finally, we also show that the individual and network characteristics of displaced East German workers with and without West contacts are balanced.

To our knowledge, no prior study exploits an as-good-as-random assignment of workplace peers in a setting that encompasses the entire economy. Important studies, such as Sacerdote (2001) and Zimmerman (2003), use random roommate assignments to identify peer effects among college students. Guryan et al. (2009) study the conditional random pairing of professional golfers. Studies of coworker effects in economy-wide analyses, such as Cornelissen et al. (2017), typically rely on fixed-effects models to account for non-random sorting. Other quasi-random designs, such as the random assignment of shifts within a single company (Mas and Moretti, 2009) or of junior lawyers to initial projects (Linos et al., 2024), focus on specific jobs rather than economy-wide assignment processes.

Another common challenge to identifying coworker effects is the presence of correlated shocks. In other words, former colleagues may be jointly affected by unobserved factors. Even if coworker assignment in the GDR was as good as random, certain types of East German workers, based on their skills or work experience, may have been particularly attractive to West German firms after reunification. This could generate spurious correlations in the relocation decisions of former coworkers. To address this concern, we combine the quasi-random assignment of coworkers in the GDR with an establishment-closure design; in using the latter, we follow Eliason et al. (2023) and others.[4] This element of the research design allows us to compare relocation decisions among displaced East German workers who face similar labor market conditions at the time of displacement, but who differ with regard to the composition of their former coworkers from their time as workers in the GDR.

[4] Unlike the mass-layoff literature following Jacobson et al. (1993) (e.g., Schmieder et al., 2023), which compares displaced to non-displaced workers, the use of establishment closures in this design serves a different purpose: it creates a setting in which all workers from a closing establishment must simultaneously search for new employment under comparable conditions, since continued employment with the current employer is no longer an option.

One important difference in our setting is that establishment closure is not only an element for identification, but rather a defining and central element of the profound, regionally concentrated job loss that is the setting of our experiment. The years following the fall of the Berlin Wall were marked by an extensive privatization program in East Germany, resulting in numerous company closures.[5]

In the first part of the paper, we combine the GDR's centralized job allocation procedure with the establishment-closure design. This allows us to identify the causal effect of having a former GDR coworker who migrated to West Germany after reunification on the relocation decision of East Germans after job displacement. Our analysis is based on a newly constructed data set that links administrative data from the GDR with register data from post-reunification Germany at the individual level. The combined data allows us to track the labor market biographies of all East Germans who worked in the GDR in 1989 over the years after reunification. We define GDR coworker networks by considering all workers who worked together in the same establishment and occupation in the GDR in 1989. We then examine establishment closures in East Germany after reunification and show that East German workers who – at the time of their job displacement – had a former GDR coworker employed in West Germany are significantly more likely to relocate to the West than similar East German workers who were displaced in the same establishment closure, had the same occupational profile, but did not have a former GDR coworker in West Germany.[6] This average coworker network effect increases the probability of migrating to the West by about six percent. We find that the effect can almost exclusively be attributed to former GDR coworkers who were employed in the same fine-grained occupation in the GDR. In contrast, neither GDR coworkers from the same establishment but different occupation nor neighbors significantly impact the migration probability. We also find that former GDR workers in West Germany direct their network contacts to the particular West German county and the *same* establishment where they are employed. In contrast, former GDR coworkers do not increase the probability of moving to a location without a contact.

Determining that former coworkers from the same fine-grained occupation provide a connection to possible employment opportunities elsewhere is only a first step, however. Ultimately, it is important to understand *why* these workplace connections lead to increased relocation. Therefore, in the second part of the paper, we examine the mechanisms underlying the average network effect. We first set up a simple model that separates two channels: an information channel, where former GDR contacts in the West provide information about vacancies and specific firm characteristics, such as amenities or career opportunities; and a support channel where former GDR contacts provide social support at the new location. The model extends the basic Roy (1951) model of migration (see Borjas, 1987) with uncertainty about the payoff at the destination and network connections. The key prediction of our model is that social support from contacts at the destination unambiguously increases the likelihood of relocation. At the same time, the effect of information depends on the quality of the destination contact's work environment and the relevance of the information to the potential migrant.

We then estimate the causal effect of the contact's work environment in West Germany on the relocation decisions of East Germans and examine how it is mediated by the relevance of the information – approximated by the similarity in the earnings potential of the two contacts. We identify these effects by focusing on East Germans with multiple former coworkers in West Germany, exploiting within-network heterogeneity in the labor market success among these West contacts. We find that East Germans relocate towards the location and employer of those former coworkers who work in a more favorable work environment, holding constant the tie strength to the different coworkers, as well as their demographics,

[5] See Section 2.3. Between 1992 and 2005, 292,368 establishments closed down in East Germany (compared to, for example, slightly less than 30,000 establishment closures in (similarly-sized) Sweden between 1990 and 2006 as documented in Eliason et al. (2023).

[6] In line with our argument of quasi-random worker allocation in the GDR, we show in Section 5.1 that East Germans with West contacts and those without are similar, conditional on a set of observable characteristics.

abilities, and labor market experience in the West. We further find that the effect of the contacts' work environment is particularly relevant for pairs of former coworkers from the GDR who are similar in their earnings potential. This evidence is consistent with the information channel where former GDR coworkers provide specific information about workplace conditions in West Germany to their East German contacts, thereby shifting the contacts' expectations about the individual returns to relocating. In contrast, we provide evidence that referrals are unlikely to be the driver of the results.

The specific setting we leverage in the analysis is well-suited, both for the methodological aspects highlighted above and the specific economic context: improvements in employment outcomes for former GDR workers typically meant seeking new jobs in West Germany. Yet, despite large and persistent gaps between East and West Germany, most East Germans stayed in the East rather than "moving to opportunity" in the West. Among the displaced East Germans in our data, only 3.6% switch to a West German employer in the years after displacement, while those who switch experience about 12% higher real wages (taking into account local differences in purchasing power) and higher job security. A possible explanation for the lack of mobility is that East Germans had difficulties in assessing their labor market prospects in the West. Based on a survey with personal interviews of East German workers conducted shortly after German reunification, Akerlof et al. (1991) find that most East Germans are reluctant to consider migration to West Germany because they overestimate the difficulties in finding employment in the West. Balleer et al. (2023) show that East Germans are more pessimistic regarding their expected labor market outcomes than West Germans. Our findings indicate that former coworkers can mitigate these information frictions by providing specific information about the work environment in West Germany.

Teasing out the mechanisms places high demands on the structure and quality of the data. Our newly constructed data combines administrative data from the GDR with register data from post-reunification Germany. Consequently, we can not only identify workplace connections in the GDR, such as who worked together with whom in the same occupation and establishment in 1989, but also examine whether these former GDR coworkers' contacts influence their relocation decisions after reunification. Key to analyzing the mechanisms underlying the coworker network effect is the combination of the quasi-random network formation with our ability to observe the specific labor market environment that former GDR workers face at their new destination in West Germany.

As we rely not on survey data but on the near-universe of GDR workers, our network measure is not subject to the typical measurement error issue of missing links (see, for example, the discussion in Munshi, 2003). At the same time, however, our networks are imperfectly measured because we assume links between all former coworkers in the same occupation, without observing actual interactions or information flows. This drawback is common to most studies on labor market networks, but substantially limited in our study for two reasons. First, our focus on former coworkers from the same fine-grained occupation makes actual interactions more likely. Second, the workplace played *the* central role in social life in the GDR that extended far beyond work.[7]

In addition to the studies mentioned above, our work relates to the literature on how professional and other networks influence migration decisions. Becker et al. (2024) examine the impact of professional networks on the emigration of Jewish academics dismissed from their jobs by Nazi Germany. They show that academics with more ties to early émigrés were more likely to emigrate themselves. Munshi (2003) focuses on (non-workplace related) origin-communities in Mexico and demonstrates that the size of each community in the United States affects the labor market outcomes of its members. In contrast to these settings, we provide evidence for the role of coworker networks in a labor market that encompasses workers across all educational backgrounds and occupations. Other related studies use network definitions based

[7] See details in Section 2.

on the municipality of origin (Beine et al., 2011) or birthplace (Stuart and Taylor, 2021; Buggle et al., 2023). Munshi (2020) provides an excellent overview.

Our study also adds to the literature that examines how the particular experience of peers and network members affects economic decisions in other contexts. Nanda and Sørensen (2010) show that workers are more likely to start businesses if their former coworkers have successfully launched a start-up. Dahl et al. (2014) find that fathers are more likely to take up parental leave if their coworkers were exogenously induced to take up leave, most likely because coworkers' experiences relieve uncertainty about how employers would react. Outside the work context, experiences of social contacts have been shown to affect housing decision (Bailey et al., 2018) and product adoption (Bailey et al., 2022) among others. In addition to examining the effect of contact experience on migration, our setting allows us to pin down the specific mechanism.

We further contribute to a broader and rapidly growing literature that examines how expectations about employment prospects and labor market outcomes affect individual behavior. Jäger et al. (2024) use survey data from Germany to demonstrate that many workers, particularly those in the lower part of the earnings distribution, underestimate the value of their outside options in the labor market. Based on survey data from the US, Conlon et al. (2018) show large information frictions in job searches. They demonstrate how workers update their expectations based on their own experiences. As in Caldwell and Harmon (2019), we show that the experiences of former coworkers are also a relevant channel to increase the information set and update expectations. As discussed above, Akerlof et al. (1991) and Balleer et al. (2023) suggest that biased expectations appear particularly relevant in East Germany and the context of migration. However, such biased information about the potential destination is not specific to the German context. Porcher et al. (2024) develop a novel estimation procedure and document that information frictions play a quantitatively important role in reducing migration flows and worker welfare in Brazil. McKenzie et al. (2013) show that migrants from Tonga significantly underestimate potential labor earnings in New Zealand. They argue that these information frictions are (at least partly) due to inaccurate information flows from extended family and older cohorts. Our finding that peer effects operate effectively only among similar workplace peers (and not among neighbors) emphasizes the importance of considering the relevant source of information.

Finally, we contribute to the literature that examines the transformation of the former GDR after reunification (e.g. Hunt, 2006; Fuchs-Schündeln and Schündeln, 2009; Prantl and Spitz-Oener, 2020; Findeisen et al., 2021). A recent paper by Heise and Porzio (2022) is particularly interesting as it quantifies the spatial frictions in the German labor market.

The paper is organized as follows: Section 2 provides historical background. Section 3 introduces our novel data and explains how we construct the analysis samples and the most important variables. Section 4 introduces our identification strategy and empirical specifications. Section 5 provides results on the average network effect. Section 6 examines the mechanisms of the network effects. Section 7 concludes. Additional details are provided in the Appendix.

2 Background

2.1 The Two Germanies at a Glance, 1949–1990

In 1949, two fundamentally different states were founded on the territory of the Allied occupation zones in Germany. The Federal Republic of Germany—commonly referred to as West Germany—was founded as a democratic state in the French, British, and American occupation zones. The German Democratic Republic (GDR)—referred to as East Germany—was established in the Soviet occupation zone as a *socialist* state. With the construction of the Berlin Wall in 1961 at the latest, the connection between

East and West Germany was almost completely severed. Freedom of movement between both German countries did not exist anymore.[8] The two opposing political systems co-existed until November 9th, 1989, when—suddenly and unexpectedly—the fall of the Berlin Wall marked the end of the Cold War era. On October 3rd, 1990, East and West Germany were officially reunited. West German institutions were extended to East Germany.

2.2 Work in the German Democratic Republic

The breakdown of the GDR was first seen as a "zero-hour" event by social scientists, politicians, and the public alike. The GDR structures, established and maintained for four decades, were regarded as "history" with no substantial implications for the transformation process. Only later, it was acknowledged that, while radical, the events of 1989 did not eradicate four decades of individual biographies (Lutz et al., 1996, p. 3ff.).

An essential part of GDR biographies is the social connections formed during GDR times, in particular, the connections formed at the workplace.[9] In the GDR, the workplace served as the focal point for all kinds of activities and social interactions. It reached beyond work by offering various services and amenities, including childcare facilities, cultural activities, sports facilities, and organized vacations. Many activities were provided to foster a sense of community among workers. People spent a lot of time at the workplace, regardless of whether there was a high or low demand for their labor services. This was due to the official absence of the concept of unemployment in the system. The population of the GDR had the constitutional "right and duty to work".[10]

The GDR workplace connections are also interesting because the labor market was organized fundamentally differently than the West German or any other Western-style labor market. In fact, in the planned economy of the GDR, there was no labor *market* (Hoene, 1991). Due to central planning, workplaces were typically organized under state control. The vast majority of businesses and industries were state-owned, and the government had significant influence and control over production and employment. Allocating people to workplaces was a centralized process heavily influenced by the state and its economic planning objectives. Individuals had limited control over their job assignments, which were determined by the needs of the state-controlled economy according to the economic plan and political alignment, and less by factors such as individuals' education and skills (e.g., Gruenert, 1996). Krueger and Pischke (1992) argue that "the Communist system operated like a large internal labor market, with rules and party membership playing an important role in the allocation of jobs and wages" (p. 408).

Ideology and the requirements of the centrally planned economy also shaped education in the GDR more generally. Compulsory education ensured a basic education for all children from age six until around 16. While individuals had some say in their educational paths beyond basic schooling, the state exerted substantial influence by guiding students toward specific careers and fields of study that aligned with its objectives and principles. The state restricted individuals' occupational choices to increase social equality, promote working-class children, or push modernization (e.g., Baker et al., 2007; Fuchs-Schündeln and Masella, 2016; Prantl and Spitz-Oener, 2020). Due to these factors, the allocation of individuals to occupations and workplaces in the GDR did not rely on their abilities, especially not those that would later become relevant in a market-driven economy, such as the West German economy. At

[8] Before the Wall's erection, 3.5 million East Germans circumvented Eastern Bloc emigration restrictions and defected from the GDR, many by crossing over the border from East Berlin into West Berlin (e.g., Black et al., 2022). Between 1961 and 1989, over 100,000 people attempted to escape from the GDR. Still, only about 5,000 people succeeded in escaping over the Wall, with an estimated death toll of those murdered by East German authorities ranging from 136 to more than 200 in and around Berlin.

[9] Lutz et al. (1996) provide a detailed discussion of the GDR labor market and the influence of the GDR state in the allocation of workers from which we draw in this section.

[10] See Article 24 of the GDR constitution at http://www.documentarchiv.de/ddr/verfddr.html. In addition, companies had a strong incentive to hide any surplus staffing to avoid being perceived as inefficient by the authorities in the GDR.

the time, the latter was on the other side of a highly fortified, impenetrable wall.

2.3 East-West Migration and Job Displacement

One of the main concerns after the fall of the Wall in 1989 was a mass exodus of East Germans to West Germany. While net migration to West Germany exceeded 350,000 in 1989 and 1990, it slowed down rapidly—a surprising development given the stark differences in economic opportunities and well-being between the two parts of the newly unified country.[11]

The low levels of East-West migration are also surprising because modernizing the GDR economy after 1989 entailed massive structural changes, including an extensive privatization of the formerly state-owned entities.[12] In the process, 292,368 establishments closed down in the years from 1992 to 2005, representing an average establishment closure rate of 7.7% per county.[13] Figure 4 illustrates the decline in establishments active in the manufacturing sector between 1989 and 1992.

As a result of these large structural changes, many East Germans were forced to switch jobs. Yet, the vast majority did not move to the West. Among all East German workers in our displaced-worker sample, 96.4% remained in East Germany in the five years after displacement, while only 3.6% took a job in West Germany.[14]

Note that these patterns are in a setting with no formal or informal impediments to "moving to opportunity." As German citizens, former residents of the GDR have unrestricted access to the entire German labor market. There is no language barrier, and the reunification treaty guarantees the recognition of educational qualifications. East and West Germans have comparable levels of formal education. Additionally, the Federal Employment Agency facilitates access to information about job opportunities nationwide, helping to match job seekers with suitable vacancies. The German tax system provides substantial subsidies to work-related relocation and mobility.

To provide evidence of the benefits of moving from East to West based on our data, Figure 1 shows the evolution of wages and employment of displaced workers, before and after job displacement. We focus on workers who started a new job within the first five years after displacement and separate between three groups: East stayers whose first new job is located in the East German state where they were displaced (red dashed line), East movers whose first new job is located in a different East German state (red solid line), and West movers whose first job after displacement was in West Germany (blue line). The first graph on the left-hand side shows employment probabilities for the three groups 5 years before the displacement event (in t=0), and 5 years thereafter. The graph shows that job displacement is associated with an initial loss of employment that is quite similar for all groups of workers. In the subsequent years, however, workers who move to West Germany are more likely to be employed than East German workers who stay in the East. Note that the graph, as well as the other graphs in this figure, do not display any differences in pre-trends across the three groups.

The other three graphs plot coefficients from event-study analyses around displacement, controlling for person fixed effects, calendar-year fixed effects, relative event-time fixed effects, and a linear time

[11] Some policies implemented at the time worked against out-migration, partly unintentionally. As part of the monetary union enacted on July 1st, 1990, East German wage contracts were converted to Deutsche Mark at a rate of one for one. Labor unions negotiated a series of wage increases in East Germany (Akerlof et al., 1991; Burda and Hunt, 2001). Rising wages between 1991 and 2000 reduced emigration, whereas the surge in unemployment did not increase it (Hunt, 2006). Overall, migration did not close the East-West gap. For instance, Schöb (2001) reports results from extensive surveys on well-being in Germany in 1993 and 1998. East Germans report lower life satisfaction, well-being, and living standards in almost all dimensions. Based on a sample of 50% of all German establishments, Heise and Porzio (2022) estimate a wage gap between East and West Germany of 20%, conditional on worker characteristics, establishment size, and industry.

[12] See Findeisen et al. (2021) for a detailed description of the structural changes. The privatization process was administered by the Treuhandanstalt and is analyzed in Mergele et al. (2025).

[13] As a comparison, Eliason et al. (2023) document slightly less than 30,000 establishment closures in similarly-sized Sweden between 1990 and 2006.

[14] Appendix A, Figure A.1, shows the evolution of migration between the East and the West. This appendix also presents evidence illustrating how the former East-West border continues to pose an intangible barrier to mobility.

trend common to both East and West movers. Compared to the reference group of East stayers, workers who relocate to a different East German location are somewhat more likely to be employed in the year following displacement; however, the effect is only substantial in the first year after displacement (graph in the upper right panel). In contrast, workers who move to West Germany are not more likely to be employed initially, but over time have significantly higher employment rates than those who stay in East Germany and those who move from East to West.

The left graph in the lower panel shows that displaced workers who find a new job in West Germany can increase their real wages (adjusted for differences in local rents and prices), whereas there is no premium for mobility within East Germany.[15] The last graph combines the employment and wage margins and shows that real earnings are substantially and significantly higher for West movers from the second year on, while there are no significant earnings effects for GDR workers who move to new jobs within the East. Our findings indicate that the displaced workers who migrate to West Germany experience substantial real earnings gains brought about by greater employment stability and higher wages. These effects appear to be driven by the gap between East and West Germany rather than reflecting a premium on job mobility.

2.4 Why are East Germans Reluctant to Move to the West?

The low levels of East-West migration are puzzling in light of the low institutional barriers and the high economic incentives to relocate. However, they align with early survey findings in Akerlof et al. (1991). Shortly after reunification, Akerlof et al. conducted personal interviews with East German workers. They concluded that "the great majority of people are reluctant to migrate and do not anticipate doing so" (Akerlof et al., p.46). They find that 65% of all respondents think that it is hard to find a job in West Germany while, in practice, "East Germans who have migrated and looked for work have found it in the West – more quickly than the West German unemployed" (Akerlof et al., p.51). Hence, one rationale for the limited migration response could be information frictions that led GDR citizens to overestimate the challenges they would face in securing employment in West Germany.[16] Evidence in Balleer et al. (2023) based on the German Socio-Economic Panel (GSOEP) shows that East Germans are generally more pessimistic regarding their expected labor market outcomes than West Germans.

How could more labor migration from East to West Germany be induced when institutional barriers are already low and economic incentives appear insufficient? We examine the responses of East Germans in the GSOEP in 1991 and find that social contacts, particularly former coworkers in the West, increase migration intentions. East Germans are about 13 percentage points more likely to state that they could imagine moving to the West if they have a former coworker in West Germany. The increase is, at least partly explained by a positive effect that former coworkers in the West have on East Germans' evaluation of the quality of life in West Germany.[17] We interpret this pattern as suggestive evidence that former coworkers can impact the migration probability by changing expectations about West Germany.

[15] We convert nominal wages from our administrative data using separate price indexes for East and West Germany. In addition, we account for level differences in local rents and prices using the local price index available for the year 2007 (BBSR 2009).

[16] Other explanations are, e.g., prohibitively high migration costs or particularly high preferences towards the home region. These factors are examined in Heise and Porzio (2022).

[17] In a mediation analysis, we find positive effects of former coworkers in West Germany on the willingness of moving to West Germany and the assessment of the quality of living conditions in West Germany. The effect on migration intentions is mediated by the effect on the assessment, accounting for approximately ten percent of the total effect. See Appendix A.2 for more details.

3 Data

Data Sources Our analysis builds on rich novel individual-level panel data that allow us to observe East Germans' workplace connections, migration choices, and labor market outcomes both in the GDR and in reunified Germany. The new data set links administrative records on the near-universe of GDR workers in 1989 with their labor market biographies after reunification. The primary source is an administrative data set from the former GDR, the *Datenspeicher Gesellschaftliches Arbeitsvermögen* (GAV), provided by the Federal Archives in Germany. The GAV data contain demographic information and labor market characteristics of around 7 million workers in the GDR in 1989.[18] The data was used by government agencies in the GDR as part of the central planning process, e.g., to assess the demand for workers in specific occupations. It includes 72 percent of the East German labor force in 1989, covering most employees in the country. Excluded, however, is the so-called *Sector X*, i.e., people working for the Ministry of the Interior, the Ministry of State Security, the Socialist Unity Party, the army, or customs authorities. Further excluded are school teachers, childcare workers, and self-employed individuals. The GAV data contain demographic characteristics such as age, gender, place of residence, the number of children under 14, the number of household members in need of care, marital status, nationality, and education (high school, vocational, or university degree). Further, the data include unique identifiers for each worker's establishment, firm, industry, and occupation, as well as information on the type of employment (full-time or part-time).

Based on names, exact dates of birth, and gender, the data were merged with the *Integrated Employment Biographies* (IEB), the German social security records administered by the Institute for Employment Research (IAB). The IEB contain full employment and earnings histories of all workers covered by the German social security system (excluding civil servants, self-employed workers, and regular students). Due to the complexity of the administrative integration process of the East German labor market into the German social security system, East Germany is only comprehensively captured in the IEB from 1992 onward. Consequently, the combined data provide highly accurate information on GDR workers and their workplace in 1989 and their full employment biographies between 1992 and 2005.[19]

To the best of our knowledge, we are the first to examine the labor market biographies of GDR workers after German reunification in combination with information from GDR times. The combined data offers a unique opportunity to observe the labor market decisions of GDR workers in the first years after the Berlin Wall fell, including information on migration to West Germany and job mobility. We refer to Appendix B and Liepmann and Müller (2018) for more detailed information on the data and the linkage process.

Sample Construction From the combined GAV-IEB data, we select GDR workers aged between 20 and 50 in 1989 and follow them until 2005.[20] Based on this full sample of working-age individuals in

[18] The GAV data was collected on an annual basis. However, limited computer storage capacities in the GDR resulted in the deletion of previous waves of the data when a new wave was collected. Therefore, we can only use the latest wave referring to 1989.

[19] GDR workers are difficult to identify in the social security records (the IEB) without the merged GAV data, as East Germany is only covered from 1992 onward in the IEB, and the data contains no information on the place of birth. In addition, the IEB contains no information on GDR workers from GDR times.

[20] Appendix Table B.1 shows summary statistics for this **full sample**. Focusing on 1989 in column 1, the GAV data show that the 4.3 million GDR workers in our full sample were, on average, 35 years old. 46 percent of the workers were women, 13 percent had no formal vocational training (or the education information is missing), and the majority (79 percent) had a vocational training degree, whereas nine percent had a university degree. These statistics closely correspond to comparable figures reported in other sources, such as Hoene (1991). Column 2 shows summary statistics for 1992, the first year the GDR workers were fully integrated into the official social security records (the IEB data). Out of the 4.3 million GDR workers observed in 1989, about 4 million were successfully merged with the social security records (i.e., 93 percent). The remaining workers either completely left the labor force, e.g., due to early retirement, or could not be matched, e.g., due to name change after marriage. The gender distribution remained unchanged, and the education distribution remained very similar.

East Germany, we construct two samples for our main analyses. First, we construct a **displaced-worker sample** that considers all workers displaced in the closure of an East German establishment between 1992 and 2005. Establishment closures are identified based on worker flows and separated from takeovers, spin-offs, or re-namings, as described by Hethey and Schmieder (2010). We restrict the sample to displaced workers who have been employed full-time in a regular job at the closing establishment for at least one year.[21] We focus on establishments from which at least two workers from different GDR backgrounds are displaced at the same time. Table 1, Panel A shows summary statistics for the displaced-worker sample. Our data includes 246,415 workers displaced from 46,914 establishments in East Germany.

Second, we construct an **East-West-pair sample** based on all former GDR workers who switch from an East German employer to a West German employer between $t \in \{1992, ..., 2005\}$ and $t+1$. We connect these East German labor migrants to their former GDR coworkers (explained in detail below) and select all East German labor migrants who have at least two former GDR coworkers employed in West Germany at time t. Each observation in the East-West pair sample represents a dyad of an East German labor migrant and a West German contact, i.e., a former coworker from the GDR employed in West Germany. Table 1, Panel B shows summary statistics for the East-West pair sample. We observe 32,245 East German labor migrants and 147,068 East-West dyads.

Network Definition We define social networks based on connections formed at the workplace during GDR times. Defining networks from administrative data where actual interactions are not observable faces the tradeoff between broad definitions (which may falsely connect individuals who do not interact) and narrow definitions (which may miss connections outside the specific group under consideration). In our baseline specification, we categorize all workers who worked within the same establishment and held positions in the same 4-digit occupation in 1989 as part of the same GDR coworker network. By considering the occupation in the network definition (as opposed to relying solely on the establishment, as typically done in the literature), our networks are more relevant for occupation-specific information. We assume that workers who shared the same occupation and establishment during GDR times can assess their respective skill sets very well. Additionally, those who have moved to the West after 1989 can assess the differences between how things were done in an occupation during GDR times and how they are done in the West German labor market. In Section 5, we demonstrate that consistent with this notion, our network effects are almost exclusively driven by workers from the same narrow occupation $\times$ establishment cell rather than by other workers from the same establishment. We will similarly show, that the effects are not driven by neighborhood networks (Schmutte, 2015).

Because we are using the near-universe of GDR workers in 1989, and not just a sample, our network measure is not subject to the typical measurement error in this area of the literature, e.g., Munshi (2003). In our main specification, we restrict all samples to networks with 100 or fewer contacts in the same occupation $\times$ establishment cell, leading to a loss of 8.8 percent of networks. The literature that defines networks based on establishments also uses this restriction because very large networks make it unlikely that network members know each other (e.g., Eliason et al., 2023, and Caldwell and Harmon, 2019). We present sensitivity analyses below that demonstrate this restriction does not impact our results.

In the displaced-worker sample, the average displaced worker has 28 former coworkers from the GDR who worked in the same establishment and occupation in 1989 (see Table 1, Panel A). In the East-West pair sample, the average East German labor migrant with multiple West German contacts has 4.6 former coworkers from the GDR employed in the West.

[21] As is common in the literature, we exclude establishments from Berlin because they cannot unambiguously be assigned to East or West Germany.

4 Empirical Strategies

The backbone of our identification strategy is the specific network formation process brought about by the GDR governance system. In Section 4.1, we provide evidence that the allocation of workers to networks in the GDR can be considered as good as random with respect to the unobservable skills valued in the West German labor market, conditional on observable characteristics. We then combine the quasi-randomness in the network formation with additional research design elements that constitute the two empirical strategies of our study. First, we use variation between workers who are displaced in the same establishment closure to identify the average network effects and the directional effects of networks (Section 4.2). Second, we combine the as-good-as-random network formation with variation within the network of East German workers who have multiple contacts at the destination to identify the detailed mechanisms (Section 4.3).

4.1 Quasi-Random Network Formation in the GDR?

An important challenge for any study of network effects is the endogeneity of network formation. If formed freely, networks are typically formed among individuals with similar characteristics. This homophily makes it difficult to disentangle the effect of network contacts from the effect of these common, partly unobserved, characteristics (Manski, 1993).[22]

The GDR context helps us to overcome the endogeneity of network formation. As discussed in Section 2, the allocation of workers in the GDR was part of the central planning process. Additionally, the fall of the Wall came as a surprise. In the GDR, who worked with whom—and thus, who was part of the same GDR coworker network—was not determined by the workers' considerations of future employment prospects in the market-driven economy of West Germany.

While the qualitative research is clear on the topic, empirical evidence is lacking. In this section, we rely on one of the advantages of the new data we have and provide the first evidence on the allocation of GDR workers to establishments and occupations. Specifically, we assess how random with respect to abilities that are important in the West German market economy the allocation during GDR times was.

Sorting in the GDR How much homophily with respect to unobserved abilities relevant to the West German labor market did the allocation process in the GDR generate? We use Abowd et al. (AKM, 1999) estimated worker fixed effects for the post-reunification period to investigate this question.[23] We interpret the estimated AKM worker fixed effects as measures of the (unobservable) transferable skills that are universally valued in a market-oriented economy and that GDR workers were likely to have possessed already during the GDR era, even though they were not relevant at that time.

To investigate sorting, we break down the total variance of the worker effects into two components, using the following decomposition:

$$Var[\hat{\alpha}_i] = \underbrace{Var[\bar{\hat{\alpha}}^g]}_{\text{between component}} + \underbrace{\sum_{g=1}^{G} \frac{N_g}{N} Var[\hat{\alpha}_i | i \in g]}_{\text{within component}}, \tag{1}$$

where $\hat{\alpha}_i$ represents the estimated AKM worker fixed effect for worker i calculated after reunification, g indexes G groups (in our case, networks defined by occupation $\times$ establishment), and $\bar{\hat{\alpha}}^g$ is the average

[22] As an example, in our context, workers with similar observed and unobserved abilities might sort into the same workplaces. As a consequence, observing two network contacts subsequently migrating to the same destination could be due to their similarity rather than the result of a network effect.

[23] We use the AKM estimates provided by Lochner et al. (2023) based on the universe of East and West German workers and, in our baseline, estimated in the 1998-2004 period. The appendix includes additional results using fixed effects estimated in different time periods.

worker effect in group g.

As indicated by Equation 1, the total variance of the worker fixed effects can be decomposed into a between-network and a within-network component. The relative shares of the two components indicate the degree to which workers with similar worker fixed effects are sorted into the same networks. The more stringent the sorting of workers, the smaller the contribution of the within component. In the extreme scenario of perfect sorting, all workers in the same network would have the same unobserved ability. In this case, the within component would account for zero percent of the total variance. Conversely, in the absence of any sorting, workers would be allocated to networks at random, resulting in a between-component of zero because, in expectation, all networks would exhibit the same worker effect.[24]

In order to assess the extent to which the sorting based on unobserved ability in the GDR can be considered random, we compare the pattern observed in our data to simulated allocations where we randomly assign each worker to a network (keeping the size of the networks constant) and examine the sorting patterns in the randomized allocation. We compute 100 random allocations.

Table 2 includes the results. Column 1 reports the total variance of estimated AKM worker effects, column 2-4 report the share of the variance within networks, where the differences across columns reflect different conditioning variables.

Column 2, row 1, indicates that in the case of random assignment, about 78% of the total variance would be within networks. The corresponding within-component of the observed GDR workers in 1989 is 60% (row 2). In the absence of any benchmark, it is difficult to interpret the 18 percentage points difference between these two figures. However, the lower figure for the observed allocation could indicate that even though the allocation of workers in the GDR was initially random, the fact that the workers performed similar tasks at the workplace reduces the variation in within-network worker fixed effects. In the next step, we therefore investigate how conditioning on observable characteristics affects the within component of the simulated data and the observed 1989 distribution of GDR workers.

We first condition on the workers' 2-digit industry, federal state, and 3-digit occupation in 1989. In column 3, the within share in the random assignment case declines to 74%. It further declines to 60% when we additionally condition on a range of observable demographic characteristics in column 4 (age, education, gender, network size, part-time status, caregiving, marital status, and children).[25] Importantly, conditioning on the observed variables moves the share in the within-network variation of the observed 1989 allocation closer to the simulation results. Conditional on these fine-grained cells, the allocation in the GDR (within-network share of 60%; column 4, row 2) cannot be distinguished from a random allocation of workers into networks. The observed allocation in the GDR corresponds to the 67th percentile in the distribution of our 100 simulated allocations, indicating a p-value of 0.67 for the test of the hypothesis that the GDR allocation is conditionally random. In contrast, the allocation of the same workers in 1995 differs substantially from a random allocation (within-network share of 4%). Overall, the exercise suggests that, conditional on the set of controls, we can consider the allocation to networks in the GDR as good as random.[26]

To provide additional evidence on the relevance of the AKM worker effects in explaining sorting patterns for GDR workers, we perform an analysis that allows us to compare the results for GDR workers with those in the West German labor market. Card et al. (2013) analyze the variance of worker effects of West German men for the period 1996 to 2002. The fourth row in Table 2 shows their numbers

[24] In practice, the boundary cases of 100% variance within or between groups will never be attained due to the discrete size of the groups.

[25] As the cells get smaller when conditioning on additional variables, it is natural that the within-component declines. The set of control variables corresponds to the control variables that we use in our empirical analysis of coworker effects in the analyses below. The variance remaining within these fine-grained cells amounts to 0.02, roughly 20% of the overall variance, and hence still leaves scope for sorting into networks.

[26] In Appendix C, we provide additional details on our estimation procedure and simulations, and show robustness to changes in the estimation period.

for sorting across occupations. The overall variance of worker effects for West German men is 0.107 (column 1), with a within-occupation share of 64% (column 5).

The third row of Table 2 shows that for the East German workers in our sample in 1995 – i.e., about five years after reunification – the total variance of worker effects (0.099) and the share of the variance within occupations (65%) are strikingly similar to the values for West Germany. The AKM worker effects are estimated in the 1998-2004 period, the closest available period to the one used by Card et al. (2013). Within a few years after reunification, GDR workers sorted across occupations according to their worker effects to a similar extent as West German men, suggesting that AKM worker effects provide relevant measures for GDR workers' abilities in a Western-style labor market.

Finally, the second row of Table 2, column 5, reports values for the East German workers in our sample based on their occupations in the GDR in 1989. There was a substantially higher degree of variation in the abilities of East Germans working in the same occupation in 1989 (82 percent compared to 65 percent for the post-reunification period), corroborating our previous findings for networks, and indicating a less stringent sorting of workers to occupations based on the skills that are relevant in the West German labor market but were not during GDR times.

Comparison of the wage structure in the GDR and West Germany To further corroborate the argument that the allocation of workers in the GDR was not affected by considerations about future labor market prospects in West Germany, we demonstrate remarkable differences between the occupational wage structures of both countries before reunification. Figure 2 shows the average monthly gross income in East and West Germany by sector for technical (blue) and commercial occupations (red), respectively.[27] As expected from a socialist state shaped by central planning, differences in gross income across sectors and occupations were much lower in the GDR than in West Germany.[28] More surprising is that within a given sector, employees in technical occupations received higher average earnings than employees in commercial occupations in West Germany, while the pattern was reversed in the GDR. Consequently, individuals in higher-paid GDR jobs were not necessarily higher paid in West Germany and vice versa. Overall, in addition to the limited individual choices for jobs that the GDR system generally offered, these differences in the wage structures in the two parts of Germany before reunification imply substantially different incentive structures.

Summary The evidence in this section leads us to three conclusions. First, the GDR context offers a setting where network homophily in unobserved ability is much less pronounced than in the networks typically analyzed in the existing literature. From the perspective of the skills valued in the West German labor market, our networks are as good as randomly assigned once we control for observable characteristics. Second, AKM worker fixed effects appear to be suitable summary measures for the abilities of GDR workers valued in a Western-style labor market that were not important (and not revealed) during GDR times, but became valuable after reunification. Third, the relatively large variation of estimated AKM worker fixed effects within networks facilitates heterogeneity analyses according to the unobserved ability within GDR coworker networks. We will utilize this feature extensively when investigating specific mechanisms.

[27] The average gross income data for the GDR stems from the "Einkommensstichprobe in Arbeiter und Angestelltenhaushalten der DDR" in 1989. The data for West Germany stems from the "Angestelltenverdienste in Industrie und Handel" in 1988. Both data sets are provided by the Federal Statistical Office.

[28] GDR: Mean 1109,81 (Std. Dev. 77,67); West Germany: Mean 4376,03 (Std. Dev. 601,75).

4.2 Establishment-closure Design

Even if social contacts are assigned quasi-randomly, different types of East German workers may be in high demand by West German firms at different times. For instance, West German firms might be looking for East Germans from a specific occupation over a certain time horizon, leading to spurious correlations between the outcomes of connected network members. We, therefore, combine the as-good-as-random assignment of network contacts with an establishment-closure design. The establishment-closure design enables us to compare the migration outcomes of East German workers displaced in the same establishment-closure who are forced to seek new employment at the same point in time and under comparable conditions (as remaining at their establishment was not a viable option). Additionally, we condition on being employed in the same occupation at the time of displacement and having the same occupation, industry, and state in 1989, prior to reunification. Therefore, the only difference between these workers is their particular establishment – and thus, the GDR coworker network – in 1989. Due to the as-good-as-random assignment of GDR coworkers, this comparison allows us to identify the causal effect of network members in West Germany by comparing the outcomes of displaced workers with GDR contacts in the West and those without contacts in the West. Section 5.1 shows that the individual and network characteristics of displaced workers with and without West contacts are balanced, thereby further substantiating our assessment of as-good-as-random assignment to networks in the GDR. In addition, the results suggest that displaced workers with GDR contacts in the West are not systematically different from those without West contacts.

Empirical Specification We implement our establishment-closure identification strategy in the displaced-worker sample where each observation corresponds to an East German worker i who is displaced from her East German establishment j in year t. We estimate the following model specification:

$$\begin{aligned} Emp^{W}_{i,t+1} = \beta_0 + \beta_1 WestContact_{i,t} + \gamma_{j(i,t)} + \eta_{o,89} + \lambda_{l,89} + \theta_{s,89} + \alpha_0 X_{i,89} \\ + \delta_{o,t} + \alpha_1 X_{i,t} + \varepsilon_{i,t+1}. \end{aligned} \tag{2}$$

The main outcome variable, $Emp^{W}_{i,t+1}$, is an indicator that equals to one if East German worker i is employed in West Germany in the year after displacement. The main explanatory variable is $WestContact_{i,t}$, an indicator that equals to one if at least one of i's former GDR coworkers is employed in West Germany at the time of displacement. In our sensitivity analyses, we also consider the actual number and the share of former GDR coworkers employed in West Germany at the time of displacement. We include establishment-closure fixed effects, $\gamma_{j(i,t)}$, to focus on the variation between workers displaced in the same establishment-closure event.

The shock to the GDR labor market brought about by the collapse of the GDR was mostly industry, occupation, and location-specific. To rule out that this shock differentially hit East Germans with and without West contacts in t, we include fixed effects for the occupation ($\eta_{o,89}$), the industry ($\lambda_{l,89}$), and the state ($\theta_{s,89}$) in which these individuals worked in 1989. Note that the 1989 occupation-specific fixed effects ensure that the identifying variation for β_1 arises from workers being in different networks in 1989, as the GDR system allocated them to different establishments.

We further control for a set of worker characteristics in 1989, $X_{i,89}$, in particular, the size of the GDR network in 1989, the gender, the marital status, whether i had children in 1989, and whether the person was the main caregiver for a family member in the household in 1989.

We also control for i's occupation in t, the year of the establishment closure, to make sure that we compare individuals with and without West contacts whose current occupation has the same potential labor market prospect in West Germany. We control for individual characteristics at the time of dis-

placement, $X_{i,t}$, in particular the age in t, education, an indicator for part-time work, and tenure at the closing establishment, with the latter meant to capture the quality of the match between the worker and the East German establishment that closes down. In all specifications, we cluster standard errors at the level of the establishment closure.

Equation (2) allows us to estimate the causal effect of having a former GDR coworker in West Germany on the migration decisions of displaced workers. Further, we use the same specification to provide additional evidence of the as-good-as-random assignment of network contacts in our setting. To this aim, we use demographic characteristics and pre-migration network characteristics as outcome variables and assess whether there are differences between displaced workers with and without contacts in the West.

4.3 Multiple-contact Design

Once we have established the average causal effects, we use a second identification strategy to investigate the underlying mechanisms. We focus on all East Germans who migrated to West Germany and had multiple former GDR coworkers in West Germany just before their move. We examine two outcomes: the probability that the East German labor migrant joins the particular West German (1) labor market district or (2) the establishment of one of her contacts. We assess whether this probability depends on the quality of the contact's work environment, which we measure in terms of (occupation-specific) labor market tightness or establishment pay, depending on the considered outcome. Importantly, we control for the strength of the network tie between the East German labor migrant and her West contact, as well as for a broad range of the West contact's observable characteristics.[29] Implicitly, therefore, we compare the differential effects of West contacts who are similar in their demographic characteristics, unobserved ability, relation to the East contact, and hierarchical position in the West, but differ in the type of work environment they experience in their local labor market or at their employer in West Germany.

The multiple-contact identification strategy complements our establishment-closure strategy in three ways. First, using only variation between West contacts of the same East German migrant addresses any remaining concern that workers with and without West contacts might systematically differ (although our balance checks in Section 5.1 do not indicate any differences in observed characteristics or labor market outcomes). Second, since identification comes from within-worker variation, we can also draw from the broader sample of all East-West migrants (not only displaced individuals) without being concerned about selective migration. Third, it allows us to examine the mechanisms driving the network effects by investigating which type of connections trigger migration responses.

Empirical Specification We draw on the East-West-pair sample where each observation corresponds to a pair of East German labor migrant i who switches from East German employer j to some West German employer in $t+1$ and GDR-network contact c who works at West German employer $k(c)$ located in region $r(k(c))$ in t. We then estimate the following specification:

$$\begin{aligned} Emp_{i,t+1}^{r(k(c))} = \beta_0 + \beta_1 HighQuality_{r(k(c),t)} + \gamma_i + \delta_{(j(i,t),k(c,t))} \\ + \alpha_0 X_t^{i=c} + \alpha_1 X_{c,t} + \varepsilon_{i,c,t+1}. \end{aligned} \tag{3}$$

The outcome variable, $Emp_{i,t+1}^{r(k(c))}$, is an indicator that equals one if migrant i joins contact c's labor market region, $r(k(c))$, which is the West German county where c's employer $k(c)$ is located. It reduces to

[29] Rajkumar et al. (2022) and Kramarz and Skans (2014), for instance, document the importance of tie strength for job mobility. Montgomery (1991), among others, shows that coworker effects could be more likely induced by high-ability, long-tenured contacts. With our specification, we abstract from these mechanisms and focus on the role of the contact's work environment.

$Emp_{i,t+1}^{k(c)}$ if we consider the binary outcome of joining c's employer $k(c)$ instead. The main explanatory variable is $HighQuality_{r(k(c),t)}$, an indicator that equals one if c's labor market region $r(k(c))$ in t is classified as a tight labor market, i.e., the ratio of job vacancies to the number of unemployed individuals in r is in the upper half of the distribution across West German counties in t. Again, when considering the second outcome variable, our main explanatory variable reduces to $HighQuality_{k(c),t}$, indicating high-paying employers instead. In our baseline, we classify establishments as high-paying if their estimated establishment fixed effects from an AKM wage decomposition are in the upper half of the distribution of West German establishment effects. We show robustness using a variety of alternative measures of establishment quality, including an indicator for growing establishments, the (inverse) churning rate, and the poaching index proposed by Bagger and Lentz (2019).

We include individual-level fixed effects, γ_i, to ensure that we exploit variation between West contacts of the same East German worker. Additionally, we control for the spatial distance between the location of the East German employer $j(i)$ and the West contact's employer $k(c)$, denoted by $\delta_{(j(i,t),k(c,t))}$, to capture the potential dependence of the migration probability on the spatial distance.

To isolate the effect of the quality of the West contact's employer from the strength of the network tie between i and c, we create a set of variables that capture various aspects of the relation between contacts: (a) whether i and c share the same gender; (b) whether they resided in the same municipality in 1989; (c) whether they work in the same occupation in t; (d) the age difference between i and c, and (e) the earnings potential (AKM worker effect) difference between i and c.

Additionally, we control for various characteristics of contacts c, specifically their age, their tenure in the West German establishment, and whether their earnings potential is above the median of the distribution of all former GDR coworkers in West Germany. These control variables capture potential differences in the contacts' position at the West German establishment and their ability to influence hiring decisions.[30]

5 Average Network Effects

5.1 Balancing Tests

Figure 3, Panel A, summarizes the results of balancing tests.[31] We estimate Equation (2) in our displaced-worker sample using various (pre-displacement) worker-level and network-level characteristics as outcome variables. The figure plots the coefficient for the $WestContact$ dummy relative to the sample mean of the respective outcome variable, along with the corresponding 95% confidence interval. If GDR coworkers are indeed as good as randomly assigned, we would expect no difference between East German workers with and without West contacts at the time of displacement.

Consistent with the evidence in Section 4, we do find that, conditional on the controls used in Equation (2), this is the case. The relevant results are those in dark blue dots in Figure 3. There are relatively precisely estimated zero differences between displaced East German workers with and without West contacts in terms of their age, their marital status, and their AKM worker effect.[32] Displaced workers with a West contact are slightly less likely to have children, but the coefficient is small. There are no significant differences in the share of workers who have switched their region, occupation, or industry between 1989 and the displacement year t. Finally, there are no differences in the average characteristics of the networks of workers with and without West contacts. In particular, we compute the average log

[30] About 11.4% of East German labor migrants have more than one West contact at the same establishment. In this case, we keep only one observation and assign the *maximum* value of each control variable among all contacts at the same establishment, e.g., the maximum age of all West contacts at the same establishment.

[31] Appendix Table D.1 shows the actual regression coefficients.

[32] Naturally, the respective outcome variable is left out from the set of controls in each regression.

wage for each displaced worker in the former GDR coworker network, as well as the employment and unemployment rates among network members, and the share of workers with higher education. We focus on these measures in 1992, the first year available after reunification, and exclude all network members who have already migrated to West Germany by 1992.[33] Again, there are no differences between networks of displaced workers with and without West contacts. If anything, the unemployment rate in networks of workers with West contacts is slightly higher.

The red and light blue dots in Figure 3 show estimates of specifications that do not include additional controls or only establishment-closure fixed effects, respectively. The results underline the importance of including these controls. Without them, there are relatively pronounced differences between workers with and without West contacts. In our full specification, these differences disappear, allowing us to compare observably similar individuals who only differ in whether they have a West contact or not.

5.2 The Effect of Former GDR Coworkers in West Germany

Figure 3, Panel B, shows the difference in the probability of being employed in West Germany in the year after displacement between workers with and without West contact. On average, 3.6 percent of workers displaced between 1992 and 2005 were employed in West Germany in the year after the establishment closure. Relative to this sample mean, having at least one former GDR coworker employed in the West at the time of displacement significantly increases the probability of migrating to the West by about six percent.

Table 3, column 1 reports the corresponding regression coefficient from estimating Equation (2) in the displaced-worker sample. Displaced workers with at least one former coworker from the GDR who was employed in the West at the time of displacement are about 0.22 percentage points more likely to find a job in West Germany by $t+1$. In relation to the sample mean, this implies an increase by 6.1 percent (average network effect).

Effect over Time Figure 5 illustrates the evolution of the network effect over time. We examine the effect of having a former GDR coworker employed in West Germany at the time of displacement t on the probability of taking up employment in West Germany in the years from $t-3$ to $t+5$. Notably, there is no difference in migration probability between workers with and without former coworkers from the former GDR in the West prior to displacement. As discussed above, the effect of having a former GDR coworker in West Germany amounts to about six percent in $t+1$. It also significantly raises the probability of migrating in $t+2$ by more than four percent. The positive effect fades out over time. However, cumulated over the five years after displacement, having a former GDR coworker in West Germany increases the migration probability by about 0.5 percentage points, a 14 percent increase relative to the sample mean in $t+1$.

Placebo A potential concern might be that the difference between workers with and without West contacts is driven by unobserved similarities between network contacts (*correlated effects* in Manski's (1993) terminology) or common experiences gained in the GDR work environment (*contextual effects*) rather than by the actual presence of the former GDR coworker in West Germany. Even conditional on the rich set of fixed effects and control variables, unobserved demand factors could induce displaced workers and their former GDR coworkers to migrate to the West. To eliminate these concerns, we employ a placebo strategy that evaluates the effect of former GDR coworkers who are still in East Germany at the time of displacement, but migrate to West Germany sometime in $t+x$, with $x \geq 1$ (i.e., the year after the displacement or later). If our findings were driven by unobserved similarities between West contacts

[33] Note that wages and employment rates were substantially higher in West Germany after reunification. Including migrants for whom we have no pre-migration characteristics would therefore bias the comparison of different networks.

and displaced workers, we would expect a positive and significant relationship between the migration probability of worker i and their (future) West contacts who move at the same time or later. However, the results in Table 3, column 2, show that former GDR coworkers employed in the West in $t+x$ but not before do not affect the probability that the displaced worker i is employed in the West in year $t+1$. Hence, our placebo analysis suggests that correlated or contextual effects do not drive our estimated average network effect.

Alternative Channels Table 3, columns 3-9, address several potential alternative explanations for our baseline result. In column 3, we separate the pull factor of having a former GDR coworker in West Germany from the push factor of having fewer or no former GDR coworkers remaining in the home location in East Germany. This distinction is potentially important since a shrinking network at the origin location could also drive out-migration and generate a spurious increase in movements towards other places, including West Germany (e.g., Buggle et al., 2023). To examine this possibility, we augment the baseline specification with an indicator for having at least one remaining former GDR coworker who works in the same East German region as individual i in displacement year t. Including this variable does not change the estimated effect of having a West contact, suggesting that the pull effect is not confounded by a shrinking local network. This result strengthens the interpretation that networks in the West causally encourage migration, rather than the effect being driven by an erosion of local ties. Still, it also suggests that remaining social ties at home matter, and that both push and pull mechanisms are potentially at play, but they are separable. In Appendix Table D.3, we show that this result is insensitive to using alternative measures of the remaining network "at home".

In addition to former coworkers from the same network, other types of social contacts could result in network effects (that potentially interfere with our main effect of interest). To explore this possibility, we include measures of two additional network types in the regressions. In column 4, we first examine the impact of West contacts who worked in the same establishment and 1-digit occupation but a different 4-digit occupation in 1989. The network effect of West contacts from the same 4-digit occupation remains unaffected. Strikingly, however, having West contacts among GDR coworkers from the same establishment but a different 4-digit occupation has no significant impact on the migration probability. Our network effects, therefore, appear to be driven by former GDR coworkers in the precise occupation of the potential migrant. This result corroborates our rationale for defining the network as the occupation $\times$ establishment cell. In addition, the results rule out the possibility that any time-constant confounding factors related to the GDR establishment in 1989 drive the results.

Second, column 5 shows the effect of West contacts among a neighborhood network of GDR workers who lived in the same municipality in 1989 and share the same 5-year age category (but were not part of the same workplace network). Schmutte (2015), for example, demonstrates that neighborhood networks are important when it comes to job changes. In contrast to this finding, our results suggest that West contacts from neighborhood networks do not significantly impact the probability of a displaced worker migrating to the West. However, the main effect of West contacts from the same workplace network even increases in this specification. Overall, the results suggest that the coworker network is not merely a proxy for "local" social capital. Instead, it is the more relevant type of network in our setting. The finding supports the hypothesis that specific work-related contacts play a uniquely important role in shaping geographic mobility in the context of adverse labor market shocks. Neighborhood networks, by contrast, appear less relevant in this setting.

In column 6, we present an additional test of whether the baseline result is driven by unobserved skills among workers who are more likely to migrate and have better coworker networks. Our baseline specification includes fixed effects for occupation, industry, and region in 1989, aiming to capture differences in the skills acquired by displaced workers in the GDR. In addition to these observables, column

6 conditions directly on the displaced worker's unobservable skills, proxied by the AKM worker fixed effect in period t. The estimated network effect remains robust, suggesting that our baseline result is not driven by high-AKM types of workers being both more likely to migrate and having better coworker networks.

Another alternative explanation for the migration choices of displaced workers and their former coworkers from the GDR is the potential for persistent common shocks at the regional or industry level (more granular than our fixed effects) that might have pushed both the West contact and eventually worker i to West Germany. To avoid the possibility that these local effects drive the results, we consider two subsets of our displaced-worker sample: In column 7, we only consider displaced workers who changed the employment district between 1989 and t. In column 8, we only consider workers who moved to a different industry between 1989 and the t. These restrictions ensure we focus on individuals who no longer work in the industry or region of their GDR workplace. Thus, we can avoid potential common local or industry shocks that might have pushed both the contact in the West and, eventually, worker i to West Germany. The relative effect size increases slightly to 7.8 percent when conditioning on workers who change localities, as shown in column 7. It slightly decreases to 4.9 percent for workers who change industries, as shown in column 8. Despite the limited sample size and the associated loss in power, the estimated effects' relative magnitude is similar to our baseline result.

Finally, a simple geographical argument could explain the common migration choices of former GDR coworkers from the same network. By definition, members of the same network were employed in the same location in 1989. Due to their proximity to the inner-German border, some locations could generally be more likely to have migrants or even regular commuters to West Germany.[34] To address this possibility, column 9 excludes individuals who worked in counties less than 80 kilometers apart from West Germany in 1989. The relative effect size of our main regressor increases slightly to 7.7 percent, suggesting that inhabitants of the border region and potential commuters are not the primary drivers of the average network effect. If anything, including them in our sample deflates the coefficient to some extent.

Sensitivity Analysis and Effect Size Appendix D shows that our baseline result on the average network effect in East-West migration is not driven by particular choices regarding the sample, network, or treatment variable definitions.

Panel A in Table D.2 explores sensitivity regarding the choice of our sample. Our effects remain significant and of similar magnitude if we restrict the displaced-worker sample to workers employed in the year after displacement (column 1), or if we consider networks that include more than 100 GDR workplace connections in the same establishment and occupation (columns 2 and 3). Finally, to assess whether the findings for displaced workers generalize beyond the sample of displaced workers to the overall population of job switchers, we focus on all job switchers (both voluntary and involuntary) in column 4. In this specification, we estimate Equation (2) in the sample of all East Germans who switch jobs between $t \in \{1992, ..., 2005\}$ and $t+1$. We replace the establishment-closure fixed effects with year $\times$ origin-establishment fixed effects. Identification, therefore, comes from variation in former GDR coworker networks between all individuals who leave the same establishment in the same year. Standard errors are clustered at the year $\times$ origin-establishment level. On average, three percent of workers who changed employers between 1992 and 2005 moved to a West German employer. Again, there are significant differences between workers with former GDR coworkers employed in the West at the time of the job switch and those without West contacts. Workers with a West contact are about 0.15 percentage points more likely to find a job in West Germany until $t+1$. Hence, the presence of a former coworker from the GDR increases the probability of migration by 5%— a strikingly similar relative effect size compared to

[34] Our data does not allow us to distinguish movers from commuters as we only observe the location of the workplace after reunification.

our displaced-worker sample.

Our baseline treatment indicator equals one if an East German worker has at least one former GDR coworker (from the same establishment and occupation in 1989) employed in the West in the year of displacement. In Panel B of Table D.2, we consider alternative origin-workplace networks (columns 2-3) and alternative treatment definitions (columns 4-6) based on the actual number or the share of former GDR coworkers employed in the West in the year of displacement. Column 2 shows that having a former coworker from the same establishment and 3-digit occupation but different 4-digit occupation in 1989 in West Germany neither significantly affects the relocation probability nor alters the estimated effect of the original network definition in a meaningful way. If anything, the estimated effect of the alternative network appears to be slightly negative. Results remain robust when adding a dummy for having a West contact from the same establishment and 1-digit occupation–but different 3- and 4-digit occupation–in 1989 (column 3). These sensitivity checks suggest that what makes the network important for the displaced worker's relocation decision is the relevant occupation-specific information–at the highly granular 4-digit level–that West contacts can provide.

Next, we consider alternative treatment definitions of our baseline treatment indicator. In a linear specification, the presence of one additional former coworker in the West increases the probability of migration by 0.06 percentage points, or approximately 1.7% (column 4). Using separate dummy variables for specific numbers of former coworkers in the West in column 5 shows that the linear approximation is quite accurate. This additional analysis enables a comparison of the effect size in our study with that of related studies. Becker et al. (2024) find that ten additional contacts abroad increase the probability of Jewish scientists to emigrate from Nazi Germany by five percentage points or 7.1%. While being much smaller in absolute terms (due to the fact that few East Germans migrated to the West while the vast majority of Jewish scientists had to flee from Nazi Germany), our effect size is of similar magnitude (or even larger) in relative terms ($10 \times 1.7 = 17\%$ vs. 7.1%). In column 6, we consider the effect of an increase in the *share* of former GDR coworkers employed in West Germany at the time of job displacement among all former GDR coworkers. We find that an increase in the share by one standard deviation (i.e., by 0.11) increases the probability of migrating to the West by $0.0078 \times 0.11 = 0.00086$ percentage points, corresponding to 2.4% of the sample mean. Consequently, our effect size is also comparable to the effect size in Munshi (2003), who shows that a one standard deviation increase in the share of established Mexican migrants in the US (0.05, his Table II) increases the probability of employment at the destination for Mexican migrants by $0.67 \times 0.05 = 0.0335$, a 3.5% increase relative to the overall mean of 0.95.

Summary Altogether, our results indicate a positive causal impact of former GDR coworkers in West Germany on the migration probability of their East German contacts. Having a former colleague in the West increases the probability of migrating to West Germany by roughly six percent. This effect size is of comparable magnitude to the effects of professional networks in Becker et al. (2024) and neighborhood networks in Munshi (2003). Our findings for the displaced-worker sample are robust across various specifications and generalize to the broader population of job switchers. We rule out several alternative explanations for the network effects. An essential addition to the extant literature on coworker networks is that we demonstrate the importance of the fine-grained occupational dimension of networks: only former coworkers from the same narrowly defined occupation trigger migration responses.

Do West contacts direct East Germans to a specific destination? The outcome in our baseline specification is an indicator for migrating to *any* West German location after job displacement in the East. We now examine whether the average network effects identified in our setting are directional, i.e., whether West contacts direct their East German network members towards their own localities or

employers (and potentially even divert them away from other destinations). In the recent literature, directional network effects are interpreted as an important argument for identifying the mechanisms of network effects. For instance, Becker et al. (2024) show that the academic network of early Jewish emigrants directed later emigrants who escaped from Nazi Germany to the same destination country while discouraging them from other destinations. Buggle et al. (2023) use the directed effects of network contacts to disentangle the push and pull effects of Jewish migration from Nazi Germany.

In columns 10-12 of Table 3, we decompose the outcome variable into three mutually exclusive categories that more precisely indicate the destination in West Germany. Column 10 shows that having a West contact does not significantly affect the probability of migrating to localities in the West other than the contact's county. Column 11 shows that having a West contact significantly increases the East German worker's probability of migrating to the same county – but to a different employer – by 0.23 percentage points. In relative terms, this corresponds to an effect size of 67.7 percent. Finally, column 12 shows that the effect size doubles to 136.4 percent when considering the probability of migrating to the West contact's employer.

Consequently, the baseline result in column 1 masks significant heterogeneity in the directive nature of the effect. A West contact not only increases the East German worker's probability of migrating to the West but also directs the worker to the contact's specific employer and the contact's locality. These findings provide us with an initial insight into the underlying mechanism. Given that having a former GDR workplace contact in the West has no discernible impact on the probability of migrating to a West German county without a contact, general information about West Germany does not seem to be the key mechanism. Instead, more specific information (e.g., about local conditions and job opportunities) or on-site social support from their West contacts could play a significant role. In the following analyses, we focus on disentangling these channels.

6 Mechanisms of Network Effects

In this section, we aim to understand *why* connections to former coworkers in West Germany are important for labor migration decisions, particularly those directed toward the contact's location or employer. On the one hand, former colleagues in West Germany can provide social support at the new location, thereby effectively reducing relocation costs or generating the pleasure of working with former colleagues. On the other hand, former peers can provide specific information on vacancies, characteristics of the work environment (e.g., amenities or career opportunities), or, for the hiring firm, viable information about the potential new hire. Distinguishing between these mechanisms is important because they have different policy implications (see Bergman et al., 2024, who experimentally vary policies that induce low-income households in the US to relocate towards high-opportunity areas). A lack of support structures could (at least partly) be addressed by relocation subsidies and similar policies. In contrast, information frictions could be reduced by information campaigns or transparency policies that diminish uncertainty. For instance, Barsbai et al. (2024) demonstrate that network contacts and information treatments can serve as close substitutes for inducing migration.

6.1 Conceptual Framework

We sketch a simple framework that characterizes these different mechanisms. The framework is based on the canonical formulation of the classical Roy (1951) model in Borjas (1987). The payoff for an East German worker i in East Germany is $y_{i,E} = \mu_E + \varepsilon_{i,E}$, where μ_E is the part of the payoff explained by observables and $\varepsilon_{i,E}$ is an unobserved component. The payoff for i in West Germany is $y_{i,W} = \mu_W + \varepsilon_{i,W}$. The unobservable components of the payoffs are normally distributed, $\varepsilon_E \sim N(0, \sigma_E^2)$ and

$\varepsilon_W \sim N(0, \sigma_W^2)$, and correlated with correlation coefficient $\rho = \frac{\sigma_{E,W}}{\sigma_E \sigma_W}$. Let C denote the costs of moving from East to West. In this model, individual i moves to the West if the West payoff exceeds the East payoff plus the moving costs, i.e., if $\mu_W + \varepsilon_{i,W} > \mu_E + \varepsilon_{i,E} + C$.

We extend this framework with two ingredients: uncertainty about the payoff at the destination and workplace connections. To introduce uncertainty about the West payoff, we change the information structure of the canonical model. In particular, the realization of $\varepsilon_{i,W}$ is observed only after the migration decision.[35] The migration decision, therefore, depends on the East German worker's *expectation* about the payoff in the West. Following the evidence in Akerlof et al. (1991), we assume that East Germans have an initial expectation about the mean of the unobserved component of the West payoff that is below the true (conditional) mean, i.e., they form biased expectation of $\varepsilon_{i,W}$, given by $E[\varepsilon^*_{i,W}] = \rho \frac{\sigma_W}{\sigma_E} \varepsilon_{i,E} - b \sim N\left(\rho \frac{\sigma_W}{\sigma_E} \varepsilon_E - b, \sigma_W^2(1-\rho^2)\right)$, where b is the bias. As a consequence, i decides to migrate if $\mu_W + E[\varepsilon^*_{i,W}] > \mu_E + \varepsilon_{i,E} + C$, and the migration probability increases with the expectation about the West payoff. If $b > 0$, there are fewer migrants than expected based on the actual payoff differences, in line with the empirical evidence for Germany after reunification (see Section 2).

As a second extension, we introduce network contacts who impact the migration decision in two ways. First, as in Munshi (2020), they shift the overall payoff in the West. We model this as a multiplier to the observed component of the West payoff. For simplicity, we only consider the extensive margin of having a contact in the West, denoted by the dummy $WestContact$.[36] The observed component of the West payoff equals $(1 + \beta WestContact)\mu_W$, where β measures the impact of having a contact in the West on the observed payoff component. If $\beta > 0$, having a contact improves the West payoff, e.g., due to social support that network contacts provide or due to an increase in utility from working with peers.

Second, network contacts in the West also provide a signal about the unobserved component of the West payoff. The signal provided by contact c is defined as $s_c = \varepsilon_{c,W} + \nu_c$ and depends on the realization of the unobservable payoff component of contact c in the West, $\varepsilon_{c,W}$, as well as a noise term, ν_c. The noise term captures how precisely the information on the unobservable payoff component is conveyed to the East German worker. We assume that ν_c is normally distributed with mean zero and variance σ^2_{ci}. A higher variance of the noise term implies that the signal is less informative to worker i. We assume that σ^2_{ci} is known to i and depends on how similar the realization of contact c's East payoff component $\varepsilon_{c,E}$ is compared to $\varepsilon_{i,E}$. After observing the signal from contact c, East German worker i updates her expectation about the West payoff to $\frac{\sigma_W^2 s_c + \sigma^2_{ci} E[\varepsilon^*_{i,W}]}{\sigma_W^2 + \sigma^2_{ci}}$.

Summarizing all terms, East German worker i migrates to West Germany if

$$\mu_W + E[\varepsilon^*_{i,W}] + WestContact\left(\beta\mu_W + \frac{\sigma_W^2}{\sigma_W^2 + \sigma^2_{ci}}\left(s_c - E[\varepsilon^*_{i,W}]\right)\right) > \mu_E + \varepsilon_{i,E} + C. \tag{4}$$

In Section 5, we document a positive causal effect of having a network contact in the West on the migration probability of East Germans. This average effect, however, does not allow us to separate the two channels in Equation (4). In the social support channel, a positive parameter β implies that West contacts unambiguously increase the migration probability, regardless of their characteristics or labor market experiences. In the information channel, the size of the network effect depends on the information signal that the West contact provides: The more the realization of the West contact's payoff deviates from the East German worker's initial expectation, the larger is the West contact's impact on the migration decision. The expectation can be updated both positively and negatively. Due to the

[35] Note that the payoff is not simply determined by the wage, which might be known before the migration decision, but also by other components that might be uncertain at the time of the migration decision. Even if the East worker knows the exact conditions of her work contract in the West, she may not be certain how much she will like working in the West, what the non-wage components of the work environment are, how many outside options there are, etc.

[36] Munshi (2020) also considers the intensive margin determined by the number of contacts at the destination.

evidence for negatively biased initial expectations, however, we expect positively updated expectations to be more prevalent. At the same time, the updating of expectations depends on how informative the signal is to worker i. The network effect is larger, the more similar the unobserved East payoffs of the East German worker and her West contact are.

We examine the empirical support for the role of the quality and the variability of the information signal in the following two sections. We rely on our multiple-contact design that examines differences in the network effect depending on variation across West contacts of the same East German labor migrant. Exploiting within-worker variation allows us to focus on the role of s_c and σ^2_{ci} while keeping the other parameters of Equation (4) constant.

6.2 The Role of the Contacts' Work Environment

We begin by examining the role of the West contact's work environment in West Germany and, hence, the information signal s_c that West contacts can provide to their former GDR coworkers in the East. The model predicts that West contacts in a more favorable work environment are more likely to induce migration responses than those in a worse environment because, conditional on their East payoff type, they have better realizations of their West payoff. Based on our finding that West contacts direct their East German network members towards their own locations and employers, we focus on these two dimensions. Specifically, we estimate the effect of having a West contact in a tight versus a slack local labor market, and at a high-paying employer versus a low-paying employer, while holding constant demographic characteristics and the East payoff of the different West contacts. Specifically, we compare two West contacts of an East German migrant who share the same age, gender, occupation, tenure, estimated AKM worker effect, and spatial distance to the East, but differ in the quality of their West German labor market or employer. We assess whether the East German migrant ends up in one of their West contacts' labor market or employer.

Table 4 presents the results for estimating Equation (3) in the East-West-pair sample. Panel A focuses on the role of local labor market tightness, while panel B focuses on the role of employer pay. In panel A, we rank all West German counties according to their labor market tightness and define counties above the median as tight markets. Specifically, we use two different measures of local labor market tightness. In columns 1 and 2, we compute tightness at the county level as the ratio of job vacancies to the number of unemployed individuals. In columns 3 and 4, we compute tightness at the county $\times$ occupation level, using detailed information on occupation-level tightness (measured as the number of vacancies in an occupation divided by the number of unemployed who had their last job in the respective occupation) and attributing it to West German regions based on the average employment shares of occupations between 1985 and 1989.

The coefficient in column 1 shows that East German labor migrants are significantly more likely to join their contact's labor market if county-level tightness is high. Relative to the average probability of 3.2% to join a particular contact's county, the coefficient indicates a 13% higher probability of joining the contact's county if it has an above-median level of labor market tightness. Labor migrants tend to follow contacts into labor markets that are objectively more attractive in terms of job opportunities.[37] However, it appears quite natural that East German labor migrants are more likely to join tight labor markets, which offer more vacancies. To test whether this is really about job opportunities within the network rather than just overall attractiveness of counties, we estimate a second specification where counties are classified as tight markets if they are in the upper half of the distribution *within* an East German labor migrant's network—regardless of their position in the overall ranking of West German counties. That

[37] Note that information on labor market tightness at the county level is only available from 2000 on. Therefore, our sample period is shorter than in the other analyses, resulting in somewhat more noisy estimates.

is, for an East German worker with two West contacts in different counties, we classify the county with a higher level as tight – even if it ranks low in the overall distribution of all West German counties. We find a significant and sizable effect of having a West contact in a relatively tighter labor market on the probability of migrating to that specific contact's county (column 2). The findings suggest that East German migrants do not simply follow contacts; they selectively choose contacts located in relatively better labor markets. This strengthens the argument that migrants use their networks to identify and access relatively better destinations.

Next, we take one step further by asking whether workers are especially likely to follow contacts into labor markets that are not only locally tight but also tight in terms of the contacts' occupation. We investigate this by altering the measure of labor market tightness. Specifically, we show in columns 3 and 4 that the effect of county × occupation-specific labor market tightness is substantially stronger than the results that only consider county-specific tightness. Having a West contact in a labor market with an above-median level of county × occupation-specific tightness increases the probability of migrating to that labor market by more than 60% (column 3). Again, the estimate remains large and significant if we consider the ranking within an East German's network (column 4). Overall, the evidence suggests that migration decisions are not only shaped by having a contact in the West but also by the availability of jobs in the West contact's labor market, and especially by whether these job opportunities match the contact's occupation. Consequently, West contacts located in occupation-specific tight labor markets appear to play a vital role in affecting mobility. This finding is further corroborated by the evidence in Appendix Table D.4, where we show that county × occupation-specific tightness is the only significant measure when we restrict the outcome to cases where an East German labor migrant joins the same location and occupation as her West contact but a different employer.

Panel B analyzes the role of the West contacts' employer quality. High-paying employers are defined as establishments with an estimated AKM establishment effect above the median establishment effect of all West German employers.[38] Column 1 shows that having a former GDR coworker employed at a high-paying employer significantly increases the probability of switching to that same employer compared to having a former GDR coworker at a low-paying employer. Relative to the average probability of joining a contact's employer, the coefficient implies a 24% increase. Again, it appears more natural for labor migrants to join employers that pay high wage premiums. Therefore, column 2 shows a specification where establishments are classified as high-paying employers if they are in the upper part of the distribution of estimated AKM establishment effects *within* the East German labor migrant's network, i.e., the "best" among their network, regardless of their overall position in the national distribution. The estimate remains unchanged, suggesting that the effect is not solely driven by top-tier employers, which are generally more attractive, but rather that the relative quality of employers within one's network matters.

The evidence suggests that, consistent with the importance of the information channel in our model, the quality of the workplace of contacts in West Germany has a positive effect on East German workers' migration decisions. Importantly, we control for the (pre-migration) AKM worker effect of the West contacts such that the West contacts' realization of the West payoff can be interpreted as a signal about the quality of the work environment, conditional on their worker type. Better signals thus induce more migration. In addition, more specific signals about occupation-specific tightness or establishment-level pay induce stronger migration responses.

Information about the Work Environment versus Referrals In principle, information about labor market opportunities can flow in two directions: As in our model, West contacts can inform their East German network members about the quality of the work location and employment opportunities in the West. Alternatively, West contacts could refer their East German contacts to their West German

[38] Alternative measures of employer quality and alternative cutoff values are discussed below.

employers if vacancies open up (Montgomery, 1991; Dustmann et al., 2016). In this paragraph, we provide evidence that referrals are not particularly important in our context. First, the large mobility towards a West contact's local market (but to a different employer) cannot be explained by referrals.

Second, the classical model of referrals (Montgomery, 1991) predicts that employers consult their best-performing employees to nominate referral hires because they anticipate positive assortative matching in networks. Hence, employers expect referrals of well-performing employees to also perform well. However, columns 3 to 5 of Table 4 show that this is not the case in our context. West contacts who receive a wage above the median of their employer's wage distribution are not more likely to trigger migration responses than those who receive a wage below the median (column 3). Moreover, the effects of West contacts in high-paying establishments remain unchanged when we control for the high-wage dummy (columns 4 and 5).

Third, Dustmann et al. (2016) predict and empirically show that employers who hire referred workers have an information advantage, resulting in higher starting wages and more stable employment relations but lower wage growth for their referral hires. Appendix Table D.7 shows that this is not the case in our context. We compare East German labor migrants who switched from an East German employer in t to a West German employer in $t+1$ and had a West contact at this employer (i.e., potential referral hires) to those who switched to the same West employer but did not have a West contact at this employer (i.e., free market hires). We do not observe any differences in tenure, starting wages, or wage growth between these two groups.

Overall, these findings do not suggest that a referral-based mechanism is important in our setting. Instead, they support the hypothesis that West contacts influence migration primarily by providing specific information about the attractiveness of local labor markets or employers, not by facilitating direct recruitment through referrals.

Sensitivity Analysis Appendix Table D.5, Panel A shows that our findings do not depend on how we classify West German employers into high- and low-paying establishments. Using the 75th percentile as a cutoff value for high-paying establishments (rather than the median in our baseline) in column 2 does not change our conclusions. In column 3, we distinguish between high- and medium-employer pay using both the median and the 75th percentile as cutoff values. Clearly, the effect is driven by employers with AKM establishment effects above the 75th percentile. Additionally, we show that our results are similar when we use alternative measures of establishment quality. In column 4, we use a year-specific indicator that equals to one if the number of workers is growing between $t-1$ and t. In column 5, we use the (inverse) churning rate. The churning rate, defined as the fraction of leavers and hires in total employment at the establishment, as a measure of (inverse) establishment quality is motivated by equilibrium models with search frictions (e.g., Burdett and Mortensen, 1998) that predict more productive establishments to have lower separation and hiring rates in equilibrium. In column 6, we use the poaching index proposed by Bagger and Lentz (2019), i.e., the ratio of new hires from other employers to new hires from unemployment. A higher poaching index indicates a higher establishment quality, as establishments can poach workers from other establishments only if they offer superior jobs in terms of wages and non-wage amenities. Regardless of the measure, the network effect is significantly higher for West contacts employed at high-quality employers.

Finally, we demonstrate that the effect of West contacts in high-quality work environments is stronger than in low-quality work environments when we employ our establishment-closure design and, consequently, an outcome variable that directly corresponds to the one in our extended Roy model. Table D.5, Panel B exploits heterogeneity in the network composition of displaced workers who lose their jobs due to the closing of an East German establishment. Compared to displaced workers without West contacts, workers with West contacts in tight local labor markets are significantly more likely to migrate

to West Germany.[39] Workers with West contacts in slack markets are also more likely to migrate than those without West contacts, but this effect is smaller and not statistically significant in both columns. In column 3, we analyze the role of the West contacts' employer pay. Again, displaced workers with West contacts at high-paying employers are significantly more likely to migrate after job displacement.[40] In contrast, displaced workers with West contacts at low-paying employers are more likely to migrate than those without West contacts, but to a smaller extent and less significantly (column 3). These results hold when we additionally control for (at least one) contact being a high-wage earner whose wage is above the median of the establishment's wage distribution (column 4) and across the different measures of establishment quality (columns 5-7).

6.3 The Role of Similarity Between Contacts

We proceed by examining the role of similarity between the East German labor migrant and her West contacts and, hence, the potential reduction in the noise of the informational signal, σ^2_{ci}, that the contact can provide. The key idea behind this analysis is that information about employment opportunities and conditions will only trigger migration responses if it is relevant to the receiver. The variance of the noise term in our model depends on how similar the potential labor migrant and her West contact are in terms of their unobserved payoff component in East Germany. We measure this similarity by the difference between the (pre-migration) AKM worker effects of the East-West pair.[41] We then classify the differences between all pairs into terciles: pairs with a large negative difference where West contact c has a higher worker effect than East German worker i, similar pairs where the difference in worker effects is small, and pairs with a large positive difference where West contact c has a lower worker effect than East German worker i.

Table 5 shows our results, again separately for mobility towards the West contact's local labor market (columns 1 and 2) or employer (columns 3 and 4).

Column 1 shows that East German labor migrants are significantly more likely to join the labor market of West contacts who have a similar AKM worker effect. Compared to West contacts with lower AKM effects, similar contacts increase the probability of joining their local market by 16.5 percent. In contrast, West contacts with higher AKM effects do not have a differential impact. The result is consistent with the hypothesis that the similarity of potential migrants and their contacts in the West reduces the noise of the signal about the West German labor market, but dissimilar pairs do not. Column 3 indicates a very similar pattern for mobility towards the West contact's employer. Here, the probability to join the employer of similar contacts is 44 percent higher than the probability of contacts with lower AKM effects.

In column 2, we interact the indicators for the three similarity groups with terciles of county-$\times$ occupation-specific labor market tightness, using West contacts with lower AKM worker effects in medium tightness markets as the reference category. The results reveal a striking pattern. In all similarity groups, having a West contact in very tight labor market increases the probability of joining that market, most pronounced for contacts who have similar potential. Only for contacts with similar AKM worker effects, we observe also a positive effect of having a contact in a medium-tight labor market. In contrast, having

[39] To construct the indicator for tight labor markets, we first compute the distribution of labor market tightness among all West German counties with GDR coworkers in the specific displacement year. If a displaced East German worker has multiple West contacts, we use the maximum labor market tightness among all West contacts. We then compute the year-specific median of the distribution of labor market tightness and classify those above the median as tight labor markets.

[40] To construct the indicator for high-paying employers, we first compute the distribution of AKM establishment effects among all West German establishments with GDR coworkers pooled across time. If a displaced East German worker has multiple West contacts, we use the maximum establishment effect among all West contacts. We then compute the median of the distribution of establishment effects and classify those above the median as high-paying employers.

[41] Below, we provide results with an alternative measure of similarity based on the highest educational degree.

a West contact in a slack labor market decreases the probability of joining that market for all types of network ties. The evidence is consistent with the notion that the signal about the quality of the local labor market has an important role in guiding mobility choices: East Germans are deterred to move if their West contacts experience slack labor markets. They are induced to move if their West contacts experience tight markets, particularly so when these contacts are similar.

The importance of similarity is even more pronounced when we consider mobility towards the West contact's employer. In column 4, we interact similarity with three groups of employer quality. As before, we define high-paying employers as establishments above the median of the AKM establishment effect distribution in West Germany. In addition, we separate employers below the West German median into those above and below the median establishment effect of the East German distribution, i.e., West employers that pay more than the median East German employer, and below (medium- and low-paying employers). The omitted category is West contacts with lower earnings potential at medium-paying employers.

East German workers are significantly more likely to move to their contact's employer if the contact has similar earnings potential and works at a high-quality establishment or medium-quality establishment (0.18 and 0.15; the two coefficients are not statistically different from each other). The effects are sizable, at 72% and 60%, respectively. In contrast, similar-potential contacts at low-quality establishments tend to discourage mobility.

When the contact in West Germany has a higher earnings potential than the East German migrant, the difference is never statistically significant (as indicated by the first three coefficients and standard errors in column 4). Overall, this suggests that similarity in earnings potential matters more than being connected to a star contact: matching with someone of similar earnings potential has consistently more robust effects than matching "up." The information provided by those 'better' contacts does not improve the signal about the West German labor employer for potential East German migrants.

The results for contacts in the West who have lower earnings potential than the East German worker suggest a (weakly statistically significant) positive effect of migrating to the contact's West German employer, provided the latter is of high quality. Even though the signal from these contacts in the West is less precise, it still increases the probability of migrating to the contact's establishment in the West, if it is a high-quality employer.

Overall, the results highlight that East German migrants are not simply following the most successful contacts or those similar to themselves. Instead, they appear to make strategic decisions that take into account the characteristics of the network ties and the quality of the employment opportunity. The latter can amplify or dampen the effect of network ties. The results are consistent with the mechanisms postulated in the conceptual framework. East German workers use informative signals from their contacts about payoffs in the West. They use contacts to reduce uncertainty, and the value of the signal they receive plays a key role in their decision. The results further corroborate that the more precise the signal (the more similar contact c is to worker i), the more the signal influences the worker's updated expectation.

Sensitivity Checks Appendix Table D.6 provides sensitivity checks for our analysis of the role of similarity. We first vary the cutoff values that determine the classification into high-, medium-, and low-paying employers in columns 2 and 3 without observing a significant change in our estimates. In columns 4 and 5, we use an alternative measure of worker similarity that classifies workers as similar if they share the same level of formal education (acquired during GDR times). Our patterns of network effects remain robust.

7 Conclusion

In a context where improved employment outcomes entail relocating to a new destination, how do former coworkers affect workers' labor migration decisions? We address this question by examining the distinctive context of German reunification in the early 1990s. At the time, East Germany was undergoing large-scale structural changes, accompanied by regionally concentrated job losses. As a result, improvements in employment outcomes for former GDR workers typically meant seeking new jobs in West Germany. Yet, despite large and persistent gaps between East and West Germany, the vast majority of East Germans stayed in the East rather than "moving to opportunity" in the West.

Building on two identification strategies, we demonstrate that former GDR coworkers in West Germany significantly contributed to the relocation of East Germans to the West. We establish that West contacts, particularly those with similar earnings potential in the market-based economy of reunified Germany, trigger migration responses among East Germans *if* they are employed in a favorable work environment. Overall, the results highlight that East German migrants are not simply following their most successful contacts. Instead, they appear to make strategic decisions that take into account the relevance of the information the contacts provide for their own potential labor market success and the quality of the employment opportunity. The latter can amplify or dampen the effect of network ties.

In contrast to the existing literature, which documents the importance of social support from network contacts in developing countries (Munshi, 2020; Blumenstock et al., 2025), our evidence emphasizes the role of the information channel in migration decisions within industrialized countries. We believe that this difference is reasonable given the large institutional differences between the countries considered in these studies and Germany, which includes a strong publicly provided social safety net as well as well-developed labor market institutions that offer general information on job opportunities. The evidence suggests that even in settings with well-developed labor market institutions, coworker networks can mitigate information frictions if they provide specific information relevant to the individual context.

A rough back-of-the-envelope calculation yields that average wages among all displaced East German workers would be 1.6% lower in the absence of information from coworkers.[42] An alternative view is that extending the information to everyone (and abstracting from any general equilibrium responses) would result in 2.4% higher migration and 1% higher average wages.[43] Extending the particularly useful information from similar coworkers at high-paying West employers to everyone would result in 9% higher migration and a 4% increase in average wages.[44]

Could the specific information provided by network contacts also be publicly provided, or – in other words – can our results inform the design of labor market policies? Barsbai et al. (2024) show how network contacts and publicly provided information can serve as substitutes under certain conditions. Making publicly available information specific and relevant would require transparency policies that extend beyond wages, including policies that facilitate social connections between workers and potential employers, as well as a more targeted approach to labor market counseling. The latter does not seem unrealistic in the age of artificial intelligence, where recommender systems can systematically process the information and experience of similar users and use it to make recommendations (see, e.g., Le Barbanchon et al., 2023, in the context of online vacancies).

[42] In total, 64.1% of all displaced East Germans have a former GDR coworker in West Germany. Having a coworker increases the migration probability by 0.22 percentage points, such that without coworkers we would observe $0.641 \times 0.22 = 0.135$ percentage points lower migration and, given the average gain in real wages of about 12%, $0.135 \times 0.12 \times 100 = 1.6\%$ lower average wages.

[43] If everyone had a West contact this would imply $(1 - 0.614) \times 0.22 = 0.085$ percentage points higher migration, i.e., an increase by $\frac{0.085}{3.6} \times 100 = 2.4\%$.

[44] 16.4% of displaced workers have a similar West contact at an above-median West employer. The migration probability increases by 0.4 percentage points for these contacts, resulting in $(1 - 0.164) \times 0.4 \times 0.12 \times 100 = 4\%$ higher wages.

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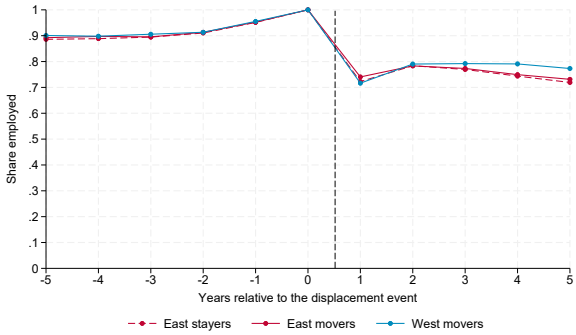
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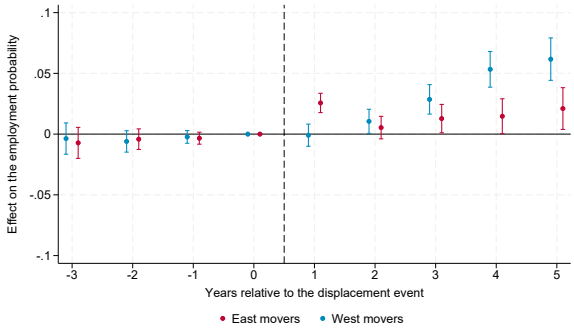
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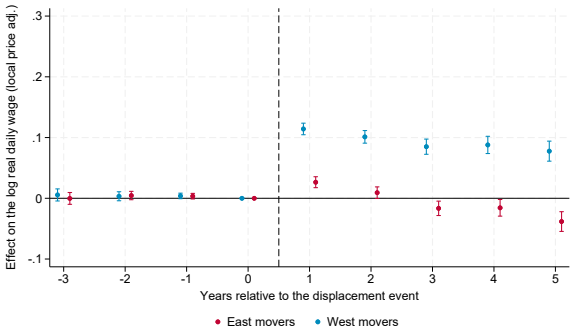
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Figure 1: Labor Market Outcomes around Job Displacement

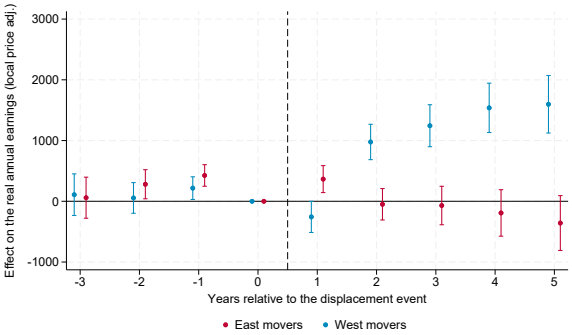




(a) Employment



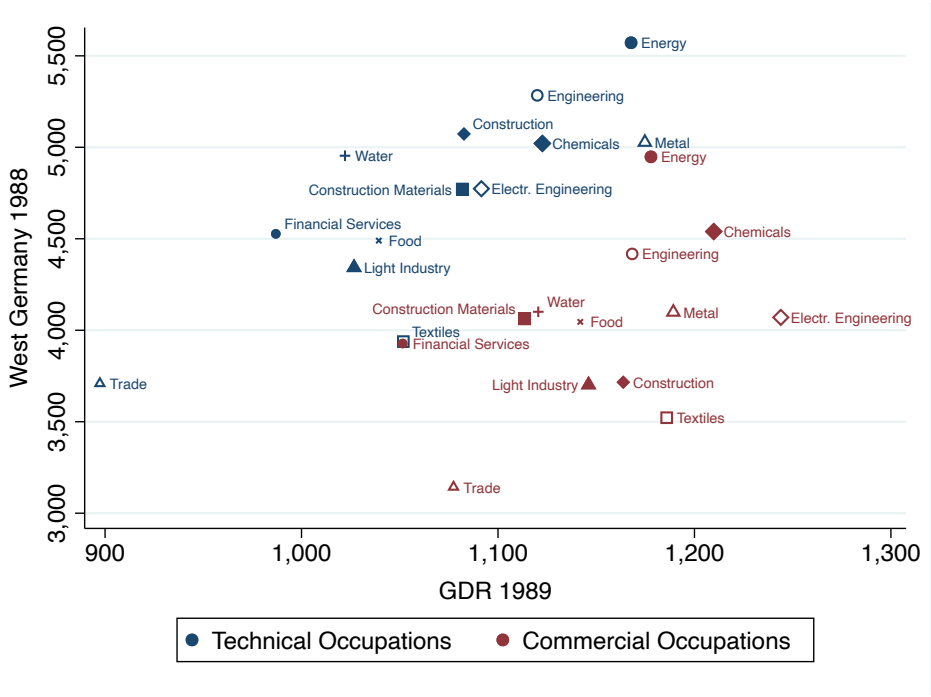
(b) Real Wages



(c) Real Earnings

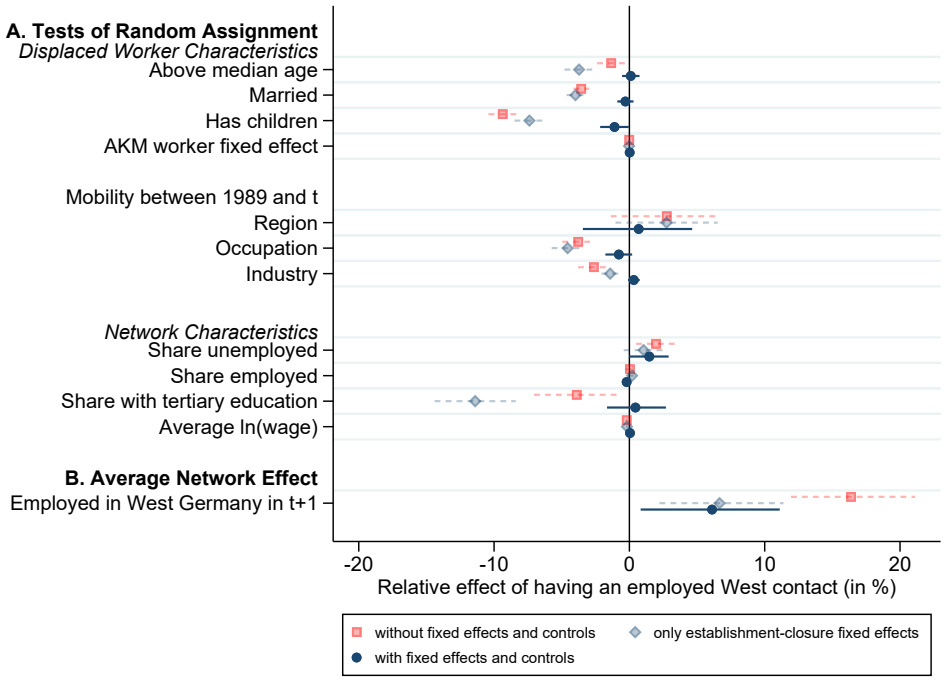
Notes: These figures plot the development of labor market outcomes over the years before and after job displacement. From our displaced-worker sample (see Section 3), we select workers who started a new job within the first five years after displacement and separate them into three groups: East stayers whose first new job is located in their displacement state (dashed red line), East movers whose first new job was in a different East German state (solid red line), and West movers whose first job after displacement was in West Germany (blue line). The graph on the top-left side shows annual means from the raw data, while the remaining graphs plot coefficients from an event-study analysis that controls for person fixed effects, calendar-year fixed effects, relative event-time fixed effects, and a linear time trend common to both East and West movers. Panel a shows employment probabilities in the years before and after job displacement. Time is measured in years and normalized relative to the establishment closure, which happens between periods 0 and 1. Panel b shows log real wages, accounting for differences in local prices and housing using the local price index from 2007 (BBSR, 2009). Panel c combines the employment and wage margins into annual earnings.

Figure 2: Average Gross Income by Broad Sector and Occupation Type in West Germany and the GDR Before Reunification



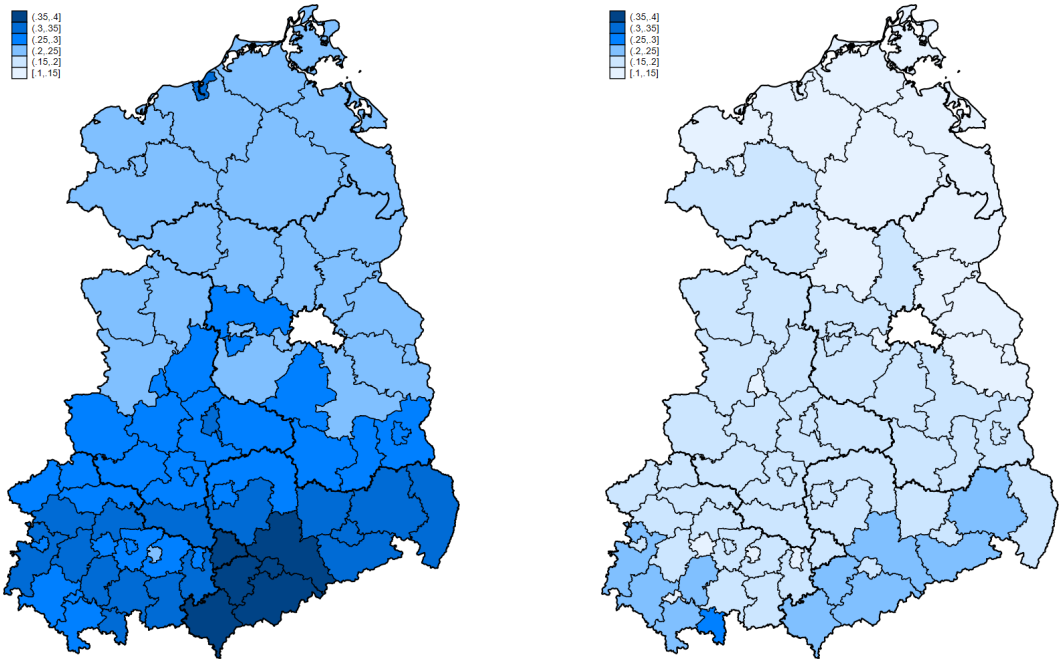
Notes: This figure plots the average monthly gross income for 13 broad sectors, separately for commercial occupations (in red) and technical occupations (in blue). Values on the horizontal axis represent data from the GDR, stemming from the "Einkommensstichprobe in Arbeiter und Angestelltenhaushalten der DDR" in 1989. Values on the vertical axis represent data from West Germany stemming from the "Angestelltenverdienste in Industrie und Handel" in 1988. As an example, within the chemicals sector (diamond shape), the average monthly gross income is higher for the blue technical occupations than for the red commercial occupations in West Germany. Conversely, the average monthly gross income is lower for the blue technical occupations than for the red commercial occupations in the GDR. All data is provided by the Federal Statistical Office.

Figure 3: Tests of As-good-as-random Allocation and Average Network Effect



Notes: This figure depicts coefficients for the $WestContact$ dummy from estimating Equation (2) in the displaced worker sample relative to the sample mean of the respective outcome variable. The error bars indicate the corresponding 95% confidence intervals. Establishments in Berlin are excluded throughout. In panel A, we show test results for the random assignment of coworkers in the GDR. In the first part of panel A, the outcome variables refer to the displaced East German worker i's characteristics. We include indicators for being above median age, married, or having children in 1989, and the AKM worker effect. Moreover, we consider whether worker i switched federal states, (1-digit) occupations, or (2-digit) industries between 1989 and t. In the second part of panel A, the outcome variables refer to worker i's network quality, i.e., the share of her network that received unemployment insurance benefits in 1992, the share employed in 1992, the share with tertiary education in 1989, and the average $ln(wage)$ of network members in 1992.
In panel B, we show results for the average network effect where the outcome variable indicates whether worker i, displaced in year t, is employed in West Germany in $t+1$.
We run three different specifications for each outcome variable: (a) a specification without fixed effects and controls, (b) a specification with establishment-closure fixed effects, and (c) a specification with the full set of controls and fixed effects. The full set of controls includes a dummy for having a non-employed West contact in t, a dummy for having a contact that already returned to East Germany in t, a female dummy, and dummies for three levels of education, tenure, age, and age squared, a dummy for being married in 1989, a dummy for care responsibilities in 1989, a dummy for kids in the household 1989, a dummy for having a part-time job in 1989, and network size. We exclude the respective control variable if it is equivalent to the outcome. Additionally, we include fixed effects for the worker's (3-digit) occupation in t and 1989, fixed effects for the industry and federal state in 1989, and fixed effects for the establishment-closure event. Standard errors are clustered at the establishment-closure level.

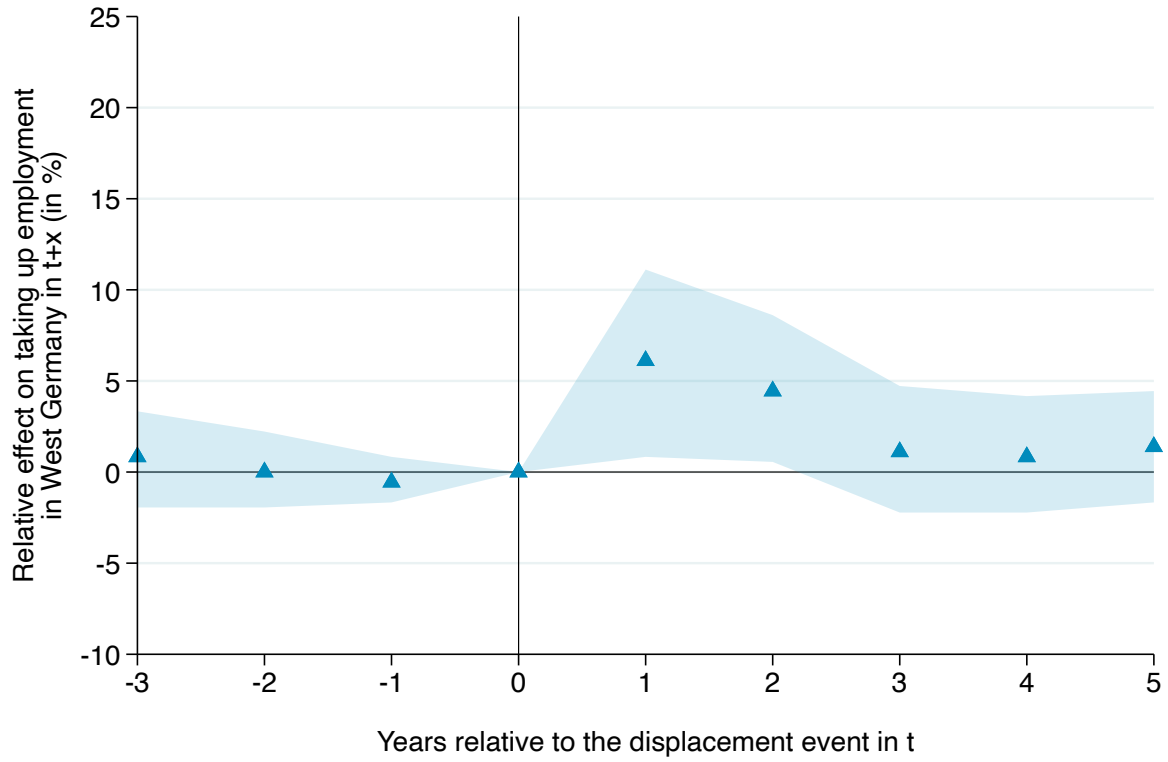
Figure 4: Manufacturing Share Before and After Reunification in East Germany



(a) 1989 (b) 1992

Notes: These maps display the county-level share of establishments active in the manufacturing sector in East Germany in (a) 1989, prior to reunification, and (b) 1992, after reunification. Darker shading indicates a higher manufacturing share.

Figure 5: Effect over Time



Notes: This figure presents the estimated effects of having an employed West contact in year t over time for the displaced worker sample, derived from separate estimations of Equation (2), along with 95% confidence intervals. Establishments in Berlin are excluded throughout. The binary outcome variables indicate whether worker i, who was displaced in year t, takes up employment in West Germany in year $t + x$. The figure depicts the estimated effects relative to the sample mean of taking up employment in West Germany in $t + 1$ (3.6%). The estimate for year $t + 1$ corresponds to the baseline result shown in panel B of Figure 3 and column 1 of Table 3.

Table 1: Summary Statistics

Panel A. Displaced-Worker Sample

Characteristics of Displaced Workers		Characteristics of Displaced Workers' Networks	
Share (in %)		Number of network contacts	
employed in West Germany in $t+1$	3.6	in 1989	28.0
female	27.7		(26.7)
lower educated in t	2.9		[6; 18; 44]
medium educated in t	88.8	employed in West in t	2.1
higher educated in t	8.3		(2.9)
married in 1989	71.6		[0; 1; 3]
with care responsibilities in 1989	0.4	non-employed in West in t	.4
with kid(s) in 1989	47.3		(.8)
with part-time contract in 1989	5.6		[0; 0; 1]
		returned from West in t	1.1
Age in t	42.9		(1.8)
	(8.2)		[0; 0; 1]
	[37; 43; 50]	Share (in %) with	
Tenure in closing establishment in t	3.8	at least one employed West contact in t	61.4
	(2.7)	1 employed West contact in t	20.4
	[1.7; 2.8; 5.0]	2-3 employed West contacts in t	20.4
		4-6 employed West contacts in t	12.7
		$\geq$7 employed West contacts in t	7.8
		with wage $\geq$ employer's median	41.1
		with wage $<$ employer's median	20.3
		at least one non-employed West contact in t	27.0
		at least one returned West contact in t	44.7
		at least one employed West contact in $t+2$	2.1
		at least one contact at home in t	80.1

Panel B. East-West-pair Sample

Characteristics of the West Contact		Characteristics of the East-West Pairs	
Age	40.4	Difference between West contact and i	
	(8.0)	in age	7.5
	[34; 40; 46]		(5.9)
Tenure in West establishment	4.2		[3; 6; 11]
	(3.2)	in earnings potential	-.04
	[1.7; 3.3; 6.0]		(.31)
			[−.21; −.04; .13]
Share (in %) of West contacts		Share (in %) of West contacts	
with earnings potential $\geq$ median in West	49.2	with same gender as i	87.9
		from same municipality as i in 1989	26.6
at high-quality employer relative to		with same occupation as i in t	11.9
all West German employers	52.1		
West German employers in i's network	54.5		
that are high-wage workers	46.2		
with higher earnings potential than i	33.4		
with similar earnings potential as i	33.5		
with lower earnings potential than i	33.2		

Notes: This table reports summary statistics for the two main analysis samples. Panel A focuses on the displaced-worker sample based on the data on GDR workers aged 20 to 50 in 1989. The sample comprises 246,415 workers displaced from 46,914 establishment closures. Berlin is excluded throughout. Characteristics from 1989 are extracted from the GAV data. An observation refers to an East German worker displaced from an East-German establishment in $t \in \{1992, ..., 2005\}$. See Section 3 for further details on the sample. Panel B focuses on the East-West-pair sample based on the data on GDR workers aged 20 to 50 in 1989 with multiple West contacts who switched jobs from an East German employer to a West German employer between $t \in \{1992, ...2005\}$ and $t+1$. The sample comprises 32,245 East German labor migrants and 147,068 East-West contact pairs. Berlin is excluded throughout. Characteristics from 1989 are extracted from the GAV data. Each observation corresponds to an East German labor migrant $\times$ West contact pair. We only include pairs of East-West migrants with employed West contacts. See Section 3 for further details on the sample. Values without brackets refer to the sample mean, values in parentheses refer to the standard deviation, and values in square brackets refer to the 25th, 50th, and 75th percentiles, respectively.

Table 2: Sorting in the GDR and Reunified Germany

	(1)	(2)	(3)	(4)	(5)
		Share within			
	Total Variance	Workplace Networks (Occ. × Establ.)			Occupations
Randomized Allocation (Simulation mean)	0.106	78%	74%	60%	
GDR workers in 1989	0.106	60%	71%	60%	82%
GDR workers in 1995	0.099	25%	11%	4%	65%
CHK (2013) West German men 1996-2002	0.107				64%
Within Industry × State × Occupation			Yes	Yes	
Within Demographic Subgroups				Yes	

Notes: This table presents results from decomposing the variance of estimated AKM worker fixed effects according to Equation (1). The first row presents the average decomposition results across 100 simulations where we randomly allocate workers to coworker networks defined by the occupation × establishments. The second and third row present decomposition results for East German men and women in 1989 and 1995 respectively, both based on AKM worker fixed effects estimated in 1998-2004. As a benchmark, the final row illustrates the results on occupational sorting for West German men after reunification by Card et al. (2013), extracted from their original Tables III and VI and referring to AKM worker fixed effects estimated using the interval 1996-2002. Column 1 indicates the overall variance of AKM worker effects. Columns 2 to 4 indicate the share of the variance within occupation × establishment cells (coworker networks) relative to this overall variance. Column 2 shows the results of the baseline decomposition without any restrictions. Column 3 considers variation within 2-digit industry, federal state, and 3-digit occupation cells, and column 4 considers, additionally, demographic subgroups defined by age groups, education, gender, part-time status, caregiving, marital status, children, and the network size in 1989. Column 5 indicates the share of the variance within occupations relative to the overall variance. Additional details are provided in Appendix C.

Table 3: The Effect of GDR Coworkers in West Germany on Migration Decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Worker i Displaced in t is Employed in West in $t+1$											
	Baseline	**Placebo**	**Alternative Channels**							**Directional Network Effects**		
		Future	Contact	Additional Connections		Controlling for	Switchers of		Excluding	County	Contact's County	
		West Contacts	at Home	Workplace	Neighborhood	AKM Worker FE	Region	Industry	Border Region	without Contact	Other Employer	Same Employer
GDR coworker employed in West in t	0.22**	0.21**	0.22**	0.24**	0.39**	0.22**	0.45*	0.19*	0.24**	-0.04	0.23***	0.03***
	(0.09)	(0.10)	(0.10)	(0.11)	(0.16)	(0.10)	(0.26)	(0.11)	(0.10)	(0.09)	(0.03)	(0.01)
in $t+x$ with $x \geq 1$		-0.05										
		(0.15)										
Contact at home in t			-0.26**									
			(0.12)									
West contact among GDR coworkers in t from												
same estab. and same 1-digit occ. but different 4-digit occ. in 1989				0.04								
				(0.12)								
same municipality and same 5-year age category in 1989 but different workplace network					0.19							
					(0.16)							
Establishment closure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	246,415	246,415	246,415	246,415	246,415	243,257	46,635	196,398	183,216	246,415	246,415	246,415
Number of establishment closures	46,914	46,914	46,914	46,914	46,914	46,027	11,230	40,566	35,532	46,914	46,914	46,914
Mean of dependent variable	3.6	3.6	3.6	3.6	3.6	3.6	5.8	3.9	3.1	3.2	0.3	0.02
(β_1/Mean Y)*100	6.1%	5.8%	6.1%	6.7%	10.8%	6.1%	7.8%	4.9%	7.7%	-1.2%	67.7%	136.4%
($\beta_{alternative}$/Mean Y)*100			-7.2%	1.1%	5.3%							
p-value of H_0: $\beta_1 = \beta_{alternative}$		0.080	0.002	0.083	0.046							

Notes: This table presents estimation results from estimating average network effects following Equation (2) in the displaced-worker sample. Establishments in Berlin are excluded throughout. For each displaced worker i, the GDR workplace network consists of all former coworkers employed at the same establishment in the same (4-digit) occupation in 1989. The sample is restricted to workers with at most 100 coworkers. In columns 1-8, the outcome variable indicates whether worker i, displaced in year t, is employed in West Germany in $t+1$. In column 9, the binary outcome variable indicates whether worker i is employed in a West German county in $t+1$ where she has no contact. In column 10, the outcome variable indicates whether worker i is employed in the same West German county as a contact in $t+1$ but not at the same employer. In column 11, the outcome variable indicates whether worker i is employed at one of her West contact's employer in $t+1$. The main regressor is the dummy variable $WestContact$, indicating whether at least one of worker i's former GDR coworkers is employed in West Germany in t. All regressions include fixed effects for the establishment-closure event, the worker's (3-digit) occupation in t and in 1989, the industry and federal state in 1989, and the following control variables: a dummy for having a non-employed West contact in t, a dummy for having a contact that already returned to East Germany in t, a female dummy, dummies for three levels of education, tenure, age, age squared, a dummy for being married in 1989, a dummy for care responsibilities in 1989, a dummy for kids in the household in 1989, a dummy for having a part-time job in 1989, and the network size. Standard errors (in parentheses) are clustered at the establishment-closure level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: The Role of the West Contact's Environment

	(1)	(2)	(3)	(4)
Panel A. West Contact's Labor Market Quality	New Job of East-West Migrant i in $t+1$ is in West Contact's County r			
	West Contact's Labor Market Environment relative to			
	all West German Labor Markets	West German Labor Markets in i's Network	all West German Labor Markets	West German Labor Markets in i's Network
GDR coworker employed in West in t is in tight labor market (vs. slack labor market) according to				
county-specific tightness	0.43***	0.27*		
	(0.17)	(0.15)		
county- × occupation-specific tightness			2.33***	1.96***
			(0.12)	(0.10)
Individual FE	Yes	Yes	Yes	Yes
Spatial distance to West contact's employer	Yes	Yes	Yes	Yes
Strength of network tie	Yes	Yes	Yes	Yes
Contact's characteristics in t	Yes	Yes	Yes	Yes
Observations	61,936	61,936	132,211	132,211
Number of East German labor migrants	13,918	13,918	31,760	31,760
Mean of dependent variable	3.32	3.32	3.69	3.69
($\beta_{highQuality}$/Mean Y)*100	13.0%	8.1%	63.1%	53.1%

	(1)	(2)	(3)	(4)	(5)
Panel B. West Contact's Employer Quality	New Job of East-West Migrant i in $t+1$ is at West Contact's Employer k				
	West Contact's Employer Environment relative to			Earnings of West Contact and West Employer relative to	
	all West German Employers	West German Employers in i's Network	Earnings of West Contact	all West German Employers	West German Employers in i's Network
GDR coworker employed in West in t is					
at high-paying employer	0.06**	0.06**		0.06*	0.06**
(vs. low-paying employer)	(0.03)	(0.03)		(0.03)	(0.03)
high-wage worker			-0.05	-0.05	-0.04
(vs. low-wage worker)			(0.03)	(0.03)	(0.03)
Individual FE	Yes	Yes	Yes	Yes	Yes
Spatial distance to West contact's employer	Yes	Yes	Yes	Yes	Yes
Strength of network tie	Yes	Yes	Yes	Yes	Yes
Contact's characteristics in t	Yes	Yes	Yes	Yes	Yes
Observations	147,068	147,068	147,068	147,068	147,068
Number of East German labor migrants	32,245	32,245	32,245	32,245	32,245
Mean of dependent variable	0.25	0.25	0.25	0.25	0.25
($\beta_{highQuality}$/Mean Y)*100	24.0%	24.0%		24.0%	24.0%
p-value of H_0: $\beta_{highQuality} = \beta_{highWage}$				0.011	0.012

Notes: This table presents regression coefficients for estimating Equation (3) in the East-West-pair sample. We exclude contacts in Berlin throughout. In panel A, the outcome variable indicates whether worker i is employed in her West contact c's county r in $t+1$. In columns 1 and 2, the main regressor indicates whether c's *county* is in the upper half of the distribution of labor market tightness among West German counties (column 1) or among counties within i's network (column 2). County-specific labor market tightness (i.e., the ratio of job vacancies to the number of unemployed persons in the county) is only available from 2000 to 2005, which reduces the number of observations. In columns 3 and 4, the main regressor indicates whether c's *occupation-* × *county*-specific tightness is in the upper half of the distribution in West Germany (column 3) or within i's network (column 4). To construct the occupation- × county-specific tightness measure, we allocate occupation-specific labor market tightness across counties based on their average employment share from 1985 to 1989. The occupation-specific tightness measure is unavailable for 2005, which slightly reduces the number of observations. In panel B, the outcome variable indicates whether worker i is employed at her West contact c's employer k in $t+1$. The main regressor indicates whether i's employer is in the upper half of the distribution of AKM establishment effects among West German employers (columns 1 and 4) or among establishments within i's network (columns 2 and 5). Columns 3-5 additionally control for a dummy that equals one if c's wage is above the median wage in their establishment. Besides fixed effects for each East German labor migrant i, we control for a quadratic polynomial in the spatial distance between i's origin establishment in t and West contact c's establishment. We include proxies for the strength of the network tie between i and c, in particular, dummy variables whether i and c share the same gender, lived in the same municipality in 1989, worked in the same occupation in t, their age difference, and the difference in their AKM worker effects. We also control for contact c's age, tenure, and whether their AKM worker effect is above the median of the distribution in West Germany. Standard errors (in parentheses) are clustered at the worker level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The Role of the West Contact's Environment and Similarity

	(1)	(2)	(3)	(4)
	New Job of East-West Migrant i in $t+1$ is in West Contact's County r		New Job of East-West Migrant i in $t+1$ is at West Contact's Employer k	
	West Contact's Similarity	West Contact's Similarity and Labor Market Tightness	West Contact's Similarity	West Contact's Similarity and Employer Environment
GDR coworker employed in West in t has				
higher earnings potential than worker i	-0.11		0.03	
	(0.24)		(0.07)	
× at high-quality labor market or employer		2.05***		0.10
		(0.31)		(0.07)
× at medium-quality labor market or employer		-0.06		0.05
		(0.30)		(0.08)
× at low-quality labor market or employer		-1.04***		0.02
		(0.30)		(0.15)
similar earnings potential as worker i	0.61***		0.11**	
	(0.19)		(0.05)	
× at high-quality labor market or employer		2.68***		0.18***
		(0.29)		(0.06)
× at medium-quality labor market or employer		0.91***		0.15**
		(0.27)		(0.06)
× at low-quality labor market or employer		-0.39		-0.18
		(0.27)		(0.12)
lower earnings potential than worker i (omitted)				
× at high-quality labor market or employer		2.50***		0.09*
		(0.26)		(0.05)
× at medium-quality labor market or employer (omitted)				
× at low-quality labor market or employer		-0.92***		0.06
		(0.22)		(0.09)
Individual FE	Yes	Yes	Yes	Yes
Spatial distance to West contact's employer	Yes	Yes	Yes	Yes
Strength of network tie	Yes	Yes	Yes	Yes
Contact's characteristics in t	Yes	Yes	Yes	Yes
Observations	132,211	132,211	147,068	147,068
Number of East German labor migrants	31,760	31,760	32,245	32,245
Mean of dependent variable	3.69	3.69	0.25	0.25
$(\beta_{higher}$/Mean Y)*100	-3.0%		12.0%	
$(\beta_{similar}$/Mean Y)*100	16.5%		44.0%	
$(\beta_{higher,high}$/Mean Y)*100		55.6%		40.0%
$(\beta_{higher,medium}$/Mean Y)*100		-1.6%		20.0%
$(\beta_{higher,low}$/Mean Y)*100		-28.2%		8.0%
$(\beta_{similar,high}$/Mean Y)*100		72.6%		72.0%
$(\beta_{similar,medium}$/Mean Y)*100		24.7%		60.0%
$(\beta_{similar,low}$/Mean Y)*100		-10.6%		-72.0%
$(\beta_{lower,high}$/Mean Y)*100		67.8%		36.0%
$(\beta_{lower,low}$/Mean Y)*100		-24.9%		24.0%
p-value of H_0: $\beta_{higher} = \beta_{similar}$	0.000		0.117	
p-value of H_0: $\beta_{similar,high} = \beta_{similar,medium}$		0.000		0.625
p-value of H_0: $\beta_{similar,high} = \beta_{similar,low}$		0.000		0.003
p-value of H_0: $\beta_{similar,medium} = \beta_{similar,low}$		0.000		0.007

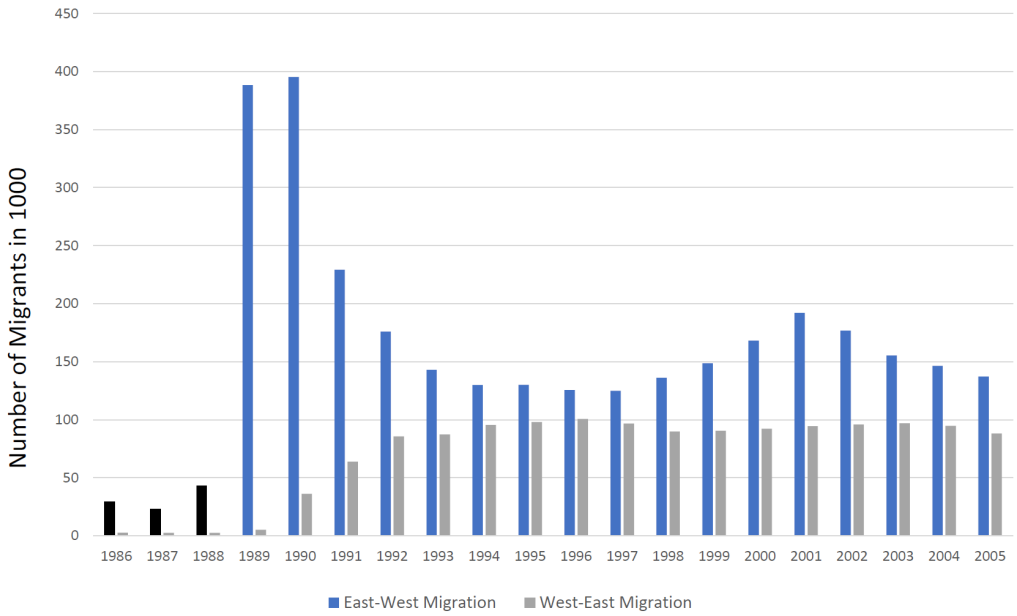
Notes: This table presents regression coefficients for estimating Equation (3) in the East-West-pair sample. We exclude establishments in Berlin throughout. In columns 1 and 2, the outcome variable indicates whether worker i is employed in her West contact c's county r in $t+1$. In columns 3 and 4, the outcome variable indicates whether worker i is employed at her West contact c's employer k in $t+1$. In columns 1 and 2, our main regressors are three groups of similarity between i and c, defined as terciles of the distribution of differences in AKM worker effects. In column 2, we interact the similarity groups with tercile-based categories of county-× occupation-specific labor market tightness, constructed from the distribution of tightness in West German counties. In column 4, we interact dummies for three groups of employer quality (AKM establishment effect above the median of the West German distribution; AKM establishment effect below the West German median but above the East German median; AKM establishment effect below the East German median) with the three similarity groups. Besides fixed effects for each East German labor migrant i, we control for a quadratic polynomial in the spatial distance between i's origin establishment in t and West contact c's establishment. We include proxies for the strength of the network tie between i and c, in particular, dummy variables whether i and c share the same gender, lived in the same municipality in 1989, worked in the same occupation in t, and their age difference. We also control for contact c's age, tenure, and whether their AKM worker effect is above the median of the distribution in West Germany. We do not control for the level of the difference in AKM worker effects between worker i and c. Standard errors (in parentheses) are clustered at the worker level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

A The Former Inner-German Border as a Hurdle to Job Mobility

After an initial upsurge after the fall of the Berlin Wall in 1989, East-West migration was rather limited (Figure A.1). Net migration to West Germany exceeded 350,000 in 1989 and 1990 but flattened soon after. It resurfaced in the early 2000s but to a much smaller extent. Labor reallocation within East Germany was high after reunification (Findeisen et al., 2021), partly triggered by the extensive privatization program administered by the Treuhandanstalt (Mergele et al., 2025). Although East Germans were frequently forced to switch jobs and locations during this transformation period—and despite significantly higher wages and better employment prospects in West Germany—the vast majority of East Germans did not move to the West.

Figure A.1: Migration between East and West Germany, 1986-2005



Notes: This figure shows the pattern of migration between East and West Germany from 1986 to 2005, separately for flows from East to West Germany (highlighted in blue) and flows from West to East Germany (in grey). The data stems from the Federal Statistical Office of Germany (2004, 2007) and captures the total number of individuals moving. In the analyses, we focus on work-related migration flows based on the GAV-IEB data.

A.1 County-level Analysis of Migration Flows

To illustrate this point, we examine worker mobility at the county level. From the linked GAV-IEB data, we construct a set of county pairs where origin county a is located in East Germany, and destination county $b \neq a$ can be located either in East or West Germany. For each county pair ab, we count all migration flows, $flows_{ab}$, where an East German worker is employed in county a in year t and in county $b \neq a$ in year $t+1$, aggregating all flows between 1992 and 2005. We then estimate the gravity equation

$$\log(flows_{ab}) = \beta_0 + \beta_1 west_b + \beta_2 dist_{ab} + \beta_3 dist_{ab}^2 + \beta_4 city_b + \delta_a + \varepsilon_{ab}, \tag{5}$$

where $west_b$ indicates whether the destination county b is located in West Germany, $dist_{ab}$ measures the distance between counties a and b in kilometers, $city_b$ indicates whether the destination county is an urban area, and δ_a are origin-county fixed effects that capture that some counties are generally more likely to lose workers. Our coefficient of interest is β_1, the difference in flows to destinations in West Germany. We exclude Berlin from the sample as it cannot be unambiguously assigned to East or West Germany.

Column 1 of Table A.1 shows that aggregate flows are roughly 90 percent lower if the destination county is in the West conditional on our controls. Column 2 shows that this result is robust to excluding West German counties further away from the East than the maximum distance between East counties. Our estimates are very similar to those obtained by Heise and Porzio (2022), who use LIAB data to show that for East Germans, the probability of switching across the East-West border is only a twentieth as high as the transition rate within regions. The striking difference in job mobility suggests that spatial frictions prevent workers from migrating to West Germany after reunification, even though there are frequent worker flows of similar distances.

Table A.1: The Effect on East German Migration Flows when the Destination is in West Germany

	(1)	(2)
	Log Aggregate Flows of East German Workers	
	All Counties (excl. Berlin)	Distance Restriction
Destination in West Germany	-0.90***	-0.86***
	(0.0199)	(0.0191)
Distance and distance^2	Yes	Yes
Destination city county	Yes	Yes
Origin county fixed effects	Yes	Yes
Observations	28,809	24,090
Adj. R^2	0.543	0.584

Notes: This table shows regression results from estimating Equation (5). The sample is constructed from the linked GAV-IEB data and includes origin-destination pairs of counties where the origin county is located in East Germany and the destination county can be located either in East or West Germany. For each origin-destination pair, we aggregate all migration flows between 1992 and 2005 where an East German worker works in the origin county in t and the destination county in $t+1$. The outcome variable is the natural logarithm of these aggregated migration flows. Column 1 uses all counties (excluding Berlin, which cannot be unambiguously assigned to East or West). Column 2 restricts the sample by dropping West German destination counties that are more distant to the origin county than the most distant East German destination county. Standard errors in parentheses. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

A.2 Willingness to Migrate, Workplace Connections, and Expectations about Living Conditions

Our main research question asks whether workplace connections can induce more migration "towards opportunity" in West Germany and why this could be the case. First motivation for the importance of former coworkers and their role in reducing hurdles to job mobility comes from an analysis of the German Socio-Economic Panel (GSOEP).[45] We examine the survey wave from 1991 where East Germans were specifically asked whether they could imagine migrating to West Germany. In addition, they were asked to rate the living conditions in East and West Germany. Furthermore, they were asked whether a former coworker, a friend, or a relative was currently living in West Germany.

We examine the sample of East German respondents aged 20-50 who were regularly employed or unemployed in 1991, excluding specific subgroups (e.g., pensioners, civil servants, the self-employed, etc.). We regress migration intentions and the rating of living conditions on a dummy for having a former coworker in West Germany. Table A.2 shows our results. The dependent variable in columns 1 and 2 is a dummy that indicates whether i could imagine moving to West Germany. In column 3, the outcome indicates whether i ranks the living conditions in West Germany higher than in East Germany. Each regression includes fixed effects for respondent i's federal state of residence, occupation, wage quartile, 10-year age category, and education level.[46] We also include controls for gender, marital status, part-time employment, experience squared, binary variables which indicate whether i has relatives in East Germany, relatives in West Germany, friends in West Germany, and categorical variables how i rates his or her worries about job security and the general economic development.

Column 1 shows that having a former GDR coworker in West Germany has, on average, a significantly positive effect on the willingness of moving to West Germany. The estimated effect corresponds to about 29% relative to the mean of the dependent variable. This finding raises the question of the underlying mechanism: How do former coworkers affect the respondents' willingness to move?

In column 2, we investigate whether the effect of having a coworker in West Germany on the respondent's willingness of moving to the West could be mediated through altering her perception of the general living conditions in West Germany relative to East Germany. The estimated coefficient suggests that ranking the West higher compared to the East, conditioning on having a GDR coworker in West Germany, significantly increases the willingness of moving to the West by approximately 25%. While the effect size of having a former coworker in the West is similar, it significantly decreases by one percentage point in all relevant mediation test statistics compared to column 1.[47] This finding suggests, that part of the West coworker effect on the willingness to move is conveyed indirectly through mediation. The proportion of the *total effect* of coworkers in the West (in column 1) that is mediated through altering the perception of relative living conditions in the West corresponds to 7.6%. Thus, only a small share of the network effect is mediated through altering general perceptions about the West. As a first stage for mediation, column 3 shows that having a coworker in the West significantly increases the probability of ranking the living conditions in the West higher relative to the East.

Figure A.2 shows how the *total effect* of coworkers in the West on the willingness to move (c) can be decomposed into a direct effect (c') and an indirect effect ($a \times b$), according to the mediation framework. In sum, the analysis suggests partial mediation. However, the small mediation share in the total effect that goes back to altering the *general* perception of the West relative to the East suggests that there might be more mediating variables that are not taken into account. For instance, the network effect

[45] The GSOEP is provided by the German Institute for Economic Research (DIW Berlin) and available to researchers upon application via https://www.diw.de/en/diw_01.c.601584.en/data_access.html. The GSOEP includes East German respondents from 1990 on.

[46] The 1-digit ISCO88 occupation and current net wage quartile indicators include an additional category for nonemployed respondents.

[47] We conduct Sobel, Aroian, and Goodman mediation tests. All of them indicate a significant decrease at the 5% level.

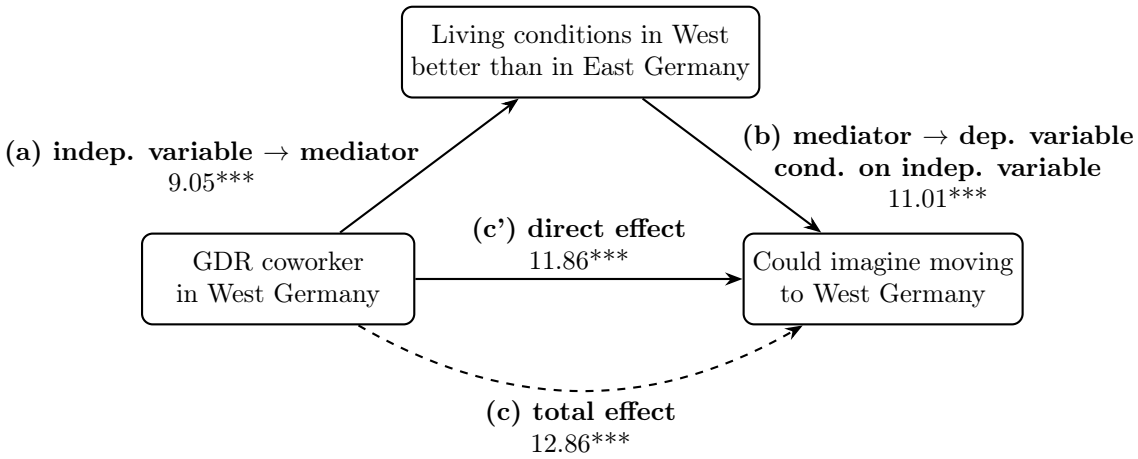
could be mediated through *specific* experiences of former workplace contacts. These results motivate our causal analyses with high-quality administrative data that allow conclusions about the underlying mechanisms.

Table A.2: Mediation Analysis Results

	(1)	(2)	(3)
	Could imagine moving to West Germany		Living conditions West better than East
GDR coworker in West Germany	12.86***	11.86***	9.05***
	(4.53)	(4.51)	(2.19)
Living conditions in West better than in East Germany		11.01***	
		(3.82)	
N	2,209	2,209	2,209
Mean of dependent variable	44.4	44.4	88.9
Relative effect GDR coworker in West	29.0%	26.7%	10.2%
Relative effect mediator		24.8%	

Notes: This table shows mediation analysis results from the 1991 wave of the German Socio-Economic Panel (SOEP). We restrict the cross-sectional data to East German survey participants aged 20 to 50. We exclude pensioners, civil servants, the self-employed, managers and executives, persons on maternity or parental leave, persons in community service, and persons who have never been employed. We estimate the effects using the `sgmediation2` command in Stata, applying Taylor-linearized standard errors. Observations are weighted using the official weights of the GSOEP 1991 wave (`hphrf`). All regressions include fixed effects for the federal state of residence, 1-digit ISCO88 occupation, current net wage quartile, 10-year age category, and education level. We also include controls for gender, marital status, part-time employment, experience squared, a dummy whether i has relatives in East Germany, relatives in West Germany, friends in West Germany, and categorical variables how i evaluates his or her worries about job security and the general economic development. Column 1 shows the effect of having a former GDR coworker in West Germany (*independent variable* on a binary variable indicating whether i could imagine moving to West Germany (*dependent variable*). Column 2 additionally includes a variable which indicates whether i ranks the living conditions in West Germany higher than in East Germany (*mediator variable*). Column 3 shows the estimated effect of having a coworker in the West on the mediator variable. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Figure A.2: Decomposed Effects of Mediation Model



Notes: This figure illustrates the decomposed effects from the estimated mediation model of Table A.2.

B Data

We use rich, novel data that combine two administrative data sets: the *Data Fund of Societal Work Power (GAV)*[48], obtained from the Federal Archives in Germany, and the *Integrated Employment Biographies (IEB)* of the German Federal Employment Agency, provided by the Institute for Employment Research (IAB). The key facts of these data sets are illustrated in Figure B.1.

The GAV data provide information on demographics and labor market characteristics of workers in the German Democratic Republic (GDR) in 1989. The IEB data contain the complete employment and earnings histories of all workers covered by the social security system in the Federal Republic of Germany. The data sets were linked to construct 'Labor Market Trajectories of East Germans around Reunification'—a joint project of the Research Data Centre (FDZ) of the Federal Employment Agency at the IAB and Humboldt-University Berlin. For more details on the initial record linkage, see Liepmann and Müller (2018); for technical documentation of subsequent improvements, see Antoni (2018).

Following the successful linkage at the individual level, we harmonized occupational and regional information across the GAV and IEB data. The resulting dataset enables the analysis of occupational and regional mobility among individuals from the former GDR over time.

B.1 The GAV Data of the Former GDR

The GAV is a large, cross-sectional data set that contains information on the demographics and labor market characteristics of workers in the GDR in 1989. The data were collected by the Government Agency for Labor and Wages (*Staatssekretariat für Arbeit und Löhne*) to inform central planning (Gebauer et al., 2004).[49] More specifically, firms and establishments had to report information on their employees to the district councils. This information formed the basis of the GAV data set. Although the data were compiled annually, only the final version—dated December 31, 1989—has been preserved[50]

The GAV data cover around 7 million workers with permanent or temporary work contracts. They include members of producers' cooperative societies (*Produktionsgenossenschaften*) and law firms (*Rechtsanwaltskollegien*), retired persons still working, and men performing compulsory military service or alternative civilian service (Dietz and Rudolph, 1990; Rathje, 1996; Gebauer et al., 2004). As with most official GDR statistics, the GAV data exclude the so-called *Sector X* which comprises individuals working for the Ministry of the Interior, the Ministry of State Security, the Socialist Unity Party, the army, or customs authorities. Additionally, separate databases existed for specific subgroups, such as certain types of teachers and childcare workers, which are thus only partially included in the GAV.[51] Overall, the data cover about 72 percent of the East German labor force in 1989.

For those workers included in the GAV data, rich information on demographic characteristics (e.g., age, gender, municipality of residence, number of children under 14, disability status, marital status, nationality), human capital (e.g., high school education, current apprenticeship training, university degree), employment characteristics (e.g., type of employment, place of employment, leave of absence, main job task, job status, work hours, occupation), as well as firm characteristics (e.g., firm type and industry) are available.

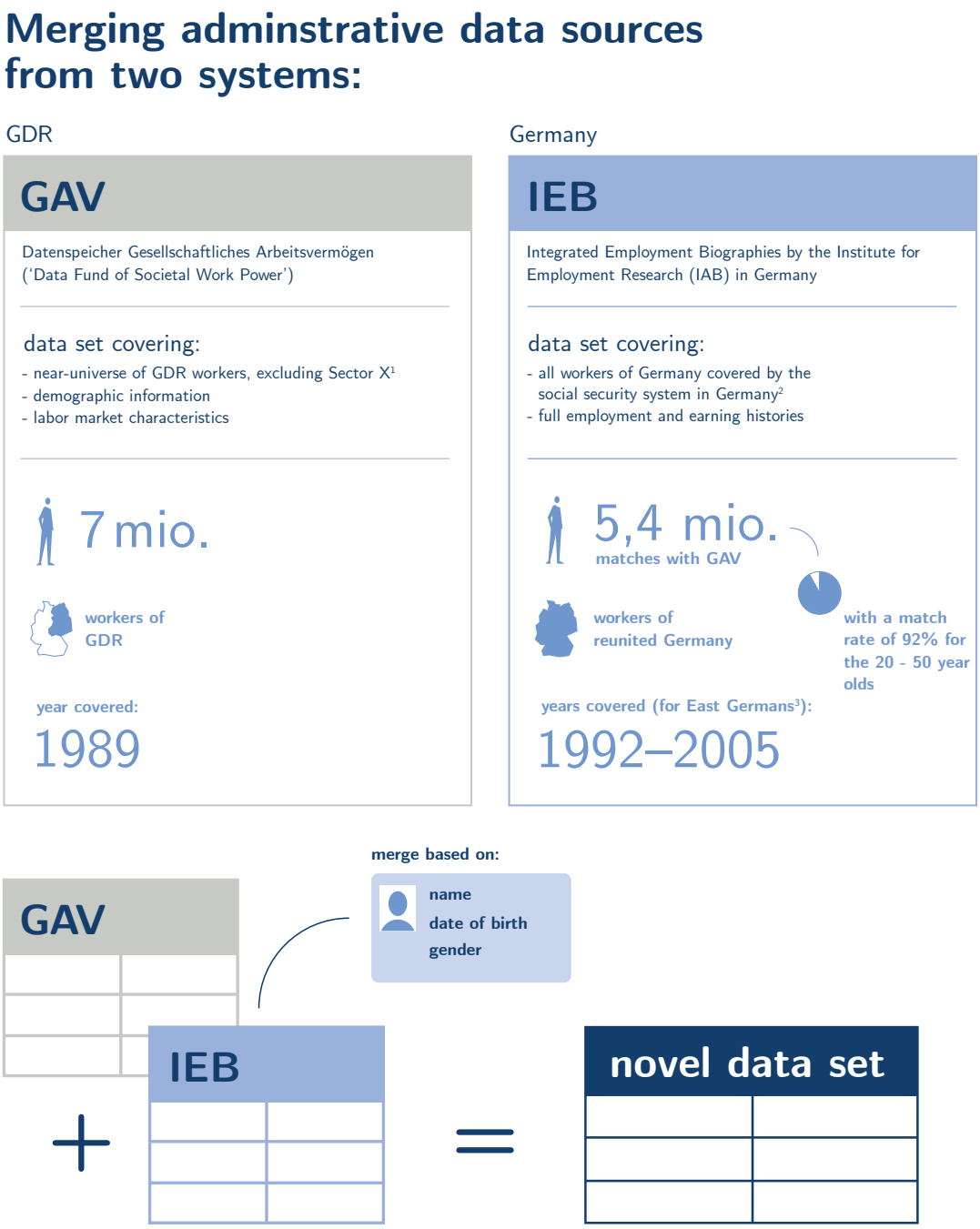
[48] In German: *Datenspeicher Gesellschaftliches Arbeitsvermögen.*

[49] Neither the original GAV data nor analytical results based on these data were made publicly available.

[50] Due to limited computing capacity in the GDR, only data from the current year were kept, whereas data from previous years were deleted (Gebauer et al., 2004).

[51] The data exclude teachers in schools and childcare workers, but include teachers at vocational schools, professors at universities, and employees in nurseries. Moreover, the GAV data exclude the self-employed and their employees. However, as the majority of craftsmen were members of producers' cooperative societies, most are included in the GAV. The data also include apprentices who began their training in the year preceding December 1989. Foreigners temporarily working in the GDR under the coverage of intergovernmental agreements are excluded, while the data include foreign GDR residents.

Figure B.1: Data



1) people working for the Ministry of the Interior, the Ministry of StateSecurity, the Socialist Unity Party, the army, or customs authorities. further excludes school teachers, childcare workers, and self-employed individuals.

2) excluding civil servants, self-employed workers, and regular students

3) East Germans only from 1992 on due to the complex integration process of the East German labor market administration into the West German system.

B.2 IEB Data

The IEB data comprise administrative records on the complete employment and earnings histories of all workers covered by the German social security system. They include information from two sources: social security notifications and internal processes of the Federal Employment Agency. The social security notifications provide detailed information on the start and end dates of employment episodes subject to social security contributions, along with data on gross wages, education, employment status, occupation, and nationality. In addition, every employer liable for social security contributions is required to submit at least one notification annually. The administrative records do not include the self-employed, civil servants, and military personnel.[52] These employment histories are available from 1975 onward for workers in West Germany and from 1992 onwards for workers in East Germany.[53] The data from internal processes of the Federal Employment Agency are organized into four distinct history files covering unemployment spells, benefit receipts, active labor market policy measures, and job-seeking histories of individuals.

B.3 Combined GAV-IEB Data

The linkage between GAV and IEB data was initially conducted by Liepmann and Müller (2018) and then further refined by Antoni (2018). The non-anonymized versions of both data sets were linked through information on first names, last names, exact dates of birth, and gender.[54] Three restrictions were imposed when drawing from the universe of individuals in the IEB data. Only individuals were included (1) who were born between 1929 and 1976, i.e., aged 13 to 70 in 1989; (2) for whom the IEB contains at least one (employment) episode between 1990 and 1996 in East or West Germany; (3) without a recorded (employment) episode in West Germany before 1990. Because of the third restriction, many West Germans are excluded from the linkage procedure, reducing the likelihood of false matches. At the same time, the restriction implies that we neglect individuals who migrated from West to East Germany before the Fall of the Berlin Wall. However, only very few West Germans moved to the GDR during this period.

77 percent of individuals from the GAV data have a match in the IEB data. According to experience from previous linkages performed at the Research Data Center of the Federal Employment Agency, this is considered a strong match rate. For most matches (88 percent), the information on first and last names, dates of birth, and gender was identical in the GAV and the IEB data. The remaining fraction was matched using record linkage techniques that tolerate a justifiable margin of error while keeping the likelihood of false matches as small as possible. Only cases with a unique match were retained. The match rate is substantially higher for individuals aged 60 or below in 1989. If older workers are excluded, the match rate increases to 82 percent. With an age limit of 50 in 1989, the match rate rises even further to 86 percent. Furthermore, the match rate is lower for women than for men. For women younger than 60 in 1989, it amounts to 77 percent, around ten percentage points lower than their male counterparts.

The combined GAV-IEB data enable analyses of the labor market trajectories of East Germans around reunification in much more detail than earlier studies.[55] In particular, the data provide each

[52] In 1995, 79.4 percent of all workers in West Germany were covered by social security and were therefore recorded in the data (FEA, 1996).

[53] As the East German labor market administration had to be integrated into the West German administration, data from East Germany are only fully available from 1992 onward.

[54] It would have been possible to rely on additional information regarding occupations, industries, and regions. However, the IEB data are fully available for East Germans only from 1992 onwards. Between 1989 and 1992, a significant fraction of East Germans changed jobs and moved between regions. Hence, using this additional information would likely have oversampled individuals who remained in the same job or region.

[55] Earlier studies on the labor market trajectories of East Germans used, for example, the BASiD data (Emmler and Fitzenberger, 2020), Microcensus (Fuchs-Schündeln and Schündeln, 2009), aggregated unemployment or migration data (Fuchs-Schündeln and Izem, 2012), or the GSOEP (Burda and Hunt, 2001; Hunt, 2006; Fuchs-Schündeln and Izem, 2012; Stauder, 2018; Emmler and Fitzenberger, 2020). The main advantages of our data are the large sample size and rich information on the employment situation in 1989, in combination with the employment history after 1992. In our study,

individual's occupation, industry, place of work, establishment, type of employment, and municipality of residence in 1989, combined with their employment histories from 1992 onwards.

From the raw GAV-IEB data, we select GDR workers aged between 20 and 50 in 1989 and construct a yearly panel in which we observe each individual's labor market status (employed, UI benefit receipt, or unregistered) as of June 30th each year up until 2005. We define both UI benefit receipt and unregistered spells, i.e., individuals unobserved in the social security records in a given year but reappearing in another year, as non-employment spells. Table B.1 shows summary statistics for this **full sample**.

Table B.1: Individual Characteristics in the Full Sample

	(1)	(2)	(3)
	1989 (GAV)	1992 (All)	1992-2005 (Employed)
Share (in %)			
female	46	46	43
lower educated (or missing)	13	11	4
medium educated	79	81	85
higher educated	9	9	11
Average age	35.1	38.1	42.8
	(8.7)	(8.7)	(8.3)
Observations	4,290,085	3,966,065	35,724,219

Notes: This table reports summary statistics for GDR workers aged 20 to 50 in 1989 in the full GAV-IEB sample. Berlin is excluded. Column 1 shows statistics for 1989 from the GDR data (GAV). Column 2 refers to all matched workers who could be identified in the German social security records (IEB) in 1992, including employed and unemployed individuals. Column 3 averages information for all employed workers for our full sample over the period from 1992 to 2005. See Section 3 for further details on the sample.

B.4 Harmonization of Variables between GAV and IEB Data

To study mobility across occupations, industries, and locations, we harmonize these key variables across the GAV and IEB data. Both data sets used different classification systems for occupations, industries, and regions. In addition, extensive local government reorganization after reunification altered the names, boundaries, and number of counties and municipalities between 1989 (the year of the GAV data) and 2017 (the reference year for regional boundaries in the IEB data). In the following, we describe how we harmonized these variables across both data sources.

Occupations The GAV data list occupations according to a GDR classification at the 7-digit level, distinguishing between 3,359 occupations. The IEB data contain occupation codes based on the 1988 occupational classification system at the 3-digit level and the 2010 classification at the 5-digit level. The 2010 occupation codes are thus more granular and more comparable to the GDR occupations. However, they are available only from 2011 onwards.

We proceed in two steps to harmonize the occupational information from the GDR with that in the IEB data. First, we manually transcode the GDR occupations into 2010 occupation codes. Two indepen-

individual-level information on transitions between geographical locations, occupations, industries, and establishments was essential.

dent teams of research assistants transcoded the occupations to ensure the accuracy of the transcoding. Second, we transcode the 2010 codes into the more aggregated 1988 occupation codes using the official correspondence provided by the Federal Employment Agency (FEA, 2011). This procedure reduces the complexity of the occupational classification. It enables us to reliably assign GDR occupations to the coarser system of the occupations in reunified Germany in 1988, at the expense of some detail in the skill level within occupations that is not captured in the 1988 occupation classification system. However, as information at this level of detail is not available in the IEB from 1992 to 2010, we believe that our procedure generates the most robust assignment achievable.

Industries The GAV data provide industries at the 5-digit level according to the *Classification of the Economic Sectors of the GDR (SVWZ)*, comprising 722 economic sectors. The IEB data contain industry codes based on the *Classification of Economic Activities (WZ)* from 1973 and 1993 (WZ73 and WZ93, respectively). We follow Dietz (1990), who provides a conversion table that assigns one (or more) WZ73 industry codes to each industry code of the SVWZ. The WZ73 classification distinguishes between 269 different codes for the economic activity of establishments at the 3-digit level.

Regions Finally, both data sets include information on municipalities or counties. The GAV data specify the municipality (or county) of residence and the location of the employee's establishment based on 1989 boundaries and the location codes used in the GDR. In total, the GAV data contain 7,795 unique locality cells. The IEB data report county of residence and the county in which the employee's establishment is located, respectively. This geographic information in the version of the IEB we used for our analysis referred to territorial boundaries as of 31 December 2017. Several reforms of local government structures in East Germany after reunification reduced the number of municipalities and counties significantly during the 1990s and 2000s. While there were 303 counties in the GDR in 1989, this number had decreased to 77 by 2017. Hence, we reassigned municipalities and counties in the GAV data to the corresponding counties in reunified Germany as of 2017.

For each person in the GAV with information on their municipality of residence or job location in 1989, we assigned the corresponding municipality in 2014 using a municipality-level conversion matrix provided by the IWH Halle (Kauffmann, 2017). We then mapped these municipalities to their respective counties in 2017.

C Sorting – Sensitivity and Simulations

In this section, we provide additional details and sensitivity checks on the decomposition of the variance of estimated AKM worker fixed effects discussed in Section 4.1.

Our main analysis showed that the total variance of AKM worker fixed effects for our sample of East German workers, estimated in the period from 1998-2004, is very similar to the total variance of AKM worker fixed effects for West German men reported in Card et al. (2013) for the period from 1996-2002. Similarly, the share of the overall variance within occupations matches the one in West Germany when we assess the allocation of East German workers to occupations in 1995 - five years after reunification. Quite in contrast, when we assess the allocation of East Germans to occupations in the GDR in 1989, the within-occupation share of the total variance is substantially higher - indicating a lower extent of assortativity in the sorting of workers to occupations in the GDR. We see a very similar pattern when focusing on sorting of East Germans to occupation × establishment cells, i.e., the networks relevant to our main analysis.

Table C.1 shows that our findings are not particular to the time period used for estimation of the AKM worker effects nor due to the time lag between estimating the AKM effects and evaluating the allocation about a decade earlier.

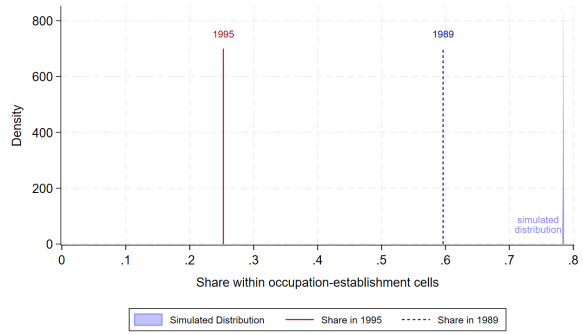
Finally, we compare our variance decompositions to analogous decompositions performed on randomly generated allocations. We proceed in three steps. In the first step, we randomly reshuffle workers across networks without any restrictions. We repeat this exercise 100 times and compute the share of the variance of estimated AKM worker effects within networks for each iteration. Figure C.1a shows the distribution of the within share across all simulations and contrasts it with the observed allocations in 1989 (dashed blue line) and 1995 (solid red line). The allocation in the GDR in 1989 is substantially closer to a random allocation than the allocation in 1995, but at the same time, significantly different from a random allocation. In the second step, we consider only the variance within cells defined by a worker's 2-digit industry in 1989, state in 1989, and 3-digit occupation in 1989. About 80% of the total variance of estimated AKM worker effects is within these cells. We decompose this remaining variance into a within- and between-network component based on both, the allocation in 1989 and the allocation in 1995, and plot the within-network shares in Figure C.1b. Contrasting the observed allocations with the distribution of random allocations (where we restrict the reshuffling to occur within industry × state × occupation cells) shows that conditional on the cell, networks in the GDR are relatively close to a random allocation while networks in 1995 are much more sorted. In the third step, we further restrict the variation to be within cells defined by industry, state, occupation, and demographics. Demographic subgroups are determined by 2-year age categories, three education levels, gender, part-time status in 1989, caregiver status in 1989, marital status in 1989, having kids in 1989, and the size of the network in 1989. About 20% of the total variance of estimated AKM worker effects are within these cells, despite their very fine-grained definition. We decompose the remaining variance into a within- and between-network component and plot the within-network shares of the observed allocations in 1989 and in 1995 in Figure C.1c. Contrasting the observed allocations with the distribution of random allocations (where we restrict the reshuffling of workers to occur within the fine-grained cells) shows that – conditional on the observables – the allocation in the GDR cannot be distinguished from a random allocation. Figure C.1d zooms in on the distribution of random allocations and reveals that the observed allocation in 1989 is at the 67th percentile of this distribution. Quite in contrast, the conditional allocation of the same workers in 1995 is sorted substantially more assortatively.

Table C.1: Sorting in the GDR and Reunified Germany

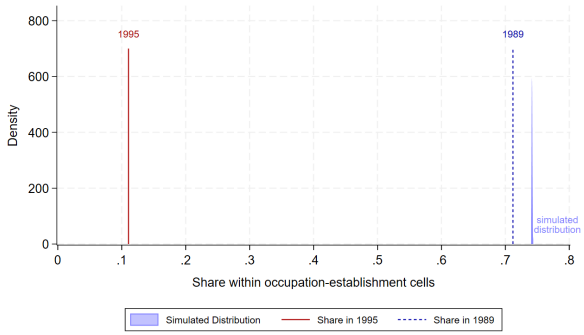
	(1)	(2)	(3)	(4)
	Estimation Period	Total	Share within	
	AKM Effects	Variance	Occupations	Occupation × Establishments
CHK (2013) West German men	1996-2002	0.107	64%	
GDR workers in 1995	1993-1999	0.078	63%	22%
	2003-2010	0.113	69%	27%
GDR workers in 1989	1993-1999	0.085	81%	59%
	2003-2010	0.123	83%	60%

Notes: This table presents sensitivity checks for variance decompositions of estimated AKM worker fixed effects according to Equation (1). Column 1 indicates the period used for estimation of the AKM effects. Column 2 reports the overall variance of AKM worker effects. Column 3 indicates the share of the variance within occupations relative to this overall variance. Columns 4 indicates the share of variance within occupation × establishment cells relative to the overall variance. The first row replicates the results on occupational sorting for West German men after reunification by Card et al. (2013). The subsequent rows present decomposition results for East German men and women in 1995 and 1989, based on AKM worker fixed effects estimated in the indicated period.

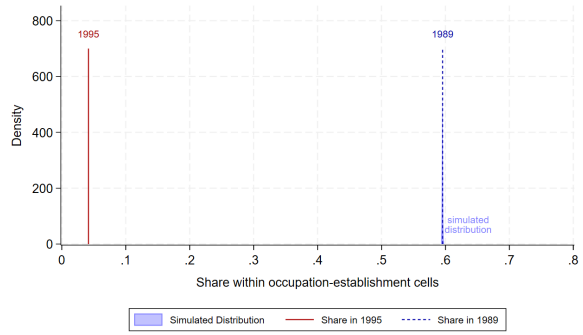
Figure C.1: Simulation Results



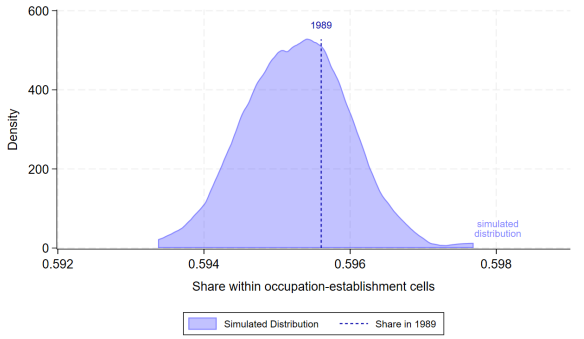
(a) Baseline



(b) Within Industry × State × Occupation Cells



(c) Within Industry × State × Occupation × Demographics Cells



(d) Within Industry × State × Occupation × Demographics Cells - Zoom

Notes: This figure contrasts the results from variance decomposition in our data with simulation results. In each panel, the solid red vertical line indicates the share of the variance within occupation × establishment cells based on the observed allocation in 1995. The dashed blue line indicates the within-share based on the observed allocation in 1989. The distribution of within-shares across 100 randomly generated allocations is shown in solid blue. Panel (a) randomly allocates workers across networks. Panel (b) randomly allocates (and evaluates the within-share) within 2-digit industry × state × 3-digit occupation cells in 1989. Panel (c) randomly allocates within 2-digit industry × state × 3-digit occupation × demographic subgroups, where demographic subgroups are determined by 2-year age categories, three education levels, gender, part time status in 1989, care giver status in 1989, marital status in 1989, having kids in 1989, and the size of the network in 1989. Finally, panel (d) zooms in on the conditional distribution in panel (c).

D Additional Tables

Table D.1: Tests of the As-good-as-random Allocation of GDR Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Panel A. Network Characteristics				**Panel B. Displaced Worker's Characteristics**						
	Unempl. Rate	Empl. Rate	Share Tertiary Educated	Average ln(wage)	Age ≥ Median	Married	Kids	AKM Worker FE	Mobility w.r.t. Region	Occupation	Industry
GDR coworker employed in West in t	0.21*	-0.16	0.03	0.0018	0.05	-0.21	-0.52**	0.0010	0.04	-0.38	0.22
	(0.11)	(0.12)	(0.07)	(0.0011)	(0.16)	(0.22)	(0.25)	(0.0010)	(0.12)	(0.25)	(0.15)
Establishment closure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	244,709	244,709	246,415	238,304	246,415	246,415	246,415	243,257	246,415	246,415	246,415
Number of establishment closures	46,647	46,647	46,914	45,586	46,914	46,914	46,914	46,027	46,914	46,914	46,914
Mean of dependent variable	14.25	79.42	6.67	3.84	48.15	71.59	47.25	4.16	5.82	49.08	66.90
(β_1/Mean Y)*100	1.47%	-0.20%	0.45%	0.05%	0.10%	-0.29%	-1.10%	0.02%	0.69%	-0.77%	0.33%

Notes: This table shows tests for random assignment of coworkers in the GDR. In particular, we show coefficients for the $WestContact$ dummy from estimating Equation (2) in the displaced-worker sample. Establishments in Berlin are excluded throughout. In Panel A, the outcome variables refer to worker i's network characteristics, i.e., the share of her network that received unemployment benefits in 1992 (column 1), the share employed in 1992 (column 2), the share with tertiary education in 1989 (column 3), and the average log wage of network members in 1992 (column 4). Network members in West Germany are excluded from the sample. In Panel B, the outcome variables refer to the East German worker i's characteristics. In particular, we consider indicators for being above median age (column 5), married (column 6), and having children in 1989 (column 7), as well as the AKM worker effect (column 8). Moreover, we consider whether worker i switched federal states, (1-digit) occupations, or (2-digit) industries between 1989 and t (columns 9-11). The full set of controls includes a dummy for having a non-employed West contact in t, a dummy for having a contact that already returned to East Germany in t, a female dummy, and dummies for three levels of education, tenure, age, and age squared, a dummy for being married in 1989, a dummy for care responsibilities in 1989, a dummy for kids in the household 1989, a dummy for having a part-time job in 1989, and the network size. We exclude the respective control variable if it is equivalent to the outcome. Additionally, we include fixed effects for the worker's (3-digit) occupation in t and 1989, fixed effects for the industry and federal state in 1989, and fixed effects for the establishment-closure event. Standard errors (in parentheses) are clustered at the establishment-closure level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.2: Sensitivity Checks - Sample Construction and Treatment Definition

Panel A. Sample Construction	(1)	(2)	(3)	(4)
	Worker i Displaced in t is Employed in West in $t+1$			
	Only Workers Employed in $t+1$	Networks $\leq$ 1000 Workers	All Networks	All Job Switchers
GDR coworker employed in West in t	0.26*	0.17**	0.16**	0.15***
	(0.14)	(0.08)	(0.08)	(0.03)
Establishment closure FE	Yes	Yes	Yes	No
Origin-establishment $\times$ year FE	No	No	No	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes
Observations	146,430	380,276	408,126	1,986,511
Number of establishment closures (origin establishments)	30,099	64,938	67,761	156,651
Mean of dependent variable	5.5	3.7	3.7	3.0
($\beta_{standard}$/Mean Y)*100	4.1%	5.0%	4.4%	5.0%

Panel B. Treatment Definition	(1)	(2)	(3)	(4)	(5)	(6)
	Worker i Displaced in t is Employed in West in $t+1$					
	Baseline	Occupational Information		Quantity		
GDR coworker employed in West in t from same estab. and same 4-digit occ. in 1989	0.22**	0.20**	0.24**			
	(0.09)	(0.10)	(0.11)			
Number of GDR coworker(s) employed in West in t				0.06**		
				(0.03)		
1 employed GDR coworker in West in t					0.19*	
					(0.10)	
2-3 employed GDR coworkers in West in t					0.27**	
					(0.13)	
4-6 employed GDR coworkers in West in t					0.48***	
					(0.18)	
$\geq$ 7 employed GDR coworkers in West in t					0.62**	
					(0.24)	
Share of GDR coworkers employed in West in t						0.78**
						(0.39)
same 3-digit occ. but different 4-digit occ. in 1989		-0.11	-0.07			
		(0.20)	(0.20)			
same 1-digit occ. but different 3- and 4-digit occ. in 1989			0.08			
			(0.13)			
GDR coworker non-employed in West in t	0.13				0.11	
	(0.10)				(0.10)	
Number of GDR coworker(s) non-employed in West in t				0.01		
				(0.06)		
Share of GDR coworkers non-employed in West in t						1.36
						(0.86)
GDR coworker returned in t	0.06				0.05	
	(0.10)				(0.10)	
Number of GDR coworker(s) returned in t				-0.02		
				(0.03)		
Share of GDR coworkers returned to East in t						-0.07
						(0.56)
Establishment closure FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	246,415	246,415	246,415	246,415	246,415	246,415
Number of establishment closures	46,914	46,914	46,914	46,914	46,914	46,914
Mean of dependent variable	3.6	3.6	3.6	3.6	3.6	3.6
($\beta_{dummy\geq 1}$/Mean Y)*100	6.1%	5.6%	6.7%			
(β_{number} * 1/Mean Y)*100				1.7%		
(β_{number}* Std. Dev./Mean Y)*100				4.9%		
(β_{dummy1}/Mean Y)*100					5.3%	
($\beta_{dummy2-3}$/Mean Y)*100					7.5%	
($\beta_{dummy4-6}$/Mean Y)*100					13.3%	
($\beta_{dummy\geq 7}$/Mean Y)*100					17.2%	
(β_{share} * 0.01/Mean Y)*100						0.2%
(β_{share}* Std. Dev./Mean Y)*100						2.4%

Notes: This table examines sensitivity to changes in the sample construction and treatment definition. Panel A estimates the baseline model of Equation (2) applying different sample restrictions. Columns 1-3 refer to the displaced-worker sample. Column 1 restricts the sample to displaced workers who are employed in $t+1$, the year after displacement. Columns 2 and 3 vary the sample by adjusting the cutoff of our standard network definition to at most 1000 coworkers in column 2 and removing the cutoff entirely in column 3. The sample in column 4 refers to all job switchers, i.e., workers employed in East Germany in 1989 who switched jobs from an East German establishment between 1992 and 2005. This column includes establishment $\times$ year fixed effects instead of fixed effects for the establishment-closure event. Panel B examines sensitivity to changes in the treatment definition. In column 1, we estimate the baseline model of Equation (2) in the displaced-worker sample but explicitly show the coefficients of the nonemployed and returned West contact indicators. Columns 2 and 3 add indicators for alternative workplace network definitions. Columns 4-6 replace the main regressor—the dummy variable $WestContact$—with alternative treatment variables that account for the quantity of West contacts. Standard errors (in parentheses) are clustered at the establishment-closure (origin-establishment in panel A column 4) level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.3: Sensitivity Checks - The Effect of Contacts at the Origin

	(1)	(2)	(3)	(4)
	Worker i Displaced in t is Employed in West in $t+1$			
GDR coworker employed in West in t	0.22**	0.24**	0.19**	0.22**
	(0.10)	(0.10)	(0.10)	(0.10)
Contact at home in t	-0.26**			
	(0.12)			
Network share still employed in				
East Germany in t		0.13		
		(0.30)		
same region as worker i in t			-0.36**	
			(0.16)	
same region as worker i in 1989				-0.01
				(0.16)
Establishment closure FE	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes
Observations	246,415	246,415	246,415	246,415
Number of establishment closures	46,914	46,914	46,914	46,914
Mean of dependent variable	3.6	3.6	3.6	3.6
(β_1/Mean Y)*100	6.1%	6.7%	5.3%	6.1%
($\beta_{contact_at_home}$/Mean Y)*100	-7.2%			
($\beta_{share_East} * 0.01$/Mean Y)*100		0.04%		
(β_{share_East}* Std. Dev./Mean Y)*100		0.64%		
($\beta_{share_region_t} * 0.01$/Mean Y)*100			-0.10%	
($\beta_{share_region_t}$* Std. Dev./Mean Y)*100			-2.86%	
($\beta_{share_region_1989} * 0.01$/Mean Y)*100				-0.003%
($\beta_{share_region_1989}$* Std. Dev/Mean Y)*100				-0.077%

Notes: This table examines sensitivity to changes in the specification of the effect of network contacts remaining in East Germany. We estimate Equation (2) in the displaced-worker sample. The outcome, control variables, and FE correspond to the baseline specification. Column 1 repeats column 3 of Table 3. Column 2 uses the share of all former GDR coworkers still employed in East Germany instead. Column 3 uses the share of all former GDR coworkers employed in the same region as displaced worker i at the time of displacement t. Column 4 uses the share of all former GDR coworkers still employed in the same region as 1989. Standard errors (in parentheses) are clustered at the establishment-closure level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.4: Further Evidence Supporting Occupation-specific Information

	(1)	(2)	(3)	(4)
Outcome: New Job of East-West Migrant i in $t+1$ is in West Contact's County r and Occupation o but at Different Employer k				
Panel A. West Contact's Labor Market Quality	West Contact's Labor Market Environment relative to			
	all West German Labor Markets	West German Labor Markets in i's Network	all West German Labor Markets	West German Labor Markets in i's Network
GDR coworker employed in West in t is in tight labor market (vs. slack labor market) according to				
county-specific tightness	0.11	0.05		
	(0.07)	(0.06)		
county- × occupation-specific tightness			0.38***	0.30***
			(0.05)	(0.04)
Observations	61,936	61,936	132,211	132,211
Number of East German labor migrants	13,918	13,918	31,760	31,760
Mean of dependent variable	0.51	0.51	0.65	0.65
($\beta_{highQuality}$/Mean Y)*100	21.6%	9.8%	58.5%	46.2%
Panel B. West Contact's Similarity and Labor Market Quality		West Contact's Similarity	West Contact's Similarity and Labor Market Tightness	
GDR coworker employed in West in t has				
higher earnings potential than worker i		-0.01		
		(0.11)		
× at high-quality labor market			0.24*	
			(0.13)	
× at medium-quality labor market			0.00	
			(0.12)	
× at low-quality labor market			-0.05	
			(0.12)	
similar earnings potential as worker i		0.21***		
		(0.08)		
× at high-quality labor market			0.73***	
			(0.13)	
× at medium-quality labor market			0.08	
			(0.11)	
× at low-quality labor market			0.08	
			(0.11)	
lower earnings potential than worker i (omitted)				
× at high-quality labor market			0.35***	
			(0.11)	
× at medium-quality labor market (omitted)				
× at low-quality labor market			-0.10	
			(0.08)	
Observations		132,211	132,211	
Number of East German labor migrants		31,760	31,760	
Mean of dependent variable		0.65	0.65	
(β_{higher}/Mean Y)*100		-1.5%		
($\beta_{similar}$/Mean Y)*100		32.3%		
($\beta_{higher,high}$/Mean Y)*100			36.9%	
($\beta_{higher,medium}$/Mean Y)*100			0%	
($\beta_{higher,low}$/Mean Y)*100			-7.7%	
($\beta_{similar,high}$/Mean Y)*100			112.3%	
($\beta_{similar,medium}$/Mean Y)*100			12.3%	
($\beta_{similar,low}$/Mean Y)*100			12.3%	
($\beta_{lower,high}$/Mean Y)*100			53.8%	
($\beta_{lower,low}$/Mean Y)*100			-15.4%	

Notes: This table presents regression coefficients for estimating Equation (3) in the East-West-pair sample. We exclude contacts in Berlin throughout. In both panels, the outcome variable indicates whether worker i is employed in her West contact c's county r and occupation o in $t+1$ but at a different employer k. In panel A, the main regressor in columns 1 and 2 indicates whether c's *county* is in the upper half of the distribution of labor market tightness among West German counties (column 1) or among counties within i's network (column 2). County-specific labor market tightness (i.e., the ratio of job vacancies to the number of unemployed persons in the county) is only available from 2000 to 2005, which reduces the number of observations. In columns 3 and 4, the main regressor indicates whether c's *occupation-* × *county*-specific tightness is in the upper half of the distribution in West Germany (column 3) or within i's network (column 4). To construct the occupation- × county-specific tightness measure, we allocate occupation-specific labor market tightness across counties based on their average employment share from 1985 to 1989. The occupation-specific tightness measure is unavailable for 2005, which slightly reduces the number of observations. In column 1 of panel B, our main regressors are three groups of similarity between i and c, defined as terciles of the distribution of differences in AKM worker effects. In column 2, we interact the similarity groups with tercile-based categories of county-× occupation-specific labor market tightness, constructed from the distribution of tightness in West German counties.
Besides fixed effects for each East German labor migrant i, we control for a quadratic polynomial in the spatial distance between i's origin establishment in t and West contact c's establishment. We include proxies for the strength of the network tie between i and c, in particular, dummy variables whether i and c share the same gender, lived in the same municipality in 1989, worked in the same occupation in t, their age difference, and the difference in their AKM worker effects. We do not control for the latter in panel B. We also control for contact c's age, tenure, and whether their AKM worker effect is above the median of the distribution in West Germany. Standard errors (in parentheses) are clustered at the worker level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.5: Sensitivity Checks - The Role of the Labor Market Environment

Panel A: Definitions	(1)	(2)	(3)	(4)	(5)	(6)
	New Job of East-West Migrant i in $t+1$ is at West Contact's Employer k					
	Baseline	Alternative Cutoff Values		Alternative Quality Measures		
	Median	75th pct	75th; 50th pct	Growing Estab.	Inv. Churning Rate	Poaching Index
GDR coworker is employed in West in t in establ. k of						
high quality (vs. low quality)	0.06**	0.11***	0.12***	0.08***	0.05*	0.11***
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
medium quality (vs. low quality)			0.02			
			(0.04)			
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial distance to West contact's employer	Yes	Yes	Yes	Yes	Yes	Yes
Strength of network tie	Yes	Yes	Yes	Yes	Yes	Yes
Contact's characteristics in t	Yes	Yes	Yes	Yes	Yes	Yes
Observations	147,068	147,068	147,068	147,068	147,068	147,068
Number of East German labor migrants	32,245	32,245	32,245	32,245	32,245	32,245
Mean of dependent variable	0.25	0.25	0.25	0.25	0.25	0.25
(β_{high}/Mean Y)*100	24%	44%	48%	32%	20%	44%
(β_{medium}/Mean Y)*100			8%			

Panel B. Establishment-closure Design	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Worker i Displaced in t is Employed in West in $t+1$						
	West Contact's Work Environment						
	Labor Market Tightness		Employer Quality				
	County	County × Occupation	AKM Estab. FE		Growing Estab.	Inv. Churning Rate	Poaching Index
GDR coworker in the West in t is							
employed in high-quality labor market	0.46*	0.29**					
	(0.26)	(0.12)					
employed in low-quality labor market	0.32	0.13					
	(0.21)	(0.10)					
employed at high-quality employer			0.30**	0.29**	0.27**	0.30**	0.24*
			(0.12)	(0.14)	(0.11)	(0.12)	(0.13)
employed at low-quality employer			0.17	0.15	0.15	0.18*	0.21**
			(0.11)	(0.12)	(0.11)	(0.10)	(0.10)
high-wage worker				0.02			
				(0.12)			
Establishment closure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	75,992	239,022	239,356	239,356	239,356	239,356	239,356
Number of establishment closures	17,042	45,190	45,806	45,806	45,806	45,806	45,806
Mean of dependent variable	5.01	3.58	3.59	3.59	3.59	3.59	3.59
(β_{high}/Mean Y)*100	9.2%	8.1%	8.4%	8.1%	7.5%	8.4%	6.7%
(β_{low}/Mean Y)*100	6.4%	3.6%	4.7%	4.2%	4.2%	5.0%	5.8%

Notes: This table examines the sensitivity of results on the role of the West contact's employer and labor market quality. Panel A estimates Equation (3) in the East-West-pair sample. We exclude contacts in Berlin throughout. Control variables and FE correspond to those in Table 4, while we vary the definition of the *HighQuality* indicator. In columns 1 and 2, a high-quality employer has an AKM establishment FE above the median (baseline) or the 75th percentile of the distribution of all West German establishments with GDR coworkers. Column 3 distinguishes three categories: high-quality employers (AKM establishment FE above the 75th percentile), medium-quality employers (between the 50th and 75th percentile), and low-quality employers (below the median). In columns 4-6, we apply alternative measures of employer quality. In column 4, high-quality establishments are those that grow. In columns 5 and 6, high-quality employers have an inverse churning rate (=# employees/(# leavers + # hires)) or poaching index (=# new hires directly from other establishments /# hires from unemployment) above the median of the West distribution. Standard errors (in parentheses) are clustered at the worker level. Panel B estimates Equation (2) in the displaced-worker sample. We split the dummy variable *WestContact* into two groups: Having a West contact in a high-quality or low-quality work environment. Both coefficients are interpreted relative to the effect of having no West contact. In column 1, high-quality local labor markets are counties with labor market tightness (i.e., the ratio of job vacancies to the number of unemployed persons in the county) above the median of the distribution among all West German counties with former GDR coworkers in year t. County-specific unemployment levels and job vacancies are only available from 2000 to 2005. In column 2, high-quality labor markets refer to county- × occupation-specific labor market tightness above the median. To construct this measure, we allocate occupation-specific labor market tightness across counties based on their average employment share from 1985 to 1989. The occupation-specific tightness measure is unavailable for 2005, which slightly reduces the number of observations. In columns 1 and 2, we use the maximum labor market tightness among displaced worker i's West contacts. In column 3, high-quality employers are those with an AKM establishment FE above the median of the distribution among all West German establishments with former GDR coworkers. We use the maximum AKM establishment FE among displaced worker i's West contacts to define the quality indicator. Column 4 adds an additional control variable that indicates whether at least one West contact receives a real wage in year t above their employer's median. We apply alternative employer quality measures in columns 4-6 (see above). Standard errors (in parentheses) are clustered at the establishment-closure level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.6: Sensitivity Checks - The Role of Similarity

	(1)	(2)	(3)	(4)	(5)
	New Job of East-West Migrant i in $t+1$ is at West Contact's Employer k				
	Baseline	Alternative Cutoff Values		Alternative Similarity Measure: Highest Educational Degree	
	Median	25th pct	75th pct		West Contact's Employer Environment
GDR coworker employed in West in t has					
higher earnings potential than worker i				0.15*	
				(0.09)	
× at high-quality employer	0.10	0.09	0.10		0.25**
	(0.07)	(0.07)	(0.08)		(0.10)
× at medium-quality employer	0.05	0.04	0.05		0.02
	(0.08)	(0.07)	(0.08)		(0.11)
× at low-quality employer	0.02	0.01	0.02		-0.17
	(0.15)	(0.06)	(0.10)		(0.21)
similar earnings potential as worker i				0.14***	
				(0.05)	
× at high-quality employer	0.18***	0.17***	0.18***		0.17***
	(0.06)	(0.06)	(0.06)		(0.06)
× at medium-quality employer	0.15**	0.12**	0.15**		0.13**
	(0.06)	(0.06)	(0.07)		(0.06)
× at low-quality employer	-0.18	0.02	0.04		0.06
	(0.12)	(0.10)	(0.09)		(0.10)
lower earnings potential than worker i (omitted)					
× at high-quality employer	0.09*	0.08*	0.09*		0.04
	(0.05)	(0.05)	(0.05)		(0.07)
× at medium-quality employer (omitted)					
× at low-quality employer	0.06	0.05	0.02		-0.16
	(0.09)	(0.12)	(0.06)		(0.11)
Individual FE	Yes	Yes	Yes	Yes	Yes
Spatial distance to West contact's employer	Yes	Yes	Yes	Yes	Yes
Strength of network tie	Yes	Yes	Yes	Yes	Yes
Contact's characteristics in t	Yes	Yes	Yes	Yes	Yes
Observations	147,068	147,068	147,068	147,068	147,068
Number of East German labor migrants	32,245	32,245	32,245	32,245	32,245
Mean of dependent variable	0.25	0.25	0.25	0.25	0.25
($\beta_{high,similar}$/Mean Y)*100	72%	68%	72%		68%
($\beta_{medium,similar}$/Mean Y)*100	60%	48%	60%		52%
($\beta_{similar}$/Mean Y)*100				56%	

Notes: This table examines the sensitivity of results on the role of similarity between East and West contacts. We estimate Equation (3) in the East-West-pair sample. We exclude establishments in Berlin throughout. Control variables and FE correspond to those in Table 5. Column 1 repeats the baseline results. Columns 2 and 3 use alternative cutoff values for the definition of high-paying establishments, in particular, the 25th percentile and the 75th percentile of the distribution of AKM establishment effects in West German establishments. In columns 4 and 5, the similarity indicator is defined by the difference in education levels as an alternative measure; employer quality in column 5 corresponds to the baseline of column 1. We control for the maximum AKM worker FE difference between worker i and her contact(s) at the potential destination establishment in both columns. Standard errors (in parentheses) are clustered at the worker level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D.7: The Effect of Having a West Contact on Starting Wages and Turnover

	(1)	(2)	(3)	(4)	(5)
	East-West Migrant is at Same West Employer in		East-West Migrant's $ln(wage_{t+1})$	East-West Migrant's Wage Growth Conditional on Staying at the Same West Employer	
	$t+2$	$t+3$		$ln(wage_{t+2}) - ln(wage_{t+1})$	$ln(wage_{t+3}) - ln(wage_{t+2})$
GDR coworker employed in West in t	-1.13	-0.44	0.006	-0.006	0.0001
	(0.93)	(0.97)	(0.0059)	(0.0059)	(0.0078)
Destination-establishment FE ($t+1$)	Yes	Yes	Yes	Yes	Yes
Origin-establishment × year FE	Yes	Yes	Yes	Yes	Yes
Occupation FE (t)	Yes	Yes	Yes	Yes	Yes
Occupation FE (1989)	Yes	Yes	Yes	Yes	Yes
Industry FE (1989)	Yes	Yes	Yes	Yes	Yes
State FE (1989)	Yes	Yes	Yes	Yes	Yes
Individual characteristics (t & 1989)	Yes	Yes	Yes	Yes	Yes
Observations	16,664	16,664	16,621	8,591	5,877
(β_1/Mean Y)*100	-1.90%	0.98%	0.14%		
Mean of dependent variable	59.46	44.85	4.1785		

Notes: The sample is based on the linked GAV-IEB data and comprises all East-West migrants who switched from an East employer in t to a West employer in $t+1$. The outcome variables in columns 1 and 2 indicate whether East-West migrant i is still employed at the same West employer in $t+2$ and $t+3$, respectively. In column 3, we use the East-West migrant's $ln(wage)$ in starting wage in $t+1$ as an outcome variable. The outcome in column 4 corresponds to the migrant's wage growth from $t+1$ to $t+2$ and from $t+1$ to $t+3$ in column 5. Wage growth is calculated conditional on staying at the same West employer. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

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