THE ECONOMIC IMPACT OF SOCIAL TIES: EVIDENCE FROM GERMAN REUNIFICATION*

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We use the fall of the Berlin Wall in 1989 to show that personal relationships which individuals maintain for noneconomic reasons can be an important determinant of regional economic growth. We show that West German households who had social ties to East Germany in 1989 experienced a persistent rise in their personal incomes after the fall of the Berlin Wall. Moreover, the presence of these households significantly affects economic performance at the regional level: it increases the returns to entrepreneurial activity, the share of households who become entrepreneurs, and the likelihood that firms based within a given West German region invest in East Germany. As a result, West German regions that (for idiosyncratic reasons) have a high concentration of households with social ties to the East exhibit substantially higher growth in income per capita in the early 1990s. A one standard deviation rise in the share of households with social ties to East Germany in 1989 is associated with a 4.7 percentage point rise in income per capita over six years. We interpret our findings as evidence of a causal link between social ties and regional economic development. *JEL* Codes: O10, O43, J61, L14, F20.

I. INTRODUCTION

A large literature on social networks and social capital argues that social ties between individuals may be an important determinant of economic performance at the macroeconomic level and at the microeconomic level (Loury 1977; Granovetter 1985, 2005). Individuals who have valuable social ties may generate a competitive advantage for the firms at which they work and the

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regions in which they live—for example, by providing "social" collateral for economic transactions or by reducing informational frictions (Coleman 1988; Greif 1993; Stiglitz 1990).[1] Social ties that are maintained for noneconomic reasons may thus affect the economic development of entire geographic regions.

Saxenian (1999) gives an example of this view. She analyzes the biographies of Indian engineers who migrated to California in the 1970s. Following the liberalization of the Indian economy in 1991, these immigrants were in a position to leverage their social ties to relatives and friends in Hyderabad and Bangalore. Many excelled in their personal careers, managing outsourcing operations for U.S. firms. Saxenian argues that by connecting Silicon Valley firms to low-cost and high-quality labor in their regions of origin, these immigrants became instrumental in the emergence of their home regions as major hubs of the global information technology services industry.

While there is compelling empirical evidence of the effect of social ties on a range of microeconomic outcomes, estimating their effect on aggregate outcomes poses additional difficulties. The reason is that social ties are likely to be nonrandom across and within regions. Individuals with social ties may have common unobserved characteristics, sort endogenously across regions, or form social ties in anticipation of future economic benefits (Manski 1993; Glaeser, Laibson, and Sacerdote 2002). Identifying a causal link between social ties and aggregate economic outcomes thus requires exogenous variation in the economic value or in the formation of social ties across regions. In general, and in the Indian example in particular, such exogenous variation either does not exist or cannot be measured.

In this article we argue that the fall of the Berlin Wall provides a unique historical setting that enables us to overcome this identification problem and estimate the causal effect of social ties between individuals living in different regions on regional economic growth. In addition, we are able to trace this effect on regional economic growth to its microeconomic underpinnings by documenting a causal effect of social ties on entrepreneurial activity, the investment behavior of firms, and household income.

The first key advantage of this setting is the fact that the partition of Germany was generally believed to be permanent. After the physical separation of the two German states in 1961,

1. Also see Varian (1990) and Besley and Coate (1995).

private economic exchange between them was impossible. West Germans maintaining social ties with East Germans during this period did so for purely noneconomic reasons. After the fall of the Berlin Wall on November 9, 1989, trade between the two Germanies suddenly became feasible. To the extent that social ties facilitate economic exchange, social ties between West and East Germans thus unexpectedly took on economic value on the day of the fall of the Berlin Wall, which we are able to measure in the data. Indeed, West Germans who reported being in contact with friends or relatives in East Germany in 1989 experienced a significant increase in the growth rate of their personal income after the fall of the Berlin Wall.

The second key advantage of the natural experiment surrounding the fall of the Berlin Wall is that the idiosyncrasies of Germany's postwar history resulted in substantial variation across West German regions in the fraction of households with social ties to East Germany. In 1945 all German residents were expelled from Pomerania, Silesia, and East Prussia (which all became part of Poland or the Soviet Union) and allocated to the areas that later became West and East Germany according to quotas fixed in the Potsdam Agreement. Between the founding of the East German state in 1949 and the construction of the Berlin Wall in 1961, the vast majority (2.8 million) of expellees allocated to East Germany migrated to the West after having lived in East Germany for up to 16 years. During the same period, an additional 3 million refugees who had lived in East Germany before World War II also fled to West Germany. We show that West German regions that received a large inflow of these two groups of migrants from East Germany (henceforth "the East") had significantly stronger social ties to the East in 1989.

Of course, the assignment of migrants to West German regions might not be random, because individuals may have strategically settled in those regions in which they saw the best prospects for long-term economic growth. However, an overwhelming concern for those arriving from the East was an acute lack of housing. During World War II, almost a third of the West German housing stock was destroyed. In some areas only 4.4% of the housing stock that existed in 1939 was habitable in 1946. Variation in wartime destruction thus made it more difficult to settle in some parts of West Germany than in others at the time when millions of migrants arrived from the East. We argue that the extent of destruction in 1946 provides the

exogenous source of variation in the regional distribution of social ties that we need to identify a causal effect of social ties on regional economic outcomes post-1989.

Using the degree of wartime destruction in 1946 as an instrument, we show a strong relationship between the share of migrants from the East settling in a given West German region, the share of households reporting social ties to the East in 1989, and changes in the growth rate of income per capita post-1989. A one standard deviation rise in the share of migrants from the East settling in a given West German region before 1961 is associated with a 4.1 percentage point rise in income per capita over the six years between 1989 and 1995.

The main identifying assumption for the causal interpretation of our region-level results is that the degree of wartime destruction in 1946 (or any omitted factors driving it) affects changes in the growth rate of income per capita post-1989 only through its effect on the settlement of migrants from East Germany after World War II, and that the presence of these migrants indeed affects growth post-1989 exclusively due to their social ties to East Germany. We corroborate this identifying assumption in various ways. For example, we show that wartime destruction has no effect on the growth rate of income per capita in West German regions in the years before 1989. Moreover, we show that our results are particular to migrants arriving from East Germany and not to migrants arriving directly from the areas that became part of Poland and the Soviet Union.

In an effort to shed light on the mechanism linking social ties to regional economic growth, we trace the effects of social ties to the behavior of entrepreneurs and firms. While both entrepreneurs and nonentrepreneurs who live in regions with strong social ties to the East experience a rise in their incomes after 1989, the incomes of entrepreneurs increase at a significantly higher rate. Moreover, the share of the population engaged in entrepreneurial activity rises in regions with strong social ties to the East. Consistent with this increase in entrepreneurial activity, West German firms that are headquartered in regions with strong social ties to the East were more likely to invest in East Germany between 1989 and 2007. While social ties to the East predict investment in the East, they do not predict investment anywhere else in the world, except for a small effect on investment in Poland and the Czech Republic. This latter finding is notable because about half of those arriving in West Germany

from East Germany before 1961 were expelled from areas in present-day Poland and the Czech Republic.

Using household-level data, we show that individual households who had social ties to East Germany in 1989 internalized part of the income growth they generated at the regional level. The income growth of households who had ties to at least one relative in the East was on average 6 percentage points higher in the six years following the fall of the Berlin Wall than that of comparable households who had no such ties.

We relate the effects of social ties on household- and region-level income growth using a model in which household income is a function of direct social ties to the East and indirect (higher order) social ties to the East, where individual households may benefit from having friends who have ties to the East, even if they have no such ties themselves. Our preferred estimates imply that, other things equal, a direct social tie to the East has the same effect on individual household income as a 49.3 percentage point (or 3.5 standard deviation) increase in the regional share of households who have such ties. From the perspective of an individual household, the incremental benefit from a direct social tie to the East is thus large compared to the incremental benefit from higher order social interaction. Nevertheless, indirect social ties to the East account for two-thirds of the aggregate effect because all of a region's households benefit from indirect social ties, whereas only a subset of the population benefits from direct social ties.

We believe the most plausible interpretation of our results is that West German households that had direct or indirect social ties to East Germany in 1989 had a comparative advantage in seizing the new economic opportunities in the East. Personal relationships with East Germans may have given them access to valuable information about local demand conditions or about the quality of East German assets that were offered to investors. (Almost the entire East German capital stock was sold to private investors between 1990 and 1994.) This comparative advantage resulted in a persistent rise in their household incomes but also appears to have generated growth in income per capita and increased returns to entrepreneurial activity at the regional level. Part of this effect on regional economic performance may be explained if firms owned by a household with social ties to the East (or firms with access to a local labor force with such ties) had a comparative advantage in investing in East Germany.

A large body of literature shows that measures of affinity between regions, such as trust, telephone volume, and patterns of historical migration, correlate strongly with aggregate economic outcomes, such as foreign direct investment (Guiso, Sapienza, and Zingales 2009), international asset flows (Portes and Rey 2005), and trade (Rauch and Trindade 2002). Relative to this literature, we innovate in three dimensions. First, we are able to establish causation rather than correlation. Second, we are able to examine economic growth as an outcome variable directly. Third, we are able to tie our aggregate measure of affinity directly to individual level data about personal relationships within an environment in which we can exclude differences in preferences, culture, and language as alternative explanations with a high degree of certainty. Our work thus provides evidence that social ties between individuals can causally affect macroeconomic outcomes, which is a major theme in the literature on social capital (Burt 1992; Putnam 1993) and a prediction of a wide range of models that feature network-based economic interaction.[2]

By tracing the effect of social ties on regional economic growth to household income, entrepreneurial activity, and firm investment, this article also provides evidence on the microeconomic channels through which social ties may affect economic growth. In this sense, our results relate to a large empirical literature that links social networks and social ties to a broad set of microeconomic outcomes.[3]

Because the patterns of social ties that we identify are driven by an internal migration post–World War II, our article also relates to the literature on the economic effects of migration (e.g., Friedberg 2001). We add to this literature by providing evidence of a distinct channel through which large-scale migration may affect long-run economic growth.

Other papers have used German reunification as a testing ground for economic theory. Most closely related are Redding and

2. See for example Rauch (1999), Kranton and Minehart (2001), Calvo-Armengol and Jackson (2004), Karlan et al. (2009), Ambrus, Mobius, and Szeidl (2010), Chaney (2011), and Jackson, Rodriguez-Barraquer, and Tan (2011).

3. These range from education (Sacerdote 2001) and employment (Munshi 2003) to performance in the financial industry (Cohen, Frazzini, and Malloy 2008) and agricultural yields (Conley and Udry 2010). Also see Bertrand, Luttmer, and Mullainathan (2000), Hochberg, Ljungqvist, and Lu (2007), Beaman (2012), Kuhnen (2009), Shue (2011), and Banerjee et al. (2012).

Sturm (2008), who estimate the importance of market access for economic development.[4]

The following section summarizes the relevant history of postwar Germany. Section III discusses the data and its construction. Section IV identifies the effect of social ties on regional economic growth. Section V traces this effect to the effect of social ties on entrepreneurial activity and firm investment. Section VI analyzes the effect of direct and indirect social ties using household-level data. Section VII concludes. The Online Appendix contains additional robustness checks and details on the construction of our data set.

II. Historical Background

II.A. Destruction of Housing Stock during World War II

German cities and towns were heavily damaged during World War II. This was mainly the result of Allied air raids, which began in 1940 and intensified until the final days of the war in 1945. They left around 500,000 dead and resulted in the destruction of a third of the West German housing stock, making it the most devastating episode of air warfare in history.[5]

In the early days of the war, the Royal Air Force attempted to slow down the advance of the German army into the Soviet Union by destroying transport infrastructure. This strategy failed and was quickly abandoned, as the technology available at the time did not permit targeted raids. At best, the pilots flying the nighttime raids were able to make out that they were above a city (and they were often unsure which city lay below). This led to the adoption of the doctrines of "morale bombing" (1941) and "fire and carpet bombing," which aimed at destroying German morale by destroying cities and towns (Kurowski 1977). By the end of the war, 50% of the 900,000 metric tons of bombs had hit residential areas, while 17% had hit industry or infrastructure.

The most heavily damaged cities during the early years of the war were those that were close to the British shore and easy to spot from the air, for example, Hamburg and Cologne. After 1944,

4. Also see Alesina and Fuchs-Schündeln (2007), Fuchs-Schündeln and Schündeln (2005), Bursztyn and Cantoni (2012), and Ahfeldt et al. (2010).

5. The information presented in this section is from USGPO (1945), Kurowski (1977), and Friedrich (2004).

the Allies used technological advances to implement firestorms, which were easiest to create in cities with highly flammable, historical centers, such as Darmstadt, Dresden, and Würzburg. Firestorms could typically not be implemented in cities that had already been hit by a large number of explosive bombs, as the rubble from earlier raids would prevent the fire from spreading. This is why the cities that were attacked late in the war (often strategically the least important) were among the most thoroughly destroyed.[6]

Figure I shows the varying intensity of destruction in West German regions. Note that none of our empirical results rely on this pattern being random or driven by specific factors. Instead, our identification strategy relies on the assumption that the pattern of wartime destruction, or any omitted variables driving it, have no direct effect on changes in growth rates of West German regions 45 years later, post-1989.

II.B. The Partition and Reunification of Germany

In 1944, as World War II entered its final phase, the United Kingdom, the United States, and the Soviet Union agreed on a protocol for the partition of prewar Germany: the areas to the east of the Rivers Oder and Neisse (Pomerania, Silesia, and East Prussia) were to be annexed by Poland and by the Soviet Union, and the remaining territory was to be divided into three sectors of roughly equal population size. The United Kingdom would occupy the northwest, the United States the south, and the Soviet Union the east. The capital, Berlin, would be jointly occupied. At the end of the war, the three armies took control of their sectors, and the United States and Britain carved a small French sector out of their territory. In 1949, with the onset of the Cold War, the three western sectors formed the Federal Republic of Germany (West Germany), and the Soviet sector became the German Democratic Republic (East Germany). Economic exchange between the two parts of Germany became increasingly difficult as the East German government rapidly introduced central planning. In 1952 the border was completely sealed, cutting

6. During a firestorm, a large section of a city catches fire, creating winds of up to 75 meters per second, depriving those exposed of oxygen and often sucking them into the fire.

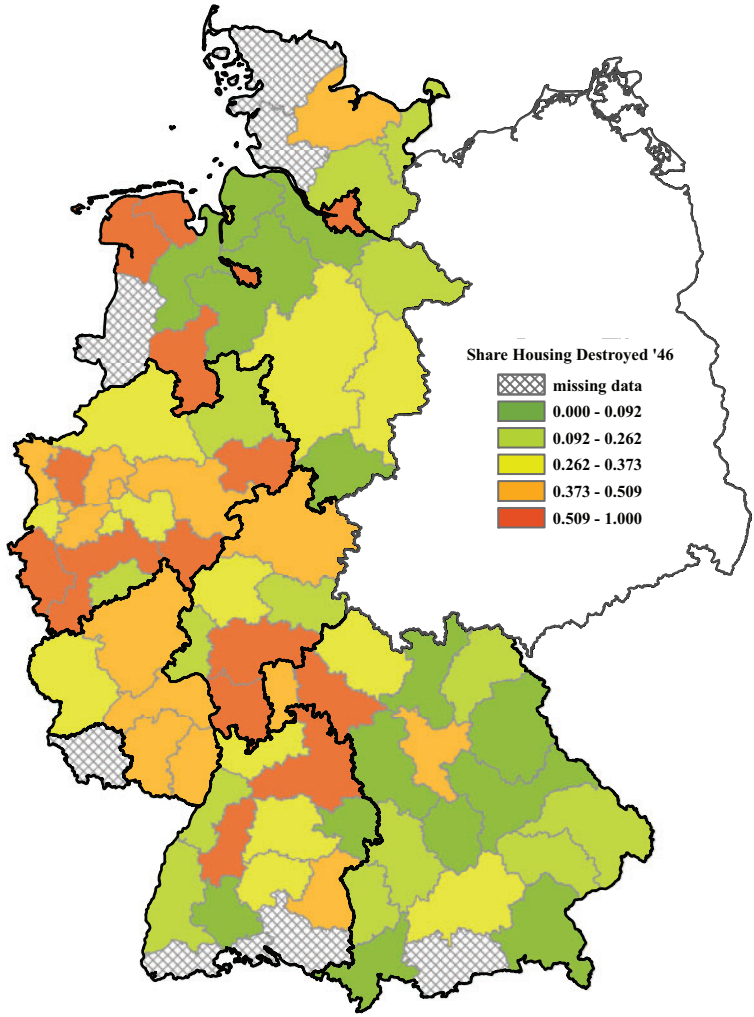

FIGURE I

Share of Housing Destroyed

The figure presents the level of *Share Housing Destroyed '46* in West German regions. The five shades refer to the five quintiles of war destruction. The median level of housing destroyed in each quintile is 0.042, 0.181, 0.328, 0.412, and 0.627, respectively. Crossed areas indicate regions for which we do not have data. Thick borders indicate West German federal states. The white area is former East Germany.

any remaining legal or illegal trade links between East and West.[7]

Until the construction of the Berlin Wall in August 1961, there remained the possibility of personal transit from East to West Berlin, which was the last remaining outlet for East Germans to flee to West Germany. After 1961, migration between East and West virtually ceased. In the following years the partition of Germany was formally recognized in various international treaties and was, until late summer 1989, generally believed to be permanent.[8]

In September 1989, it became apparent that a critical mass of East Germans had become alienated from the socialist state, its declining economic performance, and the restrictions it placed on personal freedom. Increasingly large public demonstrations led to the opening of the Berlin Wall on November 9, 1989. The first free elections in East Germany were held in March 1990, followed by the rapid political, monetary, and economic union between East and West Germany by the end of the same year.

II.C. Refugees and Expellees in West Germany, 1945–1961

In 1945 the Polish and Soviet authorities expelled all German nationals from annexed territory so that these areas could be inhabited by Polish (and Soviet) nationals. Those that did not leave of their own accord (many had fled the advancing Soviet army in the final months of the war) were marched or transported toward the four sectors. We refer to former residents of the annexed territory as expellees (*Vertriebene*). Ethnic Germans that either originally lived in or moved to the countries occupied by the German army during the war were also expelled in many cases, particularly from Czechoslovakia, Hungary, Romania, and Yugoslavia. Expellees were registered and then assigned to one of the four sectors, according to quotas fixed in

7. Throughout the period, gift giving remained possible and West Germans often sent packages to East Germans. However, the only remaining trade between the two countries was the *Interzonenhandel*, which was a system of barter transactions between the governments. In 1960 its total volume came to the equivalent of $178 million. See Holbik and Myers (1964) for a detailed description of the Interzonenhandel.

8. The most important of these treaties was the *Grundlagenvertrag* of December 1972 between East and West Germany in which both countries recognized "two German states in one German nation." Following this treaty, East and West Germany were accepted as full members of the United Nations.

the Potsdam Agreement.[9] The authorities of the sectors in turn allocated the expellees to the states within their jurisdictions and assigned them quarters wherever they could find intact housing stock.

The first wave of 5.96 million expellees arrived in the three western sectors by 1946. We refer to this group as direct expellees. As it became increasingly apparent that the division of Germany would become permanent, most of the expellees that had originally been allocated to the Soviet sector (2.8 million) also left for the West. These expellees via the Soviet sector are critical to our empirical analysis because they lived in East Germany for up to 16 years before migrating to West Germany and thus had the opportunity to form social ties to East Germans. By 1960, the total number of expellees in West Germany had risen to 9.0 million, of which roughly a third were expellees via the Soviet sector.[10]

In parallel, an increasing number of native residents of the Soviet sector who were dissatisfied with the political and economic prospects of the fledging East Germany fled to the West. This flow of refugees (*Flücḧtlinge)* peaked in the years before the construction of the Berlin Wall, with on average around 300,000 individuals illegally crossing the border each year between 1957 and 1961 (Hunt 2006). By 1961, three million East German refugees settled in the West.

While the occupying authorities in the western sectors, and later the West German authorities, had an explicit policy of supplying expellees with housing and various subsidies, there was little support for refugees. In fact, as late as 1950 the authorities actively tried to discourage refugees from entering West Germany on the grounds that they would exacerbate an already catastrophic housing situation and in fear of the political consequences of depopulating East Germany. However, the authorities

9. The official plan adopted by the Allies in November 1945 was to expel 6.65 million Germans. Of these, 2.75 million were to be allocated to the Soviet sector and 2.25 million, 1.5 million, and 0.15 million to the American, British, and French sectors, respectively (Bethlehem 1982, p. 29). Although the initial phase of expulsion was often chaotic, the quotas fixed in the Potsdam Agreement were largely enforced ex post (Frank 2007).

10. We are unable to determine exactly how many expellees remained in East Germany, as the communist government declared after 1950 that the expellees had been fully integrated into East German society and banned the concept from subsequent government statistics (Franzen and Lemberg 2001).

never attempted to deport refugees back to the East, and so refugees often made their own way in West Germany without registering with the authorities (Bethlehem 1982, chapter 3). The severe housing crisis that resulted from the inflow of millions of migrants into the heavily destroyed Western sectors remained a principal determinant of the allocation of expellees and refugees to West German cities and towns until the late 1950s.[11]

III. The Data

We use data at the household, firm, district (*Landkreis*), and regional (*Raumordnungseinheit*) levels. Districts are the equivalent of U.S. counties. Regions are the union of several districts, and each district belongs to one such unit. Regions do not have a political function but exist exclusively for statistical purposes.[12] Most of our aggregate data are available at the district level, except for data on income per capita and employment before 1995, which are available only at the regional level. Our primary units of analysis are the 75 West German regions, of which we drop 3 for which we have no information on wartime destruction. We also drop Berlin due to its unique position and historical circumstances, as well as Hamburg, which is the only federal state that has no regional subdivisions. When we use aggregate controls in our firm- and household-level analysis, we always use data at the lowest level of aggregation available.

III.A. Region-Level Data

The 1961 census reports the number of inhabitants and the number of expellees in each West German district. The census presents the data separately for expellees who arrived directly in West Germany during or after the war and for expellees who arrived in West Germany after having registered a residence in

11. In 1955 the total stock of apartments was still 30% below its 1939 level, which was reached only in the late 1960s (Sensch 2010). In the 1940s the availability of housing was the principal determinant of where the expellees were assigned quarters. After 1949, economic considerations started playing a more important role in the allocation process, and the West German government also initiated a number of programs encouraging migration to areas in which there was a relatively high demand for labor. However, these programs remained relatively limited, with less than 1 in 10 expellees participating (Bethlehem 1982, p. 29, 49).

12. In this sense they are analogous to metropolitan statistical areas in the United States, but they also include rural areas.

the Soviet sector. From these data we created the variables *Share Expellees (Direct) '61* and *Share Expellees (Sov. Sector) '61*.[13] By contrast, we do not have reliable regional data on the settlement of refugees arriving from the Soviet sector because refugees had little incentive to reveal themselves to the authorities. However, since expellees via the Soviet sector arrived in West Germany around the same time as refugees and both groups faced similar constraints regarding the shortage of housing, the settlement pattern of refugees across West German regions was likely very similar to that of expellees arriving from the Soviet sector. We therefore use *Share Expellees (Sov. Sector) '61* as our primary proxy for the intensity of social ties to the East in a given West German region. Online Appendix Figure I shows the distribution of this group across West German regions in 1961.

For our instrumental variables strategy we coded two measures of wartime destruction: the share of dwellings that were destroyed in 1946, labeled *Share Housing Destroyed '46*, and the amount of rubble in cubic meters per inhabitant, labeled *Rubble '46 (m^3 p.c.)*. Our data source reports both measures for the 199 largest West German cities and towns that collectively accounted for 47% of the total prewar population of West Germany. We also coded the number of inhabitants of these towns in 1939 and 1946 from the same source. We aggregated the data on wartime destruction by calculating the mean destruction across cities and towns in a district or region, weighted by population in 1939. (See Online Appendix Table I for details.)

Our data on income per capita are from the German *Mikrozensus*, an annual, obligatory random survey of 1% of the population. The survey lists an income bracket for each individual, which we aggregate to the region level using the mean of each bracket. Income per capita at time t is labeled *Income t (p.c.),* where t is every second year between 1985 and 1995. Because the Mikrozensus does not identify districts prior to 1995, an aggregation to districts was not possible. We also used the Mikrozensus to construct the average income of entrepreneurs (*Income Entrepreneurs t (p.c.)*), the average income of nonentrepreneurs (*Income Non-Entrepreneurs t (p.c.)*), as well as the share of entrepreneurs among the respondents (*Share Entrepreneurs t*)

13. These data were collected at the district level. Some West German district boundaries changed between 1961 and 1989. In those cases we used area weights calculated in ArcGIS to convert 1961 districts into their 1989 equivalents.

for each region. From the same source we obtained data on the share of the population working in different sectors *s* of the economy in 1989 (*Share Employed in s '89*), where *s* stands for agriculture; manufacturing; services; and government, respectively, the share of the region's population who are migrants arriving from the East in the years following the fall of the Berlin Wall (*Migration from East '91-'95*), and further variables described in Online Appendix Table I. As an additional control we calculated the distance of the center of each district or region to the former inner-German border using GIS data (*Distance to East (100 km)*).

III.B. Firm-Level Data

Our firm-level data are from the 2007 edition of the ORBIS data set, which is the edition that expands coverage to small and medium-sized German firms. It includes information on the location of the headquarters of each West German firm and a list of its subsidiaries and branches. We use the postal code to match each firm to the district in which its headquarters is located. After dropping firms based in districts for which we lack data on wartime destruction, we are left with data on 19,387 firms that have at least one subsidiary or branch in West Germany outside the district of their headquarters.[14] As a simple measure of firms' investment activity in different parts of the world, we created a dummy variable for whether the firm has a subsidiary or branch in a given location *x* (*S. & B. in x (Dummy)*). We calculated this dummy variable for East Germany, Poland and the Czech Republic, the Old EU Countries (the 14 EU member countries other than Germany prior to enlargement in 2004), the New EU Countries (excluding Poland and Cz. Republic) (all EU member countries that acceded in or after 2004), and Non-EU Countries. For the same set of firms we computed the share of each firm's subsidiaries and branches in location *x* as a fraction of its total number of subsidiaries and branches in location *x* and West Germany (*Share of Total S. & B. in x*). As proxy for the size of the firm we use the log of the number of subsidiaries and branches it operates in West Germany (*S. & B. in West Germany*). Finally, we used the NACE code given in the ORBIS

14. We exclude firms that have only one address. Because our inference uses only a single cross-section, these firms are uninteresting as the probability that they own a subsidiary or branch in the East is mechanically zero.

data set to define four sectoral fixed effects (agriculture, manufacturing, services, and government).

III.C. Household-Level Data

Our household-level data are from the German Socio-Economic Panel (SOEP), which is an annual panel of German households.[15] From the panel, we selected all West German households (again excluding Berlin) which participated in the 1985, 1989, and 1995 waves. For each of these households we use information on household income in the years 1985–2001 (*Income (SOEP)*) and information on 29 other socioeconomic characteristics of the household, including age, education, occupation, and social status of the household head as well as the composition of household income, transfers to and from relatives, and asset holdings. We also created a dummy variable that is 1 if any household member had lived in East Germany prior to 1961 (*Lived in East Germany*).[16]

Importantly, the 1991 wave of the panel contains several questions about contacts to friends and family in East Germany. Since the survey was conducted in the second year after the fall of the Berlin Wall and households had some time to renew ties with individuals in the other part of Germany, we choose not to rely on information about the intensity of contact to friends and relatives, although it is available.[17] Instead, we base our work on the response to the simple factual question "Do you have relatives in East Germany?" and generate a dummy variable that is 1 if at least one member of the household responded with yes, and 0 otherwise (*Ties to Relatives '91*). We also aggregated this variable to the region level by calculating the share of households with ties to East Germany in each West German region (*Share Ties to Relatives '91*), which we use as a secondary measure of the intensity of social ties at the regional level. A possible source of measurement error is that West German relatives of the survey respondents may have migrated to East

15. See Wagner, Frick, and Schupp (2007) for a detailed description of the SOEP.

16. Details of how we aggregated data on individuals to the household level are given in Online Appendix Table I.

17. Our results are very similar if we use information on friendships or condition on respondents indicating "close" ties to their relatives or friends.

Germany directly after the fall of the Berlin Wall and before the conclusion of the 1991 wave of the survey. However, the flow of migrants from West to East in 1990 was small (only around 30,000 individuals; Hunt 2006). It is thus safe to assume that households based in West Germany in 1989 that reported a relative in East Germany in 1991 also had a relative in East Germany in 1989.

III.D. Descriptive Statistics

Panel A of Table I presents the data on West German regions; column (1) gives the data for all regions, columns (2) and (3) divide the sample into regions with a higher and lower share of housing destroyed in 1946 than the median region. The first row of column (1) gives the mean and the standard deviation of the share of expellees via the Soviet sector in 1961. On average, expellees via the Soviet sector made up 4.8% of the 1961 population. Similarly, expellees that came directly to West Germany on average made up 12.0% of the population in 1961 (row 2), and 29.7% of the 1991 population reported having relatives in East Germany (row 3). In all three cases, these shares are higher in regions that suffered lower levels of wartime destruction. The variation in wartime destruction is considerable, with 14.9% of housing on average destroyed in regions with low destruction and 48.7% in regions with high destruction (row 4). Moreover, regions which are closer to the inner-German border tended to be less damaged than those that are further away (row 6). The pattern in income per capita is interesting: whereas regions with lower wartime destruction are slightly poorer in 1985 and 1989, they are slightly richer than the average region in 1995.

Panel B of Table I presents the data on West German firms in 2007, again split up by regions with above and below the median level of wartime destruction. On average, firms in regions with less wartime destruction are slightly smaller as measured by the number of subsidiaries they operate in West Germany (row 1). Nevertheless, they are also more likely to operate a subsidiary or branch in the East (8.3% versus 7.2%). On average, 7.8% of the firms in our sample operate a subsidiary or a branch in East Germany (row 3) and 1.0% operate in non-EU countries.

TABLE I

SUMMARY STATISTICS

	(1) All	(2) Low Destr.	(3) High Destr.
Panel A: Region-level data			
Share Expellees (Soviet Sector) '61	0.048	0.050	0.047
	(0.019)	(0.022)	(0.016)
Share Expellees (Direct) '61	0.120	0.144	0.096
	(0.045)	(0.041)	(0.036)
Share Ties to Relatives '91	0.297	0.321	0.274
	(0.140)	(0.156)	(0.122)
Share Housing Destroyed '46	0.318	0.149	0.487
	(0.210)	(0.105)	(0.143)
Rubble '46 (m^3 p.c.)	0.088	0.034	0.143
	(0.069)	(0.028)	(0.055)
Distance to East (100 km)	1.769	1.543	1.994
	(1.074)	(1.060)	(1.070)
Income 1985 (DM, p.c.)	1595	1570	1620
	(125)	(140)	(106)
Income 1989 (DM, p.c.)	1760	1752	1768
	(132)	(147)	(118)
Income 1995 (DM, p.c.)	2222	2231	2210
	(154)	(166)	(143)
N	70	35	35
Panel B: Firm-level data			
S. & B. in West Germany (log)	0.444	0.437	0.450
	(0.743)	(0.729)	(0.756)
S. & B. in East Germany (Dummy)	0.078	0.083	0.073
S. & B. in Non-EU Countries (Dummy)	0.010	0.011	0.008
N	19,387	9,706	9,681
Panel C: Household-level data	All	Ties	No Ties
Age '90	51.0	50.5	51.2
	(14.3)	(13.4)	(14.7)
Gender	0.23	0.17	0.26
Years of Education '89	12.3	12.5	12.2
	(1.85)	(1.89)	(1.83)
Income 1989 (SOEP)	3,307	3,460	3,236
	(1,822)	(1,600)	(1,911)
Capital Income '89	879	822	907
	(1,811)	(1,438)	(1,966)
Entrepreneur '89	0.048	0.053	0.046
Not Employed '89	0.052	0.046	0.055
N	1,857	583	1,274

Notes. The table presents means (and standard deviations). Variables in Panel A refer to our sample of regions used in Tables II–IV. Variables in Panel B refer to our sample of firms used in Table V. Panel C refers to our sample of households from the SOEP used in Tables VI–VII. Column (1) shows data for all observations. In Panel A, columns (2) and (3) show data for regions in which *Share Housing Destroyed '46* is above and below the median, respectively. In Panel B, columns (2) and (3) present means and standard deviations for firms headquartered in regions with *Share Housing Destroyed '46* above and below the median, respectively. *S. & B.* stands for subsidiaries and branches which firms headquartered in a given West German region operate in the indicated location. In Panel C, columns (2) and (3) show data for households with ties to relatives in East Germany and without ties to relatives in East Germany, respectively. Monetary values are given in nominal deutschemarks. The variables Age, Gender, Years of Education, Entrepreneur, and Not Employed refer to the household head. Gender is a dummy variable that is 1 if the household head is female. See Online Appendix for details.

IV. Social Ties and Regional Economic Growth

We first explore the effect of social ties between West and East Germans on income growth in West German regions. The structural equation of interest is

$$Y_r^{95} - Y_r^{89} = \alpha\, T_r^{89} + \mathbf{Z}_r'\varsigma + \varepsilon_r, \tag{1}$$

where Y_r^t is log income per capita in region r in year t. The left-hand variable is thus the growth in income per capita between 1989 and 1995. T_r^{89} denotes our measure of social ties in region r, which for our key results is the share of the region population who are expellees via the Soviet sector. $\mathbf{Z}_r$ is a vector of controls that always contains a complete set of state fixed effects, log income per capita in 1989 (Y_r^{89}), and the distance from region r to the inner-German border. The coefficient of interest is α, which measures the effect of social ties on growth in income per capita after 1989. In our standard specification we also control for the preexisting growth trend by including the growth rate of income per capita between 1985 and 1989, $Y_r^{89} - Y_r^{85}$. The coefficient α then estimates the *differential change* in the growth rate of income per capita after 1989 for regions with different intensities of social ties to the East. The assumption that the relationship between growth in income per capita and social ties is linear is made for simplicity. The error term ε_r captures all omitted influences, including any deviations from linearity. Throughout, standard errors are calculated using the Huber-White correction to ensure robustness against arbitrary heteroskedasticity.[18]

Equation (1) will consistently estimate the parameter of interest if $Cov(T_r^{89}, \varepsilon_r) = 0$. This covariance restriction may, however, not hold in the data, because the settlement of migrants from the East in West Germany prior to 1961 (and thus the strength of social ties to East Germany) may be correlated with differences in growth prospects across regions. Although we show ordinary least squares estimates of equation (1) for reference and comparison, we primarily rely on an instrumental variables strategy, which uses only the variation in T_r^{89} that is attributable to variation in wartime destruction across regions in 1946. Our first-stage specification is

$$T_r^{89} = \gamma W_r^{46} + \mathbf{Z}_r'\varsigma^{fs} + \nu_r, \tag{2}$$

18. Because there are only nine states (our sample excludes Berlin and Hamburg), we do not cluster the standard errors at the state level.

where W_r is our measure of wartime destruction and (2) contains the same covariates as (1). Our key identifying assumption is

$$Cov\left(W_r^{46}, \varepsilon_r\right) = 0. \tag{ID1}$$

It states that, conditional on the covariates we control for, (1) there is no omitted variable that drives both wartime destruction and differential changes in income growth post-1989, and (2) wartime destruction in 1946 has no effect on changes in the growth rate of income per capita after 1989 other than through its effect on the settlement of migrants who have social ties to the East.

IV.A. The First-Stage Relationship

Panel A of Table II shows our basic first-stage regressions, using the share of expellees via the Soviet sector in 1961 as a proxy for social ties in 1989. Column (1) is the most parsimonious specification as shown in equation (2). It regresses the share of expellees via the Soviet sector on the share of housing destroyed in 1946, while controlling for the distance to the inner-German border, for income per capita in 1989, and for state fixed effects. The coefficient estimate of –0.019 (std. err. = 0.004) is statistically significant at the 1% level and suggests that a 1 standard deviation increase in the share of housing destroyed in 1945 (std. dev. = 0.21) is associated with a 0.4 percentage point drop in the share of expellees via the Soviet sector in 1961.[19] (This corresponds to 8% fewer expellees via the Soviet sector relative to the mean across regions.)

As expected, the share of expellees in 1961 falls with the distance to the inner-German border. The coefficient on income in 1989 is positive and significant, suggesting that expellees tended to settle in regions that were richer in 1989, which is most likely attributable to persistent differences in income per capita between regions that predate 1961.[20]

19. The share of expellees via the Soviet sector proxies for both groups of migrants (expellees and refugees) arriving from East Germany. Since both groups were roughly of the same size, we may speculate that a 1 standard deviation increase in wartime destruction may be associated with a drop which is around twice as large in the total share of migrants from the East settling in a given West German region.

20. Income per capita in 1989 and other control variables are included in all specifications to present the first stage corresponding to the instrumental variables results discussed later.

TABLE II

WARTIME DESTRUCTION, SOCIAL TIES, AND INCOME GROWTH

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage			*Share Expellees (Sov. Sector) '61*			
Share Housing Destroyed '46	−0.019***	−0.020***		−0.020***	−0.021***	−0.020***
	(0.004)	(0.004)		(0.005)	(0.004)	(0.005)
Distance East (100km)	−0.005***	−0.005***	−0.005***		−0.004***	−0.005***
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)
Income 1989 (p.c., log)	0.042***	0.047***	0.043***	0.048***	0.025*	0.047***
	(0.011)	(0.012)	(0.014)	(0.014)	(0.015)	(0.013)
Income '89/'85 (p.c., log)		−0.026	−0.020	−0.032	−0.029	−0.026
		(0.024)	(0.024)	(0.026)	(0.026)	(0.024)
Rubble '46 (m^3 p.c.)			−0.044***			
			(0.013)			
Migration from East '91–'95						−0.006
						(0.210)
R^2	0.918	0.920	0.904	0.988	0.931	0.920

TABLE II
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: First stage (alternative)			*Share Ties to Relatives '91*			
Share Housing Destroyed '46	−0.139**	−0.143**		−0.133*	−0.120**	−0.143*
	(0.061)	(0.060)		(0.069)	(0.052)	(0.072)
Rubble '46 (m^3 p.c.)			−0.271			
			(0.180)			
Panel C: Reduced form			*Income '95/'89 (p.c., log)*			
Share Housing Destroyed '46	−0.042**	−0.048**	−0.060**	−0.050**	−0.058***	−0.047**
	(0.021)	(0.020)	(0.027)	(0.019)	(0.020)	(0.020)
Rubble '46 (m^3 p.c.)			0.046			
			(0.070)			
N	70	70	70	70	70	70
Distance quartile fixed effects	—	—	—	yes	—	—
Sector controls	—	—	—	—	yes	—

Notes. Coefficient estimates from ordinary least squares regressions at the regional level. Standard errors are given in parentheses. Standard errors are calculated using the Huber-White correction to account for potential heteroskedasticity. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. The main variable of interest in all columns except column (3) is the share of the region's 1939 housing stock that was destroyed in 1946, *Share Housing Destroyed '46*. In column (3) of Panels A and B the main variable of interest is *Rubble '46 (m^3 p.c.)*. In column (3) of Panel C both of these variables are included. The dependent variable in Panel A is our main proxy for social ties to East Germany, *Share Expellees (Sov. Sector) '61*. The dependent variable in Panel B is our alternative measure of social ties to the East: the share of the population stating in the 1991 SOEP survey to have relatives in East Germany, *Share Ties to Relatives '91*. In Panel C the dependent variable is the log of the ratio of the region's mean per capita income in 1995 and 1989. All regressions include 9 state fixed effects. All specifications in Panel B and C include the same controls as shown in Panel A. The coefficient estimates on these are not reported to save space. Column (4) controls for four distance dummies, corresponding to the quartiles of the distance measure. Column (5) controls for the share of the 1989 population working in agriculture, manufacturing, services, and government, respectively. The specification in column (6) controls for the share of the region's population who are migrants arriving from East Germany between 1991 and 1995.

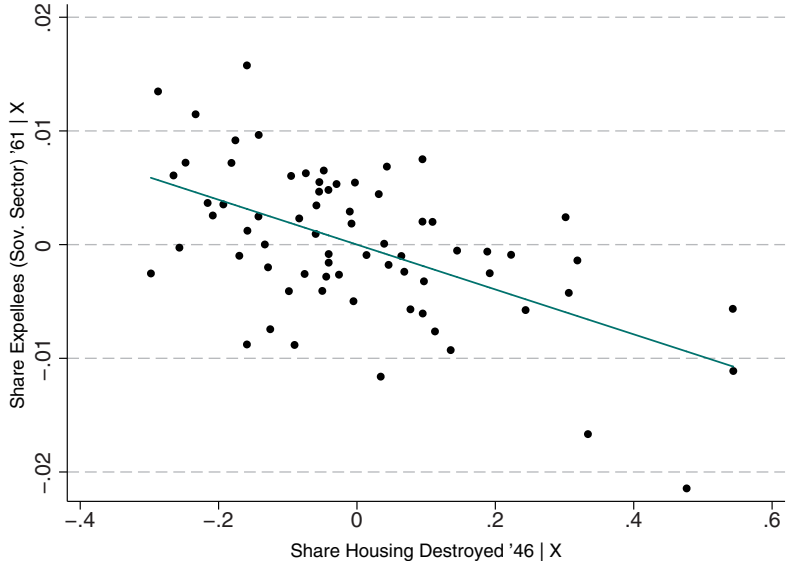

FIGURE II

Share Expellees and Share Housing Destroyed (Conditional Scatterplot)

The figure is a conditional scatterplot of *Share Housing Destroyed '46* and *Share Expellees (Soviet Sector) '61* at the regional level. The corresponding first stage regression (shown in column (2), Panel A, Table II) controls for distance to the inner-German border, the log of per capita income in 1989, the log of the ratio of per capita income in 1989 and 1985 and a full set of state fixed effects. The solid line depicts the fitted regression line. The coefficient estimate is –0.020 (std. err. = 0.004) and is significant at the 1% level.

The specification in column (2) is our standard specification. It adds income growth in the five years prior to 1989 as an additional control. The coefficient of interest remains virtually unchanged at –0.020 (std. err. = 0.004). The coefficient on income growth is statistically indistinguishable from zero at conventional significance levels, suggesting that the pattern of settlement of expellees via the Soviet sector in 1961 is not correlated with income growth in the years prior to the fall of the Berlin Wall.

Figure II plots the conditional relationship estimated in this column and shows that the first stage relationship is not driven by outliers. Columns (3)–(6) of Panel A show the first-stage regressions corresponding to robustness checks, which we perform in the instrumental variables estimation. In column (3) we use the volume of rubble per capita in 1946 as an alternative measure

of wartime destruction, which again yields a negative and significant coefficient. In column (4) we replace our control for the distance to the inner-German border with a fixed effect for each distance quartile; in column (5) we add the share of the workforce employed in agriculture, manufacturing, services, and government in 1989 (we do not report the coefficients on these variables to save space). Finally, column (6) adds the extent of migration after reunification as an additional control. In each case the coefficient of interest remains virtually unchanged and statistically significant at the 1% level.

Panel B of Table II repeats the same specifications as in Panel A, using the share of households with ties to relatives in East Germany in 1991 as an alternative proxy for social ties in 1989. In the interest of preserving space, the panel shows only the coefficient of interest. All estimates are negative and all except the ones in columns (3) and (4) are statistically significant at the 5% level (the latter is significant at 10%). The coefficient in column 1 is –0.139 (std. err. = 0.061). It implies that a 1 standard deviation rise in the share of housing destroyed in 1945 is associated with a 2.92 percentage point drop (or alternatively a 9.8% drop relative to the average) in the share of households that have a relative in East Germany in 1991. Similar results (not shown) hold for the share of households that report contact with friends in East Germany.

When we regress the share of households with ties to the East in 1991 on the of share expellees via the Soviet sector as instrumented with the share of housing destroyed in 1946 and our standard control variables the coefficient of interest is 7.229 (std. err. = 3.243), suggesting that regions that suffered worse destruction during World War II have proportionately more households with ties to the East in 1991 because they received more migration from the East before 1961.

In the following sections we use the share of expellees via the Soviet sector in 1961 as our main proxy for social ties to the East. Because it lays bare the historical source of variation, it allows us to perform a number of placebo experiments corroborating our key identifying assumption. We present corresponding results using the share of households with ties to the East in 1991 when we trace the effect on regional income growth to the household level in Section VI. The two proxies are highly correlated (0.627), as shown in Online Appendix Figure II, and yield quantitatively almost identical results, although standard errors tend

to be narrower when we use the census-based data on expellees, which are likely measured with less error.

IV.B. The Reduced-Form Relationship

As a prelude to our instrumental variables estimates, Panel C of Table II shows the reduced-form relationship between growth in income per capita after the fall of the Berlin Wall and wartime destruction. All specifications (except the one in column (3)) are again identical to the ones in Panels A and B, with the left-hand variable now being the growth in income per capita between 1989 and 1995, $Y_r^{95} - Y_r^{89}$. The coefficient of interest is negative and statistically significant at the 5% level in all columns. The estimate in column (2) (–0.048, std. err. = 0.020), suggests that regions that were least damaged during the war experienced a significantly higher increase in the growth rate of income per capita post-1989 than regions that were most damaged during the war. A one standard deviation drop in the share of housing destroyed is associated with a 1 percentage point higher growth in income per capita over the six years following 1989. The size of the estimated coefficient is stable across columns (1), (2), and (4)–(6). Online Appendix Figure III depicts this relationship graphically in a conditional scatter plot corresponding to the estimate in column (2).[21]

As a first test of the mechanism by which wartime destruction could suddenly affect economic growth 45 years after the fact, the specification in column (3) includes both the share of housing destroyed in 1946 and rubble per capita in 1946. The results are encouraging for our identification strategy: while the coefficient on the share of housing destroyed remains negative and significant at –0.060 (std. err. = 0.027), the coefficient on rubble per capita is positive and insignificant. This pattern is consistent with the view that it is primarily the lack of housing in 1946 and not wartime destruction per se that affects changes in economic growth post-1989.

21. In the plot, Wilhelmshaven looks like a significant outlier. Dropping Wilhelmshaven from the sample reduces the coefficient estimate to –0.033 (std. err. = 0.016). As a more systematic check for the effect of outliers, we run a robust regression (according to the terminology used by STATA) in which observations with a Cook's *D* value of more than 1 are dropped and weights are iteratively calculated based on the residuals of a weighted least squares regression. The robust estimate is –0.032 (std. err. = 0.014).

IV.C. Instrumental Variables Results

In our instrumental variables estimation, we explicitly test the hypothesis that a concentration of households with social ties to East Germany in 1989 in a given West German region is causally related to a rise in the growth rate of income per capita after the fall of the Berlin Wall. In Table III, we estimate equation (1) using only the variation in social ties in 1989 that is due to variation in wartime destruction by instrumenting for the share of expellees via the Soviet sector in 1961. In column (1) we instrument with the share of housing destroyed in 1946.[22] The coefficient estimate on the share of expellees is 2.169 (std. err. = 0.940), suggesting that a 1 standard deviation increase in the share of expellees in 1961 (std. dev. = 0.019) is associated with a 4.1 percentage point rise in income per capita over the six years following 1989 (or roughly a 0.7 percentage point higher growth rate per annum).[23] The coefficient on income in the base year, 1989, is negative and significant, which suggests mean reversion in income per capita across West German regions. Somewhat surprisingly, the coefficient on the distance to the inner-German border is positive, which suggests that the regions closest to the inner-German border did not immediately profit from the opening of the border (which is in line with a similar observation in Redding and Sturm 2008 that the population of West German cities close to the inner-German border grew relatively little between 1989 and 2002).[24]

Column (3) gives our standard specification, in which we control both for the level of income in 1989 and for income growth in the four years preceding 1989. The coefficient of interest rises slightly to 2.442 (std. err. = 0.874) and is significant at the 1%

22. The *F*-statistic against the null that the excluded instrument is irrelevant in the first-stage regression is 18.92. Using both measures of wartime destruction as instruments for Share Expellees (Sov. Sector), the Hansen *J* test statistic for overidentifcation is 0.460 with a *p*-value of 0.498. We thus fail to reject the null that our instruments are uncorrelated with the error term and correctly excluded from the second-stage regression.

23. Because our model contains a lagged dependent variable, there may be a mechanical bias in the coefficient of interest. Instrumenting the lagged dependent variable with its own lag ensures consistency (Anderson and Hsiao 1982). If we use log of income in 1985 and log of income in 1982 as instruments for income growth between 1985 and 1989 the coefficient estimate increases to 3.204 (std. err. =0.998) and is significant at the 1% level.

24. All region level results are robust to various parametric and nonparametric ways of controlling for the distance to the inner-German border.

TABLE III

SOCIAL TIES AND INCOME GROWTH

	(1) (IV)	(2) (OLS)	(3) (IV)	(4) (IV)	(5) (IV)	(6) (IV)	(7) (IV)
Panel A: Main Results				*Income '95/'89 (p.c., log)*			
Share Expellees (Sov. S.) '61	2.169**	1.963***	2.442***	2.453***	2.473***	2.772***	2.366***
	(0.940)	(0.570)	(0.874)	(0.871)	(0.880)	(0.848)	(0.872)
Distance East (100 km)	0.011**	0.008**	0.011**	0.011**		0.012***	0.011**
	(0.004)	(0.003)	(0.004)	(0.004)		(0.004)	(0.004)
Income 1989 (p.c., log)	−0.267***	−0.189***	−0.209***	−0.209***	−0.214***	−0.305***	−0.206***
	(0.068)	(0.059)	(0.059)	(0.059)	(0.063)	(0.071)	(0.062)
Income '89/'85 (p.c., log)		−0.362***	−0.355***	−0.355***	−0.381***	−0.278***	−0.353***
		(0.082)	(0.085)	(0.085)	(0.089)	(0.083)	(0.086)
Sh. Employed in Agricult. '89						−0.115	
						(0.293)	
Sh. Employed in Manufact. '89						−0.301	
						(0.281)	
Sh. Employed in Services '89						0.145	
						(0.288)	
Sh. Employed in Governm.'89						−0.522	
						(0.394)	
Migration from East '91–'95							0.349
							(1.122)
R^2	0.504	0.597	0.589	0.589	0.573	0.641	0.592

TABLE III
(CONTINUED)

	(1) (IV)	(2) (OLS)	(3) (IV)	(4) (IV)	(5) (IV)	(6) (IV)	(7) (IV)
Panel B: Placebo				*Income '89/'85 (p.c., log)*			
Share Expellees (Sov. S.) '61	—	0.656	0.560	0.557	0.656	0.443	0.790
	—	(0.598)	(1.016)	(1.022)	(0.926)	(1.093)	(1.034)
N	70	70	70	70	70	70	70
Distance quartile fixed effects	—	—	—	—	yes	—	—
Instruments	Housing	—	Housing	Housing & rubble	Housing	Housing	Housing

Notes. The table reports coefficient estimates from instrumental variables (IV) regressions at the regional level in columns (1) and (3) through (7). Column (2) reports results from an ordinary least squares (OLS) regression. Standard errors are given in parentheses. The standard errors are calculated using the Huber-White correction for potential heteroskedasticity. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. In Panel A the dependent variable is the log of the ratio of mean per capita income in 1995 and 1989. In Panel B it is the log of the ratio of mean per capita income in 1989 and 1985. The main variable of interest in all columns is *Share Expellees (Soviet Sector) '61*. In columns (1), (3), (5), (6), and (7) we instrument for this variable with *Share Housing Destroyed '46*. In column (4) we use *Rubble '46 (m^3 p.c.)* as an additional instrument. First-stage results are shown in Table II. All regressions include 9 state fixed effects. In Panel A all regressions control for the log of mean per capita income in 1989 and columns (2)–(7) include the log of the ratio of mean per capita income in 1989 and 1985 as a control. In Panel B all regressions control for the log of per capita income in 1985. All regressions except column (5) control for a region's distance to the inner-German border. The specifications shown in column (5) control for four distance dummies, corresponding to the quartiles of the distance measure. The regressions shown in column (6) control for the share of the 1989 population working in agriculture, manufacturing, services, and government, respectively. The specifications shown in column (7) control for the share of the region's population who are migrants arriving from East Germany between 1991 and 1995. In Panel B we do not report results for covariates to save space.

level. Regions that received a larger share of migrants from the East prior to 1961 thus experienced a significant increase in their growth rate of income per capita post 1989.

The results of column (3) are almost unchanged when we simultaneously instrument the share of expellees with both the share of housing destroyed and with rubble per capita (shown in column (4)). Column (2) shows the ordinary least squares (OLS) estimate of our standard specification for comparison. It is only about one half of a standard error lower at 1.963 (std. err. = 0.570), suggesting that the endogenous assignment of expellees to West German regions induces only a relatively mild downward bias in the OLS estimate. A Hausman test fails to reject the null hypothesis that $Cov(T_r^{89}, \varepsilon_r) = 0$.

IV.D. Validity of the Exclusion Restriction

Although the endogenous assignment of migrants from the East to West German regions does not seem to have a large effect on our results, our identifying assumption—that the degree of wartime destruction in 1946 affected changes in the growth rate of income per capita after 1989 only through its effect on the settlement of migrants and their social ties to the East—cannot be tested directly. Nevertheless, we can perform a number of falsification exercises to assess its plausibility. There are two types of potential challenges and corresponding tests.

1. Simple Challenge. The simple challenge to our identifying assumption is that wartime destruction (or an omitted variable driving it) may have had a lasting effect on income growth in West German regions that persisted for more than half a century (until 1995). We believe that we can discard this concern.

First, our standard specification controls for the growth rate of income pre-1989 and thus identifies *changes* in the growth rate of income per capita that occur after 1989.

Second, we can show that growth in income per capita in the years prior to 1989 is uncorrelated with wartime destruction and the settlement of expellees. Panel B of Table III shows a placebo experiment in which we use income growth between 1985 and 1989 as the dependent variable rather than as a control. All specifications are parallel to those in Panel A (except that we now

control for log income per capita in 1985 rather than in 1989). Throughout the panel the coefficient of interest is statistically indistinguishable from 0. The same is true when we use the growth rate of income per capita between 1982 and 1989 as dependent variable (not shown). Wartime destruction thus becomes relevant for economic growth only post-1989.[25]

Third, although we have no data on regional income per capita prior to 1982, it is a well documented fact that wartime destruction had no impact on population growth in West German cities post 1960 (Brakman, Garretsen, and Schramm 2004). In Figure III we replicate part of this result. The figure depicts coefficient estimates from city-level regressions of population growth in the years between 1929 and 2000 on the share of housing destroyed in 1946 and a constant. Not surprisingly, wartime destruction had a strong and significantly negative effect on population growth during the war (between 1939 and 1945). During the period of reconstruction, between 1946 and 1960, the cities most heavily destroyed grew fastest. However, from 1960 onward there is no statistically significant effect of wartime destruction on population growth and the coefficient estimates are virtually 0 (albeit with a large standard error in 1960). To the extent that population growth proxies for income growth, this result suggests that the direct effect of wartime destruction on income growth was short-lived.[26]

2. Sophisticated Challenge. The "sophisticated" challenge to our identifying assumption is that the pattern of wartime destruction (or some omitted variable driving it) may have affected income growth through some other channel that only "switched on" after 1989.

For example, the Allies may have targeted areas that were focused on manufacturing, and the manufacturing sector may

25. In Online Appendix Table II we pinpoint the timing of the effect at a higher frequency by regressing log income per capita for each region and year post 1985 on the interaction of year fixed effects with the share of expellees in 1961 (again instrumented with wartime destruction). The table shows no effect of the settlement of expellees on income growth rates prior to 1989, a positive effect in all years post-1989, and a statistically significant effect on growth between 1989 and 1993 and later years.

26. See Akbulut-Yuksel (2009) for a discussion of the microeconomic effects of wartime destruction in Germany.

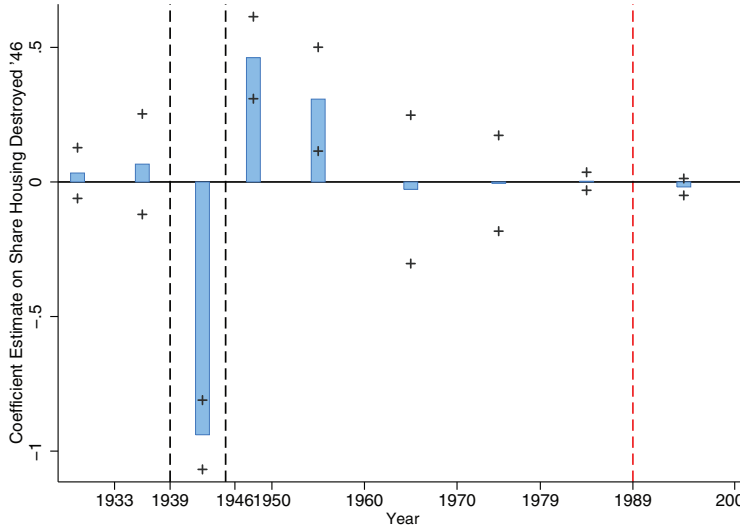

FIGURE III

Effect of Wartime Destruction on Population Growth

The figure depicts coefficient estimates from city-level regressions of population growth on *Share Housing Destroyed '46*. We ran separate regressions for each time interval shown on the horizontal axis of the figure. In each of these regressions the dependent variable is city population growth in the time interval shown. The explanatory variable is always city-level *Share Housing Destroyed '46* and a constant. The bars in the figure present the coefficient estimates on *Share Housing Destroyed '46*, the crosses give 90% confidence intervals. Standard errors are calculated using the Huber-White correction to account for potential heteroskedasticity. The city-level population panel is unbalanced. It contains information for between 144 and 165 cities on population growth between 1925–33, 1933–39, 1970–79, 1979–89, and 1989–2000; it contains information for 73 cities on population growth between 1939–46 and 1946–50, but it only contains information for 17 cities on population growth between 1950–60 and 1960–70. Missing data tend to be from smaller cities. The first two dashed lines indicate the time of World War II; the third dashed line indicates the time of the fall of the Berlin Wall.

have experienced a relative decline after 1989. However, when we control for the sectoral composition of the workforce in column (6) of Table III the coefficient of interest changes very little to 2.772 (std. err. = 0.848). The estimated coefficient on the share of the workforce employed in manufacturing is negative, but it is not statistically significant. Any variation in income growth post-

1989 due to a relative decline of the manufacturing sector is thus unrelated to the effect we identify.

More generally, we find no evidence that wartime destruction is associated with any particular socioeconomic characteristics in 1989. Conditional on our standard control variables, there are no statistically significant differences between regions with higher and lower levels wartime destruction in the sectoral composition of the workforce, educational attainment, unemployment, or the share of the population engaging in entrepreneurial activity (see Online Appendix Table IV(A)).

Another potential concern is that after 1989 highly skilled workers from East Germany may have migrated to the same regions where their relatives settled before 1961, and this migration may have increased the average wage paid in these regions. In column (7) of Table III we control for the flow of migrants from East to West post-1989, and again there is little effect on the coefficient of interest.

Although none of these observable region characteristics appear to be driving our results, there might be other (unobservable) omitted variables that may be correlated with the pattern of wartime destruction and affect changes in regional growth trajectories post-1989. Alternatively, we may be misinterpreting our results in that migrants from the East may affect changes in income growth post-1989 through some channel other than social ties.

We are able to test this, and the entire class of sophisticated challenges, by comparing the effects of expellees via the Soviet sector with the effects of expellees who arrived directly from the parts of prewar Germany that were annexed by Poland and the Soviet Union. Both groups came from the same regions and look similar on observable characteristics. Moreover, there are no systematic differences in observable characteristics in 1989 between regions in which expellees of the two groups settle (see Online Appendix A for details). The only relevant difference between the two groups is that expellees who arrived directly from the annexed areas did not spend any significant time living (and forming social ties) in East Germany. If we misinterpret our results and the effects we document are driven by some omitted variable that determined both wartime destruction and changes in post-1989 income growth, or if there was something special about expellees per se that gave them access to business opportunities after 1989,

we would expect to find that both groups of expellees have a positive effect on income growth after 1989.[27]

When we regress growth in income per capita post-1989 simultaneously on the share of both groups of expellees and our standard region level controls, the coefficient on expellees via the Soviet sector is positive, statistically significant at the 1% level, and very similar to the estimates from Table III (2.131, std. err. = 0.701). The coefficient on the share of direct expellees is negative and statistically indistinguishable from 0 (–0.092, std. err. = 0.149).

Because we have two measures of wartime destruction, we are also able to separately estimate the causal effects of expellees via the Soviet sector and of direct expellees on differential income growth after 1989, by instrumenting simultaneously with both the share of housing destroyed and the cubic meters of rubble per capita. While the coefficient on the share of expellees via the Soviet sector remains positive and statistically significant at the 10% level (3.422, std. err. = 1.796), the coefficient on the share of direct expellees is again close to 0 and statistically insignificant (–0.350, std. err. = 0.620).[28] The effect on regional economic growth is thus particular to the group that had the opportunity to form social ties to East Germans before moving to the West. We view this result as strong support in favor of our interpretation.

3. Remaining Caveats. A final concern for which we cannot control explicitly at the regional level is that expellees via the Soviet sector might be more likely than native West Germans to have restitution claims to property expropriated in East Germany. While compensation payments by law did not begin until 1996 (Southern 1993), the restitution of assets began in the early 1990s and could potentially confound our measure of income per capita. However, we argue in Online Appendix B that

27. In fact, the 1971 census, the last census in which expellees are separately identified, would suggest the opposite. It shows that both groups of expellees are slightly poorer, slightly less educated, and significantly less likely to be entrepreneurs than native West Germans. See Online Appendix Table III.

28. The two coefficients are statistically significantly different in the OLS specification (p-value: 0.008). In the instrumental variable specification the p-value is 0.117. See Online Appendix A for econometric details.

any bias they may induce in our estimates would be quantitatively small.

The same appendix presents a number of additional robustness checks and discusses proximate interpretations of our results, such as the possibility that migrants arriving from the East may have had an unobservable emotional affinity to the East.

V. Understanding the Effect on Regional Economic Growth

V.A. Entrepreneurial Activity

To understand the channel linking social ties to regional economic growth, we disaggregate regional income per capita into the average income of households whose primary income derives from entrepreneurial activity (entrepreneurs) and the average income of all other households (nonentrepreneurs).[29] In columns (1) and (2) of Table IV we rerun our standard specification from column (3) in Table III with the growth rate in the average income of entrepreneurs and nonentrepreneurs as dependent variables. Both specifications include the same covariates as our standard specification, but add the pretrend in income growth of each group and the level of average income of each group in 1989 as additional controls. Because the errors in the specifications in columns (1) and (2) are likely to be correlated, we estimate the two equations (as well as their first stage) jointly using the three-stage least squares estimator. The coefficient estimate is 4.612 (std. err. = 1.733) for entrepreneurs (column (1)) and 1.270 (std. err. = 0.686) for nonentrepreneurs (column (2)), implying that a 1 standard deviation rise in the share of expellees via the Soviet sector is associated with a 8.8 percentage point rise in the average income of entrepreneurs, but only a 2.4 percentage point rise in the average income of nonentrepreneurs.[30] Entrepreneurs who lived in a region with strong social ties to the East thus experienced a much steeper rise in their average income than nonentrepreneurs living in the same region.

29. In the German Mikrozensus these are households whose heads declare that their primary occupation is *Selbstständiger mit oder ohne Beschäftigte.*

30. The *p*-value on the null hypothesis that the two coefficients on Share Expellees (Soviet Sector) '61 are equal in columns (1) and (2) is 0.058.

TABLE IV

SOCIAL TIES AND ENTREPRENEURIAL ACTIVITY

	Income '95/'89 (p.c., log)		Share Entrepreneurs 1995–89
	(1)	(2)	(3)
	Entrepreneurs	Nonentrepreneurs	
	(3SLS)		(IV)
Share Expellees (Sov. Sec.) '61	4.612***	1.270*	0.334**
	(1.733)	(0.686)	(0.163)
Inc. Entrepreneurs '89 (p.c., log)	−0.677***		
	(0.173)		
Inc. Entrepreneurs '89/'85 (p.c., log)	0.072		
	(0.112)		
Inc. Nonentrepreneurs '89 (p.c., log)		−1.977***	
		(0.473)	
Inc. Nonentrepreneurs '89/'85 (p.c., log)		−0.444	
		(0.440)	
Share Entrepreneurs '89			−0.461***
			(0.106)
Share Entrepreneurs '89–'85			−0.156
			(0.122)
R^2	0.581	0.663	0.560
N	70	70	70
Standard region-level controls	yes	yes	yes

Notes. The table reports coefficient estimates from regressions at the regional level. Standard errors are given in parentheses. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. The dependent variable in column (1) is the log of the ratio of mean per capita income of entrepreneurs in 1995 and 1989. The dependent variable in column (2) is the log of the ratio of mean per capita income of nonentrepreneurs in 1995 and 1989. Columns (1) and (2) are estimated jointly with three-stage least squares (3SLS), instrumenting for *Share Expellees (Soviet Sector) '61* with *Share Housing Destroyed '46*. First-stage results are not reported. In column (3), which shows results from an instrumental variables (IV) regression, the dependent variable is the change in the share of the population who are entrepreneurs between 1989 and 1995. Again we instrument for *Share Expellees (Soviet Sector) '61* with *Share Housing Destroyed '46*. The standard errors in column (3) are calculated using the Huber-White correction to correct for potential heteroskedasticity. The main variable of interest in all columns is *Share Expellees (Soviet Sector) '61*. All regressions control for a region's distance to the inner-German border, the log of mean per capita income in 1989, and the log of the ratio of mean per capita income in 1989 and 1985. All regressions include 9 state fixed effects. Coefficient estimates on these controls are not reported to save space. For details on the construction of the variables see the Online Appendix. We reject the equality of the coefficients on *Share Expellees (Soviet Sector) '61* in column (1) and (2) (p-value = 0.070).

This strong effect on the income of entrepreneurs is mirrored by an increase in the number of entrepreneurs. In column (3) we rerun our standard specification but use the change in the share of the population who are entrepreneurs between 1989 and 1995 as the dependent variable, where we again add the pretrend in the change of this share and the level of this share in 1989 as additional controls. The coefficient of interest is 0.334 (std. err. = 0.163), implying that a 1 standard deviation rise in the share of expellees induces a 0.63 percentage point rise in the share of the population engaged in entrepreneurial activities.

This is a sizable effect, corresponding to a 14.7% rise relative to the mean share of entrepreneurs in 1989 (0.043).

V.B. Firm Investment

The significant rise in entrepreneurial activity in regions with strong social ties to the East suggests that firms based in these regions generated higher profits in the years following the fall of the Berlin Wall. One possible reason for such a rise in profitability is that locating in a region with strong social ties to the East may have generated a comparative advantage in investing in the East. We explore this possibility by examining the holdings of subsidiaries and branches of West German firms in East Germany.

We have data on 19,387 firms whose headquarters are located in West Germany. For these firms we construct a dummy variable that is 1 if the firm operates a subsidiary or branch in East Germany and 0 otherwise. Since West German firms could not own assets in East Germany prior to the fall of the Berlin Wall, any subsidiaries or branches that they operate in 2007 must have been acquired after 1989. Our dummy variable is thus informative both about the investment behavior of West German firms in East Germany since 1989 and about a possible long-lasting effect of social ties in 1989 on the economic structure of West Germany.

The structural equation of interest is

$$b_{kdr}^{07} = \delta T_{dr}^{89} + \mathbf{X}_{kdr}' \varsigma^{f} + \varepsilon_{kdr}, \tag{3}$$

where b_{kdr}^{07} stands for the dummy indicating whether firm k in West German district d and region r operates a subsidiary or a branch in East Germany in 2007. T_{dr}^{89} is again our proxy of social ties between the residents of district d in region r and East Germany in 1989; and $\mathbf{X}_{kdr}$ is a vector of firm- and district-level controls that contains a complete set of state fixed effects, a fixed effect for the sector in which the firm has its primary operations, the log of the number of subsidiaries and branches that firm k operates in West Germany, the distance between district d and the inner-German border, and the region's log income per capita in 1989. (Note that the latter variable is available only at the regional level and not at the district level.) We cluster all standard errors at the district level to account for likely spatial correlation.

The coefficient of interest is δ, which measures the effect of the intensity of social ties to the East in a given West German

district in 1989 on the probability that a firm headquartered within that district operates a subsidiary or branch in East Germany in 2007.[31] As in Section IV, we account for the possibility that our measure of social ties (the share of expellees via the Soviet sector settling in a West German region in 1961) is jointly determined with income growth by instrumenting T_{dr}^{89} with the share of housing destroyed in 1946. The first stage of our instrumental variables strategy is thus the analog to equation (2) at the district level.[32] Panel A of Table V shows reduced-form estimates, relating the share of housing destroyed in 1946 directly to the probability that a given firm operates a subsidiary or branch in East Germany in 2007. In column (1), we regress our dummy variable on the share of housing destroyed in the district and the log of the number of subsidiaries and branches that the firm operates in West Germany in 2007, which we use as a simple control for the size of the firm. The coefficient of interest is –0.030 (std. err. = 0.011) and statistically significant at the 1% level. The estimate implies that a 1 standard deviation rise in the share of housing destroyed in a West German district (0.24) is associated with a 0.7 percentage point drop in the probability that a firm based in that district operates a subsidiary or branch in East Germany in 2007. Unsurprisingly, the coefficient on our size control is positive and significant, reflecting the fact that larger firms are also more likely to operate in East Germany. Columns (2)–(5) add all of the now familiar district- and region-level covariates from Section IV, and column (2) gives the analog of our standard specification. Throughout, the coefficient of interest remains in a tight range between –0.026 and –0.030 and is statistically significant at the 5% level.

Panel B shows our instrumental variables estimates of equation (3), which use the variation in wartime destruction to quantify the causal effect of social ties in 1989 on the investment behavior of West German firms. All specifications contain the same covariates as those in Panel A. The estimates in all columns are positive and statistically significant at the 5% level. The estimate from our standard specification in column (2) is 1.496 (std.

31. We use a simple linear probability model, since this allows for a straightforward interpretation of the coefficient. The results are essentially unchanged when we use a probit estimator.

32. The first-stage results at the district level are almost identical those reported in Panel A of Table II. For example, the coefficient of interest in the first stage corresponding to column (2) of Table V is –0.019 (std. err. = 0.005).

TABLE V

SOCIAL TIES AND FIRM INVESTMENT

	(1)	(2)	(3)	(4)	(5)
Panel A: Reduced form		*S. & B. in East Germany (Dummy)*			
Share Housing Destroyed '46	−0.030***	−0.029**	−0.026**	−0.026**	−0.030***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
S. & B. in West Germany (log)	0.119***	0.119***	0.119***	0.119***	0.119***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Distance to East (100 km)		−0.013***		−0.014***	−0.015***
		(0.004)		(0.004)	(0.004)
Income 1989 (p.c., log)		0.002	−0.035	0.060	−0.003
		(0.030)	(0.037)	(0.064)	(0.030)
Income '89/'85 (p.c., log)		−0.010	0.038	−0.052	−0.019
		(0.050)	(0.051)	(0.056)	(0.051)
Migration from East '91–'95					−1.559
					(1.223)
Panel B: Second stage					
Share Expellees (Sov. Sector) '61	1.579**	1.496**	1.252**	1.316**	1.548**
	(0.689)	(0.673)	(0.583)	(0.639)	(0.671)
Panel C: Second stage		*S. & B. in Poland or Cz. Rep. (Dummy)*			
Share Expellees (Sov. Sector) '61	0.278*	0.295**	0.310**	0.314**	0.306**
	(0.146)	(0.144)	(0.141)	(0.148)	(0.140)
Panel D: Placebo		*S. & B. in Old EU Countries (Dummy)*			
Share Expellees (Sov. Sector) '61	0.060	0.403	0.417	0.549	0.400
	(0.580)	(0.506)	(0.471)	(0.462)	(0.498)
Panel E: Placebo		*S. & B. in New EU, exc. Poland or Cz. Rep. (Dummy)*			
Share Expellees (Sov. Sector) '61	0.220	0.213	0.201	0.198	0.248
	(0.197)	(0.205)	(0.192)	(0.222)	(0.199)
Panel F: Placebo		*S. & B. in Non-EU Countries (Dummy)*			
Share Expellees (Sov. Sector) '61	0.034	0.123	0.116	0.128	0.138
	(0.304)	(0.291)	(0.274)	(0.308)	(0.284)
N	19,387	19,387	19,387	19,387	19,387
Firm-level sector fixed effects	yes	yes	yes	yes	yes
Distance quartile fixed effects	—	—	yes	—	—
Region-level sector controls	—	—	—	yes	—

Notes. All panels report firm-level regression results, using our sample of firms that are headquartered in West Germany. Standard errors are clustered at the district level to account for likely spatial correlation. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. The sample contains 136 districts. Panel A reports results from firm-level ordinary least squares regressions. Panels B–F report results of firm-level instrumental variables regressions. The main variable of interest in these specifications is *Share Expellees (Soviet Sector) '61*. We instrument for this variable with *Share Housing Destroyed '46*. Corresponding first-stage results at the regional level are shown in Table II. The dependent variable in Panels A and B is a dummy indicating whether a firm has a subsidiary or branch in East Germany. The dependent variables in Panels C–F are dummies that indicate whether a firm has a subsidiary or branch in the specified location. All regressions include 9 state fixed effects and 4 firm-level sector fixed effects (agriculture, manufacturing, services, government). We control for distance to the inner-German border at the district level. Log of per capita income in 1989 and log of the ratio of per capita income in 1989 and 1985 are region-level controls. Column (3) controls for four distance dummies, corresponding to quartiles of the distance measure. Column (4) controls for the share of a region's 1989 population working in agriculture, manufacturing, services, and government, respectively. The specification in column (5) controls for the share of the region's population who are migrants arriving from East Germany between 1991 and 1995. All specifications in Panels B–E include the same controls as the specification in Panel A, which we do not report to save space.

err. = 0.673), which implies that a 1 standard deviation rise in the share of expellees via Soviet sector in a West German district (0.019) is associated with a 2.8 percentage point increase in the probability that a firm based in that district will operate a subsidiary or a branch in East Germany in 2007. (This corresponds to a 36% increase relative to the mean.)

The remaining panels of Table V show the results of a number of falsification exercises. If the pattern in holdings of subsidiaries and branches prevailing in 2007 is truly attributable to variation in social ties to East Germany in 1989, and not to some other factor correlated with firm investment, our measure of social ties to East Germany should predict investment in East Germany but not in other areas of the world.[33] Panels C–F repeat the same specifications as in Panel B, but with a dummy variable indicating whether a firm operates subsidiaries or branches in Poland or the Czech Republic, in the old EU countries (the 14 member countries other than Germany prior to the enlargement in 2004), in the new EU countries (the eight countries, other than Poland and the Czech Republic, that joined the EU in 2004), and in non-EU countries as the dependent variable. All estimated coefficients in Panels D, E, and F are statistically indistinguishable from 0. Firms that are based in districts with a higher share of expellees via the Soviet sector are thus *not* more likely to operate subsidiaries or branches in areas other than East Germany. Interestingly, however, the only exception from this rule is that the estimates for Poland or Czech Republic are positive and statistically significant at the 5% level in all columns. The estimated effect is about one-fifth the size of the effect estimated for East Germany. Because the largest group of expellees who settled in West Germany after 1945 actually came from areas that are today part of Poland and the Czech Republic, these results suggest a possible additional effect of social ties to Poland and the Czech Republic on the investment behavior of West German firms. (However, the size of the coefficient for Poland/Czech Republic is similar to that of some of the other, insignificant coefficients.)

In Online Appendix Table VIII we explore whether social ties may have been especially relevant for the investment behavior of

33. If firms from districts with a high fraction of expellees were merely good at capitalizing on new business opportunities, regardless of social ties, we might, for example, expect to see an effect on their holdings in other Eastern European countries following consecutive rounds of EU enlargement.

firms in any particular sector by interacting the share of expellees via the Soviet sector with each of the four sectoral fixed effects included in the specification. The estimated effects in the agriculture, government, and manufacturing sectors are statistically insignificant, whereas the effect estimated for the services sector is positive and statistically significant at the 5% level (2.121, std. err. = 1.012). We may interpret this as evidence that social ties were particularly important for firm investment in the services sector, which is arguably the sector of the economy most susceptible to informational asymmetries and reliant on knowledge of local demand. However, most of the firms in our sample have their primary focus in this sector such that we interpret this result with due caution. (Unfortunately it is not possible to further disaggregate the services sector with the data available.)

VI. Social Ties and Household Income

If the presence of households who have social ties to East Germany encourages local firms to invest in the East and has beneficial effects on income growth at the regional level, we would expect these households to internalize part of the income growth they generate at the regional level. In this section we estimate the direct effect of social ties on household income growth and use a "linear in means" model of social interaction to relate this effect to our region level estimates from Section IV.

In this model, the income growth of an individual household is a function of its own characteristics as well as a function of the average income growth of households in the region and of the share of households in the region who have social ties to the East:

$$\hat{y}_{ir}^{95} = \mu \hat{Y}_r^{95} + \beta t_{ir}^{89} + \sigma T_r^{89} + \mathbf{z}_{ir}' \zeta^{hh} + \mathbf{Z}_r' \zeta + \varepsilon_{ir}, \tag{4}$$

where $\hat{y}_{ir}^{95} = y_{ir}^{95} - y_{ir}^{89}$ denotes household income growth between 1989 and 1995 (y_{ir}^{t} again refers to log income of household i in region r and year t), and t_{ir}^{89} is the dummy variable indicating ties to East Germany. $\mathbf{z}_{ir}$ is a vector of household-level controls that contains log household income in 1989, household income growth between 1985 and 1989, and the region's distance to East Germany. The uppercase variables $\hat{Y}_r^{95}, T_r^{89}$, and $\mathbf{Z}_r$ refer to the regional averages of the corresponding household-level variables. In particular, we now interpret T_r^{89} as the share of the region's households that have social ties to the East.

While β measures the direct effect of ties to the East on the income of households who have these ties, σ and μ measure regional spill-over effects. The parameter σ measures the effect of indirect social ties, that is, the effect of the share of the region population who have ties to the East on the income of an individual household who itself may or may not have such ties. Indirect social ties affect household income in settings in which information or social collateral can be exchanged through friends of friends (higher order social ties), or in which neighbors of households with ties to the East are more likely to form their own social ties to the East.[34] The parameter μ measures the local economic multiplier, that is, the extent to which a rise in the average income of a given region raises the income of an individual household. Such local economic multipliers operate in settings in which a rise in household spending results in an increase in the demand for local nontraded goods and services.[35]

Averaging both sides of equation (4) across the households in each region and solving for $\hat{Y}_r^{95}$ yields our region-level specification (1).[36] In the light of this model, the region-level effect of social ties on economic growth, α, can be understood as a composite effect of the local multiplier and the direct and indirect effects of social ties, where

$$\alpha = \frac{\sigma + \beta}{1 - \mu}. \tag{5}$$

Though we cannot separately identify the two spill-overs σ and μ (this is the well-known reflection problem; Manski 1993), the remainder of this section uses household-level data to isolate the direct effect of social ties, β. Given an assumption about μ we will then also be able to learn about the indirect effect of social ties. (See Glaeser, Sacerdote, and Scheinkman 2003 for a closely related approach to estimating social multipliers.)

To facilitate the comparison of estimates of α and β, column (1) of Table VI reestimates equation (1) using the share of households who have ties to the East as proxy for T_r^{89}. The coefficient

34. The biography of an entrepreneur, which inspired this paper (Schulze 2005), presents an example of such interaction at the second degree of separation. For a formal argument see, for example, Karlan et al. (2009).

35. See, for example, Moretti (2010) and Mian and Sufi (2012).

36. For simplicity we abstract from the correction for Jensen's inequality and assume that the difference in the variance of log income between 1989 and 1995 is 0, such that we can write $\hat{Y}_r^{95} = Y_r^{95} - Y_r^{89}$.

TABLE VI

REGION- AND HOUSEHOLD-LEVEL INCOME

	Income '95/'89 (log)			
		Household (SOEP)		
Level (source)	Region (MZ)			
	(1)	**(2)**	**(3)**	**(4)**
Ties to Relatives '91		0.067***	0.058***	
		(0.024)	(0.021)	
Ties to Relatives '91 – Share Ties '91				0.076**
				(0.032)
Share Ties to Relatives '91	0.338*			1.116
	(0.197)			(2.509)
Income 1989 (log, SOEP)		−0.225***	−0.293***	−0.238***
		(0.028)	(0.035)	(0.033)
Income '89/'85 (log, SOEP)		−0.145***	−0.186***	−0.133***
		(0.035)	(0.032)	(0.034)
Distance to East	0.019			0.054
	(0.012)			(0.134)
Income 1989 (p.c., log, MZ)	−0.198**			−0.089
	(0.098)			(0.882)
Income '89/'85 (p.c., log, MZ)	−0.334**			−0.491
	(0.153)			(0.711)
Age '90			−0.018***	
			(0.005)	
(Age '90)^2			0.000**	
			(0.000)	
Gender			−0.076***	
			(0.024)	
N	70	1,857	1,857	1,729
Fixed effects	State	Region	Region	State

Notes. Column (1) shows results of a region-level instrumental variables regression. Columns (2)–(4) show results from household-level regressions. In columns (1) and (4) we instrument for the regional *Share Ties to Relatives '91* with *Share Housing Destroyed '46*. First-stage results for column (1) are shown in Table II. Columns (2) and (3) report results from ordinary least squares regressions. The dependent variable in column (1) is the log of the ratio of per capita income in 1995 and 1989 at the regional level. The dependent variable in columns (2)–(4) is the log of the ratio of household income in 1995 and 1989. Columns (1) and (4) control for the same region-level variables as column (3) in Table III. Columns (2) and (3) control at the household level for the log of household income in 1989 and the log of the ratio of household income in 1989 and 1985. Column (3) also controls for the gender, age, and age squared of the household head. The specifications in columns (1) and (4) include 9 state fixed effects. Standard errors are given in parentheses. Standard errors are clustered at the regional level, which for column (1) coincides with heteroskedasticity-robust standard errors. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. Columns (2) and (3) include 70 region fixed effects. See Online Appendix for details on the construction of variables.

estimate (0.338, std. err. = 0.197) is statistically significant at the 10% level and has almost identical quantitative implications as the estimates in Table III (a one standard deviation rise in the share of households with ties to the East (0.140) is associated with a 4.73 percentage point rise in income per capita). This estimate has the added advantage that the share of households with ties to the East is the regional average of the dummy indicating ties to

the East, which corresponds to the relationship between T_r^{89} and t_{ir}^{89} in (4).

VI.A. *Household-Level Estimates*

Panel C of Table I gives summary statistics for the panel of 1,857 households, and for the subsets of households that report and do not report ties to relatives in East Germany. The heads of households with ties to the East are less likely to be female (17% versus 26%) and have slightly higher income on average (DM 3,460 versus DM 3,236). However, the two subsets of households look similar on other observable dimensions, such as the amount of capital income, the share of household heads engaged in entrepreneurial activity, and the share unemployed.

We use two alternative approaches to estimating β. The first is a fixed-effects specification that uses only the variation in social ties within regions for identification. Subtracting region averages on both sides of equation (4) yields

$$\hat{y}_{ir}^{95} - \hat{Y}_r^{95} = \beta\left(t_{ir}^{89} - T_r^{89}\right) + (\mathbf{z}_{ir} - \mathbf{Z}_r)'\zeta^{hh} + \varepsilon_{ir} - \bar{\varepsilon}_r, \tag{6}$$

where $\bar{\varepsilon}_r$ stands for the region average of the error term ε_{ir} (it happens to be is identical to the error term in equation (1) multiplied with the constant $(1 - \mu)$).

Column (2) of Table VI estimates equation (6) using region fixed effects. The coefficient of interest is 0.067 (std. err. = 0.024), statistically significant at the 1% level. It suggests that households with ties to East Germany experience on average a 6.7 percentage point higher income growth over the six years following the fall of the Berlin Wall than comparable households in the same region who do not have such ties. Column (3) adds controls for gender, age, and age squared of the household head. This induces only minor changes in the coefficient of interest (0.058, std. err. = 0.021).[37]

37. This coefficient estimate is essentially unchanged for a range of different definitions of the dummy t_i that use information on friendships in the East and information on different intensities of contact between friends and relatives. Because our model contains a lagged dependent variable, there may again be a mechanical bias in the coefficient of interest. We cannot perform an Arellano-Bond–style estimation because we do not have enough pre-1989 data. However, if we drop the control for the preexisting growth trend, we can instrument for household income in 1989 with household income 1985. In this case the coefficient of interest is estimated to be 0.046 (std. err. = 0.023) and significant at the 5% level.

The identifying assumption of the specifications in columns (2) and (3) is that, conditional on the covariates we control for, there is no omitted variable that is correlated with the allocation of social ties within a given region and drives differential changes in income growth post-1989,

$$\text{(ID2)} \qquad Cov\left(t_{ir}^{89} - T_r^{89}, \varepsilon_{ir} - \bar{\varepsilon}_r\right) = 0.$$

As German reunification came as a surprise and social ties to the East had no economic value prior to the fall of the Berlin Wall, we do not believe that this condition could fail due to reverse causality. However, it may still fail if households with ties to the East are also more likely than other households to have some omitted characteristics that affect their income growth *differently* after 1989 than before 1989.

As a systematic approach to detecting any omitted variables, we select all relevant variables from the 1989 wave of the SOEP that pertain to education, occupational status, industry of employment, professional affiliation, union membership, asset holdings, and sources of income and regress them on our dummy variable indicating ties to the East as well as the control variables used in column (3) of Table VI. Conditional on these controls, there are no systematic differences between households with and without ties in the 26 variables we consider (see Online Appendix Table X). The only exceptions are that households with ties to the East are less likely to be employed in the government sector and they pay (but do not receive) significantly higher transfers to relatives living outside their household throughout the sample period. Online Appendix C shows that our results remain unchanged if we control for these and a range of other potentially interesting variables, such as educational attainment and whether the household head is an entrepreneur. In addition, the same appendix also shows that the results are robust to excluding all households from the sample that potentially received restitutions of assets from the East, and to using propensity score matching as an alternative estimator.

In Figure IV we verify that social ties had no effect on household income growth prior to 1989 by regressing household income in each year between 1985 and 2001 on the dummy indicating ties to the East and on our standard control variables. The figure shows the coefficients on t_{ir}^{89} from each of the regressions and a

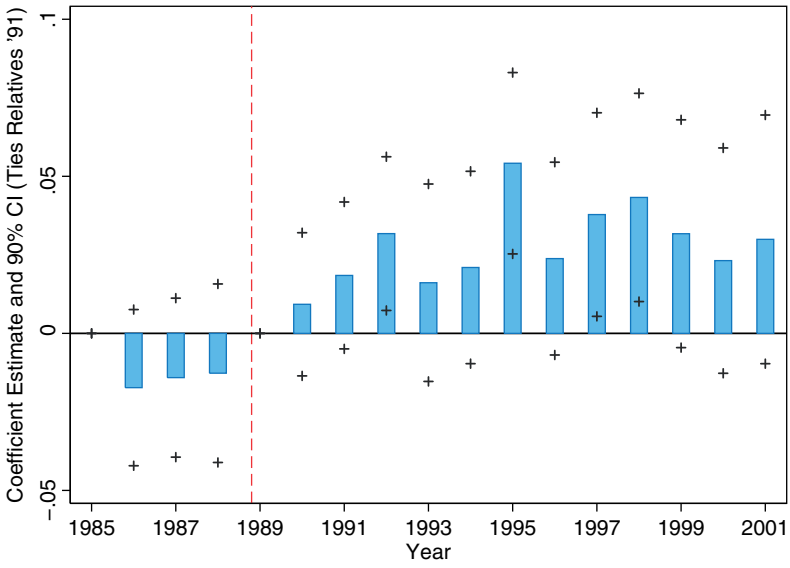

FIGURE IV

Effect on Household Income over Time

The figure depicts coefficient estimates (and 90% confidence intervals) on *Ties Relatives '91*. Each coefficient estimate is obtained from a regression corresponding to our standard specification in column (3) of Table VI, with the exception that the outcome variable is the log of household income in the year specified on the horizontal axis. The estimate for 1995 replicates the core result from our standard specification.

90% confidence interval.[38] Each coefficient measures the differential income growth of households with social ties to the East between 1989 and the indicated year, while controlling for income growth between 1985 and 1989. (This is why there is no coefficient estimate for these years.) The coefficients are negative and statistically indistinguishable from 0 until 1989, when they change sign and remain positive until the end of the sample. The coefficients for the years 1992, 1995, and 1998 are statistically significant at the 5% level and the coefficient for 1997 is significant at the 10% level. The timing of the effect is thus again supportive of the view that social ties to the East became relevant for income growth only after the fall of the Berlin Wall.

38. The sample size decreases monotonically from 1,857 in 1995 to 1,369 in 2001.

The alternative approach to estimating β is to drop the region fixed effects from equation (6) and instead estimate α and β jointly in our household-level data. Plugging equation (1) into equation (4), substituting for σ with equation (5), and simplifying yields

$$(7)\quad \hat{y}_{ir}^{95} = \alpha T_r^{89} + \beta\left(t_{ir}^{89} - T_r^{89}\right) + (\mathbf{z}_{ir} - \mathbf{Z}_r)'\zeta^{hh} + \mathbf{Z}_r'\varsigma + (\varepsilon_{ir} - \bar{\varepsilon}_r) + \varepsilon_r,$$

where $\varepsilon_r = \frac{1}{1-\mu}\bar{\varepsilon}$ and $\varsigma = \frac{\zeta^{hh}+\zeta}{1-\mu}$. Column (4) of Table VI estimates this specification while using the share of housing destroyed in 1946 as the excluded instrument for T_r^{89}. Given our identifying assumptions (ID 1) and (ID 2), this specification yields consistent estimates of both α and β. To see this, note that by construction $cov\left(T_r^{89}, \varepsilon_{ir} - \bar{\varepsilon}_r\right) = cov\left(t_{ir}^{89} - T_r^{89}, \varepsilon_r\right) = 0$. In addition, $cov(t_{ir}^{89} - T_r^{89}, \varepsilon_{ir} - \bar{\varepsilon}_r) = 0$ by assumption (ID 2). We again allow for endogenous assignment of households with social ties to the East across West German regions by instrumenting T_r^{89} with wartime destruction, which is a valid instrument under (ID 1).

Although the specification yields an estimate for β that is statistically significant at the 5% level and close to the ones in columns (2) and (3) (0.076, std. err. = 0.032), the estimate for α is statistically insignificant and around three times as large as the one in column (1). It thus appears that our household-level data lack the statistical power to identify the effect of social ties on regional income. This is not entirely surprising because our household-level data estimates region averages with considerable error (we have data for on average only 25 households per region). Instead, we therefore rely on the estimate of α from column (1) in the following discussion.

VI.B. Direct and Indirect Effects of Social Ties

Equation (5), the estimate of α from column (1) of Table VI (0.338), and the estimate of β from column (2) (0.067) jointly imply that

$$\sigma = 0.271 - 0.338\mu.$$

Online Appendix Figure IV plots this estimated relationship between the effect of indirect social ties on household income and the local multiplier along with 90% confidence intervals, which we calculate from a bootstrapped variance-covariance matrix of α and β.[39]

39. See the caption of Online Appendix Figure IV for details.

The estimated effect of indirect social ties on household income thus depends on the size of the local multiplier, which we cannot identify separately. However, we may take a guess based on the estimates of μ available in the literature. For example, Nakamura and Steinsson (2011) estimate that a 1% increase in regional government spending increases regional economic output by 1.5%. Assuming that West German households regard their income gains post-1989 as quasi-permanent, we may expect a marginal propensity to consume around 0.8, such that $\mu = 0.4$ might be a reasonable assumption. This would imply that $\sigma = 0.136$ (std. err. = 0.113). Other things equal, a direct social tie to the East thus has the same effect on individual household income as a $\frac{\beta}{\sigma} \times 100 = 49.3$ percentage point (or 3.5 standard deviation) increase in the regional share of households who have such ties.

Alternatively, we may interpret this estimate in the context of a network-based model in which households have a fixed number (say, 10) random links to other households in their region, accrue the benefit β if they have at least one first-degree (direct) tie to the East, and accrue the benefit $\kappa\beta$ from each second-degree (indirect) tie to the East. In such a model our estimates imply that $\kappa = \frac{\sigma}{10\beta} = 0.20$. Each second-degree link thus has about a fifth of the economic value of a direct tie to the East.

Both interpretations suggest that from the perspective of an individual household, the incremental benefit from a direct social tie to the East is large compared to the incremental benefit from higher order social interaction. As a result, households with direct ties to the East experience on average a rise in their household income that is more than twice as large than that of households without such ties.[40]

However, plugging our estimate of σ into equation (5) suggests that $\frac{\sigma}{(1-\mu)}/\alpha = 67.0\%$ of the effect of social ties on regional economic growth is attributable to indirect, rather than direct, social ties. The relatively smaller effect of indirect ties at the household level accounts for two-thirds of the region-level effect because it potentially affects the income of all, rather than just a subset, of a region's households.

40. The average of T_r^{89} across households with ties and without ties is 0.349 and 0.301, respectively. The average of the first two terms in equation (7) across households with ties relative to the average across households without ties is thus $\frac{(\alpha-\beta)0.349+\beta}{(\alpha-\beta)0.301} = 1.98$.

VI.C. *Heterogeneous Effects*

The wealth of information in the SOEP allows us to further explore the mechanism underlying the household-level effect of social ties and to distinguish it from two closely related notions of affinity between regions.

First, we may suspect that individuals who lived in East Germany during their youth retain knowledge about local economic conditions, which enables them to earn rents after reunification even if they do not have personal contact with East Germans. However, when we add a fixed effect for households in which at least one member reports to have lived in East Germany prior to 1961 to our standard specification (column (3) of Table VI), the coefficient on this fixed effect is negative and statistically insignificant (–0.057, std. err. = 0.045), while the coefficient on *Ties to Relatives '91* remains positive and statistically significant at the 1% level (0.068, std. err. = 0.021).

Consistent with this result, column (1) of Table VII shows that even households headed by individuals who were too young to remember living in East Germany experience a rise in their income after 1989. The specification shown again departs from our standard specification, but interacts the dummy for ties to relatives with a fixed effect for the age quartile of the household head (and also adds fixed effects for each age quartile on the right-hand side). The pattern suggests that all age groups, except those aged 40–51, profit similarly from their ties to the East. Importantly, the coefficient estimate for the youngest age quartile (those aged below 40 in 1989) is positive and significant at the 5% level, 0.121 (std. err. = 0.057). The household heads in this group were younger than 11 years old at the time when the Berlin Wall was built and thus could not have had much local economic knowledge about East Germany. However, they could keep in contact with their relatives in East Germany. Both pieces of evidence thus support our view that households profit from knowing people and not from knowing places.

Second, some of the behavior we documented at the regional level, such as an increase in entrepreneurial activity and a higher propensity of firms to invest in the East, might be explained if West Germany with social ties to the East were more optimistic about economic growth in the newly unified Germany. However, we find no evidence of systematic differences in expectations about aggregate economic development or individual job security

TABLE VII
HETEROGENEOUS EFFECTS

	Income '95/'89 (log)			
	(1)	**(2)**	**(3)**	**(4)**
Ties × Age Group below 40	0.121**			
	(0.057)			
Ties × Age Group 40–51	−0.028			
	(0.035)			
Ties × Age Group 52–62	0.076			
	(0.049)			
Ties × Age Group above 62	0.067**			
	(0.034)			
Ties to Relatives '91		0.050**	0.051**	0.050**
		(0.024)	(0.024)	(0.020)
Ties × Capital Income '89 (75th cent.)		0.031		
		(0.046)		
Ties × Capital Income '89 (95th cent.)			0.031	
			(0.053)	
Ties × Entrepreneur '89				0.168**
				(0.078)
R^2	0.285	0.277	0.276	0.276
N	1,857	1,857	1,857	1,857

Notes. The table reports coefficient estimates from weighted least squares regressions at the household level. The inverse of the sampling probability provided by SOEP is used as weights. Standard errors, clustered at the region level to account for spatial correlation, are given in parentheses. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. The dependent variable is the log of the ratio of household income in 1995 and 1989. All regressions include the same controls as column (3) of Table VI (not reported). In column (1) the variables of interest are the interactions of the dummy *Ties to Relatives '91* with four exhaustive cohort dummies, for the age quartile of the household head (below 40, between 40 and 51, between 52 and 62, and above 62). The specifications in columns (2) and (3) use *Ties to Relatives '91* as explanatory variable and control for a dummy indicating whether the household has capital income above the 75th or 95th percentile of the capital income distribution, respectively, as well as the interaction of the dummy and *Ties to Relatives '91*. In column (4) we control for whether the household head was an entrepreneur in 1989, and the interaction of this dummy and *Ties to Relatives '91*. See the Online Appendix for details.

between households with and without ties to the East (see Panel C of Online Appendix Table X).

Another interesting question is whether households who are wealthier were able to benefit more from their social ties to the East. (We would expect this to be the case if social ties act primarily as conduits for borrowing and lending in an environment in which East Germans have little access to credit markets.) In column (2) of Table VII we again depart from our standard specification and add the interaction of the ties to relatives dummy with a dummy that is 1 if the household's capital income in 1989 is above the 75th percentile (we again add the main effect of this

dummy on the right-hand side). The estimate on the ties to relatives dummy is very similar to that in our standard specification (0.050, std. err. = 0.024). However, the interacted variable remains insignificant, suggesting that West German households benefited from their ties to East Germans regardless of their wealth in 1989. Column (3) shows similar results, using the 95th percentile of household capital income.

In column (4) we add a fixed effect for household heads who are entrepreneurs and the interaction of this fixed effect with the dummy indicating ties to the East. While again the main effect of ties to the East remains stable, the interaction is now positive and statistically significant at the 5% level (0.168, std. err. = 0.078). The size of the coefficient on the interaction term is roughly triple the size of the coefficient on the main effect of social ties, suggesting that ties to the East on average raise the income of entrepreneurs four times as much as they raise the income of nonentrepreneurs. This relative effect is thus very similar to the one we find in the region level regressions of Table IV.

VI.D. East German Households

Throughout the article we have focused on the economic effects of social ties on outcomes in West Germany. An obvious question is what effects the same social ties have in East Germany. Unfortunately, we have no information on where in East Germany migrants lived before migrating to the West or on the degree of wartime destruction across East Germany. Therefore we cannot replicate our region-level results for East Germany. However, we can replicate part of our standard specification in column (3) of Table VI for households in East Germany. In particular, Online Appendix Table XI shows results of regressions relating log income of East German households in the years after German reunification to a dummy variable indicating relatives in West Germany in 1991 as well as our standard household-level covariates. Because we have no data on income before 1989, these specifications do not control for the pretrend in income growth. Nevertheless, we find that the estimate on the coefficient of interest is similar in magnitude to our estimates for the income gains of West German households. The coefficient is positive in all years between 1990 and 1995 and marginally significant in one of the six years (1992). The results are thus consistent with the view that East German households with ties to

the West have higher income growth than those without ties to the West.

VII. Conclusion

The question of how social ties between individuals relate to the capacity of societies for economic growth is of great importance in economic theory. Theorists in many fields are beginning to incorporate network-based interactions into their models. Showing that the pattern of social ties between individuals affects aggregate economic outcomes highlights the relevance of their work beyond the microeconomic context and facilitates a deeper theoretical understanding of otherwise puzzling correlations between measures of affinity between regions and aggregate economic outcomes, such as foreign direct investment, international asset flows, and trade.

Empirical work, however, has found it difficult to resolve a double reverse causality problem: both the decision of individuals to form social ties and the regional distribution of individuals who make these decisions are endogenous to economic activity.

In this article we are able to solve both layers of this reverse causality problem in the context of the natural experiment surrounding German reunification. We show that West German regions that (for idiosyncratic reasons) have a high concentration of households with social ties to the East in 1989 exhibit substantially higher growth in income per capita after the fall of the Berlin Wall. Moreover, we are able to provide evidence on the microeconomic underpinnings of this effect. We show that households who have social ties to the East in 1989 experience a persistent rise in their personal incomes and that their presence increases returns to entrepreneurial activity at the regional level as well as the likelihood that local firms invest in East Germany. These findings appear robust to a wide range of plausible variations in the estimation strategy and placebo treatments. They show that social ties between individuals can indeed facilitate economic growth.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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