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Persecution and Escape: Professional Networks and High-Skilled Emigration from Nazi Germany†

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We study the role of professional networks in facilitating emigration of Jewish academics dismissed from their positions by the Nazi government. We use individual-level exogenous variation in the timing of dismissals to estimate causal effects. Academics with more ties to early émigrés (emigrated 1933–1934) were more likely to emigrate. Early émigrés functioned as "bridging nodes" that facilitated emigration to their own destination. We also provide evidence of decay in social ties over time and show that professional networks transmit information that is not publicly observable. Finally, we study the relative importance of three types (family, community, professional) of social networks. (*JEL* I31, J44, N34, N44, Z12, Z13)

Throughout history, academics have been persecuted because of their ethnicity, political views, or religion. Possibly, the most prominent example is the persecution of Jewish academics in Nazi Germany. Academics of Jewish origin in Weimar Germany were some of the greatest scientific luminaries of the twentieth century. Nobel laureates such as Albert Einstein and Max Born shaped modern physics, while Fritz Haber made pathbreaking chemical discoveries. Indeed, the list of prominent Jewish academics cut across disciplines and included mathematicians such as John von Neumann and Emmy Noether, social scientists and philosophers such as Hannah Arendt and Theodor Adorno, and one of the world-leading art

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historians: Erwin Panofsky. German universities, especially Berlin and Göttingen, were among the world's best in many disciplines.

This flourishing academic culture was forcefully interrupted in 1933 when the Nazi Party came to power. Jewish academics were targeted with class boycotts, sporadic violence, and mass dismissal. By 1939, around 20 percent of *all* German academics had lost their position (Hartshorne 1937; Grüttner and Kinas 2007). The increasing persecution in Nazi Germany and the threat of deportation to camps meant that Jewish academics scrambled to escape through emigration.[1] The United States and the United Kingdom received a disproportionate share of world-class academics, solidifying the transition of scientific leadership from Germany to the United States. In physics, they were instrumental to the success of the Manhattan project (Figure 1). For mathematics, Raymond Fosdick, the president of the Rockefeller Foundation, argued that: "If Hitler had set out, with benevolent intent, to build up America as the world's great mathematical center, he could hardly have achieved more successfully the result which his ruthlessness has accomplished." The Institute for Advanced Study (IAS Princeton), Brown University, New York University (NYU), Harvard University, University of Chicago, University of Wisconsin, and the Massachusetts Institute of Technology (MIT) are only a few of the American institutions that have profited by this migration (Rockefeller Foundation 1942, 27).

In this paper we study the role of *professional* networks in helping German Jewish academics escape through emigration. In addition, our rich data allow us to empirically distinguish between three different kinds of social networks—professional, family, and (nonfamily) community networks— and study their relative importance. Furthermore, we study the aspects of a network's social capital that made it especially effective in facilitating emigration.

We illustrate the role of professional networks using the example of Richard Courant, a world-leading mathematician at the University of Göttingen. After the Nazis gained power, he was placed on leave. Courant left Göttingen in 1933 and spent a year at Cambridge before moving to NYU. "In spite of Courant's own troubles [to secure a permanent position] … he continued to be the person other professors … turned to for help … Letters asking for help and advice came 'by the dozens'" (Reid 1996, 159). Figure 2 and Table A1 in the online Appendix illustrate Courant's role as a "bridge" between the German and Anglo-Saxon academic networks. For example, he secured a temporary position at the University of Cambridge for Fritz John. After Courant had moved to the United States, he helped to secure a permanent appointment for John at the University of Kentucky and later brought him to NYU. Courant was also instrumental in helping others from his professional network (see Figure 2).

Estimating the effect of professional networks on the probability of emigration faces two challenges. First is the measurement of academic networks and the

[1] In the following years, they were joined by persecuted academics from other European countries, e.g., the future physics Nobel laureate Enrico Fermi who escaped from Fascist Italy. Beyond academia, the emigration wave included such intellectual giants as Bertolt Brecht, Elias Canetti, Lion Feuchtwanger, Franz Werfel, and Stefan Zweig, to name just a few.

Panel A. All key scientists

Panel B. Without émigrés from Europe

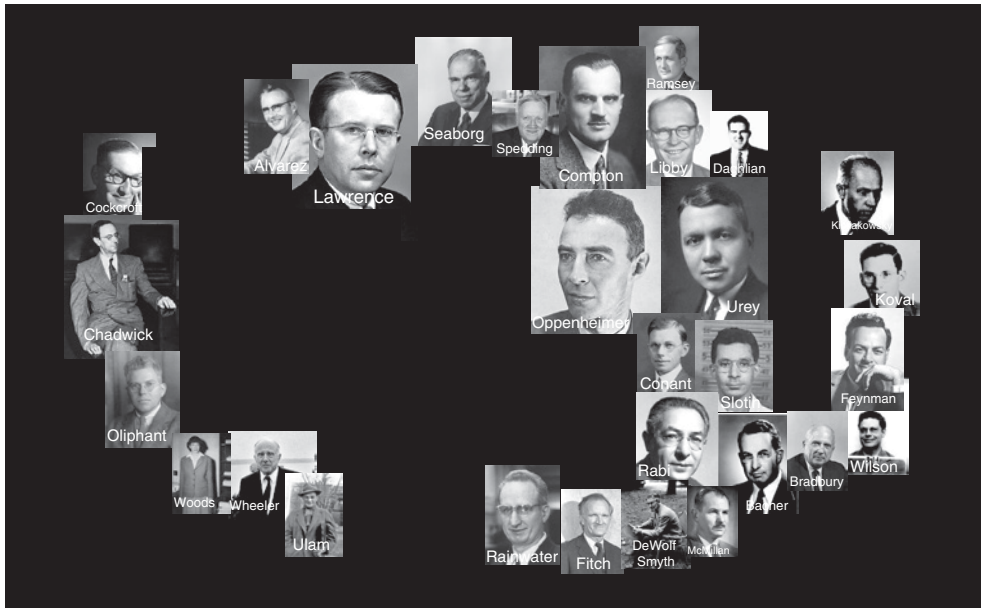


FIGURE 1. KEY SCIENTISTS INVOLVED IN THE MANHATTAN PROJECT

Notes: The figure underlines that émigrés from Europe made key contributions in their destinations. Panel A reports all scientists who were key for the success of the Manhattan Project, the research and development that produced the first nuclear weapons. Panel B excludes émigrés from Europe. The list of scientists comes from en.wikipedia.org/wiki/Manhattan_Project, which includes links to the most important scientists who were involved in the Manhattan Project. The size of the pictures reflects the importance of each scientist for the success of the project. For more details see Rhodes (1986). The Atomic Heritage Foundation argued that "[o]ne of the ironies of Hitler's desire for racial purity was that it drove out of continental Europe or into the camps many individuals who would have been extremely useful to the Axis war effort. Nowhere was this more evident than in the effort to produce an atomic bomb. A startling proportion of the most famous names on the project belonged to scientists who came to England or America to flee from the Axis" (see https://www.atomicheritage.org/article/scientist-refugees-and-manhattan-project).

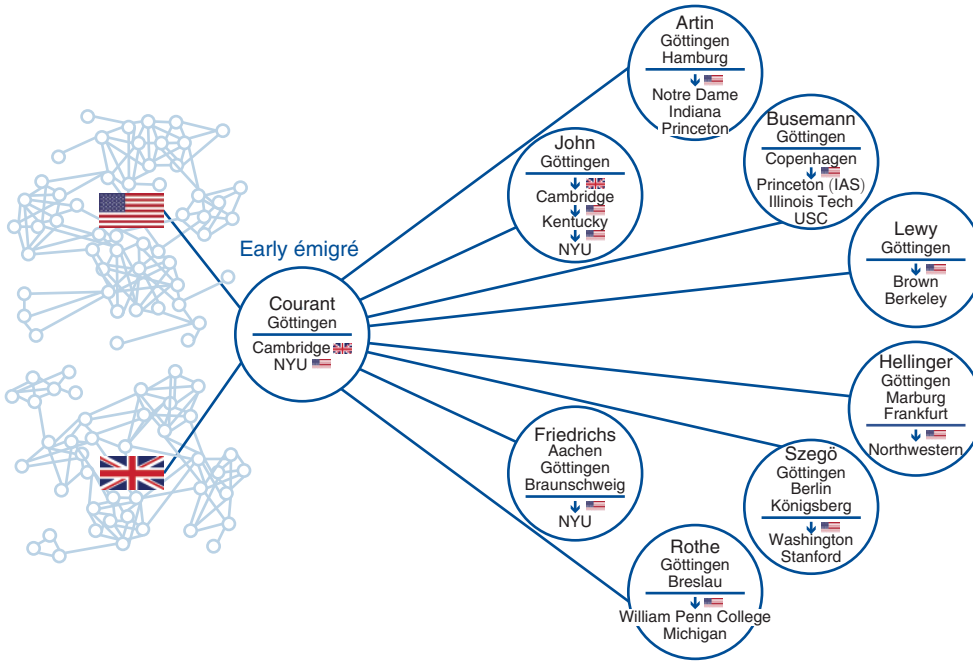


Figure 2. Richard Courant's Involvement in Securing Faculty Positions

Notes: The figure depicts professional ties for which we found explicit documentary proof (e.g., letters, testimonials, etc.) of Courant's role in faciliating emigration. For example, he secured a temporary position at the University of Cambridge for Fritz John. In his letter of support, Courant recommended him "in the strongest possible way" and argued that John combined "extraordinary gifts of the receptive kind with real originality and tenacity" (Shields 2015, 54). Courant also helped his former colleague Kurt Friedrichs who had moved to the University of Braunschweig. He "wrote letters about Friedrichs […] to everyone he knew who was interested in the development of applied mathematics. He […] presented him as "a mathematician in the style of C. Runge" (Reid 2013, 196). Courant was also instrumental in helping others from his professional network to secure positions in a wide range of institutions across the United States and the United Kingdom. Courant's involvement is represented by the small blue arrows in the figure. Friedrichs and Artin were not of Jewish origin but were persecuted because they had a Jewish wife. Table A1 in the online Appendix reports details on Courant's help to mathematicians in his network.

identification of individuals that acted as "bridging nodes" who became vital conduits of information and acted as a bridge between the domestic and the foreign academic networks. Second, networks may be endogenous because a) academics may form ties to facilitate emigration, and b) network measures may be correlated with omitted variables that enable emigration.

In order to tackle these challenges, we hand-collect rich biographical data from numerous primary and secondary sources for the *universe* of academics in Germany with a Jewish heritage. The reconstructed biographies cover not only famous but also unknown academics of Jewish origin. The data allow us to reconstruct the pre-dismissal professional network for all academics. We define the professional network as all Jewish academics who worked in the same subject and city between January 1, 1929 and January 1, 1933. E.g., for physicists who were affiliated with the University of Göttingen at any point between 1929 and 1933, we consider all

other physicists who overlapped with them in Göttingen.[2] The data are unique for studying the role of networks in migration decisions because we can measure yearly snapshots of the pre-emigration professional network. This enables us to exploit variation in the number of ties that come from pre-dismissal academic turnover. E.g., we exploit that physicists may have joined or left the University of Göttingen at different points between 1929 and 1933. Hence, they may have overlapped with slightly different sets of colleagues. This allows us to carefully control for other factors that may affect migration decisions. As highlighted by Richard Courant's example, émigrés who left Germany very soon after the Nazis gained power may have been a key factor in emigration decisions because they could facilitate information flows between the pre-emigration network of Jewish academics and foreign networks. Accordingly, we focus on ties to *early émigrés* from an academic's pre-dismissal professional network, where early émigrés are defined as academics who had emigrated by January 1, 1935.

Even if we use variation from pre-dismissal academic turnover, the number of ties to *early émigrés* may be endogenous. Individuals with more ties to early émigrés may also have other characteristics that facilitate emigration. E.g., academics who worked in multiple departments (either because they are of the "restless" type or because they are in high demand because of their ability) had more ties to early émigrés and were also more likely to emigrate. We therefore exploit individual-level exogenous variation in the timing of dismissals created by the *Law for the Restoration of the Professional Civil Service*, which was passed on April 7, 1933. Crucially for our identification strategy, the Law made important exemptions that initially allowed *some* Jewish academics to remain in their positions. After the Nuremberg racial laws in September 1935, the exemptions were revoked. The differential timing of dismissals created quasi-exogenous variation that pushed some individuals to emigrate early. This allows us to use the number of *ties to colleagues dismissed early* (dismissed 1933–1934) as an instrumental variable (IV) for the number of *ties to early émigré colleagues*. Importantly, the IV exploits early dismissals of academic's colleagues, *not* his/her own early dismissal. We show that academic *i*'s characteristics and, in fact, academic *i*'s own early dismissal status are not related to the number of early dismissals in his/her network.

Our first set of results shows that networks with more ties to "bridging nodes" facilitated emigration. Academics with 10 additional ties to early émigrés had a 5 percentage point higher probability to emigrate by 1939, an effect that persisted until 1945.[3] When we use the number of ties to academics dismissed early as an IV for the number of ties to early émigrés, we estimate a very similar effect. Crucially, in all regressions, we control for variables that may affect emigration and, at the same time, be correlated with ties to colleagues dismissed early. The controls include not only standard individual-level characteristics, such as age, gender, marital status,

[2] For academics in most cities, this measure captures their department. For academics in cities with multiple institutions, the measure captures the broader academic network of academics in the same subject and city, e.g., all Jewish physicists in Berlin. We show that the effects are somewhat larger if we measure colleagues at the department level for all cities.

[3] The mean and standard deviation of the number of early émigrés in an academic's network are 11.21 and 14.04, respectively.

and the number of children, but also characteristics such as academic reputation, academic rank, foreign languages spoken, pre-1933 employment outside Germany, and whether the academic was born outside Germany. Moreover, we control for the city × subject *employment history* of each academic in the five years before January 1, 1933. The employment history controls for many factors that may have an independent effect on emigration decisions and that may be correlated with the number of early dismissals in an academic's network. For example, they control for the *total* number of Jewish, but also non-Jewish colleagues that may assist emigration, even if these colleagues had not emigrated abroad. They also control for differences in average characteristics of colleagues in the same department. For example, physicists in Göttingen may have similar characteristics (driven by homophily), e.g., more contacts abroad or higher academic reputation, that affect migration decisions. Similarly, they control for community level factors that may affect emigration decisions. With these controls, the identifying variation comes from academic turnover during the pre-1933 period.

In the second set of results, we show that the effect of the professional network was directional. Early émigrés to the United States or the United Kingdom *increased* the probability of emigrating to these countries. In contrast, they *decreased* emigration to other countries. Similarly, early émigrés to other countries *increased* emigration to other countries but *decreased* the probability of emigrating to the United States or the United Kingdom. We also show suggestive evidence that early émigrés increased the probability that academics in their professional network emigrated and worked at the same foreign university. These results underscore the notion that early émigrés functioned as a bridge that helped academics cross over into the *same* destination. In the process, these academics were diverted away from other destinations.[4]

In our third set of results, we analyze characteristics of social ties that make them more or less effective in facilitating emigration. We provide some of the first systematic evidence that the strength of social ties "decays" over time. We find that ties to more recent colleagues were twice as important as ties to less recent colleagues. Furthermore, we find suggestive evidence that social ties decay with geographical distance, even within cities. In particular, we differentiate between ties to early émigrés from the same subject in the same department versus other departments in the same city. Our results suggest that ties to early émigrés from the same department had a larger effect on emigration than ties to early émigrés from other departments in the same city.

Theory would suggest that networks are more important when market participants have private information and credible signaling through publicly observable channels is difficult. Hence, networks should matter more for transmitting information that is not publicly observable through other channels, such as CVs or publication lists. We thus investigate the strength of social ties between broad scientific areas that differ in how easily outsiders can assess the individual quality of researchers. We show that ties to early émigrés were more important in humanities and social sciences than in natural sciences and medicine. These findings are consistent with the observation

[4] These results highlight the importance of bridging nodes, "the most difficult measure of social capital to calculate in a network" (Jackson 2020) for migration decisions.

that research quality in the hard sciences can be more objectively assessed than in the humanities and social sciences. We also find evidence that the effect of professional networks is larger in fields where academics publish longer but fewer works. In these fields, early émigrés may provide valuable information about the expected future research productivity of their former colleagues. Finally, we show corroborating evidence that professional networks may be useful at relaying information about "surprise" changes in productivity, i.e., about academics becoming a lot more (or a lot less) productive, compared to their pre-1933 reputation. In contrast, we find that professional networks do not differentially affect emigration probabilities by relaying information that is *publicly* available through CVs: networks do not differentially affect emigration for people of different ages, different pre-1933 experience at foreign universities, and even different pre-1933 productivity that would be observable on publication lists. Overall, these results indicate that professional networks are more important in situations where they provide private information about the quality of candidates that is difficult to observe from a distance.

Our fourth set of results compares the effect of *professional* networks to the effects of *family* or (*nonfamily*) *community* networks. The latter have been the focus of most empirical papers on networks and migration. We proxy family networks using data from the *List of Jewish Residents* compiled by the German Federal Archive. For our family network measure, we count the number of early émigrés with the same last name from the city of residence of each academic. Similarly, we construct a measure of nonfamily community networks that counts the number of early émigrés with a *different* last name from the city of residence of each academic. We find that early émigrés from the family network also affected emigration, but with a somewhat smaller magnitude than the professional networks. In contrast, community networks did not affect the emigration decisions of academics *at all*. This is striking because Buggle et al. (2023) find sizeable effects of community networks (using the same data source to measure community networks) for emigration from Nazi Germany in the *general* Jewish population. Our results suggest that different types of networks matter for the emigration decisions of *high-skilled* migrants than for migrants overall. Hence, analyses of the role of networks in migration decisions of high-skilled individuals would be incomplete if they ignored professional networks.

Finally, our paper provides the first comprehensive documentation of the fate of academics of Jewish origin during the Nazi period. The documentation allows us to pay homage to this exemplary group of academics. Concretely, we complement and complete the selective historical research by constructing the first *full census* of academics of Jewish origin including detailed records of their fate.[5] In striking contrast to the fate of the general German Jewish population, we unearth the surprising finding that 94 percent of Jewish academics escaped the Holocaust. It is important to highlight that even for these 94 percent, the Nazi period was a terrible experience. Their personal lives were shattered, their careers were forcefully upended, and many lost relatives and close friends in the Holocaust.

[5] Simultaneously to our data collection, Grüttner (2022) has collected similar data with more limited coverage of academic institutions in Germany. For instance, Grüttner's data exclude dismissals from technical universities and research institutes such as the Kaiser-Wilhelm-Institutes.

Our findings contribute to the literature on networks in economics by providing some of the first empirical evidence that social ties "decay" over time and that networks facilitate the transmission of private information.[6] Following the seminal work of Granovetter (1973), much of the research in economics classifies ties as either strong or weak. Ours is some of the first work in economics to empirically demonstrate that the absence of social interaction can, *over time*, result in the "natural" decay of strong ties into weak ties. Estimating decay in networks requires measuring the networks at multiple points in time. Most existing data on networks, however, is static. Dynamically measuring the evolution of pre-1933 professional networks is a key strength of our paper. While an extensive literature has studied network formation (e.g., Jackson and Watts 2002; Jackson and Rogers 2005; Galeotti and Goyal 2010), the decay of networks over time has received less attention in economics.[7] A notable exception is Banerjee et al. (forthcoming) who show that the introduction of microfinance reduces social ties in rural India, even between individuals who are unlikely to obtain microfinance.

We also contribute to the empirical literature on networks in the migration context. Existing papers usually study aggregate measures of family and community networks for *low-skilled* migrants, especially from developing countries (e.g., Munshi 2003; Winters et al. 2001; McKenzie and Rapoport 2010; Mahajan and Yang 2020).[8] More specific to our context, Buggle et al. (2023) show that emigration of members of the community network and Nazi violence increased emigration of German Jews.[9] We make four contributions to this literature. First, our rich data allow us to construct yearly snapshots of professional networks. This allows us to cleanly identify network effects by exploiting temporal variation in professional networks. Second, we introduce a new identification strategy that uses *individual-level exogenous* variation in the emigration decisions of colleagues in the network. Third, this is one of the first papers that estimates the effect of *professional* networks on migration decisions. Fourth, this paper takes a first step at analyzing the impact of a *multiplicity* of social networks in which an individual is embedded, by examining whether professional, family, and community networks matter for migration decisions.[10]

Our findings also speak to the literature on the effects of high-skilled migrants for science and innovation in the host economy (e.g., Hunt and Gauthier-Loiselle

[6] Goyal (2009), Jackson (2010), and Jackson et al. (2017) provide comprehensive surveys of the literature on networks in economics.

[7] Decay of social ties has, however, been discussed in sociology (e.g., Burt 2000, Burt 2001). Decay of ties is akin to a decline of social capital (Putnam 2000).

[8] Other papers study how, after emigration has taken place, immigrants from the same country of origin affect labor market outcomes (e.g., Edin et al. 2003; Damm 2009; Dustmann et al. 2016; Battisti et al. 2022).

[9] While not studying the role of networks, Blum and Rei (2018) show that Jews who escaped the Holocaust were from higher socioeconomic backgrounds (proxied by height) than non-Jews who remained in Europe. Recent papers examining the effects of persecution on migration in other contexts include Becker et al. (2020), Sarvimäki et al. (2022), and Becker and Ferrara (2019).

[10] Since the seminal work of Polanyi (1944) and Granovetter (1985), social scientists have emphasized the importance of an individual's embeddedness in a social context. The term "embeddedness" was coined by Karl Polanyi. He was born into a Jewish family in Vienna and became the editor of the liberal magazine *The Austrian Economist.* After the Nazis rose to power in 1933 and the establishment of the Fatherland Front government in Austria, he was forced to resign. He emigrated to London in 1933 and to the United States in 1940. Karl Polanyi is not part of our data because we focus on Jews who were academics in Germany. However, our data contain his brother, the polymath Michael Polanyi, who worked at the Technical University and the Kaiser-Wilhelm-Institute of Physical Chemistry in Berlin and made important contributions to chemistry, economics, and philosophy.

2010; Kerr and Lincoln 2010; Borjas and Doran 2012; Moser, Voena, and Waldinger 2014; Kerr, Kerr, and Lincoln 2015; Beerli et al. 2021) and to the literature on historical migration to the United States (e.g., Abramitzky, Boustan, and Eriksson 2012, 2014; Abramitzky et al. 2023; Bandiera et al. 2019; Sequeira, Nunn, and Qian 2020; Tabellini 2020; Fouka, Mazumder, and Tabellini 2022; Arkolakis, Peters, and Lee 2019).

Finally, our work relates to research on the consequences of losing high-skilled Jewish teachers (Akbulut-Yuksel and Yuksel 2015), mathematicians, physicists, and chemists (Waldinger 2010; Waldinger 2012), managers (Huber, Lindenthal, and Waldinger 2021), and doctors (Liebert and Mäder 2020) or gaining chemists in the United States (Moser, Voena, and Waldinger 2014). Compared to this earlier work, we innovate in four ways: 1) we focus on the dismissed Jewish academics *themselves* and not on their peers or students, 2) we reconstruct a *census* of *all* Jewish academics recording each year of their academic career and documenting their fate, covering *all* academic disciplines, 3) we study the role of networks in facilitating emigration, and 4) we develop a novel identification strategy that exploits differences in the timing of dismissals.

I. Historical Overview and Data

A. *The Dismissal of Jewish Academics*

After seizing power in January 1933, the Nazi government passed the *Law for the Restoration of the Professional Civil Service* on April 7, 1933. This *Civil Service Law* had a dramatic effect on the life of Jewish academics in Germany. It was used to expel the first wave of individuals of Jewish origin from civil service positions. In later years, remaining Jewish academics were dismissed so that by 1939 virtually all Jewish academics had lost their position.[11] Many considered emigration to flee from Nazi persecution and to find a university position abroad.[12]

Roster of All Dismissed Jewish Academics.—We construct a census of all dismissed Jewish academics across all academic disciplines from a large number of primary and secondary sources. The data include not only German Jews but also foreign born academics who worked at German universities, e.g., the Hungarian Nobel laureates Eugene Wigner and George de Hevesy, the Swiss Nobel laureate Ernst Bloch, and the musicologist and pioneer of atonal and twelve-tone music Arnold Schönberg from Austria. We refer to academics with at least one Jewish grandparent as "Jewish academics," consistent with the Civil Service Law. The

[11] It is important to note that dismissal did not imply emigration. Throughout the 1930s, there were no formal restrictions to emigrate from Nazi Germany. However, if emigration had taken place or was deemed imminent, the Nazis used the so-called "Reich Flight Tax" to confiscate Jewish citizens' assets.

[12] For those who did not emigrate, persecution dramatically increased over time. In October 1940, 7,000 Jews from southern Germany were deported to labor camps in southern France (Kwiet 1988, 634); some of the deportees were still permitted to emigrate. A tragic case is that of Robert Liefmann, an economist from the University of Freiburg. He was deported to the Gurs internment camp in southern France and died due to ill health. Tragically, he was just about to emigrate to the United States, to accept a position at NYU (Wiehn et al., 1995, 72). By October 1941, Jews were no longer allowed to emigrate, and the Nazis started the systematic deportations to death camps.

main source is the List of Displaced German Scholars (LDS), which was first published in 1936 and updated in 1937. Some dismissed academics did not appear on the LDS, for example, because they had died before the LDS was compiled. To obtain a complete picture of all dismissals of individuals of Jewish origin, we augment and cross-check the LDS roster against 60 university-specific and 16 subject-specific studies on the dismissals (online Appendix B.1 provides details). Combining the information from all sources, we obtain a roster of 1,370 dismissed Jewish academics.

Biographical Information on the Careers of Academics.—We reconstruct each biography covering each year of the academic's career with extensive archival and digital searches. The main sources are the LDS, the university- and subject-specific studies, biographical archives (e.g., Kürschners Deutscher Gelehrten-Kalender, Juden in Preußen, British Biographical Archive, Polskie Archiwum Biograficzne, Archivo Biográfico de España, Portugal e Iberoamérica, and the Indian Biographical Archive), shipping lists, naturalization records, newspaper articles, obituaries, death records, patents, and publications (online Appendix B provides further details). Even though some of the academics are hard to trace, we obtain almost complete biographical records.

To ensure consistency, we collect information on the exact location for each academic as of January 1 for each year. For the four dates that form the core of the empirical analysis (1929–1933, 1935, 1939, and 1945), we are able to obtain exact locations for 1,327 academics, 97 percent of all 1,370 dismissed academics.[13] Table 1 reports summary statistics.

Reconstructing Professional Networks for the Period 1929–1933.—We use the yearly snapshots (for the period 1929–1933) of the location of each academic to reconstruct the complete pre-dismissal academic network. These snapshots allow us to measure how many colleagues in each academic's network emigrated early (by January 1, 1935) and how many were dismissed in the first years after the Nazis rose to power.

B. *Fate after* 1933*: Emigration?*

Our main outcome variable is an indicator for emigration by January 1, 1939 or January 1, 1945.[14] By January 1, 1939, 74 percent of Jewish academics had managed to emigrate (Figure 3, panel A).[15] By January 1, 1945, 81 percent had

[13] Results are almost identical if we impute the most likely locations for the remaining 3 percent (online Appendix F.1).

[14] We choose January 1, 1939, because it was the last January before the beginning of WWII, and January 1, 1945, because it was the last January before the end of WWII. A few academics survived the Holocaust in concentration camps but emigrated after WWII. Hence, measuring emigration by January 1946 would not capture whether academics escaped the Holocaust.

[15] Of the 1,327 Jewish academics, 107 (310) had passed away by 1939 (1945). Some had been murdered in the Holocaust, while most of the others died of other causes, such as heart attacks or cancer. Even deaths from other causes may have been a result of persecution from the Nazis. E.g., the Nobel laureate Fritz Haber, one of the inventors of the Haber-Bosch process, died of heart failure while emigrating to Mandatory Palestine. To avoid sample

Table 1—Summary Statistics

	(1) Mean	(2) Standard deviation
Panel A. Individual characteristics		
Age in 1933	43.91	12.68
Female	0.04	
Married	0.78	
Number of children	1.05	1.27
Any foreign language	0.82	
Pre-1933 professional experience abroad	0.06	
Born abroad	0.19	
Pre-1933 quality[a]	1.18	1.59
Pre-1933 publication record[b]	0.68	0.54
Panel B. Network characteristics		
# Early émigré colleagues (pre-1933 network)	11.21	14.04
# Colleagues dismissed early (pre-1933 network)	16.91	21.58
# Early émigrés (pre-1933 family network)	0.76	2.59
# Early émigrés (pre-1933 community network)	858.63	1,125.21
Panel C. Dismissals and emigration		
Early dismissal	0.77	
Early émigré	0.52	
Emigrated by 1939	0.74	
Emigrated by 1945	0.81	
Observations	1,327	

Notes: The data on academics were collected from various historical sources.
[a]Pre-1933 quality is measured as the number of entries in bibliographical compendia that were published before 1933.
[b]Annual publications between 1928 and 1932 are reported for academics in mathematics, physics, chemistry, biochemistry, biology, medicine, and psychology.

emigrated, while 19 percent had not. Six percent had been directly or indirectly murdered by the Nazis (Figure 3, panel B).[16] The emigration rates of Jewish academics are remarkably high. They are much higher than emigration rates for the general Jewish population, which were 31 percent for 1939 and 51 percent for 1945 (Benz 1988, 738; see online Appendix B.5.2). While Jewish academics were more likely to survive the Holocaust and escape from Nazi Germany, expulsions took a terrible toll on their lives.

selection in our analysis of emigration outcomes, we assign the place of death as the location of academics in 1939 or 1945 for academics who died of other causes before 1939 or 1945. This assumption implies that academics who died of other causes in Germany would not have emigrated. Results remain unchanged if we exclude from the sample all individuals who had died from other causes or impute the emigration status for academics who died of other causes before 1939 or 1945 (see online Appendix F.4).

[16]An example of an "indirect murder" is the tragic case of Arthur Nicolaier of the University of Berlin, the discoverer of the soil bacterium that causes tetanus. After his dismissal, he worked as a doctor in Berlin. In 1942, he committed suicide when he was about to be deported to Theresienstadt. "Direct murders" are academics who died of actions by the Nazi government, e.g., because they were deported to concentration or death camps such as Auschwitz. Fifteen academics survived the Nazi period in a concentration camp. E.g., the historian Ernst Perels survived Flossenbürg concentration camp but passed away on May 10, 1945, just a few days after the German surrender. For these statistics, we count deported but surviving academics in the Dead (Murdered) category. The few Jewish academics who survived outside the camps were individuals who had initially been exempted from dismissals under the *Law for Restoration of the Professional Civil Service* with at most two Jewish grandparents. If they were not practicing Jews and were not married to Jews they were not directly targeted by the Nuremberg racial laws.

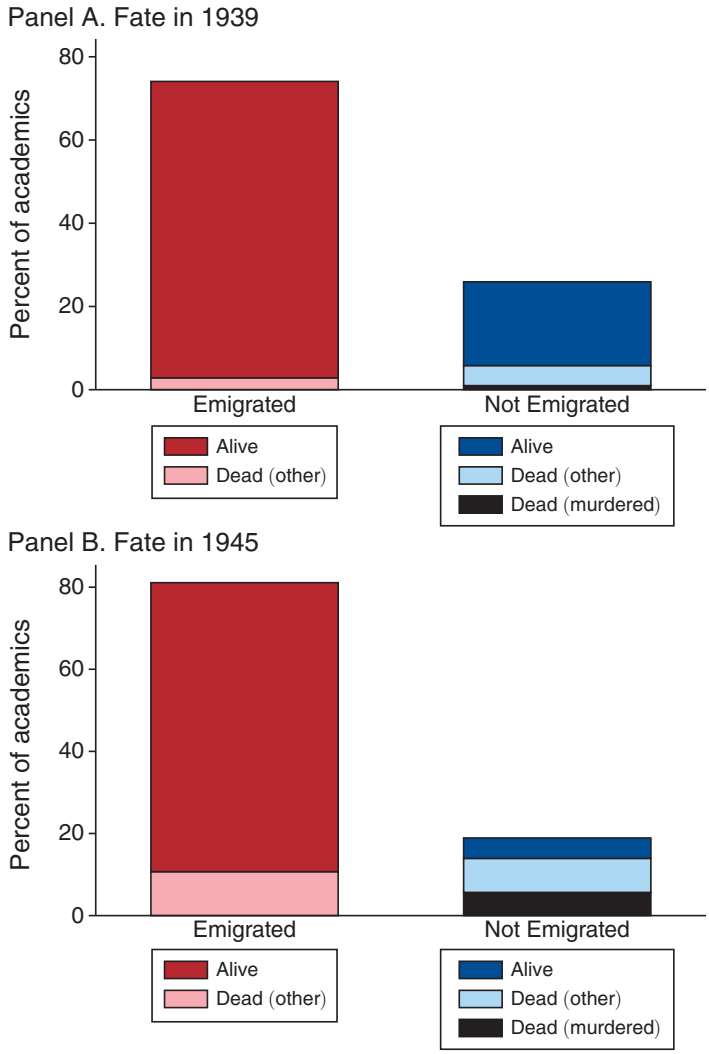


Figure 3. The Fate of Persecuted Academics

Notes: The figure reports the fate of persecuted Jewish academics. Panel A shows the fate for January 1, 1939, panel B shows the fate for January 1, 1945. The category "Dead (Murdered)" contains academics who were murdered by the Nazis but also suicides of academics and those whose death was most likely caused by Nazi persecution, e.g., academics who died of a heart attack in a concentration camp, and academics who were deported to camps but were still alive by the relevant date (January 1, 1939 or 1945). Fifteen academics survived the Nazi period in a concentration camp. E.g., the historian Ernst Perels survived Flossenbürg concentration camp but passed away on May 10, 1945, just a few days after the German surrender in WWII. The few Jewish academics who survived in Germany were individuals who had initially been exempted from dismissals under the *Law for Restoration of the Professional Civil Service* with at most two Jewish grandparents. If they were not practicing Jews and were not married to Jews they were not directly targeted by the *Nuremberg Racial Laws*. Some of them managed to survive the Holocaust in Germany. The category "Dead (Other)" contains academics who were dead by the relevant date (January 1, 1939 or 1945) and whose death was most likely not directly caused by the Nazis. It is important to note that even such deaths from other causes may have been a result of persecution from the Nazis.

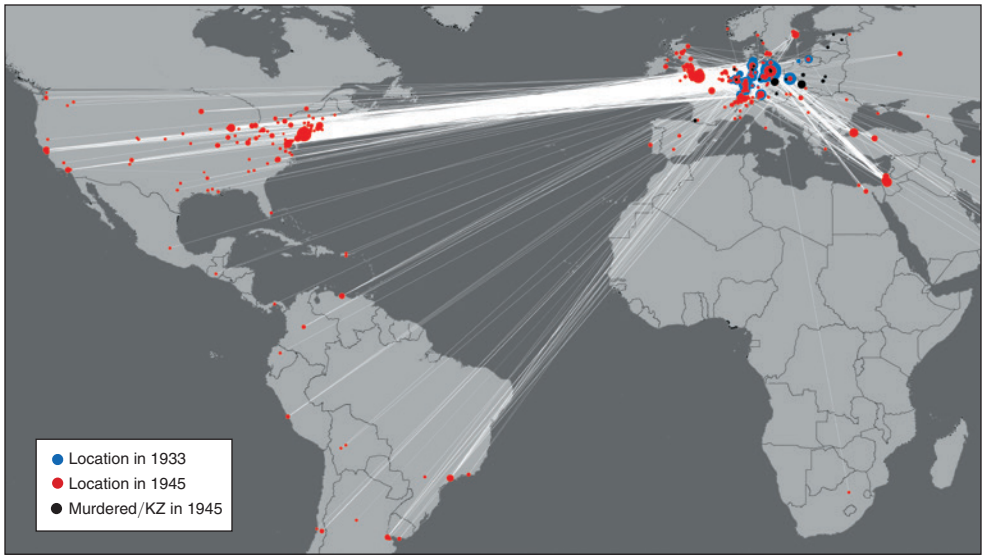


FIGURE 4. MOVEMENTS OF ACADEMICS BETWEEN 1933 AND 1945

Notes: The figure shows the location of Jewish academics on January 1, 1933 (blue dots, all in Germany) and January 1, 1945 (red dots or black dots). The size of the dots reflects the number of academics in each location. The white lines connect the locations on January 1, 1933 and January 1, 1945. The width of the lines reflects the number of academics moving between the two locations. Many academics moved to the 1945 destination via intermediate destinations that are not shown in the figure.

Our detailed biographical data allow us to observe the exact location of each academic, describing their fate. Figure 4 reports locations in 1933 and 1945. By far the two most attractive locations were the United States and the United Kingdom, home to leading universities and destinations where language and cultural barriers were lower than in other destinations (Figure 5, panel B). Cambridge, Istanbul, Oxford, Hebrew University, the New School (New York), Universiy of Paris, Columbia University, University College London, University of Chicago, and Harvard received the highest numbers of émigrés (Figure 5, panel A).

II. Professional Networks and Emigration: OLS

As highlighted earlier, ties to early émigrés may have been a key factor in emigration decisions. Accordingly, we focus on ties to *early émigré* colleagues in an academic's pre-dismissal professional network (see Figure 6, panel A for a schematic example). Early émigré colleagues are defined as academics who had emigrated by January 1, 1935 (Figure 6, panel B).

We define the pre-dismissal professional network as all Jewish academics who worked in the same subject and city between January 1, 1929 and January 1, 1933.[17]

[17] Results are similar if we measure networks for a 10-year period before January 1, 1933.

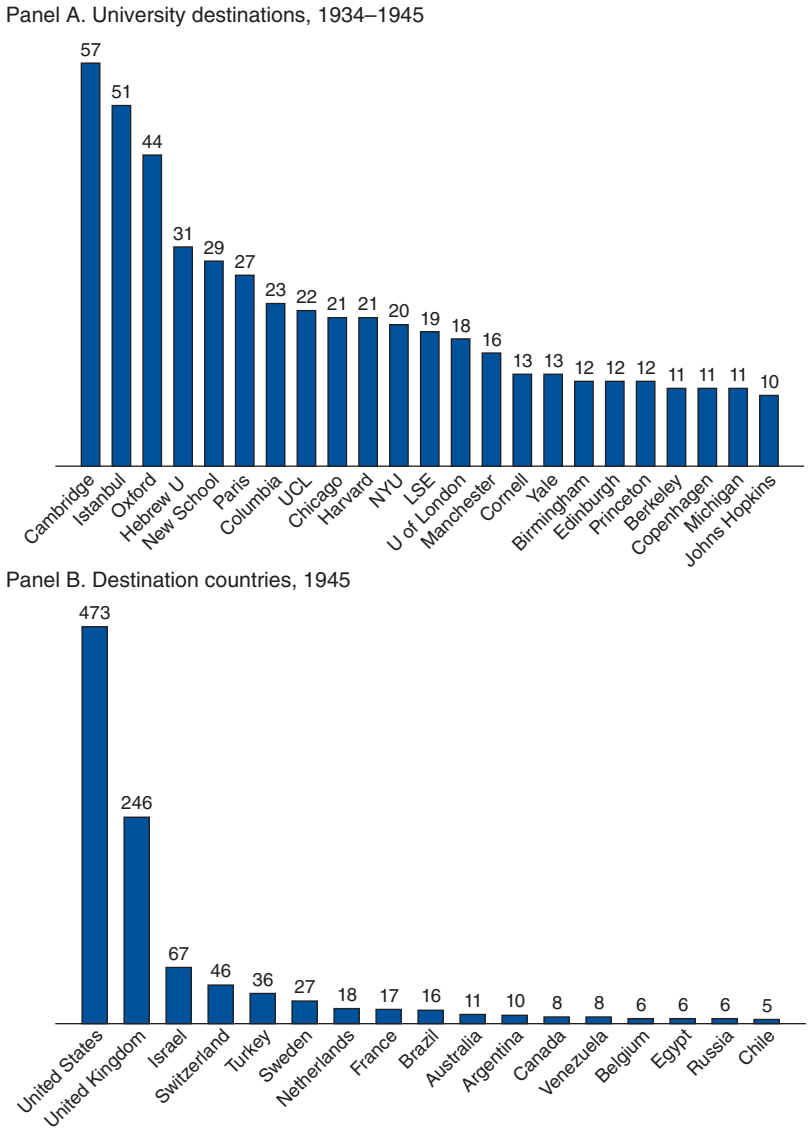


Figure 5. Main Destinations of German Jewish Academics

Notes: The figure reports the main destinations of German Jewish academics. Panel A reports the number of Jewish academics who were affiliated with the respective university at some point between January 1, 1934 and January 1, 1945. Only universities with at least 10 émigrés are reported. Panel B reports the number of Jewish academics in each destination country by January 1, 1945. Only countries with at least five émigrés are reported.

Because we measure the professional network before the Nazis assumed power, we avoid the concern that academics endogenously formed new ties in *response* to persecution after 1933. We use the yearly snapshots of pre-dismissal networks to compute the number of ties to *early émigré* colleagues. Specifically, we compute how many individuals who would later become *early émigrés* worked with the focal

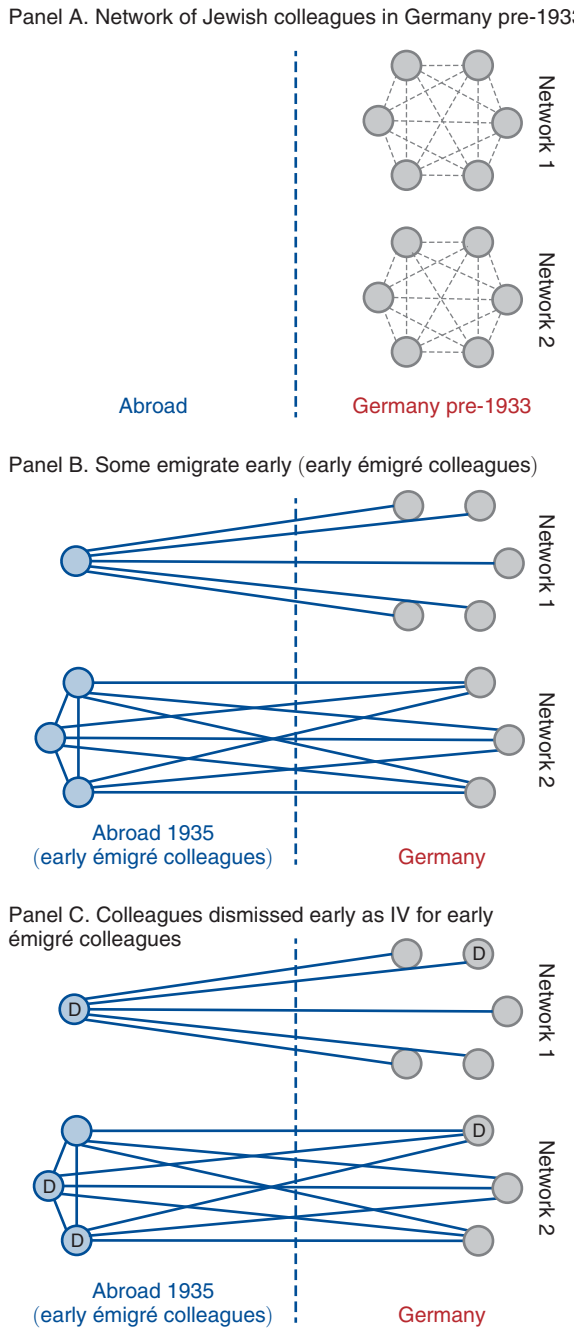


FIGURE 6. THE ROLE OF EARLY ÉMIGRÉ COLLEAGUES IN FACILITATING EMIGRATION

Notes: Panels A–C show a schematic example of two professional networks to illustrate the identification strategy. Panel A shows the pre-1933 networks of academics who worked in the same subject and city. Panel B shows the two networks in 1935 when early émigré colleagues had moved abroad. The academics in network 1 had ties to one early émigré (or to zero early émigrés for the early émigré him/herself), while the academics in network 2 had ties to three early émigrés (or to two early émigrés for the early émigrés themselves). Panel C illustrates the IV strategy. By 1935, some academics were dismissed ("early dismissals" indicated by the letter "D") and some early émigrés had moved abroad. The IV strategy uses the number of dismissed colleagues among the pre-1933 network as an IV for the number of early émigré colleagues among the pre-1933 network.

academic in the same subject and city between January 1, 1929 and January 1, 1933. In the schematic example, the academics in network 1 had ties to one early émigré (zero for the early émigré him/herself), while the academics in network 2 had ties to three early émigrés (two for the early émigrés themselves). The average academic in our sample had ties to 11.21 early émigrés (Table 1).

Figure 7 shows actual ties to early émigrés for mathematics and law. Early émigrés are marked in white, ties to early émigrés are represented by white lines. The figure aggregates locational information for the period January 1, 1929 and January 1, 1933 into one graph and shows the location for each academic in 1933. Academics from the same location in 1933 differ in the number of ties to early émigrés (white lines) because of academic turnover between 1929 and 1933. This variation is key for our identification strategy. The figure suggests that academics with links to early émigrés were more likely to emigrate by 1935 (white dots) or by 1945 (gray dots). For example, the 10 mathematicians without ties to early émigrés had a 50 percent emigration rate by 1945, while the 58 mathematicians with at least one tie to early émigrés had an 82.76 percent emigration rate.

We formally investigate how ties to early émigrés affected emigration by 1939 or 1945 by estimating the following regression:

$$(1) \qquad \textit{Emigrated By } 1939/45_i$$
$$= \beta_1 + \beta_2 \#\textit{Early Émigré Colleagues}\big(\textit{Pre-}1933\ \textit{Network}\big)_{-i}$$
$$+ \beta_3 \textit{Early Émigré}_i + \beta_c \textit{Controls}_i + \varepsilon_i.$$

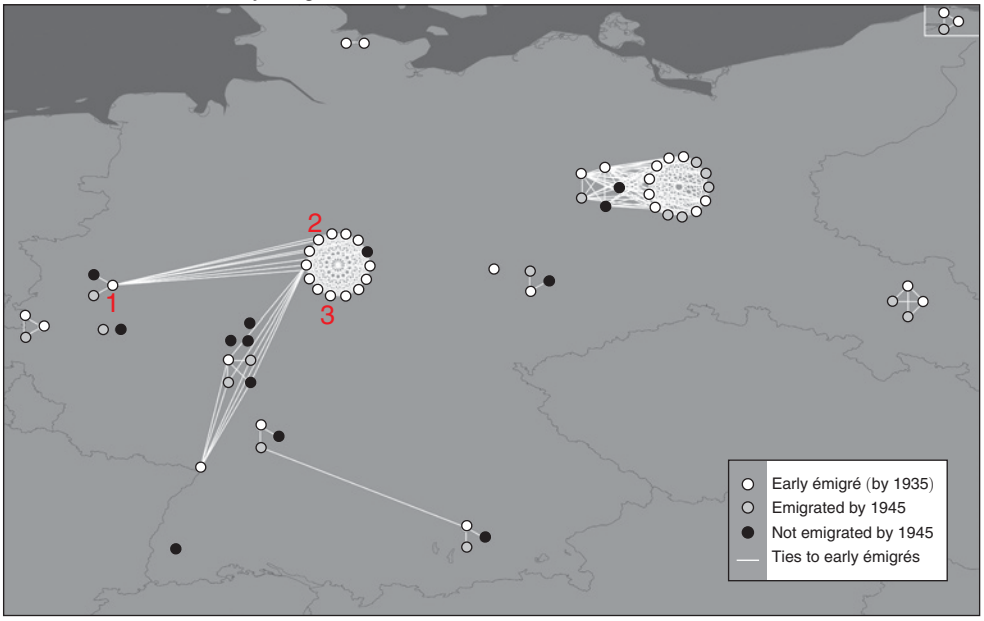
The dependent variable is an indicator equal to one if academic i had emigrated by 1939 or, alternatively, 1945. The main explanatory variable, $\#\textit{EarlyÉmigréColleagues}\big(\textit{Pre-}1933\textit{Network}\big)$ counts how many colleagues in academic i's pre-1933 professional network had emigrated by 1935, excluding academic i him/herself. To ease the reading of regression tables, we divide the number of early émigrés in the pre-1933 network by 10. Since migration choices are sticky over time (Parey and Waldinger 2011), equation (3) also includes the indicator $\textit{EarlyÉmigré}_i$ to control for academic i's own emigration status in 1935.[18]

The regression controls for individual-level variables such as academic discipline, academic rank, age, gender, marital status, children, foreign language skills, pre-1933 employment by a foreign university, and country of birth.

Most importantly, we control for academic i's city $\times$ subject employment history between 1929 and 1933. The employment history controls allow for the possibility that academics moved across cities between 1929 and 1933 and that they held multiple contemporaneous appointments. For an academic with appointments in two cities, we weight each city $\times$ subject fixed effect by 0.5. Similarly, for an academic who moved between cities, we weight the corresponding city $\times$ subject fixed effects by the number of years he/she spent in each city. For example, for a mathematician who

[18] Note: a small number of academics were abroad in 1935 but had returned to Germany by 1939.

Panel A. Actual ties to early émigrés: Mathematics



Panel B. Actual ties to early émigrés: Law

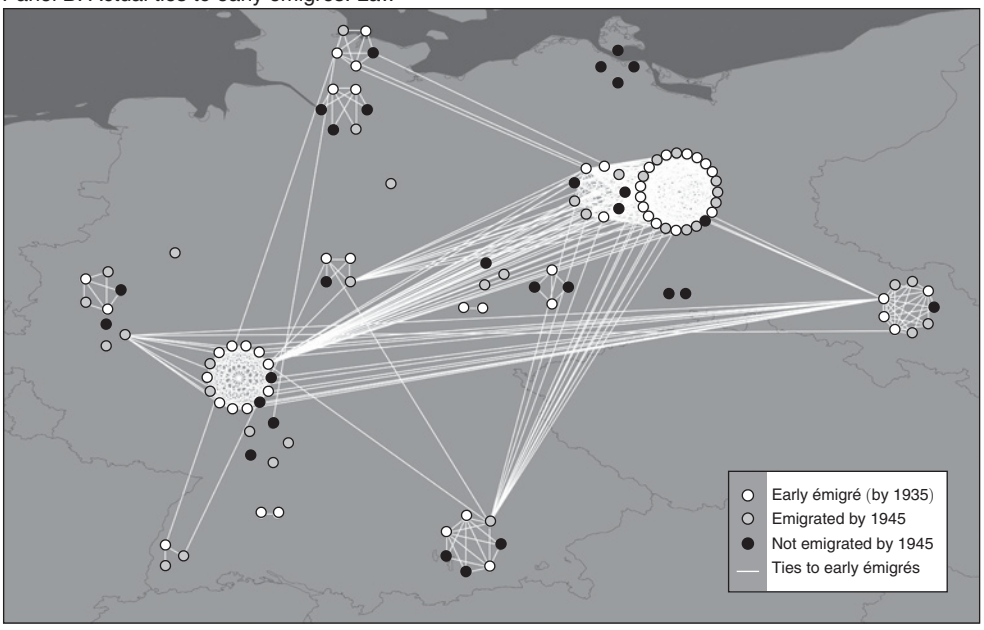


FIGURE 7. THE ROLE OF EARLY ÉMIGRÉ COLLEAGUES IN FACILITATING EMIGRATION

Notes: Panels A and B show actual ties to early émigré colleagues for mathematics (panel A) and law (panel B) in our data. The figure aggregates the information for the period January 1, 1929 and January 1, 1933 into one graph and shows the location for each academic on January 1, 1933. Academics from the same location in 1933 differ in the number of ties to early émigrés because of academic turnover between 1929 and 1933. Early émigrés (emigrated by January 1, 1935) are marked in white. Academics who were still in Germany by January 1, 1935 but who had emigrated by January 1, 1945 are marked in gray. Academics who did not emigrate by 1945 are marked in black. Ties (formed between January 1, 1929 and January 1, 1933) to early émigré colleagues are represented by white lines. For mathematics, panel A shows the two important centers Göttingen and Berlin (with two universities: the University of Berlin and the Technical University of Berlin). For law, panel B also shows a large cluster in Berlin but also other clusters in Frankfurt, Breslau, and Munich. Naturally, in both disciplines there were also smaller groups of Jewish academics in many other universities.

worked in Göttingen for three years between 1929 and 1931 and in Braunschweig for two years between 1932 and 1933, we weight the Göttingen × Math fixed effect by 0.6 and the Braunschweig × Math fixed effect by 0.4.[19]

The city × subject employment history controls for many factors that may have an independent effect on emigration decisions and that may be correlated with the number of early dismissals in an academic's network. For example, they control not only for the number of Jewish, but also for the number of non-Jewish colleagues that may assist emigration, even if these colleagues had not emigrated abroad. They also control for differences in average characteristics of colleagues in the same department. For example, physicists in Göttingen may have similar characteristics (because of homophily or assortative matching), more contacts abroad, or higher academic reputation, that affect migration decisions. Similarly, they control for community level factors, such as the size of the total community network, the fact that individuals from larger cities may have higher emigration probabilities, or for antisemitic acts by local Nazis that may affect emigration decisions.

With these controls, the identifying variation comes from academic turnover: either because academic i joined or left the same department as academic j at different points between 1929 and 1933, or alternatively because his/her colleagues joined or left. For example, Stefan Cohn-Vossen (mathematician 1 in Figure 7, panel A) moved from Göttingen to Cologne in 1930 and later became an early émigré. Hence, all mathematicians who had joined Göttingen before 1930 (e.g., mathematician 2) had ties to one additional early émigré (Cohn-Vossen) compared to the mathematicians who joined Göttingen later (e.g., mathematician 3).

OLS Results.—We first estimate equation (3) by ordinary least squares (OLS). The number of early émigrés in an academic's pre-1933 network is a strong predictor of emigration by 1939. Ties to 10 additional early émigrés increased the probability of emigration by 1939 by 5.3 percentage points (Table 2, column 1). A one standard deviation increase in ties to early émigrés (i.e., 14 additional ties) increased the probability of emigration by 1939 by 7.4 percentage points. Unsurprisingly, academic i's own emigration status in 1935 also had a strong effect on the probability of emigration by 1939. Personal characteristics, such as marital status or the number of children did not have a significant effect on emigration. In contrast, characteristics that measure an academic's international experience and characteristics that facilitate working abroad affected emigration rates by a similar magnitude as professional networks. Academics with pre-1933 professional experience abroad had 5.6 percentage points higher probability to emigrate. Similarly, being born outside Germany increased the probability to emigrate by 8.3 percentage points. Lastly, speaking a foreign language increased the probability to emigrate by 5.5 percentage points, even though the latter effect is not precisely estimated.

[19] The weighting ensures that predicted emigration probabilities of movers are not artificially inflated, which would be the case if one added the entire Göttingen and the entire Braunschweig fixed effects in the example outlined above. Results remain similar and highly significant when we condition on unweighted city × subject fixed effects for the 1933 location.

Table 2—Ties to Early Émigrés and Emigration: OLS, IV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	IV	IV	IV	IV
Dep. variable:	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1945	Emigrated by 1939	Emigrated by 1939
# Early émigré colleagues (pre-1933 network)	0.053	0.050	0.049	0.046	0.050	0.008	0.137
	(0.014)	(0.014)	(0.014)	(0.015)	(0.018)	(0.014)	(0.043)
Early émigré	0.342	0.312	0.310	0.312	0.043		
	(0.032)	(0.143)	(0.144)	(0.142)	(0.108)		
Female	0.052	0.055	0.061	0.057	0.100	−0.027	0.047
	(0.048)	(0.050)	(0.050)	(0.050)	(0.046)	(0.041)	(0.261)
Married	−0.003	−0.001	−0.002	−0.000	0.006	0.006	−0.047
	(0.017)	(0.021)	(0.021)	(0.020)	(0.027)	(0.013)	(0.060)
Number of children	0.007	0.007	0.007	0.007	0.031	−0.005	−0.013
	(0.014)	(0.014)	(0.014)	(0.014)	(0.010)	(0.004)	(0.027)
Any foreign language	0.055	0.060	0.060	0.064	0.100	−0.020	0.118
	(0.039)	(0.056)	(0.054)	(0.055)	(0.037)	(0.013)	(0.061)
Pre-1933 professional experience abroad	0.056	0.059	0.053	0.056	0.055	0.035	−0.051
	(0.030)	(0.024)	(0.026)	(0.025)	(0.058)	(0.024)	(0.106)
Born abroad	0.083	0.089	0.089	0.087	0.105	0.017	0.198
	(0.016)	(0.036)	(0.033)	(0.037)	(0.027)	(0.014)	(0.058)
Pre-1933 reputation 51–80th percentile			0.021				
			(0.025)				
Pre-1933 reputation 81–90th percentile			0.092				
			(0.037)				
Pre-1933 reputation 91–100th percentile			0.115				
			(0.047)				
Pre-1933 publications 51–80th percentile				0.039			
				(0.033)			
Pre-1933 publications 81–90th percentile				0.041			
				(0.027)			
Pre-1933 publications 91–100th percentile				−0.025			
				(0.079)			
Academic rank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × subject (1929−1933)	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(*continued*)

III. Ties to Colleagues Dismissed Early as IV for Ties to Early Émigré Colleagues

Yet, even with the rich set of controls, we cannot rule out other omitted variables that may bias the estimates. Individuals with more ties to early émigré colleagues may also have other characteristics that facilitate emigration. For example, academics who worked in multiple departments (either because they are of the "restless" type or because they are in high demand because of their ability) have more ties to early émigrés and were also more likely to emigrate. To address this endogeneity concern, we use the number of *colleagues dismissed early* in academic *i*'s pre-1933 network as an IV for the number of *early émigré colleagues* in academic *i*'s network.

A. *Early Dismissals as IV*

Early Dismissals: 1933–1934.—Variation in the timing of dismissals occurred because of exemptions to dismissals under the *Law for the Restoration of the*

Table 2—Ties to Early Émigrés and Emigration: OLS, IV (*continued*)

Dep. variable:	(1) OLS Emigrated by 1939	(2) IV Emigrated by 1939	(3) IV Emigrated by 1939	(4) IV Emigrated by 1939	(5) IV Emigrated by 1945	(6) IV Emigrated by 1939	(7) IV Emigrated by 1939
Sample:	Full sample	Full sample	Full sample	Full sample	Full sample	Emigrated by 1935	Not emigrated by 1935
Number of observations	1,327	1,327	1,327	1,327	1,327	693	634
R^2	0.649						
Kleibergen-Paap rk Wald F-statistic		56.611	66.773	48.522	56.611		
Mean of dep. variable	0.741	0.741	0.741	0.741	0.811	0.984	0.475
F-statistic (excluded instruments)						662.192	505.732

Notes: In columns 1–5, the sample includes all academics. In column 6, the sample includes academics who had emigrated by January 1, 1935. In column 7, the sample includes only academics who had not emigrated by January 1, 1935. The dependent variable in columns 1–4 and 6–7 is an indicator that equals 1 if academic *i* had emigrated by January 1, 1939. The dependent variable in column 5 is an indicator that equals 1 if academic *i* had emigrated by January 1, 1945. The main explanatory variable is the number of early émigré colleagues from the pre-1933 network. In columns 1–5 another important explanatory variable is academic *i*'s own early émigré status. We instrument these variables with the number of colleagues dismissed early among the pre-1933 network and with an indicator that equals 1 if academic *i* him/herself was dismissed early. First-stage regressions for columns 2 and 5 are reported in Table 3. All other first-stage regressions are reported in online Appendix Table D2. For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history. In column 3 we add indicators for whether academic *i* ranked in the 51–80th, 81–90th, or 91–100th percentile of the subject-level distribution of pre-1933 academic reputation, as measured by the number of entries in pre-1933 bibliographical compendia. In column 4 we add indicators for whether academic *i* ranked in the 51–80th, 81–90th, or 91–100th percentile of the pre-1933 subject-level publication distribution. The regressions also include unreported indicators for academics with missing information on academic reputation or publications. Standard errors are clustered at the city level.

Professional Civil Service of 1933 (online Appendix C provides details). Most Jewish academics were dismissed under the infamous paragraph 3:

> Civil servants who are not of Aryan descent are to be placed in retirement … This does not apply to officials who had already been in the service since the first of August, 1914, or who had fought in the World War at the front …, or whose fathers or sons had been casualties in the World War. (Hentschel 1996)

An implementation decree defined "Aryan descent" as follows: "Anyone descended from non-Aryan, and in particular Jewish, parents or grandparents, is considered non-Aryan. It is sufficient that one parent or one grandparent be non-Aryan" (Hentschel 1996, 25). Thus, even baptized Christians were dismissed if they had at least one Jewish grandparent.

Crucially for our identification strategy, Jews could retain their position if a) they had been a civil servant since August 1, 1914, or b) if they had fought at the front in

WWI, or c) if they had lost a father or son in the war.[20] Importantly, the Law and its exemptions were strictly enforced.

Late Dismissals: 1935 or Later.—Most Jewish academics who were originally exempted lost their position in the wake of the infamous *Nuremberg Racial Laws* of September 15, 1935. Some of the dismissals on the basis of the *Nuremberg Laws* dragged into 1936 (or even later). In addition, a very small number of Jewish academics were dismissed after 1935 on the basis of two other laws targeting academic civil servants (online Appendix C). Also, note that some academics who were initially exempted resigned voluntarily. For example, the physics Nobel laureate James Franck could have stayed in his position in 1933 but resigned in protest on April 17, 1933 (Hentschel 1996, 26). Almost all of these academics would have been dismissed in 1935 on the basis of the *Nuremberg Laws*. To avoid contamination of our IV, we classify all "voluntary" leavers as late dismissals.

Data on Dismissal Reasons and Years.—To implement the instrumental variables strategy, we collect new data on exact dismissal reasons for all Jewish academics from a large number of primary and secondary sources. For example, the University of Freiburg provided a list of their dismissal record to the Ministry of Education and Cultural Affairs in the federal state of Baden (see online Appendix Figure C1). We use this information to assign precise dismissal paragraphs. In other cases, we rely on secondary sources (e.g., the 60 university-specific studies plus the 16 subject-specific studies) plus extensive web searches to identify exact dismissal paragraphs for each academic.

The newly collected data indicate that academics who were dismissed early had a much higher probability of early emigration by January 1, 1935 (see Figure 8).

Colleagues Dismissed Early as IV for Early Émigré Colleagues in the Pre-1933 Network.—We measure early dismissals of colleagues in academic i's pre-1933 network (measured using the yearly snapshots of the academic network between January 1, 1929 and January 1, 1933) to construct an instrument for the number of ties to early émigrés. In the schematic example in Figure 6, panel C, academics in network 1 had ties to two colleagues who were dismissed early (indicated by the letter "D"), or to one colleague who was dismissed early if they were themselves dismissed early. The academics in network 2 had ties to three colleagues who were dismissed early (or to two colleagues who were dismissed early if they were themselves dismissed early). The average academic had ties to 16.91 academics who were dismissed early (Table 1).

[20] Direct exposure to "enemy fire" was essential for the second exemption. It was "not sufficient for someone to have stayed in the war zone […] without having confronted the enemy" (Reichsministerium des Innern 1933, as reprinted in Hentschel 1996, 47). Military doctors who had worked in field hospitals did not qualify (Kinas 2018, 78). Because few Jewish professors had been in service since 1914, most *exempted* academics qualified as combatants in WWI. A few Jewish academics were also dismissed on the basis of alternative paragraphs of the *Civil Service Law* (see online Appendix C). The majority of dismissals on the basis of paragraph 3 of the *Civil Service Law* were completed by the fall of 1933. However, a few cases dragged on because some Jewish academics tried to provide evidence that they qualified for one of the exemptions or that they should be classified as "Aryan." We therefore define early dismissals as all those that occurred between 1933 and 1934.

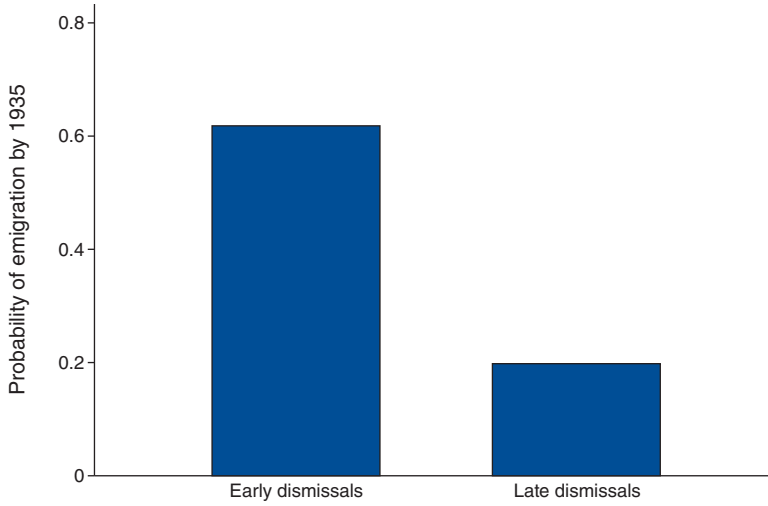


Figure 8. Probability of Emigration by 1935 by Dismissal Status

Notes: The figure shows the probability of emigration by January 1, 1935 for academics who were dismissed early (1933 or 1934) versus late (1935 or later), indicating that early dismissal is a good predictor of early emigration. To construct an IV for the number of early émigré colleagues we aggregate the number of colleagues dismissed early from academic *i*'s network.

Importantly, the IV exploits early dismissals of academic *i*'s colleagues, *not* his/her own early dismissal. In fact, academic *i*'s characteristics and academic *i*'s own early dismissal status are not related to the number of dismissals in his/her network (online Appendix Figure D1).

The number of early dismissals in an academic's network should only affect emigration by increasing the number of early émigrés in the network. In principle, the number of dismissals in the network could inform academics of the threat of the Nazi regime and, hence, have a direct effect on emigration decisions. However, the promulgation of the *Civil Service Law* affected the entire public sector and was common knowledge to all academics, independently of the number of dismissals in *their* network. As the Law was the first piece of Nazi legislation that codified *nationwide* discrimination against Jews (Evans 2005), dismissals were widely reported in newspapers: "[h]ardly a day goes by in which a new list of lecturer suspensions is not issued" (Vossische Zeitung 1933). Hence, the discriminatory nature of the Nazi regime was known to all Jewish academics.

Furthermore, the number of dismissals in the professional network could be correlated with a larger Jewish community that suddenly faced persecution, which could have an independent effect on emigration decisions. To address this concern, we include detailed controls for an academic's city $\times$ subject employment history in the regressions. Because the city $\times$ subject employment history also implicitly controls for the total number of Jewish and non-Jewish colleagues, they also address potential alternative confounders, such as increasing administrative burdens or increases in the number of PhD students non-emigrating academics had to cope with.

A further concern is that the number of ties to colleagues dismissed early (or the number of ties to early émigré colleagues) are correlated with specific employment histories. For example, an academic working at the University of Berlin between 1929 and 1930 and then at Göttingen from 1931 to 1933 may be different from an academic who worked only at Göttingen or only at Berlin. We investigate this concern by generating 1,000 placebo networks. For each placebo network, we vary the subject for each academic but keep the employment history constant. For example, we reassign art history to a physicist, but keep the actual moves across cities constant. We then recalculate ties to early émigrés and to colleagues dismissed early in each placebo network. We then estimate 1,000 regressions on the basis of the placebo networks. Of the 1,000 estimated coefficients, not a single one is as large as our main OLS or IV coefficient estimates. On average the estimated coefficients are centered around zero (Figure 9). This strongly suggests that our main results are driven by actual ties to early émigrés and not by specific employment histories.

Lastly, the number of dismissals in the professional network may also affect emigration through severing ties with colleagues who were coauthors. Coauthoring with other professors was relatively limited in this period (Waldinger 2012). Nonetheless, we show that results are very similar in a sample of academics who did not coauthor with other Jewish academics (online Appendix Table F2).

B. *First Stages*

To summarize, we use the number of *colleagues dismissed early* in academic i's pre-1933 network as an IV for the number of *early émigré colleagues* in academic i's network. As outlined above, equation (3) also controls for academic i's own emigration status in 1935. This variable suffers from similar endogeneity concerns, e.g., because better academics may have emigrated earlier. Consequently, we use academic i's *own* early dismissal status as our second IV.[21] The two first-stage regressions are:

$$\begin{aligned}(2)\qquad & \#\textit{Early Émigré Colleagues}\big(\textit{Pre}\text{-}1933\ \textit{Network}\big)_{-i} \\ &= \gamma_1 + \gamma_2 \#\textit{Colleagues Dismissed Early}\big(\textit{Pre}\text{-}1933\ \textit{Network}\big)_{-i} \\ &\quad + \gamma_3 \textit{Early Dismissal}_i + \gamma_c \textit{Controls}_i + \zeta_i,\end{aligned}$$

$$\begin{aligned}(3)\qquad & \textit{Early Émigré}_i \\ &= \lambda_1 + \lambda_2 \#\textit{Colleagues Dismissed Early}\big(\textit{Pre}\text{-}1933\ \textit{Network}\big)_{-i} \\ &\quad + \lambda_3 \textit{Early Dismissal}_i + \lambda_c \textit{Controls}_i + \xi_i.\end{aligned}$$

[21] The rules governing early dismissals meant that older academics who could have served in the German or Austro-Hungarian military were more likely to be exempted. We probe the sensitivity of our results by restricting the analysis to older academics and those born in Germany or Austria-Hungary (online Appendix D.1.2). In these samples, individual characteristics, such as academic reputation or family characteristics of early and late dismissals, are very similar. Moreover, they have similar age (48.2 for early dismissals and 49.8 for late dismissals, not statistically significantly different). Differences in their own exemption status are predominately driven by whether they experienced enemy fire in WWI or whether they served behind the frontline and not whether they served in the war at all.

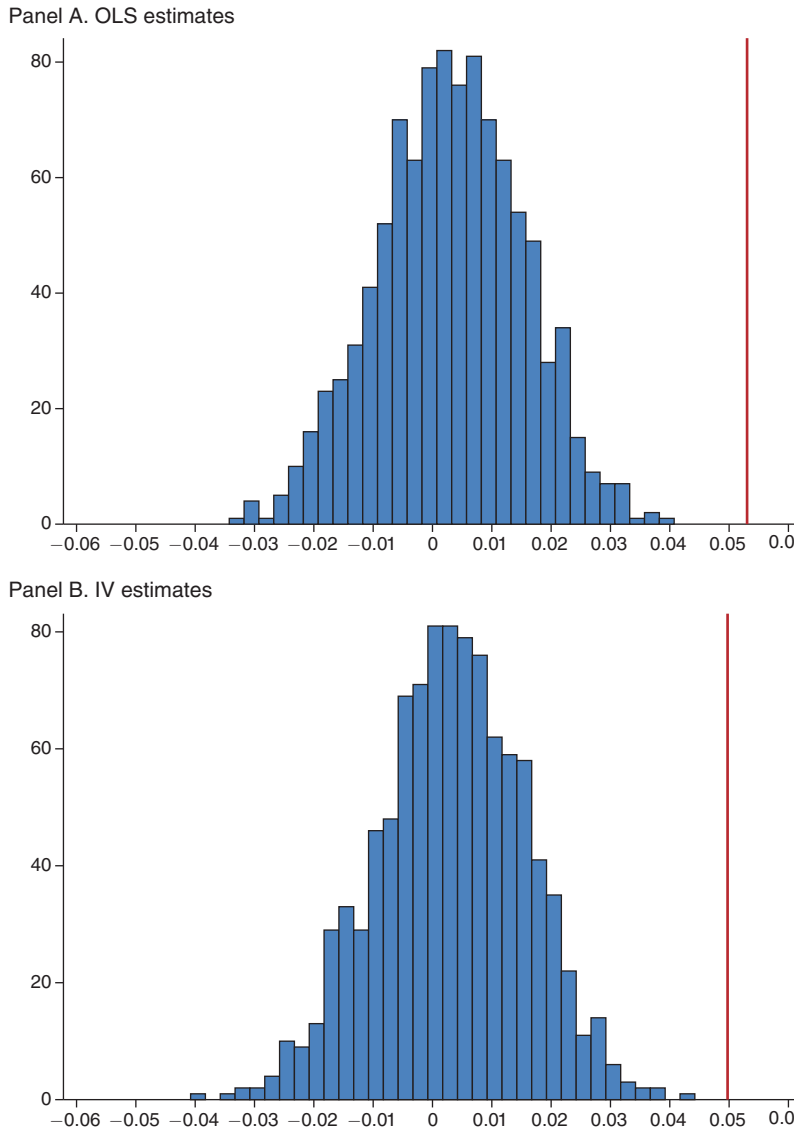


Figure 9. Placebo Networks

Notes: The figure shows distributions of coefficients for OLS and IV results using the placebo networks. Panel A reports the OLS estimates, panel B reports the IV estimates. For the OLS estimates, we generate 1,000 alternative placebo networks for # Early Émigré Colleagues (Pre-1933 Network) and estimate our baseline OLS model from Table 3, column 1 for each placebo network. For the IV estimates, we generate 1,000 alternative placebo networks for # Colleagues Dismissed Early (pre-1933 network) and # Early Émigré Colleagues (pre-1933 network) and estimate our baseline IV model from Table 3, column 2 for each placebo network. The red vertical lines indicate our baseline estimates for the OLS and IV regressions.

Table 3, column 1 reports the first-stage results for the number of early émigré colleagues from the pre-1933 professional network (equation (2)). The number of colleagues *dismissed* early in academic i's network is a strong predictor for the number of *early émigré* colleagues in academic i's network. The point estimate indicates that one additionally dismissed colleague increased the number of early

Table 3—First-Stage Results

Dep. variable:	(1) # Early émigré colleagues (Pre-1933 network)	(2) Early émigré	(3) # Early émigré colleagues (Pre-1933 network)	(4) Early émigré
# Colleagues dismissed early (pre-1933 network)	0.646	0.011	0.620	−0.010
	(0.004)	(0.005)	(0.017)	(0.033)
Early dismissal	0.037	0.300	0.036	0.283
	(0.025)	(0.024)	(0.004)	(0.026)
Female	0.062	0.098	−0.010	0.074
	(0.033)	(0.065)	(0.012)	(0.089)
Married	−0.024	0.044	−0.007	0.085
	(0.012)	(0.027)	(0.007)	(0.026)
Number of children	−0.006	0.010	−0.002	0.003
	(0.004)	(0.009)	(0.002)	(0.012)
Any foreign language	−0.026	0.119	−0.017	0.140
	(0.014)	(0.034)	(0.007)	(0.046)
Pre-1933 professional experience abroad	0.037	0.183	−0.021	0.118
	(0.020)	(0.066)	(0.011)	(0.118)
Born abroad	0.072	0.160	−0.008	0.159
	(0.031)	(0.027)	(0.006)	(0.032)
Academic rank FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
City × subject (1929–1933)			Yes	Yes
Number of observations	1,327	1,327	1,327	1,327
R^2	0.972	0.304	0.998	0.509
F-statistic (excluded instruments)	42,484.401	90.526	886.407	58.082
Kleibergen-Paap rk Wald F-statistic			56.611	
Mean of dep. variable	1.121	0.522	1.121	0.522

Notes: The table reports the first-stage regressions. The dependent variable in columns 1 and 3 is the number of early émigré colleagues from the pre-1933 network. The dependent variable in columns 2 and 4 is an indicator that equals 1 if academic i him/herself was an early émigré. The first instrument is the number of colleagues dismissed early among the pre-1933 network. The second instrument is an indicator that equals 1 if academic i him/herself was dismissed early. For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank and year of birth fixed effects. In columns 3 and 4, we also include controls for the city × subject employment history. Standard errors are clustered at the city level.

émigrés in his/her network by 0.65.[22] The academic's own early dismissal only had a small effect on the number of early émigrés in the network (Table 3, column 1). Controlling for the city × subject history hardly affects the point estimates (column 3).

Column 4 reports the first-stage results for academic i's own early émigré status (equation (3)). The number of colleagues dismissed early does not predict academic i's own early émigré status. In contrast, academic i's own early dismissal had a large effect on his/her own early émigré status.

[22]Online Appendix Figure D2 shows the first-stage relationship. The network measures aggregate the individual-level probabilities of early dismissal and early emigration, resulting in a very strong relationship. The smaller the network, the larger is the relative variation (panel B). Note that some academics in smaller departments in 1933 had large networks if they had previously worked in a large department.

The first-stage F-statistics are 58.1 and 886.4, indicating very strong relationships. With two endogenous variables and two IVs, a high first-stage F-statistic is not a sufficient condition for valid identification (Stock et al.2002). We therefore report a Kleibergen-Paap statistic of 56.6, which is much higher than the critical value of 7.03 (Stock and Yogo 2005).

C. *IV Results*

Next, we estimate equation (3) using our instrumental variables strategy. The IV estimates are somewhat smaller but similar in magnitude and significance compared to the OLS estimates. Ties to 10 additional early émigrés increased the probability of emigration by 1939 by 5.0 percentage points (Table 2, column 2). A one standard deviation increase in ties to early émigrés (i.e., 14 additional ties) increased the probability of emigration by 1939 by 7 percentage points. Unsurprisingly, academic i's own emigration status in 1935 also had a strong effect on the probability of emigration by 1939.

Importantly, the results are robust to controlling for an individual's academic reputation and publication record (columns 3–4). We proxy for academic reputation by counting the number of entries in biographical compendia that were published *before* 1933. To account for discipline-level differences, we standardize this measure by academic discipline. The measure is a good proxy for reputation. For example, Albert Einstein is the most reputed physicist. Among the top 15 physicists, based on our measure of academic reputation, are five Nobel laureates (online Appendix Table B3). We measure pre-1933 publication records using data from the *Web of Science* for academics in seven disciplines (containing 59 percent of all dismissed Jewish academics)—mathematics, physics, chemistry, biochemistry, biology, medicine and psychology. See online Appendix B.4 for details on linking academics with their publication records.[23]

The effect of 10 additional early émigrés in the professional network is about as large as the effect of having pre-1933 professional experience abroad or of being born abroad. It is also similar to the gap in emigration rates between academics in the ninth decile as compared to the bottom five deciles of academic reputation (Table 3, column 3).

In additional results, we explore the persistence of the effects. We find that ties to early émigrés had a similar effect for emigration by 1945, indicating that the professional network had long-lasting effects on emigration and escaping the Nazi terror (column 5).

We also show that ties to early émigré colleagues only affected the emigration decisions of academics who had *not* emigrated by January 1, 1935 (columns 6–7). This suggests that the support by early émigrés to their former colleagues became effective after they had settled in the new destination. Because the decision to emigrate early (by 1935) is endogenous, this is not our main specification of interest.

[23] The regression also includes an indicator equal to one if we do not have publication data for the discipline.

IV. Do Bridging Nodes Affect the Direction of Migration?

A. *Emigration to the United States/United Kingdom versus the Rest of the World*

Early émigrés could have provided general information that facilitated emigration to *any* destination or, alternatively, only to their *own* destination. To differentiate between these two alternatives, we separately analyze ties to early émigré colleagues who had emigrated to the United States/United Kingdom (the most attractive destinations) and ties to early émigré colleagues who had emigrated to other countries (Figure 5, panel B shows destination countries).[24] We estimate the following regressions:

$$
\begin{aligned}
(4)\quad & \textit{Emigrated To US/UK By } 1939_i \\
&= \delta_{11} + \delta_{12}\#\textit{Early Émigré Colleagues In US/UK}\left(\textit{Pre-}1933\ \textit{Network}\right)_{-i} \\
&\quad + \delta_{13}\#\textit{Early Émigré Colleagues In Other Countries}\left(\textit{Pre-}1933\ \textit{Network}\right)_{-i} \\
&\quad + \delta_{14}\textit{Early Émigré In US/UK}_i + \delta_{15}\textit{Early Émigré In Other Countries}_i \\
&\quad + \delta_{1c}\textit{Controls}_i + \eta_i, \\
(5)\quad & \textit{Emigrated To Other By } 1939_i \\
&= \delta_{21} + \delta_{22}\#\textit{Early Émigré Colleagues In US/UK}\left(\textit{Pre-}1933\ \textit{Network}\right)_{-i} \\
&\quad + \delta_{23}\#\textit{Early Émigré Colleagues In Other Countries}\left(\textit{Pre-}1933\ \textit{Network}\right)_{-i} \\
&\quad + \delta_{24}\textit{Early Émigré In US/UK}_i + \delta_{25}\textit{Early Émigré In Other Countries}_i \\
&\quad + \delta_{2c}\textit{Controls}_i + \mu_i.
\end{aligned}
$$

Ties to 10 additional early émigrés in the United States/United Kingdom *increased* emigration to the United States/United Kingdom by 43.4 percentage points. Ties to 10 additional early émigrés in other countries *decreased* emigration to the United States/United Kingdom by 35.6 percentage points (Table 4, column 1).[25] Naturally, an academic i's own emigration status was also very persistent. If the academic had emigrated to the United States/United Kingdom by 1935 he/she was more likely to reside in any of these two countries by 1939. If the academic had emigrated to

[24] We do not analyze separate effects of early émigré networks in the United States and the United Kingdom because a large fraction of academics who emigrated to the United States emigrated via the United Kingdom (online Appendix Figure B2). E.g., the mathematician Richard Courant and the physicist Leo Szilard both went to the United Kingdom before settling in the United States. Because early dismissals predict emigration but not emigration to a *particular destination*, we cannot use our IV strategy.

[25] For these results, ties to early émigrés are split by destination. The average academic had 5.6 ties to early émigrés in the United States/United Kingdom and 5.6 ties to early émigrés in other countries.

Table 4—Directional Effects

	(1) OLS	(2) OLS
	Emigrated by 1939 to	
Dep. variable:	US/UK	Other countries
# Early émigré colleagues in US/UK (pre-1933 network)	0.433	−0.301
	(0.111)	(0.111)
# Early émigré colleagues in other countries (pre-1933 network)	−0.356	0.334
	(0.118)	(0.137)
Emigrated to US/UK by 1935	0.507	−0.168
	(0.036)	(0.018)
Emigrated to other countries by 1935	−0.227	0.570
	(0.027)	(0.037)
Baseline controls	Yes	Yes
Academic rank FE	Yes	Yes
Year of birth FE	Yes	Yes
City × subject (1929−1933)	Yes	Yes
Number of observations	1,327	1,327
R^2	0.583	0.540
Mean of dep. variable	0.414	0.327

Notes: In column 1, the dependent variable is an indicator that equals 1 if academic *i* had emigrated to the United States or the United Kingdom by January 1, 1939. In column 2, the dependent variable is an indicator that equals 2 if academic *i* had emigrated to other countries by January 1, 1939. The first main explanatory variable is the number of early émigré colleagues in the United States or the United Kingdom from the pre-1933 network. The second main explanatory variable is the number of early émigré colleagues in other countries from the pre-1933 network. Other important explanatory variables are academic *i*'s own early émigré status in the United States/United Kingdom or in other countries. For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history. Standard errors are clustered at the city level.

another country by 1935 he/she was less likely to emigrate to the United States or the United Kingdom by 1939.

The role of bridging nodes in other counties mirrors the role of bridging nodes in the United States/United Kingdom. Ties to early émigré colleagues in other countries *increased* emigration to other countries. In contrast, ties to early émigré colleagues in the United States/United Kingdom *decreased* emigration to other countries (Table 4, column 2). These results indicate that early émigrés functioned as a bridge that helped academics cross over into the *same* destination. At the same time, these academics were diverted away from alternative destinations.

B. *Do Early Émigrés Attract Academics in the Network to the Same City?*

In additional results, we explore whether early émigrés attracted academics from their network to the same university in the foreign destination. Our identification strategy predicts early emigration but not the exact location of early émigrés. Hence, we are able to show correlations without proving causality. We use the dynamic data on professional networks to construct dyadic data for this analysis. In each year, each Jewish academic i from a certain subject (e.g., physics) can potentially work in the same department as Jewish academic $j \neq i$ from the same subject. We then check whether academic i and academic j ever overlapped in the same city

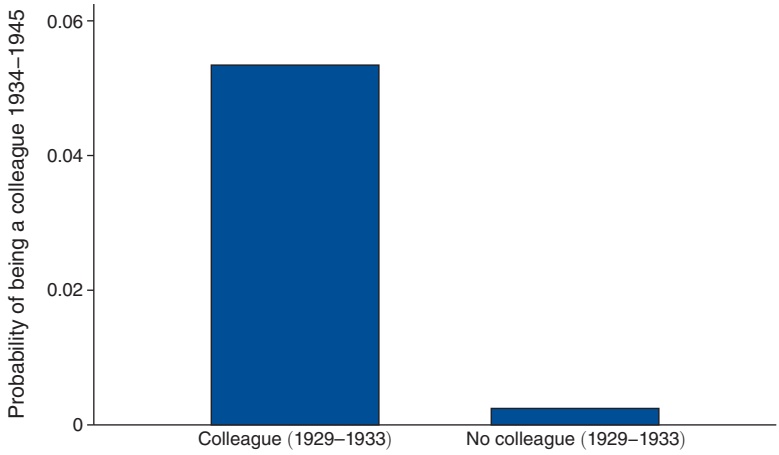


FIGURE 10. PROBABILITY OF WORKING IN THE SAME CITY 1934–1945

Notes: The figure shows the probability that academics worked in the same city and subject in the foreign destination in at least one year between 1934 and 1945, depending on whether they had been colleagues in Germany in at least one year between 1929 and 1933.

and subject in the foreign destination during the period 1934 to 1945. For example, the mathematicians Richard Courant and Fritz John overlapped at the University of Cambridge in 1934. We thus calculate the probability that academics worked in the same city and subject in the foreign destination between 1934 and 1945, depending on whether they had been colleagues in Germany before 1933. Jewish academics who had been colleagues in Germany before 1933 had a 5 percent probability of working in the same city and subject in at least one year between 1934 and 1945 (Figure 10). In contrast, Jewish academics who had not been colleagues in Germany before 1933 only had a 0.3 percent probability of ever working in the same foreign city and subject between 1934 and 1945. These results suggest that early émigrés not only helped their former colleagues to move to the same country but, in some cases, also to the same city.

V. Characteristics of Social Ties and their Effect on Emigration

Next, we analyze the characteristics of social ties that make them more or less effective in facilitating emigration.

A. *Decay of Social Ties*

Decay over Time.—We explore the "decay" of social ties over time by splitting ties to early émigré colleagues into two groups: ties to recent colleagues (overlap in 1933) and ties to less recent colleagues (overlap between 1929 and 1932, but not in 1933). Academics with 10 more ties to recent colleagues were 9.7 percentage points more likely to emigrate (Table 5, column 2). In contrast, academics with 10 more ties to less recent colleagues were only 5 percentage points more likely to

Table 5—Decay of Social Ties

Dep. variable:	(1) OLS Emigrated by 1939	(2) IV Emigrated by 1939	(3) OLS Emigrated by 1939	(4) IV Emigrated by 1939
# Early émigré colleagues (pre-1933 network–more recent colleagues)	0.109	0.097		
	(0.044)	(0.035)		
# Early émigré colleagues (pre-1933 network–less recent colleagues)	0.052	0.050		
	(0.021)	(0.019)		
# Early émigré colleagues (pre-1933 network–same department)			0.063	0.062
			(0.021)	(0.016)
# Early émigré colleagues (pre-1933 network–same city and subject, different department)			0.051	0.048
			(0.015)	(0.015)
Early émigré	0.349	0.318	0.344	0.314
	(0.030)	(0.142)	(0.031)	(0.144)
Baseline controls	Yes	Yes	Yes	Yes
Academic rank FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
City × subject (1929–1933)	Yes	Yes	Yes	Yes
Number of observations	1,327	1,327	1,327	1,327
R^2	0.649		0.649	
Kleibergen-Paap rk Wald F-statistic		38.360		35.913
Mean of dep. variable	0.741	0.741	0.741	0.741

Notes: The dependent variable is an indicator that equals 1 if academic i had emigrated by January 1, 1939. In columns 1 and 2, the first main explanatory variable is the number of early émigré colleagues from the pre-1933 network who overlapped on January 1, 1933 (more recent colleagues). The second main explanatory variable is the number of early émigré colleagues from the pre-1933 network who overlapped between January 1, 1929 and January 1, 1932, but not thereafter (less recent colleagues). In columns 3 and 4, the first main explanatory variable is the number of early émigré colleagues from the pre-1933 network from the same institution and subject. The second main explanatory variable is the number of early émigré colleagues from the pre-1933 network from other institutions in the same city and subject. Another important explanatory variable is academic i's own early émigré status. In columns 2 and 4 we instrument these variables with the number of early dismissals among the respective pre-1933 networks of colleagues and with an indicator for whether academic i him/herself was dismissed early (see online Appendix Table D3 for the first-stage results). For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history.

emigrate.[26] These results suggest that ongoing ties are more effective than past ties. In fact, a mere one-year interruption of regular interactions led to a decay in the strength of ties.[27] This is particularly surprising in the context of severe persecution during the Nazi period, with lives and livelihoods being threatened. One would have expected that academics may be willing to help former colleagues, even if they had lost touch. One possible explanation for the fast decay could be that recent interactions transmit more up-to-date information about future productivity (such as the research pipeline of an academic) that are more difficult to observe from a distance. An alternative explanation may be that academics had limited capacity to help and thus focused on more recent interactions. Both explanations are consistent with the observation that social ties can decay rapidly over time.

[26] The coefficients are significantly different from each other with p-values of 0.057 (IV) and 0.058 (OLS). We estimate a similar decay if we define recent colleagues as those with an overlap between 1932 and 1933.

[27] The differential effect of strong versus weak ties has also been highlighted in the literature on job referrals (e.g., Kramarz and Nordström Skans 2014; Dustmann et al. 2016).

Decay over Geographic Space.—We also analyze whether geographical proximity matters by differentiating between ties to early émigré colleagues from the same subject in a) the same department versus b) other departments in the *same* city. For example, a mathematician from the University of Breslau may have stronger ties to other mathematicians from the University of Breslau than to mathematicians at the Technical University of Breslau. Academics with 10 more ties to early émigrés from the *same* department were 6.2 percentage points more likely to emigrate (Table 5, column 4). The corresponding effect for early émigrés from the same subject employed by *another* institution in the same city is 4.8 percentage points. While the coefficients are not significantly different from each other, the point estimates suggest that the strength of ties in professional networks also decays across space, even *within* the same city.

B. *The Role of Networks in Information Transmission*

Theory would suggest that networks are more important when market participants have private information and when credible signaling through publicly observable channels is difficult. In contrast, networks should matter less for transmitting publicly observable information that is inferable from CVs or publication lists.

Humanities and Social Sciences versus Natural Sciences.—We begin our analysis by investigating the strength of social ties between broad scientific areas that differ in how easily outsiders can assess the individual quality of researchers. We find that ties to 10 additional early émigrés increased the probability of emigration by 3.5 percentage points for academics in the natural sciences and medicine. In contrast, ties to 10 additional early émigrés increased the probability of emigration by 14.8 percentage points for academics in the social sciences or humanities (Table 6, column 2). These findings are consistent with the observation that research quality in the hard sciences can be more objectively assessed than in the humanities and social sciences. In the sciences, the publication market was already international by the beginning of the twentieth century. Because German academics in the natural sciences were widely recognized as world leading, universities in most countries subscribed to German scientific journals, and reading German was a prerequisite in science PhD programs in the United States and elsewhere (see Iaria, Schwarz, and Waldinger 2018). Hence, foreign academics could relatively easily observe the quality of the dismissed German Jewish academics in the natural sciences. In contrast, in the humanities and social sciences, publication markets were more local and most academics published books in their own language. Thus, in these fields it was more difficult to assess the suitability and quality of academics from CVs and publication lists, which strengthened the role of professional networks.

Do Social Ties Transmit Private Information?—To further investigate whether networks can substitute for publicly observable information, we estimate the strength of networks depending on characteristics that proxy for how easily outsiders can observe the quality of individual scholars. In the first set of results, we interact the number of ties to early émigrés with characteristics that are not easily

Table 6—Social Ties: Transmitting Private Information

Dep. Variable:	(1) OLS Emigrated by 1939	(2) IV Emigrated by 1939	(3) OLS Emigrated by 1939	(4) IV Emigrated by 1939	(5) OLS Emigrated by 1939	(6) IV Emigrated by 1939
# Early émigré colleagues (pre-1933 network) × natural sciences and medicine	0.034	0.035				
	(0.016)	(0.014)				
# Early émigré colleagues (pre-1933 network) × social sciences and humanities	0.155	0.148				
	(0.051)	(0.053)				
# Early émigré colleagues (pre-1933 network) × fields with long gestation			0.192	0.215		
			(0.041)	(0.047)		
# Early émigré colleagues (pre-1933 network) × fields with short gestation			0.032	0.031		
			(0.018)	(0.016)		
# Early émigré colleagues (pre-1933 network)					0.054	0.051
					(0.015)	(0.017)
# Early émigré colleagues (pre-1933 network) × positive surprise in reputation					0.003	0.003
					(0.017)	(0.021)
# Early émigré colleagues (pre-1933 network) × negative surprise in reputation					−0.040	−0.041
					(0.020)	(0.025)
Early émigré	0.345	0.313	0.344	0.309	0.342	0.312
	(0.031)	(0.145)	(0.030)	(0.146)	(0.032)	(0.143)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Academic rank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
City × subject (1929−1933)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,327	1,327	1,327	1,327	1,327	1,327
R^2	0.650		0.650		0.651	
Kleibergen-Paap rk Wald F-statistic		36.240		35.725		30.082
Mean of dep. variable	0.741	0.741	0.741	0.741	0.741	0.741

Notes: The dependent variable is an indicator that equals 1 if academic i had emigrated by January 1, 1939. In columns 1 and 2, the first main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's specialization is in natural sciences or medicine. The second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's specialization is in social sciences or humanities. In columns 3 and 4, the first main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's specialization is in a field with long gestation. The second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's specialization is in a field with short gestation. In columns 5 and 6, the first main explanatory variable is the number of early émigré colleagues from the pre-1933 network. The second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's research reputation surprisingly improved after 1933. The third main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i's research reputation surprisingly deteriorated after 1933. We measure reputation by the number of entries in biographical compendia. In columns 5 and 6, we also control for positive surprise in reputation and negative surprise in reputation. Another important explanatory variable is academic i's own early émigré status. In columns 2, 4, and 6 we instrument these variables with the number of early dismissals among the respective pre-1933 networks of colleagues and with an indicator for whether academic i him/herself was dismissed early (see online Appendix Table D4 for the first-stage results). For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history. Standard errors are clustered at the city level.

observable and, hence, we expect that professional networks would be important for transmitting valuable information.

First, we find that the effect of professional networks is larger in subjects (e.g., history, economics, or mathematics) where academics publish longer but fewer works (Table 6, column 4). In those subjects, early émigrés may provide valuable information about expected future research productivity of their former colleagues. In contrast, in subjects with shorter gestation times (e.g., physics, medicine, or psychology), productivity trajectories are more observable and, hence, potential employers may have a better indication of expected future productivity.

Second, we show that networks can inform outsiders about productivity "surprises." For this test, we calculate two reputation measures for each academic: *pre*-1933 reputation and *life-span* reputation. We measure pre-1933 reputation by the number of entries in biographical compendia that were published *before* 1933 and life-span reputation by the number of entries in biographical compendia that were published until today.[28]

We then classify academics with a "negative surprise" in reputation as those whose pre-1933 reputation percentile was above the subject-specific fiftieth percentile but whose life-span reputation was below the fiftieth percentile. An example of such an academic is the astrophysicist Alexander Wilkens. He was dismissed from the University of Munich and emigrated to Argentina to work at the Universidad Nacional de La Plata. An obituary described him as "one of the last astronomers … whose scientific life was still rooted in that epoch of the history of astronomy which is commonly referred to as the classical one" (Stumpff 1969), suggesting that his approach to astronomy became outdated.

Similarly, we classify academics with a "positive surprise" in reputation as those whose pre-1933 reputation was below the subject-specific fiftieth percentile but whose life-span reputation was above the fiftieth percentile. An example of such an academic is the physicist Hans Bethe. After his dismissal he moved to the University of Manchester in 1933. Later he moved to Bristol and then to Cornell. He developed a theory of the deuteron in 1934, which he extended in 1949. He also studied the theory of nuclear reactions in 1935–1938. In 1967, he was awarded the Nobel Prize for the path-breaking research that he conducted in the mid-1930s and 1940s.

All other academics are classified as "no surprise" academics, which include both academics with high reputation in both periods (e.g., Albert Einstein) and those with low reputation in both periods (e.g., the relatively unknown physicist Wolfgang Gleißberg who was dismissed from Breslau and moved to the University of Istanbul).

Compared to "no surprise" academics, ties to early émigrés had a smaller effect on emigration for "negative surprise" academics, suggesting that early émigrés informed foreign networks about declining future productivity of potential hires (Table 6, columns 5 and 6). In contrast, ties to early émigrés had a somewhat larger effect on emigration of "positive surprise" academics, even though this difference is not significant. Overall, these results suggest that professional networks may be more important in situations where true quality is more difficult to observe.[29]

[28] Naturally, any measure of reputation that considers post-1933 data may potentially be endogenous. Hence, these results should be interpreted with caution.

[29] Because the effect of networks is indeed stronger in situations where theory would predict that they would matter more, the findings also suggest that the results are not driven by mechanical effects due to measurement error (see Huber 2022).

Table 7: Social Ties: Public Information

Dep. Variable:	(1) OLS Emigrated by 1939	(2) IV Emigrated by 1939	(3) OLS Emigrated by 1939	(4) IV Emigrated by 1939	(5) OLS Emigrated by 1939	(6) IV Emigrated by 1939
# Early émigré colleagues (pre-1933 network)	0.054	0.050	0.062	0.058	0.057	0.052
	(0.016)	(0.015)	(0.015)	(0.014)	(0.018)	(0.015)
# Early émigré colleagues (pre-1933 network) × international experience	–0.002	–0.002				
	(0.013)	(0.015)				
# Early émigré colleagues (pre-1933 network) × age ≤ 45			–0.011	–0.010		
			(0.009)	(0.009)		
# Early émigré colleagues (pre-1933 network) × ≤ median reputation					–0.004	–0.003
					(0.011)	(0.010)
Early émigré	0.342	0.312	0.343	0.314	0.341	0.304
	(0.032)	(0.146)	(0.032)	(0.144)	(0.033)	(0.149)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Academic rank FE	Yes	Yes	Yes	Yes	Yes	Yes
City × subject (1929-1933)	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,327	1,327	1,327	1,327	1,327	1,327
R^2	0.649		0.649		0.650	
Kleibergen-Paap rk Wald F-statistic		35.801		37.278		44.494
Mean of dep. variable	0.741	0.741	0.741	0.741	0.741	0.741

Notes: The dependent variable is an indicator that equals 1 if academic i had emigrated by January 1, 1939. The first main explanatory variable is the number of early émigré colleagues from the pre-1933 network. In columns 1 and 2 the second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i had pre-1933 international experience. In columns 3 and 4, the second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i was younger than 46 years old in 1933. In columns 5 and 6, the second main explanatory variable is the interaction of the number of early émigré colleagues from the pre-1933 network with an indicator that equals 1 if academic i had below median pre-1933 subject-level reputation, as measured by the number of entries in biographical compendia. Another important explanatory variable is academic i's own early émigré status. In columns 2, 4, and 6, we instrument these variables with the number of early dismissals among the respective pre-1933 networks of colleagues and with an indicator for whether academic i him/herself was dismissed early (see online Appendix Table D5 for the first-stage results). In columns 5 and 6, we also control for academics with below or equal to median reputation. For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history. Standard errors are clustered at the city level.

In contrast, the next set of results shows that the number of ties to early émigrés does not differentially affect emigration rates by transmitting information that is easily observable. We interact the number of ties to early émigrés with characteristics that are easily observable from CVs and publication lists. For example, employers can infer whether applicants had prior professional experience abroad from the applicant's CV. Accordingly, we find that the effect of professional networks is very similar for academics with and without experience abroad (Table 7, column 2). Similarly, employers know the applicants' age from their CV, and indeed our results indicate that the effect of professional networks is similar for younger (than 45 years) and older academics (Table 7, column 4). Finally, we find that the effect of professional networks is similar if we compare academics of higher pre-1933 reputation, which is easily observable from CVs (Table 7, column 6). As before, we proxy for academic reputation by counting the number of entries

in biographical compendia that were published *before* 1933. Overall, the results suggest that professional networks play an important role in the transmission of private information, but do not disproportionately affect emigration rates by transmitting information that is publicly available.

VI. Professional versus Family and Community Networks

Finally, we investigate the role of professional networks, relative to family and community networks, in emigration decisions. We construct a measure of family networks using data from the *List of Jewish Residents* compiled by the German Federal Archive (see online Appendix E). For our family network measure, we count the number of early émigrés (born within a $\pm$ 10-year window) with the same last name from the city of residence of each academic. The measure proxies for relatives such as wives or husbands, siblings, and cousins of each academic. The average academic had 0.8 early émigrés in his family network (Table 1). This indicates that we capture close familial ties. Furthermore, the low number of early émigrés from the family network suggests that non-academics were less likely to emigrate by 1935 than were academics. Similarly, we construct a measure of non-family community networks based on data from the *List of Jewish Residents.* The measure counts the number of early émigrés (born within a $\pm$ 10-year window) with a *different* last name from the city of residence of each academic. The average academic had 858.6 early émigrés in his non-family community network (Table 1).[30]

We reestimate equation (3) and add the measures of the family and community networks:

$$
\begin{aligned}
(6)\quad & \textit{Emigrated By } 1939_i \\
&= \alpha_1 + \alpha_2 \#\textit{Early Émigré Colleagues } \left(\textit{Pre}\text{-}1933 \textit{ Network}\right)_{-i} \\
&\quad + \alpha_3 \#\textit{Early Émigrés } \left(\textit{Pre}\text{-}1933 \textit{ Family Network}\right)_{-i} \\
&\quad + \alpha_4 \#\textit{Early Émigrés } \left(\textit{Pre}\text{-}1933 \textit{ Community Network}\right)_{-i} \\
&\quad + \alpha_5 \textit{Early Émigré}_i + \alpha_c \textit{Controls}_i + \upsilon_i.
\end{aligned}
$$

Importantly, adding the measures for family and community networks does not affect the estimated coefficients of the professional academic network (Table 8). Early émigrés from the family network also affect emigration with a somewhat smaller magnitude than the professional networks. Ties to 10 additional early émigrés from the family network increase emigration by around 4 percentage points (Table 8, columns 1–2, 5–6). Strikingly, for academics, community networks did not affect emigration decisions *at all* (columns 3–6), even though a recent paper by Buggle

[30] Results are similar if we measure family and community networks using $\pm$ five-year windows. As the city $\times$ subject employment history effectively controls for the total number of ties to early émigrés from the non-family community network in a city, we do not estimate specifications without age restrictions.

Table 8—Professional Networks, Family Networks, and Community Networks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
							OLS	IV
	OLS	IV	OLS	IV	OLS	IV	Excluding most common last names	
Dep. variable:	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939	Emigrated by 1939
# Early émigré colleagues (pre-1933 network)	0.052	0.049	0.055	0.050	0.056	0.051	0.052	0.046
	(0.014)	(0.014)	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)	(0.020)
# Early émigrés (pre-1933 family network)	0.041	0.042			0.042	0.043	0.041	0.041
	(0.012)	(0.015)			(0.011)	(0.014)	(0.017)	(0.017)
# Early émigrés (pre-1933 community network)			−0.000	−0.000	−0.000	−0.000	−0.000	0.000
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Early émigré	0.341	0.318	0.342	0.312	0.342	0.318	0.342	0.364
	(0.031)	(0.143)	(0.032)	(0.145)	(0.031)	(0.144)	(0.032)	(0.126)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Academic rank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × subject (1929−1933)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,327	1,327	1,327	1,327	1,327	1,327	1,268	1,268
R^2	0.649		0.649		0.649		0.656	
Kleibergen-Paap rk Wald F-statistic		62.637		56.409		62.761		74.073
Mean of dep. variable	0.741	0.741	0.741	0.741	0.741	0.741	0.741	0.741

Notes: The dependent variable is an indicator that equals 1 if academic i had emigrated by January 1, 1939. The first main explanatory variable is the number of early émigré colleagues from the pre-1933 network. In columns 1–2 and 5–8, the second main explanatory variable is the number of early émigrés who were born within a ± ten-year-window with the same last name as academic i and resided in cities where academic i worked between 1929 and 1933. In columns 3–8, the second or third main explanatory variable is the number of early émigrés who were born within a ± 10-year-window with a different last name as academic i and resided in cities where academic i worked between 1929 and 1933 (see online Appendix E for details). Another important explanatory variable is academic i's own early émigré status. In columns 2, 4, 6, and 8, we instrument the number of early émigré colleagues from the pre-1933 network with the number of dismissed colleagues from the pre-1933 network and the emigration status in 1935 with an indicator that equals 1 if academic i him/herself was dismissed early (see online Appendix Table D6 for the first-stage results). For a small number of academics, information on some control variables (family status, language proficiency, and the place of birth) is missing. The regressions therefore also include unreported indicators for missing information on these variables. We also include fixed effects for each academic rank, year of birth fixed effects, and controls for the city × subject employment history. Standard errors are clustered at the city level.

et al. (2023) estimates sizeable effects of community networks for the German Jewish population *overall*. We measure networks using the place of residence because they are likely to have the largest effect on emigration decisions. In contrast, Buggle et al. (2023) measure community networks based on the place of birth. We confirm our findings by showing that community networks based on the place of birth (as in Buggle et al. 2023) also have no effect on emigration decisions of academics (online Appendix Table E1).[31]

For academics with very common last names, the measure of family networks may capture relatively distant relatives or individuals who are not related. To

[31] An alternative explanation for these findings is that the measure of community networks suffers from measurement error that is almost absent from the professional network measure. However, it is important to note that Buggle et al. (2023) find sizable effects for the general Jewish population using the very same measure of community networks.

probe robustness, we exclude academics with the 10 most common last names in the *Resident List* from the sample. The estimated effects are very similar in this restricted sample (columns 7–8).

These findings show that professional networks are important for the emigration decisions of *high-skilled* individuals and have a somewhat larger effect than family networks. Community networks do not matter at all for the emigration decisions of high-skilled individuals. This is an important result, because community networks have been at the center of most empirical papers that study the effect of networks on migration decisions. Our results suggest that emigration decisions of high-skilled individuals are driven by different networks than the migration decisions of lower-skilled individuals. Furthermore, any analysis of the effect of networks on the migration decisions of high-skilled individuals would be seriously incomplete if it ignored professional networks.

VII. Conclusion

Our study shows that *professional* networks play a key role in the emigration decisions of high-skilled individuals. In particular, we show that ties to early émigrés affected emigration, highlighting the special role of bridging nodes for emigration. For high-skilled academics, professional networks were at least as important as family networks, and community networks played no role at all. We also show that social ties decay over time and over short geographic distances. Furthermore, early émigrés functioned as *bridges* that helped academics cross over into the same destination. We also show that networks are key to transmit private information that is not easily available through other channels.

Our findings indicate that professional networks cause dynamic migration responses. The temporary surge in the number of world-class academics in the United States in the wake of the Nazi's rise to power solidified the transition of scientific leadership from Germany to the United States in the postwar period. We also provide one of the first comprehensive documentations of academics of Jewish origin and their fate—whether they were murdered, escaped the Holocaust and emigrated abroad.

An important question is whether evidence on the role of professional networks during the 1930s and 1940s informs us about the role of academic networks today. While results from any empirical study are context specific, broader lessons can often be learned (see, e.g., List 2020). A number of reasons suggest that the findings of this paper may be of broad relevance. The academic job market at the time followed practices and conventions that were very similar to today. Much like today, academics were evaluated on the basis of research quality but, in addition, personal connections mattered to generate job offers. Unlike today, academics could not communicate via email or social media. Nonetheless, most of them corresponded frequently with colleagues and many postal services delivered letters multiple times a day. For example, Albert Einstein corresponded with colleagues more than 1,300 times between the years 1925 and 1927 alone (Einstein 2018). With a higher fraction of academics working at foreign universities, the role of professional networks

for international migration of academics may be even more important today than in the past.

Our results suggest that even short-term interruptions (e.g., the temporary suspension in 2020 of the H-1B visa program in the United States) or surges of high-skilled migration can have long-term implications because they affect long-term migration flows through the professional network. Similarly, brain drain caused by short-term factors, such as persecutions, can have long lasting effects because the migration decisions percolate through the professional network. For instance, in recent years, academics have faced persecution in many countries, including Hong Kong, Hungary, and Turkey. In 2020 alone there were 341 attacks on universities in 58 countries (Scholars at Risk 2020). This suggests that visa policies to attract high-skilled individuals who face persecution in their home countries can be a powerful tool to deepen a country's talent pool.

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