Repression, Exit, and Dissent in an Autocracy*

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Abstract

Does repression silence dissent or push people to leave? We study the arrest of protesters during East Germany's Peaceful Revolution, using novel micro-data from Leipzig. Residents of streets and houses with an arrested protester were more likely to have left their address once the Berlin Wall fell, and exit visa applications rose in the months after an arrest. Nearby protest activity and support for the opposition show no visible decline after an arrest. Our results suggest that repression raised the demand for exit without a visible retreat of voice, and that residents acted on this demand once leaving became possible.

Keywords: Protest, repression, emigration, exit visas, East Germany, autocratic breakdown

JEL Codes: H10, N44, P20.

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1 Introduction

Autocracies rely on violence and coercion against their own citizens (Svolik 2012; Gerschewski 2013). This repression often targets the *voice* of the opposition, as the violent crackdowns on protesters in China in 1989, Belarus in 2020, and Iran in 2022 demonstrate. Yet citizens have a second response to authoritarian rule: they can choose to *exit*, which regimes often fear as much as voice. Those who leave are disproportionately young and well-educated, so that mass emigration can have severe economic repercussions. At the same time, mass emigration can reveal the regime's lack of popular support, thus undermining regime stability (Lohmann 1994; Kuran 1991). How do these strategies relate to each other? How do individuals respond when a regime represses public dissent: do they fall silent, keep protesting, or leave? Whether the two channels substitute for or complement each other is a central question in the study of autocracies (Hirschman 1970, 1993; Pfaff and Kim 2003; Karadja and Prawitz 2019). Credible causal evidence on this question, however, remains scarce.

This paper asks how repression affects exit and voice within an empirically highly relevant historical setting. We study Leipzig, the epicenter of anti-regime mobilization during the 1989 *Peaceful Revolution* in socialist East Germany (*German Democratic Republic, GDR*) that, together with out-migration, ultimately precipitated the collapse of the GDR (Hirschman 1993). During the demonstrations of that year, local police and officials from the *Ministry of State Security* (*Stasi*) arrested several hundred peaceful protesters. Most were released within 24 hours with a signed warning, but detainees report beatings, threats, and hours-long interrogations. A third were fined substantially and some faced criminal proceedings and months in custody. Because we know where each arrested protester lived, these arrests let us follow how a single act of repression reverberated through a neighborhood and influenced the behavior of residents on the same street.

The core of our empirical contribution is new data that link the home addresses of arrested protesters to two measures of exit. The first follows every resident listed in the Leipzig telephone books of 1985, 1987, 1991, and 1996, so that we observe who left their address once the border opened. The second is a monthly street-level panel of applications for an exit visa, the only legal way for most citizens to leave the GDR after the construction of the Berlin Wall (1961). The regime treated the wish to leave as opposition and answered it with surveillance and repression (Lueders 2025), so that each application was a costly and politically salient response to autocratic rule (Hirschman 1993; Pfaff and Kim 2003). We ask whether the neighbors of an arrestee left once they could, whether their streets recorded more applications to leave in the months before, and whether voice near them changed.

We begin with a stylized model in which citizens decide whether to protest (voice) and

whether to apply for an exit visa (exit). An arrest has two opposing effects. As a salient signal of the regime's repressiveness, it raises grievances and thereby strengthens incentives for both exit and voice. At the same time, it reveals the potential costs of actions that challenge the regime, deterring both responses. Whether either response rises or falls therefore depends on the relative strength of grievance and deterrence. The data allow us to distinguish which force dominates on each margin.[1]

We find that arrests pushed people toward exit. Between 1987 and 1991, 34 percent of the residents listed on streets without an arrest left their address. On streets with an arrest in 1989, this share was 3.6 percentage points higher, about a tenth more. Among the listed residents of these streets alone, this amounts to about 570 additional departures, or 1.7 for each arrest. In houses with an arrest, it was 6.6 points higher than in other houses, where 27 percent left, and 4.8 points higher when compared only with other houses on the same street. The effect is concentrated immediately after the opening of the border, when leaving first became possible. It is not driven by the arrested themselves, whom we remove from the data.[2] Nor is it driven by a general economic decline of streets with arrests, whose number of listed shops and institutions did not fall relative to other streets. We therefore interpret the excess moves as realized emigration and discuss below why departures after 1989 are the appropriate test of an exit response formed under the regime.

This demand for exit formed before November 1989. We exploit the staggered timing of arrests over the year 1989 and, using the difference-in-differences estimator of Callaway and Sant'Anna (2021), link it to exit visa applications. On the arrestee's street, applications rose by about 0.2 per month in the six months after an arrest, compared to a pre-arrest level of 0.42 per month. On streets within 200 meters of an arrest, applications rose by 0.08 per month over the following four months. Because applications are sparse and surged city-wide in January 1989, we treat these estimates as supporting evidence for the result on moves (Section 5.1). Taken together, the two measures are consistent with arrests raising the demand to leave under the regime and with residents acting on this demand once the

[1] Our framework combines elements emphasized separately in the literature on informational autocracy and exit and voice, namely state-capacity and informational-autocracy accounts (Besley and Persson 2011; Guriev and Treisman 2020) and fear-based exit-voice frameworks (Pfaff and Kim 2003; Bühler and Madestam 2023). Its distinctive feature for our setting is to make exposure to a salient act of repression local: an arrest changes the incentives not only of the arrested individual but also of nearby residents who learn about it, with the response attenuating with distance. This maps directly into our street-level treatment and spatial spillover tests.

[2] Our measure records every departure from an address, of which leaving for West Germany, the destination of most post-1989 out-migration from Leipzig, is the main component. Between 1989 and 1991, about 1.4–1.6 million people moved from East to West Germany. Internal migration was substantially smaller and rose only from the mid-1990s, mostly from the countryside *into* cities (Berlin Institut für Bevölkerung und Entwicklung 2026).

border opened.

The circumstances of the arrests support a causal interpretation. Protesters were arrested at the site of demonstrations, not at home, and *Stasi* reports describe bulk detentions to disperse crowds instead of the targeting of specific individuals or neighborhoods (Ministry of State Security 1989a,d). For the result on moves, residents of treated and other streets moved equally often in 1985–87, before any arrest. For the visa application results, the month in which a street first saw an arrest must have been set by the protest calendar and police operations, not by developments on that street. Almost two thirds of treated streets saw their first arrest in January, May or June 1989—months before protest became a mass movement—and applications on treated and control streets moved in parallel before the first arrest. The application results survive matching on the propensity to be arrested, controls for local protest activity, and spatial standard errors. Two placebo tests support the mechanism our model proposes. The *Stasi* kept secret apartments for meetings with its informants (so-called conspiratorial flats), and the opening of such a flat near a street, which residents could not observe, produces no response in applications. Residents of the streets on which the arrested worked did not apply more either. Thus, unobservable forms of state surveillance generate no comparable response, and the response to arrests is concentrated around arrestees' residential rather than workplace environments.

Lastly, we ask whether the rise in exit came at the expense of voice: did arrests push people to substitute emigration for protest? Two measures of voice suggest that arrests did not have this effect. A weekly event study that compares treated streets with non-treated streets at the same distance from the site of the arrest finds no change in the location of protests after an arrest. Because the calendar records where protests took place, not who attended, the test reveals no visible local retreat of protest rather than to individual participation. Data we retrieved from the sign-up sheets of the *Neues Forum* opposition movement, the only record of support for the opposition that lists addresses, give the same answer. Support was concentrated at the addresses of arrested protesters. For the streets experiencing arrests in October, this was already the case before the arrest, and the dated entries show no decline in new sign-ups afterwards.

Although our analysis focuses on one city alone, the conditions surrounding repression, exit, and voice in Leipzig are not uncommon. Autocracies often repress protest visibly and locally, information about arrests travels through neighborhoods, and citizens who cannot change the regime can try to leave it, as after the crackdowns in Belarus in 2020 and Iran in 2022. Where these conditions hold, our findings suggest that repression can push citizens toward exit without a visible retreat of voice, and that its effects reach well beyond those it targets.

Our paper makes several contributions to existing scholarship. It contributes first to research on autocratic repression (Besley and Persson 2011; Yanagizawa-Drott 2014; Bautista et al. 2023; Rozenas and Zhukov 2019). Repression is a key tool through which autocrats preserve power (Acemoglu and Robinson 2001), especially when anti-regime mobilization rises (Acemoglu et al. 2018; Enikolopov et al. 2020; Beraja et al. 2023). We inquire about the costs of such repression to the regime, focusing on exit—a response that worried the regime as much as voice did. Our results speak to the limits of repression as a survival strategy: arrests were followed by more demand for and realized emigration from the affected streets and houses, while we find no visible decline in local protest or in new sign-ups for the opposition. In producing this finding, we contribute to scholarship on informational autocracies (Guriev and Treisman 2019): our results are consistent with visible repression revealing the regime's nature to citizens who were previously unaware of it, and in doing so changing their political choices. Moreover, while past work typically observes the consequences of repression at a single point in time (Lichter et al. 2021; Hager and Krakowski 2022; Nikolova et al. 2022), the fine-grained panel data we assemble for this study allow us to observe the consequences of arrests both in the immediate aftermath and years later.

We also contribute to the exit–voice–loyalty framework (Hirschman 1970; Pfaff 2006; Bühler and Madestam 2023; Karadja and Prawitz 2019). Our results align with Hirschman (1993), who argued that exit and voice did not trade off against one another but reinforced each other during the GDR's Peaceful Revolution. Consistent with this argument, we show that arrests of protesters raised exit while voice did not visibly decline. We add to the literature on protest (Bursztyn et al. 2021; Cantoni et al. 2019; Enikolopov et al. 2020; Madestam et al. 2013) by studying the consequences of the violent repression of protest, and how the response extends beyond the protesters to their neighbors.

Finally, we contribute to research on emigration from autocracies. Most of this work studies the consequences of emigration for autocratic stability (Barry et al. 2014; Barsbai et al. 2017; Sellars 2019; Lueders 2025); we study one of its causes, with data on exit visa applications and residential moves that are fine-grained in both time and space. We join work on how persecution shapes the decision to leave (Buggle et al. 2023) and on the consequences of expulsion and forced displacement (Waldinger 2010; Nakamura et al. 2022; Becker and Ferrara 2019), and we provide new quantitative evidence on popular discontent in the GDR (Buggle et al. 2026; Crabtree et al. 2018; Gläßel and Paula 2020; Martinez et al. 2022; Mohr 2021).

2 Background

2.1 Protests and the Demise of the GDR

Exit and voice grew hand in hand in the final months of the GDR (Hirschman 1993). Aside from the People's Uprising (*Volksaufstand*) in 1953, large-scale collective action only re-emerged in 1989, when mounting economic problems and political frustrations triggered a wave of public protests. An ever growing number of East Germans took to the streets to demand economic reforms, political liberalization, and freedom of movement. One early focal point was the local elections on 7 May 1989, when civic leaders gathered and distributed conclusive evidence of widespread fraud (Kowalczuk 2009).

Economic and political grievances were also met with growing demand for exit. Applications for exit visas had been rising since the late 1980s, and in the summer of 1989 leaving became possible without one: thousands of East Germans refused to return from their vacations in Hungary and Czechoslovakia. They sought refuge in the West German embassies in Budapest and Prague, and after Hungary opened its border to Austria on 11 September 1989, they left in the tens of thousands (Figure A1). The special trains that carried the Prague embassy refugees through the GDR to the West at the beginning of October were among the immediate triggers of the largest demonstrations.

Leipzig quickly became the epicenter of this Peaceful Revolution. Demonstrations grew out of Protestant peace prayers in Leipzig's Nikolai Church on Monday evenings, which developed into regular mass marches calling for political reform and freedom of movement. Weekly Monday demonstrations grew from a few hundred participants to hundreds of thousands by late October 1989, representing a large share of the city's population and drawing an increasingly forceful response from the security apparatus (Lohmann 1994; Kuran 1991). The timing of this protest peak, just weeks before the fall of the Berlin Wall, makes Leipzig an ideal setting to study how repression operates in a late-stage autocracy.

2.2 Arrests of Protesters

The East German government considered the anti-regime protests illegal, subversive, and instigated by Western powers. It responded with violence and repression to crack down on the growing opposition movement. State police forces were tasked with dispersing the growing crowds of protesters. To deter future protests, they arrested individual protesters at the protest location itself, often haphazardly. These *Zuführungen*—temporary detentions lasting typically up to 24 hours—were one of the regime's principal tools of repression against

demonstrators.[3]

Eyewitness accounts document the brutality and arbitrariness with which the police proceeded. Officers picked up participants seemingly at random, and detainees report overcrowded cells, hours-long interrogations, and beatings; Appendix C collects these accounts. Two features of the arrests are critical for our analysis. First, they occurred in public, so that neighbors and colleagues learned of them quickly (Section 5.3). Second, they were costly. The internal *Stasi* reports on arrests we consulted for this study provide detailed information on punishments for arrestees. Of the 466 persons arrested between January and July 1989, 59 percent were released the same day with a signed warning (*Belehrung*), 33 percent received an administrative fine of 100 to 500 marks—up to half of an average monthly net income (1,000 marks) (Statistical Office of the GDR 1990)—and 7 percent faced criminal proceedings under § 217 of the penal code (*Zusammenrottung*)—which usually involved fines of 3,000 to 10,000 marks.[4] Sanctions escalated in the fall: after the arrests of 11 September, most detainees were sentenced by *Strafbefehl* (penalty order) to fines of 1,000 to 5,000 marks and pressed to waive their right of appeal. Eighteen of them were held in custody for four to six months.[5] For those affected, the experience could be transformative: a 40-year-old woman arrested with her 12-year-old son was left with "the one single wish to leave this state as soon as possible."[6]

2.3 Emigration restrictions and exit visa applications

Like other socialist regimes, the GDR placed strict limits on its citizens' freedom of international movement. Emigration restrictions originated soon after the end of World War II, when an average of 250,000 East Germans migrated to West Germany every year (BArch DE 2/22422). This mass emigration quickly became worrisome to the East German authorities. It was seen as at odds with the GDR's claim to moral and political superiority (Dowty 1988). It also had important economic repercussions, as the majority of emigrants were relatively well-trained, young men (Hirschman 1993). For these reasons, the GDR government started

[3] A *Zuführung* allowed the *Stasi* to establish and register a detainee's identity and to "clarify a situation." Its legal basis was § 12 of the *Volkspolizei* (Police) Law of 1968. Detainees could also be brought in for questioning under § 95 (2) of the Criminal Procedure Code of 1968, as a criminal procedural security measure.

[4] The sheets report, for each arrest day, the number of criminal proceedings, the number of fines and their total in marks; the day-by-day figures are part of the replication package. In January the eleven proceedings were opened without fines. Note that the summary sheets count 466 persons compared with the 467 arrests recorded for the same period in other parts of the reports.

[5] Opposition documentation of the 11 and 12 September arrests in the *Citizens Movement Archive*; the *Stasi* lists for September and October record the same sanctions for each person.

[6] Source: https://www.bundesarchiv.de/assets/bundesarchiv/de/Publikationen/AuD_181_Suess_Staatssicherheit_am_Ende_A2_barrierefrei.pdf

to fortify the country's border in the early 1950s (Geier 2019). These efforts culminated in the construction of the Berlin Wall in August 1961. The Wall gave the GDR government full control over who was and was not allowed to leave the country. Figure A1 shows the monthly migration patterns until 1989.

Emigration-seekers, if they did not want to risk their lives at the border, had to apply for a formal exit visa to leave the country (*Antrag auf ständige Ausreise*). The process was costly and risky. Applications were rarely approved outright and almost always led to harassment of applicants and their families. In-person interviews were routinely used by *Stasi* officials to pressure applicants to withdraw their application (Lueders 2025; Mayer 2002; Michel et al. 2023). Despite these many formal restrictions, demand for emigration grew rapidly in the late 1980s. By the end of 1988 more than 100,000 applications were pending across the GDR (Eisenfeld 1996), and the *Stasi*'s own reports on applicants' motives read as a catalog of grievances against the regime (Ministry of State Security 1989b). A new travel regulation, adopted on 30 November 1988 and in force from 1 January 1989, for the first time set out in law who could apply for permanent emigration and how. While intended to channel the pressure, it instead produced an immediate surge of applications (Figure 1). Applications remained high for the rest of the year.

Two observations bear on how to read a rise in applications after an arrest. First, the regime did at times use exit as an instrument: it selectively granted visas to protest participants to remove them from the country, and our individual-level results in Section 5.4 show that arrested protesters were indeed more likely to have left. An increase in applications by the arrested themselves may therefore be an outcome the regime accepted or even sought. Second, that reading does not extend to the arrested person's neighbors. The regime treated applications from ordinary citizens as hostile acts, met them with surveillance and pressure to withdraw, and granted few; and it was mass demand for exit, not the departure of individual activists, that undermined its standing in 1989. Our contribution concerns the response of the people around an arrest, who were never detained and whom the regime had no wish to lose. Our application data count applications filed, not visas issued.

3 Theoretical Framework

In this section, we model how citizens respond to information about local repression through exit and voice. The model illustrates two competing effects of learning about repression. On the one hand, information about a repressive act can increase citizens' perceived disutility from living under an autocratic regime, strengthening incentives to both exit and protest. On the other hand, the same information can reveal the potential costs of actions that

challenge the regime, potentially deterring both responses. Importantly, arrests are salient events whose informational effects can extend beyond the individuals directly affected. We therefore allow information about an arrest to diffuse through residential neighborhoods, with citizens' exposure depending on their geographic proximity to the arrestee's residence. The model is illustrative and intended to organize the interpretation of our empirical results rather than to provide a structural account of citizens' behavior or the regime's choice of repression.

Each citizen i experiences disutility from autocracy

$$d_{i,t} = \alpha_i \alpha_{i,t}, \tag{1}$$

where $\alpha_i \in [0,1]$ captures citizen i's underlying aversion to autocracy and $\alpha_{i,t} \in [0,1]$ denotes the perceived repressiveness of the regime at time t. These perceptions can change when citizens learn about repressive acts. Let $A_{j,t} \in \{0,1\}$ indicate that a resident of location j is arrested at time t. Location j thus refers to the arrestee's place of residence, not to the site at which the arrest occurs. Let $w(d_{ij}) \geq 0$ capture citizen i's exposure to information about this arrest as a function of geographic distance d_{ij} from the arrestee's residence. This formulation captures the idea that information about an arrest diffuses through residential neighborhoods and local social networks, so that residents living closer to the arrestee are more likely to learn about it. We assume that informational exposure weakly decreases with distance, $w'(d) \leq 0$. Define citizen i's exposure to information about local repression as

$$R_{i,t} = \sum_j w(d_{ij}) A_{j,t}. \tag{2}$$

Perceived repressiveness depreciates at rate λ in the absence of new information but increases when citizens receive information about repressive acts:

$$\alpha_{i,t+1} = (1-\lambda)\alpha_{i,t} + \delta R_{i,t}, \tag{3}$$

where $\lambda \in (0,1]$ and $\delta > 0$ captures the sensitivity of beliefs to information about repression.[7]

Citizens can respond to their disutility from autocracy through voice, in the form of protest effort $v_{i,t}$, or exit effort $e_{i,t}$. Information about repression affects both choices through two opposing channels. First, by increasing $\alpha_{i,t}$, learning about repression raises the perceived disutility from living under the regime. We refer to this as the *grievance effect*. Second, learning about repression provides information about the potential consequences

[7]For simplicity, we focus on parameter values and realizations for which $\alpha_{i,t} \in [0,1]$. Alternatively, equation (3) can be interpreted as a local linear approximation to a bounded belief-updating process.

of actions that challenge the regime. Protest participation exposed citizens to arrest and other sanctions, while applications for permanent exit could themselves trigger surveillance, workplace pressure, and other forms of harassment. We refer to this as the *deterrence effect*.

We capture these forces with quadratic utility functions. Citizen i's payoff from exit effort is

$$U_{i,t}^{E} = \left(\beta_E d_{i,t} - \gamma_E R_{i,t-1}\right) e_{i,t} - \frac{c_E}{2} e_{i,t}^2, \tag{4}$$

while the payoff from protest effort is

$$U_{i,t}^{V} = \left(\beta_V d_{i,t} - \gamma_V R_{i,t-1}\right) v_{i,t} - \frac{c_V}{2} v_{i,t}^2. \tag{5}$$

Here, $\beta_E, \beta_V > 0$ measure how strongly disutility from autocracy motivates exit and voice, respectively; $\gamma_E, \gamma_V \geq 0$ capture the deterrent effect of information about repression; and $c_E, c_V > 0$ govern the costs of effort. The deterrence parameters need not be identical: learning about an arrest may, for example, convey more information about the risks associated with public protest than about those associated with applying for exit.

Optimal exit and protest efforts are therefore

$$e_{i,t}^{*} = \max\left\{\frac{\beta_E d_{i,t} - \gamma_E R_{i,t-1}}{c_E}, 0\right\}, \qquad v_{i,t}^{*} = \max\left\{\frac{\beta_V d_{i,t} - \gamma_V R_{i,t-1}}{c_V}, 0\right\}. \tag{6}$$

For expositional simplicity, consider interior solutions. Substituting equation (3) into the optimal exit decision shows how an arrest of a resident of location j in period $t-1$, and the information it generates, affects the subsequent exit effort of citizen i:

$$\frac{\partial e_{i,t}^{*}}{\partial A_{j,t-1}} = \frac{w(d_{ij})}{c_E}\left(\beta_E \alpha_i \delta - \gamma_E\right). \tag{7}$$

The sign of this response is theoretically ambiguous. If the grievance effect dominates, $\beta_E \alpha_i \delta > \gamma_E$, learning about an arrest increases exit effort: the information makes the regime appear sufficiently more undesirable to outweigh the increased perceived cost of acting against it. If the deterrence effect dominates, $\beta_E \alpha_i \delta < \gamma_E$, the same information reduces exit effort. Our empirical analysis therefore tests which of these competing responses dominates in the context of the East German regime.

The corresponding effect on protest effort is

$$\frac{\partial v_{i,t}^{*}}{\partial A_{j,t-1}} = \frac{w(d_{ij})}{c_V}\left(\beta_V \alpha_i \delta - \gamma_V\right). \tag{8}$$

Hence, repression need not induce a simple substitution from voice toward exit. Depending

on the relative strength of grievance and deterrence effects on the two margins, information about an arrest may increase both exit and voice, reduce both, or induce movement from one response toward the other. In particular, a positive exit response need not be accompanied by declining protest if the grievance effect also offsets deterrence on the voice margin.

The spatial structure of the model yields an additional implication. Equations 7 and 8 imply that the behavioral consequences of an arrest are strongest among citizens with the greatest exposure to information about it and attenuate as $w(d_{ij})$ declines with distance from the arrestee's residence. Repression can therefore affect citizens who were never themselves arrested: the arrest of one resident conveys information about the regime to others who learn about it through their residential environment or local social networks. This distinguishes the public-information mechanism from explanations operating exclusively through private consequences for the arrested individual, such as prosecution, fines, or heightened surveillance. Empirically, we therefore examine not only responses by arrested individuals or their households, but also responses among other residents of the same street and residents of nearby streets.

Finally, our empirical design focuses on the first arrest of a resident of a street. Such an event may be particularly informative when residents initially have incomplete information about the regime's willingness to repress dissent. In particular, the arrest of someone from one's own residential environment can make repression that was previously more distant newly salient and informative. Subsequent arrests may contain less new information once the possibility of repression has already become salient locally. While the linear updating equation above abstracts from such diminishing informational content, this interpretation motivates our focus on first exposure in the event-study analysis. The model's central predictions do not require subsequent arrests to have no effect; rather, the first arrest provides a transparent empirical measure of the onset of locally salient information about repression.

4 Data

To test the implications of our model, we collected fine-grained data on arrests of protesters, exit visa applications, residential mobility, protest activity, and support for the opposition. Our focus is on the city of Leipzig, the epicenter of the 1989 Monday Demonstrations that brought down socialist one-party rule in the GDR.

Protester Arrests. The main explanatory variable is the arrest of protesters. We use novel data on every arrest recorded in Leipzig during the anti-regime demonstrations between January 1989 and the fall of the Berlin Wall on 9 November 1989, drawn from archival material at the Leipzig branch of the former *Stasi Records Agency*, now part of the Federal

Archives. For each arrested individual, daily arrest records provide information on the full name, exact address and date of arrest, the sanction imposed, and, for a subset, the arrestee's workplace.[8] Our dataset contains 518 arrests of Leipzig residents that can be assigned to a street.[9] Arrests are concentrated in time. The 518 arrests we record in our sample occurred on 18 distinct days, spread out over 15 calendar weeks and six calendar months (January, May, June, July, September and October 1989); Table A2 gives the share of treated streets by month. Figure 1 shows the weekly pattern of all arrests, and Table A1, Panel B, their monthly distribution over the full sample, which also covers detainees living outside Leipzig. In our monthly panel, these arrest days form six treatment cohorts; Section 5.2 discusses the implications for inference. Compared with the average Leipzig resident, arrested protesters were far more likely to be men and less likely to be school students.[10] Half of them shared a street with at least one other arrestee, which is close to what random placement of arrests across residents would produce (47 to 51% in a simulation weighting streets by population).

Exit Visa Applications. We pair the arrest data with information on exit visa applications (Hollitzer et al. 2012). The data are from the Leipzig City Archive and cover the twelve months before the fall of the Berlin Wall (November 1988 to October 1989). Researchers are not allowed direct access to the application files. Archive staff extracted, for each application, the street of the applicant and, in some districts, the month of application. The available data thus define our unit of analysis. Unless otherwise noted, it is the street. We match applications to a list of all 1,876 Leipzig streets drawn from the 1988 street directory.[11] Applications were submitted by households, so the number of individuals seeking to

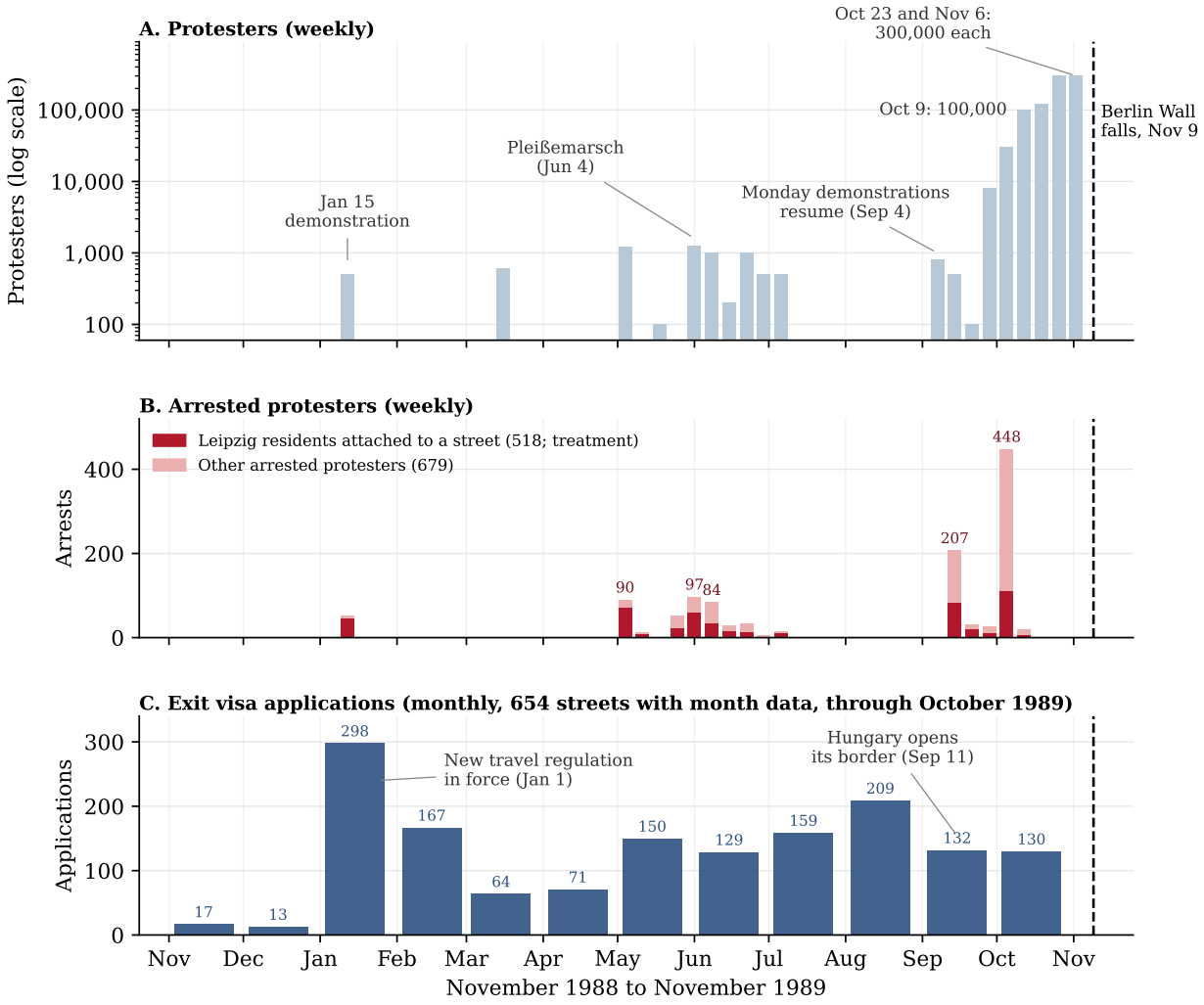
[8] Summary sheets for each arrest day also report the number of detainees with a pending exit visa application. Across all days, 27 percent of arrestees had one: 61 percent on days with Monday prayers, whose crowd was dominated by applicants, but only 10 percent at the four large protests of 15 January, 7 May, 4 June, and 10 June. These applications predate the arrest and fall in the pre-period of the event study, but they mean that streets with an arrest often house an applicant already, which contributes to their higher pre-treatment application levels (Table A5). The response does not come from the days dominated by applicants. On streets whose first arrest came at one of the four large protests, applications rose by 0.41 per month over months 0 to 3 (0.18 without January), against 0.23 on streets whose first arrest came at a Monday prayer, and residents were 4.0 percentage points more likely to have moved by 1991 (5.5 on the streets first exposed at a Monday prayer).

[9] The files record a total of 1,194 arrests. Of those detained, 514 (43%) gave a home address outside the city, and a further 162 records of Leipzig residents are repeat arrests or cannot be assigned to a street. Because treatment is defined at an arrestee's street of residence, only Leipzig residents can generate treatment. Their share falls over the year, from 92% of arrestees in January to 45% in October. Table A1 in the appendix reports the sample flow in full.

[10] Among the 205 arrestees whose employer the register records, 34 percent worked in industry, 16 percent in services, 11 percent in health care and 11 percent for a church; the industry share matches the city's (Statistisches Jahrbuch Leipzig 1991). Age brackets are from the *Stasi*'s summary sheets for 13 arrest days, covering all 765 persons detained on those days; the population share aged 18 to 24 is from the 1990 precinct data described below.

[11] Many streets were renamed during and after the transition to democracy. We used historical sources to match historical street names to their contemporary names.

Figure 1: Protests, Arrests, and Exit Visas in Leipzig 1989



Notes: Panel A shows, on a logarithmic scale, the weekly number of protesters in 1989 (Schwabe 1999). Panel B shows the number of protesters arrested each week, distinguishing the 518 arrested protesters with an identified Leipzig home address, who define treatment in our empirical analysis (Table A1), from all other arrested protesters, most of whom lived outside Leipzig. Panel C shows the number of exit visa applications filed each month on the 654 streets for which our archival sources recorded the month of application: 1,539 in total, roughly two thirds of our estimate of the citywide total over the same twelve months (about 2,380). After Hungary opened its border to East Germans on 11 September 1989, many left without applying. The dashed line marks the fall of the Berlin Wall on 9 November 1989.

emigrate was higher than the number of applications.

Whether archivists recorded the month of application differs by Leipzig districts, reflecting purely administrative decisions made 30 years after the end of the GDR. Still, our sample is likely to form a non-random subset of Leipzig as a whole. The 654 streets within our sample lie closer to the protest routes and record more applications per resident than the 1,215 streets outside the sampled districts. They carry 1,539 applications (Figure 1 and

Table A6). The sampled streets are about a third of the city's streets but held 44 percent of the residents listed in the 1996 telephone book, and they carry two thirds of the citywide total applications. Arrests, however, are balanced across the two groups, and month coverage is unrelated to treatment. Moreover, on the 1,215 streets outside our sample, an annual comparison of 1989 with 1988 also yields a positive and significant association between arrests and applications (Table A7). We thus find no sign that the districts with monthly data are unusual.[12]

Measuring Spillovers. If arrests act as local signals, as our model assumes, their effect on exit visa applications should extend beyond the arrested protester's own street to the streets nearby, where news of the arrest travels most easily. We therefore also estimate the effect on neighboring streets. Two streets are neighbors if their centroids lie within 200 meters of each other. To distinguish direct from spillover effects, we exclude streets with an arrest of their own from the spillover-treatment definition. This leaves 532 streets, 217 of them exposed to an arrest (Table A20).

Realized Emigration. To study whether the desire to leave turned into actual departures, we digitized historical telephone books—the closest analogue to individual-level named census data available—for the years closest to 1989 (1985, 1987, 1991, and 1996).[13] Having two books on each side of 1989 lets us trace mobility before and after the fall of the regime rather than in a single pre/post comparison. We link individuals across books by exact name, first at the same house, then on the same street, and finally anywhere in the city if the name is unique; approximate name matches are not counted. A person is classified as having *moved* if they are not listed at the same house in the following book, whether or not they reappear elsewhere in the city (variable `Moved` in Appendix Table A4). This is the outcome of the street- and house-level analysis in Section 5.4. It captures departure from the exposed location, which is the behavior the signal mechanism concerns, whatever the destination. A stricter outcome counts a person as having left only if not found anywhere in Leipzig in the following book. The two differ less than one might expect: the link across books requires an exact, citywide-unique name, so only 15 to 19 percent of the persons who left their house are re-identified elsewhere in the city, even in the 1996 address book that lists all residents. Appendix Table A22 reports the stricter outcome. Besides emigration,

[12] Figure A2 in the appendix maps exit visa applications and arrests for this sample; Table A3 reports descriptive statistics and Table A4 describes the variables.

[13] We follow a growing body of work that uses such city directories to locate and follow individuals in space over short time periods (e.g. Albers and Kappner 2023). For consistency we refer to all four sources as telephone books. The 1996 source is technically an address book: it lists residents irrespective of telephone ownership and contains 159,713 Leipzig entries, of which we were able to assign 139,047 entries to a street and add to our panel. This number contrasts with the 48,331, 48,782, and 56,655 georeferenced entries in 1985, 1987 and 1991.

the outcome records moves within Leipzig, deaths, changes in the listed household head and name changes at marriage. Differences between treated and untreated locations measure emigration only if arrests did not also change these other events. They occurred at the same rate on treated and untreated streets before the arrests (Section 5.4), but we cannot test whether an arrest affected them afterwards. We therefore read the estimates as excess departures from an address, most of which we interpret as emigration. Figure 2 maps residential mobility and arrests across the city.

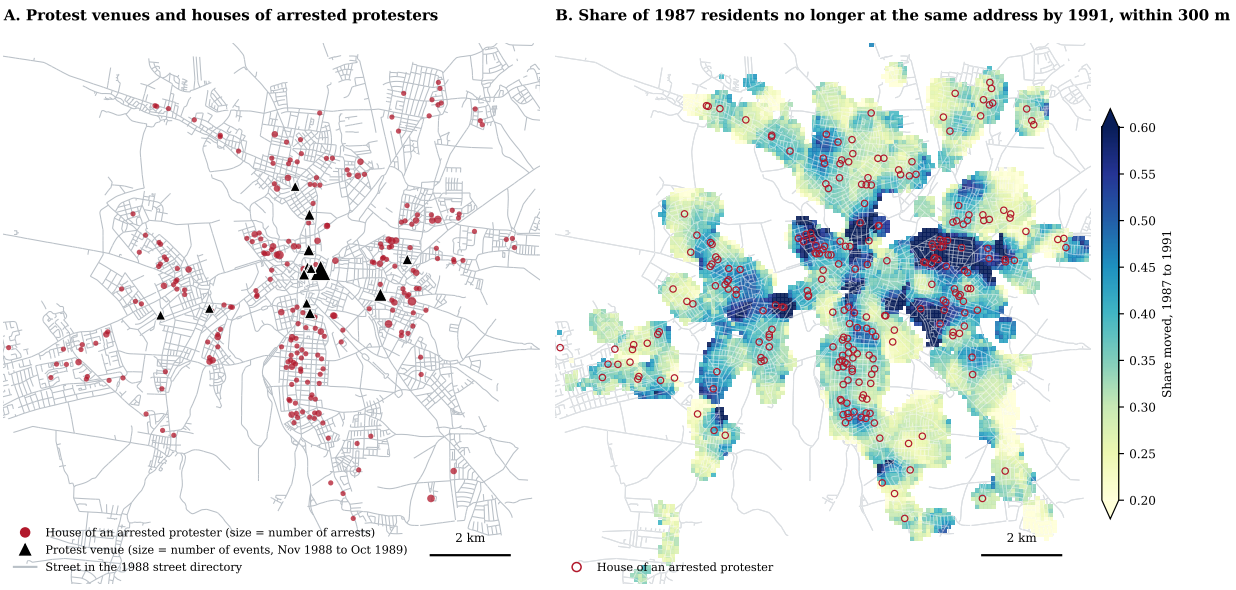
Our linked individual-level panel is limited to those households already listed in the pre-treatment books, about 48,000 in each of 1985 and 1987. At that time, only about 20% of GDR households had a telephone, raising the possibility of selection. Indeed, telephone owners were more often well-educated, employed by the state, or supportive of the regime (Bird et al. 1998). While education may raise the propensity to leave, loyalty to the regime certainly lowers it. Insofar as the listed population is more loyal than the average resident, then, our estimates are likely to understate the response of the city as a whole. Men are also overrepresented, with only about 30% of entries belonging to women. This is because (male) household heads were usually listed on behalf of the household. Moreover, we acknowledge that women who changed their surname at marriage cannot be followed. First names also suggest an approximate birth year, which we use to check that the age composition of the residents of treated streets and houses does not drive the mobility results (Appendix Table A23).

Note that we are unable to observe why people left an address. Those no longer found in the city include politically motivated emigrants, economic migrants, and people joining family in the West. We still consider the measure informative. In a planned economy, economic grievances were grievances against the state, and supply shortages, decaying housing, and the growing gap to the West featured almost as prominently at the Monday Demonstrations as calls for political liberalization (Lohmann 1994; Opp and Gern 1993).

Protest Activity. Two sources describe protest in Leipzig. The first is the chronicle of events put together by Schwabe (1999), compiled from the daily situation reports of the police and the Ministry of State Security (Crabtree et al. 2015). It gives for every week of 1989 the number of protesters and of arrested protesters at Leipzig protests.[14] We use it to describe the protest wave (Figure 1) and, in Section 5.2, as a control for city-wide protest intensity. The second is the chronicle of events assembled by the *Citizens Movement Archive*, which records the date and venue of the 56 oppositional activities in the city between November 1988 and October 1989.[15] Most took place at a handful of central venues, including ten churches,

[14]The data were made available by Archiv Bürgerbewegung Leipzig e.V. (2020).
[15]https://www.archiv-buergerbewegung.de/themen-sammlung/zeittafel#1989

Figure 2: Arrests and Migration



Notes: The figure shows the location of protester arrests and departures from listed addresses within the city of Leipzig. Panel A shows the 15 protest venues summarized in the *Citizens Movement Archive* chronicle, November 1988 to October 1989 (triangles, sized by events) and the 251 houses of arrested protesters (dots, sized by arrests). Panel B shows, on a 100 x 100 m grid, the share of 1987 telephone-book residents within 300 m who were no longer listed at the same house in 1991 (17,770 houses, 45,002 residents, city-wide rate 36 percent), excluding residents whose name matches an arrested protester. Cells with fewer than 25 residents are blank. The red rings represent houses with an arrestee.

the market square, and the inner city. The most prominent—the Nikolai Church—alone accounted for 25 of them (Appendix Figure A3). We geocoded each venue and constructed, for every street of the city and every week, an indicator for whether an activity took place within 1 km or 2 km of the street. This is our measure of protest activity in Section 5.5: it is compiled from the opposition's own records and is therefore independent of the police records from which our treatment is constructed.

Support for the Opposition Movement. Our second measure of dissent is support for the *Neues Forum*, the first countrywide opposition organization of the GDR, founded on 9 and 10 September 1989 and legalized on 8 November. We rely on sign-up sheets on which Leipzig citizens could declare their support for the movement. These sheets record each signatory's name and address. We digitized all 5,513 entries (which pertain to 4,870 unique signatories) held by the *Archiv Bürgerbewegung Leipzig*. The sheets are usually not dated; only 493 entries carry a date, most of them from October and November 1989. As such, the sheets record support declared between September 1989 and early 1990, largely in the weeks in which the regime collapsed. We use the data in two ways. At the street and house level,

we count signatories per 100 residents[16] within 100 m of a street, and whether a house holds a signatory, as measures of opposition support around streets with arrests (Section 5.5). Signatories who themselves appear on the arrest list are excluded. At the individual level, we use the roughly 2,800 signatories whose address could be located as a comparison group for the arrested protesters in Section 5.4, which examines emigration among protesters using telephone-book data: like them, they had declared opposition in 1989, but unlike them, they were not arrested.

Street Characteristics. The balance tests and cross-sectional regressions below use six street characteristics: distance to the route of the Monday demonstrations, travel time by foot and tram to the city center at the Nikolai Church, street length, churches within 600 meters, the share of the area within 100 meters destroyed in 1945, taken from the city's damage map in Enss and Knauer, eds (2023), and the number of conspiratorial *Stasi* flats within 100 meters, taken from the Bürgerkomitee Leipzig's map of the 1,062 objects known in 1989. Population within 100 meters is estimated as described above. Because this estimate dates from December 1990, it is measured after the arrests. Arrest-induced departures can lower it on treated streets by at most about four percent—the excess departures we estimate for 1987–91 (Section 5.4)—while treated streets have two and a half times as many residents within 100 meters as never-treated streets. Replacing it with the number of residents listed in the 1985 telephone book, which predates every arrest, leaves the scale-adjusted estimates of Appendix Table A10 unchanged in substance.

5 Empirical Strategy and Results

5.1 Event-Study Analysis

Empirical strategy. We estimate the effect of protester arrests on exit visa applications using the staggered difference-in-differences estimator developed by Callaway and Sant'Anna (2021). Following the model (section 3), we interpret the first arrest of a street resident as the onset of locally salient exposure to repression and define treatment accordingly. The Callaway-Sant'Anna (CS) approach is well suited for our setting because the timing of treat-

[16] No street-level population count exists for Leipzig around 1989, and the telephone books cannot serve as one because telephone ownership was spatially selective (on streets with an arrest the 1987 book lists 0.29 persons per resident of the 1996 address book, on other streets 0.36). We therefore estimate street populations from the residents recorded for the 376 precincts of the federal election of 2 December 1990, distributed to a 100-meter buffer around each street by areal overlap and scaled to the city's population of 515,911. Precinct boundaries are themselves estimated, so we average over 100 plausible boundary maps. Across streets, the estimate correlates at 0.72 with the resident count of the 1996 address book, against 0.53 for the 1987 telephone book.

ment—the first arrest of a resident on a given street—varies across streets. In such situations, conventional two-way fixed effects (TWFE) event-study regressions may yield biased estimates when treatment effects are heterogeneous across cohorts or over time (de Chaisemartin and D'Haultfœuille 2023). The behavioral response to repression is plausibly heterogeneous across streets, for example depending on housing quality, proximity to protest sites, or local surveillance intensity. The CS estimator explicitly accommodates this heterogeneity by estimating cohort-specific average treatment effects on the treated.[17] We use the regression-adjustment version of the estimator in Callaway and Sant'Anna (2021) with month fixed effects, take never-treated streets as the comparison group, and cluster standard errors by street.

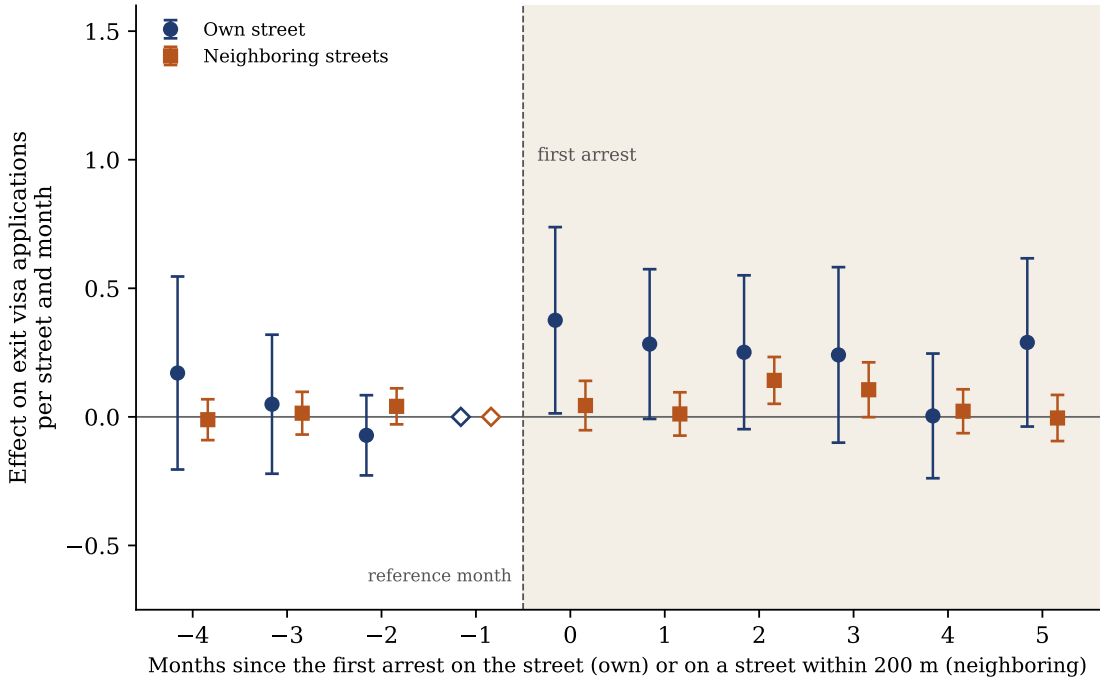
Results. Figure 3 shows the event-study estimates. The relevant coefficients of the effect on the street where the arrestee lived (the own-street effect) are small and jointly insignificant prior to treatment onset ($\chi^2(3) = 2.37$, $p = 0.50$). After an arrest, applications on the treated street increase by 0.24 per month over months 0 to 5 (s.e. 0.12), against a pre-arrest level of 0.42.[18] Alternative control groups give estimates between 0.17 and 0.30 (Appendix Table A8). About 1.4 additional applications over six months are within reach of the 1.7 arrestees on the average treated street, so this estimate alone cannot show whether the response came from the arrested households or from their neighbors. Sections 5.3 and 5.4 address this.

Three features of the data qualify this estimate. First, it is largest for the 18 streets whose first arrest came on 15 January 1989, with 0.73 (s.e. 0.34) over months 0 to 5 against 0.13 (0.12) for the later cohorts (Appendix Table A8). In that month applications rose city-wide from 13 to 298 after the new travel regulation, so we cannot separate a larger response to the first arrests from a common surge. Second, the estimate is positive in all specifications in levels, but smaller under the inverse hyperbolic sine and not detectable on the extensive margin or in a Poisson model (Appendix Tables A9 and A8). Third, treated streets are larger, so a city-wide rise that is proportional to size raises their counts more even without an arrest. When we allow untreated trends to depend on 1988 applications, population and street length (Sant'Anna and Zhao 2020), the estimate over months 0 to 3 is 0.21 (s.e.

[17] Let G_s denote the month of the first protester arrest on street s, with $G_s = \infty$ for streets that never experience one. The CS estimator identifies, for each treatment cohort g and month t, the average effect of exposure on exit visa applications, $ATT(g,t)$, by comparing the change in applications on cohort-g streets with that on never-treated streets under a conditional parallel trends assumption. We aggregate these cohort–time effects by event time $k = t - g$, weighting cohorts by their size, and report the resulting event-study coefficients β_k normalized to the month before the first arrest ($k = -1$).

[18] Because applications are dated only by month, the month of the arrest also contains applications filed before the arrest day. Excluding it, the estimate is 0.26 (s.e. 0.12) over months 1 to 3 and 0.21 (0.11) over months 1 to 5.

Figure 3: Exit Visa Applications after an Arrest



Notes: Event-time coefficients with 95 percent confidence intervals from two Callaway and Sant'Anna (2021) event studies, with standard errors clustered by street. Circles show the 122 streets on which a resident was arrested, treated from the month of the first arrest, with the 532 streets without an arrest as the comparison group. Their average over months 0 to 5 is 0.24 (s.e. 0.12), and a joint test of the three pre-treatment coefficients does not reject that they are zero ($\chi^2(3) = 2.37$, $p = 0.50$). Squares show the design for neighboring streets of Section 5.3: the 532 streets without an arrest of a resident, 217 of them treated from the month of the first arrest on a street within 200 meters, with the remaining 315 as the comparison group (Appendix Table A20). The reference month is the month before treatment (hollow diamonds). Applications are dated by month, so month 0 also contains applications filed before the day of the arrest. The window ends at month 5 because months 6 to 9 are identified from the January 1989 cohort alone. Appendix Figure A4 extends the own-street estimates to month 9. Because later cohorts are observed for fewer months, the number of treated streets contributing to each coefficient falls from 122 in month 0 to 51 in month 5 on the own street, and from 217 to 93 on neighboring streets.

0.13), about three quarters of the baseline (Appendix Table A10). We therefore treat the own-street estimate as supporting evidence and rest the exit result on realized migration (Section 5.4), which is measured as a share of residents and does not depend on the size of a street.

5.2 Identification

The estimates in this paper admit a causal reading only if streets that experienced an arrest would, in its absence, have followed the same path as streets that did not. This assumption underlies the own-street estimate, the response of neighboring streets in Section 5.3, and realized migration in Section 5.4.

Assignment of arrests. All 518 arrests in our data occurred during and at the site of protests. None took place at protesters' homes (Section 4). *Stasi* situation reports describe

them as bulk detentions of whomever remained in an area after the police ordered it to be cleared, not as the outcome of pre-identified or strategic targeting. During the banned Pleißemarsch on 4 June 1989, for example, 74 individuals were detained after refusing to clear the square in front of the SED district headquarters (Ministry of State Security 1989d). Arrests were also routinely used to break up gatherings following Monday prayers. On 12 June 1989, for example, about 650 people attended the prayer, and 27 were detained when a subsequent gathering of 50 to 60 was dispersed (Ministry of State Security 1989a). Those who lingered after a dispersal order may have been more committed than the average participant. Our identification, however, does not require arrestees to be a random draw from the crowd. It requires that the timing of the first arrest on a street is unrelated to that street's counterfactual trend in exit visa applications. We return to the comparability of arrestees and other committed dissidents in Section 5.4.

Comparability of streets. Treated and control streets differ in several pre-treatment characteristics (Table A5). Streets that ever experienced an arrest were, on average, longer, more densely populated and farther from the demonstration route, suffered less wartime destruction, and had a higher density of *Stasi* conspiratorial flats.[19] They also recorded more exit visa applications before their first arrest.[20] These are differences in levels, while the identifying assumption concerns trends. Table A11 nevertheless re-estimates the baseline on comparison groups that are closer to the treated streets. First, we drop never-treated streets with a very low estimated propensity to have an arrest. Second, we match each treated street to its three nearest neighbors on the propensity score. Third, we match on the propensity score together with pre-arrest applications.[21] The first two leave the estimate essentially unchanged. The third reduces it to about three quarters of the baseline.

Covert state attention before 1989. A second measure shows the same pattern over a longer period. The *Stasi* conspiratorial flats carry the date on which each one was set up, which gives a street-level record of covert state attention for the decade before the arrests. Between 1980 and 1988, streets whose residents were arrested in 1989 received two to four times as many new flats as streets without an arrest. This ratio does not trend: in a Poisson model, the interaction of later arrests with a linear trend is small and insignificant

[19] Distance is measured to the route of the Monday demonstrations. Measured to the churches and squares at which protests were held, treated streets lie closer than never-treated streets (Section 5.5); the two measures describe different geographies, the ring road and the venues inside it.

[20] This gap is proportional rather than a trend. In every month from December 1988 to September 1989, treated streets recorded four to six times as many applications as never-treated streets before their first arrest, with no trend in the ratio. In November and December 1988, applications were rare in both groups (between 94 and 99 percent of street-months record none), so the level gap was near zero. It appears in January 1989 because applications rose city-wide that month, on treated and never-treated streets alike.

[21] Between November 1988 and the month before the first arrest, matched controls average 0.36 applications per street and month against 0.40 on treated streets.

(Appendix Figure A5). Because new flats were set up at a rising rate everywhere, a constant ratio implies a widening gap in levels, and a linear model with street fixed effects finds one (0.009 more flats per street and year, s.e. 0.002). This is the same proportional pattern as in exit visa applications, and one reason we report estimates that allow untreated trends to depend on scale (Appendix Table A10).

Common local shocks. Growing political or economic grievances at the end of the regime could have raised both protest participation, and hence arrests, and the demand for emigration on the same streets. Four pieces of evidence speak against such a common local shock. First, the leads in Figure 3 are flat. Second, almost two thirds of treated streets received their first arrest in January, May or June—months before dissent became mass protest (Table A2). Third, the estimate is unchanged when we control for protest activity within 1 km of the street in the same month (Table A12, column 2). Fourth, local protest activity does not predict the month of a street's first arrest, conditional on street characteristics and month fixed effects (Table A13).

Inference and robustness. Appendix A reports further checks of the own-street estimate. The estimators of Sun and Abraham (2021), Borusyak et al. (2023) and Wooldridge (2021) and a two-way fixed effects regression give estimates between 0.19 and 0.32 (Appendix Figures A6 and A7). Comparing treated streets only with streets experiencing arrests later gives a similar estimate over months 0 to 5 (0.21, s.e. 0.16), which is not significant on its own (Appendix Table A14). Many never-treated streets border a street with an arrest, so the comparison group is partly exposed. Excluding these streets, together with streets with an arrest whose neighbors also had one, raises the estimate over months 0 to 3 from 0.29 to 0.37, so the mixture of exposures biases it toward zero (Appendix Table A15, Panel A). For the average over months 0 to 5, Conley standard errors that allow for spatial correlation among streets within one to three kilometers in the same month give p-values between 0.04 and 0.06. When the correlation may extend across all months, or under the SCPC inference of Müller and Watson (2022), the estimate is not significant at conventional levels at the wider settings (Appendix Table A16). A design-based test holds the treated streets fixed and reassigns only the month of the first arrest among them. Unlike clustered or spatial standard errors, it accounts for shocks common to the streets hit by the same police operation. Our estimate exceeds 86 percent of the reassigned estimates ($p = 0.14$, Table A12).

Mechanism. Three tests support the interpretation that residents responded to arrests they could observe near their home. First, the response is tied to where the arrestee lived. Residents of the streets on which arrestees worked did not submit more exit visa applications: for the 17 workplaces of arrestees that lie on streets of the estimation sample without an arrest of a resident, the estimate is insignificant (Appendix Table A17). Because applica-

tions are recorded at the applicant's home, the test concerns residents near a workplace, not coworkers. It does not rule out diffusion through workplace networks. Second, the opening of *Stasi* conspiratorial flats, which residents could not observe, produces no response (Appendix Table A18). Finally, the response does not scale with the severity of the punishment. Punishments ranged from none on 15 January to a fine for every detainee on 29 May, but within the same month severity does not predict a larger response (permutation $p = 0.74$; Appendix Table A19).

5.3 Diffusion Effects

Our model predicts that the response to an arrest need not be confined to the arrestee's own street: if information about repression diffuses locally, its effect should extend to nearby streets and attenuate with distance. There are good reasons to expect news of arrests to spread. Arrests occurred publicly, in central locations during mass demonstrations, and were witnessed by large crowds (Ministry of State Security 1989c,d). Detainees were held overnight—and sometimes much longer—in pretrial detention, which produced sudden absences from home, school and work. State authorities routinely contacted the families, and the *Stasi* conducted house searches lasting several hours, which neighbors could hardly miss (Hollitzer et al. 2012). Leipzig's churches also collected information on detainees and prayed for them by name (*Fürbitten für Inhaftierte*).

To examine diffusion, we re-estimate the event study on the 532 streets without any arrest. Treatment is the month in which a resident of a neighboring street (within 200 meters) was first arrested (Section 4). 217 streets are exposed in this sense, in six monthly cohorts. The remaining 315 serve as the comparison group. Because no resident of these streets was arrested, the design excludes two explanations specific to the arrestee's street: the applications of the arrested households themselves and reactions to a search or detention on the street. It does not rule out shocks common to a neighborhood, such as rising dissent or more intensive policing in an area, which would reach the arrestee's street and its neighbors alike. Against those, the evidence is the flat pre-period, the timing test below, and the arguments of Section 5.2.

Figure 3 places the two event studies on one scale, and Appendix Table A20 reports the averages. On both sets of streets the pre-period is flat. On the streets with an arrest, applications rise from the month of the arrest. On the neighboring streets, they rise two months later and return to zero from month 4. Over the first four months after a nearby arrest, applications rise by 0.08 per street-month (s.e. 0.04). With standard errors that allow for spatial correlation, the estimate is significant at the 10 percent level. When we randomly

reassign the month of exposure among the exposed streets, our estimate exceeds 93 percent of the reassigned estimates. When we instead reassign the month of each arrest that exposed them, so that all streets exposed to the same arrest move together, it exceeds 88 percent of them (Appendix Table A20). Thus, the response of neighboring streets is significant at the 10 percent level in two of these three tests. The response of neighboring streets does not depend on repeated exposure. It is unchanged when we drop streets that are later exposed to a further arrest nearby, and streets next to several streets with arrests do not respond more than streets next to one (Appendix Table A15, Panel B).

In levels, applications on neighboring streets thus rose after a nearby arrest, at a quarter to a third of the own-street effect and about two months later. This temporal delay suggests that news of an arrest took time to reach households that did not witness it. The delay also appears among the 133 streets exposed between January and July, which the panel observes for at least three months. Because the response shrinks once untreated trends may depend on applications before the arrests and on street size (Appendix Table A10) and could also reflect a shock common to the neighborhood, we read it as suggestive evidence of diffusion, in line with accounts of belief updating in autocracies (Kuran 1991) and with evidence that political shocks travel through proximity (Golub and Sadler 2017; Bursztyn et al. 2020).

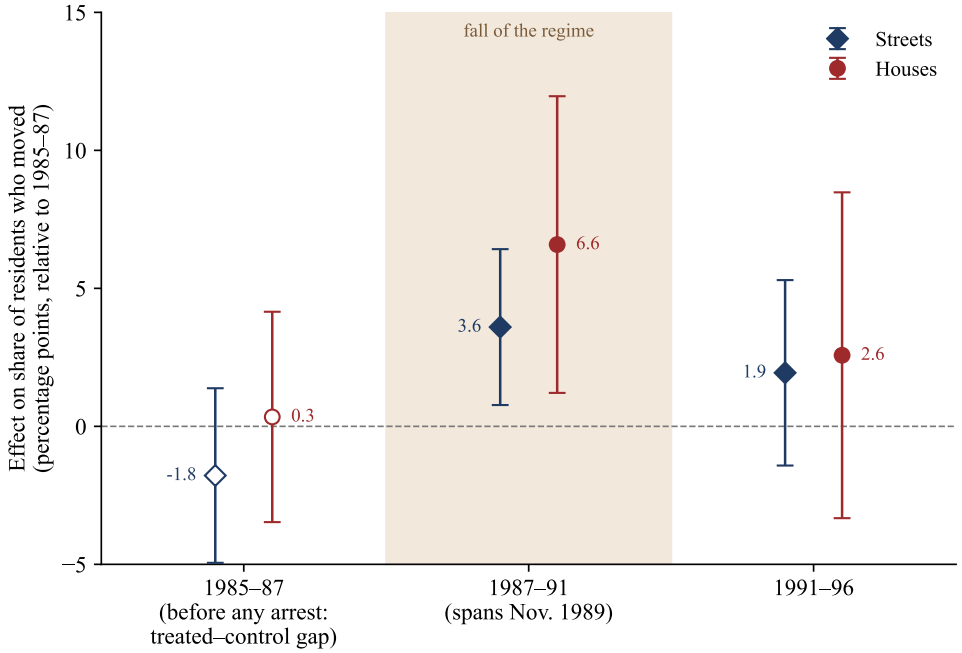
5.4 Realized Emigration

The event study measures the response to an arrest within months and focuses on exit visa applications only. We now expand the horizon beyond the short-term responses, asking whether residents exposed to an arrest actually left. To answer this question, we use an independent data source—the telephone-book panel detailed in Section 4—and a treatment definition that does not depend on the month of an arrest. Our pre-period begins four years before the first arrest.

Departures after November 1989 are the appropriate test of an exit response that formed under the regime. Until then, an application was the only legal way to leave, and few applications were granted. Once the border opened, the same wish could be acted upon directly, and about 1.5 million East Germans went west between 1989 and 1991 (Berlin Institut für Bevölkerung und Entwicklung 2026). We therefore expect the heightened demand for exit caused by repression in 1989 to materialize in heightened rates of departures in the following years. Our test compares exposed with unexposed locations. Residents often had the economic reasons to leave but differed in their exposure to protester arrests.

Aggregate analysis. The first analysis aggregates the panel to streets and to houses. For street or house s and telephone-book interval t, the outcome y_{st} is the share of residents

Figure 4: Realized Migration



Notes: Difference-in-differences estimates of the effect of a protester arrest in 1989 on the share of a street's (house's) listed residents who are not found at the same address in the next telephone book, in percentage points, with 95% confidence intervals. Standard errors are clustered by street (house). Clustering the house-level estimates by street instead leaves the standard errors unchanged (0.026 for 1987–91). The intervals are 1985–87 (reference), 1987–91, and 1991–96. The shaded interval contains the fall of the regime. The hollow markers at 1985–87 are the treated–control difference within that interval, before any arrest, estimated without unit fixed effects. Residents whose name matches an arrest record are excluded. Appendix Table A21 reports the estimates with observation counts.

listed at the start of the interval who are not found at the same house at its end (`Share_Moved` in Appendix Table A4); the same person-level definition underlies both levels, which differ only in the unit over which the share is computed. The four books (1985, 1987, 1991, and 1996) define three intervals, 1985–87, 1987–91, and 1991–96, and we estimate

$$y_{st} = \alpha_s + \lambda_t + \beta_1 \left(\text{Treat}_s \times \text{Post87-91}_t\right) + \beta_2 \left(\text{Treat}_s \times \text{Post91-96}_t\right) + \varepsilon_{st},$$

where Treat_s indicates a street (house) with an arrest of a resident in 1989, the 1985–87 interval is the omitted reference, and α_s and λ_t are unit- and interval-fixed effects. β_1 and β_2 trace the effect across the collapse of the GDR instead of collapsing the pre- and post-1989 books into a single comparison.

Figure 4 presents the results. Before any arrest (1985–87), mobility on treated streets and in treated houses was the same as elsewhere. In 1987–91, the interval that spans the

fall of the regime, residents of treated streets were 3.6 percentage points more likely to have moved than residents of other streets. Residents of treated houses, in turn, were 6.6 points more likely to do so than residents of other houses. The excess departures are largest in 1987–91. The estimates for 1991–96 are smaller, but not significantly so. Excluding the 50 most common surnames, which could confound the matching, changes little (Appendix Table A21, columns 3 and 6).

To gauge the size of the effect, consider the 15,720 residents listed in 1987 on the 179 streets with an arrest. Between 1987 and 1991, 3.6 percentage points more of them left their address than on other streets, or about 570 people. This is about one in ten of all departures from these streets, and about 1.7 additional departures for each of the 340 arrests on them. Because the telephone books list only about one in five households, the number among all residents is likely larger.

Identification. We support a causal interpretation of these estimates in multiple ways. First, residents of treated streets and houses were not already leaving at a higher rate before the arrests. With a single pre-arrest interval, this is a test of balance rather than of parallel trends.[22] Residents of streets and houses with an arrest were, however, about two to three years younger than others, which we estimate using name frequencies by birth year, and the young moved more after 1989 everywhere. Controlling for each unit's share of residents born after 1950, interacted with the intervals, leaves the street estimate unchanged and reduces the house estimate from 6.6 to 5.4 percentage points—still significant at the 5 percent level (Appendix Table A23). Second, treatment is exposure to any arrest in 1989, so the estimate does not depend on the month in which a street was first treated or on the weight of any one cohort. Third, the house-level estimate compares houses with an arrested resident to other houses, with house fixed effects, and is nearly twice the street-level estimate. A shock common to a neighborhood would not produce that gradient, whereas a signal that is more visible to those who share a stairwell would. Fourth, the arrested are removed by name: every individual whose full name matches an arrest record is dropped before the shares are computed, so the aggregate effect is by construction a response of neighbors, and the arrested are compared separately below with other committed opponents of the regime.

[22] Between 1985 and 1987, about 20% of entries cannot be re-identified in the next edition by an exact first-and-last-name match, reflecting new account holders, name changes at marriage, deaths, and changes in coverage. This baseline attrition is common to treated and control streets, as the equal shares in 1985–87 show. It therefore differences out and adds noise rather than bias, provided it remains common after 1987. A change in coverage between the 1987 and 1991 books that differed across streets would not. The within-street comparison of houses (Appendix Table A24) absorbs any such change that is common to a street. The pre-arrest difference is equally small under the city-wide outcome: among the residents listed in 1985, the share no longer found anywhere in Leipzig in 1987 differs by 0.7 percentage points (s.e. 1.0) between streets with a later arrest and other streets, and by −1.4 points (1.5) between houses with a later arrest and other houses.

Some limitations remain. The intervals cannot situate the response within the 1987–91 period, so that we are unable to link it directly to particular events. Some residents may have left before the arrest on their street, for instance because applications pending on streets with an arrest were granted in 1988 or 1989. Comparing a house with an arrest only with other houses on the same street in the same interval removes this and every other street-level factor. The house-level estimate falls from 6.6 to 4.8 percentage points (s.e. 2.9), so street-level factors account for at most about a quarter of it (Appendix Table A24). The telephone books also do not identify applicants, so we cannot follow an individual from an application to a departure. Counting a resident as having left only if not found anywhere in Leipzig, the house-level estimate is unchanged and the street-level estimate is two thirds of the baseline (Appendix Table A22), although some residents who moved within Leipzig remain unmatched because re-identification requires a unique name.[23]

Non-residential entries. A remaining concern is that arrests occurred on streets that were already emptying, so that the departure of residents was one symptom of a decline that repression did not cause. We account for the possibility with a falsification test that considers the "effect" of arrests on the schools, polyclinics, institutes, shops, and enterprises that were listed alongside households in the telephone books. Any decline in the number of these premises would be indicative of changing economic fortunes on the street rather than the direct consequence of protester arrests. Appendix Table A25 regresses the change in the log count of these premises on arrest exposure, with the same controls and zip-code fixed effects used elsewhere. The coefficient is insignificant before the arrests (1985–87) and remains so during the reunification period (1987–96). Thus, the number of premises listed on streets with arrests did not fall relative to other streets. Because premises cannot be tracked individually and the window runs to 1996, this result speaks against a broad decline of these streets, but not against the loss of particular businesses between 1987 and 1991.

Individual Analysis. A complementary analysis asks whether the arrested protesters themselves left. We compare them with the roughly 2,800 signatories of the *Neues Forum*. These individuals likely shared the protesters' grievances, as evidenced by the fact that they signed up for what was then an illegal opposition movement in the fall of 1989 and likely also marched with them in the demonstrations. But, critically, they were not arrested.[24] Because only a fifth of households had a telephone, most members of both groups never appear in

[23] A further concern is that residents of treated streets chose to be delisted after an arrest out of wariness toward state-adjacent institutions. This is unlikely: telephone listing was compulsory for connection holders, removal from the directory was permitted only in narrowly defined cases, and a routine right to having one's number not listed was not established until the Telekommunikations-Kundenschutzverordnung of 1998 (https://dejure.org/BGBl/1997/BGBl._I_S._2910).

[24] We exclude individuals who were both arrested and appear on the *Neues Forum* list.

a telephone book, so we restrict the comparison to those who can be traced in the 1987 book by name and address—34 arrestees and 343 signatories—and ask whether the person is still found anywhere in Leipzig in the 1991 book. Figure A8 shows the result; Appendix Table A26 reports it with standard errors. Half of the traced arrestees were no longer found anywhere in Leipzig in 1991, against one in five signatories. This comparison is descriptive. Signatories had to be in Leipzig in the fall of 1989 to sign, whereas protesters arrested earlier could have left before. Among the nine protesters arrested in September and October, the gap is 0.33 against 0.20 and not significant (Appendix Table A26, Panel C), so we do not read it as the effect of an arrest. The gap does not come from local moves that we observe. Every arrestee who was no longer at their 1987 address was also not found elsewhere in the city, whereas a fifth of the signatories who had moved were still listed elsewhere in the city. The 1985 telephone book allows for a placebo test. Among persons listed in 1985, arrested protesters and signatories had moved between 1985 and 1987 at the same rate, 10 percent each, although with 29 arrestees traced the interval on their side is wide.

5.5 Repression and Voice

Did the rise in exit come at the expense of voice? We approach the question with two measures of dissent. The first is a calendar of protest activities, which gives the date and venue and, for about half of all events, a crowd estimate. The second is the *Neues Forum* sign-up sheets, which record supporters by address.

Protest Activity. We first ask whether arrests had the intended effect and reduced protest activity, adopting the event-study design of Section 5.1. We use the protest calendar compiled by the *Citizens Movement Archive*, which records the date and place of all 56 significant oppositional activities between November 1988 and October 1989. For each of the city's 1,838 streets—not only the 654 with monthly application data—and each *week*[25] we construct an indicator for whether any of these activities took place within 1 km or 2 km of the street with an arrest. Because arrests occurred at the protests themselves, and the arrestees tended to live closer to the protest site than the average Leipzig resident, we match the comparison group of each cohort on distance to a particular week's protest site.[26] Figure 5 shows no decline in protest activity around treated streets at either radius. The pre-treatment coefficients are flat, and the averages over the four weeks following the arrest are insignificant (Appendix Table A27). Allowing for spatial correlation between streets

[25] Protests followed the rhythm of the Monday prayers, so an arrest and the next protest fall in adjacent weeks, and a monthly panel would place most of them in the same period.

[26] The median treated street lies 1.3 km from the nearest site, the median never-treated street 2.2 km. After matching, treated and comparison streets differ by at most two percentage points in the share lying within 1 km and within 2 km of that venue, in each of the fourteen weekly cohorts.

near the same venue widens the standard errors by up to half. Within 2 km, the confidence interval still excludes a decline of more than a quarter of the pre-arrest level. Within 1 km, it is too wide to exclude a large decline. Replacing the indicator by the inverse hyperbolic sine of the reported *turnout* at the events within each radius does not change the results (Panel C of the same table).[27]

What this test can detect is limited by where protests took place. 206 of the 308 streets with an arrest have a protest site within 2 km (Appendix Figure A3). An arrest on a residential street could therefore lower the measure only if the opposition abandoned or relocated a protest scheduled near that street, which is the form a visible local demobilization would have taken. The test can thus rule out a large visible decline in protest within 2 km of a street, but not moderate or individual-level effects. The arrest records themselves add one direct observation on the residents of streets with an arrest: on 103 of the 308 streets with an arrest, a resident was arrested again on a later day, on 41 of them twice. This is not a counterfactual, since streets with protesters produce arrests for that reason alone, but it shows that protest by residents of these streets continued after the first arrest.

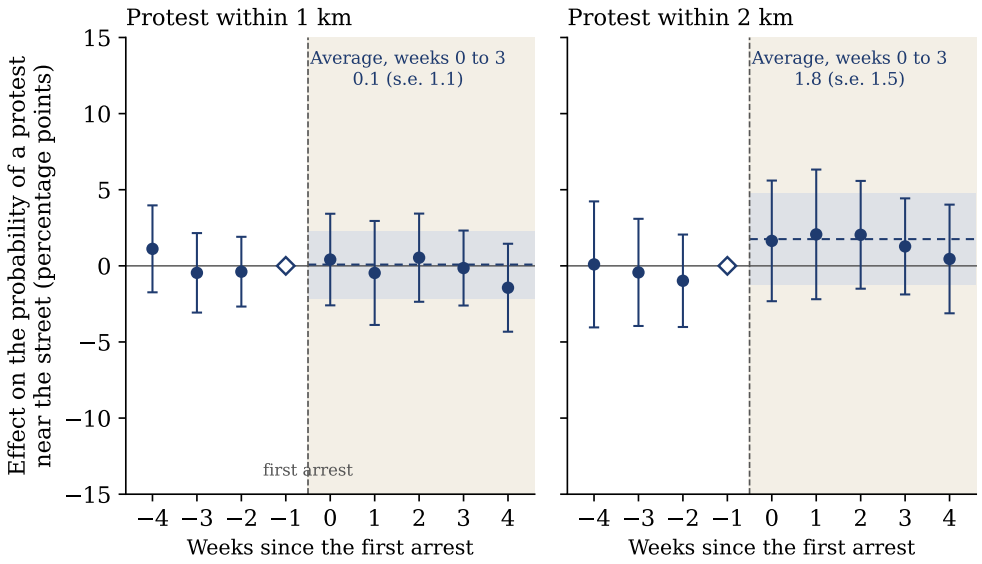
Support for the Opposition Movement. Our second measure of voice does not have this limitation. It relies on the sign-up sheets of the *Neues Forum*, which record the address of each signatory (Section 4). For most treated streets, the first arrest came before the *Neues Forum* existed, so the level of support cannot separate where the arrests took place from how residents responded to them. Across the city, each additional arrested resident of a street is associated with 0.34 more signatories per 100 residents in its vicinity, compared to a mean of 1.05. Within streets, the house of an arrested protester is 6.5 percentage points more likely to hold a signatory, compared to a mean of 9.0 (Appendix Table A28).[28] Streets and houses with arrests thus had high support for the opposition at the end of 1989.

The relevant question for voice is whether residents stopped signing up after an arrest. The sheets record when support was declared, not whether it was later withdrawn, so this is a test of new declarations. About 10% of the unique signatories carry a date, which allows a single comparison before and after 7 October 1989 for the 49 streets whose first arrest occurred on 7 October or later. Before these arrests, the streets were already 6.1 percentage points more likely to have a signatory than streets without any arrest, conditional on the resident count and zip-code fixed effects. After 7 October, the gap is 8.6 percentage points. The change from before to after is small and imprecisely estimated. A broader early set,

[27] The chronicle reports a crowd estimate for 29 of the 56 located events, nearly all of them Monday prayers at the Nikolai Church; an event without an estimate enters as a quiet week, and imputing the median crowd outside October, 500 participants, leaves the estimates unchanged.

[28] Signatories who appear on the arrest list are excluded throughout. At the house level, they would make an address with an arrest positive by construction.

Figure 5: Local Protest Activity around a Street's First Arrest



Notes: Event-time coefficients of the Callaway and Sant'Anna (2021) estimator for weeks −4 to +4 around a street's first arrest, in percentage points, with the week before the first arrest as the reference (hollow diamond), on the weekly panel of all 1,838 streets. The dependent variable is an indicator for whether a protest activity recorded in the *Citizens Movement Archive* calendar took place within 1 km or 2 km of the street in that week. The comparison group consists of never-treated streets matched on their distance to the venue at which the cohort's arrests took place.

which adds sign-up sheets whose dated entries fall almost entirely before October, gives a change of the opposite sign. Neither set thus detects a decline in new sign-ups after an arrest, although with a tenth of signatories dated the comparison has little power (Appendix Table A29).

Taken together, neither measure shows the local decline in dissent that would arise if deterrence dominated the grievance effect. This is consistent with the ambiguous prediction for protest in Section 3. Protest activity near streets with arrests did not visibly decline. Support for the opposition was higher on streets with arrests, and on the streets with a first arrest in October residents kept signing up afterwards. These results are consistent with the account of Hirschman (1993), according to which exit and voice reinforced each other in the fall of 1989.

6 Conclusion

This paper examines how targeted repression shapes political behavior in an autocratic regime. Using newly assembled micro-data from the East German city of Leipzig during the 1989 Peaceful Revolution, we show that residents of streets and houses with an arrested

protester were more likely to have left their address once the border opened. Exit visa applications on these streets and on neighboring streets also rose after an arrest, which is consistent with arrests sending visible signals of repression beyond the arrestees.

Arrests did not silence voice. Protest near streets housing arrested individuals did not visibly decline, measured against an independent record of where protests took place. Support for the opposition movement was high around arrestees' addresses at the end of 1989, and on the streets with a first arrest in October residents kept signing up after the arrest. Both measures are imprecise, and neither observes individual participation. They rule out a large decline in protest near streets with an arrest, but not smaller declines or effects on individual residents. On the evidence we have, coercion raised the demand to leave and, once the border opened, departures from the affected streets, without suppressing the forms of dissent we can observe. Taken together, the evidence is consistent with belief updating that deepened grievances against the regime sufficiently to outweigh the deterrent effect of repression on the exit margin, while we find no evidence that deterrence dominated on the voice margin.

Our data suggest that localized repression can push citizens toward exit without a visible retreat of voice, and that it does so among neighbors who were never arrested themselves.

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Appendix (for online publication)

A Additional Tables

Table A1: Arrest Sample Flow

Panel A. From archival record to estimation sample	
Arrest records transcribed from BStU files	1,194
less: resident outside Leipzig	-514
Arrests of Leipzig residents	680
with a recorded name	672
unique individuals	457
less: also on the *Neues Forum* sign-up list	-27
Individual-level treatment group	**430**
Arrests in the street-level analysis file	518
Leipzig streets with at least one arrest	308
of which in the 654-street estimation sample	122
Arrests carried by those streets, Nov 1988–Oct 1989	**202**

Panel B. Residence of the arrested, by month			
	Arrests	Leipzig	% non-Leipzig
Jan 1989	52	48	8
May 1989	168	117	30
Jun 1989	233	122	48
Jul 1989	14	11	21
Sep 1989	263	174	34
Oct 1989	464	207	55

Notes: Panel A traces the arrest records from the archival transcription to the two estimation samples; Panel B reports the residence of the arrested by month. Treatment in the event study is an arrest of a resident of street s, so arrestees living outside Leipzig cannot generate treatment on any Leipzig street and are therefore dropped from the sample. One record has no city recorded and is retained among the Leipzig residents. Unique individuals are distinct names in the arrest file; the 27 who also appear on the *Neues Forum* sign-up sheets are excluded from the individual-level analysis so that treatment and control groups do not overlap. The step from 680 arrests of Leipzig residents to the 518 that can be attached to a street collapses repeat arrests recorded at the same address: 115 of the 162 records concerned share an address that is retained. The raw transcription dates the 89 individual records of 11 September 1989 as 9 November, a transposition of day and month; they are counted in September.

Table A2: Treatment Months

January	14.75%
May	27.05%
June	22.13%
July	0.83%
September	17.21%
October	18.03%

Notes: Distribution of the 122 ever-treated streets across the six months in which a street's first arrest occurred (18, 33, 27, 1, 21, and 22 streets, respectively). These six months are the treatment cohorts of the Callaway–Sant'Anna estimator; see Section 5.2 for the implications for inference.

Table A3: Descriptive Statistics at the Street Level

	Mean	SD	Median	Min	Max	N
Panel A. Street characteristics						
Ever Arrest in Street (0/1)	0.19	0.39	0.00	0	1	654
Distance to Protest Route (in m)	2,523.55	2,059.85	2,382.64	1	8,156	654
Travel Time to City Center (in minutes)	32.55	11.42	31.11	4	70	654
Street Length (in m)	461.70	455.74	326.05	0	3,308	654
Population (100m radius)	311.74	424.95	176.54	0	4,572	654
Number of Stasi Apartments (100m radius)	3.13	4.30	2.00	0	40	654
Number of Churches (600m radius)	0.64	0.72	1.00	0	3	654
War Destruction (100m radius)	0.19	0.24	0.04	0	1	654
Panel B. Street × month						
Exit Visa Applications (monthly)	0.20	0.81	0.00	0	16	7,848
Arrests (monthly)	0.03	0.19	0.00	0	5	7,848

Notes: Panel A reports characteristics of the 654 streets in the estimation sample, one observation per street; all are fixed over the sample period. Panel B reports the two time-varying variables over the 7,848 street–month cells. The estimation window runs from November 1988 to October 1989. Distance to the protest route is the straight-line distance to the nearest route; travel time to the city center is estimated for walking and the historic tram network.

Table A4: Variable Description Table

Variable	Description
Panel A. Street–month panel: exit visa applications (Section 5.1)	
`exitvisas_st`	Number of exit visa applications from street s in month t. Outcome variable.
`arrest_st`	Indicator equal to 1 if a resident of street s was arrested at a protest in month t, and 0 otherwise. Arrests took place at protest venues; the street is that of the arrestee's home address.
`Ever_Arrest`	Indicator equal to 1 if at least one resident of street s was arrested over the observation period. Defines the treated group in balance tests.
`Neighbor_Arrest`	Indicator equal to 1 from the month in which a resident of a street whose centroid lies within 200 meters was first arrested. Defined for streets without an arrested resident of their own. Diffusion measure, capturing spatial dispersion through neighborhoods.
Panel B. Street characteristics (controls, balance and heterogeneity)	
`Distance_to_Protest`	Distance (in meters) from a street to the nearest protest route.
`TravelTime_CityCenter`	Estimated travel time (in minutes) from street s to the city center (the Nikolai Church) via walking and tram.
`Street_Length`	Length of the street (in meters).
`Population`	Estimated population of the street area (within a 100-meter buffer).
`StasiApts_100m`	Number of *Stasi* conspiratorial flats within 100 meters of the street, from the Bürgerkomitee Leipzig's map. A measure of *Stasi* presence; the flats were secret at the time (Appendix Table A18).
`Churches_600m`	Number of churches within 600 meters, capturing religious and organizational networks.
`War Destruction_100m`	Share of buildings destroyed in WWII within 100 meters. Proxy for housing quality and regime alignment.
`NeuesForum_Signups`	Number of individuals per street who signed up in support of the *Neues Forum* opposition movement on sign-up sheets collected from September 1989 to early 1990.
`NF_per100`	`NeuesForum_Signups` divided by `Population`, multiplied by 100. Density rather than count of opposition supporters.
`Propensity_Score`	Estimated probability that a street ever experiences an arrest, from a logit on the distance to the protest route, the travel time to the city center, street length, population, *Stasi* flats, churches and war destruction. It excludes *Neues Forum* support, which is measured after most arrests. Used for trimming, matching and inverse-probability weighting.
Panel C. Protest activity (Section 5.5)	

continued on the next page

Table A4 – continued from the previous page

Variable	Description
`Protest_1km`, `Protest_2km`	Indicators equal to 1 if at least one protest event was recorded within 1 km or 2 km of street s in week t (month t for the control in the exit visa regressions). Measured from an independent record of protest locations, i.e. not from arrests. `Protest_1km` also enters the exit visa regressions as a control.
`Protesters_City`	Estimated number of protest participants in Leipzig, by week or month.
`Protesters_Country`	Estimated number of protest participants across the GDR, by week. Included to absorb nationwide protest dynamics.
Panel D. Telephone-book panel: realized migration (Section 5.4)	
`Moved`	Indicator equal to 1 if an individual listed in one telephone-book wave is not observed at the same house in the following wave. Outcome variable of the migration analysis.
`Share_Moved`	Share of listed residents of a house (or street) with `Moved` = 1 over a given interval.
`Arrested_i`	Indicator equal to 1 if individual i was arrested during a protest in 1989. Treatment in the individual-level analysis.
`Street_Exposure`	Indicator equal to 1 if at least one resident of the street was arrested in 1989. Treatment in the street-level estimates.
`House_Exposure`	Indicator equal to 1 if at least one resident of the house (street key and house number) was arrested in 1989. Treatment in the house-level estimates, which compare such houses with other houses using house fixed effects, and in the within-street comparison with street-by-year fixed effects.
`Birth_Year_Expected`	Expected birth year of the listed individual, inferred from the first name: the rank-weighted mean of the birth years 1900–1960 in which the name was among the most popular names of newborns in Germany, or the mean birth year of *Neues Forum* signatories with that name. The share of residents born after 1950 enters the age-composition check (Appendix Table A23).

Table A5: Two-sample t-tests by Treatment Status

	Control	Treated	Difference
Distance to Protest	2441.49	2881.39	-439.90***
Travel Time to City Center	32.36	33.36	-1.01**
Street Length (m)	408.62	693.18	-284.56***
Population (100m radius)	245.21	601.85	-356.64***
Stasi Apartments (100m)	2.89	4.15	-1.25***
Churches (600m)	0.62	0.74	-0.12***
War Destruction (100m)	0.20	0.15	0.05***
Exit Visa Applications (pre-arrest)	0.11	0.42	-0.31***

Notes: This table compares pre-treatment characteristics of streets that ever experienced a protester arrest ("Treated") with those that never did ("Control"). For each covariate, we report mean values for both groups and the difference between them, along with significance levels from two-sample t-tests. The variables include distance to the nearest protest route, travel time to the city center, street length, local population (within a 100-meter radius), the number of Stasi apartments within 100 meters, the number of churches within 600 meters, the share of buildings destroyed in WWII within 100 meters, and monthly exit visa applications. Because applications are an outcome rather than a fixed characteristic, the exit-visa row uses only street-months before the first arrest for treated streets, while control streets contribute all street-months; the treated mean therefore measures the pre-arrest baseline used in Section 5.1. Support for the *Neues Forum* is not included here: the sign-up sheets record support declared from September 1989 onward, by which point 79 of the 122 treated streets (65%) had already experienced their first arrest, so the measure is post-treatment for most of the treated group and is reported separately in Table A28 rather than as a pre-treatment characteristic. Significant differences indicate observable selection into treatment: treated streets tend to be longer and more densely populated, to lie farther from the protest routes and marginally farther from the city center, and to house a higher density of *Stasi* apartments; they also suffered less wartime destruction. Asterisks denote significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Sampled and Unsampled Streets

	No month data	Month data	Difference
Distance to Protest (m)	4071.61	2531.84	1539.77***
Travel Time to City Center (min)	41.81	32.59	9.22***
Street Length (m)	566.00	461.70	104.30***
Population (100m radius)	262.65	311.74	-49.09*
Stasi Apartments (100m)	1.39	3.13	-1.73***
Churches (600m)	0.20	0.64	-0.44***
War Destruction (100m)	0.05	0.19	-0.14***
NF Members (per 100 pop.)	1.40	0.95	0.45**
Exit Visas 1988 (annual)	0.06	0.40	-0.34***
Exit Visas 1989 (annual)	0.25	3.08	-2.82***
Exit Visas 1988 (per 100 pop.)	0.01	0.16	-0.15***
Exit Visas 1989 (per 100 pop.)	0.07	0.99	-0.92***
Arrests (count)	0.26	0.31	-0.05
Ever Arrest (0/1)	0.15	0.18	-0.03

Notes: This table compares the 654 streets that enter the monthly estimation sample ("Month data") with the 1,215 streets that do not, on the covariates of Table A5. All variables are observed for every street in the city, including the annual count of exit visa applications, which is recorded city-wide. The sampled streets differ substantially in location and built environment: normalized differences exceed 0.25 in absolute value for distance to protest (-0.76), travel time (-0.70), war destruction (0.72), churches (0.72), *Stasi* apartments (0.48) and annual exit visas (0.53), and the covariates jointly predict membership in the sample ($F(9, 1777) = 104.2$, $p < 0.001$, $R^2 = 0.35$; clustered by district, $F(9, 24) = 163.1$, $p < 0.001$). The two rows that do not differ are the ones the design turns on: arrest counts and the ever-arrested indicator. Support for the *Neues Forum* is reported for completeness only; as in Table A5 it is measured from September 1989 onward and is post-treatment for most treated streets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Annual Effect on the Full Sample of Streets

	(1) Estimation sample	(2) Streets dropped	(3) Whole city	(4) Whole city, extra pre-year
Arrests × Post	3.162***	0.700***	1.642***	1.510***
	(0.831)	(0.204)	(0.329)	(0.343)
Streets used	Month data	No month data	All streets	All streets
Years	1988/89	1988/89	1988/89	1987-89
Streets	654	1,215	1,876	1,876
Street-years	1,308	2,430	3,752	5,628
Pre-period mean of dep. var.	0.401	0.056	0.176	0.218
Effect / pre-period mean	7.9	12.5	9.3	6.9

Notes: Difference-in-differences estimates of protester arrests on *annual* exit visa applications, with street and year fixed effects and standard errors clustered by street. Arrests is the number of residents of the street arrested in 1989, and Post indicates 1989. Because applications are counted by year, the 1989 total also contains applications filed before a street's first arrest, so the table measures an annual association rather than the timing of the monthly event study. The annual data are recorded for all 1,876 streets of the 1988 street directory, whereas the month-level data used everywhere else in the paper cover only 654; this table exploits that difference. Column (1) re-estimates the effect annually on exactly the 654 streets of our monthly estimation sample and serves as the benchmark. Column (2) runs the same regression on the 1,215 streets that the monthly panel discards—streets that appear nowhere else in the paper—and is the test of whether our results are an artifact of which districts the archive happened to date to the month. Column (3) pools both groups. Column (4) repeats column (3) with 1987 added as a second pre-treatment year; it is a robustness check on the pre-period rather than a different sample, and it is not our preferred specification because the "1987" bin pools every application dated before 1988. The effect is positive and significant at the 1% level in all four columns. The raw coefficient is larger in column (1) than in column (2), and a triple interaction rejects equality (2.470, s.e. 0.855, $p = 0.004$). However, the two columns are estimated on outcomes of very different size: the pre-treatment mean is 0.401 applications per street-year in column (1) against 0.056 in column (2). Scaled by that mean the ordering reverses (7.9 against 12.5), so the level gap tracks the level of the outcome rather than a different response to repression. We do not read the difference in raw coefficients as a different response to repression. Seven streets in the sampled districts—inner-city passages without a street key that record no applications or arrests—enter only column (3), so columns (1) and (2) add up to 1,869. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Own-street effect over months 0–5 by control group and cohort

	All cohorts		Without January		January cohort only	
Control group	Estimate	(s.e.)	Estimate	(s.e.)	Estimate	(s.e.)
All never-treated	0.241**	(0.118)	0.127	(0.121)	0.731**	(0.343)
Never- and not-yet-treated	0.227*	(0.117)	0.124	(0.121)	0.731**	(0.343)
Common support	0.239**	(0.120)	0.128	(0.124)	0.699**	(0.343)
3-NN matched	0.211*	(0.119)	0.108	(0.122)	0.660*	(0.345)
Matched on pre-arrest applications	0.168	(0.119)	0.074	(0.122)	0.574*	(0.345)
Streets with any application	0.301	(0.199)	0.147	(0.216)	0.826*	(0.473)
Treated streets	122		104		18	

Notes: Callaway–Sant'Anna estimates of the effect of an arrest on exit visa applications on the arrestee's street, averaged over months 0 to 5 after the first arrest, in applications per street and month. Control groups as in Table A11; the matched sample pairs each treated street with never-treated streets on applications in the months before the arrest. Without January drops the 18 streets whose first arrest occurred on 15 January 1989; January cohort only keeps them as the only treated streets. Standard errors clustered by street. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Functional Form

	Exit Visa Applications			
	(1) Levels	(2) asinh	(3) Any application	(4) Poisson
Pre Average (−4 to −2)	0.022	-0.030	-0.048	
	(0.091)	(0.042)	(0.030)	
Months 0–5	0.241**	0.082	0.018	-0.079
	(0.118)	(0.056)	(0.039)	(0.122)
Months 0–1	0.329**	0.107*	0.030	0.084
	(0.158)	(0.063)	(0.040)	(0.152)
Months 0–3	0.288**	0.093*	0.027	0.023
	(0.131)	(0.057)	(0.039)	(0.105)
All post-treatment months	0.424**	0.185**	0.053	-0.069
	(0.171)	(0.087)	(0.058)	(0.174)
Streets	654	654	654	654
Months	12	12	12	12
Observations	7,848	7,848	7,848	7,848

Notes: This table re-estimates the baseline event study of Figure 3 under specifications that remove differences in scale between treated and control streets. Column (1) is the baseline specification. Column (2) uses the inverse hyperbolic sine of applications as the outcome. Column (3) is a linear probability model for whether the street records any application in the month (sample mean 0.10). Column (4) is the extended two-way fixed effects Poisson estimator of Wooldridge (2021), implemented with `jwdid`; coefficients are in log points, controls are never- and not-yet-treated streets, and the estimator has no pre-period estimand. 452 of the 654 streets (56 treated, 396 never-treated) record no application in any month; they do not contribute to the Poisson likelihood, so column (4) is identified from the remaining 202 streets, which is why its standard errors are large. Pre Average is the mean of the event-time coefficients for months −4 to −2; Post Average is the mean of the post-treatment event-time coefficients in columns (1)–(3) and, for the Poisson estimator in column (4), the cohort-size-weighted average of all post-treatment cells; Months 0–1 and 0–3 are the means over the first two and first four months after the first arrest. Standard errors clustered by street in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Parallel Trends Conditional on Baseline Scale

	Streets with an arrest			Neighboring streets		
	(1)	(2)	(3)	(4)	(5)	(6)
Months 0–3	0.288**	0.237*	0.213*	0.076**	0.045	0.033
	(0.137)	(0.127)	(0.126)	(0.036)	(0.040)	(0.041)
Months 0–5	0.241**	0.172	0.145	0.053	0.015	0.002
	(0.123)	(0.113)	(0.112)	(0.034)	(0.037)	(0.038)
Pre-treatment average (−4 to −2)	0.049	0.022	0.015	0.015	−0.001	−0.013
	(0.119)	(0.110)	(0.110)	(0.032)	(0.037)	(0.039)
1988 applications		Yes	Yes		Yes	Yes
Log population and street length			Yes			Yes
Treated streets	122	122	122	217	217	217
Comparison streets	532	532	532	315	315	315

Notes: Callaway–Sant'Anna estimates in applications per street and month. Columns (1) to (3) use the own-street design of Figure 3, with the 532 never-treated streets as the comparison group. Columns (4) to (6) use the neighboring-street design of Appendix Table A20, with the 315 streets without an arrested neighbor as the comparison group. Columns (1) and (4) repeat the baseline, which assumes that treated and comparison streets would have followed the same trend in levels. The other columns use the outcome-regression version of the estimator (Sant'Anna and Zhao 2020). For each cohort and month, the change in applications since the month before treatment is regressed on the covariates among comparison streets, and the effect is the mean difference between the observed and the predicted change among treated streets. Parallel trends then need to hold only conditional on the covariates. Columns (2) and (5) condition on the number of exit visa applications filed on the street in 1988, before any arrest, and an indicator for any such application. Columns (3) and (6) add log population within 100 meters and log street length. The population estimate dates from December 1990. Replacing it with the log number of residents listed in the 1985 telephone book gives 0.244 and 0.178 in column (3) and 0.051 and 0.020 in column (6), for months 0 to 3 and 0 to 5. Standard errors in parentheses are from a bootstrap over streets with 500 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Alternative Control Groups

	$Exitvisas_{it}$			
	All never treated	Common support	3-NN matched	Pre-arrest matched
	(1)	(2)	(3)	(4)
Pre Average (−4 to −2)	0.022	0.014	-0.015	0.011
	(0.091)	(0.096)	(0.092)	(0.092)
Months 0–5	0.241**	0.239**	0.211*	0.168
	(0.118)	(0.120)	(0.119)	(0.119)
All post-treatment months	0.424**	0.409**	0.389**	0.309*
	(0.171)	(0.172)	(0.172)	(0.173)
Streets	654	509	316	322
Observations	7,848	6,108	3,792	3,864

Notes: This table re-estimates the baseline event study under alternative definitions of the comparison group. Column (1) uses all never-treated streets. Column (2) discards never-treated streets whose estimated propensity to be arrested falls below the fifth percentile of the treated distribution, removing streets that could never plausibly have experienced an arrest. Column (3) matches each treated street to its three nearest never-treated neighbors on the propensity score. Column (4) matches each treated street to its three nearest never-treated streets on a standardized distance combining the propensity score of columns (2) and (3) with the street's mean monthly exit visa applications from November 1988 to the month before its cohort's first arrest. Here, never-treated streets may serve as a match for more than one treated street. Matched controls average 0.36 applications per month over the pre-arrest window, against 0.40 on treated streets. Pre Average is the mean of the pre-treatment event-time coefficients; Post Average is the mean of the post-treatment event-time coefficients. Standard errors clustered by street in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Robustness Tests

	Exit visa applications		
	Placebo	Protest control	Baseline
	(1)	(2)	(3)
Months 0–5	0.018	0.236**	0.241**
	(0.087)	(0.118)	(0.118)
All post months	-0.092	0.413**	0.424**
	(0.093)	(0.170)	(0.171)
Permutation p: all streets			0.001
Permutation p: timing only			0.138
Streets	654	654	654
Months	12	12	12
Observations	7,632	7,848	7,848

Notes: This table summarizes robustness checks for the Callaway–Sant'Anna (2021) event-study estimator applied to monthly street-level exit visa applications. Each column reports the post-treatment average effect relative to the month before a street's first arrest; the baseline specification itself is reported in Figure 3. Column (1) reports a placebo test in which treatment dates are shifted two months earlier. Its average includes pseudo-post months that are actual post-arrest months, and it omits the January cohort, whose shifted date has no base month in the panel. Restricted to the two pseudo-months before the actual arrest, the placebo estimate is −0.071 (s.e. 0.109). Column (2) adds an indicator for a protest recorded in the *Citizens Movement Archive* calendar within 1 km of the street in the month. Column (3) reports the baseline estimate together with two one-sided permutation p-values for months 0 to 5 that address the concern that streets arrested in the same crackdown share a common shock (the same police operation, the same day's media coverage) and so are not independent conditional on street-level clustering. In the first test the six treatment cohorts are reassigned at random across all 654 streets, holding cohort sizes fixed; in the second the 122 treated streets are held fixed and only the month of their first arrest is reassigned among them. The first test treats all streets as exchangeable, which the differences in Table A5 rule out. Only the second is design-based. Each test uses 1,000 permutations, and the p-value is the number of permuted estimates at or above the actual one, plus one, divided by 1,001. Post Average is the equal-weighted mean of the post-treatment event-time coefficients; Appendix Table A16 reports spatial standard errors for the baseline. Standard errors in parentheses are clustered by street. Streets reports the number of streets in the estimation sample; Months indicates the panel length; Observations refers to total street-month cells. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Predicting the Treatment

	$FirstArrest_{it}$	
	(1)	(2)
Protest within 1 km, same month	0.001	
	(0.005)	
Protest within 1 km, previous month		-0.000
		(0.005)
Observations	7,389	6,735

Notes: OLS regressions of an indicator for the first arrest on a street in the month. The sample contains each street until the month of its first arrest, since a street can be first arrested only once. The explanatory variable is an indicator for a protest recorded in the *Citizens Movement Archive* calendar within 1 km of the street in the month (column 1) or in the previous month (column 2). Controls are the distance to the protest route, the travel time to the city center, street length, the population within 100 m, *Stasi* flats within 100 m, churches within 600 m, war destruction within 100 m, and month fixed effects, which absorb city-wide protest intensity. Standard errors are clustered by street. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Not-Yet-Treated Streets as the Comparison Group

	(1) Never + not-yet treated	(2) Not-yet only treated
Pre Average (−4 to −2)	0.026	0.054
	(0.092)	(0.110)
Months 0–5	0.227*	0.208
	(0.117)	(0.159)
All post-treatment months	0.408**	0.256
	(0.170)	(0.204)
Streets	654	122
Months	12	11
Observations	7,848	1,342

Notes: Callaway–Sant'Anna (2021) event-study estimates on the monthly street panel, November 1988 to October 1989, with treatment defined as the month of a street's first protester arrest. Column (1) uses the full sample of 654 streets and takes never-treated and not-yet-treated streets as the comparison group. Column (2) drops the 532 never-treated streets and estimates the model on the 122 treated streets alone, so that the comparison group for each cohort consists only of streets whose first arrest lies in the future; in October 1989 every street in this sample has been treated, so no cohort-time effect can be estimated for that month. The October cohort contributes no post-treatment cells, and every earlier cohort loses its October cell, so the sample covers 11 months (1,342 observations) and the post-treatment average in column (2) omits the longest event times of the early cohorts. Both columns use the baseline specification. Pre Average is the mean of the pre-treatment event-time coefficients; Post Average is the mean of the post-treatment event-time coefficients. Column (2) isolates the variation in the timing of arrests across treated streets, the same variation that the second permutation test in Table A12 uses. Its estimate is less precise than the baseline because the comparison group shrinks with every cohort, but it is positive, its pre-period is flat, and it lies within the confidence interval of the baseline. The estimate is compatible with the baseline but not significant on its own: the direction of the effect does not depend on the never-treated streets, but its precision does. Standard errors clustered by street in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Spillover Exposure of Treated and Comparison Streets

	(1)	(2)	(3)	(4)
Panel A. Streets with an arrest, by exposure of treated and comparison streets				
	All never-treated	Unexposed controls	Unexposed both sides	
Months 0–3	0.288	0.301	0.369	
	(0.121)	(0.121)	(0.159)	
Months 0–5	0.241	0.253	0.314	
	(0.111)	(0.111)	(0.144)	
Post-treatment average	0.424	0.450	0.561	
	(0.169)	(0.169)	(0.195)	
Treated streets	122	122	73	
Comparison streets	532	315	315	
Panel B. Streets next to a street with an arrest, by number of such neighbors				
	All	One	Two or more	No later re-exposure
Months 0–3	0.076	0.110	0.000	0.082
	(0.035)	(0.041)	(0.057)	(0.040)
Months 0–5	0.053	0.087	−0.014	0.062
	(0.032)	(0.041)	(0.042)	(0.039)
Post-treatment average	0.148	0.286	−0.019	0.271
	(0.099)	(0.174)	(0.034)	(0.172)
Treated streets	217	161	56	174
Comparison streets	315	315	315	315

Notes: Callaway–Sant'Anna estimates as in Figure 3, in applications per street and month, with standard errors clustered by street. Two streets are neighbors if their centroids lie within 200 meters of each other (Section 4). Panel A repeats the own-street design. Column (1) uses all 532 never-treated streets as the comparison group. Column (2) uses only the 315 never-treated streets without an arrested neighbor, which are not exposed to spillovers. Stricter radii are not feasible: only 62 never-treated streets have no arrested neighbor within 500 meters, and together they record 12 applications over the twelve months. Column (3) in addition drops the 49 arrested streets with an arrested neighbor in a month other than their own first arrest, so that neither treated nor comparison streets are exposed to arrests on neighboring streets. Panel B repeats the neighboring-street design of Appendix Table A20, with the same 315 unexposed streets as the comparison group throughout. Column (1) uses all 217 exposed streets. Columns (2) and (3) split them by the number of arrested neighbors. Column (4) drops the 43 streets that gain a further arrested neighbor in a month after their first exposure. Months 0–3 and months 0–5 are means of the event-time coefficients over these months; the post-treatment average is the mean of all post-treatment coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Spatial Standard Errors for the Baseline Estimate

	Exit Visa Applications	
	Post Average	Months 0–5
	(1)	(2)
Estimate	0.424	0.241
Standard error [p-value]		
Street cluster	0.169 [0.012]	0.111 [0.030]
Conley, Bartlett 1 km, within month	0.175 [0.016]	0.115 [0.037]
Conley, Bartlett 2 km, within month	0.188 [0.024]	0.125 [0.054]
Conley, Bartlett 3 km, within month	0.200 [0.034]	0.128 [0.059]
Conley, uniform 1 km, within month	0.187 [0.023]	0.130 [0.065]
Conley, uniform 2 km, within month	0.215 [0.049]	0.138 [0.081]
Conley, uniform 3 km, within month	0.223 [0.057]	0.128 [0.060]
Conley, Bartlett 1 km, all months	0.198 [0.032]	0.144 [0.096]
Conley, Bartlett 2 km, all months	0.238 [0.075]	0.174 [0.167]
Zip code cluster (8 clusters)	0.271 [0.118]	0.191 [0.209]
SCPC, average pairwise correlation ≤ 0.01	0.114 [0.002]	0.113 [0.052]
SCPC, average pairwise correlation ≤ 0.03	0.149 [0.030]	0.142 [0.164]
SCPC, average pairwise correlation ≤ 0.10	0.365 [0.427]	0.245 [0.499]
Streets	654	654
Months	12	12
Observations	7,848	7,848

Notes: Alternative standard errors for the baseline event study of Figure 3. Column (1) is the post-treatment average, the mean of the post-treatment event-time coefficients; column (2) is the mean of the event-time coefficients for months 0 to 5. Each row reports the standard error of the estimate in the header, with the two-sided p-value in brackets. The street-cluster row is the baseline; it treats the cohort weights as fixed, whereas the standard error that Callaway–Sant'Anna (2021) reports for the same estimate, 0.171 in Table A12, also accounts for the estimation of the cohort shares. The Conley rows keep arbitrary serial correlation within a street and add spatial correlation among streets whose centroids lie within the stated distance of each other, with Bartlett (linearly declining) or uniform kernel weights; the within-month rows allow this correlation only among observations of the same month, the all-months rows in every pair of months, so that at 2 km each street shares a cluster with a quarter of the sample (the average street has 61 others within 1 km and 164 within 2 km). The zip-code row clusters by the eight postal districts and is shown for completeness only. The SCPC rows apply the spatial correlation principal components method of Müller and Watson (2022), implemented in their Stata package, which bounds the average pairwise correlation among streets by the stated value, projects the estimator on the principal components of the worst-case spatial covariance implied by that bound, and uses critical values adjusted accordingly; 0.03 is the authors' default. Because the estimate is the sum over streets of influence contributions, a regression of the pseudo-observation estimate $+ N\times$ contribution on a constant reproduces the estimate and its street-clustered variance exactly, and SCPC is applied to that regression with the street centroids, projected to kilometers, as locations. Because the estimator without covariates is a difference of cohort-by-month means, every aggregate is a linear function of the street-month outcomes and the variances are computed exactly from the cohort-by-month residuals, without asymptotic refinements.

Table A17: The Arrestee's Workplace Street Instead of Their Home Street

	Home street (1)	Workplace street (2)
Month of the arrest	0.376	−0.004
	(0.170)	[0.90]
Months 0–3	0.288	0.010
	(0.121)	[0.91]
Months 0–5	0.241	−0.006
	(0.111)	[0.92]
All post-treatment months	0.424	−0.058
	(0.169)	[0.57]
Treated streets	122	17
Control streets	532	515

Notes: The estimator of Figure 3 with the treated unit redefined. Column (1) repeats the baseline: the 122 streets on which a resident was arrested, comparison group. Column (2) treats instead the street on which the arrestee worked. The arrest register names an employer for 205 of the 518 arrestees and a work address for 95, of which 86 are in Leipzig and 82 resolve to a single street of the 1988 street directory. 17 of those streets belong to the estimation sample and never had an arrest of a resident. The estimation drops every street on which a resident was arrested, so that no street is treated in both senses. Because seventeen treated streets are too few for cluster-robust inference, column (2) reports randomization inference in brackets: the vector of workplace treatment months is permuted across the same 532 streets 1,000 times and the observed estimate is placed in that distribution. The permutation standard deviation is 0.166 in the month of the arrest, 0.091 over months 0 to 3 and 0.130 for the post-treatment average, so the test would reject an effect of the size estimated in column (1) for months 0 to 3 with probability 0.82. Standard errors clustered by street in parentheses in column (1); randomization p-values in brackets in column (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Placebo: The Opening of a *Stasi* Conspiratorial Flat on or near the Street

	Flat opened on the street (1)	Flat opened within 100 m (2)
Pre-treatment average	-0.026	-0.040
	(0.074)	(0.080)
Post-treatment average	0.038	-0.013
	(0.076)	(0.033)
Months 0 to 3	0.032	0.003
	(0.146)	(0.054)
Exposed streets	21	83
Streets	532	532
Observations	6,360	6,264

Notes: The estimator of Figure 3 on the 532 streets without an arrest, with treatment defined as the month in which the *Stasi* set up a conspiratorial flat (*konspirative Wohnung*) on the street (column 1) or within 100 meters of it (column 2). The flats are those of the Bürgerkomitee Leipzig's map of the 1,062 objects the *Stasi* maintained in the city in 1989, with the date each was set up from its file; 83 were opened between November 1988 and October 1989. The flats were secret and became known only after 1989, most of them in 2019. Streets without an opening in the panel are the comparison group. Pre-treatment average: mean of the four leads; post-treatment average: mean of the post-treatment event-time coefficients; months 0 to 3: their mean over the first four months. Standard errors clustered by street. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: The Severity of the Sanction as a Dose

	All arrest days		May and June only	
	Harsh (1)	Lenient (2)	Harsh (3)	Lenient (4)
Months 0–3	−0.072	0.741	−0.047	0.388
	(0.169)	(0.239)	(0.236)	(0.202)
Post-treatment average	−0.117	0.643	−0.119	0.313
	(0.134)	(0.225)	(0.186)	(0.174)
Harsh − lenient, months 0–3	−0.812		−0.435	
	(0.293)		(0.311)	
Harsh − lenient, post average	−0.760		−0.432	
	(0.262)		(0.255)	
Treated streets	36	40	24	33
Two-way fixed effects, severity in standard deviations				
Post × severity, within treatment month	0.022			
	(0.077)			
also controlling for the size of the raid	0.076			
	(0.129)			

Notes: Severity is the share of those detained on an arrest day who were fined (*Ordnungsstrafverfügung*) or against whom criminal proceedings were opened (*Ermittlungsverfahren*). Data taken from the *Stasi* summary sheet for that day. Eleven days carry a complete sheet, covering 275 of the 518 arrests and the first arrest of 76 of the 122 treated streets. Streets whose first arrest fell on a day without a sheet are dropped from both the treated and the comparison group. Severity ranges from 0 to 1 with a standard deviation of 0.294, of which 0.260 is within treatment month. Columns (1) and (2) split the treated streets at the median severity and estimate the model of Figure 3 separately for each half against the same never-treated comparison group; columns (3) and (4) repeat the split within the May and June cohorts, the two months containing several arrest days. The difference between halves treats the two estimates as independent, which overstates its standard error because they share a comparison group. The lower panel reports the coefficient on the interaction of a post-treatment indicator with severity in standard deviations, in a regression of applications on street and month fixed effects with a separate post-treatment effect for each treatment month, so that the dose is identified only from streets first arrested on different days of the same month. Severity correlates −0.59 with the number of people detained that day; the second row adds that count, likewise in standard deviations, as a further interaction. Standard errors clustered by street in parentheses. Because severity is assigned by arrest day, we also reassign the severities of the arrest days within each treatment month (all 576 permutations of the four May and four June days); the two-sided permutation p-value for the within-month interaction is 0.74. The splits in columns (1) and (2) compare days in different months and are confounded with the timing of the cohorts, in particular with the January raid, which imposed no fines. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20: Spatial Spillovers

	(1) Neighboring street
Pre Average	-0.009
	(0.043)
Post Average	0.148
	(0.099)
Months 0–3	0.076**
	(0.035)
	[0.043]
Permutation p, months 0–3: exposed streets	0.074
Permutation p, months 0–3: source arrests	0.117
Streets	532
Months	12
Observations	6,384

Notes: The table reports pre-treatment and post-treatment averages from the Callaway–Sant'Anna specification of the main results, with the sample restricted to streets that never had an arrest of a resident and treatment defined as the month in which a resident of a neighboring street, one within 200 meters, was first arrested. Months 0–3 is the mean of the event-time coefficients for the first four months after exposure, with its standard error from the delta method; the Post Average is the mean of the post-exposure event-time coefficients, the later of which are identified from fewer cohorts. Stars refer to standard errors clustered by street; the Conley standard error in brackets (Bartlett kernel, 1 km, within month) allows for correlation between nearby streets. The two permutation p-values are one-sided tests for months 0–3. The first holds the 217 exposed streets fixed and reassigns the month of their first neighboring arrest among them, preserving cohort sizes. The second reassigns the months of the first arrest among the 122 arrested streets and rebuilds the exposure of every street, so that streets exposed to the same arrest move together. Each reports the number of 1,000 draws at or above the actual estimate, plus one, divided by 1,001. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A21: Realized Migration

	Share Moved$_{st}$					
	Street-Level			House-Level		
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested$_i$ × 1987–91	0.036**		0.035***	0.066**		0.063**
	(0.014)		(0.013)	(0.027)		(0.027)
Arrested$_i$ × 1991–96	0.019		0.020	0.026		0.038
	(0.017)		(0.015)	(0.030)		(0.029)
Treated$_{i,1985-87}$ (before arrests)		-0.018			0.003	
		(0.016)			(0.019)	
Adj. R-Squared	0.313	0.000	0.294	0.189	-0.000	0.180
Observations	4,098	1,366	4,098	43,014	14,338	43,014
Mean of Outcome	0.327	0.217	0.282	0.275	0.151	0.238
Street-Level FE	Yes	No	Yes	No	No	No
House-Level FE	No	No	No	Yes	No	Yes

Notes: Difference-in-differences estimates of the effect of a protester arrest in 1989 on residential mobility, at the street level (columns 1–3) and the house level (columns 4–6). The dependent variable is the share of residents listed at the start of a telephone-book interval (1985–87, 1987–91, 1991–96) who are not found at the same house at the start of the next one (`Share_Moved`, Table A4); 1985–87 is the reference interval. Columns (2) and (5) report the treated-control difference within 1985–87 alone, without unit fixed effects; columns (3) and (6) exclude the 50 most common surnames. The other columns include street or house and interval fixed effects. Standard errors in parentheses, clustered by street or house. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Realized Migration: City-Wide Outcome

Share of residents ...	Street level		House level	
	not at the same house (1)	not found in Leipzig (2)	not at the same house (3)	not found in Leipzig (4)
$\text{Arrested}_i \times$ 1987–91	0.036**	0.024*	0.066**	0.069**
	(0.014)	(0.014)	(0.027)	(0.027)
$\text{Arrested}_i \times$ 1991–96	0.019	0.008	0.026	0.040
	(0.017)	(0.016)	(0.030)	(0.028)
Units	1,366	1,366	14,338	14,338
Observations	4,098	4,098	43,014	43,014
Mean of outcome	0.327	0.265	0.275	0.231

Notes: Columns (1) and (3) repeat columns (1) and (4) of Table A21. Columns (2) and (4) replace the outcome by the share of residents not found anywhere in Leipzig in the next book, that is, neither at the same house, nor on the same street, nor by a unique name match elsewhere in the city. Entries whose name matches an arrested protester are excluded. All columns include unit and interval fixed effects; standard errors (in parentheses) are clustered by street or house. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A23: Realized Migration: Age Composition of the Listed Residents

	Share Moved$_{st}$	
	Street-Level	House-Level
	(1)	(2)
Arrested$_i$ × 1987–91	0.038**	0.054**
	(0.015)	(0.027)
Arrested$_i$ × 1991–96	0.019	0.021
	(0.017)	(0.031)
Share born after 1950$_{i,1985}$ x 1987-91	-0.062	0.027**
	(0.064)	(0.014)
Share born after 1950$_{i,1985}$ x 1991-96	0.013	0.015
	(0.088)	(0.016)
Adj. R-Squared	0.316	0.190
Observations	4,059	40,158
Mean of Outcome	0.327	0.275
Street-Level FE	Yes	No
House-Level FE	No	Yes

Notes: Columns (1) and (4) of Table A21 with the age composition of the residents added. Each listed person receives an expected birth year estimated on the basis of their first name, the rank-weighted mean of the birth years 1900 to 1960 in which the name was among the most popular names of newborns in Germany, or, for names absent from those lists, the mean birth year of *Neues Forum* signatories with that name; 84% of entries are covered. The share of a street's (house's) residents born after 1950 in the 1985 book is interacted with the intervals. Residents of streets with arrests were on average 2 years younger than others, residents of houses with an arrest 3 years. Unit and interval fixed effects; standard errors in parentheses, clustered by street or house. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A24: Realized Migration: Within-Street Comparison and Pending Applications

	House level		Street level	
	(1)	(2)	(3)	(4)
Arrest × 1987–91	0.066**	0.048*	0.033	0.026
	(0.027)	(0.029)	(0.029)	(0.030)
Arrest × 1991–96	0.026	0.009	−0.015	−0.013
	(0.030)	(0.029)	(0.031)	(0.031)
House fixed effects	Yes	Yes		
Street fixed effects			Yes	Yes
Interval fixed effects	Yes		Yes	Yes
Street × interval fixed effects		Yes		
1988 applications × interval				Yes
Units	14,338	14,338	470	470
Treated units	203	203	63	63

Notes: The outcome is the share of listed residents who are no longer listed at the same address in the next book, as in Table A21. Column (1) repeats column (4) of Table A21. Column (2) replaces the interval fixed effects by street-by-interval fixed effects, so that an arrested house is compared only with other houses on the same street in the same interval. This removes every factor that affected the whole street, including exit visa applications that were pending before the arrests. Column (3) repeats the street-level estimate of Table A21 on the 470 streets of the estimation districts, for which monthly exit visa applications are available. Column (4) adds the number of applications filed on the street in 1988, before any arrest, interacted with the two intervals. Standard errors in parentheses, clustered by house in columns (1) and (2) and by street in columns (3) and (4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A25: Residents and Non-Residential Entries on Streets with Arrests

	Residents Share gone (1)	Non-person entries Δ log count (2)
Panel A. 1985–87, before the arrests		
Arrested$_i$	-0.028	0.001
	(0.018)	(0.028)
Mean of outcome	0.226	0.007
Streets	510	654
Panel B. 1987–96, Wende and after		
Arrested$_i$	0.035	0.109
	(0.022)	(0.073)
Mean of outcome	0.550	0.281
Streets	519	654
Street controls	Yes	Yes
ZIP code fixed effects	Yes	Yes

Notes: Street-level regressions of the change in each outcome on an indicator for whether a resident of the street was arrested in 1989, with the street controls of Table A5 and zip-code fixed effects; heteroskedasticity-robust standard errors in parentheses. Column (1) is the share of the residents listed at the start of a window who are no longer listed anywhere in the city at its end, the city-wide outcome of Table A22. Column (2) is the change in the log number of non-person entries per street: schools, polyclinics, institutes, shops, offices and enterprises, flagged in the transcription of the 1985, 1987 and 1996 directories (9,421, 9,313 and 12,548 entries with a street key). The 1991 edition carries no such flag and none can be recovered from it, which fixes the windows at 1985–87 and 1987–96. The outcome is a count rather than a survival rate because individual premises cannot be tracked across the transition: the Treuhand privatized or liquidated essentially the entire state sector between 1990 and 1994. Only 3.1 percent of the 1985 institution names reappear in 1996, and a matched disappearance rate is accordingly about 0.97 on nearly every street. Column (2) is therefore a net change and does not measure turnover, which column (1) does for residents. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A26: Realized Migration of Individual Arrested Protesters and *Neues Forum* Signatories

	Arrested protesters	*Neues Forum* signatories	Difference	Difference, zip-code FE	Fisher exact p
	(1)	(2)	(3)	(4)	(5)
Panel A. Persons listed in the 1987 telephone book: status in the 1991 book					
Not at the same house	0.500	0.254	0.246**	0.237**	0.004
			(0.099)	(0.107)	
Not found anywhere in Leipzig	0.500	0.204	0.296***	0.294***	0.000
			(0.098)	(0.105)	
Persons	34	343			
Share of group traced in 1987	0.079	0.123			
Panel B. Pre-period: persons listed in the 1985 telephone book, status in the 1987 book					
Not at the same house	0.103	0.105	-0.001	-0.010	1.000
			(0.062)	(0.057)	
Not found anywhere in Leipzig	0.103	0.095	0.009	-0.000	0.748
			(0.062)	(0.058)	
Persons	29	296			
Share of group traced in 1985	0.067	0.106			
Panel C. Persons of Panel A by arrest month: not found anywhere in Leipzig in 1991					
Arrested January to July 1989	0.560	0.204	0.356***	0.380***	0.000
			(0.109)	(0.112)	
Persons	25	343			
Arrested September or October 1989	0.333	0.204	0.129	0.052	0.400
			(0.170)	(0.219)	
Persons	9	343			

Notes: The table reports the estimates plotted in Figure A8. Each arrested protester and each *Neues Forum* signatory is searched in the telephone book at the start of an interval, first by exact first and last name at the person's address, otherwise by surname at that address. Persons who appear in both lists are excluded. Panel A traces the persons found in the 1987 book into the 1991 book, the interval containing the fall of the regime; Panel B repeats the exercise for the 1985 book traced into the 1987 book, an interval that precedes every arrest. *Not at the same house* is an indicator for not being found at the same house at the end of the interval, *Not found anywhere in Leipzig* for not being found at any address in the city. Columns (1) and (2) give the group means, column (3) their difference, and column (4) the difference from an OLS regression with zip-code fixed effects; robust standard errors in parentheses. Column (5) reports the p-value of Fisher's exact test. *Share of group traced* is the fraction of all arrested protesters, respectively all signatories, found in the book at the start of the interval; most members of both groups never appear in a telephone book, since only about a fifth of households had a connection. Panel C splits the arrested protesters of Panel A by the month of their arrest and compares each group with all signatories. Signatories had to be in Leipzig in the fall of 1989 to sign, whereas protesters arrested before September could have left before the *Neues Forum* was founded, so only the second row compares groups that were in the city at the same time. The comparison is descriptive. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A27: Protester Arrests and Local Protest Activity: Weekly Panel, All Streets

	Protest occurred within … (0/1)	
	1 km	2 km
	(1)	(2)
Panel A. Comparison streets matched on distance to the venue of the arrest		
Pre Average (weeks −4 to −2)	0.001	-0.004
	(0.012)	(0.016)
Post Average (weeks 0 to 3)	0.001	0.018
	(0.011)	(0.015)
Post Average (weeks 0 to 12)	0.001	0.016
	(0.012)	(0.016)
Comparison streets	682	682
Panel B. All never-treated streets		
Pre Average (weeks −4 to −2)	-0.002	-0.003
	(0.010)	(0.014)
Post Average (weeks 0 to 3)	0.008	0.050***
	(0.010)	(0.014)
Post Average (weeks 0 to 12)	0.006	0.025*
	(0.011)	(0.014)
Comparison streets	1,530	1,530
Panel C. Matched comparison, outcome: asinh(reported turnout at the nearby events)		
Pre Average (weeks −4 to −2)	-0.013	-0.012
	(0.088)	(0.132)
Post Average (weeks 0 to 3)	-0.016	0.062
	(0.079)	(0.121)
Post Average (weeks 0 to 12)	-0.032	0.026
	(0.068)	(0.094)
Comparison streets	682	682
Pre-treatment mean, treated streets (%)	4.3	11.5
Mean, never-treated streets (%)	5.1	11.0
Treated streets	308	308
Cohorts (weeks)	14	14
Weeks	53	53

Notes: Callaway–Sant'Anna (2021) event-study estimates on a weekly panel of all 1,838 Leipzig streets with coordinates, 1 November 1988 to 31 October 1989 (53 weeks). Treatment is the week of a street's first protester arrest (308 streets, 14 cohorts). The dependent variable is an indicator for whether any activity listed in the *Citizens Movement Archive* protest calendar took place within 1 km or 2 km of the street in that week. Pre Average is the mean of the event-time coefficients for weeks −4 to −2; Post Average the mean for weeks 0 to 3 and 0 to 12. Panel A uses, for each cohort, the never-treated streets matched to its treated streets on log distance to the nearest protest venue active in the arrest week (three per treated street; the estimates of Figure 5). Panel B uses all never-treated streets. The last rows give the share of street-weeks with a protest within the radius before treatment for treated streets and over the whole year for never-treated streets. Standard errors clustered by street in parentheses. Conley standard errors (Bartlett kernel, 2 km) for weeks 0–3 are 1.20 (within 1 km) and 1.90 (within 2 km) when correlation is allowed within a week, and 1.66 and 2.32 when it is allowed across all weeks. Panel C repeats Panel A with the inverse hyperbolic sine of the reported turnout at the events within the radius in place of the indicator, so that the coefficients are approximately in log points; the crowd estimates come from the weekly protest chronicle, which reports one for 29 of the 56 located events. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A28: Arrests and Support for the *Neues Forum*

	Street level	House level
	Signatories per 100 residents	Any signatory (pp)
	(1)	(2)
No. arrested on the street	0.344***	
	(0.091)	
Arrest at the house (0/1)		6.47***
		(2.21)
Sample	All streets	All houses
Fixed effects	Zip code	Street
Controls	Yes	Yes
Adj. R-squared	0.046	0.026
Observations	1,741	17,739
Mean of outcome	1.046	9.003

Notes: OLS regressions of support for the *Neues Forum* on arrests, across the whole city, at the street level in column (1) and at the house level in column (2). The street sample comprises the streets of the 1988 street directory with a street key, a zip code and a positive resident count within 100 m. The house sample comprises the houses of the 1987 telephone book with coordinates. The outcome is the number of *Neues Forum* signatories per 100 residents within 100 m of the street in column (1), and 100 times an indicator for at least one signatory at the house in column (2). Column (1) uses the number of arrested residents of the street, column (2) an indicator for an arrest at the house itself. All signatories are used, regardless of when they signed; signatories who appear on the arrest list are excluded. Street-level controls are the resident count within 100 m, war destruction, street length, travel time to the city center and centered coordinates; the house-level control is the number of listed residents. Standard errors are robust in column (1) and clustered by street in column (2). Because most signatories cannot be dated and most treated streets had their first arrest before the *Neues Forum* was founded, the table cannot separate where the arrests took place from how residents responded to them. With the same controls and fixed effects, an arrest on a street within 200 m is associated with 0.34 signatories per 100 residents (s.e. 0.18) on the streets without an arrest, and an arrest at another house within 100 m with 0.3 percentage points (s.e. 0.7) among the houses without an arrest. Appendix Table A29 shows that streets arrested in October already had more signatories before their arrest and tests whether this changed afterwards. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

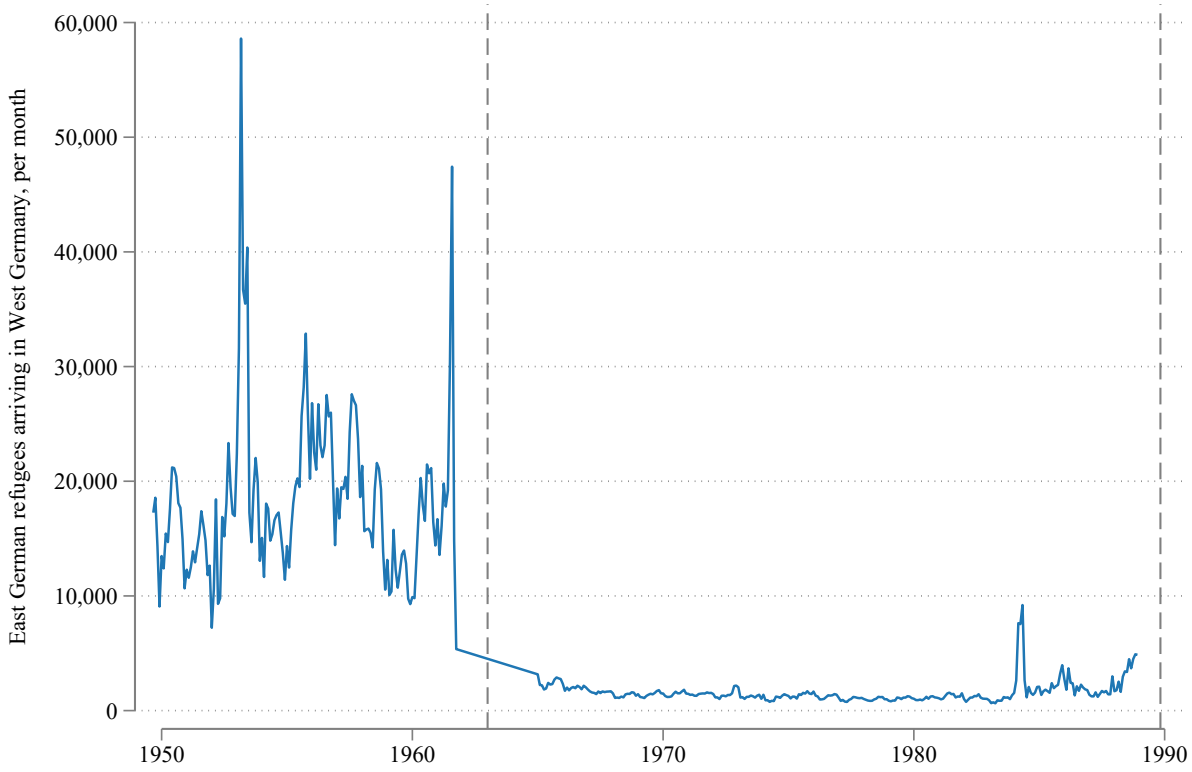
Table A29: *Neues Forum* Signatories before and after the October Arrests, Dated Entries

	Streets by the date of their first arrest				Gap, (4) against (1)
	Never	Before 11 Sep	11 Sep to 6 Oct	7 Oct or later	percentage points
	(1)	(2)	(3)	(4)	(5)
Share of streets with at least one signatory (percent)					
Before 7 October, dated entries	1.4	12.3	9.3	8.2	6.1 (3.9)
Before 7 October, dated entries and folder 3	7.4	31.4	29.6	22.4	11.7 (5.7)
After 7 October, dated entries	4.0	14.2	11.1	14.3	8.6 (4.6)
Change from before to after 7 October, (4) against (1)					
Early set: dated entries					3.5 (5.4)
Early set: dated entries and folder 3					−4.8 (7.1)
Streets	1,474	204	54	49	

Notes: Streets of the 1988 street directory with a street key, coordinates and a positive resident count within 100 m, grouped by the date of the first arrest of a resident: never, before the *Neues Forum* was founded on 10 September 1989, between 11 September and 6 October, and on 7 October or later. Each row reports the share of streets with at least one *Neues Forum* signatory in the set named in the row. *Dated entries* are signatories with a date written for their entry, together with undated signatories on a sheet that carries such a date; 493 of the 4,870 unique signatories carry a date. *Folder 3* adds all signatories in the archive folder labeled 21 September 1989, whose dated entries fall before 2 October in 59 of 60 cases; other folders are not used, since their dated entries contradict their labels. Of the 49 streets in column (4), 45 had their first arrest on 7 October and four on 9 October; two dated entries fall on 8 October. Column (5) reports the difference between columns (4) and (1) from a regression of the row's indicator on an indicator for the October group, the log resident count and zip-code fixed effects, with robust standard errors. The change stacks the early and the late indicator and reports the interaction of the October group with the late period, with standard errors clustered by street. In raw shares, the difference between columns (4) and (1) has Fisher exact p-values of 0.007, 0.001 and 0.005 in the three rows. The outcome is the presence of any signatory rather than a density, since too few entries are dated. No comparison is reported at the house level: none of the 79 houses arrested in October holds a signatory dated before 7 October.

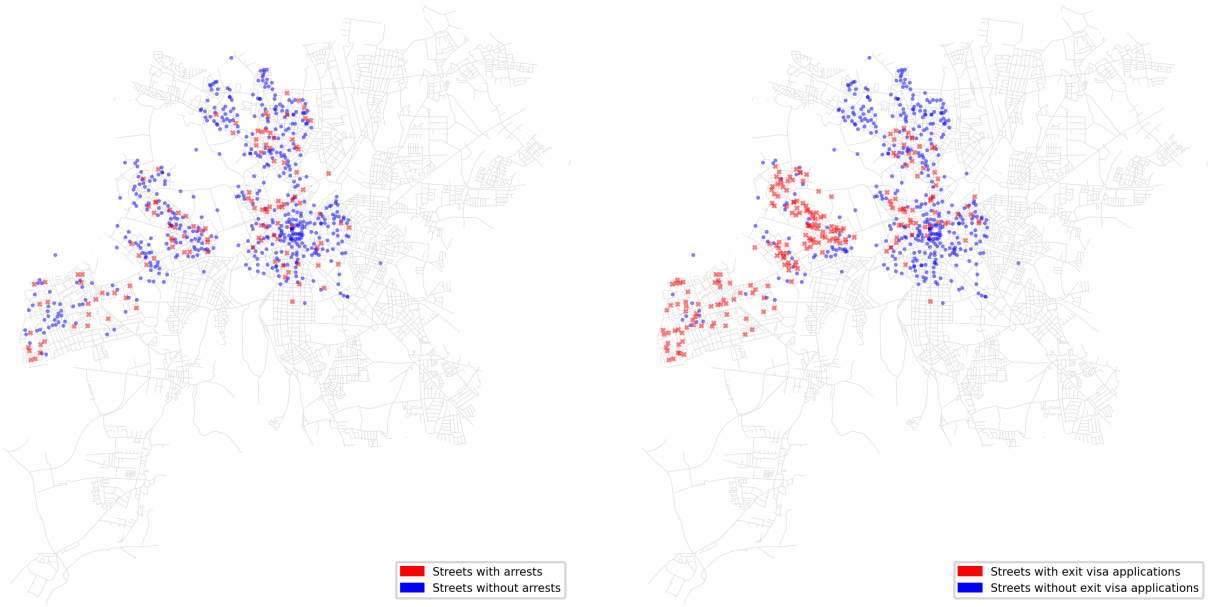
B Additional Figures

Figure A1: East Refugees in West Germany



Notes: Number of East German refugees registered in West Germany. Data from Horz and Marbach (2022).

Figure A2: Arrests and Exit Visa Applications

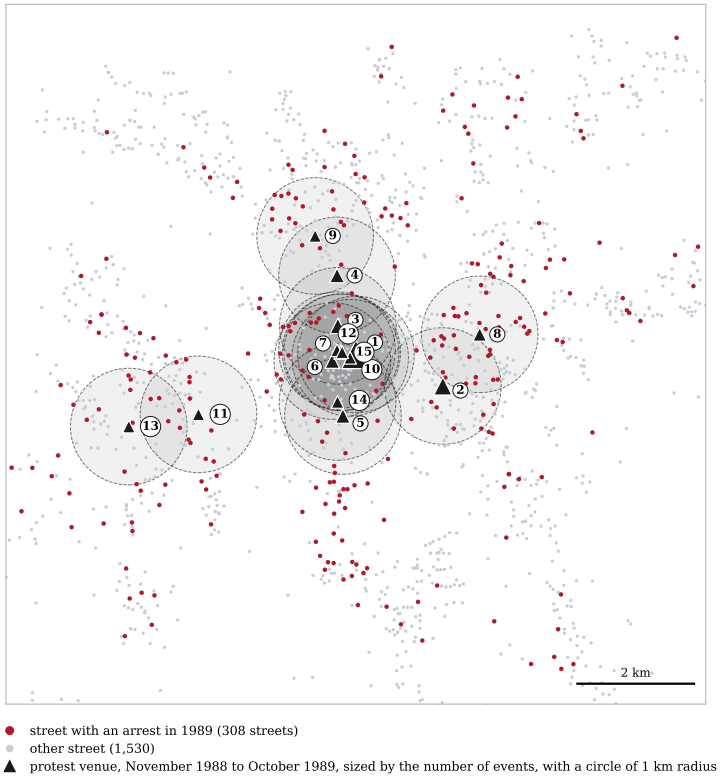


(a) Arrests

(b) Exit Visa Applications

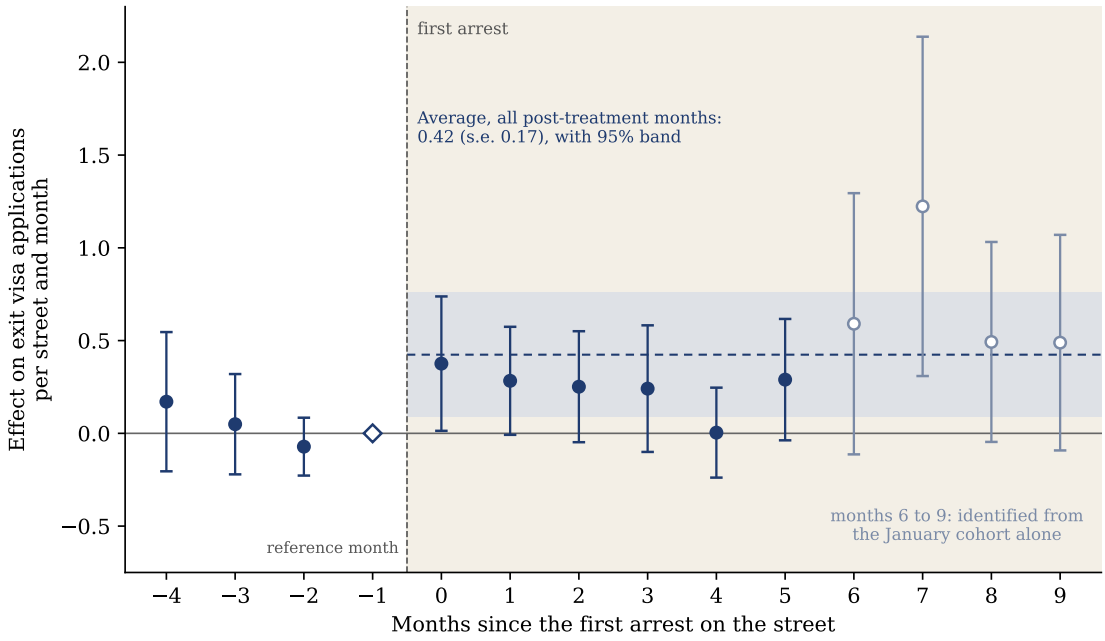
Notes: Each marker is the centroid of one of the 654 streets of the estimation sample. Red crosses mark the 122 streets with an arrest of a resident in 1989 (panel a) and the streets with at least one recorded exit visa application (panel b); blue dots mark the other sample streets. The gray lines show the street grid of Leipzig as of 1989, excluding areas incorporated after 1989.

Figure A3: Streets with an Arrest and the Protest Venues



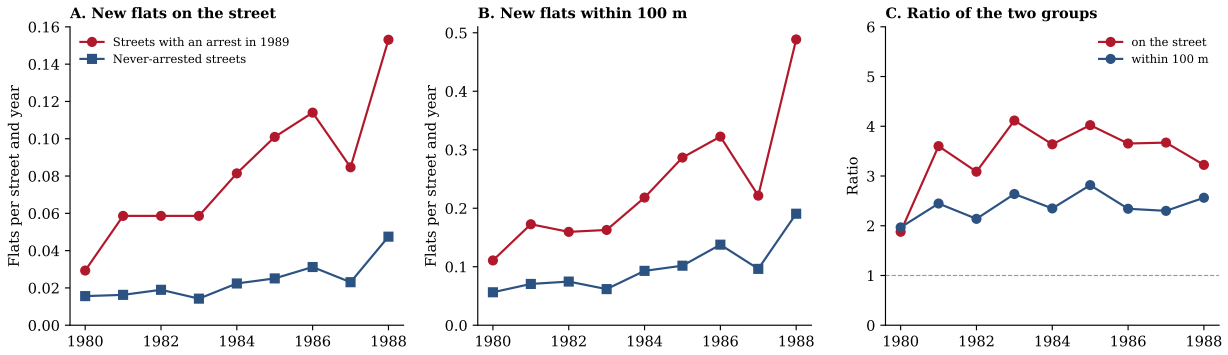
Notes: Streets with an arrest in 1989 (308 streets, red), other streets (1,530, gray), and the protest venues of November 1988 to October 1989, sized by the number of events and each surrounded by a circle of 1 km radius; a street inside a circle has a protest within 1 km. The venues, with the number of activities and, where the calendar reports one, the largest crowd, are 1 Nikolaikirche, 25 activities, up to 70,000 participants; 2 Markusgemeinde, 7; 3 Reformierte Kirche, 4, up to 300; 4 Michaeliskirche, 3; 5 Peterskirche, 3; 6 Thomaskirche, 3; 7 Leipziger Marktplatz, 2, up to 1,000; 8 Lukaskirche, 2; 9 Friedenskirche, 1; 10 Grimmaische Straße, 1; 11 Heilandskirche, 1; 12 Innenstadt, 1; 13 Liebfrauenkirche, 1; 14 Pleißemarsch, 1, up to 120; and 15 Straßenmusik in der Innenstadt, 1.

Figure A4: Exit Visa Applications around the First Arrest, Months −4 to +9



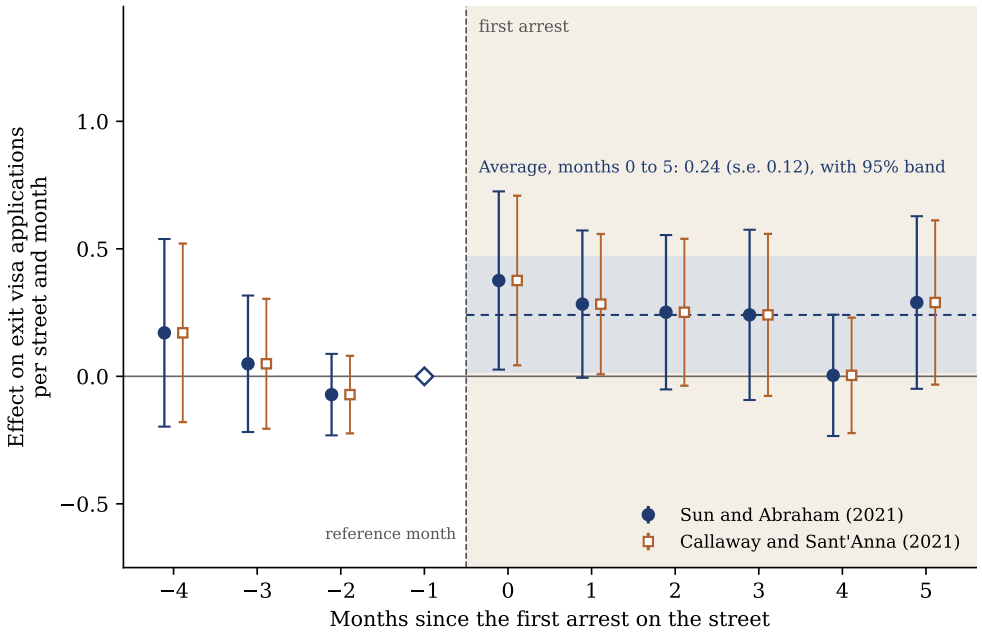
Notes: The event study of Figure 3 extended to month +9. Hollow circles mark months 6 to 9, which are identified from the 18 streets whose first arrest came in January 1989 alone. The dashed line and band are the average of all post-treatment coefficients, 0.42 (s.e. 0.17), with its 95% interval. Because later cohorts are observed for fewer months, the number of treated streets contributing to each coefficient falls from 122 in month 0 to 51 in month 5 and 18 in months 6 to 9.

Figure A5: New *Stasi* Conspiratorial Flats before 1989, by Later Arrest Status



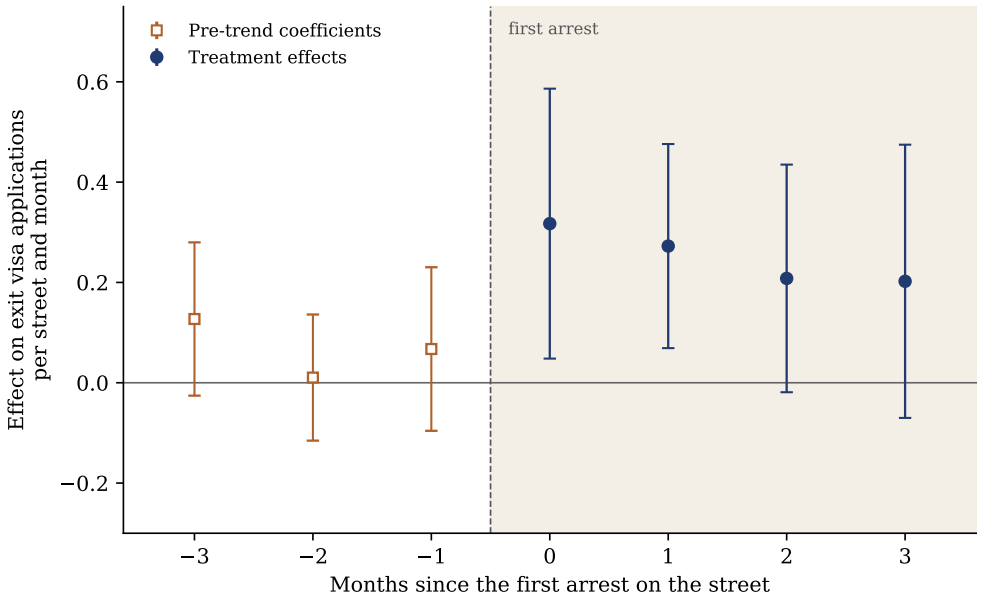
Notes: Conspiratorial flats (*konspirative Wohnungen*) set up in each year from 1980 to 1988, per street, from the Bürgerkomitee Leipzig's map of the 1,062 objects the *Stasi* maintained in the city. 986 carry the date on which they were set up, 591 of them in these nine years. A flat counts for a street when the nearest street line lies within 60 meters (panel A) or within 100 meters (panel B). The sample is the 1,781 streets of the 1988 directory with a street key and a positive resident count within 100 meters, 306 of which had a resident arrested in 1989. Panel C gives the ratio of the two group means. A Poisson regression of the count on the arrested indicator interacted with a linear trend, with year fixed effects and standard errors clustered by street, gives an interaction of +0.020 (s.e. 0.034) in panel A and +0.015 (0.019) in panel B against level gaps of 3.4 and 2.4; on the 654 streets of the estimation sample the interactions are −0.012 (0.046) and +0.026 (0.028). A linear regression with street and year fixed effects gives differential trends in levels of +0.009 (s.e. 0.002) and +0.022 (0.004) on all streets, and +0.005 (0.004) and +0.017 (0.007) on the 654 streets. The flats were secret and became known only after 1989.

Figure A6: Interaction-Weighted Estimator of Sun and Abraham (2021)



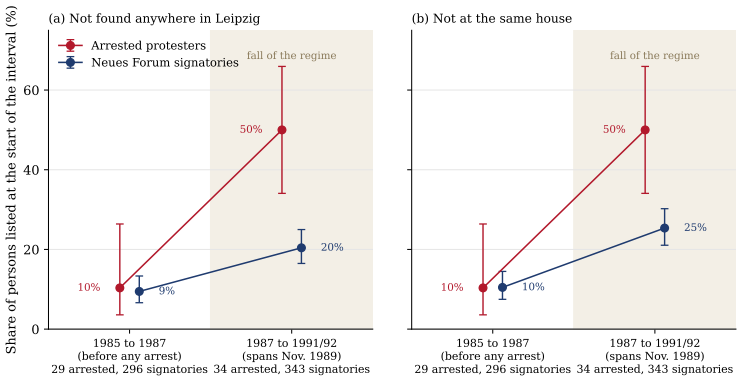
Notes: Event-time coefficients with 95 percent confidence intervals from the interaction-weighted estimator of Sun and Abraham (2021) (filled circles): a regression of applications on street and month fixed effects and a full set of cohort-by-event-time indicators, with the month before the first arrest omitted and the 532 never-arrested streets as the comparison group, whose cohort-specific coefficients are averaged at each event time with the cohorts' shares of treated streets as weights. Hollow squares repeat the Callaway–Sant'Anna estimates of Figure 3. With a never-treated comparison group and these weights the two estimators coincide, so the point estimates are identical; the standard errors, clustered by street, are about 5 percent larger here because the regression applies a small-sample correction. The dashed line and band give the Sun–Abraham average over months 0 to 5, 0.24 (s.e. 0.12).

Figure A7: Imputation Estimator of Borusyak, Jaravel, and Spiess (2023)



Notes: Event-study estimates from the imputation estimator of Borusyak et al. (2023), with street and zip-code-by-month fixed effects, never-treated streets as the comparison group, and 95 percent confidence intervals from standard errors clustered by street. Filled circles show the treatment effects in months 0 to 3 after the first arrest. Hollow squares show the pre-trend coefficients, which the estimator obtains from untreated observations; no month is normalized to zero. Joint tests: pre-trend coefficients $\chi^2(3) = 4.39$, $p = 0.222$; treatment effects $\chi^2(4) = 9.67$, $p = 0.046$.

Figure A8: Realized Migration of Arrested Protesters and *Neues Forum* Signatories



Notes: Share of arrested protesters and *Neues Forum* signatories who can be traced in the telephone book at the start of an interval, by exact name and address or by surname at the address, and who are no longer found anywhere in Leipzig at the end of the interval. The 1985–87 interval precedes any arrest; the 1987–91 interval contains the fall of the regime. The two intervals use the persons traceable at their start, given under each interval; bars are 95% Wilson intervals. Appendix Table A26 reports the underlying estimates.

C Eyewitness Accounts of Arrests

This appendix collects the eyewitness testimony summarized in Section 2. It illustrates three features of the arrests studied in this paper: (i) that they were carried out arbitrarily and in public, (ii) that detention could involve intimidation and physical abuse, and (iii) that the experience of arrest could change the behavior of those arrested and of the people around them. The accounts are selected and include episodes from Berlin and Dresden; they show what an arrest could entail, not how common each feature was across the Leipzig arrest days. None of this material enters the estimation; it is reported to convey the nature of the treatment.

Arrests on the street. When Leipzig authorities sought to disperse a non-authorized street music festival on 10 June 1989, they sent in 15 officers who picked up musicians seemingly at random and forced them onto a police truck.[29] This was not an isolated incident. Eyewitnesses routinely described the police behavior as "very brutal," as, for example, during the arrest of about 50 protesters during a demonstration on 11 September 1989 in front of Leipzig's main station.[30]

Further accounts of police brutality come from Berlin, which, as the East German capital, was another epicenter of anti-regime mobilization. One focal point was the 40th anniversary of the GDR on 7 October 1989. While the East German elite celebrated with Soviet leader Mikhail Gorbachev, Romanian President Nicolae Ceauşescu, PLO head Yasser Arafat, and numerous other national and international leaders, East Berliners took to the streets. The police response was as brutal as in Leipzig. Police forces usually did not distinguish between protesters and bystanders. One protester who ended up in detention, for example, draws attention to the many uninvolved individuals who were targeted. He recalls an elderly woman with a cane who was walking her dog. When she begged the police to stop the arrests, tears in her eyes, she was told to shut up if she did not want to end up on one of the trucks herself.[31] Angelika Otto was on her way back from a motorcycle ride with her partner Carsten when she got caught up in the demonstration. She saw a girl being beaten up by the police and simply wanted to help her. But, as she recalls, "I got beaten up myself. All I know is that I had bruised ribs [...] we ended up on a truck with a lot of other people who were also in a lot of pain."[32]

Detention. Arrested individuals often endured prolonged intimidation and mistreatment while in detention. Eyewitness accounts tell us how arrested protesters were crammed into overcrowded cells, threatened, harassed, and subjected to sleep deprivation and beatings—abuse and humiliation described as unprecedented by GDR standards. Some report how arrested protesters "were taken down to the basement, where Stasi officers interrogated them. Screams were heard from there, and some were bleeding when they were brought back to the corridor" (Bundeszentrale für Politische Bildung 2025b). Another eyewitness account recalls that "some of them were crammed into overcrowded cells (46 people in 16 square meters of floor space). [...] The citizens brought in, terrified to death along the way by their guards with sadistic remarks ('Now we're taking you to the garbage dump!') Every

[29] Source: https://www.archiv-buergerbewegung.de/archiv/interviewsammlung/aktionen-und-ereignisse/608-die-konsequenzen-sind-fuer-viele-hart-katrin-hattenhauer-leipzig-august-1989

[30] Source: https://www.jugendopposition.de/jugendopposition/material/564613/radio-glasnost-jochen-laessig-berichtet-von-der-montagsdemonstration-am-11-september-1989-in-leipzig/

[31] Source: https://www.stasi-mediathek.de/medien/gedaechtnisprotokolle-tage-und-naechte-nach-dem-7-oktober-1989-in-ost-berlin/blatt/50/

[32] Source: https://www.deutschlandfunkkultur.de/es-geschah-in-berlin-110.html

imaginable form of harassment was used: squats, hopping like rabbits through hallways and up stairs, sleep deprivation, complete stripping of the men in the hallway in the presence of women, standing in the airman's position, noise, and beatings turned the hours into an ordeal" (Bundeszentrale für Politische Bildung 2025a). Preventive arrests could last longer. The opposition activist Uwe Schwabe, detained at his home on 12 January 1989 with eleven others ahead of the demonstration of 15 January, was held for a week, his cell lit brightly every ten minutes, and interrogated at irregular hours, once for twelve hours (Schwabe 2025). Such preventive arrests are not part of our data, which cover arrests at protest locations.

Consequences for arrestees and their families. Historical accounts document how the experience of an arrest often changed the motivations and behaviors of arrested individuals and members of their broader social networks. Annegret Schowalter recalls how her husband returned "completely distraught" from an overnight detention, prompting her to attend the Monday Demonstration on 9 October 1989 without him.[33] Martin Dulig, in turn, realized that something had changed fundamentally when he learned about the 48 hours of physical and emotional abuse that his older brother had to endure following an arrest in nearby Dresden: "that's when I understood that now it's about something else," he remembers.[34]

[33]Source: https://www.zeitzeugen-portal.de/zeitraeume/jahrzehnte/1980/wir-sind-das-volk-leipzig-im-oktober/qZnwgOA4XFc

[34]Source: https://www.zeitzeugen-portal.de/zeitraeume/jahrzehnte/1980/wir-sind-das-volk-leipzig-im-oktober/IPO9VFSwJpM