

After the Genocide: Proximity to Victims and Support for Punishing Ingroup Crimes

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Abstract

What explains divergent transitional justice preferences among political elites after genocide? We argue that elite preferences vary with their proximity to the victimized group. Individuals who know the victims personally and/or have witnessed violence against them may be more likely to support punishing the perpetrators, possibly because they experience collective guilt. We support this argument using an original biographical dataset on the members of the West German parliament, linking their location and experiences during the Third Reich to free roll-call votes on extending the statute of limitations for murder in 1965-69. We find that proximity to synagogues, particularly those attacked in November 1938, predicts support for extending the statute, conditional on party, state, mandate type, denomination, and a host of personal attributes. We also find significantly lower support for extending the statute among former NSDAP members. Our findings highlight the importance of bystander experiences in shaping support for retributive justice.

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1 Introduction

Eight decades after the Holocaust, German prosecutors were racing against the clock to deliver justice to the former guards and administrators at concentration camps, now in their 90s. This seemed impossible after WWII when Germans perceived the Allied efforts to prosecute Nazi crimes as victor's justice (Art 2006, Ch.3). After West Germany regained sovereignty, it granted amnesty to Nazi-era officials and disregarded the Nuremberg legal precedent, treating Nazi perpetrators as regular criminals. War atrocities were thus subject to the statute of limitations, at 20 years for murder and 15 years for manslaughter. The overrepresentation of Nazi judges in courts ensured that Nazi criminals received lenient sentences or evaded justice altogether (Kern and Vanberg 2023).

The domestic consensus that Germans were themselves Nazi victims and did not deserve further punishment began to unravel only in the mid-1960s when the Frankfurt Auschwitz trial publicized gruesome details of Nazi extermination camps and drew attention to the pending expiration of the statute of limitations on murder. The Bundestag narrowly approved the statute's extension, first by five years (1965) and then by ten years (1969). In 1979, the statute was abolished altogether. The roll-call votes revealed considerable variation in support for extension within and across parties. Why did some parliamentarians push for punishing Nazi crimes at a time when transitional justice was deeply unpopular among voters? More broadly, what explains divergent transitional justice preferences among political elites in the aftermath of genocide?

In cases concerning moral or ethical issues, including transitional justice, legislators are often allowed a free vote. That is, party discipline is suspended and MPs are allowed to vote in line with their personal beliefs and values (Baumann et al. 2015).¹ We argue that in such relatively unconstrained situations, support for transitional justice is, in part, shaped by politicians' life experiences during the repressive period. We focus on one factor in particular:

proximity to the victimized group.

Bystanders' experiences during genocide vary dramatically based on the demographic composition of their locality. Those who live near the persecuted group are more likely to know the victims personally and to witness the full extent of the violence. Contact with the victims may promote empathy and neutralize state propaganda used to justify atrocities. Furthermore, exposure to victims' suffering may provoke collective guilt for failing to help or increase certainty in perpetrators' accountability. Through these related channels, proximity to victims is likely to strengthen support for punishing perpetrators. The opposite effect is also possible. Proximity to violence implicates by association and increases opportunities to benefit from genocide, which may reduce support for transitional justice.

To examine the role of bystander experiences during genocide we focus on West Germany, a paradigmatic case in transitional justice literature (Elster 2004, xi). We collected an original biographical dataset of MPs who voted on the statute of limitations, i.e. all members of the 4th and 5th Bundestag (1961 – 1969). For each MP, we establish whether his or her place of birth and location during the Third Reich had at least one synagogue, a proxy for the visibility of the Jewish community and anti-Semitic violence. We also code whether MPs were former NSDAP members, experienced Nazi repression personally, or fought in WWI/WWII.

We find that deputies born in places with a synagogue were about six percentage points more likely to support extending the limitations statute in both 1965 and 1969. Conversely, former NSDAP members – more than one-fifth of all deputies – were about 11 percentage points less likely to vote for the extension of the statute. We find similar results when we consider the presence of synagogues in an MP's location during 1933–1939 instead. We further dissect the mechanisms underlying our results by differentiating between synagogues that were attacked during the November 1938 pogroms and those that were not. We find that our results are driven by MPs who lived in places with attacked synagogues. These

additional results suggest that exposure to violence against the outgroup is a crucial mechanism driving our results that goes beyond knowing the victims personally. These results, obtained after controlling for party affiliation, type of mandate, state, and a host of demographic characteristics, are notable in light of the large literature that does not expect MPs' personal experiences and attributes to influence their legislative behavior (Sieberer 2010). We find that exposure to the Jewish community matters most for MPs who were not personally victimized or joined the NSDAP, i.e. can be considered bystanders to genocide rather than Nazi victims or perpetrators.

We find suggestive evidence that similar mechanisms apply among the general public, although here our results are more speculative given large-scale population movements in postwar Germany and the complexity of interpreting aggregate voting behavior. Using a difference-in-differences approach, we show that after the Auschwitz trials and the first Bundestag debate on the statute of limitations, parties that endorsed transitional justice gained electoral support in cities with synagogues before the Holocaust.

Our paper advances the scholarship on transitional justice preferences by focusing on political elites, who decide whether and how to deal with the difficult past and shape public opinion on transitional justice (Art 2006). To the best of our knowledge, this is the first study to investigate how transitional justice bills become law in the legislature by combining personal biographies and quantitative methods. We thus fill a significant gap in the existing literature that has focused predominantly on country-level processes leading to the adoption of transitional justice policies (Sikkink 2011; Subotic 2009; Hopgood 2013; Elster 2004) and on attitudes among the mass public (Balcells 2012; Hall et al. 2018; Aguilar et al. 2011; Daly 2018; Capoccia and Pop-Eleches 2020; Penic et al. 2018; Gibson 2004).

In showing that proximity to victims is important for understanding transitional justice preferences, we highlight the importance of ethnic context above and beyond individual status as a victim or perpetrator, emphasized in earlier research on transitional justice. We

thus contribute to the broader literature on how *local* experiences with genocide and other forms of mass violence shape political behavior and attitudes (e.g., [Charnysh 2015](#); [Charnysh and Finkel 2017](#); [Hadzic et al. 2017](#); [Penic et al. 2018](#); [Rozenas and Zhukov 2019](#); [Homola et al. 2020](#); [de Juan et al. 2023a,b](#)).²

2 Research on transitional justice after genocide

One of the key determinants of support for transitional justice is one’s status as a victim, perpetrator, or bystander. These categories are fluid, highly contested, and non-mutually exclusive: perpetrators often perceive themselves as victims; victims may become perpetrators when trying to survive; bystanders can join in with perpetrators or become victimized for failing to do so. Despite their overlap, these three categories offer a useful framework for understanding support for transitional justice in the aftermath of genocide and other forms of mass violence ([Vollhardt and Bilewicz 2013](#); [Ehrenreich and Cole 2005](#); [Hilberg 1992](#)).

Personal and family victimization generally increases support for transitional justice policies, particularly those of a more retributive nature. For example, [Aguilar et al. \(2011\)](#) show that victims of the civil war and Franco dictatorship in Spain as well as their descendants are more likely to support trials (also see [Balcells \(2012\)](#)). Victims exposed to more serious crimes are more likely to endorse retributive measures that lead to harsh punishments and to oppose reconciliatory measures, such as forgiveness for perpetrators who come forward, as shown in contexts as different as post-war Burundi ([Samii 2013](#)) and Bosnia ([Hall et al. 2018](#)).

Conversely, perpetrators are rarely willing participants in transitional justice processes. In a study of Colombian ex-paramilitaries, [Daly \(2018\)](#) shows that perpetrators’ support for transitional justice decreases with the extent of their involvement in the victimization of civilians and their proximity to places where violent acts were committed. However, there

is some correlational evidence that members of the perpetrator group are more willing to acknowledge past wrongdoing if they have greater *post-conflict* interaction with members of the victimized group. In postwar Bosnia and Herzegovina, Serbian adolescents were more likely to acknowledge ingroup responsibility for the violence if they had friends from other ethnic groups (Čehajić and Brown 2010). In South Africa, whites who had greater contact with black South Africans were more likely to accept the truth about apartheid and reconcile (Gibson 2004, Ch.4). Relatedly, Bosnian victims who shared social ties with and lived near perpetrators were less likely to support harsh punishments and more likely to endorse restorative justice (Hall et al. 2018).

Attitudes toward transitional justice among bystanders have received the least scholarly attention, even though this category is the most numerous. Bystanders include not only individuals who are “physically present during genocide, but also distant spectators” in the same country or abroad (Vollhardt and Bilewicz 2013, 7). Ehrenreich and Cole (2005, 217) define bystanders as people “who are not directly involved in the destruction process but who belong to either the same ethnic group as the perpetrators or an ethnic group that perpetrators view as closely related and readily accepted.” Thus, for bystanders, perpetrators are typically ingroup members while victims are outgroup members.

Bystanders’ positionality vis-à-vis victims and perpetrators produces cross-pressures on bystander preferences. Bystanders may feel some degree of personal responsibility for the violence due to their ethnic ties with the perpetrators and (theoretical) ability to help the victims (Lickel et al. 2004; Doosje et al. 1998). They may endorse retributive justice as a way of setting themselves apart from the perpetrators and thus improving their reputation (van Prooijen 2009, 2013). At the same time, shared ingroup ties may bias bystanders in perpetrators’ favor when punishment is being considered, particularly when perpetrators’ guilt is uncertain (van Prooijen 2013). Bystanders may therefore support more lenient measures or oppose transitional justice altogether. For instance, Capoccia and Pop-Eleches (2020) argue that the West German public rejected imposing severe punishment on Nazi

defendants as they perceived them as members of their own group, whose guilt was a matter of debate, considering the lack of viable alternatives for noncompliance during the Nazi era.

3 Theoretical framework

We theorize that bystanders’ support for retributive measures against perpetrators of mass violence depends on whether the victimized group is present in their community. There are several distinct ways in which proximity to the victimized group matters.

First, individuals who live in places where the victimized group is sufficiently numerous and socially integrated are more likely to know the victims personally. Studies show that interpersonal contact with the victims increases empathy and facilitates perspective-taking (Pettigrew and Tropp 2008).³ Relatedly, individuals who live next to victims are less susceptible to state propaganda and have greater awareness of the victims’ innocence and perpetrators’ responsibility than individuals whose main source of information is state media. Genocidal regimes often frame the targets of violence as enemies in order to secure the approval of their domestic audience and deter criticism (Josua 2022). State propaganda will be less effective among individuals who have first-hand contact with the victimized population. In the aftermath of genocide, individuals who feel more empathy for the victims and/or harbor less prejudice against victims will be more likely to support retributive measures against perpetrators.

Second, individuals who live next to victims are more likely to witness violence and realize the extent of the damage inflicted. As a result, they may experience shame or guilt for not doing more to protect the victims. This is particularly likely when bystanders share group identity with the perpetrators, as is often the case in genocide cases. For instance, Penic et al. (2018, 146) argue that proximity to the victimized population can be “the only systematic source of critical knowledge [...] about events of in-group perpetration and out-

group suffering.” In a representative survey across former Yugoslavia, they find stronger collective guilt in ethnically heterogeneous places, where individuals learned about outgroup suffering and ingroup atrocities, than in homogeneous places. Research in social psychology further shows that the level of moral outrage predicts support for harsher punishment among bystanders (e.g., [Darley 2009](#); [Carlsmith 2006](#)).

Direct exposure to violence also increases certainty about perpetrators’ accountability. As argued by [Capoccia and Pop-Eleches \(2020, 401\)](#), bystanders’ “assessment of the proportionality of punishment is highly sensitive to the contextual circumstances in which the offense was perpetrated.” Bystanders who live next to the victimized population have a clearer idea of who is to blame. When bystanders are more certain of ingroup members’ blameworthiness, they support harsher punishments ([van Prooijen 2009, 2013](#)).

These four related mechanisms – familiarity with the victims, a more positive view of the victims, the feelings of guilt or shame in the face of violence, and certainty about perpetrators’ guilt – suggest that exposure to the victimized group will increase support for retributive justice among bystanders.

At the same time, proximity to the victims invariably implicates bystanders and carries assumptions about culpability. Exposure to the Holocaust has indirectly benefited the gentile population in ethnically mixed communities across Europe ([Aly 2006](#); [Dean 2008](#)) and sometimes bystanders joined in the pogroms against their Jewish neighbors ([Kopstein and Wittenberg 2018](#)). These experiences can undermine support for retributive justice through the psychological mechanism of cognitive dissonance ([Festinger 1957](#); [Davis and Jones 1960](#)). This reaction is particularly likely when perpetrators are perceived as ingroup members. As a result, proximity to victims during genocide may lead bystanders to downplay the severity of crimes or to denigrate the victims to avoid mental discomfort and preserve a positive group identity ([Branscombe and Miron 2004](#), [Branscombe et al. 2007](#)). These related processes could produce opposition to retributive justice.

In line with this discussion, we derive several competing predictions about the effect of proximity to the victimized group on support for punishing perpetrators among bystanders:

- H_{1a} : Support for transitional justice will increase with contact with the victims.
- H_{1b} : Support for transitional justice will increase with exposure to violence against the victims.
- H_{2a} : Support for transitional justice will decrease with contact with the victims.
- H_{2b} : Support for transitional justice will decrease with exposure to violence against the victims.

We also consider a null hypothesis (H_0) that proximity to the victims will have no influence on support for transitional justice among bystanders and a related hypothesis (H_3) that only personal status as a victim or perpetrator matters.

4 Context: From the Third Reich to the Auschwitz Trial

4.1 Jews and Germans in Nazi Germany

One of the key sources of variation in how non-Jewish Germans experienced the Nazi regime during the 1930s was the presence and visibility of Jews in their locality.⁴ At less than 1% of the population in 1933, Jews lived predominantly in urban areas, scattered around some 2,000 towns of different sizes. They were organized into more than 1,600 official communities (*Gemeinde*), which levied taxes to fund synagogues, schools, and charitable activities. Despite their small number, Jews were highly visible because of their predominance in commerce, and in the free professions such as medicine, law, and journalism. Before 1933,

everyday contact between Jews and non-Jewish Germans was extensive and intermarriage was common.

After Adolf Hitler's appointment as chancellor in January 1933, Jews were gradually stripped of all legal rights and excluded from the public sphere. Contact between 'Jews' and 'Aryans' dwindled. As Rabbie Joachim Prinz observed in his diary in 1935, "the Jew" no longer had neighbors "to observe his troubles and strivings" (Morina 2019, 148-49). Stormtroopers and the SS organized multiple boycotts of Jewish businesses, put up anti-Semitic street signs, and desecrated cemeteries. In 1935, the Nuremberg laws were enacted to further marginalize Jews, Roma, and other so-called 'non-Aryan' groups. After 1935, Jews were barred from living in large municipal housing projects and after 1939 they were concentrated in specific buildings, which further limited contact with non-Jewish Germans (Maurer 2005, Ch. 19). Jewish property was "Aryanized," that is, confiscated.

In November 1938, the Nazi regime set off a coordinated wave of pogroms across the country. During what was later euphemistically called 'Kristallnacht' ('Night of the Broken Glass'), Nazi paramilitaries destroyed 929 synagogues, 7,500 Jewish businesses, and deported up to 30,000 Jewish males to concentration camps (United States Holocaust Memorial Museum 2019). Before the synagogues were destroyed, they served as sites of Jewish humiliation and suffering. For example, in Baden-Baden, in the morning of the pogrom, the SS herded a group of Jews into the synagogue, forcing them to read a passage from Hitler's *Mein Kampf* and to sing the Nazi anthem, as onlookers watched (Kreutzmüller 2019, 135).

Growing repression accelerated Jewish emigration. The size of the Jewish minority dropped from 522,000 people in 1933 to just 214,000 in 1939.⁵ Smaller Jewish communities disappeared as Jews migrated to be near larger communities for security reasons (Maurer 2005, 274-76). In September 1941, Jews were required to wear a yellow star and subjected to forced labor. By May 1943, the remaining Jews were deported to Theresienstadt and Auschwitz, and the Reich was declared "free of Jews" (*'judenrein'*). Altogether, about six

million Jews were murdered in the Holocaust, including about 165,000 German Jews.

Because Jews were so few in number and increasingly segregated and concentrated, most Germans obtained information about them from the Nazi-controlled media. Nazi propaganda blamed Jews for Germany's economic crisis and defeat in WWI (Gellately 2001, 6-7). Anti-Semitic policies were covered in misleading ways, if at all. For example, the arrests of Jews in November 1938 were framed as necessary to shield them from the angry German public. Photographers who tried to document the pogrom were stopped by the police, so most images of "Kristallnacht" capture only the aftermath, with no perpetrators or explicit violence in sight (Kreutzmüller 2019). The dissemination of alternative information beyond one's immediate social circle was challenging due to the strict penalties imposed by the 1934 Treachery Act, which aimed to suppress any negative news or criticism of the Nazi regime.

Germans who lived near active Jewish communities were more likely to know Jews personally and after 1933, to directly observe Nazi policies. According to historical sources, both active participation and open resistance to Nazi policies were rare (Evans 2005, 542). Some Germans benefited from the "Aryanization" policies by moving into Jewish apartments and taking over Jewish property, while others sought to help their Jewish friends and neighbors. Many distanced themselves from their Jewish contacts in public but expressed sympathy when interacting in a familiar place (Morina 2019, 157).

Germans who lived in a town with a synagogue could not help but witness the consequences of the November 1938 pogrom. This was "a radical consciousness-raising experience" because for the first time, Jews experienced outright violence and large-scale incarceration (Maurer 2005, 350). Such open violence reportedly produced "a deep shock" among bystanders (Bankier 1992; Panayi 2003, 86). Some "expressed criticism and spontaneous sympathy or even attempted to call for help or intervene themselves [but...] quickly withdrew [.. when it became] "clear that this was a Nazi Party action" (Maurer 2005, 352).

Eyewitness reports from the *Lebendiges Museum Online* (LEMO), an online portal on

German history, suggest that the spectacle of vandalized or burned synagogues provoked guilt and shame. Interestingly, some Germans mentioned being unaware of Jewish presence until after the pogrom. For example, Dorothea Günther (born in 1914) describes her visit to the town square after the November pogrom as follows: “We couldn’t believe what we were seeing: the destroyed and looted shops, inside the pale faces of the owners, if anyone showed up at all. Also in the shop we wanted to go to: smashed shop windows and a devastated interior. SA men stood guard in front of it, legs apart [...] It was actually *embarrassing* how little I had heard of the situation of the Jews up until then. I later went so far as to feel *guilty*. The critical thinking that I was proud of seemed to have totally failed” (emphasis added, [Günther 2010](#)). Wolfgang Findeisen (born 1926) recalled the November pogrom as follows: “The synagogues are burning. Many shops on Prager Strasse have signs saying “Aryanization in progress” - most of the time we *didn’t know* that these shops were owned by Jews” (emphasis added, [Findeisen 2000](#)). Another eyewitness, Cornelia Ziegler (born in 1922) observes that Germans who lived near Jewish communities were aware that the November pogrom did not erupt spontaneously, as claimed in the Nazi propaganda. Ziegler herself learned about the fate of Jewish families through family and school contacts ([Ziegler 2018](#)). The image of a burning and vandalized synagogue was mentioned by many others.

It thus appears that because the Jewish community was small, an average German became aware of the human consequences of Nazi policies after witnessing pogroms, particularly in November 1938. Exposure to violence provoked guilt and shame, though few intervened. Those who witnessed the pogroms had no doubt about the involvement of the Nazi party in organizing them. There is also some evidence that personal ties developed prior to Jewish exclusion from German institutions increased awareness about the persecution of Jews.

4.2 Transitional justice in postwar Germany

West Germany was slow to take responsibility for the Nazi past. The first transitional justice policies were imposed by the occupying powers and resented by the German population. In 1946, future chancellor Konrad Adenauer (Christian Democratic Union (CDU), 1949-1965) argued that it was time to stop punishing ordinary citizens for the crimes perpetrated by what he considered to be a small group of Nazi fanatics (Art 2006, 50). Shortly after Adenauer's election, amnesty laws were passed to overturn previous sentences. By the 1950s, West German elites converged on the position that Germans were themselves victims of the Nazi regime and that the main perpetrators were already punished (Art 2006, 53–57).

West German jurists – many of whom had been dedicated NSDAP members themselves — insisted on prosecuting Nazi perpetrators under the German penal code as common criminals, rejecting the Nuremberg precedent (Bazylar 2017). Nazi crimes were thus subject to a statute of limitations. The 1949 constitution set a 15-year limit for manslaughter and a 20-year limit for murder. In May 1960, Walter Menzel of the SPD filed a motion to extend the 15-year limit for manslaughter, but the legal committee of the Bundestag rejected the motion. In 1964, the government announced that it would not extend the statute of limitations on murder, set to expire on May 8, 1965, ostensibly to avoid the Nazi practice of making exceptions to legal principles (The New York Times r 12).

By the mid-1960s, however, the consensus on forgoing further trials began to unravel. An important milestone was the Frankfurt Auschwitz trial (1963-65), initiated by Hessian Attorney General Fritz Bauer, a Jewish émigré who fled Nazi Germany in 1936 and returned in 1949. The trial involved 22 former SS members who were involved in running the Auschwitz-Birkenau concentration camp. An outspoken critic of the statute of limitations, Bauer argued that because of the time constraints, only 22 out of 800 defendants under investigation were charged (Wittmann 2002, 353).

As new details about Nazi crimes came into focus, disagreements over how to deal with Nazi crimes emerged both across and within parties. While the opponents of extending the statute of limitations emphasized legal arguments, such as concerns about the retroactive application of justice and lower quality of evidence with the passage of time, the proponents framed transitional justice as a moral decision and emphasized guilt and accountability of all Germans for what had happened.

The SPD perceived continuing to prosecute Nazi criminals as an essential element of democratization. The party submitted motions to the Bundestag for extending the statute of limitations in both 1960 and 1965. Its leader Willy Brandt was close to Bauer and supported the Auschwitz trial. Unlike other parties, the SPD emphasized the collective guilt of all Germans for the Nazi crimes (Pendas et al. 2013, 202). Some SPD members appear to have been influenced by witnessing Jewish suffering in their locality. SPD deputy Adolf Arndt (born in 1904), closed his speech in favor of extending the statute of limitations before the 1965 Bundestag vote with the following reflection:

Finally, I want to share a personal memory (...) I consider myself *guilty*. Because I did not go out on the streets and scream when I saw Jews being deported from our community with transport wagons. I did not put on the yellow star and yell: me too! (...) I cannot claim that I did enough. And I'm not sure if anyone can claim he did. But this legacy, this inheritance, does put *responsibility* on us" [emphasis added]. ⁶

The CDU/CSU was split down the middle. Its liberal wing agreed that all Germans were complicit in Nazi atrocities and endorsed extending the statute of limitations for political and moral reasons. For instance, Ernst Benda argued that "[a] people's sense of justice would be corrupted if the murders had to go unpunished."⁷ The party's conservative wing emphasized individual conceptions of guilt and the rule of law instead. MP Rainer Barzel that the "German people did not become collectively guilty" (Ibid). In a letter to *Die Zeit*,

Walter Gaßmann, a former NSDAP member, claimed that ordinary Germans did not know about – and thus were not complicit in – atrocities against Jews in the east.⁸

Most members of the liberal Free Democratic Party (*Freie Demokratische Partei, FDP*), the CDU/CSU’s coalition partner, disagreed with collective responsibility for the Nazi past and advocated amnesty for war criminals (Pendas et al. 2013, 187). They claimed that extending the statute of limitations on murder would undermine public faith in the rule of law (Sharples 2004). In the interview with *Der Spiegel* in 1965, the FDP’s Ewald Bucher, then the Minister of Justice, explained that Germans “must be prepared if necessary to live with a few murderers” in a rule-of-law state (Sharples 2004, 85). However, a handful of FDP politicians ended up supporting transitional justice.

The strongest opposition to prosecuting Nazi crimes came from the far right, represented by the German Reich Party (*Deutsche Reichspartei, DRP*) and its successor, the National-Democratic Party of Germany (*Nationaldemokratische Partei Deutschlands, NPD*), founded in 1964. The NPD viewed the Auschwitz trial as “national masochism” and argued that Germans were already demoralized and robbed of national pride (Pendas et al. 2013, 200).

In March 1965, the Bundestag held a roll-call vote on extending the statute of limitations.⁹ At the time of the vote, the majority of Germans (57%) opposed further trials (fn. 1068 in Pendas 2013). Although German MPs typically follow the (formally nonbinding) recommendations by their respective party leaders in roll call votes, in this case, they were free to vote according to their conscience (‘*Gewissensentscheidung*’).¹⁰ They voted 344 to 96 to extend the limit by four years, to December 31, 1969. This was a compromise, justified as resetting the clock to 20 years since the formation of the Federal Republic in 1949, rather than since the end of WWII in 1945. Four years later, in June 1969, the Bundestag held another vote on the limitations statute. As explained by Dr. Adolf Süsterhenn (CDU), “In questions that touch on the conscience – and this really is a question of conscience of the first order – only one thing is necessary: that everyone remains free in his decision and that

everyone respects the reasoned opinion of the other.”¹¹ This time the MPs voted 279 to 126 in favor of extending the Statute for an additional ten years. The issue would be revisited again in 1979 when all time limits on murder would be abolished.

Table 1: Summary of free roll-call votes by party for transitional justice legislation.

Statute of limitations 1965: extend by four years				
Party	Yes	No	Abstained	Total MPs
CDU/CSU	180 (72%)	37 (14.8%)	2 (0.8%)	250
SPD	177 (87.2%)	0 (0%)	1 (0.5%)	203
FDP	4 (6%)	59 (88.1%)	0 (0%)	67
Statute of limitations 1969: extend by ten years				
Party	Yes	No	Abstained	Total MPs
CDU/CSU	107 (42.3%)	91 (36%)	3 (1.2%)	253
SPD	188 (85.8%)	2 (0.9%)	1 (0.5%)	219
FDP	0 (0%)	35 (70%)	1 (2%)	50

Note: Some MPs did not formally abstain but were not present during the vote, which included those who were sick or absent for other reasons. Percentages are calculated as a share of all MPs, including absentees. The far-right parties did not secure Bundestag seats. The party labels refer to the center-right Christian Democratic Union/Christian Social Union (CDU/CSU), the liberal Free Democratic Party (FDP), and the center-left Social Democratic Party of Germany (SPD).

As shown in Table 1, there were significant differences of opinion *within* parties, and only the SPD consistently supported abolishing limits on prosecuting Nazi crimes. Why did some MPs vote in favor of the statute and others against it? We argue that because party discipline was suspended, MP’s personal convictions – based on their life experiences before and during the Third Reich – influenced their vote.

A growing number of studies use a personal biography approach to show that elites’ personal attributes and life experiences affect political outcomes in systematic, predictable ways (Krcmaric et al. 2020). This work has highlighted the relevance of ascriptive characteristics such as gender, ethnicity, and religion, as well as of socializing life experiences including military experience, childhood trauma, poverty, divorce, and having daughters (Stam et al. 2015; Washington 2008; Meyersson 2014). Elite biographies matter most when elites have

a high level of autonomy and are not constrained. In the German context, [Baumann et al. \(2015\)](#) shows that MPs’ characteristics influence voting and bill sponsorship on moral issues.

We use data on MPs’ biographies to understand their experiences with the Nazi regime as bystanders, victims, or perpetrators. We are particularly interested in their exposure to the Jewish minority. MP’s biographies sometimes mention such encounters explicitly. For example, Walter Menzel (SPD deputy 1949–1963), worked as a district administrator in Berlin during the Nazi period, administering the assets of many Jewish emigres ([Vierhaus 2002](#)). Fritz Sanger (SPD deputy in 1961–1969) worked as a stenographer in Berlin and helped many Jews by hiding them or organizing their escape ([Vierhaus 2002](#)). J. Hermann Siemer (CDU deputy in 1953–1972) lived in the same building as a Jewish ophthalmologist in the 1930s ([Klausch 2008](#)). Furthermore, MPs occasionally referred to personal experiences when justifying their vote. For instance, Adolf Arndt (SPD, quoted above) mentioned witnessing deportations of Jews (see above). Ferdinand Friedensburg (CDU deputy in 1952–1965), referred to his own imprisonment by the Gestapo.¹² We discuss how we operationalize relevant experiences in the next section.

5 Measurement and Empirical Strategy

5.1 Outcome: attitudes towards transitional justice bills

We measure MPs’ support for transitional justice using the roll call votes for the statute of limitations extension bills. We use the data collected by [Sieberer et al. \(2020\)](#) as well as archival records from the 243rd session of the 5th Bundestag (June 26, 1969) to create this variable.¹³ As outlined in Section 4, these were free votes, which meant that MPs were not bound to the party line but were encouraged to follow their conscience. This suspension of party discipline produced considerable within-party variation in voting behavior, especially within the CDU/CSU, nearly evenly split on the statute in 1969 (Table 1).

5.2 Information about MPs’ characteristics and experiences

We combine the roll call voting data with detailed biographical information on each MP’s experience during the ‘Third Reich’ coded manually based on the three-volume *Biographisches Handbuch der Mitglieder des Deutschen Bundestages: 1949–2002* (2002) and online sources.¹⁴ Table 2 lists our main variables. We present summary statistics in Table A.1.¹⁵

Table 2: Overview of biography covariates

Variable	CDU/CSU	FDP	SPD
Synagogue in hometown	61.13	67.57	72.06
Synagogue in 1933–1939 location	76.00	84.06	81.25
NSDAP member	24.58	45.95	10.93
WW1 Veteran	12.29	12.16	7.29
WW2 Veteran	59.80	79.73	61.13
WW2 POW	31.89	40.54	33.20
Soviet POW	3.32	5.41	3.64
Repressed or in exile during WW2	9.30	6.76	25.51
Resistance against Nazis	1.00	0.00	7.29
Catholic	60.47	17.57	18.22

The Table shows the share of MPs with a given characteristic by party (in %).

Exposure to the victimized group: We operationalize our key explanatory variable, MPs’ exposure to the victimized community, as the presence of at least one synagogue in an MP’s birth municipality (*Gemeinde*) in 1933 (see Figure 1).¹⁶ The data on synagogues and their destruction was compiled by Solomon (2021) based on the Synagogue Memorial Beth Ashkenaz project.

We also coded the presence of synagogues in MPs’ locations during the years 1933–39, when the Jewish community was persecuted. We found that some MPs moved away from places to work or study; some spent the 1930s in exile. Sometimes MPs were present in two places at once; we privileged their location during “Kristallnacht” where possible. If the biography did not mention alternative locations for MPs’ education, workplace, etc.

during the 1930s, we assumed that MPs stayed in their place of birth, except for cases where biographies suggested MPs moved but did not give precise location (location for 8.7% of MPs in our dataset is therefore coded NA). Because precisely establishing MPs' residence in the 1930s is harder than establishing MPs' birthplace, we treat the former as a supplementary variable.

We focus on synagogues for theoretical and practical reasons. Synagogues indicate the presence of a Jewish community (*Gemeinde*), a legal entity that organized Jewish communal life. While some Jews were not community members, the Jewish population was much more visible in places that had a Gemeinde and thus a synagogue. During the Third Reich, synagogues became even more important for the Jewish community as more Jews turned to religion or became dependent on the Gemeinde for social and economic needs (Maurer 2005, 324-26). For Germans, synagogues were a symbol of German Jewry, which is why a Hitler Youth song incited Germans to set synagogues on fire and why synagogues were frequently vandalized (Maurer 2005, 351). The image of a burning synagogue from the November 1938 pogrom would become a symbol of Jewish genocide after the war (Jacobs 2008). As discussed in Section 4.1, many Germans claimed to have been unaware of Jewish presence until they witnessed Nazi attacks against synagogues. Using data on synagogues also allows us to capture the variation in the visibility of Nazi violence by coding which synagogues were attacked and which were spared during "Kristallnacht".

In supplementary analysis (Table A.4), we use the Jewish population share in a given locality as an alternative indicator. This data was compiled by Buggle et al. (2023). As expected, there is a clear relationship between the presence of synagogues and the size of the Jewish population across localities: we estimate the average share of Jews in localities with a synagogue was about seven times higher compared to localities without a synagogue.¹⁷ We only observe Jewish population figures for localities within the borders of post-WWII Germany.

Although both measures – the presence of the synagogue and the share of Jews in the population – capture Jewish presence, they speak to different mechanisms. The share of Jews, particularly *before* the Third Reich, is better for assessing the role of contact between Jews and Germans. The larger the share of Jews, the greater probability of intergroup contact during the Weimar period, when Jews were well-integrated. By contrast, the presence of a synagogue is a better proxy for exposure to Nazi policies and violence against Jews. After 1933, Jews were increasingly segregated and banned from public places, so a larger share of Jews did not necessarily increase contact. Using the presence of a synagogue also reduces measurement error, given the high rates of post-1933 internal and international Jewish migration.

Thus, the assumptions behind our measurement strategy are that (1) MPs who lived in a place with a larger Jewish population would have been, on average, more likely to know at least one Jewish person than those who lived in places with fewer Jews and (2) MPs who lived in a place with a synagogue, particularly one attacked during "Kristallnacht," would have been more likely to observe Nazi violence against Jews. We acknowledge that neither measure is ideal as we cannot establish what the MPs witnessed directly. Although interviewing bystanders about their real-life experiences may reveal more detail, there is no guarantee that bystander accounts will be truthful and, in our case, bystanders are no longer alive.

We also collected data on additional MP characteristics highlighted as relevant by the literature on transitional justice and/or potentially correlated with exposure to Jewish victims.

NSDAP Membership: Approximately 20% of all MPs in the 4th and 5th Bundestag were former NSDAP members. A few served the Nazi regime in prominent positions. In line with other sources, we find that former NSDAP members were overrepresented in the FDP, at 46% of MPs, and the CDU/CSU, at 25% of all party MPs. By contrast, "only"

Figure 1: Synagogues in Germany 1933.



Note: The map shows the distribution of synagogues in 1933. The data was originally collected by [Solomon \(2021\)](#). See Figure A.1 in the appendix for a map of synagogues that were (71.9%) or were not (28.1%) attacked during "Kristallnacht".

11% of SPD MPs were former members of the NSDAP. These percentages are significantly higher than the 7% membership rate estimated for the adult population in 1939. Individuals who joined the NSDAP are arguably more complicit in Nazi violence than non-members. In line with research that links perpetrator status to reduced support for transitional justice, we expect former NSDAP members to oppose the extension of the statute of limitations. NSDAP membership may also be positively correlated with Jewish presence to the extent that it was more common for MPs from diverse urban areas. Omitting this variable from our analysis thus might lead us to overestimate the effect of proximity to the Jewish minority on support for transitional justice.

Nazi persecution: Some MPs were themselves victims of the Nazi regime, either because they opposed Nazi policies or because of their Jewish ancestry. This was often the case for (future) SPD deputies, several of whom were arrested and imprisoned in concentration camps (e.g., Fritz Steinhoff, Georg Stierle) for opposing the Nazi regime. In our dataset, 16% of MPs were repressed and/or spent (a part of) the Nazi period in exile, and 3% of MPs were involved in active resistance.¹⁸ We expect these victimized MPs to endorse transitional justice. Furthermore, because Nazi persecution is positively correlated with Jewish presence, failing to control for MPs' personal victimization status could bias the effect estimate for our main independent variable, exposure to the victimized group, upward.

Veteran status: We also consider the relevance of military service in WWII and captivity. The overwhelming majority (90%) of MPs were men, nearly two-thirds of whom fought in WWII. One-third endured captivity. Individuals drafted to fight in WWII were subjected to additional socialization into Nazi values, risked their lives for the 'Third Reich', and were more likely to participate in violence against civilians in occupied territories. This experience could reduce support for punishing Nazi crimes after the war. Some MPs became prisoners of war, experiencing years of captivity and forced labor. German POWs in the Soviet Union were treated particularly poorly.

Catholicism: We collected information about MPs' religion (Catholic, Protestant, Unaffiliated, or Other), an important determinant of Nazi support and exposure to violence. In particular, Catholics were less likely to vote for the NSDAP than Protestants, in part because the Catholic clergy criticized Hitler and the NSDAP (Spenkuch and Tillmann 2017). Starting in 1933, the NSDAP began to repress Catholic clergy, interfere with religious activities, and banned Catholic associations. Correspondingly, we expect Catholic MPs to be more supportive of punishing Nazi crimes than Protestant or unaffiliated MPs.¹⁹

Additional MP covariates: We also observe information about MPs’ gender and year of birth (Sieberer et al. 2020). We note that 90% of MPs who voted on the 1965/69 statute of limitations extension were born before 1926 and thus witnessed the Nazi regime and discrimination against Jews as adults. Because age is correlated with both exposure to the victimization of Jews and support for transitional justice in our setting, this is an important control variable. Figure A.2 presents a histogram of the year of birth of all MPs in our dataset.

Strategic voting incentives: Models of legislative voting behavior expect MPs to care about reelection and furthering their careers within the party, in addition to shaping policy in line with personal preferences. Germany has a mixed-member proportional representation system, with most candidates competing in single-member districts as well as running on party lists. The candidates who achieve a plurality and win district mandates (*Direktmandate*) have more incentives to appeal to their constituencies (Sieberer 2010). Relatedly, scholars of the Bundestag have argued that in a free vote, when party discipline does not apply, MPs cater to their constituencies because this increases their chances of reelection (Baumann et al. 2015). It is thus possible that MPs’ vote on the limitations statute reflects strategic considerations rather than beliefs about transitional justice shaped by Nazi-era experiences. To account for this, we control for (i) the type of mandate the MP holds (district vs. list), (ii) whether the MP ran as a dual candidate, and (iii) the closeness of the district race. These variables were coded by Sieberer et al. (2020).

5.3 Estimation

We analyze the roll call voting data using linear probability models of the following form:

$$Y_{icpst} = \alpha_p + \gamma_t + \theta_s + \tau T_c + \beta \mathbf{x}_i + \epsilon_{icpst}$$

where Y_{icpst} is a binary indicator that equals one for a given MP i of party p born in municipality c (nested in state s) if she voted ‘yes’ on the extension of the statute of limitations in election cycle t (1965 or 1969). Our outcome variable is coded as zero for MPs who explicitly abstained; MPs who did not cast a vote are excluded from the analysis.²⁰ T_c is a binary variable that indicates the presence of at least one synagogue in the birth municipality c of MP i as of 1933. For our main specification, we include fixed effects for the election period (i.e. bill) (γ_t), party (α_p), and state of birth of each individual MP (θ_s). Because we include fixed effects for the state in which MPs were born, we only leverage relatively local variation in exposure to Jews (e.g. within Bavaria); we do not compare across distant regions (e.g. between Bavaria and Prussia). We also include a series of individual-level covariates \mathbf{x}_i in the model, including all biography covariates listed in Table 2. In addition, we control for year of birth, gender, type of mandate, dual candidacy, and district closeness (see Section 5.2).²¹ Our main data set consists of 888 roll-call vote observations for MPs who voted on the statute of limitations bills in 1965 ($N = 460$) and 1969 ($N = 428$). 266 MPs were re-elected and voted on both bills. Our results are robust to including a dummy indicator for re-elected MPs in the estimation (Table A.6) and clustering standard errors by MP (Table A.7).

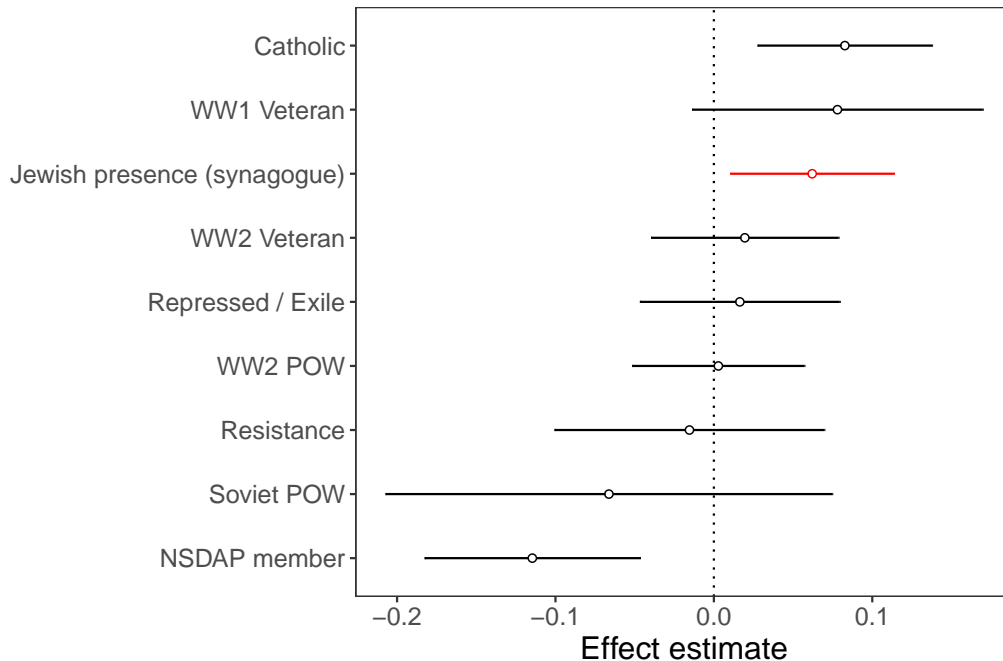
Beyond the control variables included in our models, the presence of a synagogue might be correlated with other latent determinants of MP voting behavior. It might be the case, for example, that Jews lived in localities with lower levels of anti-Semitism. Likewise, Jews were more likely to live in cities, which in turn could be correlated with MP ideology. We systematically address such concerns in Section 6.1.

6 Results: vote on extending the statute of limitations

We present our main results in Figure 2. Here we show estimates from our most demanding specification including party fixed effects, state fixed effects, and additional individual-level

controls (see Section 5.3). We examine the robustness of our results across a variety of alternative specifications in Table 3. Because all biography covariates and our outcome are binary, the coefficient estimates can be interpreted as expected changes in the probability to vote in favor of extending the statute of limitations for any given characteristic.

Figure 2: Main results: support for extending the statute of limitations



Notes: The figure shows OLS effect estimates for MP covariates. The specification includes party fixed effects, state fixed effects, and additional individual-level controls (see Section 5.3). Standard errors are clustered by municipality/birthplace. Error bars indicate 95% confidence intervals.

Three key predictors of support for extending the limitations statute stand out. First, among all variables we consider, prior NSDAP membership is by far the strongest negative predictor, decreasing the probability to vote for the extension by 11 percentage points in our preferred specification (Model 6, Table 3). This finding is consistent with lower support for transitional justice among individuals who are more implicated in crimes of the old regime.

Second, we find that MPs born in localities with a synagogue are substantially more likely to support extending the statute. Based on our preferred specification, we estimate that local exposure to Jews increased the probability to vote in favor of extension by about

Table 3: Exposure to victims and support for transitional justice. Robustness.

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.100*** (0.038)	0.097*** (0.030)	0.059** (0.027)	0.087** (0.035)	0.096*** (0.028)	0.062** (0.026)
NSDAP member	−0.289*** (0.045)	−0.106*** (0.036)	−0.106*** (0.036)	−0.252*** (0.044)	−0.112*** (0.036)	−0.115*** (0.035)
Catholic	0.004 (0.035)	0.063** (0.029)	0.065** (0.027)	0.023 (0.040)	0.091*** (0.029)	0.083*** (0.028)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
DV mean	0.74	0.74	0.74	0.74	0.74	0.74
DV s.d.	0.44	0.44	0.44	0.44	0.44	0.44
N	888	888	888	888	888	888

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The units of observation are individual MPs. Standard errors clustered by municipality/birthplace are shown in parentheses below the respective point estimates. ***p < .01; **p < .05; *p < .1

six percentage points (Model 6, Table 3). These results remain unchanged when we subset to MPs from the CDU/CSU – the faction with the highest degree of within-party variation (see Table A.8) or exclude expellees (see Table A.9).

Third, Catholic MPs are more likely to support extending the statute of limitations, as expected. The estimated coefficient is at eight percentage points in the most demanding specification (Model 6, Table 3). Because Catholics also experienced Nazi repression, though to a much lesser extent than Jews, this finding indirectly supports the role of ingroup victimization in shaping opinions about retributive justice (Balcells 2012; Aguilar et al. 2011).

Other aspects of individual experience during the Nazi era do not predict MPs' voting behavior. MPs who were in exile during the war, repressed by the Nazis, or active members of the resistance were not significantly more likely to vote for the transitional justice bills. Note that these null findings might be explained by the fact that most variation in these variables is soaked up by party fixed effects: nearly all MPs whom we identified as members

of the resistance, for example, were SPD members (see Table 2). At the same time, the SPD almost unanimously supported the extension of the limitations statute (see Table 1). We likewise do not find statistically significant evidence that either WWI or WWII frontline experiences shaped MP attitudes.

6.1 Threats to inference & robustness

In this section, we systematically examine a series of alternative explanations. Specifically, we provide evidence that (i) our results are robust to alternative measures of Jewish presence; (ii) our results are likely not driven by urban-rural differences, (iii) localities with (without) synagogues did not differ in their voting behavior – including support for the Nazi Party – prior to WWII, (iv) our results are not reducible to voter demands, and (v) MPs from localities with (without) synagogues do not diverge in their roll call voting behavior on bills that do not relate to the Nazi past.

First, we use data on the share of Jews in MPs’ hometowns as an alternative indicator of exposure to the victimized group. Results are presented in Table A.4 and lead to similar conclusions. MPs born in places with a larger Jewish community are more likely to support the extension of the statute of limitations. Substantively, a one-standard-deviation increase in Jewish share predicts a 6.2% increase in the probability of supporting the extension (i.e. similar effect size magnitude as for our main treatment variable). We also consider whether MPs moved, recoding our main explanatory variable based on the presence of a synagogue in MPs’ location in 1933-39. We again find similar results, as shown in Tables A.10. The similarity between the results for hometown locations and ”Kristallnacht” locations is likely because relatively few MPs (about 20%) move from places with synagogues to places without synagogues or vice versa.

Second, Jewish communities were not randomly distributed. Jews were clustered in large cities such as Berlin and Frankfurt (see Table A.2 for the number of synagogues by

city). Urban-rural differences, in turn, might be correlated with attitudes toward transitional justice. To address this concern, we conducted three tests. We demonstrate in Table A.11 that our results are robust to controlling for population size. In addition, we show that our results remain unchanged when we exclude the largest cities with respect to population size (Table A.12) or the cities with the highest density of synagogues (Table A.13). Finally, we conduct a jackknife analysis and demonstrate that our results are robust to sequentially dropping individual cities from the sample (see Figure A.3). While our results hold across all of these additional specifications, we acknowledge that urban-rural differences are difficult to rule out beyond doubt.

Third, we further test for systematic differences in ideology between localities with (without) synagogues by examining voting behavior in the 1930s. Perhaps the biggest concern in this regard is that Jewish (synagogue) presence correlates with Nazi support. Jews may have been more likely to settle in places that were less anti-Semitic. If this were the case, we would see systematically lower levels of support for the NSDAP in localities with synagogues during the Weimar period. To test this empirical implication, we draw on county-level voting data for the November 1932 German federal election – the last free and fair election before WWII (de Juan et al. 2023b).²² The results presented in Table A.14 show that synagogue presence does not predict electoral support for the Nazi Party, the Social Democrats, or the Communist Party (KPD). Across specifications and parties, the effect estimates are not statistically significant and small in magnitude. While the Jewish population was not evenly distributed across Germany, these settlement patterns were uncorrelated with pre-WWII voting behavior.

Fourth, we explore whether the patterns in MP voting could be driven by the constituent preferences rather than the MPs’ own views. To examine this possibility, we reconstructed the borders of Germany’s electoral districts (*Wahlkreise*) for the 1961 and 1965 federal elections based on maps published by Statistisches Bundesamt (1965) and matched the birthplaces of MPs holding district mandates to their electoral districts. We find that only

about one-third of district MPs were born in the district they are representing. Next, we re-run our analysis for the subset of district MPs and add the number of synagogues in each MP’s district as an additional predictor (see Table A.15).²³ The effect estimate for this variable is small in magnitude and does not reach conventional levels of statistical significance. By contrast, the estimate for synagogue presence in an MP’s birthplace doubles in magnitude when controlling for district characteristics and subsetting to district MPs. These results align with our hypothesis that MP votes on the statute of limitations were influenced by personal experiences.

Finally, we conduct a placebo analysis to scrutinize the possibility that MPs born in towns with Jewish communities vary in their propensity to deviate from the majority position of their party. If this were the case, we would also expect to see systematic differences in MPs’ voting behavior on bills unrelated to the Nazi past. Following this logic, we conduct a placebo test using all other roll call votes during our study period. In total, we observe more than 31,000 votes by individual MPs on a total of 60 bills during our study period 1961–1969 (Sieberer 2010). For each bill, we estimate analogous specifications as for our main results including party fixed effects, state fixed effects, and MP-covariates. Our outcome variable is a binary indicator that equals one for MPs who deviated from their respective party line for a given bill (see Section A.1.3 for more details). We present the distribution of test statistics across all placebo votes in Figure A.5 in the SI. As we would expect under the null hypothesis, we find that this distribution is centered around zero. The median test statistic across bills is ≈ 0 (solid line in Figure A.5). We only find statistically significant effect estimates (t-statistic above 1.96 or below -1.96) for 5% of the bills in our sample. Reassuringly, we thus do not find evidence for systematic differences in deviant voting behavior between MPs with/without pre-war exposure to Jews with respect to issue areas that do not directly relate to the Nazi past.

6.2 Exploring the mechanisms

In this section, we conduct additional analyses to explore the mechanisms underlying our main finding – higher support for extending the statute of limitations among MPs exposed to the Jewish minority before the Holocaust.

Exposure to the victimized group measured by the presence of a synagogue is a bundled treatment: MPs from towns with a synagogue had greater contact with the Jewish minority and witnessed greater violence following the Nazi takeover. We can get additional leverage on this using data on the destruction of synagogues during “Kristallnacht” (Solomon 2021), which appears to have left a particularly vivid impression on German bystanders (see Section 4.1). While the majority of municipalities with a synagogue experienced an attack, some synagogues did not (see Figure A.1). This variation in violence across synagogues enables us to disaggregate our treatment variable into (i) *synagogue present but not attacked* and (ii) *synagogue present and attacked*. We combine this with hand-coded information on MPs’ location during the years 1933 – 1939 (see also Table A.10). In other words, we distinguish between MPs who – during the Nazi era – lived in places with a Jewish community that did (did not) experience a synagogue attack during ‘Kristallnacht’.

The results of this analysis are presented in Table 4. We find a positive, statistically significant effect of living in a locality with synagogue attacks during the Nazi period on support for the statute of limitations extension in the 1960s. In contrast, we do not find similar effects for MPs who lived in places where a Jewish minority existed, but the synagogue was not attacked. We interpret this as suggestive evidence that witnessing anti-Semitic violence is a crucial mechanism underlying our results.

As highlighted in Section 5.2, the share of Jews in the population may be a better proxy for intergroup contact before 1933, whereas the presence of a synagogue captures the visibility of the Jewish community and, after 1933, Nazi violence against Jews. The two

Table 4: Synagogue attacks and support for transitional justice.

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Synagogue: attacked	0.117** (0.049)	0.102*** (0.034)	0.066* (0.034)	0.097** (0.044)	0.098*** (0.032)	0.069** (0.033)
Synagogue: not attacked	0.027 (0.094)	0.044 (0.053)	0.002 (0.052)	−0.023 (0.087)	0.033 (0.055)	−0.008 (0.053)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	810	810	810	810	810	810

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We code whether MPs witnessed synagogue attacks based on their location in 1933 – 1939 (see also Table A.10). The unit of observation is individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

measures are correlated, and in Section 6.1 we show that our results are robust to using either. To further probe the distinction between contact with Jews and witnessing violence against them, we include both the share of the Jewish population and dummy for attacked synagogues in the same regression model. Results in Table A.20 again suggest that violence is a more likely mechanism: the coefficient on the share of Jews is not significant and extremely small. An important caveat when interpreting these results is that even when the synagogue was not attacked during the November pogrom, the local Jewish community experienced various forms of violence both during and after “Kristallnacht”; furthermore, the attacks are endogenous to the size and visibility of the Jewish community (Solomon 2021).

Our analysis so far suggests that MPs voted differently because of their bystander experiences. This presumes that MPs were old enough to comprehend the Nazi treatment of Jews. For the 1965 and 1969 votes, the median year of birth is 1911, suggesting this is reasonable. As time passed, however, the number of MPs with such experiences dwindled. When the Bundestag returned to the issue of extending the statute of limitations in 1979, its members’ median year of birth was 1929. Many deputies had only fleeting experiences with Jews and

were educated in the Nazi system. Among this group, we expect being born in a place with a synagogue to be less predictive of views on transitional justice. This is indeed what we find when we examine the 1979 vote on abolishing the statute (see Table A.18). In the full sample, the coefficient estimate for synagogue presence is small and insignificant. When we divide the sample into MPs born before and after 1930, we find a positive effect estimate within the subset of older MPs with memories of the Third Reich. The effect estimate is of similar magnitude as for the 1965/69 votes and statistically significant when we focus on synagogues in MPs’ 1933–39 locations. The point estimate for the group of younger MPs – i.e. those MPs who had fewer experiences with the Jewish minority – is negative, small in magnitude, and not significant.

Our argument applies to bystanders, individuals who, on the one hand, shared German identity with the perpetrators, and on the other hand, were not directly involved in the genocidal violence. The bystander is a heterogeneous and fluid category, which is nevertheless distinct from that of victims and perpetrators. German MPs in the 1960s arguably played all three roles: most were clearly bystanders, but some were personally victimized by the Nazi regime while others joined the NSDAP and even held high-ranking positions during the Third Reich. In Table A.19 we perform analyses on these three subsets of MPs, demonstrating that our results are driven by MPs who fit best in the bystander category. The coefficient on synagogue presence increases and remains statistically significant in this subset, but does not reach significance within the other two groups.

6.3 Voting behavior after the Auschwitz trials

Our argument about the importance of exposure to the victimized group among political elites can be extended to the general public, as it does not depend on electoral incentives. We study this in the context of the Frankfurt Auschwitz trial (1963–65), which publicized gruesome details of Nazi extermination camps and drew attention to the pending expiration

of the limitations statute on murder. If proximity to Jewish communities increased empathy toward Jews and support for trying Nazi perpetrators, we would expect a disproportionate increase in support for the SPD – the party most critical of the Nazi regime and unequivocally supportive of transitional justice – in places with Jewish communities after the Auschwitz trials. Conversely, we expect to see a decrease in support for the FDP and NPD – the two parties most critical of transitional justice.

To test these hypotheses, we obtained county-level electoral returns for the West German federal elections in 1953, 1957, 1961, and 1965 from the Bundeswahlleiter. We estimate a series of difference-in-differences models to examine electoral shifts after the Auschwitz trials. We provide more details on this analysis and additional robustness checks we conducted in section A.2 in the SI. In line with our findings among Bundestag MPs, we find that after the Auschwitz trials, the SPD saw an increase in electoral support by approximately one percentage point in communities with a pre-war Jewish presence. The FDP, critical of transitional justice and advocating for amnesty for war criminals, lost about two percentage points in cities with a synagogue. Far-right parties (NPD/DRP) likewise experienced a decline in electoral support.

Overall, we interpret these findings as suggestive evidence that bystander experiences not only shifted preferences among German elites but the public at large. However, we emphasize that these findings should be viewed as suggestive only. In contrast to our main analysis of MPs, the analysis of voting behavior is based on ecological inference at a relatively high level of aggregation (county), which may lead to spurious results despite the use of panel data. Between the war and the 1960s, there were substantial population movements, which led to significant changes in the demographic composition of many cities. Furthermore, whereas MPs are public figures, whose biographies, speeches, and roll-call votes are written down, voters' experiences and views toward the past are harder to pin down using aggregate electoral and demographic indicators.

7 Discussion

Experiences with genocide and other forms of mass violence are inherently local and personal. Where individuals live affects how they perceive the victimization of others and what they think of transitional justice. We show that German MPs who had a synagogue in their locality were more likely to support the extension of the statute of limitations on murder. Living in a place with a synagogue increased the probability of witnessing Nazi violence against Jews, including against one’s neighbors and acquaintances. We theorize that the resulting feelings of guilt and shame for doing little in the face of violence as well as greater certainty in perpetrators’ accountability may have strengthened support for punishing Nazi crimes.

This is an important and surprising finding in light of other work on the legacies of violence, which indicates that bystander populations often react in the opposite way, becoming intolerant toward minorities and voting for parties that deny ingroup wrongdoing (e.g., Charnysh 2015; Homola et al. 2020; Hoerner et al. 2019). These studies use proximity to Nazi concentration and death camps created for Jews, Roma, and other marginalized groups by the Nazi regime as the ‘treatment’. It is possible that violence that occurred in concentration camps rather than on familiar streets and squares affects bystanders differently. When victims are confined and monitored, bystanders’ experiences are more superficial and do not involve direct contact. In such settings, bystanders have fewer opportunities to intervene and may be less likely to feel guilt.

A key implication of our findings is that support for punishing ingroup perpetrators increases with localized exposure to their crimes. Educating people about atrocities that occurred *in their neighborhoods* through exhibitions, commemorative plaques, and remembrance events may thus be useful for shaping support for transitional justice. People fill the places they inhabit with personalized meanings and emotions, and violence that occurred

in one's hometown appears to matter more than remote violence (Wójcik et al. 2010). An important task for future investigation, however, is to disentangle and examine different mechanisms that link such localized experiences to support for punishing ingroup crimes. Potential mechanisms include collective guilt for failing to intervene and standing by atrocities, heightened awareness of victims' suffering, and greater certainty of perpetrators' culpability.

We note that our study focuses on punishing Nazi crimes, and thus primarily speaks to retributive forms of transitional justice. Such policies are particularly apt in the aftermath of genocide and other forms of mass violence. It is possible that proximity to the victimized group will have different consequences for attitudes toward restorative justice which aims toward reconciliation and forgiveness through measures such as truth commissions and amnesties (Hall et al. 2018).

Notes

¹The free vote is most common in Westminster systems, but also occurs in West European parliaments (Baumann et al. 2015).

²Echoing our findings, de Juan et al. (2023a) demonstrate that exposure to violence during the death marches reduced voting for right-wing, nationalist parties after WWII in Bavaria, especially in elections where the Nazi crimes were politically salient.

³More generally, a large body of research on the 'contact hypothesis' (Allport 1954; Pettigrew and Tropp 2006; Paluck et al. 2019; Brown et al. 2021) concludes that contact with outgroups can reduce outgroup prejudice.

⁴While the Nazi regime also engaged in violence against other groups. Jehovah's Witnesses, Roma and Sinti communities, homosexuals, and Jews were by far the most common and numerous Nazi victims.

⁵This number is for Germany within the 1937 borders (United States Holocaust Memorial Museum 2022).

⁶Deutscher Bundestag. 4 Wahperiode, 170 Sitzung. March 10, 1965. pp. 8552-8553.

⁷Deutscher Bundestag. 4 Wahperiode, 170 Sitzung. March 10, 1965.

⁸Schiefes Weltbild. R. Z.: Verfahren gegen KZ-Opfer. "Wie MdB Gaßmann die Welt sieht", ZEIT Nr. 6 . Freitag, den 26. February 1965.

⁹Only a minority of votes (5% of all motions) in the Bundestag are conducted by a roll call, typically on request by a parliamentary party group or 5% of all MPs (Hohendorf et al. 2020).

¹⁰Such free or conscience votes typically occur when bills relate to broader moral issues, such as abortion, same-sex marriage, and organ donation.

¹¹Bundestag Plenary minutes, 243rd session on June 26, 1969, p. 13554.

- ¹²Deutscher Bundestag. 4 Wahperiode, 170 Sitzung. March 10, 1965. pp. 8554-55.
- ¹³We hand-coded MP votes on the 1969 bill because it was not covered by [Sieberer et al. \(2020\)](#).
- ¹⁴Haus der Bayerischen Geschichte Bavariathek; Hessian Regional History Information System (LAGIS); Wikipedia.
- ¹⁵Replication materials and code can be found at [Charnysh and Riaz \(2023\)](#).
- ¹⁶To match birthplaces to synagogues, we use contemporary municipal borders. The average municipality is 33 km². For MPs born outside of Germany’s post-WWII borders, we hand-coded the presence of synagogues.
- ¹⁷The correlation between the number of synagogues and the number of Jews in a given locality is very high, at $r = 0.94$. These calculations are based on data collected by [Buggle et al. \(2023\)](#), mapped to present-day municipality borders. The summary statistics are provided for the subset of localities in our main dataset. The Jewish population share variable was calculated by scaling by the present-day population, and may thus contain measurement error.
- ¹⁸We do not code occasional demotions and employment discrimination as persecution.
- ¹⁹For 19% of MPs information on religion was missing. We code these MPs as non-Catholic on the assumption that had their Catholic values been important to them, this would be mentioned in their biography.
- ²⁰We demonstrate that our results are robust to modeling explicit abstentions/absentees as a distinct outcome category using a multinomial logistic regression in Table A.5.
- ²¹We control for expellee status by including it as a separate category as part of our state fixed effects. For religion, we include a binary indicator for Catholic MPs. District closeness is defined as the difference between the district winner and runner-up in vote shares. We discretized this variable and included a separate category for cases in which it is not defined. We refer to [Sieberer et al. \(2020\)](#) for more details on the coding of MP covariates.
- ²²The March 1933 election followed the Nazi seizure of power and was marred by violence against the SPD and the KPD.
- ²³Because electoral districts are large geographical units that frequently combine multiple counties (*Kreise*), we use the count of synagogues in the district rather than a binary indicator for this analysis. There is little variation in the binary indicator as more than 90% of the electoral districts in our data contain at least one synagogue. The vast majority of districts without synagogues are located in rural Bavaria. We present additional specifications excluding our main treatment variable (Table A.16) and using a dummy for the presence of a synagogue in MP’s electoral district (Table A.17) in the SI. Our substantive conclusions remain unchanged.

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A Supporting Information (Online Only)

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A.1 Roll call voting

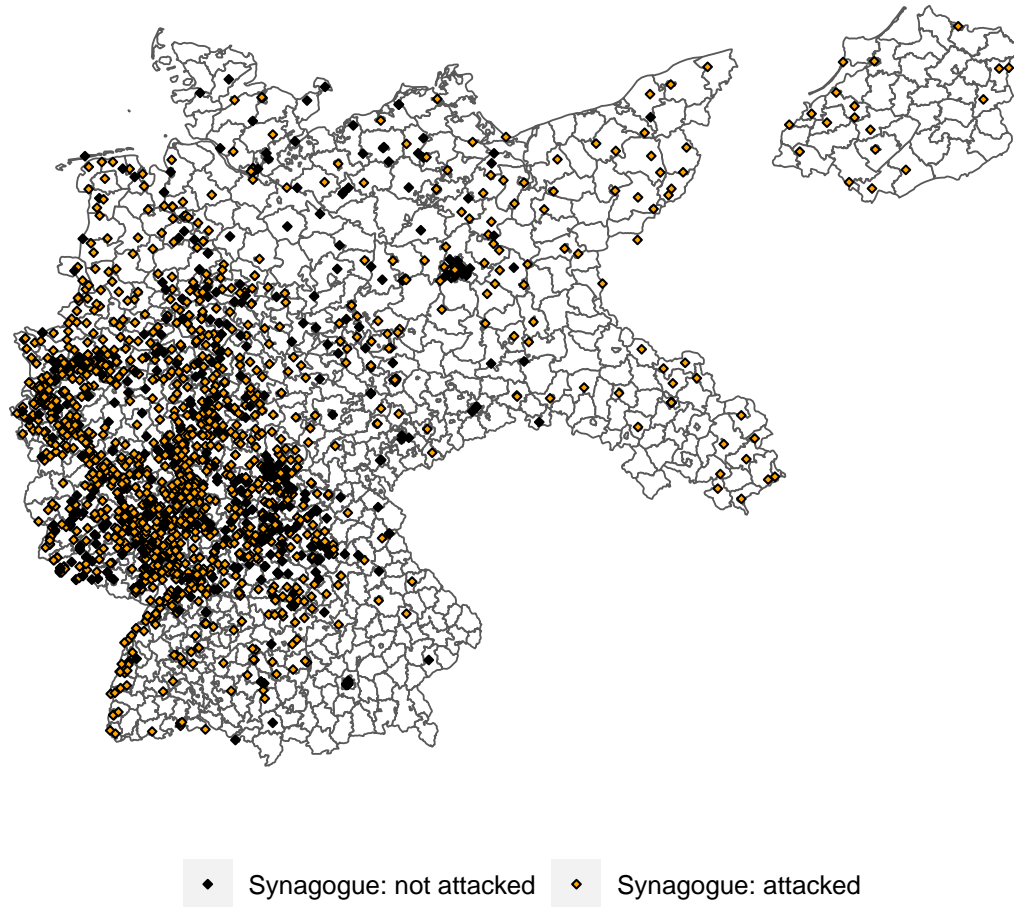
A.1.1 Descriptive statistics

Table A.1: Summary statistics, roll call voting data

Variable	Mean	Median	S.D.	N	Min	Max
Year of birth	1912.00	1911.00	9.97	888	1876	1935.0
Male (0/1)	0.90	1.00	0.30	888	0	1.0
District mandate (0/1)	0.42	0.00	0.49	888	0	1.0
District closeness	0.17	0.12	0.14	723	0	0.7
Dual candidate (0/1)	0.61	1.00	0.49	888	0	1.0
NSDAP member	0.21	0.00	0.40	888	0	1.0
Synagogue in hometown of MP	0.65	1.00	0.48	888	0	1.0
Synagogue in location 1933-1939	0.79	1.00	0.41	810	0	1.0
Count of synagogues in electoral district	7.63	5.00	7.82	723	0	41.0
Synagogue in electoral district (0/1)	0.93	1.00	0.26	723	0	1.0
Catholic	0.38	0.00	0.49	888	0	1.0
WW1 Veteran	0.09	0.00	0.28	888	0	1.0
WW2 Veteran	0.64	1.00	0.48	888	0	1.0
WW2 POW	0.34	0.00	0.47	888	0	1.0
Soviet POW	0.03	0.00	0.18	888	0	1.0
Repressed / Exile	0.16	0.00	0.36	888	0	1.0
Resistance	0.03	0.00	0.18	888	0	1.0

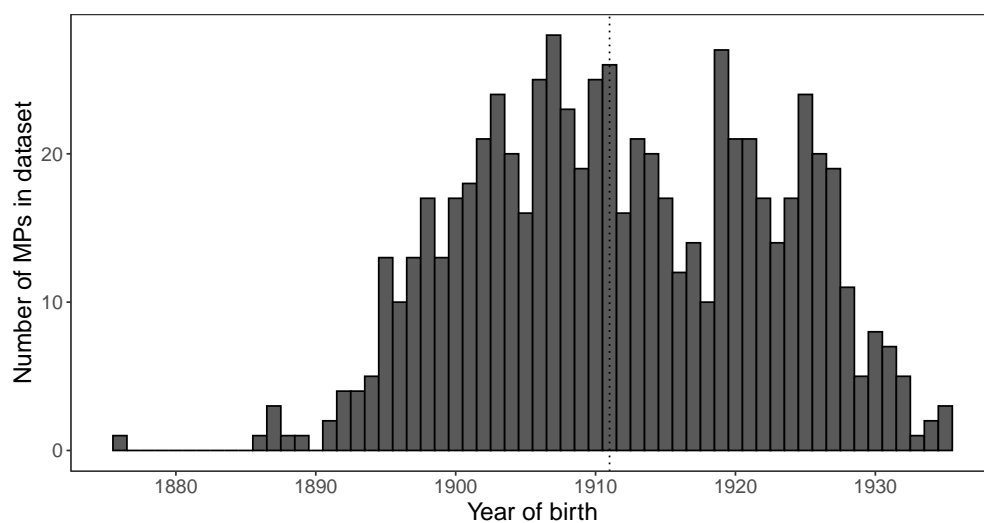
Note: The table shows summary statistics for the roll call voting data.

Figure A.1: November 1938 pogroms



Note: The map shows the spatial distribution of synagogues as of 1933 across Germany. The data was originally collected by [Solomon \(2021\)](#). Orange dots indicate synagogues that were attacked during the November pogroms in 1938.

Figure A.2: Histogram, year of birth of members of parliament



Note: The figure shows a histogram of the year of birth across MPs who voted on the 1965/69 statute of limitations extension bills. The median year of birth is 1911 (dotted line on the plot).

Table A.2: Top 10 cities by number of synagogues

City	Number of Synagogues in city
Berlin	91
Frankfurt am Main	37
Leipzig	16
Hamburg	15
Wiesbaden	13
München	13
Düsseldorf	12
Dortmund	12
Mainz	9
Mönchengladbach	8
Bonn	8
Stuttgart	8

Table A.3: Top 10 places of birth in MP sample

City	Number of MPs born in city
Berlin	51
München	25
Hamburg	21
Dortmund	20
Köln	20
Breslau	16
Wuppertal	12
Stuttgart	11
Bochum	10
Nürnberg	10

A.1.2 Additional results

Table A.4: Jewish population share as alternative treatment measure.

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Jewish pop. share (in %)	0.047*** (0.008)	0.025*** (0.006)	0.042 (0.028)	0.031*** (0.009)	0.023*** (0.006)	0.048* (0.025)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	770	770	770	770	770	770

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The units of observation are individual MPs. Standard errors are clustered by municipality/birthplace. The independent variable measures the Jewish population share, based on data collected by [Buggle et al. \(2023\)](#). The sample only includes MPs born within Germany's post-WW2 borders. The population figures were mapped to present-day municipality borders and scaled by contemporary total population figures (as of 2014). ***p < .01; **p < .05; *p < .1

Table A.5: Results from Multinomial Regression Models

Estimate	Std. error	P-value	Covariates	Fixed effects
0.6277***	0.1592	0.0001	No	Election period
0.8460***	0.2073	0.0000	No	Election period + Party
0.4802**	0.2353	0.0415	No	Election period + Party + State
0.4758***	0.1596	0.0029	Yes	Election period
0.8817***	0.1338	0.0000	Yes	Election period + Party
0.5590***	0.1412	0.0001	Yes	Election period + Party + State

Coefficient estimates for synagogue in MP's hometown (0/1) from multinomial regression models. The sample includes absentees, i.e., MPs who did not cast a ballot. The reference category is voting against the statute of limitations extensions in 1965/1969. We present the coefficients that predict voting in support of the bills.

Table A.6: Controlling for re-election of MPs

DV: Vote for Statute of Limitations Extension (0/1)						
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.126*** (0.037)	0.100*** (0.027)	0.059** (0.028)	0.086** (0.036)	0.092*** (0.027)	0.058** (0.027)
MP re-elected (0/1)	0.013 (0.033)	-0.044* (0.024)	-0.045* (0.024)	-0.005 (0.032)	-0.044* (0.025)	-0.045* (0.024)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	888	888	888	888	888	888

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.7: Standard errors clustered by MP

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.100*** (0.036)	0.097*** (0.027)	0.059** (0.028)	0.087** (0.035)	0.096*** (0.027)	0.062** (0.027)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	888	888	888	888	888	888

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. We replicate the same analysis as for Table 3 with one difference: standard errors are clustered by MP. ***p < .01; **p < .05; *p < .1

Table A.8: Subset of CDU/CSU MPs

	DV: Vote for SoL Extension (0/1)			
	(1)	(2)	(3)	(4)
Syn. in hometown	0.201*** (0.055)	0.108** (0.050)	0.190*** (0.055)	0.111** (0.047)
State FE	No	Yes	No	Yes
Covariates	No	No	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes
N	420	420	420	420

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for CDU/CSU MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.9: Excluding Expellees

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.170*** (0.044)	0.126*** (0.034)	0.077** (0.031)	0.136*** (0.040)	0.118*** (0.032)	0.083*** (0.030)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	773	773	773	773	773	773

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. We exclude expellees from the analysis. ***p < .01; **p < .05; *p < .1

Table A.10: Presence of Jewish community in MPS' location in 1933 – 1939 and support for extending the statute of limitations.

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Synagogue: 1933-1939	0.110** (0.048)	0.097*** (0.034)	0.059* (0.034)	0.087** (0.044)	0.093*** (0.032)	0.062* (0.032)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
DV mean	0.74	0.74	0.74	0.74	0.74	0.74
DV s.d.	0.44	0.44	0.44	0.44	0.44	0.44
N	810	810	810	810	810	810

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The units of observation are individual MPs. In contrast to our main results, we code the presence of a Jewish synagogue in MP's place of residence during the years 1933 – 1939. ***p < .01; **p < .05; *p < .1

Table A.11: Controlling for population size

	DV: Vote for SoL Extension (0/1)		
	(1)	(2)	(3)
Syn. in hometown	0.135*** (0.040)	0.113*** (0.032)	0.080** (0.034)
Party FE	No	Yes	Yes
State FE	No	No	Yes
Covariate: pop. size	Yes	Yes	Yes
Other covariates	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes
N	770	770	770

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We control for municipality population size (as of 2014) in all models. We only observe this variable for MPs born within Germany's post-WWII borders (including the GDR), hence this analysis excludes expellees. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.12: Excluding MPs born in largest cities by population size

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.127*** (0.037)	0.108*** (0.028)	0.065** (0.028)	0.091*** (0.034)	0.099*** (0.028)	0.061** (0.027)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	791	791	791	791	791	791

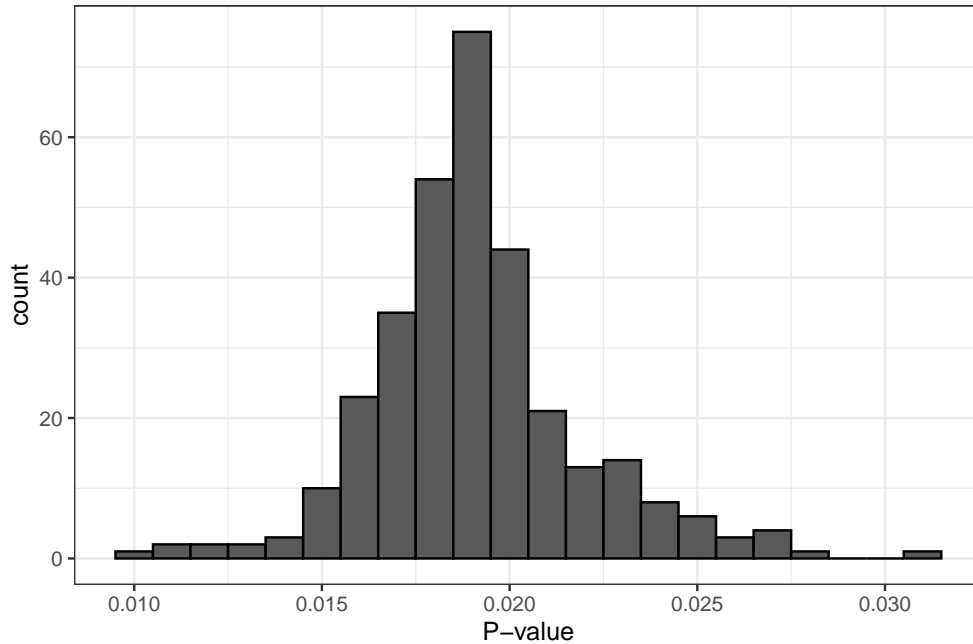
Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. We exclude MPs who were born in Berlin, Hamburg, or Munich. ***p < .01; **p < .05; *p < .1

Table A.13: Excluding MPs born in cities with highest number of synagogues

DV: Vote for Statute of Limitations Extension (0/1)						
	(1)	(2)	(3)	(4)	(5)	(6)
Syn. in hometown	0.110*** (0.038)	0.095*** (0.030)	0.058** (0.027)	0.078** (0.035)	0.088*** (0.029)	0.058** (0.026)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	825	825	825	825	825	825

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. The unit of observation are individual MPs. Standard errors are clustered by municipality/place of birth. We exclude MPs who were born in Berlin, Frankfurt, or Leipzig. ***p < .01; **p < .05; *p < .1

Figure A.3: Excluding cities one by one



Note: The figure shows a histogram of p-values for the synagogue in MP's hometown coefficient across 322 OLS models. For each model, we exclude one municipality from the sample. We use our most demanding specification, including party FE, state FE, vote FE, and covariates. The maximum p-value across specifications is 0.031.

Table A.14: Synagogue presence and voting behavior prior to WWII

	NSDAP		SPD		KPD	
	(1)	(2)	(3)	(4)	(5)	(6)
Synagogue (0/1)	0.842 (0.886)	-0.142 (0.902)	-0.171 (0.640)	-0.419 (0.579)	-0.128 (0.475)	0.049 (0.458)
State FE	No	Yes	No	Yes	No	Yes
DV mean	34.77	34.77	17.76	17.76	12.31	12.31
DV s.d.	13.44	13.44	9.57	9.57	7.12	7.12
N	923	923	923	923	923	923

Notes: The Table shows the results from OLS regressions where the county-level vote share of the NSDAP/SPD/KPD in the November 1932 federal elections is the dependent variable. The election data was retrieved from the replication data of [de Juan et al. \(2023b\)](#). We regress the vote share for a given party on a binary indicator for the presence of at least one synagogue in a given county. The units of analysis are counties as of 1933. ***p < .01; **p < .05; *p < .1

Table A.15: Synagogues in electoral district as alternative predictor

	DV: Vote for 1965 Statute of Lim. Extension					
	(1)	(2)	(3)	(4)	(5)	(6)
No. of Synagogues in Elec. District	0.006* (0.003)	0.002 (0.003)	0.002 (0.003)	0.003 (0.003)	0.001 (0.003)	0.0004 (0.003)
Synagogue in MPs hometown	0.226*** (0.057)	0.161*** (0.051)	0.098** (0.045)	0.198*** (0.051)	0.180*** (0.049)	0.119*** (0.043)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
N	374	374	374	374	374	374

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We subset to MPs with district mandates. The bivariate correlation between the number of synagogues in the electoral district and the binary indicator for synagogue presence in MP's hometown is low at $r = 0.05$. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.16: Synagogues in electoral district as alternative predictor

	DV: Vote for 1965 Statute of Lim. Extension					
	(1)	(2)	(3)	(4)	(5)	(6)
No. of Synagogues in Elec. District	0.007** (0.003)	0.003 (0.003)	0.002 (0.003)	0.004 (0.003)	0.002 (0.003)	0.001 (0.003)
N	374	374	374	374	374	374
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
N	374	374	374	374	374	374

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We subset to MPs with district mandates. Standard errors are clustered by municipality/place of birth. The specifications are analogous to the results presented in Table A.15 but exclude the binary indicator for synagogue presence in MPs' hometowns. ***p < .01; **p < .05; *p < .1

Table A.17: Binary indicator for synagogue in electoral district as alternative predictor

	DV: Vote for 1965 Statute of Lim. Extension					
	(1)	(2)	(3)	(4)	(5)	(6)
Synagogue in Elec. District (0/1)	0.359*** (0.109)	0.261*** (0.098)	0.165** (0.081)	0.270*** (0.087)	0.227** (0.096)	0.126* (0.073)
Synagogue in MPs hometown	0.219*** (0.054)	0.158*** (0.048)	0.098** (0.044)	0.196*** (0.049)	0.177*** (0.047)	0.118*** (0.043)
N	374	374	374	374	374	374
N	374	374	374	374	374	374
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
N	374	374	374	374	374	374

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We subset to MPs with district mandates. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.18: Vote on abolishing the statute of limitations in 1979.

	DV: Vote for Abolishing the Statute of Limitations			
	(1)	(2)	(3)	(4)
Synagogue: hometown	0.0112 (0.0302)	-0.0193 (0.0440)	0.0645 (0.0407)	
Synagogue: 1933-1939				0.0896** (0.0450)
Party	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes
Year of birth	Full sample	after 1930	before 1930	before 1930
Covariates	Yes	Yes	Yes	Yes
Observations	496	238	258	246

Notes: The table shows results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the abolition of the statute of limitations in 1979. The unit of observation is individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

Table A.19: Support for transitional justice in 1965-69 for different subsets of MPs.

MP subset	DV: Vote for Statute of Limitations Extension (0/1)			
	Full sample (1)	Bystanders (2)	NSDAP members (3)	Victims (4)
Synagogue: hometown	0.0635** (0.0272)	0.0723** (0.0311)	0.0490 (0.0681)	-0.0754 (0.0676)
Vote	Yes	Yes	Yes	Yes
Party	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes
Observations	888	568	183	137

Notes: Results in different subsets of MPs: NSDAP members, who were most complicit in Nazi violence; victims, defined as MPs who were repressed or in exile; and bystanders, the group that encompasses the remaining MPs. Clustered (Municipality/place of birth) standard-errors in parentheses. ***p < .01; **p < .05; *p < .1

Table A.20: Synagogue attacks and support for transitional justice.

	DV: Vote for Statute of Limitations Extension (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Jewish pop. share (in %)	0.004 (0.007)	0.006 (0.005)	−0.0005 (0.007)	0.003 (0.007)	0.006 (0.006)	−0.001 (0.006)
Synagogue: attacked	0.157*** (0.053)	0.107*** (0.038)	0.082** (0.036)	0.141*** (0.045)	0.101*** (0.035)	0.083** (0.035)
Party FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Covariates	No	No	No	Yes	Yes	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
N	715	715	715	715	715	715

Notes: Results from OLS regressions where the outcome is a binary indicator that equals one for MPs who voted for the 1965/1969 statute of limitations extension. We code whether MPs witnessed synagogue attacks based on their location 1933 – 1939 (see also Tables 4 and A.10). The Jewish pop. share variable is likewise calculated for MP’s 1933–1939 location (Buggle et al. 2023). The sample only includes MPs born within Germany’s post-WW2 borders. The population figures were mapped to present-day municipality borders and scaled by contemporary total population figures (as of 2014). The unit of observation is individual MPs. Standard errors are clustered by municipality/place of birth. ***p < .01; **p < .05; *p < .1

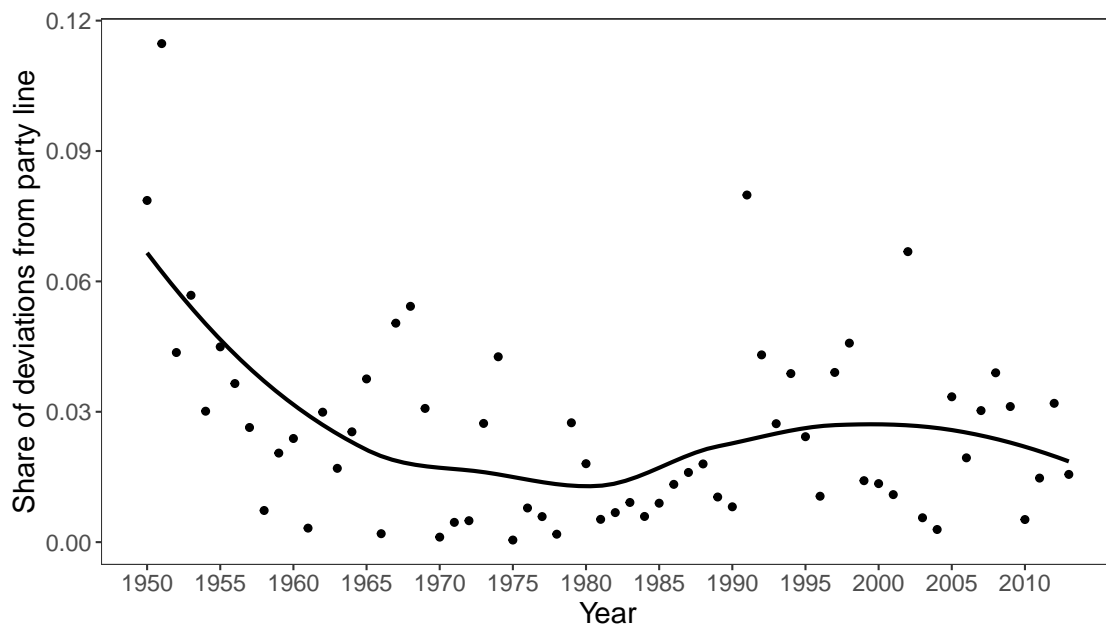
A.1.3 Additional details: placebo analysis

For the placebo analysis, we again draw on the roll call voting data collected by [Sieberer et al. \(2020\)](#). To code deviant voting behavior, we followed the coding in [Sieberer et al. \(2020\)](#), who record ‘strong’ and ‘weak’ deviations from the party line as follows:

“The party line is defined as the absolute majority position within the party group in the roll call vote; if there is no absolute majority position, the position taken by the chair of the parliamentary party group is coded as party line. Strong deviation: MP votes yes and party group votes no or the other way around; weak deviation: MP or party group abstains and the other votes yes or no (quoted from codebook provided by [Sieberer et al. \(2020\)](#)).

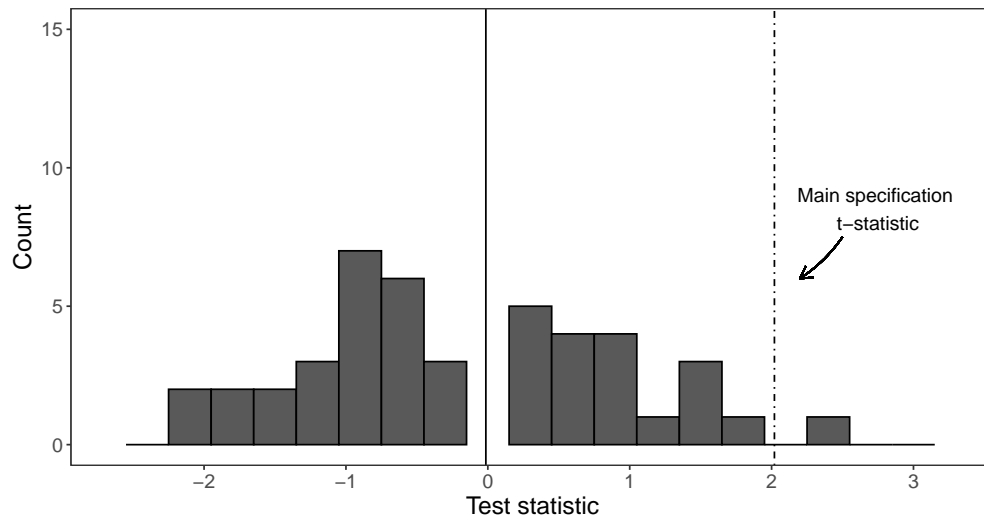
Our binary indicator outcome variable equals one for MPs who strongly or weakly deviated from their respective party line for a given vote. We plot the share of deviant votes by year in Figure A.4 below.

Figure A.4: Deviations from party line by year



Note: The figure shows the share of votes deviating from the party line by year (see also Section A.1.3).

Figure A.5: Distribution of test statistic across placebo roll call votes



Note: The figure shows t-statistics for the binary synagogue in MP's hometown presence indicator across placebo OLS regressions. The outcome is a binary indicator that equals one for MOPs who deviated from the party line in a given roll call vote. Standard errors are clustered at the municipality level. The median test statistic across all placebo specifications is shown as a solid line. The t-statistic for our main specification is shown by the dotted line.

A.2 Voting behavior after the Auschwitz trials

We investigate how voters responded to parties’ divergent positions on transitional justice, laid bare during the Bundestag debate over the limitations statute, at the time when the Holocaust was salient in political discourse due to the widely publicized Frankfurt Auschwitz trial (1963–65). While the CDU/CSU was divided internally, other parties – SPD, FDP, and NDP – adopted clear-cut positions in favor of or against dealing with the Nazi past (see section 4.2). Against this background, we examine whether proximity to Jews increased electoral support for parties that unequivocally supported transitional justice. If proximity to Jewish communities increased empathy toward Jews and support for trying Nazi perpetrators, we would expect a disproportionate increase in support for the SPD – the party most critical of the Nazi regime and unequivocally supportive of transitional justice – in places with Jewish communities after the Auschwitz trials. Conversely, we expect to see a decrease in support for the FDP, and especially for the NPD – the two parties most critical of transitional justice.²⁴

A.2.1 Data and Empirical Strategy

To test these empirical implications, we obtained county-level electoral returns for the West German federal elections in 1953, 1957, 1961, and 1965 from the *Bundeswahlleiter* (see Table A.21 for summary statistics). To examine electoral shifts after the Auschwitz trials, we estimate a series of two-period difference-in-differences models of the following form:

$$Y_{c,t,p} = \alpha_c + \gamma_t + \beta T_c \times \text{post}_t + \epsilon_{c,t,p}$$

where $Y_{c,t,p}$ vote share of party p in county c at time t . The terms α_c and γ_t denote unit and time fixed effects, respectively. T_c is a binary treatment indicator. In line with our analysis of the roll call voting data, the treatment is defined as the presence of at least one synagogue in a given county as of 1933. We are interested in estimating β : the effect of Jewish (synagogue) presence before the Holocaust on the vote share of party p after the Auschwitz trial. This parameter gives us the divergence in the level of electoral support for different parties in counties with and without Jewish communities between different election periods. We use standard errors clustered at the county level for all inferences in this part of the analysis.

Table A.21: Summary statistics

Variable	Mean	Median	S.D.	N	Min	Max
CDU/CSU vote share (in %)	44.51	43.86	8.89	547	20.98	70.61
SPD vote share (in %)	36.71	36.84	9.00	547	12.29	59.72
Far-right vote share (NPD/DRP, in %)	1.48	0.90	1.66	547	0.00	11.15
FDP vote share (in %)	9.86	9.10	4.66	547	1.90	28.34
Jewish presence (0/1)	0.84	1.00	0.37	547	0.00	1.00

Note: The table shows summary statistics for the county-level voting data, pooled across the federal elections 1953, 1957, 1961, and 1965.

Before moving on, we note two features of our empirical strategy. First, we conduct our analysis within the subset of 136 ‘city-counties’ (*Stadtkreise*, see Table A.23 for a complete list).²⁵ We focus on cities rather than the full sample of counties because our identification assumptions are more likely to hold within this subset. As the Jewish population was concentrated in cities rather than the countryside, comparing votes for urban and rural counties would conflate trends in places with synagogues and trends in urban areas. We note that our substantive conclusions remain unchanged when we conduct the analysis in the full sample of counties (see Table A.22). Second, we recognize the downsides of using two-way fixed effects estimators in panel settings with more than two periods (Imai and Kim 2021). We, therefore, adopt a conservative approach and do not pool more than two periods in a single model; all of our federal election voting analyses are based on two-period panels covering consecutive election periods.

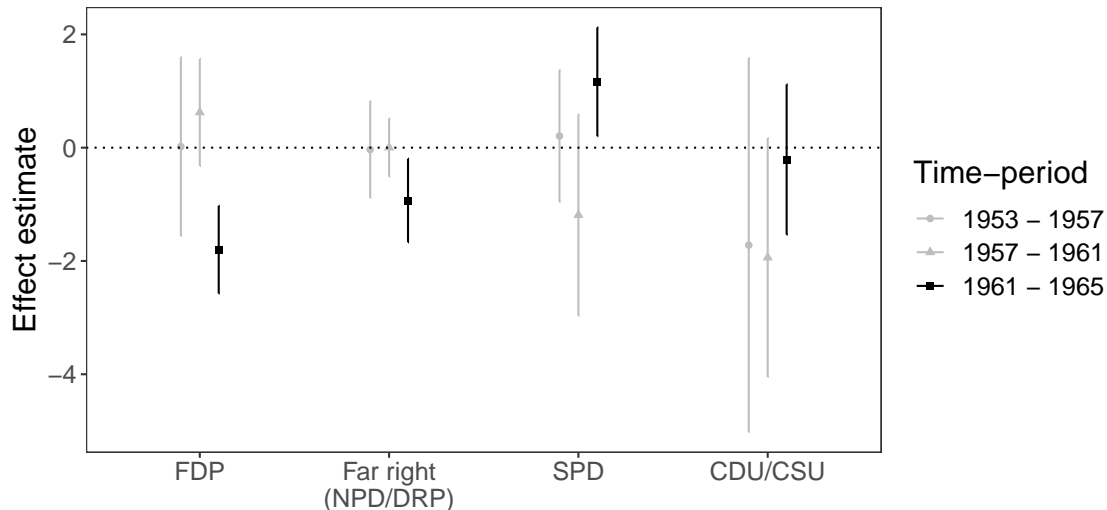
A.2.2 Results

We present our main results in Figure A.6. In line with individual-level results for German legislators, we find that electoral support for the SPD, which endorsed transitional justice, increased by about one percentage point in communities with a Jewish presence before WWII (0.13 s.d.). Conversely, the Free Democratic Party (FDP), which was a vocal critic of transitional justice and advocated for amnesty for war criminals, lost about two percentage points in cities that had a synagogue (0.39 s.d.). We find consistent findings for the parties on the far right (NPD/DRP), which lost about one percentage point in electoral support (0.56 s.d.).

Overall, we see a greater change in support for the far right parties that opposed transitional justice rather than for the SPD, the party that pushed for punishing Nazi crimes. One reason could be that the SPD platform was broader and dominated by economic issues:

the party criticized the social market economy model and called for the nationalization of basic industries. By contrast, the Nazi past and transitional justice were central to the NPD platform.

Figure A.6: The effect of synagogue presence on voting behavior after the Auschwitz trials (1961 – 1965) and in prior placebo periods.



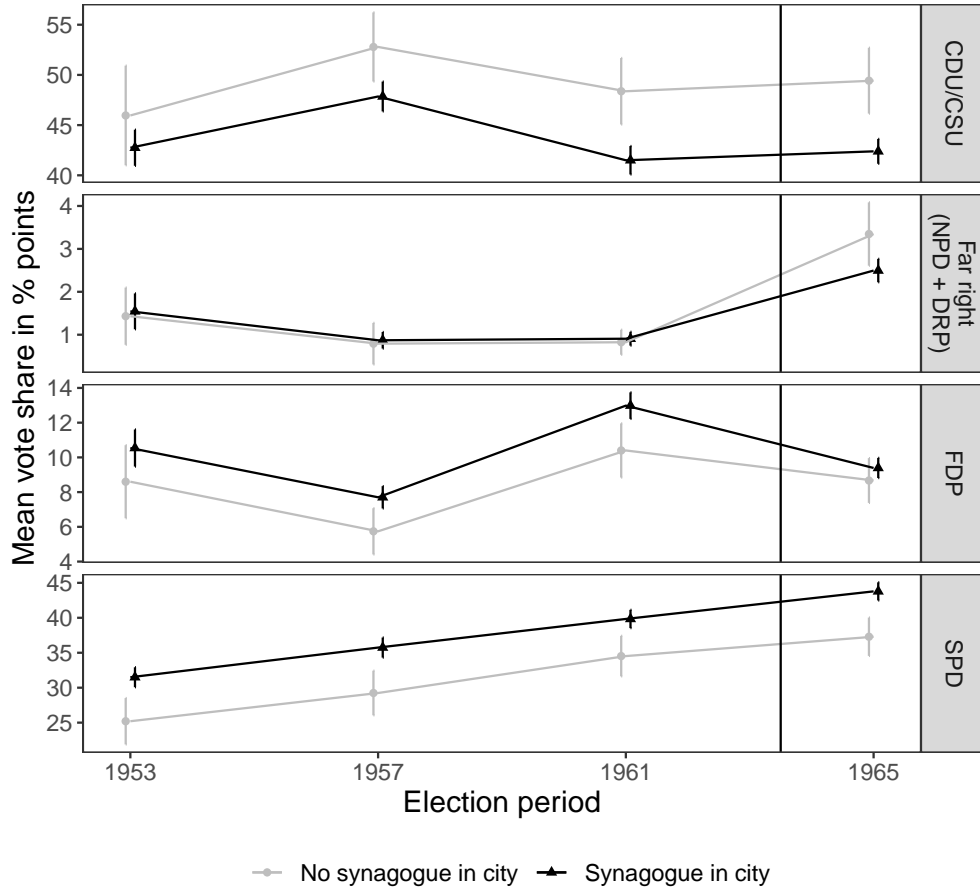
Note: The figure shows treatment effect estimates from difference-in-differences regression models. We estimated separate models for each election period and each outcome. Gray bars indicate effect estimates for placebo periods, prior to the Auschwitz trial.

A.2.3 Robustness

To substantiate these results, we performed a series of robustness checks. Most importantly, our analysis assumes that electoral results in cities with and without Jewish communities prior to WWII would have evolved in parallel between 1961 and 1965, had the Auschwitz trials not taken place. While we cannot directly test this assumption, we provide several pieces of evidence that support its plausibility. First, we demonstrate parallel trends prior to the Auschwitz trials by performing placebo regressions in two pre-treatment electoral cycles. Specifically, we show that electoral support for all major parties evolved in parallel in treated and control counties for ten years prior to the trial (light gray bars in Figure A.6).

In addition, we demonstrate that the regression results are mirrored in the raw data and are hence unlikely to be driven by idiosyncratic model specification choices. We present the raw vote shares in cities with and without synagogues in Figure A.7 in the appendix. After the Auschwitz trials, the SPD gains in cities that had a Jewish community prior to the war,

Figure A.7: Mean vote shares of different parties in cities with and without synagogues over time.



Note: The figure shows the average vote shares for different parties in counties with and without synagogues over time. Our sample only consists of cities (*Stadtkreise*).

whereas the FDP and far-right parties lose electoral support. We also show that our main results remain unchanged when we do not subset to cities, but instead retain all counties in the sample (see Table A.22).

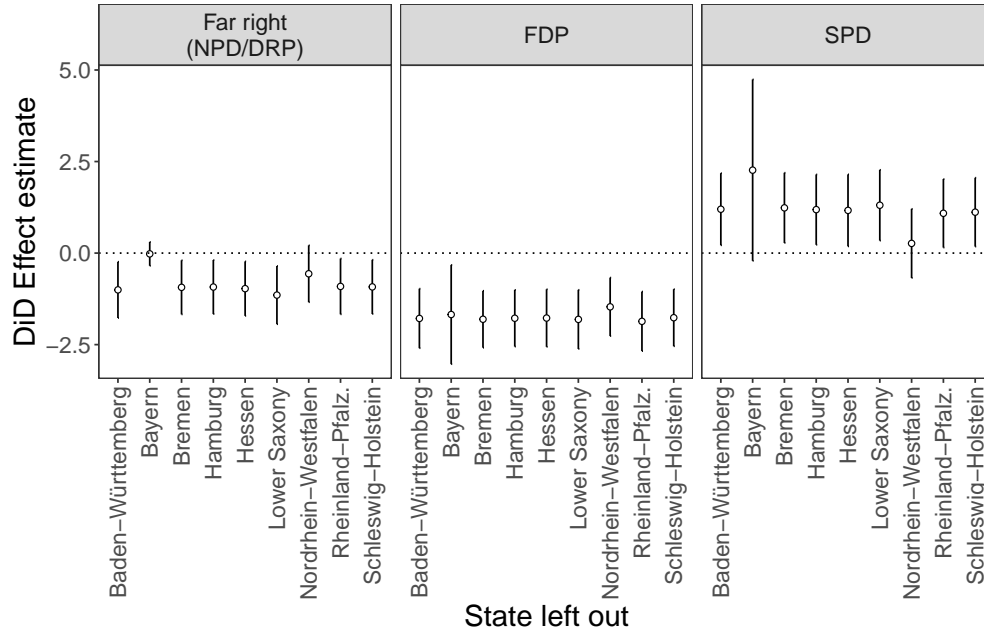
Finally, we demonstrate that our results are not driven by spatial clusters of synagogues. Specifically, we repeat our main analysis and sequentially exclude one of the nine West German federal states one by one. We present the results in Figure A.8. We find that our main results remain unchanged when we exclude different spatial subsets of our sample.

Table A.22: The effect of the Auschwitz trials on voting in the full sample of all West German counties.

	FDP (1)	Far-right (2)	CDU/CSU (3)	SPD (4)
Synagogue \times post	-1.380*** (0.187)	-0.283*** (0.106)	-0.672* (0.402)	0.490** (0.194)
N	1124	1124	1124	1124
R-squared	0.960	0.821	0.978	0.989

Notes: The Table shows treatment effect estimates from difference in differences models for the time period 1961 to 1965. For this analysis, we do not subset to city counties (*Stadtkreise*). Instead, we retain all West German counties (as of 1950). Otherwise, the analysis is identical to our main results. The binary treatment is defined as the presence of at least one synagogue in a given city. Standard errors are clustered at the county level. ***p < .01; **p < .05; *p < .1

Figure A.8: The effect of the Auschwitz trials on voting behavior when excluding spatial clusters of synagogues.



Note: The figure shows treatment effect estimates from difference in differences models for the time period 1961 to 1965. We repeat the same analysis as for our main results (see Figure A.6), but sequentially exclude one of the nine West-German federal states one by one. Standard errors are clustered at the county level. We fit separate models for each vote share outcome variable.

A.2.4 List of city counties in sample

Table A.23: City counties in sample

Aachen	Günzburg	Neuburg an der Donau
Amberg	Hagen	Neumarkt in der Oberpfalz
Ansbach	Hamburg	Neumünster
Aschaffenburg	Hameln	Neustadt an der Weinstraße
Augsburg	Hamm (Westfalen)	Neustadt bei Coburg
Bad Kissingen	Hanau am Main	Neuß
Bad Reichenhall	Hannover	Nördlingen
Baden-Baden	Heidelberg	Nürnberg
Bamberg	Heilbronn	Oberhausen
Bayreuth	Herford	Offenbach am Main
Bielefeld	Herne	Oldenburg (Oldenburg)
Bocholt	Hildesheim	Osnabrück
Bochum	Hof	Passau
Bonn	Ingolstadt	Pforzheim
Bottrop	Iserlohn	Pirmasens
Braunschweig	Kaiserslautern	Recklinghausen
Bremen	Karlsruhe	Regensburg
Bremerhaven	Kassel	Remscheid
Castrop-Rauxel	Kaufbeuren	Rheydt
Celle	Kempen (Allgäu)	Rosenheim
Coburg	Kiel	Rothenburg ob der Tauber
Cuxhaven	Kitzingen	Schwabach
Darmstadt	Koblenz	Schwandorf in Bayern
Deggendorf	Krefeld	Schweinfurt
Delmenhorst	Kulmbach	Selb
Dillingen an der Donau	Köln	Siegen
Dortmund	Landau in der Pfalz	Solingen
Duisburg	Landsberg am Lech	Speyer
Düsseldorf	Landshut	Straubing
Eichstätt	Lindau (Bodensee)	Stuttgart
Emden	Ludwigshafen am Rhein	Traunstein
Erlangen	Lübeck	Trier
Essen	Lüdenscheid	Ulm
Flensburg	Lüneburg	Viersen
Forchheim	Lünen	Wanne-Eickel
Frankenthal (Pfalz)	Mainz	Watenstedt-Salzgitter
Frankfurt am Main	Mannheim	Wattenscheid
Freiburg im Breisgau	Marburg an der Lahn	Weiden in der Oberpfalz
Freising	Marktredwitz	Weißenburg in Bayern
Fulda	Memmingen	Wiesbaden
Fürth	Mönchen Gladbach	Wilhelmshaven
Gelsenkirchen	Mülheim an der Ruhr	Witten
Gießen	München	Worms
Gladbeck	Münster	Wuppertal
Goslar	Neu-Ulm	Würzburg
		Zweibrücken