

Democracy and Retribution: Transitional Justice and Regime Support in Postwar West Germany

Comparative Political Studies

2020, Vol. 53(3-4) 399-433

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DOI: 10.1177/0010414019852704

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Abstract

How harshly should perpetrators of past abuses be punished, to reinforce the legitimacy of a new democracy? Drawing on sociopsychological theories, we hypothesize that prodemocratic mass attitudes are favored by the perception that defendants in transitional justice trials have been punished in a way that is *morally proportional* to their offenses. This perception is shaped by the social categorization of defendants and the opinions about the certainty of their guilt that predominate in the mass public. When defendants are largely seen as co-ethnics and their guilt is contested, like in the West German case, prodemocratic attitudes are likely to be strengthened by lighter punishments and undermined by harsher sanctions. The analysis of subnational variation in patterns of punishment in postwar West Germany confirms this hypothesis and shows that these attitudinal effects persist in the medium term. Our findings have implications for research on transitional justice and democratization.

Keywords

democratization and regime change, political psychology, transitional justice, Germany

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Introduction

What are the effects of transitional justice (TJ) policies on the prospects of democracy in post-authoritarian settings? TJ policies aim at “righting past wrongs” after a regime transition. These include trying and punishing (or amnestying) wrongdoers, providing compensation and reparation for victims, and implementing truth-telling measures (e.g., Elster, 2004). In recent years scholars have analyzed the impact of TJ policies, separately or in combination, on democratic consolidation (e.g., Olsen, Payne, & Reiter, 2010a), the rule of law (e.g., Sikkink & Walling, 2007), the likelihood of post-conflict peace (e.g., Loyle & Appel, 2017), the level of state repression (e.g., Kim & Sikkink, 2010; Lynch & Marchesi, 2015), the emergence of a diffuse culture of respect for human rights (Gibson, 2004a), and racial reconciliation (Gibson, 2004b).

This recent literature has furthered our understanding of how TJ policies affect the legitimacy and viability of new democracies. Innovative data collection efforts (e.g., Binningsbø, Loyle, Elster, & Gates, 2012; Olsen, Payne, & Reiter, 2010b; Sikkink & Kim, 2013) have complemented a long-standing tradition of case study research, and have spurred a wave of work that analyzes a large number of countries over time, testing general propositions and arriving at new findings (e.g., Backer, 2009; Balasco, 2013; Olsen et al., 2010a). Importantly, some of this work (see, for example, Wiebelhaus-Brahm, 2010, p. 137; Kim & Sikkink, 2010, p. 950) has addressed the long-standing methodological challenge of the endogeneity of TJ policies to the politics of transition and post-transition phase (e.g., Elster, 1998; Nalepa, 2010). This challenge—which TJ scholars have long acknowledged (e.g., David, 2006, pp. 82-85; Gibson, 2004b, p. 212; 2006; 2009, pp. 128-130)—had typically rendered it difficult to assess the independent impact of TJ policies on democracy and to establish the direction of causality between TJ and other factors.

Despite these advances, problems remain. First, large-N comparative studies of the consequences of TJ often adopt a rather coarse operationalization of TJ policies. These studies typically estimate the impact of the presence of certain TJ institutions (e.g., an amnesty law or a specialized court) in a country during a given year on quantitative indices of democracy, state repression, or human rights (e.g., Lynch & Marchesi, 2015; Olsen et al., 2010a; Sikkink & Walling, 2007). Conducting the analysis at the polity (or polity-year) level is appropriate if, for example, the hypothesized causal path is that human rights trials deter elites in transitional or authoritarian regimes from engaging in further human rights violations (Kim & Sikkink, 2010; see also Loyle & Appel, 2017). However, a polity-year analysis would not be

suitable to account for another important causal path: attitudinal change (e.g., Backer, 2009; Gibson, 2002, 2004a, 2004b; Weinstein, Fletcher, Vinck, & Pham, 2010). In this alternative causal pathway, TJ policies may affect mass attitudes toward democracy, which in turn may enhance or undermine the viability of the new democratic regime (e.g., Gibson, 2006, pp. 430 ff.).¹

Furthermore, most of the recent literature (not only multi-country quantitative analyses but also case studies; e.g., Boed, 1999; Nalepa, 2012) explicitly or implicitly conceptualizes TJ policies as formal rules and institutions.² This operationalization, often imposed by data limitations, is unlikely to capture fully the actual democratic implications of TJ policies, as their purely formal conceptualization might hide varying levels of enforcement (e.g., Levitsky & Murillo, 2009). However, the legitimacy of TJ institutions and rules with the mass public will likely depend more on how these are enforced, rather than on their formal design.³

Our analysis focuses on TJ trials of individuals who had different types of responsibility in supporting—or benefiting from—the past authoritarian regime. In particular, we analyze the effects on democratic attitudes of the outcomes of “denazification” trials held under the supervision of U.S., U.K., and French authorities in their respective occupation zones during 1945–1949. Even though modern TJ relies on a broader panoply of institutions and policies that in addition to trials include measures of restitution, reparation, and truth-telling, trying and punishing perpetrators of human rights violations remains an essential part of most TJ programs. Our theoretical framework centers on the idea, well developed in the social psychology literature, that the attitudinal consequences of punishment depend on the extent to which individuals not directly involved in the trial see punitive measures as *proportional with the perceived degree of moral responsibility* of the punished individuals. As sociopsychological research has shown, this assessment of the proportionality of punishment is highly sensitive to the contextual circumstances in which the offense was perpetrated—in particular, how certain the members of the public are of defendants’ guilt, and whether they consider them in-group or out-group members (e.g., Hamilton, 1986; van Prooijen, 2006).⁴

Our research design uses subnational variation in TJ policies in West Germany after 1945, for which the historical circumstances allow us to mitigate substantially the inferential problem of TJ being endogenous to post-transition politics. The delimitation of the U.S., U.K., and French zones of occupation was determined by diplomacy and by military “facts on the ground,” and the three military governments conducted their TJ policies largely independently from each other, guided by their geopolitical strategies. Therefore, German citizens in different occupation zones

experienced significantly different TJ policies, and these differences were largely exogenous to their political preferences.⁵ Furthermore, the three occupation authorities used the same system to classify the responsibilities of individuals targeted by TJ measures and the sanctions imposed, which facilitates cross-zone comparability.

Analyzing data from three German public opinion surveys fielded in 1953, 1955, and 1957, we find that in line with the proportionality hypothesis applied to our case—where, as we discuss below, defendants were largely seen as in-group members and their guilt was widely contested among the population—democratic support among the general public was strengthened by lighter types of TJ punishment and undermined by harsher types of punishment. To increase confidence in our findings, we investigate the effects of individuals' exposure to channels through which they might have become aware of subnational patterns of punishment. We find that the effects above are consistently stronger for individuals who were more socially active and who had a higher exposure to mass media.

The article is organized as follows. In the next sections, we lay out our theoretical framework and articulate our hypotheses. Next, we explain our research design and briefly outline denazification policies in the Western German territories and their historical context. We then present our data and analysis. In the conclusion, we discuss the implications of our study for the further theorization and comparative analysis of the democratic effects of TJ policies.

Theory: Retribution and Morally Proportional Punishment

Even though deterrence has been often invoked as an important function of punishment in general (e.g., Nagin, 1998, 2013; see also discussions in Allen, 1975; Gardner, 1976; Schulhofer, 1974) and of TJ trials in particular (e.g., Kim & Sikkink, 2010, pp. 943-944; Loyle & Appel, 2017, pp. 691-692; Olsen et al., 2010a, p. 983; Orentlicher, 1991, p. 2542), several strands of experimental evidence in social psychology and behavioral economics support the view that deterrence-based conceptions of punishment play a very limited role in shaping the psychological motivations at the basis of most individuals' desire to punish transgressors (for reviews, see, for example, Carlsmith & Darley, 2008; Darley, 2009; Doob & Webster, 2003). Indeed, a wealth of empirical studies have shown that the psychological motivations to punish those who violate the law, or transgress generally accepted community norms, correspond to a retribution-based, "just deserts" view of punishment.

In the “just deserts” view, punishment is not an instrument to deter future abuses, but has value in itself as a way to restore the balance of community norms that the transgressor has violated (e.g., Vidmar & Miller, 1980, pp. 570-580). For this reason, punishment should be *proportional to the moral wrong* represented by the offense (e.g., Carlsmith, 2008, p. 120; Carlsmith, Darley, & Robinson, 2002, p. 284; Darley, 2009, p. 3). Research in social psychology has shown that the decision as to what constitutes an appropriate punishment for a perpetrator of a certain offense is not rooted in reasoned neural processes, but in intuitive ones, which are automatic and emotionally loaded (e.g., Carlsmith, 2008; Carlsmith & Darley, 2008, pp. 211-213; Darley, 2009, pp. 3-6; Kahneman, 2003; Robinson & Darley, 1997, p. 134). Even when in abstract terms an individual may favor forms of punishment that aim at deterring future crimes (Carlsmith, 2008), when faced with the description of a specific crime, she will generally experience a strong and immediate desire to punish the offender, a desire that is rooted in the emotion of *moral outrage*. The level of moral outrage felt by the responder is a strong predictor of the harshness of the punishment that she would impose on the offender (e.g., Carlsmith, 2006; Carlsmith et al., 2002, pp. 296-297; Darley, Carlsmith, & Robinson, 2000, pp. 676-678). Importantly, respondents follow the criterion of the moral proportionality in deciding on an adequate punishment for a specific offense not only when the offense in question affects them directly (e.g., Strelan & van Prooijen, 2013) but also when they are third parties, onlookers to an abuse that does not affect them but affects other individuals in their community (e.g., de Quervain et al., 2004; Fehr & Fishbacker, 2004; Vidmar & Miller, 1980; see also the discussion in Darley, 2009, pp. 11-14).

Such a conception of what constitutes an appropriate punishment is typically not sensitive to the characteristics of individual respondents (Carlsmith & Darley, 2008, pp. 206-209), and there is evidence that some of its basic elements also hold in different legal and political cultures (Henrich et al., 2006). At the same time, being based on an intuitive assessment of the moral wrong represented by a specific offense, individual decisions on the appropriate—that is, morally proportional—punishment for a certain abuse will be highly sensitive to the specific circumstances in which the offense was committed. Previous studies have consistently shown that factors such as the intentionality of the violation as well as extenuating (or aggravating) circumstances will typically influence the level of moral outrage felt by respondents and consequentially the level of punishment that they consider morally appropriate for the crime in question (e.g., Carlsmith, 2006, p. 438; Carlsmith, 2008, p. 120; Carlsmith et al., 2002 p. 297; Darley, 2009, pp. 7-8; Darley et al., 2000; Finkel, Maloney, Valbuena, & Groscup, 1996; Weiner, Graham, & Reyna, 1997).

An important and reoccurring extenuating circumstance that reduces the certainty of defendants' blameworthiness in the eyes of external observers is the extent to which defendants are seen as merely following orders as members of a hierarchical organization (e.g., Hamilton, 1986; Hamilton & Hagiwara, 1992). Gibson and Gouws (1999) have found that this same factor influences blame attribution in TJ trials. Recent research has found that whether such uncertainty of the defendant's guilt reduces or enhances the moral outrage felt by observers, and therefore their decision on what constitutes a morally proportional punishment, depends on the social categorization of the defendants. Although studies of racial bias in sentencing have found that harsher punishment is often considered more legitimate for out-groups than for in-groups (e.g., Kerr, Hymes, Anderson, & Weathers, 1995; Sommers & Ellsworth, 2000; see also Vidmar & Miller, 1980, p. 589), individuals concerned with their group's reputation might favor a harsher punishment for an in-group member if the certainty of guilt is high (van Prooijen, 2013, p. 64; see also Vidmar, 2002). Conversely, when guilt is uncertain, individuals will prefer a lower punishment for in-group members (Gromet & Darley, 2009, p. 53; van Prooijen 2006).

Finally, social-psychology research has shown a robust linkage between the perceived legitimacy of punishment and that of the institutions imposing it (e.g., Thibaut & Walker, 1975; Tyler, 1984, 1987; Tyler, Casper, & Fisher, 1989; Walker & Lind, 1984; see also Baird, 2001). In the context of TJ analysis, several scholars have documented the effects of TJ trials on the legitimacy and the public image of national judiciaries, when the latter are directly involved in trying representatives of the old regime (e.g., Gonzalez & Ocantos, 2016; Huneus, 2010; Pion-Berlin, 2004). More generally though, such judgment on the legitimacy of the punishment imposed on defendants typically transfers to the new regime, given the strict connection between the perception of TJ policies and the legitimacy of the new democratic regime shown by a large amount of scholarly work on TJ (e.g., Choi & David, 2012; David, 2003; Łos, 1995; Méndez, 1997; Olsen et al., 2010a).

Hypotheses

In the context of early postwar West Germany, the public is very likely to have perceived targets of denazification as in-group members—in particular, as *Germans*. In principle, bias in favor of in-group members can be triggered by almost any type of group boundaries (Tajfel, 1982), and the politically divisive nature of TJ processes may well create new divisions or reinforce existing ones (Nalepa, 2010). However, recent research in political science has shown that when salient, an overarching national identity

can provide a stronger source of identification than smaller social groups (e.g., Robinson, 2016; Transue, 2007). The circumstances in which denazification was designed and implemented—under the supervision of foreign occupiers following military defeat—are likely to have increased the salience of national identity over other potentially significant social boundaries (Sa'Adah, 2006, p. 309).⁶

Regarding the perception of defendants' guilt, it is likely that most respondents of the surveys that we analyze harbored substantial uncertainty about the guilt of most individuals targeted by denazification. The issue of the perception of guilt on the part of the German population has been the object of long-standing debate that has explored its many dimensions, a full account of which is impossible here.⁷ During the 1950s, the period that is most relevant for our analysis, various types of evidence show that the majority of Germans rejected conceptions of "collective guilt" that the Allies (in particular the United States) wanted to affirm. When faced with the atrocities of the Nazi Regime, most Germans clearly distinguished between actual perpetrators and the rest of the population, emphasizing that the totalitarian nature of the regime did not allow any opposition or dissent on the part of common citizens.⁸ At the same time, the majority of Germans agreed that no realistic choice of noncompliance existed even for most individuals who were directly involved in the Nazi regime's repressive apparatus. Indeed, the well-known use in the Nuremberg trials for top Nazi officials of the line of defense whereby denazification targets were "following orders" (e.g., Davidson, 1966, pp. 352, 411) resonated with diffuse attitudes in German society.⁹

This attitude emerges clearly in the widespread skepticism surrounding Allies-run war crimes trials, which involved several thousand members of the *Wehrmacht*, and which at times reached wide public resonance in the media. For example, when former Major General Hermann Ramcke declared, speaking before a meeting of former SS officers in 1952, that the list of German war criminals was an honor roll, and that the Allies, who bombed cities, killing women and children, were the real war criminals, more than half of a national sample of West Germans found his comments essentially (31%) or partly (25%) correct, while only one respondent out of four rejected them (R. Merritt, 1995, p. 163). Several surveys from 1950-1952 confirm the widespread acceptance of the view that most targets of war trials were largely following orders—a view that was easily extended to denazification trials, which were easily associated with war trials as both were seen as the consequence of military defeat (R. Merritt, 1995, pp. 163-164, 167-169, 172).¹⁰ In November 1953, a survey of the *Institut für Demoskopie Allensbach* found that 40% of respondents (half of those with an opinion on the matter) saw denazification either as "unnecessary and badly carried out" (26%) or even as

mere “bullying” (*Schikane*) on the part of the Allies (14%). An additional 23% thought that denazification, although necessary, had been badly carried out. Only 5% of respondents had an unequivocally positive judgment on denazification (Noelle & Neumann, 1956, p. 142).

Not only survey data testify to the skeptical attitude toward the guilt of defendants in denazification trials among large sectors of the German population. The same attitudes are revealed, for example, by the enormous editorial success of a book such as Ernst von Salomon’s autobiography *Der Fragebogen*, published in 1951. The title of the book refers to the denazification questionnaires (*Fragebogen*) that the Allies forced many Germans to complete to report their activities during the years of the Nazi regime. The book takes some distance from Nazi “excesses” but portrays denazification in a near-parodic fashion, equating the Nuremberg International Tribunal with Nazi political tribunals, and considering the behavior of the Allies during the initial years of denazification simply as the expression of a different autocracy following the demise of the Nazi regime (Olick, 2005, pp. 122-123; Parkinson, 2015, p. 94). The book disavowed any form of “collective guilt” and described the options available to common citizens during the Nazi regime as “behaving like a fool or as a coward” (i.e., incurring the harsh sanction that would have certainly derived from opposing the regime, or complying). As the book’s sales illustrate,¹¹ this view resonated with large strata of German society at the time, and so did the nationalist ideology that constitutes the background of the narrative (e.g. Foschepoth, 1997; Moeller, 2002; Trevor-Roper, 1952, pp. 76-79).¹²

The rejection of collective guilt and the diffuse conviction that even the guilt of those who were involved in the Nazi repressive apparatus was uncertain since they were following orders, together with the identification of defendants in denazification trials as in-group members, grounds the expectation that Germans were more likely to consider harsh punishments (to prison, labor camp, or even death) as less proportional than light punishments, such as monetary sanctions or professional demotions. Our first hypothesis is therefore as follows:

Hypothesis 1 (H1): More widespread harsh punishments are less conducive to democratic support than more widespread light punishments.

The effect of TJ outcomes on democratic attitudes of the mass public hinges on respondents being *informed* about the judicial fate of other individuals (e.g., Gibson, 2009, pp. 131-132). Information about proportionality of outcomes affects the attitude of punishment targets (e.g., van den Bos, Lind, Vermunt, & Wilke, 1997), and the argument can easily be extended to

individuals not directly involved in trials. At the time, radio and newspapers, and the participation in social activities represented the main sources of information and political awareness. Hence, the broad patterns predicted by our theoretical framework in the circumstances characterizing West Germany at the time—that is, more widespread light punishment increasing support for democracy among members of the public and more widespread harsh punishment decreasing it—should be *more pronounced* among more informed and socially connected individuals.

Testing the impact of media and social connections on attitudes toward punishment is also important given the structure of our data, as our treatment variable (patterns of punishment) is only measured at a high level of aggregation (the *Land*). As well-established in the literature, predicting individual effects of a treatment variable that is geographically highly aggregated may yield spurious effects (e.g., Wong, Bowers, Williams, & Drake, 2012, pp. 1155-1157). Therefore, our confidence in our main findings increases if the attitudinal effects of regional punishment patterns are stronger for individuals who are more likely to be well-informed about these patterns. At the time, although national outlets existed, German media were mainly local and regional, which increases the probability that individuals would be aware of the pattern of denazification punishment in their respective *Land* (Bösch, 2015; Lange-Quassowski, 1982; Lindemann & Koszyk, 1966, pp. 88-89). Similarly, a higher frequency of social interactions, which were also mostly locally bounded, would also facilitate the transmission of information about the local prevalence and severity of denazification punishments. This yields the following predictions:

Hypothesis 2a (H2a): The effects of TJ punishment are stronger among individuals with higher levels of media consumption;

Hypothesis 2b (H2b): The effects of TJ punishment are stronger among individuals who participate more in social activities.

Research Strategy and Case

As mentioned, we analyze subnational variation in denazification outcomes between the U.S., the U.K., and French occupation zones in the historical case of Western Germany, where the Allies' military authorities held sovereign power until 1949 (e.g., Merkl, 1982).¹³ One advantage of a subnational design over cross-national ones is that it allows keeping constant at least some of the predictors pertaining to rival explanations (e.g., Snyder, 2001). Furthermore, importantly for our identification strategy, both the establishment of the zone boundaries and the general approach to denazification on

the part of the Western Allies were largely exogenous to the preferences of the German citizens in the respective occupation zones. Therefore, comparing political attitudes across *Länder* of the three allied occupation zones allows us to avoid the endogeneity concerns inherent in most efforts to assess the effects of TJ approaches. However, as we discuss in greater detail below, the three occupation zones were geographically clustered (and not randomly assigned), which means that unlike in an experimental design our analysis needs to account for alternative explanations of democratic support.

The Drawing of the Occupation Zones and the Allies' Denazification Policies

Convergent historical accounts show that the zones' boundaries were based on considerations that were largely indifferent to the preferences of the German citizenry (e.g., Harrington, 2009; Mosely, 1950; Sharp, 1975). The boundaries of the Soviet zone were agreed in January 1944 based on the expected extent of the Red Army's advance. After that, the U.S. and the U.K. governments had a 9-month diplomatic dispute on the right to occupy the northwestern regions of Germany, to exploit the strategic and logistical advantages of controlling the German ports in those regions (Sharp, 1975, pp. 56-70). The dispute was settled only in September 1944 by granting the United States direct control of the Bremen enclave and its port, Bremerhaven, plus the rights of access to them through the British zone (Mosely, 1950, pp. 596-597).¹⁴ The French zone was then carved out from the other two Western zones, again based on logistical considerations of the U.K. and U.S. governments (Willis, 1962, p. 96). The British ceded the southernmost part of their zone, while the boundary with the U.S. zone reflected the American intention to control the Stuttgart-Karlsruhe highway. The ensuing partition of both Baden and Württemberg, historical territories that had conserved a modicum of regional identity, was at odds with both German preferences and the declared U.S. aim to encourage federalism in Germany by preserving historical boundaries—but again logistical considerations were paramount (Mosely, 1950, p. 600).

Despite convergence on the legal framework of denazification, discussed below, the American, British, and French occupation governments approached denazification in largely distinct ways, which again were independent from the preferences of German citizens in their zones. Their approaches were shaped by the strategic considerations that they attached to the occupation of Germany in the context of their foreign economic and security policies, as well as by public opinion at home (e.g., Biddiscombe, 2007; Fitzgibbon, 1969; Fürstenau, 1969; Vollnhals, 1991).

In the U.S. zone, a retributive attitude prevailed. Roosevelt initially supported the Morgenthau Plan, which envisaged harsh and encompassing purges together with the partition and deindustrialization of Germany. Moderate voices in the U.S. administration could only move the President away from these initial positions by designing a highly punitive denazification policy, as shown in the harsh rules on denazification that the U.S. Military authorities issued in the first months after capitulation (e.g., Bloxam, 2004, p. 27; Biddiscombe, 2007, pp. 30-32).¹⁵ A large majority of the American public supported retributive measures. For example, in May 1945, 35% of Americans thought that Germany should be harshly punished, an additional 45% that it should be supervised, controlled, and disarmed, while fewer than 10% favored rehabilitation and reeducation (R. Merritt, 1995, pp. 41-43).¹⁶

In the United Kingdom, although positions favoring harsh retribution existed, more pragmatic views on denazification were predominant (e.g., Biddiscombe, 2007, p. 84; Bower 1981, p. 196; Jones, 1990, pp. 147-150). This attitude reflected important strategic concerns. Already in the summer 1946 (earlier than its allies) the U.K. government followed a policy of stabilizing the division between a Western German state under the influence of the United States, and the eastern part of Germany, as best suited to British interests (Foschepoth, 1986). A further consideration was that leaving in place part of the German administrative and economic machinery would improve economic efficiency and reduce costs for the U.K. Treasury (Cohen, 2006, p. 77; Turner, 1989). And even though U.K. public opinion was unsurprisingly hostile toward the Germans, even in 1944 only about one third of Britons wanted the Allies' treatment of Germany to be harshly punitive (Biddiscombe, 2007, p. 84). In November 1945, more than one third of respondents either "sympathized" with the fate of Germans or were "indifferent" to it (Institut Français d'Opinion Publique [IFOP], November 16, 1945, p. 216).

Foreign and security policy priorities, recognizable throughout the occupation period despite the divisions among French policy-makers, also shaped the French government's approach to denazification. The experience of three destructive wars against Germany during the previous 70 years led many to advocate for the dismemberment of Germany or at least for an extremely decentralized federal system, the exploitation of the economy of the occupied zone to provide reparations, and the necessity of reeducating Germans to French values of freedom and democracy (Grohnert, 1991; Willis, 1962, pp. 95, 126, 148). In September 1944, 88% of French respondents agreed that Germany should be forced to pay reparations for war damages. Only 6% disagreed (IFOP, April 1, 1945, p. 81). In August 1945, 78% supported the dismemberment of Germany, while 12% disagreed (IFOP, September 1-16, 1945, p. 173). The denazification program was designed to pursue these

overarching goals. Its implementation was decentralized, with no common zonal approach until 1947 (e.g., Fürstenau, 1969, p. 135). Like in the British zone, French authorities saw a tradeoff between purges and the necessity to have a functioning economy and administration, which was important to resource extraction. Finally, the French explicitly rejected collective conceptions of guilt—with automatic sanctions for particular categories of individuals—in favor of considering each individual case on its merit (Biddiscombe, 2007, p. 158; Grohnert, 1991; Henke, 1983).

Patterns of TJ Punishment in the Western Zones

These different approaches were reflected in the denazification outcomes in the three occupation zones. Broadly speaking, denazification went through two phases. In the first phase, military authorities directly implemented restrictive measures, including arrests, internments, and dismissals. In the later phase, the Allies put in place quasi-judicial procedures that included German representatives. The occupiers' different approaches to denazification are visible through both phases. The U.S. authorities adopted the strictest attitude of all, aiming, at least initially, at fully eradicating Nazi elements from society (Turner, 1989). In the first phase, the United States adopted much harsher and more encompassing purges than its Western allies, generally without much consideration of whether the individuals purged, if a public employee, could be replaced (e.g., Fürstenau, 1969, pp. 103-104; Vollnhals, 1991, p. 11). Furthermore, the *whole* adult population of the U.S. zone had to fill out questionnaires (*Meldebogen*) about their Nazi past, which then constituted the basis for prosecution (Olick, 2005, p. 125). In the U.K. and French zones, instead, only individuals employed at senior levels in public and semi-public administrations were required to fill out questionnaires (*Fragebogen*; e.g., Biddiscombe, 2007, p. 168; Vollnhals, 1991, p. 26).¹⁷

Their intention of preserving a functioning administration led the British authorities to target the smallest number of individuals of all three zones (e.g., Biddiscombe, 2007, p. 115; Bower 1981). Hence, while hardcore Nazis were generally prosecuted, individuals who had useful technical or administrative competences were excluded from prosecution, and even industrialists who had significantly helped the German war effort were treated leniently (Biddiscombe, 2007, pp. 98-101; Bower 1983, p. 140, 323; Turner, 1989, pp. 256-257). The French authorities' decentralized approach meant that initially each *Land* in their zone implemented partially different denazification systems (e.g. Grohnert, 1991; Henke, 1983; Vollnhals, 1991, pp. 36-42). Overall, denazification in the French zone involved, in percentage terms, more individuals than in the other

two zones, but the severity of the sanctions was overall rather mild (Biddiscombe, 2007, p. 172; Fürstenau, 1969).

For reasons of costs and feasibility, the occupying powers eventually switched to the quasi-judicial procedures that are the focus of this analysis.¹⁸ The new system was established in March 1946 in the U.S. zone with the so-called “Liberation Act” (*Befreiungsgesetz*), and shortly afterwards adopted, with some procedural differences, in the U.K. and French zones (Fürstenau, 1969; Grohnert, 1991, pp. 109-111, 144).¹⁹ Conceptually, this strand of denazification policies presents some hybrid characteristics. Akin to “lustration” policies (Nalepa, 2010), many individuals were punished for their membership in various Nazi organizations, or for their position in the civil service. At the same time, however, the adjudication bodies (*Spruchkammern*) also targeted individuals for explicit *acts* (Herz, 1982, p. 17). Evidence of having belonged to the NSDAP (National Socialist German Workers Party) or other organizations was seriously considered in reaching a verdict, but sanctions could in principle be adjusted upward or downward based on the defendant’s behavior and actual level of responsibility during the Nazi period. Furthermore, the sanctions meted out by the *Spruchkammern* went beyond the restrictions on employment and public office of most lustration laws and entailed restrictions of various other rights, including prison or death sentences for the higher categories of guilt.

Under the *Spruchkammern* system, witnesses could be heard and the accused had a right to a defense (the burden of proof, however, was reversed: guilt was presupposed and the defense had to prove the innocence of the accused; e.g., Vollnhals, 1991, p. 17). The 545 *Spruchkammern* were staffed by about 22,000 members of the new or reconstituted German democratic parties that had been meanwhile licensed by the Allies (Olick, 2005, p. 128; see also Capoccia & Pop-Eleches 2019b). Their decisions could be appealed before a chamber presided over by a judge or a lawyer with the same level of legal training.

The Directive N. 38 of the Control Council (an inter-allied body), approved in October 1946 and adopted shortly afterward in all three zones, classified defendants in denazification trials in five categories on the basis of their level of involvement with the Nazi regime and responsibility for Nazi crimes: “Major Offenders,” “Offenders,” “Lesser Offenders,” “Fellow Travelers,” and “Exonerated Persons.” The Directive spelled out the range of sanctions for each category. The *Spruchkammern* decided how to classify individuals, passed judgments, and imposed sanctions ranging from the death penalty or hard labor for “Major Offenders” to fines and some rights restrictions for “Fellow Travelers.”²⁰ The adoption of common categories of guilt and corresponding sanctions allows comparing the percentages of individuals affected by different levels of TJ punishment in each *Land*, which is our

treatment variable. Figure 1 visualizes how widespread the different levels of punishment were in each *Land* and occupation zone. In both Figure 1 and our analysis, we merge the two categories of “Major Offenders” and “Offenders,” due to the small number of convictions in either category and the similarity of the sanctions.²¹

The maps show a marked difference between the three zones with “Major Offender” and “Offender” convictions in the U.S. zone significantly higher than in the other two zones. The territorial distribution of the convictions of “Lesser Offenders” confirms the pattern: The trials in the U.S. zone show consistently more severity (with the partial exception of the Bremen enclave), whereas sentencing in the other two zones (in particular, in the French one) displays more marked cross-*Land* differences. Finally, convictions of “Fellow Travelers” were also roughly twice as high in the U.S. zone as in the U.K. zone. The much higher values for the *Länder* of the French zone illustrate that denazification there was most encompassing but entailed rather lenient sanctions.²²

By mid-1948, internal criticisms of the *Spruchkammern*, which were increasingly accused of corruption, favoritism, and political bias (e.g., Niethammer, 1982; Olick, 2005, pp. 128-132), and most importantly mounting tensions with the USSR (Union of Soviet Socialist Republics) induced the Western Allies to wind down denazification. With the creation of the Federal Republic in 1949, formal competences were passed to the *Länder*, which in some cases quickly passed laws to terminate denazification. In October 1950, the *Bundestag* issued a “formal advice” to the *Länder* to end all trials except for “Major Offenders” and “Offenders,” and at the same time to reduce the sanctions for individuals in these categories and allow them to apply for inclusion in a lower category (Frei, 1997, pp. 54-69). By 1953 all *Länder* had adapted their legislation to these recommendations (Fürstenau, 1969, p. 158).

Data and Variables

To test the impact of these differential denazification policies in the Western occupation zones, we present evidence from three public opinion surveys from 1953 to 1957. The 1953 and 1957 surveys are part of the *Institut für Demoskopie Allensbach* election studies, while the 1955 study was commissioned by the U.S. Information Agency.²³ These surveys are an invaluable yet underused data source to test the short- to medium-term consequences of the patterns of denazification punishments in the different areas of West Germany.²⁴

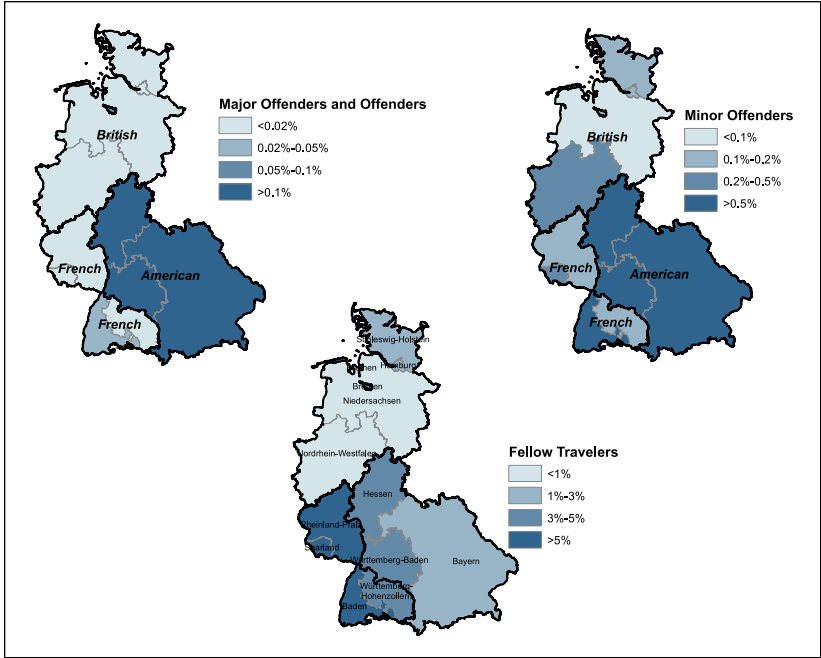


Figure 1. Percentage of convictions in each *Land*. Data on convictions at August 31, 1949. U.K. zone data for Major Offenders and Offenders from Cohen (2006, p. 71). All other data are from Vollnhals (1991, p. 33). Authors' elaboration. Thicker lines mark zone boundaries, and thinner lines *Land* boundaries. Maps exclude West Berlin occupation sectors.

We use two dependent variables. The first is based on a set of questions that capture respondents' views about the desirability of a one-party political system. Given the nature of the Nazi regime, support for such a system should be higher among those with nostalgia for the old regime and should be lower to the extent that postwar authorities were successful in winning over the hearts and minds of German citizens.²⁵ Despite some wording differences, this question has the advantage of having been asked in all three surveys, thereby allowing us to assess the durability of TJ treatment differences across different parts of West Germany. To facilitate interpretation, the variable is coded dichotomously, with "1" denoting opposition to a one-party regime, so that positive effects indicate stronger democratic support.

The second dependent variable is a democracy index constructed from four questions asked in the 1957 survey. Besides the one-party question discussed above, this index is based on three additional questions: The first

asked how respondents would react to the revival of the National Socialist Party in Germany and allowed for five options (active support, passive support, indifference, passive opposition, and active opposition); the second asked respondents to choose between a statement that advocated giving all the power to the “best” politician so that “things would get done,” and another statement indicating a preference for collective decisions to avoid power abuses; the third question asked whether Germany needed a parliament.²⁶

As mentioned above, our treatment variable is the percentage of the population in each *Land* represented by individuals punished harshly (as Major Offenders and Offenders, whom we consider together, and to a lesser extent as Lesser Offenders) or leniently (as Fellow Travelers). As we lay out in more detail in Table A2 in the supplemental appendix, we consider individuals convicted as Major Offenders and Offenders as subject to harsh punishment as they were sanctioned with prison or labor camp sentences, and in some cases with the death penalty. Lesser Offenders were held in a probation period of 2 to 3 years during which their freedoms were significantly limited and at the end of which they would be classified either as Offenders or as Fellow Travelers, depending on their conduct. Sanctions imposed on Fellow Travelers were instead significantly more lenient, entailing (and not in all cases) only limited restrictions of freedom and mainly pecuniary losses or professional demotions.

Information included in the surveys allows us to assign to each respondent *Land*-specific denazification conviction rates and other covariates. Even though the boundaries of the Western occupation zones were not endogenous to the political preferences of German citizens, the *Länder* in the three occupation zones may still differ along a number of dimensions that may shape evaluations of the TJ process, the Nazi past, and subsequent political attitudes. To reduce the potential for omitted variable bias arising from other policies (besides denazification) of the Allies in their respective zones, we rerun our models with dummy variables for the three occupation zones, and our findings are unchanged (Supplemental Appendix, Table A15). Other potentially important variables vary within zone and across *Länder*. First, given that support for the Nazi regime was generally stronger in northern Germany, we need to control for the extent of this support in the different *Länder* prior to the allied occupation. No geographically disaggregated data exist on the levels of support for the Nazi regime at the end of the war and before the start of denazification. Therefore, we control for the vote shares of the Nazi party in the July 1932 *Reichstag* elections in each of the postwar *Länder*.²⁷

A further important factor was the share of expellees from the Eastern territories that had been historically part of Germany and that were lost in the

war. Expellees matter in the context of denazification policies because many of them hailed from areas of the *Reich* that had heavily supported the Nazis in the 1930s, so they had a higher probability of being sympathetic to right-wing appeals. Furthermore, the traumatic experience of being displaced from their homes by the Soviets and their allies made them promising targets for the various parties of the *nationale Rechte* that wanted to relativize war guilt by pointing to the suffering of Germans. While we capture both factors above by including an individual-level indicator of expellee status in our regressions, we expect a large presence of expellees in a *Land* to place additional stress on native residents and make them more susceptible to nationalist appeals.

Finally, to ensure that political attitudes were not driven by differences in short-term economic performance across different parts of Germany, we control for *Land*-level unemployment rates in the year preceding the survey. Because the variables we have discussed so far only vary at the *Land level*, our regression results present standard errors clustered at that level to reflect the lower number of degrees of freedom at that level.²⁸ In addition to these *Land*-level controls, all the regression models include individual-level controls for demographic indicators that may affect political attitudes: age cohorts, occupational categories, sex, marital status, education, size of locality, income, and religious denomination. All of these are operationalized as a series of dummy variables to avoid imposing arbitrary linearity assumptions.

In subsidiary analyses, we control for other potentially important *Land*-level political covariates, such as the Allies' licensing of extreme right parties and the popularity of occupation troops (R. Merritt, 1995; see Supplemental Appendix, Tables A13 and A14). We also control for the differences in the specific procedures through which the three Allies' military governments implemented the common legal framework described above. To ensure that our findings are not driven by such procedural differences, we capture these in an index and show that our results are robust to cross-*Land* variation in said index (Tyler, 1988; see Supplemental Appendix, Tables A3 and A3.1 for definition, measures, and data sources, and Table A10 for the analysis). Finally, to account for the potential influence of the reemerging German democratic politics at the local level, we also control for the result of the 1946-1947 regional elections in the Western *Länder* (Supplemental Appendix, Table A12). Our results are robust to all these alternative specifications.

We test the role of mass media and social interactions as mechanisms for disseminating information about denazification with a series of questions from the 1953 and 1957 surveys. For mass media, we use 6-point measures of the frequency of reading newspapers and listening to news on the radio (ranging from *never* to *daily*.) For social interactions, we used a dichotomous

indicator of whether respondents reported having many acquaintances, as well as an indicator of whether they frequently went bowling.²⁹

Analysis

In Table 1, we test the effects of denazification outcomes on democratic support. The first three sets of regressions are probit models testing the impact of denazification on opposition to one-party regimes in the 1953, 1955, and 1957 surveys respectively, while the final set presents ordinary least squares (OLS) regressions on the democracy support index for the 1957 survey. Each set consists of a model that includes the percentage of individuals in each Land punished as Major Offenders and Offenders (that we consider together), on the one hand, and as Lesser Offenders, on the other, as measures of “harsh punishment”. We include these predictor variables separately given their collinearity.

The results in Table 1 provide consistent support for H1 across the three surveys and two different democracy measures: More widespread convictions in the two highest guilt categories (with harsher punishment) had statistically significant negative effects on democratic support, and this negative effect even applied to the intermediate category of “Lesser Offenders.” By contrast, more extensive “Fellow Travelers” convictions promoted democratic support, with statistically significant effects in six of the eight models.³⁰ In other words, our findings suggest that in line with H1, Germans were more supportive of democracy in areas in which more individuals were punished lightly (as “Fellow Travelers”), and held more anti-democratic attitudes in areas in which a higher number of individuals were targeted with the harsher punishments imposed on higher guilt categories.³¹

The next two hypotheses posited that the effects of official conviction rates on democratic attitudes would be more pronounced among individuals who were more likely to be aware of the nature of denazification outcomes based on either their mass media consumption (H2a) or social interaction patterns (H2b). To test these predictions, we interacted the *Land*-level conviction rates with the indicators of individual media exposure and social interaction discussed in the previous section. To facilitate the interpretation of results, Figure 2, which is based on data from the 1957 survey, illustrates the predicted effect on the democracy support index of a change from the 10th to the 90th percentile in combined conviction rates of Major Offenders and Offenders, on one hand, and the more lenient punishment category of “Fellow Travelers” for individuals at either the low or the high end of media and social exposure.³² Although not all the interaction terms were statistically significant at conventional

Table 1. Denazification Outcomes, Procedures, and Democratic Support.

Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Opposed to one-party system						Democracy index	
	1953	1953	1955	1955	1957	1957	1957	1957
Offender convictions	-1.219** (.397)		-1.016† (.592)		-2.842** (.816)		-.386** (.122)	
Lesser offender convictions		-.274* (.110)		-.258† (.137)		-.588** (.180)		-.083* (.031)
Fellow traveler convictions	.043** (.016)	.044* (.020)	.062* (.029)	.070* (.028)	.076* (.034)	.077* (.036)	.006 (.005)	.006 (.005)
Land-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R ²	.079	.079	.054	.054	.068	.067		
R-squared							.104	.103
Observations	2,523	2,523	1,479	1,479	1,968	1,968	1,954	1,954

Standard errors clustered at the Land-level in parentheses.

†p < .1. *p < .05. **p < .01.

levels, the overall pattern is quite clear: Both the antidemocratic impact of more widespread “Offender” convictions and the prodemocratic effects of broader “Fellow Travelers” sanctions are more pronounced among respondents with greater mass media consumption and more active social interactions. For frequent newspaper readers and radio listeners, the effects of higher *Land*-level conviction rates were statistically significant and fairly substantial, while individuals with minimal mass media usage were unaffected by denazification outcomes. Similarly, respondents who reported having many acquaintances, or frequently going bowling, were quite responsive to regional variations in denazification outcomes, while the effects were substantively smaller and statistically insignificant for less socially connected respondents. We obtain very similar results (albeit using different survey questions) from the analysis of the 1953 survey (Supplemental Appendix, Table A24).

In addition to highlighting some of the mechanisms through which TJ outcomes affected public attitudes toward democracy, these results strengthen the internal validity of our argument because they suggest that the effects of regional patterns of punishment on democratic support are much more pronounced among individuals with higher likelihood of having greater information about denazification outcomes. Conversely, the attitudinal effects among respondents with limited social interactions and mass media exposure can be interpreted as placebo tests and thus, the weak impact of *Land*-level conviction rates on such individuals in Figure 2 is reassuring.

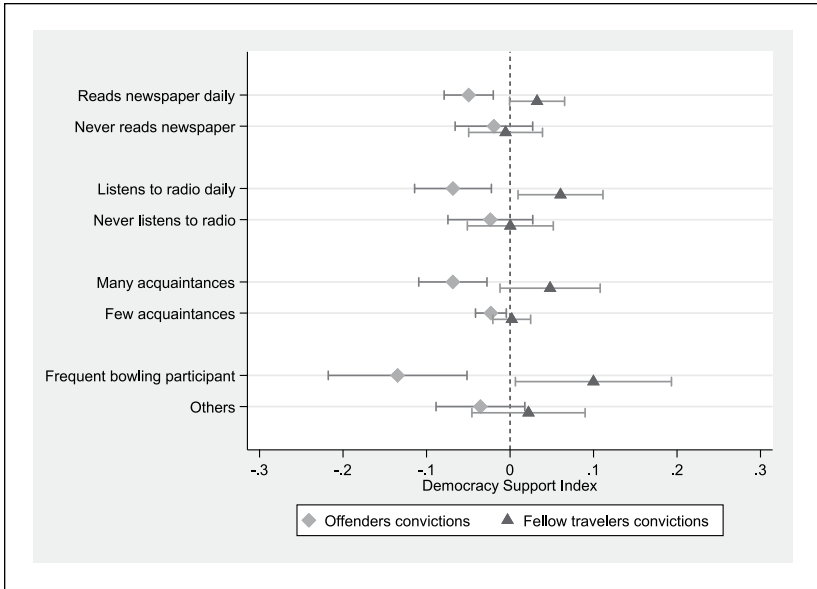


Figure 2. Denazification outcomes and democracy support: Media and social context.

Figure 2, based on the regression in Table A23 in the Supplemental Appendix, illustrates the predicted effects on the democracy support index (with 95% confidence intervals) of going from the 10th to the 90th percentile in conviction rates for individuals with high versus low mass media consumption and social interactions. The figure shows that the antidemocratic effects of more widespread Offender convictions (which here include, as explained above, the number of convictions in both the official category of “Major Offenders” and that of “Offenders”) and the prodemocratic effects of more widespread Fellow Travelers convictions are much more pronounced for respondents with daily radio/newspaper exposure and active social lives.

Conclusion

Over the past three decades, the practice of setting up TJ programs to “address past wrongs” after transitions from authoritarianism, civil wars, or other mass atrocities has become widespread (Sikkink, 2011). Indeed, a large international epistemic community promotes the idea that enacting TJ programs helps prevent the recurrence of human rights violations, consolidate the rule of law, promote reconciliation, and improve democracy. The literature on the effects of TJ on democratic legitimacy explores different causal pathways through which these effects may be expected to take place, but lacks robust conclusions on exactly what TJ programs can be expected to have

unequivocal positive effects on democracy. In this article, we join a number of scholars who have focused on the attitudinal effects of TJ programs on the mass public (e.g., Aguilar, Balcells, & Cebolla-Boado, 2011; Backer & Kulkarni, 2016; Gibson, 2002; Gonzalez Ocantos, 2016; Nalepa, 2010), and analyze in an important historical case the consequences on mass attitudes toward a new democracy of a core aspect of most TJ programs in post-authoritarian democracies: trials and punishment of representatives and supporters of the previous regime. To do so, we build on established findings in the social psychology of criminal justice that the perception of punishment as *morally proportional* to the offense shapes individual attitudes toward the legitimacy of the punishment itself and the institutions imposing it. Our analysis of post-World War II West Germany confirms that these propositions may also apply to TJ programs: Prodemocratic attitudes of the German public were strengthened in areas where the punishment regimes were more in line with popular views of the degree of guilt of defendants in TJ trials, and weakened otherwise. These findings are consistent across three separate surveys spanning a 5-year period roughly a decade after the peak of the denazification process. The consistency of these findings increases our confidence in their reliability, and substantiates the medium-term temporal persistence of attitudinal consequences of TJ programs, which is broadly on par with the temporal patterns of historical legacy effects on democratic attitudes in other contexts (e.g., Pop-Eleches & Tucker, 2017, pp. 247-281).

What are the implications of our analysis for future research on the effects of TJ on democratic legitimacy? To test the moral proportionality hypothesis on other cases, future research would first have to establish how the mass public predominantly perceived perpetrators and their guilt, and then analyze the extent to which the match between these perceptions and TJ punishments correlates with attitudes toward democracy.

To measure punishment, future research would have to concentrate on the analysis of the *implementation* of TJ programs, rather than their formal design, which is the approach that still dominates many analyses. Problems of data availability may be significant in some cases, but scholars have started to tackle these challenges (e.g., Payne & Sikkink, 2010; Vinck & Pham, 2010). The challenges of ascertaining the predominant views of which level of punishment of perpetrators in TJ trials is considered as morally proportional are also likely to be substantial but not insurmountable. To be sure, the social conditions in which many contemporary TJ programs are implemented (e.g., in post-conflict situations) may render it impossible for researchers to generate reliable data on mass attitudes (Backer & Kulkarni, 2016). However, it bears reminding here that even when the social context could in principle make survey research practically viable, mass orientations on the moral

proportionality of TJ punishment may only be ascertained *indirectly*. Indeed, asking direct survey questions on what punishment is considered adequate and why is widely considered by psychologists to be an unreliable method, as the deeply intuitive nature of the judgment of moral proportionality typically makes the judgment in question impervious to introspection (e.g., Carlsmith, 2006, pp. 438-439; Carlsmith, 2008, p. 121; Carlsmith & Darley, 2008, p. 198; Darley, 2009, p. 4; Nisbett & Wilson, 1997). Even though strategies of qualitative and experimental inquiry may be open to researchers to gain a sense of what views of proportionality may be common among the population, survey questions may be more useful to establish the typical contextual circumstances that are most likely to affect proportionality perceptions: the level of certainty that respondents hold about the defendants' guilt, and the perception of the defendants as in- or out-group members. The latter, in particular, is likely to be much more nuanced in cases of ethnic conflict, and more generally whenever TJ punishment is imposed by domestic actors than in the case analyzed here, where the predominance of foreign occupiers in initiating TJ gave particular salience to national identity. As discussed in the article, these factors can jointly generate different predictions about the effect of TJ punishment on democratic attitudes that can then be tested through the analysis at close range of theoretically interesting cases.

In this respect, scholars interested in testing the validity of our findings in other contexts need not limit themselves to the analysis of contemporary cases of TJ, or to the analysis of cases that are recent enough to allow questioning direct witnesses of TJ programs. *Historical* cases of TJ, such as the one analyzed here, provide a trove of still largely untapped quantitative and qualitative evidence, both primary and secondary. Through the analysis and the triangulation of historical surveys, historical statistics, newspapers and other publications of the time, and other archival sources, researchers wishing to test the "moral proportionality of punishment" hypothesis on historical cases of TJ may form a reliable image of the social categorization of defendants and the conceptions of their guilt that were held by different strata of the population. The challenges of collecting and analyzing such historical evidence are not necessarily harder than those that researchers analyzing contemporary cases of TJ are likely to face. Indeed, such challenges are likely to be similar to those routinely faced by scholars working on other historical aspects of democratization (Capoccia & Ziblatt, 2010) who have analyzed different facets of the historical evolution of democratic institutions including electoral systems, taxation regimes, among many others (e.g., Kreuzer, 2010; Mares & Queralt, 2015). The analysis of causes and consequences of historical cases of TJ policies would be a welcome addition to the analysis of

the contested and piecemeal process through which democratic regimes are painfully, slowly, and more or less successfully constructed in the long run.

Extending our outlook to the past, by collecting and analyzing new historical evidence, and re-appreciating old evidence in light of new theoretical questions, would have important payoffs not only for the study of democratization but also for the analysis of TJ in its own right. As the few existing historical overviews already demonstrate (most importantly, Elster, 2004), the enlargement of our comparisons to important historical cases is likely to reveal variation on important aspects of TJ that may provide important insights for the analysis of contemporary TJ programs, and to raise new questions that may inform new policy-relevant research on TJ. Much work remains to be done, but the challenge seems well worth taking up.

Acknowledgments

For comments and feedback on previous versions of this paper we thank Mark Beissinger, Jennifer Dixon, Camilo Ehrlichman, Monika Nalepa, and Elsa Voytas as well as participants in panel discussions at meetings of the American Political Science Association and the Council for European Studies. We also thank the audiences of invited talks at Princeton University, University of Texas at Austin, College of William and Mary, University of Warwick, and McGill University.


Declaration of Conflicting Interests


The authors declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The authors received no financial support for the research, authorship, and/or publication of this article.

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Supplemental Material

Supplemental material and replication files for this article are available online at the *CPS* website <http://journals.sagepub.com/doi/suppl/10.1177/0010414019852704>

Notes

1. Further causal pathways explored in the literature include, among others, the demonstrative effects of high-profile trials on democratic norms (e.g., Lutz &

- Sikkink, 2000, 2001) and the legal empowerment of marginalized groups (e.g., Kurze, Lamont, & Robins, 2015).
2. A few studies also discuss the implementation of TJ policies and their consequences (e.g., Mallinder, 2008; Subotic, 2006).
 3. This is not necessarily the case for other potential subpopulations affected by TJ trials, such as defendants, whose views on the legitimacy of the trial's outcome are likely to be influenced by the fairness of the formal procedures adopted (e.g., Tyler, 1984) and by consideration of distributive justice (on TJ, see Capocchia & Pop-Eleches, 2019a), or victims, the focus of an emerging literature on TJ programs (e.g., McEvoy & McConnachie, 2013; Mendeloff, 2009; Pham et al., 2016; van der Merwe, 2009). Here we focus on the attitudes of the mass public. For a general discussion of the psychological effects of punishment on different audiences, see Vidmar and Miller (1980).
 4. Although we acknowledge that TJ trials have an irreducible political significance as they are intimately connected to the transition between two often radically different political orders, we share the analytical perspective of several scholars in the field who have argued that the procedural and symbolical similarities of TJ trials with ordinary justice processes render them amenable to be studied with the same analytical tools (e.g., Elster, 1998, p. 48; Gibson, 2002; Posner & Vermeule, 2004; see also Meierhenrich & Pendas, 2018, pp. 28-48).
 5. For a similar argument on a different research question, see Voigtländer and Voth (2014).
 6. As discussed below, this does not necessarily mean that most Germans identified morally or politically with the perpetrators of the worst abuses during the Nazi regime. However, we expect most of our survey respondents to identify them as co-ethnics because ethnic diversity was minimal in Germany at the time (Vogt et al., 2015), and because the Allies' definition of guilty behavior reached quite far down into German society. The salience of national identity in judging externally imposed political justice was likely further enhanced by the parallel trials for war crimes conducted by the Allies, which affected several thousand members of the German Armed Forces (e.g., Ruckerl, 1979).
 7. For a reconstruction of the debate on collective guilt in the war aftermath, which involved, among others, intellectuals of the caliber of Karl Jaspers, Carl Jung, Hannah Arendt, and Thomas Mann, see, for example, Olick (2005, pp. 270-296) and Parkinson (2015, pp. 25-65, 88-103).
 8. Even though victims of the Nazi regime certainly existed at the time, most individuals who were not directly involved with the state or party apparatus did not portray themselves as such. The Nazi regime's social penetration tended to compromise large strata of German society: Membership of many Nazi organizations numbered in millions; many Socialists, trade unionists, and other regime opponents had served in the *Wehrmacht*; and the atrocities committed under a nationalist banner often discouraged many of those who were less compromised from openly claiming victim status (Sa'Adah, 2006, pp. 308-309).
 9. Historians agree that the Nazi state ruthlessly repressed most of its real or perceived enemies, but a lively debate exists on whether actually state repression

and terror were the main sources of compliance among common citizens during the Nazi regime, as opposed to a diffuse consensus among large strata of the population (see, for example, Johnson, 2011; Loeffel, 2007). Our focus here, rather than on the actual extent of Nazi repression, is on the narrative on such repression that was predominant among the German populations during the years of the surveys that we analyze.

10. Indeed, despite a short-lived public predominance of a wholesale rejection of Nazism in the months immediately following Nazi capitulation (R. Merritt, 1995, p. 97; Trevor-Roper, 1952, pp. 227-228), scholars who interviewed German prisoners of war (POWs) or civilians, such as Janowitz (1946, pp. 143-145) or, less systematically, Padover (1946), document that the view that most members of the Nazi repressive and military apparatus had been following order was common before denazification started even among many Germans who had not supported or benefited from the regime. Furthermore, survey evidence shows that in 1949 only a minority of Germans (36%) considered Nazism a “bad idea” (21% among former NSDAP [National Socialist German Workers Party] members), while clear majorities agreed that it was “a good idea badly carried out” (Allensbach Institut für Demoskopie, 1949). Surveys administered in the U.S. zone in the early postwar years show that around 70% of respondents rejected the notion of collective guilt (A. Merritt & R. Merritt, 1970, p. 36; see also R. Merritt, 1995, p. 201).
11. The book sold 250,000 copies in the 24 months following its release, and had continuous reprints in the subsequent years—rather exceptional numbers for Germany in those years. These sales numbers are even more remarkable given that the book was 800-page long and cost 19.90 DM (Parkinson, 2015, pp. 103, 203, 212). At the time, the sum in question corresponded to the cost of about 7 hr of unskilled labor (<http://www.historicalstatistics.org/Currencyconverter.html>, accessed August 12, 2018).
12. The German public at the time had serious doubts also about their collective guilt toward the Jews: Only 5% of respondents in December 1951 admitted feeling “guilty” toward Jews, and a further 29% acknowledged that Germany owed some restitution to the Jewish people. About two fifths of respondents thought that only people “who really committed something” were responsible and should pay, and 21% thought that “Jews were partly responsible for what happened to them during the Third Reich.” When the 1952 Luxembourg *Wiedergutmachung* agreement, by which Germany committed to reparations toward Israel, was debated in the *Bundestag* on March 13, 1953, the parties supporting Adenauer’s center-right coalition were divided, with the Free Democrats abstaining and more than one hundred Christian Democratic MPs voting against (Judt, 2006, pp. 271-272).
13. For the *Länder* included in each zone, see Table A1 in the Supplemental Appendix.
14. Confirming the importance of this issue for both parties, the details of the U.S. control of these enclaves remained controversial for several months (Mosely, 1950, pp. 598-599).

15. See, in particular, the draconian measures in the “Directive N. 1067 of the Joint Chiefs of Staff” of May 1945, and the “Military Government Law N. 8” of September 1945 (R. Merritt, 1995, p. 180; Tent, 1984, p. 51).
16. The desire to punish “Germany” in abstract (with unconditional surrender, dismemberment, disarmament, and reparations) went hand in hand in the U.S. public opinion with the intention to punish *individual Germans* as well. For example, in April 1945, a Gallup survey asking “After the war should three to four million German men be sent to Russia to help rebuild destroyed cities there?” found almost five in six respondents in agreement and only 11% disagreeing. A similar question asked in January 1945 in a National Opinion Research Center survey, regarding not Russia but “devastated areas” or “some of the countries [the *Wehrmacht*] had fought against” saw 62% agreeing and only 22% disagreeing (R. Merritt, 1995, p. 42).
17. Also in the U.S. zone, every individual who sought public responsibilities or had business with the occupation authorities had to compile a *Fragebogen* between January and March 1946. About 1.4 million individuals filled such questionnaires. *Meldebogen* (which roughly translates as “self-reporting questionnaires”) were instead introduced for the whole adult population of the U.S. zone in September 1946, and formed the basis for denazification in that zone (Olick, 2005, pp. 121-122, 125).
18. The analysis of the available data on the other streams of Allies-imposed harsh TJ punishment, such as the arrests and internments of the first phase of denazification and the rate of death sentences imposed by military tribunals, confirms our main findings (see Supplemental Appendix, Tables A18 and A19).
19. See Supplemental Appendix, Tables A3 and A3.1 for a discussion of the procedures used in the three zones.
20. See Supplemental Appendix, Table A2 for the criteria for inclusion in each category and the relevant sanctions.
21. Some individuals were punished for their membership in the three Nazi organizations declared “criminal” at Nuremberg—the NSDAP, the SS, and the Gestapo. The U.S. and French authorities folded these trials in the *Spruchkammern* system. For the *Länder* included in these zones, convictions data include these individuals (Vollnhals, 1991, p. 33). In the U.K. zone, these individuals were instead tried through a separate system of *Spruchgerichte*, German-staffed tribunals instituted in British internment camps (Cohen, 2006, p. 71). To have conviction data for the U.K. zone that is comparable to the other two zones, we impute the number of defendants in the *Spruchgerichte* to denazification guilt categories on the basis of the sanctions imposed on them, as reported by Wember (1992, p. 318). See Supplemental Appendix, Table A20 for analyses using alternative estimations of high-level conviction data in the U.K. zone.
22. The amnesties implemented in the French zone during 1947, and in particular in July 1948 for many classified as “Fellow Travelers,” are part of the same approach (e.g., Grohnert, 1991, pp. 203-206).
23. For details on the surveys, see Section 1, “Data and Variables,” in the supplemental appendix.

24. It is worth noting that at the time of the surveys very few new prosecutions of ex-Nazis were initiated in ordinary German courts (Rückerl, 1979, pp. 39-71, 121). This increases our confidence that subnational variation in the more temporally proximate trials against ex-Nazis held in German courts is unlikely to have influenced respondents' attitudes.
25. "One party regime" could of course also refer to a communist regime. However, in one of the surveys the question referred explicitly to a "national" party ("eine nationale Partei"), an adjective that especially at the time was associated with the political right. Full question wording is reported in Supplemental Appendix Table A4. Moreover, higher Communist vote shares are not associated with greater one-party support (Supplemental Appendix Table A11).
26. The index was created by averaging standardized versions of the four questions using the *alpha* command in Stata 13. Cronbach's alpha for the resulting standardized index was .71. Factor analysis confirmed that the variables had one common underlying factor (with an eigenvalue of 2.2). All other factors had eigenvalues below 1.
27. Controlling for other measures of prewar Nazi support, such as the geographic provenance of the "letters to the editor" of the Nazi magazine *Der Stürmer* (Voigtländer & Voth, 2012), or the NSDAP vote in the November 1932 elections, does not change our results (Supplemental Appendix Tables A16 and A17).
28. Our findings are robust to running hierarchical models (see Supplemental Appendix Table A9). As one of the mixed effects models failed to converge, we present the clustered standard errors results in the main paper.
29. On the communal dimension of bowling, see Putnam (2000, pp. 111-113).
30. These results are also robust to excluding respondents who reported that they or their family members had been affected by denazification (see Supplemental Appendix Table A22).
31. The magnitude of these effects is moderate, but not trivial. In Models 1, 3, and 5 the effects of going from the 10th to the 90th percentile in "Major Offender" and "Offender" convictions (which corresponds to a change from .02% to .13% of the *Land* population) were associated with a decline of about 4% to 6% in the proportion of respondents opposed to a one-party regime, while in Model 7, it was associated with a decrease in the democracy index of about 15% of a standard deviation. Models 2, 4, 6, and 8 reveal comparable effects for going from the 10th to the 90th percentile in "Lesser Offender" convictions (which corresponds to a change from .05% to .61% of the *Land* population). Meanwhile, a corresponding increase in "Fellow Traveler" convictions was associated with a 5% to 6% increase in the proportion of opponents of one-party regimes in Models 1 to 6.
32. We find very similar patterns in the 1957 survey by replacing Offender (i.e., "Major Offenders" plus "Offenders" convictions) with Lesser Offender convictions. (See Supplemental Appendix Figure A1.) We report the full regression results in Supplemental Appendix Table A23.

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