

POLITICS IN THE COURTROOM: POLITICAL IDEOLOGY AND JURY DECISION MAKING

Shamena Anwar
Rand Corporation

Patrick Bayer
Duke University

Randi Hjalmarsson
University of Gothenburg

Abstract

This paper uses data from the Gothenburg District Court in Sweden and a research design that exploits the random assignment of politically appointed jurors (termed *nämndemän*) to make three contributions to the literature on jury decision-making: (i) an assessment of whether systematic biases exist in the Swedish *nämndemän* system, (ii) causal evidence on the impact of juror political party on verdicts, and (iii) an empirical examination of the role of peer effects in jury decision-making. The results reveal a number of systematic biases: convictions for young defendants and those with distinctly Arabic names increase substantially when they are randomly assigned jurors from the far-right (nationalist) Swedish Democrat party, whereas convictions in cases with a female victim increase markedly when they are assigned jurors from the far-left (feminist) *Vänster* party. An analysis of peer effects implies that jurors from the far-left and far-right parties influence the votes of *nämndemän* from centrist parties in a way that is consistent with their respective party platforms. This analysis also suggests that at least some of these peer effects result in genuine changes of opinions (affecting trial outcomes), rather than vote changes motivated solely to reach unanimous decisions. (JEL: K14, K40)

The editor in charge of this paper was M. Daniele Paserman.

Acknowledgments: This research would not have been possible without the assistance of two research assistants (Ulrika Kjellström and Magnus Landergren) and a number of individuals at the Gothenburg District Court, including Assistant Judge Christoffer Önrup, Judge of Appeal Christer Ranch, and Assistant Judge David Caldevik as well as Mikael Sjöberg in the Archives. We would also like to thank Matthew Lindquist, Emma von Essen, Erik Hjalmarsson, Johan Lönnroth, and Jordi Blanes I Vidal and seminar participants at Queen Mary University of London, Gothenburg University, Aarhus, Leciester University, Stockholm School of Economics, Lund University, Växjö, SOFI, Paris School of Economics and the Bath Crime Economics Workshop for valuable feedback. Hjalmarsson would like to acknowledge funding from Vetenskapsrådet (The Swedish Research Council) and Jan Wallanders och Tom Hedelius Stiftelse Tore Browaldhs Stiftelse, P2014-0200:1. Bayer is a Research Associate at NBER. Hjalmarsson is a Research Fellow at CEPR.

E-mail: sanwar@rand.org (Anwar); patrick.bayer@duke.edu (Bayer); randi.hjalmarsson@economics.gu.se (Hjalmarsson)

1. Introduction

The right of a defendant to a fair trial is an essential feature of any criminal justice system that respects the rule of law and, as such, is inscribed in the majority of modern-day constitutions. In countries with representative governments, criminal trials are typically decided by juries drawn from the local population in a manner intended to create representative participation in the legal system analogous to that in the political system, although the exact way juries are formed varies widely across countries. Many, particularly those that follow the English legal tradition, utilize juries of randomly selected ordinary citizens, whereas others, including most of continental Europe, draw on a pool of lay judges or expert jurors selected through local elections or as political appointments (Jackson and Kovalev 2006). The latter practice has become increasingly controversial in light of the surge of far-right parties in many European countries.

Although the use of representative juries may provide a basis for popular legitimacy, the integrity of any justice system ultimately depends on how close it comes to achieving the abstract promise of a fair trial for all defendants. A particular concern is often whether juries exhibit systematic biases for or against certain defendants (or victims) in a way that impacts verdicts beyond the objective quality of the evidence in the case. Although no system is likely to eliminate all such bias, much of the academic and legal debate surrounding alternative jury systems focuses on whether certain designs are more likely to minimize it (Hans 2008).

Despite the fundamental importance of these questions for the fairness and integrity of criminal justice systems, there is surprisingly little evidence on jury decision-making using actual trials anywhere in the world. The current empirical literature consists primarily of a small number of correlation studies using U.S. data.^{1,2} Unfortunately, these studies are generally subject to concerns about the nonrandom selection of the seated jury, making it difficult to draw strong causal conclusions about the effects of jury composition on verdicts.

A recent pair of studies (Anwar, Bayer, and Hjalmarsson 2012, 2014) provides causal evidence of biases in U.S. felony jury decisions in two Florida counties. Taking advantage of the random variation in the day-to-day composition of the jury pool from which the jury is chosen, these papers show that verdicts are not simply a function of the objective quality of the evidence in the case; rather trial outcomes vary systematically with random fluctuations in the composition of juries (including juror race and age).

1. See Diamond and Rose (2005) for a review of studies using data from jury trials. More recently, Lehmann and Smith (2013) study the impact of seated juror characteristics (e.g., race, gender, age, religiousness, education) on trial outcomes and Lee (2017) shows that states that switched from key-man to more random jury selection procedures (to increase black representation) saw a drop in the share of non-Whites newly admitted to prison.

2. See Devine et al. (2001) for a survey of the mock trial literature. The resulting conclusions are limited by both the simplifications made for experimental expediency and the substantially lower stakes compared to real trials.

This kind of causal evidence is, however, so far limited to this single setting, raising concern about the external validity of the findings.³

With this context in mind, this paper uses a novel data set of criminal cases tried in Gothenburg, Sweden to make three broad contributions to the empirical literature on jury decision-making. By way of background, serious criminal cases in Sweden are decided by a professional judge in collaboration with three lay jurors (termed *nämndemän*), who are drawn (essentially) randomly from a pool of eligible *nämndemän* in the district. *Nämndemän* are politically affiliated, appointed officials that serve on many trials during their four-year term.

First, taking advantage of the random assignment of *nämndemän* triplets to cases, we assess whether *nämndemän* exhibit systematic biases based on the characteristics of the defendant or case. This design naturally avoids the concerns of correlation studies in which the seated jury is “selected” pretrial, allowing us to test whether *nämndemän* systematically reach different verdicts when faced with statistically identical evidence.

Second, we provide the first causal evidence on the role of political affiliations in jury decision-making.⁴ Whether political ideology affects jury decisions may be particularly relevant throughout Europe today, given the substantial electoral gains of right and far-right political parties in the last twenty years in countries like Austria, Denmark, Finland, France, Poland, and Sweden. That many of these parties have a nationalist, anti-immigration platform compounds the importance of this issue, raising the question of whether non-native defendants (who make up a disproportionate share of Swedish defendants) will be treated equally.⁵

In contrast to the jury literature, political ideology has been considered more extensively in other contexts where individuals are politically appointed or elected, including congressional voting (Snyder and Groseclose 2000; Lee et al. 2004), mayoral behavior (Ferreira and Gyourko 2009), and judicial decision-making.⁶ Studying

3. There is a more substantial literature on judges, both in the United States and internationally. A number of papers look at random assignment of judges, including Abrams et al. (2012) who study racial differences in sentencing and Ashenfelter et al. (1995) who study the effect of judicial ideology in Federal courts. See Shayo and Zussman (2011) and Gazal-Ayal and Sulitzeanu-Kenan (2010) for studies of judicial in-group bias in Israeli small claims courts and bail hearings, respectively. Casper and Zeisel (1972) conducted an early study of German lay judges.

4. Given the perception that criminal justice views are related to political ideology, it is surprising that political party does not appear in reviews of more than 100 jury studies (Devine et al. 2001; Devine 2012).

5. http://www.nytimes.com/interactive/2016/05/22/world/europe/europe-right-wing-austria-hungary.html?_r=0.

6. The judicial literature considers the effect of judge party on a wide range of cases: EPA (Revesz 1997; Miles and Sunstein 2006), NLRB (Miles and Sunstein 2006), criminal appeals, death penalty, and abortion (Sunstein et al. 2006). It generally finds that a judge’s politics matter—Democratic judges are more favorable in liberal cases whereas Republican judges are more favorable in conservative cases (Spitzer and Talley 2013). Schanzenbach and Tiller (2008) and Fischman and Schanzenbach (2011) find evidence of harsher criminal sentences from Republican appointed judges. The study of the Supreme Court of Iaryczower and Shum (2012) finds that although the justices’ initial leanings are changed in more than 40% of cases after incorporating “information”, such changes have become less likely as appointments

the impact of political ideology in the context of decision making by Swedish *nämndemän* distinguishes itself from the existing literature in three ways. First, Swedish *nämndemän* are more like jurors in the U.S. system than judges; though they try more than one case, they have limited training and are not professionals concerned with their judicial careers. Second, the “criminal” literature on judges has focused almost exclusively on sentencing severity rather than the fundamental question of whether the law is applied impartially in judging guilt. Third, we examine biases related to specific elements of the platforms of a more diverse set of political parties (e.g., the anti-immigration and feminist platforms of the far-right Swedish Democrats and far-left *Vänsters*, respectively).

The final component of our analysis explores the role of peers in jury decision-making. In particular, the repeated randomized structure of *nämndemän* assignment provides interesting variation not only in the matching of *nämndemän* to defendants but also in the matching of *nämndemän* to each other. We thus observe the same individual serving with different sets of peers across a large number of cases. This unique structure allows us to identify the causal impact of *nämndemän* on one another.

Because of U.S. legislation in the 1950s prohibiting the observation and recording of deliberations, how real juries arrive at verdicts largely remains a black box. Much of what is known about jury deliberations is based on mock trials.⁷ One recent exception is Hans and Waters (2009) analysis of juror surveys from The Hung Jury Project; more than half of the unanimous jury verdicts had at least one juror who would have voted differently than the actual verdict if voting independently. Although this clearly suggests that group dynamics matter, we are unaware of any studies that directly estimate the impact of peers on jury decision-making.⁸

The data used in this paper consist of more than 950 closed crimes against person cases for the Gothenburg District Court for 2009 to 2012, representing more than 1,150 defendant by case observations. Case characteristics and outcomes as well as *nämndemän* and judge names were manually transcribed from archival records and merged with a secondary data set of *nämndemän* and judge characteristics. Since there is at least one Social Democrat and one Moderate on 74% and 69% of cases,

become more politicized. Finally, Lim, Snyder, and Strömberg (2015) find that media coverage leads to harsher sentences for nonpartisan judges but has no effect on partisan judges.

7. MacCoun’s (1989) brief review highlights two phenomena: (i) a majority effect, where a verdict can be predicted fairly reliably by a two-third majority, even when there is a unanimous decision rule and (ii) group polarization, where deliberations make group members more extreme in their views than they were before deliberating.

8. A related literature studies panel effects in U.S. circuit court judicial decision-making. Much of this literature documents the existence of judicial peer effects; see, for instance, Boyd et al. (2010) for a study of judge gender. Fischman (2015) reanalyzes more than ten previous studies of judge panel effects, concluding that the estimated peer effects are consistent with a strong norm of consensus. Other papers test the potential mechanisms underlying these peer effects; see, for instance, Miles (2012). Amongst the main mechanisms put forth are whistleblowing (Cross and Tiller 1998), dissent aversion (Posner 2008; Epstein et al. 2011), and the dynamics of deliberation (Sunstein et al. 2006). Blanes i Vidal and Leaver (2013) study social interactions in judge citation behavior.

respectively, variation in the composition of the nämndemän triplets comes primarily from those parties that are more extreme in the political spectrum—the Vänster and Green on the left and the Swedish Democrats on the right.

We first use this defendant by case level data to assess whether certain kinds of defendants are more (or less) likely to be convicted when randomly assigned nämndemän from the most liberal and conservative parties, respectively. The results, which are robust to a variety of specifications, reveal substantial systematic biases with respect to political affiliation. The presence of a Swedish Democrat in the triplet increases the likelihood of conviction by 17 percentage points for defendants with Arabic names, which is consistent with the Swedish Democrats' nationalist, anti-immigration platform. Having a Vänster in the triplet increases convictions by 14 percentage points when there is a female victim, consistent with the party's feminist platform. Thus, the impact of party on convictions appears to not just reflect the parties' views on crime, but rather the multidimensional nature of political platforms. Finally, we find that defendants younger than 21 years old are more (less) likely to be convicted if facing a triplet with a Swedish Democrat (Vänster) nämndeman.

The reduced form peer effects analysis finds strong evidence that nämndemän from both the far-left and far-right parties affect the votes of their more centrist peers in ways that are consistent with their party platforms. We also provide suggestive evidence that the peer effects are driven by both (i) dissent aversion (switching one's vote simply to avoid dissenting or to attain unanimity) and (ii) sway effects (actually changing the opinion of one's peers). This is an important distinction, as it is only the latter channel that actually impacts trial outcomes.

Finally, we also find that the presence of a nämndeman from the far-left or right sometimes affects verdicts or peer votes in ways not explicitly consistent with party platforms. For instance, the presence of a far-left peer increases the chance that an individual nämndeman votes to convict Arabic named defendants, even though the Vänster party is not known for an anti-immigration platform. Although we can put forth a potential explanation consistent with these results—the presence of the far-left Vänster causes the other nämndemän to dig-in to their own beliefs—we cannot offer a theory as to when such an effect should appear. More research is clearly necessary to fully understand the deliberation dynamics underlying jury verdicts.

The remainder of the paper proceeds as follows. Section 2 provides background on the Swedish nämndemän system and uses survey data to characterize Swedish politics. Section 3 discusses the data. Section 4 presents the empirical methodology, tests random nämndemän assignment, and the main defendant by case level analysis. Section 5 presents our analysis of peer effects in jury decision making. Section 6 concludes and discusses the implications of our results for various policy proposals on the table to reform the Swedish nämndemän system.

2. Background

2.1. *Nämndemän (Lay Judges) in Sweden*

Criminal cases in Sweden are adjudicated in the 48 District Courts. Each court employs a pool of professional judges with varying levels of experience and has an associated pool of *nämndemän*. In contrast to the United States, where the jury pool is drawn from the general population, individuals must put themselves forward to become *nämndemän*.⁹ A *nämndäman* is appointed for a four-year term by the municipality or county council after being nominated by a political party. The share of each party in a jurisdiction's *nämndemän* pool is proportional to the party's representation in that jurisdiction. However, although almost all *nämndemän* are politically affiliated, they are not meant to be politically active; this increases the external validity of our findings to less politically based jury systems. An online search for the *nämndemän* in our data set suggests that about half of the *nämndemän* do not currently sit and have not sat in the past on any other political committees whereas 20% sit on just one other committee. Finally, one should note that the *nämndemän* pool is far from representative of the population. This is particularly true with respect to age; in 2010, just 16% of *nämndemän* were younger than 45 whereas 46% were older than 65.¹⁰ The unrepresentativeness and political nature of the *nämndemän* system, particularly with the growth of more extreme parties like the Swedish Democrats, is a growing source of public concern.

A defendant's case (verdict, sentence, and oftentimes, damages) is decided at the main hearing (which is the Swedish equivalent of a trial).¹¹ For a criminal case in which imprisonment is a possible sentence, the court is comprised of one professional judge and three *nämndemän*. If the harshest punishment is a fine, the court is typically comprised of a single professional judge. The analysis in this paper focuses on the former, in which the charges are serious enough to require the presence of *nämndemän*.

Working as a lay judge typically only requires 10–15 days per year; most *nämndemän* are otherwise employed or retired. In the courtroom, each *nämndemän* listens to the proceedings and may ask additional questions. After the hearing, the judge and *nämndemän* discuss the possible decisions, including the verdict and sentence, and express their viewpoints. In contrast to the United States, verdicts do not have to be unanimous, and each lay judge's opinion carries equal weight as that of the presiding judge. If all members of the court are not in agreement, then dissenting opinions are recorded. These dissenting opinions could be with regard to a specific charge

9. Almost all Swedish citizens over the age of 18 who do not have criminal records are eligible to become *nämndemän*; some professionals, such as court employees or attorneys, cannot become *nämndemän* to avoid a conflict of interest.

10. http://www.domstol.se/templates/DV_Press.aspx?id=18997.

11. This main hearing occurs for most criminal defendants. In contrast to the United States where more than 90% of cases are pled before trial, even if the defendant admits their guilt, this is typically done in the main hearing.

(as opposed to all charges) or even the sentence. A defendant is convicted if the majority finds him guilty, even if the presiding professional judge believes he should be acquitted.

2.2. *Additional Details about Gothenburg and the Gothenburg District Court*

Gothenburg, the second largest city in Sweden, reported 18,413 crimes per 100,000 persons from 2009 to 2011. This is somewhat higher than the average crime rate throughout Sweden (14,906 per 100,000) but lower than that of the other main Swedish cities of Stockholm and Malmö (with crime rates of 22,078 and 20,834 per 100,000, respectively) (Brå 2012). The Gothenburg District Court had almost 350 *nämndemän* on the roster in the beginning of 2013, representing nine political parties. The two predominant parties are the Social Democrats and the Moderates, which comprise 32% and 29% of the *nämndemän* pool, respectively. The other parties are: (i) Vänster (left) and Green, which are often in alliance with the Social Democrat party,¹² (ii) Folk, Center, and Christian Democrats, which form a coalition with the Moderate party (referred to as the Center-Right Block), (iii) Swedish Democrats (far-right), and (iv) the Vägvalet, which is a local party that opposes the congestion tax in Gothenburg.¹³

According to the Gothenburg District Court webpage, “Neither a professional judge nor a lay judge may choose which cases they adjudicate. The court draws lots to determine who will adjudicate in different cases.” Thus, in contrast to the “deselection” of the U.S. jury through *voir dire*, there is meant to be a random nature to the assignment of *nämndemän* to cases. If such randomness exists, then the characteristics of the *nämndemän* and judge should be unrelated to the characteristics of the case; we test this directly in the data in Section 4.

How specifically does the roster of more than 300 *nämndemän* get assigned to cases? Upon election, each *nämndeman* fills out a form indicating availability for each day of the week. *Nämndemän* coordinators allocate each *nämndeman* 15 dates in the upcoming year, such as every third Tuesday, taking into account the typical caseload on a given day. Importantly, *nämndemän* are assigned dates far in advance of *any* knowledge about the cases to be tried on those dates. For each pool of *nämndemän* assigned to a given day, the coordinators then form triplets; each *nämndeman* assigned to the same triplet is meant to have the same schedule and sit on cases together in the upcoming year. In practice, however, there is more than expected variation in the

12. Though the Green party is not officially part of the left or right block, many voters do consider them to be left.

13. Gothenburg is not an anomaly with respect to the far right and left parties. After the 2010 election, Vänsters held approximately 7%, 9%, and 8% of the seats in the municipality councils in Malmö, Gothenburg, and Stockholm respectively; Greens held approximately 8%, 11%, and 16% respectively; and Swedish Democrats held approximately 12%, 4%, and 0% respectively. After the 2014 election, however, even Stockholm had enough Swedish Democrat voters to be represented with a 6% share on the council, compared to 7% and 15% in Gothenburg and Malmö, respectively. In terms of the main parties, Malmö and Gothenburg are historically Social Democrat strongholds whereas Stockholm is a Moderate party stronghold.

composition of the triplets due to the use of substitutes because of sick (or otherwise unavailable) nämndemän or a higher caseload on a given day than anticipated. At the end of the year, new dates and triplets are assigned to account for changing schedules. The fact that the data spans the 2010 election provides additional variation in triplet composition.

According to the coordinators, the formation of triplets is done with some attempt to balance gender (e.g., no all female triplets), age, and political party. With respect to party, the coordinators try to assign different parties to a triplet, which is not always feasible given the high share of Social Democrats and Moderates.¹⁴ Finally, the coordinators assign triplets to courtrooms scheduled to be in session by going down the list of triplets in order, as each triplet has a number associated with it, without taking into account the characteristics of the case or nämndemän. That is, the first triplet is assigned to the first courtroom, the second to the second courtroom, and so on. The only exception is for the most serious charges of murder or rape, in which case the coordinators try to assign triplets with some prior experience. The main results are robust, however, to dropping these most serious cases from the analysis.

One potential concern that would invalidate the random assignment of triplets to cases is whether nämndemän can withdraw from a case on the basis of its characteristics. Although we do not have explicit data on nämndemän absences, one should note that these cannot be correlated with case characteristics, such as offense type, since nämndemän do not know the case until arriving at the courthouse. In fact, according to the nämndemän coordinators, until they arrive at the courthouse, nämndemän do not know what case or judge they will see.¹⁵

2.3. Political Party Platforms

To provide some context about Swedish politics, this section analyzes two surveys. The VU Election Survey (Valundersökning), conducted by the SOM-institute at the University of Gothenburg at every general election, is a random sample from Statistics Sweden's population registry of 18–80 year olds who are eligible to vote and residing in Sweden (Holmberg and Oscarsson 2012). In 2010, 2,736 individuals were interviewed in total, though only 1,336 had full-scale interviews. The second survey is the Riks-SOM, which is an annual nationwide postal survey of a random sample of 16–85 year olds residing in Sweden. The 2010 survey round had 5,007 respondents (SOM-Institute 2011).

14. They also try to assign one nämndeman who is identified as having special juvenile qualifications, for example, school teacher. Finally, they ensure that personally connected nämndemän, for example, married, are not on the same triplet.

15. The description of the nämndemän assignment process characterizes the new system implemented in March 2010, when the Göteborg and Mölndal district courts merged. Prior to this date, judges sat with one of two sets of triplets for a given day; that is, the nämndemän regularly came into contact with the same judges.

The first order question is how each party, on average, views the criminal justice system. We focus in the VU survey on questions related to the support of longer prison sentences and more law and order and in the Riks SOM survey on whether the courts give reasonable punishments. Table 1 presents the results of regressing a respondent's responses to these questions (coded as categorical variables) on party affiliation and controls for age, gender, citizenship, education, and urbanicity. Relative to Social Democrats, Vänsters (Greens) are 15 (19) percentage points less likely to favor harsher prison sentences, 13 (14) percentage points less likely to favor more law and order, and 16 (17) percentage points more likely to oppose harsher prison sentences. In contrast, Swedish Democrats are 24 percentage points more likely to favor harsher sentences, 28 percentage points more likely to favor more law and order, and 26 percentage points more likely to think that the courts give unreasonable punishments. These results are surprisingly insensitive to adding controls, including a measure for how left/right respondents perceive themselves; political party affiliation appears to be a better predictor of an individual's attitudes than observable demographic and socioeconomic characteristics.

Thus, the available survey evidence suggests that the Vänster and Green parties are, on average, relatively soft on crime whereas Swedish Democrats are hard on crime: these parties generally fall on opposite ends of the spectrum, with all other parties somewhere in the middle. This is very much consistent with anecdotal evidence and popular perceptions. In fact, the Swedish Democrat platform advocates the use of real life sentences without parole; under the current system, life sentences usually imply 18 years (and parole after 12 years).

Given the multifaceted nature of both politics and criminal cases, it is perhaps too narrow-minded to only consider a party's attitude toward crime. Other issues that may play prominent roles are feminism and nationalism/immigration. In fact, one of the primary platforms of the Vänster party is as a feminist party; according to the party's manifesto: "The superiority of men and subordination of women is no law of nature. The struggle for women's rights has yielded significant results. Feminism gives arguments and tools for the women's struggle that must be waged to break the power of the patriarchy. This struggle is carried out in politics and in the workplace, but also in private life."¹⁶ This paper will assess whether the struggle is also carried out in the courtroom. Empirical evidence of the Vänster party feminist platform is provided in Table 1; Vänster party members are 17 (15) percentage points more likely than Social Democrats to strongly support gender equality in the VU (SOM) survey.

In addition, similar to other far right parties throughout Europe, Swedish Democrats largely run on a nationalist, anti-immigration platform.¹⁷ A quote from their party

16. Translated by the author: "Mäns överordning och kvinnors underordning är ingen naturlag. Kampen för kvinnors rättigheter har givit viktiga resultat. Feminismen ger argument och redskap för den särskilda kvinnokamp som måste föras för att patriarkatets makt skall kunna brytas. Denna kamp förs inom politiken och arbetslivet, men också i privatlivet." <http://www.vansterpartiet.se/material/partiprogram/>.

17. These include, for instance, the National Front Party in France, the Golden Dawn Party in Greece, the Flemish Interest Party in Belgium and the Danish People's Party in Denmark.

TABLE 1. Survey evidence of party beliefs.

	VU 2010 election survey analysis				RIKS SOM 2010 survey analysis			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Favor or strongly favor harsher prison sentences	Favor working to a society with more law and order	Against or strongly against harsher prison sentences	Strongly support gender equality	Courts give unreasonable punishments	Immigrants are punished harsher	Immigrants are not punished harsher	Strongly support gender equality
Vänster (left)	-0.150** (0.0610)	-0.126** (0.0593)	0.158*** (0.0523)	0.172*** (0.0607)	-0.00833 (0.0396)	0.126*** (0.0323)	-0.0548 (0.0383)	0.150*** (0.0352)
Green	-0.186*** (0.0505)	-0.138*** (0.0490)	0.167*** (0.0433)	0.114** (0.0505)	0.0629* (0.0360)	0.0302 (0.0308)	-0.0446 (0.0365)	0.144*** (0.0300)
Center	-0.133** (0.0556)	0.0368 (0.0536)	0.0285 (0.0477)	0.0223 (0.0552)	-0.0576 (0.0376)	0.0126 (0.0329)	-0.00855 (0.0390)	-0.0910*** (0.0324)
Folk	0.0260 (0.0527)	0.174*** (0.0509)	-0.0487 (0.0452)	0.00655 (0.0526)	0.0238 (0.0338)	-0.0670** (0.0288)	0.0157 (0.0342)	-0.0656** (0.0304)
Christian democrat	0.0326 (0.0601)	0.266*** (0.0581)	-0.0573 (0.0516)	-0.172*** (0.0599)	-0.0557 (0.0380)	-0.0685** (0.0333)	0.0507 (0.0395)	-0.120*** (0.0334)
Moderates	0.0922*** (0.0351)	0.174*** (0.0341)	-0.0964*** (0.0302)	-0.0400 (0.0351)	0.0316 (0.0218)	-0.0576*** (0.0188)	0.0398* (0.0223)	-0.0853*** (0.0187)
Swedish democrats	0.239*** (0.0772)	0.278*** (0.0751)	-0.0986 (0.0662)	-0.131* (0.0774)	0.256*** (0.0444)	-0.0694* (0.0361)	0.305*** (0.0428)	-0.0363 (0.0408)
Constant	0.764*** (0.103)	0.619*** (0.100)	0.109 (0.0885)	0.430*** (0.103)	0.412*** (0.0392)	0.157*** (0.0331)	0.245*** (0.0392)	0.483*** (0.0335)
N	1209	1218	1209	1221	2360	1877	1877	4556
R-squared	0.173	0.128	0.143	0.048	0.047	0.028	0.042	0.056

Notes: Standard errors in parentheses. The dependent variable in each specification is listed at the top of the column. All specifications control for age, gender, citizenship, education, and whether respondents live in an urban area. Note that the omitted category is social democrats. Also, note that adding the controls had little impact on the magnitude of the baseline estimates. In addition, controlling for left/right ideology generally has little impact. *Significant at 10%; **significant at 5%; ***significant at 1%.

website (at the beginning of this project) clearly demonstrates their views: “In recent decades, Sweden has accepted too many immigrants in too short a time. . . . This reckless policy has created impoverished and marginalized areas around the country. Areas that, when they were originally built, were regarded as fine middle-class neighborhoods have turned into segregated areas where unemployment and crime is high.”¹⁸ These views are apparent in the SOM questions on whether immigrants are punished harsher than nonimmigrants. Swedish Democrats are more than 30 percentage points more likely than Social Democrats to think that immigrants are not punished more harshly; Vänsters, in contrast, are 13 percentage points more likely to think that immigrants are punished more harshly (consistent with their antiracism position).

3. Data

3.1. Data Description

The data are based on a sample of closed crimes against person cases for the Gothenburg District Court for 2009 through 2012. Specifically, we obtained all cases (almost 400) with at least one charge of murder, rape, robbery, manslaughter, sexual assault, and aggravated assault (or an attempt) during this period. Given the time intensive nature of manually coding all data, we requested a random sample of almost 200 (of 400) unlawful threat cases and 400 (of 1,500) simple assault cases. As all of these violent offense charges have imprisonment on the table, they were tried by a professional judge and three nämndemän.¹⁹ The resulting data set includes 956 cases and, due to multiple defendant cases, 1,152 unique defendant-case observations. Just 28 defendants appear in the data more than once.

Two documents for each case number were obtained from the Gothenburg District Court Archive: *Anteckningar* (notes) and *Dom* (Judgment).²⁰ For each case, we extracted the nämndemän and judge names as well as trial dates and times from the *Anteckningar*. From the *Dom*, we extract information about the defendant(s), offense(s) charged, case, and verdict. Defendant information includes name, country of citizenship, and personal identity number, from which we can identify birth date and gender. Though we do not know ethnicity, we have coded defendant names as having distinctly Arabic names (all of the 255 defendants coded as such have Arabic first and last names). We do recognize that not all of these defendants are actually Arabic, but

18. Translated by the author: “De senaste decennierna har Sverige tagit emot allt för många människor på allt för kort tid. . . . Denna ansvarslösa politik har gjort att det ständigt skapats växande utanförskapsområden runt om i landet. Områden som när de byggdes betraktades som fina medelklasskvarter har förvandlats till segregerade områden där arbetslösheten och brottsligheten är hög.”

19. Because the Court could not directly identify nämndemän cases, the data request conditioned on offense categories guaranteed to be eligible for prison, regardless of criminal history. This immediately excludes many property crimes, which are sentenced with fines.

20. A handful of requested cases were not included in the final data because of missing documents in the archives.

may simply have names “similar” to Arabic names. We cannot distinguish these two groups of individuals in the data; however, the same may be true for the *nämndemän* who are deciding their cases. We also coded two additional categories of defendant names: (i) distinctly Swedish and (ii) other (which contains, but is not limited to, many Eastern European names, as well as those that are difficult to otherwise classify).²¹

The case and verdict information includes the victim’s name(s), attorney names, the charges on which the defendant was acquitted or convicted, damages requested and awarded, and the sentence. The most common sentences are fine, probation, imprisonment, suspended sentence, and community service. Juveniles (under 18) can be sentenced to community service or the child welfare authorities and are generally not eligible for adult prison sentences.²² From a description of the evidence presented, we determine whether the defendant fully or partially admits guilt (i.e., admits some aspects/circumstances of the incident). From the *Dom*, we also code the number of previous episodes in the crime registry, and whether there are any violent offenses. For acquitted individuals, we obtain this information from an additional document (*belastingsregistret*) from the archives. Finally, the *Dom* includes a description of dissenting opinions (*skiljaktig mening*) amongst the judge and/or *nämndemän* in the verdict and sentence.

We merged this case by defendant level data with information about the judge’s age and gender from the district court and the *nämndeman*’s municipality, gender (based on name), birth year, first year as a *nämndeman*, and political party.²³ Though we do not have a measure of *nämndeman* ethnicity, we coded individuals as having non-Swedish names. There are 486 *nämndemän* observed in the sample; 22% are seen on just one or two cases, 25% on three to five cases, 25% on six to nine cases, and the remainder on ten or more cases. There are 93 unique judges in our data, though just 37 judges are assigned to 10 or more cases.

3.2. Descriptive Statistics

Table 2 provides descriptive statistics characterizing the defendants, case characteristics, verdicts and sentences, judges, and *nämndemän* triplets. We begin

21. These names were coded by a Swedish research assistant. For a native Swede, it is quite obvious when a name is distinctly Swedish. For instance, 23% of Swedish names contain the letters å, ä, or ö whereas 0.4% and 4%, respectively, of those in the Arabic and non-Swedish, non-Arabic categories contain these letters. Similarly, 37% of Swedish last names end in “son” compared to less than 1% of the other name categories. The three most common Swedish first names in the data are Anders (9%), Hans (8%), and Lars (7.5%); these names are seen between 0% and .4% in the other categories. Names coded as Arabic are also quite distinct from the other categories: 15% contain Mohamed (or an alternative spelling), 11% contain Ali, and 7% each contain Abdul, Abdi, Hassan, and Hussein. In contrast, 0% of Swedish names and Non-Swedish, Non-Arabic names contain Mohamed, Hassan, Hussein, and Abdi; about 2% contain “Ali” but this can be any part of a name, like Alice.

22. In Sweden, defendants under age 18 are not eligible for an adult prison sentence, while there is some discretion for those between 18 and 21. There is no discretion for defendants above age 21.

23. The current *nämndemän* roster was obtained from the Gothenburg district court. For *nämndemän* on the roster prior to the 2010 election, data was obtained via communications with each municipality (and/or their websites) in the Gothenburg district court catchment area.

TABLE 2. Summary statistics for case by defendant level data.

Variable	Obs.	Mean	Std. Dev.
<i>Nämndemän triplet characteristics</i>			
Any social democrats?	1129	0.74	0.44
Any Vänster (left)?	1129	0.20	0.40
Any miljö (Green)?	1129	0.27	0.44
Any Christian democrats?	1129	0.13	0.33
Any moderates?	1129	0.69	0.46
Any folk party?	1129	0.29	0.45
Any center party?	1129	0.02	0.15
Any Swedish Democrats?	1129	0.09	0.29
Any local party (vägvalet)?	1129	0.10	0.30
Majority nämndemän male?	1152	0.46	0.50
Any näm. with non-Swedish name?	1147	0.40	0.49
Average age of triplet	1151	58.06	9.01
Average age ≥ 50 ?	1151	0.83	0.38
Average experience 2–4 years?	1150	0.36	0.48
Average experience 4–6 years?	1150	0.24	0.43
Average experience >6 years?	1150	0.23	0.42
<i>Defendant characteristics</i>			
Defendant age	1143	32.20	14.06
Defendant male	1137	0.91	0.28
Defendant non-Swedish citizen	1152	0.12	0.32
Defendant has Arabic name?	1152	0.22	0.42
Any past convictions?	1144	0.36	0.48
Any violent past convictions?	1144	0.20	0.40
<i>Case characteristics</i>			
# offenses	1143	1.87	1.47
Any robbery charge?	1144	0.05	0.22
Any rape (child/adult) charge?	1144	0.08	0.28
Any assault charge?	1144	0.56	0.50
Any aggravated assault charge?	1144	0.13	0.34
Any unlawful threat charge?	1144	0.26	0.44
Any manslaughter/murder charge?	1144	0.03	0.17
Any drunk driving charge?	1144	0.02	0.14
Any drug charge?	1144	0.11	0.31
Any other sex offense charge?	1144	0.07	0.26
Any other charge?	1144	0.28	0.45
# victims	1148	1.51	1.22
Any female victims?	1129	0.50	0.50
# defendants	1151	1.69	1.62
Full admission of guilt?	1124	0.09	0.29
Partial admission of guilt?	1124	0.46	0.50
<i>Case outcomes</i>			
Any current convictions?	1152	0.88	0.33
Share of current offenses convicted	1142	0.83	0.34
Any prison?	1008	0.30	0.46
Any dissenting opinions?	1152	0.08	0.27
Any dissenting opinion up?	1152	0.05	0.21
Any dissenting opinion down?	1152	0.03	0.18
<i>Judge variables</i>			
Judge age	1107	49.99	9.67
Male judge?	1152	0.55	0.50

with the nämndemän triplets. Despite the facts that nämndemän are assigned to triplets that should ordinarily serve together and there are a little more than 100 such triplets at any given time, 793 unique triplets are observed in the data set. This variation arises from (i) a substantial change in the nämndemän roster after the 2010 election, (ii) annual adjustments to account for changes in schedules, and (iii) the substitution of nämndemän due to unscheduled absences or an unexpectedly high caseload. Table 2 shows that there is a male majority in the triplet in 46% of the observations, the average nämndemän age in a triplet is 58, and the average age is older than 50 in 83% of cases. In addition, 17%, 36%, 24%, and 23% of defendants face triplets with less than two, two to four, four to six, and more than six years of experience, respectively. Finally, 40% of triplets have at least one nämndeman with a non-Swedish name.

In terms of political affiliations, Table 2 shows that 74% and 69% of triplets have at least one Social Democrat and Moderate, respectively. With respect to the other parties, 27% and 20%, respectively, have members of the Green and Vänster parties (both of which sometimes cooperate with the Social Democrats). For parties closely aligned with the Moderates, 29%, 13%, and 2% have at least one Folk, Christian Democrat, and Center party member, respectively. A Swedish Democrat is in the triplet in 9% of observations.

Turning to the defendants and cases, Table 2 shows that 91% of defendants are male, 12% are not Swedish citizens, 22% have an Arabic name, and the average defendant age is 32. Thirty-six percent of defendants have at least one past conviction and 20% have at least one violent conviction. Cases have, on average, 1.7 defendants (74% of observations have just one defendant), 1.5 victims (71% have just one victim), and 1.9 charged offenses (57% have just one offense). Fifty percent of the observations have at least one female victim, where victim gender was determined by victim name or the pronouns used in the transcript for cases with anonymous victims (e.g., some sex related offenses). In 9% of observations, the defendant fully admits guilt; the conviction analyses exclude these cases, as there is no room for discretion in deciding the verdict. Forty-four percent of defendants fully deny guilt whereas 46% partially admit guilt, that is, they have either admitted guilt to one offense but not another, or they have admitted some of the aspects of the charge but do not take full responsibility (e.g., he admits to hitting him, but it was in self-defense).

Current offense charges are grouped into nine categories; 28% of the observations include an offense that does not fall into one of these categories. Though the data were created using a crime against person selection criterion, other offenses appear in multicharge cases. The most common offenses are: assault (56%), unlawful threat (26%), aggravated assault (13%), rape (8%), robbery (5%), drugs (11%), and other sex offenses (7%).

The primary outcome considered in this paper is conviction. Table 2 shows that 83% of charges result in conviction, and that 88% of defendants are convicted on at least one charge. For the sample of defendants that do not fully admit their guilt (the main analysis sample), 81.5% of charges result in conviction. A closer look at the distribution of the share of charges convicted for this sample indicates that 74% are convicted on all charges, 13% are completely acquitted, 6% are convicted on half of

their charges, with the remainder somewhere in between. However, since more than half of defendants (56%) are charged with just one offense, these statistics may be a little misleading. When conditioning on those with more than one charge, we see less than 5% fully acquitted versus 66% convicted of all charges; 14% of multiple charge defendants are convicted of half the charges, with another 5% convicted of two third of the charges.²⁴ Though not studied in the paper, we see that 30% of all convicted defendants are sentenced to prison; 35% of those who are older than 18 and eligible for adult prison receive such a sentence.

Eight percent of observations have at least one dissenting opinion with respect to the verdict for at least one charge or the sentence (about 3% from a professional judge and 5% from at least one *nämndeman*). Five percent of observations have a dissenting opinion in favor of a harsher outcome (guilt or a harsher sentence), whereas 3% favor a more lenient outcome. Dissenting opinion data will be used to create the individual *nämndemän* level data set analyzed in Section 5. Finally, judges are on average 50 years old and 55% are male. Judge fixed effects are often included to account for differential judge characteristics.

4. Main Case by Defendant Level Analysis

4.1. Methodology and Identification

The first part of the empirical analysis uses the defendant by case level data set described previously to examine the relationship between *nämndemän* triplet political affiliations and case outcomes. To this end, the basic empirical specification is as follows:

$$Y_{ij} = \alpha + \beta_1 \text{Vänster}_j + \beta_2 \text{Green}_j + \beta_3 \text{SwedishDem}_j + X_j \gamma + Z_{ij} \pi + \lambda_{judge} + \varepsilon_{ij} \quad (1)$$

where Y represents the outcome (e.g., share of charges resulting in conviction) for defendant i associated with case j . *Vänster*, *Green*, and *SwedishDem* are dummy variables indicating if at least one of the *nämndemän* in the triplet belongs to each of these far left or right parties, respectively. Since these extreme parties rarely have more than one representative on the triplet, we focus on the extensive margin.²⁵ Our analysis emphasizes these extreme parties rather than all parties since almost all triplets have a Social Democrat and Moderate block member (Moderate, Center, Christian Democrats, and Folk parties). As both the Social Democrat and Moderate block parties are seen as more central on the political spectrum, oftentimes indistinguishable from each other in many political arenas, we focus on the three extreme parties. The political

24. The share of charges resulting in conviction is substantially smaller (75%) when conditioning on the almost 500 defendants who fully deny guilt, that is, when there is presumably more room for discretion.

25. There are 26 observations with two Greens versus 275 with one, 15 observations with two Vänsters versus 215 with one, and 101 observations with one Swedish Democrat versus zero with more than one.

viewpoints of the Vänster, Green, and Swedish Democrat parties diverge greatly from the central parties, such that they are not part of any formal alliance. It is important to note that controlling for the other parties has little impact on the results (as will be demonstrated later); thus, excluding them from the main analysis greatly eases the exposition, particularly for those specifications with many interactions. X represents a vector of other nämndemän triplet characteristics, including the average age and experience of the triplet, an indicator for whether the majority of the triplet is male, and an indicator for whether the triplet contains at least one member with a non-Swedish name. Finally, Z represents a vector of 20 offense, case and defendant characteristics and λ_{judge} represents judge fixed effects.

For the β s to represent a causal effect of nämndemän political affiliations on case outcomes, nämndemän triplets must be randomly assigned to cases, or at least randomly assigned conditional on known assignment factors. Because the nämndemän coordinators randomly assigned nämndemän triplets to courtrooms, the characteristics of the triplet should be unrelated to the characteristics of the defendant and case. Thus, columns (1)–(10) of Table 3 assess whether there is evidence of nonrandom assignment of triplets to cases by regressing ten triplet characteristics (party, gender, non-Swedish name, average age, average experience) on four defendant characteristics (gender, age, citizenship, and Arabic names), 11 offense characteristics (number of charges and 10 offense category dummies), five case characteristics (full and partial admission, number of defendants, number of victims, any female victims), and two criminal history measures (any past convictions and any violent past convictions). We summarize the results by conducting F -tests of the joint significance of the controls in each category, counting the number of significant coefficients, and presenting the R -squared.

Of the 40 F -tests conducted (four tests for 10 regressions), just two are significant at the 5% level (and in bold)—the offense characteristics in column (3) for Christian Democrats in the triplet and the case characteristics in column (5) for Swedish Democrats in the triplet. However, the joint significance of both of these tests is driven by the significance of just one coefficient. In fact, in nine of the 10 regressions, there are just two (or fewer) significant coefficients. There are four significant coefficients when looking at average experience of the triplet, though just one of the joint F -tests are significant at the 10% level; if there were to be nonrandom assignment in any dimension, it would be with respect to triplet experience given statements by the nämndemän coordinators about trying to assign triplets with experience to the most serious cases. Finally, this large set of 22 case characteristics explains very little of the variation in triplet characteristics; the R -squared ranges from 0.016 to 0.037.

Columns (11) and (12) of Table 3 test for the random assignment of judges to cases and nämndemän triplets. We regress judge gender and age on the same sets of case characteristics included in the previous specifications plus two sets of variables characterizing the political affiliations and other characteristics of the nämndemän triplet, respectively. None of the joint tests of nämndemän characteristics are significant: consistent with courthouse discussions, nämndemän triplets do not appear to be assigned to courtrooms on the basis of the judge assigned to the case.

TABLE 3. Random assignment of nämndemän triplets and judges.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Any Vänster näm.n.?	Any Green näm.n.?	Any Christ. dem. näm.n.?	Any folk party näm.n.	Any Swedish dem. näm.n.	Any Vägvalet näm.n.	Majority of näm. male	Any non-Swedish näm.n.	Triplet avg. age	Triplet avg. experience	Male judge	Judge age
	0.89 (0.47)	0.77 (0.54)	0.68 (0.61)	0.19 (0.95)	0.71 (0.58)	1.23 (0.30)	0.47 (0.76)	1.05 (0.38)	0.81 (0.52)	1.44 (0.22)	2.01 (0.09)	2.24 (0.06)
Defendant characteristics (4 variables)												
	0.81 (0.63)	1.46 (0.14)	3.36 (0.00)	1.21 (0.28)	1.07 (0.38)	1.46 (0.14)	0.75 (0.69)	1.21 (0.27)	0.72 (0.72)	1.75 (0.06)	1.85 (0.04)	0.50 (0.90)
Offense characteristics (11 variables)												
	0.95 (0.45)	0.81 (0.54)	0.77 (0.57)	0.35 (0.88)	3.31 (0.01)	0.38 (0.87)	1.04 (0.40)	0.34 (0.89)	1.41 (0.22)	1.24 (0.29)	1.64 (0.15)	0.67 (0.65)
Case characteristics (5 variables)												
	0.17 (0.84)	1.98 (0.14)	0.84 (0.43)	0.00 (1.00)	1.20 (0.30)	1.40 (0.25)	0.37 (0.69)	0.69 (0.50)	0.25 (0.78)	0.48 (0.62)	3.77 (0.02)	1.19 (0.30)
Criminal history characteristics (2 variables)												
Nämndemän political affiliations (7 variables)												
Other nämndemän characteristics (5 variables)												
	0	2	1	1	1	2	0	0	1	4	4	0
# coefficients sig at 5% level	1051	1051	1051	1051	1051	1051	1074	1069	1074	1073	1051	1011
N	0.025	0.022	0.021	0.016	0.028	0.034	0.023	0.023	0.030	0.037	0.072	0.043
R-squared												

Notes: Each column presents the results of a regression of a nämndemän triplet or judge characteristics (listed at the top of the column) and a large set of controls that can be grouped into six categories: defendant characteristics, offense characteristics, case characteristics, criminal history, nämndemän political affiliations, and other nämndemän characteristics. The results of these regressions are summarized in the table by presenting the p -values for F -tests of the joint significance of the variables included in these categories. F -tests significant at the 5% level are in bold. The table also reports the number of significant coefficients and the R -squared for each regression. F -tests are based on robust standard errors, clustered by case.

In terms of the random assignment of judges to case characteristics, we see four significant coefficients in the judge gender specification and an R -squared of 0.072; none of the coefficients are significant at the 5% level for judge age. These results are consistent with courthouse statements that judges are assigned to cases according to a roster, with some load balancing and with certain cases requiring more senior judges.

Taken as a whole, the results presented in Table 3 are strongly indicative of the random assignment of nämndemän triplets to cases and judges. However, it should also be emphasized that to the extent there is not random assignment, we control for almost all potentially confounding factors with judge fixed effects and the same set of nämndemän characteristics that the coordinators have at their disposal. Finally, as shown in what follows, the insensitivity of our results to including these controls is also consistent with random assignment.

An additional identification concern is whether the composition of the triplet itself is random, regardless of whether the triplet is randomly assigned to cases. For instance, if the presence of the Vänster on the triplet affects the party affiliation of others assigned to the triplet, then one may be potentially unable to disentangle whether β_1 captures the Vänster effect or that of his or her peers. This is clearly a potential issue given the balancing procedure described by the nämndemän coordinators in forming the triplets. We take two steps to address this issue after presenting the main results. First, we compare the actual triplets to simulated triplets created by random assignment to assess the extent to which balancing occurs. Second, we conduct robustness checks that focus on certain types of nämndeman triplets.

4.2. Baseline Results

Table 4 presents the results of estimating equation (1) with the full set of controls; only the coefficients on the variables of interest (having at least one Vänster, Green, or Swedish Democrat in the triplet) are presented. The dependent variable is the share of current charges resulting in conviction in columns (1) and (2) and whether any charge results in conviction in columns (3) and (4).²⁶ Two key results emerge. First, having a hard-on-crime Swedish Democrat in the triplet does not affect the likelihood of conviction. Second, having a soft-on-crime Vänster in the triplet, if anything, *increases* the likelihood of conviction, by more than five percentage points when looking at cases in which the defendant did not fully admit guilt (columns (1) and (3)) and by more than 13 percentage points when looking at cases in which the defendant completely denied guilt and there is arguably more discretion (columns (2) and (4)). One potential explanation for this surprising result is that it is an artifact of the way that triplets are balanced—that is, that it reflects not the effect of the Vänster but of systematically assigned to sit on the same triplet as the Vänster; we will be able to rule out this explanation empirically. An alternative explanation is that attitudes about crime are

26. Note that controlling for the other, more central, parties in this regression has little impact on the results for the extreme left and right parties; in addition, these other parties also do not significantly affect conviction rates.

TABLE 4. Baseline specification for all cases.

	(1) Share of current offenses convicted	(2)	(3) Any current convictions?	(4)
Any Vänster nämndemän?	0.0584 (0.0381)	0.146* (0.0753)	0.0572 (0.0373)	0.139* (0.0776)
Any Green nämndemän?	0.0255 (0.0322)	0.0811 (0.0607)	-0.00227 (0.0268)	0.0469 (0.0587)
Any Swedish democrat nämndemän?	-0.0377 (0.0525)	-0.00466 (0.0968)	-0.00865 (0.0497)	-0.00361 (0.0959)
Constant	0.927*** (0.0848)	0.737*** (0.193)	0.826*** (0.0837)	0.711*** (0.181)
Other nämndemän controls	Yes	Yes	Yes	Yes
Judge fixed effects	Yes	Yes	Yes	Yes
Def./offense/case controls	Yes	Yes	Yes	Yes
Sample	Full admit=0	No admit=1	Full admit=0	No admit=1
N	961	466	962	466
R-squared	0.212	0.261	0.230	0.253

Notes: Each column corresponds to a regression of the conviction outcome (denoted at the top of the column) on a dummy variable indicating the presence of each of three main extreme parties of interest, and include controls for other nämndemän characteristics (such as age, gender, non-Swedish, and experience), defendant characteristics, offense characteristics and other case characteristics. Columns (1) and (3) use the full sample of cases that did not fully admit their guilt, whereas columns (2) and (4) focus on those cases that completely deny their guilt. Robust standard errors, clustered on judge name, are in parentheses. * $p < 0.10$; *** $p < 0.01$.

not monolithic—that is, that Vänsters may be more likely to convict in some kinds of cases and less likely in others; we explore this possibility first.

The specifications presented in Table 4 implicitly assume that juror political party has the same impact on every case, regardless of the characteristics of the defendant or case. Given the multidimensional nature of party platforms, however, this is a very strong assumption. To explore the impact of political party in a more nuanced way, Table 5 expands the specification to include an interaction of each party variable with a series of defendant/case characteristics: whether there are any female victims in column (1), whether the defendant has an Arabic name in column (2), and whether the defendant is younger than 21 in column (3).²⁷ The choice of case characteristics is based on key aspects of the political party platforms and data availability; the main Vänster and Swedish Democrat platforms are feminism and anti-immigration, respectively, which we can directly look at in the data in victim gender and defendant ethnicity. As the main Green platform is the environment, there is no direct measure in the data of an issue particularly salient to their platform. Given the disproportionately young defendants and disproportionately old nämndemän (a well-known concern in Sweden), we also consider heterogeneity by defendant age.²⁸ For readability, we only

27. Age 21 is a natural cutoff given the differential treatment with respect to sentencing.

28. We have not tested interactions with any other case characteristics; in fact, there are few such other case characteristics available that we can consider. In theory, one might ask whether there is heterogeneity by offense type, such as rape, which may be particularly salient to Vänster nämndemän or consider

TABLE 5. Baseline party platform \times case characteristic results.

	(1)	(2)	(3)	(4)	(5)	(6)
	Share of current offenses convicted			Any current convictions?		
Any Vänster näm.n.?	-0.0126 (0.0520)	0.0595 (0.0377)	0.108** (0.0424)	-0.0115 (0.0475)	0.0637* (0.0354)	0.0924** (0.0432)
Any Swedish democrat näm.n.?	-0.0702 (0.0760)	-0.0866 (0.0586)	-0.0788 (0.0568)	0.00134 (0.0625)	-0.0553 (0.0568)	-0.0392 (0.0550)
Any Vänster \times any female victim	0.148*** (0.0558)			0.143*** (0.0527)		
Any Swedish dem. \times any female victim	0.0637 (0.0958)			-0.0163 (0.0783)		
Any Vänster \times defendant Arabic name		-0.0115 (0.0661)			-0.0276 (0.0597)	
Any Swedish dem. \times defendant Arabic name		0.260** (0.100)			0.255*** (0.0851)	
Any Vänster \times defendant <21			-0.153** (0.0622)			-0.105* (0.0606)
Any Swedish dem. \times defendant <21			0.239*** (0.0764)			0.169** (0.0810)
Defendant Arabic name?	-0.00483 (0.0327)	-0.0236 (0.0374)	-0.00146 (0.0312)	0.00393 (0.0334)	0.00270 (0.0370)	0.00912 (0.0321)
Any female victim?	-0.0218 (0.0390)	0.00252 (0.0320)	0.00295 (0.0310)	-0.00903 (0.0398)	0.00421 (0.0325)	0.00466 (0.0314)
Defendant <21?	-0.0268 (0.0347)	-0.0235 (0.0357)	-0.0303 (0.0432)	-0.0209 (0.0296)	-0.0193 (0.0313)	-0.0345 (0.0380)
Constant	0.945*** (0.0886)	0.928*** (0.0863)	0.905*** (0.0850)	0.836*** (0.0835)	0.828*** (0.0852)	0.809*** (0.0848)
<i>N</i>	961	961	961	962	962	962
<i>R</i> -squared	0.219	0.217	0.225	0.238	0.237	0.240

Notes: Robust standard errors, clustered on judge name, are in parentheses. All specifications use the sample of cases where guilt is not fully admitted and include controls for judge fixed effects, defendant, offense, case and nämndemän characteristics; note that having a Green in the triplet, and the corresponding interactions, are also included. The dependent variable in columns (1)–(3) is the share of charges resulting in a conviction, whereas in columns (4)–(6), it is whether there is at least one conviction. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

show results for the presence of Swedish Democrat and Vänster nämndemän; Green party affiliation is included (and appropriately interacted) in all specifications.

Some striking patterns emerge. First, the Vänster effect suggested in Table 4 is driven by cases with a female victim. Specifically, column (1) shows that having a Vänster in the triplet when there is a female victim increases the share of current charges resulting in conviction by about 14 percentage points (0.148–0.013); this is

heterogeneity by defendant gender. But, in practice, there are simply not enough rape cases or female defendants for such analyses.

significant at the 1% level.²⁹ On the other hand, when the victim is male, having a Vänster in the triplet does not have a significant impact.³⁰

Second, column (2) shows that having a Swedish Democrat in the triplet significantly increases the share of current charges resulting in conviction for defendants with Arabic names by 17 percentage points (0.269–0.087). Adding a Swedish Democrat to the triplet does not have a significant impact on conviction when the defendant has a non-Arabic name. These results are consistent with the Swedish Democrats' nationalist, anti-immigration platforms. They are also consistent with Marten's (2015) recent finding that asylum appeals in Sweden are more likely to be rejected when there is a Swedish Democrat on the triplet. Column (5) of Table A.1 breaks non-Swedish, non-Arabic named defendants out of the control group. Having a Swedish Democrat on the triplet only significantly increases conviction rates for Arabic named defendants.³¹

Finally, column (3) indicates that adding a Swedish Democrat to the triplet increases the share of current charges resulting in conviction for defendants younger than 21 by 16 percentage points ($-0.079 + 0.239$), with an associated p -value of 0.008. Although the Swedish Democrat party platform does not explicitly take a position regarding the treatment of youths in the criminal justice system, these results are consistent with other aspects of the party platform including those related to schooling and personal responsibility. Though having a Vänster nämndeman has an impact on younger (age less than 21) defendants that is 15.3 percentage points less than the effect these nämndemän have on older defendants, the total effect of having a Vänster on a case with a younger defendant is an insignificant reduction in the chance of conviction of 4.5 percentage points (0.108–0.153), with an associated p -value of 0.40. Thus, although Vänsters do not appear "soft" on offenders overall, they are relatively more lenient on young defendants.³² Taken together, the results in columns (1)–(3) suggest that it is important to take into account the multidimensional nature of party platforms when examining how political affiliations might affect conviction decisions, as opposed to only considering the party's general stance on crime.

29. Here we are describing the results for the total effect of adding a Vänster when the victim is female—that is, summing the coefficient on *any Vänster* and *any Vänster* × *any female victim* and characterizing whether that coefficient significantly differs from zero. Unless otherwise stated, we follow this approach for reporting results throughout this section.

30. Table A.1 shows that these results are not driven by cases with female defendants and female victims.

31. This is consistent with the correspondence study of Carlsson and Rooth (2007) on discrimination in the Swedish labor market with distinct Swedish sounding and/or Middle Eastern names. They argue that empirical studies indicate that discrimination is greatest against individuals with Middle Eastern backgrounds.

32. This combined effect is smaller when using the alternative dependent variable of any convictions rather than share of charges convicted but generally larger when testing for robustness to controls in Table 6.

4.3. Interpretation

The effect sizes in Tables 4 and 5 reveal that the random variation in assignment of *nämndemän* from different parties is associated with substantial shifts in the probability of conviction in statistically identical cases involving certain types of defendants and cases. Sections 4.4 and 4.5 examine the robustness of these findings to alternative specifications as well as their heterogeneity along several dimensions. Before proceeding, however, it is important to clarify the nature of the conclusions that can be drawn from this evidence.

First, and very directly, the results clearly indicate that there is an incredible amount of arbitrariness in trial outcomes for certain defendants. Arabic named defendants, for example, are convicted 26 percentage points more often when a Swedish Democrat is randomly assigned as a *nämndeman* for the trial than in the great majority of cases in which a Swedish Democrat is not assigned. Some arbitrariness in conviction rates is to be expected, of course, in any judicial system given the natural variation in the experience and judgment of jury members. To the extent that a particular *nämndeman* implicitly requires a lower standard of evidence in order to vote to convict, for example, conviction rates will be systematically higher for the cases on which they serve. But the magnitude of the arbitrariness in these cases and its relation to specific characteristics of the defendants and victims raises obvious concerns about whether the law is being applied equally in the basic determination of guilt versus innocence.

Although the results presented in Tables 4 and 5 lead to the clear conclusion that there is differential treatment of particular kinds of statistically identical cases when different *nämndemän* are assigned, without any direct measures of the objective quality of evidence in each case, it is impossible to determine which *nämndemän*—for example, those treating certain defendants more versus less harshly—are coming closer to reaching the objectively correct decision. That there is such a great amount of heterogeneity in the ultimate adjudication of these cases due completely to the assignment of *nämndemän* from different political parties has two potential implications. First, *nämndemän* associated with particular political parties exhibit substantial biases for or against particular types of defendants. Alternatively, the evidence presented in these criminal cases is subject to such wildly different subjective conclusions regarding guilt in a way that happens to fall exactly in line with the kinds of biases that might be expected given the platforms of the *nämndeman*'s political party.

4.4. Robustness

The central results presented in the first three columns of Table 5 are robust to a number of alternative specifications. Columns (4)–(6) of Table 5 replicate the first three columns, but change the dependent variable to an indicator of whether a defendant was convicted of at least one charge; the same pattern of results emerge. Table 6 demonstrates that the main results are not sensitive to the exclusion/inclusion of the baseline set of controls included in Table 5: defendant, offense, case and *nämndemän*

TABLE 6. Robustness of main results to controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable = share of current charges convicted								
Any Vänster näm.?	0.00671	0.0387	0.0414	0.0352	0.0299	0.0276	-0.00605	0.0420	0.0314
Any Swedish dem. näm.?	-0.155*	-0.171*	-0.172*	-0.219**	-0.218**	-0.212**	-0.222**	-0.205**	-0.207**
Any Vänster × any female victim	0.120**	0.130**	0.131**	0.127**	0.135**	0.135**	0.206**	0.134**	0.133**
Any Swedish dem. × any female victim	0.0838	0.0869	0.0894	0.147	0.154	0.154	0.173	0.155	0.150
Any Vänster × defendant Arabic name	-0.0217	-0.00188	-0.00520	0.000409	-0.0227	-0.0261	-0.0294	-0.0863	-0.0376
Any Swedish dem. × def. Arabic name	0.170**	0.217**	0.228**	0.241**	0.252**	0.258**	0.257**	0.219*	0.260**
Any Vänster × defendant <21	-0.0610	-0.113*	-0.126*	-0.131**	-0.134*	-0.143*	-0.160**	-0.146**	-0.125
Any Swedish dem. × defendant <21	0.281***	0.295***	0.281***	0.296***	0.319***	0.320***	0.319***	0.316***	0.329***
Def. Arabic name?	-0.00974	-0.0274	-0.0294	-0.0236	-0.0127	-0.0102	-0.00689	0.170	-0.0120
Any female victim?	-0.0272	-0.0284	-0.0277	-0.0235	-0.0289	-0.0289	-0.223	-0.0311	-0.0287
Defendant <21?	0.0138	0.00223	0.00376	-0.0409	-0.0124	-0.00937	-0.00487	-0.0126	-0.0555
Judge fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other näm. char. (age, experience, Swedish name)	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Def., case, off type, criminal history	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Hearing year and day of week FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Prosecution attorney FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Other party dummies	No	No	No	No	No	Yes	Yes	Yes	Yes
Other party dummies × any female victim	No	No	No	No	No	No	Yes	No	No
Other party dummies × def. Arabic name	No	No	No	No	No	No	No	Yes	No
Other party dummies × defendant <21	No	No	No	No	No	No	No	No	Yes

Notes: Each column presents the results of regressing the share of charges resulting in a conviction on the variables listed and the corresponding variables and interaction terms for the Green party. No controls are included in column (1); each subsequent column includes the additional set of controls listed in the lower panel of the table. Robust standard errors, clustered on judge name, are used to determine significance. Finally, all specifications condition on the sample who did not fully admit guilt. * $p < 0.10$; ** $p < 0.05$; *** $p > 0.01$.

characteristics and judge fixed effects. Column (1) of Table 6 includes just the main nämndemän political party variables and their simultaneous interactions with the three case characteristics considered in Table 5.³³ Column (2) adds judge fixed effects, which increases the magnitude of some of the coefficients but does not change the qualitative pattern of results; this is consistent with the results from the previous section suggesting judges are not perfectly randomly assigned to cases. Controlling for other nämndemän triplet characteristics (gender, age, experience, Swedish name) in column (3) does not affect the results, indicating that it is the nämndeman's party that matters and not another characteristic of nämndemän correlated with party choice. Column (4) controls for the vectors of defendant, case, offense, and criminal history characteristics; the robustness to this large set of controls is again indicative of the random assignment of triplets to cases. Finally, controlling for hearing year and day of week effects in column (5) as well as a dummy for each of the 81 prosecutors has no effect, indicating that the results are not driven by changes in the nämndemän pool and party views on crime over time or by attorney characteristics.³⁴

Finally, columns (6)–(9) of Table 6 demonstrate that the basic pattern of results in Table 5 is not driven by our choice to focus on the extreme left and right parties rather than looking at all parties. Column (6) adds dummies for each of the other six parties whereas columns (7)–(9) interact these party dummies with each case characteristic of interest: victim gender, defendant name, and victim age. These less extreme Social Democrat and Moderate block parties generally do not have a significant impact themselves, and the inclusion of these extra controls has little impact on the estimated effects of the presence of the extreme party nämndeman.

As discussed earlier, a possible concern with the results presented in Tables 5 and 6 is that nämndemän are not perfectly randomly assigned to each other. As a first step to assess whether balancing on the basis of gender, party and/or age is a concern, we compare the characteristics of the 793 triplets observed in the data to that of simulated triplets created by random assignment. To create the set of simulated triplets, we consider the rosters of nämndemän (seen in the data) before and after the 2010 election. For each roster, we randomly assign nämndemän to triplets, and note the average triplet characteristics. We run these simulations 10,000 times (for each roster). Table A.2 presents the average simulation value for each roster, and the weighted average across the two periods as well as the corresponding sample means

33. Note that because this specification includes all three interactions simultaneously, the coefficients on *any Vänster* and *any Swedish Democrat* will not be the same as in Table 5. However, the same general pattern of results is found among the subgroups of cases (i.e., female victims, Arabic named defendants) when one plugs in the mean level of the variables in the other interaction terms.

34. This specification rules out, for example, that the Vänster-female victim result is driven by the fact that Vänster prosecutors can present a more persuasive case to a triplet with a Vänster nämndeman. Controlling for fixed effects for the 232 defense attorneys (a demanding specification given there are only about 1,000 observations) has little qualitative impact, though there is some loss of precision for a couple of estimates and some changes in the coefficient magnitudes (some larger and some smaller).

for the actual triplets observed in each period and overall.³⁵ To evaluate where the actual sample mean falls in the simulated empirical distribution for each characteristic, we present the share of simulations that are less than, equal to, and greater than the observed sample mean. We find strong evidence consistent with the nämndemän coordinators' statements that they balance on gender. With random assignment, 10% and 15%, respectively, of triplets in the whole period would be all male and all female; but, in the real data, just 1% of triplets are all of the same gender. There is also evidence that the coordinators balance on parties, but to a lesser extent than gender. With random assignment, 13% and 12% of all triplets would contain no "soft" and all "soft" nämndemän, respectively, compared to 6% and 8% in the real data.³⁶ With respect to age, the simulated triplets are in fact quite comparable to the observed triplets (in contrast to the extreme gender and party composition, the differences in simulated and actual age are not significant).

The lower panels of Table A.2 present the simulated and actual characteristics of triplets with at least one Vänster nämndeman or one Swedish Democrat nämndeman. These statistics demonstrate that the nämndemän peers assigned by the coordinators to Vänsters and Swedish Democrats are quite comparable to those that would have been assigned under random assignment. The main exception is that Vänsters and Swedish Democrats are more likely to be assigned to each other. The extent to which this is a concern, however, is limited by the fact that our baseline specification controls for the presence of *both* Vänsters and Swedish Democrats. Moreover, Table A.1 demonstrates that our results are robust to excluding such triplets (with both a Vänster and Swedish Democrat) from the analysis.

As a final investigation into this issue, we focus on a subset of triplets that are comparable to each other, with the exception of the presence of the extreme party member. Specifically, we restrict the sample to cases with at least one Social Democrat and one Moderate block member, such that it is only the party of the third nämndeman that varies. This restriction, however, is extremely demanding on the data, as it keeps just 47 of 94 Swedish Democrat cases and 65 of 203 Vänster cases. The results are presented in Table 7 and are quite comparable to our baseline estimates, though there is not surprisingly some loss of precision. Despite this loss of precision, only the effect

35. The weighted average of the pre- and post-election means weights the simulated means by the share of triplets observed in that period in the real data. As we have more post-election data, this puts more weight on the 2011–2012 simulated triplets, which is consistent with more triplets being observed in the real data in that period as well.

36. One possibility we considered is that the balancing on parties is a result of the balancing on gender, due to correlations between gender and party membership. But simulations that formed random triplets, conditional on the presence of at least one male and female, yielded almost identical sample means for the other characteristics. We also tried to simulate triplets conditional on the days of the week they are observed in the real data, which more directly mimics the real assignment process (as nämndemän fill out forms indicating the days of the week they are available). But, once again, the triplet compositions are quite comparable to those created with simple random assignment, suggesting there is no systematic sorting of nämndeman parties across days of the week.

TABLE 7. Driven by extreme party? Restrictions to triplets with both social democrat and moderate block.

Sample:	Dependent variable: share/any current charges convicted											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)			
	Share All	Share Restricted	Any Restricted	Share All	Share Restricted	Any Restricted	Share All	Share Restricted	Any Restricted			
Any Vänster nämnd.?	-0.0126 (0.0520)	-0.00892 (0.0881)	0.0173 (0.0625)	0.0595 (0.0377)	0.0971 (0.0730)	0.109* (0.0550)	0.108** (0.0424)	0.157** (0.0645)	0.119* (0.0609)			
Any Swedish dem. nämnd.?	-0.0702 (0.0760)	-0.113 (0.109)	-0.0326 (0.0967)	-0.0866 (0.0586)	-0.104 (0.0801)	-0.0861 (0.0678)	-0.0788 (0.0568)	-0.125 (0.0823)	-0.0869 (0.0717)			
Any Vänster × any female victim	0.148*** (0.0558)	0.225*** (0.106)	0.166* (0.0965)									
Any Swedish dem. × any female victim	0.0637 (0.0958)	0.0677 (0.137)	-0.0243 (0.123)									
Any Vänster × defendant Arabic name				-0.0115 (0.0661)	0.0766 (0.0898)	-0.0119 (0.0844)						
Any Swedish dem. × defendant Arabic name				0.260*** (0.100)	0.135 (0.135)	0.202* (0.106)						
Any Vänster × defendant <21							-0.153** (0.0622)	-0.153 (0.113)	-0.0473 (0.0828)			
Any Swedish dem. × defendant <21							0.239*** (0.0764)	0.241*** (0.113)	0.187* (0.107)			
Defendant Arabic name?	-0.00483 (0.0327)	0.00692 (0.0327)	0.00432 (0.0301)	-0.0236 (0.0374)	-0.00322 (0.0444)	0.00968 (0.0416)	-0.00146 (0.0312)	0.00555 (0.0328)	0.00804 (0.0299)			
Any female victim?	-0.0218 (0.0390)	-0.00772 (0.0426)	0.00416 (0.0470)	0.00252 (0.0320)	0.0300 (0.0397)	0.0322 (0.0444)	0.00295 (0.0310)	0.0291 (0.0385)	0.0284 (0.0430)			
Defendant <21?	-0.0268 (0.0347)	0.0540 (0.0427)	0.0554 (0.0401)	-0.0235 (0.0357)	0.0540 (0.0420)	0.0539 (0.0401)	-0.0303 (0.0432)	0.0439 (0.0489)	0.0324 (0.0468)			
N	961	629	629	961	629	629	961	629	629			
R-squared	0.219	0.318	0.341	0.217	0.314	0.342	0.225	0.321	0.343			

Notes: Columns (1), (4), and (7) present the baseline using the full sample. The remaining columns restrict to cases with at least one social democrat and one member of the moderate block, so that it is just the composition of the third nämndeman that varies. This restriction is extreme, as it keeps just 47 of 94 Swedish democrat cases and 65 of 203 Vänster cases. Each specification includes the baseline controls: judge fixed effects, defendant, case, offense, and criminal history controls, and parallel interactions for Green party membership. Robust standard errors, clustered on judge, are used. All specifications condition on the sample who did not fully admit guilt. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

of Vänster nämndemän on young defendants falls below conventional thresholds for statistical significance in these specifications.

Restricting the analysis to triplets with one Social Democrat and one Moderate block member also begins to get at the idea that the impact of adding an extreme party nämndeman may depend on the remaining composition of the triplet. When a Vänster or Green sits on such a triplet, the majority of the triplet is “Left” leaning, whereas when a Swedish Democrat sits on the triplet, the majority is right-leaning. Table A.3 examines this question more broadly in the full set of triplets. Specifically, we expand the baseline specifications presented in Table 5 to include an additional interaction with whether the majority of the triplet is from a party considered to be on the left of the political spectrum (i.e., Social Democrats, Greens, and Vänsters). We do this separately for each case characteristic. We find that the previously seen effect of having a Vänster on the triplet for female victim cases is driven by cases in which the majority of the triplet is from the left. More difficult to understand is the finding that the impact of Swedish Democrats on female victim cases is greater in the cases in which the majority of nämndemän are from the “left”. In contrast, the total effect of a Vänster is negative when sitting with a majority of “right” nämndemän. Although we have empirically tested and ruled out that this result is driven by Vänster and Swedish Democrat nämndemän sitting together, we cannot provide an intuitive explanation. Having a Swedish Democrat in the triplet increases (substantially) the share of convicted charges for an Arabic named defendant, regardless of whether the majority composition is from the left.

5. Peer Effects in the Deliberation Process

The previous section documented two key results: (i) having a Vänster in the triplet increases convictions when there is a female victim and (ii) having a Swedish Democrat increases convictions when there is an Arabic named defendant. The most obvious way in which adding nämndemän from the far-left and far-right parties can have an impact on verdicts is that their vote simply shifts the majority opinion in the case toward their viewpoint. We term this the *direct effect*. However, it is also possible that in addition to having a direct effect, these nämndemän have an *indirect effect* by influencing the votes of their peers.

There are a number of different ways in which the presence of an extreme party nämndeman can influence the vote of his or her peers during jury deliberations. First, there can be *sway effects*, in which one’s opinion moves toward the views of a peer due to persuasion or the sharing of relevant information. Second, and in contrast to sway effects, an individual can *dig-in* or harden their own initial position in the presence of a particular kind of peer. MacCoun (1989) terms this group polarization, where deliberations make group members more extreme in their views than they were before deliberating. Empirically, a sway effect would imply that an individual votes in the same direction or in accordance with the views of his extreme party peer whereas a digging-in effect would imply the opposite. Third, termed *conformity*, an individual’s

vote may be influenced by his basic desire to reach the same judgment as one's peers. Finally, one may engage in *dissent aversion*, that is, a willingness to switch one's vote to avoid a formal dissent (or a preference for unanimity).

The unique structure of the Gothenburg District Court, whereby the same nämndeman sits on numerous court cases with different peers in their triplet, allows us to explicitly determine whether peer effects are present. Section 5.1 presents the results of a reduced-form analysis of peer effects, providing empirical evidence that Vänsters and Swedish Democrats impact the votes of their peers in cases with female victims and Arabic named defendants, respectively. A fundamental question in understanding the importance of these peer effects is whether jurors are only changing their votes to avoid dissent in cases in which their vote would not be pivotal or whether the peer effects affect decision-making in a way that actually impacts trial outcomes. Although we cannot provide firm causal evidence on this question, our peer effects analysis concludes with suggestive evidence that at least some of the peer influence is such that it has consequences on actual outcomes.

5.1. *Measuring Reduced Form Peer Effects*

We begin our analysis of peer effects by estimating whether a nämndeman's vote is affected when they are seated in a triplet with either a Vänster or Swedish Democrat. To do this, we transform the defendant level data set that we have been using into an individual nämndeman-defendant data set. That is, each observation in the original defendant data set now represents three (nämndemän) observations in the new data set associated with each nämndemän's verdict.³⁷ Recall that since verdicts do not need to be unanimous, the three nämndemän who sit on the same case may vote differently with respect to whether to convict. In reality, however, less than 2% of nämndemän dissent on the verdict. Importantly, we define dissent on the verdict to mean dissent with respect to at least one charge (if there are multiple charges).³⁸

37. Note that there are some nämndemän for which information on background characteristics, particularly age and experience, is missing. So as to not decrease the sample size in the main case level analysis, nämndemän triplet characteristics were based on the average for those nämndemän for which this information is observed. In the individual level analysis, we omit those observations with missing own nämndeman characteristics, so there are some cases from the main data set that only translate into two nämndemän level observations.

38. The fact that there are so few dissents suggests that some of the potential peer effects we might find are due to dissent aversion. This will be explicitly discussed in Section 5.2. An examination of the determinants of individual nämndeman dissents (available from the authors upon request) shows that few variables are significantly related to dissenting. However, one does see that sitting with extreme peers from the same side of the political spectrum as oneself significantly decreases the likelihood of dissenting: "right" and "left" nämndemän sitting with Swedish Democrat and Vänster peers, respectively, are less likely to dissent. In contrast, sitting with extreme peers from the opposite end of the political spectrum as oneself (i.e., "left" nämndemän with Swedish Democrat peers and "right" nämndemän with Vänster peers) increases the likelihood of dissenting; though the point estimates are as large as those for peers on the same side of the spectrum, they are insignificant.

The reduced-form peer effects specification that we take to the data is given by

$$Y_{nij} = \beta \text{PeerChar}_{nj} + Z_{ij} \pi + \lambda_{judge} + \alpha_n + \varepsilon_{nij}, \quad (2)$$

where Y represents the share of charges on which nämndeman n assigned to case j with defendant i votes to convict. PeerChar_{nj} is a vector of peer characteristics including indicators for whether any of nämndeman n 's peers on case j are members of the Vänster, Swedish Democrat, and Green parties, respectively, as well as a vector of other peer nämndemän characteristics (e.g., age, gender, experience). As stated previously, Z is a vector of case and defendant characteristics and λ_{judge} represents judge fixed effects. Finally, α_n represents nämndeman fixed effects. All standard errors in the individual analysis are clustered at the case level.³⁹

The peer characteristic coefficient, β , is the reduced form effect on nämndeman n 's vote to convict of having, for instance, a Vänster peer on the triplet. Underlying this reduced form effect are two channels that we cannot disentangle from one another (Manski 1993). The first, exogenous or contextual peer effects, reflects the effect of the simple presence of the Vänster peer and potentially the beliefs associated with his party affiliation. In this regard, it is important to keep in mind that the presence of a Vänster peer can alter the nature of a discussion and evaluation of evidence during deliberations, regardless of how this Vänster individual eventually votes. Thus, the contextual peer effect is such that it is the characteristics of the extreme party peer that matter and not the behavior (or vote) of the peer. In contrast, the endogenous peer effect is the direct impact of the extreme party peer's vote on the vote of nämndeman n .

Using nämndeman fixed effects implicitly focuses the analysis on the variation across the cases on which a given nämndeman sits, measuring how their vote is affected when they sit with a Vänster or Swedish Democrat relative to when they sit with more centrist peers. To interpret β as a causal peer effect, two assumptions must be satisfied. The first is that nämndemän are randomly assigned to Vänster and Swedish Democrat peers. The second is that a given nämndeman's pairing with a Vänster or Swedish Democrat peer is uncorrelated with defendant and case characteristics. Clearly, concerns may arise regarding the first assumption, given the nämndemän coordinators statements regarding attempts to balance triplets based on gender, age, and political party. However, as discussed previously, our simulations of random triplets from the rosters indicate that this balancing is primarily done with respect to gender. With respect to political party, the main balancing action was to avoid three triplet members from the same side of the political spectrum and to over-assign Vänsters and Swedish Democrats to each other. The results presented in what follows are robust to excluding triplets with both Vänsters and Swedish Democrats seated.

With regards to the second assumption, the pairing of nämndemän with each other does appear to be independent of case and defendant characteristics; this is borne out in the results presented in Table 3, which show that the composition of the triplet is

39. The main peer effect results are insensitive to clustering the standard errors on the individual nämndeman.

largely unrelated to case and defendant characteristics. Further, in results not shown, we regress the characteristics of a given nämndeman's peers (such as whether they have a Vänster peer, etc.) on case and defendant characteristics along with nämndeman fixed effects. The overall pattern of results is similar to Table 3 in that there are very few significant coefficients.

Table 8 presents the results of estimating equation (2); the dependent variables in Panels A and B, respectively, are whether the individual nämndeman votes to convict on any charges and the share of charges on which he votes to convict. Because we would expect Vänsters and Swedish Democrats to exert peer effects in the cases for which they have distinctly different viewpoints, we accordingly test for the presence of peer effects separately for cases with and without female victims (columns (1) and (2)), cases with and without Arabic named defendants (columns (3) and (4)), and cases with younger (<21) and older defendants (columns (5) and (6)). For brevity, we only report the coefficients associated with peer effects.

The results in column (1) indicate that for female-victim cases, sitting with a Vänster peer increases the share of charges on which a nämndeman votes to convict by 13 percentage points versus sitting with a more centrist peer. Column (2) reveals that there is no effect of having a Vänster peer in nonfemale victim cases. Likewise, columns (3) and (4) reveal that having a Swedish Democrat peer increases the share of convictions for cases with Arabic named defendants, but has no effect for defendants with non-Arabic names. A more difficult to understand result is that we see the same pattern of results for nämndemän sitting with a Vänster peer on Arabic named defendant cases. Such an effect is generally inconsistent with the Vänster party's political viewpoints, who, as seen in Table 1, differ from most other parties in terms of their views of immigrants.⁴⁰ Column (5) shows that for cases with young defendants, having a Swedish Democrat peer increases conviction rates, although the effect is not significant and column (6) reveals that among cases with older defendants, the presence of a Vänster peer increases conviction rates.

Overall, the results from Table 8 provide strong evidence of peer effects for the cases in which Vänsters and Swedish Democrats have distinct opinions. Moreover, in most cases, the peer effects these more extreme parties exert are consistent with their direct effect on trial outcomes. Additional analyses reported in a previous version of this paper of whether some types of nämndemän (characterized by gender, age, and experience) are more affected by their peers suggests that peer effects tend to influence the most inexperienced nämndemän most strongly (Anwar, Bayer, and Hjalmarsson 2015).⁴¹

40. Although one possible explanation for this finding is that an extreme left Vänster peer causes members of other parties to dig in or take a firmer stand, we cannot provide conclusive evidence that this is the case. In addition, we do not have any theory as to what circumstances lead to digging in.

41. Previous field studies of actual juries have looked at which jurors are the most influential. These studies found that (i) male jurors are more influential on their peers than females (Mills and Bohannon 1980; Marcus et al. 2000), (ii) higher socioeconomic status jurors are more influential (York and Cornwell 2006), and (iii) more extraverted jurors are more influential (Marcus et al. 2000; Clark et al. 2007). These

TABLE 8. Reduced form peer effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	Any female victim cases	No female victim cases	Defendant Arabic name cases	Defendant non-Arabic name cases	Defendant <21 cases	Defendant >21 cases
<i>Panel A. Dependent Variable: any current offenses convicted by individual nämndeman</i>						
Any Vänster peers on triplet?	0.123*** (0.0347)	0.00869 (0.0413)	0.242*** (0.0748)	0.0552* (0.0302)	-0.0108 (0.0567)	0.0917*** (0.0328)
Any Swedish dem. peers on triplet?	0.0381 (0.0646)	-0.0629 (0.0735)	0.294** (0.144)	-0.00953 (0.0479)	0.0173 (0.0860)	-0.00218 (0.0514)
<i>Panel B. Dependent variable: share of current offenses convicted by individual nämndeman</i>						
Any Vänster peers on triplet?	0.127*** (0.0380)	0.00606 (0.0430)	0.236*** (0.0793)	0.0434 (0.0311)	-0.0298 (0.0536)	0.0989*** (0.0333)
Any Swedish dem. peers on triplet?	0.0517 (0.0667)	-0.107 (0.0771)	0.276* (0.162)	-0.0363 (0.0501)	0.0835 (0.0945)	-0.0333 (0.0556)
Judge fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Def./offense/case controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Own nämndemän fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1474	1378	655	2197	801	2051

Notes: Each specification is still restricted to the sample for which the defendant did not fully admit guilt and by defendant or case characteristics listed at the top of the column. All specifications include the baseline set of controls, as well as nämndeman fixed effects. Robust standard errors, clustered on case, are in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Finally, we show explicit evidence that Vänsters and Swedish Democrats do themselves exert a direct effect on the trial outcome in Table A.4.⁴² The analysis in this table exploits the fact that we observe different nämndemän sitting with the same set of peers to identify the effects of a nämndeman's own political party on their decision to convict.⁴³ These results firmly establish that both Vänsters and Swedish Democrats are themselves more likely to vote to convict in cases with female victims and Arabic named defendants relative to cases with male victims and non-Arabic named defendants, respectively.

5.2. Do Peer Effects Impact Trial Outcomes?

The previous results demonstrate that Vänsters and Swedish Democrats typically exert peer effects on nämndemän from centrist parties that are consistent with their respective party platforms. This section aims to determine whether these peer influences actually affect trial outcomes, or are, in some sense, inconsequential to the verdict. That is, are individuals simply changing nonpivotal votes in order to make the final decision unanimous (i.e., dissent aversion)? A nämndeman may wish to avoid a formal dissent because of a potential negative collegial effect on the judge (who must write up a brief summary of the dissent) and the other nämndemän in the majority opinion, given that they may sit together again in future cases.⁴⁴ Or, are the nämndeman's underlying opinions and decisions actually affected (as in the sway, digging in, or conformity mechanisms) by the extreme party peers in a way that affects verdicts?

Although we cannot provide conclusive evidence with respect to these questions, Table 9 provides suggestive evidence that a "sway" effect plays some role.⁴⁵ Specifically, Table 9 aggregates the individual vote data back up to the case level and presents tabulations of the number of nämndemän who voted to convict on at least one charge: 0 votes, 1 vote, 2 votes, or 3 votes. Columns (1) and (2) consider how these tabulations change when adding a Vänster nämndeman to cases with a female victim whereas columns (3) and (4) present the parallel tabulations for nonfemale victim cases. Columns (5) and (6) consider the addition of a Swedish Democrat nämndeman

studies often use small case samples and are based on the "selected" seated jury. See Devine (2012) for a more thorough review of this literature.

42. Note that if there were no direct effect, the overall impact of adding a Vänster or Swedish Democrat would come about *solely* because *other* parties became more likely to convict when they sat with these extreme party members.

43. Specifically, we regress an individual nämndeman's vote to convict on a vector of dummies that capture the nämndeman's own party affiliation, judge fixed effects, the full set of case controls, and peer fixed effects.

44. The idea of dissent aversion was put forth in studies of judge panel effects in federal courts of appeals (Posner 2008; Epstein et al. 2011) where a dissent can impose a cost both on the dissenting judge (who writes up the dissenting opinion) and the judge that writes up the majority opinion (and must respond to the dissent). Subsequently, Fischman (2011) used a structural model to show that judicial voting patterns in federal appeals courts were consistent with judges having a cost of dissent.

45. We thank the editor for suggesting this analysis.

TABLE 9. Sway effects versus dissent aversion: Does the presence of the extreme nämndemän affect case outcomes?

	(1) Any female victim		(2) Vänster nämän.		(3) No female victim		(4) Vänster nämän.		(5) Defendant Arabic name		(6) Swedish defendant non-Arabic name		(7) No Swedish defendant non-Arabic name		(8) Swedish defendant non-Arabic name	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
0 votes to convict	53	13	2	2	47	13	16	15	27	13	0	0	82	12	11	14
1 vote to convict	4	1	1	1	1	0	2	2	3	1	0	0	3	0	2	3
2 votes to convict	8	2	0	0	6	2	2	2	4	2	1	6	8	1	3	4
3 votes to convict	359	85	87	97	307	85	90	82	180	84	17	94	599	87	60	79
<i>p</i> -value on test that share with 2 or 3 votes is equal across subsamples	0.0066		0.4182		0.0894		0.2323									

Notes: Tabulations of the number of votes to convict on at least one offense by nämndemän (maximum guilty votes = 3) for each case in which the defendant did not fully admit guilt. Each column corresponds to a different subsample. For instance, column (1) looks at the number of and percent of cases with 0, 1, 2, or 3 nämndemän guilty votes for female victim cases with no Vänster nämndemän.

to Arabic named defendant cases whereas columns (7) and (8) present the non-Arabic defendant cases.

There are two clear phenomena apparent in the table. First, there is a high degree of unanimity regardless of whether the extreme party *nämndeman* is sitting on the triplet or not. Second, when the extreme party *nämndeman* is seated on cases salient to their platform, almost all of the unanimous verdicts are for conviction. For instance, for the female victim without a *Vänster* *nämndeman* sample, 13% have 0 votes to convict, 1% have 1 vote, 2% have 2 votes, and 85% have 3 votes. Adding the *Vänster* *nämndeman* shifts almost all of the weight to the unanimous conviction: 2%, 1%, 0%, and 97% respectively. A similar pattern is seen when adding a Swedish Democrat *nämndeman* to triplets for Arabic named defendant cases. With no Swedish Democrat, 13% unanimously acquit whereas 85% unanimously convict compared to 2% versus 97%, respectively, for Arabic named defendant cases with a Swedish Democrat. In contrast, when adding an extreme party *nämndeman* to the nonsalient cases (nonfemale victim and non-Arabic named defendants), the unanimous acquittals do not disappear.

What do these results tell us about the channel underlying peer effects? First, the extent of unanimous decisions indeed suggests (not surprisingly) that dissent aversion plays a significant role. However, we would also argue that these results are suggestive that there is at least some consequential sway effect underlying the reduced form peer effect because adding the extreme party *nämndeman* shifts almost all of the unanimous acquittals to unanimous convictions. The unanimous acquittals seen without the extreme party *nämndeman* can be composed of two types of verdicts: one where someone is avoiding dissent and would rather convict and one where there is a true unanimous opinion to acquit. In the former case, adding an extreme *nämndeman* who would vote to convict could result in a unanimous conviction either because of dissent aversion (it would just be a different *nämndeman* dissenting) or sway. But in the latter case with a true unanimous acquittal, adding the extreme party *nämndeman* should not result in a shift to a unanimous conviction via dissent aversion. Thus, under the assumption that at least some portion of the unanimous acquittals represent true votes to acquit (which seems reasonable), then it is likely that sway effects play some role.

6. Conclusion

Our findings imply that significant biases exist in the Swedish *nämndemän* system—biases that are, in fact, closely associated with positions of the *nämndeman*'s political party. For instance, having a far-left *Vänster* or far-right Swedish Democrat in the *nämndeman* triplet significantly impacts the likelihood of conviction in cases closely related to the party's platform—female victims (feminism) and Arabic named defendants (anti-immigration), respectively. The latter finding may be particularly relevant outside of Sweden, given the rise of far right parties throughout Europe that are similar in platform to the Swedish Democrats.

These results are important for two reasons. First, they highlight that systematic biases are prevalent in criminal justice systems around the world, regardless of the system under which jurors are chosen. Second, they highlight the arbitrary nature and inherent *ex post* unfairness of these criminal justice systems, especially for minority defendants.

Recent empirical studies and a long history of anecdotal evidence have raised serious concerns about biases in the U.S. justice system, where a “representative” jury is typically determined by a random draw from a list of eligible jurors from the local community. Yet, the current study demonstrates that the same types of biases exist in Sweden’s *nämndeman* or lay judge system. Similar to other European countries, Sweden uses proportionate political representation as an alternative approach to selecting jurors representative of the local population’s political ideologies.⁴⁶ One may then ask why such “biases” are a concern? Is this not exactly what the system was designed to achieve? Unfortunately, we would argue that it is not. Although the pool of available *nämndemän* may represent a cross section of societal opinion, a defendant’s case is not decided by the entire pool—just three *nämndemän* randomly chosen from the pool. In a seated jury of three individuals and society with nine political parties that are oftentimes far apart on the political spectrum, small sample problems imply that defendants are unlikely to face a triplet that is in and of itself representative of society.

The second issue is one of fairness and the arbitrary nature of the justice system. Consider two individuals charged with identical female victim crimes. On the day that these individuals are arrested, one can say that the justice system is fair—they have the same *a priori* chance of being convicted (since they have the same chance of having a *Vänster* on their triplet). Their chance of being convicted remains equal as their trials approach and they walk into the courthouse and up to the doors of the courtroom. But, as they step over the threshold, one enters a courtroom with a *Vänster* *nämndeman* and the other has no *Vänster* on the triplet. Without any evaluation of the evidence of the case, these two identical defendants now (*ex post*) have different chances of being convicted. The random assignment of defendants to courtrooms, which is in itself intended to promote fairness, in fact can lead to *ex post* unfair trial outcomes. Should not the goal be a criminal justice system that is fair throughout?

From the perspective of the public policy debate in Sweden concerning the *nämndemän* system, a key finding of our study is that the political affiliations of *nämndemän* affected the actual verdicts in the case and were not just reflected in dissenting opinions. Thus, contrary to some opinions in Sweden and around the world, *nämndemän* (and lay judges more generally) are not inconsequential, even when deliberating in collaboration with a professional judge. This implies that proposals to reduce the role of *nämndemän* (or eliminate them from some cases) would have a real impact on verdicts.

46. There are, of course, other key differences between the U.S. and Swedish systems. American verdicts are unanimous although a majority is required in Sweden and verdicts are decided by American juries without the presence of the professional judge, whereas Swedish verdicts are decided by a professional judge in collaboration with three lay judges.

Alternative appointment processes that aim to de-emphasize the role of politics have also been suggested. Recent government proposals to change the nämndemän system included suggestions to: (i) not appoint nämndemän in direct connection with elections and (ii) investigate how to introduce a share of nämndemän who would be chosen outside the political parties. Whether these measures would reduce biases in the nämndemän system associated with political ideology depends, of course, on whether the nämndemän that would be appointed through this process would be materially different than those that serve in the current system.

Appendix: Robust Checks and Additional Analyses

TABLE A.1. Sensitivity checks of main triplet level results.

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline	No female defendants	(a)	Baseline	Break out "other" ethnic category	(a)	Baseline	(a)
Any Vänster nämn.?	-0.013	-0.031	0.060	0.039	0.057	0.11**	0.10**
Any Swedish democrat nämn.?	-0.070	-0.072	-0.087	-0.11	-0.094	-0.079	-0.092
Any Vänster × any female victim	0.15***	0.15**					
Any Swedish dem. × any female victim	0.064	0.052					
Any Vänster × defendant Arabic name			-0.012	0.012	-0.032		
Any Swedish dem. × defendant Arabic name			0.26**	0.28**	0.20*		
Any Vänster × defendant non-Arabic/non-Swedish name			0.071	0.071			
Any Swedish dem. × defendant non-Arabic/non-Swedish name			0.050				
Any Vänster × defendant < 21							
Any Swedish dem. × defendant < 21							
Defendant Arabic name	-0.0048	0.0077	-0.024	-0.056	-0.015	-0.0015	-0.0065
Any female victim?	-0.022	-0.021	0.0025	0.0063	0.00071	0.0030	0.000057
Defendant < 21?	-0.027	-0.039	-0.024	-0.023	-0.026	-0.030	-0.030
Defendant non-Arabic/non-Swedish name				-0.087**			
Constant	0.95***	0.97***	0.93***	0.97***	0.94***	0.91***	0.92***
<i>N</i>	161	875	961	961	945	961	945
<i>R</i> -squared	0.219	0.220	0.217	0.227	0.217	0.225	0.227

Notes: Columns labeled baseline use the full sample of cases for which guilt was not fully admitted. Column (2) excludes female defendants. All columns labeled (a) exclude those cases with both a Vänster and Swedish democrat sitting together. Each specification includes the baseline set of controls: judge fixed effects, defendant case, offense and criminal history controls, and parallel interactions for Green party membership. Robust standard errors, clustered on judge, are used. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

TABLE A.2. Comparison of actual triplet composition to simulated random triplets.

Variable	2011–2012 Roster				2009–2010 Roster				2009–2012	
	10,000 Simulations (A)		Actual triplets		10,000 Simulations (B)		Actual triplets		Wgt. Avg. of Sim. A and B Mean	Actual triplets Mean
	Mean	$p < x$	$p = x$	$p > x$	Mean	$p < x$	$p = x$	$p > x$		
<i>All triplets</i>										
Average birth year	1955.50	$N = 464$	1952.89	0.423	0.014	0.562	1954.24	1952.18	1954.98	$N = 793$
Share all male nämn.	0.11	0.01	0.000	1.000	1.000	0.09	0.01	0.000	0.10	1952.59
Share all female nämn.	0.14	0.00	0.000	1.000	1.000	0.17	0.02	0.000	0.15	0.01
Share no soft nämn.	0.14	0.06	0.000	0.999	0.001	0.11	0.05	0.003	0.13	0.06
Share all soft nämn.	0.11	0.05	0.002	0.994	0.004	0.14	0.11	0.144	0.12	0.08
<i>Triplets with at least one Vänster</i>										
Avg. # Vänster nämn.	1.07	1.06	0.467	0.533	0.000	1.08	1.06	0.200	1.07	$N = 115$
Avg. # social dem. nämn.	0.65	0.48	0.082	0.904	0.014	0.76	0.79	0.537	0.70	1.06
Avg. # Swedish dem. nämn.	0.07	0.15	0.941	0.059	0.000	0.02	0.06	0.903	0.05	0.62
Avg. # folk party nämn.	0.19	0.19	0.480	0.517	0.004	0.26	0.36	0.834	0.22	0.26
Avg. # center party nämn.	0.03	0.04	0.484	0.460	0.056	0.02	0.01	0.000	0.02	0.03
Avg. # Christian dem. nämn.	0.08	0.06	0.423	0.577	0.000	0.15	0.13	0.398	0.11	0.09
Avg. # moderate nämn.	0.57	0.69	0.821	0.179	0.000	0.52	0.44	0.271	0.55	0.58
Avg. # Green nämn.	0.20	0.21	0.573	0.427	0.000	0.16	0.16	0.505	0.18	0.19
<i>Triplets with at least one Swedish democrat</i>										
Avg. # Vänster nämn.	0.14	0.19	0.772	0.216	0.012	0.15	0.21	0.626	0.14	$N = 87$
Avg. # social dem. nämn.	0.64	0.68	0.505	0.378	0.117	0.74	0.89	0.315	0.66	0.20
Avg. # Swedish dem. nämn.	1.03	1.00	0.000	0.311	0.689	1.01	1.00	0.000	1.03	1.00
Avg. # folk party nämn.	0.19	0.16	0.334	0.479	0.187	0.26	0.11	0.440	0.20	0.15
Avg. # center party nämn.	0.03	0.00	0.000	0.725	0.000	0.02	0.05	0.999	0.03	0.01
Avg. # Christian dem. nämn.	0.08	0.00	0.000	0.594	0.406	0.15	0.00	0.000	0.09	0.00
Avg. # moderate nämn.	0.56	0.69	0.776	0.217	0.006	0.50	0.58	0.532	0.55	0.67
Avg. # Green nämn.	0.20	0.22	0.576	0.423	0.001	0.15	0.16	0.619	0.19	0.21

Notes: Using the observed roster of *nämndemän* for 2011–2012 (post-election) and 2009–2010 (pre-election), randomly assigned sets of triplets are simulated 10,000 times (separately for each roster). These empirical distributions of roster characteristics are compared to the characteristics of the actual triplets in those periods. The last two columns look at the combined sample period, where the means for the simulated samples are weighted by the share of real triplets observed in the data in each period: 0.59 for the post period and 0.41 for the pre period. For each period, we report the share of simulated assignments that are less than the observed actual value, as well as the shares equal and greater. Since many of the variables are discrete (and oftentimes take on few values), this helps to understand where the observed value lies in the empirical distribution. Average # vägvallet is not shown for presentation reasons and since they are not on the 2009–2010 roster.

TABLE A.3. The effect of triplet majority.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: share of current charges convicted case characteristics					
	Any female victim?		Defendant Arabic name?		Defendant <21?	
Any Vänster näm.n.?	-0.0126 (0.0520)	-0.00634 (0.0512)	0.0595 (0.0377)	0.0621 (0.0382)	0.108** (0.0424)	0.112** (0.0452)
Any Swedish dem. näm.n.?	-0.0702 (0.0760)	-0.0741 (0.0769)	-0.0866 (0.0586)	-0.0879 (0.0582)	-0.0788 (0.0568)	-0.0806 (0.0568)
Any Vänster × case characteristic	0.148*** (0.0558)	0.0496 (0.0750)	-0.0115 (0.0661)	-0.118 (0.0749)	-0.153** (0.0622)	-0.187** (0.0855)
Any Swedish dem. × case characteristic	0.0637 (0.0958)	0.0374 (0.103)	0.260** (0.100)	0.234* (0.127)	0.239*** (0.0764)	0.228** (0.0891)
Any Vänster × case char × majority “left”		0.123* (0.0705)		0.191 (0.117)		0.0619 (0.0979)
Any Swedish dem. × case char × majority “left”		0.201* (0.105)		0.125 (0.144)		0.0267 (0.122)
Any female victim?	-0.0218 (0.0390)	-0.0190 (0.0388)	0.00252 (0.0320)	0.00316 (0.0315)	0.00295 (0.0310)	0.00329 (0.0307)
Defendant Arabic name?	-0.00483 (0.0327)	-0.00560 (0.0327)	-0.0236 (0.0374)	-0.0194 (0.0371)	-0.00146 (0.0312)	0.00154 (0.0317)
Defendant <21?	-0.0268 (0.0347)	-0.0212 (0.0359)	-0.0235 (0.0357)	-0.0180 (0.0348)	-0.0303 (0.0432)	-0.0261 (0.0445)
Majority “left”		-0.0292 (0.0307)		-0.0132 (0.0279)		-0.00989 (0.0298)
<i>N</i>	961	961	961	961	961	961
<i>R</i> -squared	0.219	0.224	0.217	0.222	0.225	0.228

Notes: The dependent variable in each specification is the share of current charges resulting in conviction. All specifications include the full set of controls and same restrictions as main specifications (i.e., individuals who do not fully admit guilt). Specifically, controls for judge fixed effects, defendant, offense, case, and nämndemän characteristics are characteristics; note that having a Green in the triplet, and the corresponding interactions, are also included. “Left” is defined as Vänster, Green, and social democratic parties. The case characteristic included in the interaction varies across columns and is denoted at the top of each column. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

TABLE A.4. Own party effects on conviction by case characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: share of current charges on which individual nämndeman votes to convict					
	Any female victim cases	No female victim cases	Defendant Arabic name cases	Defendant non-Arabic name cases	Defendant <21 cases	Defendant >21 cases
Vänster nämn. (self)	0.0967** (0.0409)	0.0274 (0.0518)	0.0805 (0.0784)	0.0452 (0.0385)	0.0475 (0.0742)	0.0618* (0.0365)
Green nämn. (self)	0.0271 (0.0413)	0.0210 (0.0508)	-0.132 (0.0807)	0.00161 (0.0368)	0.191** (0.0860)	-0.0245 (0.0369)
Swedish dem. nämn. (self)	0.00766 (0.0746)	-0.140 (0.0937)	0.0948 (0.168)	-0.0831 (0.0583)	0.183 (0.140)	-0.0958 (0.0641)
Moderate nämn. (self)	-0.0162 (0.0300)	-0.0270 (0.0395)	-0.0990 (0.0719)	-0.0211 (0.0251)	0.164** (0.0822)	-0.0203 (0.0257)
Folk, Christian dem., or center nämn. (self)	-0.00809 (0.0341)	0.0340 (0.0465)	-0.199** (0.0778)	0.0528* (0.0301)	0.169** (0.0764)	0.0225 (0.0288)
Judge fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Def./offense/case controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer nämndeman FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1469	1373	657	2185	795	2047
R-squared	0.665	0.677	0.855	0.535	0.721	0.532

Notes: Dependent variable = share of current charges that individual nämndeman votes to convict on. The sample includes those individuals who did not fully admit guilt. Each column further restricts the sample to a subsample indicated at the top of the column. All specifications control for judge fixed effects, defendant, offense, and case characteristics, as well as fixed effects for each nämndeman peer. In addition, own characteristics (other than party) are included as controls. Robust standard errors, clustered on case, are in parentheses. * $p > 0.10$; ** $p < 0.05$.

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Supplementary Data

Supplementary data are available at [JEEA](https://academic.oup.com/jeea/article/17/3/834/4981454) online.