

# Politics in the Courtroom: Political Ideology and Jury Decision Making

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## *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, while convictions in cases with a female victim increase markedly when they are assigned jurors from the far-left (feminist) *Vänster* party. The results also indicate the presence of multiple types of peer effects including some instances where jurors from the far-left and far-right parties sway their peers towards their own positions and others where their presence on the jury hardens the opposing views of their peers.

JEL No. K14, K40

Keywords: jury decisions, peer effects, group decision-making, political party, *nämndemän*

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\* This research would not have been possible without the assistance of a number of individuals at the Gothenburg District Court. In particular, we would like to thank the two *nämndemän* coordinators, Assistant Judge Christoffer Önrup, Judge of Appeal Christer Ranch, and Assistant Judge David Caldevik for their help answering numerous questions and Mikael Sjöberg in the Archives for his assistance collating all of the court documents. We would also like to thank two research assistants (Ulrika Kjellström and Magnus Landergren) for their help collecting the data and Matthew Lindquist, Emma von Essen, Erik Hjalmarsson, Johan Lönroth, and Jordi Blanes I Vidal as well as seminar participants at Queen Mary University of London, Gothenburg University, Aarhus, Leciester University, Stockholm School of Economics, Lund University, Växjö, the Swedish Institute for Social Research at Stockholm University, the Paris School of Economics and the Bath Crime Economics Workshop for valuable feedback. Hjalmarsson would also like to acknowledge funding from Vetenskapsrådet (The Swedish Research Council) and Jan Wallanders och Tom Hedelius Stiftelse Tore Browaldhs Stiftelse, P2014-0200:1. Corresponding author: Randi Hjalmarsson, Email: randi.hjalmarsson@economics.gu.se

## *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 modern constitutions of the vast majority of nations. 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 ordinary citizens selected randomly from the community, while 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.

While 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. While 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 hard evidence on jury decision-making using data from actual trials anywhere in the world. Due to restrictions and severe limitations on the use of trial data, the current empirical literature consists primarily of a small number of

correlation studies using data from the United States.<sup>1,2</sup> Unfortunately, these studies are generally subject to concerns about the non-random 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. jury decisions for felony cases 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 make clear that verdicts are not simply a function of the objective quality of the evidence in the case; rather trial outcomes vary systematically and significantly with random fluctuations in the composition of juries (including juror race and age). Importantly, this kind of causal evidence is limited so far to this single setting, raising concern about the external validity of the existing findings; analogous studies simply do not exist for other jurisdictions within the U.S. or for alternative jury systems in other countries.<sup>3</sup>

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.

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<sup>1</sup> See Diamond and Rose (2005) for a review of studies using data from jury trials. More recently, Lehmann and Smith (2013) look at the impact of a variety of seated juror characteristics (race, gender, age, religiousness, education, and income) on trial outcomes and Lee (2014) shows that states that switched from key-man jury selection procedures to more random selection procedures, which were meant to increase black representation on juries, saw a resulting drop in the share of non-whites newly admitted to prison.

<sup>2</sup> In the absence of data from real trials, researchers have also conducted mock trials, but the resulting conclusions are limited by both the simplifications made for experimental expediency and the substantially lower stakes compared to real criminal trials. See Devine et. al. (2001) for a survey of this literature.

<sup>3</sup> There is a more substantial literature on judges, both in the US and internationally, as opposed to juries. 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, for instance, 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.

The first main goal of our analysis is to assess whether nämndemän exhibit systematic biases based on the characteristics of the defendant or case. To do so, we take advantage of the random assignment of nämndemän triplets (juries) to cases, examining how conviction rates vary with the assignment of different types of nämndemän. This research design naturally avoids the primary concerns with correlation studies in settings where the jury is seated through a pre-trial selection process and allows us to test whether nämndemän systematically reach different verdicts when faced with statistically identical evidence.

Second, our analysis provides the first causal evidence on the role that political affiliations play in jury decision-making. Given the general perception that views on criminal justice issues are related to political and ideological leanings, it is surprising that political party does not appear in reviews of more than 100 jury studies (Devine et al, 2001; Devine, 2012).<sup>4</sup> Whether an individual's political ideology affects jury decision making may be particularly relevant throughout Europe today, given the increasing presence of right and far-right political parties in the last twenty years. In fact, substantial electoral gains have been made by such parties in Austria, Denmark, Finland, France, Poland and Sweden. The fact that many of these parties have a nationalist, anti-immigration platform compounds the importance of this issue and brings up the question of whether non-native defendants (who are disproportionately represented as defendants in Sweden) will be treated equally.<sup>5</sup>

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, Moretti and Butler, 2004), mayoral behavior (Ferreira and Gyourko, 2009), and judicial decision-making.<sup>6</sup> Studying the impact of political

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<sup>4</sup> The only reference we have come across is Tuerkheimer's (2008) discussion of a mock trial conducted by a litigation consulting firm (Zagnoli McEvoy Foley). The findings of this study suggest that political leanings are related to a perspective juror's views on civil damages, but not his actions in the courtroom.

<sup>5</sup> [http://www.nytimes.com/interactive/2016/05/22/world/europe/europe-right-wing-austria-hungary.html?\\_r=0](http://www.nytimes.com/interactive/2016/05/22/world/europe/europe-right-wing-austria-hungary.html?_r=0)

<sup>6</sup> The judicial literature is arguably most closely related to the current study. It typically considers the effect of Democratic versus Republican judges on a wide range of case types, including EPA (Revesz, 1997; Miles and

ideology on the decision making of Swedish nämndemän distinguishes itself from the existing literature in three key ways. First, Swedish nämndemän or lay judges 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 with concerns about their judicial careers. Second, the ‘criminal’ literature on judges has focused almost exclusively on the severity of sentencing rather than the fundamental question of whether the law is applied impartially in judging guilt vs. innocence. Third, our analysis will examine biases in jury decisions related to specific elements of the platforms of a more diverse set of political parties (e.g., the anti-immigration platform of the far-right Swedish Democrats and the feminist platform of the far-left Vänster party).

The final component of our analysis uses the random assignment of nämndemän triplets to cases in a complementary way to explore the role of peers in jury decision making and, in so doing, begins to provide some empirical evidence on how jurors work together to reach decisions. 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. In this way, we observe the same individual serving with different sets of peers across a large number of different 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

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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 while Republican judges are more favorable in conservative cases (Spitzer and Talley, 2013). With respect to sentencing in criminal cases, Schanzenbach and Tiller (2008) and Fischman and Schanzenbach (2011) find evidence of harsher sentences from Republican appointed judges. Iaryczower and Shum’s (2012) study of the Supreme Court finds that while the justices’ initial leanings are changed in more than 40 percent of cases after incorporating ‘information’, such changes have become less likely as appointments to the court has become more politicized. Finally, Lim, Snyder, and Strömberg (forthcoming) study how media coverage differentially affects sentence length for partisan and non-partisan judges; they find that it leads to harsher sentences for the latter but has no effect on the former.

known about jury deliberations is based on mock trials.<sup>7</sup> 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. While this clearly suggests that group dynamics matter, we are unaware of any studies that directly estimate the impact of peers on jury decision-making.<sup>8</sup>

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. For each case, documents were obtained from the Court Archives that provide the names of the judge and nämndemän, defendant and case characteristics, the outcome, and any dissenting opinions (as verdicts do not have to be unanimous in the Swedish judicial system). These data were merged with a secondary data set of nämndemän characteristics, including political party, gender, birth year, and years of experience.

There are nine political parties represented in the data. The two predominant parties, as in Swedish politics in general, are the Social Democrats and Moderates; in fact, there is at least one Social Democrat and one Moderate on 74 and 69 percent of cases, respectively. Thus, variation in the composition of the nämndemän triplets comes primarily from those parties that are more extreme in the political spectrum and, as a result, our empirical specifications focus on two parties on the far left (Vänster (left) and Green) and one on the far right (Swedish Democrats), who have the most soft- and hard-on-crime attitudes, respectively.

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<sup>7</sup> MacCoun's (1989) brief review of this literature highlights two phenomena: (i) a majority effect, whereby a verdict can be predicted fairly reliably by a two-thirds 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.

<sup>8</sup> 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 (2013) 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 (Epstein et al, 2011; Posner, 2008), and the dynamics of deliberation (Sunstein et al, 2006). Blanes I Vidal and Leaver (2013) study social interactions in judge citation behavior in English appellate courts.

The first part of our analysis uses 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 multi-dimensional nature of political platforms.

The remainder of the empirical analysis focuses on understanding the role of peer effects in jury decision-making. Specifically, we analyze the impact of peers on each nämndeman's vote, taking into account dissenting opinions. We find strong evidence that nämndemän from both the far-left and far-right parties affect the votes of their peers. A bounding exercise suggests that a considerable portion of these peer effects may take the form of *dissent aversion*, where non-pivotal voters simply change their votes simply to ensure a unanimous decision. But even in the specifications that maximize the attribution of peer effects to dissent aversion, we identify many instances in which the presence of nämndemän from both the far-left and far-right parties truly sway their peers – i.e., draw the votes of their more centrist peers towards their own positions – as well as at least one instance in which jurors from the Vänster party instead appear to harden the opinions of peers with opposing views.

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 of the identifying assumption that nämndemän assignment to cases is random, and the main analysis of the

defendant by case level data. 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 U.S., where the jury pool is drawn from the general population, individuals must put themselves forward to become nämndemän.<sup>9</sup> 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 while 20 percent 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 percent of nämndemän were younger than 45 while 46 percent were older than 65.<sup>10</sup> 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.

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<sup>9</sup> 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.

<sup>10</sup> <http://www.borastingsratt.domstol.se/Om-Sveriges-Domstolar/Pressrum/Nyhetsarkiv/2010/Regeringens-mal-Fler-yngre-namndeman/>



A defendant's case (verdict, sentence, and oftentimes, damages) is decided at the main hearing (which is the Swedish equivalent of a trial).<sup>11</sup> 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 to 15 days per year; most nämndemän are otherwise employed or retired. In the courtroom, each nämndeman 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 U.S., 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 (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 percent of the nämndemän pool respectively. The

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<sup>11</sup> This main hearing occurs for most criminal defendants. In contrast to the U.S. 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.

other parties are: (i) Vänster (left) and Green, which are often in alliance with the Social Democrat party,<sup>12</sup> (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.<sup>13</sup>

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 ‘de-selection’ 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 directly test this 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 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

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<sup>12</sup> Though the green party is not officially part of the left or right block, many voters do consider them to be left.

<sup>13</sup> 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 Malmo, 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 Malmo, respectively. In terms of the main parties, Malmo and Gothenburg are historically social democrat strongholds while Stockholm is a moderate party stronghold.

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.<sup>14</sup> 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, etc. 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. While 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.<sup>15</sup>

### 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

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

<sup>15</sup> 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.

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 1336 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).

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 left and green parties are, on average, relatively soft on crime while 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 multi-faceted nature of both politics and criminal cases, it is perhaps too narrow-minded to only consider a party's attitude towards 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."<sup>16</sup> 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.<sup>17</sup> A quote from their party website 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 which, when they were originally built, were regarded as fine middle-class neighborhoods have turned into segregated areas where unemployment and crime is high."<sup>18</sup> These views are apparent in the SOM questions on whether

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<sup>16</sup> 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/>

<sup>17</sup> 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.

<sup>18</sup> 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." <https://sverigedemokraterna.se/var-politik/vara-viktigaste-fragor-2/invandring/>

immigrants are punished harsher than non-immigrants. 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 less likely to think that immigrants are punished more harshly (consistent with their anti-racism 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 1500) 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.<sup>19</sup> The resulting data set includes 956 cases and, due to multiple defendant cases, 1152 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 (Judgement).<sup>20</sup> For each case, we extracted the nämndemän and judge names as well as trial dates 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

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<sup>19</sup> 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.

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

such have Arabic first and last names). We do recognize that not all of these defendants are actually Arabic, but 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).<sup>21</sup>

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.<sup>22</sup> 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 (belastningsregistret) 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 a data set that we created of nämndemän and judge characteristics.<sup>23</sup> The primary variables identified for nämndemän included

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<sup>21</sup> 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 percent of Swedish names contain the letters å, ä, or ö while .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.

<sup>22</sup> 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

<sup>23</sup> 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.

municipality, gender (based on name), birth year, first year as a nämndeman, and political party. 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. With respect to judges, we have data on age and gender from the district court. There are 93 unique judges in our data, though just 37 judges are assigned to ten 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 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) substitute nämndemän due to unscheduled absences or an unexpectedly high caseload.

Table 2 shows that the majority of nämndemän in the triplet are male in 46 percent of the observations, the average nämndemän age in a triplet is 58, and the average age is older than 50 in 83 percent of cases. In addition, 17, 36, 24, and 23 percent of defendants face triplets with less than two, two to four, four to six, and more than six years of experience, respectively. Finally, 40 percent of triplets have at least one nämndeman with a non-Swedish name.

In terms of political affiliations, Table 2 shows that 74 and 69 percent of triplets have at least one Social Democrat and Moderate, respectively. With respect to the other parties, 27 and 20 percent, 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 percent have at least one Folk, Christian Democrat, and Center party member, respectively. A Swedish Democrat is in the triplet in nine percent of observations.

Turning to the defendants and cases, Table 2 shows that 91 percent of defendants are male, 12 percent are not Swedish citizens, 22 percent have an Arabic name, and the average defendant age is 32. 36 percent of defendants have at least one past conviction and 20 percent 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). 50 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 percent of observations, the defendant fully admits guilt; the conviction analyses exclude these cases, as there is no room for discretion in deciding the verdict. 44 percent of defendants fully deny guilt while 46 percent partially admit guilt, i.e. 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 percent of the observations include an offense that does not fall into one of these categories. Though the data were created using a 'crimes against person' selection criterion, other offenses appear in multi-charge 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 percent of charges result in conviction, and that 88 percent 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 percent of charges result in conviction. A closer look at the distribution of the share of charges convicted for this sample indicates that 74 percent are convicted on all charges,

13 percent are completely acquitted, six percent 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 five percent fully acquitted versus 66 percent convicted of all charges; 14 percent of multiple charge defendants are convicted of half the charges, with another five percent convicted of two-thirds of the charges.<sup>24</sup> Though not studied in the paper, we see that 30 percent of all convicted defendants are sentenced to prison; 35 percent 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 three percent from a professional judge and five percent from at least one nämndeman). Five percent of observations have a dissenting opinion in favor of a harsher outcome (guilt or a harsher sentence), while three percent favor a more lenient outcome. The dissenting opinion data will be used when we create the individual nämndemän level data set described in Section 5. Finally, the average age of judges is 50 years old and 55 percent are male. We include judge fixed effects in almost all specifications to account for these and other 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 above to examine the relationship between nämndemän triplet political affiliations and case outcomes.

To this end, the basic empirical specification is as follows:

$$(1) Y_{ij} = \alpha + \beta_1 Vänster_j + \beta_2 Green_j + \beta_3 SwedishDem_j + X_j \gamma + Z_{ij} \pi + \lambda_{judge} + \varepsilon_{ij},$$

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<sup>24</sup> The share of charges resulting in conviction is substantially smaller (75 percent) when conditioning on the almost 500 defendants who fully deny guilt, i.e. when there is presumably more room for discretion.

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.<sup>25</sup> 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 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  $\lambda_{judge}$  represents judge fixed effects.

For the  $\beta$ '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

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<sup>25</sup> 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.

3 assess whether there is evidence of non-random 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), eleven offense characteristics (number of charges and ten 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 five percent level – 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 ten 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 ten percent level; if there were to be non-random 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. 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 five percent 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 below, 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 who else is assigned to sit on the triplet, then one may be potentially unable to disentangle whether  $\beta_1$  captures the vänster effect or that of his 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 will compare the actual triplets to simulated triplets created by random assignment to assess the extent to which ‘balancing’ occurs. Second, we will 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).<sup>26</sup> 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 where the defendant did not fully admit guilt (columns (1) and (3)) and by more than 13 percentage points when looking at cases where the defendant completely denied guilt and there is arguably more discretion (columns (2) and (4)). This is clearly surprising given that Vänsters are generally associated with being soft on crime. One possible explanation is that the presence of the Vänster nämndeman causes other members of the triplet to dig into or become firmer in their beliefs. Alternatively, a subset of cases, for which the results are not so counterintuitive, may be driving this finding.

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 multi-dimensional nature of party platforms, however, this is a very strong assumption. Is the surprisingly hard on crime Vänster relationship driven by a subset of cases where such an attitude makes sense? 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 columns (2) and (3), and whether the defendant is younger than 21 in column (4).<sup>27</sup> 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,

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<sup>26</sup> 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.

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

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.<sup>28</sup> For readability, we only 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; this is significant at the 1 percent level.<sup>29</sup> On the other hand, when the victim is male, having a Vänster in the triplet does not have a significant impact.<sup>30</sup>

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. 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 7 of Appendix Table 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.<sup>31</sup>

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<sup>28</sup> 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 heterogeneity by defendant gender. But, in practice, there are simply not enough rape cases or female defendants for such analyses.

<sup>29</sup> Here we are describing the results for the total effect of adding a Vänster when the victim is female – i.e., summing the coefficient on *any\_vänster* and *any\_vänster\*any\_femalevic* and characterizing whether that coefficient significantly differs from zero. We follow this approach for reporting results throughout this section.

<sup>30</sup> Appendix Table 1 shows that these results are not driven by cases with female-defendants and female victims.

<sup>31</sup> This is consistent with Carlsson and Rooth's (2007) correspondence study 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.

Finally, Column (4) 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. While 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. Taken together, the results in Columns (1)-(4) suggest that it is important to take into account the multi-dimensional 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.

#### 4.3. *Interpretation*

The size of the effects in Tables 4 and 5 reveals 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.

While 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 – e.g., 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 implies that either (i) nämndemän associated with particular political parties exhibit substantial biases for or against particular types of defendants or (ii) 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 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.<sup>32</sup> 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.<sup>33</sup>

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 while columns (7) – (9) interact these party dummies with each case characteristics 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 nämndemän.

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

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<sup>32</sup> Note that because this specification includes all three interactions simultaneously, the coefficients on *any\_vänster* and *any\_swededem* 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.

<sup>33</sup> 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 1000 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).

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 1000 times and present the sample means in column (1) of Appendix Table 2.<sup>34</sup> We present the corresponding sample means for the actual triplets observed in the data in column (2). We find strong evidence consistent with the nämndemän coordinators' statements that they balance on gender. With random assignment, 10 and 15 percent, respectively, of triplets would be all male and all female; but, in the real data, just one percent 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 percent of all triplets would contain no 'soft' and all 'soft' nämndemän, respectively, compared to 6 and 8 percent in the real data.<sup>35</sup> With respect to age, the simulated triplets are in fact quite comparable to the observed triplets.

The lower panels of Appendix Table 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

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<sup>34</sup> Note that the overall mean presented in Appendix Table 2 is the weighted average of the pre and post election means, where the weights are 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 there being more triplets observed in the real data in that period as well.

<sup>35</sup> 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.

that our baseline specification controls for the presence of *both* Vänsters and Swedish democrats. Moreover, Appendix Table 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 (presented in the (b) columns of Appendix Table 1) are quite comparable to our baseline estimates, though there is not surprisingly some loss of precision. Despite this loss of precision, only the effect of Vänster nämndemän on young defendants falls below conventional thresholds for statistical significance in these specifications.

#### 4.5. *Triplet Majority Composition*

The main analysis presented thus far considers only the presence of at least one Vänster or Swedish Democrat in the triplet and does not take into account the composition of the remainder of the triplet. Because some parties are closer to others in terms of political platforms, one might expect the impact on conviction from adding nämndemän from these extreme parties to depend on the remaining composition of the jury. We began to get at this in the previous section when looking at triplets with one social democrat and one member of the moderate block. 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, there is not such a majority. Table 7 examines this more broadly, without restricting the analysis to such a limited 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; odd numbered columns present the baseline specification from Table 5, while the even numbered columns present the expanded specification. To ease the presentation of the results, we indicate the case characteristic considered in each column at the top of the column (female victim, Arabic named defendants, and young defendants). Column (1) shows that having a Vänster in the triplet increases the share of charges convicted by almost 14 percentage points when there is a female victim. Column (2) further interacts the three party-female victim interactions with a dummy for majority ‘left’ (such that there are six interaction terms included in the regression). It indicates that the Vänster-female victim effect is driven by cases in which the majority of the triplet is from the left. Interestingly, we also see 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’.<sup>36</sup> Columns (3) and (4) present the results for the Arabic named defendant specifications. Importantly, having a Swedish democrat in the triplet increases (substantially) the share of charges convicted for an Arabic named defendant, regardless of whether the majority composition is from the left. Likewise, columns (5) and (6) indicate that having a Swedish Democrat in the triplet increases the share of charges convicted for young defendants, regardless of the majority composition of the triplet.

##### *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 towards their viewpoint. We term this the

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<sup>36</sup> Note that this effect is not driven by Vänsters and Swedish Democrats sitting together.

*direct effect*. However, it is also possible that in addition to having a direct effect, these nämndemän also influence the votes of their peers.

A variety of different types of peer effects have been described in the literature on decision-making in small groups and it is helpful to highlight several important potential mechanisms that may operate in the context of jury deliberations:

- i. Sway Effects – the movement of one’s opinion towards the views of a peer due to persuasion or the sharing of relevant information.
- ii. Digging In – the hardening of one’s own initial position in the presence of a particular kind of peer.
- iii. Conformity – the basic desire to reach the same judgment as one’s peers.
- iv. Dissent Aversion – the willingness to switch one’s vote in order to avoid a formal dissent (or a preference for unanimity).

While the literature delineates a number of additional mechanisms and sometimes more finely distinguishes variations of the effects mentioned here, these four categories are helpful in presenting our analysis since they cover the broad kinds of effects that our study might uncover.

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. In Section 5.2, we take up the question of 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 that we uncover affect decision-making in a way that actually impacts trial outcomes.

### *5.1. Measuring 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.<sup>37</sup> 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 two percent 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).<sup>38</sup>

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

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

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 above,  $Z$  is a vector of case and defendant characteristics and  $\lambda_{judge}$  represents judge fixed effects. Finally,  $\alpha_n$  represents nämndeman fixed effects. All standard errors in the individual analysis are clustered at the case level.<sup>39</sup>

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

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<sup>37</sup> 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.

<sup>38</sup> 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.

<sup>39</sup> The main peer effects results are insensitive to clustering the standard errors on the individual nämndeman.

with a Vänster or Swedish Democrat relative to when they sit with more centrist peers. To interpret  $\beta$  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 below 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 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 non-female 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. Surprisingly, we see the same pattern of results for nämndemän sitting with a Vänster peer on Arabic named defendant cases. Since such an effect is generally inconsistent with the Vänster party's political viewpoints, it appears that having an extreme left Vänster peer causes members of other parties to dig in or take a firmer stand; as seen in Table 1, the Vänster opinion on immigrants differs from that of most other parties. 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 be strongest amongst the most inexperienced nämndemän (Anwar, Bayer and Hjalmarsson, 2015).<sup>40</sup>

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<sup>40</sup> 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

Finally, we show explicit evidence that Vänsters and Swedish Democrats do themselves exert a direct effect on the trial outcome in Appendix Table 3.<sup>41</sup> 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.<sup>42</sup> 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 Mechanisms and Deliberation Dynamics*

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 further distinguish among potential explanations for these peer effects and, more importantly, determine whether these peer influences actually affect trial outcomes.

An important distinction between the peer effects mechanisms described above relates to whether nämndemän are actively affecting the underlying decisions of their peers (as in the Sway, Digging In, or Conformity mechanisms) or whether individuals are simply changing non-pivotal votes in order to make the final decision unanimous (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.<sup>43</sup> Distinguishing

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extraverted jurors are more influential (Clark et al, 2007; Marcus et al, 2000). These 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.

<sup>41</sup> 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.

<sup>42</sup> 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.

<sup>43</sup> The idea of dissent aversion was put forth in studies of judge panel effects in federal courts of appeals (Epstein et al., 2011; Posner, 2008) 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).

between these mechanisms is important for determining whether peer effects actually impact trial outcomes or are purely incidental.

In the nomenclature of Manski (1993), dissent aversion is a type of endogenous peer effect, whereby *nämndemän* are affected by the actual votes of their peers rather than their underlying characteristics. Such endogenous peer effects can be added to the specification in equation (2) by including the actual votes of *nämndeman n*'s peers on the right hand side:

$$(3) \quad Y_{nij} = \gamma PeerChar_{nj} + \delta N_{(-n)j} + Z_{ij}\pi + \lambda_{judge} + \alpha_n + \varepsilon_{nij},$$

where  $N_{(-n)j}$  is a vector of indicator variables that measure the number of votes to convict (0, 1, or 2) by *nämndeman n*'s peers. While it is straightforward to include these effects in the model, identification of causal endogenous peer effects is a notoriously difficult problem. In fact, in most empirical settings, researchers typically settle for estimating equation (2), which is the reduced-form of equation (3), recognizing that the resulting estimated peer effects capture some combination of the exogenous and endogenous peer effects shown in equation (3).

The core difficulty in estimating endogenous peer effects relates to the simultaneity of decisions, the reflection problem. While this problem is completely intractable in a linear-in-means specification when the dependent variable is continuous, Brock and Durlauf (2007) and Soetevent and Kooreman (2007) have shown that identification of equation (3) is possible with random assignment of peers when the dependent variable is binary. Unfortunately, the identification results provided by these papers require a strong assumption that is unlikely to hold in our setting (and, frankly, most others) – that there is no group-level unobservable. In our setting, the strength of evidence in the case and the persuasiveness of the trial attorneys would represent the kind of group-level unobservable that prevents identification.

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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.

Given the random assignment of jurors to cases, estimating equation (3) in the presence of a group-level unobservable has the effect of biasing the magnitude of the estimated endogenous peer effects upwards – i.e., any correlation in voting behavior due to unobserved case characteristics is wrongly attributed to the direct effect of peers' votes. The overstatement of magnitude of the endogenous peer effect leads in turn to an attenuation of the exogenous peer effects as in the Manski linear-in-means model. It is important to note that the nature of these biases is predictable because of the random assignment of jurors to cases – which ensures that the exogenous juror attributes are uncorrelated with the group-level unobservable.

Thus, despite not being able to obtain unbiased estimates of the coefficients in equation (3) in the presence of group unobservables, the results of Brock and Durlauf (2007) and Soetevent and Kooreman (2007) suggest that this specification can be used instead to provide an upper bound on the size of the endogenous peer effects and a lower bound on the size of the exogenous peer effects. With this understanding of the nature of the bias due to the presence of a group-level unobservable in mind, Table 9 provides estimates of the coefficients for a specification of the model that adds endogenous peer effects to the reduced-form specification shown in Table 8.

The resulting estimates suggest a large role for endogenous peer effects, which is not surprising given the fact that unanimous decisions are, in fact, returned in the vast majority of cases and that the magnitude of this coefficient is biased upwards in the presence of group unobservables. It is also important to point out that while we were motivated to include endogenous peer effects as a way to bound the potential importance of formal dissent aversion, some forms of endogenous peer effects – in particular, conformity – would be consistent with *nämndemän* exerting a real impact on the underlying decisions of their peers.

What is most important about the results presented in Table 9 is whether we obtain a meaningful lower bound for the size of the exogenous peer effects. As expected, the inclusion

of endogenous peer effects substantially erodes the magnitude and statistical significance of all of the important peer effects estimated in Table 8, but statistically significant peer effects remain for Vänsters for cases involving female victims and Arabic named defendants. The parameters imply, for example, that in cases involving female victims, at least 16 percent of the reduced-form effect of Vänster peers estimated in Table 8 reflects the power of Vänsters to sway the opinions of their peers in a meaningful way, beyond any impetus to reach a unanimous decision. More generally, the magnitudes of the estimates shown in Table 9 imply that a minimum of 15-30 percent of the important reduced-form peer effects estimated in Table 8 reflect the influence of peers from the far-right and far-left parties beyond any perfunctory effect related to the desire to reach unanimous decisions. Importantly, these results survive despite the fact that the endogenous peer effects are likely substantially overstated in the specifications reported in Table 3 as a result of any correlation in voting behavior that would naturally be expected to arise due to variation in the quality of evidence across cases.

## *6. Conclusion*

That a defendant should receive a fair trial in which the verdict is decided only on the basis of the evidence is a fundamental principle of most criminal justice systems. But the challenge of designing and implementing a system that is capable of achieving this abstract goal is immediately obvious as soon as one confronts the practical questions of how jurors should be assigned, especially in settings where members of the local population may have biases for or against certain kinds of defendants.

Faced with this challenge, many justice systems use a selection process that attempts to form juries that are somehow representative of the local population or public opinion. In the United States, a ‘representative’ jury is typically determined by a random draw from a list of eligible jurors from the local community. But recent empirical studies, as well as a long history

of anecdotal evidence, have raised serious concerns about biases in this system, particularly when minority members of the local population (where minority could be defined in any number of dimensions including race, ethnicity, and religion) face juries that are representative and, therefore, include few, if any, members of the minority group.

Sweden's *nämndemän* system provides an alternative approach to selecting jurors that are representative of the local population. In the *nämndemän* system, the share of lay judges from each political party is proportionate to how the local population voted in the last election, thereby achieving representativeness in the sense of the locality's political ideologies.<sup>44</sup> Given the lack of existing empirical evidence on jury verdicts outside of the United States, our paper provides an opportunity to assess whether the Swedish system is capable of delivering a 'fair' trial – does the presence (and vote) of the professional judge and the fact that the lay judges are politically appointed to office rather than randomly drawn from the population lead to impartiality?

Our findings imply that significant biases exist in the Swedish 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 assigned to a case 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 similar in platform to the *Swedish Democrats*.

Importantly, the political affiliations of the *nämndemän* assigned to a case affected the actual verdicts in the case and were not just reflected in dissenting opinions – e.g., the random inclusion of a *Swedish Democrat* increases the conviction rate of Arabic named defendants by

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<sup>44</sup> There are, of course, other key differences between the U.S. and Swedish systems. American verdicts are unanimous while a majority is required in Sweden and verdicts are decided by American juries without the presence of the professional judge, while Swedish verdicts are decided by a professional judge in collaboration with three lay judges.

26 percentage points. 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.

Alternative appointment processes that aim to de-emphasize the role of politics have also been suggested. The most recent government proposal 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.<sup>45</sup> 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.

The selection of jurors is, of course, only one aspect of the design of jury systems and other dimensions (including the size of the jury, the voting rules, and the requirements for conviction) impact the collective decisions reached by groups of jurors. In fact, we know very little about how jurors decide verdicts collectively in real trial settings. By exploiting variation in the assignment of jurors not only to cases but to one another, our analysis also contributes to the nascent literature on the dynamics of jury deliberations. Our results provide the first empirical evidence in the literature based on actual trials on the ways that jurors influence one another during group decision-making. The nature of the estimated peer effects imply that, in some cases, groups of jurors do more than simply aggregate (or average) opinions and that jury deliberation and decision-making processes can indeed have systematic effects on verdicts.

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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)
_cons	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-sq	0.173	0.128	0.143	0.048	0.047	0.028	0.042	0.056

Standard errors in parentheses. \* is significant at 10%, \*\* at 5% and \*\*\* at 1%. 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.

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 nonswedish 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	1144	0.05	0.22
any_rape (child/adult)	1144	0.08	0.28
any_assault	1144	0.56	0.50
any_aggravated assault	1144	0.13	0.34
any_unlawful threat	1144	0.26	0.44
any manslaughter/murder	1144	0.03	0.17
any drunk drive	1144	0.02	0.14
any_drugs	1144	0.11	0.31
any_other_sex_off	1144	0.07	0.26
any_other	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
dissenting opinion up?	1152	0.05	0.21
dissenting opinion down?	1152	0.03	0.18
<i>Judge Variables</i>			
judge_age	1107	49.99	9.67
judge_male	1152	0.55	0.50

Table 3. Random Assignment of Nämndemän Triplets and Judges

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	any_ vänster	any_ green	any_ krist dem	any_ folk	any_ swede dem	any_ vägvalet	maj_ nämnr_ male	any_ non swedish	triplet_ average age	triplet_ average exp	judge_ male	judge_ age
<i>F-test (p-value) of joint significance of:</i>												
defendant characteristics (4 variables)	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)
offense characteristics (11 variables)	0.81 (0.63)	1.46 (0.14)	<b>3.36</b> <b>(0.00)</b>	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)	<b>1.85</b> <b>(0.04)</b>	0.50 (0.90)
case characteristics (5 variables)	0.95 (0.45)	0.81 (0.54)	0.77 (0.57)	0.35 (0.88)	<b>3.31</b> <b>(0.01)</b>	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)
criminal history characteristics (2 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)	<b>3.77</b> <b>(0.02)</b>	1.19 (0.30)
nämndemän political affiliations (7 variables)											0.67 (0.70)	0.79 (0.60)
other nämndemän characteristics (5 variables)											0.89 (0.49)	1.75 (0.12)
# coef significant at 5% level	0	2	1	1	1	2	0	0	1	4	4	0
N	1051	1051	1051	1051	1051	1051	1074	1069	1074	1073	1051	1011
R-sq	0.025	0.022	0.021	0.016	0.028	0.034	0.023	0.023	0.030	0.037	0.072	0.043

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 above table by presenting the F-tests of the joint significance of the variables included in these categories. 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.

Table 4. Baseline Specification for All Cases

	(1) Share of Current Offenses Convicted	(2)	(3)	(4)
			Any current convictions?	
any_vänster	0.0584 (0.0381)	0.146* (0.0753)	0.0572 (0.0373)	0.139* (0.0776)
any_green	0.0255 (0.0322)	0.0811 (0.0607)	-0.00227 (0.0268)	0.0469 (0.0587)
any_swededem	-0.0377 (0.0525)	-0.00466 (0.0968)	-0.00865 (0.0497)	-0.00361 (0.0959)
_cons	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-sq	0.212	0.261	0.230	0.253

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, while 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<0.05, \*\*\*p<0.01.

Table 5. Baseline Party Platform\*Case Characteristic Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Share of Current Offenses Convicted			Any Current Convictions?		
any_vänster	-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_swededem	-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*any_femalevic	0.148*** (0.0558)			0.143*** (0.0527)		
any_swedem*any_femalevic	0.0637 (0.0958)			-0.0163 (0.0783)		
any_vänster*def_arabic name		-0.0115 (0.0661)			-0.0276 (0.0597)	
any_swedem*def_arabic name		0.260** (0.100)			0.255*** (0.0851)	
any_vänster*def<21			-0.153** (0.0622)			-0.105* (0.0606)
any_swedem*def<21			0.239*** (0.0764)			0.169** (0.0810)
def_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_vic	-0.0218 (0.0390)	0.00252 (0.0320)	0.00295 (0.0310)	-0.00903 (0.0398)	0.00421 (0.0325)	0.00466 (0.0314)
def<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)
_cons	0.945*** (0.0886)	0.928*** (0.0863)	0.905*** (0.0850)	0.836*** (0.0835)	0.828*** (0.0852)	0.809*** (0.0848)
N	961	961	961	962	962	962
R-sq	0.219	0.217	0.225	0.238	0.237	0.240

Robust standard errors, clustered on judge name, are in parentheses.\* p<0.10, \*\* p<0.05, \*\*\*p<0.01. 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, while in columns (4)-(6), it is whether there is at least one conviction.

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	0.00671	0.0387	0.0414	0.0352	0.0299	0.0276	-0.00605	0.0420	0.0314
any_swedish democrat	-0.155*	-0.171*	-0.172*	-0.219**	-0.218**	-0.212**	-0.222**	-0.205**	-0.207**
any_vänster*female victim	0.120**	0.130**	0.131**	0.127**	0.135**	0.135**	0.206**	0.134**	0.133**
any_swedish democrat * female victim	0.0838	0.0869	0.0894	0.147	0.154	0.154	0.173	0.155	0.150
any_vänster *arabic named defendant	-0.0217	-0.00188	-0.00520	0.000409	-0.0227	-0.0261	-0.0294	-0.0863	-0.0376
any_swedish democrat * arabic named defendant	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 democrat *defendant < 21	0.281***	0.295***	0.281***	0.296***	0.319***	0.320***	0.319***	0.316***	0.329***
arabic named defendant	-0.00974	-0.0274	-0.0294	-0.0236	-0.0127	-0.0102	-0.00689	0.170	-0.0120
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 Nämn. 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 * Female Vic	no	no	no	no	no	no	yes	no	no
Other Party Dummies * Arabic Def	no	no	no	no	no	no	no	yes	no
Other Party Dummies * Young Def	no	no	no	no	no	no	no	no	yes

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, where\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\* $p < 0.01$ . Finally, all specifications condition on the sample who did not fully admit guilt.



Table 7. The Effect of Triplet Majority

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Share of Current Charges Convicted					
	Case Characteristics:					
	Female Victim		Arabic Named Defendant		Young (<21) Defendant	
any_vänster	-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_swededem	-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_swededem*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_swededem*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)
N	961	961	961	961	961	961
R-sq	0.219	0.224	0.217	0.222	0.225	0.228

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, miljö and social democratic parties. The case characteristic included in the interaction varies across columns and is denoted at the top of each column.

Table 8. Reduced Form Peer Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	female victim cases	no female victim cases	arabic named defendant	nonarabic named defendant	defendant <21 cases	defendant > 21 cases
<i>Panel A. Dependent Variable: Any Current Offenses Convicted by Individual Nämndeman</i>						
any_peers_vänster	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_peers_swededem	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_peers_vänster	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_peers_swededem	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 FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1474	1378	655	2197	801	2051

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.

Table 9. Endogenous versus Exogenous Peer Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	female victim cases	no female victim cases	arabic named defendant	nonarabic named defendant	defendant <21 cases	defendant > 21 cases
<i>Dependent Variable: Any Charges Convicted by Individual Nämndeman?</i>						
<u>Exogenous Peer Effects</u>						
any_peers_vän	0.0200* (0.0103)	0.00379 (0.0126)	0.0963** (0.0385)	0.00360 (0.00740)	-0.00698 (0.0225)	0.0111 (0.00891)
any_peers_sweddem	0.0121 (0.0179)	0.00393 (0.0225)	0.0742 (0.0540)	0.0144 (0.0120)	0.00119 (0.0289)	0.0243* (0.0130)
<u>Endogenous Peer Effects</u>						
zero_peers_convict	-0.589*** (0.168)	-0.767*** (0.190)	-0.572 (0.376)	-0.668*** (0.137)	-0.541 (0.355)	-0.664*** (0.134)
two_peers_convict	0.358** (0.163)	0.186 (0.178)	0.300 (0.340)	0.295** (0.131)	0.407 (0.357)	0.295** (0.126)
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 FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1474	1378	655	2197	801	2051

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.

Appendix Table 1. Sensitivity Checks of Main Triplet Level Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Share	Share	Share	Share	Any	Share	Share	Share	Share	Any	Share	Share	Share	Any
	baseline	no females	(a)	(b)	(b)	baseline	break out 'other' ethnic category	(a)	(b)	(b)	baseline	(a)	(b)	(b)
Any vänster?	-0.013	-0.012	-0.031	-0.0089	0.017	0.060	0.039	0.057	0.097	0.11*	0.11**	0.10**	0.16**	0.12*
Any Swedish Dem?	-0.070	-0.072	-0.11	-0.11	-0.033	-0.087	-0.11	-0.094	-0.10	-0.086	-0.079	-0.092	-0.13	-0.087
Any vänster * any female vic	0.15***	0.15**	0.16***	0.23**	0.17*									
Any Swedish Dem * any female vic	0.064	0.052	0.094	0.068	-0.024									
Any vänster * Arabic named def						-0.012	0.012	-0.032	0.077	-0.012				
Any Swed Dem * Arabic named def						0.26**	0.28**	0.20*	0.14	0.20*				
Any vänster * NonArabic/NonSw def							0.071							
Any Swed Dem * NonArabic/NonSw def							0.050							
Any vänster * defendant < 21											-0.15**	-0.15**	-0.15	-0.047
Any Swedish Dem * defendant < 21											0.24***	0.22**	0.24**	0.19*
Arabic Defendant name	-0.0048	0.0077	-0.0064	0.0069	0.0043	-0.024	-0.056	-0.015	-0.0032	0.0097	-0.0015	-0.0065	0.0056	0.0080
Any female victim	-0.022	-0.021	-0.026	-0.0077	0.0042	0.0025	0.0063	0.00071	0.030	0.032	0.0030	0.000057	0.029	0.028
Defendant < 21	-0.027	-0.039	-0.030	0.054	0.055	-0.024	-0.023	-0.026	0.054	0.054	-0.030	-0.030	0.044	0.032
NonArabic/Non Swedish Def Name							-0.087**							
cons	0.95***	0.97***	0.97***	0.73***	0.59***	0.93***	0.97***	0.94***	0.69***	0.57***	0.91***	0.92***	0.67***	0.54***
N	961	875	945	629	629	961	961	945	629	629	961	945	629	629
R-sq	0.219	0.220	0.222	0.318	0.341	0.217	0.227	0.217	0.314	0.342	0.225	0.227	0.321	0.343

(a) excludes from the sample those cases with both vänster and swedish democrat sitting together

(b) restricts the sample 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.

Restriction (b), however, keeps just 47 of 94 swedish democrat cases and 65 of 203 vänster cases.

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 name, are used to determine significance, where\* p<0.10, \*\* p<0.05, \*\*\*p<0.01. Finally, all specifications condition on the sample who did not fully admit guilt.

Appendix Table 2. Comparison of Actual Triplet Composition to Simulated Random Triplets

	<i>Average Characteristics of Randomly Assigned Simulated Triplets</i>	<i>2009-2012 Mean Characteristics of Actual Triplets Observed in the Data (793 triplets, 115 vänster trips, 87 Swedish Dem trips)</i>
<i>All Triplets</i>		
Average Birth Year	1955.13	1952.59
Share All Male	0.10	0.01
Share All Female	0.15	0.01
Share No 'soft'	0.13	0.06
Share All 'soft'	0.12	0.08
any_vänster	0.20	0.20
any_green	0.25	0.27
any_socdem	0.71	0.76
any_moderate	0.60	0.66
any_folk	0.29	0.29
any_center	0.04	0.03
any_krist	0.15	0.12
any_swededem	0.07	0.11
any_vägvalet	0.08	0.09
<i>Triplets with at Least One Vänster</i>		
Average # vänster	1.07	1.06
Average # socdem	0.71	0.62
Average # swededem	0.05	0.11
Average # folk	0.23	0.26
Average # center	0.03	0.03
Average # krist	0.11	0.09
Average # moderate	0.55	0.58
Average # green	0.18	0.19
Average # vägvalet	0.05	0.07
<i>Triplets with at Least One Swedish Democrat</i>		
Average # vänster	0.14	0.20
Average # socdem	0.67	0.72
Average # swededem	1.03	1.00
Average # folk	0.20	0.15
Average # center	0.03	0.01
Average # krist	0.09	0.00
Average # moderate	0.55	0.67
Average # green	0.19	0.21
Average # vägvalet	0.08	0.05

The simulated averages are based on 1000 simulations for the 2011 and 2012 hearing years (i.e. post election and the new roster) and 2009 and 2010 hearing years (pre-election). The pre- and post- election means 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.

Appendix Table 3. 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					
	female victim cases	no female victim cases	arabic defendant cases	nonarabic defendant cases	defendant < 21 cases	defendant >21 cases
own_vänster	0.0967** (0.0409)	0.0274 (0.0518)	0.0805 (0.0784)	0.0452 (0.0385)	0.0475 (0.0742)	0.0618* (0.0365)
own_green	0.0271 (0.0413)	0.0210 (0.0508)	-0.132 (0.0807)	0.00161 (0.0368)	0.191** (0.0860)	-0.0245 (0.0369)
own_swededem	0.00766 (0.0746)	-0.140 (0.0937)	0.0948 (0.168)	-0.0831 (0.0583)	0.183 (0.140)	-0.0958 (0.0641)
own_moderate	-0.0162 (0.0300)	-0.0270 (0.0395)	-0.0990 (0.0719)	-0.0211 (0.0251)	0.164** (0.0822)	-0.0203 (0.0257)
own_fkrc (folk, christ, center)	-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 Nämndemän FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1469	1373	657	2185	795	2047
R-sq	0.665	0.677	0.855	0.535	0.721	0.532

Dependent Variable = Share of Current Charges that Individual Nämndemän votes to convict on. The sample includes those individuals who did not fully admit guilt. Each column further restricts the sample to a sub-sample 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 also included as controls. Robust standard errors, clustered on case, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\*p<0.01.