Causal Inference with Ranking Data: Application to Blame Attribution in Police Violence and Ballot Order Effects in Ranked-Choice Voting

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Abstract

While rankings are at the heart of social science research, little is known about how to analyze ranking data in experimental studies. This paper introduces a potential-outcomes framework to perform causal inference when outcome data are ranking data. It introduces a class of causal estimands tailored to ranked outcomes and develops methods for estimation and inference. Furthermore, it extends the framework to partially ranked data. I show that partial rankings can be considered a selection problem and propose nonparametric sharp bounds for the treatment effects. Using the methods, I reanalyze the recent study on blame attribution in the Stephon Clark shooting, finding that people's attitudes toward officer-involved shootings are robust to contextual information. I also apply the framework to study ballot order effects in three ranked-choice voting (RCV) elections in 2022, proposing a new theory of pattern rankings in RCV. Finally, I present three applications in international relations.

Keywords: Ranking data, causal inference, police violence, ranked-choice voting, ballot order effects

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Introduction

Rankings are at the heart of social sciences (Luce, 1959; Arrow, 2012; Regenwetter et al., 2006; Train, 2009). Political scientists, for example, have used the concept of rankings and ranking data (e.g., 15243) to study diverse topics across subfields, including American politics (e.g., Kaufman, King and Komisarchik, 2021), comparative politics (e.g., Baldwin and Huber, 2010), and international relations (e.g., Kelley and Simmons, 2015) (see Online Appendix A for forty applications). Moreover, a growing body of experimental works has analyzed ranking data to study an array of political phenomena, such as voting and social sanctions (Gerber et al., 2016), political values (Reeskens et al., 2021), racism and media framing (Nelson, Clawson and Oxley, 1997), police violence (Boudreau, MacKenzie and Simmons, 2019), gender and candidate selection (Jankowski and Rehmert, 2022), indigenous recognition (McMurry, 2022), blame attribution (Malhotra and Kuo, 2008), distributive justice (Frohlich, Oppenheimer and Eavey, 1987; Frohlich and Oppenheimer, 1990), ballot design (Horiuchi and Lange, 2019), economic coercion (Gueorguiev, McDowell and Steinberg, 2020), foreign aid (Dietrich, 2016), and sexual violence and military intervention (Agerberg and Kreft, 2022). In these works, researchers study the treatment effects on ranking data as multidimensional outcome data (Marden, 1995; Alvo and Philip, 2014; Yu, Gu and Xu, 2019). However, despite the promising development, it has not been clear what kinds of treatment effects can be studied from ranking outcome data in political science, let alone how to estimate and perform inference on them.

This paper introduces a potential-outcomes framework of causal inference with ranking outcome data. Moreover, it uses the framework to study two topics in American and comparative politics: (1) people’s attitudes toward police violence (Davis, 2017; Boudreau, MacKenzie and Simmons, 2019; McGowen and Wylie, 2020) and (2) ballot order effects in ranked-choice voting (RCV) (Orr, 2002; King and Leigh, 2009; Curtice and Marsh, 2014). Furthermore, Online Appendix M presents three applications in international relations by examining conflict-related sexual violence (Agerberg and Kreft, 2022), foreign aid (Dietrich, 2016), and U.S.-China currency relations (Gueorguiev, McDowell and Steinberg, 2020).

This work makes several contributions. First, I define a class of causal estimands tailored to ranking outcome data based on the potential-outcomes framework (Imbens and Rubin, 2015). I also propose nonparametric estimators for point identification and discuss inferential methods for two causal estimands: average rank effects and average pairwise rank effects. Furthermore, I also extend the framework to partially ranked data, where rankers (e.g., voters) do not rank all available items (e.g., candidates) (Critchlow, 2012) and thus data are
partially missing. Building on the framework of principal stratification (Frangakis and Rubin, 2002), I show that partial rankings can be considered a selection problem (Knox, Lowe and Mummolo, 2020). To address the missing data problem in partially ranked data, I propose nonparametric sharp bounds on the treatment effects based solely on the logical constrain of ranking data and expected directions of treatment effects (Horowitz and Manski, 2000; Manski, 2009). This paper also discusses a range of causal estimands for ranking data that can be useful in political science. The concluding section offers practical guidance for future research about how to choose appropriate estimands and study more complex hypotheses.

Next, I apply the proposed framework to the research on police violence and RCV. In the first study, I reanalyze the experimental data collected by Boudreau, MacKenzie and Simmons (2019). This study examines the effects of contextual information on people’s responses to an officer-involved shooting in Sacramento, California. In their experiment, survey respondents were asked to rank order seven relevant parties, including an unarmed African American victim and two white police officers who shot the victim, according to their perceived levels of responsibility for the victim’s death. Unlike the original study using parametric models, I find that people’s responses to police violence are remarkably robust to receiving contextual information. Moreover, respondents of different races show widely different opinions even when they receive the same information (echoing Jefferson, Neuner and Pasek, 2021), reaffirming the importance of jury selection in police violence cases (Cook, 2017; Futrell, 2018; Davis, 1994).

In the second study, I develop an experimental design to examine how ballot order (i.e., candidate order) affects candidate ranks under RCV, where partial rankings frequently appear (Burnett and Kogan, 2015; Kigour, Grégoire and Foley, 2020). I perform three survey experiments in the Oakland mayoral election, the U.S. House election in Alaska, and the U.S. Senate election in Alaska in 2022. The experiments reveal a new puzzle — while ballot order still matters, it affects how people vote under RCV in a way that cannot be fully explained by previous research (e.g., Alvarez, Sinclair and Hasen, 2006). To explain the puzzle, I propose a new theory of pattern rankings and offer evidence for potential voters who vote by following geometric patterns instead of offering their sincere ranked preferences.

Finally, this paper seeks to unify the literature on ranking data and causal inference. On the one hand, it offers opportunities for scholars to use ranking data with the Neyman-Rubin causal model (Neyman, 1923; Imbens and Rubin, 2015). While the ranking data literature has studied the analysis of randomized block designs (Bradley and Terry, 1952; Alvo and Cabilio, 1993; Alvo and Philip, 2014), no practical method has been available for political scientists to analyze ranked outcomes more generally. On the other hand, this work con-
tributes to the causal inference literature by addressing another type of complex outcome data. Recent works have studied the treatment effects on multidimensional and/or structured data, such as text (Fong and Grimmer, 2016; Egami et al., 2018), ordinal data (Chiba, 2018; Lu, Ding and Dasgupta, 2018; Yamauchi, 2020), and multivariate outcomes (Lupparelli and Mattei, 2017). This work sheds light on opportunities and challenges in analyzing multidimensional and structured outcomes in the form of rankings.

1 Motivating Application: People’s Attitudes toward Police Violence

This section introduces the first motivating research on people’s responses to the use of deadly force by police officers toward unarmed Black men.

1.1 Motivation, Design, Data, and Hypotheses

How does contextual information affect people’s blame attribution in police violence? With increasing attention to officer-involved shootings (Davis, 2017), its subsequent trials (or lack thereof) (Fiarfax, 2017), and mass protests against them (Reny and Newman, 2021), answers to this question have important implications for multiple areas of research. Such include the role of media in shaping public opinions on race and law enforcement (Porter, Wood and Cohen, 2018; Jefferison, Neuner and Pasek, 2021), the role of prosecutors in grand and trial jury proceedings in police violence cases (Cook, 2017; Futrell, 2018; Davis, 1994), and racial differences in perceptions of officer-involved shootings (Mullinix, Bolsen and Norris, 2021; McGowen and Wylie, 2020; Strickler and Lawson, 2022).

Boudreau, MacKenzie and Simmons (2019) address this question by performing a survey experiment. More specifically, the authors examine people’s responses to the shooting of Stephon Clark on March 18, 2018, in Sacramento, California. In this incident, Stephon Clark, an unarmed 22-year-old African American man, was shot multiple times and killed by two white police officers from the Sacramento Police Department. After about a year, Sacramento County District Attorney (DA) declined to file criminal charges against the officers. Multiple protests and social unrest followed both the shooting and the DA’s announcement. To study whether contextual information shapes people’s perceptions of the shooting, the authors conducted a survey using samples from four California counties, including Sacramento.

The survey experiment shows respondents different contextual information about the shooting. Next, it shows respondents a list of seven parties related to the incident, including the victim, the two white police
officers, the chief of the Police Department, the mayor of Sacramento, the DA, the governor of California, and two California Senators (including Kamala Harris). Finally, it asks respondents to rank order the seven parties according to how much each party is responsible for the death of the victim (i.e., 1 means the most and 7 means the least responsible).

Table 1 displays the first six observations from their replication data. For example, the first respondent reports a ranked outcome (7,1,2,6,3,5,4). This indicates that the individual thinks that the officers are the most responsible and the police chief is the second most responsible, while the victim is the least responsible. In contrast, the sixth respondent reports a ranked outcome (1,2,6,5,7,3,4). This suggests that the person believes that the victim is the most responsible and the officers are the second most responsible, while the mayor is the least responsible. Thus, each respondent has a vector of values as outcome data (as opposed to a scalar) in this design.

| ID     | County     | Group    | Victim | Officers | Chief | DA | Mayor | Governor | Senators |
|--------|------------|----------|--------|----------|-------|----|-------|----------|----------|
| 100001 | Orange     | control  | 7      | 1        | 2     | 6  | 3     | 5        | 4        |
| 100002 | Sacramento | pattern  | 7      | 1        | 2     | 3  | 5     | 4        | 6        |
| 100003 | Sacramento | reform   | 1      | 2        | 3     | 4  | 5     | 6        | 7        |
| 100004 | Los Angeles| pattern  | 2      | 1        | 3     | 4  | 5     | 7        | 6        |
| 100005 | Riverside  | control  | 1      | 2        | 3     | 5  | 4     | 7        | 6        |
| 100006 | Riverside  | pattern  | 1      | 2        | 6     | 5  | 7     | 3        | 4        |

Table 1: Rank Outcomes from the Police Violence Data in Boudreau, MacKenzie and Simmons (2019).

Note: This table displays the first six observations from replication data of Boudreau, MacKenzie and Simmons (2019). The variable names were modified for simplicity.

The treatment in this experiment is the type of contextual information. Respondents are randomly assigned to one of three groups, including the control, “pattern-of-violence” treatment, and “reform” treatment groups. The control units are exposed to episodic information about the shooting. In contrast, respondents in the first treatment group are exposed to both episodic information and additional information, which describe that similar incidents involving police brutality have occurred in Sacramento. Similarly, in the second treatment condition, respondents are given both episodic information and additional information, which explains that the police department has implemented an extensive reform to prevent officer-involved shootings. Online Appendix B presents the full vignettes.

The experiment is motivated by two complex hypotheses (Boudreau, MacKenzie and Simmons, 2019, 1103). The first hypothesis states:
"H1. Citizens who receive thematic information describing a pattern of police violence will place less blame on the victim, state, and federal officials and more blame on the police and local officials who oversee them than citizens who receive only episodic information about police violence” (emphasis added).

The second hypothesis follows:

"H2. Citizens who receive thematic information summarizing reforms will place less blame on the victim, the police, and local officials and more blame on state and federal officials than citizens who receive only episodic information about police violence” (emphasis added).

1.2 Limitations in the Original Analysis

To test these hypotheses, the study uses a rank-ordered logit model, which is an extension of multinomial logit for ranking data (Beggs, Cardell and Hausman, 1981; Train, 2009). Online Appendix B details how the authors use their estimated coefficients to compute their (somewhat ad hoc and non-causal) quantities of interest. Substantively, the study finds evidence that supports its hypotheses, concluding that contextual information alters people's blame attribution in the shooting of Stephon Clark. While the study offers new insights on an important issue with a novel experimental design, there are several limitations in the original analysis.

First, its model specification does not offer direct evidence for its hypotheses. If properly modeled, the study could have recovered the treatment effects on the probability that each party will be the most responsible for the victim’s death. However, this still does not answer whether each treatment causes people to put more blame on some parties and less blame on others. Second, the study does not clearly define its causal quantities of interest and clarify how its model-based approach recovers its target effects. While it is essential to define causal estimands in any study before designing and analyzing experiments, it is more so with ranking data due to its more complex structure than conventional outcome data (e.g., continuous and binary outcomes).

2 A Potential-Outcomes Framework for Ranking Data

This section proposes a potential-outcomes framework to overcome the above challenges.
2.1 Notation

Rankings arise when people put a sequence of numbers on multiple items according to given criteria (e.g., relative preference and importance) (Marden, 1995; Alvo and Philip, 2014; Yu, Gu and Xu, 2019). For example, survey respondents may rank order three issue items \{Abortion, Gun, Inflation\} from the most important (1) to the least important (3) in a pre-election poll. Here, the outcome data for each respondent is multidimensional ordered data as it is a vector containing an ordered sequence of numbers (e.g., 132).

Formally, let \( A = \{A_1, ..., A_J\} \) be a set of \( J \) labelled items \( A_j \) (\( j = 1, ..., J \)). Let \( \mathcal{P}_J \) be a permutation space of size \( J! \). As a function, ranking is defined as a mapping \( R : \mathcal{A} \mapsto \mathcal{P}_J \) from the finite set of labeled items to the permutation space.

Let \( \mathbf{R}_i \in \mathcal{P}_J \) be a specific ranking provided by person or unit \( i \) (\( i = 1, ..., N \)). In the above example, unit i’s ranking – \( \mathbf{R}_i \) – is an element of the permutation space \( \mathcal{P}_3 = \{(123), (132), (213), (231), (312), (321)\} \). In other words, \( \mathcal{P}_J \) is the sample space for ranking data with \( J \) items.\(^1\)

Finally, I use \( R_{ij} \) to denote a marginal ranking of item \( j \) assigned by person \( i \). For example, when \( \mathbf{R}_i = (312) \), unit \( i \)’s marginal rankings become \( R_{i1} = 3, R_{i2} = 1, \) and \( R_{i3} = 2 \) (e.g., ranker \( i \) thinks that the third item is the second most important). Similarly, I use \( I(R_{ij} < R_{ik}) \) to denote an indicator function for pairwise ranking that takes 1 if person \( i \) prefers item \( j \) to item \( k \) (e.g., person \( i \) likes item \( j \) more than item \( k \)) and 0 otherwise.

2.2 Potential Outcomes and Causal Estimands

Consider a completely randomized experiment where \( N \) subjects are drawn from an infinite super population of size \( N_{sp} \) (where \( N_{sp} \) is large relative to \( N \)) via simple random sampling with replacement. Thus, this paper takes the super-population perspective, where potential outcomes are considered to be stochastic (Ding, Li and Miratrix, 2017; Imbens and Rubin, 2015, 109-112). Consider also a simple treatment regime in which each individual is assigned to the treatment or control group.

Let \( \mathbf{R}_i(1) \) be the potential ranking under the treatment and \( \mathbf{R}_i(0) \) the potential ranking under the control for unit \( i \). Let \( D_i \) be a binary random variable denoting whether unit \( i \) is assigned to the treatment (\( D_i = 1 \)) or the control group (\( D_i = 0 \)). Then, unit \( i \)’s observed ranking can be represented by a function of the two

\(^1\)Importantly, this distinguishes ranking data from rank-based statistics commonly used in nonparametric statistical tests (Conover, 1999), where rankings are used to summarize scalar-based outcomes.
potential rankings and the treatment indicator:

\[ R^{\text{obs}}_i = R_i(1)D_i + R_i(0)(1 - D_i) \]  

(1)

2.2.1 General Average Treatment Effects for Ranked Outcomes

Let \( g() \) be a known function that maps rankings onto \( n \)-th dimensional real numbers \( g : \mathcal{P}_J \mapsto \mathbb{R}^n \), where \( n \leq J! \). Let \( d(\alpha, \beta) \) be a known distance function that quantifies the distance between two vectors \( \alpha \) and \( \beta \).

The general form of average treatment effects on ranking outcome data can be defined as follows:

\[ ATE_{g,d} \equiv \mathbb{E}[d(g(R_i(1)), g(R_i(0)))] \]  

(2)

Here, \( g() \) can take any form on a spectrum of functions that vary in how much data reduction occurs. For example, in the limit case with no data reduction, it becomes an identity function \( g(R_i) = R_i \) that returns an original ranking of size \( J! \). In the other benchmark scenario, it can be an indicator function \( g(R_i) = I(R_{ij} = 1) \) that returns 1 if item \( j \) is most preferred and 0 otherwise. All other \( g \) functions can be located between the two cases on the spectrum.

When \( \alpha \) and \( \beta \) are scalars, \( d(\alpha, \beta) \) can be a simple difference \( \alpha - \beta \). Namely,

\[ ATE_g \equiv \mathbb{E}[g(R_i(1)) - g(R_i(0))] \]  

(3)

Without loss of generality, the rest of this paper assumes that \( d() \) is a simple difference. When \( \alpha \) and \( \beta \) are vectors, it can be a rank distance function (e.g., the Kendall distance) or any other distance function, such as the Mahalanobis distance (Marden, 1995; Alvo and Philip, 2014).

The key insight is that researchers can apply the same principle in the potential-outcomes framework to ranking data once they specify \( g() \). Importantly, which \( g() \) researchers should use depends upon their hypothesis. If the hypothesis concerns whether the treatment increases the rank of a specific item \( j \) (e.g., DA), using marginal ranks \( g(R_i) = R_{ij} \) can be useful. Similarly, if the hypothesis focuses on a pair of items \( j \) and \( k \) (e.g., the victim vs. the officers), using pairwise rankings \( g(R_i) = I(R_{ij} < R_{ik}) \) may be informative.

Motivated by the running application, this paper focuses on two \( g() \) functions that address average ranking and pairwise ranking. Moreover, I consider different causal estimands below. In Practical Guidance for Future
Research, I further discuss the choice of \( g() \).

### 2.2.2 Average Rank Effect (ARE)

The first effect measure quantifies how much the treatment increases the average rank of an item \( j \).\(^2\) Let \( R_{ij}(1) \) and \( R_{ij}(0) \) be a potential marginal ranking that unit \( i \) would assign to item \( j \) had she been exposed to the treatment and control conditions, respectively. I define the super-population average rank effect (ARE) for item \( j \) (\( j = 1, 2, \ldots, J \)) as follows:

\[
\tau_j \equiv \mathbb{E}[R_{ij}(1) - R_{ij}(0)]
= \mathbb{E}[R_{ij}(1)] - \mathbb{E}[R_{ij}(0)],
\]

where the expectation is taken over both the randomization distribution and the sampling distribution (Imbens and Rubin, 2015, 99).

The logical bound for this effect is \( \tau_j \in (1 - J, J - 1) \). Equation (4b) means that the ARE is defined as the difference between item \( j \)'s average rankings under the treatment and control conditions. Since the ARE can be defined for each item, there are \( J \) causal effects in total \( \tau = (\tau_1, \ldots, \tau_J)^T \). The ARE is informative when researchers are interested in the overall effects of the treatment on each item separately.

### 2.2.3 Average Pairwise Rank Effect (APE)

A second effect measure quantifies how much the treatment increases the probability that item \( j \) is preferred to item \( k \). Let \( I(R_{ij} < R_{ik})(1) \) denote a potential pairwise ranking under the treatment and \( I(R_{ij} < R_{ik})(0) \) under the control. For two items \( j \) and \( k \), I define a pairwise ranking effect (APE) as follows:

\[
\tau_{jk}^P \equiv \mathbb{E}[I(R_{ij} < R_{ik})(1) - I(R_{ij} < R_{ik})(0)]
= \mathbb{E}[I(R_{ij} < R_{ik})(1)] - \mathbb{E}[I(R_{ij} < R_{ik})(0)]
= \mathbb{P}((R_{ij} < R_{ik})(1)) - \mathbb{P}((R_{ij} < R_{ik})(0))
\]

The logical bound for this estimand is \( \tau_{jk}^P \in (-1, 1) \). Substantively, the APE means the difference between the probabilities that one item is preferred to another. Since there are \( \binom{J}{2} \) possible pairwise comparisons with

\(^2\)Gerber et al. (2016) implicitly used this estimand. Research on conjoint analysis has considered similar quantities (Abramson, Koçak and Magazinnik, 2022; Bansak et al., 2022).
$J$ items, there are $\binom{J}{2} \times 2$ causal effects: $\tau^P = (\tau^P_{12}, ..., \tau^P_{J(J-1)})$. For example, when $J = 3$, there exist $\binom{3}{2} \times 2 = (3 \times 2) \times 2 = 12$ possible APEs. The APE is informative when there are several pairs of items that scholars seek to study. When $J = 2$, both the ARE and APE become a conventional average treatment effect on binary outcome data (Online Appendix C).

### 2.3 Alternative Quantities of Interest

Many other estimands can be useful in different applications. For example, an alternative estimand may address the treatment effect on the probability that each item has a specific rank, such as $\mathbb{P}(R_{ij} = 1)$. This can be, for example, the probability that indigenous people in the Philippines rank tribal identity as the most important identity (McMurry, 2022). Moreover, scholars may focus on the probability that each item is ranked higher than a specific rank, such as $\mathbb{P}(R_{ij} \leq 3)$. An example can be the probability that citizens of Nigeria and Niger rank religion (Islam) as one of the three most important (e.g., top-3) identity attributes (Miles and Rochefort, 1991).

Furthermore, researchers may focus on the conditional treatment effect on the average rank of each item when some other items are ranked lower than specific ranks, such as $\mathbb{E}[R_{ij}(1) - R_{ij}(0) | R_{ik} > 3]$. Similarly, they may also study the treatment effect on the probability that a pair of items will be ranked higher than specific ranks, such as $\mathbb{P}(R_{ij} \leq 2$ and $R_{ik} \leq 4$). Finally, analysts may also be interested in whether the treatment changes the entire distribution of rankings. In this situation, they could focus on the rank distance (Alvo and Cabilio, 1993) between the set of rankings in the treatment and control conditions. Depending on the choice of $g()$ and $d()$, Equation (2) offers an array of causal estimands that can be useful in political science.

### 3 Identifying Assumptions, Estimators, and Inferential Methods

This section discusses two estimators for the proposed effects and statistical inference for them.

#### 3.1 Estimators

For the following estimators to be unbiased estimators for their corresponding effects, I make two standard assumptions:

**Assumption 1** (Ignorability). Both potential rankings are independent from treatment assignment: $\left( R_i(1), R_i(0) \right) \perp \perp D_i$

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3Online Appendix D also discusses partial identification based on nonparametric bounds.
Assumption 2 (SUTVA). Each unit’s observed ranking only depends on her treatment assignment, and there is only a single version of the treatment (Rubin, 1980).

While this paper focuses on unit-wise ignorability and SUTVA, future research may relax the two assumptions by focusing on a particular item or a pair of items.

3.1.1 Difference-in-Mean-Ranks Estimator

First, I consider the ARE. Let \( R_{ij}^{obs} \) be unit \( i \)'s observed marginal ranking for item \( j \). A natural estimator for the ARE is the difference-in-mean-ranks estimator:

\[
\hat{\tau}_j = \hat{E}[R_{ij}(1)] - \hat{E}[R_{ij}(0)]
\]
\[
= \hat{E}[R_{ij}^{obs}|D_i = 1] - \hat{E}[R_{ij}^{obs}|D_i = 0]
\]
\[
= \frac{\sum_{i=1}^{N} R_{ij}^{obs} D_i}{\sum_{i=1}^{N} D_i} - \frac{\sum_{i=1}^{N} R_{ij}^{obs} (1 - D_i)}{\sum_{i=1}^{N} (1 - D_i)},
\]

where \( \frac{\sum_{i=1}^{N} R_{ij}^{obs} D_i}{\sum_{i=1}^{N} D_i} \in (1, J) \) is the mean rank of item \( j \) for treated units and \( \frac{\sum_{i=1}^{N} R_{ij}^{obs} (1 - D_i)}{\sum_{i=1}^{N} (1 - D_i)} \in (1, J) \) is the mean rank of the same item for control units. These values can be easily calculated by taking the average of observed ranks for item \( j \) among the treated and control units. The proof is very standard and omitted. A mean rank is an important summary statistic for ranking data, and it represents the centrality of a given item in the sample (Yu, Gu and Xu, 2019). As there are \( J \) items, researchers can obtain a vector of estimated effects with the length of \( J \): \( \hat{\tau} = (\hat{\tau}_1, ..., \hat{\tau}_j)^T \).
3.1.2 Difference-in-Pairwise-Relative-Frequency Estimator

Next, a natural estimator for the APE is the difference-in-pairwise-relative-frequency estimator:

\[
\hat{\tau}_{jk}^P = \hat{P}((R_{ij} < R_{ik})(1)) - \hat{P}((R_{ij} < R_{ik})(0))
\]

\[= \hat{P}(R_{ij}^{obs} < R_{ik}^{obs}|D_i = 1) - \hat{P}(R_{ij}^{obs} < R_{ik}^{obs}|D_i = 0)\]  

\[= \frac{\sum_{i=1}^{N} I(R_{ij}^{obs} < R_{ik}^{obs})D_i}{\sum_{i=1}^{N} D_i} - \frac{\sum_{i=1}^{N} I(R_{ij}^{obs} < R_{ik}^{obs})(1 - D_i)}{\sum_{i=1}^{N} (1 - D_i)},\]  

(7c)

where \(\sum_{i=1}^{N} I(R_{ij}^{obs} < R_{ik}^{obs})D_i\) is the proportion that item \(j\) is ranked higher than item \(k\) in the treatment group, whereas \(\sum_{i=1}^{N} I(R_{ij}^{obs} < R_{ik}^{obs})(1 - D_i)\) is the same proportion for control units. These values can be easily calculated by creating a binary indicator that item \(j\) is preferred to item \(k\) for each unit and taking the average of all indicators in the sample. Again, the proof is standard and omitted. Since there are \(\binom{J}{2} \times 2\) effects, researchers can obtain a vector of estimated APEs: \(\hat{\tau}^P = (\hat{\tau}_{jk}^P, ..., \hat{\tau}_{J,J-1}^P)^\top\).

3.2 Constructing Standard Errors

To construct confidence intervals for each effect, I use a normal approximation of its sampling distribution in large samples. Online Appendix E illustrates simulation studies that report that the normal approximation appears reasonable for the proposed estimators.

3.3 Hypothesis Testing

Hypothesis testing for AREs and APEs faces a unique challenge for two reasons. First, it usually requires multiple hypothesis testing since researchers can estimate multiple effects at the same time. Second, such multiple tests may not be independent of each other. To account for these challenges, I adopt the Benjamini-Hochberg (BH) procedure to conduct hypothesis testing (Benjamini and Hochberg, 1995) (Online Appendix F), while future research may adopt other methods.

4 Empirical Illustration: Blame Attribution in Police Violence

This section reanalyzes the experimental data in Boudreau, MacKenzie and Simmons (2019) using the proposed methods. Contrary to the original study, I find that contextual information does not affect people’s attitudes
toward which parties are more (and less) responsible for the killing of the Black civilian.4

4.1 Average Rank Effects

The first hypothesis in the study is that the “pattern-of-violence” treatment causes citizens to put more blame on the police officers, the police chief, the mayor, and the DA, while making them put less blame on the victim, the governor, and the senators. The second hypothesis states that the “policereform” treatment causes citizens to blame the state and federal officials more, while making them blame the victim, the police officers, the police chief, and the local officials less.

To test these hypotheses, I first examine the AREs of the two treatments. The left panel of Figure 1 shows the estimated AREs of the pattern-of-violence treatment for the seven parties. Seeing effects on the left side of the panel (“More Blame”) means that respondents put higher rankings and equivalently more blame on each party. Here, I follow the convention by saying that a rank is higher in the treatment group than in the control group when the rank has a smaller value in the former than in the latter (e.g., number one has the highest rank). Consequently, lower values mean positive effects and vice versa.

I find that the pattern-of-violence treatment did not affect almost all parties. While the direction of most point estimates is consistent with the author’s hypothesis, these estimates are not statistically (and substantively) different from zero. One exception is the effect on the mayor, implying that knowing that the Clark shooting is not an isolated incident but belongs to a series of police shootings in the same city makes people blame the mayor slightly more.

The right panel of Figure 1 shows the estimated AREs of the reform treatment. Again, negative values mean positive effects. The result shows that while most estimated effects are in the expected direction (except for the effect on the governor), all effects appear to be statistically insignificant based on the estimated 95% confidence intervals. One exception is the effect on the DA, suggesting that knowing about the Police Department’s effort to reform makes people blame slightly less on the DA. Online Appendix G also performs multiple hypothesis testing, finding substantively similar results.

The above analysis suggests that people’s perception of the Clark shooting is robust to the presence and content of additional contextual information. However, it does not account for potential racially heterogeneous effects despite the centrality of race in the discussion of police violence (Davis, 2017). To address this concern,

4I analyzed only non-Sacramento non-African American subjects because it is for these subjects that the authors find the most consistent evidence for their hypotheses, leading to the same conclusion.
Figure 1: **Average Rank Effects of the Pattern-of-Violence and Reform Treatments**

*Note:* The left (right) panel shows the estimated population average rank effects for seven parties for the pattern-of-violence (reform) treatment.

I replicate the above analysis for White, Black, Latino, and Asian respondents separately. Figure G.1 shows that across the subpopulations, almost all effects are statistically and substantially insignificant, casting doubt on the presence of heterogeneous effects based on race and ethnicity.

In sum, the reanalysis did not find evidence that supports the hypotheses. Substantively, it implies that how officer-involved shootings are presented (such by media or prosecutors) may not alter people’s evaluations of police violence.

### 4.2 Average Pairwise Rank Effects

The hypotheses focus on each party separately. However, it could also be informative to analyze whether the treatment made respondents blame each party more *relative to* the victim as well as to the police officers — the central figures in the shooting. With this motivation, I estimate the average pairwise rank effects (APEs) of the two treatments. The upper two panels of Figure 2 show the results for the pattern-of-violence treatment, whereas the lower two panels present the findings for the police reform treatment.
Figure 2: Average Pairwise Rank Effects of the Pattern-of-Violence and Police Reform Treatments

Note: The upper panels show the estimated APEs of the pattern-of-violence treatment, whereas the lower panels visualize the obtained APEs of the police reform treatment. The left (right) panels present the effects while setting the victim (police officers) as a reference category.

The upper left panel reports the estimated APEs of the pattern-of-violence treatment with the victim as a reference category. Lower values mean that the treatment increases the probability that each party is ranked higher than the victim (i.e., blaming each party more than the victim). Similarly, the upper right panel presents the estimated effects with the officers as a reference category. Both panels suggest that the estimated APEs are not statistically significantly distinct from zero. This offers additional evidence against the original study's
expectations. Similar patterns are also observed in the lower two panels, suggesting that the reform treatment does not affect people’s pairwise rankings in the experiment.

To examine potential heterogeneous APEs, I also replicate the above analysis by focusing on the samples of Asian, Black, Latino, and White respondents, respectively. Figures G.2-G.5 suggest that my substantive conclusion holds within all racial groups. Combined with the earlier findings, the analysis of APEs reveals that none of the active treatments alters people’s attitudes toward officer-involved shootings.

What then determines people’s blame attribution in police violence? Recent research suggests that Black and White Americans hold distinct perceptions about officer-involved shootings even when they receive the same information (e.g., Jefferson, Neuner and Pasek, 2021). Motivated by such research and the above findings, Figure G.6 shows the estimated average rank of each party for Asian, Black, Latino, and White samples. I find that the way people hold the police force accountable significantly differs by race and ethnicity, reaffirming the importance of jury selection in police violence cases (Cook, 2017; Futrell, 2018; Davis, 1994).

5 Extension to Partially Ranked Data

So far, I have assumed that people rank all items. In many political science applications, however, researchers observe partially ranked data, where people only rank a subset of available items (Critchlow, 2012). In preferential voting, for example, voters often choose not to rank any candidate beyond their first-choice candidates (Reilly, 2001, 150-158). Similarly, in the study of postmaterialism, survey respondents are asked to rank the two most important items among four items (Inglehart and Abramson, 1999). In such scenarios, the above framework cannot be directly applicable because outcome data are partially missing. To overcome this challenge, this section extends the proposed framework to partially ranked data.

5.1 Motivating Application: Ballot Order Effects in Ranked-Choice Voting

As a motivating application, I study ballot order effects in ranked-choice voting (RCV) (Orr, 2002; King and Leigh, 2009; Curtice and Marsh, 2014; Robson and Walsh, 1974; Ortega Villodres and Garcia de la Puerta, 2004; Ortega Villodres, 2008).5

Ballot order effects are the effects of the order in which candidates appear on the ballot on voters’ stated

5More generally, the discussion here applies any preferential voting system, including Alternative Vote and Single Transferable Vote. For more variants of RCV, see Santucci (2021).
preferences and candidate vote shares.\textsuperscript{6} To date, especially since Bush v. Gore (2000), a body of works has estimated these effects, explained their causes, and discussed their implications under first-past-the-post (FPTP) (Ho and Imai, 2006, 2008; Meredith and Salant, 2013; Grant, 2017; Chen et al., 2014; Lutz, 2010; Barker and Lijphart, 1980; Koppell and Steen, 2004; Miller and Krosnick, 1998), proportional representation (Geys and Heyndels, 2003; Marcinkiewicz, 2014; Faas and Schoen, 2006; Blom-Hansen et al., 2016; Gulzar, Robinson and Ruiz, 2022), and other systems (Song, 2022). Most importantly, the literature has documented evidence for “primacy” (first position) and “latency” (last position) effects in elections (Alvarez, Sinclair and Hasen, 2006) and survey research more broadly (Kim, Krosnick and Casasanto, 2015).

RCV is an electoral system in which voters can vote by ranking multiple candidates (Santucci, 2021). The literature has suggested that, while mostly focusing on voters’ first-choice preferences, ballot order effects are present and consequential in RCV too (Orr, 2002; King and Leigh, 2009; Curtice and Marsh, 2014; Robson and Walsh, 1974; Ortega Villodres and García de la Puerta, 2004; Ortega Villodres, 2008; Marcinkiewicz and Stegmaier, 2015; Söderlund, von Schoultz and Papageorgiou, 2021).\textsuperscript{7} Yet, despite the increasing attention to RCV among scholars (Drutman, 2020) and candidates (Peters, 2022),\textsuperscript{8} there has been almost no research that analyzes the ballot order effects on voters’ entire ranked ballot, let alone on partially ranked ballot.\textsuperscript{9}

Motivated by this challenge, I develop a generic strategy to identify and estimate the treatment effects on any partially ranked outcomes (beyond the running application). While this paper focuses on the average rank effects (ARE), the following strategy can be applied to any quantity in the class of estimands discussed in Equation (2).

5.2 Illustration of Partially Ranked Data

To illustrate partially ranked data, suppose that analysts want to know whether ballot order affects candidate ranks. Suppose that they randomly assign study subjects (hereafter called voters) to either the control or treatment group. Figure 3 visualizes what voters see under the two conditions in the simplest experimental design. In the control group, voters see a set of candidates in a particular order. In the treatment group, voters see the same list of candidates in a slightly different order. Here, Gregory Hodge now appears in the first position as

\textsuperscript{6}More broadly, ballot order effects appear in survey research with inattentive respondents (e.g., Atsusaka and Stevenson, 2021).

\textsuperscript{7}One extreme type of ballot order effect in RCV has been documented as the “donkey vote” (Orr, 2002) that has been “observed at federal elections in Australia, in which some electors simply number sequentially from 1 onward down the ballot paper” (Reilly, 2001, 158).

\textsuperscript{8}One candidate for the Oakland mayoral election mentioned, “The [ballot] order is very important, extremely important in elections where there’s no incumbent running. This could force as much as a five percent point differential” (Peters, 2022).

\textsuperscript{9}One notable exception is Horiuchi and Lange (2019).
opposed to the second place, and Seneca Scott appears in the second position as opposed to the first place (i.e., experimental manipulation). When voters rank all candidates, researchers can estimate the treatment effect on, for example, Gregory Hodge’s average rank using the basic framework discussed above.

| Control Group                          | Treatment Group                          |
|---------------------------------------|------------------------------------------|
| Rank up to 3 candidates. Mark no more than 1 box in each column. | Rank up to 3 candidates. Mark no more than 1 box in each column. |
| 1st                                   | 1st                                      |
| 2nd                                   | 2nd                                      |
| 3rd                                   | 3rd                                      |
| Seneca Scott                          | Gregory Hodge                            |
| Gregory Hodge                         | Seneca Scott                             |
| Loren Manuel Taylor                   | Loren Manuel Taylor                      |
| Peter Y. Liu                          | Peter Y. Liu                             |
| Sheng Thao                            | Sheng Thao                               |
| Ignacio De La Fuente                  | Ignacio De La Fuente                     |
| Allyssa Victory Villanueva            | Allyssa Victory Villanueva               |
| John Reimann                          | John Reimann                             |
| Tyron C. Jordan                       | Tyron C. Jordan                          |
| Treva D. Reid                         | Treva D. Reid                            |

Figure 3: Example of Experimental Designs for Studying Ballot Order Effects in RCV

Note: The ten candidates are drawn from the 2022 Oakland Mayoral Election.

Now, suppose that voters are only allowed to rank up to three candidates as in many RCV elections in the U.S. Table 2 presents an example of partially ranked data that scholars would observe in this condition. The first three units are control, while the last three units are treated.

The first voter ranked three candidates, including Reinmann, Scott, and Taylor, while not ranking the other candidates (denoted by “—”). In contrast, the second voter ranked the first three candidates she sees on the ballot, potentially engaging in what is known as the “donkey vote” (see footnote 11) (Orr, 2002). Finally, the third voter only ranked two candidates, even though he could have ranked three candidates.

Suppose that researchers wish to estimate the treatment effect on Hodge’s average rank. The challenge
Table 2: Example of Partially Ranked Outcome Ballot in Randomized Experiments

Note: The observed stated ranked preferences for ten candidates in a hypothetical experiment. “—” means that no rank is observed. Fuente = De La Fuente.

| Unit | Group  | Scott | Hodge | Taylor | Liu | Thao | Fuente | Villanueva | Reinmann | Jordan | Reid |
|------|--------|-------|-------|--------|-----|------|--------|------------|----------|--------|------|
| 1    | Control| 2     | —     | 3      | —   | —    |        | —          | 1        | —      | —    |
| 2    | Control| 1     | 2     | —      | —   | —    | —      | —          | 2        | —      | —    |
| 3    | Control| 1     | —     | —      | —   | —    | —      | —          | 2        | —      | —    |
| 4    | Treated| —     | 1     | —      | —   | —    | —      | —          | —        | —      | —    |
| 5    | Treated| 2     | 1     | —      | —   | —    | —      | —          | —        | —      | —    |
| 6    | Treated| 2     | 1     | —      | —   | —    | 3      | —          | —        | —      | —    |

here is that because the first and third voters did not rank him, Hodge’s rank is not observed and missing for these units (denoted by “—”). Moreover, voters seem more likely to rank Hodge when they are treated than otherwise. How can researchers estimate the treatment effect with partially missing ranking data like this? As discussed below, when the treatment affects the missingness of Hodge’s rank, performing listwise deletion on Hodge (i.e., dropping missing observations) leads to a statistical bias with respect to the treatment effect of interest. While the running example focuses on RCV, this is a general problem that arises in causal inference with any partially ranked outcomes.

5.3 Partial Rankings as a Selection Problem

To address this problem, I consider partially ranked data as a selection problem, in which outcome data are defined but missing for some units. I also assume that missingness is affected by the treatment assignment. While there are different types of selection problems, the proposed framework is similar to the one discussed for racial bias in policing (Knox, Lowe and Mummolo, 2020) and different from the “truncation by death” problem, where missing outcome data are not defined for truncated units (Zhang and Rubin, 2003).

Figure 4 represents the general structure of the problem in a directed-acyclic graph (DAG) (Glymour, Pearl and Jewell, 2016). Here, \( D_{ij} \) is a treatment indicator (e.g., ballot order) showing whether unit \( i \) is treated for item \( j (D_{ij} = 1) \) or not \( (D_{ij} = 0) \). Similarly, \( M_{ij} \) is a mediator (or selection) variable denoting whether unit \( i \) decides to rank item \( j (M_{ij} = 1) \) or not \( (M_{ij} = 0) \). Finally, \( R_{ij} \in\{1, 2, ..., J\} \) is unit \( i \)’s marginal ranking for item \( j \), whereas \( U_{ij} \) is a set of unobserved confounders that affect both \( M_{ij} \) and \( R_{ij} \) (e.g., perceived candidate quality).

Here, a naïve approach is to estimate the effect of \( D_{ij} \) on \( R_{ij} \) after dropping all units with \( M_{ij} = 0 \) (i.e.,
Figure 4: Directed Acyclic Graph for Partially Ranked Data
Note: $D_{ij}$, $M_{ij}$, and $R_{ij}$ are observed while $U_{ij}$ is not observed by researchers.

listwise deletion). However, since $\mathbb{P}(M_{ij} = 0)$ itself is affected by $D_{ij}$, only keeping units with $M_{ij} = 1$ (selection on the value of $M_{ij}$) means conditioning on a post-treatment variable (Montgomery, Nyhan and Torres, 2018). A danger for causal inference arises when a set of unmeasured factors $U_{ij}$ affect both $M_{ij}$ and $R_{ij}$. For example, perceived candidate quality (i.e., to what extent voters think a candidate is appealing) may affect whether and how they rank the candidate.\footnote{Estimating the effect among units whose outcomes are not missing (e.g., $\mathbb{E}[R_{ij}(D_{ij} = 1) - R_{ij}(D_{ij} = 0)|M_{ij} = 1]$) may lead to a non-causal quantity (Knox, Lowe and Mummolo, 2020). In Table 2, the bias arises because voters who ranked Hodge when they saw him at the top of the ballot (i.e., treated) are fundamentally different from voters who ranked him (even) when they do not see him in the first position (i.e., control).}

To study treatment effects with partially ranked data, this paper draws from the framework of principal stratification by Frangakis and Rubin (2002). For each candidate $j$, I define four subsets of units (called principal strata) based on their pre-treatment characteristics that define how they react to the active treatment (Page et al., 2015). Among the four strata, I primarily focus on order-rankers, which is a group of units that rank item $j$ only when they are in the active treatment condition.

5.4 Causal Estimands for Partially Ranked Data

Building on the above argument, I now introduce two causal estimands for partially ranked data. The first estimand is the average rank effect (ARE) for item $j$ ($\tau_j$) as defined in Equation (4a).

The second quantity of interest is what I call the order-ranker ARE. Again, order-rankers refer to people who only rank item $j$ when they are treated. Let $R_{ij}(D_{ij}, M_{ij}(D_{ij}))$ be unit $i$’s potential marginal ranking for item $j$ under the treatment assignment $D_{ij}$ and the potential selection state under the same condition $M_{ij}(D_{ij})$. The order-ranker ARE is defined as follows:

$$
\tau_{j,o} = \mathbb{E}[R_{ij}(1, M_{ij}(1)) - R_{ij}(0, M_{ij}(0))|M_{ij} = 1]
$$

(8a)

$$
= \mathbb{E}[R_{ij}(1, M_{ij}(1))|M_{ij} = 1] - \mathbb{E}[(0, M_{ij}(0))|M_{ij} = 1]
$$

(8b)
This estimand, often called a local average treatment effect (LATE) or compiler average causal effect (CASE), is of particular interest in the context of RCV. I focus on this effect because I expect ballot order effects to be present primarily among “swing” or “floating” voters with no pre-arranged voting schedule. Online Appendix H discusses the identification result for the order-ranker ARE.

5.5 Partial Identification via Nonparametric Bounds

The fundamental problem in partially ranked data is that \( \tau_j \) and \( \tau_{j,o} \) are never point identified (i.e., cannot be estimated) because the outcome data are partially missing. However, it is still possible to construct nonparametric sharp bounds on (i.e., partially identify) \( \tau_j \) and \( \tau_{j,o} \). Such bounds cover the lowest and highest possible treatment effects consistent with the available information (Manski, 1990; Horowitz and Manski, 2000).

To construct the nonparametric bounds, I make two assumptions.

**Assumption 3 (Bounded Support).** For each ranker, the unobserved marginal ranking of a given item must be lower than any observed marginal ranking and higher than or equal to the lowest possible marginal ranking. Formally, \( \max(R_{ij}(D_{ij}, 1)) < R_{ij}(D_{ij}, 0) \leq J \) for all \( i, j \), and \( D_{ij} \).

Bounded support is based on the nature of rankings (i.e., logical bound) and thus always holds (Manski, 1990). Table 3 shows the logical bound of Hodge’s unobserved (and potential) rank for the first voter in Table 2. Here, the highest rank for Hodge must be 4 because the highest observed rank is 3. Similarly, the lowest rank for him must be 10 because it is the lowest possible rank when there are ten candidates. Bounded support states that Hodge’s potential ranks must be between 4 and 10.

|          | Scott | Hodge | Taylor | Liu | Thao | Fuente | Villanueva | Reinmann | Jordan | Reid |
|----------|-------|-------|--------|-----|------|--------|------------|----------|--------|------|
| Observed rank | 2     | —     | 3      | —   | —    | —      | —          | 1        | —      | —    |
| Highest rank  | 2     | 4     | 3      | —   | —    | —      | —          | 1        | —      | —    |
| Lowest rank   | 2     | 10    | 3      | —   | —    | —      | —          | 1        | —      | —    |

**Table 3: Illustration of Bounded Support**

*Note:* The highest rank is 4 because the highest observed rank is 3. The lowest rank is 9 because it is the lowest possible given that there are ten candidates. Fuente = De La Fuente.

---

11 More precisely, this strategy imputes potential outcomes for both sampled (but missing) and unsampled units under the super-population perspective. Another approach would be constructing confidence sets for bounds (Horowitz and Manski, 2000; Imbens and Manski, 2004).
Assumption 4 (Positive Average Treatment Effects). Average treatment effects are positive. Formally, \( \tau_j > 0 \) and \( \tau_{j,o} > 0 \).

With Assumptions 3-4, the proposed strategy first creates two imputed samples: one for the “best-case” scenario, where the maximal treatment effect is assumed, and another for the “worse-case” scenario, where the minimal treatment effect is assumed (see also Coppock and Kaur, 2022). Let \( \overline{R}_{ij}^* \) and \( \underline{R}_{ij}^* \) be the imputed samples based on the maximal and minimal treatment effects. The imputed samples are defined as follows:

\[
\underline{R}_{ij}^* = \frac{M_{ij}R_{ij}}{\text{observed rank}} + (1 - M_{ij}) \left\{ D_{ij}(\max(R_{ij'}) + 1) + (1 - D_{ij})J \right\}
\]

\[
\overline{R}_{ij}^* = \frac{M_{ij}R_{ij}}{\text{observed rank}} + (1 - M_{ij}) \left\{ D_{ij}J + (1 - D_{ij})(\max(R_{ij'}) + 1) \right\}
\]

Finally, the proposed strategy constructs nonparametric sharp bounds using the two extreme effects.

Proposition 1 (Nonparametric Bounds on \( \tau_j \) and \( \tau_{j,o} \)). Let \( \overline{\tau}_j = \mathbb{E}[\overline{R}_{ij}^* (1, M_{ij}) - \overline{R}_{ij}^* (0, M_{ij})] \) be the lowest possible ARE and \( \underline{\tau}_j = \mathbb{E}[\underline{R}_{ij}^* (1, M_{ij}) - \underline{R}_{ij}^* (0, M_{ij})] \) be the highest possible ARE. Then, the ARE can be partially identified with the following bound:

\[
\tau_j = \left[ \tau_j, \overline{\tau}_j \right]
\]

Similarly, the order-ranker ARE can be partially identified with the following bound:\(^{12}\)

\[
\tau_{j,o} = \left[ \frac{\tau_j}{\pi_o}, \frac{\overline{\tau}_j}{\pi_o} \right]
\]

where \( \pi_o \) is the proportion of order-rankers. To study the properties of the bounds, Online Appendix I presents simulation studies.

\(^{12}\)In practice, \( \overline{\pi}_o \) is used instead of \( \pi_o \). More specifically, both bounds must be within \( [- (J - 1), J - 1] \), although I suppress such constraint for clarity.
6 Application to Ballot Order Effects in RCV

I now apply the extended framework to study ballot order effects in RCV.

6.1 Identification and Estimation of Ballot Order Effects

I define generalized ballot order effects building on the example in Figure 3. Online Appendix J discusses the identification and estimation of generalized ballot order effects and three additional assumptions, including ballot order randomization. The key result is that the randomization of candidate positions at the unit level (i.e., random assignment) allows researchers to study the effect of each position on candidate ranks averaged over all counterfactual positions and the ordering of other candidates.\(^{13}\)

6.2 Three Survey Experiments

To study ballot order effects in RCV, I perform survey experiments in (1) the 2022 Oakland mayoral election, (2) the U.S. House of Representatives election in Alaska, and (3) the U.S. Senate election in Alaska. All surveys and analysis plans were pre-registered. These contests vary in the type of office, the level of national attention, voters’ partisan leaning, and the novelty of RCV, offering rich contextual variation to study ballot order effects.

Figure 5 shows an example of the main survey questions. In the experiments, I showed respondents a list of actual candidates (with their occupations in Oakland and registered parties in Alaska). I designed the experimental questions by carefully mocking actual official sample ballots in Oakland and Alaska.\(^{14}\) Unlike the actual RCV elections, however, the order of candidates was randomized at the respondent level.

Here, the treatment is whether each candidate appears in a particular position on the ballot (\(D_{ij} = 1\)) or not (\(D_{ij} = 0\)). When focusing on the effect of the first position, for example, respondents who see Figure 5 are treated with respect to Treva Reid (but not with respect to the other candidates).

To obtain outcome data, I asked respondents to rank up to three (in Oakland) or four (in Alaska) candidates according to their actual RCV rules by marking radio buttons. Additionally, I also asked them to rank all candidates to obtain fully ranked data, which I use as supplemental information below. Online Appendix K presents a set of survey instructions, attention checks, and experimental questions used in the studies.

\(^{13}\)The idea is similar to the average marginal component effects in conjoint analysis (Hainmueller, Hopkins and Yamamoto, 2014).

\(^{14}\)I used and obtained sample ballot for the last two elections from the City of Oakland website and the Alaska Division of Elections website.
Imagine that you are about to vote. 10 candidates are running for the Oakland mayoral election.

If you are using a mobile device, you may want to rotate your device so that you can see all candidates.

Please tell us how you would vote by ranking up to three candidates according to your preference. Here, 1 represents "the most preferred candidate." Again, if you wish, you do not need to rank all three candidates.

| Candidate                                           | 1 | 2 | 3 |
|-----------------------------------------------------|---|---|---|
| Treva D. Reid (Councilmember/Senior Caregiver)      |   |   |   |
| Allyssa Victory Villanueva (Civil Rights Attorney)  |   |   |   |
| Seneca Scott (Small Business Owner)                 |   |   |   |
| Ignacio De La Fuente (Business Solution Strategist) |   |   |   |
| Peter Y. Liu (Entertainer)                          |   |   |   |
| Loren Manuel Taylor (Councilmember/Small Businessperson) |   |   |   |
| Gregory Hodge (Non-Profit Executive)                |   |   |   |
| Tyron C. Jordan (Legal Assistant)                   |   |   |   |
| John Reimann (Retired Carpenter)                    |   |   |   |
| Sheng Thao (Oakland City Councilmember)             |   |   |   |

Figure 5: An Example of Experimental Survey Question

Note: Candidate order (treatment) was randomized at the respondent level.

The survey experiments were implemented via the Lucid Marketplace (October 10th – November 7th, 2022). Unlike many studies using nationally representative samples, I geo-targeted respondents by sampling within Oakland and Alaska, respectively. To obtain the largest possible sample while achieving demographic representativeness, I used the 2020 Census to set quotas based on gender, race, and ethnicity in each area. The total number of respondents was 253 (Oakland) and 358 (Alaska), respectively. Only keeping respondents who passed four attention checks I used in the surveys (following Aronow et al., 2020), I obtained 169 (Oakland) and 190 (Alaska) respondents.

15Respondents received $3 for participating in the surveys.
6.3 Study I (2022 Oakland Mayoral Election)

Figure 6 shows the effects of the first position for ten candidates in the Oakland mayoral election. Panel A visualizes the nonparametric bounds for the ARE (left window) and order-ranker ARE (right window). Taylor, Thao, and De La Fuente are candidates with political experience, while others do not have any office-holding record. There are several notable findings.

![Graph A showing average rank effects and order-ranker average rank effects for each candidate.]

First, all nonparametric bounds on the ARE cover zero. This suggests that none of the ten candidates seem...
to have received higher average ranks because they appeared in the first position on the ballot. Moreover, I verify this finding using fully ranked data as ground truth data. The point and interval estimates imply that ballot order effects are statistically and substantively insignificant, except for Thao — one of the three competitive candidates.

Moreover, five sharp bounds on the order-ranker ARE also cover zero. This means that respondents who only rank these candidates when treated (i.e., seeing them in the first position) did not give higher marginal rankings to the selected candidates. Importantly, the order-ranker ARE was not identified for the lower five candidates due to the violation of Assumption A1 (Ranking Monotonicity).

Panel B displays the proportion of observed ranks, $\hat{P}(M_{ij} = 1)$, for all candidates among treated and control units. It shows that respondents are more likely to rank five candidates (Scott, Liu, Reimann, Jordan, and Thao) when they see them at the top of the ballot. By doing so, it documents a novel form of ballot order effects under RCV. Namely, ballot order affects not only how people rank each candidate but also whether they rank a given candidate. However, respondents are less likely to do so for the other candidates in the same condition (suggesting the presence of “defiers”). Below, I discuss this puzzling pattern in more detail.

Furthermore, Figure 7 presents the results for all positions (Panel A) and all pairs of adjacent positions (Panel B). It suggests that most of the 190 treatment effects are not statistically and substantively significant, including the effects of the first and the last positions. Indeed, at the $\alpha = 0.05$ level, about 10 cases out of the 190 effects could be statistically significant solely by chance. This finding seems puzzling since the primacy (first position) and latency (last position) effects are the most common forms of ballot order effects studied in the literature (Alvarez, Sinclair and Hasen, 2006; Orr, 2002; Curtice and Marsh, 2014). Finally, I find evidence that the fourth and eighth positions may yield lower ranks for some candidates, which, again, cannot be fully explained by previous research.

### 6.4 Studies II-III (U.S. House and U.S. Senate Elections in Alaska)

Online Appendix L reports the results for the U.S. House and U.S. Senate elections in Alaska. The overall findings are consistent with the findings from Oakland. Below, I examine the Alaskan data more.
Figure 7: Ballot Order Effects Across Positions in Oakland

Note: Panel A shows the effect of candidates appearing at each position of the ballot in the Oakland mayoral election data. Panel B shows the effect of the two positions combined. Negative values mean positive ballot order effects.

### 6.5 New Puzzle

The experimental results shed light on a new puzzle in the research on ballot order effects in RCV and item order effects in survey research more generally. I find that ballot order still matters, affecting how people might vote by ranking multiple candidates in RCV. However, I do not find any evidence for the primacy (first position) and latency (last position) effects, even though they are the most common forms of ballot order effects discussed in the literature (Alvarez, Sinclair and Hasen, 2006; Ho and Imai, 2008). Furthermore, while ballot order seems to affect whether people rank a given candidate, the direction of such effects is inconsistent.

### 6.6 Alternative Theory of Ballot Order Effects

To explain this puzzle, I propose an alternative theory of ballot order effects, which I call pattern ranking. The theory states that people vote in RCV and answer rank-order questions by following geometric patterns (instead of revealing their sincere preferences). And this happens when they do not have full ranked preferences and avoid "looking bad" (e.g., by engaging in seemingly cheating patterns, such as 1234). One observable implication
of the theory is that some geometric patterns are observed with a significantly higher frequency than other patterns.

To test this hypothesis, Figure 8 plots the empirical distributions of all observed rankings in the U.S. House (left) and U.S. Senate (right) races in Alaska (with 95% confidence intervals). It also shows the results from the main experimental question (upper bars) and the supplemental question (lower bars) separately. Recent research shows that when (1) item order is randomized and (2) respondents reveal their sincere ranked preferences, the distribution of observed rankings will be uniform (Anonymous, 2022). Therefore, non-uniform distributions of observed rankings suggest the presence of “pattern rankers.”

![Figure 8: The Distributions of Observed Rankings in the U.S. House and Senate elections in Alaska. Note: A dashed line shows the uniform distribution in each panel. Each panel shows the proportion of unique rankings based on partially (red) and fully ranked (blue) data. 95% confidence intervals are displayed.](image)

The figure confirms the above expectation. Some geometric patterns (e.g., 3241) are selected significantly more often than other patterns (e.g., 1234). Pearson’s $\chi^2$ test rejects the null hypothesis that each of the four
distributions is uniform. This result is remarkable because it challenges the conventional wisdom that the donkey vote (choosing 1234) is the most common form of ballot order effects in RCV, if any (Orr, 2002; King and Leigh, 2009; Curtice and Marsh, 2014; Robson and Walsh, 1974; Ortega Villodres and García de la Puerta, 2004; Ortega Villodres, 2008).

These non-uniform distributions have an important consequence in actual RCV (where candidate order is not randomized). For example, in the Alaskan contests, the most salient ranking profile 3241 corresponds to the ordering Tshibaka (R, Trump-endorsed) $\succ$ Kelley (R, withdrew) $\succ$ Chesbro (D) $\succ$ Murkowski (R, Incumbent) for the U.S. Senate and Peltora (D, Incumbent) $\succ$ Bye (I) $\succ$ Begich (R) $\succ$ Palin (R, Trump-endorsed) for the U.S. House, respectively ($A \succ B$ means A is preferred to B).

Furthermore, these pattern rankings were produced by attentive respondents who passed the four attention checks in the surveys. When I replicate the analysis for inattentive respondents, I find that the resulting distributions are uniform (Figure I.3), suggesting that inattentive respondents offer actually random rankings. It is still unclear whether pattern rankings exist only among survey respondents or they also exist among actual voters. Future research must examine the nature and consequences of pattern rankings in RCV and survey research in more depth.

This way, the proposed framework not only promotes new empirical analyses (e.g., people’s attitudes toward police violence) but also contributes to the development of novel theories (e.g., ballot order effects in RCV) in political science.

7 Practical Guidance for Future Research

This paper introduced a potential-outcomes framework to perform causal inference when outcome data are ranking data. The class of estimands in Equation (2) can be applied to various contexts of political science. How can researchers choose the “right” $g()$ (and thus estimand) when analyzing treatment effects on ranking data?

The choice of $g()$ should be motivated by their hypothesis of interest. For example, if the hypothesis focuses on how the relative importance (i.e., rank) of a particular item is affected by the treatment, the ARE can be a helpful estimand. Similarly, the APE can be an appropriate quantity to study if the hypothesis addresses the treatment effect on the relative importance of one or more pairs of items. Importantly, researchers do not need to hypothesize about all available items if their theoretical predictions only concern a subset of available items.

16I also analyze those who have either voted or are likely to vote, finding the same patterns.
Online Appendix M illustrates the recommendation by studying conflict-related sexual violence (Agerberg and Kreft, 2022), foreign donor (Dietrich, 2016), and economic coercion (Gueorguiev, McDowell and Steinberg, 2020).

As discussed in Alternative Quantities of Interest, researchers can study many kinds of treatment effects with ranking data. While it is critical that researchers carefully examine their hypotheses at the design stage of experiments, an array of causal estimands can also motivate researchers to find new hypotheses (Grimmer, Roberts and Stewart, 2021). One principled way to approach this problem is to (1) start with estimands that only address one or a few items (e.g., average ranks and probabilities of being in top-\(k\)) and (2) move on to estimands that cover multiple items (e.g., pairwise rankings, rank distances, and conditional effects). This way, causal inference with ranking data sheds light on the possibility that political scientists can draw more complex hypotheses from their substantive theories than they do with more traditional outcome data.

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Online Appendix

For “Causal Inference with Ranking Data: Application to Blame Attribution in Police Violence and Ballot Order Effects in Ranked-Choice Voting”

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A Previous Applications of Rankings in Political Science

Political scientists have long used rankings and ranking data to study various political phenomena. For example, research in American politics has studied

- political parties (Aldrich, 1995)
- Iowa caucuses (Brady, 1990)
- committees (Stewart III and Groseclose, 1999; Groseclose and Stewart III, 1998)
- seniority-based nominations (Cirone, Cox and Fiva, 2021)
- party disciplines (McCarty, Poole and Rosenthal, 2001)
- political texts (Carlson and Montgomery, 2017)
- legislative speech (Quinn et al., 2010)
- redistricting (Kaufman, King and Komisarchik, 2021)
- representation (Tate, 2003)
- political knowledge (Prior, 2005)
- political values (Ciuk, 2016)
- political efficacy (King et al., 2004)
- issue voting (Rivers, 1988)
- referendum (Loewen, Rubenson and Spirling, 2012)

Moreover, research in comparative politics has examined

- party-list proportional representation (Cox et al., 2021; Buisseret et al., 2022)
- ranked-choice voting (RCV) (Tolbert and Kuznetsova, 2021)
- nationalism and ethnic identity (Miles and Rochefort, 1991)
- strategic voting (Cain, 1978)
- ethnic diversity (Baldwin and Huber, 2010)
- postmaterialism (Duch and Taylor, 1993; Inglehart and Abramson, 1999)
- the Chinese Communist Party (Kung and Chen, 2011; Shih, Adolph and Liu, 2012)

Furthermore, studies in international relations have investigated

- human rights (McFarland and Mathews, 2005)
- international governance (Kelley and Simmons, 2015, 2021)
- global regulatory behavior (Doshi, Kelley and Simmons, 2019)
- civil wars and democratic reversals (Goldstone et al., 2010)
- burden-sharing in security governance (Dorussen, Kirchner and Sperling, 2009)
- compliance in international agreements (Dai, 2005)

Finally, political scientists have also studied other topics using rankings

- behavioral social choice (Regenwetter et al., 2006)
• policy punctuation (Jones, Sulkin and Larsen, 2003)
• diffusion in judicial doctrines (Canon and Baum, 1981)
• diffusion in public policy (Desmarais, Harden and Boehmke, 2015)
• local public goods (Olken, 2010)
• academic journals (Carter and Spirling, 2008)
• political science departments (Hix, 2004; Frese, 2022)

B Discussion on Boudreau, MacKenzie and Simmons (2019)

The Experimental Designs

The full vignette for the control is: “Stephon Clark, a 22-year-old black man, was shot and killed on the evening of March 18, 2018, by two Sacramento Police Department officers. The officers were looking for a suspect who was breaking windows in the Meadowview neighborhood of Sacramento. They confronted Clark, who they found in the yard of his grandmother’s house, where he lived. The officers ordered Clark to stop and show his hands. Clark then ran from the officers. According to the police, Clark turned and held an object in front of him while he moved toward the officers. The officers believed that Clark was pointing a gun at them and, fearing their lives were in danger, they shot and killed Clark. After the shooting, the police reported that Clark was only carrying a cell phone” (Boudreau, MacKenzie and Simmons, 2019, 2).

The additional vignette for the pattern-of-violence condition is: “The Stephon Clark shooting is one of several controversial incidents involving police officers and the use of deadly force in Sacramento. In 2017, there were 25 incidents in Sacramento County that involved the use of force resulting in serious injury or death of the suspect, up from 20 in 2016. In July of 2016, two Sacramento Police Department officers shot and killed Joseph Mann, a 51-year old mentally-ill black man who was carrying a pocketknife and acting erratically. Mann refused to comply when the police ordered him to drop his knife and get on the ground. Shortly after, cameras recorded the two officers trying to run over Mann with their police cruiser. When they missed, the officers stopped the cruiser and chased Mann on foot. Moments later, the officers fired 18 times and killed Mann. The Clark and Mann shootings are among a series of incidents where civilians not armed with guns and arguably posing no real imminent threat to officers or others, were shot and killed by the Sacramento police” (Boudreau, MacKenzie and Simmons, 2019b, 3).

The additional vignette for the reform group is: “In response to several controversial incidents involving police officers and the use of deadly force, the Sacramento Police Department has adopted multiple reforms aimed at preventing incidents like the Stephon Clark shooting. Chief among them are the creation of a citizen oversight commission to review police actions and a video-release policy that requires the department to release footage in officer-involved shootings within 30 days. Both reforms go beyond what many other police departments in California have done to improve transparency. The Sacramento Police Department also now requires officers to undergo a 40-hour Crisis Intervention course, which trains police on how to de-escalate encounters with the public. And the Police Department is now equipping all officers with less lethal weapons, including bean bag guns and pepper ball guns. After the Clark shooting, the Police Department also changed its policies for chasing suspects to further emphasize officer and public safety” (Boudreau, MacKenzie and Simmons, 2019b, 5).
The Original Analysis

The authors assume the following likelihood function:

$$L(\text{Parameters}|\text{Rankings of } J \text{ Items by } N \text{ Respondents}) \propto \prod_{i=1}^{N} \prod_{j=1}^{J-1} \frac{\exp(\alpha_{ij})}{\sum_{j \in S} \exp(\alpha_{ij})}$$

(B.1)

with interactive terms of unit and item-specific covariates for support parameter $\alpha_{ij}$:

$$\alpha_{ij} = 3 \sum_{j=1}^{3} \gamma_j Z_j + 3 \sum_{j=1}^{3} \sum_{t=1}^{2} \beta_{jt} Z_j X_{it} + 3 \sum_{j=1}^{3} \sum_{s=1}^{2} \zeta_j S_{is} + 3 \sum_{j=1}^{3} \sum_{t=1}^{2} \sum_{s=1}^{2} \delta_{jts} Z_j X_{it} S_{is} + \epsilon_{ij},$$

(B.2)

where $Z_j$ is an item specific binary covariate that takes 1 if item $j$ is either “Police” ($j = 1$), “Local officials” ($j = 2$), or “State or federal officials” ($j = 3$), $X_{it}$ is a binary variable for each of the treatment conditions that takes 1 if unit $i$ is in the pattern-of-violence treatment group ($X_{i1}$) and the reform treatment group ($X_{i2}$). The model also includes a unit specific binary covariate $S_{is}$ that takes 1 if unit $i$ is a resident in Sacramento Country (where the Clark shooting occurred) ($S_{i1}$) or African American ($S_{i2}$). Here, $j \in S$ in Equation (B.1) represents the set of remaining items following the conventional multinomial logit model.

After estimating the model, the authors compute several quantities of interest by combining the estimated parameters. For example, using $\hat{\gamma}_1 = 1.06$ (the coefficient on the police officers dummy), $\hat{\beta}_{11} = 0.460$ (the coefficient on the interaction of the police officers and the pattern-of-violence treatment dummies), and $\hat{\beta}_{12} = -0.269$ (the coefficient on the interaction of the police officers and the reform treatment dummies), the original study reports that the “likelihood” that non-Sacramento residents who are not African American rank the police officers higher than the victim (i.e., blame more on the police officers than on the victim) is $\hat{\gamma}_1 = 1.06$ in the control group, $\hat{\gamma}_1 + \hat{\beta}_{11} = 1.52$ in the pattern-of-violence group, and $\hat{\gamma}_1 + \hat{\beta}_{12} = 0.79$ in the reform group, respectively. Finally, the study claims that the causal effects of the two treatments for this particular subpopulation are $1.52 - 1.06 = 0.46$ (which is $\hat{\beta}_{11}$ by definition) and $0.79 - 1.06 = -0.27$ (which is $\hat{\beta}_{12}$ by definition), respectively (Boudreau, MacKenzie and Simmons, 2019, 1106).

Table B.1 shows the estimated coefficients for non-Sacramento non-African American respondents, for which the authors find supportive evidence for their hypotheses. Columns 5-6 show the estimated effects of the two treatments for three chosen categories while setting the victim as a reference category. Based on these results, the original study suggests that, for example, the pattern-of-violence treatment “increased the likelihood” that the respondents rank the police officers higher (i.e., blame them more) than the victim by 0.46, which is statistically significant at the 0.05 level. In contrast, the reform treatment “decreased the likelihood” that the respondents rank the police officers higher than the victim by 0.27 at the same significance level (Boudreau, MacKenzie and Simmons, 2019, 1106). Taken together, the original study concludes that it finds supportive evidence for its two hypotheses for this specific subpopulation (but not for other subpopulations).

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1The details of the original specification are available in the Online Appendix (pages 20-24) of Boudreau, MacKenzie and Simmons (2019). Mean rankings by the group are also reported in Table A.1 in their Online Appendix while the authors do not refer to the table in their main text and these tests are not motivated by any causal quantity of interest.

2The original study seems to misreport that the results are for “non-Sacramento respondents” as opposed to non-Sacramento non-African American respondents. However, I focus on its modeling strategy rather than casting doubt on its conclusion based on this misreporting.
### C Additional Discussion on Causal Estimands

#### Causal Estimands Degenerate Into Difference-in-Means Estimands for Binary Outcomes

**Average Rank Effects**

To demonstrate, I use $Y_i \in \{0, 1\}$ to denote a binary outcome and $Y_i(1)$ and $Y_i(0)$ to represent its potential outcomes under the treatment and control conditions, respectively. A conventional average treatment effect measure is:

$$\tau \equiv E[Y_i(1) - Y_i(0)]$$ (C.1)

Now consider that the binary choice is generated by rankings of two items $j$ and $k$ (e.g., NO and YES), where marginal rankings are $R_{ij} = \{1, 2\}$. Without changing any substantive meaning, consider a slightly modified version of the marginal rankings $R_{ij}^* = \{0, 1\}$, where 0 means that item $j$ is preferred to item $k$ (e.g., NO) and 1 means otherwise (e.g., YES). Then, it is straightforward to see $E[Y_i] = E[R_{ij}]$ (the average number of YESs is equal to the average ranking of NO). Using this relationship, it can be shown that the ARE can be seamlessly related to the conventional effect measure:

$$\tau_1 = E[R^*_i(1) - R^*_i(0)]$$ (C.2a)
$$= E[R^*_i(1)] - E[R^*_i(0)]$$ (C.2b)
$$= E[Y_i(1)] - E[Y_i(0)]$$ (C.2c)
$$= E[Y_i(1) - Y_i(0)]$$ (C.2d)
$$= E[R_{ij} - R_{ij}]$$ (C.2e)

**Average Pairwise Rank Effects**

$$\tau_p \equiv E[1(A_{i,j} < A_{i,k})(1)] - 1(A_{i,j} < A_{i,k})(0)$$ (C.3a)
$$= E[Y_{j<k}(1) - Y_{j<k}(0)]$$ (C.3b)
$$= E[R_{ij}(1) - R_{ij}(0)]$$ (C.3c)
$$= \tau_{M,j}$$ (C.3d)

#### D Partial Identification for Rank Treatment Effects

**Nonparametric Sharp Bounds**

Sharp bounds on causal effects are determined by the minimum and maximum values in the support of the outcome variable ($\text{Supp}[Y_i(d)] \subseteq [\text{Min}, \text{Max}]$). Below, I consider such nonparametric bounds for AREs and
AEPs.

**Sharp Bounds for the ARE**

To present nonparametric bounds for AREs, I consider the minimum and maximum possible values for the marginal ranking $R_{ij}$. Since this work focuses on full rankings (as opposed to partial rankings), the support of the marginal ranking is a vector of $t$ numbers. Hence, $\text{Supp}(R_{ij}) \subseteq \{1, ..., t\} \subseteq [1, J]$.

$$
\tau_j \in \left[ \mathbb{E}[R_{ij} | D_i = 1] \mathbb{P}(D_i = 1) + \mathbb{P}(D_i = 0) - (\mathbb{E}[R_{ij} | D_i = 0] \mathbb{P}(D_i = 0) + t \mathbb{P}(D_i = 1)) \right],
$$

$$
(\mathbb{E}[R_{ij} | D_i = 1] \mathbb{P}(D_i = 1) + t \mathbb{P}(D_i = 0) - (\mathbb{E}[R_{ij} | D_i = 0] \mathbb{P}(D_i = 0) + \mathbb{P}(D_i = 1)) \right],
$$

(D.1a)

(D.1b)

**Sharp Bounds for the APE**

Next, partial identification for the APE is straightforward as it is based on a set of indicator functions. Since the support of indicator functions is $\text{Supp}[I(R_{ij} < R_{ik})(d)] \subseteq \{0, 1\} \subseteq [0, 1]$, the sharp bounds for the APE is:

$$
\tau_{jk}^p \in \left[ \mathbb{P}(R_{ij} < R_{ik} | D_i = 1) \mathbb{P}(D_i = 1) - (\mathbb{P}(R_{ij} < R_{ik} | D_i = 0) \mathbb{P}(D_i = 0) + \mathbb{P}(D_i = 1)) \right],
$$

(D.2a)

$$
(\mathbb{P}(R_{ij} < R_{ik} | D_i = 1) \mathbb{P}(D_i = 1) + \mathbb{P}(D_i = 0)) - \mathbb{P}(R_{ij} < R_{ik} | D_i = 0) \mathbb{P}(D_i = 0)
$$

(D.2b)

### E Results for Simulation Studies

**Standard Errors**

Accordingly, I consider the sampling variance of the estimated ARE for item $j$ as follows:

$$
\mathbb{V} (\hat{\tau}_j) = \frac{S_t^2}{\sum_{i=1}^{N} D_i} + \frac{S_c^2}{\sum_{i=1}^{N} (1 - D_i)} - \frac{S_{tc}^2}{N},
$$

(E.1)

where $S_t^2 = \frac{1}{N-1} \sum_{i=1}^{N} (R_{ij}(1) - \bar{R}_{ij}(1))^2$, $S_c^2 = \frac{1}{N-1} \sum_{i=1}^{N} (R_{ij}(0) - \bar{R}_{ij}(0))^2$, and $S_{tc}^2 = \frac{1}{N-1} \sum_{i=1}^{N} (R_{ij}(1) - R_{ij}(0) - \tau_j)^2$. Here, $\bar{R}_{ij}(1)$ and $\bar{R}_{ij}(0)$ represent the average potential mean ranks for item $j$ under the treatment and control, respectively. To obtain the estimated variance of the estimator for item $j$, I adopt the Neyman estimator of the sampling variance (Imbens and Rubin, 2015, 96):

$$
\widehat{\mathbb{V}} (\hat{\tau}_j) = \frac{s_t^2}{\sum_{i=1}^{N} D_i} + \frac{s_c^2}{\sum_{i=1}^{N} (1 - D_i)},
$$

(E.2)

where $s_t^2 = \frac{\sum_{i=1}^{N} D_i (R_{ij} - \bar{R}_{ij})^2}{\sum_{i=1}^{N} D_i - 1}$ and $s_c^2 = \frac{\sum_{i=1}^{N} (1 - D_i)(R_{ij} - \bar{R}_{ij})^2}{\sum_{i=1}^{N} (1 - D_i) - 1}$ are sample variances for sample mean ranks among the treated and control units, respectively, whereas $\bar{R}_{tj}$ and $\bar{R}_{cj}$ are mean ranks of item $j$ for the treated and control units.

Likewise, the sampling variance of the APE for items $j$ and $k$ is

$$
\mathbb{V} (\hat{\tau}_{jk}) = \frac{S_t'^2}{\sum_{i=1}^{N} D_i} + \frac{S_c'^2}{\sum_{i=1}^{N} (1 - D_i)} - \frac{S_{tc}'^2}{N},
$$

(E.3)

where $S_t'^2 = \frac{1}{N-1} \sum_{i=1}^{N} (I(R_{ij} < R_{ik})(1) - \bar{R}_{jk}(1))^2$, $S_c'^2 = \frac{1}{N-1} \sum_{i=1}^{N} (I(R_{ij} < R_{ik})(0) - \bar{R}_{jk}(0))^2$, and $S_{tc}'^2 = \frac{1}{N-1} \sum_{i=1}^{N} (I(R_{ij} < R_{ik})(1) - I(R_{ij} < R_{ik})(0) - \hat{\tau}_{jk})^2$. 

6
and $S_{tc}^2 = \frac{1}{N-1} \sum_{i=1}^N \left( R_{ij}(1) - R_{ij}(0) - \tau_j \right)^2$. Here, $\overline{R}_{jk}(1)$ and $\overline{R}_{jk}(0)$ represent the probability that item $j$ is preferred to item $k$ in the treatment and control groups, respectively. A corresponding estimator of this sampling variance is:

\[ \hat{V}(\hat{\tau}_{jk}) = \frac{s_t^2}{\sum_{i=1}^N D_i} + \frac{s_c^2}{\sum_{i=1}^N (1 - D_i)}, \]

(E.4)

where $s_t^2 = \frac{\sum_{i=1}^N D_i (I(R_{ij} < R_{ik}) - \overline{R}_{tk})^2}{\sum_{i=1}^N D_i - 1}$ and $s_c^2 = \frac{\sum_{i=1}^N (1 - D_i) (I(R_{ij} < R_{ik}) - \overline{R}_{ck})^2}{\sum_{i=1}^N (1 - D_i) - 1}$ are sample variances for sample mean ranks among the treated and control units, respectively, whereas $\overline{R}_{tk}$ and $\overline{R}_{ck}$ are the sample proportion that item $j$ is ranked higher than item $k$ in the treatment and control, respectively.

Future research must examine the population and sample variances for the between-population diversity estimator based on the chi-squared approximation (Alvo and Philip, 2014, 68).

**Plausibility of Normal Approximation of Treatment Effects**

Here, I present simulation results for the empirical distribution of the proposed treatment effects for ranked outcomes. Figure E.1 shows the distribution of null population average rank effects (ARE) for four items with scale parameters (0.5, 0.3, 0.2, 0.1). In this simulation, I draw 2000 samples from a Plackett-Luce model with the specified scale parameters for the quasi-treatment and quasi-control groups. I then compute the ARE using the samples from the two groups and repeat this process 3000 times. Finally, I divide the obtained AREs by their sample standard deviation. Each panel shows that the empirical distribution of a null ARE seems to be well approximated by a standard normal density function, which is represented by a red curve.

![Figure E.1: Distribution of Null population average rank effects for Four Items](image-url)
Figure E.2: Distribution of Null average pairwise rank effects for Four Items

F Additional Discussions on Multiple Hypothesis Testing

The BH method takes the following steps. First, compute p-values for the J hypothesis tests and sort the obtained p-values by their size: \( p(1) < \cdots < p(m) \), where \( p(j) \) is the \( j \)-th smallest p-value and \( m \) being the highest p-value. Next, with a pre-specified level \( \alpha \) (e.g., researchers may pre-specify \( \alpha = 0.05 \) to have the false discovery rate less than 5%), define an anchor value \( l_i = \frac{\alpha}{\sum_{i=1}^{m}(1/i)m} \) for non-independent p-values (Wasserman, 2004, 167), and set the number of total rejections to the maximum number of tests for which their p-values are lower than their corresponding anchors: \( r = \max \{ i : p_i < l_i \} \). Finally, define the BH rejection threshold \( T = p(r) \) and reject all null hypotheses \( H_0i \) for which \( p_i \leq T \) (or simply reject \( r \) null hypotheses with the \( r \) smallest p-values).

Let \( m_0 \) be the number of null hypotheses that are true. Let \( U \) be the number of null hypotheses that are true and not rejected, \( T \) be the number of null hypotheses that are false and yet not rejected (false discovery), and \( V \) be the number of null hypotheses that are true and yet rejected (false rejection). Suppose that there are \( m \) hypothesis tests and \( r \leq m \) tests in which the null is rejected. I then define the false discovery proportion (FDP) as \( \text{FDP} = \frac{V}{r} \). In words, the FDR is a proportion of false rejection among \( r \) rejected hypotheses. When \( R = 0 \), the FDR is defined to be 0. This is considered to be an unbiased estimator of the false discovery rate (FDR). The BH method controls the FDR via \( \text{FDR} = \mathbb{E}[\text{FDP}] \leq \frac{m_0}{m} \alpha \leq \alpha \).

G Additional Findings and Discussions on Police Violence

In this section, I replicate my primary analysis in the main text.

Multiple Hypothesis Testing for the Reform Treatment

So far, the reanalysis has focused on the statistical significance of each estimated effect based on its (estimated) 95% confidence interval. Nevertheless, it is critical to perform multiple hypothesis testing to test the authors’ hypotheses, considering all seven estimated effects. To do so, I first clarify multiple null hypotheses (that I hope to reject) based on Boudreau, MacKenzie and Simmons (2019). For example, for the pattern-of-violence
treatment, I consider the following composite hypothesis:

\[
H_{0v} : \tau_v \leq 0 \quad \text{(not less blame for victim)}
\]
\[
H_{0g} : \tau_g \leq 0 \quad \text{(not less blame for governor)}
\]
\[
H_{0s} : \tau_s \leq 0 \quad \text{(not less blame for senators)}
\]
\[
H_{0o} : \tau_o \geq 0 \quad \text{(not more blame for officers)}
\]
\[
H_{0c} : \tau_c \geq 0 \quad \text{(not more blame for chief)}
\]
\[
H_{0m} : \tau_m \geq 0 \quad \text{(not more blame for mayor)}
\]
\[
H_{0d} : \tau_d \geq 0 \quad \text{(not more blame for district attorney)}
\]

Second, with the above hypothesis, I compute seven \(p\)-values at the significance level of \(\alpha = 0.05\) based on the one-tailed test. To illustrate, I first estimate, for example, the effect for the victim as \(\hat{\tau}_v = 0.172\) with its standard error \(\hat{\sigma}_v = 0.238\). With these values, I then compute a Wald statistic \(w_v = \frac{\hat{\tau}_v - \theta_0}{\hat{\sigma}_v} = \frac{0.172}{0.238} = 0.701\), where \(\theta_0 = 0\) is the null target value. Using the normal approximation to the distribution of the Wald statistic, I then compute the \(p\)-value for the victim as \(1 - P(Z > w_v) = 0.242\). Following the same procedure, I compute the \(p\)-values of all estimated effects (from \(H_{0v}\) to \(H_{0d}\)).

Third, I sort these \(p\)-values from the smallest to the largest: (0.000, 0.076, 0.204, 0.218, 0.242, 0.347, and 0.441) as shown in Table G.1. As described earlier, I then compute a vector of anchor values \(l = (0.007, 0.01, 0.012, 0.014, 0.016, 0.018, \text{and } 0.019)\). This yields the number of total rejections \(r = \max \{i : p_i < l_i\} = 1\), which in turn determines the BH rejection threshold \(T = p(r) = 0.0004\) (the one smallest \(p\)-value in the sorted list). Finally, I reject all null hypotheses \(H_0\) for which \(p_i \leq T\). Consequently, I reject the null hypothesis \(H_{0m}\) but not the other six null hypotheses. Substantively, this means that I find supporting evidence only for the mayor, which casts doubt on the original finding about the first hypothesis. Table G.1 presents the full list of statistics for the multiple testing. Table G.2 shows the results for the reform treatment.

| Null   | Party    | \(\hat{\tau}\) | \(\hat{\sigma}\) | Wald \((\hat{\tau}/\hat{\sigma})\) | \(p\)-value | Threshold \((T)\) | Reject if \(p \leq T\) |
|--------|----------|-----------------|-----------------|-----------------|-------------|-----------------|-------------------------|
| \(H_{0m}\) | Mayor    | -0.138          | 0.033           | -4.869          | 0.000       | 0.000           | Reject                  |
| \(H_{0c}\) | Chief    | -0.087          | 0.060           | -1.433          | 0.076       | 0.000           | Not reject              |
| \(H_{0s}\) | Senators | 0.064           | 0.083           | 0.826           | 0.204       | 0.000           | Not reject              |
| \(H_{0d}\) | DA       | 0.035           | 0.047           | 0.778           | 0.218       | 0.000           | Not reject              |
| \(H_{0v}\) | Victim   | 0.172           | 0.238           | 0.701           | 0.242       | 0.000           | Not reject              |
| \(H_{0g}\) | Governor | -0.030          | 0.080           | -0.394          | 0.347       | 0.000           | Not reject              |
| \(H_{0o}\) | Officers | -0.016          | 0.112           | -0.149          | 0.441       | 0.000           | Not reject              |

Table G.1: Multiple Hypothesis Testing for the Pattern-of-Violence Treatment

*Note:* This table presents a series of statistics required for and the result for the Benjamini-Hochberg method of multiple hypothesis testing. In this reanalysis, \(\alpha = 0.05\) and \(\theta_0 = 0\).
Table G.2: **Multiple Hypothesis Testing for the Reform Treatment**

*Note:* This table presents a series of statistics required for and the result for the Benjamini-Hochberg method of multiple hypothesis testing. In this reanalysis, $\alpha = 0.05$ and $\theta_0 = 0$.

**Average Rank Effects by Race and Ethnicity**
Average Pairwise Rank Effects by Race and Ethnicity

Figures G.2-G.5 visualize the estimated PREs for each of the racial and ethnic groups.
Figure G.2: Average Rank Effects of the Pattern-of-Violence and Reform Treatments for Whites

Figure G.3: Average Rank Effects of the Pattern-of-Violence and Reform Treatments for Blacks
Figure G.4: Average Rank Effects of the Pattern-of-Violence and Reform Treatments for Latinos

Figure G.5: Average Rank Effects of the Pattern-of-Violence and Reform Treatments for Asians
Racial Differences in Blame Attribution in Police Violence

Figure G.6 visualizes the estimated average rank (with bootstrapped 95% confidence interval) of each party for each of the four groups (analyzed in the original study). The figure shows that while all groups think that the two police officers are the most responsible for the victim’s death among the seven parties, a stark racial difference appears in their blame attribution for the victim. Within both treatment conditions, whites think that the victim is responsible for his death at the almost same level as the police officers, whereas blacks believe that the victim is one of the most minor responsible parties in the four counties. Both panels show that Latinos are closer to blacks, whereas Asians hold a similar position to whites regarding the victim. This finding suggests that the way people hold the police force accountable, at least in the context of officer-involved shootings, significantly differs by race and ethnicity, which underpins the importance of grand jury selection in police violence cases.

Figure G.6: Mean Ranks of Seven Parties by Race

Note: Each panel shows the estimated average ranks (with 95% confidence intervals based on bootstrapping) for whites, blacks, Latinos, and Asians for the seven parties of interest, from the victim to the senators, within the same experimental condition. "1" in the y-axis means that a party is the most responsible (ranked first) for the shooting. The upper panel displays the result for the subjects in the control condition, whereas the lower panel shows the result for the respondents in the reform treatment group.
H Identification of Order-Ranker Average Rank Effects

First, always-rankers is a set of voters who always rank candidate $j$ regardless of the treatment assignment. Second, order-rankers is a group of voters who rank candidate $j$ only when they are in the active treatment condition. Third, never-rankers mean voters who never rank candidate $j$ regardless of the treatment assignment. Finally, defiers is a set of voters who only rank candidate $j$ when they do not see the candidate at the top of the ballot.

Let $S \in \{\text{order-rankers}, \text{always-rankers}, \text{never-rankers}, \text{defiers}\}$ denote a stratum membership and $s$ denote a specific type of voters in $S$. Let $\pi_o$, $\pi_a$, $\pi_n$, $\pi_d$ be the proportion of order-rankers, always-rankers, never-rankers, and defiers, respectively. Here, the candidate index $j$ is suppressed for clarity.

To identify the order-ranker ARE, I first rewrite the ARE as a weighted average of strata-specific AREs:

$$\tau_j = \mathbb{E}[\tau_j | S = s] = \pi_o \tau_{j,o} + \pi_a \tau_{j,a} + \pi_n \tau_{j,n} + \pi_d \tau_{j,d}$$

Next, to simply the above identity, I introduce two standard assumptions:

Assumption A1 (Ranking Monotonicity). Voter $i$ is at least as likely to rank candidate $j$ when the candidate appears at the top of ballot than otherwise. Formally, $M_{ij}(0) \leq M_{ij}(1)$ for all $i$ and $j$ with probability 1. Equivalently, $\pi_d = 0$.

Ranking monotonicity means that ballot order ($D_{ij}$) has a positive effect on whether voters rank candidate $j$ ($M_{ij}$), and it rules out the presence of defiers (but also see Alvarez, Sinclair and Hasen, 2006). This is a “refutable” assumption in that its plausibility can be tested by observed data (Manski, 2009, 46-48).

Assumption A2 (Exclusion Restriction). Ballot order affects candidate ranking only through its effect on voters’ decision to rank a given candidate. Consequently, the ARE is zero for always-rankers and never-rankers; $\tau_a = \tau_n = 0$.

Exclusion restriction was suggested by Figure 4, where there is no direct causal path from ballot order ($D_{ij}$) to candidate ranking ($R_{ij}$). Because of this, ballot order ($D_{ij}$) can be considered as an instrumental variable with respect to voters’ ranking decision ($M_{ij}$) (Angrist, Imbens and Rubin, 1996). Exclusion restriction will be violated if always-rankers (never-rankers) put a higher (lower) rank on a given candidate because they saw the candidate at the top of the ballot. I justify this assumption in the running application because always-rankers and never-rankers engage in “ticket voting,” in which “a complete preference ordering could be expressed simply by adopting a party’s pre-arranged preference schedule” (Reilly, 2001, 125).

When Assumptions A1-A2 hold, it is then possible to simplify the ARE as follows:

$$\tau_j = \tau_{j,o} \pi_o + \tau_{j,a} \pi_a + \tau_{j,n} \pi_n + \tau_{j,d} \pi_d$$

Rearranging the above, the order-ranker ARE can be expressed as follows:

$$\tau_{j,o} = \frac{\tau_j}{\pi_o}$$
where $\pi_o$ can be expressed as the difference in the proportions of voters who rank candidate $j$ in the treatment and control conditions:

$$
\pi_o = \frac{\mathbb{P}(M_{ij} = 1|D_{ij} = 1) - \mathbb{P}(M_{ij} = 1|D_{ij} = 0)}{\text{Prop. of order-rankers and always-rankers}} - \frac{\mathbb{P}(M_{ij} = 1|D_{ij} = 0)}{\text{Prop. of always-rankers}} 
$$

(H.4)

Thus, the violation of Assumptions A1-A2 identifies $\tau_{j,o}$ larger than its true value (and the corresponding estimator becomes positively biased).

I Discussion on Simulation

Set Up

Based on the Luce-Plackett model of ranking data, I assume that each voter has a random utility for each of the six candidates: $V_{A,i} = 2\epsilon_A + \epsilon_A$, $V_{B,i} = 3\epsilon_B + \epsilon_B$, $V_{C,i} = \epsilon_C + \epsilon_C$, $V_{D,i} = 3\epsilon_D + \epsilon_D$, $V_{E,i} = 3\epsilon_E + \epsilon_E$, and $V_{F,i} = 4\epsilon_F + \epsilon_F$, where $\epsilon_j \sim N(0, 1)$ and $\epsilon_j \sim N(0, 1)$, respectively. I then define that $V_{A,i} = 2\epsilon_A + \epsilon_A + \tau$ in the treatment group and $V_{B,i} = 3\epsilon_B + \epsilon_B + \tau$ in the control group, where $\tau = 2$. Building on these utility functions, I then generate $M_j$ by simulating a utility threshold for each voter. For each voter, I draw a value from $N(0, 1)$ and replace each candidate’s utility with 0 if the candidate’s utility is less than the voter’s utility threshold. This represents a behavioral assumption that voters only rank candidates whose utilities are greater than or equal to their internally defined utility benchmark. They first choose which candidates they decide to rank and then rank the candidates. Finally, I generate $R_j$ by applying the Plackett-Luce model of ranking data, where one candidate is sequentially chosen from a set of remaining candidates. Importantly, the utilities of unranked candidates are set to 0 and thus do not affect the ranking of each ranked candidate.

Simulation Study 1

In Figure I.1, I vary the number of experimental subjects and the distribution of voters’ utilities over available candidates. Here, I fix the number of candidates subjects can rank (i.e., they can express up to their top-3 choice). There are several takeaways. First, the comparison between Panel A ($N = 250$) and Panel B ($N = 2500$) shows that sample size barely affects the width of the bound. The same pattern appears in the comparison between Panel C ($N = 250$) and Panel D ($N = 2500$). Second, the bound width is highly susceptible to the distribution of candidate utilities. The pattern is confirmed by comparing Panel A and Panel C and comparing Panel B and Panel D. This fits the logic behind the nonparametric bound: the more missing ranks a candidate has, the wider the bound for the candidate becomes (because we have less information we can rely on in the data). Consequently, when voters’ support is concentrated around a few candidates, we have more information and narrower bounds for the popular candidates. At the same time, this suggests that we have less information and wider bounds for the remaining candidates.

Simulation Study 2

In Figure I.2, I vary the number of candidates voters can rank up to and the distribution of voters’ utilities over available candidates. While varying the two parameters, I fix the number of experimental subjects to $N = 250$. The comparisons between Panel A and Panel B, as well as Panel C and Panel D, show that the bound width is susceptible to how many candidates voters can rank. This pattern fits the logic behind the nonparametric bound: the more non-missing ranks we have, the narrower the bound becomes.

---

1This estimand can be thought of the two stage least square estimand in the context of instrumental variables.
Figure I.1: Simulation Results by Sample Size and Distribution of Candidate Utilities

Figure I.2: Simulation Results by Maximum Number of Ranked Candidates and Distribution of Candidate Utilities
J Identification and Estimation of Generalized Ballot Order Effects

First, I present a more general experiment design for quantifying ballot order effects. Let \( O_{i,j} \) be the position of candidate \( j \) that voter \( i \) sees in the ordered set of candidates. For example, \( O_{i,j} = 1 \) means that candidate \( j \) is the first listed candidate, and \( O_{i,j} = J \) suggests that the candidate is in the last position (for voter \( i \)). Let \( O_{i[-j]} \) be a random vector denoting the ordering of the other \( J - 1 \) candidates in the set, and \( o^* \) be a realization of the random vector in the permutation space of \( O^{J-1} \).

In the above example, I considered the effect of candidate \( j \) being listed in the first position as opposed to the second position relative to a particular ordering of other candidates. Formally, this effect can be written as:

\[
\tau_{BO}^{ij[2]}(o^*) \equiv \mathbb{E} [R_{ij}(O_{ij} = 1) - R_{ij}(O_{ij} = 2)|O_{i[-j]} = o^*]
\]  

(J.1)

More generally, I define the average ballot order effect for candidate \( j \) as the average rank effect of being listed first \((O_{ij} = t_1)\) as opposed to any other position averaged over the joint probability distribution of (1) the other positions (for all \( t \)) and (2) the ordering of other candidates (for all \( o^* \)): \(^1\)

\[
\tau_{BO}^j \equiv \mathbb{E} [R_{ij}(D_{i,j} = 1) - R_{ij}(D_{i,j} = 0)]
\]

(J.2)

\[
= \mathbb{E} \left[ \mathbb{E} \left[ R_{ij}(O_{ij} = t_1, O_{i[-j]} = o^*) - R_{ij}(O_{ij} = t, O_{i[-j]} = o^*) | O_{i[-j]} = o^* \right] \right]
\]

(J.3)

\[
= \sum_{o^* \in O^{J}} \left\{ \sum_{t=2}^{J} \mathbb{E} \left[ \begin{array}{c}
R_{ij}(O_{ij} = t_1, O_{i[-j]} = o^*) - R_{ij}(O_{ij} = t, O_{i[-j]} = o^*) \\
\text{potential rank under the treatment} \\
\text{potential rank under the control} \\
\text{Ordering of other candidates}
\end{array} \right] \right\}
\]

(J.4)

\[
\times \frac{\mathbb{P}(t_1, t)|O_{i[-j]} = o^*}{\text{conditional probability of candidate } j \text{ listed in 1st and } t\text{th positions}}
\]

\[
\times \frac{\mathbb{P}(O_{i[-j]} = o^*)}{\text{probability of a particular ballot order } o^*}
\]

Finally, I make an additional assumption to simplify the identification of the average ballot order effect. Specifically, I assume that it is equally likely that each voter sees a particular comparison between \( O_j = t \) and \( O_j = t_1 \) as well as a particular ordering of other candidates \( O_i = o^* \). Since there are \( J - 1 \) possible numbers of pairwise comparisons and \((J - 1)!\) possible numbers of candidate permutation, I introduce the following:

**Assumption A3 (Ballot Order Randomization).** Ballot order is randomized at the voter level such that \( \mathbb{P}(R_{ij}(O_j = 1, O_{i,-j} = o^*) - R_{ij}(O_j = t, O_{i,-j} = o^*)|O_{i,-j} = o^*) = \frac{1}{J-1} \) and \( \mathbb{P}(O_{i,-j} = o^*) = \frac{1}{(J-1)!} \). Here, it is also assumed that both probabilities are greater than zero and less than one.

With this assumption (and assuming full rankings), I define the following:

**Proposition A1 (Nonparametric Estimator for the General Ballot Order Effect).** Given Assumptions 1-7, the average ballot order effect for candidate \( j \) is estimated by the following doubly-averaged-difference-in-mean-

\(^1\)More generally, \( t_1 \) can be defined as any combination of multiple positions (e.g., first and second positions combined).
ranks estimator:

\[
\hat{\tau}_{j}^{BO} = \hat{E}[R_{ij}|D_{i,j} = 1, O_{i} = o^*] - \hat{E}[R_{ij}|D_{i,j} = 0, O_{i} = o^*] \tag{J.5}
\]

\[
= \frac{1}{(J - 1)!} \sum_{o^* \in O^{J-1}} \frac{1}{J - 1} \sum_{t=2}^{J} \left\{ \hat{E}[R_{ij}|O_{j} = 1, O_{i} = o^*] - \hat{E}[R_{ij}|O_{j} = t, O_{i} = o^*] \right\} \tag{J.6}
\]

The above quantity can be estimable by the within-strata difference-in-mean ranks as well as by ordinary least square regression. One advantage of this design is that it allows researchers to decompose the general ballot order effect both by (a) the position of comparison and (b) the ordering of other candidates. Meanwhile, one challenge is that the number of possible treatment-control comparisons is quite large. In the running application, for example, there are \((J - 1)! (J - 1) J = 8! \times 8 \times 9 = 2903040\) such comparisons for ten candidates.

One way to regularize the number of possible strata is to assume a constant rank order effect by the remaining ballot order \(O_{i[-j]}\) as well as candidate \(j\)'s counterfactual position \(t\).

**Assumption A4 (Mean Independence on Remaining Ballot Order).** \{\(E[R_{ij}(O_{ij} = t_1)], E[R_{ij}(O_{ij} = t)]\}\} \(\perp O_{i[-j]}\) for all \(i, j, t, j, t\).

**Assumption A5 (Mean Independence on Counterfactual Position).** \(E[R_{ij}(O_{ij} = t)] = E[R_{ij}(O_{ij} = t^*)]\) for all \(t \neq t_1\) and \(t^* \neq t_1\).

These assumptions allow one to pool the information from different treatment-control comparison groups. The validity of each assumption can be empirically tested by estimating (and comparing) the ballot order effect within each stratum. With these assumptions, it is possible to simplify the estimator further.

**Proposition A2 (General Ballot Order Effect via Position and Ordering Mean Independence).** Given Proposition 2 and Assumptions 8-9, the average ballot order effect for candidate \(j\) is estimated by the following difference-in-mean-ranks estimator:

\[
\hat{\tau}_{j}^{BO} = \frac{R_{ij} I(O_{ij} = t_1)}{N_{j1}} - \frac{1}{J - 1} \sum_{t=2}^{J} \left( \frac{R_{ij} I(O_{ij} = t)}{N_{jt}} \right) - \frac{R_{ij} I(O_{ij} \neq t_1)}{\sum_{t=2}^{J} N_{jt}} \tag{J.7}
\]

\[
= \frac{R_{ij} I(O_{ij} = t_1)}{N_{j1}} - \frac{\sum_{t=2}^{J} R_{ij} I(O_{ij} \neq t_1)}{\sum_{t=2}^{J} N_{jt}} \tag{J.8}
\]

where \(N_{j1} = \sum_{i=1}^{N} I(O_{ij} = t_1)\) be the number of treated units (in which candidate \(j\) appears at the top of the ballot) and \(N_{jt} = \sum_{i=1}^{N} I(O_{ij} = t)\) be the number of control units (who see candidate \(j\) in the \(t\)-th position of the ballot) such that \(N = N_{j1} + \sum_{t=2}^{J} N_{jt}\).

Finally, in the presence of partial rankings, researchers can construct the nonparametric bounds by applying the estimator to the two imputed samples.
K Survey Experiment Questions

This section presents the copies of survey instruction and questions for the Oakland 2022 mayoral election. The same set of instructions and questions were used for the US House and US Senate elections in Alaska.

Perfect!

Next, we would like to ask you about the upcoming Oakland mayoral election on November 8th, 2022.

This election will be held using the electoral system known as ranked-choice voting. This means that you can express your opinions by ranking multiple candidates.

We want to better understand how people like you would vote in the upcoming Oakland mayoral election.

When you are ready, please move on to the next page.

Figure K.1: Instruction for Experimental Questions

Figure K.2: Attention Check prior to Experimental Questions

Note: The other three attention checks include “Do you agree to participate?” (Yes or No), “For our research, careful attention to survey questions is critical! We thank you for your care.” (I understand or I do not understand), and “People are very busy these days and many do not have time to follow what goes on in the government. We are testing whether people read questions. To show that you’ve read this much, answer both “extremely interested” and “very interested.” (Extremely interested, Very interested, Moderately interested, Slightly interested, or Not interested at all). Attentive respondents are those who select all underlined items.
Oops! No, we actually talked about ranked-choice voting.

Now, here is how you can vote under ranked-choice voting. Please read the following note and instructions carefully before moving on.

If you wish, **you do not need to rank all candidates**.

---

In our example, you are required to rank all candidates in the Oakland mayoral election.

Please **rank all candidates** according to your preference.

Notice that the **order of candidates was shuffled**.

| Rank | Candidate Name                          |
|------|----------------------------------------|
| 1    | Gregory Rudge (Non-Profit Executive)   |
| 2    | Ignacio de la Pena (Business Solution Strategist) |
| 3    | Seneca Scott (Small Business Owner)    |
| 4    | John Raimond (Retired Carpenter)      |
| 5    | Shang Thao (Oakland City Councilmember) |
| 6    | Loren Manuel Taylor (Councilmember/Small Business Owner) |
| 7    | Peter Y Liu (Entrepreneur)             |
| 8    | Alyssa Villarueva (Civil Rights Attorney) |
| 9    | Tyron C. Jordan (Legal Assistant)      |
| 10   | Treva D. Reid (Councilmember/Senior Caregiver) |

Figure K.3: Post-Attention Check Instruction

Figure K.4: Secondary Experimental Question
L Additional Results on Ballot Order Effects

This section presents the experimental results for the U.S. House and Senate elections in Alaska.

Figure L.1 shows the results for the U.S. House and Senate elections in Alaska. The house election featured four candidates, including Mary Peltora (Democratic incumbent), Nicholas Begich (Republican), Sarah Palin (Republican), and Chris Bye (Libertarian). The senate race had four candidates, including Senator Lisa Murkowski (Republican Incumbent), Kelly Tshibaka (Republican), Patricia Chesbro (Democrat), and Buzz Kelley (Republican). The design reflects Alaska’s design that voters can rank as many or as few candidates as they wish.

![Figure L.1](image_url)

**Figure L.1: Nonparametric Ballot Order Effects in the 2022 U.S. House of Representatives Election in Alaska**

*Note:* Panels A-B report results for the U.S. House and Panel C-D present findings for the U.S. Senate elections, respectively.

In Panels A and C, the nonparametric bounds suggest that the ballot order (i.e., first position) does not give candidates higher ranks in both elections. The fully ranked data confirm this finding while showing that the ballot order effects are statistically significant for Peltola and Palin in the U.S. House election. Moreover, order-ranker AREs are identified for all candidates in the House race but only for one candidate in the Senate race. Panels B and D show why — respondents are more likely to rank Begich, Bye, Peltola, Palin, and Tshibaka, but less likely to rank Murkowski, Chesbro, and Kelly when they appear at the top of the ballot. Figure L.2 shows...
that most ballot order effects were statistically insignificant based on fully ranked data in Alaska.

Figure L.2: **Generalized Ballot Order Effects Across Positions in Alaska**

*Note:* Panel A is based on the U.S. House of Representatives election in Alaska. Panel B is based on the U.S. Senate election in Alaska.

Figure L.3: **The Distributions of Observed Rankings among Inattentive Respondents.**

*Note:* A dashed line shows the uniform distribution in each panel. Each panel shows the proportion of unique rankings based on partially (red) and fully ranked (blue) data. 95% confidence intervals are displayed.
M Applications in International Relations

Conflict-Related Sexual Violence

Agerberg and Kreft (2022) study whether people are more likely to support humanitarian intervention in armed conflicts when the case of interest involves sexual violence. The authors’ gendered protection norm theory states that conflict-related sexual violence “elicits support for intervention because it activates gendered protection norms grounded in benevolent sexist notions of women as innocent, vulnerable victims” (Agerberg and Kreft, 2022, 4).

In their vignette experiment, survey respondents from the U.S., the U.K., and Sweden were exposed to one of three conditions:

“A poor country has been ravaged by an increasingly violent civil war for the last seven years. After the president refused to step down following his defeat in the presidential election, rebel groups started challenging the government. The conflict soon spiraled out of control, [experimental manipulation]. Several diplomatic attempts at conflict resolution have been unsuccessful. The United Nations have strongly condemned the fighting, and the [widespread violence/widespread use of sexual violence/widespread ethnic violence].

Experimental manipulation

- Control: “resulting in widespread violence”
- Main treatment: “resulting in widespread violence with widespread use of sexual violence by all armed actors”
- Auxiliary treatment: “resulting in widespread ethnic violence”

After the treatment assignment, the authors measure two outcome variables. The secondary outcome variable is based on the following question:

“In societies emerging from conflict, international organizations, aid agencies and states promote and support a set of initiatives and projects. Yet, especially when facing budget constraints, international actors often disagree about the relative importance of different issues. In the conflict scenario described above, how would you rank the following priorities in order of importance, once the violence has ended? Rank the different priorities using number 1 to 5, with 1 being most important.”

- Job generation and employment initiatives
- Reconciliation activities between victims and perpetrators of violence
- Rebuilding schools and hospitals
- Including and empowering women in the peace process
- Improving governance and accountability

The original analysis (presented in their Online Appendix B) examines the treatment effects of the probability that the inclusion and empowerment of women is the most prioritized or \( P(R_{i,j}=1) \). I call this class of effects the average first-rank effects. The authors estimate these effects by first estimating logistic regression and
computing predicted probabilities that women’s inclusion and empowerment is selected as the most important item.

However, this strategy is sub-optimal given their hypotheses of interest, which concern the relative priority of the item over the other four items (emphasis added):

**H2a:** Respondents are **more likely to prioritize** women’s empowerment and active inclusion in the peace process after a conflict with widespread sexual violence compared to a conflict with overall widespread violence

**H2b:** Respondents are **more likely to prioritize** women’s empowerment and active inclusion in the peace process after a conflict with widespread sexual violence compared to a conflict with widespread ethnic violence.

To better test these hypotheses, I suggest that average rank effects be used for the reanalysis. Figure M.1 reports the estimated average rank effects for the five issues by the treatment type and across countries. The results support the original conclusion for H2a based on the average first-rank effects: the sexual violence treatment (versus the control) makes people more likely to prioritize women’s inclusion and empowerment in post-conflict regions only in the U.S. and the U.K. Moreover, the figure also shows that the sexual violence treatment (as opposed to the control) did not affect the average ranks of the other four items, except for reconciliation in the U.K. While the other four items were not examined in the original study, doing so provides additional support for H2a.

Regarding H2b, however, the reanalysis did not find statistically significant average rank effects of the sexual violence treatment (versus the ethnic violence treatment). While I find that the ethnic violence treatment (versus the control) does not affect the average rank of women’s inclusion and empowerment, the reanalysis implies that there is not enough evidence to confirm or deny H2b.

![Figure M.1: Average Rank Effects for Five Issues in Post-Conflict Regions](image-url)
Donor Political Economy and Aid Effectiveness

Dietrich (2016) examines whether the quality of governance of an aid-receiving country affects the aid decision-makers in an aid-providing country. To test her hypothesis, the author performs a vignette experiment on former or current aid officials from France, Japan, Germany, the U.S., and Sweden. In the experiment, each official was exposed to “three hypothetical low-income-country scenarios, which differ only in the quality of governance” (91). These three conditions include:

- Control (Country A): a well-performing low-income country with relatively low levels of corruption and strong state institutions.
- Treatment I (Country B): weak state institutions—while corruption levels are relatively low
- Treatment II (Country C): a large-scale corruption scandal involving public-sector officials—while state institutions are relatively strong

After exposing to each condition (the order of B and C was randomized), survey respondents are asked to “rank-order five aid delivery channels” (Dietrich, 2016, 91), including

- The recipient government
- International organizations
- International NGOs
- Local NGOs
- Private-sector actors

Although the study collected ranking data on the five items, the study only analyzed the outcome for the recipient government. The author hypothesizes by noting that “I expect officials from the United States and Sweden, on average, to change toward greater [support for using the recipient government as a bypass] than their counterparts from France, Japan, and Germany” (93). Importantly, the study uses the recipient government as the alternative to nonstate development actors. Thus, “If respondents rank the recipient government as a 5 it means that they have a clear NO BYPASS preference” (91).

To test this hypothesis, the original study performs a “difference-in-difference” analysis, where (1) it takes the difference between the ranks of the recipient government between the control group and each treatment condition and (2) takes another difference between the political economy type (0 for France, Japan, and Germany; 1 for USA and Sweden) after that. Importantly, the quantity of interest was not clearly defined and thus it is not entirely clear if the estimated “effects” support the above hypothesis.

To better test the author’s hypothesis, I estimate the conditional average rank effect for the recipient government by subsetting the sample based on the political economy type. Figure M.2 presents the estimated effects by the political economy type and the treatment conditions. The figure shows that both treatments lead to higher ranks (negative values) of the recipient government. Moreover, the effect size appears to be larger among aid officials from the US and Sweden than from France, Japan, and Germany, which provides more explicit evidence for the hypothesis.
However, to further provide evidence for the author’s argument, it is essential to examine whether the treatments cause people to prioritize the recipient government over each of the other delivery channels (international organizations, international NGOs, local NGOs, and private-sector actors). Especially, it is informative to study the **pairwise ranking of the recipient government and private-sector actors** given the argument in the original study. This way, the proposed causal inference framework with ranking data allows researchers to perform more analyses than they conventionally do without the framework.

![Figure M.2: Average Rank Effects for Recipient Government as a Preferred Delivery Channel](image)

**Impact of Economic Coercion on Support for Appreciation**

Gueorguiev, McDowell and Steinberg (2020) study whether economic coercion by the U.S. government affects public opinion — specifically support for appreciation — in China. In their survey experiment, respondents were exposed to the following information and one of the three additional explanations.

"An increase in the value of the RMB relative to other currencies makes imports cheaper whereas a decrease makes China’s exports more competitive in world markets. [Additional information]"

- Control: (No additional statement)
- Encouragement (Treatment I): “America has encouraged China to increase the value of the RMB relative to the dollar.”
- Threat (Treatment II): “America has threatened to impose taxes on its imports of Chinese-made goods if China does not increase the value of the RMB relative to the dollar.”

One of the core analyses is to estimate the average treatment effects on people’s support for appreciation after subsetting the full sample by how survey respondents perceive US-China currency relations. More specifically, after the experimental manipulation, respondents are asked to rank order the following statements:

- Adversaries: China and America are adversaries
- Competitors: China and America are cooperative competitors
- Partners: China and America are international partners
The authors then separately estimate the effect of each treatment by focusing on respondents who choose the “adversaries” option first and those who do not. However, since the rank-order question followed the experimental manipulation, it is critical to assess whether the treatment does not affect people’s rankings on the three statements. If, indeed, the treatment affects their rankings, it is likely that their original conclusion may be susceptible to post-treatment bias.

Motivated by this problem, I estimate the average first-rank effect \( P(R_{ij} = 1)(1) - P(R_{ij} = 1)(0) \) and average rank effect \( E[R_{ij}(1)] - E[R_{ij}(0)] \) of each treatment on the three options. The upper panel of Figure M.3 reports the estimated average first-rank effects of all three options. The results show that people’s rankings are not affected by the treatments, which suggests that post-treatment bias seems less likely. While the original study subsets the entire sample by using the “adversary” option, it is encouraging to confirm that the other two options do not get affected by either treatment.

What if each treatment affects their entire rankings while not affecting their first choice? To address this problem, I also estimate the average rank effect of each treatment for all options. The lower panel reports the estimated effects, which turn out to be all statistically and substantively not significant. Taken together, I find that the original analysis is robust to the above additional examination of the treatment effects on people’s rankings.

![Figure M.3: Average First-Rank and Average Rank Effects for Three Perceptions of US-China Currency Relations](image)
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