WORKING PAPERS

N° 1603

December 2024

"Who is Targeted, and By Whom? The Role of Distortions and Ethno-partisanship in Normative Evaluations of Clientelism"

Jeremy Horowitz, Giacomo Lemoli and Kristin Michelitch

Toulouse School of Economics

Who is Targeted, and By Whom? The Role of Distortions and Ethno-partisanship in Normative Evaluations of Clientelism*

Jeremy Horowitz

Center for Effective Global Action (CEGA),

UC Berkeley

Giacomo Lemoli

IAST,

Toulouse School of Economics

Kristin Michelitch

IAST and Social and Behavioral Science Department,

Toulouse School of Economics

December 2024

*IAST funding from the French National Research Agency (ANR) under grant ANR-17-EURE-0010 (Investissements d'Avenir program). We thank Nicole Audette for invaluable research assistance. This project would not have been possible without the help of our fantastic enumeration team led by Walter Ludenyo. We thank Andy Harris for sharing the ethnicity data used to design the sampling strategy. We thank LAPOP staff at Vanderbilt University for support with programming the survey and Lotje van der Linden for programming the Implicit Association Tests. We are grateful for funding from the Carnegie Foundation. IRB approval was received from Vanderbilt University (#170737) and Dartmouth College (#30291). Kenyan research permit NACOSTI/P/17/7385/17300. Pre-registration available at https://osf.io/sn4jf.

Abstract

Where political parties form around coalitions of ethnic groups, as in many sub-Saharan African democracies, political actors' favoritism toward their own supporters plays a prominent and normatively fraught role in electoral competition and public service delivery. However, little is known about how citizens normatively evaluate whether such "clientelistic behaviors" should be considered illegal and punishable. This study hypothesizes that citizens will desire greater punishment for clientelistic actions when (a) the behavior is more distortionary (e.g., targeting coethnics vs. copartisans vs. general people), and (b) the citizen holds opposing ethnopartisanship to the actor. We also posit a positive interaction between the two. Using a survey experiment conducted in Kenya (n=1,946) with Kikuyu and Luo respondents ahead of the 2017 national elections, we ask participants to assign punishment for various clientelistic behaviors. The results show that citizens systematically award more punishment when actors target their supporters rather than general people, with little difference between coethnic versus copartisan targeting. Citizens also punish actors more from the opposing ethnopartisanship, but there is no systematic interaction effect between the level of distortion and (un)shared ethnopartisanship.

The role of commonplace clientelism in low-income countries is normatively fraught, constituting a legal "gray area" (VandeWalle 2007). On the one hand, clientelism can offer citizens such benefits as electoral handouts (Kramon 2016), greater accountability for local public goods (Gottlieb, Larreguy, et al. 2020), or improved connection with government (Klaus et al. 2023). On the other hand, clientelism carries normative harms to society — distorting universal or needs-based access to state resources (Ejdemyr et al. 2018; Beiser-McGrath et al. 2021), stymieing economic development (Keefer and Khemani 2004; Robinson and Verdier 2013), undermining transparent formal procedures (Hicken 2011), and fueling corruption (Lindberg et al. 2022). In African politics, clientelism by party leaders to nested ethnic groups is also faulted as exacerbating ethno-partisan tensions and violence (Michelitch 2015; Klaus and Mitchell 2015; Fjelde and Höglund 2016), and destabilizing democracy as a whole (Horowitz 1985; Cederman et al. 2011). However, despite wide-ranging normative trade-offs of clientelistic politics identified by *scholars*, we know very little about what governs *citizens'* normative evaluations of such practices.

In this study, set in Kenya, we examine the role of two prominent factors that may explain citizens' normative evaluations of clientelistic behavior. Normative evaluation refers to the process of assessing a clientelistic behavior as to whether it meets a standard of what *ought to be* according to one's ideal expectations, rather than a descriptive evaluation, which is an assessment of what *is actually happening* (Bahník et al. 2021). Understanding citizens' normative evaluations as to how and why certain political practices should be regulated or punished is crucial for reforming public policies and laws to improve system legitimacy and confidence in democracy.

The first factor we examine is the extent of distortion — the degree of narrowness in targeting — specifically, to coethnics, copartisans, or members of the general public. We hypothesize that more narrowly-focused targeting will lead to more negative normative evaluations. As targeting becomes more narrow, perceptions of normative harms to society increase (e.g., by deviating further from a universal or needs-based approach, exacerbating ethnopartisan tensions), thereby increasing normative undesirability. Additionally, these categories are important in African multi-party democracies, where historical and contemporary tensions exist around the coethnic targeting axiom of politics, but where politics are

shifting towards copartisan or universalistic targeting as parties seek to achieve a majority (Horowitz 2022; Harding and Stasavage 2014).

Second, we question whether citizens are "normative relativists" depending on whether clientelist targeting is perpetrated among members of their own party or an opposing party. We propose that clientelistic actions perpetrated by opposing ethnopartisans will generate more negative normative evaluations. In addition to interdisciplinary theories of ingroup/outgroup bias, perceptions of personal and ingroup material and psychological benefits from clientelism within one's own party may offset normative harms, reducing normative undesirability. Further, when ethnopartisanship is shared, the imagined benefit might be higher when targeting is more narrow versus diluted when widened out to more people. Thus, we posit an interactive hypothesis: the distortion penalty may be larger for actors from the opposing party. If citizens are normative relativists, it could help us understand why clientelism persists even while producing societal harms — people may overlook their own party's actions, continuing to imbibe in clientelistic politics, while simultaneously reacting (or reacting in a bigger way) to the same actions by opposing parties.

To investigate these hypotheses, we implemented a survey experiment ahead of the 2017 Kenyan elections, in which two main party coalitions competed over national office. The survey sampled members of the Kikuyu and Luo ethnic groups (N=1,946), who have classically held the leadership of the two main opposing party coalitions – Jubilee and NASA respectively. Subjects were presented with hypothetical actions committed by six different political actors - President, Member of Parliament, Bureaucrat, Judge, Police Officer, and an average citizen. These actions involved some form of targeting, such as access to jobs or contracts, individual electoral handouts, influencing distribution of public resources, or appeals for future favoritism. In a cross-cutting 3x2 design, the six actors were randomly assigned in one arm to target coethnics, copartisans or unspecified general people, and in the other arm to have an identity of a Kikuyu in Jubilee or a Luo in NASA. Citizens were asked to assess whether the action should be legally punished, and if so, point to the degree of punishment on a scale ranging from a small fine to life in jail.

The data show a few robust general patterns of interest. First, citizens generally award harsher punishment for targeting *coethnics or copartisans* versus *unspecified people*. However,

no consistent difference exists between punishment levels for coethnics versus copartisans on most actions, contrary to hypothesis. For instance, the average preferred punishment for an MP candidate who uses CDF funds to give university scholarships to "people" corresponds in our scale to a value between a small monetary fine and less than one year in prison; when the scholarships are given to the MP's coethnics or copartisans, the average preferred punishment corresponds to between 3 and 5 years in prison, an increase of 1.08 of a control standard deviation.

This finding indicates that targeting a group of supporters is strongly perceived as something negative and punishable, but that at the same time, there is little difference in the minds of citizens between the narrowness of coethnic targeting versus the broader targeting of a copartisan group, which encompasses several ethnicities. This is important, because recent work has shown that parties have increasingly concentrated campaigning efforts towards ethnic groups in the party coalitions who are not the "core" ethnic groups of the presidential candidates (Horowitz 2022). Widening clientelism to copartisans does not help the normative case. A notable exception is the President, who is punished more for favoring coethnics relative to copartisans.[1]

Second, citizens award harsher punishment to opposing ethnopartisan actors, which is consistent with ethnopartisan bias in other political attitudes and behaviors (Michelitch 2015; Adida et al. 2017; Carlson 2016; Choi et al. 2022). However, and acknowledging reduced statistical power, generally no interaction effect exists between narrowness of targeting and (un)shared ethnopartisanship. This finding indicates that co-ethnopartisans cannot escape the distortion penalty — citizens are willing to increase punishment for targeting supporters equally for their party and the opposing party. In other words, the ethnopartisan ingroup/outgroup bias and the distortion penalty from targeting supporters (over the general public) are largely independent effects.[2]

[1]For instance, the average preferred punishment for the generic promise of favoritism "if I win, it is your time to eat" corresponds in our scale to about 2 years in prison; when the promise is made to the president's coethnics (e.g. "it is Kikuyu's time to eat"), the average preferred punishment corresponds to a period between 5 and 10 years, an increase of 0.69 of a control standard deviation. When the promise is made to the president's copartisans (e.g. "it is the Jubilee party's time to eat"), the average preferred punishment is between 3 and 5 years, an increase of 0.53 of a control standard deviation.

[2]In a companion paper, we provide more description and discussion of normative evaluations of clientelistic behaviors alongside other common behaviors, as well as explore ingroup/outgroup bias more deeply [citation omitted]. Here, we focus primarily on the role of distortion in targeting and whether an interaction exists

These findings expand our knowledge of the factors that govern citizens' normative evaluations of clientelism, which has thus far mostly focused on vote buying using vignettes in survey experiments. The "distortion penalty" between party supporters versus the general public is distinct but related to Pellicer and Wegner (2023), who find that citizens deem targeting a group more acceptable than an individual in South Africa and Tunisia. However, the finding stands somewhat in contrast to Gonzalez Ocantos et al. (2014), who find in Bolivia that vote buying is more normatively acceptable with a co-partisan versus a non-copartisan (perhaps because "turnout buying" does not aim to persuade away from one's preferred option). This difference may be due to their specification of "non-copartisan," rather than general people, or the presence of more ideological politics.[3]

More broadly, these findings are important in the vast landscape of what we know about the evolution of clientelism, especially where co-ethnicity with political leadership has been thought to play an important role. The traditional view is that clientelism is the dominant practice that works, and that coethnicity with leadership facilitates it (VandeWalle 2007; Wantchekon 2003; Hassan et al. 2023), with its exact forms fine-tuned to conditions of local ethnic geographies or type of good (Ichino and Nathan 2013; Ejdemyr et al. 2018; Beiser-McGrath et al. 2021). Yet over time, scholars have documented distaste for outright clientelistic ethnic appeals (Kim and Horowitz 2022), as well as increasing evolution of alternative, attractive, and coexisting/parallel strategies to target non-coethnic copartisans (Horowitz 2022), swing voters (Brierley and Kramon 2020; Weghorst and Lindberg 2013), or use programmatic politics to reach the general public (Harding and Stasavage 2014; Harding 2015). The evolution away from clientelistic practices tends to be slow given its embeddedness and utility as a credible strategy in weak states (Ochieng'Opalo 2022; Cheeseman et al. 2021). Disseminating statistics on citizens' normative evaluations of clientelism, such as those estimated in this study, could update public understanding of the general level of tolerance for clientelism, nudging this evolution forward.

based on sharing an actor's ethnopartisan identity.

[3]Citizens also view vote buying as more normatively acceptable with more equality in interactions between the politician and citizen (Pellicer and Wegner 2023), and when the recipient has lower socioeconomic status (Gonzalez Ocantos et al. 2014). Poorer individuals may accept clientelism more than the middle or upper classes, who can prioritize normative evaluations over financial concerns (Weitz-Shapiro 2012).

1 Factors Governing Normative Evaluations of Clientelism

In this study, we examine factors that govern citizens' normative evaluations of common clientelistic practices in multiparty democracies in which parties nest ethnic groups. Are citizens' attitudes towards the legal punishment of clientelistic actions governed by the narrowness of targeting to coethnics, versus more broadly to copartisans, versus the general public? And, are citizens "normative relativists" — do such attitudes depend on whether the clientelist targeting is perpetrated by members of their own, versus an opposing party? Specifically, we investigate whether these factors and their interaction affect the degree to which citizens believe the action should be legally punished by the state.

Clientelism has been difficult to define, in part because actions that are commonly referred to as clientelistic vary starkly over time, local institutional and cultural context, sector of politics or public services, the actors involved, whether it is "one-shot" or an iterative reciprocal relationship, whether the exchange is directly monitored or based on diffuse expectations of reciprocity, and so on.[4] We follow Hicken and Nathan (2020) who suggest that scholars explain what we mean by clientelism for our context in particular, while allowing our study to generally fall under the realm of and dialogue with the clientelism scholarship writ large. In this study, we focus on common behaviors between political actors and citizens where political actors use their position to improve their political clientele's access to the state and citizens elicit favorable access to the state from their patrons. We conceive of the political clientele as citizens that commonly support the same political party as the government actor.

Most studies of clientelism (often in contrast to programmatic politics) study the extent of its existence or conditions under which it occurs in electoral or distributive politics, as well as its appeal to politicians and citizens. This literature is vast (for reviews, see Hicken 2011; Hicken and Nathan 2020). In countries where parties nest ethnic groups, the study of

[4]In a seminal work, Stokes (2005) refers to clientelism as a subset of non-programmatic politics in which political patrons exchange particularist benefits with citizens for political support. Traditionally the exchange has been said to be quid pro quo and iterative as in "machine politics" (the transaction is monitored and benefit delivery is contingent on receipt of support and vice versa) observed in certain Latin American countries (Hicken 2011). However, in many regions of the world, such as Sub-Saharan Africa and Southeast Asia, parties and governments are often too weak to engage in direct monitoring of iterative exchanges to ensure that such relationships are "contingent" (Kitschelt and Wilkinson 2007; VandeWalle 2007). In the end, the vast majority of clientelistic practices studied do not involve direct monitoring and contingency (Hicken and Nathan 2020).

clientelism has often been led by investigations of whether the government leadership have improved access to state resources (perhaps leading to better outcomes) of coethnics (e.g., Kramon and Posner 2013; Ejdemyr et al. 2018; Beiser-McGrath et al. 2021; Choi et al. 2022; Cheeseman et al. 2021; Hassan et al. 2023; Lemarchand 1972; VandeWalle 2007), and the strength and nature of coethnic courtship in electoral politics (e.g., Carlson 2015; Adida et al. 2017; Brierley and Kramon 2020; Ichino and Nathan 2013). More recently, scholars have argued that targeting has evolved: while coethnic targeting may have been common under dictatorships that followed independence in many countries, the advent of multiparty democracy has broadened targeting to include not just the coethnics of the president, but other ethnic groups represented in a party's multiethnic coalition - or copartisans (Horowitz 2022; Posner 2007; Posner 2005).

1.1 Normative Debates Surrounding Clientelism

A scholarly debate exists surrounding the normative aspects of clientelism. In this study, we are primarily interested in what we think are the most salient to citizens in the context under question: distortion and ethnopartisan polarization. First, clientelism distorts access to state resources, favoring clienteles in public goods provision and undermining universal delivery or needs-based standards of service delivery (Keefer and Khemani 2004; Robinson and Verdier 2013; Canen and Wantchekon 2022; Ejdemyr et al. 2018; Gottlieb, Larreguy, et al. 2020). Favoritism in contracts and jobs, based on political connections rather than merit, creates inefficiencies, reduces competition, and allows firms to capture the state through bureaucratic appointments or lobbying for preferential treatment (Canen et al. 2023).

Second, distortion in favor of supporters — coethnics and/or copartisans — can exacerbate ethnopartisan polarization. Without strong state fiscal capacity to deliver state resources universally or needs-based, economic interests become deeply intertwined with political competition, leading to a perception that the prosperity of an ethnic or partisan group hinges on having one of their own in power to have a "turn to eat" (Ochieng'Opalo 2022; Posner 2005). As Fjelde and Höglund (2016) observes, "in such a contest, electoral defeat entails not only political marginalization but is also perceived as having severe economic consequences for both elites and constituents" (p 302). As a consequence of such distortion,

and as the climax of political competition, elections can therefore fuel ethnopartisan conflict, violence, and even regime destabilization (Michelitch 2015; Klaus and Mitchell 2015; Horowitz 1985; Fjelde and Höglund 2016). Pointing to the polarizing nature of clientelism, recent studies have underscored how outright appeals by actors to favor supporters, especially coethnics, backfire or have little effect (Kim and Horowitz 2022; Horowitz and Klaus 2020).

Other harmful consequences of clientelism discussed in the literature may also be at play in the minds of citizens. Clientelism reduces economic growth through its distortions (Bhandari 2022; Canen and Wantchekon 2022) but also by incentivizing corruption (Lindberg et al. 2022). Clientelism undermines transparency and the rule of law, with selective enforcement of rules and a culture of impunity, as officials prioritize political clienteles (Holland 2016; Bhandari 2022). Vote-buying carries a concern of skewing vote choice away from other more principled criteria voters may use (such as performance or ideology) — although its systemic impact on election outcomes remains debated (Hidalgo and Nichter 2016; Gallego et al. 2023). Finally, many academics have cited concerns about hierarchical clientelist relationships that might be exploitative of ordinary citizens, especially the poor (Kitschelt and Wilkinson 2007), although others have emphasized that citizens may either not view such hierarchichal relationships as normatively problematic (Piliavsky 2014) or that such relationships are not hierarchical and sometimes fueled to a large degree by citizen demands on politicians (Lindberg 2010).

However, newer scholarship has cited the utility of clientelism to citizens, and a failure of academics (often based in the Global North, examining Global South contexts) to appreciate that citizens could therefore view clientelism differently (Piliavsky 2014; Pellicer and Wegner 2023). Indeed, political clienteles can stand to gain diverse material and psychological benefits of clientelism, especially if their party is in power. Clientelism in distributive politics can empower citizens to hold politicians accountable for delivering vital public goods and services (Gottlieb 2016; Cheeseman et al. 2021). Receiving such targeted collective goods, such as improved access to schooling, clean water, or health provision, may fulfill citizens' expectations of political responsiveness, particularly where programmatic politics is less credible due to state fiscal capacity constraints (Ochieng'Opalo 2022). Clientelist relationships can further

help marginalized citizens navigate bureaucratic barriers and access resources individually through the help of brokers (Nichter 2018; Auerbach and Thachil 2018). Around election time, unmonitored electoral gifts (non-quid-pro-quo "vote buying"), common in Sub-Saharan Africa, are often seen as expected gestures at campaign events (Michelitch and Utych 2018) and credible signals of future representation (Kramon 2016).[5] Finally, clientelist relationships can provide social recognition, as citizens feel seen, heard, and included by leaders. Beyond material benefits, this sense of being valued and given a voice in politics may make clientelism normatively acceptable to many (Klaus et al. 2023).

Despite these debates, we know little about what governs citizens' normative evaluations of clientelism. Normative evaluations are important to study because it helps us understand whether citizens — even if they partake in clientelism as the most credible mode of politics — believe such practices are desirable and legitimate in their democracy, or if they would support reforming aspects about the way politics works. Although any changes between clientelism and programmatic politics is slow moving, changing normative expectations present one catalyst for change (Ochieng'Opalo 2022).

To date, only a few studies investigate citizens' evaluations of whether targeting is acceptable as a normal or non-controversial practice in their democracy (Pellicer and Wegner 2023; Gonzalez Ocantos et al. 2014). This small literature, which focuses on views on electoral handouts ("vote buying"), emphasizes that citizens in low-income democracies can have wide-ranging opinions that tend to be more permissive regarding commonplace targeting behaviors than those of researchers. Using survey experiments of vote-buying vignettes, Pellicer and Wegner (2023) explore how variation in the nature of the interaction between the politician and citizen affects respondents' views on the acceptability of the interaction, finding that citizens view as more acceptable vote buying interactions with more equality between the politician and citizen (reducing exploitation concerns), and group versus individual targeting (reducing distortion). Gonzalez Ocantos et al. (2014) show also using survey experiments with vignettes that citizens find vote buying more acceptable when the

[5]Tribute is a traditional practice of gift exchange between patron and client involving bonds of reciprocity and trust found in communal living. VandeWalle (2007) argues that this historical form of clientelism is non-existant in contemporary politics but may be evoked when electoral handouts are given, reducing perceptions of harm as "vote buying".

recipient has lower socioeconomic status (since they are needier), and the candidate is a copartisan versus a non-copartisan (since the handout does not aim to persuade away from one's preferred option).

We build on these studies by focusing on two interrelated factors that may govern citizens' normative evaluations that are relevant for politics in democracies where parties have formed to nest ethnic groups. The first examines distortions in who is targeted - coethnics, copartisans, or the general public. The second is by whom the targeting is occurring - an actor with shared or unshared ethnopartisanship with the citizen. We discuss these factors, and their possible interaction, below.

1.2 Distortion to Coethnics, Copartisans, or General Public

We first consider the effect of particularism based on coethnicity versus copartisanship versus unspecified individuals in a context in which parties nest ethnic groups.[6] In such a context, to achieve a majority, parties need to target coethnics to the core leadership, but also other key groups to make a coalition, and even swing groups (Horowitz 2022).[7] We believe that normative evaluations of clientelism around coethnicity and copartisanship is especially important in these contexts. Historical beliefs around targeting are strong and expectations regarding future targeting are resistant to change (Horowitz and Michelitch 2021). These beliefs and expectations are linked to intense fears that losers are shut out of state resources (Posner 2005).

There are a few reasons why more narrow targeting might lead to greater perception of normative harm. First, we expect that the narrower the targeting, the more distortionary, and therefore the more normatively problematic. Coethnic targeting very narrowly targets a political actors' own ethnic group, which is the most exclusive targeting. By contrast,

[6]We note that in our pre-analysis plan, we name the comparison of coethnic to copartisan targeting as a main hypothesis, and the comparison of these treatments with the unspecified general people treatment as an exploratory hypothesis. The reason for that latter is that we were not sure whether people would imagine coethnic or copartisan targeting was occurring in the treatment where actors interact with the general public. That is to say, where the beliefs are uncontrolled, respondents can "fill in" their own beliefs, rendering the treatment indistinct.

[7]While some may question the existence of meaningful partisanship in new democracies, many have demonstrated by now how it operates similarly to partisanship in older democracies - increasing political participation, aiding vote choice, and inducing partisan motivated reasoning (Carlson 2016; Harding and Michelitch 2021; Bleck and Walle 2019).

copartisan targeting includes targeting, not just an actor's coethnics, but individuals from other ethnic groups that support the party. Of course, copartisan targeting is more narrow criteria than opening up the targeting to the public writ large. This argument dovetails Pellicer and Wegner (2023) who argue and demonstrate that clientelism is less acceptable to citizens when an individual is targeted versus a locality, with the example being individually preferred access to health care at a clinic versus a community getting a clinic. Here, however, we compare just not the *number* targeted, but the *narrowness of the criteria used* across a range of actions that at times target individuals, firms, communities, groups of voters, etc.

Second, coethnic targeting may be considered normatively worse than copartisan targeting because of the nature of the group identity as a criterion for targeting. Accountable politicians in a democratic system may be expected to be responsive to the particular preferences of their copartisan supporters - this is a foundational principle of democracy (e.g. Ferejohn 1999; Przeworski 2010). While democratic theorists are implicitly focusing on contexts of ideological politics with impartial service delivery, the logic still stands that people may expect that serving those who made the choice to help put an actor in power is a normal and normatively acceptable part of democracy. By contrast, more narrow coethnic targeting is based on non-voluntary, inherited traits (Chandra 2012) which excludes part of the voter base for the party - the non-coethnic copartisans - and may be therefore less condoned.

Relatedly, coethnic targeting may be seen as especially pernicious for harkening back to the era of more authoritarian politics, while copartisan targeting might be associated with more contemporary politics. In the past, leaders were believed to narrowly target coethnics under postcolonial neo-patrimonial regimes (Lemarchand 1972; VandeWalle 2007; Bleck and Walle 2019; Horowitz 1985; Posner 2007). The evolution of multiparty competition and the partisanship as a multiethnic politically relevant group identity to compete over the state represents more current notions of politics (Michelitch 2015; Michelitch and Utych 2018; Harding and Michelitch 2021). Thus, copartisan targeting might be seen as an evolution toward more contemporary politics of multiparty democracy.[8]

Taken together, we submit the following hypothesis:

[8]See such works as VandeWalle (2007) and Lemarchand (1972) for discussions of historical clientelism under the post-colonial period, and such works as Posner (2007) and Ochieng'Opalo (2022) for discussions of more contemporary evolutions under multipartyism.

Hypothesis 1 (Distortion Penalty): Punishment is greater if an action more narrowly targets coethnics rather than copartisans rather than unspecified people.[9]

1.3 Shared Ethnopartisanship, and its Interaction with Distortion

Where parties nest ethnic groups, ethnopartisanship is a nested identity where a citizen shares the ethnicity and therefore the partisanship with a political actor. Ingroup/outgroup discrimination - where individuals are biased in favor of those with shared group membership, and against those with unshared group membership, can therefore occur on ethnopartisan lines (Michelitch 2015). Indeed, a wide-ranging interdisciplinary literature supports the notion that individuals tend to discriminate against an "outgroup" and favor an "ingroup" member across a wide variety of behaviors and across a wide range of naturally occurring and minimally-induced identity groups (for reviews see (Brewer and Kramer 1985; Cikara and Van Bavel 2014; Riach and Rich 2002; Tajfel and Turner 2004)). Scholars of African politics have found evidence for ingroup/outgroup discrimination across both ethnic lines (Robinson 2020)), as well as across ethnopartisan lines (e.g., (Carlson 2016; Carlin and Love 2013; Iyengar et al. 2019; Michelitch 2015; Adida et al. 2017)).

Regarding punishing behavior, ingroup/outgroupism may emerge for a few reasons related to psychological bias. These cognitive biases may not be consciously applied but automatic in producing ingroup/outgroup discrimination — due to it being "hard-wired" (Cikara and Van Bavel 2014) and rooted in evolutionary competition between groups (Choi and Bowles 2007; Fehr et al. 2008). One set of factors relates to feelings towards ingroup versus outgroup members. One may have stronger other-regarding preferences towards ingroup members (Cooper 2014; Fehr et al. 2008; Fehr and Schmidt 1999; Tajfel and Turner 2004; Tajfel et al. 1979; Whitt and Wilson 2007), feel more empathy towards ingroup members and hostility towards outgroup members (Cikara and Van Bavel 2014), or hold more favorable implicit biases towards ingroup members (e.g., assigning more positive regard) (Paluck and Green

[9]We note that in our pre-analysis plan, we name the comparison of coethnic to copartisan targeting as a main hypothesis, and the comparison of these treatments with the unspecified general people treatment as an exploratory hypothesis. The reason for that latter is that we were not sure whether people would imagine coethnic or copartisan targeting was occurring in the treatment where actors interact with the general public. That is to say, where the beliefs are uncontrolled, respondents can "fill in" their own beliefs (here an assumption that coethnic or copartisan targeting was occurring), rendering the treatment indistinct.

2009). Individuals may therefore receive disutility from punishing ingroup members, here co-ethnopartisans, and/or utility from punishing outgroup members, producing a discriminatory gap in punishment assignment.

A second category of psychological bias involves biased information processing. First, individuals might be prone to attribution error. Applied here, it would be a tendency for individuals to interpret negative behaviors by outgroup members as "dispositional" having hostile intent or stemming from negative personality characteristics, while interpreting negative behaviors by ingroup members as "situational" or being aberrations or due to being a victim of bad circumstances (Jones and Harris 1967). This bias has been shown to lead to ingroup/outgroup bias in desire for legal punishment of criminal acts across ethnic lines in the United States (Trahan and Laird 2018). Distinct but related, ethnopartisan motivated reasoning may be present – where citizens overlook information that conflicts with their priors – usually positive beliefs about their own party and negative beliefs about opposing parties (Carlson 2015; Adida et al. 2017; Horowitz and Michelitch 2021).Taken together, citizens could downplay an action's normative harm for ingroup members, reducing extent of punishment.

However, ingroup/outgroup bias may emerge on ethnopartisan lines due to concerns over material benefits, or at least, imagined material benefits. When ethnopartisanship is shared, then one might imagine that if one's group is targeted, there is a material benefit that accrues to group members, while when ethnopartisanship is unshared, there may be no perceived benefits. Carlson (2015) for example, finds that the behaviors or characteristics of actors of opposing partisanships (here elected representatives) are not salient to citizens since they feel they would not benefit from those actors' actions in any case. Of course, the mechanism for this "self-interest" is based on the idea that people view ingroup members as interchangeable exemplars, gaining utility from group members' utility. Such benefits from clientelism within one's own party may offset normative harms, reducing normative undesirability. Further, when ethnopartisanship is shared, the imagined benefit might be higher when targeting is more narrow (coethnics) versus diluted when widened out to more people (copartisans, or even further, unspecified general people). Thus, we posit an interactive hypothesis: the distortion penalty may be larger for actors from the opposing party.

Finally, while most literature suggests that individuals tend to favor ingroup members over outgroup members, other research suggests that ingroup policing for norm violations can be highly salient. A few scholars such as Fearon and Laitin (1996) have suggested that there are incentives to police the norms of the ingroup to avoid larger intergroup conflict. In our context of interest, we are interested in evaluations of behaviors for the purposes of third-party punishment from the state rather than within parties or ethnic groups, so the logic may be less applicable. Nonetheless, if norms of ingroup policing are present in a society, they may be applied subconsciously or automatically to a similar social setting.

We thus generate the following theoretical hypotheses:

Hypothesis 2 (Outgroup Penalty): punishment is greater if an action is committed by an actor with unshared ethnopartisanship.

Hypothesis 3 (Compounded Distortion and Outgroup Penalty): punishment is greater if an action more narrowly targets coethnics rather than copartisans, and *the effect is greater if the action is committed by an actor with unshared ethnopartisanship.*

2 Context of the Study

This study takes place in Kenya, where parties have historically nested ethnic groups (Horowitz 2022) and beliefs about ethnopartisan targeting are strong (Ochieng'Opalo 2022). Ethnicity in the study context, and indeed more broadly, is readily discernible through language, accent, and name, while partisanship can be inferred from ethnicity due to common knowledge of how parties nest ethnic groups - often inferences made based on the ethnicity of party leaders (Horowitz 2019).

In this study, we focus on citizens from the two major ethnopartisan groups, the Kikuyu and Luo, who have been on opposing sides of the political divide throughout much of the period since independence (gained from Britain in 1963). The Kikuyu have been in power for much of the post-independence period, with three of the country's four presidents coming from the Kikuyu. Politics in Kenya are often described as a zero-sum game between these rival ethnic factions. Voters support coethnic candidates and the parties that best incorporate them out of the belief that ethnic patrons serve as more faithful representatives of their

clientele (Horowitz 2022; Kramon 2016; Ochieng'Opalo 2022; Cheeseman et al. 2021; Oyugi 1997). These beliefs are often "taken for granted" across many studies, given how conventional they are. (Horowitz and Michelitch 2021), however, investigate these beliefs directly, finding they are generally quite strong among ordinary citizens, but variable depending on the administration, the public service delivery sector, and also the ethnopartisan identity of the respondent. Namely, individuals have weaker beliefs that their own ethnopartisan leaders targeted their supporters, and significantly stronger beliefs that competing ethnopartisan leaders did so.

We conducted our study shortly before Kenya's 2017 national elections. The partisan landscape at the time of our investigation was dominated by two main parties largely drawing support from distinct ethnic bases. The incumbent party, Jubilee, which came to power in 2013, is often viewed as a Kikuyu-Kalenjin alliance owing to the ethnic identities of its two senior leaders, incumbent president Uhuru Kenyatta (Kikuyu) and vice president William Ruto (Kalenjin). The main opposition party, the National Super Alliance (NASA), which emerged in the months leading up to the election, is commonly seen as a Luo-Kamba-Luhya coalition, again owing to the identities of its top leaders, Raila Odinga (Luo), Kalonzo Musyoka (Kamba), and Musalia Mudavadi and Moses Wetangula (Luhya). While both parties draw support from groups other than their core ethnic communities and bloc voting within groups is rarely perfectly uniform, these ethnic associations are well cemented in voters' minds.

Data from a large national survey carried out by Ipsos (n=2,026) from May 11-23 2017 showed that support (intended vote choice) for Jubilee vs. NASA in the presidential race was as follows: 91% vs. 4% for Kikuyus, 79% vs. 15% for Kalenjins, 24% vs. 58% for Kambas, 23% vs. 63% for Luhyas, and 6% vs. 86% for Luos.

3 Research Design

3.1 Population and Sampling

The target population for this study are Kenyans identifying as either Kikuyu or Luo and most directly associated with the leading parties at the time of the survey.

Our sample of Luos and Kikuyus endeavored to reach a total of 2,000 respondents, evenly divided between urban and rural locations, and the achieved sample was N=1,946. The urban sample is from Nairobi, the capital city, and the rural samples are drawn from Nyeri and Kisumu counties for Kikuyus and Luos respectively. Within each rural county, we further divided the sample between those who reside in the main towns (Nyeri Town and Kisumu Town) and smaller towns and villages, with a minimum of 100 respondents in each of the main towns (to achieve this quota, we over-sampled Nyeri Town).

Within each county (Nairobi, Nyeri, and Kisumu), we allocated the sample across parliamentary constituencies using population proportional to size (PPS). Within constituencies, we randomly selected polling stations (typically primary or secondary schools) using PPS based on the number of registered voters (using the Chromy method implemented in Stata with the `ppschromy` command). Enumerators were assigned to work in pairs each day, with the daily pairings assigned randomly. We instructed enumerators to use a random-walk procedure to select households. Within households, we randomly selected respondents from those adults (18+) who were at home at the time of the visit. A small number of sampling points in Nairobi were dropped due to insecurity and we excluded Karen, an affluent neighborhood, after determining that respondents in that area were almost always unwilling to participate.

Because we sought to include only Kikuyu and Luos respondents, we limited the sample for Nairobi Country to polling stations that were majority Kikuyu or Luo, based on data from Andy Harris which estimates ethnic shares using last names from the voter rolls (Harris 2015). In practice, we found the ethnicity data to be imprecise and dropped localities where the enumerators could not locate the target populations. In rural counties, we did not stratify by local ethnicity since the counties are both fairly homogenous: data from the 2009 census shows that Kikuyus make up 94% of the population in Nyeri County and Luos 89% in Kisumu County (KNBS 2014).

In the rural areas, we sought to ensure the inclusion of some areas that were not connected to the national electricity grid. However, lacking detailed information about local-level electrification, we were not able to stratify on this dimension. Once it became clear that our sampling strategy in the rural counties was producing relatively few respondents from

localities without electricity, we consulted local authorities to identify un-electrified areas and added additional sampling points in those places.

All interviews were conducted by coethnic enumerators to hold constant any enumerator identity effects (Adida et al. 2016).

3.2 Survey Experiment

In the survey experiment, respondents were presented with a set of hypothetical but common clientelistic actions by six actors coming from across the political and administrative sectors: a Member of Parliament candidate, the President, a Bureaucrat, a Judge, a Police Officer, and an average citizen (a Shopkeeper). The set of actors and actions that we analyze are reported in Table 1.

Table 1: Actors and actions presented in the survey experiment

Actor	Action
MP	Giving money at a rally
	Providing transportation to vote
	Using CDF funds to give university scholarships
President	Making an appeal about someone's time to eat
	Using personal funds to provide more BVR kits for registration
	Influencing parliament to accept a budget for school construction
Bureaucrat	Providing funds to connect houses to the electricity grid
	Granting a contract to a firm without a competitive bidding process
Judge	Giving a light sentence to someone who was caught vote rigging
	Giving a light sentence to someone who stole a car
	Giving a job to someone as a lawyer without a competitive hiring process
Police officer	Letting off someone for a traffic offense
Shopkeeper	Making an appeal about someone's time to eat if a politician wins
	Requesting a politician for a contract for his firm, without a competitive bidding process

3.2.1 Treatment Conditions and Randomization

The experiment has a 2×3 cross-cutting design, which creates 6 treatment conditions. Treatment assignment is at the level of the actor, thus the 6 actors are assigned to the 6 treatments in a complete random assignment.

Randomization 1: Actor's Ethnopartisan Identity

In the first arm, we randomly varied the ethnopartisan identity of the actor. This attribute is communicated by using surnames informative of the ethnicity and specifying party affiliation. There were two conditions:

1. Kikuyu surname + Jubilee party
2. Luo surname + NASA party

The surnames were randomly selected from a set of common but easily identifiable surnames, such that each actor has a unique surname. We piloted the list of surnames with convenience samples to ensure that individuals would have no trouble associating the surname with either the Kikuyu or Luo ethnic group. We also avoided the surnames of any known politicians.

Randomization 2: Group Targeted as Coethnic, Copartisan, or "Unspecified"

In the second arm, we randomly varied the group targeted by the clientelistic action. There were three possible conditions:

1. Copartisans are targeted
2. Coethnics are targeted
3. It is unspecified who was targeted

Each respondent was presented with all the actors and actions, but the 6 actors were allocated to one of the 6 treatment conditions by complete random assignment. Therefore, each respondent experienced all 6 treatment combinations, one combination for each actor. This means that, when we focus on a specific actor, the variation we use is between subjects, and the analyses of different actors have different treatment and control groups in the sample.

Two further comments should be made about this design. First, we assigned the treatment at the level of an actor, rather than of the action, because continuously switching the identity and the target across actions of the same actor could confuse respondents. Second,

we allocated actors to conditions using a complete random assignment to make sure that each respondent would be part of all 6 treatment groups and that there would be roughly the same amount of respondents in each treatment group for each actor.

3.2.2 Outcome Measurement

For each hypothetical action, respondents were asked whether they thought it should be punishable by law, and if yes what the punishment should be. They could choose from the following scale: No punishment/Small fine/1 year or less in jail/About 2 years in jail/About 3 years in jail/About 5 years in jail/About 10 years in jail/About 15 years in jail/About 20 years in jail/About 25 years in jail/Life in jail/Don't know or no answer.

Our preferred approach to operationalize this outcome is to simply use it as a continuous measure of intensity of preferred punishment: we transform it into a index ranging from 0 to 10 and we use it as a continuous outcome variable. In the regressions, we standardize each outcome variable using the mean and standard deviation of the relevant control group. In other pre-registered tests, reported in the Appendix, we also use a binary transformation of the scale, which takes value 1 for any level of punishment beyond none at all.

To summarize the results more concisely in terms of actors, and to reduce the number of statistical tests, we compute actor-specific indexes that summarize the punishment given for all actions committed by a specific actor. Our preferred choice, which we report in the main results, is an Inverse Covariance Weighted (ICW) index, but in the Appendix we also report results using the first Principal Component as an actor-level index. For descriptive analyses and plots, we compute the index by aggregating the raw (unstandardized) punishment variables. For the statistical tests, we use a slightly different version of the indexes, where we first standardize the component variables using the mean and standard deviation of the control group before aggregating them. Then, we re-standardize the resulting index in order to interpret a treatment effect in terms of control standard deviations of the index itself.

4 Results

The data show two systematic patterns. First, individuals punish actors more for targeting supporters versus the general public, but typically there is no significant difference between targeting coethnics versus copartisans. Second, individuals exhibit ingroup/outgroup bias on ethnopartisan lines in the expected way — punishment is higher for actors from the opposing party. These effects are not interactive - there is no joint effect between (un)shared ethnopartisanship with the actor and the narrowness with which the actors has targeted. See the online appendix or updated versions of the working paper for the full set of results indicated in the pre-analysis plan.

To first visualize variation in the outcomes across treatment conditions, we plot group averages of preferred punishment for each actor in Figure 1. We plot averages of the ICW index of punishment, which aggregates respondents' preferred punishment across all the actions that an actor commits.[10] The x-axes are arranged so that going from left to right increases the degree of narrowness in targeting (unspecified, copartisan, and coethnic). Note that the y-axis range is not held constant across the panels of the figures, which made results difficult to see visually, but exhibits punishment scaled to view effects most easily.

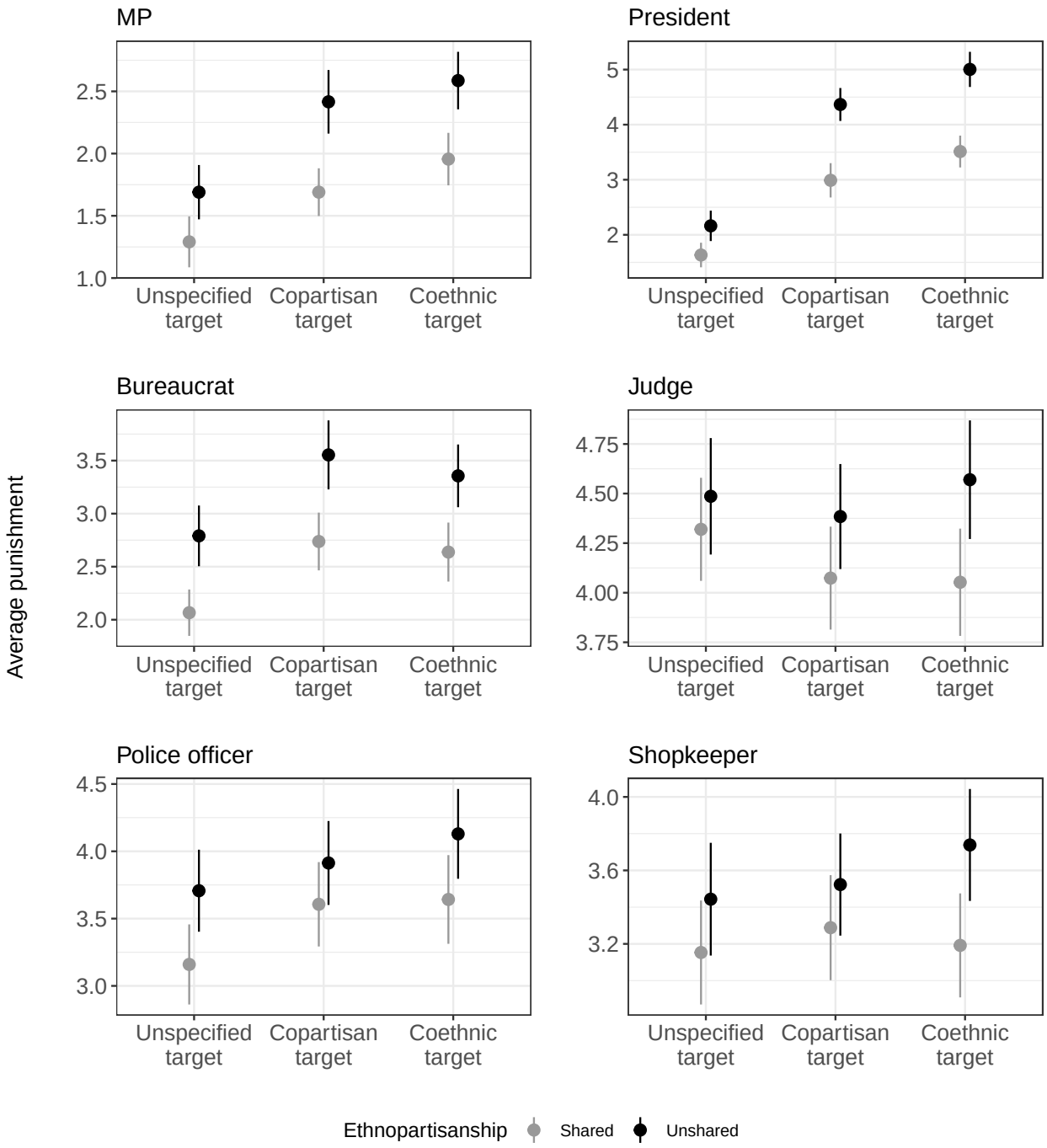
As a first descriptive observation from Figure 1, we note that considerable variation exists in the levels of punishment *across actors*: while clientelistic behavior by the president or the judge is severely punished, there is on average more tolerance for MP candidates. With all the limitations inherent in comparing levels across actors and actions from different social domains, this exercise shows that citizens' views are complex and nuanced.

Second, for almost all actors average punishment increases in the narrowness of targeting. The largest differences are observed between the unspecified condition and the other two, while the differences between copartisan and coethnic targeting are smaller and, as we discuss below, generally not statistically significant. The only exception is the judge, who is punished less for narrower targeting, even if the difference is not statistically significant.

Third, for almost all actors and levels of targeting, co-ethnopartisans are punished less than those from the other group. Even visually, this ethnopartisan penalty is large and

[10]Technically, the police officer is an exception. Since this actor has only one action associated, the index coincides with the punishment index of that action.

Figure 1: Average levels of indicated punishment by actor and treatment condition



noticeable.

We now move to the effect estimates. In this section we show results with a more aggregate definition of targeting, where we compare any group targeting (coethnic or copartisan)

with the unspecified one. The differences between coethnic and copartisan targeting are reported in the Appendix and discussed below.

In Table 2 we report the main results. These results come from unconditional regressions of the standardized, actor-specific indexes on the treatment indicators. Each column corresponds to a different actor. Panel (A) reports the effects of the targeting treatment (coethnic or copartisan vs unspecified), Panel (B) reports the effects of the ethnopartisanship treatment (unshared vs shared), and Panel (C) reports the effects of both treatments together and their interaction. All estimated effects can be read in terms of control group standard deviations. In addition to the treatment effects and the standard errors, we report both standard p-values and p-values adjusted for multiple comparisons using the Benjamini-Hochberg method (in brackets).

Panel (A) confirms the patterns visible in Figure 1. Group targeting (whether coethnic or copartisan) has generally a positive and significant effect on preferred punishment across actors. The only notable exception is the judge, where the effect is negatively signed and not statistically significant, and the shopkeeper, where the effect has a positive sign but is small in magnitude and falls short of statistical significance. There is variation in effect sizes: for the MP and the president the effects are about 0.4 and 0.6 of a standard deviation respectively. For the bureaucrat and the police officer, they are considerably smaller, between 0.1 and 0.3 of a standard deviation, although still statistically significant.

Panel (B) shows the estimated effects of unshared ethnopartisanship. Different from the first treatment, the effects are consistently positive and statistically significant. The penalty for out-group members ranges, across the actors, from 0.15 to 0.5 of a standard deviation.

Finally, in Panel (C) we investigate whether the two treatments interact with each other. It should be noted that, while the previous tests can detect effects larger than 0.15 standard deviations with a power of 80% or more, the test for the interaction effect has much worse power, so these results should be read with this caveat in mind.Indeed, the coefficients in Panel (C) do not clearly indicate evidence of an interaction effect between group targeting and unshared ethnopartisanship. The partial exceptions are the MP and the president, as already suggested by Figure 1. For the MP, the interaction term is positive and substantively meaningful, but not statistically significant. For the president, the interaction is considerable,

Table 2: Effects of targeting and ethnopartisanship, actor-specific indexes

	MP	President	Bureaucrat	Judge	Police officer	Shopkeeper
(A) Targeting						
Any target	0.444***	0.597***	0.276***	-0.070	0.134*	0.079
	(0.052)	(0.051)	(0.051)	(0.050)	(0.051)	(0.050)
	0.000, [0.000]	0.000, [0.000]	0.000, [0.000]	0.163, [0.163]	0.009, [0.013]	0.115, [0.138]
Num.Obs.	1795	1787	1784	1788	1782	1783
R2	0.037	0.068	0.015	0.001	0.004	0.001
R2 Adj.	0.037	0.068	0.014	0.001	0.003	0.001
Mean Y_c	1.500	1.892	2.425	4.397	3.454	3.296
SD Y_c	1.859	2.220	2.271	2.418	2.679	2.609
(B) Unshared EP						
Unshared EP	0.308***	0.490***	0.334***	0.149**	0.153**	0.181***
	(0.051)	(0.051)	(0.052)	(0.048)	(0.047)	(0.048)
	0.000, [0.000]	0.000, [0.000]	0.000, [0.000]	0.002, [0.002]	0.001, [0.001]	0.000, [0.000]
Num.Obs.	1795	1787	1784	1788	1782	1783
R2	0.020	0.049	0.023	0.005	0.006	0.008
R2 Adj.	0.020	0.048	0.022	0.005	0.005	0.007
Mean Y_c	1.661	2.701	2.474	4.152	3.480	3.211
SD Y_c	1.813	2.522	2.270	2.354	2.782	2.485
(C) Interaction						
Any target	0.351***	0.420***	0.321***	-0.109+	0.184*	0.035
	(0.062)	(0.067)	(0.079)	(0.057)	(0.076)	(0.064)
	0.000, [0.000]	0.000, [0.000]	0.000, [0.000]	0.055, [0.099]	0.015, [0.030]	0.583, [0.617]
Unshared EP	0.194*	0.255**	0.384***	0.065	0.217*	0.111
	(0.074)	(0.083)	(0.097)	(0.069)	(0.086)	(0.076)
	0.009, [0.023]	0.002, [0.007]	0.000, [0.000]	0.347, [0.446]	0.012, [0.027]	0.147, [0.221]
Any target × Unshared EP	0.154	0.320**	0.016	0.092	-0.060	0.077
	(0.093)	(0.103)	(0.125)	(0.084)	(0.108)	(0.093)
	0.098, [0.160]	0.002, [0.007]	0.901, [0.901]	0.274, [0.379]	0.577, [0.617]	0.410, [0.491]
Num.Obs.	1795	1787	1784	1788	1782	1783
R2	0.065	0.115	0.036	0.008	0.010	0.010
R2 Adj.	0.063	0.114	0.035	0.006	0.009	0.008
Mean Y_c	1.291	1.633	2.066	4.320	3.159	3.153
SD Y_c	1.737	1.988	1.939	2.360	2.522	2.525

Notes: Coefficients are expressed in terms of control standard deviations. The raw sample moments reported are computed using non-normalized indexes. Heteroskedasticity-robust standard errors in parentheses. P-values reported under the standard errors are unadjusted (not bracketed) and adjusted with the Benjamini-Hochberg method (in brackets). The significance stars are based on the adjusted p-value. + <0.1, * <0.05, ** <0.01, *** <0.001.

both substantively and statistically. It is worth noting that the non-interacted targeting treatment is, with the exception of the judge, generally positive and statistically significant. This is suggestive that respondents are willing to punish an actor for targeting benefits even when that actor is an ingroup member.

4.1 Disaggregated Results and Robustness

In Appendix Section B we report more disaggregated analyses at the action level. First of all, we investigate the difference between coethnic and copartisan targeting in Section B.1. We generally do not find significant differences in punishment between the two levels of targeting, which suggests that, contrary to our hypothesis, the narrowness of clientelistic targeting is less relevant for respondents than the fact of a specified targeting. The only exception is the president, for which benefits to coethnics are punished significantly more than a benefits to copartisans. We then report the effects of unshared identity on the single actions (Section B.2) and the interaction of the two treatments (Section B.3). In the Appendix, we also report additional specifications where we control for covariates (age, gender, socio-economic status, urban residency, and ethnicity-partisanship) or use a binary version of the outcome variable. In Appendix Section C we explore the treatment effects along the distribution of preferred punishment, using quantile regressions and distribution regressions.

5 Discussion and Conclusion

We conducted a survey experiment to better understand the variation in attitudes towards clientelism and appetite for its legal punishment in Kenya. By varying the behavior and identity of a set of hypothetical actors, we were able to assess whether the preferred punishment for clientelism is affected by the scope of group targeting (copartisans or, more narrowly, coethnics) and the out-group identity of the clientelistic actor. We found sizable penalties given to coethnic or copartisan targeting relative to a condition where the beneficiaries are not specified; however, this penalty is generally not different when the targets are coethnics versus copartisans. We also found large penalties for actors belonging to an oppo-

site ethnopartisan group relative to the ingroup, but no clear evidence that the two effects may reinforce each other. We also documented significant heterogeneity across the actors involved: elected politicians and bureaucrats are associated with larger effects compared to members of the bureaucracy and the judicial system, while the punishment of average citizens is not affected.

This paper is among the first comprehensive attempts at evaluating normative evaluations of clientelism in African democracies. Beyond their relevance for scholars of political behavior and development, our results also have implications for policy debates on confidence in the democratic system more generally. Data on citizens' normative evaluations are important to collect and report. Indeed, reporting hitherto unknown statistics as to the majority public opinion on what behavior *ought to be* has been shown to lead to updates in attitudes and behaviors consistent with the norm in social psychology ("injunctive norm" interventions) (Cialdini et al. 1990; Kallgren et al. 2000).[11] In political science, future research could test similar interventions to inform citizens, activists/journalists, or politicians with public opinion statistics about latent but unknown normative evaluations of clientelism, and examine consequences for political behavior. Changing expectations for the way politics works may be one catalyst to evolve clientelism in the direction desired by the public (Ochieng'Opalo 2022).

[11] While most such studies regard binge drinking alcohol or pro-environment behaviors, one study found effects regarding intention to vote (French Bourgeois and Sablonnière 2023).

References

Adida, C., J. Gottlieb, E. Kramon, and G. McClendon (2017). "Overcoming or reinforcing coethnic preferences? an experiment on information and ethnic voting". *Quarterly Journal of Political Science* 12.4, pp. 437–477.

Adida, C. L., K. E. Ferree, D. N. Posner, and A. L. Robinson (2016). "Who's asking? Interviewer coethnicity effects in African survey data". *Comparative Political Studies* 49.12, pp. 1630–1660.

Auerbach, A. M. and T. Thachil (2018). "How clients select brokers: Competition and choice in India's slums". *American Political Science Review* 112.4, pp. 775–791.

Bahník, Š., E. Efendic, and M. A. Vranka (2021). "Sacrificing oneself or another: The difference between prescriptive and normative judgments in moral evaluation". *Psychological Reports* 124.1, pp. 108–130.

Beiser-McGrath, J., C. Müller-Crepon, and Y. I. Pengl (2021). "Who benefits? How local ethnic demography shapes political favoritism in Africa". *British Journal of Political Science* 51.4, pp. 1582–1600.

Bhandari, A. (2022). "Political determinants of economic exchange: Evidence from a business experiment in Senegal". *American Journal of Political Science* 66.4, pp. 835–852.

Bleck, J. and N. Van de Walle (2019). *Electoral politics in Africa since 1990: Continuity in change*. Cambridge University Press.

Brewer, M. B. and R. M. Kramer (1985). "The psychology of intergroup attitudes and behavior". *Annual review of psychology* 36.1, pp. 219–243.

Brierley, S. and E. Kramon (2020). "Party campaign strategies in Ghana: Rallies, canvassing and handouts". *African Affairs* 119.477, pp. 587–603.

Canen, N., R. Ch, and L. Wantchekon (2023). "Political uncertainty and the forms of state capture". *Journal of Development Economics* 160, p. 102972.

Canen, N. and L. Wantchekon (2022). "Political distortions, state capture, and economic development in Africa". *Journal of Economic Perspectives* 36.1, pp. 101–124.

Carlin, R. E. and G. J. Love (2013). "The politics of interpersonal trust and reciprocity: An experimental approach". *Political Behavior* 35, pp. 43–63.

Carlson, E. (2015). "Ethnic voting and accountability in Africa: A choice experiment in Uganda". *World Politics* 67.2, pp. 353–385.

— (2016). "Finding partisanship where we least expect it: Evidence of partisan bias in a new African democracy". *Political Behavior* 38.1, pp. 129–154.

Cederman, L.-E., N. B. Weidmann, and K. S. Gleditsch (2011). "Horizontal inequalities and ethnonationalist civil war: A global comparison". *American political science review* 105.3, pp. 478–495.

Chandra, K. (2012). *Constructivist theories of ethnic politics*. Oxford University Press.

Cheeseman, N., G. Lynch, and J. Willis (2021). *The Moral Economy of Elections in Africa: Democracy, Voting and Virtue*. Cambridge University Press.

Choi, D. D., J. A. Harris, and F. Shen-Bayh (2022). "Ethnic Bias in Judicial Decision Making: Evidence from Criminal Appeals in Kenya". *American Political Science Review* 116.3, 1067–1080.

Choi, J.-K. and S. Bowles (2007). "The coevolution of parochial altruism and war". *science* 318.5850, pp. 636–640.

Cialdini, R. B., R. R. Reno, and C. A. Kallgren (1990). "A focus theory of normative conduct: Recycling the concept of norms to reduce littering in public places." *Journal of personality and social psychology* 58.6, p. 1015.

Cikara, M. and J. J. Van Bavel (2014). "The neuroscience of intergroup relations: An integrative review". *Perspectives on Psychological Science* 9.3, pp. 245–274.

Cooper, D. J. (2014). *Other regarding preferences: A selective survey of experimental results. Kagel JH, Roth AE, eds. Handbook of Experimental Economics, Vol. 2.*

Ejdemyr, S., E. Kramon, and A. L. Robinson (2018). "Segregation, ethnic favoritism, and the strategic targeting of local public goods". *Comparative Political Studies* 51.9, pp. 1111–1143.

Fearon, J. D. and D. D. Laitin (1996). "Explaining interethnic cooperation". *American political science review* 90.4, pp. 715–735.

Fehr, E., H. Bernhard, and B. Rockenbach (2008). "Egalitarianism in young children". *Nature* 454.7208, pp. 1079–1083.

Fehr, E. and K. M. Schmidt (1999). "A theory of fairness, competition, and cooperation". *The quarterly journal of economics* 114.3, pp. 817–868.

Ferejohn, J. (1999). "Accountability and authority: toward a theory of political accountability". In: *Democracy, Accountability, and Representation*. Ed. by A. Przeworski, S. C. Stokes, and B. Manin. Cambridge University Press, pp. 131–153.

Fjelde, H. and K. Höglund (2016). "Electoral institutions and electoral violence in sub-Saharan Africa". *British Journal of Political Science* 46.2, pp. 297–320.

French Bourgeois, L. and R. de la Sablonnière (2023). "Realigning individual behavior with societal values: The role of planning in injunctive-norm interventions aimed at increasing voter turnout". *Analyses of Social Issues and Public Policy* 23.1, pp. 155–173.

Gallego, J., J. Guardado, and L. Wantchekon (2023). "Do gifts buy votes? Evidence from sub-Saharan Africa and Latin America". *World Development* 162, p. 106125.

Gonzalez Ocantos, E., C. K. De Jonge, and D. W. Nickerson (2014). "The conditionality of vote-buying norms: Experimental evidence from Latin America". *American Journal of Political Science* 58.1, pp. 197–211.

Gottlieb, J. (2016). "Greater expectations: A field experiment to improve accountability in Mali". *American Journal of Political Science* 60.1, pp. 143–157.

Gottlieb, J., H. Larreguy, et al. (2020). "An informational theory of electoral targeting in young clientelistic democracies: Evidence from Senegal". *Quarterly Journal of Political Science* 15.1, pp. 73–104.

Harding, R. (2015). "Attribution and accountability: Voting for roads in Ghana". *World Politics* 67.4, pp. 656–689.

Harding, R. and K. Michelitch (2021). "Candidate coethnicity, rural/urban divides, and partisanship in Africa". *Party Politics* 27.4, pp. 791–802.

Harding, R. and D. Stasavage (2014). "What democracy does (and doesn't do) for basic services: School fees, school inputs, and African elections". *The Journal of Politics* 76.1, pp. 229–245.

Harris, J. A. (2015). "What's in a name? A method for extracting information about ethnicity from names". *Political Analysis* 23.2, pp. 212–224.

Hassan, M., H. Larreguy, and S. Russell (2023). "Who Gets Hired? Political Patronage and Bureaucratic Favoritism". *American Political Science Review*, pp. 1–18.

Hicken, A. (2011). "Clientelism". *Annual review of political science* 14, pp. 289–310.

Hicken, A. and N. L. Nathan (2020). "Clientelism's red herrings: dead ends and new directions in the study of nonprogrammatic politics". *Annual Review of Political Science* 23, pp. 277–294.

Hidalgo, F. D. and S. Nichter (2016). "Voter buying: Shaping the electorate through clientelism". *American Journal of Political Science* 60.2, pp. 436–455.

Holland, A. C. (2016). "Forbearance". *American political science review* 110.2, pp. 232–246.

Horowitz, D. L. (1985). *Ethnic groups in conflict*. Univ of California Press.

Horowitz, J. (2019). "Ethnicity and the swing vote in Africa's emerging democracies: Evidence from Kenya". *British Journal of Political Science* 49.3, pp. 901–921.

— (2022). *Multiethnic democracy: The logic of elections and policymaking in Kenya*. Oxford University Press.

Horowitz, J. and K. Klaus (2020). "Can politicians exploit ethnic grievances? An experimental study of land appeals in Kenya". *Political Behavior* 42.1, pp. 35–58.

Horowitz, J. and K. Michelitch (2021). "Beliefs about Clientelism are Partisan and Sticky, even if Desire for Impartial Government is Not". *American Political Science Association Annual Conference Paper*.

Ichino, N. and N. L. Nathan (2013). "Crossing the line: Local ethnic geography and voting in Ghana". *American Political Science Review* 107.2, pp. 344–361.

Iyengar, S., Y. Lelkes, M. Levendusky, N. Malhotra, and S. J. Westwood (2019). "The origins and consequences of affective polarization in the United States". *Annual review of political science* 22.1, pp. 129–146.

Jones, E. E. and V. A. Harris (1967). "The attribution of attitudes". *Journal of experimental social psychology* 3.1, pp. 1–24.

Kallgren, C. A., R. R. Reno, and R. B. Cialdini (2000). "A focus theory of normative conduct: When norms do and do not affect behavior". *Personality and social psychology bulletin* 26.8, pp. 1002–1012.

Keefer, P. and S. Khemani (2004). "Why do the poor receive poor services?" *Economic and Political Weekly*, pp. 935–943.

Kim, H.-S. and J. Horowitz (2022). "Rejecting ethnic pandering in urban Africa: A survey experiment on voter preferences in Nairobi, Kenya". *Political Research Quarterly* 75.4, pp. 1240–1254.

Kitschelt, H. and S. I. Wilkinson (2007). *Patrons, clients and policies: Patterns of democratic accountability and political competition*. Cambridge University Press.

Klaus, K. and M. I. Mitchell (2015). "Land grievances and the mobilization of electoral violence: Evidence from Côte d'Ivoire and Kenya". *Journal of Peace Research* 52.5, pp. 622–635.

Klaus, K., J. W. Paller, and M. Wilfahrt (2023). "Demanding recognition: a new Framework for the Study of Political Clientelism". *African Affairs* 122.487, pp. 185–203.

Kramon, E. (2016). "Electoral handouts as information: Explaining unmonitored vote buying". *World Politics* 68.3, pp. 454–498.

Kramon, E. and D. N. Posner (2013). "Who benefits from distributive politics? How the outcome one studies affects the answer one gets". *Perspectives on Politics* 11.2, pp. 461–474.

Lemarchand, R. (1972). "Political clientelism and ethnicity in tropical africa:* competing solidarities in nation-building". *American political science review* 66.1, pp. 68–90.

Lindberg, S. I. (2010). "What accountability pressures do MPs in Africa face and how do they respond? Evidence from Ghana". *The Journal of Modern African Studies* 48.1, pp. 117–142.

Lindberg, S. I., M. C. L. Bue, and K. Sen (2022). "Clientelism, corruption and the rule of law". *World Development* 158, p. 105989.

Michelitch, K. (2015). "Does electoral competition exacerbate interethnic or interpartisan economic discrimination? Evidence from a field experiment in market price bargaining". *American Political Science Review* 109.1, pp. 43–61.

Michelitch, K. and S. Utych (2018). "Electoral cycle fluctuations in partisanship: Global evidence from eighty-six countries". *The Journal of Politics* 80.2, pp. 412–427.

Nichter, S. (2018). *Votes for survival: Relational clientelism in Latin America*. Cambridge University Press.

Ochieng'Opalo, K. (2022). "Formalizing clientelism in Kenya: from harambee to the constituency development fund". *World Development* 152, p. 105794.

Oyugi, W. O. (1997). "Ethnicity in the electoral process: The 1992 general elections in Kenya". *African Journal of Political Science/Revue Africaine de Science Politique*, pp. 41–69.

Paluck, E. L. and D. P. Green (2009). "Prejudice reduction: What works? A review and assessment of research and practice". *Annual review of psychology* 60.1, pp. 339–367.

Pellicer, M. and E. Wegner (2023). "What is bad about clientelism? Citizen perceptions in poor communities in South Africa and Tunisia". *Electoral Studies* 84, p. 102620.

Piliavsky, A. (2014). *Patronage as politics in South Asia*. Cambridge University Press.

Posner, D. N. (2005). *Institutions and ethnic politics in Africa*. Cambridge University Press.

— (2007). "Regime change and ethnic cleavages in Africa". *Comparative political studies* 40.11, pp. 1302–1327.

Przeworski, A. (2010). *Democracy and the limits of self-government*. 9. Cambridge University Press.

Riach, P. A. and J. Rich (2002). "Field experiments of discrimination in the market place". *The economic journal* 112.483, F480–F518.

Robinson, A. L. (2020). "Ethnic diversity, segregation and ethnocentric trust in Africa". *British Journal of Political Science* 50.1, pp. 217–239.

Robinson, J. A. and T. Verdier (2013). "The political economy of clientelism". *The Scandinavian Journal of Economics* 115.2, pp. 260–291.

Stokes, S. C. (2005). "Perverse accountability: A formal model of machine politics with evidence from Argentina". *American political science review* 99.3, pp. 315–325.

Tajfel, H. and J. C. Turner (2004). "The social identity theory of intergroup behavior". In: *Political psychology*. Psychology Press, pp. 276–293.

Tajfel, H., J. C. Turner, W. G. Austin, and S. Worchel (1979). "An integrative theory of intergroup conflict". *Organizational identity: A reader* 56.65, pp. 9780203505984–16.

Trahan, A. and K. Laird (2018). "The nexus between attribution theory and racial attitudes: a test of racial attribution and public opinion of capital punishment". *Criminal Justice Studies* 31.4, pp. 352–367.

VandeWalle, N. (2007). "Meet the new boss, same as the old boss? The evolution of political clientelism in Africa". *Patrons, clients and policies: Patterns of democratic accountability and political competition* 1, pp. 50–67.

Wantchekon, L. (2003). "Clientelism and voting behavior: Evidence from a field experiment in Benin". *World politics* 55.3, pp. 399–422.

Weghorst, K. R. and S. I. Lindberg (2013). "What drives the swing voter in Africa?" *American Journal of Political Science* 57.3, pp. 717–734.

Weitz-Shapiro, R. (2012). "What wins votes: Why some politicians opt out of clientelism". *American journal of political science* 56.3, pp. 568–583.

Whitt, S. and R. K. Wilson (2007). "The dictator game, fairness and ethnicity in postwar Bosnia". *American journal of political science* 51.3, pp. 655–668.

Who is Targeted and by Whom? Supplemental Information

A Descriptive Statistics

Table A.3 reports descriptive statistics for the action-specific outcome variables used in the analysis.

Table A.3: Descriptive statistics of the outcome variables

	Mean	SD	Min	Max	N
MP: Cash at rally	1.999	2.450	0	10	1784
MP: Transport	1.310	2.332	0	10	1787
MP: CDF	3.498	3.319	0	10	1781
Pres: Appeal	4.342	3.716	0	10	1764
Pres: Funds	3.282	3.400	0	10	1774
Pres: Schools	2.791	3.179	0	10	1771
Buro: Connect	2.403	2.886	0	10	1777
Buro: Contract	3.242	2.765	0	10	1770
Judge: Rig	4.717	3.239	0	10	1782
Judge: Steal	4.245	2.899	0	10	1782
Judge: Job	4.148	2.954	0	10	1779
Police: Traffic	3.692	2.787	0	10	1782
Shop: Appeal	3.730	3.266	0	10	1775
Shop: Contract	3.295	2.628	0	10	1773

B Action-level Results

In this section, we report our complete pre-registered main analysis. Section B.1 reports the results for the first part of HP1, comparing the coethnic targeting condition to the copartisan targeting condition. Here we code "coethnic targeting" as the treatment, "copartisan targeting" as the control, and exclude the unspecified condition. Section B.2 reports the action-level results for HP2 (unshared identity), and Section B.3 reports the results from interacting coethnic targeting (versus copartisan targeting) and unshared identity. Also In these analyses, the unspecified targeting group is excluded.

Each table shows the results for the action-specific punishment, the results on the actor-specific ICW index (which, for the unshared identity effects, replicate those of Table 2), and the results on an alternative actor-specific index defined as the first principal component of the action variables.

In every table, we report three specifications. In Panel (A), the base (unconditional) treatment effect estimates. In Panel (B), we report the regression-adjusted estimates. We control for the following covariates: age, gender, an index of socio-economic status, urban residency, ethnicity/partisanship. We use the specification proposed by Lin (2013), where we center all the covariates and interact them with the treatment indicator. When we study the average effect of one treatment in isolation, we add the excluded treatment indicator to the controls, after centering and interacting it. Finally, in Panel (C) we estimate effects using a binary version of the outcomes, that takes value 1 when any punishment is given to an action as opposed to none at all.

In Panel (A) and (B) of the tables, the outcome variable is standardized using the mean and standard deviation of the relevant control group, so that the treatment effects can be interpreted in terms of control standard deviations, and are comparable across columns and tables. For reference, we also report the raw means and standard deviations in the control group or in the full sample.

B.1 Coethnic versus Copartisan Targeting

Table B.1: Effects of coethnic targeting versus copartisan targeting, MP

	Handout	Transport	CDF	ICW	PC1
(A): Base					
Target coethnic	0.033	0.195**	0.022	0.094	0.114+
	(0.058)	(0.060)	(0.057)	(0.058)	(0.059)
Num.Obs.	1203	1204	1201	1212	1188
R2	0.000	0.009	0.000	0.002	0.003
R2 Adj.	-0.001	0.008	-0.001	0.001	0.002
Mean Y_c	1.991	1.175	4.417	2.033	0.103
SD Y_c	2.491	2.249	3.197	1.970	1.308
(B): Covariates					
Target coethnic	0.026	0.192**	0.015	0.088	0.107+
	(0.057)	(0.059)	(0.056)	(0.057)	(0.058)
Age_c	0.006+	0.005	-0.000	0.005	0.005
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
$Male_c$	-0.160+	-0.271***	-0.153+	-0.253**	-0.254**
	(0.084)	(0.077)	(0.081)	(0.080)	(0.081)
SES $Index_c$	0.041	-0.072	0.009	-0.015	0.000
	(0.129)	(0.118)	(0.114)	(0.117)	(0.120)
$Urban_c$	0.044	-0.072	0.113	0.041	0.035
	(0.084)	(0.081)	(0.082)	(0.081)	(0.083)
$Luo/NASA_c$	0.009	-0.010	-0.007	-0.009	-0.007
	(0.090)	(0.088)	(0.089)	(0.087)	(0.089)
Unshared EP_c	0.241**	0.268**	0.391***	0.404***	0.380***
	(0.085)	(0.082)	(0.081)	(0.082)	(0.083)
Target coethnic × Age_c	-0.001	-0.005	0.001	-0.003	-0.002
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Target coethnic × $Male_c$	0.168	0.177	0.015	0.162	0.156
	(0.115)	(0.118)	(0.112)	(0.113)	(0.115)
Target coethnic × SES $Index_c$	0.032	0.058	0.132	0.109	0.086
	(0.176)	(0.176)	(0.155)	(0.162)	(0.167)
Target coethnic × $Urban_c$	-0.157	0.021	0.049	-0.043	-0.037
	(0.115)	(0.122)	(0.114)	(0.115)	(0.118)
Target coethnic × $Luo/NASA_c$	-0.033	-0.098	-0.057	-0.076	-0.075
	(0.125)	(0.128)	(0.123)	(0.121)	(0.124)
Target coethnic × Unshared EP_c	0.030	-0.022	-0.173	-0.083	-0.062
	(0.115)	(0.119)	(0.112)	(0.114)	(0.116)
Num.Obs.	1203	1204	1201	1212	1188
R2	0.026	0.037	0.040	0.047	0.045
R2 Adj.	0.015	0.026	0.029	0.037	0.034
Mean Y_c	1.991	1.175	4.417	2.033	0.103
SD Y_c	2.491	2.249	3.197	1.970	1.308
(C): Binary outcome (y>0)					
Target coethnic	0.015	0.130***	0.035*		
	(0.026)	(0.028)	(0.015)		
Num.Obs.	1203	1204	1201		
R2	0.000	0.017	0.005		
R2 Adj.	-0.001	0.016	0.004		
Mean Y (full)	1.999	1.310	3.498		
SD Y (full)	2.450	2.332	3.319		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.2: Effects of coethnic targeting versus copartisan targeting, President

	Appeal	Funds	Schools	ICW	PC1
(A): Base					
Target coethnic	0.148*	0.062	0.246***	0.190**	0.186**
	(0.059)	(0.060)	(0.059)	(0.060)	(0.061)
Num.Obs.	1173	1180	1175	1189	1158
R2	0.005	0.001	0.015	0.008	0.008
R2 Adj.	0.004	0.000	0.014	0.008	0.007
Mean Y_c	4.785	3.594	3.315	3.731	0.218
SD Y_c	3.579	3.297	3.158	2.708	1.363
(B): Covariates					
Target coethnic	0.162**	0.083	0.259***	0.211***	0.207***
	(0.058)	(0.059)	(0.058)	(0.058)	(0.059)
Age_c	-0.004	-0.000	0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
$Male_c$	-0.022	-0.038	-0.103	-0.062	-0.077
	(0.086)	(0.087)	(0.087)	(0.086)	(0.087)
SES $Index_c$	0.127	-0.087	0.077	0.070	0.018
	(0.125)	(0.130)	(0.123)	(0.126)	(0.127)
$Urban_c$	0.007	0.174*	-0.048	0.044	0.072
	(0.084)	(0.085)	(0.085)	(0.083)	(0.084)
$Luo/NASA_c$	-0.023	0.083	0.060	0.050	0.042
	(0.093)	(0.092)	(0.090)	(0.091)	(0.091)
Unshared EP_c	0.459***	0.454***	0.384***	0.522***	0.544***
	(0.083)	(0.082)	(0.083)	(0.082)	(0.083)
Target coethnic × Age_c	-0.003	0.006	-0.002	0.000	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Target coethnic × $Male_c$	-0.078	-0.142	-0.036	-0.107	-0.097
	(0.119)	(0.120)	(0.119)	(0.120)	(0.121)
Target coethnic × SES $Index_c$	0.011	-0.075	-0.205	-0.129	-0.104
	(0.166)	(0.175)	(0.167)	(0.169)	(0.173)
Target coethnic × $Urban_c$	-0.033	-0.116	0.060	-0.018	-0.069
	(0.117)	(0.119)	(0.118)	(0.117)	(0.118)
Target coethnic × $Luo/NASA_c$	-0.107	-0.112	-0.142	-0.142	-0.157
	(0.126)	(0.128)	(0.124)	(0.127)	(0.128)
Target coethnic × Unshared EP_c	0.089	0.011	0.016	0.057	0.044
	(0.115)	(0.116)	(0.115)	(0.116)	(0.117)
Num.Obs.	1173	1180	1175	1189	1158
R2	0.080	0.063	0.058	0.086	0.090
R2 Adj.	0.069	0.053	0.047	0.075	0.079
Mean Y_c	4.785	3.594	3.315	3.731	0.218
SD Y_c	3.579	3.297	3.158	2.708	1.363
(C): Binary outcome (y>0)					
Target coethnic	0.026	0.016	0.108***		
	(0.019)	(0.023)	(0.021)		
Num.Obs.	1173	1180	1175		
R2	0.002	0.000	0.022		
R2 Adj.	0.001	0.000	0.021		
Mean Y (full)	4.342	3.282	2.791		
SD Y (full)	3.716	3.400	3.179		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.3: Effects of coethnic targeting versus copartisan targeting, Bureaucrat

	Connect	Contract	ICW	PC1
(A): Base				
Target coethnic	-0.134*	0.024	-0.061	-0.062
	(0.058)	(0.058)	(0.058)	(0.058)
Num.Obs.	1180	1180	1185	1175
R2	0.005	0.000	0.001	0.001
R2 Adj.	0.004	-0.001	0.000	0.000
Mean Y_c	2.898	3.359	3.147	0.138
SD Y_c	2.959	2.822	2.605	1.293
(B): Covariates				
Target coethnic	-0.131*	0.027	-0.059	-0.059
	(0.057)	(0.058)	(0.058)	(0.058)
Age_c	0.005	0.003	0.004	0.005
	(0.004)	(0.004)	(0.004)	(0.004)
$Male_c$	-0.119	0.131	0.006	0.003
	(0.083)	(0.084)	(0.083)	(0.083)
SES $Index_c$	-0.047	0.046	-0.001	-0.001
	(0.130)	(0.124)	(0.125)	(0.125)
$Urban_c$	0.097	0.065	0.092	0.090
	(0.084)	(0.085)	(0.084)	(0.085)
$Luo/NASA_c$	-0.094	-0.028	-0.071	-0.064
	(0.090)	(0.092)	(0.091)	(0.091)
Unshared EP_c	0.326***	0.257**	0.322***	0.326***
	(0.082)	(0.084)	(0.083)	(0.083)
Target coethnic × Age_c	0.001	-0.001	0.000	0.000
	(0.005)	(0.005)	(0.005)	(0.005)
Target coethnic × $Male_c$	0.009	-0.186	-0.100	-0.095
	(0.115)	(0.117)	(0.116)	(0.116)
Target coethnic × SES $Index_c$	-0.039	0.135	0.044	0.061
	(0.166)	(0.164)	(0.164)	(0.163)
Target coethnic × $Urban_c$	0.016	0.132	0.071	0.090
	(0.116)	(0.118)	(0.117)	(0.117)
Target coethnic × $Luo/NASA_c$	0.200	0.074	0.147	0.157
	(0.126)	(0.126)	(0.125)	(0.126)
Target coethnic × Unshared EP_c	0.014	-0.076	-0.044	-0.026
	(0.114)	(0.116)	(0.115)	(0.115)
Num.Obs.	1180	1180	1185	1175
R2	0.046	0.023	0.032	0.035
R2 Adj.	0.036	0.012	0.021	0.024
Mean Y_c	2.898	3.359	3.147	0.138
SD Y_c	2.959	2.822	2.605	1.293
(C): Binary outcome (y>0)				
Target coethnic	-0.126***	0.018		
	(0.027)	(0.018)		
Num.Obs.	1180	1180		
R2	0.019	0.001		
R2 Adj.	0.018	0.000		
Mean Y (full)	2.403	3.242		
SD Y (full)	2.886	2.765		

Notes

All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, *<0.05, ** <0.01, *** <0.001

Table B.4: Effects of coethnic targeting versus copartisan targeting, Judge

	Rig	Steal	Give job	ICW	PC1
(A): Base					
Target coethnic	0.011	0.002	0.055	0.032	0.024
	(0.058)	(0.057)	(0.058)	(0.059)	(0.059)
Num.Obs.	1192	1193	1189	1196	1184
R2	0.000	0.000	0.001	0.000	0.000
R2 Adj.	-0.001	-0.001	0.000	-0.001	-0.001
Mean Y_c	4.560	4.254	4.035	4.228	-0.040
SD Y_c	3.208	2.941	2.952	2.388	1.371
(B): Covariates					
Target coethnic	0.002	-0.012	0.054	0.021	0.014
	(0.057)	(0.057)	(0.058)	(0.058)	(0.058)
Age_c	0.006+	0.012***	0.005	0.010**	0.010**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	-0.179*	-0.131	-0.038	-0.144+	-0.152+
	(0.080)	(0.080)	(0.081)	(0.080)	(0.081)
SES $Index_c$	-0.213+	-0.098	0.024	-0.122	-0.110
	(0.114)	(0.114)	(0.107)	(0.108)	(0.110)
$Urban_c$	0.002	0.048	0.097	0.076	0.048
	(0.082)	(0.082)	(0.083)	(0.082)	(0.082)
$Luo/NASA_c$	-0.060	-0.102	-0.171*	-0.142+	-0.136
	(0.089)	(0.086)	(0.087)	(0.085)	(0.086)
Unshared EP_c	0.086	0.167*	0.019	0.114	0.113
	(0.079)	(0.079)	(0.080)	(0.078)	(0.079)
Target coethnic × Age_c	-0.000	-0.001	-0.004	-0.003	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Target coethnic × $Male_c$	0.125	0.029	-0.015	0.053	0.069
	(0.116)	(0.116)	(0.118)	(0.118)	(0.118)
Target coethnic × SES $Index_c$	0.353*	0.098	0.102	0.230	0.227
	(0.164)	(0.164)	(0.160)	(0.164)	(0.165)
Target coethnic × $Urban_c$	0.077	0.083	-0.107	0.006	0.037
	(0.118)	(0.117)	(0.119)	(0.120)	(0.120)
Target coethnic × $Luo/NASA_c$	0.238+	0.254*	0.183	0.280*	0.284*
	(0.127)	(0.123)	(0.129)	(0.127)	(0.127)
Target coethnic × Unshared EP_c	0.278*	-0.197+	0.216+	0.125	0.114
	(0.115)	(0.114)	(0.116)	(0.116)	(0.116)
Num.Obs.	1192	1193	1189	1196	1184
R2	0.034	0.029	0.016	0.026	0.027
R2 Adj.	0.023	0.019	0.005	0.016	0.016
Mean Y_c	4.560	4.254	4.035	4.228	-0.040
SD Y_c	3.208	2.941	2.952	2.388	1.371
(C): Binary outcome (y>0)					
Target coethnic	-0.011	0.010	0.004		
	(0.017)	(0.015)	(0.013)		
Num.Obs.	1192	1193	1189		
R2	0.000	0.000	0.000		
R2 Adj.	0.000	0.000	-0.001		
Mean Y (full)	4.717	4.245	4.148		
SD Y (full)	3.239	2.899	2.954		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.5: Effects of coethnic targeting versus copartisan targeting, Police officer

	Traffic
(A): Base	
Target coethnic	0.045
	(0.059)
Num.Obs.	1185
R2	0.000
R2 Adj.	0.000
Mean Y_c	3.752
SD Y_c	2.787
(B): Covariates	
Target coethnic	0.044
	(0.059)
Age_c	-0.001
	(0.003)
$Male_c$	-0.050
	(0.082)
SES $Index_c$	0.010
	(0.117)
$Urban_c$	-0.098
	(0.083)
Luo/$NASA_c$	-0.101
	(0.086)
Unshared EP_c	0.109
	(0.082)
Target coethnic × Age_c	-0.003
	(0.005)
Target coethnic × $Male_c$	-0.044
	(0.119)
Target coethnic × SES $Index_c$	-0.008
	(0.170)
Target coethnic × $Urban_c$	-0.043
	(0.122)
Target coethnic × Luo/$NASA_c$	0.095
	(0.130)
Target coethnic × Unshared EP_c	0.079
	(0.119)
Num.Obs.	1185
R2	0.012
R2 Adj.	0.001
Mean Y_c	3.752
SD Y_c	2.787
(C): Binary outcome (y>0)	
Target coethnic	-0.010
	(0.010)
Num.Obs.	1185
R2	0.001
R2 Adj.	0.000
Mean Y (full)	3.692
SD Y (full)	2.787

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.6: Effects of coethnic targeting versus copartisan targeting, Shopkeeper

	Appeal	Contract	ICW	PC1
(A): Base				
Target coethnic	0.113+	-0.014	0.052	0.053
	(0.060)	(0.058)	(0.059)	(0.059)
Num.Obs.	1174	1174	1182	1166
R2	0.003	0.000	0.001	0.001
R2 Adj.	0.002	-0.001	0.000	0.000
Mean Y_c	3.692	3.344	3.409	0.003
SD Y_c	3.180	2.615	2.520	1.240
(B): Covariates				
Target coethnic	0.111+	-0.016	0.051	0.051
	(0.059)	(0.058)	(0.059)	(0.059)
Age_c	-0.003	-0.000	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	0.101	0.119	0.122	0.125
	(0.080)	(0.080)	(0.080)	(0.081)
SES $Index_c$	0.160	0.105	0.139	0.155
	(0.109)	(0.107)	(0.108)	(0.108)
$Urban_c$	-0.019	0.120	0.052	0.066
	(0.082)	(0.081)	(0.082)	(0.082)
$Luo/NASA_c$	-0.029	-0.123	-0.096	-0.078
	(0.086)	(0.084)	(0.085)	(0.086)
Unshared EP_c	0.187*	0.053	0.130	0.134+
	(0.080)	(0.080)	(0.080)	(0.081)
Target coethnic × Age_c	0.005	0.006	0.006	0.006
	(0.005)	(0.005)	(0.005)	(0.005)
Target coethnic × $Male_c$	-0.019	-0.076	-0.063	-0.044
	(0.120)	(0.116)	(0.119)	(0.119)
Target coethnic × SES $Index_c$	0.042	-0.037	0.012	-0.013
	(0.164)	(0.162)	(0.163)	(0.167)
Target coethnic × $Urban_c$	0.143	-0.030	0.063	0.058
	(0.120)	(0.116)	(0.119)	(0.120)
Target coethnic × $Luo/NASA_c$	-0.045	0.023	-0.006	-0.016
	(0.129)	(0.122)	(0.126)	(0.127)
Target coethnic × Unshared EP_c	0.186	0.098	0.161	0.152
	(0.118)	(0.116)	(0.117)	(0.118)
Num.Obs.	1174	1174	1182	1166
R2	0.035	0.016	0.025	0.026
R2 Adj.	0.024	0.005	0.014	0.015
Mean Y_c	3.692	3.344	3.409	0.003
SD Y_c	3.180	2.615	2.520	1.240
(C): Binary outcome (y>0)				
Target coethnic	0.005	0.006		
	(0.021)	(0.016)		
Num.Obs.	1174	1174		
R2	0.000	0.000		
R2 Adj.	-0.001	-0.001		
Mean Y (full)	3.730	3.295		
SD Y (full)	3.266	2.628		

Notes

All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

B.2 Unshared versus Shared Identity

Table B.7: Effects of unshared ethnopartisanship, MP

	Handout	Transport	CDF	ICW	PC1
(A): Base					
Unshared EP	0.280***	0.229***	0.199***	0.308***	0.300***
	(0.053)	(0.052)	(0.050)	(0.051)	(0.051)
Num.Obs.	1784	1787	1781	1795	1766
R2	0.016	0.011	0.009	0.020	0.019
R2 Adj.	0.015	0.010	0.008	0.020	0.019
Mean Y_c	1.695	1.071	3.186	1.661	-0.187
SD Y_c	2.182	2.096	3.141	1.813	1.213
(B): Covariates					
Unshared EP	0.281***	0.230***	0.232***	0.328***	0.313***
	(0.053)	(0.052)	(0.045)	(0.049)	(0.050)
Age_c	0.005+	0.003	0.004	0.005+	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	0.061	-0.042	-0.090	-0.040	-0.027
	(0.067)	(0.068)	(0.062)	(0.065)	(0.067)
SES $Index_c$	0.103	0.053	0.070	0.097	0.096
	(0.101)	(0.095)	(0.096)	(0.096)	(0.098)
$Urban_c$	-0.039	0.003	0.099	0.033	0.031
	(0.068)	(0.067)	(0.062)	(0.065)	(0.067)
$Luo/NASA_c$	-0.064	-0.001	-0.074	-0.067	-0.062
	(0.074)	(0.072)	(0.068)	(0.070)	(0.071)
$(Target2)_c$	0.042	-0.036	0.776***	0.429***	0.318***
	(0.082)	(0.077)	(0.073)	(0.078)	(0.079)
$(Target3)_c$	0.049	0.181*	0.879***	0.564***	0.460***
	(0.083)	(0.086)	(0.074)	(0.080)	(0.082)
Unshared EP × Age_c	0.002	-0.000	-0.002	0.001	-0.001
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Unshared EP × $Male_c$	-0.216*	-0.268*	-0.112	-0.247*	-0.260**
	(0.106)	(0.104)	(0.091)	(0.097)	(0.099)
Unshared EP × SES $Index_c$	-0.170	-0.218	-0.089	-0.200	-0.211
	(0.161)	(0.152)	(0.128)	(0.140)	(0.145)
Unshared EP × $Urban_c$	0.052	0.042	0.067	0.076	0.067
	(0.107)	(0.105)	(0.090)	(0.098)	(0.100)
Unshared EP × $Luo/NASA_c$	0.101	-0.021	0.072	0.066	0.067
	(0.116)	(0.113)	(0.099)	(0.104)	(0.107)
Unshared EP × $(Target2)_c$	0.012	0.155	0.325**	0.247*	0.210+
	(0.133)	(0.124)	(0.109)	(0.119)	(0.121)
Unshared EP × $(Target3)_c$	0.061	0.148	0.150	0.164	0.159
	(0.127)	(0.128)	(0.106)	(0.117)	(0.120)
Num.Obs.	1784	1787	1781	1795	1766
R2	0.025	0.035	0.200	0.105	0.081
R2 Adj.	0.017	0.027	0.193	0.098	0.074
Mean Y_c	1.695	1.071	3.186	1.661	-0.187
SD Y_c	2.182	2.096	3.141	1.813	1.213
(C): Binary outcome (y>0)					
Unshared EP	0.087***	0.081***	0.008		
	(0.022)	(0.023)	(0.021)		
Num.Obs.	1784	1787	1781		
R2	0.009	0.007	0.000		
R2 Adj.	0.008	0.006	0.000		
Mean Y (full)	1.999	1.310	3.498		
SD Y (full)	2.450	2.332	3.319		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.8: Effects of unshared ethnopartisanship, President

	Appeal	Funds	Schools	ICW	PC1
(A): Base					
Unshared EP	0.459***	0.387***	0.337***	0.490***	0.492***
	(0.050)	(0.052)	(0.052)	(0.051)	(0.052)
Num.Obs.	1764	1774	1771	1787	1747
R2	0.045	0.031	0.023	0.049	0.049
R2 Adj.	0.045	0.030	0.023	0.048	0.049
Mean Y_c	3.533	2.673	2.293	2.701	-0.324
SD Y_c	3.454	3.072	2.887	2.522	1.278
(B): Covariates					
Unshared EP	0.448***	0.383***	0.317***	0.476***	0.479***
	(0.048)	(0.051)	(0.046)	(0.048)	(0.048)
Age_c	-0.001	0.008**	0.003	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	0.145*	0.097	0.010	0.111+	0.101
	(0.068)	(0.069)	(0.065)	(0.066)	(0.067)
SES $Index_c$	0.163+	-0.014	-0.016	0.069	0.050
	(0.096)	(0.095)	(0.096)	(0.091)	(0.093)
$Urban_c$	0.141*	0.049	0.011	0.089	0.083
	(0.066)	(0.068)	(0.063)	(0.065)	(0.065)
$Luo/NASA_c$	-0.153*	-0.053	-0.011	-0.088	-0.099
	(0.071)	(0.073)	(0.066)	(0.069)	(0.070)
$(Target2)_c$	0.426***	0.183*	0.640***	0.512***	0.509***
	(0.081)	(0.084)	(0.075)	(0.079)	(0.080)
$(Target3)_c$	0.553***	0.267***	0.916***	0.703***	0.710***
	(0.077)	(0.080)	(0.072)	(0.074)	(0.075)
Unshared EP × Age_c	0.003	0.000	-0.001	0.001	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Unshared EP × $Male_c$	-0.275**	-0.225*	-0.223*	-0.302**	-0.296**
	(0.098)	(0.103)	(0.095)	(0.097)	(0.098)
Unshared EP × SES $Index_c$	-0.011	-0.112	-0.045	-0.047	-0.097
	(0.137)	(0.145)	(0.138)	(0.136)	(0.138)
Unshared EP × $Urban_c$	-0.211*	0.009	-0.160+	-0.151	-0.148
	(0.097)	(0.103)	(0.094)	(0.096)	(0.097)
Unshared EP × $Luo/NASA_c$	0.189+	0.181	0.001	0.158	0.150
	(0.105)	(0.111)	(0.100)	(0.105)	(0.106)
Unshared EP × $(Target2)_c$	0.210+	0.332**	0.345**	0.342**	0.379**
	(0.118)	(0.125)	(0.110)	(0.115)	(0.115)
Unshared EP × $(Target3)_c$	0.296*	0.333**	0.349**	0.394***	0.413***
	(0.116)	(0.124)	(0.107)	(0.114)	(0.115)
Num.Obs.	1764	1774	1771	1787	1747
R2	0.139	0.078	0.213	0.184	0.189
R2 Adj.	0.131	0.070	0.206	0.177	0.182
Mean Y_c	3.533	2.673	2.293	2.701	-0.324
SD Y_c	3.454	3.072	2.887	2.522	1.278
(C): Binary outcome (y>0)					
Unshared EP	0.079***	0.074***	0.076***		
	(0.019)	(0.022)	(0.023)		
Num.Obs.	1764	1774	1771		
R2	0.010	0.007	0.006		
R2 Adj.	0.009	0.006	0.006		
Mean Y (full)	4.342	3.282	2.791		
SD Y (full)	3.716	3.400	3.179		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.9: Effects of unshared ethnopartisanship, Bureaucrat

	Connect	Contract	ICW	PC1
(A): Base				
Unshared EP	0.363***	0.247***	0.334***	0.352***
	(0.053)	(0.050)	(0.052)	(0.052)
Num.Obs.	1777	1770	1784	1763
R2	0.026	0.013	0.023	0.025
R2 Adj.	0.025	0.013	0.022	0.024
Mean Y_c	1.935	2.920	2.474	-0.210
SD Y_c	2.556	2.591	2.270	1.119
(B): Covariates				
Unshared EP	0.360***	0.246***	0.332***	0.349***
	(0.052)	(0.050)	(0.051)	(0.052)
Age_c	0.004	0.006*	0.006*	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	-0.084	0.037	-0.022	-0.023
	(0.067)	(0.067)	(0.067)	(0.067)
SES $Index_c$	-0.020	0.333***	0.197*	0.192*
	(0.090)	(0.085)	(0.086)	(0.088)
$Urban_c$	0.004	0.099	0.060	0.064
	(0.067)	(0.068)	(0.067)	(0.068)
$Luo/NASA_c$	-0.085	0.005	-0.039	-0.045
	(0.074)	(0.071)	(0.072)	(0.072)
$(Target2)_c$	0.380***	0.163*	0.297***	0.304***
	(0.079)	(0.078)	(0.079)	(0.079)
$(Target3)_c$	0.215**	0.237**	0.267***	0.249**
	(0.079)	(0.081)	(0.079)	(0.080)
Unshared EP × Age_c	0.001	-0.002	-0.001	-0.000
	(0.004)	(0.004)	(0.004)	(0.004)
Unshared EP × $Male_c$	-0.114	-0.008	-0.048	-0.081
	(0.105)	(0.102)	(0.104)	(0.105)
Unshared EP × SES $Index_c$	-0.124	-0.485***	-0.370*	-0.358*
	(0.154)	(0.138)	(0.146)	(0.149)
Unshared EP × $Urban_c$	0.045	0.029	0.050	0.035
	(0.105)	(0.101)	(0.104)	(0.105)
Unshared EP × $Luo/NASA_c$	0.229*	-0.052	0.067	0.112
	(0.114)	(0.107)	(0.111)	(0.112)
Unshared EP × $(Target2)_c$	0.066	0.005	0.033	0.042
	(0.128)	(0.122)	(0.125)	(0.127)
Unshared EP × $(Target3)_c$	0.085	-0.087	-0.025	0.010
	(0.124)	(0.120)	(0.121)	(0.122)
Num.Obs.	1777	1770	1784	1763
R2	0.061	0.033	0.047	0.051
R2 Adj.	0.053	0.025	0.039	0.043
Mean Y_c	1.935	2.920	2.474	-0.210
SD Y_c	2.556	2.591	2.270	1.119
(C): Binary outcome (y>0)				
Unshared EP	0.094***	0.012		
	(0.023)	(0.014)		
Num.Obs.	1777	1770		
R2	0.010	0.000		
R2 Adj.	0.009	0.000		
Mean Y (full)	2.403	3.242		
SD Y (full)	2.886	2.765		

Notes
All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.10: Effects of unshared ethnopartisanship, Judge

	Rig	Steal	Give job	ICW	PC1
(A): Base					
Unshared EP	0.180***	0.073	0.100*	0.149**	0.142**
	(0.048)	(0.049)	(0.049)	(0.048)	(0.049)
Num.Obs.	1782	1782	1779	1788	1771
R2	0.008	0.001	0.002	0.005	0.005
R2 Adj.	0.007	0.001	0.002	0.005	0.004
Mean Y_c	4.440	4.146	4.011	4.152	-0.091
SD Y_c	3.181	2.822	2.838	2.354	1.358
(B): Covariates					
Unshared EP	0.182***	0.067	0.097*	0.147**	0.138**
	(0.048)	(0.049)	(0.049)	(0.048)	(0.048)
Age_c	0.004	0.011***	0.003	0.007*	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	-0.076	-0.097	-0.001	-0.070	-0.075
	(0.067)	(0.067)	(0.067)	(0.067)	(0.068)
SES $Index_c$	0.121	0.055	0.104	0.108	0.130
	(0.096)	(0.094)	(0.091)	(0.094)	(0.094)
$Urban_c$	0.049	0.040	0.107	0.085	0.075
	(0.068)	(0.068)	(0.068)	(0.068)	(0.068)
$Luo/NASA_c$	0.033	0.015	-0.151*	-0.048	-0.032
	(0.072)	(0.071)	(0.073)	(0.070)	(0.071)
$(Target2)_c$	-0.141+	-0.061	-0.070	-0.116	-0.106
	(0.082)	(0.079)	(0.080)	(0.080)	(0.080)
$(Target3)_c$	-0.270***	0.036	-0.126	-0.155+	-0.141+
	(0.080)	(0.081)	(0.080)	(0.081)	(0.081)
Unshared EP × Age_c	0.001	-0.002	-0.003	-0.001	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Unshared EP × $Male_c$	-0.077	-0.046	-0.088	-0.091	-0.087
	(0.097)	(0.098)	(0.101)	(0.097)	(0.098)
Unshared EP × SES $Index_c$	-0.285*	-0.091	-0.051	-0.176	-0.192
	(0.137)	(0.142)	(0.140)	(0.137)	(0.139)
Unshared EP × $Urban_c$	0.001	0.025	-0.147	-0.052	-0.043
	(0.098)	(0.100)	(0.101)	(0.099)	(0.099)
Unshared EP × $Luo/NASA_c$	0.066	0.114	0.119	0.132	0.113
	(0.105)	(0.105)	(0.108)	(0.103)	(0.104)
Unshared EP × $(Target2)_c$	-0.003	0.144	0.001	0.055	0.059
	(0.116)	(0.118)	(0.119)	(0.116)	(0.117)
Unshared EP × $(Target3)_c$	0.269*	-0.074	0.225+	0.179	0.162
	(0.120)	(0.120)	(0.123)	(0.121)	(0.122)
Num.Obs.	1782	1782	1779	1788	1771
R2	0.027	0.022	0.014	0.021	0.020
R2 Adj.	0.019	0.014	0.005	0.012	0.012
Mean Y_c	4.440	4.146	4.011	4.152	-0.091
SD Y_c	3.181	2.822	2.838	2.354	1.358
(C): Binary outcome (y>0)					
Unshared EP	0.002	0.009	0.015		
	(0.013)	(0.012)	(0.010)		
Num.Obs.	1782	1782	1779		
R2	0.000	0.000	0.001		
R2 Adj.	-0.001	0.000	0.001		
Mean Y (full)	4.717	4.245	4.148		
SD Y (full)	3.239	2.899	2.954		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.11: Effects of unshared ethnopartisanship, Police officer

	Traffic
(A): Base	
Unshared EP	0.153**
	(0.047)
Num.Obs.	1782
R2	0.006
R2 Adj.	0.005
Mean Y_c	3.480
SD Y_c	2.782
(B): Covariates	
Unshared EP	0.164***
	(0.047)
Age_c	-0.002
	(0.003)
$Male_c$	-0.025
	(0.067)
SES $Index_c$	0.112
	(0.098)
$Urban_c$	-0.035
	(0.068)
$Luo/NASA_c$	-0.038
	(0.072)
$(Target2)_c$	0.161*
	(0.079)
$(Target3)_c$	0.175*
	(0.081)
Unshared EP × Age_c	0.002
	(0.004)
Unshared EP × $Male_c$	-0.165+
	(0.095)
Unshared EP × SES $Index_c$	-0.351**
	(0.136)
Unshared EP × $Urban_c$	-0.058
	(0.097)
Unshared EP × $Luo/NASA_c$	-0.059
	(0.102)
Unshared EP × $(Target2)_c$	-0.099
	(0.113)
Unshared EP × $(Target3)_c$	-0.035
	(0.116)
Num.Obs.	1782
R2	0.023
R2 Adj.	0.015
Mean Y_c	3.480
SD Y_c	2.782
(C): Binary outcome (y>0)	
Unshared EP	0.003
	(0.008)
Num.Obs.	1782
R2	0.000
R2 Adj.	0.000
Mean Y (full)	3.692
SD Y (full)	2.787

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.12: Effects of unshared ethnopartisanship, Shopkeeper

	Appeal	Contract	ICW	PC1
(A): Base				
Unshared EP	0.222***	0.096*	0.181***	0.174***
	(0.048)	(0.049)	(0.048)	(0.049)
Num.Obs.	1775	1773	1783	1765
R2	0.012	0.002	0.008	0.007
R2 Adj.	0.011	0.002	0.007	0.007
Mean Y_c	3.373	3.172	3.211	-0.113
SD Y_c	3.184	2.545	2.485	1.238
(B): Covariates				
Unshared EP	0.223***	0.099*	0.184***	0.176***
	(0.048)	(0.049)	(0.048)	(0.048)
Age_c	0.003	0.006*	0.005	0.005+
	(0.003)	(0.003)	(0.003)	(0.003)
$Male_c$	0.048	0.024	0.037	0.043
	(0.069)	(0.068)	(0.069)	(0.069)
SES $Index_c$	0.129	0.157+	0.153	0.164+
	(0.095)	(0.094)	(0.094)	(0.096)
$Urban_c$	0.050	0.084	0.067	0.081
	(0.068)	(0.068)	(0.068)	(0.068)
$Luo/NASA_c$	-0.109	-0.100	-0.121+	-0.111
	(0.073)	(0.073)	(0.073)	(0.073)
$(Target2)_c$	0.029	0.063	0.047	0.055
	(0.082)	(0.083)	(0.082)	(0.082)
$(Target3)_c$	0.045	-0.013	0.012	0.024
	(0.084)	(0.081)	(0.083)	(0.083)
Unshared EP × Age_c	-0.007+	-0.009*	-0.009*	-0.009*
	(0.004)	(0.004)	(0.004)	(0.004)
Unshared EP × $Male_c$	-0.053	-0.015	-0.034	-0.041
	(0.098)	(0.099)	(0.098)	(0.098)
Unshared EP × SES $Index_c$	-0.105	-0.194	-0.162	-0.171
	(0.136)	(0.137)	(0.136)	(0.137)
Unshared EP × $Urban_c$	0.029	0.010	0.029	0.014
	(0.099)	(0.100)	(0.099)	(0.099)
Unshared EP × $Luo/NASA_c$	0.094	-0.035	0.036	0.030
	(0.105)	(0.106)	(0.105)	(0.106)
Unshared EP × $(Target2)_c$	0.069	-0.045	0.011	0.018
	(0.116)	(0.120)	(0.117)	(0.117)
Unshared EP × $(Target3)_c$	0.263*	0.080	0.193	0.189
	(0.121)	(0.121)	(0.120)	(0.121)
Num.Obs.	1775	1773	1783	1765
R2	0.027	0.014	0.022	0.022
R2 Adj.	0.018	0.005	0.013	0.013
Mean Y_c	3.373	3.172	3.211	-0.113
SD Y_c	3.184	2.545	2.485	1.238
(C): Binary outcome (y>0)				
Unshared EP	0.069***	0.009		
	(0.018)	(0.013)		
Num.Obs.	1775	1773		
R2	0.008	0.000		
R2 Adj.	0.008	0.000		
Mean Y (full)	3.730	3.295		
SD Y (full)	3.266	2.628		

Notes
All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

B.3 Interaction of Coethnic versus Copartisan Targeting and Unshared Identity

Table B.13: Interactive effects of coethnic targeting and unshared ethnopartisanship, MP

	Handout	Transport	CDF	ICW	PC1
(A): Base					
Target coethnic	0.013	0.247**	0.111	0.124	0.154+
	(0.082)	(0.092)	(0.082)	(0.081)	(0.084)
Unshared EP	0.272**	0.330**	0.421***	0.450***	0.423***
	(0.097)	(0.103)	(0.087)	(0.092)	(0.094)
Target coethnic × Unshared EP	0.039	-0.025	-0.199+	-0.105	-0.073
	(0.131)	(0.149)	(0.120)	(0.125)	(0.130)
Num.Obs.	1203	1204	1201	1212	1188
R2	0.017	0.024	0.025	0.034	0.032
R2 Adj.	0.014	0.021	0.023	0.032	0.030
Mean Y_c	1.713	0.889	3.819	1.690	-0.132
SD Y_c	2.164	1.837	3.009	1.714	1.165
(B): Covariates					
Target coethnic	0.011	0.248**	0.101	0.119	0.149+
	(0.082)	(0.091)	(0.082)	(0.081)	(0.084)
Unshared EP	0.265**	0.323**	0.414***	0.442***	0.413***
	(0.095)	(0.101)	(0.087)	(0.090)	(0.091)
Age_c	0.008	0.011+	0.006	0.009+	0.011+
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
$Male_c$	0.015	-0.071	-0.052	-0.040	-0.035
	(0.117)	(0.116)	(0.116)	(0.116)	(0.117)
SES $Index_c$	0.300	0.129	0.023	0.175	0.215
	(0.200)	(0.188)	(0.183)	(0.191)	(0.195)
$Urban_c$	0.049	-0.180	0.039	-0.003	-0.028
	(0.116)	(0.117)	(0.116)	(0.116)	(0.118)
EP $Group_c$	0.068	0.074	-0.046	0.030	0.028
	(0.123)	(0.128)	(0.127)	(0.126)	(0.128)
Target coethnic × Unshared EP	0.045	-0.011	-0.173	-0.085	-0.052
	(0.130)	(0.148)	(0.120)	(0.125)	(0.129)
Target coethnic × Age_c	-0.003	-0.011	-0.003	-0.006	-0.006
	(0.007)	(0.009)	(0.007)	(0.008)	(0.008)
Target coethnic × $Male_c$	0.004	0.037	-0.026	0.003	-0.001
	(0.165)	(0.189)	(0.164)	(0.166)	(0.173)
Target coethnic × SES $Index_c$	-0.319	0.012	0.256	0.003	-0.051
	(0.266)	(0.285)	(0.256)	(0.254)	(0.264)
Target coethnic × $Urban_c$	-0.268	0.162	0.115	-0.029	-0.009
	(0.166)	(0.188)	(0.165)	(0.166)	(0.172)
Target coethnic × EP $Group_c$	-0.207	-0.164	-0.085	-0.193	-0.178
	(0.180)	(0.202)	(0.182)	(0.178)	(0.186)
Unshared EP × Age_c	-0.003	-0.010	-0.014*	-0.009	-0.013
	(0.008)	(0.010)	(0.007)	(0.009)	(0.009)
Unshared EP × $Male_c$	-0.439*	-0.569**	-0.216	-0.481**	-0.528**
	(0.193)	(0.199)	(0.176)	(0.181)	(0.183)
Unshared EP × SES $Index_c$	-0.482+	-0.431	-0.043	-0.371	-0.416
	(0.290)	(0.290)	(0.248)	(0.258)	(0.269)
Unshared EP × $Urban_c$	-0.056	0.124	0.145	0.074	0.073
	(0.195)	(0.207)	(0.179)	(0.183)	(0.188)
Unshared EP × EP $Group_c$	-0.121	-0.191	0.080	-0.092	-0.081
	(0.205)	(0.221)	(0.192)	(0.193)	(0.197)
Target coethnic × Unshared EP × Age_c	0.003	0.010	0.011	0.008	0.010
	(0.011)	(0.014)	(0.010)	(0.011)	(0.012)
Target coethnic × Unshared EP × $Male_c$	0.409	0.397	0.057	0.336	0.365
	(0.263)	(0.301)	(0.243)	(0.252)	(0.262)
Target coethnic × Unshared EP × SES $Index_c$	0.653+	0.143	-0.199	0.245	0.283
	(0.393)	(0.435)	(0.339)	(0.354)	(0.374)
Target coethnic × Unshared EP × $Urban_c$	0.251	-0.204	-0.081	0.039	0.014
	(0.265)	(0.310)	(0.246)	(0.254)	(0.265)
Target coethnic × Unshared EP × EP $Group_c$	0.347	0.120	0.065	0.264	0.214
	(0.285)	(0.321)	(0.266)	(0.267)	(0.277)
Num.Obs.	1203	1204	1201	1212	1188
R2	0.036	0.048	0.049	0.059	0.058
R2 Adj.	0.017	0.030	0.030	0.040	0.040
Mean Y_c	1.713	0.889	3.819	1.690	-0.132
SD Y_c	2.164	1.837	3.009	1.714	1.165
(C): Binary outcome (y>0)					
Target coethnic	-0.044	0.104**	0.335		
	(0.038)	(0.039)	(0.246)		
Unshared EP	0.027	0.070+	1.265***		
	(0.039)	(0.040)	(0.263)		
Target coethnic × Unshared EP	0.118*	0.048	-0.598+		
	(0.053)	(0.056)	(0.362)		
Num.Obs.	1203	1204	1201		
R2	0.014	0.027	0.025		
R2 Adj.	0.011	0.025	0.023		
Mean Y (full)	1.999	1.310	3.498		
SD Y (full)	2.450	2.332	3.319		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001.

Table B.14: Interactive effects of coethnic targeting and unshared ethnopartisanship, President

	Appeal	Funds	Schools	ICW	PC1
(A): Base					
Target coethnic	0.124	0.080	0.273**	0.184*	0.194*
	(0.084)	(0.085)	(0.086)	(0.084)	(0.085)
Unshared EP	0.480***	0.489***	0.415***	0.548***	0.569***
	(0.085)	(0.089)	(0.090)	(0.085)	(0.086)
Target coethnic × Unshared EP	0.089	0.002	0.021	0.060	0.041
	(0.118)	(0.125)	(0.126)	(0.120)	(0.122)
Num.Obs.	1173	1180	1175	1189	1158
R2	0.069	0.050	0.052	0.081	0.083
R2 Adj.	0.067	0.047	0.049	0.079	0.080
Mean Y_c	3.884	2.797	2.668	2.989	-0.183
SD Y_c	3.491	3.036	2.884	2.567	1.304
(B): Covariates					
Target coethnic	0.121	0.086	0.273**	0.185*	0.193*
	(0.085)	(0.085)	(0.088)	(0.085)	(0.086)
Unshared EP	0.476***	0.496***	0.423***	0.552***	0.573***
	(0.085)	(0.089)	(0.092)	(0.086)	(0.087)
Age_c	-0.003	0.003	-0.001	-0.001	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Male_c$	0.199	0.008	-0.061	0.083	0.049
	(0.136)	(0.133)	(0.133)	(0.135)	(0.137)
SES $Index_c$	0.193	0.037	0.213	0.189	0.162
	(0.202)	(0.201)	(0.193)	(0.200)	(0.203)
$Urban_c$	0.102	0.269*	0.031	0.154	0.170
	(0.127)	(0.126)	(0.127)	(0.126)	(0.127)
EP $Group_c$	-0.125	0.048	0.051	-0.019	-0.022
	(0.143)	(0.139)	(0.137)	(0.139)	(0.139)
Target coethnic × Unshared EP	0.078	0.008	0.011	0.055	0.038
	(0.118)	(0.126)	(0.127)	(0.121)	(0.123)
Target coethnic × Age_c	-0.004	0.001	0.000	-0.002	-0.001
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Target coethnic × $Male_c$	-0.153	-0.020	0.094	-0.048	-0.030
	(0.180)	(0.180)	(0.184)	(0.181)	(0.183)
Target coethnic × SES $Index_c$	-0.156	-0.197	-0.367	-0.296	-0.265
	(0.252)	(0.254)	(0.252)	(0.249)	(0.255)
Target coethnic × $Urban_c$	0.042	-0.222	0.023	-0.047	-0.076
	(0.172)	(0.172)	(0.178)	(0.171)	(0.173)
Target coethnic × EP $Group_c$	-0.125	-0.269	-0.219	-0.236	-0.249
	(0.185)	(0.185)	(0.187)	(0.183)	(0.184)
Unshared EP × Age_c	-0.001	-0.004	0.004	-0.001	-0.001
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Unshared EP × $Male_c$	-0.396*	-0.083	-0.095	-0.260	-0.232
	(0.179)	(0.187)	(0.189)	(0.181)	(0.184)
Unshared EP × SES $Index_c$	-0.119	-0.253	-0.235	-0.201	-0.275
	(0.260)	(0.281)	(0.273)	(0.266)	(0.268)
Unshared EP × $Urban_c$	-0.154	-0.128	-0.137	-0.179	-0.145
	(0.174)	(0.184)	(0.187)	(0.175)	(0.177)
Unshared EP × EP $Group_c$	0.171	0.065	0.030	0.112	0.107
	(0.192)	(0.200)	(0.198)	(0.192)	(0.192)
Target coethnic × Unshared EP × Age_c	0.000	0.009	-0.006	0.001	0.001
	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
Target coethnic × Unshared EP × $Male_c$	0.083	-0.268	-0.279	-0.164	-0.179
	(0.245)	(0.259)	(0.260)	(0.250)	(0.254)
Target coethnic × Unshared EP × SES $Index_c$	0.330	0.225	0.249	0.330	0.293
	(0.344)	(0.376)	(0.368)	(0.351)	(0.362)
Target coethnic × Unshared EP × $Urban_c$	-0.207	0.125	0.030	-0.022	-0.066
	(0.240)	(0.257)	(0.259)	(0.244)	(0.248)
Target coethnic × Unshared EP × EP $Group_c$	0.075	0.302	0.126	0.204	0.194
	(0.259)	(0.276)	(0.271)	(0.264)	(0.267)
Num.Obs.	1173	1180	1175	1189	1158
R2	0.093	0.072	0.064	0.099	0.102
R2 Adj.	0.075	0.054	0.046	0.081	0.083
Mean Y_c	3.884	2.797	2.668	2.989	-0.183
SD Y_c	3.491	3.036	2.884	2.567	1.304
(C): Binary outcome (y>0)					
Target coethnic	0.056+	0.059	0.143***		
	(0.032)	(0.037)	(0.034)		
Unshared EP	0.121***	0.142***	0.112**		
	(0.029)	(0.034)	(0.035)		
Target coethnic × Unshared EP	-0.051	-0.077	-0.061		
	(0.038)	(0.047)	(0.043)		
Num.Obs.	1173	1180	1175		
R2	0.025	0.019	0.035		
R2 Adj.	0.022	0.016	0.033		
Mean Y (full)	4.342	3.282	2.791		
SD Y (full)	3.716	3.400	3.179		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.15: Interactive effects of coethnic targeting and unshared ethnopartisanship, Bureaucrat

	Connect	Contract	ICW	PC1
(A): Base				
Target coethnic	-0.167*	0.060	-0.046	-0.064
	(0.084)	(0.087)	(0.085)	(0.085)
Unshared EP	0.368***	0.271**	0.352***	0.360***
	(0.093)	(0.091)	(0.093)	(0.093)
Target coethnic × Unshared EP	0.021	-0.073	-0.041	-0.020
	(0.129)	(0.127)	(0.128)	(0.129)
Num.Obs.	1180	1180	1185	1175
R2	0.033	0.012	0.023	0.026
R2 Adj.	0.030	0.009	0.020	0.023
Mean Y_c	2.411	3.007	2.737	-0.071
SD Y_c	2.632	2.595	2.331	1.156
(B): Covariates				
Target coethnic	-0.156+	0.072	-0.034	-0.053
	(0.084)	(0.086)	(0.085)	(0.084)
Unshared EP	0.365***	0.287**	0.360***	0.367***
	(0.093)	(0.092)	(0.093)	(0.093)
Age_c	0.005	0.006	0.007	0.007
	(0.005)	(0.005)	(0.005)	(0.005)
$Male_c$	-0.022	0.207+	0.112	0.106
	(0.117)	(0.120)	(0.119)	(0.119)
SES $Index_c$	-0.152	0.238	0.062	0.051
	(0.178)	(0.163)	(0.169)	(0.169)
$Urban_c$	0.159	0.135	0.164	0.164
	(0.115)	(0.119)	(0.117)	(0.117)
EP $Group_c$	-0.106	0.115	0.012	0.006
	(0.129)	(0.133)	(0.130)	(0.129)
Target coethnic × Unshared EP	0.018	-0.093	-0.055	-0.031
	(0.128)	(0.127)	(0.128)	(0.128)
Target coethnic × Age_c	-0.004	-0.008	-0.007	-0.007
	(0.008)	(0.007)	(0.008)	(0.007)
Target coethnic × $Male_c$	0.011	-0.278	-0.166	-0.150
	(0.171)	(0.173)	(0.171)	(0.170)
Target coethnic × SES $Index_c$	0.222	0.229	0.227	0.285
	(0.232)	(0.222)	(0.224)	(0.222)
Target coethnic × $Urban_c$	-0.091	0.059	-0.022	-0.012
	(0.168)	(0.173)	(0.169)	(0.169)
Target coethnic × EP $Group_c$	-0.025	-0.155	-0.111	-0.100
	(0.191)	(0.187)	(0.186)	(0.186)
Unshared EP × Age_c	0.001	-0.006	-0.004	-0.002
	(0.008)	(0.008)	(0.008)	(0.008)
Unshared EP × $Male_c$	-0.224	-0.111	-0.179	-0.193
	(0.187)	(0.184)	(0.185)	(0.185)
Unshared EP × SES $Index_c$	0.200	-0.374	-0.116	-0.103
	(0.293)	(0.265)	(0.277)	(0.278)
Unshared EP × $Urban_c$	-0.087	-0.117	-0.113	-0.116
	(0.189)	(0.184)	(0.188)	(0.188)
Unshared EP × EP $Group_c$	0.015	-0.289	-0.170	-0.148
	(0.206)	(0.201)	(0.203)	(0.203)
Target coethnic × Unshared EP × Age_c	0.010	0.014	0.013	0.013
	(0.011)	(0.011)	(0.011)	(0.011)
Target coethnic × Unshared EP × $Male_c$	0.074	0.202	0.162	0.159
	(0.261)	(0.257)	(0.259)	(0.260)
Target coethnic × Unshared EP × SES $Index_c$	-0.593	-0.247	-0.423	-0.511
	(0.380)	(0.354)	(0.367)	(0.367)
Target coethnic × Unshared EP × $Urban_c$	0.171	0.139	0.173	0.176
	(0.259)	(0.256)	(0.258)	(0.259)
Target coethnic × Unshared EP × EP $Group_c$	0.444	0.460+	0.503+	0.512+
	(0.284)	(0.274)	(0.279)	(0.279)
Num.Obs.	1180	1180	1185	1175
R2	0.060	0.036	0.044	0.051
R2 Adj.	0.041	0.017	0.026	0.032
Mean Y_c	2.411	3.007	2.737	-0.071
SD Y_c	2.632	2.595	2.331	1.156
(C): Binary outcome (y>0)				
Target coethnic	-0.153***	0.014		
	(0.039)	(0.026)		
Unshared EP	0.062+	0.007		
	(0.036)	(0.027)		
Target coethnic × Unshared EP	0.051	0.008		
	(0.053)	(0.036)		
Num.Obs.	1180	1180		
R2	0.029	0.001		
R2 Adj.	0.026	-0.001		
Mean Y (full)	2.403	3.242		
SD Y (full)	2.886	2.765		

Notes All control variables are centered to the sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.16: Interactive effects of coethnic targeting and unshared ethnopartisanship, Judge

	Rig	Steal	Give job	ICW	PC1
(A): Base					
Target coethnic	-0.121	0.112	-0.051	-0.032	-0.026
	(0.077)	(0.081)	(0.080)	(0.080)	(0.081)
Unshared EP	0.095	0.189*	0.028	0.127	0.129
	(0.078)	(0.082)	(0.081)	(0.079)	(0.080)
Target coethnic × Unshared EP	0.270*	-0.221+	0.219+	0.132	0.103
	(0.114)	(0.119)	(0.119)	(0.117)	(0.118)
Num.Obs.	1192	1193	1189	1196	1184
R2	0.017	0.005	0.008	0.010	0.008
R2 Adj.	0.015	0.002	0.005	0.007	0.006
Mean Y_c	4.407	3.987	3.994	4.074	-0.129
SD Y_c	3.252	2.832	2.898	2.364	1.370
(B): Covariates					
Target coethnic	-0.129+	0.087	-0.057	-0.049	-0.042
	(0.078)	(0.080)	(0.080)	(0.080)	(0.080)
Unshared EP	0.093	0.178*	0.012	0.115	0.115
	(0.078)	(0.082)	(0.082)	(0.080)	(0.080)
Age_c	0.007	0.012**	0.004	0.010*	0.010*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Male_c$	-0.153	-0.199+	-0.039	-0.152	-0.180
	(0.118)	(0.114)	(0.117)	(0.117)	(0.119)
SES $Index_c$	-0.146	-0.119	-0.094	-0.162	-0.131
	(0.160)	(0.155)	(0.147)	(0.151)	(0.155)
$Urban_c$	-0.048	-0.023	0.234*	0.083	0.042
	(0.118)	(0.115)	(0.117)	(0.116)	(0.117)
EP $Group_c$	-0.155	-0.208+	-0.275*	-0.273*	-0.254*
	(0.128)	(0.125)	(0.129)	(0.120)	(0.121)
Target coethnic × Unshared EP	0.277*	-0.209+	0.226+	0.143	0.115
	(0.114)	(0.117)	(0.119)	(0.117)	(0.117)
Target coethnic × Age_c	-0.002	0.004	-0.000	-0.001	0.003
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Target coethnic × $Male_c$	0.072	0.211	0.025	0.106	0.164
	(0.160)	(0.165)	(0.164)	(0.166)	(0.166)
Target coethnic × SES $Index_c$	0.429+	0.159	0.330	0.392+	0.383+
	(0.222)	(0.224)	(0.208)	(0.221)	(0.222)
Target coethnic × $Urban_c$	0.220	0.295+	-0.193	0.111	0.168
	(0.162)	(0.164)	(0.165)	(0.166)	(0.166)
Target coethnic × EP $Group_c$	0.300+	0.235	0.175	0.298+	0.299+
	(0.173)	(0.172)	(0.179)	(0.174)	(0.174)
Unshared EP × Age_c	-0.002	-0.000	0.002	-0.000	0.000
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Unshared EP × $Male_c$	-0.063	0.116	0.018	0.005	0.051
	(0.161)	(0.166)	(0.167)	(0.163)	(0.164)
Unshared EP × SES $Index_c$	-0.173	0.043	0.234	0.047	0.023
	(0.229)	(0.243)	(0.224)	(0.221)	(0.224)
Unshared EP × $Urban_c$	0.104	0.148	-0.263	-0.011	0.018
	(0.163)	(0.171)	(0.170)	(0.166)	(0.167)
Unshared EP × EP $Group_c$	0.193	0.205	0.180	0.257	0.227
	(0.177)	(0.178)	(0.179)	(0.171)	(0.172)
Target coethnic × Unshared EP × Age_c	0.004	-0.010	-0.007	-0.004	-0.008
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Target coethnic × Unshared EP × $Male_c$	0.157	-0.334	-0.091	-0.067	-0.154
	(0.234)	(0.240)	(0.245)	(0.239)	(0.239)
Target coethnic × Unshared EP × SES $Index_c$	-0.185	-0.123	-0.476	-0.324	-0.332
	(0.324)	(0.342)	(0.334)	(0.331)	(0.332)
Target coethnic × Unshared EP × $Urban_c$	-0.315	-0.389	0.166	-0.209	-0.259
	(0.235)	(0.242)	(0.245)	(0.242)	(0.242)
Target coethnic × Unshared EP × EP $Group_c$	-0.153	0.067	0.045	-0.029	-0.023
	(0.254)	(0.255)	(0.266)	(0.257)	(0.257)
Num.Obs.	1192	1193	1189	1196	1184
R2	0.041	0.038	0.024	0.035	0.035
R2 Adj.	0.022	0.020	0.005	0.016	0.016
Mean Y_c	4.407	3.987	3.994	4.074	-0.129
SD Y_c	3.252	2.832	2.898	2.364	1.370
(C): Binary outcome (y>0)					
Target coethnic	-0.006	0.011	-0.007		
	(0.024)	(0.022)	(0.020)		
Unshared EP	0.015	0.015	0.013		
	(0.022)	(0.021)	(0.018)		
Target coethnic × Unshared EP	-0.009	-0.003	0.021		
	(0.033)	(0.029)	(0.026)		
Num.Obs.	1192	1193	1189		
R2	0.001	0.001	0.003		
R2 Adj.	-0.002	-0.001	0.001		
Mean Y (full)	4.717	4.245	4.148		
SD Y (full)	3.239	2.899	2.954		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.17: Interactive effects of coethnic targeting and unshared ethnopartisanship, Police officer

	Traffic
(A): Base	
Target coethnic	0.013
	(0.081)
Unshared EP	0.107
	(0.079)
Target coethnic × Unshared EP	0.063
	(0.115)
Num.Obs.	1185
R2	0.006
R2 Adj.	0.003
Mean Y_c	3.606
SD Y_c	2.861
(B): Covariates	
Target coethnic	0.011
	(0.081)
Unshared EP	0.102
	(0.079)
Age_c	0.002
	(0.004)
$Male_c$	-0.099
	(0.112)
SES $Index_c$	0.065
	(0.163)
$Urban_c$	-0.136
	(0.113)
EP $Group_c$	-0.135
	(0.118)
Target coethnic × Unshared EP	0.073
	(0.116)
Target coethnic × Age_c	-0.006
	(0.006)
Target coethnic × $Male_c$	0.168
	(0.162)
Target coethnic × SES $Index_c$	0.180
	(0.239)
Target coethnic × $Urban_c$	0.043
	(0.166)
Target coethnic × EP $Group_c$	0.239
	(0.176)
Unshared EP × Age_c	-0.007
	(0.006)
Unshared EP × $Male_c$	0.122
	(0.160)
Unshared EP × SES $Index_c$	-0.097
	(0.228)
Unshared EP × $Urban_c$	0.072
	(0.159)
Unshared EP × EP $Group_c$	0.085
	(0.167)
Target coethnic × Unshared EP × Age_c	0.008
	(0.009)
Target coethnic × Unshared EP × $Male_c$	-0.464*
	(0.231)
Target coethnic × Unshared EP × SES $Index_c$	-0.369
	(0.330)
Target coethnic × Unshared EP × $Urban_c$	-0.148
	(0.235)
Target coethnic × Unshared EP × EP $Group_c$	-0.310
	(0.252)
Num.Obs.	1185
R2	0.022
R2 Adj.	0.003
Mean Y_c	3.606
SD Y_c	2.861
(C): Binary outcome (y>0)	
Target coethnic	-0.012
	(0.013)
Unshared EP	-0.009
	(0.013)
Target coethnic × Unshared EP	0.003
	(0.020)
Num.Obs.	1185
R2	0.001
R2 Adj.	-0.001
Mean Y (full)	3.692
SD Y (full)	2.787

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

Table B.18: Interactive effects of coethnic targeting and unshared ethnopartisanship, Shopkeeper

	Appeal	Contract	ICW	PC1
(A): Base				
Target coethnic	0.026	-0.063	-0.026	-0.021
	(0.085)	(0.081)	(0.083)	(0.083)
Unshared EP	0.191*	0.047	0.125	0.132
	(0.082)	(0.081)	(0.081)	(0.081)
Target coethnic × Unshared EP	0.179	0.099	0.153	0.151
	(0.121)	(0.116)	(0.118)	(0.119)
Num.Obs.	1174	1174	1182	1166
R2	0.023	0.003	0.012	0.012
R2 Adj.	0.020	0.000	0.009	0.009
Mean Y_c	3.384	3.281	3.288	-0.080
SD Y_c	3.122	2.607	2.513	1.232
(B): Covariates				
Target coethnic	0.015	-0.070	-0.035	-0.030
	(0.085)	(0.081)	(0.084)	(0.084)
Unshared EP	0.191*	0.048	0.126	0.132
	(0.082)	(0.081)	(0.081)	(0.082)
Age_c	-0.002	0.001	-0.000	0.000
	(0.004)	(0.005)	(0.004)	(0.004)
$Male_c$	0.122	0.179	0.170	0.170
	(0.117)	(0.117)	(0.117)	(0.118)
SES $Index_c$	0.244	0.137	0.198	0.218
	(0.158)	(0.152)	(0.153)	(0.154)
$Urban_c$	0.067	0.105	0.089	0.104
	(0.121)	(0.119)	(0.120)	(0.121)
EP $Group_c$	-0.072	-0.137	-0.132	-0.107
	(0.126)	(0.122)	(0.125)	(0.126)
Target coethnic × Unshared EP	0.188	0.102	0.161	0.154
	(0.122)	(0.117)	(0.119)	(0.120)
Target coethnic × Age_c	0.002	0.005	0.004	0.004
	(0.007)	(0.007)	(0.007)	(0.007)
Target coethnic × $Male_c$	0.137	-0.009	0.055	0.079
	(0.175)	(0.165)	(0.171)	(0.172)
Target coethnic × SES $Index_c$	-0.217	-0.179	-0.220	-0.221
	(0.238)	(0.223)	(0.230)	(0.235)
Target coethnic × $Urban_c$	0.017	0.012	0.013	0.020
	(0.172)	(0.162)	(0.168)	(0.169)
Target coethnic × EP $Group_c$	-0.112	-0.024	-0.062	-0.084
	(0.183)	(0.171)	(0.177)	(0.179)
Unshared EP × Age_c	-0.003	-0.003	-0.003	-0.004
	(0.007)	(0.007)	(0.006)	(0.006)
Unshared EP × $Male_c$	-0.040	-0.115	-0.092	-0.086
	(0.165)	(0.163)	(0.163)	(0.164)
Unshared EP × SES $Index_c$	-0.167	-0.073	-0.128	-0.133
	(0.226)	(0.218)	(0.220)	(0.221)
Unshared EP × $Urban_c$	-0.152	0.037	-0.054	-0.062
	(0.170)	(0.165)	(0.168)	(0.169)
Unshared EP × EP $Group_c$	0.075	0.028	0.062	0.050
	(0.176)	(0.171)	(0.174)	(0.175)
Target coethnic × Unshared EP × Age_c	0.007	0.002	0.005	0.006
	(0.010)	(0.010)	(0.010)	(0.010)
Target coethnic × Unshared EP × $Male_c$	-0.297	-0.137	-0.225	-0.245
	(0.247)	(0.235)	(0.240)	(0.242)
Target coethnic × Unshared EP × SES $Index_c$	0.530	0.288	0.462	0.431
	(0.341)	(0.329)	(0.332)	(0.340)
Target coethnic × Unshared EP × $Urban_c$	0.215	-0.110	0.056	0.041
	(0.249)	(0.237)	(0.243)	(0.245)
Target coethnic × Unshared EP × EP $Group_c$	0.155	0.103	0.130	0.152
	(0.264)	(0.248)	(0.255)	(0.257)
Num.Obs.	1174	1174	1182	1166
R2	0.041	0.019	0.030	0.031
R2 Adj.	0.022	-0.001	0.011	0.011
Mean Y_c	3.384	3.281	3.288	-0.080
SD Y_c	3.122	2.607	2.513	1.232
(C): Binary outcome (y>0)				
Target coethnic	-0.010	-0.008		
	(0.033)	(0.023)		
Unshared EP	0.055+	-0.001		
	(0.029)	(0.022)		
Target coethnic × Unshared EP	0.031	0.027		
	(0.042)	(0.032)		
Num.Obs.	1174	1174		
R2	0.010	0.001		
R2 Adj.	0.007	-0.001		
Mean Y (full)	3.730	3.295		
SD Y (full)	3.266	2.628		

Notes All control variables are centered to their sample mean. Heteroskedasticity-robust standard errors in parentheses. + <0.1, * <0.05, ** <0.01, *** <0.001

C Other Distribution Effects

In this section, we report estimates of treatment effects on other parts of the distribution beyond the average. Table C.19 shows the estimated effects on the 25^{th}, 50^{th}, and 75^{th} quantile. For this analysis, our outcome of interest is the actor-level ICW index of punishment (not standardized using the control group moments). We also run distribution regressions, where we estimate treatment effects on the probability that the outcome variable is greater than a given threshold (Chernozhukov et al. 2013). In Figures C.1 and C.2 we report the pointwise estimates and standard errors of these effects along the distribution of each outcome variable.

Table C.19: Quantile treatment effects

	Coethnic targeting			Unshared EP		
Actor	0.25	0.5	0.75	0.25	0.5	0.75
MP	0.166+	0.216*	0.170	0.274***	0.498***	0.950***
	(0.087)	(0.103)	(0.172)	(0.081)	(0.09)	(0.161)
Pres	0.549**	0.499*	0.726*	0.566***	1.451***	1.871***
	(0.177)	(0.225)	(0.314)	(0.115)	(0.178)	(0.22)
Buro	-0.000	-0.171	-0.359	0.453***	0.906***	1.282***
	(0.043)	(0.252)	(0.314)	(0.027)	(0.139)	(0.218)
Judge	0.086	0.271	0.218	0.344**	0.253	0.516**
	(0.165)	(0.18)	(0.227)	(0.131)	(0.167)	(0.192)
Pol.of	-0.000	0.000	1.000***	0.000	0.000	1.000***
	(0.002)	(0.106)	(0.202)	(0.001)	(0.007)	(0.176)
Shop	0.233*	0.000	0.233	0.233**	0.397*	0.534**
	(0.112)	(0.232)	(0.206)	(0.089)	(0.191)	(0.192)

Notes: Quantile regressions estimated at the 25th, 50th, and 75th percentiles. Robust standard errors in parentheses, computed via wild bootstrap. + p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Figure C.1: Distribution regression results

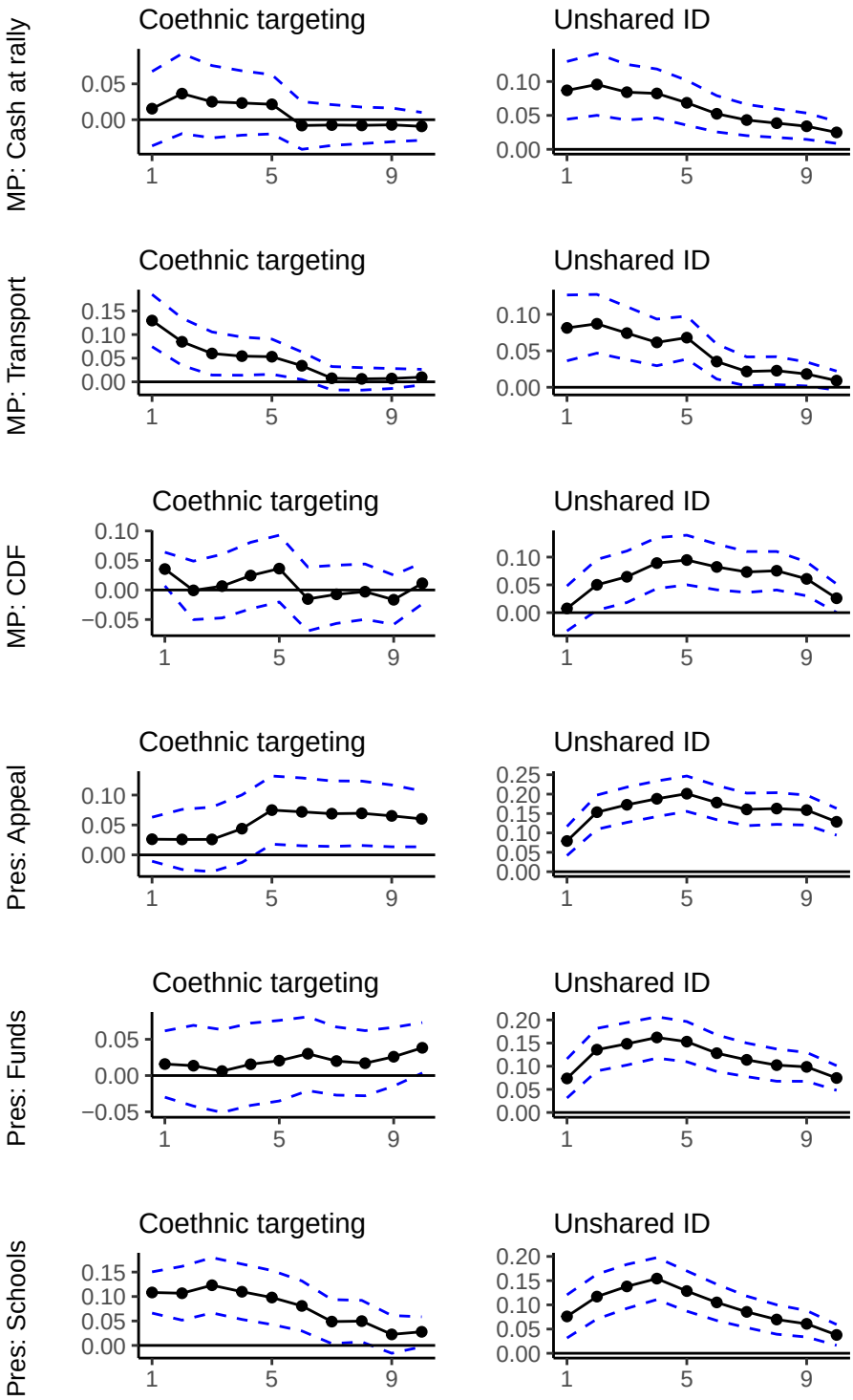
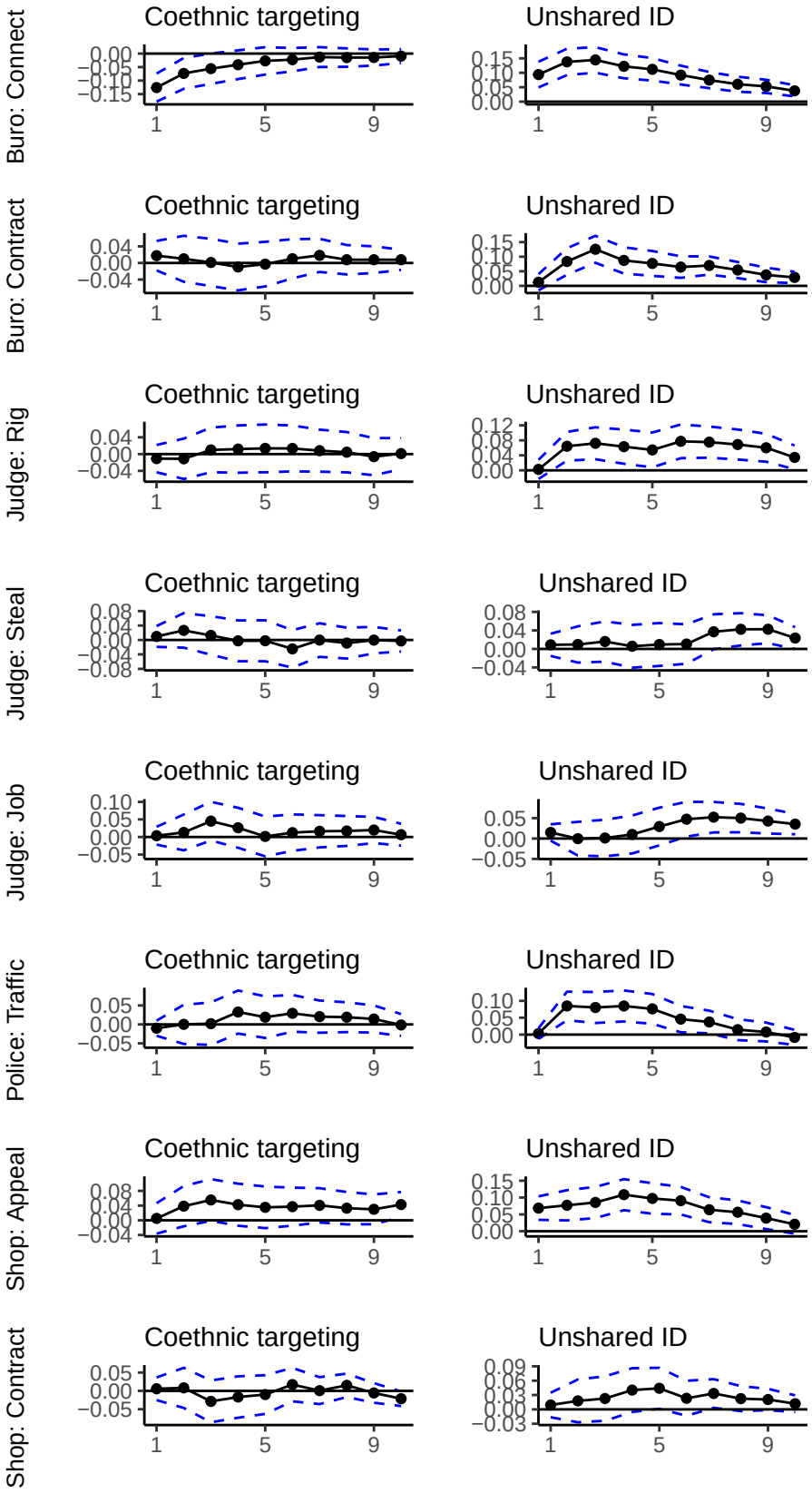


Figure C.2: Distribution regression results (continued)



SI References

Chernozhukov, V., I. Fernández-Val, and B. Melly (2013). "Inference on counterfactual distributions". *Econometrica* 81.6, pp. 2205–2268.

Lin, W. (2013). "Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique". *Annals of Applied Statistics* 7.1, pp. 295–318.

Software Appendix

The regression analyses were performed in `R` using Blair et al. (2024) and Koenker (2023). The tables were generated with Arel-Bundock (2022) and Dahl et al. (2019) and the plots with Arel-Bundock (2024) and Kassambara (2023).

Software References

Arel-Bundock, V. (2022). "modelsummary: Data and Model Summaries in R". *Journal of Statistical Software* 103.1, pp. 1–23.

— (2024). *marginaleffects: Predictions, Comparisons, Slopes, Marginal Means, and Hypothesis Tests*. R package version 0.19.0. URL: https://CRAN.R-project.org/package=marginaleffects.

Blair, G., J. Cooper, A. Coppock, M. Humphreys, and L. Sonnet (2024). *estimatr: Fast Estimators for Design-Based Inference*. R package version 1.0.4. URL: https://CRAN.R-project.org/package=estimatr.

Dahl, D. B., D. Scott, C. Roosen, A. Magnusson, and J. Swinton (2019). *xtable: Export Tables to LaTeX or HTML*. R package version 1.8-4. URL: https://CRAN.R-project.org/package=xtable.

Kassambara, A. (2023). *ggpubr: 'ggplot2' Based Publication Ready Plots*. R package version 0.6.0. URL: https://CRAN.R-project.org/package=ggpubr.

Koenker, R. (2023). *quantreg: Quantile Regression*. R package version 5.97. URL: https://CRAN.R-project.org/package=quantreg.