

## Ingroup Bias in Official Behavior: A National Field Experiment in China\*

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### ABSTRACT

Do ingroup biases distort the behavior of public officials? Recent studies detect large ethnic biases in elite political behavior, but their case selection leaves open the possibility that bias obtains under relatively narrow historical and institutional conditions. We clarify these scope conditions by studying ingroup bias in the radically different political, historical, and ethnic environment of contemporary China. In a national field experiment, local officials were 33% less likely to provide assistance to citizens with ethnic Muslim names than to ethnically-unmarked peers. We find evidence consistent with the ingroup bias interpretation of this finding and detect little role for

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strategic incentives mediating this effect. This result demonstrates that neither legacies of institutionalized racism nor electoral politics are necessary to produce large ingroup biases in official behavior. It also suggests that ethnically motivated distortions to governance are more prevalent than previously documented.

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To what extent does ingroup bias distort the behavior of public officials? Officials make decisions that are hugely consequential for the lives of citizens. They allocate resources, determine who receives timely access to public services, and decide which voices are represented in government. Typically these choices are made under the public expectation of impartiality; officials ought not favor members of their own ascriptive groups. At the same time, decades of psychological research document the tendency for humans to exhibit precisely this ingroup favoritism (Tajfel *et al.*, 1971).

Recent studies find that this ingroup bias manifests in the political behavior of public officials (Butler and Broockman, 2011; McClendon, 2013; Broockman, 2013). The distorting effects of bias documented by these studies are large; public officials from seven different ethnic groups are 25–44% less likely to respond to appeals from ethnic outgroups (see Table 2). But how generalizable are these results? The settings of early research — the United States and South Africa — prompt concerns that the large biases they detect may be artifacts of case selection.

The first consideration involves legacies of institutionalized racism. Both the United States and South Africa have powerful legacies of institutionalized racial discrimination, abolished only in the face of national political movements. The institutionalization of racial exclusion may increase the salience of ethnic cleavages across society, particularly in political life. If individuals or their family members have experienced social and political exclusion as a result of their ethnic identities, these identities are likely to be more salient in political cognition and behavior. Increased salience of ethnic identity may exacerbate ingroup favoritism expressed in political behavior. The saliency of group identity can induce greater contributions to public goods and ingroup cooperation (Espinoza and Garza, 1985; Eckel and Grossman, 2005) and increase bias against outgroups (Shayo and Zussman, 2011). Indeed, in both the countries the ethnic divide between whites and non-whites continues to manifest strongly in political cognition and

behavior (Dunning, 2010; Piston, 2010). The powerful racial identifications produced by legacies of institutionalized racism in the United States and South Africa may exacerbate the expression of ingroup bias among political elites. If so, then our early impressions of the magnitude and prevalence of ethnic bias among public officials may be inflated by researchers' case selection.

A second case selection concern is these studies' exclusive focus on elected representatives. Institutions of electoral representation may activate stronger norms of reciprocity among members of the same ethnic group. Elected officials are in reciprocal relationships with potential voters, in which "representation" is exchanged for votes. Lab-in-the-field experimentation suggests that norms of reciprocity are stronger among coethnics in cooperative activities (Habyarimana *et al.*, 2007). These norms of cooperation among ingroup members may help explain the favoritism that elected representative display toward coethnics even when statistical evidence (such as the high rate of support for the Democratic Party among U.S. blacks) appears to incentivize outgroup favoritism.<sup>1</sup>

This article brings the study of ingroup bias in official behavior to a radically different historical, institutional, and ethnic context. We implement a national field experiment on unelected local officials in contemporary China, a setting that lacks the institutionalized racism and electoral reciprocity that characterize sites of previous research. Randomizing the names of putative citizens to signal ethnic identity, we find that local officials are nonetheless 33% less likely to offer assistance to ethnic Muslims than to ethnically-unmarked peers. We subject this finding to additional analysis and rule out key strategic concerns as motivating the observed effect.

This study — to our knowledge the first national field experiment in the study of Chinese politics — demonstrates that neither legacies of institutionalized racism nor electoral politics are necessary conditions for the emergence of large biases in the behavior of political elites. This implies that the distortionary effects of ethnic bias on governance may be more widespread than previously understood.

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<sup>1</sup> Broockman (2013) demonstrates that elected representatives in the United States continue to exhibit ingroup favoritism even when constituents are ineligible to vote in relevant elections, indicating that any such expectations of reciprocity are irrational. However, the norms of reciprocity involved in electoral representation may still activate ingroup preferences, even irrational preferences.

## 1 Minority Governance and Official Incentives in China

We address the case selection concerns described above by studying local officials in contemporary China. One key difference from previous sites of research is China's modern history of ethnic policy, which lacks the forms of institutionalized racism found in the United States and South Africa. China is ethnically dominated by a single ethnic group — the Han — who represent 92% of the total population. Officially recognized ethnic minorities, despite occupying a small percentage of the population, number over 110 million. Among them are 23 million ethnic Muslims, spanning 10 ethnic groups.<sup>2</sup> These groups are among China's most visible and restive minorities, including two large groups: the Uyghur (10.1 million people) and the Hui (10.6 million). Together they account for roughly 1.6% of China's total population and 18% of its minority population. Members of these groups may differ from the majority Han in their dress, diet, and religion. We study Chinese officials' interactions with these ethnically Muslim minorities.

The political significance of minority membership in China reflects its Communist origins. Since its founding in 1949, the People's Republic of China has pursued a modified Leninist approach to ethnic minority incorporation. Minority groups were delineated and provided concessions of regional autonomy, policy privileges, and preference in official promotions. Consequently, minority groups today enjoy benefits in important areas of social policy, including minority quotas and lower entrance score requirements in universities (Clothey, 2005; Wang, 2007; Gladney, 1998) as well as accommodations in family planning, poverty alleviation, and even criminal justice (He, 2005; Gladney, 1998; Sautman, 2010). Empirically, minority status has been linked to career advancement in the China Communist Party (Shih *et al.*, 2012) and preference in state sector hiring (Zang, 2011).

China's minority policies depart from the legacies of institutionalized political exclusion found in South Africa and the United States. However, social discrimination against minority groups is evident, particularly toward the Muslims minorities that we study. Ethnographic studies

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<sup>2</sup> Ethnic demographics from People's Republic of China Sixth National Population Census, 2010. Available online at: <http://www.stats.gov.cn/tjsj/pcsj/rkpc/6rp/indexch.htm>. The 10 Muslim ethnic groups are the Hui, Uyghur, Kazakh, Dongxiang, Kyrgyz, Salar, Tajik, Uzbek, Bonan, and Tatar.

suggest that Muslims are generally viewed as exotic and less educated (Gladney, 2004, p. 262) and quote Han respondents describing Uyghur Muslims as unreasonable, primitive, and criminal (Cliff, 2012; Kaltman, 2007, Chapter 3). These stereotypes are accompanied by evidence of bias in the labor market and law enforcement. A study of wages in western China attributed the private-sector earnings gap between Han and ethnic Muslims to beliefs that the minorities are more “lazy” and “savage” than Han (Zang, 2011, pp. 146–147), and a variety of minorities in Beijing also report perceptions of job discrimination (Hasmath, 2009). Urban police crackdowns have targeted Muslim enclaves for petty crime, and some ethnic Muslims believe themselves to be unfairly targeted by law enforcement (Kaltman, 2007; Sullivan, 2013).

We investigate whether these biases distort the behavior of China’s *unelected* officials, a second departure from previous research. Millions<sup>3</sup> of local officials in China are the key actors in delivering social services, regulating economic activity, and overseeing local policy implementation, and they enjoy considerable discretion in these activities (Birney, 2014). Unlike elected representatives, these officials are recruited through processes controlled by the Communist Party and government bureaucracies (Burns and Xiaoqi, 2010). Their daily tasks, bonuses, and prospects for promotion are governed by bureaucratic superiors; therefore their dealings with the public do not involve the same kind of reciprocity as electoral representation.

The absence of electoral reciprocity does not mean that authoritarian officials have no incentive to be responsive to citizens. Nondemocratic states have significant informational needs that arise from the absence of elections (Lorentzen, 2013b). Institutions that allow citizens to contact officials with questions and complaints help fill this informational gap (Luehrmann, 2003), so long as the institutions remain minimally responsive to citizen needs (Dimitrov, 2013). Thus, in place of electoral reciprocity are a variety of official mandates for local governments to be transparent and responsive to citizens. In 2008, a national reform ordered local governments to disclose government information in response to citizen requests (Horsley, 2007), and the contacting channel utilized by our experiment originated with a national

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<sup>3</sup> The precise count of officials in China is under debate in part because Chinese bureaucracies often employ more than their allocated number of official posts; estimates of public employees for 1998 ranged from 33.7 to 40.5 million (Ang, 2012).

initiative for local governments to develop websites that serve public needs (Hartford, 2005).

The local officials tasked with implementing responsive institutions in China are evaluated against quantitative performance targets that may create additional incentives to demonstrate responsiveness to citizens. These targets typically focus on fostering economic development, implementing birth control policies, and maintaining social stability (Edin, 2003; Liu and Tao, 2007; Chan and Gao, 2008; Lü and Landry, forthcoming). The social stability (*weiwén*) imperative has played an increasingly important role in the evaluation of China's local officials, with a single breakdown in public order capable of ruining official careers (Chen, 2013). This creates additional incentives for China's local officials to address citizen questions and complaints in order to reduce public discontent with government. This combination of the social stability imperative with recent episodes of high-visibility unrest among ethnic minorities may create incentives for Chinese officials to exhibit *pro-minority* bias in responsiveness, an idea we explore more fully after introducing our main empirical findings.<sup>4</sup>

In summary, the legacies of institutionalized discrimination and electoral reciprocity that distinguish previous settings of research are absent from contemporary China. The emphasis placed on social stability in official performance evaluations may even incentivize official favoritism towards restive minority groups. Nonetheless, biased attitudes and behavior are observed in society. Below we describe a field experiment to investigate whether these ingroup biases still distort official responsiveness to citizens under these institutional and historical conditions.

## 2 The Experiment

To study bias among political elites in China, we requested assistance from local government offices and randomly manipulated the presence of ethnically informative requester names, similar to the design of Butler and

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<sup>4</sup> Tsai (2007) identifies another important source of elite incentives to assist citizens in China; the moral standing that local civil society can confer upon public servants. However, this mechanism was identified in rural villages with a mean size of 1200 inhabitants, whereas we study bureaucrats in prefectures that average 4 million inhabitants. Citizens in our research setting have at best limited ability to confer moral standing to high-performing bureaucrats.

Broockman (2011). Contacting government agencies is a common form of citizen political participation in China for the purposes of lodging complaints, resolving disputes, and accessing government services (O'Brien and Li, 1995; Shi, 1997; Michelson, 2007). With the diffusion of internet access in China, many local governments have established websites that allow citizens to contact the office of the mayor, a service commonly referred to as the Mayor's Mailbox (Hartford, 2005). Individuals are invited to directly address the mayor in their queries, and the government agency replies via the website, email, or phone call. Our study uses this request channel to ask for China's Minimum Livelihood Guarantee (*dibao*), a program administered by local governments that provides cash transfers to low-income households (Solinger and Hu, 2012). Citizens seeking to enroll need to identify the relevant government agencies, contact them, and provide evidence that they qualify for a means-tested cash transfer, placing relatively high informational demands on low-income households.

Local government offices in our study received the following electronic communication from a putative constituent:

Respected leader:

I have been unable to find stable work for a long time, and my economic situation is not good. Do I have the opportunity to apply for the Minimum Livelihood Guarantee? What conditions would I need to satisfy to receive the Minimum Livelihood Guarantee?

With gratitude for leaders' care,

[*Ethnically unmarked name*]<sup>5</sup>  
[*Ethnic Muslim name*]

The message was intended both to request information and to signal the low economic status of the requester. As we are primarily interested in understanding how minority status affects official behavior, we sought to ensure

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<sup>5</sup> Although Chinese names can signal membership in Muslim minority groups, they cannot signal membership in the majority Han group. As China's population is 92% Han Chinese (2010 Census) an ethnically unmarked name is likely in most localities to be interpreted as belonging to a Han requester. See Appendix A for details.

that signaling minority identity did not additionally alter perceptions of the citizen's economic well-being or social class (Fryer and Levitt, 2004). The letter offers a clear signal that the requester is of low economic status, which is also pertinent to their request for income assistance. We designed this request to be simple and minimally burdensome to the officials who received it. Each office received one message, and the requested policy information was straightforward. The unremarkable nature of the request itself and the lack of attention drawn to it also means that this study does not foreclose the possibility of future researchers using similar channels to study government responsiveness in China.

After randomizing assignment to either the Muslim or ethnically unmarked names, messages were submitted to 258 political jurisdictions which collectively govern over one billion people. Details on our sample and randomization are presented in Appendix B. We checked for replies after four weeks to record whether local agencies provided helpful information about the Minimum Livelihood Guarantee. A majority of the replies were posted on the government websites, and were available either publicly or by using login information provided during the submission process. A minority of responses were sent to the email addresses used to submit the request. Any replies attempted by phone could not be collected, as we provided out-of-service phone numbers. We coded a binary disclosure indicator according to whether local governments provided at least one of five categories of relevant information in response to our fictitious citizens.<sup>6</sup>

### 3 Results

Officials' disclosure rate fell by 14.9 percentage points when contacted with the Muslim minority alias (Table 1). As disclosure for the ethnically unmarked alias was 44.6% this represents a decline in the probability of disclosure of 33%. This estimate of the treatment effect is robust to alternative

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<sup>6</sup> These five indicators are whether the government disclosed: (i) the name of the government agency to contact for income assistance; (ii) conditions for accessing the Minimum Livelihood Guarantee; (iii) procedures for approval; (iv) contact information of the government agency; and (v) compensation level. Appendix Table C2 presents descriptive data on government response rates and quality of response.



**Table 1.** Ethnic identity and government disclosure.

	Unmarked alias	Muslim alias	Difference
Disclosure rate (%)	44.6 (4.4)	29.7 (4.1)	-14.9 (6.0)
95% CI upper	53.3	37.7	-3.2
95% CI lower	36.0	21.7	-26.7
Obs.	130	128	

*Notes:* Estimated mean disclosure rates across control and treatment groups. Disclosure measured four weeks after requesting information from China's prefectural governments in July and August 2012. Standard errors (in parentheses) and 95% confidence intervals reported below. Two-sided *t*-test (unequal variance) *p*-value = 0.013. Wilcoxon rank-sum test *p*-value = 0.013.

time cutoffs for government response and the inclusion of local covariate controls in OLS regression (Appendix Tables C4 and C5).

The magnitude of the estimated effect in localities with non-Muslim mayors (now excluding the 12 prefectures with ethnically Muslim mayors from our sample) is quite comparable to ingroup biases detected in similar contacting field experiments in South Africa and the United States (Table 2). We find no evidence that the absence of legacies of institutionalized racism, nondemocratic government, or China's distinctive ethnic policies counteract the phenomenon of ingroup ethnic bias expressed in official behavior.<sup>7</sup>

We detect some evidence that the officials exerted less effort when they responded to the Muslim alias as well. Among prefectures that disclosed information, the mean reply length for the control group was 510 characters, compared to 258 for the Muslim alias. However, the effect is imprecisely estimated, with a *p*-value of 0.08 (two-sided *t*-test, unequal variances).

<sup>7</sup> The response rate of Chinese officials is also comparable to those of elected representatives in democracies (Table 2). It is intriguing that unelected officials in an authoritarian regime respond to their constituents as often as democratically elected representatives, but this finding is not the focus of our study.

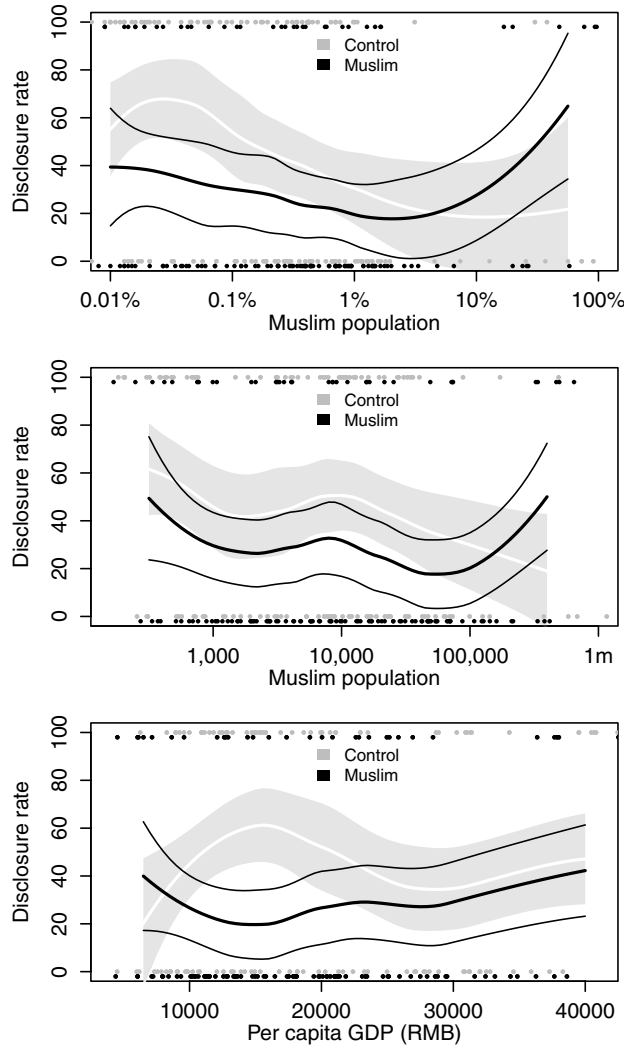
**Table 2.** Ingroup bias among political elites in South Africa, China, and the United States.

Country	Elite group	Response rates by ethnicity (%)			Diff.	pval	Change in probability of response (%)
		Ingroup	Outgroup				
South Africa	Xhosa councillors	19.4	10.9	-8.5	0.03	-43.8	
<b>China</b>	<b>Non-Muslim mayors</b>	44.5	27.8	-16.7	0.01	<b>-37.5</b>	
South Africa	Zulu councillors	14.6	9.5	-5.1	0.03	-34.9	
United States	Af-Am state legislators	41.9	29.1	-12.8	0.15	-30.5	
South Africa	English councillors	49.4	35.8	-13.6	0.04	-27.5	
South Africa	Afrikaaner councillors	40.9	29.7	-11.2	0.04	-27.4	
United States	White state legislators	61.2	45.9	-15.3	0.07	-25.0	

*Notes:* Reports the results from three elite-contacting experiments that use names to manipulate the apparent ethnic/racial identity of citizens contacting political elites. South Africa results from McClendon (2013); United States results from Butler and Broockman (2011).

We also find regional variation in the treatment effect consistent with the ingroup bias interpretation of this finding. Disclosure rates by local Muslim population are presented in the first two plots of Figure 1. They show a bias against the Muslim alias for most prefectures, but response rates trend upward after roughly 3% local Muslim population or 50,000 Muslim individuals. As the proportion of minority officials varies with the local population share of minority groups (Newland, 2013), these high-Muslim localities are the regions in which officials receiving our messages are most likely to be ethnic Muslims. At the highest concentrations of Muslim population, we detect an imprecisely estimated *pro-Muslim* bias, although data sparsity is a concern for this subgroup. (Figure 1)

One alternative to our account of ingroup bias is that, in certain regions, minority names may appear unusual and trigger increased scrutiny by officials handling the requests. If the unfamiliar name led to additional investigations of the minority requester, such as attempting to verify the accompanying phone number or address, officials would have more opportunity to learn that the supplied phone numbers and street addresses were fictional. In this account, the lower disclosure rates for the minority alias might be an artifact of our use of fictional phone numbers and addresses. If the minority alias was subjected to greater scrutiny, we would expect local governments' messages to mention inaccurate contact information more often for



**Figure 1.** Disclosure rates by local Muslim population and economic development.

*Notes:* Locally weighted polynomial regression fits (loess) for government disclosure rates responding to control (gray) and Muslim (black) requesters. Thick lines indicate the estimated response rate, with bands showing the 95% confidence interval. The vertical distance between the thick black line and thick white line represents the estimated treatment effect at that level of the running variable. Dots on the top (disclosed) and the bottom (did not disclose) show the individual observations that form the basis for the loess estimation. Note the log scaling for the first two plots.

the Muslim alias. However, 11.5% of the local governments noted problems with the contact information for the unmarked alias, compared to 11.7% for the Muslim alias ( $t$ -test  $p$ -value = 0.964).<sup>8</sup> We find no evidence that additional scrutiny applied to the minority alias produced the different levels of responsiveness observed between control and treatment groups.

### 3.1 Social Stability and Responsiveness

As discussed above, China's local officials are under great pressure to maintain social stability and avert episodes of public unrest (Chen, 2013). Responding to citizens grievances and requests is one way to address discontent and reduce the threat of trouble-making. Under this logic, citizens who are perceived to pose greater threats to social stability should experience greater government responsiveness, as officials prioritize addressing their concerns over other members of the public.

Ethnic issues are not the primary source of social instability in China, but periodic ethnic conflict is a feature of China's social landscape, and violent episodes involving Uyghur Muslims have gained prominence in the recent years. In July 2009, ethnic riots in Urumqi led to roughly 200 deaths and over 1000 arrests by the police. In 2011, violence in Hotan and Kashgar led to four death sentences for Muslim participants,<sup>9</sup> and additional fatal clashes between Uyghurs and authorities have been reported through 2012 and 2013.<sup>10</sup> These episodes of unrest have elicited wide media coverage in China and commanded public attention. If officials consider disgruntled Uyghurs more likely to disrupt social order, they may attempt to placate these individuals by prioritizing their requests and exhibiting greater responsiveness.

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<sup>8</sup> Please see Appendix Table C3 for more detail. If we apply the same test to only the prefectures that actually replied to requests, the difference between treatment and control remains statistically insignificant. We cannot exclude the possibility that officials both applied greater scrutiny to the Muslim alias and then opted to not respond to those messages in greater proportion than for the unmarked alias. However, that decision not to respond represents another form of bias against the Muslim requester.

<sup>9</sup> Andrew Jacobs. "China Sentences 4 Uyghurs to Death Over Unrest." *The New York Times*. Sep. 16, 2011.

<sup>10</sup> Edward Wong. "Deadly Clashes Erupt in Western China." *The New York Times*. Mar. 1, 2012. Peter Ford. "Fight over snack in China lights up blogosphere; Controversy over how police handled a fight between ethnic minority snack vendors and a Han Chinese costumer went viral in China, highlighting discontent with 'leniency' for minorities." *The Christian Science Monitor* Dec. 4, 2012. Chris Buckley. "China Calls Clash in Tense Region a Terrorist Attack." *The New York Times*. Jun. 27, 2013.

Such behavior would constitute *pro-minority* strategic discrimination. In fact, it is not difficult to find opinions among Han Chinese that restive minorities enjoy this sort of preferential treatment from authorities.

Our main effect refutes claims that ethnic Muslims generally enjoy increased responsiveness from government agencies. However, we also tested whether official responsiveness varied with the perceived threat of local Muslim population to social stability. As illustrated in Figure 1, we found higher responsiveness to Muslims in high-Muslim regions of China. This is consistent with the social stability hypothesis. However, in these regions the combination of high concentrations of minority officials (Newland, 2013) and elevated ethnic conflict make it impossible to disentangle the effects of ingroup favoritism (now between ethnically Muslim authorities and citizens) from strategic incentives to placate restive minority populations.

To address this, we conducted additional analyses to determine whether social stability concerns moderated official responsiveness to Muslims. First, we examined effect heterogeneity by Muslim population in only localities with less than 10% Muslim population. In these localities we are unlikely to see any effects of Muslim ingroup bias, as the bureaucrats managing the Mayor's Mailbox are unlikely to be Muslim. The main effect remains robust, and we detect no effect for the interaction between local Muslim population and the experimental treatment (Table 3, columns 2 and 3). Next, we created an original data set of Muslim-related ethnic conflict in China.<sup>11</sup> Outside of Xinjiang, which we treated as an entire province afflicted by Muslim-related ethnic conflict, we recorded episodes of Muslim-related conflict in 37 prefectures over the last 25 years. Although news sources undoubtedly underreport total episodes of ethnic conflict, we use these data with the assumption that prefectures with publicized episodes of ethnic unrest, on average, exhibit higher levels of ethnic conflict than those with no reported episodes. We coded a binary indicator of Muslim-related ethnic conflict at the prefecture level and interacted this indicator with our treatment.

Local episodes of Muslim unrest should increase the probability that officials perceive Muslim citizens as threats to social stability. If these strategic considerations play a role in responsiveness, we expect to observe greater

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<sup>11</sup> We gathered reports of Muslim unrest in China from the LexisNexis English news archive, the annual Report to the U.S. Congress on International Religious Freedom, the Hong Kong-based Information Center for Human Rights and Democracy, and the Uyghur issues website <http://www.uighurbiz.net>.

**Table 3.** Heterogeneous treatment effects, Muslim population, and history of ethnic conflict.

	<10% Muslim pop. only			All prefs
	(1)	(2)	(3)	(4)
Treatment	-18.8 (5.7)	-22.0 (6.1)	-19.0 (5.8)	-15.6 (5.9)
Treatment*		5.8 (4.5)		
Muslim pop. (%)				
Muslim pop. (%)		-10.4 (4.1)		
Treatment*			0.1 (0.2)	
Muslim pop. (thousand)				
Muslim pop. (thousand)			-0.3 (0.2)	
Treatment*				-5.1 (18.5)
Ethnic conflict				
Ethnic conflict				-15.6 (16.0)
General covars	✓	✓	✓	✓
Ethnic covars	✓	✓	✓	✓
Replies covars	✓	✓	✓	✓
Observations	236	236	236	255

*Notes:* OLS estimates of the heterogeneous treatment effects. Outcome is local government response to requests for help (binary). Columns 2 and 3 interact the treatment with local Muslim population, operationalized as both percent Muslim and the total Muslim population in thousands. By limiting our sample to prefectures of under 10% Muslim population, we bound the probability that the official responding to our messages was himself/herself an ethnic Muslim. Column 4 interacts local record of Muslim-related ethnic conflict (a binary indicator) with the treatment. Robust standard errors clustered by province are in parentheses. For the list of general, ethnic, and replies covariates see the Appendix Table B1.

responsiveness to Muslims in the high-conflict regions. However, we find no significant difference in responsiveness to Muslims between localities with and without histories of Muslim-related conflict (Table 3, column 4). The point estimate is negative, consistent with the claim that the threat of out-group violence activates greater ingroup bias (Shayo and Zussman, 2011), but the estimate is highly imprecise. We find no evidence that strategic considerations surrounding social stability mediate official responsiveness to Muslims.

#### **4 Discussion**

This study demonstrates that neither legacies of institutionalized discrimination nor political incentives surrounding democratic representation are necessary conditions for large ingroup biases to distort official behavior. The troubling implication of this finding is that ethnically motivated distortions to governance may be more widespread than previously understood. Whereas few countries have the extreme legacies of racial oppression found in the United States and South Africa, many more exhibit the kind of out-group stereotyping observed for Muslim minorities in China. Our findings suggest that this broader set of countries may exhibit similarly high levels of ingroup bias in public administration.

A similar logic holds for our finding of large biases in the behavior of unelected officials. Although the elected representatives targeted by previous research are of great theoretical and substantive importance, unelected officials are more numerous and responsible for implementing a wide range of government policies. Unelected street-level bureaucrats enforce regulations, provide access to government services, and are in many ways the public face of government (Lipsky, 2010). Citizens rely upon unelected officials for a wide range of services and goods. In the United States, unelected public employees decide applications for a variety of social benefits, adjudicate legal proceedings, and police our neighborhoods. Where these agents exercise discretion, ingroup biases may distort important elements of service provision.

By documenting ingroup bias in the behavior of this new set of political actors, this study contributes to a growing empirical regularity in the study of human behavior. Individuals express ingroup biases in behavior even when making relatively consequential decisions. In addition to the stud-

ies of elected representatives cited above, scholars document ingroup bias in the labor market (Bertrand and Mullainathan, 2004), housing market (Yinger, 1995), car sales (Ayres and Siegelman, 1995), criminal sentencing (Alesina and Ferrara, 2011; Everett and Wojtkiewicz, 2002), judicial review (Sen, 2012), and even NBA officiating (Price and Wolfers, 2010). Bias in public service is particularly troubling because the equality of persons before the state is an important normative goal of democratic governance (Rothstein and Teorell, 2008).

Our findings highlight a second emerging regularity in the study of ingroup biases; it is difficult to explain these biases in terms of strategic behavior. A longstanding debate about ethnoracial bias pits intrinsic, taste-based motivations against rational, statistical ones (Becker, 1971). It is impossible to exclude all strategic interpretations for human behavior, but our study and others fail to explain observed bias with the commonly understood incentives of political officials. We detected little role for social stability incentives in official biases in China, and Butler and Broockman (2011) find that despite the strong signal of Democratic partisanship carried by black identity in the United States, white Democratic state legislators exhibited bias against black individuals. Broockman (2013) extended this work to show that black legislators help black citizens even when these citizens are not constituents. Recent work in other subfields also fails to explain ingroup–outgroup political cognition with the tools of economic rationality; Hainmueller and Hiscox (2010) find in a nationwide survey on immigration attitudes in the United States that, “economic self-interest, at least as currently theorized, does not explain voter attitudes toward immigration.”

The prevalence of ingroup bias and its insensitivity to strategic incentives present obstacles to democratic equality, but the regularity of these findings represents progress for generalized theories of human political behavior. The durability of ingroup bias across diverse cultures, ethnicities, and political regimes offers a specific example of universal regularity in human political behavior. From the perspective of public policy, social scientific investigation may have a constructive role to play in the reduction of elite biases. In the wake of media coverage of a study revealing racial bias in NBA officiating, Pope *et al.* (2013) find that bias disappeared from referees’ subsequent behavior.

The limitations of our study point to areas of future research on ingroup bias. Field experimental investigation of ingroup biases, including this study,



have to date examined relatively low-stakes transactions with officials, such as requests for information. Future work could helpfully address the extent to which ingroup biases distort higher-stakes transactions, with attention to the varying levels of official discretion present in different types of decisions. Open questions also remain about how different types of outgroups affect the magnitude of bias. How salient must ethnic cleavages be before they distort official behavior? Social psychology research has shown that even highly artificial, thin groupings can induce behavioral biases in the laboratory (Tajfel *et al.*, 1971), but it remains to be clarified which real-world groups induce distortions to consequential political behavior.

This study has additional implications for China's ethnic policy and authoritarian politics more broadly. We offer a mixed picture of how China's minorities fare in transactions with government officials. On the one hand, where Muslims are most numerous we find they receive equal or perhaps slightly preferential treatment (Figure 1). Given the skepticism with which many scholars view minority governance in China, this result is surprising and supports regime claims that government in high-minority regions is, in certain domains, responsive to local needs. On the other hand, we detect a high degree of official bias against Muslims where they are in the minority, offering quantitative support for the ethnographic and journalistic reports of discrimination faced by Muslims, particularly Uyghurs, in urban China. Eradicating behavioral biases is an uncertain project, but simply raising awareness among officials tasked with responding to citizen complaints may mitigate the bias we observed (Pope *et al.*, 2013).

In the field of authoritarian politics, our results shed new light on the importance of participatory institutions to nondemocratic state-society relations. Recent scholarship highlights the role of institutions — legislatures (Gandhi, 2008; Truex, forthcoming), political parties (Gandhi, 2008), the media (Egorov *et al.*, 2009; Lorentzen, 2013a; Distelhorst, 2013), and deliberative platforms (He and Warren, 2011) — in nondemocratic rule. We contribute by examining an institution of authoritarian contacting and find that China's local governments exhibited relatively high levels of responsiveness to citizens of low socioeconomic status. The official response rate to a generic Chinese male name was 44%, quite comparable to that of the elected councillors and representatives contacted in South Africa and the United States (Table 2). The comparison is not wholly apt, as the elected officials in previous studies had fewer resources than China's prefectural governments do,

but even so our finding adds to the growing list of authoritarian institution that are more than democratic window-dressing. China's online channels for contacting officials often deliver relevant information in a timely manner.

Highlighting the responsiveness of authoritarian officials to citizen requests opens up new research questions in authoritarian politics. Is government responsiveness to the public driven by resources, developmental imperatives, or the need to maintain social stability? Which citizen grievances receive the speediest and most effortful responses? Is authoritarian constituent service also sensitive to jurisdictional considerations (Loewen and MacKenzie, 2013)? These questions can be addressed using similar techniques to those we employ here. Authoritarian regimes generally offer sparser and less accurate public data on government activities, and media coverage of the government is also distorted by politically motivated censorship. Field experimentation involving government agencies provides an opportunity to generate original data on governance in these difficult-to-study settings. Given our finding that their contacting channels function comparably to those of elected representatives in mature democracies, perhaps China's local governments have less to fear from transparency than they think.

## **Appendix A: Names and Ethnicity in China**

The experiment compares government responsiveness to letters from a name signaling membership in a Muslim minority group (treatment) to an ethnically unmarked name (control). Similar experiments have used names that signal membership in the majority ethnic group (Butler and Broockman, 2011; Bertrand and Mullainathan, 2004; McClendon, 2013). Unfortunately Chinese names do not permit for unambiguously signalling Han ethnicity to the exclusion of all other ethnic groups. Instead, we use an ethnically unmarked name in our control scenario; for examples see the left-hand column of Table A1. The name used was highly common. China's most popular social networking website, Renren, listed over 5000 users with this full name.<sup>12</sup> As China's population was 92% Han Chinese in 2010, an ethnically

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<sup>12</sup> Searched Renren.com on Oct. 6, 2012. The total number of registered users is about 200 million.

**Table A1.** Examples of unmarked and Muslim names.

Unmarked names	Muslim names
Li Jun De	Ba La Ti (Barat)
Zhang Chao Yang	Re He Man (Rahman)
Wang Zhi Hong	Ai Er Ken (Erkin)

*Notes:* Examples of ethnically unmarked and Muslim-identifying names in Chinese, similar to those used in the experiment. They are displayed first as romanizations (pinyin) of Chinese characters, highlighting that each name consists of three characters corresponding to three spoken syllables. Common English transliterations of the Muslim names are included in parentheses. We do not disclose the actual names used in the study to protect the privacy of official responses to our requests, which were occasionally published to government webpages.

unmarked name is likely in most localities to be interpreted as belonging to a Han requester.<sup>13</sup>

The experiment uses an Arabic-root name to signal Muslim minority identity. Examples of such names are given in right-hand column of Table A1. Our treatment name was also quite common; over 3000 Renren users were registered under the treatment name.<sup>14</sup> Both the Muslim and unmarked names consist of three Chinese characters which correspond to three spoken syllables. Both are also strong signals of male gender; among registered Renren users 95% of the unmarked name and 97% of Muslim name were male.<sup>14</sup> If the officials receiving our requests searched the Internet for the treatment name (a reasonable expectation given that they were responding to an online message), they would find images of a member of the Uyghur Muslim ethnic group. In contrast, searching for our unmarked name returned images of several non-minority individuals.<sup>15</sup>

How prevalent are distinctive names among China's 23 million Muslims? China recognizes 10 ethnically Muslim minority groups, but the largest are

<sup>13</sup> People's Republic of China Sixth National Population Census, 2010. Available online at: <http://www.stats.gov.cn/tjsj/pcsj/rkpc/6rp/indexch.htm>.

<sup>14</sup> Searched Renren.com on Oct. 6, 2012.

<sup>15</sup> Image searches conducted using Baidu.com, China's most popular internet search engine, October 2012.

**Table A2.** Ethnic names in China's legislature.

Ethnic group	Population	NPC delegates	
		Total	Muslim names
Hui	10.6 m	60	0
Uyghur	10.1 m	25	25
Other Muslim (8)	2.5 m	12	9

*Notes:* Names are marked ethnically Muslim if they derive from a non-Chinese language such as Arabic, Persian, or Uyghur. Population data come from the 2010 Census. The list of 2013 National People's Congress delegates was accessed on Dec. 26, 2013 at <http://politics.people.com.cn/n/2013/0227/c1001-20621484.html>.

the Uyghur and Hui, with roughly 10 million people each. The Uyghur commonly take distinctive names that derive from Arabic, Persian, or the Uyghur language (Wushouer, 2005), whereas Hui tend to have fewer distinguishing features from the majority population. The distribution of names observed among minority delegates to China's legislature affirms this difference. Among the 97 delegates from Muslim ethnic groups in the 2013 National People's Congress, all 25 Uyghurs and a majority of delegates from the eight smaller Muslim ethnicities had distinctive names. However, Hui delegates' names did not evidently draw upon Arabic or other non-Chinese languages. The findings of our experiment are therefore most relevant to Uyghurs and other Chinese Muslims that take distinctive names.

## Appendix B: Sample and Treatment Assignment

We assessed 336 prefectural governments in China<sup>16</sup> for eligibility for inclusion in the study. We identified 299 eligible localities that offered online contacting channels and did not require letter-writers to provide their identification card numbers.<sup>17</sup> We randomly allocated 150 localities to the treatment group (message from Muslim name) and 149 to the control group

<sup>16</sup> This includes the four province-level cities of Beijing, Shanghai, Chongqing, and Tianjin, which are technically one administrative level above ordinary prefectures.



**Figure B1.** Chinese prefectures included in field experiment.

*Notes:* 258 Chinese prefectures contacted in the field experiment shaded blue. Satellite imagery ©2013 NASA, Terrametrics, accessed using Google Maps API.

(ethnically unmarked name) without stratification. We experienced attrition of 22 treated and 19 control localities due to non-functioning online interfaces and invalid email addresses. The 258 contacted political jurisdictions govern over one billion people (Figure B1). Pretreatment covariate balance for contacted localities in treatment and control groups is reported in Table B1.

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<sup>17</sup> We found 57 local variants of online channels for contacting local government, including Government Mailbox, Ask-About-Government Web, Mayor's Hotline, Write to the Mayor, and Leaders' Window. The most common name for this institution was Mayor's Mailbox (65%).

**Table B1.** Balance table for submitted requests.

	Muslim alias	Unmarked alias	<i>T</i> pval	KS pval
<b>General</b>				
Area (sq km)	26,089	29,635	0.59	0.83
Population (million)	3.93	4.05	0.76	0.37
GDP (billion RMB)	94.9	111.2	0.30	0.98
per capita GDP (RMB)	27,202	28,702	0.70	0.74
Primary GDP (%)	15	16	0.70	0.75
Secondary GDP (%)	49	47	0.20	0.44
Tertiary GDP (%)	36	37	0.15	0.43
Govt revenue (billion RMB)	6.06	7.92	0.20	0.77
Govt spending (billion RMB)	12.41	14.66	0.22	0.80
Imports (million USD)	1221	2758	0.13	0.33
Exports (million USD)	1676	4144	0.09	0.39
FDI (million USD)	332	480	0.22	0.78
<b>Ethnicity</b>				
Muslim (%)	4.5	3.3	0.48	0.21
Han (%)	84.4	86.8	0.43	0.91
History of Muslim conflict (%)	14.8	16.2	0.77	
<b>Posted replies to citizen requests, June 2012</b>				
>10 replies (%)	47	46	0.91	
1–10 replies (%)	20	23	0.59	
No replies (%)	33	31	0.73	
Obs.	128	130		

*Notes:* Means of pretreatment covariates for prefectures in the treatment (Muslim alias) and control (Unmarked alias) groups. Right-hand columns report *p*-values for two-sided *t*-tests and the univariate bootstrap Kolmogorov–Smirnov tests, which tests for differences in distributions of non-dichotomous variables. Posted replies to citizen requests were recorded by the researchers during visits to government webpages. Ethnic data are from the 2000 China Census, and all other data are from *China Statistical Yearbook for Regional Economy 2010* (China Statistics Press). The *F*-test for regressing treatment status on all listed covariates gives a *p*-value of 0.698.

**Appendix C: Supplementary Tables****Table C1.** Contacted and unreachable prefectures.

	Contacted	Unreachable	<i>T</i> pval
<b>General</b>			
Area (sq km)	27,876	29,605	0.80
Population (million)	3.99	3.57	0.24
GDP (billion RMB)	103.1	134.5	0.27
per capita GDP (RMB)	27,958	31,095	0.42
Primary GDP (%)	16	15	0.59
Secondary GDP (%)	48	46	0.39
Tertiary GDP (%)	37	39	0.10
Govt revenue (billion RMB)	7.0	12.5	0.20
Govt spending (billion RMB)	13.5	18.9	0.27
Imports (million USD)	1996	6244	0.16
Exports (million USD)	2919	5691	0.27
FDI (million USD)	406	633	0.26
<b>Ethnicity</b>			
Muslim (%)	3.9	2.1	0.12
Han (%)	85.6	83.1	0.50
History of Muslim conflict (%)	16	14	0.76
<b>Posted replies to citizen requests, June 2012</b>			
>10 replies (%)	47	24	0.00
1–10 replies (%)	22	21	0.82
No replies (%)	32	55	0.00
Obs.	258	78	

*Notes:* Covariate means for prefectures contacted in the field experiment compared with uncontacted prefectures. Right-hand column reports *p*-values for two-sided *t*-test. Ethnic data come from 2000 Census, all other economic and demographic data from 2010 statistical yearbooks. Posted replies in the Mayor's Mailbox were recorded by the researchers in July 2012.

**Table C2.** Government response rates and quality of response ( $N = 258$ ).

Disclosed any relevant information	43.0%
Disclosed . . .	
Name of government agency	38.8%
Standards for support	28.3%
Procedures for approval	20.2%
Contact information	11.6%
Compensation level	2.7%
Mean score for disclosures (max. 5)	2.36
Replied, no disclosure	7.4%
No reply	49.6%

**Table C3.** Government response noted inaccurate contact information.

	Unmarked	Muslim	Difference
Noted inaccurate info (%)	11.5 (2.8)	11.7 (2.9)	0.2 (4.0)
95% CI upper	17.1	17.4	7.7
95% CI lower	6.0	6.1	-8.1
Obs.	130	128	

*Notes:* Outcome is whether contacted agency responded that requester's phone or address was inaccurate. For all requests, phone numbers and street addresses were designed to be plausible but inaccurate. Standard errors (in parentheses) and 95% confidence intervals reported below. Two-sided  $t$ -test (unequal variance)  $p$ -value = 0.964.



**Table C4.** Ethnicity and government disclosure, alternative time cutoffs.

	Unmarked	Muslim	Difference	<i>p</i> -Value
Disclosure (%) after:				
1 week	30.0	18.0	−12.0	0.02
2 weeks	38.4	22.7	−15.8	0.01
3 weeks	41.5	28.1	−13.4	0.02
4 weeks*	44.6	29.7	−14.9	0.01
5 weeks	44.6	31.3	−13.4	0.03
6 weeks	46.9	35.2	−11.8	0.06
...				
24 weeks	49.2	36.7	−12.5	0.04

*Notes:* Comparing mean disclosure rates for control and treatment groups at 7, 14, 21, 28, 35, and 42 days after requesting information from China's prefectural governments in July and August 2012. Our standard specification of the dependent variable uses a four-week cutoff. Rightmost column reports *p*-values for two-sided *t*-tests (unequal variance).

**Table C5.** Ethnicity and government disclosure, regression estimates.

	(1)	(2)	(3)	(4)	(5)
Muslim	−14.9 (6.0)	−14.6 (6.2)	−16.1 (6.3)	−16.3 (5.9)	−15.8 (6.2)
General covars		✓	✓	✓	✓
Ethnic covars			✓	✓	✓
Replies covars				✓	✓
Province FE					✓
Obs.	258	258	255	255	255

*Notes:* OLS estimates for effect of muslim alias on government disclosure (robust standard errors in parentheses). The dependent variable is government disclosure four weeks after the request was made. General, ethnic, and replies covariates match the variables listed in Table B1. Three prefectures did not report ethnic data, which is why these observations are missing in estimates (3), (4), and (5).

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