

Bringing Together the Studies of Ethnic Prejudice and Conflict in Chinese Politics

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Patterns of bias against Muslim individuals are frequently observed in China. Uyghur Muslims are more likely to receive unfavorable treatment by the police and other government agencies; they face difficulties in the labor and housing markets; and they seldom appear in top political and corporate positions. However, in the social science literature, there is little work on systematically identifying ethnic bias and discrimination in China. China is the world's most populous country with 23 million ethnic Muslims; it also has experienced a surge in Islamic extremism, resulting in violent events in the recent decades. Practically, studying ethnic prejudice could produce useful policy implications to alleviating the current ethnic tensions; theoretically, China provides an important yet under-studied empirical case in the ethnic politics literature.

This memo introduces recent developments in the literature on the study of ethnic conflict and prejudice in China with a focus on the Chinese Han-Muslim relations,² and proposes ways for more synthesis of these two bodies of scholarship.

I. Ethnic Conflict in China

Ethnic violence in China has increased in frequency and severity since the late 1980s, particularly between the majority Han Chinese and the Uyghurs, a Sunni Muslim ethnic group with a population of 10.1 million.³ One of the most serious outbursts occurred in July 2009 in Urumqi, the capital of the Northwestern Xinjiang Autonomous Region, where the majority of Uyghurs reside. A protest quickly turned into a harrowing ethnic riot, resulting in almost 200 deaths, more than 1700 injuries, and an additional two days of violence due to Han retaliation (Han 2013). Ethnic violence such as this is difficult to study in China because of data and access

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² Here, I do not wish to argue that Islam has a distinctive effect on ethnic prejudice and violence in China. This piece focuses on the Han-Uyghur relations because the literature on this particular pair of ethnicities is more developed.

³ According to the 2010 Population Census of China published by the National Bureau of Statistics. Available at <http://www.stats.gov.cn/tjsj/pcsj/rkpc/6rp/indexch.htm>.

constraints: reporters and scholars have limited access to Xinjiang, and many details of incidents go unreported (Hong and Yang 2017).

Despite these difficulties, there is a rich historical, anthropological, and ethnographic literature on Xinjiang and conflict within it (e.g., Bovingdon 2013; Cliff 2016; Finley 2013; Han 2011; Jacobs 2016; Millward 2007; Rudelson 1997). Two recent studies represent a new trend in studying ethnic conflict in China: *using rigorous empirical tools and large datasets to make causal inference about ethnic conflict*. Hong and Yang (2017) study the effects of natural resources on the onset of violent incidents in Xinjiang. They compiled all available violence data from 1949 to 2014 in Xinjiang, building upon important data sources such as Bovingdon (2013). Using this dataset and a few other data sources, they find no evidence to support the “resource curse” hypothesis in China: Areas with more resource production or endowments have lower rate of conflict, but this effect goes away in areas with high mosque density.

The finding that areas with stronger presence of religious institutions are more prone to violence is in direct contrast to a second recent study by Cao et al. (2016), where the authors argue that religious institutions in Xinjiang have a “conflict-dampening” effect because they provide local public goods, enable information flow between the local population and the government, and facilitate collective bargaining.⁴ Cao et al. (2016) focus on ethnic violence in Xinjiang between 1990 and 2005 to create a comprehensive database, the Ethnic Violence in China (EVC) database, identifying 213 violent events including bombings, assassinations, riots, arson and armed attacks. Using this data source, they find that between-group inequalities motivate oppressed groups to revolt, but the effect is dampened by the strength of religious institutions.

These two papers represent one of the first attempts to systematically analyze quantitative conflict data in China. The willingness of the authors to share their conflict datasets will likely generate interest in studying ethnic conflict in China from both China scholars and conflict scholars.⁵ One area for more scrutiny is that neither paper provides convincing evidence on the causes of conflict in China. Informed by earlier theories on grievances, collective action, and conflict, Cao, et al. (2016) propose the causal chain that between-group inequality, proxied by education attainment and occupation categories, produces grievances, which in turns fuel ethnic conflict in

⁴ The diverging effect of religious institutions on ethnic conflict in these two articles could come from the different time periods their dataset cover, how they define violence, and how they measure the presence or strength of religious institutions.

⁵ See Cao et al. (2017) that delineates the EVC dataset in detail.

China. These propositions are plausible, but they need more fine-grained empirical evidence to back each part of this causal chain. Does economic inequality always lead to political grievance (Sambanis 2001)? How do minority leaders use grievance to mobilize the discontent into actionable products in a strong state? Are ethnic minorities more likely to use violence to channel grievance? Similarly, Hong and Yang (2017) admit that they do not have enough evidence to analyze the causes of the fluctuation in violence in Xinjiang. Their proposed hypothesis is that areas with more natural resource extraction experience more abundant employment opportunities, and these benefits are shared by both Han and Muslim residents, resulting in lower rates of violence.

Many questions regarding the causes of ethnic conflict remain to be answered in this new line of works. Does the lack of sustained communication across groups always instigate intergroup violence (Han 2010)? Do economic development and employment always suppress inter-group grievance? Are there systematic biases against ethnic minorities in the public sphere (Grossman et al. 2015)? How does discontent translate into group violence? From the perspective of the political elite, how exactly do they mobilize ethnic groups and achieve mass compliance? What are the psychological and small-group mechanisms to achieve compliance (Green and Seher 2003)?

Green and Seher call for a synthesis of the study of ethnic violence and ethnic bias, as the incorporation of the latter would provide more systematic evaluation of causal hypotheses to the study of ethnic conflict. Similarly, Sambanis (2002) suggests that the civil violence literature should continue to develop a closer fit between macro-level and micro-level theory. Such synthesis is also very much needed in the study of ethnic politics in China. In the next section, I review some new works on the measurement of ethnic bias in politics, in the labor market, and in attitudes towards certain public policies in China. They provide answers to some of the questions above and I argue that this line of work could contribute to the micro-foundations of the scholarship on conflict.

II. Ethnic Bias in China

Earlier works on ethnic bias in China, just like the general literature on discrimination and bias, mostly employ individual-level outcome regressions, where ethnic bias is estimated after controlling for other observable factors that might also determine the outcomes, such as income or employment (e.g., see Hannum and Xie 1998; Wu and Song 2014; Zang 2008). The biggest limitation of this approach is these works often suffer from omitted variable bias: in the labor market, for instance, ethnicity could be

“correlated with other proxies for productivity that are unobservable to the research but observed by the employer” (Bertrand and Duflo 2017,7). Also, minority workers could have sorted themselves into industries with limited discrimination, but industry-specific discrimination would not be identified if industry fixed effects are included in the specifications (Bertrand and Duflo 2017).

Experiments offer a solution to the problems of the traditional regression approach when studying ethnic bias. Below I introduce three new studies that use this approach to measure ethnic bias in China.

Do political elites exhibit bias towards their co-ethnics? Using a national audit experiment where they send fictitious informational requests to mayors and altering names which indicate different ethnic identities, Distelhorst and Hou (2014) find that Chinese officials were 33% less likely to respond to informational request to citizens with Uyghur names than Han names.

Do individuals react to violence differently, if the perpetrators are coethnic? In another working paper, Hou and Quek (2017) study individuals’ responses towards security threats imposed by coethnic and non-coethnic nationals. Using a series of survey experiments on a national sample of Chinese citizens, where respondents were randomly selected to view reports on episodes of violent acts, they find that individuals are more likely to approve the government’s use of lethal force, independent of the identities of the perpetrators. They find suggestive evidence that individuals exhibit intergroup biases when exposed to reports of violence.

Do ethnic minorities face discrimination on the job market, even if they appear as qualified as their Han competitors? In a work-in-progress, Liu et al. (2017) use a correspondence study research design where they create fictitious job applicants who send resumes to employers to apply for jobs. Each job applicant is assigned either a Muslim or a non-Muslim identity. They measure bias (or the lack of) by comparing the outcomes (i.e., call back rates) between similar applicants with and without an ethnic identity marker. Maurer-Fazio (2012) conducted a similar experiment where she compares Han, Mongolian, and Tibetan applicants in China; Liu et al. (2017) builds upon this earlier study and include another dimension of treatment; specifically, they included a variable — the asking salary — to measure the “elasticity” of ethnic bias.

These experiments have their own limitations: the outcomes variables are usually a coarse proxy of what they are intended to measure; the experimental scenarios can look unrealistic; and there are ethical concerns regarding the use of deception. Nevertheless,

these works provide “disciplined evidence”⁶ regarding individual-level ethnic bias or the lack thereof in official behavior, individual attitudes towards certain public policy, and the labor market.⁷

III. Conclusion

A micro-level understanding of ethnic bias can provide important insights to scholars interested in examining outcomes and mechanisms in the conflict literature. Can group preference be constructed or manipulated by politicians? Under what conditions can bias manifest into violence? How do elites make decisions? Are decisions and attitude appeared to be based on ethnicity in fact driven by other qualities of that group? Along similar lines, the scholarship of ethnic bias, often accused of lacking external validity and focusing on narrow sets of outcomes, could benefit from more discussion on the consequences of bias, one of which is its influence on ethnic conflict. There is great need, and much to be gained, from more dialogue between scholars of ethnic bias and ethnic conflict.

⁶ This disciplined approach does not involve “ad hoc (even if intuitive) addition to the utility function (animus toward certain groups) to help rationalize a puzzling behavior” (Bertrand and Duflo 2017, 3).

⁷ And if designed well, they could help distinguish between taste-based discrimination and statistical discrimination. For an explanation between these two types of discriminations, see Bertrand and Duflo (2017), 3–4.

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