

Minority Language Recognition and Political Trust in Authoritarian Regimes

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Abstract

While authoritarian regimes are often characterized by their civil liberty restrictions, some dictatorships acknowledge the ethnolinguistic diversity of their population. Are minorities in multiethnic authoritarian states more likely to trust the government when their language is recognized? In this paper, we argue while recognition of a group's language improves trust in democracies through a substantive representation mechanism, the same cannot be said in authoritarian regimes. Instead, recognition is a mere symbolic gesture. Such window-dressing efforts call attention to the horizontal inequality between hegemon and minority groups—and such, minority language recognition is associated with negative political trust. We test our argument with the World Values Survey. By identifying which minority groups have been afforded linguistic recognition, we find evidence of a significant—but negative—link between recognition and political trust.

Keywords

authoritarianism, ethnic politics, language politics, political trust

Authoritarian regimes are often characterized by their limitations on political participation and civil liberties. Yet, some dictators embrace the ethnolinguistic diversity of their population. The Soviet Union, under Lenin, for example, recognized the linguistic heterogeneity of the country—as evident by the use of 15 languages on the *ruble* banknote (Grenoble 2003). Likewise, in China, under Mao, the CCP counted over 50 ethnic groups—from the Kazakhs to the Mongols, from the Tibetan to the Uyghurs (see Tang, Hu, and Jin 2016). And in North Vietnam, Ho Chi Minh's government was more accommodating of the ethnic diversity of its citizens than its American-backed Southern counterpart (Vasavakul 2003). This is by no means a communist-phenomenon: From Paraguay's Stroessner (Miranda 1990) to Singapore's PAP (Ostwald, Ong, and Gueorguiev 2019), we see recognition of minority languages. In this paper, we ask: Are minorities in multiethnic authoritarian states more likely to trust the government when their language is recognized?

On the one hand, we know that substantive representation matters in democracies. When minorities have a voice to extract policy outcomes, this ensures confidence in the government (see Huebert and Liu 2017; Huo et al. 1996). On the other hand, given that barriers for political participation are much higher in authoritarian regimes—whether it is at the ballot box or on the streets (Baum and

Lake 2003; Brown and Hunter 2004)—it is possible that minority language recognition may not generate the same effects. Recognition is not just a legal act; it requires actual resources to back it. It is one matter to make a minority language the language of the anthem; but it is another matter if no one is learning the meaning behind the words—let alone the language. Likewise, it is one thing to allow minority languages to be broadcasted on radio; it is another thing if the content and the number of hours are heavily censored. In this paper, we examine whether minority language recognition has a similar positive effect on political trust in authoritarian regimes. We argue no. In authoritarian regimes, recognition is less about substantive policy concessions and more about symbolic gestures—that is, it is window-dressing. What is seen as linguistic concessions are in fact an instrument for dictators to (1) paint themselves publicly as benevolent and magnanimous and (2) buy off the minority political elites.

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And as a result, minority language recognition exacerbates the horizontal inequalities between the hegemon and minority groups—thereby undermining political trust.

We test our argument on the authoritarian regimes sampled in the sixth wave of the World Values Survey. We begin by identifying whether a minority language is recognized—and if so, in what capacity (education, regional public administration, and/or national language). We focus on language because of its importance for group identity (Hu 2020; Medeiros 2017; Ricks 2020). How a government responds to a language—especially a minority language—is a signal of the regime’s broader ideology and specific attitudes toward speakers of that language. Recognition validates a group’s existence. And in contrast, denial of the language suggests inferiority (see Blommaert 2011; Cardinal 2005).

The remainder of the paper is structured as follows. We begin by reviewing the literature to explain political trust in authoritarian regimes—citing concerns with existing work and the contributions of this paper. We then explain the logic underlying our argument, emphasizing (1) why dictators would extend linguistic recognition and (2) drawing extensively on theories of horizontal inequality to explain why recognition does not beget political trust. We test the effects of linguistic recognition using data from the World Values Survey. The results—robust to different model estimators and model specifications—indicate that, as expected, linguistic recognition matters for political trust, but in a negative direction. We conclude by highlighting the normative implications.

Explaining Political Trust in Authoritarian Regimes

What explains political trust? Whether the focus is on government performance (Andrews-Lee and Liu 2020; Askvik, Jamil, and Dhakal 2011; Chanley, Rudolph, and Rahn 2000; Espinal, Hartlyn, and Kelly 2006; Morris and Klesner 2010; Wong et al. 2011); citizenship participation (Freitag and Bühlmann 2009; Newton 2001; Rothstein and Stolle 2008); or institutional designs (Hoddie and Hartzell 2003; Kittilson and Schwindt-Bayer 2010; O’Leary 2019), one common denominator among these works is the exclusive focus on democracies (c.f., Hu 2020; Ricks 2020). Yet, political trust is by no means democracy-specific.

From an empirical standpoint, political trust is not necessarily higher in democracies. In fact, per the World Values Survey’s most recent wave, average trust levels (“how much confidence do you have in your government [in your national capital]”) are lower in democracies

(1.27) than in non-democracies (1.66). This is consistent with the likes of Huang and Schuler (2018) and Mauk (2020) who find that citizens in certain authoritarian regimes exhibit higher levels of political trust than their democratic counterparts. And while this gap could be the result of regime-driven preference falsification, its presence begs attention.

Related, from a theoretical standpoint, if institutional trust is an indicator of diffuse political support (Easton 1965), it is possible that some dictators may care more about political trust than their democratic counterparts. For one, an exit from political office for a dictator can be more seismic than the simple voted-out-of-office mechanism found in democracies. The consequences are more punitive (Debs and Goemans 2010). And thus, dictators must employ different—although not mutually exclusive—strategies to stay in power.

One is to simply repress—a strategy that is neither effective at generating public support nor cheap (Acemoglu and Robinson 2005). Alternatively, dictators can censor and disseminate *which* information the public sees and *how* it sees it—thereby influencing public trust (Barber and Miller 2019; Cantoni et al. 2017; Chen and Xu 2017; Kao 2021; Stockmann 2013; Voigtländer and Voth 2015). Third, autocrats can offer rents to buy political support. In exchange for material benefits (Burns 2003; Gimpelson and Treisman 2002; Smith 2004), the coopted elites become vested in the system—thus making them more likely to support the regime (Geddes 2006; Greene 2007). Finally, the dictator can make policy concessions (Gandhi and Przeworski 2007; Magaloni 2006; Malesky and Schuler 2010)—credible commitments to not expropriate or renege (Gehlbach and Keefer 2012; Wright 2008).

Regardless of strategy, consistent across most of these works is an emphasis on the economy. Governments are more likely to stay in power when the economy is robust. When the economy is strong, there is some inherent authoritarian bargain between governments and business elites. When the economy is weak, however, splits within and defections from the ruling party are more likely (Gandhi and Reuter 2011; Pepinsky 2009). Mass protests are also more common with a floundering economy (Bratton and van de Walle 1997).

Yet, the economy is not the only sphere of relevance. Social and cultural matters can also be important. Repressing a group’s identity—whether it is an outright prohibition or relative limitations—can lead to distrust of the political system. Bangladesh’s 1971 secession (Mohsin 2003); Malaysia’s 1969 riots (Liu 2015); and Somalia’s 1969 military coup (Laitin 1977) are all examples of what happens when a group perceives its linguistic voice being stifled. In this paper, we shift our attention toward social policies. We

are interested in the effects on political trust when authoritarian governments recognize minority languages.

The Effects of Minority Language Recognition

On a global scale, linguistic diversity is the norm. Consider that (1) most of the major languages in the world are spoken across multiple countries and (2) most countries are homes to multiple languages. As such, language policy is a matter confronting most governments. Given this discussion, are minorities more likely to trust the government when their language is recognized?

On the one hand, there are theoretical priors to believe withholding recognition has a negative effect on trust—specifically, among the minority population (Liu, Brown, and Dunn 2015). When a government withholds linguistic recognition from a group, it signals the group's cultural inferiority and social backwardness—a message Taiwan's Kuomintang aggressively sent to the Hoklo- and Hakka-speaking Benschengren population (Wu 2021a). To deny a group's language is to kill a group's very identity (Safran and Liu 2012). Moreover, when a minority group is denied substantive representation, this can translate into negative political trust (see Bratton, Haynie, and Reingold 2007). Critically, this means the absence of *diffuse* support, that is, not trusting the government in general. This is important because when there is a future policy that is deemed less than ideal, the response becomes the (call for the) dismantling of the system as opposed to accepting the outcome (see Huebert and Liu 2017; Huo et al. 1996).

On the other hand, there are theoretical considerations that give us pause on whether recognition necessarily increases political trust—particularly in dictatorships. The link between popular preferences and policy outcomes is found more commonly in democracies than in authoritarian regimes (Cheibub, Gandhi, and Vreeland 2010). It is possible that given the absence of free and fair elections, minority language recognition does not necessarily improve political trust for the autocrat. And in fact, Hu (2020) finds that in China political trust increases when exchanges with bureaucrats take place in the hegemon vernacular versus the minority vernacular.

Instead, dictators are offering recognition not as a policy concession per se to acknowledge the minority group but rather as a symbolic gesture. Consider Catalan in Spain. When Francisco Franco's troops entered Catalonia in 1939, Catalan was immediately banned from public use (Branchadell 1999). However, by the late 1940s, Franco made concessions: Catalan was allowed in publications, theater performances, and some public places. Then in the 1960s and early 1970s—after several waves of political opposition movements

(Balcells 1996) and cultural resistance (Pujol Casademont 2020) – the Franco regime allowed for Catalan cultural organizations and created Chairs of Catalan language and literature at universities (Claesson 2022: 63–64). According to Dowling (2018), this shift in language policy aimed to coopt conservative Catalan-speaking elites (379). Note, however, that these linguistic concessions were not substantive. Severe censorship was enforced: Catalan could only be used for unessential products; even cookbooks and children's books in Catalan were prohibited (Pujol Casademont 2020). Moreover, Catalan cultural organizations risked being shut down at any time (Dowling 2018: 377). These symbolic concessions—set against substantive restrictions—would not affect winning political trust among the Catalans (Dowling 2018).

Yet, dictators make such symbolic gestures to minority groups for two reasons. First, it allows the autocrat to publicly claim its benevolence and magnanimity. The intended audience can include the general domestic public (e.g., Spain under Franco) and/or an international party (e.g., Singapore under the PAP pre-1963—see Liu 2015). It is no coincidence that (almost) every communist regime started out recognizing minority languages. The regime needed to portray itself as being inclusive—hoping it would yield political trust (Wintrobe 2001: 38). Second, autocrats can use minority language recognition to coopt minority political elites. Even if the gestures are mere window-dressing, the printing of road signs, the developing of textbooks, and the owning of media channels are all opportunities for rents. These rents ensure the minority political elites stay loyal to the regime—and for them to stay relevant with their respective minority populations.

While recognition may buy off the political elites, its hollow nature eventually calls attention to and exacerbates the inequity between the minority group and the hegemon. For those in the minority, horizontal inequality along cultural dimensions matters (see Stewart 2000, 2005). When recognition is seen more as window-dressing than one of substance, it activates a minority's perceptions of cultural (and possibly political) inequity between them and those in the hegemon group (Horváth, Csata, and Székely 2021; Medeiros, Fournier, and Benet-Martinez 2017). The activation need not be overnight, but instead it is something that builds up over time. Mundane, repeated interactions with bureaucrats can remind minorities and reinforce the notion that they are second-class citizens (Jap 2021)—despite the supposed legal recognition of their language. This frustration of course happens in democracies as well (see Csata et al. 2021b), but the difference is that in democracies, the minority population can mobilize and have political leaders make such demands. In authoritarian regimes, such mobilization may not be possible; moreover, the political leaders may be

non-sympathetic as they have been coopted (see [Wu 2021b](#)). Given this discussion, we argue the following hypothesis:

Hypothesis: *Minority language recognition in authoritarian regimes is associated with negative political trust among the minorities.*

Note that our hypothesis is strictly about the linguistic minorities. Here, we remain agnostic as to the effects for those in the hegemon group. In a democracy, freedom of assembly, information, and movement mean there is a normative tolerance for multiculturalism ([Liu 2017](#)). Ethnic minorities can also mobilize as a political party—gaining the support of a larger party. These mechanisms—while they *can* exist—are generally absent in authoritarian regimes. Therefore, we have no expectations about the linkage between minority language recognition and political trust among the hegemon group.

Research Design

To test our argument, we employ survey data from the sixth wave of the World Values Survey (2010–2014). The WVS offers two advantages over other cross-national survey datasets. First, the WVS sample includes more authoritarian regimes—a necessary condition for our analysis—than the regional barometer samples. In the sixth wave, there are 31 “non-democracies” per Freedom House¹—that is, countries with either “partially free” (3–5) or “not free” (6–7) political rights—and 22 authoritarian regimes per Polity.² While this may be a smaller sample of countries than we usually see in the literature (often a democracy sample), it is important to recognize that by definition surveys are hard to administer in authoritarian regimes. There are places where they are forbidden. And then there are cases where they are permitted but politically sensitive questions are not. In short, despite the small N , we are confident that this is the largest N possible given the structural constraints. To help bolster our confidence in the results, we test for model sensitivity to outliers (more below).

The second advantage is that unlike other global surveys (e.g., International Social Survey Program’s Role of Government), the WVS survey asks the necessary question regarding ethnolinguistic identity. Instead of using conventional census classifications—categories that have been institutionalized and are politically assigned (see [Csata, Hlatky, and Liu 2021](#))—we focus on the respondent’s mother tongue: “What language do you normally speak at home?” Answers to this question allow us to identify the relevant minority sample based on how respondents see themselves and not how governments view them. This distinction is

particularly important in authoritarian contexts where governments may have an incentive to repress—if not outright deny the existence of—certain minority groups. In Turkey, for example, the Kurds—despite speaking a language distinct from Turkish—were considered “Mountain Turks” by the government ([Bartkus 1999](#)). Prior to 1991, such classifications would have rendered Kurds and Turks into the same ethnic classifications although the two vernaculars are mutually unintelligible.

Using the mother tongue question, we removed respondents who indicated they spoke the hegemon language at home—per [Cederman, Wimmer, and Min \(2010\)](#) first and then [Leclerc \(2020\)](#). We do so because we are interested specifically in how ethnic minorities—and not the population at-large—respond to recognition from the government. However, since there may be multiple minority languages in a country, we restrict our focus to only the largest minority group. We do so for three distinct reasons. First, from a theoretical standpoint, there is no reason to believe the recognition of *any* minority language will have the same effect on *all* speakers of *a* minority language. Whether the mechanism between recognition and political trust is about minorities feeling legitimized (positive) or perceiving inequities (negative), the recognized language is group-specific. In fact, it is possible that one minority group may prefer no recognition for all minority groups than a rival minority group being the only one afforded recognition.

Second, from a research design standpoint, when coding for whether a minority language is recognized presupposes there is a minority group with a distinct language. But whether a distinct vernacular is considered a language is subject to the ideology and narratives put forth by the national government. A language, after all, is a dialect with an army and a navy (see [Safran and Liu 2012](#)). Therefore, it is possible that a WVS respondent says they speak a distinct vernacular at home but datasets like [Cederman, Wimmer, and Min \(2010\)](#) would consider them members of a larger group. In Malaysia, for example, respondents can choose “Cantonese” as an option for a language spoken at home. From a linguistic standpoint, Cantonese is mutually unintelligible with Mandarin Chinese (at below 20%). Yet, the [Cederman, Wimmer, and Min \(2010\)](#) dataset—and the Malaysian government—would consider these individuals as members of the Chinese minority. Many countries and many “groups” have this coding conundrum. To minimize the coding error and normative biases, we focus only on the largest minority group. Thus, in the aforementioned Malaysia example, the Chinese are recognized as the minority group. And from there, we code respondents who speak any Chinese vernacular as members of the group.

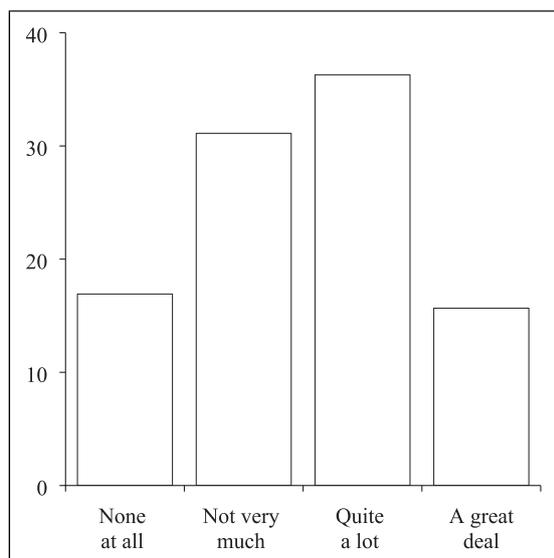


Figure 1. Distribution of political trust among largest minority (% respondent, $N=5213$).

The third reason for focusing on only the largest minority group is an empirical one. There are two considerations. On the one hand, in some countries, some respondents speak languages that are not traditionally associated with that country. In Algeria for example, 14 respondents out of a total of 1200 indicated they speak French at home. While Algeria was a former French colony, French is not usually considered a minority language akin to Amazigh ($N = 68$). Recognition of the colonial language in this case should have no bearing on whether a minority feels legitimized or perceives inequity. On the other hand, in some countries, there are respondents who speak other autochthonous minority languages. And while they are included in the survey, they are often severely under-sampled. For example, Kazakhs are the second largest ethnic minority in Uzbekistan (2.5%). Yet, there are only 18 Kazakhs in the WVS survey (1.2%). This discrepancy—combined with the inherent smaller group size—makes any credible estimation and inference difficult. The identity of the minority group for each country can be found in the [Supplemental appendix](#) (table A).

Political Trust

To measure trust, we use the following question (V115):

“Could you tell me how much confidence you have in [the government (in your nation’s capital)]: is it a great deal of confidence, quite a lot of confidence, not very much confidence, or none at all?”

After removing the “no answer” ($N = 467$; 1.02%) and “don’t know” ($N = 977$; 2.15%) responses, the question ranges along a four-point scale from “none at all” (0) to “a great deal” (3). Note that we have recoded the measures such that higher values correspond to more trust. As illustrated in [Figure 1](#), the distribution for this variable is largely normal with a mean between “not very much” and “quite a lot” (1.55). More than 36% of the respondents said they had quite a lot of confidence in the government. The distribution for each country can be found in the [Supplemental appendix](#) (table B).

Here, we forego other measures of political trust—that is, parliament, political parties, courts, and bureaucracy—for both theoretical and empirical reasons. We are aware that the convention is to either run a model against a battery of different measures (see [Liu, Brown, and Dunn 2015](#)) or to aggregate them into one single index (see [Stoyan et al. 2016](#)). However, the fact that these institutions vary significantly in authoritarian regimes gives us reason to pause. The variations are not simply about electoral rules and other power-sharing arrangements; nor are they about the separation of power across multiple veto players. In some cases, it is actually about the lack of such institutions. Citizens cannot trust a parliament when no such legislative body exists. Similarly, people cannot evaluate a party system when parties are banned. And indeed, in some of the countries in our sample, both legislatures and parties are proscribed. Given these constraints, the only institution consistent across all countries is the national government.

Minority Language Recognition

It is essential to recognize that recognition is multifaceted. There is a qualitative difference between symbolic and substantive recognition. The former is window-dressing and cosmetic. It allows the government to credit-claim minority language recognition, but for whatever reason—for example, there are additional restrictions—speakers of the minority language are not truly conferred the requisite equality and legitimacy. Examples of this include recognition of indigenous languages. Indigenous languages are often afforded some recognition given the historical circumstances; the use of the language, however, is limited ([Foxworth, Liu, and Sokhey 2015](#)). In contrast, substantive recognition is more than just cosmetic; it is something concrete. Minority languages are used meaningfully in official context. In Yugoslavia under Josip Tito’s tenure, for instance, Serbo-Croatian, Macedonian, and Slovenian were all recognized as languages of the federal state. Additionally, minorities in Kosovo and Vojvodina were able to use their native languages: Albanian and Hungarian, respectively ([Leclerc 2020](#)).

To measure recognition, we use data from Jacques Leclerc's (2020) *L'aménagement linguistique dans le monde* database. The database, based at the Université Laval in Quebec, is an encyclopedic reference of language policies in all countries and many subnational territories. We code recognition as a four-point ordinal index. Using a multi-point index allows us to assess not just whether there is recognition but also the level of recognition. First, if the minority language is recognized as official and used in public administration at the *national* level—and therefore by default is also taught in schools—we assign recognition a value of “3.” An example of this is Malay in Singapore. Second, there are some cases, however, where the minority language is used only in regional administrative matters but still in schools—for example, Tatar in Russia. In these instances, recognition is a “2.” Third, for minority languages that have no official status in public administration but are still allowed in the classrooms—for example, the Chinese in Malaysia—we code them with a “1.” It does not matter how many hours or the extent of penetration in the curriculum. And finally, if the minority language is not recognized in any capacity, the variable is assigned a value of “0.” This would be the case of the Kurdish population in Turkey. Per our coding, the largest minority language is recognized in some capacity in five out of every eight countries (in our sample). However, only one out of every eight recognizes the minority language as a language of *national* public administration and the education curriculum.

Control Variables

We include control variables at both the individual and country levels. At the individual level, we consider the respondent's demographics to measure some deeply embedded disposition. For instance, older, well-educated, and/or male respondents tend to trust the authorities more (Fitzgerald and Wolak 2016). We focus on four such demographics: gender (1 if female), age (average: 40), educational attainment (ordered nine-point scale), and marital status (1 if married).

At the country level, we consider three sets of controls. The first is the country's economic growth in the surveyed year (data source: World Development Indicators). We do so because a strong economy can increase confidence in the government (Chanley, Rudolph, and Rahn 2000; Espinal, Hartlyn, and Kelly 2006).³

Second, we control for the ethnic makeup of the country. The logic is that diverse countries tend to be more conflict prone (Cederman, Wimmer, and Min 2010); these differences in turn can corrode public trust (Medeiros, von Schultz, and Wass 2019). Here, we use two different sets of four measures. We begin with Selway's (2011) four

measures: ethnolinguistic fractionalization, ethno-religious cross-cuttingness (i.e., how much ethnolinguistic and religious boundaries overlap), ethno-geographic cross-cuttingness, and ethno-income cross-cuttingness. All four measures range from 0 (complete homogeneity) to 1 (complete heterogeneity). The contributions of the Selway measures are a theoretical one. Ethnic diversity does not happen independently of other group identity cleavages—that is, religion, geography, and income. For example, the effects of high ethnolinguistic diversity may be moderated by religious homogeneity (Birnir and Satana 2020). Likewise, diversity in homogeneity at the local levels can be misconstrued as heterogeneity at the national level. The limitations of the Selway measures, however, are an empirical one. Selway draws from surveys—specifically, the weights they put on the demographic variables. While an ingenious approach, data are limited to countries that allow for and do such data collection. Using these variables drops almost half of the countries from the analysis.

To address this data concern (but wherein we forfeit the theoretical intersectionality), as a robustness check, we use four other measures. We begin with standard measures of ethnic fractionalization and religious fractionalization (Alesina et al. 2003). Next, we consider whether the country is federal. The intuition is that federal countries often devolve policy-making authorities to the subnational units—including language and education matters (data source: *Institutions and Elections Project*). Last, we consider the economic situation of the minority group. The *Minority at Risk* database has a measure for whether a minority group is economically advantaged or extremely disadvantaged across six indicators: income, land/property, higher education, presence in commerce, presence in professions, and presence in official positions. To help with interpretation, the measure is trichotomous where -1 indicates the minority is economically disadvantaged; 0, no economic disadvantage/advantage; and 1, economically advantaged.

Third, we consider the political regime. While all countries in our sample are non-democracies, there are variations in authoritarian type—suggesting different degrees of competition, constraints, and participation. Here, we use data from Wright (2008) to distinguish between monarchies, militaries, party states, personalist regimes, or some combination of the latter three. In total, there should be eight types. In our sample, there are no cases of either military-party dictatorships or military-party-personalist hybrids. The most common type is the party state. The reference category is party-personalist. As an alternative measure, instead of conceptualizing regimes as types, we use Polity, which ranges from -10 (mathematical minimum) to 5 (theoretical maximum). Per convention, Polity scores of 6 or higher are considered democracies (Liu 2017)—rendering their

Table 1. Effects of Minority Language Recognition on Political Trust in Authoritarian Regimes.

Confidence in government	Workhorse ^a	Alt sample ^a	Alt measure	Alt measure ^a	Alt measure ^a	Alt estimator ^b
	FH≥3	polity<6	recognition	Ethnicity	Authoritarian	Multilevel
	(1)	(2)	(3)	(4)	(5)	(6)
Minority language recognition	−0.70 (0.07) [‡]	−0.26 (0.03) [‡]		−0.91 (0.10) [‡]	−0.93 (0.08) [‡]	−0.10 (0.02) [‡]
Education only (Recognition=1) ^c			−0.37 (0.06) [‡]			
Regional (Recognition=2) ^c			−0.39 (0.07) [‡]			
National (Recognition=3) ^c			−0.39 (0.04) [‡]			
Female	0.13 (0.07)*	0.13 (0.08)	0.13 (0.07)*	0.12 (0.06) [†]	0.12 (0.07)	0.13 (0.07)*
Age	0.01 (0.00) [‡]	0.01 (0.00) [‡]	0.01 (0.00) [‡]	0.01 (0.00) [‡]	0.01 (0.00) [‡]	0.01 (0.00) [‡]
Education	0.02 (0.04)	0.03 (0.05)	0.02 (0.04)	0.01 (0.03)	0.02 (0.05)	0.02 (0.04)
Married	−0.04 (0.05)	−0.04 (0.05)	−0.04 (0.05)	−0.05 (0.03)*	−0.04 (0.05)	−0.04 (0.05)
WDI: Growth	0.32 (0.03) [‡]	0.11 (0.00) [‡]	0.00 (0.02)	−0.19 (0.01) [‡]	−0.25 (0.02) [‡]	−0.02 (0.02)
Selway: Ethnic fractionalization	−6.25 (0.85) [‡]	−1.83 (0.18) [‡]	−1.10 (0.19) [‡]		−8.03 (0.69) [‡]	−0.32 (0.13) [‡]
Ethnic-religious cross-cut	−5.50 (0.79) [‡]	−0.30 (0.10) [‡]	−1.39 (0.25) [‡]		5.61 (0.16) [‡]	−0.37 (0.12) [‡]
Ethnic-geographic cross-cut	−2.23 (0.64) [‡]	5.37 (0.43) [‡]	0.67 (0.37)*		−10.61 (1.17) [‡]	0.75 (0.49)
Ethnic-income cross-cut	−1.20 (0.48) [†]	−10.25 (0.21) [‡]	−1.78 (0.67) [‡]		−18.96 (0.34) [‡]	−2.77 (0.50) [‡]
Wright: Monarchy ^d	0.18 (0.11)*	−2.34 (0.30) [‡]	−1.57 (0.12) [‡]	3.00 (0.30) [‡]		−1.44 (0.14) [‡]
Military ^d	−2.53 (0.10) [‡]	−3.26 (0.38) [‡]	−2.22 (0.11) [‡]	−3.38 (0.19) [‡]		−1.98 (0.08) [‡]
Party ^d	−0.90 (0.05) [‡]	−1.65 (0.18) [‡]	−1.98 (0.14) [‡]	−0.11 (0.17)		−1.77 (0.11) [‡]
Personalist ^d	−1.63 (0.04) [‡]	−1.54 (0.12) [‡]	−2.11 (0.14) [‡]	−5.79 (0.32) [‡]		−1.74 (0.07) [‡]
Military-personalist ^d	1.97 (0.38) [‡]	0.00 (0.00)	−1.76 (0.13) [‡]	5.19 (0.62) [‡]		−1.96 (0.21) [‡]
Alesina et al.: Ethnic fraction				−9.69 (0.76) [‡]		
Religious fraction				−0.32 (0.10) [‡]		
laep: Federal				−2.96 (0.25) [‡]		
MAR: Minority advantaged				0.45 (0.05) [‡]		
Polity					0.13 (0.01) [‡]	
N	3287	2633	3287	4465	2649	3287

* $p \leq .10$, [†] $p \leq .05$, [‡] $p \leq .01$.

^aEstimation: ordered probit with country fixed effects and errors clustered by country.

^bEstimation: multilevel probit with random coefficient.

^cReference category—No Recognition.

^dReference category—GWF: *Party-Personalist*.

exclusion from the analysis. The average Polity score for the sample is -2 .

Political Trust in Authoritarian Regimes

To estimate the models, we use ordered probit with country fixed effects and standard errors clustered by country. We begin with a simple bivariate regression model to confirm our variable of interest is significant and in the correct direction ($\beta = -0.33$; $SE = 0.01$).⁴ From here, we expand our model to include other key explanatory variables. The results can be found in [Table 1](#). Model 1 is our baseline workhorse model. The authoritarian sample is defined with Freedom House (i.e.,

any country scoring “not free” or “partly free” in the surveyed year). The results are striking. First, consistent with our theoretical predictions, the recognition of a minority language has a significant, negative correlation with political trust ($\beta = -0.70$; $SE = 0.07$). Recall, recognition is a four-point scale variable. Substantively, shifting from only education recognition to education recognition *with* regional administrative recognition can mean going from “quite a lot of confidence” to “not very much confidence.” To assess whether these results are driven by one particular country, we conduct a series of outlier sensitivity analyses that drop each country in turn. The results do not change (see [Supplemental appendix—table D](#)).

This negative coefficient for recognition holds across all alternative model specifications, including the use of Polity's definition to identify the authoritarian sample (**model 2**); disaggregating recognition into a series of dummies (**model 3**); the use of alternative ethnicity measures in place of Selway's measures (**model 4**); the use of the continuous Polity measure in lieu of the Wright's categorical measure for political regimes (**model 5**); and the use of a multilevel probit estimator with random coefficients (**model 6**). In short, consistent with hypothesis 2, more recognition—or in fact, recognition at *any* level—has a blowback effect.

The magnitude of the effect does attenuate with the latter models, but its relation vis-à-vis the other variables does not. While the marginal effects of language recognition are much smaller than those of ethnicity (regardless of measure), they are consistently larger than any of the individual controls and economic growth. Moreover, they are comparable than those for political regime.

In general, ethnic diversity has a negative effect on political trust. This is the case even when we consider the conditional effects of religion, geography, and income on ethnic diversity. Put differently, when the cross-cutting cleavages are high—that is, ethnic minorities are spread across different religions, ethnic minorities can be found spatially everywhere (as opposed to just in one region), and no ethnic group is associated exclusively as rich (or poor)—we see the oft-purported links between diversity and low trust. In fact, political trust is higher when the ethnic minority is economically advantaged over the politically dominant. The one possible exception is when it comes to religion. It seems that religion *can* serve as an alternative focal point to attenuate the otherwise adverse effects of ethnolinguistic diversity.

Political regimes matter. Trust levels are generally the lowest when the regime is either a military (see model 2) or a personalist (see model 4)—although the effects are mixed if it is a military-personalist regime. But whether it is a civilian or military, party or individual, the results in model 5 demonstrate the larger finding. As executive constraints increase, executive recruitment widens, and participation costs decrease, confidence in the government goes up as well. A one-standard deviation shift in the *Polity* score can increase political trust by 0.03.

Parsing out the Causal Mechanism

We argue that dictators extend linguistic recognition in part to coopt the minority political elites; this symbolic concession calls attention to horizontal inequality—thereby depressing political trust. Unlike in democracies, the political landscape in authoritarian regimes is rarely free and fair. However, the limited political contestation is far from uniform. At one extreme, there

are parties that are allowed to mobilize and compete for legislative seats. At the other extreme, there is no pretense whatsoever of nominally democratic institutions. In these cases, authoritarian governments may recognize minority languages in lieu of yielding political space—that is, minority language recognition is merely a symbolic concession instead of something more substantive. In line with Liu, Gandhi, and Bell (2018), who do not find evidence of this substitution mechanism, we contend it is possible that linguistic recognition in the absence of a political voice is inadequate to shoring up political trust. In fact, as we saw in model 3, affording a minority language official recognition at the national level—with implications of it being used in public administration and schools—can still have a negative effect. This suggests the horizontal equality is not strictly about culture; instead, the political inequity matters as well.

To consider this possibility, we first rerun model 4 from Table 1. But this time, we include a measure for whether there are parties and legislative elections. When there are neither multiple parties nor legislative elections, the variable is assigned a value of 0. And conversely, when there are multiple parties and/or legislative elections, we code the variable as a 1 (data source: Gandhi 2008). The results in Table 2 (with coefficients for the other controls suppressed) highlight two patterns of interest. The first is that recognition still remains negatively signed. However, the coefficient is substantially and significantly smaller when there are multiple parties and/or legislative elections. All else being equal, a shift from no constraining institutions to constraining ones can increase government confidence by up to 25 percentage points.

To better assess the effects of these partisan, legislative institutions, we employ a two-prong approach. First, we split the sample into subsamples. The first subsample is restricted to cases where there are no multiparty legislative elections; and the second, where there are multiple parties and/or legislative elections per Gandhi (2008). If political equity is the source for the negative correlation, we should see a significant difference between the two coefficients for recognition—with that of the first subsample (model 2) much larger and much more negative than that of the second (model 3). The results corroborate. When minority groups have no political voice, government efforts to recognize their language can substantially undermine political trust ($\beta = -2.40$; $SE = 0.02$). Conversely, when there are multiple parties and/or legislative elections, minorities are much less likely to evaluate the government poorly ($\beta = -0.14$; $SE = 0.03$)—a 30-fold difference and a finding consistent with Liu, Gandhi, and Bell (2018).

Next, to get a better sense of the effects, we run the full sample with an interaction between minority

Table 2. Effects of Recognition Across Legislative Elections.

Confidence in government	Baseline ^a Full sample	Subsample 1 ^a Gandhi=0	Subsample 2 ^a Gandhi>0	Interaction Full sample
	(1)	(2)	(3)	(4)
Minority recognition	-0.87 (0.10) [‡]	-2.40 (0.02) [‡]	-0.14 (0.03) [‡]	-0.26 (0.06) [‡]
Legislative elections: Gandhi	0.48 (0.02) [‡]			0.44 (0.10) [‡]
Recognition * elections				0.17 (0.04) [‡]
Individual controls ^b	●	●	●	●
Country controls ^b	●	●	●	●
Country FE ^b	●	●	●	●
N	4465	762	3703	4465
Log Pseudolikelihood	-5550.84	-965.51	-4562.18	-5566.33

* $p \leq .10$, [†] $p \leq .05$, [‡] $p \leq .01$.

^aEstimation: ordered probit with country fixed effects and errors clustered by country.

^bCoefficients for control variables not reported.

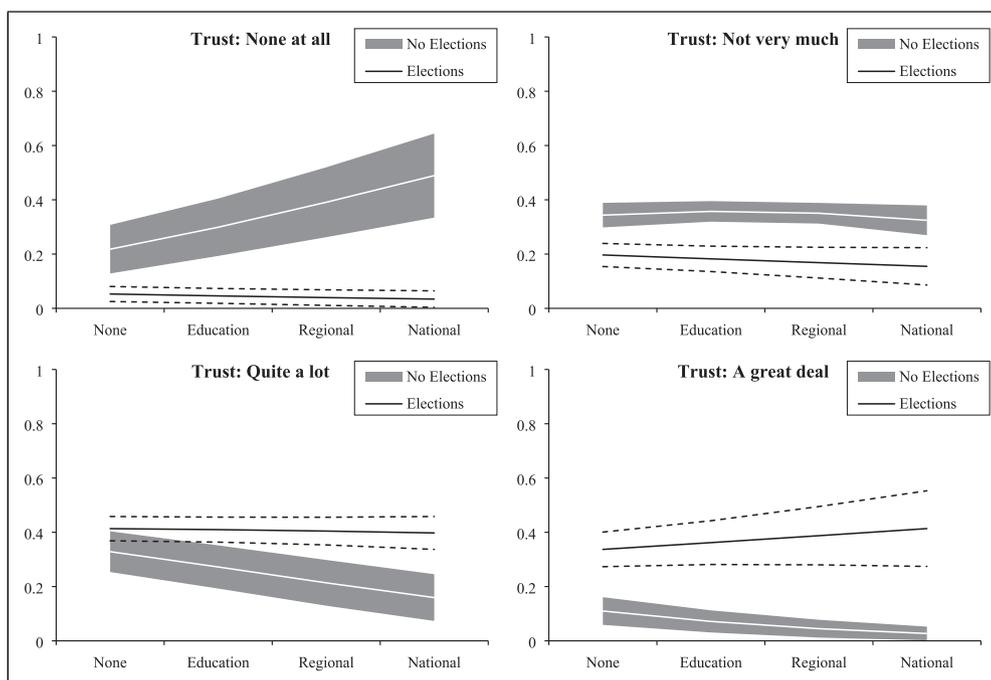


Figure 2. Conditional effects of legislative elections on political trust.

language recognition and legislative elections (model 4). We now see that minority language recognition in of itself remains statistically significant and negative; but what is of interest is its conditional effect on whether there are legislative elections. To make better sense of these results, we plot the predicted values in Figure 2. The results indicate that the level of recognition has a negative on political trust—whether it is “none at all” or “quite a lot”—but only when there are no elections.

Conversely, where are elections, it seems recognition has no bearing on political trust.

Possible Endogeneity: Recognition as a Strategic Response to Low Trust

Our results thus far suggest a strong statistical link between minority language recognition and political trust. But as is,

Table 3. Effects of Minority Language Recognition on Political Trust. (Sample: Imazighen in Morocco).

Confidence in government	Minority recognition: Ordered ^a	Minority recognition: Categorical
	(1)	(2)
Minority language recognition	-0.16 (0.07) [†]	
Education only (recognition = 1) ^b		-0.16 (0.07) [‡]
Regional (recognition = 2) ^c		0.26 (0.08) [‡]
Individual controls ^c	●	●
N	261	261
Log Pseudolikelihood	-344.50	-344.50

* $p \leq .10$, [†] $p \leq .05$, [‡] $p \leq .01$.

^aEstimation: ordered probit with country fixed effects and errors clustered by country.

^bReference category—No Recognition.

^cCoefficients for control variables not reported.

we cannot explain the causal story with our cross-sectional, observational data. As such, it is possible that governments are extending linguistic recognition in response to low trust levels in the first place. In an ideal scenario, we would want to compare political trust before and after the onset of minority language recognition. However, many of the minority languages in our sample had their recognition status codified awhile back—for example, Russian was codified as an official language in the 1996 Belarussian constitution and reaffirmed in 2002 as a language of instruction (Liu, Sokhey, and Roosevelt 2017). Likewise, Singapore recognized Malay as early as 1959 (Liu 2015). The earliness of recognition in these cases often predates available survey data—thereby making a systematic testing of before-and-after across multiple countries difficult.

We can, however, glean *some* insight about the link between minority language recognition and political trust in one particular case. Morocco is the one country where we have multiple survey years that match up against before and after recognition. The country appears in the fourth (1999–2004), fifth (2005–2009), and sixth (2010–2014) waves. Fourth wave surveys were administered July 15 to August 25, 2001. This precise period is important as it predates Dahir #1-01-299—that is, the establishment of the Royal Institute of Amazigh Culture (October 17, 2001). Before Dahir #1-01-299, the use of Amazigh had been largely proscribed; it was even illegal for Imazighens to give children Amazigh names. This means there is no minority language recognition in the fourth wave—but there was education recognition by the fifth wave (2007). Interestingly, the sixth wave surveys were administered at the exact same time as the constitutional reforms (May 25–June 18, 2011)—which saw the recognition of Amazigh as an official language.

We rerun the workhorse model from before. This time, however, we focus on how recognition of the Amazigh

language has affected political trust over three survey waves. We can think of the fourth wave—sans recognition—as the control group. And with each subsequent wave, we have an additional treatment. As with previous models, we focus on the largest minority—for example, the Imazighen respondents ($N=310$ before control variables). We include the same battery of individual controls (gender, age, educational attainment, and marital status). We also consider the respondent's race (whether they identify as white), religion (whether they identify as Muslim), social class (whether they perceived themselves as upper, working, or lower class), and town size (as a proxy for region).

The results in Table 3 (model 1) suggest Amazigh recognition seems to be negatively linked to political trust ($\beta = -0.16$; $SE = 0.07$). This is consistent with some of the on-the-ground developments. Although the Royal Institute of Amazigh Culture adapted the Tifinagh alphabet to help the learning of the Amazigh languages, the teaching of the language in schools has been slow. Language classes are optional; school administrators do not encourage the teaching of it (Maddy-Weitzman 2012). There are, however, some surprising findings. In model 2, when we split minority recognition into its constituent categories, we see that recognizing the Amazigh as an official language improved political confidence ($\beta = 0.26$; $SE = 0.08$). Two comments merit attention. The first is that given how soon the sixth wave was administered during the constitutional reform, it is possible that respondents were noting their political trust based on their future beliefs of language recognition in a honeymoon period.

Second, it is crucial to recognize that this recognition—even if it is more than just symbolic—exists to support further Arabization (Maddy-Weitzman 2017). Additionally, without proper resources from the government to support the teaching and learning of the language across the country and in different sectors beyond education, the

horizontal inequality between Amazigh and Arabic remains pronounced (Akinou 2021). While an illustrative case—one country with a small number of survey respondents—the example of the Imazighen in Morocco offers some insight into the relationship between minority language recognition and political trust.

Conclusion

In this paper we examine whether minorities are more likely to trust the government when their language is recognized. While the relationship is positive in democracies—that is, substantive representation matters—we are dubious the same mechanism applies in authoritarian regimes. Instead, we argue minority language recognition—often mere window-dressing—highlights the horizontal inequality between the hegemon and minority groups. This inequality is not just about culture; it permeates into the political, social, and economic dimensions. The empirical evidence corroborates this claim. While we cannot fully distinguish the causal flow between recognition and political trust, our results are suggestive that people do evaluate their governments from a cultural standpoint in authoritarian regimes.

Admittedly, sample biases are a major concern. By focusing on authoritarian regimes, we face at least two types of biases. The first is that surveys are not administered in every country. The missing ones—from North Korea to Saudi Arabia—are not missing at random. Certain political regimes are more likely to ban such data collection efforts. The second bias type is the social desirability bias. It is possible that respondents are overstating their confidence in the government because they are afraid of the repercussions. Unfortunately, we cannot get around these two types of biases in a large, systematic way. But we contend that they bias against our results. For instance, if we had more countries where there are no minority recognition, no political contestation, and where political trust is high (because of social desirability bias), this would strengthen our results. Likewise, if respondents were overstating their trust, we should observe even lower reported levels in the cases where the regime recognized minority languages.

While these results are suggestive, they also provide the impetus for future research. One possible avenue of future research is to expand the analysis to include other minority groups. Currently, we focus only on the most populous minority groups. There are no reasons to believe recognition of *any* minority language has the same effect on *all* minority groups. In fact, it is possible that if tensions were high between two minority groups, one group would prefer no recognition to another group getting it. A second avenue of research is to consider the effects on generalized trust. The focus in this paper has been on political trust. But if

governments are recognizing minority languages in places where members of the minority and hegemon groups interact—for example, in schools—this can also affect inter-group interactions.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Algeria, Armenia, Azerbaijan, Belarus, China*, Colombia, Ecuador*, Egypt, Iraq, Jordan, Kazakhstan, Kuwait, Kyrgyzstan, Lebanon, Libya, Malaysia, Mexico, Morocco, Nigeria, Pakistan, Philippines*, Qatar, Russia, Rwanda, Singapore, Tunisia*, Turkey, Ukraine, Uzbekistan, Yemen, and Zimbabwe. Note: Asterisks denote countries that do not ask respondents about language spoken at home. Since we cannot identify a "minority sample," we drop these countries from the analysis.
2. Algeria, Armenia, Azerbaijan, Belarus, China*, Ecuador*, Egypt, Iraq, Jordan, Kazakhstan, Kuwait, Kyrgyzstan, Libya, Morocco, Nigeria, Qatar, Russia, Rwanda, Singapore, Uzbekistan, Yemen, and Zimbabwe. Note: Asterisks denote

countries that do not ask respondents about language spoken at home. Since we cannot identify a “minority sample,” we drop these countries from the analysis.

3. There is the risk that minority recognition also affects economic growth—thereby biasing our regression coefficients. To address this concern, we rerun the models with economic growth removed. The results remain substantively unchanged.
4. The results are substantively no different if we estimate with an OLS regression ($\beta = -0.31$; $SE = 0.00$). See table C in [Supplemental appendix](#) for full OLS regression models.

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