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STATE-BUILDING IN A DIVERSE SOCIETY

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ABSTRACT

Diversity can pose fundamental challenges to state-building and development. The Tanzanian Ujamaa policy—one of post-colonial Africa’s largest state-building experiments—addressed these challenges by resettling a diverse population in planned villages, where children received political education. We combine differences in exposure to Ujamaa across space and age to identify long-term impacts of the policy. Analysis of contemporary surveys shows persistent, positive effects on national identity and state legitimacy. Exposed cohorts are also more likely to marry across ethnic lines. Our preferred interpretation, supported by evidence that considers alternative hypotheses, is that changes to educational content drive our results. Our findings also point to trade-offs associated with state-building: while the policy contributed to establishing the new state as a legitimate central authority, simultaneously it lowered demands for democratic accountability.

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An online appendix is available at <http://www.nber.org/data-appendix/w30731>

1. Introduction

Diversity can pose fundamental challenges to state-building and development, including the under-provision of public goods due to heterogeneous preferences (Alesina et al., 1999), inter-group conflict driven by inequality and competition for resources (Ray and Esteban, 2017), and difficulties with communication that hinder the establishment of social trust (Wimmer, 2018). In more diverse polities, citizens may not feel that public policies represent their preferences sufficiently (Alesina and Spolaore, 1997), posing a challenge to state legitimacy (Holsti, 1996). Such a lack of perceived state legitimacy is in turn associated with weak state capacity, poor economic performance, and even violence (Besley and Persson, 2011).

To overcome these challenges, political leaders throughout history have worked to build new *states* through *nation*-building endeavors that include the promotion of common values, identity, and language (often through public education) and population resettlement (Tilly and Ardant, 1975; Smith, 1986; Hobsbawm, 1992).¹ However, the implementation of such measures requires state capacity and reach, which may be hindered by diversity in the first place (Miguel and Gugerty, 2005). In addition, political leaders may encounter resistance from existing elites and embedded group-specific social norms. Leaders from early modern Europe to the post-colonial era have therefore relied on bundles of “homogenizing, territorializing, and mobilizing” activities (Smith, 1986) to address multiple challenges simultaneously.² It is an empirical question what the long-run results of these activities are.

This question is particularly difficult to answer since nation-building activities typically unfold over long time spans and affect entire populations simultaneously. While scholars have identified causal effects of specific elements of nation-building policies, states generally implement such policies as bundles of activities. Moreover, interactions between different components of a given bundle may lead to combined effects distinct from the sum of their parts. This paper aims to identify such combined effects.

We study a setting that provides sharp variation in exposure to a bundled state-building effort. Specifically, we investigate the long-run effects of one of post-colonial Africa’s most ambitious state-building experiments — the Tanzanian *Ujamaa*³ policy — on the development of national identity in a highly diverse society. We also examine whether, in building the Tanzanian *nation*, *Ujamaa* strengthened the nascent Tanzanian *state*, looking at how *Ujamaa*

¹ We understand state-building as the construction of a state apparatus that can establish a monopoly on the legitimate use of violence, to protect property rights, collect taxes, and provide public goods in a given territory (Tilly and Ardant, 1975). Nation-building captures the formation of a national identity with which citizens feel a sufficient sense of emotional commonality that they wish to remain together. Nation-building may facilitate state-building (Alesina et al., 2021).

² Examples include efforts in the 19th-century to turn “peasants into Frenchmen” (Weber, 1976) and “make Italians” following that country’s unification (Duggan, 2008), as well as the early 20th-century construction of national identity in China (Wimmer, 2018) and the Soviet Union (Martin, 2001).

³ *Ujamaa* roughly translates as “familyhood” (Sitari, 1983).

affected citizens' attitudes towards and engagement with the state and state institutions in the long run. We digitize historical administrative data and combine it with contemporary survey data to operationalize our empirical strategy.

While the political economy of state building in diverse societies is globally relevant, it is particularly pertinent in post-colonial Africa. Newly independent African states inherited artificial borders that contained a multitude of ethnic groups with little history of centralized governance or strong shared identity (Alesina et al., 2011).⁴ The salience of ethnic differences had been “reinforced [and] exaggerated” (Mamdani, 2003, p. 139) to serve the interests of the colonizers. As a result, post-independence leaders in Africa faced a number of challenges to state building: fragmented populations scattered over inhospitable territories beyond the state's reach (Herbst, 2014), a mix of potentially rival ethnic groups within arbitrary borders (Michalopoulos and Papaioannou, 2016), and no workable social contract between the state and population (Scott, 1998). These challenges made it difficult for the new states to build capacity and develop common values around which to unite their diverse populations.

The *Ujamaa* policy, implemented from 1970 to 1981, addressed these challenges simultaneously by bringing the Tanzanian population into the state's reach and forging a national identity through the public education system. The key tool for expanding the state's reach was the so-called ‘villagization’ program, which resettled much of the multi-ethnic rural population in planned villages under state administration. Villagization was remarkably successful, with over 80 percent of the country's rural population living in registered villages within just a few years. As Scott (1998) explains, “the purpose of forced settlement is always disorientation and then reorientation” (p. 235). In the Tanzanian case, “reorientation” was facilitated by abolishing traditional authorities and replacing them with state bureaucrats and democratically elected village governments (Sitari, 1983).⁵ Critically, *Ujamaa* also put education in the planned villages into the hands of the new state, which concurrently revamped the curriculum to reflect its political goals. The *Ujamaa* policy thereby allowed the state “to capture the peasantry” both geographically and ideologically (Hydén, 1980).

Exposure to *Ujamaa* varied both across space and across age cohorts. Villagization intensity varied across districts as implementation was largely left to local officials, who enforced resettlement with varying degrees of conviction. Sharp and plausibly exogenous variation existed across cohorts given the policy's inclusion of a targeted education reform. Only those young enough to enter school after the policy's introduction were exposed to the entire state-building bundle, including the revamped curricula in the planned villages.

We are thus able to study impacts of *Ujamaa* using a difference-in-differences cohort

⁴ Afrobarometer data, from rounds 3 and 4, reveals that over 50% of the African population identifies more with their ethnic group than with their nation.

⁵ Other elements of the *Ujamaa* policy included the abolition of individual titles to property, collectivization of agricultural production, and the nationalization of certain enterprises.

design in the spirit of Duflo (2001). Our empirical specification interacts time-invariant local villagization intensity with birth cohort indicators. We neither assume that levels of villagization intensity varied randomly across space, nor that there were no underlying differences between different cohorts in the absence of villagization. We control for systematic differences between districts and cohorts with district and region-specific cohort fixed effects. Only the interaction between the two sources of variation constitutes exogenous variation in citizens' exposure to the *Ujamaa* policy under a parallel trends assumption. Our data supports this assumption. The sharp timing and short-lived nature of the policy allow us to compare differences in outcomes for cohorts that were of primary school age during the policy with cohorts that were slightly too young or too old to be affected by the bundle of activities, for more and less intensely villagized areas.⁶

We find large, significant, and persistent positive effects of *Ujamaa* on different measures of national identity. In surveys conducted around two decades after the policy ended, a one standard deviation (SD) increase in villagization leads members of the primary school-age cohort to identify 0.23 SD more strongly with the Tanzanian nation rather than with their ethnic group, compared to the control cohort.⁷ Further, men in treated cohorts in districts villagized by 1 SD more are 6.1 percentage-points more likely to marry women from ethnic groups different from their own, suggesting a decreased salience of ethnic divisions. The latter revealed-preference evidence provides support for the idea that *Ujamaa* had real impacts on citizens' behavior and national identity in the long term.

The results withstand a battery of robustness checks. We find no differences in the trends across cohorts of primary school age before and after the policy period for more and less intensively villagized districts, lending support to the parallel trends assumption. In addition, our results are robust to controlling for pre-policy district characteristics interacted with cohort fixed effects. We also conduct a bounding exercise which shows that selective migration is unlikely to explain the majority of the estimated effects. Finally, we control for respondents' beliefs about surveyors to reduce concerns related to social desirability bias.

We interpret our main coefficient as the differential effect of the bundled treatment compared to the effect of villagization alone, i.e. holding constant the average effect of villagization in the overall population.⁸ While villagization alone may in theory have affected national identity across all cohorts, only those of primary schooling age during *Ujamaa* were

⁶ Our empirical strategy exploits differences across districts in villagization intensity at the height of the *Ujamaa* period but not differences across districts in the timing of implementation, as this is not clearly documented. The variation in timing stems from variation in respondents' birth cohorts.

⁷ The difference between national identity of the treated and the control cohort, for a district with complete villagization compared to a district without any villagization, corresponds to the difference between a respondent "feeling more Tanzanian" and "feeling more a member of her ethnic group [than Tanzanian]."

⁸ The effect of villagization alone appears to be small. Average levels of national identity are no higher in more intensively villagized districts for cohorts outside of primary schooling age during the *Ujamaa* policy.

exposed to both villagization and the education reform. The fixed effects in our specification control for the variation in the intensity of villagization across space as well as overall differences across cohorts. Our results thus imply that villagization served to make the education reform a more effective means of bolstering national identity. We understand this as resulting from the disruption of existing norms and hierarchies that villagization engendered, including resettlement and the abolishment of traditional village leaders in favor of bureaucrats (Feierman, 1990). This speaks to the bundled nature of the policy, highlighting the importance of possibly interacting activities. Understanding how the pieces of the bundle in isolation affected national identity is not the focus of this paper.

Nevertheless, we examine potential channels driving our results. Our preferred explanation is that *Ujamaa* primarily influenced national identity through political education provided in planned villages. However, we explore evidence for other cohort-specific factors that may have interacted with villagization. We find empirical support for our preferred explanation but little evidence for alternatives. In particular, we find that exposure to *Ujamaa* increased national identity only among those who attended primary school when it was in effect. We also find evidence supporting the impact of changes to the public school curricula, rather than as a result of other factors that coincide with attending primary school, such as intergroup contact or increased human capital. Moreover, we find a large effect of *Ujamaa* on the cohort that was of primary school age during the policy period, but little effect on the cohorts that went to school after the policy ended. This suggests that changes to the content of education rather than a general increase in its supply explain our results.

Having established that *Ujamaa* helped bolster national identity, we next examine whether the policy also served to foster state legitimacy in the long run. This is an important question given the ambitions of state-builders throughout history to use nation-building tools to overcome divisions and establish authority. Moreover, the policy's effect is ex-ante unclear. It could either enhance state legitimacy by increasing the population's identification with the nation and the state, or provoke backlash, undermining state legitimacy.

We find that cohorts exposed to *Ujamaa* are more likely to respect state authority and approve of one-party rule, and to have higher trust in government institutions such as state media. Interestingly, we find little effect on generalized inter-ethnic trust.⁹ These findings suggest that *Ujamaa* primarily contributed to the new Tanzanian state's ability to consolidate power. However, it also reduced demands for democratic accountability.

Tanzania's experience with state-building offers relevant insights for a wide range of

⁹ This result may at first seem at odds with our findings for ethnic intermarriage. However, we should not necessarily understand intermarriage as an expression of inter-ethnic, or generalized trust. Rather, intermarriage reflects beliefs one has about a particular individual they marry from a certain group, versus beliefs about a random member from that group. Thus, we should understand intermarriage as an expression of *personalized* trust facilitated by repeated bilateral interactions. Generalized and personalized trust need not be correlated (Guiso et al., 2009).

countries which face cleavages based on history, geography, and culture (see e.g., [Alesina and La Ferrara, 2005](#); [Ray and Esteban, 2017](#)). Difficult starting conditions may contribute to why these countries – many of which have a history of colonialism – struggle to sustain strong states. Our study finds that bundled nation-building policies can help overcome such challenging initial conditions.

We build on a rich theoretical and historical literature on the processes of nation- and state-building (e.g., [Tilly and Ardant, 1975](#); [Smith, 1986](#); [Anderson, 1991](#)). A more recent economics literature has provided empirical evidence in support of these ideas and theories – primarily identifying causal effects of specific aspects of broader nation-building policies ([Rohner and Zhuravskaya, 2023](#)). For example, [Bazzi et al. \(2019\)](#) study a population resettlement program in Indonesia to identify effects of intergroup contact on national integration.¹⁰ Economists studying state-building have also turned their attention to Africa. Here, our work is related to [Depetris-Chauvin et al. \(2020\)](#), who investigate the short-term impact of national football teams’ victories on national identity in Sub-Saharan Africa. [Blouin and Mukand \(2019\)](#) examine how propaganda broadcast over radio helped to change inter-ethnic attitudes in post-genocide Rwanda.¹¹ [Okunogbe \(2024\)](#) studies inter-group contact and national integration in the context of national service in Nigeria.

Our contribution to the literature on state-building is twofold. First, we study a bundle of measures aimed at bolstering national identity and state legitimacy. This is important because policies in practice typically manifest as bundles; as noted above, the effects of specific measures may be very different when studied in isolation compared to when combined with other measures. For example, when enacted in isolation, policies mandating the forced coexistence of diverse ethnic groups have led to backlash and even spurred inter-group conflict ([Dippel, 2014](#)).¹² The lack of backlash to *Ujamaa* speaks to the bundled nature of the policy, highlighting the importance of interacting activities. Second, we study attitudes towards the state in addition to national identity, finding that effective state-building may come at the cost of making citizens comply with or even prefer non-democratic governance. These findings provide new insights for the emerging literature on nation-building, which has not extensively examined potential trade-offs vis-à-vis acquiescence to authoritarianism.

This paper also speaks to scholarship on the role of education in state-building, which

¹⁰ As in newly independent Tanzania, the Indonesian government was concurrently engaged in a number of other activities oriented toward nation-building. These interventions included resettlement, school construction, a nationalization of state media, confronting religious schools and other measures.

¹¹ This was part of a bundled policy attempt to reconcile inter-ethnic relations, which included setting up a National Unity and Reconciliation Commission; national radio; revising the constitution to criminalize divisionism; memorial sites to promote collective memory; new national symbols, administrative restructuring and renaming places; rewriting history textbooks; and introduction of social programs.

¹² Two notable examples in Africa are post-independence governments of Mozambique and Ethiopia, which both ‘villagized’ 2 and 12 million people respectively ([Lorgen, 2000](#)). In both cases, forced coexistence eventually led to the downfall of the instigated political parties and intense conflict over long periods.

philosophers and politicians have recognized dating back to Plato’s *Republic*. Economists have also begun studying how education can contribute to national identity and ideology (Cantoni and Yuchtman, 2013) – in a wide range of settings and with mixed outcomes (Cantoni et al., 2017; Clots-Figueras and Masella, 2013; Cinnirella and Schueler, 2018; Bandiera et al., 2019; Fouka, 2020; Blanc and Kubo, 2021; Bazzi et al., 2023). Much of this work is grounded in the ideas put forth by psychologists and sociologists that adolescence comprises particularly “impressionable years,” which serve as a formative phase for establishing cultural orientations (Krosnick and Alwin, 1989) and identity (Erikson, 1968).

We contribute to the literature on education and state-building in several ways. First, this literature has typically – and by design – isolated changes to education from various other institutional changes that go along with state-building efforts. In contrast, we study an education reform in conjunction with other institutional changes that disrupted existing social structures and hierarchies. Second, we study the effect of education on nation-building in a particularly pertinent and understudied context. Much of the existing work in this area has studied education in contexts where the government could tap into an existing shared history or culture (Hobsbawm, 1992; Gellner, 2006). Post-colonial Africa in contrast was characterized by high diversity and little “sharedness” due to artificial borders and the decolonization process, making it an unlikely place for such an approach to nation-building.

Our paper bridges two well-established literatures in the social sciences that have previously been largely separate – the economics literature on nation-building and education (Alesina et al., 2021) and the anthropological literature on state-building (Scott, 1998).¹³

We also contribute to literature that traces the impact of pre- and post-colonial institutions on African development. Much of this work examines the negative effects of artificial colonial borders (Easterly and Levine, 1997; Alesina et al., 2011; Michalopoulos and Papaioannou, 2016), particularly the resulting ethno-linguistic fractionalization. Less attention has been given to national institutions that aim at overcoming the problems resulting from colonization (Michalopoulos and Papaioannou, 2013; Burgess et al., 2015). In this context, our paper is one of the first to empirically examine the state-building consequences of *Ujamaa*.

Scholars have predominantly examined *Ujamaa*’s negative economic impacts (Hydén, 1980; Putterman, 1986; Collier et al., 1986; Scott, 1998), with limited causal inference efforts. A notable exception is Osafo-Kwaako’s (2012) PhD thesis, which provides an important point of departure for the present study.¹⁴ The thesis examines villagization as an exemplar of

¹³ Other contemporary work on nation-building examines how religious pilgrimage (Clingsmith et al., 2009); war, occupation and repression (Dehdari and Gehring, 2022; Abramenko et al., 2024); foreign enemies (Dell and Querubin, 2018; Gehring, 2022); national leaders (Assouad, 2021); political boundaries (Bluhm et al., 2021); and representation and redistributive institutions (Giuliano et al., 2023) shape national identity.

¹⁴ Another study employing a causal inference framework to examine *Ujamaa* is the PhD thesis by Jarotschkin (2018). It utilizes a cross-sectional instrumental variables strategy to investigate the policy’s effects on economic development and inter-ethnic trust.

state-led development planning, focusing on the impacts of living in previously villagized areas on various social and economic outcomes: it identifies positive effects on schooling and public goods provision, alongside negative effects on consumption and perceptions of corruption. In contrast to this paper, the thesis finds a positive impact on ethnic (vs. national) identity and a negative impact on support for one-party rule. These contrasting findings are a consequence of the distinct research questions asked by the two studies, which lead to different cross-cohort comparisons and treatment construction: we focus on the effects of *Ujamaa* on state-building and compare birth cohorts eligible for political education during *Ujamaa* with older cohorts. We thus study the effects of the *bundle* of villagization interacted with political education. [Osafa-Kwaako \(2012\)](#) studies *Ujamaa* as an exemplar of state-led development. He compares cohorts exposed to villagization (but not to political education) or cohorts exposed to political education (but not under villagization) with older cohorts, but not the cohorts exposed to the bundle. This approach captures *Ujamaa*'s legacy and necessarily excludes this paper's treated cohorts (those of the age to receive political education during *Ujamaa*). Given our distinct focus on state-building, we also examine different outcomes, including inter-ethnic marriage, respect of state authority, democratic accountability, and trust in the media. Lastly, [Miguel \(2004\)](#)'s influential article, which examines how nation-building in Tanzania and neighboring Kenya has affected inter-ethnic cooperation (or the lack thereof) and public goods provision, is also closely related to our work. We complement his work by adding evidence on changed attitudes towards the state and democratic preferences.¹⁵

This paper proceeds in seven sections. Section 2 describes the *Ujamaa* policy and contextual background. Section 3 presents the data we use to measure exposure to the *Ujamaa* policy and its consequences. Section 4 outlines our empirical strategy to address challenges to identification. Section 5 presents our main results on national identity, conducts several robustness checks on our core findings, and considers alternative channels. Section 6 discusses implications of our findings on views of the state and the legitimacy of state authority. Finally, Section 7 offers concluding thoughts.

2. Background

Ujamaa comprised a series of reforms in post-independence Tanzania. These included the institutionalization of one-party rule and efforts to promote equality across all spheres of society, such as the nationalization of banks and large industrial enterprises and state price

¹⁵ [Miguel \(2004\)](#) makes a cross-sectional comparison of two nearby districts in Tanzania and Kenya which are separated by a national border but are similar in terms of local ethnic diversity. The study finds a negative relationship between ethnic diversity and public goods in Kenya, but a positive relationship in Tanzania. This is explained by Tanzania having implemented effective nation building-policies to ameliorate ethnic divisions, whereas Kenya did not. Focusing on Tanzania, our study provides direct evidence supporting this explanation – further looking at *all* districts on the mainland.

control (Ergas, 1980). Most important for this study – and arguably the most wide-reaching reforms – were the resettling of the rural population into planned villages and harnessing public education as a nation-building tool. These activities aimed at organizing the country around the new state (Nyerere, 1969a,b). The villagization program – implemented primarily between 1970-1981 – mandated the rural population to live in state-administered villages (Scott, 1998). Moreover, in those villages, primary school-age children were exposed to a new political education curriculum centered around building a national identity and establishing the Tanzanian state as a legitimate authority (Nyerere, 1982). We now discuss key elements of the policy. Appendix Table A.1 summarizes the timing of key events.

When Tanzania gained independence from Britain in 1961, Prime Minister Julius K. Nyerere¹⁶ encountered a challenge common to many new leaders of post-colonial states: how to organize diverse people, spread across a large territory, around a common mode of governance. Nyerere’s challenge was particularly acute: the country’s population comprised over 120 ethnic groups with different languages or dialects (Omari, 1995), making Tanzania one of the world’s most diverse countries. Furthermore, much of the country was sparsely populated. As of the 1967 Census, 12 million inhabitants were spread over nearly one million square kilometers, with nearly 95 percent of the population residing in rural areas.

Nyerere developed a multi-faceted agenda to unite his country’s diverse and geographically dispersed population, embarking on what Scott (1998) describes as one of the most ambitious nation-building programs in post-colonial Africa. In 1963, Nyerere consolidated political power and declared the governing Tanganyika African National Union (TANU) as the sole legal party, a measure that was understood to reduce societal divisions (Tripp, 1999). Nyerere subsequently expounded his ideas for the Tanzanian state in his 1967 landmark speech, the *Arusha Declaration*. The speech presented his vision of *Ujamaa*, which included the central role of villages for state building. The speech also outlined various measures aimed at reforming the economy in line with socialist principles, such as economic self-reliance, nationalizing commercial farms and businesses, and creating co-operatives rather than individual family farms to avoid class formation.

At the time, the majority of the Tanzanian population resided in scattered hamlets in the rural countryside. The *villagization* initiative was proposed to resettle the rural majority into planned villages. The new *Ujamaa* villages were also intended to promote communal farming with modern agricultural methods, which would generate surplus income to finance social infrastructure. Various inducements, such as the provision of social infrastructure (schools, clean water and dispensaries), were used to promote movement to the planned villages.

At the end of 1969, the government began mandatory villagization, though no guidelines for implementation were provided apart from declaring 1976 as the target year for full rural

¹⁶ On 9 December 1962, a republican constitution was implemented with Nyerere as the country’s first president.

villagization.¹⁷ Implementation was largely left to district officials, who pursued the endeavor with different degrees of conviction (McHenry, 1979) through a series of “Operations.” In 1975, the *Village and Ujamaa Village Act* was promulgated, mandating registration of villages with 250 or more households as legal entities (Bryceson, 1982). The Act also ushered in various governance reforms at village level, including the establishment of new decision-making bodies. As a result, “The traditional extended family or kinship group with collective responsibility for its members was replaced... by self-governing *ujamaa* village divided into ten-house cells, and the traditional chiefs and elders by democratically elected village administration” (Sitari, 1983, p. 2). The Act thus served to further one of Nyerere’s key aims since taking power after independence – namely, removing traditional authorities from power (Tripp, 1999). The 1975 Act also abolished existing primary cooperative societies, officially replacing them with the village governments (Hydén, 1980).

As a resettlement initiative, the villagization program was quite successful, with over 80 percent of the country’s rural population living in registered villages by the end of 1976 (McHenry, 1979). However, it is important to note that the average distance people moved was rather small – usually within eight kilometers (Sitari, 1983). Appendix Figure A.1 depicts the rapid growth of the population in registered villages over the period after independence. As Scott (1998) explains, the speed with which villagization was carried out was intended to “rip the peasantry from their traditional surroundings and networks” (p. 235). Remarkably, in most cases this resettlement was not met with violent resistance (Ergas, 1980).

Ujamaa implementation varied across districts in terms of scope and intensity due to largely idiosyncratic factors. This reflects the program’s being implemented in a decentralized manner. Regional party secretaries, the chief representatives of the President in the regions, often led the efforts personally (Hydén, 1980). Other drivers of villagization intensity included the provision of drought or flood relief (Lorgen, 2000; Hydén, 1980). Scholars have also noted that certain regions saw very little impact of *Ujamaa* given that farmers were already living in populous villages and that cash crops – which provided vital state revenue – were produced in large quantities in these areas (Scott, 1998; Ergas, 1980; Sitari, 1983). We discuss potential correlates with villagization further in Section 3 (Data) and Section 5 (Results).

Concurrent with villagization, the state centralized and consolidated the education sector. It was clear to Nyerere that to create a strong national identity, the education system had to be part of the process (Miguel, 2004). As the Minister of Education Solomon Eliufoo put it in 1968, education was “a cementing matrix of *Ujamaa* and progress [to establish national unity]” (Peeples, 2018, p. 46).

The *Ujamaa* education reforms were outlined in Nyerere’s second key declaration of 1967, the *Education for Self-Reliance* (ESR) paper. It consisted of two key elements: bringing

¹⁷ Presidential Circular No. 1 (Mung’ong’o 1995:80-1 in Kikula, 1997).

the education sector into the hands of the state and changing the school curriculum to help foster nation-building. To execute the ESR, the government nationalized all schools with the Education Act of 1969. While only 7% of the population had completed some public education by 1967, data from the 1978 census showed almost 90% enrollment ([Government of Tanzania, 1984](#)).¹⁸ The revised curriculum was likely more effectively implemented in the villagized areas and for the *Ujamaa* period school cohorts.

The Ministerial Circular of 1968 introduced Political Education (known in Swahili as *Elimu ya Siasa*) as a new subject to the primary school curriculum for students enrolled in Standards IV-VII (equivalent to 4th grade onwards). The introduction of Political Education meant new syllabi and new textbooks, and also impacted teaching in other social studies (history, geography, and civics). However, the directive to overhaul the curriculum was not necessarily accompanied by additional resources. As [Komba \(1996, pg. 108\)](#) highlights: “teachers were advised to use party documents, namely the Arusha Declaration, *Ujamaa Vijijini* (meaning, rural socialism), and Education for Self Reliance.”

Political Education served primarily as a tool for ideological indoctrination, emphasizing the importance of the nation and the state over tribal identities ([Komba, 1996](#)). In addition, Education Circular No. 2 of 1967 mandated Swahili as the national language of instruction in all public schools by November 1969. Analysis of educational materials during the *Ujamaa* period reveals explicit emphasis on indoctrinating students on national identity centered around the state. As [Komba \(1996, p. 111-112\)](#) notes, “the dominant theme was, obviously, nationalism” and “the general tendency was in the direction of political indoctrination rather than genuine Political Education”. Appendix Figure [A.2](#) enumerates the contents of a typical Political Education textbook, highlighting the themes that speak to nation- and state-building. Appendix Figure [A.3](#) depicts excerpts from a Swahili textbook of the period, illustrating how nationalist themes were found in all subjects.¹⁹

Economic failures associated with the villagization campaign and attempts to organize the country’s economy along socialist lines ([Hydén, 1980](#); [Collier, 1988](#)) ultimately led to the end of *Ujamaa*. This was codified with the repeal of the *Villages and Ujamaa Villages Act* in 1982. However, the end of *Ujamaa* was arguably hastened by key external factors. These included the surprise invasion by Uganda under Idi Amin in 1978 ([Roberts, 2014](#)) and the introduction of an IMF economic liberalization program ([Kaiser, 1996](#)). Tanzania

¹⁸ Data on historical school construction is unavailable.

¹⁹ The new political education curriculum was delivered alongside efforts to spread government propaganda through the media. Indeed, Nyerere’s 1968 *Ujamaa* manifesto listed the news media as a key means of production and exchange to facilitate his vision ([Sturmer, 1998](#)). In 1965, the Tanganyika Broadcasting Corporation was renamed Radio Tanzania Dar es Salaam (RTD) and inaugurated as a department of the Ministry of Information, Broadcasting and Tourism. In 1970, Nyerere nationalized the country’s most influential newspapers, *The Standard* and its sister *Sunday News*, which up until then had been under foreign ownership. That said, the media channel had relatively limited reach compared to the new school curriculum, not the least due to unavailability of newspapers in most of the countryside.

was a victim of the global debt crisis, which began when the Federal Reserve increased U.S. interest rates from 10 percent to over 20 percent between 1979 and 1982. This policy would have drastic consequences for Tanzania and other countries in the region, as debt servicing costs crowded out social spending. Then Federal Reserve Chair Paul Volcker later noted that “Africa was not even on my radar screen” (Brautigam, 2023). The end of *Ujamaa* ultimately led to a shift in the country’s political and economic orientation away from socialism and ideological indoctrination, which has been sustained by subsequent governments (Costello, 1996). This shift also led to the abandonment of the Political Education curriculum, and ultimately, the official reintroduction of ‘Civics’ in its place in 1992 (Komba, 1996).

As we show in what follows, the *Ujamaa* experiment – though spanning little more than a decade (1970-81, reflecting the time period the bundled treatment was in effect) – had a profound and lasting impact on the hearts and minds of Tanzanian citizens, particularly those exposed to public education in planned villages during the *Ujamaa* period.

3. Data

Our sample includes individuals responding to contemporary surveys, from which we obtain outcomes and birth dates to capture variation in their exposure to the *Ujamaa* policy over time. Using information on respondents’ place of residence, we link this sample to historical census data to capture variation in their exposure to villagization over space. Table 1 provides descriptive statistics. Appendix Tables A.2 and A.3 provide an overview and description of the variables used in the paper.²⁰

3.1. Historical District-level Data

We use newly digitized data from the 1978 population census (Bureau of Statistics, Ministry of Planning and Economic Affairs, 1981) to measure the historical extent of villagization across space. The historical intensity in an individual’s district of residence proxies for their exposure to villagization. While we have also obtained data on villagization from the historical population censuses at finer geographical units, it cannot be linked to outcomes from contemporary surveys, which identify respondents at the district level at most.²¹ Appendix Figure A.4 shows an excerpt of the 1978 census data. We measure villagization as the share of a district’s rural population living in registered government villages in 1978:

²⁰ Due to the rural nature of the policy, we exclude the capital Dar es Salaam (Mzizima) as well as the semi-autonomous islands (Zanzibar, Pemba, Mafia) from our sample. The islands’ governments have separate authority over a number of government functions, including education.

²¹ We do not observe respondents’ district of birth or district of residence during the *Ujamaa* policy. We discuss in detail further below how our using the current district of residence affects the interpretation of our estimates and potential threats to identification, including selective migration.

$$V_d = \frac{P_{d,1978}^{registered}}{P_{d,1978}^{rural}}$$

where $P_{d,1978}^{registered}$ denotes the number of individuals living in registered villages in district d and $P_{d,1978}^{rural}$ denotes total rural district population in district d in 1978. We aggregate districts to their 1967 boundaries since this is the unit of variation of our pre-policy controls.²²

As shown in Table 1, Panel A the villagization measure has a mean of 0.95, and significant dispersion with a minimum of 0.52 and a maximum of 1. The average extent of villagization is relatively high because some geographic zones of the country were completely villagized. However, there is considerable within-zone variation in other areas, which we exploit in our empirical analysis. Appendix Figure A.5 illustrates the variation.²³

The variation in the extent of villagization across space can largely be explained by the fact that its implementation was left to district officials (a further reason for our focus on the district as a unit of analysis), who enforced the policy with different degrees of conviction (McHenry, 1979). This variation may not be random across space nor does it need to be for our empirical strategy to be valid, as we explain in Section 4. Nevertheless, we control for the most important potential correlates of villagization intensity, as identified in historical accounts. These include: pre-*Ujamaa* primary school enrollment rate, district revenues to capture local government capacity, ethnolinguistic fractionalization, geographic characteristics, availability of public health infrastructure, weather shocks, the degree of urbanization, initial national identity, population density, levels of economic development, agricultural production, distance to Uganda, and the presence of colonial missions and roads (Ergas, 1980; Hydén, 1980). These baseline characteristics variables are mostly digitized from historical district-level government statistics (Jensen, 1968) based on the 1967 population census or from the 1967 population census itself (Central Statistical Bureau, Ministry of Economic Affairs and Development Planning, 1968), and complemented by data from various other mostly historical sources. We discuss these correlates in Appendix A.

3.2. Individual-level Survey Data

Our primary data on long-run outcomes and other individual-level characteristics is from the geo-coded Afrobarometer public opinion survey Rounds 3 (2005) and 4 (2008) (Afrobarome-

²² In contrast, Osafo-Kwaako (2012) employs the 1978 post-policy district boundaries. Additionally, our measure is constructed based on the rural population (excluding urban areas as they contain no villages) while Osafo-Kwaako (2012) incorporates urban and institutional populations (e.g. military and prisoners).

²³ Tanzania has two main subnational administrative units: the region and the district. A zone is a larger subnational geographic area that is not an official administrative unit but is commonly used by organizations such as the Demographic and Health Surveys (DHS) and the Tanzania Ministry of Health. A zone contains three regions or six districts on average.

ter, 2017; BenYishay et al., 2017).²⁴ The Afrobarometer surveys are widely used, nationally representative surveys conducted by a pan-African research institution unaffiliated with any national government. The survey rounds we use comprise a sample of 1,797 individual respondents from across Tanzania. We focus on respondents born in 1948-1987, which ensures that the individuals in the sample would have been able to complete their primary education in the post-independence period. We use information on individuals' districts as reported in the survey to match them to our district-level data. Our focal dependent variable captures national identity and is based on the following question:

“Let us suppose that you had to choose between being a Tanzanian and being a [respondent’s previously reported ethnic group]. Which of the following statements best expresses your feelings?”

Respondents could report that they feel *only/more/equally/less/not at all Tanzanian* as compared to their ethnic group. We code our baseline outcome variable on a 0 to 1 scale with quarterly increments, where 0 indicates that the respondent identifies only with her ethnic group and 1 indicates that she identifies exclusively with Tanzania.²⁵ We interpret a higher score as reflecting a stronger national identity. This measure of national identity is standard in the literature (see, e.g., Depetris-Chauvin et al., 2020).

We use several other variables from the Afrobarometer survey data to investigate channels and measure outcomes, such as respondents' views of the state and trust in the media.²⁶ We describe these variables as we introduce them for the empirical analysis in Sections 5 and 6. We also use respondents' birth years from the Afrobarometer survey to measure their temporal exposure to the *Ujamaa* policy. Finally, we construct a dummy variable for whether the respondent has completed primary schooling, which we use in complementary analyses.

Table 1, Panel B shows individual-level descriptive statistics for the baseline sample used in our analysis. The mean measure of national identity is 0.89. National identity is high on average in Tanzania compared to the rest of Africa, presumably in part due to the *Ujamaa* policy.²⁷ The average birth year of respondents in our sample is 1962. Most of the respondents (88%) completed some schooling.

²⁴ Our choice of these rounds reflects data availability (key questions were incomparable in their wording in Rounds 1 and 2) and time since the villagization policy ended.

²⁵ Moreover, we code the variable as 1 for respondents who do not identify with any ethnic group according to another survey question. The question in the main text quoted above is based on the Round 4 codebook. The text for Round 3, which we code up analogously, is as follows: *Let us suppose that you had to choose between being a Tanzania and being a [respondent’s previously reported ethnic group]. Which of these two groups do you feel most strongly attached to? I feel only/more/equally [respondent’s group] than Tanzanian or more/only Tanzanian.*

²⁶ The sample size for the analysis of some of these questions is smaller because they were only asked in one rather than both Afrobarometer rounds used in this paper.

²⁷ Tanzanians are ranked the fourth-highest on the continent for their feelings of national identity after Burundi, Guinea and Sao Tome and Principe. The lowest national identity is held by Nigeria at 0.54 (Afrobarometer data).

3.3. Other Data

We incorporate data from various other sources for additional analyses and robustness checks. These include Round 1 of the Tanzania National Panel Survey (TNPS) in 2008/2009 ([National Bureau of Statistics, 2010](#)), which is part of the World Bank’s Living Standard Measurement Surveys (LSMS) and includes several thousand respondents from across Tanzania. We use this dataset in robustness checks related to migration.²⁸ We also use data on individuals’ occupations and household consumption from the TNPS to investigate channels. The majority of the respondents work in agriculture (82%).²⁹

We use data from the Demographic and Health Surveys (DHS) in 1991 and 1996 ([Ngallaba et al., 1993](#); [Bureau of Statistics/Tanzania and Macro International, 1997](#)) to study intermarriage as a revealed-preference measure of national identity. These two survey waves report married couples’ ethnic affiliations (later waves do not report ethnicity). We code a dummy that equals 1 if a respondent shares his or her spouse’s ethnic group, and 0 otherwise. In our sample, between 66% and 70% of marriages occur within the same ethnic group, depending on whether we select the sample based on the husband’s or wife’s cohort.

Finally, we use data from the IPUMS sample of the 1988 population census ([Minnesota Population Center, 2015](#)) to study educational attainment. We use data on electoral outcomes from the National Electoral Commission in Tanzania ([Election Study Committee, 1974](#); [Carlitz, 2017](#)), which we describe in Section 5.

4. Empirical Strategy

This section outlines our empirical strategy to estimate effects of the *Ujamaa* policy, which included villagization and a public education reform, on national identity and attitudes towards the state in the long run. The ideal experiment would randomly assign *Ujamaa* to some communities or individuals but not to others, and then compare their outcomes. However, *Ujamaa*, like most policy reforms throughout history, was not carried out in such a manner and thus there may be joint determinants of villagization, exposure to education, and outcomes of interest. We use a difference-in-differences specification to address such confounders. The first difference is spatial and comes from varying intensity in villagization across individuals’ home districts. The second difference is temporal and comes from plausibly exogenous variation in the exposure of age cohorts induced by the timing of the *Ujamaa* policy (and in particular the curricula reform). We discuss our identifying assumptions and potential threats to identification after first introducing our specification.

²⁸ The Afrobarometer data do not contain information about migration history or birth district.

²⁹ For a small number of respondents, age and birth year are not consistent. We exclude these observations.

Our empirical strategy relies on the fact that the date of birth and villagization intensity in an individual’s district jointly determine her exposure to *Ujamaa*. Individuals born in 1959 or earlier were older than the official political education age when the *Ujamaa* policy took effect in 1970. They should thus not be affected by the full “treatment.” In contrast, individuals born between 1960 and 1971 were old enough to be exposed to both villagization and the government’s new political education curriculum, which was taught from 4th grade. They were also young enough to attend 4th grade before the villagization period officially ended in 1982. In the main specification, our “control” cohort was thus born in 1948–1959, and our “treated” cohort was born in 1960–1971. In complementary analyses, we also consider other cohorts. The two cohorts in our baseline sample consist of 849 individuals in 52 districts.

4.1. Identifying Effects of *Ujamaa*

For our baseline, we estimate the following specification:

$$y_{idzt} = \beta(V_{dz} \cdot treatedcohort_t) + (\mathbf{X}'_{dz} \cdot treatedcohort_t)\mathbf{\Gamma} + \alpha_{dz} + \delta_{zt} + \epsilon_{idzt} \quad (1)$$

where y_{idzt} is an outcome of individual i in district d , zone z , and cohort t . V_{dz} is our district-level measure of historical villagization as described in Section 3, and $treatedcohort_t$ is a dummy that equals 1 if individual i was born between 1960–1971 (0 if born between 1948–1959). α_{dz} denotes district fixed effects (which also include zone fixed effects), δ_{zt} denotes zone-cohort fixed effects and \mathbf{X}'_{dz} is a vector of controls. Our preferred specification includes the pre-*Ujamaa* district-level primary schooling rate interacted with the cohort dummy, as well as survey year fixed effects, as control variables. We control for other variables in robustness checks, discussed in Results Section 5. We cluster standard errors at the district.

Our coefficient of interest is on the first interaction term. β captures the average difference in outcomes between high- and low-villagization districts for individuals of political education age during the *Ujamaa* period. Alternatively, it represents the average difference in outcomes between individuals of political education age and other cohorts within a high-villagization district.³⁰ Since we do not directly observe individual exposure to villagization but villagization intensity at the district level, we interpret β as an intent-to-treat effect, which is likely smaller than a treatment effect on the treated. The baseline specification is appealing due to its simplicity and because pooling individuals into two cohorts increases statistical power. However, we also estimate a specification that allows treatment effects by cohorts to vary more flexibly over time. The main advantage of this second specification is that it allows us to assess pre-trends. In addition, we can test whether treatment effects are stronger for those that were of primary schooling age during the entire villagization period than for those with

³⁰ In the baseline, we use a continuous measure of villagization rather than a high-/low-villagization dummy.

only partial temporal overlap in exposure to the *Ujamaa* policy. The flexible specification is as follows:

$$y_{idzt} = \sum_{t=2}^{10} \beta_t (V_{dz} \cdot cohort_t) + \sum_{t=2}^{10} (\mathbf{X}'_{dz} \cdot cohort_t) \Gamma_t + \alpha_{dz} + \delta_{zt} + \epsilon_{idzt} \quad (2)$$

where $cohort_t$ is a dummy that indicates whether individual i belongs to cohort t . We divide our sample into ten 4-year cohorts (born between 1948-1951, 1952-1955, ..., 1984-1987). The cohort born between 1948 and 1951 ($t = 1$) is the omitted category. We interpret each of the parameters β_t as the impact of the *Ujamaa* policy on cohort t . Since villagization lasted from 1970 to 1981, we expect the coefficients for the cohorts born between 1960 and 1971 to be greater than 0 and the coefficients for the cohorts born before 1960 to be equal to 0. The coefficients for cohorts born after 1971 are somewhat ambiguous ex-ante but we expect them to decrease over time.³¹

4.2. Assumptions and Threats to Identification

The difference-in-differences estimate β in equation (1) can be interpreted as a causal ITT effect of the *Ujamaa* policy under the parallel trends assumption that, in the absence of the policy, the changes in the outcome variables across cohorts would not have been systematically different in low and high villagization districts within a zone. We now discuss the most important potential challenges to this and other assumptions.³²

Level differences across districts. Districts with lower initial levels of national identity may have implemented the villagization policy more or less intensively. There may also be other joint determinants of villagization and national identity across districts, as discussed in [Appendix A](#). Such level differences, insofar they are constant across cohorts, are subsumed by district fixed effects and are not a threat to identification given our approach.

National or regional policies and other differences between cohorts. The timing of the *Ujamaa* policy may have been correlated with other factors that shaped national identity across the country, such as macroeconomic developments, trends in education, or regional policies. Such factors, insofar as they affected the country or the districts within a subnational geographic zone similarly, are subsumed by cohort-zone fixed effects. Similarly,

³¹ The emphasis on nation-building was removed from the official curriculum in 1992 (and likely received considerably less emphasis after villagization was officially abandoned in 1982). However, those who remained in the registered villages still likely enjoyed better access to public schooling than those in other areas, and the textbooks used in schools likely remained the same for a few years given a lack of alternatives.

³² Moreover, we assume that the policy was unanticipated. We discuss potential violations of SUTVA (spillovers) in the robustness section. We are not concerned about the identification challenges related to difference-in-differences settings with staggered treatment highlighted in the recent econometrics literature (see [Roth et al., 2023](#)). The treatment in our main specification is not staggered and we do not estimate dynamic treatment effects.

general differences in national identity between cohorts (for example due to age effects), insofar they are constant within zones, are not a concern given the cohort fixed effects.

Different trends over cohorts across districts. The development of national identity may have followed different pre-existing trends over cohorts across districts with differential intensity of villagization. For example, school cohorts in more remote districts, that were treated less (or more) intensively due to their distance from government institutions, may have had lower levels of national identity initially, and would have caught up with other districts post-independence even in the absence of the villagization policy. If not accounted for, such trend differences could violate the parallel trends assumption and thus pose a threat to identification (as opposed to level differences). The descriptive evidence in Figure 1 shows that this is unlikely to be a major concern, as low- and high-intensity districts follow parallel trends over cohorts that were of primary schooling age pre- and post-policy. We also look for signs of differential pre-trends more systematically in Section 5.

In addition, we interact cohort fixed effects with numerous pre-*Ujamaa* district characteristics that may be correlated with villagization intensity and that may affect national identity differently for different cohorts in the long run (see Appendix A). Our baseline includes the primary school enrollment rate in 1967 interacted with cohort fixed effects.³³ In a battery of robustness checks, we also include a number of other controls in 1967 interacted with cohort fixed effects (see Section 5.4).

Migration. We observe respondents' districts at the time the contemporary surveys were conducted (1991-2008) but not at the time of villagization (1970-1981). If a respondent lived in a different district when they were of primary schooling age vs. when they responded to a survey, this could bias the estimate of our coefficient of interest in different directions, depending on what determines migration. However, not all forms of migration threaten the validity of our results. First, note that within-district migration does not affect our results as our measure of villagization varies at the district level. Second, if migration across districts is uncorrelated with villagization or national identity, this is akin to classical measurement error in the villagization variable, biasing our estimate towards zero. In that case, our coefficient estimate would be a lower bound on the true effect of the *Ujamaa* policy on outcomes. We are more concerned about selective migration. In Section 5.4, we provide several pieces of evidence to show that selective migration is unlikely to explain our results.

³³ Pre-policy enrollment is significantly correlated with villagization intensity; other district characteristics we discuss in Appendix A are not significantly correlated with villagization, conditional on zone FE.

5. Results: Ujamaa and National Identity

5.1. Descriptive Evidence

Before we discuss our main results, we show descriptive evidence on the levels of national identity by age cohort and intensity of villagization. Figure 1 plots the mean of our main measure of national identity in 2005/2008 by age cohort, relative to the level of the first cohort in our sample (born 1948-1951), for high- and low-villagization districts separately (above or equal to and below median villagization in the sample). For ease of interpretation, we standardize the measure of national identity to have a mean of 0 and a standard deviation of 1. We group respondents in 4-year cohorts to reduce noise. We see few systematic differences across cohorts for low-villagization districts, where the *Ujamaa* policy was implemented to a lesser degree. In contrast, in high-villagization districts, national identity is higher on average for the cohorts that were of schooling age during villagization compared to the younger and older cohorts. Mean national identity of cohorts that were too old or too young during villagization (born 1948–1959 or 1972–1987) moves in parallel for high- and low-villagization districts, but diverges for cohorts that were of schooling age during villagization (those born 1960–1971). While this evidence is reassuring, we do not interpret it as causal as it does not account for the set of fixed effects and controls that we include in our main difference-in-differences specification, which we discuss next.

5.2. National Identity: Difference-in-differences Estimates

Table 2 shows our main difference-in-differences estimates of the ITT effect of the *Ujamaa* policy in 1970–1981 on national identity in 2005–2008 (β in equation (1)). Column (1) shows that national identity is on average 0.165 standard deviations higher for the treated cohort compared to the control cohort for every 1 standard deviation increase in villagization, controlling for district and cohort fixed effects, pre-*Ujamaa* primary schooling rates interacted with cohort fixed effects, and survey year fixed effects. Column (2), our preferred specification, additionally controls for zone-cohort fixed effects. Our main result in column (2) is that a one standard deviation increase in exposure to the *Ujamaa* policy increases national identity around two decades later by 0.226 standard deviations. The effect is statistically significant (p-value < 0.01). The difference between average expressed national identity of the treated and the control cohort, for a district with complete villagization compared to a district without any villagization, corresponds to the difference between a respondent “feeling more Tanzanian” and “feeling more a member of her ethnic group [than Tanzanian]”.

In columns (3) to (8), we additionally control for the 1967 district characteristics indicated in the column heads interacted with the cohort dummy. These district characteristics are potential correlates with villagization discussed in Appendix A (distance to capital, district

revenue, ethnolinguistic fractionalization, geographical features, hospital beds, and weather shocks). All coefficients on the interaction between villagization and the cohort dummy remain qualitatively similar and are significant at the 5% or 1% level. In the Robustness Section 5.4 further below, we include further controls interacted with the cohort dummy.³⁴

5.3. *Parallel Trends*

Figure 2 displays the regression coefficients of our flexible specification including all cohorts (equation (2)). Each coefficient shows the differential effect of a one standard deviation increase in villagization on national identity for the birth cohort indicated on the x-axis compared to the reference cohort (born in 1948-1951). In line with our main results, we find positive effects of villagization on national identity for the cohorts that were of primary-school age during the policy (largest and significant at the 5% level for the cohort born in 1968–1971, i.e., in 4th grade age at the height of villagization) but not for older cohorts. We find a positive, albeit statistically insignificant effect on the cohort that entered primary-school age right after villagization officially ended. This is not surprising given that the schools established in the registered villages remained operational after the policy ended and political education was not abolished until 1992, although the zeal of its implementation likely waned after the end of villagization. For cohorts of primary school age longer after villagization ended, there is only a very small differential effect of villagization on national identity.

Figure 2 provides support for the parallel trends assumption. We see no differential pre-trends in national identity between high- and low-villagization districts over cohorts that were of primary schooling age before the policy. Nor do we see differential trends for cohorts of primary schooling age many years after the policy ended. If our main results were explained by unobserved factors correlated with villagization, they would need to have different effects for different cohorts following this specific pattern.

Appendix Table A.4 column (2) reports the point estimates and standard errors displayed in Figure 2. In addition, we re-estimate this specification, now controlling for the same variables as in our main Table 2, all interacted with cohort fixed effects. The coefficients of interest are qualitatively similar across all columns. The interaction for the birth cohort of 1968–1971, which was of schooling age at the height of villagization, is statistically significant at the 1% or 5% level in all columns except column (6) (p-value = 0.108).

³⁴ Note that we cannot control for all district characteristics simultaneously, each interacted with cohort fixed effects, due to a lack of statistical power. However, in Table 3 columns (8) and (9), we show robustness to controlling for the first principal component of all controls interacted with cohort fixed effects.

5.4. Robustness

Here we address several other potential empirical concerns. We return to our baseline specification in equation (1), which pools the treated cohorts born in 1960–1971 and compares them to the pooled cohorts born in 1948–1959. Tables 3 and A.5 show the results of these robustness checks for our preferred specification.

Additional controls. In Table A.5 columns (3) to (8), we show that the main result presented in Table 2 is robust to controlling for further pre-*Ujamaa* district characteristics interacted with the cohort dummy. Columns (1) and (2) are identical to Table 2 for comparison. The additional controls include pre-*Ujamaa* levels of national identity based on a survey conducted in 1967 (Prewitt et al., 1970), urbanization, population density, GDP, the value of agricultural production (all from Jensen (1968)), distance to the border with Uganda, the presence of colonial missions (Hedde-von Westernhagen and Becker, 2022), and the total length of roads in the district in 1968 (Jedwab and Storeygard, 2022), respectively scaled by district population and area. The coefficients in columns (3) to (8) are qualitatively similar to the baseline in column (2) and remain highly statistically significant.³⁵ In Table 3 columns (8) and (9), we show robustness to controlling for the first principal component of all controls interacted with cohort fixed effects.³⁶ These checks address concerns that the national identity of cohorts in districts differing in these characteristics followed differential trends for reasons unrelated to *Ujamaa*.

Migration. Migration during the time between villagization and the survey could bias our estimates since we only observe the current district of respondents at the time of survey and not where they lived during villagization. However, as discussed in Section 4.2, only selective migration threatens the interpretation in a way that goes against our results. In Appendix B, we conduct two exercises to assess robustness of our findings to potential selective migration. Table 3 column (5) drops all districts with either an in- or an out-migration rate in the highest deciles, with similar results to our baseline. As we explain in Appendix B, Columns (6) and (7) show that even under strong assumptions working against our findings, potential selective migration is unlikely to explain the majority of the positive relationship between the *Ujamaa* policy and national identity we find.

Other robustness checks. In Appendix C, we show robustness to alternative regression weights, samples and functional forms, and address concerns about social desirability bias.

³⁵ The coefficient in column (3) is slightly smaller than our main estimate, which can be explained by the fact that the sample is different because data on national identity in 1967 is not available for some regions.

³⁶ Data on national identity in 1967 is not available in all districts. In column (8), we leave away this variable. In column (9), we include this variable with the caveat that the sample is different from the baseline.

5.5. *Intermarriage*

So far, we have presented evidence suggesting that the *Ujamaa* policy bolstered self-reported national identity in the long run. We now examine whether this translates into real-world outcomes, examining the effect of the *Ujamaa* policy on ethnic intermarriage.

We follow a similar empirical approach as previously but construct a new outcome variable using data from the DHS in 1991 and 1996.³⁷ We construct an outcome variable that equals 1 if both partners belong to the same ethnic group. The treatment variable is based on the husband or wife’s district and birth year.

Table 4 presents the results. Using the husband’s cohort (column (1)), we find that a one standard deviation increase in villagization is associated with a 6.1 percentage point (0.128 standard deviations) decrease in the likelihood of partners sharing the same ethnic group for people from cohorts of primary schooling age during the villagization period, compared to pre-villagization cohorts. The effect is statistically significant at the 10% level. In other words, the *Ujamaa* policy increased the rate of inter-ethnic marriages. We interpret this as “revealed preference” evidence for a declining importance of ethnic identity relative to national identity as a consequence of the *Ujamaa* policy. One potential concern with this exercise is that our conditioning variable – whether a respondent is married – could be endogenous to our treatment. However, as column (3) in Table 4 shows, our treatment has no large or statistically significant effect on the probability that a given individual in the DHS rounds we study is married.

Column (2) shows the coefficient of the same exercise based on the wife’s instead of the husband’s cohort. Although the effect of *Ujamaa* on intermarriage goes in the same direction, the coefficient is smaller and statistically insignificant at conventional levels. We identify three main reasons for a discrepancy between the results based on the husband’s and wife’s cohort: first, wives are on average seven years younger than their husbands. Thus, if a wife is treated, her husband tends to be in the older cohort. To corroborate this explanation, columns (4) and (5) interact the *Ujamaa* treatment with the age difference between husband and wife. Consistent with our explanation, we find that the results on intermarriage based on wife’s birth year are largest and significant for couples with a small age difference. A second explanation reflects gender norms. Women’s autonomy in marriage choice tends to be constrained in Tanzania,³⁸ so we would expect results on intermarriage to be weaker when using the wife’s birth cohort. Thirdly, girls may have been more weakly affected by the treatment than boys due to lower rates of school attendance, which reflect gendered patterns of household labor (Chamie, 1983). The political education curriculum also coincided with

³⁷ These two survey waves record the ethnic group of each respondent and identifiers that allow us to link couples. Subsequent waves did not ask about ethnicity. We include all married couples in the sample.

³⁸ Recent data from Tanzania indicates that 36 percent of women marry before the age of 18, and that 30 percent have limited say in choosing their life partners (Green et al., 2023).

the onset of puberty, which has been shown to further constrain girls’ school attendance given insufficient menstrual hygiene management resources and practices (Benshaul-Tolonen et al., 2020). Women may thus have been exposed to political education at lower rates and the treatment effects based on women’s cohorts would be weaker than those based on men’s cohorts. Consistent with this explanation, we also find a smaller effect of *Ujamaa* on national identity for females than males, as Table 5 columns (7) and (8) show.

5.6. Channels

We conduct several exercises to examine two sets of channels that plausibly explain our results. The first, and our preferred explanation, is that *Ujamaa* primarily shaped the identities of students through public education provided in planned villages. Alternatively, the outcomes we observe may reflect other cohort-specific factors that interacted with villagization. Note that the district fixed effects hold constant the average effect of villagization alone across all cohorts, so any channel must be specific to the cohort that was of the age to receive political education during *Ujamaa*. While villagization alone may have affected national identity across all cohorts, only those of political education age during *Ujamaa* were exposed to the bundled treatment (combining villagization with education). Hence, our main coefficient can be interpreted as the differential effect of the bundled treatment compared to the effect of villagization alone.³⁹

To differentiate between the two sets of potential channels, we examine heterogeneous treatment effects by whether respondents attended primary school. In Table 5, we interact our treatment with a dummy that indicates whether the respondent completed at least some formal primary schooling. We also control for the un-interacted schooling dummy as well as its interactions with the cohort dummy and the villagization variable. Column (4) indicates that the coefficient on the treatment is 0.331 standard deviations larger for those who attended primary school compared to those who did not. There is no significant effect of the treatment on national identity for individuals who did not obtain any formal schooling. This result is consistent with public schooling as the primary channel through which the *Ujamaa* policy shaped national identity, rather than other differences between age cohorts that may have interacted with villagization.⁴⁰ The results are less precisely estimated for intermarriage (columns (5) and (6)) but the sign on the interaction coefficient is consistent. That the

³⁹ The effect of villagization alone appears to be small. Levels of national identity are no higher in more intensively villagized districts for cohorts outside of schooling age during the *Ujamaa* era (see Figure 1).

⁴⁰ An alternative explanation for this heterogeneity result is selection into primary school: it is theoretically possible that parents opposed to the regime’s effort to indoctrinate their children were less likely to send their children to school during *Ujamaa*. If these children report less strong feelings of national identity in later surveys, this selection rather than the effect of attending primary school could explain the heterogeneity result. However, such selection into schooling is unlikely to be an important channel underlying our main result on national identity given that the majority of children in official villages were enrolled.

effect of *Ujamaa* on national identity is stronger for males, who had higher rates of school attendance than females (Chamie, 1983) (Table 5 columns (7) and (8)), corroborates the importance of schooling.

Having established public education as the most plausible channel through which the *Ujamaa* policy influenced national identity, we turn to an exploration of more specific mechanisms. First and foremost, the content of the new political education curriculum was clearly oriented toward establishing national identity, as we outline in Section 2 (Background). As we detail below, we find empirical support for this mechanism as the most important one driving the public education channel. We also explore the possibility that establishing Swahili as the language of instruction played a role. Finally, we note that public education could have also facilitated the formation of a national identity through inter-group contact across ethnic lines and may have had implications for human capital, income, and occupational choice. We examine these and other potential mechanisms below.

Public education curriculum. Villagization served in part to disrupt traditional norms and networks. As Scott (1998) notes, “the purpose of forced settlement is always disorientation and then reorientation” (p. 235). In the Tanzanian case, reorientation – away from distinct, ethnic identities, and toward a consolidated, national identity – was facilitated primarily through the primary school curriculum. This reform was arguably more effective than concurrent policies aimed at promoting national identity as it targeted citizens during particularly “impressionable years” (Krosnick and Alwin, 1989).

Changes to the curriculum were implemented alongside a nation-wide increase in the supply of education. However, our analysis suggests that changes in the *content* of education that was delivered under the *Ujamaa* policy in the official villages, rather than a general increase in the *supply* of education, explain our results.⁴¹ Recall from Figure 2 that we see few differential effects of villagization on cohorts who were of primary schooling age after the end of *Ujamaa* – and especially for cohorts that entered primary schooling age after the abolition of political education in 1992. Moreover, the effect of villagization on school completion for the treated cohort specifically was modest in size (Table A.6, column (1)).⁴²

We further isolate the impact of the revised curriculum by ruling out the possibility that the policy served to improve education *quality*. First, if the quality of education were higher among treated cohorts, we would expect to see increased human capital in those cohorts. However, as we discuss below, we find no positive or significant effects on outcomes that

⁴¹ The overall supply of public education increased for all cohorts, even after the *Ujamaa* policy ended, since the public schooling infrastructure remained. However, the elements of the policy that explicitly targeted nation-building waned with the policy’s repeal.

⁴² On the one hand, enrollment increased nationwide; on the other hand, as documented in Appendix A, pre-*Ujamaa* enrollment, which we control for, is positively correlated with villagization. This is likely due to the villagization campaign being easier to implement in places that had existing infrastructure, which in turn weakens the differential effect of villagization on primary school completion.

should reflect higher human capital (occupational choice and income). Second, as we discuss in Section 2 (Background), the directive to overhaul the curriculum was not necessarily accompanied by additional resources.⁴³ Third, one of Nyerere’s goals with the Education for Self-Reliance policy was to “reduce elitism and the tendency for schooling to further social and other inequalities and class formation.” Thus it seems unlikely that the results we observe are driven by an increase in school quality, but rather, the *content* of the new curriculum.

Swahili as the national language of instruction. In addition to revising the content of the public school curriculum, the *Ujamaa* era saw the establishment of Swahili as the national language of instruction in all primary schools. Around the world, the establishment of a common, national language has played an important role in strengthening national identity (see e.g., [Alesina et al., 2021](#)). To see whether this is the case in our setting, we examine whether treated individuals are more likely to be proficient in Swahili. Appendix Table A.6 column (2) shows the effect of our treatment on literacy in Swahili based on census 1988 data from IPUMS. The effect is positive and statistically significant but small. This is likely due to the fact that the overall level of Swahili proficiency is very high – both in Tanzania and in our sample. According to Afrobarometer Round 4, only 8 out of 1208 respondents in our sample report not speaking Swahili well, and 99.0% of respondents in Round 3 and 93.7% of respondents in Round 4 report their home language being from the Bantu language family (a group of languages that are linguistically very similar; Swahili is the most common Bantu language). Moreover, all Afrobarometer interviews were conducted in Swahili, so all respondents must be proficient.

Taken together, while Swahili as a national language may have helped with nation-building in Tanzania, everyone’s similar level of Swahili proficiency means it’s unlikely to explain why *Ujamaa* had a different impact on national identity among certain groups.

Intergroup contact. An alternative and perhaps complementary channel through which the *Ujamaa* policy might have shaped national identity is through intergroup contact across ethnic lines. Intergroup contact has a long history in the social sciences, beginning with psychologist Gordon [Allport \(1954\)](#) specifying the conditions under which it can reduce prejudice. [Allport’s \(1954\)](#) hypothesis has been confirmed in more recent meta-studies ([Hewstone et al., 2014](#)) and economists have also begun to more rigorously identify its effects – for example showing that collaborative intergroup contact between individuals from different social groups may foster cooperation and reduce the salience of group identity ([Bazzi et al., 2019](#); [Rao, 2019](#); [Lowe, 2021](#)). In the context of villagization, bringing children from different ethnic groups together in public schools in *Ujamaa* villages could have decreased the

⁴³ While Nyerere’s post-independence reforms included additional investments (a general expansion of public goods and services including water, land, agricultural and veterinary production supplies and equipment), which served in part as inducements to encourage resettlement into the *Ujamaa* villages ([McHenry, 1979](#)), these seem unlikely to have had cohort-specific effects on national identity.

salience of ethnic identity in favor of national identity. However, scholars have also shown that intergroup contact can also foster exclusionary attitudes (Enos, 2014) and challenge social solidarity (Putnam, 2007). In our context, it is also possible that intergroup contact *sharpened* the salience of ethnic identities, for example due to intergroup competition for limited resources in the villages.

If intergroup contact played an important role in explaining the effect of our treatment on national identity, we would expect larger effects in places where such contact was more likely to take place. To test this idea, we interact our treatment (cohort interacted with villagization) with district-level ethnolinguistic fractionalization (ELF) in 1967.⁴⁴ We also control for the lower-level interactions that are not included in the fixed effects. Table 5 column (1) shows the results for our main measure of national identity and columns (2) and (3) show the results for intermarriage. For both outcomes, the triple interaction of villagization \times cohort \times ELF is small and statistically insignificant.

In addition, as discussed in Section 6 below, we note that *Ujamaa* did not lead to an increase in generalized trust, as would be consistent with intergroup contact as the main channel underlying our results.

Occupational choice. Schooling under *Ujamaa* not only included political education but may have also provided students with enhanced human capital and skills. Resulting occupational choices may have in turn bolstered national identity. For example, those who obtained public education during the *Ujamaa* period may be more likely to work in public sector jobs. We investigate this in Appendix Table A.7 which is analogous to our baseline specification but with dummies for different occupations as outcome variables (employed in the government (including parastatal), employed in the private sector, agriculture or self-employed in other sectors).⁴⁵ All coefficients are neither sizeable nor statistically significant, indicating that the treatment had little to no effect on occupational choice.

Economic effects. As explained above, it is possible that education under *Ujamaa* led to improved human capital or income. If schooling increased individuals' earnings, which in turn fostered a sense of gratitude towards the new Tanzanian nation-state, this may be an alternative explanation for our finding on the effect of *Ujamaa* on national identity.

At the outset, it is important to note that *Ujamaa* likely had negative economic effects on individuals on average (Collier, 1988). If anything this goes against our main finding on national identity if economic well-being and national identity are positively correlated. Economic effects are thus unlikely to play a major role in driving our findings. Nevertheless,

⁴⁴ ELF is computed as 1 minus the Herfindahl concentration index of ethnolinguistic group shares in each district given by the 1967 population census data. A higher ELF index corresponds to a higher degree of ethnic diversity within a district.

⁴⁵ The data is from the Tanzania National Panel Survey Round 1 (2008). We use this data, which also contains the birth years and district of respondents, since information on occupation is available for only a small subsample of respondents in the Afrobarometer data.

we proceed to test this potential mechanism by examining the economic effects of *Ujamaa* using various measures of income. Appendix Table A.8 columns (1) to (4) show the results for household consumption and expenditures from the Tanzania National Panel Survey 2008 (as above), as well as for a dummy that equals 1 if an individual has a job that earns a cash income and a wealth index constructed from Afrobarometer data (Afrobarometer does not contain consumption data). We find that treated individuals have lower measures of income and wealth in 2008 compared to the control cohort. That is, schooling under *Ujamaa* appears to have coincided with an overall *decrease* in income. In addition, columns (5) and (6) show that national identity and the proxies of economic well-being from Afrobarometer are weakly positively correlated. These results together mean that *Ujamaa* if anything had negative economic effects that translated into weakly negative effects on national identity. Hence, this potential channel does not plausibly explain our results on national identity.

The end of *Ujamaa*. A final question is whether the external developments that led to the end of *Ujamaa*, including a global economic crisis and the invasion by Uganda (see Background section for summary), had cohort-specific effects that explain our results. The short answer is no. We elaborate on our answer and provide empirical tests in [Appendix D](#).

6. Results: *Ujamaa* and State Legitimacy

We now attempt to answer the question of whether, in building the Tanzanian *nation*, *Ujamaa* strengthened the nascent Tanzanian *state*. This is an important question given the ambitions of Nyerere and other state-builders throughout history to use nation-building as a way to overcome societal divisions and establish the state as a legitimate, central authority. Our results point to success in this regard, but also highlight important trade-offs. As we detail below, treated cohorts are more likely to express attitudes in favor of a strong, central state. However, the results also suggest that efforts to build a strong state through *Ujamaa* engendered acquiescence to authoritarianism.

In what follows, we examine the impact of *Ujamaa* on a range of attitudes toward the state, including respect for authority, support for one-party rule, and trust in government and government-run media. We compare the latter to trust in independent media and generalized trust. We also look at attitudes and actions related to citizens' engagement with the state and state institutions. All specifications follow the same empirical strategy as outlined in Section 4, replacing national identity with different outcome variables from Afrobarometer. Figure 3 illustrates the results. We show the corresponding coefficients in Appendix Table A.9. As above, we report standardized coefficients to facilitate interpretation for all non-binary outcomes. The coefficient of interest is the interaction between the respondent's district-level measure of villagization and a dummy that indicates whether the respondent is

in the treated cohort. We interpret this coefficient as the effect of the *Ujamaa* policy on the outcomes stated on the y-axis in the Figure (column heads in the table).

The first outcome is based on a question asking respondents which one of two statements regarding views of the state is closest to their views: *Statement 1: Citizens should be more active in questioning the actions of leaders* or *Statement 2: In our country, citizens should show more respect for authority*. We find that exposure to *Ujamaa* has a sizable and statistically significant effect on respect for state authority: a one standard deviation increase in the treatment increases the outcome variable by 0.169 standard deviations ($p < 0.05$). Prior studies have interpreted this variable as measuring citizens' critical attitudes toward government (Doorenspleet, 2012), which has also been understood as an indicator of demand for democracy (Inglehart, 1997). We interpret these results as suggesting that citizens who were exposed to the *Ujamaa* policy are more likely to see the state as a legitimate central authority, but less likely to express demand for democracy.

The second outcome examines respondents' stated approval for a system of government where only one political party is allowed to stand for election and hold office. We find that exposure to the *Ujamaa* policy is positively correlated with approval of one-party rule: a one standard deviation increase in the treatment is associated with a 0.097 standard deviations higher measure of approval of one-party rule, although the coefficient is imprecisely estimated ($p = 0.138$). This is perhaps unsurprising, given that, as discussed in the Background (Section 2) above, Nyerere saw single party rule as necessary to foster national integration in a country characterized by substantial ethnic differences (Komba, 1996). Our results speak to the legacy of single-party rule, as well as the intertwined nature of the party and the state, which has persisted despite the (re-) introduction of multi-partyism in 1992 (Paget, 2021).

The third outcome is based on a question asking respondents which of two statements is closest to their views: *Statement 1: People are like children; the government should take care of them like a parent* or *Statement 2: Government is like an employee; the people should be the bosses who control the government*. We find that exposure to *Ujamaa* has a sizable and statistically significant effect on pro-government attitudes: a one standard deviation increase in the treatment is estimated to increase the outcome variable by 0.230 standard deviations ($p < 0.05$). Such trust in government can facilitate voluntary compliance with state policies. Scholars point to extensive buy-in of public health measures such as malaria control (Croke, 2012) among Tanzanians – in contrast to citizens of neighboring states. Such compliance is difficult to achieve without acceptance of the state as a legitimate authority.

For the fourth and fifth outcome, we calculate the outcome variable as the difference between stated trust in government newspapers (TV/radio) and in independent newspapers (TV/radio). Exposure to the *Ujamaa* policy is positively correlated with stated trust in government broadcasting compared to independent media (0.097 and 0.205 standard deviations).

The sixth outcome indicates whether respondents report getting together with others to

raise an issue to the government, which we interpret as another proxy for critical attitudes toward government. The negative and only marginally insignificant coefficient (-5.1 percentage points ($p = 0.106$)) is in line with the result for the first outcome described above, in that it indicates greater acceptance of the state as a legitimate authority.

Appendix Table A.10 presents a number of additional results to examine how *Ujamaa* shifted attitudes related to state legitimacy. The first provides evidence consistent with *Ujamaa* strengthening citizens' expectations and preferences for public goods provision by the state: column (1) shows the coefficient of a regression of support for free schooling on our treatment (analogous to our main specification). The coefficient is positive and statistically significant at all conventional levels. The second set of results considers a set of placebo outcomes: it shows that *Ujamaa* appears to have little effect on gender norms. In sum, these results show that *Ujamaa* had a persistent effect on citizens' preferences along the dimensions it explicitly sought to influence.

Finally, we examine whether exposure to *Ujamaa* influences contemporary political support for the ruling party, which has been in power in different forms since independence and is thus the heir to Nyerere's TANU regime. First, Appendix Table A.10 column (4), which is based on Afrobarometer data and exploits variation across districts and cohorts, shows that *Ujamaa* had a small positive effect on respondents reporting feeling close to the ruling CCM party.⁴⁶ Second, we analyze voting patterns at the district level using electoral data. Note that we cannot conduct the analysis using our standard specification because electoral data is only available at the constituency level but not by age cohorts. As we show in column (5), there is a positive and statistically significant correlation between historical villagization in a district and support for the CCM in 2000-2005 (proxied by turnout multiplied with the CCM presidential candidate's vote share in the national elections),⁴⁷ controlling for turnout in 1970 (i.e., pre-*Ujamaa*), primary school enrollment rate in 1967, and zone fixed effects (analogously to our main empirical strategy).⁴⁸ The results are consistent with *Ujamaa* fostering support for the ruling CCM party in the long run, and also dovetail with the results showing support for one-party rule among the treated cohort.

In sum, we find that cohorts exposed to *Ujamaa* are more likely to respect state authority and approve of one-party rule, and have higher trust in central government institutions such as state media. These findings suggest that the *Ujamaa* policy contributed to establishing the new Tanzanian state as a legitimate central authority. There is evidence to suggest that a strong shared identity and trust in a commonly accepted central state are important

⁴⁶ The estimates are statistically insignificant at conventional levels. This could be partly explained by there being little multi-party competition in the post-*Ujamaa* years or by potential reporting bias.

⁴⁷ Turnout is a commonly used proxy for support for the ruling party in a hegemonic party system.

⁴⁸ Data on 2000-2005 voting are from Carlitz (2017) and are based on data from the National Electoral Commission. The data on 1970 elections are from Election Study Committee (1974).

ingredients for avoiding fragility and civil conflict (Besley, 2020). Within Africa, intra-state conflict has been more prevalent in countries where a smaller proportion of the population identifies with the nation as a whole (Besley and Reynal-Querol, 2014), as we show in Appendix Figure A.6. This correlation is consistent with our narrative that creating a strong national identity may contribute to loyalty to the state and more political stability as a result. As we show in Appendix Figure A.7, countries with high ethnic diversity (as measured by ethnolinguistic fractionalization) tend to have a higher incidence of internal conflict. In contrast, Tanzania today is a clear outlier amongst its peers, with a very low prevalence of internal conflict despite being one of the most ethnically diverse countries.⁴⁹

However, we note that efforts to forge a strong, shared national identity do not necessarily generate more cooperation and generalized trust among the population. While exposure to the *Ujamaa* policy generated higher levels of trust in state institutions, it did not have the same impact on generalized intra- or inter-ethnic trust. As we show in Figure 3 and Appendix Table A.9, we find only small and statistically insignificant effects of the *Ujamaa* policy on trust in members of one’s own ethnic group (0.055 standard deviations) or in members of other ethnic groups (-0.141 standard deviations). If anything, the *Ujamaa* policy generally *decreased* inter-ethnic trust. This result may at first seem at odds with what we find above for ethnic intermarriage. However, we understand intermarriage as an expression of *personalized* rather than *generalized* trust in members of other groups (which is what the Afrobarometer question we use on intergroup trust reflects). Importantly, personalized and generalized trust are not necessarily correlated (Guiso et al., 2009). It appears that in our setting, political education and the disruptions to existing social networks and hierarchies caused by villagization (Scott, 1998) contributed to marriage across ethnic lines by reducing the salience of ethnicity, rather than by increasing generalized intergroup trust. This result is in keeping with the evidence we present in Section 5.6, indicating that intergroup contact does not drive our main national identity results.⁵⁰

In sum, the results we obtain for attitudes toward the state and generalized inter-ethnic trust indicate that the creation of a national identity in a top-down manner may have first and foremost strengthened the one-party state’s ability to govern but that the extent of social cohesion across ethnic groups may have remained limited to personal relationships.

⁴⁹ We leave it to future research to further test this narrative by examining the direct impact of *Ujamaa* on conflict prevalence within Tanzania. Data on conflict would need to be at the individual or cohort level given our identification strategy.

⁵⁰ Other recent studies have findings that are consistent with this explanation. Okunogbe’s (2024) study of Nigeria’s national youth service program (which, like *Ujamaa* villagization, involved resettlement within the country) finds that exposure to other ethnic groups increased participants’ reported sense of national pride, as well as their propensity to be in inter-ethnic romantic relationships. However, these results are not accompanied by a meaningful improvement in feelings of trust or closeness towards other ethnic groups.

7. Conclusion

Leaders throughout history have attempted to overcome the challenges of governing diverse populations by using ‘bundles’ of nation-building measures, including public education and resettlement. We study the consequences of one of the largest such efforts in post-colonial Africa — the Tanzanian *Ujamaa* policy, which combined the resettlement of millions of people with a public education reform. We find individuals most affected by the policy are more likely to primarily identify as Tanzanian rather than with their ethnic group, and are more likely to marry across ethnic lines. The effects are persistent and substantive.

Ujamaa’s positive impact on national identity stands in contrast to previous studies of state-building policies involving forced resettlement of diverse populations, which document that such efforts frequently resulted in inter-group conflict (e.g., [Dippel, 2014](#)). The contrasting result for *Ujamaa* arguably reflects the policy’s bundled nature: by combining resettlement with an education reform, the nascent Tanzanian state was able to project its ideology to a similarly young and impressionable population.

We also find that *Ujamaa* helped the new Tanzanian state establish itself as a legitimate central authority. At the same time, the policy reduced demand for government accountability and engendered greater preferences for non-democratic governance, highlighting the difficult balance inherent to effective state-building ([Acemoglu and Robinson, 2020](#)). Members of the treated cohort are more likely to trust state institutions and less likely to question state authority. However, we find few signs of increased cooperation and generalized trust in other ethnic groups among those most exposed to *Ujamaa*.

Our examination of *Ujamaa* has broader implications for the study of state-building in diverse societies, and the sub-field of education and governance in particular. Since [Dewey \(1916\)](#), prominent scholars have argued that education cultivates a “culture of democracy”, driving political legitimacy and societal trust ([Lipset, 1959](#); [Putnam et al., 1994](#)). In contrast, theories of nation-building ([Hobsbawm, 1992](#); [Tilly and Ardant, 1975](#)) imply that mass education is primarily used as a vehicle of indoctrination – by autocracies as well as democracies. Our findings suggest a need for nuance: we find that while state education can successfully foster national identity and inter-ethnic marriages, such efforts may not fundamentally shift attitudes towards higher societal trust and preference for democracy.

Our finding that efforts to build a national identity can promote relatively unquestioning support for state authority raises important questions related to the potentially anti-democratic nature of state-building reforms. These questions are particularly important in light of widespread concern about democratic backsliding and institutional erosion around the world ([Hyde, 2020](#)). Our results encourage further scrutiny of the political economy of state-building in diverse societies. We demonstrate that the choices leaders make on how to build a shared identity from diverse groups are first order and deserve further attention.

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TABLES AND FIGURES

Table 1: Descriptive Statistics

Variable	Data Source	Mean	Std.Dev.	Min.	Max.	Obs.
Panel A. District level, 1967 borders (baseline sample)						
Villagization (share of rural population in official villages in 1978)	Population Census	0.95	0.08	0.52	1.00	52
Primary school enrollment rate in 1967 (per '000 inhabitants)	Jensen (1968)	70.90	23.90	32.70	155.80	52
Distance to Dar-Es-Salaam (km)	UC Davis DataLab, HDX	517.80	272.40	6.37	1021.20	52
District revenue per capita in 1966 ('000 shs)	Jensen (1968)	0.01	0.00	0.00	0.02	52
Ethnolinguistic fractionalization in 1967	Population Census	0.55	0.24	0.07	0.91	52
Centroid latitude	UC Davis DataLab, HDX	-5.85	2.90	-11.00	-1.34	52
Centroid longitude	UC Davis DataLab, HDX	35.00	2.76	30.20	40.00	52
Average altitude in meters	UC Davis DataLab, HDX	1034.20	423.70	111.00	1729.10	52
Average slope	UC Davis DataLab, HDX	25.50	15.20	5.13	63.70	52
Hospital beds in 1967 (per '000.000 inhabitants)	Jensen (1968)	1.07	0.72	0.14	3.32	52
Drought in 1974 (censored z-score)	Tanzania Meteo	-0.03	0.10	-0.46	0.00	52
Drought in 1975 (censored z-score)	Tanzania Meteo	-0.30	0.31	-0.95	0.00	52
Drought in 1976 (censored z-score)	Tanzania Meteo	-0.57	0.43	-1.49	0.00	52
In-migration rate 1970-2004	TNPS	0.11	0.08	0.01	0.38	52
Out-migration rate 1970-2004	TNPS	0.16	0.09	0.01	0.35	52
National Identity in 1967	Prewitt et al. (1970)	0.80	0.08	0.66	0.92	48
Share urban in 1967 (% of population)	Jensen (1968)	0.03	0.05	0.00	0.24	52
Population density in 1967	Jensen (1968)	70.50	76.40	3.50	436.80	52
GDP per capita in 1967 ('000 shs)	Jensen (1968)	0.36	0.20	0.18	1.19	52
Tot. market agricultural production per capita in 1967 ('000 shs)	Jensen (1968)	0.08	0.07	0.01	0.34	52
Distance to Uganda (km)	UC Davis DataLab, HDX	593.80	346.30	51.40	1245.00	52
Number of missions in 1968 (per '000 inhabitants)	Hedde-von Westernhagen and Becker (2022)	0.01	0.01	0.00	0.06	52
Total length of roads in 1968 over district area (km/km2)	Jedwab and Storeygard (2022)	0.01	0.01	0.00	0.04	52
CCM support in 2000 and 2005 elections	Electoral data	0.59	0.12	0.14	0.83	112
Panel B. Individual level (baseline sample)						
National identity	Afrobarometer	0.89	0.23	0.00	1.00	849
Birth year	Afrobarometer	1962.30	6.48	1948.00	1971.00	849
Believe surveyor sent by government	Afrobarometer	0.59	0.49	0.00	1.00	849
Urban	Afrobarometer	0.16	0.37	0.00	1.00	849
Completed primary school	Afrobarometer	0.88	0.32	0.00	1.00	724
Respect authority	Afrobarometer	0.21	0.31	0.00	1.00	835
Support one party rule	Afrobarometer	0.44	0.39	0.00	1.00	829
See government as parent	Afrobarometer	0.58	0.41	0.00	1.00	352
Trust government newspapers	Afrobarometer	0.08	0.21	-1.00	1.00	413
Trust government TV/radio	Afrobarometer	0.09	0.23	-1.00	1.00	422
Took action to hold government accountable	Afrobarometer	0.77	0.42	0.00	1.00	844
Trust own ethnic group	Afrobarometer	0.71	0.25	0.00	1.00	477
Trust other ethnic groups	Afrobarometer	0.64	0.27	0.00	1.00	476
Cash Income Job	Afrobarometer	0.34	0.41	0.00	1.00	849
Wealth Index	Afrobarometer	-0.12	1.52	-4.03	4.16	841
Support free schooling	Afrobarometer	0.55	0.44	0.00	1.00	474
Agree women should have equal rights	Afrobarometer	0.89	0.25	0.00	1.00	477
Agree women should have equal election chances	Afrobarometer	0.91	0.23	0.00	1.00	477
Support CCM	Afrobarometer	0.94	0.24	0.00	1.00	684
Completed primary school	IPUMS	0.67	0.47	0.00	1.00	606938
Kiswahili Literate	IPUMS	0.72	0.45	0.00	1.00	705476
Annual real household consumption, per adult ('000 shs)	TNPS	554.40	427.00	82.30	4072.30	1633
Annual nominal furnishings and household expenditures ('000 shs)	TNPS	56.00	144.00	0.00	3332.00	1633
Main occupation: employed in agriculture	TNPS	0.82	0.38	0.00	1.00	1599
Main occupation: employed in government	TNPS	0.04	0.19	0.00	1.00	1599
Main occupation: employed in private sector	TNPS	0.02	0.15	0.00	1.00	1599
Main occupation: self-employed	TNPS	0.08	0.27	0.00	1.00	1599
Married within same ethnic group (if married, husband)	DHS	0.66	0.47	0.00	1.00	1112
Married within same ethnic group (if married, wife)	DHS	0.70	0.46	0.00	1.00	1169
Married	DHS	0.74	0.44	0.00	1.00	11059

Notes: Data includes observations from cohorts used in the main analysis (born 1948-1971). The capital Dar es Salaam (Mzizima) and the islands Zanzibar, Pemba and Mafia are excluded from all analyses. CCM support in elections is based on election years 2000 and 2005. Data construction and sources are described in detail in Section 3, Table A.2 and Table A.3.

Table 2: The Effect of *Ujamaa* on National Identity, Main Results

	Dependent Variable: National vs. Ethnic Identity							
	no zone-cohort FE	baseline	controlling for treated cohort dummy interacted with					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			distance to capital	district revenue	ethnolinguistic fractionalization	geographical features	hospital beds	weather shocks
Villagization × Treated Cohort	0.165** (0.079)	0.226*** (0.081)	0.217** (0.083)	0.204*** (0.066)	0.208** (0.078)	0.186** (0.070)	0.233** (0.091)	0.207** (0.080)
Observations	849	849	849	849	849	849	849	849
Number of clusters	52	52	52	52	52	52	52	52
R-squared	0.121	0.139	0.140	0.144	0.140	0.146	0.142	0.140
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Zone-Cohort FE		✓	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is an individual Afrobarometer respondent i in district d belonging to school cohort l . All columns regress the measure of an individual's current national identity on the interaction between that individual's district-level measure of historical villagization and a dummy that indicates whether the individual is in the treated cohort, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy, survey year fixed effects, and the other controls and fixed effects indicated in the table. The un-interacted variables are included in the fixed effects in all columns. The dependent variable is from Afrobarometer rounds 3 and 4 (2005-2008), and was originally on a five-level Likert scale; it has been recoded from 0 to 1 in 1/4 increments (1 = respondent identifies only with the nation as a whole; 0 = respondent identifies only with her ethnic group). Treated cohort is a dummy that equals 1 if the respondent was born in 1960-1971. The reference group is the cohort born in 1948-1959. The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. All controls, interacted with the individual-level cohort dummy, are at the district level and based on the 1967 Census (except weather shocks and geographical features). Geographical features include latitude, longitude, altitude and slope. Weather shocks are measured as z-scores of rainfall during the planting season in 1974, 1975 and 1976 (main period of forced relocation), censored at 0 to capture droughts. The weather data is from UC Davis DataLab, HDX, and based on readings from 107 weather stations which are spatially interpolated at the district level using Kriging. The villagization and outcome variables are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. All regressions are weighted using the survey weights provided by Afrobarometer. Standard errors are clustered at the district level and reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The Effect of *Ujamaa* on National Identity, Robustness

	Dependent Variable: National vs. Ethnic Identity											
	baseline	unweighted	control for respondent beliefs about surveyor	rural sample only	dropping districts with highest migration rates	migration bounding exercise extreme case	migration bounding exercise alternative case	pca control	pca control	ordered probit	ordered logit	too young vs too old
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Villagization × Treated Cohort	0.226*** (0.081)	0.179*** (0.060)	0.215*** (0.078)	0.326*** (0.126)	0.224*** (0.088)	0.142*** (0.067)	0.164*** (0.070)	0.224*** (0.081)	0.150*** (0.069)	0.520*** (0.206)	0.861*** (0.400)	0.071 (0.065)
Villagization × 1976-1987 Cohort												
Observations	849	849	849	710	668	849	849	849	785	849	849	936
Number of clusters	52	52	52	47	38	52	52	52	48	52	52	52
R-squared	0.139	0.123	0.144	0.167	0.136	0.133	0.134	0.139	0.135			0.113
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Zone-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is an individual Afrobarometer respondent i in district d belonging to school cohort t . Columns (1) to (11) regress the measure of an individual's current national identity on the interaction between that individual's district-level measure of historical villagization and a dummy that indicates whether the respondent is in the treated cohort, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy, survey year fixed effects, and the fixed effects indicated in the table. The un-interacted variables are included in the fixed effects in all columns. Column (12) uses the same specification, except that the treated cohort dummy takes value 1 for the 1976-1987 birth cohort. The dependent variable is from Afrobarometer rounds 3 and 4 (2005-2008), and was originally on a five-level Likert scale; it has been recoded from 0 to 1 in 1/4 increments (1 = respondent identifies only with the nation as a whole; 0 = respondent identifies only with her ethnic group). Treated cohort is a dummy that equals 1 if the respondent was born in 1960-1971. In all columns, the reference group is the cohort born in 1948-1959. The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The villagization and outcome variables are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation, except in columns (10) and (11) in which the outcome is left discrete, not standardized. All regressions are weighted using the survey weights provided by Afrobarometer, except column (2) which does not apply the survey weights. Column (3) controls for a dummy variable that indicates whether the respondent erroneously believes that the Afrobarometer surveyor was sent by a Tanzanian government entity. Column (4) includes only rural respondents. Column (5) drops all districts with either an in- or an out-migration rate in the highest deciles. Columns (6) and (7) are explained in [Appendix B](#). Column (8) includes also the first principal component of all controls added in columns (3) to (8) of table 2 and columns (4) to (10) of table A.5. Column (9) includes the first principal component obtained including also national identity in 1967. Column (10) uses an ordered probit regression. Column (11) uses an ordered logit regression. The displayed coefficient shows the differential change in the ordered log-odds of reporting a higher level of the outcome variable, given a one-standard deviation increase in the villagization measure, for the treated cohort relative to the control one. The interpretation of the coefficient in column (8) is similar, but in a different scale as it is expressed in units of the probit index. Column (12) compares the 1976-1987 cohort to the 1948-1959 one, using only observations from these cohorts. Standard errors are clustered at the district level and reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The Effect of *Ujamaa* on Intermarriage

Dep. Variable:	Married Same Ethnicity		Married (3)	Married Same Ethnicity	
	Husband Cohort (1)	Wife Cohort (2)		Husband Cohort (4)	Wife Cohort (5)
Villagization × Treated Cohort	-0.061* (0.033)	-0.027 (0.024)	0.008 (0.010)	-0.047 (0.049)	-0.161** (0.067)
Villagization × Age Difference				0.003 (0.004)	-0.009* (0.005)
Treated Cohort × Age Difference				0.022 (0.018)	0.005 (0.011)
Villagization × Treated Cohort × Age Difference				0.001 (0.007)	0.014** (0.005)
Age Difference				-0.009** (0.004)	-0.000 (0.004)
Observations	1,112	1,169	11,059	1,112	1,169
Number of clusters	52	52	52	52	52
R-squared	0.106	0.082	0.032	0.112	0.088
District FE	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓
Zone-Cohort FE	✓	✓	✓	✓	✓
Sample	Married Couples	Married Couples	Individuals	Married Couples	Married Couples

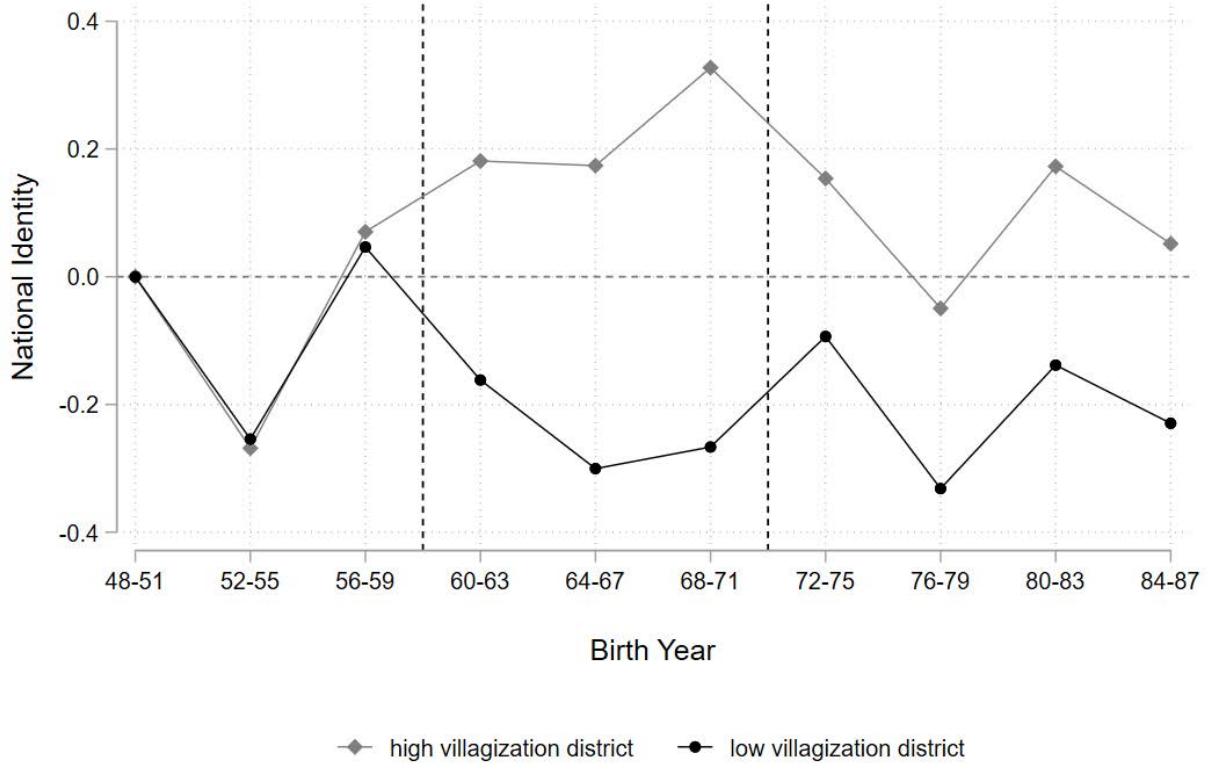
Notes: The unit of observation in columns (1), (2), (4) and (5) is a married couple i of DHS respondents in district d , school cohort t is assigned based on the husband's birth year in columns (1) and (4) and the wife's birth year in columns (2) and (5). In these columns, the dependent variable is a dummy for whether the spouses are of the same ethnic group. In column (3) the unit of observation is a DHS respondent i in district d and school cohort t and the dependent variable is a dummy for whether the respondent is currently married. In columns (1) to (3) the dependent variable is regressed on the interaction between that individual's district-level measure of historical villagization and a dummy that indicates whether the individual is in the treated cohort, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy, survey year fixed effects, and the fixed effects indicated in the table. In columns (4) and (5) the dependent variable is regressed on the interaction between that observation's district-level measure of historical villagization and a dummy that indicates whether the observation is in the treated cohort, and the absolute value of the age difference between the husband and the wife, all double and triple interactions between villagization, cohort and age difference (that are not included in the fixed effects), controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy and age difference as well as the corresponding double interactions, survey year fixed effects, and the fixed effects indicated in the table. The variables varying at the district- or cohort-level only are included in the fixed effects in all columns. Treated cohort is a dummy that equals 1 if the respondent was born in 1960-1971. The reference group is the cohort born in 1948-1959. The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The villagization measure are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. Standard errors are clustered at the district level and reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: The Effect of *Ujamaa*: Heterogeneity by Ethnolinguistic Fractionalization, Primary School Completion and Gender

Dep. Variable:	National Identity		Married Same Ethnicity		National Identity		Married Same Ethnicity		National Identity			
	(1)	(2)	Husband Cohort	Wife Cohort	(3)	(4)	Husband Cohort	Wife Cohort	(5)	(6)	Males	Females
											(7)	(8)
Villagization × Treated Cohort	0.206** (0.101)	-0.060* (0.031)	-0.034 (0.028)	-0.121 (0.147)	-0.073 (0.062)	-0.065* (0.038)	0.267** (0.130)	0.129 (0.095)				
Treated Cohort × ELF	0.245 (0.260)	0.103 (0.080)	-0.070 (0.081)									
Villagization × Treated Cohort × ELF	0.027 (0.101)	0.028 (0.044)	0.017 (0.048)									
Villagization × Completed School				-0.111 (0.110)	0.040 (0.051)	0.046 (0.081)						
Treated Cohort × Completed School				0.050 (0.478)	0.101 (0.211)	0.012 (0.199)						
Villagization × Treated Cohort × Completed School				0.331* (0.188)	-0.022 (0.054)	-0.051 (0.087)						
Completed School				-0.049 (0.217)	-0.108* (0.061)	-0.188** (0.083)						
Observations	849	1,112	1,169	724	842	949	453	394				
Number of clusters	52	52	52	52	52	52	50	51				
R-squared	0.142	0.112	0.083	0.171	0.120	0.105	0.227	0.185				
District FE	✓	✓	✓	✓	✓	✓	✓	✓				✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓				✓
Zone-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓				✓
Source	Afrobarometer		DHS	Afrobarometer	DHS	DHS	Afrobarometer	DHS	Afrobarometer	Afrobarometer	Afrobarometer	Afrobarometer

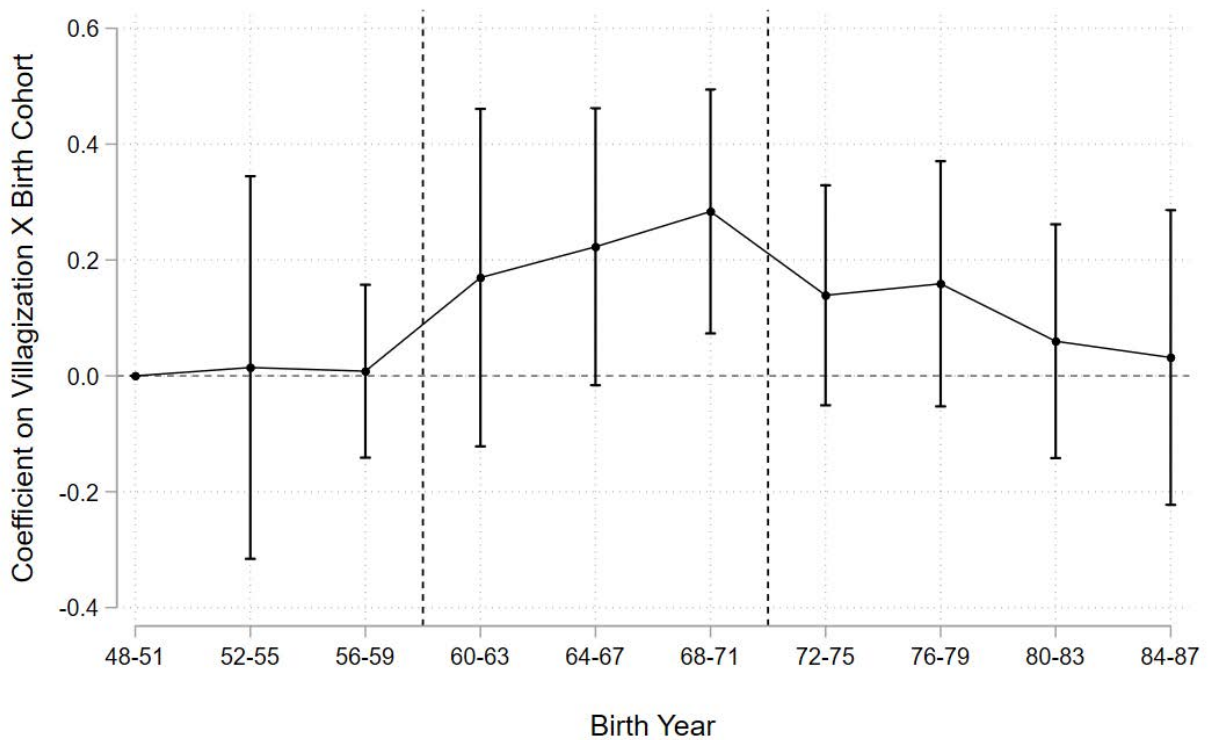
Notes: The unit of observation in columns (1) and (4) is an individual Afrobarometer respondent i in district d belonging to school cohort t and the outcome is a measure of an individual's current national identity. In columns (7) and (8) this sample is restricted to male and female respondents, respectively. In the remaining columns, the unit of observation is a married couple i of DHS respondents in district d , school cohort t is assigned based on the husband's birth year in columns (2) and (5) and the wife's birth year in columns (3) and (6), and the outcome is a dummy for whether the spouses are of the same ethnic group. In columns (1) to (3) the dependent variable is regressed on the interaction between that observation's district-level measure of historical villagization, a dummy that indicates whether the observation is in the treated cohort, and the ELF measure, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy and the ELF measure as well as the corresponding double interactions, survey year fixed effects, and the fixed effects indicated in the table. In columns (4) to (6) the dependent variable is regressed on the interaction between that observation's district-level measure of historical villagization and a dummy that indicates whether the observation is in the treated cohort, a dummy that indicates whether the husband, for column (5), and the wife, for column (6), ever completed formal primary schooling, all double and triple interactions between villagization, cohort, and schooling (that are not included in the table). Columns (7) and (8) regress the measure of an individual's current national identity on the interaction between that individual's district-level measure of historical villagization and a dummy that indicates whether the respondent is in the treated cohort, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy and the dummy for primary school completion as well as the corresponding double interactions, survey year fixed effects, and the fixed effects indicated in the table. The national identity outcome is from Afrobarometer rounds 3 and 4 (2005-2008), and was originally on a five-level Likert scale; it has been recoded from 0 to 1 in 1/4 increments (1 = respondent identifies only with the nation as a whole; 0 = respondent identifies only with her ethnic group). Treated cohort is a dummy that equals 1 if the respondent was born in 1960-1971. The reference group is the cohort born in 1948-1959. The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The villagization, ELF, and the measure of national identity are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. Regressions in columns (1), (4), (7) and (8) are weighted using the survey weights provided by Afrobarometer. Standard errors are clustered at the district level and reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: National Identity by Cohort and Villagization



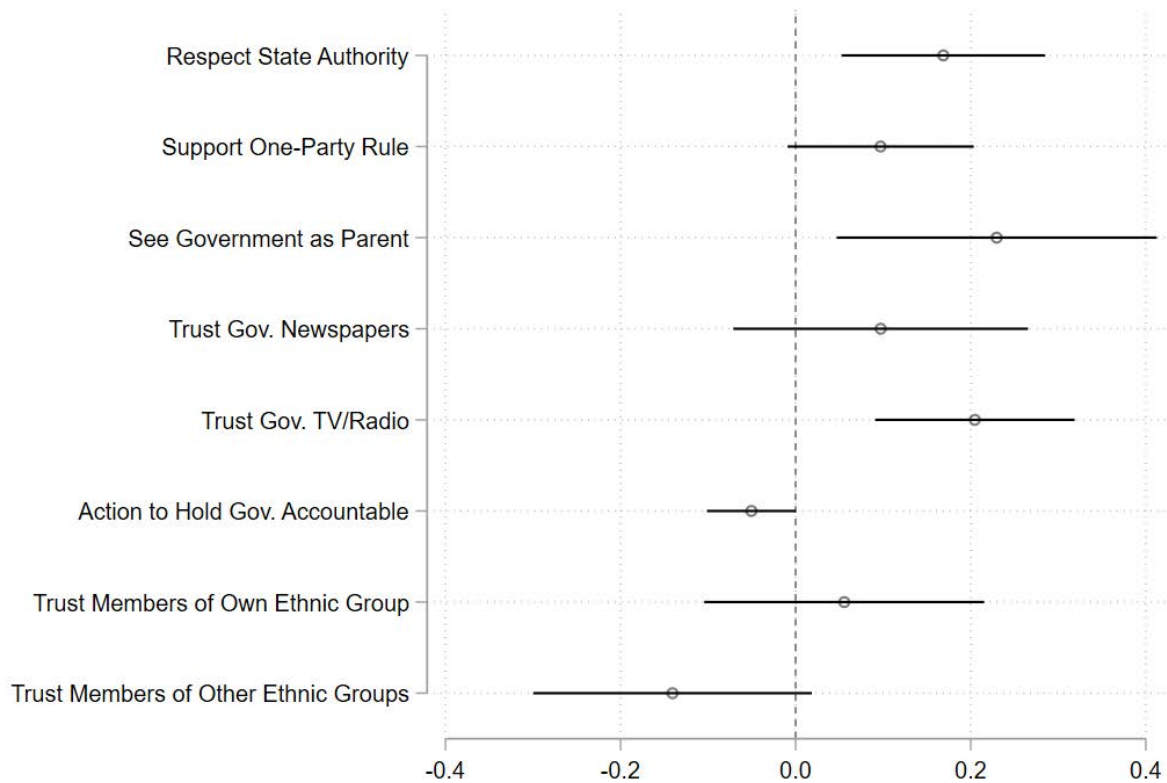
Notes: The unit of observation is an individual Afrobarometer respondent i in district d belonging to birth cohort t . Each birth cohort includes four years between 1948 and 1987 (1948-1951, 1952-1955, ...). The dashed lines indicate the first and last birth cohorts that were of 4th grade age (10 years old) during the villagization period (1970-1981). The gray (black) line plots the coefficients from a regression of the measure of an individual's current national identity on birth cohort dummies among high (low) villagization districts (high = above or equal to sample median, low = below sample median). The two regressions include no fixed effects or controls. The estimates are relative to the 1948-1951 birth cohort, which is the omitted category. The dependent variable is from Afrobarometer rounds 3 and 4 (2005-2008), and was originally on a five-level Likert scale; it has been recoded from 0 to 1 in 1/4 increments (1 = respondent identifies only with the nation as a whole, 0 = respondent identifies only with her ethnic group). The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The outcome variable is standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. The regressions are weighted using the survey weights provided by Afrobarometer.

Figure 2: The Effect of *Ujamaa* on National Identity, All Cohorts



Notes: The unit of observation is an individual Afrobarometer respondent i in district d belonging to birth cohort t . Each birth cohort includes four years between 1948 and 1987 (1948-1951, 1952-1955, ...). The dashed lines indicate the first and last birth cohorts that were of 4th grade age (10 years old) during the villagization period (1970-1981). The thick line plots the coefficients from a regression of the measure of an individual's current national identity on the interaction between that individual's district-level measure of historical villagization, dummies that indicate whether the individual is in each of the cohorts shown, controlling for the 1967 district primary school enrollment rate interacted with the birth cohort dummies, survey year fixed effects, 1967 district fixed effects, birth cohort fixed effects and zone-cohort fixed effects. The vertical solid lines show 95% confidence intervals based on robust standard errors clustered at the district level. The estimates are relative to the 1948-1951 birth cohort, which is the omitted category. District and cohort fixed effects as well as all baseline controls (1967 district primary school enrollment rate interacted with cohort fixed effects, zone-cohort fixed effects, survey year fixed effects,) are included. The un-interacted variables are included in the fixed effects. The dependent variable is from Afrobarometer rounds 3 and 4 (2005-2008), and was originally on a five-level Likert scale; it has been recoded from 0 to 1 in 1/4 increments (1 = respondent identifies only with the nation as a whole, 0 = respondent identifies only with her ethnic group). The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The villagization and outcome variables are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. The regression is weighted using the survey weights provided by Afrobarometer.

Figure 3: The Effects of *Ujamaa* on State Legitimacy and Accountability



Notes: The unit of observation is an individual Afrobarometer respondent i in district d belonging to school cohort t . The dots show the coefficients from regressions of the outcome stated on the left on the interaction between an individual's district-level measure of historical villagization and a dummy that indicates whether that individual is in the treated cohort, controlling for the 1967 district primary school enrollment rate interacted with the cohort dummy, survey year fixed effects, 1967 district fixed effects, the cohort dummy and zone-cohort fixed effects. The un-interacted variables are included in the fixed effects in all regressions. The dependent variables are from Afrobarometer rounds 3 and/or 4 (2005-2008) and are as follows (recoded from Likert scale). (1) "Choose Statement 1 or Statement 2. Do you agree or agree very strongly? Statement 1: Citizens should be more active in questioning the actions of leaders. Statement 2: In our country, citizens should show more respect for authority." The dependent variable registers agreement with Statement 2.; (2) "There are many ways to govern a country. Would you disapprove or approve of the following alternatives?. Only one political party is allowed to stand for election and hold office." The dependent variable registers approval with this option.; (3) "Choose Statement 1 or Statement 2. Do you agree or agree very strongly? Statement 1: People are like children; the government should take care of them like a parent. Statement 2: Government is like an employee; the people should be the bosses who control the government." The dependent variable registers agreement with Statement 1.; (4) "How much do you trust each of the following, or haven't you heard enough about them to say: Government (relative to independent) newspapers?"; (5) "Government (relative to independent) broadcasting service (TV / radio)?""; (6) "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year" Got together with others to raise an issue"; (7) "How much do you trust each of the following types of people: "People from your own ethnic group?"; (8) "People from other ethnic groups?". Questions (4), (5), (7) and (8) were asked only in Round 3, question (3) only in Round 4. Treated cohort is a dummy that equals 1 if the respondent was born in 1960-1971. The reference group is the cohort born in 1948-1959. The villagization variable is the share of the respondent's current district's rural population that lived in official government villages by 1978 according to the 1978 Tanzania Population Census. The villagization and outcome variables, except outcome (6) which is binary, are standardized to have a mean of 0 and a standard deviation of 1 to facilitate interpretation. All regressions are weighted using the survey weights provided by Afrobarometer. Standard errors are clustered at the district level. The horizontal bars show 90% confidence intervals.