American Economic Review 101 (December 2011): 3221–3252
http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.7.3221

Note: for the purposes of Assignment 4, you can skip the parts highlighted in pink.

The Slave Trade and the Origins of Mistrust in Africa[†]

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We show that current differences in trust levels within Africa can be traced back to the transatlantic and Indian Ocean slave trades. Combining contemporary individual-level survey data with historical data on slave shipments by ethnic group, we find that individuals whose ancestors were heavily raided during the slave trade are less trusting today. Evidence from a variety of identification strategies suggests that the relationship is causal. Examining causal mechanisms, we show that most of the impact of the slave trade is through factors that are internal to the individual, such as cultural norms, beliefs, and values. (*JEL* J15, N57, Z13)

In a recent study, Nunn (2008) examines the long-term impacts of Africa's slave trade. He finds that the slave trade, which occurred over a period of more than 400 years, had a significant negative effect on long-term economic development. Although the article arguably identifies a negative causal relationship between the slave trade and income today, the analysis is unable to establish the exact causal mechanisms underlying this reduced-form relationship.

In this article, we examine one of the channels through which the slave trade may affect economic development today. Combining contemporary individual-level survey data with historical data on slave shipments by ethnic group, we ask whether the slave trade caused a culture of mistrust to develop within Africa. Initially, slaves were captured primarily through state organized raids and warfare, but as the trade progressed, the environment of ubiquitous insecurity caused individuals to turn on others—including friends and family members—and to kidnap, trick, and sell each other into slavery (Sigismund Wilhelm Koelle 1854; P. E. H. Hair 1965; Charles Piot 1996). We hypothesize that in this environment, a culture of mistrust may have evolved, which may persist to this day.

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[†] To view additional materials, visit the article page at http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.7.3221.

Our hypothesis builds on the well-established insight from cultural anthropology that in environments where information acquisition is either costly or imperfect, the use of heuristic decision-making strategies, or "rules-of-thumb," can be optimal (Robert Boyd and Peter J. Richerson 1985; Boyd and Richerson 1995). These general rules or beliefs about the "right" action in different situations save the individual from the costs of acquiring information. Of course, these rules-of-thumb do not develop in a vacuum but rather evolve according to which yield the highest payoff. Our hypothesis is that in areas heavily exposed to the slave trade, norms of mistrust towards others were likely more beneficial than norms of trust, and therefore they would have become more prevalent over time. If these beliefs and norms persist, then the relationship between such norms and the history of the slave trade may still be felt today—almost 100 years after the slave trade has ended.

To test our hypothesis, we use data from the 2005 Afrobarometer survey to examine whether individuals belonging to ethnic groups that were heavily targeted by the slave trade in the past are less trusting of others today. We find that individuals belonging to ethnic groups that were most exposed to the slave trades exhibit lower levels of trust in their relatives, neighbors, coethnics, and local government today. This finding is consistent with the historical fact that by the end of the slave trade, it was not uncommon for individuals to be sold into slavery by neighbors, friends, and family members.

An alternative explanation for our finding is that more slaves were supplied by ethnic groups that initially were less trusting, and that these lower levels of trust continue to persist today. Alternatively, there may be other historical events, such as formal colonial rule, that are correlated with the severity of the slave trade and subsequent levels of trust. We pursue a number of strategies to determine whether the correlations we uncover are, in fact, causal.

Our first strategy is to control for other forms of European influence, most notably the period of formal colonial rule that followed the slave trade. We also control for certain precolonial characteristics of ethnic groups, including initial prosperity and political development. We find that controlling for these observable characteristics has little effect on the estimated effect of the slave trade on trust.

Our second strategy is to use the recent insights from Joseph G. Altonji, Todd E. Elder, and Christopher R. Taber (2005) to calculate how much greater the influence of unobservable factors would need to be, relative to observable factors, to completely explain away the negative relationship between the slave trade and trust. We find that the influence of unobservable factors would have to be between three and 11 times greater than observable factors. Therefore, it is unlikely that our estimates can be fully attributed to unobserved heterogeneity.

Our third strategy uses the distance of ethnic groups from the coast at the time of the slave trade as an instrument for the number of slaves taken. The unique history of sub-Saharan Africa provides a basis for the instrument's exogeneity. Prior to the transatlantic and Indian Ocean slave trades, Africans were not engaged in overseas external trade. Therefore, it is unlikely that closer proximity to the ocean had an impact on trust, other than through the slave trade. Because distance from the coast may be correlated with access and involvement in the early trade across the Sahara Desert, we control directly for distance to Saharan routes and the cities involved in the Saharan trade. We also control for a number of measures of European influence,

and for the historical dependence on fishing, both of which are potentially correlated with distance from the coast. The IV regressions produce estimates that are qualitatively identical to the OLS estimates.

To address the concern of whether the exclusion restriction is satisfied, we perform a number of falsification tests that examine the reduced-form relationship between distance from the coast and trust inside and outside of Africa. Within Africa, we find a strong positive relationship between distance from the coast and trust. This is expected, given our IV estimates. Places farther from the coast had fewer slaves taken, and therefore exhibit higher levels of trust today. If distance from the coast affects trust only through the slave trade (i.e., if our exclusion restriction is satisfied), then there should be no relationship between distance from the coast and trust outside of Africa, where there was no slave trade. This is exactly what we find. Looking at samples from Asia and Europe, we estimate a statistically insignificant relationship between distance from the coast and trust.

After establishing that the slave trade adversely affected trust, we turn to the task of distinguishing between channels of causality. One mechanism, which is the article's focus, is that the slave trade altered the cultural norms of the ethnic groups exposed to it, making them less trusting of others. However, there is also a second potential channel. Because the slave trade resulted in a long-term deterioration of legal and political institutions, the residents of heavily affected regions may now be able to cheat others more easily. Individuals may be less trusting today because those around them are less trustworthy.

We undertake three exercises to identify the relative importance of these channels. First, we consider the determinants of respondents' trust in their local government and examine how the estimated effect of the slave trade changes when we control for measures of individuals' perceptions about the trustworthiness of their local government. After doing this, the estimated coefficient for slave exports decreases by slightly less than 50 percent but remains precisely estimated, highly significant, and very stable across specifications.

In the second exercise, rather than controlling for perceived trustworthiness, we directly control for the effects of the slave trade on the trustworthiness of others. We again estimate the determinants of intergroup trust, but this time we control directly for the impact of the slave trade on the other ethnic groups living in the same location as the respondent. Our estimates show that ethnic groups whose ancestors were heavily enslaved in the past are less trusted today. This is consistent with the slave trade's adversely affecting the trustworthiness of individuals today. We find that the estimated effect of the slave trade on internal norms of trust remains robust, even after we control for the effect of the slave trade on the trustworthiness of others.

Our final strategy decomposes the effect of the slave trade into two channels: its effect on factors internal to the individual, and its effect on factors external to the individual. We do this by constructing a second measure of slave exports: the average number of slaves taken from the geographic location where the individual lives today. This is different from our baseline measure, which is the average number of slaves taken from an individual's ethnic group. The logic behind including both measures in our equation derives from the fact that when individuals relocate, their internal beliefs move with them, even though their external environment changes. Therefore, the two variables distinguish between the effects of the slave trade on trust

working through internal factors that are geographically mobile—such as individuals' internal beliefs and values—versus through external factors that are less geographically mobile, including political, legal, institutional, and social structures. If the slave trade primarily affects trust through internal beliefs and values, then across individuals, what should matter is whether their ancestors were heavily affected by the slave trade. If instead the slave trade affects trust primarily through its deterioration of institutions, social structures, or other factors external to the individual, then what should matter is whether the individual's external environment was heavily affected by the slave trade. Our estimates show that both channels are important, but that the internal channel is at least twice as large as the external channel.

These results complement recent studies documenting the importance of trust: for economic development (Stephen Knack and Philip Keefer 1997; Marcel Fafchamps 2006; Guido Tabellini 2007; Yann Algan and Pierre Cahuc 2010); for international trade (Avner Greif 1989; Luigi Guiso, Paola Sapienza, and Luigi Zingales 2007a); for political institutions (Robert Putnam 2000); and for firm management practices (Nicholas Bloom, Raffaella Sadun, and John Van Reenen 2008). Given the mounting evidence of the importance of trust, our contribution here is in helping to understand and explain its origins. Our evidence most directly complements those few studies that also consider the historical determinants of differences in cultural norms of behavior, such as Guiso, Sapienza, and Zingales's (2007b) study that empirically links differences in social capital within Italy to the independence of cities during the eleventh to fourteenth centuries. Our work also complements Tabellini's (2007) study which shows that the levels of education and democracy in eighteenth century Europe are important determinants of interpersonal trust today.

Our focus on the long-term historical determinants of cultural norms is not meant to suggest that short-run determinants are unimportant. There is substantial evidence that nonhistorical determinants of trust—for example income, education, information flows, organization membership, and current experiences—are also important (Raymond Fisman and Tarun Khanna 1999; Alberto Alesina and Eliana La Ferrara 2002; John Bellows and Edward Miguel 2008).

In Section I, we begin our analysis by first laying out the historical and conceptual groundwork. We discuss the theoretical literature that seeks to understand how and why norms evolve, as well as historical literature describing the slave trade and the environment of insecurity that it generated. In Section II, we turn to a description of the data, before reporting our estimates in Sections III and IV. In Section V, we examine specific mechanisms and test whether the slave trade affects trust through internal cultural norms or through societies' institutional and legal structures, which in turn affect the trustworthiness of its citizens. Section VI concludes.

I. Historical Background and Conceptual Framework

A. *Historical Background*

Early in the slave trade, nearly all slaves were taken in large-scale conflicts or raids, which created an environment of extreme insecurity outside of the local community (Mario Azevedo 1982; Andrew Hubbell 2001; Joseph E. Inikori 2000; Martin Klein 2001). Ironically, this in turn caused insecurity within communities, as individuals

began to turn on others close to them, including neighbors, friends, and even family. Unlike most other environments of conflict and insecurity, the slave trade had one unique feature: individuals could partially protect themselves by turning against others within their community.[1] By engaging in trickery, local kidnappings, or other forms of small scale violence, one could exchange slaves (with Europeans, or slave merchants) for guns and iron weapons (Abdullahi Mahadi 1992; Walter Hawthorne 1999). Slave merchants and raiders also played a role in promoting internal conflict, often forming strategic alliances with key groups within villages and states in order to extract slaves (Boubacar Barry 1992; Inikori 2003; Klein 2003).

In his book *Planting Rice and Harvesting Slaves*, Walter Hawthorne documents the decentralized and interpersonal nature of slave capture among the Beafares of the Guinea-Bissau region of Africa. He writes that "the Atlantic slave trade was insidious because its effects penetrated deep into the social fabric of the Upper Guinea Coast—beyond the level of the state and to the level of the village and household … Hence, in many areas, the slave trade pitted neighbor against neighbor" (Hawthorne 2003, pp. 106–107). Hawthorne provides a particularly telling example, taken from André Alvarez d'Almada (1984). Households located near ports were able to profit from the slave trade by "tricking" unsuspecting strangers and then selling them to merchants. Almada writes that "these Beafares are so smart, that if a yokel arrives from the interior, they pretend that they want to give him shelter, and they receive him into their homes. After a few days have passed, they persuade him that they have friends on the ships, and that they would like to take him and have a party. But when they go to the ships, they sell him. In this way they trick many yokels" (Almada 1984, p. 121).[2]

Data on the manner of enslavement in the nineteenth century confirm the descriptive evidence: by the end of the slave trade, individuals entered slavery in a variety of ways, including by being sold into slavery by acquaintances, friends, and family. During the 1840s, German missionary and linguist Sigismund Koelle (1854) collected information on the manner of enslavement of 144 former slaves living in Free Town, Sierra Leone. In his sample, the most common manner of enslavement was kidnapping, with just over 40 percent of the slaves being taken in this manner. Just under 25 percent of the slaves were captured during wars. Amazingly, almost 20 percent of the slaves were sold by relatives or friends. Koelle's interviews document numerous accounts of individuals being sold into slavery by family members, relatives, and "supposed friends." One of the more notable accounts is of a slave who was sold into slavery after being "enticed on board of a Portuguese vessel" by "a treacherous friend." Another example is the custom of the Kabre (from Northern Togo), developed during the nineteenth century, of selling their own kin into slavery (Piot 1996). The final process of enslavement was through the judicial system. Sixteen percent of the Koelle sample entered slavery this way. This form of enslavement includes the historically common practice of accusing others of crimes such as witchcraft, theft, adultery, or murder in order to obtain slaves. If found guilty, the

[1] One environment that arguably featured a similar dynamic to the slave trade was communist East Germany during the Cold War. Because of the strategies employed by the Stasi, an individual's best option often was to become an informant, turning on others, even those close to him or her (Marcus Jacob and Marcel Tyrell 2010).

[2] Also see the discussion in Hawthorne (2003).

accused (and often their family) were sentenced to slavery (Paul E. Lovejoy 2000). It even became common for the leaders of local communities to obtain slaves in this manner (Hawthorne 1999, 2003; Klein 2001).

The fact that slaves often were taken or tricked into slavery by individuals close to them suggests that the slave trade may have eroded trust even in the most intimate social relationships. Furthermore, because chiefs often were slave traders, or were forced to sell their own people into slavery, the slave trade also may have engendered a mistrust of political figures, particularly local leaders. Our analysis tests for these effects, examining whether individuals whose ancestors were most heavily threatened during the slave trade have less trust in their family, neighbors, and their local government council.

B. *Conceptual Framework*

Our notion of culture is taken from research in evolutionary anthropology that views culture as decision-making rules-of-thumb employed in uncertain or complex environments. Using theoretic models, Boyd and Richerson (1985, 2005) show that if information acquisition is either costly or imperfect, it can be optimal for individuals to develop heuristics or rules-of-thumb in decision making. By relying on general beliefs about the "right" thing to do in different situations, individuals may not behave in a manner that is optimal in every instance, but they save on the costs of obtaining the information necessary to always behave optimally. In these models, different behavioral rules evolve through a process of natural selection determined by the relative payoffs from different rules-of-thumb. Within this framework, the hypothesis we test is whether the environment of insecurity caused by the slave trade increased the returns to rules-of-thumb based on mistrust relative to rules-of-thumb based on trust, thus causing a culture of greater mistrust to develop.

The natural question that we face, though, is why we expect to find evidence of increased mistrust among the descendants of those exposed to the slave trade 100 years after its end. One explanation for the persistence of mistrust can be found in the models developed by Guiso, Sapienza, and Zingales (2007b) and Tabellini (2008); they show how multiple equilibria in cultural outcomes can arise. The long-run effects that we find may be the result of permanent movements to equilibria characterized by high levels of mistrust among the lineages most affected by the slave trade.

Another explanation for the persistence of mistrust is rooted in the existence of complementarities between cultural norms and domestic institutions. This phenomenon is highlighted by the model developed in Tabellini (2008). In the model, individuals inherit norms of cooperation from their parents and make political choices (through voting) that determine the quality of domestic institutions. Through this mechanism, norms of cooperation will affect the equilibrium quality of domestic institutions. When there is a negative shock to internal norms of cooperation, the next generation will not only be less trusting, but also will choose institutions with weaker enforcement, resulting in poor behavior and low levels of trust among future generations. This creates a self-enforcing outcome by which low levels of trust and weak institutions persist among future generations. When applied to Africa, the

model suggests that areas with low levels of trust have developed weaker institutions, and the weaker institutions in turn have resulted in worse behavior and still lower levels of trust. These societies remain trapped in an equilibrium of uncooperative behavior, mistrust, and inefficient institutions.

It is also possible that our results arise not because African societies remain trapped in low trust equilibria, but because the shocks caused by the slave trade—an event lasting for over 400 years—have not yet fully dissipated. This explanation is consistent with the dominant presumption that cultural change occurs slowly (e.g., Alberto Bisin and Thierry Verdier 2000; Bisin and Verdier 2001, 2008). Rare evidence of the speed of cultural change is provided by Alesina and Nicola Fuchs-Schündeln's (2007) study of the effects of the division of Germany between 1945 and 1990 on individuals' beliefs about the benefits of redistribution and government intervention. They find that East Germans view government intervention more favorably than West Germans and that since reunification the beliefs of East Germans have begun to slowly converge to those of West Germans. Although this particular shock lasted only 45 years, the authors estimate that the differences generated by the shock will take 20–40 years to diminish to zero. In that case, the effects of the shock will have persisted for nearly as long as the shock itself. In our study, the negative shock lasted well over 400 years, and we are examining its effects just over 100 years later. Therefore, it is possible that the effects of the shock on mistrust are still being felt today, even if they are actually temporary and ultimately will die out in the long run.

Additional evidence can also be gleaned from the experiments of Karla Hoff, Mayuresh Kshetramade, and Ernst Fehr (2009), which show that the legacy of the caste system within India continues to manifest itself over half a century after its abolition, and that this finding is robust to the inclusion of controls for education and wealth. They find that individuals from low castes are less willing to sanction violations of a cooperation or reciprocity norm than individuals from a high-caste background.[3]

A fourth explanation is also possible. The mechanism may not be the result of cultural learning, as in the previous three explanations. Instead, it is (at least theoretically) possible that the persistent change in cultural norms arises because a greater number of inherently more trusting individuals were captured and shipped from the continent, leaving a greater proportion of inherently less trusting individuals. Therefore, even without any cultural learning, the slave trade could still have had a large effect on mistrust today. This explanation requires that mistrust be an inherent or hard-wired characteristic, which would occur, for example, if trust were genetically determined.[4]

Our analysis is not able to distinguish between these finer transmission mechanisms. Instead, our aim and intended contribution is to empirically estimate the long-term causal effects of the slave trade on trust, and to identify the extent to which this relationship arises because of the slave trade's effects on norms, beliefs,

[3] For related evidence of the cultural legacy of the caste system in India and its potential effects on trust, see Hoff and Priyanka Pandey (2005).

[4] A number of recent studies provide suggestive evidence that genetics may be a determinant of trust, as well as other behavioral characteristics. See, for example, David Cesarini et al. (2008).

and rules-of-thumb, all of which are internal to the individual and transmitted from parents to children over time.

II. Data Sources and Description

A. *Afrobarometer Data*

The individual-level data are from the 2005 Afrobarometer surveys. These nationally representative surveys are based on interviews conducted in the local languages of a random sample of either 1,200 or 2,400 individuals of voting age in each country. The surveys cover 17 sub-Saharan African countries: Benin, Botswana, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe. The sample countries are concentrated in West Africa, Eastern Africa, and Southern Africa. West Central Africa is not included (e.g., Cameroon, Gabon, Democratic Republic of Congo, Angola), nor are countries inland of the Red Sea (e.g., Sudan, Ethiopia, Eritrea).

From the surveys we have a potential sample of 21,822 respondents. Within this sample, 120 of the respondents: (i) list "other" as their ethnicity; (ii) list their country as their ethnicity; (iii) belong to an ethnic group that is not an indigenous Africa ethnicity; or (iv) list an indigenous ethnicity that could not be matched cleanly to the slave trade data. Removing these observations leaves us with 21,702 potential observations.

The Afrobarometer asks respondents how much they trust their relatives, neighbors, and their locally elected government council. It also asks about trust of those in the same country from other ethnic groups, and those from the same ethnic group. The respondents choose between four possible answers: not at all, just a little, somewhat, or a lot. The distributions of responses for each question are reported in the paper's online Appendix. Not surprisingly, the data show that individuals exhibit more trust in those closer to them than in those further from them. Despite this, 7 percent still report that they do not trust their relatives at all, and 18 percent report that they trust their relatives only a little.

Since respondents' answers to the trust questions are categorical, there are a number of possible estimation strategies. The first is to convert the categorical responses into a variable that assigns a number to each response. Following this strategy, we construct a measure of trust that takes on the value of 0, 1, 2, or 3: 0 corresponds to the response "not at all"; 1 to "just a little"; 2 to "somewhat"; and 3 to the response "a lot." (These are the numeric values assigned to each answer in the Afrobarometer survey.) An alternative strategy is to maintain the categorical nature of the answers and instead estimate an ordered logit model. As we discuss below, the estimates are qualitatively identical if we pursue this alternative strategy.

B. *Ethnicity-Level Data on Slave Exports*

The estimates of the number of slaves taken from each ethnic group rely on country-level slave export figures from Nunn (2008). They were constructed by

combining data on the total number of slaves shipped from all ports and regions of Africa with data on the slaves' ethnic identities. The country-level estimates cover Africa's four slave trades (the transatlantic, Indian Ocean, Red Sea, and trans-Saharan) between 1400 and 1900.[5]

We disaggregate the country-level slave export figures to the ethnicity level using the same ethnicity samples as Nunn (2008). Since only two of the four slave trades—the transatlantic and Indian Ocean—have ethnicity data detailed enough to construct reliable estimates of the number of slaves taken from each ethnicity, our empirical analysis is restricted to the transatlantic and Indian Ocean slave trades. Since the transatlantic slave trade was by far the largest of the slave trades, the omission of the Red Sea and trans-Saharan slave trades likely will have little impact. Nunn (2008) has previously shown that the impact of the slave trades as a whole is driven almost solely by the transatlantic slave trade. Furthermore, the countries that were most affected by the Red Sea and trans-Saharan slave trades—namely Sudan, Ethiopia, and Chad—are not included in the Afrobarometer sample. We also show that our results are robust to omitting observations from the two countries in our sample—Kenya and Mali—that shipped a significant number of slaves during either the trans-Saharan or Red Sea slave trades.

For the transatlantic slave trade, Nunn (2008) has collected a sample of 80,656 slaves whose ethnic identity is known. The aggregate sample comprises 54 different samples that report 229 distinct ethnic designations. For the Indian Ocean slave trade, Nunn's aggregate sample has 21,048 slaves, covering 80 distinct ethnic groups.

One important step in estimating the number of slaves taken from each ethnic group is matching the ethnic identities in the historical records to the ethnic classification in the Afrobarometer surveys. We achieve this by first linking the original ethnic groups to a classification that is constructed and mapped by George Peter Murdock (1959).[6] Since Murdock's classification is similar to the one used in the Afrobarometer surveys, it is easy to move from his classification to the Afrobarometer data.

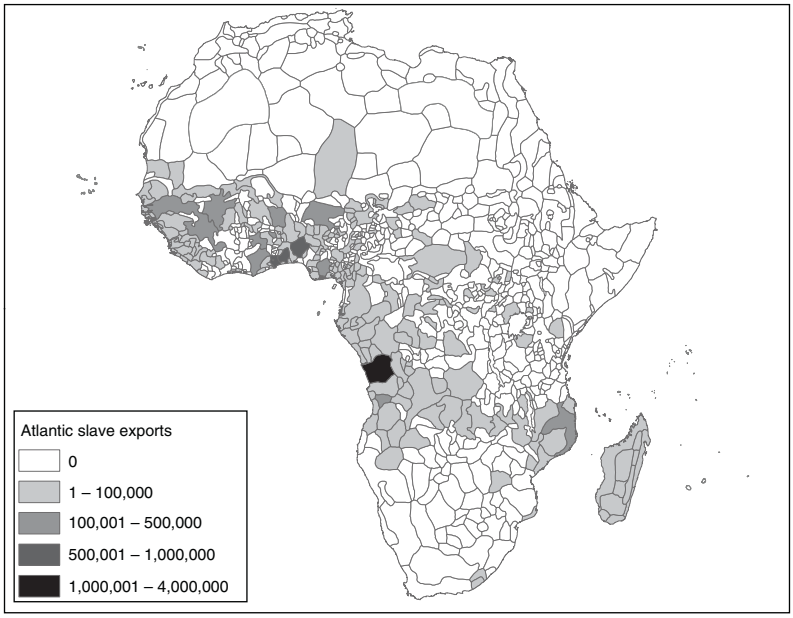
Figures 1A and 1B map the historical boundaries (in the late nineteenth century) according to Murdock (1959). To provide a visual representation of the spatial distribution of each slave trade, we have grouped total slave exports between the years 1400 and 1900 into five broad categories, and we denote greater numbers of slaves shipped with darker shades.

As Figure 1A shows, the transatlantic slave trade affected much of the African continent. Slaves were not only taken from West Africa and West-Central Africa, but also from Eastern Africa and Madagascar. The much smaller Indian Ocean slave trade was confined primarily to Eastern Africa (see Figure 1B). These patterns of enslavement are consistent with the qualitative evidence on the sources of slaves taken during the two slave trades (e.g., Patrick Manning 1990; Lovejoy 2000).

[5] Full details of the underlying data, their sources, and the construction procedure are provided in Nunn (2008).

[6] The authors of the secondary sources, from which much of the data are taken, typically provide a detailed analysis of the meanings and historical locations of the ethnic groups identified in the historical documents. This information greatly facilitated the matching.

Panel A. Transatlantic slave trade



Panel B. Indian Ocean slave trade

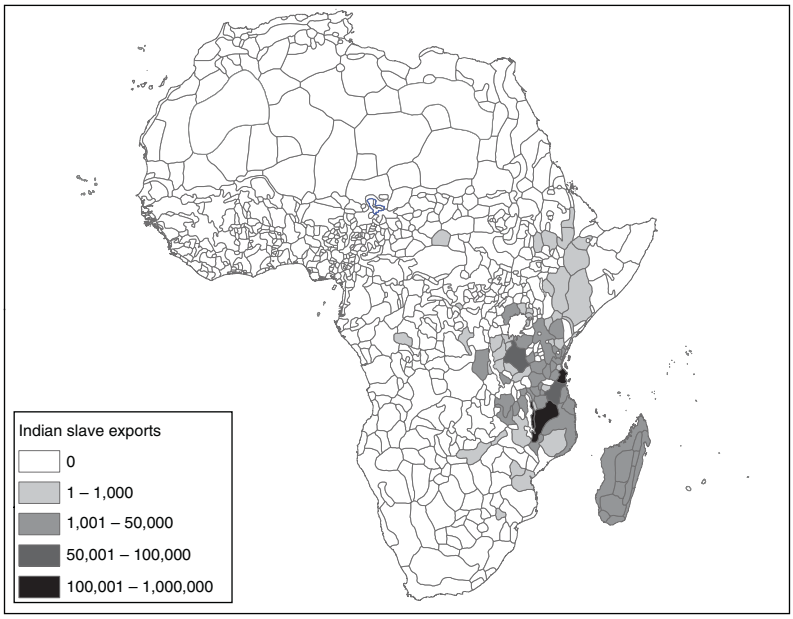


FIGURE 1

Note: Maps display the total number of slaves of each ethnicity shipped during the transatlantic and Indian Ocean slave trades.

III. Estimating Equations and Empirical Results

A. *OLS Estimates*

We begin by estimating the relationship between the number of slaves that were taken from an individual's ethnic group and the individual's current level of trust. Our baseline estimating equation is:

$$(1) \quad trust_{i,e,d,c} = \alpha_c + \beta slave\ exports_e + \mathbf{X}'_{i,e,d,c}\mathbf{\Gamma} + \mathbf{X}'_{d,c}\mathbf{\Omega} + \mathbf{X}'_e\mathbf{\Phi} + \varepsilon_{i,e,d,c},$$

where i indexes individuals, e ethnic groups, d districts, and c countries. The variable $trust_{i,e,d,c}$ denotes one of our five measures of trust, which vary across individuals. α_c denotes country fixed effects, which are included to capture country-specific factors, such as government regulations, that may affect trust (e.g., Philippe Aghion et al. 2010; Aghion, Algan, and Cahuc 2008). $slave\ exports_e$ is a measure of the number of slaves taken from ethnic group e during the slave trade. (We discuss this variable in more detail below.) Our coefficient of interest is β, the estimated relationship between the slave exports of an individual's ethnic group and the individual's current level of trust.

The vector $\mathbf{X}'_{i,e,d,c}$ denotes a set of individual-level covariates, which include the respondent's age, age squared, a gender indicator variable, an indicator variable that equals one if the respondent lives in an urban location, five fixed effects for the respondent's living conditions, ten fixed effects for the educational attainment of the respondent, 18 religion fixed effects, and 25 occupation fixed effects.[7] Many of the controls are intended to proxy for individual income, which has been shown to be correlated with trust. Although we do not have a direct measure of income, occupation, education, and living conditions are all very good proxies.[8]

The vector $\mathbf{X}'_d$ consists of two variables designed to capture the ethnic composition of the district in which the respondent lives.[9] The first variable is the ethnic fractionalization of the respondent's district. Previous studies, such as William Easterly and Ross Levine (1997), have documented a relationship between ethnic fractionalization and income. Perhaps through this channel, the ethnic fractionalization of a respondent's location may affect the respondent's trust.[10] Second, we also control for the share of the district's population that is of the same ethnicity as the respondent.[11] When respondents are part of an ethnic minority, they may be less trusting of others; Alesina and La Ferrara (2002) find evidence of this within the United States. Both of our measures of ethnic composition are constructed using the sample of individuals in the Afrobarometer survey.[12]

[7] A full description of these controls is provided in the online Appendix.

[8] Occupation, as well as proxying for income, may be an important determinant of trust itself. Patrick Francois, Thomas Fujiwara, and Tanguy van Ypersele (2010) provide evidence showing that within the United States, individuals who work in more competitive sectors have higher levels of trust.

[9] A district is the level of disaggregation finer than a region/province and coarser than a village. The sample includes 1,292 districts.

[10] Ethnic fractionalization is constructed in the standard manner. See Easterly and Levine (1997) for details.

[11] This measure actually varies at the district and ethnicity level. As a result, there is a slight abuse of notation in equation (1) in our use of $\mathbf{X}'_{d,c}$ to denote the two variables.

[12] For the average respondent, 48 other individuals in the survey live in the same district.

TABLE 1—OLS ESTIMATES OF THE DETERMINANTS OF TRUST IN NEIGHBORS

Dependent variable: Trust of neighbors	Slave exports (thousands) (1)	Exports/ area (2)	Exports/ historical pop (3)	ln (1 + exports) (4)	ln (1 + exports/ area) (5)	ln (1 + exports/ historical pop) (6)
Estimated coefficient	−0.00068	−0.019	−0.531	−0.037	−0.159	−0.743
	[0.00014]	[0.005]	[0.147]	[0.014]	[0.034]	[0.187]
	(0.00015)	(0.005)	(0.147)	(0.014)	(0.034)	(0.187)
	{0.00013}	{0.005}	{0.165}	{0.015}	{0.034}	{0.212}
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	20,027	20,027	17,644	20,027	20,027	17,644
Number of ethnicities	185	185	157	185	185	157
Number of districts	1,257	1,257	1,214	1,257	1,257	1,214
R^2	0.16	0.16	0.15	0.15	0.16	0.15

Notes: The table reports OLS estimates. The unit of observation is an individual. Below each coefficient three standard errors are reported. The first, reported in square brackets, is standard errors adjusted for clustering within ethnic groups. The second, reported in parentheses, is standard errors adjusted for two-way clustering within ethnic groups and within districts. The third, reported in curly brackets, is T. G. Conley (1999) standard errors adjusted for two-dimensional spatial autocorrelation. The standard errors are constructed assuming a window with weights equal to one for observations less than five degrees apart and zero for observations further apart. The individual controls are for age, age squared, a gender indicator variable, five living conditions fixed effects, ten education fixed effects, 18 religion fixed effects, 25 occupation fixed effects, and an indicator for whether the respondent lives in an urban location. The district controls include ethnic fractionalization of each district and the share of the district's population that is the same ethnicity as the respondent.

The vector $\mathbf{X}'_e$ denotes a vector of ethnicity-level variables that are meant to capture the historical characteristics of ethnicities, as well as the differing impacts of colonial rule on separate ethnic groups. They are important controls for our analysis, and we discuss them as they are introduced.

Estimates of equation (1), with trust measured by individuals' trust in their neighbors, are reported in Table 1. In the first column, we use the total number of slaves taken from an ethnic group (expressed in thousands of people) as our measure of the intensity of the slave trade. The estimated coefficient for slave exports, β, is negative and statistically significant. This is consistent with the hypothesis that the slave trade adversely affected individuals' trust of those around them. Because the distribution of the exports is highly left skewed, with a small number of observations taking on large values, in column 2, we report estimates using the natural log of the slave export measure. The results are similar: we continue to find a significant negative correlation between slave exports and trust.[13]

Many of the explanatory variables in equation (1) do not vary across individuals. Rather, they vary at either the ethnicity level (e.g., *slave exports*$_e$ and $\mathbf{X}'_e$) or the district level (e.g., $\mathbf{X}'_d$). Given the potential for within-group correlation of the residuals, we adjust all standard errors for potential clustering. In Table 1, we report in square brackets standard errors adjusted for clustering of observations of the same

[13] To conserve on space, we do not report the coefficient estimates of the control variables throughout the paper. The estimates generally are in agreement with the findings from previous studies. Consistent with Alesina and La Ferrara's (2002) findings from a US sample, trust is increasing at a decreasing rate in age and is higher for males than for females.

ethnicity. We also calculate standard errors and report them in parentheses, adjusted for two-way clustering within ethnic groups and within districts.[14] A third strategy is to calculate Timothy Conley (1999) standard errors adjusted for two-dimensional spatial dependence. These are reported in curly brackets in Table 1. These three methods all produce standard errors that are essentially identical. For the remainder of the article, we report standard errors adjusted for two-way clustering within ethnic groups and districts.

The estimates reported in column 1 use the total number of slaves as a measure of the impact of the slave trade. One shortcoming of the measure is that it does not account for differences in the size of ethnic groups. Column 2 reports estimates using an alternative slave export measure that normalizes the number of slaves taken by the area of land inhabited by the ethnic group during the nineteenth century. The results are similar using this alternative slave export measure.

Ideally, we would prefer to use a measure of slave exports that is normalized by the population of each ethnic group prior to the slave trade. Unfortunately, these data are unavailable. Some historical population data are available from Murdock (1959), but they are from the colonial period (approximately the early twentieth century) after the end of the slave trade, and they exist for only about 85 percent of the ethnicities in the sample. Column 3 reports estimates normalizing slave exports using these colonial population figures. We obtain similar estimates using this alternative measure. Columns 4–6 report estimates using the natural log of one plus the normalized slave export measures from columns 1–3. Again, this is done to reduce the skewness in the slave export variables. The results remain robust to this alternative specification.

For the remainder of the analysis, we use, as our baseline measure, the natural log of one plus slave exports normalized by land area (the specification from column 5). This provides a measure that is normalized by the size of ethnic groups and uses a denominator that is precisely measured and available for all ethnic groups in our sample. However, as Table 1 illustrates, the results of the article do not rest on this choice.

We now turn to the other measures of trust. Table 2 reports OLS estimates for all five trust measures. The estimates show that the slave trade is negatively correlated with all five measures of trust, including intragroup trust and trust of relatives. This is consistent with the historical evidence: that the effects of the slave trade penetrated deep into the social fabric of societies and eventually turned friends, families, and neighbors against each other.

Not only are the negative coefficient estimates of Table 2 statistically significant, but they are also economically meaningful. To see this, first note that the standard deviation of our baseline slave export variable is close to one (0.95).[15] Also, the standard deviation of each trust measure is close to one, ranging from 0.96 to 1.10. Therefore, the reported coefficients are close to standardized "beta" coefficients, which report the number of standard deviation changes in the dependent variable for a one–standard deviation change in the independent variable. As we have seen, the coefficients for slave exports (for the full sample) range from −0.10 to −0.16.

[14] See Colin Cameron, Jonah Gelbach, and Douglas Miller (2006) for details on multiway clustering.
[15] Summary statistics are reported in the online Appendix.

TABLE 2—OLS ESTIMATES OF THE DETERMINANTS OF THE TRUST OF OTHERS

	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intra-group trust (4)	Inter-group trust (5)
ln (1 + exports/area)	−0.133***	−0.159***	−0.111***	−0.144***	−0.097***
	(0.037)	(0.034)	(0.021)	(0.032)	(0.028)
Individual controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	20,062	20,027	19,733	19,952	19,765
Number of ethnicity clusters	185	185	185	185	185
Number of district clusters	1,257	1,257	1,283	1,257	1,255
R^2	0.13	0.16	0.20	0.14	0.11

Notes: The table reports OLS estimates. The unit of observation is an individual. Standard errors are adjusted for two-way clustering at the ethnicity and district levels. The individual controls are for age, age squared, a gender indicator variable, five living conditions fixed effects, ten education fixed effects, 18 religion fixed effects, 25 occupation fixed effects, and an indicator for whether the respondent lives in an urban location. The district controls include ethnic fractionalization in the district and the share of the district's population that is the same ethnicity as the respondent.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

An alternative way to assess the magnitude of the slave export coefficients is to compare their explanatory power against other variables in the regression. To do this, we compare the slave exports variable with all other explanatory variables in the estimating equation (other than the country fixed effects). Using the estimates from column 5 of Table 1, and performing a standard variance decomposition, we find that slave exports and the other covariates together explain 5.4 percent of the total variation of trust in neighbors. Of this 5.4 percent, 16–27 percent is explained by slave exports.

We undertake a number of robustness and sensitivity checks which we describe only briefly here. The details are reported in the online Appendix. Because we have estimates for only the transatlantic and Indian Ocean slave trades, we verify that our results are not biased by the omission of slaves exported during the trans-Saharan and Red Sea slave trade. We also check our results for robustness to the omission of respondents living in Kenya and Mali, the two countries in our sample that were strongly affected by the trans-Saharan or Red Sea slave trades. Removing the two countries results in point estimates that are nearly identical to the baseline estimates.

Finally, we check for robustness to alternative estimation methods. Using an ordered logit model produces estimates that are qualitatively identical to our baseline OLS estimates. Similarly, estimating versions of equation (1) where the unit of observation is an ethnicity rather than an individual produces similar results. The individual-level estimating equation, which is our baseline specification, has a number of advantages. First, it allows us to explicitly control for individual-level characteristics, which result in more precise estimates of β. Second, the finer unit of observation is necessary to test for the causal mechanisms (see Section V). The tests require variation across individuals and are not possible at the ethnicity level.

IV. Identifying Causal Relationships

The negative correlation between slave exports and trust that is documented in the previous section is consistent with our hypothesis that the slave trade engendered a culture of mistrust. However, the correlation could also be explained by omitted variables that are correlated with selection into the slave trade and with subsequent trust. For example, if ethnic groups that were inherently less trusting were more likely to be taken during the slave trades, and if these groups continue to be less trusting today, then this could generate a negative relationship between the slave trade and trust.

In this section, we pursue three strategies to assess whether the correlations documented to this point are causal. First, we control for observable characteristics of ethnic groups that may be correlated with the slave trade and subsequent trust. Second, we use selection on observable variables to assess the likelihood that our estimates are being driven by unobserved heterogeneity across ethnic groups. Finally, we use the historical distance from the coast of an individual's ethnic group as an instrument for slave exports.

A. *Controlling for Observables: Initial Conditions and Colonial Rule*

Within the historical context of Africa, the most important potentially omitted factor is colonial rule, which followed the slave trade and lasted from 1885 until independence. If the parts of Africa that were most affected by the slave trade were also the most affected by colonial rule, then not controlling for colonial rule might lead to falsely attributing its effects to the slave trade. Therefore, we control for a number of ethnicity-level variables that are intended to capture subnational variation in colonial rule and its determinants.[16] We specifically follow Daron Acemoglu, Simon Johnson, and James A. Robinson (2001, 2002), who put forth two primary determinants of the type of institutions implemented during colonial rule: the deadliness of the disease environment for early European settlers and precolonial prosperity.

We measure an ethnic group's initial disease environment using the malaria ecology of the land it inhabited. The underlying data are from the Malaria Stability Index constructed by Anthony Kiszewski et al. (2004). The index takes into account the prevalence and type of mosquitoes indigenous to a region, their human biting rate, their daily survival rate, and their incubation period. It has been constructed for 0.5-degree-by-0.5-degree grid-cells globally. Combining the malaria index and the digitized map of historical ethnic boundaries, we construct a measure of average malaria presence in land historically inhabited by each ethnic group.[17]

We also construct measures of precolonial prosperity, which Acemoglu, Johnson, and Robinson (2002) argue affected the strategies undertaken by the colonizers. Our ideal measure of initial prosperity would be precolonial population density, or urbanization rates measured at the ethnicity level. Unfortunately, no such data exist. The earliest period for which systematic population data are available (from Murdock

[16] All estimates include country fixed effects. Since colonial boundaries are nearly identical to current country boundaries, our estimates already control for any effects of colonial rule that vary at the national level.

[17] Although the malaria transmission index is taken from contemporary data, it likely provides a close approximation to historical conditions. This is because the indicators it is based on (prevalence and type of mosquitoes, including their biting rates, within Africa) have not changed drastically over time.

1959) is from the colonial period, approximately the early twentieth century. We use this to construct an ethnicity-level measure of colonial population density. However, in addition to not being measured in the precolonial period, the variable suffers from a second shortcoming: the data are missing for about 15 percent of the ethnic groups in the sample.

Given these shortcomings, we also construct additional measures of precolonial prosperity. The first exploits information on the locations and sizes of urban centers. Using data from Tertius Chandler (1987) on the location of African cities with more than 20,000 inhabitants in year 1400, we construct an indicator variable that equals one if there was a city located on the land inhabited by each ethnic group. This provides an indicator of ethnic groups that were densely populated prior to the slave trade.

We also use historical data from the *Ethnographic Atlas* to construct two additional proxies for initial levels of prosperity. The first is a set of indicator variables that quantify the precolonial settlement patterns of ethnic groups. These variables identify whether ethnic groups were fully nomadic (migratory), seminomadic, semisedentary, lived in compact and impermanent settlements, in neighborhoods of dispersed family homes, in separated hamlets forming a single community, in compact and relatively permanent settlements, or in complex settlements. The categories are listed in order of increasing economic and social development and are, almost by definition, increasing in initial population density. The second variable that we construct from the *Ethnographic Atlas* is the number of jurisdictional hierarchies beyond the local community. This measures the sophistication of an ethnic group's political institutions.

Finally, we construct a number of ethnicity-level variables that directly measure European influence during the colonial period. Using information on the location of railway lines in the first decade of the twentieth century from Century Company (1911), we construct an indicator variable that equals one if any part of the railway network was built on land historically inhabited by the ethnic group. This is meant to proxy for whether ethnic groups were historically connected to the colonial railway networks. Using the same source, we construct an indicator variable that equals one if a European explorer traveled through land historically occupied by the ethnic group.[18] Third, we construct a variable to capture European missionary contact during the colonial period. Using information on the historical location of missions in the early twentieth century from William R. M. Roome (1924), we calculate the number of missions per square kilometer for each ethnic group.

Our intention is that by controlling for this extensive set of covariates, we capture any potential effects of non–slave trade European influence on long-term trust. Estimates of equation (1) controlling for the additional controls are reported in Table 3. For each measure of trust, the estimated slave export coefficients remain negative and highly significant.[19]

[18] The variable captures exploration routes between 1768 and 1894.

[19] Because of missing colonial population density data, the sample is reduced slightly when this variable is included as a covariate. Estimates using the larger sample when this variable is excluded are very similar. These are reported in the online Appendix.

TABLE 3—OLS ESTIMATES OF THE DETERMINANTS OF THE TRUST OF OTHERS, WITH ADDITIONAL CONTROLS

	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intragroup trust (4)	Intergroup trust (5)
ln (1 + exports/area)	−0.178***	−0.202***	−0.129***	−0.188***	−0.115***
	(0.032)	(0.031)	(0.022)	(0.033)	(0.030)
Colonial population density	Yes	Yes	Yes	Yes	Yes
Ethnicity-level colonial controls	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	16,709	16,679	15,905	16,636	16,473
Number of ethnicity clusters	147	147	146	147	147
Number of district clusters	1,187	1,187	1,194	1,186	1,184
R^2	0.13	0.16	0.21	0.16	0.12

Notes: The table reports OLS estimates. The unit of observation is an individual. Standard errors are adjusted for two-way clustering at the ethnicity and district levels. The individual controls are for age, age squared, a gender indicator variable, five living conditions fixed effects, ten education fixed effects, 18 religion fixed effects, 25 occupation fixed effects, and an indicator for whether the respondent lives in an urban location. The district controls include ethnic fractionalization in the district and the share of the district's population that is the same ethnicity as the respondent. Ethnicity-level colonial controls include the prevalence of malaria, a 1400 urbanization indicator variable, eight fixed effects for the sophistication of precolonial settlement, the number of jurisdictional political hierarchies beyond the local community in the precolonial period, an indicator for integration with the colonial rail network, an indicator for contact with precolonial European explorers, and the number of missions per square kilometer during colonial rule. Colonial population density is the natural log of an ethnicity's population density during the colonial period.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

B. *Using Selection on Observables to Assess the Bias from Unobservables*

Despite our attempts to control for observable factors, such as initial prosperity and the impacts of colonial rule, the estimates reported in Table 3 may still be biased by unobservable factors correlated with selection into the slave trade and subsequent trust. In this section, we assess the likelihood that the estimates are biased by unobservables.

The strategy that we use exploits the insight from Altonji, Elder, and Taber (2005) that selection on observables can be used to assess the potential bias from unobservables. The authors provide a measure to gauge the strength of the likely bias arising from unobservables: how much stronger selection on unobservables, relative to selection on observables, must be to explain away the full estimated effect.[20]

To see how this measure is calculated, consider two regressions: one with a restricted set of control variables, and one with a full set of controls. Denote the estimated coefficient for the variable of interest from the first regression $\hat{\beta}^R$ (where

[20] Altonji, Elder, and Taber (2005) consider the situation where the explanatory variable is a binary explanatory variable. Bellows and Miguel (2009) develop the same test, but for the case where the variable of interest is continuous. Full details of the test are provided in the working paper version of their study, Bellows and Miguel (2008).

TABLE 4—USING SELECTION ON OBSERVABLES TO ASSESS THE BIAS FROM UNOBSERVABLES

Controls in the restricted set	Controls in the full set	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intragroup trust (4)	Intergroup trust (5)
None	Full set of controls from equation (1)	4.31	4.23	3.03	4.13	3.32
None	Full set of controls from equation (1), ethnicity-level colonial controls, and colonial population density	11.54	6.98	2.65	9.22	3.80
Age, age squared, gender	Full set of controls from equation (1)	4.17	3.99	2.89	3.91	3.12
Age, age squared, gender	Full set of controls from equation (1), ethnicity-level colonial controls, and colonial population density	10.93	6.52	2.57	8.44	3.59

Notes: Each cell of the table reports ratios based on the coefficient for ln (1 + exports/area) from two individual-level regressions. In one, the covariates include the "restricted set" of control variables. Call this coefficient β^R. In the other, the covariates include the "full set" of controls. Call this coefficient β^F. In both regressions, the sample sizes are the same, and country fixed effects are included. The reported ratio is calculated as: $\beta^F/(\beta^R - \beta^F)$. See Table 3 for the description of the full set of controls from equation (1), the ethnicity-level colonial controls, and colonial population density.

R stands for *R*estricted) and the estimated coefficient from the second regression $\hat{\beta}^F$ (where *F* stands for *F*ull). Then, the ratio can be calculated as: $\hat{\beta}^F/(\hat{\beta}^R - \hat{\beta}^F)$.[21] The intuition behind the formula is straightforward. First, consider why the ratio is decreasing in $(\hat{\beta}^R - \hat{\beta}^F)$. The smaller is the difference between $\hat{\beta}^R$ and $\hat{\beta}^F$, the less the estimate is affected by selection on observables, and the stronger selection on unobservables needs to be (relative to observables) to explain away the entire effect. Next, consider the intuition behind $\hat{\beta}^F$ in the numerator. The larger $\hat{\beta}^F$, the greater is the effect that needs to be explained away by selection on unobservables, and therefore the higher is the ratio.

We consider two sets of restricted covariates: one with no controls and another with a sparse set of individual controls that includes only age, age squared, and the gender indicator variable. We also consider two sets of full covariates: the baseline set of controls from equation (1), and a second adding to this the ethnicity-level colonial control variables, including colonial population density. Given our two restricted and two unrestricted sets of covariates, there are four combinations of restricted and unrestricted controls that can be used to calculate the ratios. The ratios, for each of our five measures of trust, are reported in the cells of Table 4.

Of the 20 ratios reported in Table 4, none is less than one. The ratios range from 3.0 to 11.5, with a median ratio of 4.1. Therefore, to attribute the entire OLS estimate to selection effects, selection on unobservables would have to be at least three times greater than selection on observables and, on average, over four times greater. In our view, these results make it less likely that the estimated effect of the slave trade is fully driven by unobservables. In the following section, we examine this issue further by undertaking an alternative strategy.

[21] See Bellows and Miguel (2008) for the formal derivation. As well, see Altonji, Elder, and Taber (2005) for details of the underlying assumptions.

C. *IV Estimates*

Our final strategy is the use of instrumental variables. This requires an instrument that is correlated with the number of slaves taken from an ethnic group but uncorrelated with any characteristics of the ethnic group that may affect the trust of descendants. We use a measure of the distance of an individual's ethnic group from the coast during the slave trade. The instrument captures an ethnic group's exposure to the external demand for slaves, since slaves were purchased at the coast before being shipped overseas. Further, distance from the coast is plausibly uncorrelated with other factors that affected the trust of their descendants.

The instrument is constructed using data from Murdock (1959) on the historical borders of ethnic groups during the nineteenth century. (The borders are shown in Figures 1A and 1B.) We calculate the distance from the centroid of each ethnicity to the closest point along the coast.

The history of Africa's slave trades leaves little doubt that the instrument is relevant. Various authors, including Joseph C. Miller (1996), describe the slave trade as progressing in waves of destruction that originated from the coast. The critical issue is whether an ethnic group's distance from the coast in the past is uncorrelated with factors, other than the slave trade, that may affect how trusting the ethnic group is today—for example, initial prosperity, which may have affected an ethnic group's susceptibility to the slave trade, as well as its subsequent trust. Generally, we would expect distance from the coast to be correlated with overseas trade, and thus with initial prosperity. However, because of Africa's particular history, this is not a concern. In the regions in our sample, there was no overseas trade prior to the transatlantic and Indian Ocean slave trades. This alleviates concerns that initial distance from the coast may have had a direct effect on initial development via preexisting trade.

Despite this fact, there remain a number of other reasons why the exclusion restriction may not be satisfied. First, distance from the coast may be correlated with other forms of European contact, like colonial rule, which followed the slave trade. For this reason, we only report IV estimates after controlling for our full set of ethnicity-level colonial control variables. Second, locations closer to the coast were more likely to rely on fishing as a form of subsistence. Although it is not obvious how this may affect future trust, to be as thorough as possible we control for ethnicities' historical reliance on fishing. Third, for some parts of Africa, proximity to the coast implies greater distance from the ancient trade networks across the Sahara Desert. Because long-term trust may have been affected by a group's involvement in this inland trade, we also control for the average distance to the closest city in the Saharan trade, as well as the average distance to the closest route of the Saharan trade.[22]

We report IV estimates for each of the five measures of trust in Tables 5 and 6. Table 5 reports IV estimates controlling for our baseline set of control variables, the

[22] An additional concern is that the distance of an individual's ethnic group from the coast in the past may be correlated with the individual's distance from the coast today, which may be correlated with current income and trust. However, throughout the analysis we control for a number of proxies for income, such as education, occupation, and living condition fixed effects. An alternative strategy is to also control for a respondent's current distance from the coast. This yields results that are similar to those we report here. The full estimates are reported in the online Appendix.

TABLE 5—IV ESTIMATES OF THE EFFECT OF THE SLAVE TRADE ON TRUST

	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intragroup trust (4)	Intergroup trust (5)
Second stage: Dependent variable is an individual's trust					
ln (1 + exports/area)	−0.190***	−0.245***	−0.221***	−0.251***	−0.174**
	(0.067)	(0.070)	(0.060)	(0.088)	(0.080)
Hausman test (p-value)	0.88	0.53	0.09	0.44	0.41
R^2	0.13	0.16	0.20	0.15	0.12
First stage: Dependent variable is ln (1 + exports/area)					
Historical distance of ethnic group from coast	−0.0014***	−0.0014***	−0.0014***	−0.0014***	−0.0014***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Colonial population density	Yes	Yes	Yes	Yes	Yes
Ethnicity-level colonial controls	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	16,709	16,679	15,905	16,636	16,473
Number of clusters	147 / 1,187	147 / 1,187	146 / 1,194	147 / 1,186	147 / 1,184
F-stat of excl. instrument	26.9	26.8	27.4	27.1	27.0
R^2	0.81	0.81	0.81	0.81	0.81

Notes: The table reports IV estimates. The top panel reports the second-stage estimates, and the bottom panel reports first-stage estimates. Standard errors are adjusted for two-way clustering at the ethnicity and district levels. The individual controls, district controls, ethnicity-level colonial controls, and colonial population density measures are described in Table 3. The null hypothesis of the Hausman test is that the OLS estimates are consistent.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

ethnicity-level colonial controls, and colonial population density.[23] The first-stage estimates show that historical distance from the coast is negatively correlated with slave exports. Consistent with the historical record, ethnic groups that were further from the coast exported fewer slaves. The second-stage estimates report a negative and highly significant effect of the slave trade on trust. Furthermore, the magnitudes of the estimates are remarkably similar to the OLS estimates. In fact, in all specifications, the Durbin-Wu-Hausman test cannot reject the null hypothesis of the consistency of the OLS estimates at the 5 percent level or lower. These results suggest that selection into the slave trade is not strongly biasing the OLS estimates. This is consistent with the findings in the previous section, and with the findings in Nunn (2008), where the IV estimates of the effect of the slave trade on per capita income across countries were similar to the OLS estimates.

Table 6 reports estimates with controls for each ethnic group's historical reliance on fishing and two measures of its distance from the Saharan trade. The variable for the reliance on fishing measure is from Murdock (1967) and is measured as the fraction of food from fish. The distance from the Saharan trade variables are: the distance to the closest city involved in the Saharan trade and the distance to the closest route of the Saharan trade. Data on the historical locations of towns and routes are originally from Roland Oliver (2000) and have been digitized by Matthew T. Ciolek

[23] The results are similar if we do not include the colonial population density control. The results from this specification are reported in the online Appendix.

TABLE 6—IV ESTIMATES OF THE EFFECT OF THE SLAVE TRADE ON TRUST, WITH ADDITIONAL CONTROLS

	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intragroup trust (4)	Intergroup trust (5)
Second stage: Dependent variable is an individual's trust					
ln (1 + exports/area)	−0.172**	−0.271***	−0.262***	−0.254**	−0.189*
	(0.076)	(0.088)	(0.075)	(0.109)	(0.103)
Hausman test (p-value)	0.98	0.42	0.05	0.53	0.44
R^2	0.13	0.16	0.20	0.15	0.12
First stage: Dependent variable is ln (1 + exports/area)					
Historical distance of ethnic group from coast	−0.0015***	−0.0015***	−0.0015***	−0.0015***	−0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Reliance on fishing	Yes	Yes	Yes	Yes	Yes
Distances to Saharan city, route	Yes	Yes	Yes	Yes	Yes
Colonial population density	Yes	Yes	Yes	Yes	Yes
Ethnicity-level colonial controls	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	16,709	16,679	15,905	16,636	16,473
Number of clusters	147 / 1,187	147 / 1,187	146 / 1,194	147 / 1,186	147 / 1,184
F-stat of excl. instrument	21.7	21.6	22.2	21.8	21.6
R^2	0.81	0.81	0.81	0.81	0.81

Notes: The table reports IV estimates. The top panel reports the second-stage estimates, and the bottom panel reports first-stage estimates. Standard errors are adjusted for two-way clustering at the ethnicity and district levels. The individual controls, district controls, ethnicity-level colonial controls, and colonial population density measures are described in Table 3.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

(2001). The estimates are similar when controlling for these additional factors; they remain negative, significant, and virtually identical in magnitude.

Our measures capture slaves exported anytime after 1400. For West, West-Central, and Southern African countries in our sample, overseas trade did not exist prior to this date. However, the Indian Ocean slave trade and overseas trade in legitimate commodities predate 1400, the first period for which we have slave data. Therefore, it is possible that, for this region of Africa, distance from the coast directly affected the characteristics of ethnic groups prior to the first year of our analysis. However, IV estimates omitting ethnic groups from coastal East Africa yield similar results.[24]

Falsification Tests.—When we examine the reduced form, we find a strong positive relationship between the historical distance from the coast of individuals' ancestors and their level of trust today.[25] This correlation is consistent with the first- and second-stage IV estimates reported in Tables 5 and 6: ethnic groups that lived closer to the coast were more exposed to the slave trade, and today their descendants are less trusting. Our IV strategy rests on the assumption that the slave trade is the only channel through which historical distance from the coast affects current trust. If this

[24] Space constraints prevent us from reporting the estimates here; they are reported in the online Appendix.
[25] A similar relationship is found if one examines an individual's current distance from the coast and trust.

assumption is correct, then a positive relationship between distance from the coast and trust should not exist in parts of the world that did not experience the slave trade.

To assess the validity of the IV estimates, we undertake this falsification test: we use two additional surveys that ask the same, or similar, trust questions as the Afrobarometer survey and we identify the locations of individuals in the surveys. The first sample comes from the 2003 Asiabarometer and includes individuals from the following ten Asian countries: Japan, South Korea, China, Malaysia, Thailand, Vietnam, Myanmar, India, Sri Lanka, and Uzbekistan. Using data on the locations of the survey respondents, we calculate each person's current distance from the nearest coastline.[26]

The Asiabarometer asks respondents how much they trust their local government. The question is worded "How much do you trust your local government?" Although this differs slightly from the Afrobarometer question, which asks "How much do you trust your locally elected government council?" both questions convey the same general meaning. Moreover, the available answers for the two questions are the same, further suggesting that they are comparable. We construct our dependent variable the same way for both samples. Because income, occupation, and ethnic fractionalization measures are unavailable from the Asiabarometer survey, these covariates are not included in the estimating equations of either the African or Asian samples. The covariates that are common to the two samples are also measured slightly differently, so we report all specifications with country fixed effects only.

The first two columns of Table 7 report the reduced-form estimates of the relationship between distance from the coast and trust in the local government within Africa. With or without the control variables, there is a strong positive relationship between ethnic groups' historical distance from the coast and their trust in their local council.[27] Columns 3 and 4 report the same reduced-form estimates within Asia. Unlike the African sample, the Asian sample shows no systematic relationship between an individual's distance from the coast and trust. Both point estimates for Asia are close to zero, and highly insignificant.

We also undertake a second falsification exercise using the 1990 World Values Survey (WVS). The sample includes individuals from Chile, Norway, Sweden, Great Britain, and Northern Ireland, the only countries in the first four rounds of the WVS for which the geographic location of respondents is collected and a trust question similar to one of the Afrobarometer trust questions is asked. The WVS asks: "How much do you trust <nationality> people in general?" This is similar to the Afrobarometer question: "How much do you trust <nationality> people from other ethnic groups?" The possible responses for the WVS answers are slightly different from the Afrobarometer categories. In addition to the four answers in the Afrobarometer survey—"not at all," "not very much," "a little," and "completely"—the WVS allows respondents the additional choice of "neither trust or distrust." For the WVS variable, as with the Afrobarometer measure, we assign the values 0 and 1 to the two least trustful answers, and the values of 2 and 3 to the two most trustful

[26] Note that here we are using each respondent's current distance from the coast since we do not have a measure of his ancestor's historical distance from the coast. Given the persistence in family locations over time, and the strong correlation between historical and current distance within the Africa sample, we feel that the current distance from the coast is a useful proxy for historical distance.

[27] The relationship is similar if current distance from the coast is used instead of the historical distance from the coast. See the online Appendix for full estimates.

TABLE 7—REDUCED FORM RELATIONSHIP BETWEEN THE DISTANCE FROM THE COAST AND TRUST WITHIN AFRICA AND ASIA

	Trust of local government council			
	Afrobarometer sample		Asiabarometer sample	
	(1)	(2)	(3)	(4)
Distance from the coast	0.00039***	0.00031***	−0.00001	0.00001
	(0.00009)	(0.00008)	(0.00010)	(0.00009)
Country fixed effects	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes
Number of observations	19,913	19,913	5,409	5,409
Number of clusters	185	185	62	62
R^2	0.16	0.18	0.19	0.22

Notes: The table reports OLS estimates. The unit of observation is an individual. The dependent variable in the Asiabarometer sample is the respondent's answer to the question: "How much do you trust your local government?" The categories for the answers are the same in the Asiabarometer as in the Afrobarometer. Standard errors are clustered at the ethnicity level in the Afrobarometer regressions and at the location (city) level in the Asiabarometer and the WVS samples. The individual controls are for age, age squared, a gender indicator, education fixed effects, and religion fixed effects.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

answers. For the additional intermediate category, "neither trust or distrust," we assign a value of 1.5. This coding ensures that both variables have the same range, from 0 to 3.[28]

The estimation results are reported in Table 8. Columns 1 and 2 report the reduced-form relationship within Africa, with and without the set of controls. (Because the WVS does not include measures of education, living conditions, or religion, these are not included in the regressions.) Again, we find a strong positive relationship between an individual's distance from the coast and trust. Columns 3 and 4 report the same estimates using the WVS sample. For this non-African sample, we do not find evidence of a positive relationship between distance from the coast and trust. Both coefficients are statistically insignificant.

One potential concern is that the differences in the relationship between distance from the coast and trust within and outside of Africa are driven solely by differences in the two surveys, such as their sample sizes or the precision of the data. However, as shown in column 5, when we look only at respondents from African countries within the WVS—i.e., Nigeria—we estimate a strong positive relationship between individuals' distance from the coast and trust. This suggests that the different reduced-form relationships that we find are not the result of differences in the underlying surveys.

Robustness to Violations of Perfect Exogeneity.—Although our falsification tests do provide evidence for the validity of our instrument, we recognize that the requirement of perfect exogeneity is a knife requirement that, strictly speaking, is unlikely to hold exactly. To gain a sense of the robustness of our IV estimates, we relax the

[28] The results are not sensitive to this assumption. They are qualitatively identical if we instead use a trust variable that takes on the values 0, 1, 2, 3, and 4.

Table 8—Reduced Form Relationship between the Distance from the Coast and Trust within and Outside of Africa

	Intergroup trust				
	Afrobarometer sample		WVS non-Africa sample		WVS Nigeria
	(1)	(2)	(3)	(4)	(5)
Distance from the coast	0.00039***	0.00037***	−0.00020	−0.00019	0.00054***
	(0.00013)	(0.00012)	(0.00014)	(0.00012)	(0.00010)
Country fixed effects	Yes	Yes	Yes	Yes	n/a
Individual controls	No	Yes	No	Yes	Yes
Number of observations	19,970	19,970	10,308	10,308	974
Number of clusters	185	185	107	107	16
R^2	0.09	0.10	0.09	0.11	0.06

Notes : The table reports OLS estimates. The unit of observation is an individual. The dependent variable in the WVS sample is the respondent's answer to the question: "How much do you trust <nationality> people in general?" The categories for the respondent's answers are: "not at all," "not very much," "neither trust nor distrust," "a little," and "completely." The responses take on the values 0, 1, 1.5, 2, and 3. Standard errors are clustered at the ethnicity level in the Afrobarometer regressions and at the location (city) level in the Asiabarometer and the WVS samples. The individual controls are for age, age squared, a gender indicator, an indicator for living in an urban location, and occupation fixed effects.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

assumption of perfect exogeneity and examine the bounds we are able to place on the true effect of the slave trade on trust as we deviate from perfect exogeneity.

Consider a generalization of the standard IV equations that allows the instrument to also enter linearly in the second-stage regression with a coefficient γ. In other words, we allow distance from the coast to affect trust directly. Conley, Christian Hansen, and Peter E. Rossi (2008) show how one can obtain consistent estimates of the effect of interest (in our case, the slave trade on trust β) if γ is known. Furthermore, the estimates of the relationship between distance from the coast and trust in countries where there was no slave trade provide consistent estimates of γ.[29]

Applying Conley, Hansen, and Rossi (2008), the first finding is that in our setting when $\gamma < 0$, the bounds on the strength of β are actually further from zero (i.e., a stronger effect) relative to the IV estimate of β. In other words, if areas further from the coast have lower trust, then the IV coefficient provides an underestimate of the true effect of the slave trade on trust. This is reassuring, since three of our four falsification exercises report negative point estimates for the correlation between distance from the coast and trust, γ.

Applying Conley, Hansen, and Rossi (2008), we can show that the positive estimate of γ reported in column 4 is not above the value of γ necessary to lose confidence in the finding of a negative impact of the slave trade on trust. For the 90 percent confidence interval for β to include zero, γ must be larger than 56×10^{-6}. This is over eight times greater than the estimate of 7×10^{-6} from column 4 of

[29] To see this, note that in general the reduced-form relationship between distance from the coast and trust captures both β and γ. But if we are certain that β is zero (as is the case in the parts of the world where there was no slave trade), then it only captures γ.

Table 4. Therefore, even allowing for plausible amounts of imperfect exogeneity, we are still able to confirm a negative effect of the slave trade on trust.[30]

V. Testing for Channels of Causality: Effects of the Slave Trade on Internal Norms versus External Factors

Up to this point, we have asked whether the slave trade caused the descendants of those exposed to it to become less trusting. The evidence we presented is consistent with our hypothesis that the evolution of behavioral norms was influenced during the 400-year period of the slave trade. Those exposed to the trade became less trusting, and their descendants remain less trusting today. However, a second explanation is also possible. The slave trade may be correlated with lower trust today because it resulted in a deterioration of preexisting states, institutions, and legal structures. If these institutional effects persist, then people today may have lower levels of trust because poor institutions permit poor behavior, which engenders mistrust.

In this section, we perform three empirical tests to distinguish between the two channels. The first focuses on individuals' trust in their local government council. We have already shown that individuals with heavily threatened ancestors have less trust today in their local government. This relationship could be due in part to the adverse effects of the slave trade on local institutions. Individuals may mistrust their local government council not because they have developed internal norms of mistrust, but rather because the council is not trustworthy. We account for this by controlling directly for the perceived trustworthiness of the local government council.

In the survey, respondents were asked whether they approve or disapprove of the way their locally elected government councilor performed his/her job over the past 12 months. Respondents chose from the following responses: strongly disapprove, disapprove, approve, or strongly approve. Respondents also were asked two additional questions: (i) how many of their locally elected councilors were corrupt, and (ii) whether their local council members listen to their concerns. For the corruption question, the respondents were given the option of answering that none, some, most, or all of the councilors are corrupt. For the question about whether councilors listen, the respondents were given the option of answering: never, only sometimes, often, or always.

In the data, we find that individuals with ancestors who were more heavily affected by the slave trade are more likely to disapprove of their local government council, to report that more of their councilors are corrupt, and to feel that councilors do not listen. Therefore, it is possible that the relationship between the slave trade and mistrust in the local council arises because the slave trade adversely affected the actual trustworthiness of the local government council.

In column 1 of Table 9 we check for this possibility by estimating equation (1) while controlling for the three measures of the perceived quality of the local council.[31] We include three sets of fixed effects constructed from the responses to each

[30] These results are from an IV regression with trust in the local council as the dependent variable, and controlling for the baseline controls and the ethnicity-level colonial controls.

[31] Throughout this section, colonial population density is included in the regression. As we show in the online Appendix, the results are qualitatively identical if this control is omitted.

Table 9—Identifying Channels of Causality

	Trust of local council		Intergroup trust		
			Within town	Within district	Within province
	(1)	(2)	(3)	(4)	(5)
Ethnicity-based slave export measure (baseline measure)	−0.072*** (0.019)	−0.070*** (0.019)	−0.102*** (0.028)	−0.120*** (0.027)	−0.098*** (0.029)
Average slave export measure among other ethnicities in the same location			−0.037 (0.029)	−0.063** (0.030)	−0.091*** (0.035)
Council trustworthiness fixed effects	Yes	Yes	No	No	No
Five public goods fixed effects	No	Yes	No	No	No
Colonial population density	Yes	Yes	Yes	Yes	Yes
Ethnicity-level colonial controls	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	12,827	12,203	9,673	12,513	15,999
Number of clusters	146/1,172	145/1,130	147/725	147/737	147/1,127
R^2	0.37	0.37	0.12	0.12	0.12

Notes: The table reports OLS estimates. The unit of observation is an individual. Standard errors are adjusted for two-way clustering at the ethnicity-based ethnicity level and at the location-based ethnicity level. "Average slave export measure among other ethnicities in the same location" is the average slave export measure of respondents in the Afrobarometer survey living in the same village, district, or region as the respondent. The "Five public goods fixed effects" are for the existence of the following public goods in the respondent's town/village: school, health clinic, sewage, piped water, and electricity. See Table 3 for a description of the baseline controls, the ethnnicity-level colonial controls, and the colonial population density variables.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

question. Even with the inclusion of these additional controls, the estimated relationship between slave exports and trust remains negative and highly significant. The estimates of β from a regression without the quality of local council fixed effects, but using the same sample of observations, is -0.141 (the standard error is 0.024). Therefore, controlling for the quality of the local council decreases the magnitude of the estimated coefficient by just under 50 percent. This result suggests that over half of the estimated relationship between slave exports and trust cannot be explained by a deterioration in the trustworthiness of the local council.

It is possible that including further controls for the trustworthiness of the local council would result in point estimates that are close to zero. To cast doubt on this possibility, we include additional controls. Column 2 of Table 9 shows that including objective proxies for the quality of the local government—measured by the existence of public goods—has little effect on the coefficient. The variables are constructed from Afrobarometer survey questions that ask whether electricity, piped water, sewage, health clinics, and schools are available in the respondent's village. Using this information, we control for five indicator variables that equal one if the respondent has access to each of the five public goods.

In a second exercise, we further distinguish between the effects of the slave trade through a change in the internal norms of trust versus a change in the trustworthiness of others. We focus on intergroup trust and reestimate equation (1) while controlling for how much the slave trade affected others from different ethnic groups living in the same area. For each observation, we calculate the average slave export intensity of those belonging to different ethnic groups living in the respondent's town,

district, or region. The measure is intended to capture any effects of the slave trade on the trustworthiness of other ethnic groups living near the individual.

The estimates are reported in columns 3–5 of Table 9. Column 3 reports estimates of equation (1), controlling for the average interethnic slave export intensity of others in the respondent's town. If there are no individuals from other ethnic groups living in the same town in the Afrobarometer sample, then the variable takes on a missing value. As a result, controlling for the measure results in a smaller sample of 9,673 observations. Columns 4 and 5 report estimates using the district and region as the geographic area when constructing the interethnic slave trade variable. As the geographic region is broadened, there are fewer missing observations, because it is more likely that other ethnicities in the sample live in the same location as the respondent.

The estimated effect of the slave trade on intergroup trust is robust to controlling for the effect of the slave trade on trustworthiness. Moreover, if we compare the point estimates to estimates using the same specification and sample, but not including the interethnic slave exports control (which are −0.104, −0.126 and −0.107, respectively), we find that the point estimates are barely affected by the inclusion of the control. These findings suggest that essentially all of the estimated effect of the slave trade on intergroup trust is not explained by the effect of the slave trade on the trustworthiness of others.[32]

Our final strategy is to estimate directly how much of the slave trade's effect on trust works through an individual's external environment—such as the rule of law and the trustworthiness of others—versus through individuals' internal norms of mistrust. We do this by constructing a second slave-export variable. Unlike our baseline measure of the number of slaves taken from an individual's *ethnic group*, it measures the number of slaves taken from the *geographic area* in which the individual is currently living. We first identify the current location of each respondent and then determine which ethnic group historically inhabited that location. The location-based slave-export variable takes on the value of the slave exports measure for the ethnic group that historically lived in the location. Therefore, the second variable measures the slave trade's impact on an individual's geographic location, rather than on the individual's ancestors.

The two slave export measures identify the internal and external channels by exploiting the fact that when individuals relocate, their cultural beliefs, norms, and values move with them, but their external environment is left behind. Therefore, if the slave trade primarily affects trust through internal factors, then mistrust should be most strongly correlated with the extent to which individuals' ancestors were affected by the slave trade. If the slave trade affects trust primarily through external factors, like the deterioration of domestic institutions, which lead to a decline in the trustworthiness of others, then mistrust should be most strongly correlated with the slave trade's impact on the environment in which the individual lives today. By including the ethnicity-based and location-based slave export variables in our

[32] This is not to say that the slave trade did not affect trustworthiness. The estimates provide evidence for this. The coefficients for the interethnic slave exports variable are negative in all three specifications, and significant in two of the three. This is consistent with the slave trade's negatively affecting the trustworthiness of individuals, which causes them to be trusted less by others today.

Table 10—Identifying Channels of Causality

	Trust of relatives (1)	Trust of neighbors (2)	Trust of local council (3)	Intragroup trust (4)	Intergroup trust (5)
Ethnicity-based slave export measure (baseline measure)	−0.155*** (0.029)	−0.182*** (0.029)	−0.100*** (0.023)	−0.169*** (0.033)	−0.090*** (0.030)
Location-based slave export measure	−0.045*** (0.014)	−0.045*** (0.016)	−0.045** (0.018)	−0.043** (0.018)	−0.047** (0.020)
Colonial population density	Yes	Yes	Yes	Yes	Yes
Ethnicity-level colonial controls	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	15,999	15,972	15,221	15,931	15,773
Number of clusters	146/269	146/269	145/272	146/269	146/269
R^2	0.13	0.16	0.20	0.16	0.12

Notes: The table reports OLS estimates. The unit of observation is an individual. Standard errors are adjusted for two-way clustering at the ethnicity-based ethnicity level and at the location-based ethnicity level. "Ethnicity-based slave export measure" is our baseline measure of slave exports used throughout the article; it is the log of the number of slaves taken from an individual's ethnic group (normalized by land area). "Location-based slave export measure" is our alternative measure of slave exports, which is the log of the number of slaves taken from the location where an individual is currently living (normalized by land area). See Table 3 for a description of the baseline controls, the ethnicity-level colonial controls, and the colonial population density variables.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

estimating equation, we are able to distinguish between the effects of the slave trade through the two channels.[33]

If an individual currently lives where his ancestors lived, then the two slave export measures will be the same.[34] When we include both variables in the estimating equation, the "movers" in the sample (i.e., those living in a location different from their ancestors) are the source of identification. Therefore, the estimates are an average effect among the movers only, and they may not apply to the population more generally. Because movers constitute 45 percent of the population, knowing the average effect among this group is still informative.[35]

Estimates of equation (1) with both slave-export variables included are reported in Table 10. The estimates for the baseline ethnicity-based slave export measure remain robust to the inclusion of the location-based slave-export variable. The coefficients remain negative and highly significant, and their magnitudes decrease by only about 10–15 percent when the location-based slave-exports variable is included. (The estimates of β without the location-based control are −0.187, −0.204, −0.136, −0.190, −0.116, respectively.)

[33] The logic of the test is the same as that used in previous studies that examine migrants to test whether cultural differences can explain differences in on-the-job shirking (Andrea Ichino and Giovanni Maggi 2000), financial decisions (Guiso, Sapienza, and Zingales 2004), living arrangements (Paola Giuliano 2007), or female labor force participation and fertility (Raquel Fernández and Alessandra Fogli 2007).

[34] Not surprisingly, we find that the geography- and ethnicity-based measures of slave exports are highly correlated (the correlation coefficient is 0.74). For 55 percent of the respondents in the sample, both variables take on the same value.

[35] Relative to nonmovers, movers are more likely to live in urban locations that are more ethnically fragmented and with fewer coethnics. This is consistent with the migration patterns observed within African countries, where individuals, in search of better employment opportunities, move from ethnically homogenous rural villages to larger, more ethnically diverse urban centers. Full details of these differences are reported in the online Appendix.

The location-based measure of slave exports always enters with a negative and significant coefficient, suggesting that the slave trade affects trust through geographically fixed factors, like domestic institutions. Comparing the magnitudes of the coefficients for the two variables, we find that the ethnicity-based slave export coefficient is always at least twice the magnitude of the location-based slave export coefficient. This suggests that, although the slave trade adversely affected trust through factors both internal and external to the individual, the internal channel was more important.

Overall, the results from our three tests suggest that much of the slave trade's effect on trust, identified in Sections III and IV, arises from a change in the internal norms and beliefs of the descendants of those affected by the slave trade. Our first test suggests that over 50 percent of the relationship between the slave trade and trust in the local council can be explained by internal norms. Our second and third tests suggest that internal norms explain 85–100 percent of the total effect of the slave trade on interpersonal trust.

VI. Conclusions

This article adds to a new and growing literature in economics that seeks to better understand the role that culture, norms, and beliefs play in individual decision making. Generally, the empirical literature has focused on either showing that culture exists or on identifying the economic impacts of cultural differences. The next natural step is to try to understand the origins of cultural differences, which this study does by looking back into history.

We have shown that within Africa low levels of trust can be traced back to the legacy of the slave trade. Individuals' trust in their relatives, neighbors, coethnics, and local government is lower if their ancestors were heavily affected by the slave trade.

To determine whether this relationship is causal, we pursued a number of different strategies. First, we controlled for initial ethnicity characteristics and for the potential impact of colonial rule. Second, using recently developed techniques from Altonji, Elder, and Taber (2005), we showed that on average selection based on unobservables would have to be four times greater than selection on observables in order for the negative effect of the slave trade on trust to be completely spurious. Finally, we reported IV estimates that use the historical distance from the coast of an individual's ethnic group as an instrument for slave exports. The IV estimates also show a negative effect of the slave trade on trust.

Motivated by the possibility that the instrument does not satisfy the exclusion restriction, we then performed a number of falsification exercises. Within Africa, we observe a robust positive reduced-form relationship between distance from the coast and trust. However, in samples outside of Africa, we find no reduced-form relationship. These correlations are consistent with distance from the coast affecting trust only through the slave trade (i.e., that the exclusion restriction is satisfied).

We then turned to specific mechanisms and examined two explanations for the relationship between the slave trade and trust. The first is that over the 400 years of insecurity generated by the slave trade, general beliefs or "rules-of-thumb" based on mistrust evolved. These beliefs were then transmitted from parents to children over time, and they continue to manifest themselves today, more than 100 years after the

end of the slave trade. The second explanation is that the slave trade resulted in a deterioration of legal and political institutions. Because these weakened institutions continue to persist today, individuals are not constrained to act in a trustworthy manner, and this lack of trustworthiness results in lower trust.

We performed three tests to determine the relative importance of the two channels. We find evidence for both mechanisms. The evidence suggests that the slave trade had an adverse effect on the external environment, which continues to affect trustworthiness to this day. We also find evidence that the slave trade altered the trust of modern Africans through internal factors, such as norms, beliefs, and values. Our tests suggest that the internal channel accounts for at least half of the reduced-form effect of the slave trade on trust.

Overall, the findings provide evidence for the importance of internal norms and beliefs in transmitting the impacts of a historical shock, in this case the slave trade. One reason that history matters today is through the evolution of cultural norms.

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