

Cross-Cutting Cleavages and Ethnic Voting:
Results from an Experiment in Mali

Thad Dunning¹ and Lauren Harrison²
Yale University

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¹ Assistant Professor, Department of Political Science, Yale University

² Yale College, Class of 2009

Abstract: Social scientists often attribute the absence or moderation of ethnic conflict, in ethnically diverse societies, to the presence of cross-cutting cleavages—that is, to dimensions of identity or interest along which members of the same ethnic group may have diverse allegiances. Yet estimating the causal effects of cross-cutting cleavages is difficult. We develop an experimental research design to examine why ethnicity appears to have little political salience in Mali, an ethnically heterogenous sub-Saharan African country where ethnic identity is a poor predictor of vote choice and parties do not form along ethnic lines. We argue that the cross-cutting ties afforded by an informal institution called cousinage can help explain the weak association between ethnicity and individual vote choice. Ethnic and cousinage ties between voters and politicians both enhance the credibility of politicians' policy promises, yet neither dimension of identity becomes dominant as a basis for vote choice due to their cross-cutting nature.

I. Introduction

Social scientists often attribute the absence or moderation of ethnic conflict, in ethnically diverse societies, to the presence of cross-cutting cleavages—that is, to dimensions of identity or interest along which members of the same ethnic group may have diverse allegiances (Lipset and Rokkan 1967, Dahl 1982). According to many scholars, cross-cutting ties may inhibit ethnic polarization and thereby influence whether ethnicity provides an important basis for political mobilization. Where ethnicity itself has multiple dimensions—for example, where language, race, religion, caste, or clan can each provide the basis for different ethnic identities—the way in which different identities interact can explain whether conflict intensifies along any single ethnic dimension, as well as which dimension of ethnic identity is politically salient (Chandra 2005; Posner 2004a, 2005). A rich theoretical literature suggests that the degree to which cleavages are cross-cutting may affect individual vote choice (Roemer et al. 2007), patterns of party competition (Bartolini and Mair 1990), and other outcomes of interest to students of ethnic politics.

Yet estimating the causal effects of cross-cutting cleavages on patterns of ethnic conflict or cooperation is notoriously difficult. Cross-country comparisons can be problematic, because a country's cleavage structure may be related to many confounding factors that might also explain patterns of ethnic conflict. In addition, it may be difficult to define precisely what is the relevant manipulation or intervention, the causal effects of which we would like to estimate.

The West African country of Mali, for instance, has experienced a striking absence of overt ethnic conflict, despite the country's substantial ethnic heterogeneity.³ Since independence, Mali has avoided the sorts of ethnic civil wars that have ravaged many other African countries, such as the Nigerian Biafran War (1967-1970), the genocidal conflict in Rwanda (1994), or the recent ethnic war in the neighboring Ivory Coast (2002-2007). In addition, although survey evidence suggests that ethnic identification is socially important in Mali, ethnicity is not especially salient politically.

³ The exception to this claim involves conflict between Tuareg groups in the north of the country and other ethnicities in Mali. We return to this exception below, which is relevant for our hypotheses.

In Mali, unlike some Sub-Saharan countries, ethnicity is a poor predictor of individual vote choice (Dowd and Driessen 2008), and political parties do not form along ethnic lines. The country's limited degree of ethnic polarization and the negligible role of ethnic voting make Mali an outlier among ethnically-diverse countries in sub-Saharan Africa.

One explanation for this African anomaly may lie in the cross-cutting ties afforded by an informal institution called cousinage (loosely translated as “joking cousinship”).⁴ In Mali as well as in Sénégal, the Gambia, Guinea, and western Burkina Faso, families historically formed alliances on the basis of patronyms. These historical alliances are now invoked in everyday social interactions in Mali and in these other countries. For instance, if someone with the last name of Keita meets someone named Coulibaly in Mali, these two fictive cousins may invoke a standard set of jokes, even if they have never previously met. The jokes reinforce the social bonds understood to inhere in their relationship.

Cousinage alliances in Mali constitute cross-cutting cleavages because they occur across as well as within ethnic groups. Certain members of one ethnic group will have patronym-based alliances with certain members of a different ethnic group. Such ties will not exist between other members of each group, however, who may in turn be linked to individuals in yet other ethnic groups through alternative cousinage alliances.⁵ The interlocking nature of ethnicity and cousinage ties, according to some scholars, helps to explain why ethnic polarization and overt ethnic conflict has been limited in Mali.

Although intuitively plausible, this theory evokes a critical question: is it true? And if true, what mechanisms can help account for the effect? Despite an extensive literature on the pacifying effects of cousinage,⁶ we believe that this hypothesis has been not been subjected to empirical scrutiny in a way that would allow us to draw valid causal inferences. This reflects not so much the absence of rigorous empirical research on the topic as it does the difficulties involved in defining the manipulations or interventions of interest and validly estimating their causal effects.

⁴ The standard French terms used to describe fictive or joking kinship relations in Mali include *cousinage* and *parenté à plaisanterie*; the Bamanakan (Bambara) term is *senankunya*.

⁵ For example, the Keita typically come from the Malinké ethnic group, while the Coulibaly are ethnically Bambara.

⁶ See *inter alia* Arcin (1906), Delafosse (1912), Humbert (1918), Mauss (1928), Radcliffe-Brown (1940), Griaule (1948), Dieterlen (1955), Pageard (1958), Molinié (1959); more recently, Canut and Smith (2006), Davidheiser (2006: 837), Douyon (2006), Galvan (2006), Launay (2006), and Smith (2006).

In this paper, we extend existing hypotheses to explain not only the absence of ethnic conflict in Mali, generically, but also the seemingly negligible role of ethnicity in electoral politics. We are interested in understanding why, in this ethnically-diverse African polity, ethnicity does not predict individual vote choice, and why parties do not form along ethnic lines. Our extension of the cousinage hypothesis to explain political preferences as well as patterns of ethnic voting in Mali is new and, to our knowledge, has not previously been tested.

To test our hypotheses, we develop an experimental design aimed at estimating the effects of cousinage relations on evaluations of political candidates and their speeches. In brief, we showed videotaped political speeches to experimental subjects in Bamako, Mali's capital, and then asked subjects to evaluate the quality of the speech and the attractiveness of the candidate along various dimensions. The content of speeches viewed by each subject was identical; the experimental manipulation consisted of what subjects were told about the politician's last name. Because last name conveys information about both ethnic identity and cousinage ties in Mali, varying the politician's last name allowed us to vary the treatment along two dimensions: the ethnic relationship of the politician and the subject (same ethnicity/different ethnicity) and the cousinage relationship of the politician to the subject (joking cousin/not joking cousin). The resulting experimental design thus allows us to compare, for example, subjects' evaluations of cousins from a different ethnic group and their evaluations of non-cousins from their own ethnic group, while holding constant the content of the speech as well as any fixed effects associated with the candidate.

We find that cousinage alliances do impact candidate evaluations, and in the anticipated direction: cousins are evaluated more favorably than non-cousins. In contrast to the idea that ethnic identity is not politically salient in Mali, we also find that subjects favor co-ethnics over politicians from a different ethnic group. However, we find that cousinage ties help to counteract the negative impact of ethnic differences on candidate evaluations. For example, subjects' evaluations of candidates who are cousins from a different ethnic group are statistically indistinguishable from their evaluations of candidates who are non-cousins from their own ethnic group. The evidence thus supports the claim that cousinage alliances provide an alternate dimension of identity or interest

which cross-cuts ethnicity and which shapes evaluations of politicians from different ethnic groups.

We then turn to the mechanisms that might help to explain the interacting effects of ethnicity and cousinage alliances. On the one hand, voters might prefer co-ethnics to non-coethnics, and cousins to non-cousins, because of their affective evaluations of the candidates. For example, voters might find their co-ethnics or cousins to be more competent, more intelligent, more likeable, or more impressive in general. On the other hand, voters might prefer co-ethnics or cousins because of their expectations about politicians' ex-post performance once elected to office. For instance, they might find co-ethnics or cousins more worthy of confidence, more likely to do a good job or to defend others once in office, and more likely to have good motivations for seeking office in the first place (which presumably says something about voters' expectations about politicians' ex-post performance as well). Because they reflect voters' beliefs about politicians' post-election behavior, these latter factors concern the credibility of politicians' campaign promises.

Our evidence suggests that the latter focus on credibility best explains the treatment effects we find. While there are some differences in subjects' affective evaluations of candidates across treatment conditions, these differences cannot explain the tendency of subjects to prefer their joking cousins, among politicians from a different ethnic group. However, among non-coethnic politicians, cousins are judged more worthy of confidence and more likely defend others and do a good job once in office than are non-cousins.

We interpret this evidence as underscoring the role of clientelistic politics in heightening the salience of cousinage alliances. Just as ethnicity provides a visible marker of identity, plausibly making it advantageous for politicians to target benefits to co-ethnics and heightening the credibility of their promises to do so (Bates 1983, Fearon 1999b, Chandra 2004, Keefer and Vlaicu 2005), cousinage alliances shape voters' expectations about the post-election performance of politicians. Our experimental data, as well as qualitative evidence from the field, suggest that voters anticipate being able to make requests of as well as sanction their cousins, once their cousins are elected to office. The social networks associated with cousinage alliances may help them to do so; for

example, subjects assigned to view speeches by joking cousins from a different ethnic group report having significantly more friends and acquaintances with the politician's last name, than do subjects assigned to view speeches by non-cousins of a different ethnicity.

Whatever the reason that voters find both cousins and co-ethnics more trustworthy and more likely to defend others once in office, the key point is that cousinage alliances and co-ethnicity may both affect the degree to which politicians can commit to distribute benefits to voters who support them. Yet because of the cross-cutting nature of ethnic and cousinage ties, neither dimension of identity becomes a predominant source of political preferences. The presence of a cross-cutting cleavage may therefore attenuate the correlation between ethnicity and individual vote choice and, in general, limit the salience of ethnicity in electoral competition.

Our experimental evidence therefore sheds light on the causal effects of cross-cutting alliances on candidate evaluations as well as on broader patterns of political competition in Mali. While some features of our research design are best suited to the study of cousinage and ethnic politics in West Africa, analogous experiments may be employed in other settings to estimate the direct and interactive effects of cross-cutting cleavages. Our hope is that similar experimental designs will prove useful in other settings beyond Mali.

II. Cross-Cutting Cleavages and Ethnic Politics

Students of comparative politics often stress the role of cross-cutting cleavages in inhibiting ethnic polarization, while the presence of overlapping or reinforcing cleavages is said to exacerbate the potential for conflict (Dahl 1956: 104-5). One can, for instance, contrast stable and integrated polities like Switzerland, where language and religion provide cross-cutting cleavages, with more fractured and unstable polities such as Belgium, where regional, economic, and linguistic conflicts overlap in reinforcing cleavages (Newton and Van Deth 2005: 144).⁷

Cross-cutting cleavages occur when individuals who are members of the same

⁷ Some authors use the word “cleavage” to refer not to the “objective” conditions such as ethnic identification or social class on which political preferences might be based but rather the crystallization of those preferences in a particular party system (see Bartolini and Mair 1990). Here, however, I refer more to cleavages as the set of social categories (such as ethnic groups) that distinguish individuals along any particular dimension of identity.

group or social category on one dimension of identity (say, ethnicity) are members of different groups on another dimension (say, social class). With overlapping or reinforcing cleavages, on the other hand, members of one group on one dimension are members of the same group on another dimension as well (see Taylor and Rae 1969). The presence of cross-cutting cleavages has long been thought to inhibit social and political polarization along any one dimension of conflict. As Lewis Coser (1956: 72-81) once put it, “The interdependence of antagonistic groups and the crisscrossing within such societies of conflicts, which serve to ‘sew the social system together’ by canceling each other out, thus prevent disintegration along one primary line of cleavage.” This idea has a long and distinguished pedigree in political sociology and other fields (Allardt and Littunen 1964, Dahl 1956, 1982, Dahrendorf 1959, Lipset 1959, Lipset and Rokkan 1967, Simmel 1955, Ross 1920).

The presence of cross-cutting cleavages may have implications for democratic stability as well. Lipset (1959: 31, 88-89), for example, argued that “a stable democracy requires a situation in which all the major political parties include supporters from many segments of the population. A system in which the support of different parties corresponds too closely to basic social divisions cannot continue on a democratic basis, for it reflects a state of conflict so intense and clear-cut as to rule out compromise....The chances for stable democracy are enhanced to the extent that groups and individuals have a number of crosscutting, politically relevant affiliations.” In particular, party competition based on ethnic appeals might undermine democratic stability, as it may lead to polarization through an ethnic “outbidding” effect, leading parties to take ever more extreme or uncompromising positions on ethnic political questions (Rabushka and Shepsle 1972). However, Chandra (2005) argues that where there are multiple, cross-cutting dimensions of ethnicity itself (for instance, language, caste, or religion), political competition along ethnic lines can instead stabilize democracy.

Despite this rich theoretical literature, substantial difficulties arise in evaluating these claims empirically. The cleavage structure may reflect deeper social and political forces or it may be associated with other confounding factors; with observational (non-experimental) evidence, it may be especially difficult to separate the effects of these other factors from the effects of the cleavage structure per se. Moreover, although non-

manipulationist accounts of causation obviously exist, defining a valid causal variable often involves defining the hypothetical manipulation, the causal effects of which we would like to estimate (Cochran 1955).⁸ Empirical studies should thus seek to isolate the causal effects of aspects of the cleavage structure that are in principle subject to manipulation. These points suggest the value of constructing experimental research designs to study the relationship between cross-cutting cleavages and ethnic politics. We turn next to the specific topic we consider in this paper, cross-cutting cleavages in Mali, before describing our experimental design.

III. Ethnicity and Cousinage as Cross-Cutting Cleavages in Mali

Mali constitutes a useful setting for evaluating these hypotheses. Located in West Africa to the north of the Ivory Coast and to the east of Senegal, Mali is ethnically heterogeneous, with more than 12 linguistically defined ethnic groups.⁹ As measured by its ethnolinguistic fractionalization (ELF) score of 0.69, for example, ethnic diversity in Mali surpasses such countries as Mozambique, Malawi, or Namibia (Alesina et al. 2003).

In addition, according to survey data as well as our own experimental evidence presented below, ethnicity is a highly relevant social category. In separate surveys taken in 2001 and 2002, respectively, 40% and 37% of Malian respondents ranked ethnicity as the group to which they felt they belonged “first and foremost,” ahead of religion, occupation, or gender; these percentages are higher than the average of 31% for ten African countries surveyed (Eifert, Miguel, and Posner 2007: 8). In surveys taken in 2005, 76% of respondents said they felt “some” or “a lot” of confidence in people from their own ethnic group, while just 54% said the same of people from other ethnic groups (Afrobarometer 2007: 51). Different regions of the country are identified with and populated by particular ethnic groups, and while residential neighborhoods in the capital of Bamako tend to be well-integrated, some *quartiers* are also ethnically identified.¹⁰

Nonetheless, despite the social importance of ethnic identification, in Mali

⁸ For discussion of manipulationist accounts of causation, see Goldthorpe (2001) or Brady (2002).

⁹ Ethnic groups include the Bambara/Bamanan (an estimated 26% of the population), Peulh/Fula (14%), Songhai/Sonrai (10%), Soninké/Sarakolé (9%), Maninka/Malinké (7%), Dogon (6%), Bobo (3%), Sénoufo (3%), Mianga (3%), Khasonké (2%), Tuareg (2%), and Bozo (1%), among others (Afrobarometer 2007: 51).

¹⁰ In response to questions, an interviewee was able to identify the name of a neighborhood associated with each of 10 different ethnic groups. Bamako, October 14, 2008.

ethnicity does not appear to be especially salient politically. In the post-independence period, Mali has not experienced the sorts of ethnic civil wars that have recently ravaged African countries such as Rwanda or Sudan. Since the return to democracy in 1992, electoral competition also does not appear to be defined by ethnicity, as it has during some democratic periods in sub-Saharan African countries such as Nigeria or Kenya. For one, as several studies suggest, individual ethnicity is a poor predictor of individual vote choice. Dowd and Driessen (2008: 6-8), for example, calculate a measure of association between individual ethnicity and party identification and find that Mali has one of the lower scores among the seventeen African countries surveyed.

Ethnicity also does not provide a strong basis of party competition or mobilization in Mali. Posner (2004), using country reports and other sources to code how often ethnicity is mentioned as a basis for party formation or political identity, creates a Politically-Relevant Ethnic Group (PREG) fractionalization index; with its score of 0.13, Mali ranks far below countries with similar levels of ethnolinguistic fractionalization, such as Mozambique (0.36), Ghana (0.44), Malawi (0.55), or Namibia (0.55). Cabinets in Mali often include ministers from various ethnic groups; for reasons discussed below, the current president is sometimes seen by citizens as belonging to one of several ethnic groups. In Mali, political parties are not ethnic parties in the sense of Chandra (2004): they do not portray themselves as “the champion of a particular ethnic group or category to the exclusion of others” or “make such a strategy central to [their] strategy to mobilize voters” (Chandra 2004: 3).

What explains this apparent African anomaly? Colonists, anthropologists, political scientists, and Malian political actors of various stripes have long pointed to the conflict-mitigating role of the practice or institution¹¹ of cousinage in Mali, as well as elsewhere in West Africa. In the rest of this section, we discuss aspects of the history and contemporary practice of cousinage, before turning to our experimental research below.

Cousinage has played a role in what is now the country of Mali since at least the time of the Mali Empire (1230-1600s), especially since the rule of the emperor Sundiata Keita (c. 1235-1255). According to a griot (oral historian) interviewed in Bamako,

¹¹ We refer here to cousinage as an informal institution, in the sense of North (1990) or Grief (2006: 30): “An institution is a system of rules, beliefs, norms and organizations that together generate a regularity of social behavior.”

alliances between patronyms existed even before Sundiata Keita's time.¹² However, Sundiata Keita played a key role in codifying and sanctioning these relations in his charter of Kurukan Fuga, which was an official set of prescriptions for how the Mali Empire should be organized and governed (CITES). Today cousinage alliances exist not only in Mali but also in other places that were either under the authority of the Mali Empire—such as Sénégal, Guinea, the Gambia, and western Burkina Faso—or that have since experienced substantial immigration from these areas, such as the northern Ivory Coast.

Various kinds of alliances may go under the name of senankunya (the Bambara term for cousinage) as well as a related institution of blood pacts, tanamannyonya. Joking can take place between members of different occupational specialties or castes, between grandparents and grandchildren, and in some cases, even between entire ethnic groups (Jones 2007). There is often a hierarchy involved in joking relations, with one patronym in an alliance historically associated with a higher caste (say, the caste of nobles) than the other. In some sense, part of the point appears to be precisely that the leveling influence of joking subverts hierarchies of power, at least temporarily; joking cousins sometimes compete to call each other their “slave” (esclave).¹³ Although there are various kinds of cousinage relations, in this paper we focus on alliances based on family names, since these provide the closest exemplar of a cross-cutting cleavage and are conceivably most responsible for limiting ethnic polarization and the politicization of ethnic identity.

One interesting question concerns how one should conceptualize the patronyms on which cousinage alliances are constructed. For example, does family name simply constitute an alternate dimension of ethnic identity, one separate from the language-based ethnic groups that we refer to here simply as ethnic groups? After all, common patronyms may imply common descent, and descent is often taken to be a defining feature of ethnic identity (Weber 1979, Laitin 1998: 3-35, Fearon 1999a, Horowitz 2000, Chandra 2006). In fact, although family names might be thought to constitute “clans”—

¹² Interview, Noumoussa Kante, Bamako, October 12, 2008.

¹³ One interviewee criticized the institution of cousinage precisely for its leveling tendencies, complaining that bosses of enterprises could be provoked or attacked by joking cousins who were their employees. Interview, Bamako, October 1, 2008.

and indeed the Bamanakan word for patronym, *jamu*, has sometimes been translated this way (Launay 1972)—having a common patronym only implies common descent in the loosest way. As Launey (2006: 767-8) explains, “in a large community, it is not uncommon for two entirely unrelated descent groups to share the same patronym...in principle at least, the relationship of senankunya transcends all locality, linking all people who share one name with all people who share another.”¹⁴ Patronyms in Mali are thus perhaps best thought of not as another dimension of ethnic identity—they do not quite imply, for instance, membership in a clan—but rather as simply an alternate set of social categories in which individuals' membership differs.

If we cannot conceptualize cousinage relationships as providing an alternate dimension of ethnic identity, then how might these cousinage alliance help to mitigate ethnic conflict? A key point is that although cousinage alliances allow for joking or teasing between cousins, they are also said to imply a set of reciprocal obligations. As one ethnic Senoufo interviewee told us, he could “never hurt his cousin” and would “have to do what his cousin asks,”¹⁵ a sentiment shared by other interviewees as well. Anthropologists and other scholars have alleged that patterns of joking kinship may “result in greater willingness to make voluntary material sacrifices (of resources, time, willingness to voluntarily cede in disputes, etc.) for people thought to belong to different groups” (Galvan, 2006).¹⁶ If such reciprocal obligations exist between some members of one ethnic group and some members of another, but not between other members of both groups, it is easy to see how such cross-cutting obligations could inhibit polarization along ethnic lines.

Patterns of conflict in West Africa do suggest the plausibility of the hypothesis that cousinage alliances inhibit ethnic war. As in Mali, in other areas in which cousinage relations prevail—for instance, Sénégal, Guinea, or Burkina Faso—interethnic relations

¹⁴ This is perhaps why anthropologists have discussed cousinage in terms of fictive kinship relations (even as important strands of anthropology and constructivist political science emphasize that all kinship relations, including ethnicity, are fictive in the sense that they are socially constructed or imagined).

¹⁵ Interview, Mamadou Sonogo. Bamako, October 8, 2008.

¹⁶ The Malian musician Salif Keita offers this perspective on cousinage: “The Coulibalys and the Keitas are cousins. They stay together. The Maigas and the Keitas stay together. You see? In fact, Mali is a big family. That's why it's difficult to put someone in prison there. Yes. When you find someone has done something, but then you look closely, and you find he's a relative of yours. In the ancient times, he was like a parent to you.” Interview, Banning Eyre and Sean Barlow, Afropop Worldwide, September 2002.

Downloaded on October 24, 2008 from <http://www.afropop.org/multi/interview/ID/33>.

have been peaceful, relative to many other sub-Saharan countries. The recent ethnic civil war in the Ivory Coast (2002-2007) only underscores this point because conflict took place between Mandé groups in the northern part of the country (many of them immigrants from Burkina Faso or Mali) and other ethnic groups in the south; and while cousinage relations exist between various northern groups, they do not exist between these groups and their ethnic antagonists to the south. The existence of conflict between the Malian state and Tuareg (Tamasheq) groups in the north of Mali—the one area of the country in which ethnic conflict has persisted—also only reinforces the point, because unlike Black Malians who were formerly under the Mali Empire, Tuaregs (who speak a Berber language found throughout North Africa) generally are not integrated into the cousinage system in Mali.

Yet do cousinage relations also affect patterns of ethnic voting and electoral competition? In Mali and similar settings, where a transition to democracy took place in the early 1990s, the relationship of cousinage to electoral politics is perhaps even more relevant than the question of ethnic civil war. Several scholars have pointed to the instantiation of cousinage relations by politicians to serve political ends (Douyon 2006, Canut 2006). Douyon (2006: 899), for example, says “it is certain that numerous candidates in the legislative or municipal elections have solicited the vote of their *senankun* allies, who are manipulated more easily than (even) direct relatives.”¹⁷ Politicians can sometimes find ways to extend the range of cousinage relations to which they can legitimately appeal politically, for example by drawing on maternal as well as paternal patronyms. Douyon (2006: 896-7) even alleges that “the changing of patronyms is very frequent near elections” and that “children of a man with two wives can bear different patronyms,” drawing on their paternal patronym as well as either “maternal” last name, as circumstances require. Beyond direct electoral considerations, Ministers of Culture, intellectuals, and other elites who have an interest in social and political stability also frequently emphasize the pacifying effects of cousinage for nation-building, imbuing the practice of cousinage with a positive normative value (Canut and Smith 2006).

Our own field research supported the claim that politicians appeal to cousinage relations for electoral ends. For example, a Malinké deputy in the National Assembly

¹⁷ Translated from the original French; all translations are ours, unless otherwise noted.

named Keita told one of us (Thad Dunning) that he uses cousinage relations to establish rapport and “win the loyalty” of voters in a village in which there are many ethnic Bambaras named Samaké, who are his joking cousins.¹⁸ Mali's current President, Amadou Toumani Touré (known popularly as ATT), often refers in speeches to his joking cousins. His patronym of Touré grants the president joking alliances with patronyms from at least four ethnic groups—namely, the Coulibaly (ethnic Bamanans), the Maiga (ethnic Songhais), the Keita and the Sissoko (ethnic Malinkés), and the Guindo (ethnic Dogons)—so the president can engage in cousinage joking nearly wherever he travels in the country (Douyon: 899).¹⁹ This is not atypical, because each last name or jamu in Mali typically has not one but several joking partners, across various ethnic groups (Launay 2006: 797). Cousinage alliances may therefore help the same politician to seek the support of voters of several different ethnic groups.

Observational evidence from our study also suggests that ethnicity and cousinage may also shape voter's evaluations of candidates (as one would expect if politicians are indeed able to use cousinage alliances effectively). In post-treatment questionnaires, for example, 32 percent of our experimental subjects told us they paid attention to the candidate's last name in reflecting on his merits; 30 percent also told us that they would, in general, be more likely to vote for a candidate who shares their own patronym. Finally, 38 percent of subjects said that they would be more likely to vote for a candidate who is their joking cousin than a candidate who is not their joking cousin.

Cousinage alliances may therefore account for the apparent absence of ethnic voting in Mali. If voters tend to favor politicians who are their cousins from a different ethnic group, while other voters from the same ethnic group do not, this will surely attenuate the empirical association between ethnicity and individual vote choice. The cross-cutting ties associated with cousinage alliances may also decrease incentives for politicians to cast appeals targeted only at their ethnic group.

In addition, one of the salient ways in which cousinage may shape voter's evaluations about candidates stems from the fact that Mali, like many sub-Saharan

¹⁸ Interview, Lanceni Balla Keita, Bamako, October 12, 2008.

¹⁹ The president benefits from an even more extended set of cousinage alliances than may be usual, because Touré is patronym that can be found among several different ethnic groups, including the Peul, Soninké, Songhai, and Malinké (Douyon: 898, note 26).

African countries, is a highly clientelistic polity. Since the transition to democracy in the early 1990s, clientelist distributions—the exchange of individualized benefits in return for votes—has played a key role in electoral competition between Mali's multiple parties, who often have ideological platforms that are similar on key dimensions. In nationally-representative surveys taken in 2005, 83 percent of respondents said that politicians offer gifts to voters during electoral campaigns “always” or “often” (Afrobarometer 2007: 50). At the same time, the credibility of politicians is generally low (a point on which our experimental evidence below concurs). For instance, 90 percent of respondents said that politicians “often” or “always” make promises just to get elected, while 84 percent said politicians “never” or “rarely” keep their promises after elections (Afrobarometer 2007: 50). If voters prefer their co-ethnics or their cousins for office, it may be that they do so because politicians who are co-ethnics or cousins can overcome some of these sharp credibility problems, for example by more credibly promising to improve the lot of co-ethnic voters or voters with whom they share cousinage alliances.

However, the field research and other observational evidence discussed in this section, as important as they are, may not leave us convinced that cousinage alliances in fact shape political preferences or voting patterns. It may be difficult to separate the effects of cousinage alliances from the effects of other political and economic factors that are responsible for ethnic cooperation. Even if the observational evidence leaves us convinced that, in general, cousinage alliances may have an effect, existing research cannot begin to tell us how this effect might compare to other sources of political preferences—such as ethnicity. We therefore turn now to an experimental design that helps us to address these issues.

IV. Experimental Design

Our experimental design was designed to allow us to estimate the effects of cousinage relations as well as ethnicity on evaluations of political candidates and their speeches. We showed videotaped political speeches to experimental subjects in Bamako, Mali's capital. Subjects were told that the candidate in the video was a political independent who was considering launching a campaign for deputy in the National Assembly. We asked subjects to evaluate the quality of the speech and the attractiveness

of the candidate along various dimensions. Subjects were shown identical speeches in all treatment and control conditions.

The experimental manipulation consisted of what subjects were told about the politician's last name. In Mali, as discussed in the previous section, last name conveys information about both ethnic identity and about cousinage ties. Thus, varying the politician's last name allowed us to vary the treatment along two dimensions: the ethnic relationship of the politician and the subject (same ethnicity/different ethnicity) and their cousinage relationship (joking cousins/not joking cousins). Research assistants mentioned the politician's last name when they introduced the videotape and repeated it in each question they asked about the candidate or the speech in the post-treatment questionnaire.

Our resulting experimental design had six treatment and control conditions. In the four treatment conditions shown in Table 1, for example, the subject and candidate may be cousins from the same ethnic group; non-cousins from the same ethnic group; cousins from different ethnic groups; or non-cousins from different ethnic groups. According to our hypotheses, a joking cousin relationship between voters and politicians should moderate the negative effect of ethnicity on voters' evaluations of politicians. Thus, we expect evaluations of politicians to be more positive on average if the politician is a co-ethnic; we also expect cousins to be evaluated more positively than non-cousins. The main point, however, is that we expect cousins from a different ethnic group (bottom-left cell of Table 1) to be evaluated more positively than non-cousins from a different ethnic group (bottom-right cell of Table 1).²⁰ Such a finding would suggest that members of the same ethnic group have diverse allegiances along a cross-cutting dimension of identity, due to cousinage alliances.²¹

We also added two additional control conditions to the experimental design. In the fifth condition, the subject was provided with no information about the last name of the politician (and thus no information about the politician's ethnicity or position in cousinage networks). Adding this fifth condition allows us to estimate baseline evaluations of the candidates' speeches and to estimate treatment effects relative to this

²⁰ However, based on our qualitative research, we believe that subjects may not clearly distinguish between cousins and non-cousins, among their co-ethnics.

²¹ We do not have strong expectations about the sign of any interaction between co-ethnicity and cousinage.

baseline. Finally, in the sixth condition, the politician had the same last name as the subject. This sixth condition allows us to compare effects stemming from cousinage or co-ethnicity to a sameness or clan effect: perhaps people simply want to vote for politicians who share their family names.

Table 1: Experimental Design (Four of Six Treatments)

| | Joking cousins | Not joking cousins |
|---------------------|----------------|--------------------|
| Same ethnicity | | |
| Different ethnicity | | |

Table entries refer to subject-politician pairs. For example, “same ethnicity” means that the experimental subject and the politician are from the same ethnic group.

Subjects were shown identical political speeches, and they were randomized to treatment and control conditions, so any differences in average evaluations across treatment conditions are presumably due to our manipulation of the last name of the politician. We now further describe the experimental stimulus, the process of recruiting subjects, and the procedure for randomizing subjects to treatment and control conditions, before turning to our analysis of the experimental data.

Experimental stimulus

To create the political speech to be viewed by the experimental subjects, we drew on fieldwork conducted by one of us (Lauren Harrison) in Bamako during Mali's parliamentary elections in 2007, as well as secondary sources. The speech focused on themes that are standard in Malian political speeches, such as the need to improve infrastructure, invest in schools, and relieve electricity blackouts.²² In field trials in Bamako, 56% percent of experimental subjects said the speech “reminded them of a

²² The political speech and the screening and post-speech questionnaires are posted online (in French) at <http://pantheon.yale.edu/~td244/research.html>.

speech they had heard on a previous occasion.” We hired two Malian actors who each separately recorded the political speech.²³

The speech was delivered in Bambara (Bamanakan), which is the lingua franca of Bamako (and of Mali). Although Bambara is the native language of one ethnic group in Mali, its use does not necessarily imply a particular ethnic identity on the part of the politician. Malians from other ethnic groups may learn Bambara as a first language and also primarily use Bambara in daily life. For example, among our experimental subjects who identify primarily with an ethnicity other than Bambara/Bamanan, 61 percent speak Bambara more frequently than any other language in daily life, while another 14 percent speak both Bambara and French and 13 percent speak primarily French—leaving just 12 percent of non-Bambaras who use a language other than Bambara most frequently in daily life. In addition, when experimental subjects were not provided with the politician's last name (in the fifth condition described above), their guesses about the politician's ethnicity roughly tracked the distribution of ethnic groups in Bamako. In other words, subjects did not disproportionately attribute a Bamanan/Bambara identity to the politician, even though the speech was given in Bambara. On the other hand, when the last name of the politician was provided, subjects inferred the intended ethnicity of politicians with great accuracy. Given only the politician's last name, and choosing from more than 14 possible ethnic categories, subjects correctly classified the politician's ethnic group more than 80% of the time.

Subjects viewed the videotaped political speech on a portable DVD player or laptop, using headphones. Only the experimental subjects could hear the speech through the headphones, and subjects answered follow-up questions on their own. When subjects were found in groups, only one subject was recruited per group; this limits the potential that subjects' responses to treatment depended on the treatment assignment of other subjects, which would violate the standard assumption in experimental analysis of “no

²³ We sought to achieve a within-subjects component to the experimental design, in order to reduce the variance of estimated treatment effects. Thus some subjects saw the speech twice, with a different actor each time. Such subjects were randomized to a new treatment condition for each version of the speech. For all subjects, the identity of the actor reading the first version of the speech was assigned at random, with the other actor then shown in the subsequent version (for those subjects who saw two versions). Interestingly, relatively few subjects commented on the fact that the content of the speech was identical across the two versions; we also saw substantial within-subject variance in responses across the two versions of the speech. The results reported below are robust to dropping all data from second versions of the speech.

interference between units” (Cox 1958), or what Rubin (1978) called the “stable unit treatment value assumption” (SUTVA). Follow-up questions focused on the content of the speech and the politician who delivered it. For instance, subjects answered questions about the global quality of the speech, whether the speech made them want to vote for the candidate, and specific questions about candidate attributes such as competence, likeability, and intelligence.

The experimental manipulation was reinforced through repetition of the politician's name. For example, before playing the videotape (and after random assignment to one of the treatment conditions), our research assistants told subjects “we will now show you a political speech by *politician's last name*.²⁴ After playing the videotape, the research assistants then repeated the politician's last name when asking each question about the speech or the candidate.²⁵

Subject recruitment

We recruited experimental subjects by canvassing in almost all of Bamako's neighborhoods (*quartiers*), approaching men and women sitting outside homes (or knocking on doors) and asking if they would participate in a study on political speeches. The experimental population is a convenience sample, but distributions on several measured variables are similar to those given for Bamako by the census. However, the experiment severely under-represents women, who comprise just 26 percent of the experimental population.²⁶

We administered a screening questionnaire to each potential subject, asking for each subjects' first and last name and ethnic identity, along with various other personal information.²⁷ The information gathered during screening allowed us to assign subjects randomly to the treatment conditions, as described below. Some subjects were not

²⁴ For the “no last name” condition described below, *this candidate* replaced “*politician's last name*.”

²⁵ We supervised our research assistants intensively during the initial trial phase of the experiment. In total, one of us (Thad Dunning or Lauren Harrison) was present for 11 percent of interviews, including the trial phase.

²⁶ Our male research assistants insisted it was much more difficult to recruit women, an experience validated by our experience when we worked with our research assistants in the field. In Bamako, women tend to be doing work inside houses or compounds, while men tend to be outside sipping tea.

²⁷ First name and other identifying information of subjects was subsequently discarded, as described in our protocol approved by Yale's human subjects review board.

eligible to participate in the experiment, because their last names were not included in the random assignment matrix described below. For such ineligible cases, we showed one version of the speech to the subject and administered a highly abbreviated post-speech questionnaire; however, ineligible cases are not included in the analysis, because they could not be properly randomized to treatment (even though we selected the last name of the politician in the video they saw at random, from a list of 26 names). Interviews were conducted by Malian research assistants in French, Bambara, or a mixture of the two languages.

Randomization of treatment assignment

In order to assign subjects at random to one of the six treatment or control conditions, we created a large matrix, each row of which corresponds to a Malian last name that we could expect to encounter in the field. Table 2 shows a typical row of the matrix, this one for a subject named Keita from the Malinké/Maninka ethnic group. Last name usually implies ethnicity in Mali, as implied by our experimental design, yet one will occasionally encounter Keitas who identify ethnically as, say, Soninké or Bamanan. In each row of the matrix, we therefore specify not just the last name of the subject but also his or her ethnicity (and we included separate rows of the matrix for last names that may imply one of several ethnicities). We should note, however, that in most cases last name implies a single ethnicity in Mali. As mentioned above, the accuracy with which experimental subjects inferred the ethnicity of politicians, given only their last name, underscores this fact.

The columns of Table 2 give the last names associated with each of our six treatment conditions, for a subject with the last name and ethnicity corresponding to this row of the matrix. For example, the names in the first two columns are all from the same ethnic group, but Sissoko and Konaté (first column) are considered cousins of the Keita, while Diané (second column) is not. The names in the third and fourth columns, on the other hand, are names associated with other ethnic groups, some of them cousins of the Keita (third column) and some of them not (fourth column). In cells with multiple entries, such as in the first, third, and fourth column in Table Two, the politician's assigned last name was selected at random from the names in the cell.

Table 2: A typical row of our random assignment matrix

| | (1) Co-ethnic/ Cousin | (2) Co-ethnic/ Not cousin | (3) Not co-ethnic/ Cousin | (4) Not co-ethnic/ Not cousin | (5) No name | (6) Same name |
|---------------------------|-----------------------------|---------------------------------|---|---|-------------------|---------------------------|
| Keita (Maninka) | 1. Sissoko 2. Konaté | 1. Diané | 1. Doucouré 2. Sacko 3. Sylla 4. Coulibaly 5. Touré | 1. Diallo 2. Cissé 3. Dambélé 4. Théra 5. Dabo 6. Togola 7. Watarra | Pas de nom | Keita (Maninka) |

To construct this random assignment matrix, we first reviewed the secondary literature and conducted interviews with experts on cousinage as well as ordinary Malian informants in Bamako. This enabled us to determine, as an initial matter, the cousins that are associated with many Malian last names and to construct a preliminary, skeletal matrix. Next, we field-tested an initial version of the matrix on 169 subjects; as we discuss below, one of the practical difficulties involved in the experiment involved not just errors in our initial random assignment matrix but also variation across subjects in the identity of their perceived cousins. Data from our initial field trial, as well as additional qualitative information obtained in the field, allowed us to expand and improve the matrix again, and 47 more subjects participated in a second phase of the experiment using our improved matrix. Finally, we revised the matrix in mid-August 2008 and used this final version of the matrix for the main phase of our experiment. The final version of the matrix includes more than 200 names in the left-hand column, including all of the most typical Malian names.²⁸

The experimental data analyzed below come from the roll-out of the experiment implemented from mid-August to early October 2008, using the final version of our random assignment matrix. Ineligible subjects—those whose last names and ethnicities were not found in the left-hand column of our random assignment matrix and who thus

²⁸ The random assignment matrix is posted online at <http://pantheon.yale.edu/~td244/research.html>.

could not be randomized to treatment—were shown a single version of the videotaped speech and then were administered a shortened questionnaire; here, we do not analyze data from ineligible subjects. In total, 113 of the 937 potential subjects screened for inclusion in the study were ineligible, leaving 824 subjects in the experimental population. Eligible subjects were assigned to the six treatment conditions with equal probability of assignment, using a computer-generated list of pseudo-random integers between 1 and 6 (inclusive) to randomize subjects to one of the six conditions. The experimental protocol and the hypotheses to be tested were made public prior to data analysis (Dunning forthcoming).

It bears emphasis that this experiment may constitute a difficult test for the hypothesis that cousinage alliances shape political preferences and ethnic voting patterns. For several reasons, we might expect treatment effects to be weak. First, viewing a videotaped political speech may not closely approximate the experience of attending a real political rally; despite our attempt to create a speech that is similar to typical campaign speeches, the delivery of the speech over a laptop or DVD may make the stimulus somewhat artificial. Also, subjects were also not asked to evaluate politicians who they already know and thus who they can be certain are true candidates for office. Second, changing the last name of the politician across different treatment conditions may not provide a strong stimulus of ethnic identification or cousinage alliance; the content of the speech is identical across treatment conditions and does not prime either ethnic or cousinage ties. Third, even if there are large treatment effects in truth, measurement error and other issues may make it more difficult to estimate these effects (see the discussion below).

Finally, as our discussion of cousinage in the previous section suggests, cousinage relations may be strategically invoked in social and political settings. As Launay (2006: 95, 99) insists, “joking [between cousins] has constantly to be instantiated....Such relationships are virtually never automatic. They are either deliberately staged...or voluntarily instantiated by one or both parties. For there to be a joking relationship, someone actually has to do the joking.” By simply changing the last name of the politician giving a speech, we may fail to capture the subtle and perhaps more powerful ways in which political actors call upon the institution of cousinage to serve strategic

purposes.

Our substantive wager in conducting this experiment, however, was that cousinage-based alliances are sufficiently important that merely changing the last name of the politician would lead to treatment effects. The true effects of cousinage alliances may nonetheless be substantially stronger than those captured by this experiment.

V. Experimental Results

We now turn to analyzing the experimental data. We begin by estimating the effect of treatment assignment on candidate evaluations, that is, on the degree to which subjects support the candidate. We then explore various mechanisms that might help to explain this overall effect, drawing on other questions from our post-treatment questionnaire. Finally, we turn away from intention-to-treat analysis, reporting estimation of effects of treatment on the treated.

Candidate Evaluations: Intention-to-Treat Analysis

The best way to analyze experimental data is often to compare average responses among subjects randomly assigned to the various treatment and control conditions (Freedman 2006, see also Dunning and Hyde 2008). Such intention-to-treat analysis estimates the causal effect of treatment assignment. In a medical trial, for instance, intention-to-treat analysis involves a comparison of subjects who were randomly assigned to receive a new drug and those randomly assigned to receive the placebo, regardless of who actually got the drug. In our experiment, the intention-to-treat principle implies that we ignore (for the moment) whether subjects actually believed the politician to be their co-ethnic or not and whether subjects believed him to be their cousin or not; we will return to the important issue of subjects' perceptions below.

Our most direct and important outcome indicator involves subjects' global preferences for candidates. After viewing the videotaped political speech, subjects were asked

- On a scale of 1 to 7, how much does this speech make you want to vote for

(*name of politician*)?²⁹

For subjects assigned to the control condition in which no last name was given for the politician, *this candidate replaced name of politician*. Higher numbers indicate more favorable evaluations.

Table 3 reports average candidate evaluations by treatment assignment category. As the table shows, the most favorable evaluations, on average, come from subjects assigned to view speeches by candidates who are from their ethnic group and who are also their cousins. Among the other treatment conditions, non-cousins from the same ethnic group rank second, while cousins from a different ethnic group rank third. Subjects expressed the least favorable opinions of candidates from a different ethnic group who are not their cousins.³⁰

Both co-ethnicity and cousinage alliances affect candidate evaluations. For example, pooling cousins and non-cousins (across the columns of Table 3), the average evaluation of co-ethnics is 4.82 (standard error 0.11), while the average evaluation of non-coethnics is 4.18 (s.e. 0.10); the difference of these means is highly statistically significant (t-ratio 4.29). Pooling co-ethnics and non-coethnics (across the rows of Table 3), the average evaluation of cousins is 4.76 (s.e. 0.11), while the average evaluation of non-cousins is 4.23 (s.e. 0.10); again, the difference of means is highly statistically significant (t-ratio 3.52). The effects of co-ethnicity and cousinage alliances are therefore similar in size. On average, assignment to view a speech by a cousin rather than a non-cousin, or by a co-ethnic rather than a non-coethnic, increases subjects' preferences for the candidate by more than one-half of a point, that is, nearly one-half of one standard deviation.³¹ Taken together, assignment to view a speech by a co-ethnic cousin, rather than a non-coethnic non-cousin, raises the evaluation of candidates by an estimated 1.09

²⁹ In French, the question reads: “Sur une échelle de 1 à 7, est-ce que le discours de (*nom du politicien/ce candidat*) vous donne envie de préférer ce candidat? (1= non, pas du tout, 7= oui, tout à fait).” The verb “préférer” may be slightly ambiguous, as it may be interpreted as “to prefer” or “vote for” in this context.

³⁰ Among the control conditions, subjects assigned to the same-name (and perforce, the same ethnicity) condition also favored the candidates; in fact, such subjects gave candidates the second highest evaluations, on average. Subjects assigned to the no-name condition gave candidates lower ratings than, for instance, co-ethnic cousins, but lower ratings than non-coethnic, non-cousins.

³¹ In other words, the evidence does not suggest an interaction between the effects of ethnicity and cousinage alliances: the positive effect of co-ethnicity on candidate evaluations is about the same whether the politician is a cousin or a non-cousin, while the positive effect of cousinage is about the same whether the candidate is a co-ethnic or not.

Table 3: Average Candidate Evaluations, by Treatment Assignment

| | Joking Cousins | Not Joking Cousins |
|---------------------|-----------------------|-----------------------|
| Same Ethnicity | 5.05 (0.15, N=136) | 4.57 (0.16, N=122) |
| Different Ethnicity | 4.44 (0.17, N=124) | 3.96 (0.13, N=152) |
| | Control conditions | |
| No last name | | 4.33 (0.12, N=132) |
| Same last name | | 4.84 (0.15, N=158) |

The cells report average answers to the question, “On a scale of 1 to 7, how much does this speech make you want to vote for (*name of politician/this candidate*)?” Estimated standard errors and the number of subjects randomized to each treatment condition are reported in parentheses.

points on average (compare the top-left and bottom-right cells of the table), a difference that is highly statistically significant (t-ratio 5.54). It is interesting that the evidence suggests additive, rather than interactive effects: the causal effect of co-ethnicity is about the same whether the politician is a joking cousin or not, while the causal effect of a cousinage alliance is about the same whether the politician is from the same or a different ethnic group.³²

Most importantly for our purposes, the evidence suggests that cousinage relations can help to counteract the negative effects of ethnic differences on candidate evaluations. Across all treatment conditions (including the two control conditions), subjects rated non-cousins from a different ethnic group least favorably, giving them an average ranking of

³² The evidence on mechanisms below, however, suggests more interactive effects.

3.96 (s.e. 0.13). Yet the average evaluation of cousins from a different ethnic group (4.44) is statistically indistinguishable from the average evaluation of non-cousins from the same ethnic group (4.57). Thus, cousins from a different ethnic group are evaluated just as favorably as co-ethnics who are not cousins.

We subjected these results to a variety of robustness tests. Non-parametric, two-sample Wilcoxon rank-sum tests, which are based on the median rather than the mean, tell the same story as the parametric analysis: co-ethnics are significantly preferred to non-coethnics and cousins are significantly preferred to non-cousins, while preferences for joking cousins from a different ethnic group and non-cousins from the same ethnic group are statistically indistinguishable. (Full results available from the authors upon request). We also found very similar treatment effects for similar questions, such as “On a scale of 1 to 7, how would you rate the global quality of this speech?”

The treatment effects reported in Table 3 are not enormous, but nor are they trivial: assignment to view a speech by a co-ethnic cousin leads to an increase of more than one point on the seven-point scale, relative to assignment to view a speech by a non co-ethnic non-cousin. Moreover, the intention-to-treat analysis in this section leads to a conservative estimate of the effect of treatment receipt. As we show below, treatment effects are larger once we adjust for some mismatches between treatment assignment and subjects' perceptions—that is, when we estimate the effects of treatment on compliers. In sum, then, the aggregate evidence supports the view that while non-cousins and politicians from different ethnic groups are disfavored, cousinage alliances provide cross-cutting ties that ameliorate the negative evaluations of non-coethnics.

Qualitative data gathered during the experiment provided additional evidence on the political salience of both ethnicity and cousinage. Our research assistants often recorded parenthetical remarks made by subjects during post-treatment interviews, and it was not uncommon for subjects to make disparaging remarks about candidates who were non-cousins from a different ethnic group. For example, a Bamanan subject who saw a speech by a politician named Guindo (a patronym from the Dogon ethnic group) said someone named Guindo could never do a good job as a politician or rise to the challenges of his mandate; another Bamanan said that Dogons “don't know how to lead.” An ethnic Songhai suggested that Bobo ethnics “don't know anything about politics,” while an

ethnic Malinké subject said the same of Dogons. An ethnic Soninké subject, in turn, offered the opinion that “the Malinkés are not intelligent.” Subjects tended to offer more positive comments about co-ethnics, in ways that underscored their co-ethnicity: for instance, a Bamanan subject named Koné saw a speech by a co-ethnic named Diarra and said “the Koné and the Diarra are the same thing” (“les Koné et les Diarra sont la même chose”). One subject who said he paid attention to the candidate's family name in reflecting on his merits said he did so because “it is important to know the identity of the candidate.” This qualitative evidence supports the claim that for many subjects, ethnicity is salient in evaluating politicians and in deciding whether to prefer them.

At the same time, various comments also support the claim that cousinage alliances counteracted the tendency of subjects to disfavor politicians from different ethnic groups and also suggest reasons they may do so. The ability of cousins to reprimand one another particularly seemed to play a role in subjects' more positive evaluations of cousins. As one subject put it, if a joking cousin “is not serious, we will correct him” (“s'il n'est pas sérieux, on va le corriger”). When asked if she would be more susceptible to voting for a cousin, another subject said yes, because “if [the politician] does not respect his promises, I will bring him to heel, because he is a cousin” (“s'il ne respecte pas ses promesses, je vais le rappeler à l'ordre, parce que c'est un senanku”). This evidence echoes the claims of Douyon (2006: 899) that “voters tend to vote for their allies (cousins), saying that in case of problems—administrative, political, or social—the elected ally would more promptly intervene than he would be even with a direct member of his own family.” The logic of this claim may suggest one mechanism through which cousinage alliances affect candidate evaluations, as we discuss below.

Interestingly, next to co-ethnic cousins, the most positive evaluations were given for politicians who shared subjects' last names (see Table 3). Such politicians are co-ethnics and are plausibly perceived as members of the same extended clan, but are not cousins.³³ The qualitative evidence recorded by our research assistants also tended to reinforce the view that subjects held a special regard for politicians sharing their last name. The following comments by subjects were typical: “The Anne family is

³³ Among subjects assigned to see a speech by a politician with the same last name, 98 percent said the politician was not a cousin, while 90 percent said the politician was a co-ethnic.

composed of intellectuals,” from a subject with the last name Anne; or, from a subject named Sacko, “A Sacko is a hard worker.” Another Sacko agreed that “Sackos are very cultured,”³⁴ while a member of the griot caste, a Kouyate, commented that “if a griot (Djely, Kouyate) is a candidate, it is because he is capable of many things.”³⁵ A Malinké subject named Koné said that she paid attention to the politician’s name because the “Konés are nobles” (the Konés were members of the caste of nobles established during the Mali Empire). Finally, a subject named Keita, when asked whether she would be more susceptible of voting for a candidate sharing her family name said “yes, like uncle IBK”—a reference to an opposition candidate during the 2007 presidential elections, whose patronym is Keita. This evidence also reinforces the salience of patronyms as a marker of social identity in Mali—also the basis of cousinage alliances and thus of an identity that cross-cuts ethnic identification.

Finally, the qualitative evidence also suggests several mechanisms that might explain why the treatment effects occur. Notwithstanding the fact that a substantial proportion of subjects (32 percent) said they paid attention to the candidate’s last name in evaluating his merits, a common refrain we heard from subjects was also “kinship does not matter in politics” (“il n’y a pas de parenté dans la politique”). Disparaging remarks about the general lack of credibility of politicians’ promises were also common: for instance, the candidate “would construct his village with the country’s money, forgetting all of his ideals” or the politician “will only defend his own village.” While this might suggest that ethnicity or cousinage would have relatively little effect on candidate evaluations-- politicians as a lot are not a popular group -- ethnicity or cousinage ties might help to counteract politicians’ general lack of credibility: even if politicians cannot be trusted in general to carry out programs that serve the public good, they might be more trusted to serve their co-ethnics or cousins. This suggests one hypothesis to explain the average treatment effects for candidate evaluations. We now more systematically evaluate the evidence for this hypothesis, using other questions drawn from our post-treatment questionnaire.

³⁴ “La famille Anne est composée d’intellectuels,” “Un Sacko est un travailleur,” and “Les Sackos sont très cultivés,” respectively.

³⁵ “Si un Djely est candidat, c'est qu'il est capable de beaucoup de choses.”

Mechanisms: The Role of Credibility

In principle, there could be many different factors that lead subjects to prefer those with whom they have ethnic or cousinage ties. For instance, subjects might find their co-ethnics and their cousins to be more likeable, intelligent, competent, or impressive. This broad class of explanations might be referred to as involving affective evaluations of candidate attributes. Or, subjects find their cousins and co-ethnics more worthy of confidence, might expect them to do a better job once in office, and might think they have better motivations for running for office in the first place. Here, subjects focus on their expectations over the ex-post performance of candidates once elected to office; this broad class of explanations therefore seems to reflect concerns about candidate credibility.

We created two aggregate variables to explore these two broad classes of explanations. The variable *affective* is based on subjects' answers to the following questions:

- In your opinion, is politician's last name/this candidate very unlikeable/unlikeable/neither unlikeable nor likeable/likeable/very likeable?
- In your opinion, is politician's last name/this candidate not at all intelligent/a little intelligent/intelligent/quite intelligent/very intelligent?
- In your opinion, is politician's last name/this candidate not at all competent/a little competent/competent/quite competent/very competent?
- On a scale of 1 to 7, how impressed were you by politician's last name/this candidate?

With 5 points possible for the first three questions and 7 for the last question, the resulting scale runs from 4 to 22. We normalized the scale to run from 0-1 for purposes of comparability with our second variable, *credible*, which is based on the following questions:

- In your opinion, is politician's last name/this candidate not at all worthy of confidence/a little worthy of confidence/worthy of confidence/quite worthy of confidence/very worthy of confidence?
- On a scale of 1 to 7, how good a job would politician's last name/this

candidate do if he were elected?

- On a scale of 1 to 7, would *politician's last name/this candidate* defend others and fight for his ideals?
- On a scale of 1 to 7, does *politician's last name/this candidate* have good motivations for running for office?
- On a scale of 1 to 7, how well will *politician's last name/this candidate* face the challenges of this office?

The final two questions may relate to either affective evaluations or to credibility, though they seem most likely to relate to the latter: after all, a question about a candidate's motivations for running for office presumably relates to expectations about his behavior once in office, as does a question about his ability to deal with the challenges of the office. However, the results presented below are not affected by the exclusion of these questions (or their inclusion in the *affective* variable).³⁶ Again, we normalized responses to these questions about credibility to create an aggregate variable running from 0 to 1.

Table 4 presents the means and standard errors of *affective* and *credible* by treatment assignment.³⁷ As the table suggests, treatment assignment did not strongly shape subjects' affective evaluations of the candidates. While mean affective evaluations of subjects assigned to the same ethnicity, joking cousin condition are significantly higher than mean evaluations of subjects assigned to the different ethnicity, not joking cousin condition (t-ratio 2.66), as are mean affective evaluations of subjects assigned to the same ethnicity, not joking cousin relationship (t-ratio 2.43), most other differences of means across treatment conditions are insignificant for the *affective* variable. Most importantly, affective evaluations of cousins and non-cousins, for subjects assigned to view a speech by a politician from a different ethnic group, are not significantly different from each other.

³⁶ We also asked questions about whether the subject agreed with the politician's political ideas. We did not see any variation by treatment assignment on this variable.

³⁷ In Table 4, the numbers of subjects in each treatment condition differ very slightly from Table 3 due to small amounts of missing data, which stems from non-response on component questions of each index. The small amount of missingness appears consistent across treatment conditions, suggesting the plausibility of that the non-response is unrelated to treatment assignment.

Table 4: Credibility or Affective Evaluations?
(Means of normalized 0-1 scales, by treatment assignment)

| | Joking Cousins | Not Joking Cousins |
|---------------------|--|--|
| Same Ethnicity | <i>affective:</i> 0.59 (0.01, N=135) | <i>affective:</i> 0.59 (0.01, N=121) |
| Different Ethnicity | <i>affective:</i> 0.57 (0.02, N=124) | <i>affective:</i> 0.54 (0.01, N=151) |
| Control conditions | | |
| No last name | <i>affective:</i> 0.56 (0.01, N=129) | <i>credible:</i> 0.47 (0.01, N=129) |
| Same last name | <i>affective:</i> 0.62 (0.02, N=157) | <i>credible:</i> 0.54 (0.02, N=154) |

The cells report average answers to the question, “On a scale of 1 to 7, how much does this speech make you want to prefer Mr. (*candidate's last name*)/this candidate?” Standard errors and numbers of subjects in each condition are in parentheses.

However, treatment assignment does more strongly shape subjects' evaluations of candidates' credibility. As for the *affective* variable, the means of *credible* for subjects assigned to the same ethnicity, joking cousins condition and the same ethnicity, not joking cousins condition are both significantly higher than the mean for subjects assigned to the different ethnicity, not joking cousins condition (t-ratios of 3.31 and 2.74, respectively). Now, however, subjects assigned to see a speech by a cousin from a different ethnic group also rate the candidate as significantly more credible than subjects

assigned to see a non-cousin from a different ethnic group (bolded entries of Table 4). Assignment of subjects to the different ethnicity, joking cousins condition causes *credible* to rise an estimated 0.06 points relative to the different ethnicity, not joking cousins condition, a difference that is highly statistically significant (t-ratio 2.68).

While we present evidence in Table 4 on the aggregated *affective* and *credible* variables, similar patterns hold at the disaggregated level. Of the four component dimensions included in *affective*—likeability, intelligence, competence, and impressiveness—for only one dimension is the difference between cousins and non-cousins of a different ethnicity statistically significant (subjects found their cousins more likeable). On the other hand, of the five component dimensions of *credible*—the extent to which the politician is worthy of confidence, would do a good job in office, would defend others and his ideals once in office, has good motivations for seeking office, and would successfully face the challenges of his mandate—the first three variables are significant at least at the 0.5 level, while the latter two are significant at the 0.1 level. The strongest treatment effects are for the question asking subjects if the politician is worthy of confidence.

As noted above, the qualitative reasons that subjects give for supporting their cousins for office supports our interpretation that concerns about credibility are key. Statements such as “if he is not serious, we will correct him,” or “if [the politician] does not respect his promises, I will bring him to heel, because he is a cousin” suggest that voters feel they can exercise a greater degree of control over ex-post behavior of politicians, when the politician is a cousin. As Douyon (2006: 899, quoted above) suggests, voters may feel that their cousins are more likely than other politicians (at least other non-coethnics) to help them with their administrative, political, or social problems. Similar foci seem to explain why voters prefer politicians with their own family names as well.³⁸

This evidence also appears consistent with claims by other scholars about why ethnicity has an important political role, in sub-Saharan African settings in which

³⁸ One Kouyaté subject, when asked if the politician was worthy of confidence, noted that “the Djelys (griots, Kouyatés) are trustworthy” (“les Djelys sont des hommes de confiance”). Interestingly, the subject named Anne mentioned above, who praised the intellectual merits of the Anne family, also explained in the following terms why the politician named Anne would not defend others or fight for his ideals, once in office: “He will only think of his family” (“il ne va penser qu'à sa famille”).

clientelism is an important part of politics (Fearon 1999b, Keefer and Vlaicu 2005). The evidence thus suggests that concerns about credibility are most likely to explain why co-ethnics are preferred to non-coethnics, and particularly, why subjects prefer cousins to non-cousins among politicians of a different ethnicity.

The Role of Networks

Our discussion of the possible role of clientelist politics in heightening the salience of both cousinage alliances and ethnicity suggests another hypothesis. Various scholars have emphasized the role of social networks in monitoring and sanctioning politicians and voters in clientelist polities (e.g., Stokes 2006; see also Fearon and Laitin 1996). If voters prefer cousins and co-ethnics because they feel they have a greater chance of sanctioning poor ex-post performance, or at least of guaranteeing clientelistic distributions from co-ethnics and from cousins, it may be the case that voters are more tightly linked in social networks with cousins and co-ethnics.

In the post-treatment questionnaire, we asked subjects how many of their close friends and how many of their acquaintances (people they see regularly but who are not as close as friends) bear the same last name as the politician in the video. Possible answers ranged from 0 to 10 or more. It should be borne in mind that this is an observational, not experimental, quantity: we cannot manipulate the number of friends subjects have with people with different last names or randomly assign subjects to different numbers of friends. The status of social networks as a causal variable may therefore be suspect. Nonetheless, since social networks may conceivably play an important role in explaining why subjects do favor their co-ethnics and their cousins, it is useful to examine the available evidence.

Table 5 displays the average number of close friends and acquaintances bearing the last name of the politician in the video, by treatment assignment. On average, subjects assigned to the same ethnicity, joking cousin condition (top-left cell of the table) have nearly three more friends and more than three and one-half more acquaintances bearing the last name of the politician than do subjects assigned to the different ethnicity, not joking cousin condition (bottom-right cell); these differences are highly statistically significant. Among those assigned to view a speech by a politician from a different

ethnic group, subjects in the joking cousin condition have many more friends and acquaintances with the politician's last name than subjects assigned to see a speech by a non-cousin. Strikingly, subjects assigned to the different ethnicity, joking cousin condition actually have more friends and acquaintances with the politician's last name than subjects assigned to the same ethnicity, not joking cousin condition, though the difference is not significant. However, all other differences across the cells of Table 5 are highly statistically significant.

The evidence therefore suggests that social networks may play some role in explaining why subjects prefer co-ethnics and cousins to politicians from different ethnic groups, or those who are not their cousins. Voters are linked socially as well as politically with politicians who are their joking cousins, and this effect is strong even when, or especially when, the politician is from a different ethnic group. Various mechanisms may link social networks to the treatment effects we find. Perhaps subjects are simply more familiar with cousins and co-ethnics, and this greater familiarity may breed greater trust; as suggested above, trust may play a role in explaining the greater credibility of politicians who are cousins or co-ethnics. Or, perhaps social networks allow voters more effectively to monitor and sanction their cousins as well as their co-ethnics (Stokes 2006, Fearon and Laitin 1996). Whatever the mechanism, this observational evidence on subjects' social networks may help explain why subjects prefer candidates who are their co-ethnics and/or their cousins.

Heterogeneity of Treatment Effects

The salience of cousinage alliances and ethnic ties may differ for different subsets of the experimental population. Perhaps more educated people are less prone to evaluate politicians in terms of ethnicity and cousinage alliances. Perhaps cousinage ties are stronger in rural settings than in the capital, so that their effects are stronger for people who are born or have lived outside Bamako.³⁹ Perhaps causal effects differ for men and women, and perhaps more politically-active citizens—those who vote regularly and/or belong to a political party—pay more or less attention to cousinage.

³⁹ For the view that the salience of cousinage relations may be on the decline, see O'Bannon (2008).

Table 5: The role of social networks
**(Mean number of friends and acquaintances with politician's last name,
by treatment assignment)**

| | Joking Cousins | Not Joking Cousins |
|---------------------|--|--|
| Same Ethnicity | <i>friends:</i> 4.73 (0.36, N=135) <i>acquaintances:</i> 7.10 (0.32, N=135) | <i>friends:</i> 3.35 (0.35, N=120) <i>acquaintances:</i> 5.01 (0.40, N=120) |
| Different Ethnicity | <i>friends:</i> 3.74 (0.34, N=124) <i>acquaintances:</i> 5.50 (0.38, N=124) | <i>friends:</i> 1.75 (0.22, N=151) <i>acquaintances:</i> 3.52 (0.32, N=151) |
| | Control conditions | |
| No last name | | -- |
| Same last name | <i>friends:</i> 6.89 (0.31, N=151) | <i>acquaintances:</i> 8.11 (0.27, N=152) |

The cells report average answers to the questions, “How many of your close friends have the family name of *politician's last name?*” and “How many of your acquaintances (people whom you see regularly but who are not as close as friends) have the family name *politician's last name?*” Possible answers run from 0 to 10 or more (which is coded as 10). Standard errors are in parentheses.

We evaluate these hypotheses but do not report full results here.⁴⁰ In brief, there is not significant treatment effect heterogeneity by education; if anything, treatment effects are stronger for more educated people (those who have at least the baccalaureat)

⁴⁰ Results available from the authors upon request, or see <http://pantheon.yale.edu/~td244/research.html>.

than for less educated people. Nor does gender appear to matter greatly (though estimated treatment effects are similar in size for both gender, some effects are not significant for women, presumably because of the smaller size of this sub-group in the experimental population). We do find some evidence of treatment effect heterogeneity by age, with treatment effects somewhat stronger for subjects who are above-average age.⁴¹ Finally, while we have not yet fully explored heterogeneity of treatment effects by political participation (for example, whether subjects are members of a political party) or place of birth, we have found treatment effect heterogeneity is not important for stratifying variables such as whether the subject is registered to vote, or whether the subject has lived outside of Bamako.

The Impact of Cousinage: Estimating the Effect of Treatment on the Treated

Before concluding our discussion of the experimental analysis, we turn to a final topic. One inferential issue in our experiment is that subjects may not perceive themselves to be in the intended cell of Table 1—that is, the treatment condition to which they had been randomly assigned. Consider an analogy to experimental crossover. In a typical experiment to evaluate a new medical drug, not all subjects will participate in the protocol: some subjects assigned to the treatment regime may refuse the drug, while subjects assigned to control may seek out the treatment. Crossover from the treatment to the control arm of the experiment tends to dilute the effects of treatment assignment; intention-to-treat analysis will then typically provide a conservative estimate of the effect of treatment receipt (Freedman 2006).

In our experiment, the analogous issue is that some subjects may perceive a subject-politician pair other than the one we intended. This can be problematic. After all, what we care about in this study is the effect of subject perceptions—we want to know how *perceiving* oneself as being a cousin or not being a cousin of the politician, or his co-ethnic or not, shapes evaluations of the candidate's speech.

⁴¹ The average age in the experimental population is about 32 years old (the average year of birth is 1976). The difference between the different ethnicity, joking cousin and different ethnicity, not joking cousin conditions is statistically significant for subjects of above-average age, while it is not quite significant for subjects of below-average age ($p=0.16$ for a two-tailed test, $p=0.08$ for a one-tailed test). Because the above-average age group is much smaller than the below-average group in both treatment conditions ($N=44$ and $N=53$ versus $N=80$ and $N=99$), this provides some evidence of treatment effect heterogeneity. Estimated treatment effects are similar in size for both groups, however.

Two questions on our post-speech questionnaire allow us to assess whether subjects perceived themselves to be in the intended cell. First, we asked subjects to identify the ethnic group to which the politician in the video belonged. Second, we also asked subjects whether the politician in the video was the subject's joking cousin or not. Using these two questions as well as our screening question asking subjects the ethnic group with which they themselves identified, we are able to code whether subjects assigned to a particular treatment condition in fact perceived the subject-politician pair as we had intended. For instance, a subject who reported the politician's ethnicity as the same as his or her own self-reported ethnic group, and who said that the politician was his or her cousin, would be scored as self-assigned to the co-ethnic, cousin condition.

Table 4 cross-tabulates treatment assignment and treatment receipt, where treatment receipt indicates the condition which subjects assigned themselves. The mismatch between treatment assignment and treatment receipt is worse for some treatment conditions than for others.⁴² Subjects assigned to the same ethnicity, joking cousin and same ethnicity, not joking cousin tended to perceive themselves in each of the other three treatment conditions with some regularity. On the other hand, subjects assigned to the different ethnicity, joking cousin and different ethnicity, not joking cousin conditions tended only to perceive themselves in one of those two treatment conditions (bolded cells in Table 4).

In this experiment, mismatches between treatment assignment and treatment receipt probably occurred for two reasons. First, correctly classifying cousinage relations for over 200 Malian last names is a complex and imperfect science. Even after improving our random assignment matrix through beta trials as well as qualitative interviews, the matrix likely remained imperfect.⁴³ Second and perhaps more importantly, however, even if we could create a perfectly accurate matrix of cousinage relations, as understood by key informants, people vary in their knowledge of cousinage

⁴² In general, as mentioned above, subjects inferred the intended ethnicity of politicians with great accuracy, correctly classifying the politician's ethnic group more than 80% of the time. Subjects more frequently labeled cousins as non-cousins, or non-cousins as cousins.

⁴³ There was also a tradeoff involved in limiting the names of potential subjects. On the one hand, cousinage relations are much better understood by us (and by Malians) for a few very common names, such as Keita, Coulibaly, Touré, or Cissé, than for less common names, so we might have had a better overall accuracy/compliance rate had we limited the study population to subjects with such last names. On the other hand, limiting the number of names implied more inefficient and costly subject recruitment.

relations in Mali. For instance, are the Keita and the Doucouré (third column of Table 2) really cousins? Reasonable minds can apparently disagree. As one leading expert on cousinage puts it, “The question of which *jamu* [patronym] actually jokes with whom is subject to considerable indeterminacy. Lists of the joking partners of any given *jamu* may vary from community to community, or even from individual speaker to speaker” (Launay 2006: 799). Our own experience in the field validated this observation.

Table 4: Treatment Assignment and Treatment Receipt

| | | Treatment Receipt | | | | Totals |
|----------------------|--|-------------------------------|-----------------------------------|------------------------------------|--|--------|
| | | Same Ethnicity, Joking Cousin | Same Ethnicity, Not Joking Cousin | Different Ethnicity, Joking Cousin | Different Ethnicity, Not Joking Cousin | |
| Treatment Assignment | Same Ethnicity, Joking Cousin | 37 | 69 | 12 | 18 | 136 |
| | Same Ethnicity, Not Joking Cousin | 26 | 67 | 24 | 5 | 122 |
| | Different Ethnicity, Joking Cousin | 1 | 3 | 98 | 22 | 124 |
| | Different Ethnicity, Not Joking Cousin | 0 | 3 | 18 | 131 | 152 |
| | | Totals | 64 | 142 | 152 | 173 |

In designing the experiment, we countered this problem to the extent possible by limiting names in the first and third column of Table 2 to those cousinage links that are in fact widely understood (everyone agrees that the Keita and the Coulibaly are cousins), while also only including names in the second and fourth cell that we thought would maximize the chance of correct identification as non-cousins. After initial beta trials in the field, we were able to substantially improve the fit between treatment assignment and treatment perception, such that the intention-to-treat analysis reported above provides a

valid (if perhaps conservative) data-analytic technique. Nonetheless, as Table 4 suggests, some mismatch between treatment assignment and treatment perception remained despite our efforts.

One feasible technique for adjusting for experimental crossover involves estimation of the effect of treatment on compliers, sometimes called the effect of treatment on the treated. Here, essentially, treatment assignment is used as an instrumental variable for treatment receipt.⁴⁴ In the present setting, full instrumental variables estimation for the multi-valued treatment variable is complicated. As Table 4 suggests, subjects crossed over from each assigned treatment condition to several other conditions, making more complex the estimation of effects of treatment on the treated for all cells in Table 1. The requisite identifying restrictions may not plausible in this setting (see Freedman 2006, Imbens and Angrist 1994).

However, Table 4 suggests that it may be reasonable to adjust the data for purposes of comparing subjects assigned to view speeches by cousins and non-cousins, among politicians from a different ethnic group. Note for example that nearly all of the “crossover” from assignment to the different ethnicity, joking cousin (third row of Table 4) is to the different ethnicity, not joking cousin condition (fourth column of Table 4), while nearly all of the crossover from assignment to the different ethnicity, not joking cousin (fourth row of Table 4) is to the different ethnicity, joking cousin condition (third column of Table 4). In addition, treatment assignment provides a strong instrumental variable for these conditions. For example, nearly 80 percent (98/124) of subjects assigned to the different ethnicity, joking cousin condition correctly perceived themselves in that condition, while the same held for more than 85 percent (131/152) of subjects assigned to the different ethnicity, not joking cousin condition.

⁴⁴ See Freedman 2006 or Imbens and Angrist 1994 for discussion, and Freedman, Petitti, and Robins 2004 for an application.

Ignoring the very minimal degree of crossover from these two conditions to other treatments, then, we can estimate the effect of treatment on the treated (ETT), for the change from a non-cousin to a cousin among politicians from a different ethnic group, as follows:

$$ETT = \frac{4.44 - 3.96}{0.79 - 0.12} = 0.72 \quad (1)$$

The numerator of the estimator in equation (1) is the estimated intention-to-treat parameter, which is calculated by subtracting the bottom-right cell of Table 3 from the bottom-left cell. The denominator is the net “contact rate,” namely, the proportion of subjects assigned to the different ethnicity, joking cousin condition who perceive themselves to be viewing a speech by a joking cousin from a different ethnic group (98/124) minus the proportion of subjects assigned to the different ethnicity, not joking cousin condition who also perceive themselves to be viewing a speech by a joking cousin from a different ethnic group (18/151). See Freedman (2006) as well as Imbens and Angrist (1994) for further discussion of this estimator. The evidence therefore suggests that the true effect of treatment is stronger than the intention-to-treat analysis above revealed: on average, perceiving oneself to be viewing a speech by a joking cousin of a different ethnic group raised evaluations of the candidate by an estimated 0.72 points, relative to perceiving oneself to be viewing a speech by non-cousin of a different ethnicity.

VI. Conclusion

Social scientists have often suggested that cross-cutting cleavages may shape patterns of ethnic conflict and cooperation. In this study, we assess the extent to which cross-cutting ties can elucidate the puzzling absence of ethnic conflict in Mali, an ethnically-heterogenous sub-Saharan African country, and especially the extent to which it can explain the relative unimportance of ethnicity in ethnic politics. We develop an experimental research design that helps us explore the twin effects of co-ethnicity and

cousinage alliances in voter evaluations of politicians.

Our experimental results suggest several conclusions. First, cousinage alliances and co-ethnicity do indeed shape voter evaluations of politicians. Contrary to the view that ethnic identity is not politically salient in Mali, we find that assignment to view a speech by a co-ethnic political candidate makes subjects' evaluations of the candidate significantly more positive. The effect of cousinage alliances is similar in size to the effect of co-ethnicity: subjects' evaluations of their joking cousins are increased relative to their non-cousins by about the same amount as their evaluations of co-ethnics, relative to non-coethnics.

Second, however, the experimental evidence suggests that cousinage alliances and co-ethnicity may each appear to lack political salience, in observational data, because of the cross-cutting nature of cousinage and ethnic cleavages. We find that cousinage ties counteract the negative effects of ethnic difference on candidate evaluations: for instance, subjects evaluate their non-coethnic cousins as favorably as they do their co-ethnic non-cousins. Two voters of the same ethnic group may have diverse preferences over a political candidate, because their cousinage relations with that candidate differ. Thus, cousinage may well attenuate the aggregate relationship between ethnicity and individual vote choice, helping to account for the low correlation between these variables in survey data.

Third, the mechanism through which these effects take place seems to have to do mostly with expectations over ex-post political behavior, once candidates are elected to office. Affective evaluations, while important, do not explain why joking cousins are preferred to non-cousins, among politicians of a different ethnic group. Instead, the credibility of campaign promises by cousins and co-ethnics seem higher than those by non-cousins and non-coethnics.

This last observation may suggest the role of clientelistic politics in heightening the salience of cousinage. As a range of scholars have suggested, ethnicity may take on special importance in settings rife with clientelistic or patronage politics. Because ethnicity provides a visible marker of identity, politicians may find it advantageous to target pork to co-ethnics, and for a range of reasons their promises to do so may be especially credible (Bates 1983, Fearon 1999b, Keefer and Vlaicu 2005). The same may

be true of cousinage. Our experimental data, as well as qualitative evidence from the field, suggest that voters anticipate being able to make requests of as well as sanction their cousins, once their cousins are elected to office.

Finally, however, our research suggests why such clientelistic impulses may not end up reifying political conflicts between ethnic groups in Mali, unlike, say, in Kenya. Our findings suggest several useful avenues for future research. For example, because cousinage ties cross-cut ethnic ties, the impulse to build political coalitions along ethnic lines may be tempered by the ability of politicians to distribute goods along cousinage lines—plausibly making ethnic parties less attractive to at least some members of each ethnic group, and the construction of ethnic coalitions therefore less advantageous to politicians. While future research should probe such issues further, the experimental evidence we present in this paper represents an important first step, as it supports the claim that cross-cutting ties inhibit ethnic voting in Mali. Related experimental designs may be usefully employed in Mali as well as other contexts to explore other claims about the effects of cross-cutting cleavages on ethnic politics.

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