

## **African Borders as Sources of Natural Experiments\***

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African borders provide valuable sources of natural experiments.<sup>1</sup> They lend themselves to this purpose because the boundaries between African countries were, for the most part, demarcated with little attention to the social or cultural “facts on the ground” and can thus be treated as arbitrary. This permits the researcher to treat the division of peoples by the drawing of the border as analogous to the random assignment of subjects to different treatments, and to explain differences in attitudes, behavior, or beliefs found among the populations living on either side of the border as a consequence of differential exposure to the national social and political currents in each country. This has great methodological advantages, and it is for this reason that growing numbers of researchers have situated their work astride African country borders (e.g., Laitin 1986; Firmin-Sellers 2000; Miles and Rochefort 1991; Miles 1994; MacLean 2004; Miguel 2004; Posner 2004).<sup>2</sup>

Along with the methodological strength of these studies, however, comes a critical weakness that threatens to undermine the power of cross-border work in Africa. The problem stems from the fact that, unlike in pure experiments, the treatment groups in cross-border natural experiments are not closed. African borders tend to be extremely porous and residents living in the border area can fairly easily move from one side to the other, thereby undermining the independence of the treatment groups. Population movements are also possible, within each country, into or out of the border area. Ironically, the more that a researcher tries to take advantage of the inherent controls offered by cross-border studies by locating his or her research sites very close to the border (as, for example, in Miles 1994 or Posner 2004), the more vulnerable the researcher is to this potential problem. In the case of African cross-border studies, the likelihood of entry into and/or exit from the treatment groups is heightened by the fact that the drawing of the borders (and, with them, the allocation of subjects to treatment groups) took place more than a hundred years before the measurements of the outcomes of interest were taken.<sup>3</sup> There are thus not only few safeguards against movement into, out of, and between treatment groups, but lots of time for this to have happened.

If migration into, out of, or across treatment groups was entirely random, then the methodological cost is simply that the true effects of the country treatments will be attenuated, and this may lead to null findings when the true effects are, in fact, sizeable. The more worrisome problem occurs when the movements were not random—for

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<sup>1</sup> Dunning (forthcoming) provides a useful overview of the use of natural experiments in Political Science, as well as a discussion of many of the methodological issues discussed here.

<sup>2</sup> Africa is not the only place where researchers have taken advantage of the analytic leverage provided by the borders between countries. Sahlins (1989) explores the division of the Cerdanya region along the French-Spanish border in the Pyrenees; Elton (2005) analyzes the division of the Jivaro people on the Peru-Ecuador border; and Paasi (2005) examines the Karelia border separating Russia and Finland. In addition a number of other studies in Africa situate themselves in areas that span country boundaries (Konings 2005; Asiwaju 2003; Phiri 1985), though they do not self-consciously exploit the country border as the source of a natural experiment.

<sup>3</sup> Most African borders were demarcated at the end of the 19<sup>th</sup> century, many at the Berlin Conference of 1885. The first studies that explicitly employed African borders as sources of natural experiments did not begin to appear until the 1990s.

example if residents on one side of the border migrated across it precisely to take advantage of the political or social circumstances whose impact we seek to assess. If this is the case, then we risk identifying spurious effects of country treatments that are really products of endogenous sorting. This inferential pitfall threatens to undermine the otherwise powerful advantages afforded by cross-border studies in Africa.

In this paper, we suggest a step-by-step approach for dealing with this potential problem. We then demonstrate the use of this approach by applying it to a cross-border study on the salience of religious identities among people living on either side of the boundary between Cote d'Ivoire and Burkina Faso.

### *African Borders as Sources of Natural Experiments*

Social scientists typically make causal inferences through observational studies: analyses where the variables that the researcher is studying acquire their values through the unfolding of real-world events that are outside the researcher's control (Collier et al 2004). The problem with observational studies is that they make it difficult or impossible to rule out the possibility that other, unmeasured variables may be the cause of the observed relationship between the variables of interest. For example, suppose we are interested in the linkage between smoking and cancer. To investigate the connection, we might recruit a large number of people, divide them into smokers and non-smokers, and compare the cancer rates of each group. If we found higher cancer rates among the smokers, we might be tempted to conclude that smoking does indeed cause cancer. But we would be wrong to reach such a conclusion unless we could first rule out the possibility that other unobserved factors – for example, poverty or stress or diet – might have caused people both to smoke more and to be more susceptible to disease. If we just took smokers and non-smokers “as we found them” – which is what researchers in observational studies do – then we would be unable to rule out these potentially serious competing explanations. This is the weakness of observational studies. They may be useful for identifying possible causal effects, but they cannot reliably establish causation (Gerber et al 2004).

Experimental studies, such as those commonly undertaken in the natural sciences, do permit causal inferences to be made. In experimental research, the values on the key independent variables are products of the random assignment of subjects by the researcher to different treatments. It is precisely the random assignment of subjects to treatments that permits the researcher to rule out confounding explanations of the sort described above. The analog to the cancer and smoking example, set up as an experiment, would be to choose subjects at random and then randomly assign them to two groups. The first group would be forced to smoke; the second group would be prevented from doing so during the course of the experiment. Since the assignment to the smoking treatment is randomized, we can safely rule out the possibility that some unmeasured factor might be responsible for the decision on the part of the subjects in the first group to smoke, or for the health outcomes that we observe. For social scientists, the major drawback to experimental studies is that they are difficult to undertake outside

of the laboratory (and, as this particular illustration makes clear, also sometimes unethical). Also, experiments done in the lab often have questionable validity in the real world. Nonetheless, precisely because of their advantages in making causal inferences, the use of experimental methods in the social sciences – increasingly through both laboratory experiments and field studies (e.g., experiments that take place outside the laboratory) – is growing (McDermott 2002; Gerring and McDermott 2006).

Natural experiments occupy a middle ground between observational and experimental studies. Like observational studies, they derive from naturally occurring phenomena rather than the manipulation of the experimenter. But, unlike observational studies, they permit the researcher to “make a credible claim that the assignment of the non-experimental subjects to treatment and control conditions is ‘as if’ random” (Dunning forthcoming). They therefore combine the methodological power of experiments with the “out of the lab” flexibility of observational studies. The trick, however, is to find them, for good ones are as rare as they are valuable.

Africa’s arbitrary borders provide a rich source of potential natural experiments. This is because African boundaries were, for the most part, arrived at with no reference at all to the social or cultural characteristics of the people they partitioned (Asiwaju 1985; Englebert et al 2002; Hargreaves 2005). A clear indication of the arbitrary nature of Africa’s borders is the fact that 44% of them follow meridians or parallels; another 30% follow other rectilinear or curved lines; and the remaining 26% follow geographical features such as rivers and watersheds (Sautter 1982: 8, cited in Englebert 2000: 88). Their disregard for the populations they bisect is reflected in Asiwaju’s (1985) estimate that Africa’s 104 distinct borders divide 177 cultural or ethnic groups. With a handful of exceptions, Africa’s borders can thus be taken as exogenous to all the potentially relevant sociological, cultural and historical facts on the ground.<sup>4</sup>

If we accept the physical location of African borders as arbitrary, then we can treat the division of people on either side of a given international boundary as a random assignment of subjects to different treatments – the treatments provided by the different political institutions, country-level demographics, histories, and public policies in effect in the two countries that the border separates. Comparisons can thus be made of outcomes that differ on either side of the border, and any differences that we may find can be attributed to differences in the treatments to which the people in each country have been exposed.

This power has been exploited by a growing number of researchers. For example, Miles and Rochefort (1991) take advantage of the arbitrary boundary between Nigeria and Niger to explore the factors associated with national identification. They find that the salience of national identity varies among villagers living on either side of the border, and they attribute this variance to the differential degrees of colonial intrusion in the two countries (also see Miles 1994). Laitin (1986) takes advantage of the bisection of the

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<sup>4</sup> For an argument that the artificiality of a state’s borders can be operationalized in terms of the number of ethnic groups they divide and the extent to which they resemble geometric features, see Englebert (2000) and Alesina, Easterly and Matuszeski (2006).

Yoruba people by the Nigeria-Benin border to test attachments to city-state versus world religions. His argument that the greater weight attached to city-state identities is a product of the British colonial experience is supported by the fact that city-state identities are comparatively weaker among Yoruba living across the border in Benin, where the colonial legacy was Francophone. Firmin-Sellers (2000) also locates her study along a border that separates British and French colonial legacies—that between Côte d’Ivoire and Ghana—which she uses to demonstrate the impact of colonial institutions on contemporary patterns of property rights, landholding patterns and class formation. MacLean (2004) conducts a “carefully controlled experiment” along the same border to show that participation in homogeneous voluntary associations (which were present in Ghana but not Côte d’Ivoire) strengthened values associated with democratic consolidation. Miguel (2004) uses the border between Kenya and Tanzania to develop an argument about the impact of nation-building policies on local-level inter-ethnic cooperation. He accounts for differences in public goods contributions by pointing to different nation-building policies pursued by the post-independence leaders in each country. Posner (2004) exploits the division of the Chewa and Tumbuka ethnic groups by the Zambia-Malawi border to investigate the conditions under which cultural cleavages become politically salient. He finds that divisions between the Chewa and Tumbuka are much more politically salient in Malawi than in Zambia, and he attributes this difference to the different sizes of the Chewa and Tumbuka communities in each country—large relative to the national political arena in Malawi; small relative to the national political arena in Zambia.

By taking advantage of the arbitrary nature of African country borders, all of these authors reap large methodological dividends. In every case, the power of the inferences they draw is strengthened by their ability to treat the comparison as a natural experiment in which the people whose attitudes or behavior are being measured have been assigned to two different treatments. Despite their strengths, however, all of these studies are vulnerable, to a greater or lesser degree, to the problem that treatment groups in cross-border studies are not closed.<sup>5</sup> To deal with the potential inferential difficulties that arise from this fact, researchers seeking to use African borders as sources of a natural experiment must ask a series of pointed questions about their projects. The next section describes these questions and outlines a series of remedies that can be applied if they are

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<sup>5</sup> Cross-border studies are also potentially vulnerable to the criticism that the country boundary may not have been entirely arbitrarily drawn. For the border between two countries to be a source of a natural experiment, the assignment of subjects to one side of the border or the other must be “as if random” (Dunning forthcoming). This will only be the case if the border was drawn without any regard for either the characteristics of the people it partitioned or the topography, history, or other factors that might affect those people’s behavior or preferences. Many, perhaps most, African borders meet these strict conditions. But some of them do not. Brownlie notes that the borders of Swaziland, for example, were drawn with reference to “tribal” distribution; the northern and western portions of the Nigeria border were built on traditional political structures; the French and British division of Cameroon was intended to avoid artificial partitioning of ethnological groups; borders between Ethiopia and Kenya, Sudan and Uganda, Guinea and Senegal, Angola and Zambia, and Botswana and Rhodesia (among several others) were also drawn with either ethnic group cohesion or respect for pre-existing polities in mind (Brownlie 1979: 6-7). Thus, a researcher seeking to use African borders as the source of a natural experiment must begin his or her study by justifying that the borders in question can in fact be taken as arbitrary.

deemed to be necessary. The section that follows then applies these lessons and remedies to a study designed to account for the relative salience of religious identities among respondents living on either side of the border between Côte d'Ivoire and Burkina Faso.

### *The Step-by-Step Approach*

Most cross-border studies follow a similar format. The researcher collects data on some outcome of interest among subjects on either side of a national boundary. Response patterns are compared across the two cohorts of subjects—often with a simple difference-of-means test. And an inference is made about the differential impact of the two country treatments based on the results of this test. Because this inference depends critically on the assumptions of pre-treatment equivalence (i.e., that the two cohorts were identical before they were partitioned by the drawing of the border) and experimental control (i.e., that the subjects did not migrate either between the two cohorts or in or out of the study area between the time the border was drawn and the time the data was collected), it is essential that a researcher explore whether these assumptions hold and, if not, take steps to minimize the problems that arise as a consequence. To accomplish this, we recommend that the researcher follow the steps illustrated in Figure 1.

[Figure 1 here]

First, the researcher must ask if there are any noticeable differences in the observable characteristics of the two cross-border treatment groups. This question represents Node 1 on the decision tree. If there are no differences, then the arbitrary demarcation of the border can be treated as having been an instrument for random assignment, and the researcher can treat the findings of the study as the product of a natural experiment. If differences do exist, the researcher should continue to the question at Node 2.

At Node 2, the researcher should ask: Are the differences in treatment groups with respect to any potentially relevant variables? Differences that in no way pertain to the outcome that the researcher is trying to explain are not critical (although they do raise questions about the applicability of the “natural experiment” moniker), and any differences that we observe among subjects on either side of the border can be reasonably treated as outcomes of treatment effects. However, if differences in the treatment groups do exist and are related to the outcome of interest, a third step is required.

At Node 3, the researcher should determine whether the cross-country differences that are potentially relevant to the outcome arose before or after the demarcation of the border. In either case, we suggest that at this point the cross-border study ceases to be a natural experiment, because the assumption of “as if random” assignment (Dunning forthcoming) is violated. Nevertheless, valid causal inferences can still be drawn from the cross-border study. If the researcher finds, through careful historical research, that the relevant differences arose *before* the drawing of the boundary (approximately a century ago), then the value of the study can be preserved simply by controlling for those

differences. The claim that the border was arbitrary would be weakened by pre-existing differences, but if the researcher controls for those differences in the analysis, the causal effect of living on one side of the border as opposed to the other can still be determined. In the absence of convincing historical evidence that the differences existed prior to the demarcation of the border, however, a conservative research approach dictates that those potentially relevant differences be treated as having arisen *after* the drawing of the border. In this case, the differences might be correlated with the national treatment whose effects the researcher is seeking to test, raising the possibility of spurious causation. To address this concern, the researcher should proceed to Node 4.

At Node 4, the researcher should ask if those potentially relevant differences that arose after the drawing of the boundary are the product of post-assignment sorting. In other words, did the drawing of the border cause individuals to re-group themselves—thereby changing the composition of populations on each side of the border—for reasons potentially related to the outcome of interest? If not, the implication is that those differences came about through happenstance and do not pose a threat to the study’s validity. If, on the other hand, individuals (either survey respondents or, more likely, their ancestors) sorted themselves in response to their assignment to one country or the other, it could be the case that what appears to be a national treatment effect is actually a sorting effect. In this case, the researcher must control for characteristics that may have caused this post-assignment sorting.<sup>6</sup>

By following these steps, researchers will be able to overcome the problem of non-closed treatment groups. If any of the answers at nodes 1-4 are “yes,” the cross border study will, strictly speaking, cease to be a natural experiment. However, by including the suggested controls in his or her analysis, researchers will still be in a position to draw valid causal inferences from their studies.

To this point, the approach we suggest for rescuing cross-border experiments from the pitfall of non-closed treatment groups has been purely in the abstract. We now provide a beginning-to-end application of the approach using a real project to demonstrate how decisions are to be made in practice at each step in the process.

### ***Application: Religious Identity on the Burkina Faso-Côte d’Ivoire Border***

The present border between Burkina Faso and Côte d’Ivoire was originally established in the 1880s as the boundary between administrative districts (*cercles*) in what was then French Sudan.<sup>7</sup> It runs from west to east, from the intersection of the Bani and Léraba Rivers in the southwestern corner of Mali to the Black Volta River on the

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<sup>6</sup> It may appear that Node 4 is not necessary, because, starting at Node 3, all potentially relevant differences are controlled for. The point of the fourth step, however, is to distinguish the reasons for which controls may be needed.

<sup>7</sup> The *cercles* were Gaoua and Bobo (in present day Burkina Faso) and Bondoukou and Kong (in present day Côte d’Ivoire). French Sudan was divided into Côte d’Ivoire and Haut-Senegal-Niger in 1899 (Brownlie 1979: 375). In 1919, Haut-Senegal-Niger was divided and the colony of Upper Volta created. Upper Volta was renamed Burkina Faso in 1984.

western edge of Ghana. As with many African borders, there is no evidence that the colonial officials who demarcated the boundary followed anything other than the commonplace practice of tracing natural geographic features in deciding where it would be located.<sup>8</sup> Figure 2 illustrates that most of the 584-kilometer border was drawn along seasonal rivers and streams; the remaining portions were little more than an exercise in connecting the dots. Because this boundary was not drawn with respect to the social, cultural, or economic conditions of populations on the ground, the preliminary condition exists for a natural experiment.

[Figure 2 here]

We take advantage of the arbitrary nature of the border's demarcation to investigate the impact of politics on religious identification. In keeping with the findings of the large literature on the context-dependence of social identities (Mitchell 1956; Young 1976; Horowitz 1985; Chandra 2004; Eifert, Miguel and Posner 2007), we provide evidence that the salience of religion is a product of the environment in which a person is located—in particular, whether the principal lines of conflict in the political system correspond with the society's religious divisions. Specifically, we show that, among otherwise identical people living on either side of the Burkina Faso-Côte d'Ivoire border, those living on the Côte d'Ivoire side are nearly three times as likely to say they rank religion as their most important social identity as those living just a few kilometers away across the border in Burkina Faso. We attribute this difference to the fact that religion has been politicized as a consequence of Côte d'Ivoire's civil war whereas peace has prevailed and the politicization of religion has been absent in Burkina Faso.

### *Research Design*

To document the differences in religious identification in each country, we administered a questionnaire to 197 respondents distributed across two pairs of research sites—one rural pair and one urban pair (see Figure 3). The two rural locations are the predominantly Lobi villages of Boussoukoula, Burkina Faso and Kalamou, Côte d'Ivoire. The villages are situated approximately seven km apart, directly across the border from each other in the eastern section of the border zone, near Ghana. The two urban locations, approximately 50 kilometers apart, are the market towns of Niangoloko, Burkina Faso and Ouangolodougou, Côte d'Ivoire (populations roughly 30,000 each). Both towns lie on the main road connecting Côte d'Ivoire and Burkina Faso, approximately 300 km west of the two rural research sites. Our *a priori* assumption is that, because the paired villages are so close to an arbitrarily imposed border, there are no noticeable differences in the treatment groups.

[Figure 3 here]

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<sup>8</sup> Maurice Delafosse, a French colonial administrator who spent 16 years in the region and who published an account of the people of the area, provides detailed descriptions of the habits of dress, scarification, circumcision, and weaponry of the communities living in the border region but makes no mention of any dissimilarities in those living on either side (Delafosse 1912: 327-51).



Approximately 50 respondents were recruited in each site via a random stratified sampling procedure, with stratification by age and gender. The surveys were administered in Dioula and/or French in the two urban sites and in Lobiri (and occasionally in French) in the two rural locations. To probe the salience of religious identities, we asked our respondents four simple questions:

- Each person has several ways of identifying him/herself: nationality, religion, ethnic group, occupation, gender, personality, point of view, etc. For you, what identity is most important?
- After that, what identity would you place in second position?
- Could you marry a person of a different religion?
- To whom do you feel closer: a person of your country who is not of your religion, or a person of your religion who is not of your country?

Questions regarding self-identification can be posed in a variety of ways. Open-ended questions ensure that responses are free from constraints (Bratton et al 2005: 56), but in pre-test questioning we found that an open-ended question regarding self-identification (with no example responses provided) was difficult for some respondents to comprehend. Another approach is to provide respondents with a list of responses that can be ranked (as in Miles and Rochefort 1991). The advantage of this method is that responses are comparable across research subjects, but the limitation is that respondents are constrained to a fixed set of choices that may or may not contain the categories that respondents would have chosen on their own. We chose a middle ground, offering a systematic set of examples but allowing respondents to answer in any way they desired.<sup>9</sup> We also recorded individuals' secondary identities, which provided some of the insight that a ranking conveys.

Figure 4 shows the frequency distribution of responses to the first question—"Which identity is most important to you?"—separated by country. As the figure makes clear, respondents in Côte d'Ivoire were far more likely than those in Burkina Faso to identify themselves primarily according to their religion (28 percent vs. 10 percent), and far less likely to mention nationality (25 percent vs. 42 percent).<sup>10</sup> These two cross-country differences are both significant at the  $p < .01$  level in two-tailed tests. The other differences (with respect to ethnicity, occupation, gender, and other identities) are not

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<sup>9</sup> See the structure of the first key question above. Research has shown that these responses are generally among the most common for questions regarding self-identification (Miles 1994: 48-49), so we feel comfortable that this design clarified the question without biasing respondents. The Afrobarometer Surveys use a very similar question design to get at the salience of competing categories of social identity (see Eifert, Miguel and Posner 2007).

<sup>10</sup> The figure of 28 percent who prioritize their religious identity in Côte d'Ivoire is high not just relative to Burkina Faso, but also vis-à-vis other African countries. Compared to results from the 16 countries in which the same question was asked in Round 2 of the Afrobarometer Survey, the figure from our data for Côte d'Ivoire is higher than 14 of the 16. Only Ghana (33 percent) and Senegal (44 percent) were higher.

statistically significant. Individuals in Côte d’Ivoire also ranked religion among their top two identities—a figure derived from a combination of responses to the first two key questions—at a much higher rate (50 percent vs. 23 percent) than did individuals in Burkina. This difference is also significant at the  $p < .01$  level in a two-tailed test.

[Figure 4 here]

The two key questions regarding social attitudes and religion tell a similar story. Figure 5 shows the comparative frequency distributions of respondents 1) acknowledging that they could marry someone of a different religion, and 2) who feel closer to a person of their country who does not share their religion, as opposed to a person of their religion but a different country. Respondents at the Ivoirian sites are statistically significantly less likely than their Burkinabé counterparts to say that they could marry a person of a different religion (57 percent vs. 74 percent,  $p < .01$ ). Individuals in Côte d’Ivoire are also statistically significantly less likely to favor co-nationalists over co-religionists (40 percent vs. 77 percent,  $p < .01$ ).

[Figure 5 here]

In Table 1, we revisit these findings in a simple regression framework that includes controls for urban/rural location, gender, age, marital status, and education. Our estimates indicate that some of these variables affect the outcomes we seek to explain: middle-aged adults are more likely than the elderly to select their religious identity as most important; males, younger adults, and better educated individuals are more willing to marry someone of a different religion; and males are more likely to favor a co-national as opposed to a co-religionist. However, in all four regressions, the country in which the respondent lives remains crucially important in how he/she responds to our questions regarding religious identity.

[Table 1 here]

If we take these findings at face value, there would appear to be large, statistically significant differences between residents living on either side of the border with respect to the importance that they attach to religion. But how confident can we be in these initial findings? If this were a true natural experiment—that is, if the only difference between the survey respondents on either side of the border were their physical location in Côte d’Ivoire or Burkina Faso—then we could conclude based on these simple difference-of-means tests that differences do in fact exist between residents of Burkina Faso and Côte d’Ivoire living a handful of kilometers from each other. But is this a true natural experiment? To answer this question, we need to revisit the step-by-step approach presented above.

### *Is this a Natural Experiment?*

We begin at Node 1 by asking whether there are any differences in the observable characteristics of the two treatments groups. To do this, we conduct difference-of-means tests across a comprehensive set of population characteristics of the sub-samples from each country (see Table 2). We find several significant differences ( $p < .05$ ), which we highlight in boldface. Residents of Côte d'Ivoire have lived at their research sites for less time, are less likely to have been born at the research site, and are more likely to have been born across the border. Respondents in Burkina Faso are more likely to receive national news regularly and to attend religious services. Ivoirian respondents are more likely to be Muslim and less likely to be Catholic. Respondents in Burkina Faso are more likely to be of the Gouin ethnicity, while those in Côte d'Ivoire are more likely to be members of the Senefou ethnic group.<sup>11</sup> These differences may or may not threaten the finding that Ivoirians attach greater importance to their religious identities than Burkinabé. To find out, we must proceed to Node 2 in the decision tree.

[Table 2 here]

Node 2 asks us to determine if the differences that exist across the populations are with respect to variables that are potentially relevant for our outcome of interest (in this case, the selection of religion as one's most important identity). To answer this question, we conduct a series of binary regressions where the dependent variable is whether respondents rank religious identity first and the independent variables are each of the characteristics for which we found statistically significant cross-border differences in Table 2. Statistically significant coefficients indicate that the characteristic in question does in fact have an impact on the salience of religion. Our results are presented in Table 3.

[Table 3 here]

The regression results suggest that of the ten statistically significant differences between the Burkinabé and Ivoirian populations samples, two of them—length of residence in the survey village and Muslim religious affiliation—are associated with differences in the extent to which subjects identify themselves first and foremost in religious terms. Residents who have lived for a longer period at the research site (or, put somewhat differently, have not migrated) are less likely to self-identify in religious terms. Muslims, meanwhile, are more likely to self-identify in religious terms.<sup>12</sup> Since we know that these characteristics differ across the two subject cohorts, these findings are potentially worrisome: they suggest the possibility that the cross-border differences we have identified in religious identity might be products not of the Côte d'Ivoire or Burkina Faso “country treatments,” but of the different degrees of “settledness” and the different share of Muslims on either side of the border.

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<sup>11</sup> It is worth noting, however, that the Gouin are considered a sub-division of the Senefou ethnic group.

<sup>12</sup> Catholics are also less likely to prioritize their religious identity, though this result is not statistically significant at traditional levels.

A clear advantage of following the step-by-step process we have outlined is that it makes it possible to identify confounding factors such as these. While their presence undermines the experimental nature of the study—having identified these factors, we can no longer use the term “natural experiment” to describe the study—they need not stand in the way of making valid inferences about the relationship we are trying to explain. To do this, however, we must complete the process outlined in Figure 1.

### *Completing the Cross-Border Study*

At Node 3, we are asked to decide if the differences that exist across the Burkinabé and Ivoirian samples arose before or after the demarcation of the border. If they happened to have been present before the border was drawn, we can simply control for them. If they arose after the demarcation, the possibility exists that, in the aftermath of the division, one side became more salubrious for certain types of people than for others, which could have stimulated in- and/or out-migration related to the importance that individuals attached to their religious identities.

We have limited data from the period of demarcation, and what we do have suggests that the populations on each side of the border were relatively similar (see Appendix A1). At any rate, because we cannot document that the differences presented in Table 2 were present prior to the imposition of national boundaries, a conservative approach dictates that we treat those differences as having arisen after the border was drawn. Thus, we continue to Node 4.

Node 4 asks us to decide if the differences we discovered in the two treatment groups are the product of post-assignment sorting. If they are not, then we have arrived at the treatment effects—whatever they may be—that arise from assignment to residence on one side of the border or the other. If they are a product of post-assignment sorting (through migration), an additional task is required if valid causal inferences are to be drawn: the post-assignment sorting must be controlled for. In this particular case, we cannot rule out the possibility that some post-assignment sorting has taken place. Coding individuals’ migrant status based on their birth location, we find that approximately 40 percent of the respondents in our Côte d’Ivoire sample migrated from elsewhere, mostly from across the Burkina Faso-Côte d’Ivoire border. Côte d’Ivoire’s liberal immigration policies during the Houphouët-Boigny period, adopted to attract labor for the commercial agricultural sector, are a principal reason for the in-migration nationwide, but it is also true that 60 percent of those who migrated into the Ivoirian study area from another country are Muslim, a trait that we have already shown to be a potentially relevant explanation for our outcome of interest (religious self-identity).

To deal with this problem, we need to control for differences in ethnicity, religion, religious participation, information attainment, and time spent in the village (because they represent differences in treatment groups that likely arose after the demarcation of the border; Nodes 1 and 3). Principal among these is the difference in religions, which we know to be significantly correlated with our variable of interest in binary regressions

(Node 2). Based on our answer at Node 4, we also need to control for migration patterns. Finally, the possible sorting of subjects along religious lines may represent an intentional re-grouping of Muslims south of the border (and Catholics north of the divide), so we should consider interaction effects of religions and migration. Table 4 presents these findings, using the selection of religion as one's most important identity as the common dependent variable.

[Table 4 here]

In Column 1 of Table 4, we reproduce the regression with control variables from Column 1 of Table 1, this time adding ethnic group fixed effects (to account for the cross-border difference in ethnic composition). Doing so does not diminish the explanatory power of residence on one side of the border as opposed to the other: the *Country* variable is significant at  $p < .01$ , and the marginal effects interpretation is that an individual living on the Côte d'Ivoire side of the border is 17 percent more likely than an otherwise identical person on the Burkina Faso side of the border to identify him/herself in religious terms.

In Column 2 of Table 4, we account for three more cross-border differences that could explain the greater salience of religion on the Ivoirian side: time spent in the study village, information attainment (measured with a dummy variable coded 1 for individuals who receive national news at least once a week), and religious participation (coded 0-4, with 0 for individuals who never attend religious services and 4 for those who attend daily religious services). None of these variables has an effect on the individual's propensity to select religion as his/her most important identity: the *Country* variable remains virtually unchanged and highly significant.

Religious group variables are added in Column 3. Relative to being Protestant (the omitted category), being Muslim—the most significant explanatory factor in the binary regressions from Table 3—turns out to have no significant effect on an individual's propensity to prioritize religion once the other variables in the regression are included. Living on the Côte d'Ivoire side of the border remains an important factor in explaining religious self-identification.

We remind the reader that there are more Muslim respondents on the Ivoirian side of the border, and it is also true that the Muslims we interviewed were more likely than non-Muslims to prioritize their religious identity (26 percent vs. 11 percent). We suggest that something about living in Côte d'Ivoire causes people to prioritize their Muslim faith, but it could also be the case that Muslims (who favor their religious identity) choose to live in Côte d'Ivoire. To address uncertainty over the direction of the causal arrow, we repeat the analysis from Column 3 in Column 4, this time removing the *Country* variable. If residence in Côte d'Ivoire were masking the effects of being Muslim, we would expect the *Muslim* variable to show a strong, positive effect in this version of the model. In fact, being Muslim still does not make individuals statistically more likely (than Protestants) to prioritize their religious identity. There is only a one

percentage point increase in the substantive marginal effect (from six percent to seven percent) and still no statistical significance.

In Column 5, we further address the post-assignment sorting concern that arises at Node 4 in our step-by-step approach. To account for the fact that individuals may have migrated into (or out of) our study areas for reasons related to the social identities they choose to prioritize, we introduce dummy variables that denote whether the individual is a native of a nearby village, an internal migrant (born in-country, more than 150 kilometers away), a migrant from across the Burkina Faso – Côte d’Ivoire border, or a migrant from a third country. Our finding is that none of these migration variables has a statistically significant effect on the individual’s propensity to self-identify in religious terms. In this version of the model, living in Côte d’Ivoire is the only factor that has a statistical effect on religious self-identification.

Being Muslim and being a cross-border migrant do not by themselves make individuals more likely to prioritize religion once we control for other factors, but there may be an interaction effect: Muslims may have intentionally migrated south of the border to exploit a friendlier religious environment, in which case the interaction between the *Muslim* and *Cross-border migrant* variables would have an impact on religious self-identification. Similarly, Catholics may have moved from Côte d’Ivoire to Burkina Faso for similar reasons. We introduce these variables in Column 6 of Table 4. The marginal effects interpretation suggests that being a Muslim cross-border migrant does have a positive effect on religious self-identification, but the relationship is not statistically significant at anywhere near traditionally accepted levels. The *Country* variable, on the other hand, remains statistically significant. The marginal effects interpretation indicates that simply by living on the Ivoirian side as opposed to the Burkinabé side of the border, an otherwise identical individual becomes 14 percent more likely to self-identify in religious terms.

At this point, although we concede that our study (like most that apply the “natural experiment” moniker) should not be considered a true natural experiment, we have addressed the problem of non-closed treatment groups by completing the step-by-step approach. In doing so, we have demonstrated that a “national treatment” effect on religious self-identification persists: there is something about living on the Côte d’Ivoire side of the arbitrarily-imposed border between the two countries that causes individuals to be more likely to align themselves with their religious identity. However, as Dunning (forthcoming) rightly stresses, the problem with experiments is that while they may establish the existence of a causal relationship, they leave unresolved the knotty issue of explaining why it exists.

Careful data collection on either side of the border between two countries may put us in a position to conclude definitively that there is something about living on one side of the border or the other that has a demonstrable causal effect on some outcome of interest. We may even be in a position, as we are here, to rule out a host of candidate explanations for these differences. But, absent further analysis and counterfactual argumentation, we can not know precisely what accounts for the variation in the

outcomes we observe in each setting. We can suggest plausible arguments to account for the patterns of variation we uncover, but the evidentiary status of these arguments as “proof” is necessarily much weaker than the evidentiary status of the causal relationship that we are trying to account for. Users of natural experiments need to keep in mind that the method only gets them half way home. To complete our cross-border study we must address the question of what, precisely, constitutes the Ivoirian (or Burkinabé) “national treatment.” What exactly is it about Côte d’Ivoire (or Burkina Faso) that causes individuals to identify themselves more (or less) strongly in terms of their religions?

### *A Proposed Causal Explanation*

Secularization theorists, who argue that exposure to wealth and material development weakens social ties to religion (Almond 1960; Apter 1964; Wilson 1966; Bruce 1992), might suggest that individuals living on the Côte d’Ivoire side of the border cling to their religious identities because they are poorer and/or have lower levels of “existential security” than their peers across the border in Burkina Faso (Norris and Inglebert 2005). In fact, our respondents in the Côte d’Ivoire study area were more likely than their counterparts in Burkina Faso to have been coded as having a “high” standard of living and less likely to have been coded as having a “medium” one—a pattern exactly opposite of what this line of argument would require.<sup>13</sup> Nevertheless, to test this possibility systematically, we introduce the *Standard of Living* variable into our regression analyses. In no model specification does this variable have an effect on the individual’s propensity to self-identify in religious terms (see Appendix A3 for results). The substantive and statistical significance of the *Country* variable is almost completely unchanged.

A second argument comes from supply-side economists, who might propose that religious identity is more salient in Côte d’Ivoire because of a greater supply of religious institutions on that side of the border, which forces those institutions (and the religions in general) to compete for members, thereby raising the salience of religion (Iannaccone 1995, 1998). Table 5 illustrates that this explanation cannot account for the variation that we find, because the supply of religion is almost identical in the study areas on each side of the Burkina Faso-Côte d’Ivoire border. Comparing the two rural villages and the two urban towns, the number, size, and period of establishment of each type of religious institution is held extremely similar, so an explanation for the cross-border variation in the importance of religious identity must lie elsewhere.

[Table 5 here]

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<sup>13</sup> Living standards were coded into three categories (high, medium, and low) based on a combination of subjective characterizations by the enumerator and information gleaned from survey questions about asset ownership (e.g., radio, television, bicycle, cell phone, moped, car, animals, fields, etc.). Codings were not standardized across urban and rural sites, so a “high” status person in the rural villages of Boussoukoulou or Kalamou might not be as well off as a respondent coded similarly in Niangoloko or Ouangolodougou.

What, then, could be causing the greater salience of religion in Côte d'Ivoire than in Burkina Faso? The most plausible explanation is that religion matters more in Côte d'Ivoire because it has been politicized. Under Félix Houphouët-Boigny, Côte d'Ivoire's president from independence in 1960 until his death in 1993, Côte d'Ivoire was a country where religious and ethnic differences were largely unimportant. However, in the power struggles that erupted after his death, politicians seized upon ethnic and religious differences as a means of winning and maintaining themselves in power.<sup>14</sup> The change was most evident in President Henri Konan Bédié's policy of *Ivoirité* (Ivoirianness).<sup>15</sup> Developed largely as a means to prevent opposition leader Alassane Ouattara (a Muslim whose father was born in what is now Burkina Faso) from competing in the 1995 presidential election, the policy distinguished sharply between Ivoirians of "authentic" native origin and those of mixed or foreign descent.<sup>16</sup> In so doing, it opened deep fissures between southern (largely Christian) Ivoirians and northern (largely Muslim) Ivoirians, who were often identified with foreign migrants.

Although the rhetoric of *Ivoirité* was always couched in terms of the distinction between "authentic" Ivoirians and foreigners, "at the street level 'foreigners' translated rather loosely into Dioula-Mossi-Muslim" (Daddieh 2001: 17-18). Hence the xenophobic appeals very easily led to divisions along religious lines. These divisions were only deepened by the coup that toppled Bédié in on Christmas Eve 1999. The six years that followed witnessed the continued exclusion of Ouattara and ongoing strife, occasionally manifested in Christian-Muslim violence.<sup>17</sup> By the time of our field work in August-September 2005, Côte d'Ivoire had become a place where religious differences, along with north-south divisions, were the principal lens through which national politics was viewed.

Against this backdrop, it is perhaps not surprising that survey respondents in Ouangolodougou and Kalamou (the Ivoirian villages) were more likely than their counterparts across the border in Niangoloko and Boussoukoula to view religion as their most important social identity. Nor is it surprising that the Ivoirian respondents were much more likely than their Burkinabé counterparts to say that they feel closer to someone from their religious group than someone from their country, or that they were less willing to marry across religious lines. Suggestive evidence that these differences are, in fact, products of the post-Houphouët mobilization of religious identities comes in Figure 6, which charts answers to a question asking respondents whether they pray less, the same, or more today than ten years ago. Religiosity (as measured by how often

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<sup>14</sup> For useful summaries, see Daddieh (2001), Toungara (2001), and International Crisis Group (2003). The account in this paragraph draws heavily on these sources.

<sup>15</sup> Bédié was the former speaker of the National Assembly who succeeded Houphouët-Boigny.

<sup>16</sup> One of the hallmarks of Côte d'Ivoire's economic development strategy under Houphouët-Boigny was the importation of unskilled laborers from neighboring countries (especially Burkina Faso, Guinea, Mali, and Niger) to work on the country's cocoa and coffee plantations. A common estimate is that such migrants make up more than 25% of the Ivoirian population.

<sup>17</sup> In October 2000, the bodies of 57 mostly Muslim men were found on the outskirts of Yopougon in what was widely interpreted as an anti-Muslim massacre. In December of that year, a church was set ablaze in the town of Kong (Daddieh 2001). In 2003, four Muslim clerics were murdered (COSIM 2006), and attacks on churches continued in 2004 (U.S. Dept. of State 2005).



people pray) appears to be rising among respondents in both countries. But whereas Burkinabé respondents are roughly 2.5 times more likely to say they pray “more” than “less” (42 percent vs. 17 percent) compared with ten years ago, Ivoirian respondents are almost five times more likely to say this (55 percent vs. 12 percent). If the respondents’ answers are to be trusted (and if whether one prays more today than ten years ago is a meaningful measure of increasing religiosity), then Ivoirians are becoming more religious *faster* than Burkinabé – a finding that is consistent with the differential politicization of religious differences in each country over the past ten years, roughly the period of political conflict in Côte d’Ivoire.

[Figure 6 Here]

### ***Conclusion***

As we have stressed, the evidence for any causal mechanism explaining a cross-border difference will always be weaker than the evidence that demonstrates a cross-border difference in the first place. Our study is no different. The strategy that we employ to develop a credible causal story—and one that we suggest all social science researchers with similar agendas employ—is to rule out alternative explanations and then rely on the facts on the ground to suggest the most plausible causal mechanism. In the application we present, that causal mechanism is the politicization of the religious social cleavage in Côte d’Ivoire.

While one of this paper’s goals is to document and suggest an explanation for this cross-border difference, another has been to use the example of the Côte d’Ivoire-Burkina Faso border study to illustrate the usefulness of a step-by-step approach to dealing with the non-closed treatment groups problem that plagues all cross-border studies. To start, the researcher needs to determine if the border used to assign treatment was arbitrarily imposed. In our application, we demonstrate that the border between Burkina Faso and Côte d’Ivoire was drawn along rivers and seasonal streambeds, without regard for ethnological cohesion or pre-existing polities. This is the preliminary condition for a natural experiment.

The researcher then must determine if there are any notable differences in the treatment populations, aside from the side of the border on which they live. Through a series of difference-of-means tests, we find several such differences in our application. Next, the researcher should ask whether any of these differences are potentially relevant ones. In our study on the Burkina Faso-Côte d’Ivoire border, we do this by conducting binary regressions of the outcome of interest on the variables for which there are differences across treatment groups and find that the religious group to which individuals belong (particularly, being Muslim) has an effect on the importance they attach to their religious identity. Researchers must then decide if the differences in treatment populations arose before or after the demarcation of the border. Lacking compelling evidence to suggest otherwise, we assume in our application that the differences in our Burkinabé and Ivoirian samples came about after the border was drawn. Finally, the

researcher must determine whether the differences that exist are a product of post-assignment sorting or simply the effect of the “national treatment.” Because of extensive migration, primarily into the Ivoirian study area, we treat the differences that we find in our application as products of post-assignment sorting, and we control for them. Having followed these procedures, we can conclude definitively that living on the Côte d’Ivoire side of the border does in fact intensify an individual’s self-identification with religion.

Natural experiments are a powerful tool for researchers, and Africa’s arbitrary borders provide a potential treasure trove of opportunities for exploiting them. Unfortunately, finding border areas that preserve the experimental nature of these studies is much more difficult than previously acknowledged. Researchers can still make valid causal inferences even without an experimental set-up, as long as a careful accounting of potentially confounding variables is conducted. We have shown in this paper that cross-border studies that lack a true experimental design require an extra set of steps, but that, when those steps are performed, the same causal insights can be achieved.

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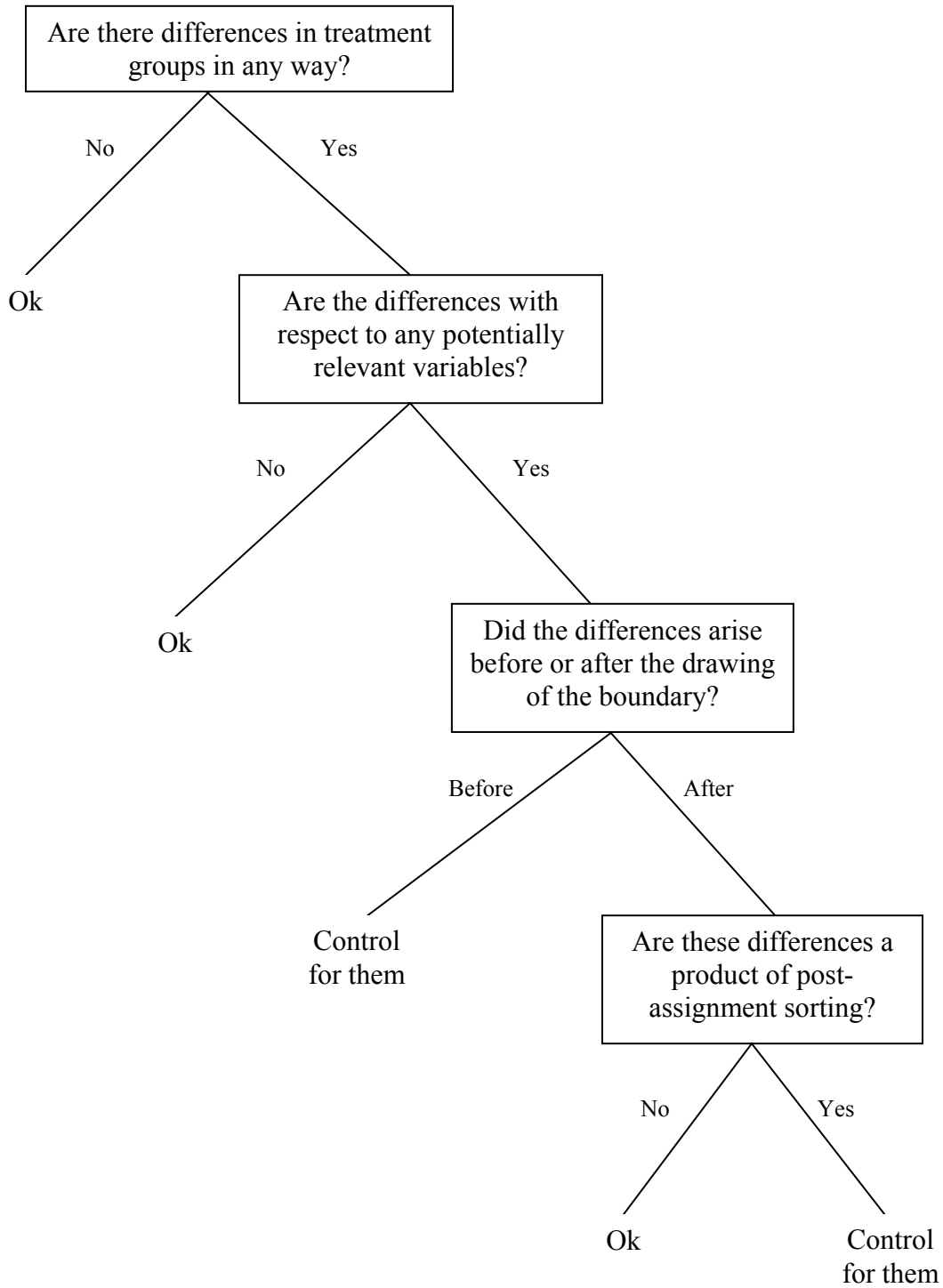
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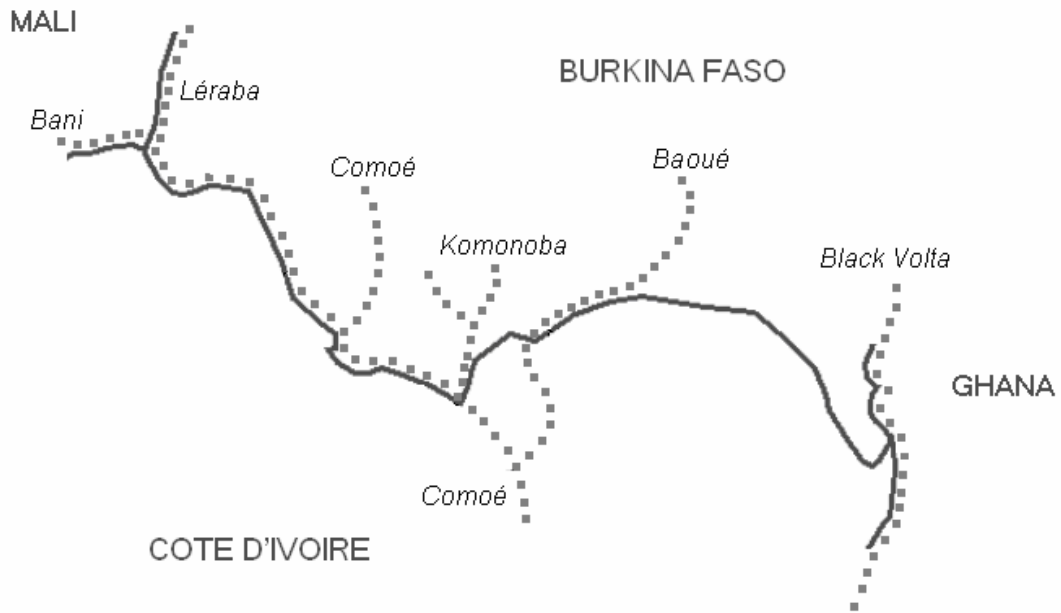
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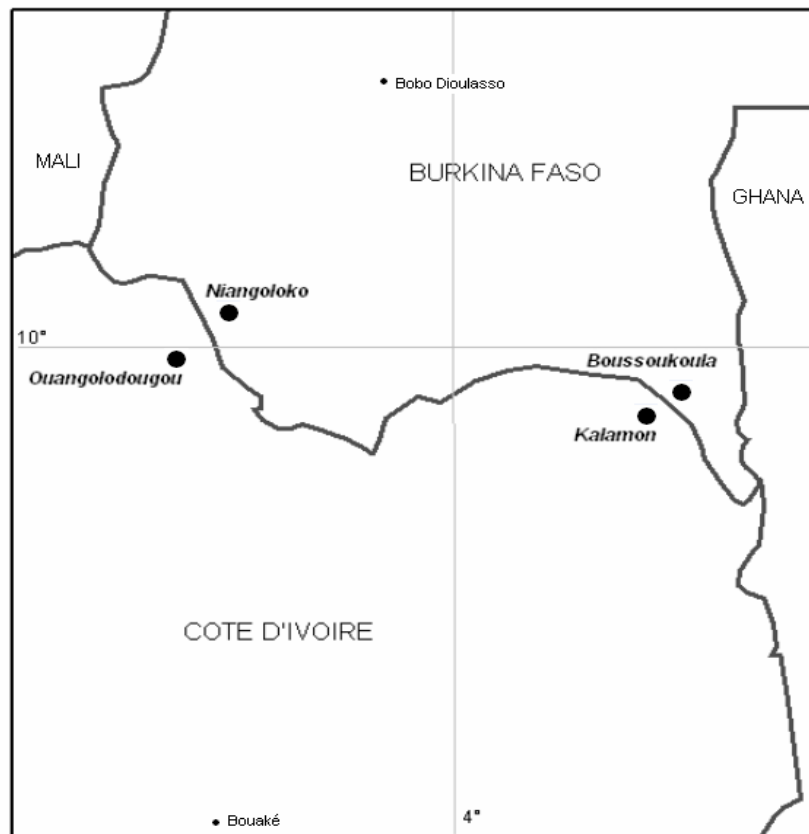
**Figure 1: Decision Tree for Assessing Potential Problems with Using African Borders as Sources of Natural Experiments, Even if they are Arbitrary**



**Figure 2. Rivers and Streambeds on the Burkina Faso – Côte d'Ivoire Border.**

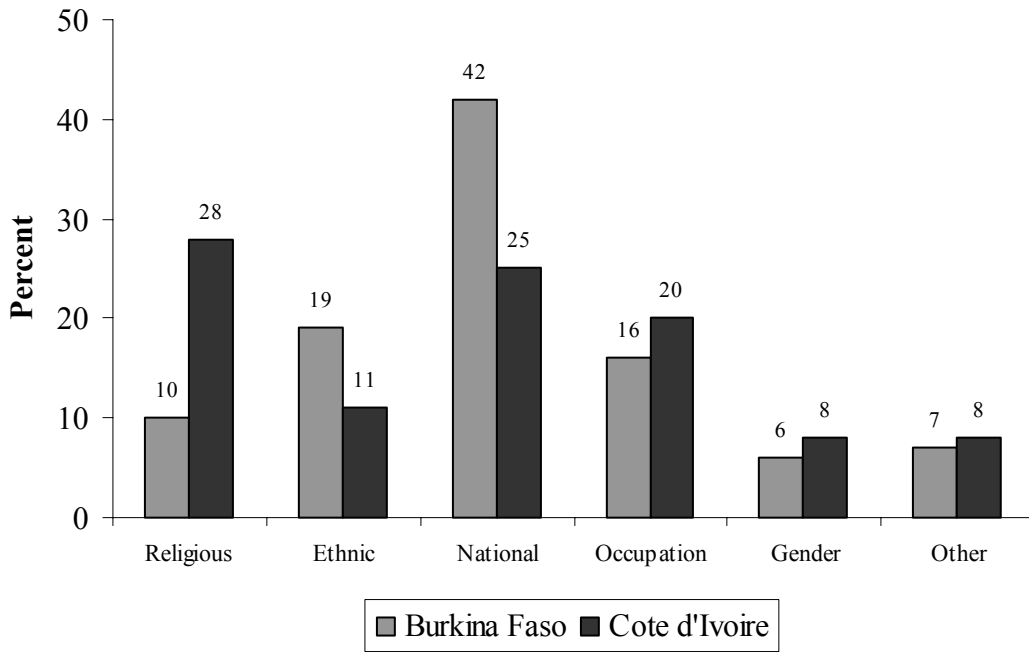


**Figure 3: Map of Research Sites.**

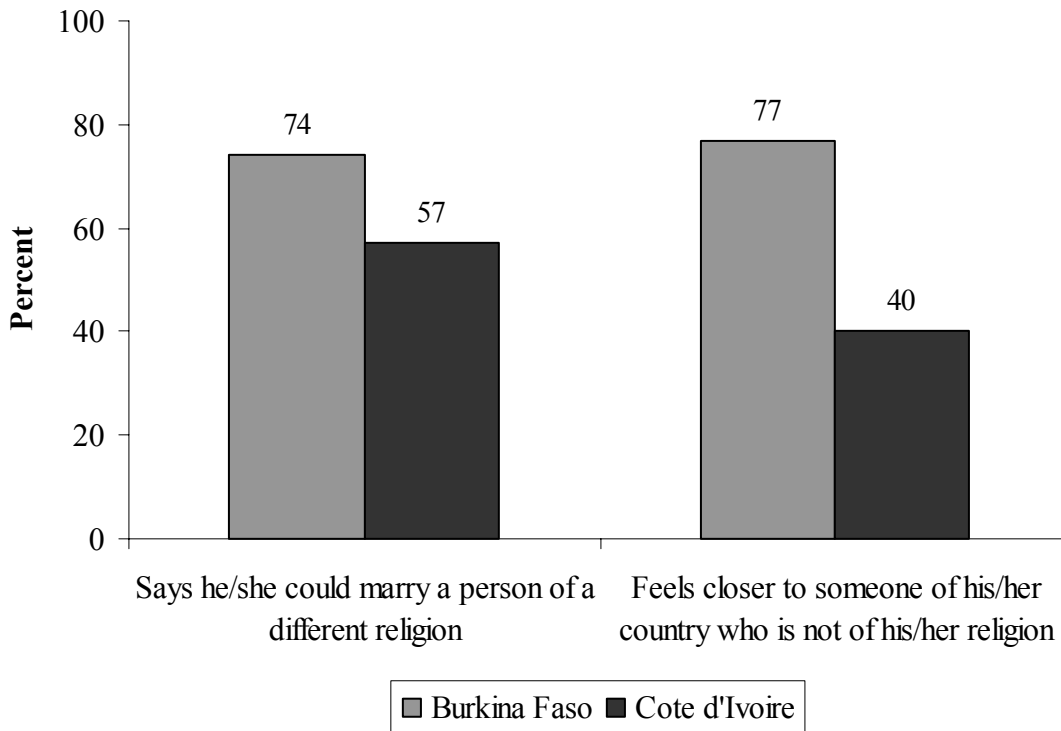




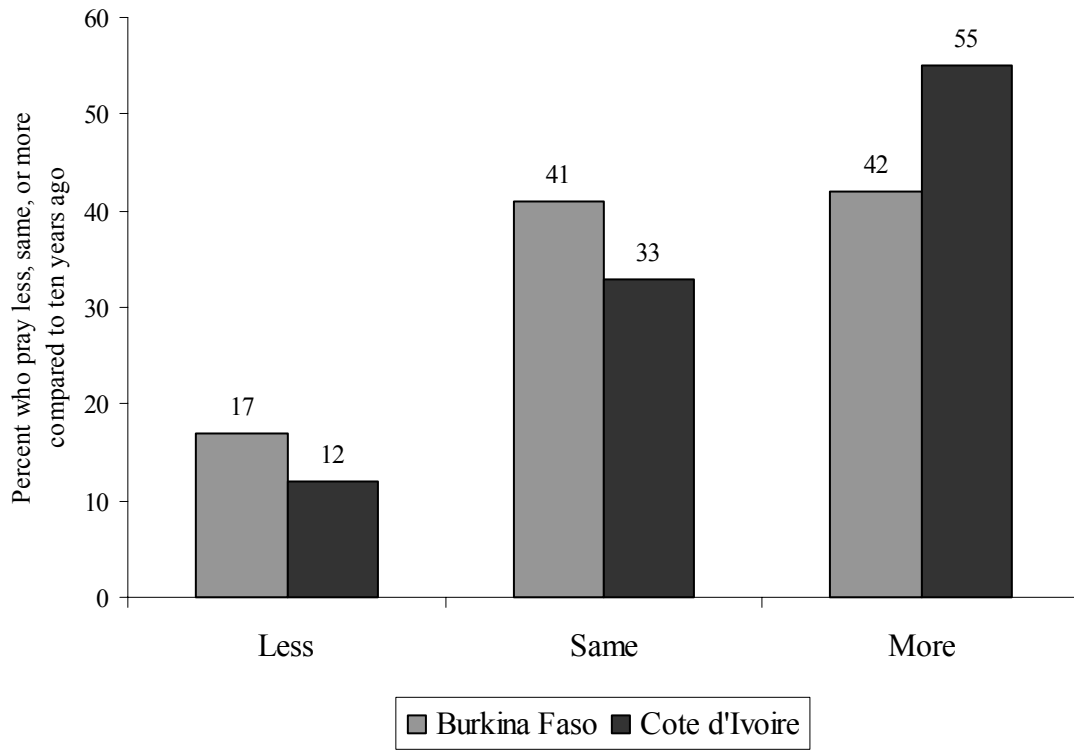
**Figure 4. Personal Identifications in Burkina Faso and Côte d'Ivoire, Compared**



**Figure 5. Social Attitudes Regarding Religion in Burkina Faso and Côte d'Ivoire**



**Figure 6. Changes in Religiosity in Burkina Faso and Côte d'Ivoire Compared**



**Table 1. Determinants of Religious Identification in Burkina Faso and Côte d'Ivoire**

	Ranks religion as primary identity	Ranks religion as 1 <sup>st</sup> or 2 <sup>nd</sup> identity	Says he/she could marry a person of a different religion	Feels closer to someone of his/her country who is not of his/her religion
	(1)	(2)	(3)	(4)
Country	0.16 *** (0.05)	0.27 *** (0.07)	-0.19 *** (0.07)	-0.37 *** (0.07)
Urban/rural area	0.06 (0.05)	0.10 (0.07)	0.06 (0.07)	-0.05 (0.08)
Gender	-0.05 (0.05)	-0.06 (0.07)	0.27 *** (0.07)	0.05 (0.08)
Age 18-27	0.11 (0.09)	0.04 (0.10)	0.20 ** (0.08)	0.11 (0.10)
Age 28-45	0.15 * (0.08)	0.12 (0.09)	0.06 (0.08)	0.05 (0.10)
Marital status	0.01 (0.06)	0.07 (0.08)	0.13 (0.08)	0.27 *** (0.09)
Years of schooling	-0.01 (0.01)	-0.00 (0.01)	0.03 *** (0.01)	0.02 * (0.01)
Ethnic Group				
Fixed effects	No	No	No	No
Pseudo R <sup>2</sup>	0.09	0.08	0.22	0.16
N	196	196	195	185

*Notes:* Marginal effects logit estimation with standard errors in parentheses. \*p<.10, \*\*p<.05, \*\*\*p<.01. “Country” is coded as 0 for Burkina Faso, 1 for Côte d'Ivoire. “Urban/rural” is coded as 0 for rural, 1 for urban. “Gender” is coded as 0 for female, 1 for male. The omitted age category is 46 years and older. “Marital status” is coded 0 for unmarried, widowed, or divorced; 1 for married. Pseudo R<sup>2</sup> values from standard logit estimation.

**Table 2. Differences in the Sample Composition of the  
Burkina Faso and Cote d'Ivoire Survey Sites**

	Burkina Faso	Côte d'Ivoire	p-value
Sample size	100	97	
Percent urban	50	52	0.828
Percent male	53	51	0.727
Percent age 18-27	35	35	0.994
Percent age 28-45	34	35	0.877
Percent age 46 and up	31	30	0.866
Percent married	65	60	0.451
Avg. number of years of schooling	3.7	3.5	0.828
<b>Avg. number of years spent living in survey town</b>	<b>25.2</b>	<b>20.0</b>	<b>0.050</b>
<b>Percent who receiving news at least 1-2 times/wk</b>	<b>57</b>	<b>49</b>	<b>0.006</b>
Percent whose standard of living is "high"	8	15	0.114
Percent whose standard of living is "medium"	67	58	0.198
Percent whose standard of living is "low"	25	27	0.805
<b>Percent who never participate in religious services</b>	<b>9</b>	<b>22</b>	<b>0.016</b>
<b>Percent who participate in religious services daily</b>	<b>31</b>	<b>16</b>	<b>0.018</b>
<b>Percent Muslim</b>	<b>37</b>	<b>66</b>	<b>0.000</b>
<b>Percent Catholic</b>	<b>36</b>	<b>11</b>	<b>0.000</b>
Percent Protestant	4	6	0.488
Percent Animist	22	15	0.172
Percent Citing "No Religion"	1	2	0.551
<b>Percent Born in Research Town/Village</b>	<b>52</b>	<b>26</b>	<b>0.000</b>
Percent From Nearby Villages (<150 km)	17	20	0.611
Percent Internal Migrants (>150 km)	15	13	0.613
<b>Percent Migrants from Across BF/CI Border</b>	<b>13</b>	<b>35</b>	<b>0.001</b>
Percent Migrants from a Third Country	1	5	0.125
Percent Lobi	35	27	0.215
<b>Percent Gouin</b>	<b>22</b>	<b>1</b>	<b>0.001</b>
Percent Mossi	19	9	0.055
<b>Percent Senefou</b>	<b>1</b>	<b>21</b>	<b>0.002</b>
Percent Dioula	5	10	0.169
<b>Percent from Other Ethnic Group</b>	<b>18</b>	<b>32</b>	<b>0.025</b>

**Table 3. Are the Factors that Vary Relevant for the Outcome of Interest?**

<b>Variable</b>	<b>Coefficient</b>	<b>p-value</b>
<b>Avg. number of years spent living in survey village</b>	<b>-0.02</b>	<b>0.058</b>
Percent who receiving news at least 1-2 times/wk	-0.07	0.615
Religious service participation	0.04	0.746
<b>Percent Muslim</b>	<b>0.99</b>	<b>0.012</b>
Percent Catholic	-0.82	0.109
Percent Born in Research Town/Village	-0.51	0.198
Percent Migrants from Across BF/CI Border	0.37	0.362
Percent Gouin	-0.97	0.203
Percent Senefou	0.34	0.534
Percent from Other Ethnic Group	-0.22	0.612

**Table 4. Determinants of Religious Self-Identity in Burkina Faso and Côte d'Ivoire**

Dependent Variable: Respondent selects religion as “most important” identity						
	(1)	(2)	(3)	(4)	(5)	(6)
Country	0.17 *** (0.06)	0.16 *** (0.06)	0.14 ** (0.06)		0.12 * (0.07)	0.14 ** (0.07)
Urban/rural area	0.09 (0.08)	0.03 (0.08)	0.01 (0.08)	-0.01 (0.08)	-0.02 (0.08)	-0.05 (0.09)
Gender	-0.04 (0.05)	-0.04 (0.06)	-0.03 (0.05)	-0.03 (0.06)	-0.03 (0.06)	-0.04 (0.06)
Age 18-27	0.15 (0.09)	0.10 (0.10)	0.09 (0.09)	0.07 (0.09)	0.03 (0.09)	0.05 (0.10)
Age 28-45	0.21 * (0.09)	0.14 (0.09)	0.13 (0.09)	0.13 (0.09)	0.12 (0.09)	0.11 (0.09)
Marital status	-0.01 (0.01)	-0.02 (0.06)	-0.03 (0.06)	-0.04 (0.06)	-0.03 (0.06)	-0.04 (0.06)
Years of schooling	-0.01 (0.01)	-0.01 ** (0.01)	-0.01 * (0.01)	-0.01 ** (0.01)	-0.01 (0.01)	-0.01 ** (0.01)
Years spent in village		-0.00 (0.00)	0.00 (0.00)	-0.00 * (0.00)	-0.00 (0.00)	-0.00 (0.00)
Receive national news		0.01 (0.02)	0.01 (0.02)	0.00 (0.02)	0.01 (0.02)	0.02 (0.02)
Religious Participation		0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)
Muslim			0.06 (0.10)	0.07 (0.11)	0.02 (0.10)	-0.09 (0.12)
Catholic			-0.01 (0.11)	-0.04 (0.11)	-0.03 (0.10)	-0.01 (0.12)
Animist			-0.09 (0.08)	-0.11 (0.08)	-0.09 (0.08)	-0.09 (0.08)
Born in nearby village					-0.10 (0.06)	-0.10 * (0.06)
Internal migrant					-0.01 (0.10)	0.02 (0.11)
Born across border					-0.00 (0.08)	-0.11 (0.09)
Born in a 3 <sup>rd</sup> country					0.22 (0.27)	0.33 (0.33)
Muslim*Cross-border						0.46 (0.32)
Catholic*Cross-border						0.00 + (0.00)
Ethnic group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R <sup>2</sup>	0.13	0.16	0.17	0.15	0.21	0.24
N	196	193	193	193	188	181

Notes: Marginal effects logit estimation with standard errors in parentheses. \*p<.10, \*\*p<.05, \*\*\*p<.01. + indicates 0 cases. See Appendix A2 for coding rules. Pseudo R<sup>2</sup> values from standard logit estimation.

**Table 5. Supply of Religion by Village/Town**

	Rural Villages		Urban Towns	
	<b>Boussoukoula</b>	<b>Kalamon</b>	<b>Niangoloko</b>	<b>Ouangolo.</b>
Country	BF	CI	BF	CI
Number of Mosques	2	2	1 Community 3 Mid-size 9 Neighbor'd	1 Community 6 Mid-size
Est. of first mosque	1990	1984	1880s	1880s
Avg. weekly attendance/ mosque	40-60	75	3,000	3-4,000
Number of Catholic Churches	1	1	1	1
Est. of first Catholic church	1995	2000	1941	c. 1945
Avg. Weekly attendance/ church	60	40	5-600	400
Number of Protestant Churches	1	1	5	4
Est. of first Protestant church	2002	1997	1930s	c. 1940
Avg. weekly attendance/ church	15	30	80-100	60

*Notes:* All estimates provided by religious leaders in respective villages. The majority of Muslims in the region attend only Friday prayer services. Muslim attendance estimates for the urban towns refer to the Community mosques, which are large, non-denominational mosques. Represented among the mid-size mosques in both urban towns are the Sunni, Tidjanniya, and Hamadiyya denominations. Represented among the Protestant churches in both urban towns are the Assembly of God, Baptist, Alliance, and Mission International churches. Leaders of the places of worship in all four sites reported adherents coming primarily from the village/town itself, with a small minority coming from neighboring villages.

## Appendix

### A1. Demarcation-Era Comparison of Burkina Faso and Côte d'Ivoire Border Districts, based on Data from Huilery (2006).

District	Rural Pair		Urban Pair	
	Gaoua	Bondoukou	Bobo	Kong
Country	BF	CI	BF	CI
Dummy for presence of a pre-colonial centralized political power	No	No	Yes	Yes
Year of population submission to French	1930	1929	1904	1898
European population in 1910	12	6	26	19
Avg. annual indigenous taxes collected, 1910-1928, in 1910 units	125,101	186,602	386,508	462,968
Avg # of medical staff per 100,000, 1910-1930	3.71	4.09	1.23	1.83
Avg # of schools between 1910 and 1928, in 1910 units	1.91	1.58	2.29	2.40
Est. literacy rate among indigenous colonial contemporaries	0.03	0.03	0.11	0.07
Secondary school rate among indigenous colonial contemporaries	0.003	0.007	0.02	0.03

### A2. Coding Rules for Table 4.

*Country* coded as 0 for Burkina Faso, 1 for Côte d'Ivoire.

*Urban/rural* is coded as 0 for rural, 1 for urban.

*Gender* is coded as 0 for female, 1 for male.

The omitted age category is 46 years and older.

*Marital status* is coded 0 for unmarried, widowed, or divorced; 1 for married.

*Receive National News* is coded 1 for individuals who receive news at least once/week, 0 otherwise.

*Religious Participation* is coded 0 for never attending religious ceremonies, 1 for attending 1-2 times per year, 2 for attending 1-2 times per month, 3 for attending 1-2 times per week, 4 for attending everyday.

The omitted religious group is Protestant.

The omitted migrant category is lifelong resident of the village.

Ethnic group fixed effects includes dummy variables for membership in the Dioula, Gouin, Lobi, Mossi, or Senefou tribes. The omitted category is "Other Ethnic Group."



### A3. Determinants of Religious Self-Identity in Burkina Faso and Côte d'Ivoire, Controlling for Standard of Living

Dependent Variable: Respondent selects religion as “most important” identity

	(1)	(2)	(3)	(4)	(5)	(6)
Country	0.17 *** (0.06)	0.16 *** (0.06)	0.14 ** (0.06)		0.11 * (0.07)	0.13 ** (0.07)
Urban/rural area	0.09 (0.08)	0.03 (0.08)	0.01 (0.08)	-0.02 (0.08)	-0.03 (0.09)	-0.06 (0.09)
Gender	-0.04 (0.05)	-0.03 (0.06)	-0.03 (0.06)	-0.03 (0.06)	-0.02 (0.06)	-0.03 (0.06)
Age 18-27	0.15 (0.09)	0.10 (0.10)	0.09 (0.10)	0.07 (0.09)	0.03 (0.09)	0.05 (0.10)
Age 28-45	0.21 * (0.09)	0.14 (0.09)	0.13 (0.09)	0.13 (0.09)	0.12 (0.09)	0.11 (0.09)
Marital status	-0.01 (0.01)	-0.01 (0.06)	-0.02 (0.06)	-0.04 (0.06)	-0.03 (0.06)	-0.04 (0.06)
Years of schooling	-0.01 (0.01)	-0.01 * (0.01)	-0.01 * (0.01)	-0.01 * (0.01)	-0.01 (0.01)	-0.01 * (0.01)
Years spent in village		-0.00 (0.00)	0.00 (0.00)	-0.00 * (0.00)	-0.00 (0.00)	-0.00 (0.00)
Receive national news		0.01 (0.02)	0.01 (0.02)	0.00 (0.02)	0.01 (0.02)	0.02 (0.02)
Religious Participation		0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)
Muslim			0.06 (0.11)	0.07 (0.11)	0.03 (0.11)	-0.07 (0.13)
Catholic			-0.01 (0.11)	-0.05 (0.10)	-0.04 (0.10)	-0.01 (0.12)
Animist			-0.09 (0.08)	-0.11 (0.08)	-0.09 (0.08)	-0.09 (0.09)
Born in nearby village					-0.10 ** (0.05)	-0.11 ** (0.05)
Internal migrant					-0.01 (0.10)	0.02 (0.11)
Born across border					-0.00 (0.08)	-0.11 (0.09)
Born in a 3 <sup>rd</sup> country					0.21 (0.28)	0.31 (0.32)
Muslim*Cross-border						0.45 (0.32)
Catholic*Cross-border						0.00 + (0.00)
<b>Standard of living</b>		<b>-0.02</b> <b>(0.04)</b>	<b>-0.02</b> <b>(0.04)</b>	<b>-0.03</b> <b>(0.04)</b>	<b>-0.04</b> <b>(0.05)</b>	<b>-0.04</b> <b>(0.05)</b>
Ethnic group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R <sup>2</sup>	0.13	0.16	0.18	0.15	0.21	0.25
N	196	192	192	192	187	180

Notes: Marginal effects logit estimation with standard errors in parentheses. \*p<.10, \*\*p<.05, \*\*\*p<.01.  
+ indicates 0 cases. See appendix for coding rules. Pseudo R<sup>2</sup> values from standard logit estimation.  
Standard of living is coded 1 for “low,” 2 for “medium,” 3 for “high.”