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**Authors**

Mccauley, John F  
Posner, Daniel N

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## African Borders as Sources of Natural Experiments Promise and Pitfalls\*

JOHN F. MCCAULEY AND DANIEL N. POSNER

**A**frica's arbitrary country borders have been seized upon as sources of "natural experiments": having randomly assigned people to different country treatments, differences in outcomes on either side of the border can then be attributed to the institutions, demographics, or policies put in place in each country. While methodologically attractive, the use of African borders as sources of natural experiments presents several potential pitfalls. We describe these pitfalls—some common to all studies that employ jurisdictional boundaries, some unique to African borders—and offer guidelines for overcoming them. We conclude that African cross-border studies can provide research advantages similar to well-executed comparative case studies, but that they frequently offer weaker inferential leverage than is claimed.

**T**he borders that separate African countries are largely products of colonial era boundary making, drawn without respect to—and in some cases in nearly complete ignorance of—the sociological, cultural, and historical characteristics of the peoples they partitioned (Asiwaju 1985; Englebert, Tarango and Carter 2002; Hargreaves 2005). This has made them scapegoats for a host of contemporary problems in Africa, ranging from ethnic conflict to poor economic performance to civil war. In recent years, however, Africa's arbitrary borders have been seized upon for a different purpose: as a source of natural experiments in the social sciences. The drawing of the boundary and the allocation of populations to each political unit is interpreted as having randomly assigned the inhabitants of the border area to different treatments; differences in attitudes, behavior patterns, or other outcomes observed among the people living in each country's border area are then attributed to differences in the political institutions, country-level demographics, histories, and public policies to which they have each been exposed.

Table 1 provides a partial listing of studies that exploit African borders for this purpose. While the studies vary in the explicitness of their emphasis on causal identification, all are motivated by the intuition that comparing outcomes across an arbitrarily drawn national boundary can provide leverage for understanding the impact of the characteristics that differ on either side.<sup>1</sup>

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\* John F. McCauley, Assistant Professor of Government & Politics, Department of Government & Politics, University of Maryland, College Park, 3140 Tydings Hall, College Park MD 20742, USA (mccauley@umd.edu). Daniel N. Posner, Professor of International Development, Department of Political Science, University of California, Los Angeles 4289 Bunche Hall, Los Angeles CA 90095, USA (dposner@polisci.ucla.edu). The authors thank the UCLA Globalization Research Center Africa for financial support; Ibrahima Ouattara, Aimé Bado, Ollo Edmond Da, and Warhanti Da for their excellent research assistance, and Denis Cogneau, Elise Huillery, and the DIAL research team for sharing data. Useful comments were received from Daniel de Kadt, Chad Hazlett, members of the Working Group in African Political Economy, participants at the 2012 Princeton Experimental Research Workshop, and two anonymous reviewers.

<sup>1</sup> Africa is not the only region in which researchers have exploited jurisdictional borders for explanatory leverage. Sahlins (1989) employs a similar strategy on the France–Spain border, as does Elton (2005) on the border between Peru and Ecuador, Passi (2005) on border of Russia and Finland, and Mendoza (2000) on the border between Spain and Portugal. A number of studies in the American Politics literature exploit the borders between US states (Card and Krueger 1994), municipalities (Keele and Titiunik 2014a), and media markets (Krasno and Green 2008; Gerber et al. 2011; Keele and Titiunik 2014b) for similar purposes.

TABLE 1 *Studies that Use African Borders as Sources of Quasi-Experimental Leverage*

Study	Border Studied	Claimed Treatment; Cross-Border Difference	Outcome of Interest that Varied
Asiwaju (1976)	Nigeria–Benin	Colonial practices	Evolution of traditional customs
Berger (2009) <sup>a</sup>	Northern–Southern Nigeria	Tax institutions	Quality of government services
Bubb (2013)	Ghana–Côte d’Ivoire	De jure property rights	Local development; de facto property rights
Coast (2002)	Kenya–Tanzania	Socialist versus capitalist national policies	Socio-economic conditions
Cogneau and Moradi (forthcoming)	Ghana–Togo	Colonial policy toward missionary schools	Literacy and religious affiliation
Cogneau, Mesplé-Somps and Spielvogel (forthcoming)	Five West African borders	Cash crop production and pricing policies	Socio-economic welfare
Firmin-Sellers (2000)	Ghana–Côte d’Ivoire	Property rights policy	Class formation
Huillery (2009) <sup>a</sup>	Former French West Africa	Colonial public investment	Development and education outcomes
Jones, Pattanayak and Sills (2006)	Benin–Togo	Democratic institutions	Household innovation
Laitin (1986)	Nigeria–Benin	Modes of political administration	Salience of religious identity
Lee and Schultz (2012) <sup>a</sup>	French–British Cameroon	Colonial institutions (indirect/direct rule)	Wealth; public goods provision
McCauley and Posner (2014)	Burkina Faso–Côte d’Ivoire	Elite mobilization during conflict	Attachment to different social identities
Miles and Rochefort (1991)	Nigeria–Niger	Colonial reliance on traditional administration	Attachment to different social identities
Miles (1994)	Nigeria–Niger	Colonial experience	Various
Miles (2005)	Nigeria–Niger	Degree of administration and boundary control	Attachment to national and local identities
MacLean (2010)	Ghana–Côte d’Ivoire	State-building experience	Reciprocity norms; informal institutions
Miguel (2004)	Kenya–Tanzania	Nation-building policies	Public goods provision
Michalopoulos and Papaioanno (2011)	Various	Presence of partitioned ethnic group	Conflict and violence
Michalopoulos and Papaioanno (2014)	Various	National institutions	Subnational economic performance
Posner (2004)	Zambia–Malawi	Relative size of ethnic communities	Salience of identities; inter-group conflict
Ralston (2012)	Kenya–Uganda	Disarmament policy	Violence; livestock raids
Robinson (2014)	Zambia–Malawi	Salience of national identity	Interpersonal trust
Welch (1966)	Togo–Ghana	Colonial and nationalist experiences	African political unity

Note: <sup>a</sup>These studies exploit former colonial boundaries within present-day African states; they may also be considered part of this literature.

Although the attractiveness of African borders as sources of quasi-experimental leverage is rooted in their arbitrary nature, their utility for making causal claims goes beyond this key characteristic. The fact that Africa's country borders were drawn by actors who have long since left the stage also provides an advantage because it permits researchers to rule out the possibility that the assignment to treatment was a strategic political decision made in light of its anticipated effects. This problem plagues many experimental studies built around policy interventions (e.g., redistricting or the implementation of quotas), but it can safely be ruled out for most African border studies, as the assignment of populations to different political jurisdictions was carried out by decision makers who had no way of benefiting from, let alone anticipating, most outcomes of interest to contemporary researchers. In addition, because most African borders were wholly novel creations, rather than alterations of previous boundaries, their use avoids the problem posed in many jurisdictional studies by sub-groups within the treated and untreated units (e.g., legacies of previous border demarcations) for whom outcomes may vary (Sekhon and Titiunik 2012).

Notwithstanding these clear benefits, however, African cross-border studies are not without their pitfalls. Some of these pitfalls—especially those related to the exploitation of a jurisdictional boundary—have been highlighted in the spate of recent books and articles on natural experiments (see especially Keele and Titiunik 2014a; Keele and Titiunik 2014b, but also Dunning 2008; Druckman et al. 2011; Dunning 2012; Gerber and Green 2012; Sekhon and Titiunik 2012 for more general treatments). Others are unique to the subset of natural experiments that specifically involve African borders. The objective of this brief note is to outline four of these potential pitfalls and to recommend strategies for overcoming them. A secondary goal is to underscore that, even when these fixes are applied, researchers may still need to qualify their claims about the methodological advantages of using African borders as sources of natural experiments.

#### IS THE BORDER ARBITRARY?

First, African cross-border studies are potentially vulnerable to a very basic criticism: that the boundaries they exploit may not, in fact, have been arbitrarily drawn. For the border between two countries to be amenable to study as a natural experiment, the assignment of subjects to one side of the border or the other must be as-if random (Dunning 2008). This condition is necessary to ensure that the potential outcomes of the units being compared on either side of the border—be they individuals, communities, bureaucracies, or economies—are independent of treatment assignment. Where African borders follow meridians, parallels, or other rectilinear or curved lines (as they do in about 80 percent of cases), they can plausibly be taken as exogenous to the characteristics of people and places on the ground (Englebert, Tarango and Carter 2002; Alesina, Easterly and Matuszeski 2011). However, where they follow geographical features such as rivers and watersheds, or where the negotiations that led to their drawing explicitly took account of historical or demographic factors, it is possible that pre-partition settlement patterns caused the populations lying on either side to differ in material ways. For example, the high-density border between Benin and Nigeria exploited in Asiwaju's (1976) study of traditional customs was intentionally drawn according to local ethnic settlement patterns (see Brownlie 1979; Green 2012). That between Nigeria and Niger, exploited in Miles' several studies, was drawn to correspond to the northern edge of Usman dan Fodio's jihad conquest (Englebert, Tarango and Carter 2002). In general, to the extent that potential outcomes are related to social identities, they may differ among the populations lying on each side of these borders.

As shown in Table 2, fewer than half of the studies listed in Table 1 contain an explicit evaluation of the arbitrariness of the border they study. For those designing new research set

TABLE 2 *Evaluation of Cross-Border Studies in Overcoming Methodological Pitfalls*

Study	Evaluates Arbitrariness of Border?	Employs Cluster Randomization?	Evaluates Alternative Treatments?	Accounts for Non-Closed Borders?
Asiwaju (1976)	✓			
Berger (2009) <sup>a</sup>	✓		✓	✓
Bubb (2013)	✓	✓	✓	✓
Coast (2002)				
Cogneau and Moradi (forthcoming)	✓		✓	✓
Cogneau Mesplé-Somps and Spielvogel (forthcoming)	✓	✓	✓	✓
Firmin-Sellers (2000)				
Huillery (2009)	✓	✓	✓	
Jones, Pattanayak and Sills (2006)				
Laitin (1986)			✓	
Lee and Schultz (2012)	✓	✓	✓	
McCauley and Posner (2014)	✓	✓	✓	✓
Miles and Rochefort (1991)				
Miles (1994)				
Miles (2005)				
MacLean (2010)				
Miguel (2004)	✓		✓	✓
Michalopoulos and Papaioanno (2013) <sup>a</sup>		✓	✓	✓
Michalopoulos and Papaioanno (2014) <sup>a</sup>		✓	✓	✓
Posner (2004)	✓		✓	
Ralston (2012)			✓	
Robinson (2014)			✓	
Welch (1966)			✓	

Note: <sup>a</sup>These studies focus on borders that partition ethnic groups; as a result, they assume (though do not explicitly evaluate) the arbitrariness of those borders.

astride African borders, historical records and databases of boundary demarcation will provide many of the answers needed to determine the plausibility of the as-if random assumption. Data collected by Alesina, Easterly and Matuszeski (2011) on how straight or “squiggly” each country’s borders are also provide useful information.<sup>2</sup> Alesina, Easterly and Matuszeski (2011) and Englebert, Tarango and Carter (2002) both provide information about the extent to which a country’s borders partition ethnic groups, which provides another indicator of arbitrariness.<sup>3</sup> Brownlie’s dictionary of African borders (1979) details negotiations between colonial and traditional administrators and identifies a number of specific exceptions to the assumption of arbitrary demarcation.<sup>4</sup> In addition, socio-economic and demographic data, as well as anthropological accounts from the first decades of colonial rule, if available, can help to bolster the claim of pre-treatment equivalence across populations on each side of the border.<sup>5</sup>

<sup>2</sup> Note, however, that the Alesina, Easterly and Matuszeski (2011) data are only provided at the country level, aggregating information from all of a country’s bilateral borders. For most purposes, what researchers will need is data about a particular bilateral border (or, in some cases, just a specific section of it). Helpful additional sources include the International River Boundaries Database at <https://www.dur.ac.uk/ibru/resources/irbd/> and the International Boundary Study at <http://www.law.fsu.edu/library/collection/limitsinseas/numericalibs-template.html>.

<sup>3</sup> Englebert, Tarango and Carter’s (2002) data are provided at the country-dyad level, and thus may be more useful for many studies.

<sup>4</sup> Green (2012) also provides examples of borders whose boundaries were drawn by colonial rulers in consideration of their commercial interests and/or their desire not to bisect ethnic communities.

<sup>5</sup> Huillery (2006) employs data of this sort to compare communities across national borders in West Africa.

#### CLUSTER RANDOMIZATION

A second potential pitfall stems from the fact that, as in all natural experiments built around jurisdictional boundaries, individuals in African cross-border studies are not individually randomized to treatment but are instead assigned as a cluster to one country treatment or the other.<sup>6</sup> True randomization would involve first defining the population in the border area and then assigning people to sides of the border via a mechanism such as a lottery.

Cluster randomization presents a problem to the extent that potential outcomes are correlated with geographic proximity: that is, if people who live near each other react similarly to the same treatment, but differently from those who live elsewhere. Because treatment is assigned at the cluster level—the level of the regions directly abutting either side of the border in most of the cross-border studies listed in Table 1—one must take seriously the possibility that potential outcomes within each cluster may correlate, which will reduce the study’s effective sample size and increase the variance of the estimated treatment effect. Researchers will therefore have artificially inflated confidence in their findings if they conduct their analyses incorrectly (but as is often done) at the individual level (Dunning 2012).

This problem is reduced if the treatment groups on either side of the border are nearly as geographically proximate as the people in each cluster are to one another, as this will generate similar distributions of characteristics in the populations on either side of the border and reduce the gap between the within-cluster and cross-cluster variance in potential outcomes—at least to the extent that relevant covariates are correlated with adjacency. Adjacency is thus a design feature strongly advocated by Keele and Titunik (2014a), and it is part of the rationale in many cross-border studies for selecting villages for comparison that are located extremely close to one another on either side of the border; the other of course is that the assumption of as-if random assignment is most plausible right at the country discontinuity. Indeed, of the studies we list in Table 1, all except Miguel (2004) situate their cross-border clusters in close proximity to the boundary, or at least at sites with a shared predominant ethnic identity in adjacent regions. Even so, the most conservative approach is to analyze the data by cluster mean rather than at the individual level. As shown in Table 2, however, only a minority of studies actually adopt this strategy.

#### WHAT IS THE TREATMENT?

A third challenge in cross-border studies in Africa lies in identifying what exactly the treatment is. In many natural experiments, the treatment is unambiguous: (narrowly) winning an election; being exposed to a radio broadcast; being drafted into the military; having one’s village council presidency be reserved for a woman or a member of a historically disadvantaged caste. By contrast, in natural experiments built around the arbitrary demarcation of African country borders, the exact nature of the treatment is often far from straightforward. Is the variation the researcher seeks to explain owing to differences in the two countries’ colonial legacies? Differences in their post-partition histories and institutions (and, if so, which ones)? Differences in their demographic composition? Miguel (2004), for example, asserts that the differential outcomes he observes in Kenya and Tanzania are because of the nation-building strategies initiated by Tanzania’s President Julius Nyerere. However, they may be just as plausibly a function of Kenya’s more polarized ethnic demography, more competitive politics, greater degree of market integration, or any number of other factors. In MacLean’s (2010) account of informal institutions along the Côte d’Ivoire–Ghana border, differences in informal reciprocity are explained by

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<sup>6</sup> Or at any rate they were assigned to one country treatment or the other at the time the border was drawn. Whether they (or their descendants) moved subsequently is a separate issue, which we discuss below.

the centralized nature of the state in Côte d'Ivoire. However, they might equally result from the relatively weaker presence of labor migrants in Ghana. The problem in both of these cases—and in every other African border study—is that multiple treatments co-vary and affect the areas of interest simultaneously, making it very difficult to parse each treatment's relative contribution to the outcome the researcher seeks to explain (Keele and Titiunik 2014a; Keele and Titiunik 2014b). Dunning (2012) terms this the “bundling” or “compound treatment” problem, and while it threatens many natural experiments, it applies to African cross-border studies with particular force as the country border also defines the boundary between the multiple treatments that stem from the whole range of differing national institutions and policies in each country. The challenge is deepened by the number of changes that are likely to have taken place on either side of the border since it was drawn, and thus the number of different treatments that must be considered.

The solution to this problem involves weighing competing explanations with careful historical and institutional analysis, the consideration of geographic and demographic variables, and other valuable and appropriate—though almost always non-experimental—research techniques (see, as an example, Posner 2004). The methodological attractiveness of the cross-border framework is thus blunted by the fact that the ultimate explanation for the outcome of interest almost always relies on non-experimental analysis and argumentation.

An additional complication is that these designs, like differences-in-differences and other strategies, estimate the difference in outcomes under two (or more) treatments, and in many cases neither may be an obvious “control” condition for the other. For example, in Jones, Pattanayak and Sills's (2006) study of household innovation across the Benin–Togo boundary, differences in rates of innovation adoption may be a function of either democratic institutions in Benin or authoritarian institutions in Togo. We can conclude that regime type matters, but not whether democracy is conducive to innovation or authoritarianism depresses it. In Cogneau and Moradi (forthcoming) and Lee and Schultz (2012), both of which exploit the ostensibly arbitrary border between former French and British colonies, is it the French colonial legacy or the British colonial legacy that matters? The point is that most African cross-border studies present a more complicated inferential problem than, say, a medical experiment, where a control group is present and the source of the observed variation is unambiguous.<sup>7</sup> Thus, researchers employing African cross-border research designs must attempt to assess whether these differences stem from some characteristic of Country A, some characteristic of Country B, or a combination of the two.

#### TREATMENT GROUPS ARE NOT CLOSED

Finally, while spillover of treatments across borders is a concern in any cross-border study, another—and in the context of African cross-border designs, potentially bigger—problem is the spillover of research subjects.<sup>8</sup> This problem stems from the fact that, unlike in, for example, a

<sup>7</sup> Note that the absence of a true control group is not just a problem with cross-border experiments. Many experimental designs involve the comparison of multiple treatments without a control (e.g., Merolla and Zechmeister 2013; Angrist and Lavy 1999).

<sup>8</sup> Spillover of treatments would violate the stable-unit treatment-value assumption. Although the resulting bias will often be conservative, it is also possible for treatment spillover to increase estimates of treatment effects. For example, Miles' (1994) finding that villagers in Niger attach greater importance to their national identity than villagers in Nigeria could plausibly stem from the former's reaction to the lack of comity they observe across the border and their desire to distance themselves by emphasizing that they are citizens of Niger rather than Nigeria. A parallel argument could be made in McCauley and Posner's (2014) study of villagers on either side of the Burkina Faso–Côte d'Ivoire border: the higher rate of national identification in Burkina Faso could plausibly result from the desire of Burkinabe to distance themselves from the civil conflict in Côte d'Ivoire, the exposure to which has spilled across the border.

pharmaceutical trial, the treatment groups in cross-border studies are not closed. Populations living in the border areas of African countries are relatively unconstrained from moving from one side of the border to the other or, within each country, into or out of the border area. Cross-border migration and movements into the border region from other parts of the country can be thought of as problems of non-compliance, whereas movements out of the border area can be thought of as a problem of attrition.<sup>9</sup> If such population movements take place at random, they create analytical noise that may compromise the detection of treatment effects. More worrying is if the individual characteristics associated with migration decisions are correlated with the treatment, in which case the population movements may generate spurious treatment effects or erroneous null findings. All impact analyses of place-based programs are potentially affected by the movement of subjects between, into, or out of control and treatment groups (Bloom 2005). However, in Africa, the problem is exacerbated by both the permeability of national borders and the very long period of time that has elapsed since their demarcation, and hence the extended interval during which population movements may have taken place.<sup>10</sup>

Mitigating the effects of non-closed treatment groups is tricky. Testing the robustness of the cross-border difference to controls for migration will be insufficient, as the researcher will have no way of confirming that migration status is uniformly related to potential outcomes. If Countries A and B start out with the same distribution of individuals, but then those with high-potential outcomes move from A to B and those with low-potential outcomes move from B to A, then the numbers of migrants in each country may be the same, but the distribution of potential outcomes in each country will differ.

The safest, and most conservative, approach is to conduct the analysis using country of origin rather than country of residence as the treatment variable—a form of intention-to-treat analysis. A complexity to keep in mind about this strategy, however, is that the drawing of the border took place more than a hundred years ago, so a respondent's country of origin is not strictly speaking a product of as-if random assignment (though that of his or her parents may have been). Moreover, not even this fix deals entirely with the potentially endogenous exit of residents out of the border area (and hence out of the researcher's sample).

## CONCLUSION

In an environment where experimental—and especially natural experimental—designs are increasingly prized in the social sciences, a growing number of scholars have seized upon the arbitrary nature of African borders as a source of explanatory leverage. As we suggest here, researchers who use African borders for this purpose must be mindful of the potential pitfalls of such a strategy, and of the steps that should be taken to test for and mitigate these pitfalls. While many of these issues are common to all jurisdictional studies, the porosity of most African borders and the lengthy interval between the drawing of the border and the collection of data create particularly daunting challenges of non-compliance and attrition for cross-border studies in the African context. Although some authors recognize and account for this issue (and the others we highlight), as Table 2 makes clear, many do not.

Even if a researcher does take heed of these warnings, however, African cross-border studies may still be less methodologically advantageous than is often assumed. Africa's arbitrary

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<sup>9</sup> For discussions of these issues see Dunning (2012, Chapter 5) and Gerber and Green (2012, Chapters 5–7).

<sup>10</sup> Most African borders were demarcated at the end of the 19th century following the Berlin Conference of 1885, but the first studies that employed African borders as sources of natural experiments did not begin to appear until the 1970s, with the most methodologically self-conscious studies only appearing within the last decade.

borders are attractive because they appear to offer researchers all of the inferential benefits of a natural experiment. However, as many authors underscore (e.g., Gerber and Green 2012), the fact that the assignment process relies on argumentation about the as-if random nature of the assignment rather than true randomization implies that they should properly be viewed as observational studies rather than as experiments. Moreover, we have underscored that many of the methodological issues raised by African cross-border studies must be addressed through non-experimental methods similar to those used in observational research. Taking these steps further undermines the experimental status of the research and, with it, the in-built methodological advantages of the research design. This is not, of course, to suggest that such research is without merit. Well-executed cross-border studies, like well-executed comparative case studies (Slater and Ziblatt 2013), can generate important contributions to knowledge—so long as researchers are mindful of their pitfalls as well as their promise.

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