

Measuring the Effect of “Ethnicity”

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Most tests of hypotheses about the effects of “ethnicity” on outcomes use data or measures that confuse or conflate what are termed *ethnic structure* and *ethnic practice*. This article presents a conceptualization of ethnicity that makes the distinction between these concepts clear; it demonstrates how confusion between structure and practice hampers the ability to test theories; and it presents two new measures of ethnic practice—ECI (the ethnic concentration index) and EVOTE (the percentage of the vote obtained by ethnic parties)—that illustrate the pay-offs of making this distinction and collecting data accordingly, using examples from the civil war literature.

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What is the effect of ethnicity on violence? On voting? On economic growth? On democratic stability? On the distribution of public goods? On the nature of party systems? Measuring the effect of ethnicity on economic and political outcomes and processes such as these, across countries, has become one of the most important research questions in comparative politics in the last 20 years.¹

To measure the effect of ethnicity on anything, we require a conceptualization of the term. First, we need a conceptualization to collect meaningful data—without it, how will we know that the identities that we count as ethnic are indeed ethnic and that the identities that we do not count as ethnic are not ethnic? Second, we need a conceptualization to design reliable measures for the effect of ethnicity—otherwise, how will we know whether

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the assumptions that we make in designing our measures are reasonable? Third, we need a conceptualization to tell us how to design empirical models to test for the effect of ethnicity using these data and measures. Finally, we need a conceptualization to interpret the results of these models.

Most cross-national empirical research measuring the effect of ethnicity, however, has proceeded independently of a conceptual foundation. We use data on ethnic identities across the world collected without a definition of what those identities are. The authors of one of the influential cross-national data sets on ethnic groups, for instance, declare ethnicity to be “a rather vague and amorphous concept” (Alesina, Devleeschauwer, Easterly, Kurlat, & Wacziarg, 2003, p. 160) that resists definition, and they proceed to count ethnic groups around the world without one. When we do propose definitions, they do not match our classifications, including those in our own previous work.² Our principal measure for the effect of ethnicity—the index of ethnolinguistic fractionalization (ELF)—employs nonobvious assumptions not justified by a conceptualization of the term. We design empirical models somehow, without knowing how the data were generated and what they mean. Our interpretation of models that employ these data and measures is similarly *ad hoc*.

As a result, we do not know what we know: Our studies produce a set of statistical associations but not meaningful conclusions. We also do not know how to improve them. Studies of the effect of ethnic diversity on the onset of civil war, for instance, find that different measures of ethnic diversity are positively related to conflict (Collier & Hoeffler, 2001), positively related to low-intensity armed conflict but not civil war (Hegre & Sambanis, 2001), or not at all significantly related to the probability of civil war onset (Fearon & Laitin, 2003). Studies about the effect of ethnic diversity on democracy or economic growth are similarly contradictory. If we had some yardstick for gauging the match between concepts, data, methods and interpretations, we could adjudicate between these contradictory studies, treat as “provisional findings” the conclusions of the studies which have the closest match, and then revise and refine these findings through cumulative research. But the opaqueness of the ELF measure and the data on which it is based makes cumulative progress much more difficult. Without knowing what the data mean, or how the measures, models and interpretations relate to our concepts, it is hard to know how to improve upon them.

This article evaluates previous efforts to measure the effect of “ethnicity” using the ELF index in cross-national studies and proposes new measures based on a new conceptual framework to think about ethnic identity. We summarize that foundation here, but refer the reader to other work for a defense

and more fully developed discussion (Chandra 2006, 2007 and Chandra and Boulet 2006). Throughout, we make a plea for narrowness in our concepts and measures of “ethnicity.” “Ethnicity,” like “politics,” is a big concept – so big as to be meaningless. When we want to study how “politics” matters, we do not construct datasets on and measures of “politics” in general. Instead, we name some specific aspects of political structure relevant to the outcome—for instance, democratic v/s dictatorial regimes, presidential v/s parliamentary systems, majoritarian v/s proportional electoral rules, the effective number of parties in a country—or political practice—for instance, the content of campaign rhetoric, the size of government expenditure, the number of roll call votes—and collect data and design measures that operationalize these narrower concepts. By the same logic, when studying how “ethnicity” matters, we must also replace that large concept narrower, more meaningful ones.

The key elements of this conceptual framework are as follows: We define an ethnic identity as a *category* in which descent-based *attributes* are necessary for membership. Nominal ethnic identities are those for which we possess the attributes of membership while activated ethnic identities are that subset of our nominal categories in which we profess membership or are assigned membership by others. Virtually all social science definitions agree that descent is somehow important in defining an ethnic group, and many stipulate additional features, such as a common culture, in combination with descent. The principal innovation in our definition is in its precise specification of the role of descent—introduced in the distinction between categories and attributes—and in its elimination of features other than descent. We then use this definition to introduce a distinction between ethnic “structure” and different types of ethnic “practice.” The ethnic “structure of a population consists of the distributions of attributes in that population and is typically multi-dimensional. Ethnic “practice” refers to the ethnic categories activated in different contexts. The ethnic categories activated in any given context need not belong to any single dimension, or be mutually exclusive (what we define later as the problem of overlap). They also need not be exhaustive – they can describe only some individuals in a population, while others are activated on non-ethnic identities (what we define as the problem of incompleteness).

Based on this foundation, we argue that even the most careful studies using the ELF index generate indeterminate conclusions about the effect of “ethnicity” on a range of outcomes, and we introduce and evaluate two alternative measures of ethnic practice that improve upon it—“Ethnic Imbalance (ECI)” —a measure of the representation of groups in colonial governments—and “EVOTE”—the percentage of the vote obtained by ethnic parties. In sum, our critiques of studies that use the ELF index are as follows: (1) We do not know what the

data it is based on are intended to describe – ethnic structure or ethnic practice. Either way, they are flawed. If intended to describe ethnic structure, they are flawed in that they ignore the problem of multidimensionality. If intended to describe ethnic practice they are flawed in that they ignore the problems of overlap and incompleteness. (2) We also do not know what the ELF index is intended to measure—ethnic structure or ethnic practice. Either way, it is flawed. As a measure of ethnic structure must be able to take multidimensionality into account. But the ELF index does not accommodate multiple dimensions. As a measure of ethnic practice, it must address the problems of overlap and incompleteness. But the ELF measure employs the strict assumptions of mutual exclusiveness and exhaustiveness. (3) The interpretations of the statistical effect of the ELF index are not consistent with our definitions and classifications, often within the same body of work. By contrast, EVOTE and ECI are based on data specifically intended to capture very specific aspects of ethnic practice and make no claim to capturing ethnic structure (we think that is much more difficult). As measures of ethnic practice, they do not make any assumptions about exhaustiveness and exclusiveness: both measures hold up even when the activated ethnic categories are incomplete and overlap. And, although they have their own flaws, the definitional and operational criteria guiding their design are transparent enough to produce justifiable interpretations and to generate ways to improve upon them. The main contribution of these new measures is not that they change our previous conclusions about the effect of “ethnicity” but that they generate meaningful conclusions which can serve the foundations for better ones.

This article builds on the insights of several important critiques of the ELF index that have been made in recent years (see Posner 2004, Posner and Laitin 2001, Cederman and Girardin 2007). Posner in particular has both identified several weaknesses of ELF as well as proposed an alternative index—“politically relevant ethnic groups,” or “PREG”—to understand the role that ethnicity plays in explaining economic growth rates in Africa (Posner, 2004). These critiques are important in their own right and we concur independently with many of them. Where we differ is in grounding our critique in a single, integrated conceptual framework that often produces different substantive criticisms with different implications. Take for instance the previous critique that the ELF index does not capture many important aspects of ethnic diversity such as the spatial distribution of groups or the depth of ethnic differences (Posner 2004, 851). This point is not derived from any particular definition of ethnic identity- and it holds independently of any particular definition. The principal implication is that the ELF index may overlook important aspects of ethnic diversity. By contrast, take the instance of our criticism that the ELF

index cannot reasonably make the assumptions of exhaustiveness and exclusiveness. This argument is derived from a particular definition of nominal and activated ethnic identities. Different definitions might yield different critiques. Indeed, in another effort to ground a critique of the ELF measure in a distinct conceptualization of ethnic identity, Mozaffar et al. have identified different criticisms (Mozaffar 2007, Mozaffar, Scarritt and Galaich 2003). And the import of the point we make is that the ELF measure is logically flawed as a measure of ethnic diversity on its own terms, even without taking the spatial distribution of ethnic identities or the depth of ethnic differences into account. Further, we are able to evaluate new measures using the same yardsticks that we use to criticize the flaws in previous work—and judge the substance and nature of improvements we can make in using them. In that sense, starting from a fixed conceptual position allows us to go beyond critique to the generation of new research agendas in a cumulative fashion.

The first section in this article proposes a definition of ethnic identity. The second section proposes one way to break down the large concept of ethnicity into its component parts using this definition. The third section uses the conceptual discussion in the first two sections to evaluate the reliability of the conclusions about the effect of ethnicity reached by using the ELF index as a measure. The fourth section uses the same concepts to introduce and evaluate the measures of ECI and EVOTE, situating them in relation to other alternatives to the ELF index. The fifth section asks, how might these new measures change our conclusions about the effect of ethnicity?

What Is Ethnicity?

Since the publication of Donald Horowitz's *Ethnic Groups in Conflict* (1985), there has been a convergence among comparative political scientists on *which* identities we classify as ethnic. We agree that ethnic identity is an umbrella concept that includes identity categories associated with one or more of the following types: religion, sect, language, dialect, tribe, clan, race, physical differences, nationality, region, and caste.³

We diverge, however, on *why* we classify these identities as *ethnic*—that is, on the definition that justifies placing identities belonging to these types and only some identities belonging to these types—in a separate analytical family. We—that is, social scientists interested in some concept related to ethnic identity as an independent variable—routinely and interchangeably use the term *identity* for a category (a descriptive label that can be used to sort individuals regardless of their own feelings on the matter) and a group (a subset of categories that describes a collection of individuals who not

only share a descriptive label but also think of themselves as a community) (Brubaker, 2004). Regarding what makes an identity ethnic, a survey of definitions in the field reveals that we agree that descent is important but differ over how to specify the rule of descent and which additional features distinguish an ethnic identity in addition to descent (Fearon, 2003; Fearon & Laitin, 2000; Horowitz, 1985; Hutchinson & Smith, 1996). Most social science definitions specify the role of descent in different ways to mean a common ancestry, a myth of common ancestry, a common place of origin, a myth of a common place of origin, or a group descent rule for membership, according to which an ethnic group is one that is defined by a membership rule of the following form: You are coded as A if your parents and grandparents were also coded as As (Fearon & Laitin, 2000). We differ over other characteristics that might define ethnic identities independently or in combination with descent. These additional features include a common culture, a common language, a common history, a common territory, and conceptual autonomy. Of these additional characteristics, a common culture is perhaps the one most frequently associated with ethnic identities, so much so that the term *ethnic group* is taken to be synonymous with the term *cultural community* (see, e.g., Gutmann, 2003; Kymlicka, 1995).

We propose here a new definition. We use the term *identity* to refer to categories rather than groups.⁴ Ethnic identities, in turn, are defined as an arbitrary subset of categories in which descent-based attributes are necessary for membership. We say an *arbitrary* subset because although we can write out the restrictions that wall off the subset of ethnic identities from the larger subset of descent-based identities (Chandra, 2006), these restrictions have not so far been shown to have an analytical value. Nominal ethnic identities are those ethnic identity categories in which an individual is eligible for membership based on the attributes that she or he possesses. Activated ethnic identities are those ethnic categories in which she or he professes membership or to which she or he is assigned by others as a member. All individuals have a repertoire of nominal ethnic identity categories. This consists of all the meaningful membership rules that can be fashioned from an individual's given set of descent-based attributes, with each rule corresponding to a nominal category. The ethnic identity that an individual actually activates is chosen from this repertoire.

The key innovations in this definition are its specification of the role of descent and in its emptying the definition of all features other than descent. In specifying the role of descent, we make a distinction between ethnic identity categories and the descent-based attributes, which are necessary to qualify individuals for membership in those categories. These two concepts

have typically been conflated in the literature, but they are distinct in an important way. The properties of descent-based categories are derived from the properties of descent-based attributes but are different from them. In particular, descent-based attributes have the property of changing over the long term but being fixed in the short term. An individual's repertoire of nominal ethnic identities is also fixed in the short term because it is generated from a fixed set of descent-based attributes. By contrast, the categories that we activate based on these attributes *can* change even in the short term (although they need not). This difference is critical to how we should think about ethnic identities.

Consider, for instance, a woman living in New York with attributes such as dark skin, birth in Trinidad, and descent from parents of African origin (based on characters described in Waters, 1990). In the short term, we can take these attributes as being fixed. If she has dark skin now, she is likely to have dark skin 10 years from now. Because these attributes are fixed in the short term, so is her nominal repertoire of descent-based categories. This repertoire includes the categories West Indian, Black, Trinidadian, and so on, but it does not include the categories Asian or German, for which she does not have the requisite descent-based attributes. But the categories that she activates from this fixed set can change, often quite rapidly. She may well switch back and forth between the identities Black, West Indian, Trinidadian, and others, depending on the incentives that she faces without any change in her underlying set of attributes (based on Waters, 1990).

In the long term, even her descent-based attributes might change and with it, her repertoire of nominal ethnic identity categories. Take, for instance, the attribute of dark skin. The commonsense interpretation of some shade of skin color as *dark* and another shade as *light* is the product of a humanly created system of interpretation. These commonsense frameworks can indeed change, although such changes are typically slow and take place over generations.

The principal justification for this definition is that it captures the conventional classification of ethnic identity categories in comparative politics to a greater degree than the alternatives. We substantiate this claim in some detail elsewhere (Chandra, 2007) by considering the match between each of the definitions proposed above, including ours, and a representative sample of sets of identities chosen from across continents that are classified as *ethnic* by at least three of four main cross-national counts: *Atlas Narodov Mira* (Miklukho-Maklai Ethnological Institute, 1964), Horowitz (1985), Fearon (2003), and Alesina et al. (2003). This includes Black and White in the United States; Serb and Croat in the former Yugoslavia; Muhajir, Punjabi, Pathan, and Baluch in Pakistan; Flemish and Walloon in Belgium; Aymara

and Quechua in Bolivia; Yoruba, Ibo, Hausa, and Fulani in Nigeria; and Zulu, Xhosa, and Coloured in South Africa.

The purpose of constructing a definition by proposing criteria that justify a conventional classification—rather than stipulating some objective meaning of the term—is to use it to construct cumulative rather than self-standing propositions about ethnic identity. A self-standing proposition about ethnic identity takes the simple form “Ethnic identity is associated with Y.” A cumulative proposition takes the more complex form “Consistent with or contrary to previous research, ethnic identity is associated with Y.” For self-standing propositions, one can simply stipulate a definition—“By the term ethnic identity, we mean A”—based on some conception of the objective essence of the term, independent of its usage or its etymological structure, or even on entirely arbitrary criteria. As long as the proposition is consistent with the stipulated definition within a single piece of research, nothing more is required. But a proposition that builds on or rebuilds the foundation of previous research must either use the term *ethnic identity* in the same way or, when there is a departure, specify the relationship between past and present usage. Suppose that in previous usage, the term *ethnic identity* meant A and the present proposition takes the term to mean B. Then we cannot make a statement of the form “Consistent with or contrary to previous work, ethnic identity is associated with Y,” unless we know what the relationship between A and B is and can be confident that the statement is consistent with that relationship. Thus, constructing a definition that captures previous usage as far as possible is essential to moving ahead in a cumulative fashion.

Given, however, that we indicate that the basis of this classification is arbitrary—that the restrictions that separate the subset of ethnic identities from the set of descent-based identities is arbitrary—should we continue to use the concepts of ethnic and nonethnic identities or discard them in favor of the concepts of descent-based and non-descent-based identities? The answer depends on how we trade off the benefits of greater precision with the cost of theorizing about and collecting data on the new concepts that would replace it. Descent is correlated to a greater degree with ethnic identities than with nonethnic identities: Even though all descent-based identities are not ethnic, all ethnic identities, we have argued, are based on descent, whereas nonethnic identities are based on many attributes other than those generated by descent. Thus, the distinction between ethnic and nonethnic identities does indeed capture a meaningful distinction between descent-based and non-descent-based identities, although this is an imprecise one. If we seek to make general claims about average trends across

multiple countries and over long periods, the gain in precision by using a new concept may not be worth inventing a new term and reinterpreting the results of previous research using this term. However, we should recognize that there is likely to be a lot of unexplained variance, even in claims about average tendencies, and we should interpret our claims accordingly. If we seek to make point predictions about a single outcome in a single country in a single period, then the gain in precision may be large enough to make replacing the concept worthwhile.

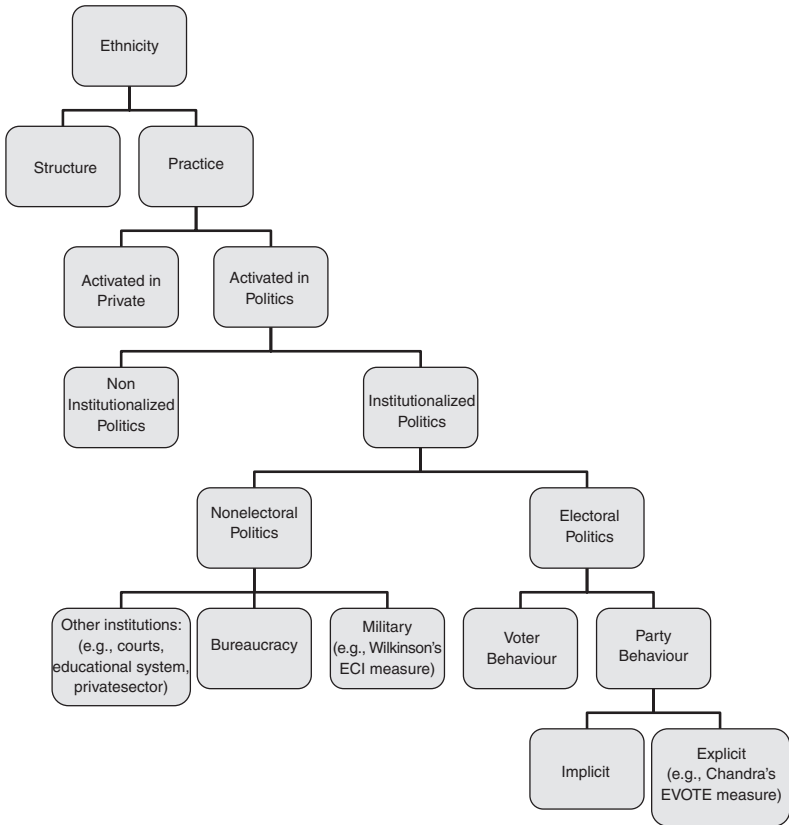
Breaking Down the Concept of Ethnicity

In Figure 1, we diagrammatically outline one way of breaking down the concept of ethnicity. Each node represents a concept, and each branch connects this concept to the narrower concepts contained within it. We focus selectively here on those concepts most relevant to the discussion that follows. A full representation of this tree would include additional branches at each node.

At the broadest level, we can imagine the term *ethnicity* as encompassing two families of concepts—the structure of ethnic identities and the practice of ethnic identification. *Ethnic structure* refers to the distribution of descent-based attributes—and, therefore, the sets of nominal identities—that all individuals in a population possess, whether they identify with them or not. *Ethnic practice* refers to the act of using one or more identities embedded in this structure to guide behavior. In other words, it refers to the set of activated identities that individuals employ in any given context. The set of activated ethnic identities for any given country is typically a subset of the identities contained in the ethnic structure.⁵ The distinction between ethnic structure and ethnic practice is more precise than but is consistent with distinctions that others have made between commonsense real and mobilized ethnic identities, latent and active identities, and so on.⁶ But importantly, there is no basis on which to assume, as others have in the past, that activated identities belong to any single dimension—they could consist of attributes from several dimensions.

The ethnic structure of a population tends to be fixed in the short term because it is based on the distribution of descent-based attributes; however, because ethnic practice is based on activated categories, it can change. For instance, Little (1998) shows us how even though the ethnic attributes of members of Kenya's Il Chamus community have remained largely fixed

Figure 1
Ethnic Structure and Ethnic Practice



over the past century, community leaders have activated membership in different ethnic categories over time, identifying as Chamus in some periods and as members of the larger ethnic categories of Samburu or Masaai at others (Little, 1998). Such changes in activated categories, a large constructivist literature has shown us, can be triggered by incentives created by political institutions, employment opportunities, social interactions, and so on. The possibility of change in the identities activated in practice need not be realized—we might often see stability in the activated ethnic categories that describe a population for long periods. But such stability should be

demonstrated rather than presumed to exist, and where it exists, it should be treated not as natural fact but as something to be explained.

Over the long term, however, ethnic structure can change, too. Indeed, there may be a relationship between change in ethnic practice in the short term and ethnic structure in the long term. Imagine, for instance, that at some initial point in history, political entrepreneurs work to transform the interpretation of some descent-based attribute in the ethnic structure—for instance, taking a shade of skin color commonly interpreted as dark and arguing that we should see it in a differentiated way, separating light brown, dark brown, and black. If successful, this enterprise would make a piecemeal change in ethnic structure in the future. Today's structure, then, can be in part the product of the ethnic practice of a distant yesterday, and today's ethnic practice can affect the ethnic structure of a distant tomorrow. But at any given time, we can distinguish between structure and practice.

The ethnic identities activated in practice differ according to whether they are activated in private life, and the set of ethnic identities that are activated in political life. By *private life*, we mean that aspect of life that concerns individuals alone or their immediate family and friends. By *political life*, we mean that aspect of life that concerns collective action by individuals who are not bound by immediate personal ties. Caste identity in Sri Lanka (e.g., Goyigama) is an example of an identity that informs private actions, such as the choice of a marriage partner. But religious and linguistic identities (e.g., Buddhist or Sinhala) rather than caste are the principal identities invoked in collective action (Rajasingham-Senanayake, 1999; Tambiah, 1986).

Among the set of identities that are politically activated, we can distinguish again between identities that are activated in institutionalized politics, parliament, party politics, the legal system, and so on, and identities that are activated in noninstitutionalized contexts, such as civil war, riots, and social movements. In many countries, the set of identities activated in both contexts may be identical, but in others—especially in states that outlaw certain types of political participation—they can diverge. In Indonesia in the 1992 elections, for instance, institutionalized participation by political parties activates religious identities (Muslim and Christian), whereas regional identities are more likely to be found in the arena of noninstitutionalized politics. And in the Belgian Congo, until shortly before independence in 1960, the Belgians banned colony-wide political parties—but not tribal and regional associations, of which there were 317 by 1956—which led to the structuring of identities in formal politics along quite different lines from those in associational life (Willame, 1972).

Among identities that are activated in institutionalized politics, we can distinguish further between identities activated in electoral contexts, party politics, and voting behavior and those in nonelectoral contexts, in the corridors of parliament, the military, the judiciary, and the bureaucracy. In Uganda, for instance, the identity of Nubian was an identity activated principally in the military and the corridors of the bureaucracy of Idi Amin's regime, whereas the identities Baganda and Catholic have frequently been activated in the course of electoral politics (Kasfir, 1976, 1979).

Among the identities activated in electoral politics, we can distinguish between identities that drive voter behavior and those that drive party strategy. In principle, we should expect there to be some connection between the identities that parties activate and those that condition voter behavior. But the two concepts are analytically distinct and may sometimes diverge. In South Africa in the 1994 elections, for instance, the African National Congress activates voters based on multiethnic appeals targeted to all South Africans and in some contexts, on appeals based on the racial category *Black*. But voters often voted for it, not on the basis of being South Africans or Blacks, but on the basis of their tribal identities (Xhosa, Zulu, etc.).

Among identities activated in party politics, we can distinguish between identities activated using implicit and explicit appeals. The Willie Horton advertisements used by the Republican Party in its 1988 presidential campaign in the United States are an example of an implicit appeal to race (Mendelberg, 2001). By contrast, the election campaign run by Slobodan Milosevic in 1992, in the immediate aftermath of communist rule, made an explicit appeal to Serbian identity.

We could go on breaking down ethnic practice further at each node and conducting a similar breakdown for ethnic structure. But the discussion above should be sufficient to situate the discussion that follows. By abandoning the use of the term *ethnicity* in favor of these narrower concepts, our goal is not to argue that any one of them works in isolation. They may well work together. But separating these concepts makes it possible to identify precise relationships and test for them with some confidence about what we are testing. Furthermore, we can ask questions about the effect of one aspect of ethnicity on another, which we would not be able to do if we did not draw these distinctions in the first place. For instance, once we separate ethnic imbalance in colonial government from ethnic imbalance in post-colonial governments, we can ask if one has an effect on the other. If we separate ethnic imbalance from ethnic mobilization of voters, we can ask if imbalance is a cause of such mobilization. If we separate the implicit mobilization of ethnic identity from explicit mobilization, we can ask whether each type of mobilization has different consequences.

What Have We Learned From Previous Work That Uses the ELF index

The principal measure of the effect of ethnicity in cross-national empirical work is the ELF index, calculated according to the formula $1 - \sum s_i^2$, where s_i is the proportion of the i th activated ethnic category, $i = \{1, 2, \dots, n\}$.⁷ The ELF index is also the standard measure of the effect of ethnicity in empirical research, not sampled here, in which ethnicity is not the main focus but is used as a control variable (e.g., Przeworski, Alvarez, Cheibub, & Limongi, 2000). In both types of work, the index is typically calculated on one or more sources of the following three sources of data: *Atlas Narodov Mira*, published in 1964 by Soviet ethnographers at the Miklukho-Maklai Ethnological Institute, a data set on ethnic groups in 190 countries published by Alesina et al. in 2003, and a comparable count of ethnic groups in 160 countries published by Fearon in the same year (Fearon, 2003).

It is difficult, in much cross-national research, to make inferences about the effect of ethnicity on dependent variables from studies that employ ELF and its variants for three reasons: (1) The lack of a match between data and concept. (2) The lack of a match between measure and concept. (3) The lack of a match between the interpretation of the analyses using these measure and data and the concept of “ethnicity.” We discuss each point in turn.

Lack of a Match Between Data and Concept

Structure or practice? It is not clear whether the data sets on which the ELF index is based capture ethnic structure, defined above as the set of ethnic categories that are commonsensically real, or ethnic practice, defined above as the ethnic categories that are activated. Consequently, we cannot interpret an association between a measure using these data and the outcome of interest.

The approach taken by *Atlas Narodov Mira* (Miklukho-Maklai Ethnological Institute, 1964) is not spelled out. Alesina et al. (2003) refer to ethnic groups as the product of “persistent identification” (p. 161), thus conflating structure and practice. Fearon (2003), by contrast, makes an explicit distinction between the two, aiming to code for ethnic structure by trying to capture commonsensically real identities—“how people in the country mentally divide the social terrain in ethnic terms” (p. 203)—and not ethnic practice, at least in the political realm. But the criteria for operationalizing this distinction in the coding procedures are not laid out, and if we look at the data, we find that all three data sets veer inconsistently between collecting data on ethnic structure and ethnic practice.

Consider the example of Albania. Each of the three data sets code ethnic groups in Albania as Albanian, Greek, and Macedonian. But why not include in their count ethnic groups based on religion (Catholic, Orthodox, and Muslim) or dialect (Gheg speakers, concentrated in the north, and Tosk speakers, concentrated in the south)? These other identities also appear to be commonsensically real. The principal distinction between the excluded groups and the included ones seems to be that the Albanian, Greek, and Macedonian groups have more political resonance in the present than do the others. Inadvertently, then, the data sets appear to be coding practice rather than structure in the case of Albania.

But in Italy, the pattern is reversed. The *Atlas Narodov Mira* (Miklukho-Maklai Ethnological Institute, 1964) codes Italy as being 98% Italian, with a range of smaller groups making up the remaining 2%, including Austrians, French, Slovenians, and Albanians. Fearon (2003) codes Italy as an almost entirely homogeneous country, with a 98% majority of Italians. Alesina et al. (2003) code Italy in a comparable way, as 94.0% Italian, 2.7% Sardinian, 1.3% Rhaetian, and 1.9% other. But if we look at politically mobilized identities in Italy, at least in electoral politics, we see several other identities mobilized by political parties, including regional identities (North and South), subregional identities (Milan and Lombardy), and racial identities (native versus immigrant Italians). Germany, similarly, is consistently coded by all three data sets as having an overwhelming German majority, ranging from 91.0% in Alesina et al. to 98.8% in *Atlas Narodov Mira*, with several exceptionally small minorities. But why only count groups on the basis of nationality or race in Germany and not on region—East Germans, for instance, or Bavarians—which our study of election campaigns in these countries, discussed in the fourth section, reveals are identities activated by political parties? By excluding these identities, the data sets all appear to be attempting to capture some undefined notion of structure rather than practice.

Structure. If the data refer to ethnic structure, then they are flawed in that they ignore the problem of multidimensionality and level of aggregation. The structure of ethnic identities in most countries is multidimensional, although the number and type of dimensions can vary. The set of identities that are commonsensically real in the United States, for instance, includes identities based at least on the dimensions of race, nationality, region, religion, and tribe. In India, it includes identities based on the dimensions of caste, language, tribe, region and religion. In Zambia, it includes identities based on the dimensions of tribe and language. In South Africa, it includes identities

based on the dimensions of race and tribe. In Malaysia, it includes identities based on the dimensions of race, language, region, religion, and tribe.

Previous critiques have suggested that in inputting data into the ELF index, we first need to make a decision about which of these dimensions is the most relevant one (Posner 2004). But, indeed, why should we assume that there is only one dimension to choose? If the set of commonsensical identities in a country is multidimensional, we then need to faithfully represent that multidimensionality.

Furthermore, categories on each dimension are arrayed at multiple levels—how do we decide which ones to include and at which level of aggregation (Laitin & Posner, 2001)? When faced with the dimension of tribe in India, should we count categories at the highest level of aggregation (e.g., scheduled tribe) or at the lowest level (Santhal, Munda, Bhil, etc.) or somewhere in between? When faced with the dimension of religion in the United States, should we count on the basis of metacategories (Christian, Muslim, Jewish) or micro categories (Methodist, Baptist, Presbyterian, Shia, Sunni, Ismaili, Hasidic, Orthodox, etc.)?

Alesina et al. (2003) and Fearon (2003) acknowledge these problems and discuss them at some length, but they do not furnish the decision rule that they employ to solve them, and we cannot infer the rule from looking at their data. To illustrate, consider the coding for India across the three data sets, displayed in Table 1.

The *Atlas Narodov Mira* (Miklukho-Maklai Ethnological Institute, 1964) counts a cluster of groups on the basis of several dimensions—language (e.g., Hindi speakers and Tamils), nationality (Portuguese, English), tribe (Santals, Munda), religion (Jews), caste (Gujars, Jats). But the choice of groups included from each dimension is arbitrary, and we cannot discern the logic to the level of aggregation chosen.

Alesina et al. (2003) include two groups from the highest level of aggregation on the dimension of language (Indo-Aryan and Dravidian). But given that they are concerned with a count of ethnic groups, it is not clear why they choose the dimension of language rather than the dimensions of caste, religion, region, and tribe, and it is not clear why they choose the highest level of aggregation in this case, in contradiction of their intention to collect data at as disaggregated a level as possible. (In the same data set, Alesina et al., 2003, collect data separately on the dimensions of religion and language but never make clear how these dimensions are separate from, rather than contained within, the concept of ethnic identities.)

Fearon (2003) reports the ethnic structure as being made up of several groups defined on the dimension of language—speakers of Hindi (.39),

Table 1
Different Codings for Indian Ethnicity Across Three Data Sets

Groups Included in Count	ELF
<i>Atlas Narodov Mira</i> (Miklukho-Maklai Ethnological Institute, 1964)	
Hindi-speaking peoples of North India (.25), Bihars, Maraths, Bengals, Gujarats, Rajastans, Oriya, Panjabs, Assams, Kumaoni, Kashmirs, Bhils, Gujars, Sindhi, Gurkhi, Pars, Jhats, Shina, Kho, Kohians, English, Jews, Pushtuns, Portuguese, Telugs, Tamils, Kannara, Malayali, Gondi, Tulu, Oraoni, Kandhi, Kodagu, Badaga, Irula, Urali, Maler, Mannans, Malavedans, Kurumba, Kadari, Paniabs, Toda, Kota, Chenchu, Santals, Munda, Ho, Savara, Korku, Bhumidji, Kharia, Gadaba, Djuangs, Minipuri, Naga, Garo, Balti, Lushei, Kachars, Tipera, Mikiri, Kirats, Kuki, Tamangs, Ladahs, Bhoti, Thado, Miri, Abor, Mishmi, Dafla, Limbu, Lepcha, Kanauri, Lahauli, Gurungs, Nevars, Magars, Sherps, Sunvars, Burmese, Kachins, Chinese, Khanti, Khasi, Nikobars, Burishs, Andamanese	.89
Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003)	
Indo-Aryan (.72), Dravidian (.25), other (3.0)	.42
Fearon (2003)	
Speakers of Hindi (.39), Bengali (.08), Telugu (.08), Marathi (.07), Tamil (.06), Gujarati, Malayalam (.05), Kannada (.04), Oriya (.03), Punjabi (.03), Sikhs (.02), Assamese (.01)	.81

Note: ELF = ethnolinguistic fractionalization.

Bengali (.08), Telugu (.08), Marathi (.07), Tamil (.06), Gujarati, Malayalam (.05), Kannada (.04), Oriya (.03), Punjabi (.03), Assamese (.01), and one group on the dimension of religion, Sikhs (.02). But why not include other groups on the dimensions of religion (Hindus and Muslims, e.g.) or tribe (scheduled tribes and others) or caste (at the highest level of aggregation, this would include upper castes, backward castes, and scheduled castes)? And on the dimension of language, why not include groups at a higher level of aggregation—for example, Indo-Aryan languages (including Hindi, Bengali, Gujarati, Punjabi) and Dravidian languages (including Telugu, Tamil, Kannada, and Malayalam)?⁸

In an improvement over the other two data sets, Fearon (2003) does provide a conceptual justification for his count. He attempts to include groups that fulfill as many of the following prototypical criteria as possible: First, membership is reckoned primarily by descent. Second, members are conscious of group membership. Third, members share distinguishing cultural features. Fourth, these cultural features are valued by a majority of members.

Fifth, the group has or remembers a homeland. Sixth, the group has a shared history as a group that is “not wholly manufactured but has some basis in fact.” Seventh, the group “is potentially stand alone in a conceptual sense—that is, it is not a caste or caste-like group” (p. 201).

But we cannot tell how these conceptual criteria are operationalized in the coding process. Determining whether the members of a group have a factual rather than a fictitious history or whether they value distinguishing cultural features or whether they are conscious of group membership is no easy task, even for those who specialize in a particular country or a particular group. How might a coder make these decisions, and how might others replicate them? Nor is it clear how many prototypical criteria a group must satisfy to be included or how a coder should decide between multiple candidate groups on multiple dimensions that fit the prototypical criteria. Why, for instance, was the category *Jat* (included in *Atlas Narodov Mira* [Miklukho-Maklai Ethnological Institute, 1964] but not in Fearon’s work [2003]), which appears to meet the first six criteria but not the seventh, not chosen over the category *Punjabi*, which appears to meet the first, third, fourth, and fifth criteria but not the second, sixth, and arguably, seventh? Some of the groups included in Fearon’s count do not meet several of the conditions. Hindi speakers, for instance, are not a group in which members are conscious of group membership, share distinguishing cultural features that are valued by a majority of members, and have or remember a homeland. Furthermore, several groups excluded from this count appear to meet several of the prototypical criteria, such as Indo-Aryans and Dravidians, Hindus and Muslims, and scheduled tribes. These differences are consequential: The ELF index jumps from .42 to .89 depending on the data chosen, and the size of the largest ethnic group, also a common measure constructed on the basis of these data sets, ranges from .25 (the size of Hindi speakers in the *Atlas Narodov Mira* data) to .39 (the size of Hindi speakers in the Fearon data) to .72 (the size of Indo-Aryans in the Alesina et al. data [2003]).

Practice. If the data refer to ethnic practice—the set of identities that are politically or otherwise activated—then they are flawed because they overlook the problem of overlap and incompleteness. Take, first, examples of overlap. We have no reason to expect that the ethnic categories that individuals activate in practice should be mutually exclusive. Indeed, in many of the cases at which we have looked, the mobilized categories are overlapping. In a count of politically activated ethnic categories in India in the 1991 elections (described at more length in the fourth section), political parties activated the following ethnic categories (proportion of the population in

parentheses): Hindus (.82), Muslims (.12), Sikhs (.02), other backward classes (.52), scheduled castes (.16), Jharkhandis (.03), Assamese (.26), and Tamils (.07). The categories of Hindu, Muslim, and Sikh are mutually exclusive in relation to one another but overlap with the categories other backward classes, scheduled castes, Jharkhandis, Assamese, and Tamils. Sometimes, there are cases of total overlap so that one category is entirely nested within another. In Belgium, for instance, among the categories mobilized by political parties are native Belgians, who constitute 91% of the population, and French speakers, who constitute 42% of the population and who are largely contained within the category of native Belgian.

Now take examples of incompleteness. There is no rule that individuals in a population should all activate their ethnic identities exclusively. Indeed, it is only a few very polarized countries at particular points in time, such as Yugoslavia in 1992, where almost the entire population lines up behind an ethnic identity—but even in such countries, the ethnic identification may not be complete. The data on political parties that we discuss below shows that 86% of the population in Yugoslavia voted for ethnic parties in 1992, leaving a minority of voters who voted for other types of parties. In other countries, we typically see several types of identities activated in practice. In the 2004 U.S. presidential elections, for instance, some voters activated their class identities (e.g., middle class), others their party identities (Republican and Democrat), others based on age (e.g., pensioners), and still others their racial identities (e.g., Black). But the point is not confined to identities activated by political parties. If we surveyed individuals in any country about which identities they were activated on, our guess is that it would be a rare country in which 100% of the population indicated an ethnic affiliation. When our data sets produce mutually exclusive and exhaustive counts that add up to 100% of a population, then what exactly do these data mean?

Lack of a Match Between Measure and Concept

Recall the formula for the ELF index: $1 - \sum s_i^2$, where s_i is the proportion of the i th activated ethnic category, $i = \{1, 2, \dots, n\}$. This formula requires the ethnic categories to be mutually exclusive (i.e., if you are in Ethnic Category 1, then you are not in Ethnic Categories 2– n) and exhaustive (every member of the population is in some ethnic category). Given mutual exclusiveness and exhaustiveness, this index measures the probability that two randomly chosen individuals from a country's population belong to different groups. Thus, a society with two groups, a majority of 80% and a minority of 20%, would have an ELF index score of $1 - (.64 + .04) = .32$.

A society with several small groups of 25% each would have a higher ELF index score of $1 - (.0625 + .0625 + .0625 + .0625) = .75$.

But as we have noted above, the multidimensionality of ethnic structures and the overlap and incompleteness associated with ethnic practice violate one or both assumptions. Thus, we cannot take the ELF index to be a reasonable measure of either ethnic structure or ethnic practice.

To illustrate the problem that the multidimensionality of ethnic structures poses for the ELF index as a measure of ethnic diversity, compare India and Zambia. We note above that at least five dimensions of ethnic identity are commonsensically real in India, with approximately two to seven categories arrayed on each, if we confine ourselves to the highest level of aggregation. In Zambia, only two dimensions of identity are salient: tribe and language (Posner, 2005). The dimension of language currently has four groups arrayed on it at the highest level of aggregation: {Bemba speakers, Nyanja speakers, Tonga speakers, Lozi speakers}. The dimension of tribe has roughly 70: {Chewa, Tembuka, Bemba, . . . }. Which country is more diverse given variation in the number of dimensions and categories in each? The ELF index cannot tell us, because it cannot incorporate these data.

To illustrate the problem that the overlap and incompleteness of ethnic categories activated in practice poses for the ELF index as a measure of ethnic diversity, let us return to the example of India. As we note above, in 1991 political parties activated the following categories in the Indian parliamentary elections: Hindus (.82), Muslims (.12), Sikhs (.019), other backward classes (.52), scheduled castes (.16), Jharkhandis (.031), Assamese (.26), and Tamils (.066). Given that these categories are neither exhaustive nor mutually exclusive, putting them into the ELF index yields a nonsensical number.

Lack of a Match Between Concept and Interpretation of the Measure and Data

Even if we assume that the data and the measure are conceptually justified, the interpretations of the results using that data and measure are not, for one or more of three reasons: First, they are inconsistent with our understanding of what ethnic identity is; second, they do not make a distinction between the effect of some feature intrinsic to ethnic identity and the effect of a feature of the context in which ethnicity is activated; and, third, they treat the ELF index as an all-purpose measure of ethnicity rather than a measure of one narrow concept related to it.

Interpretations that are inconsistent with our conceptual understanding. Much of the work that uses the ELF index interprets it as a measure of

something exogenous to individual choice. Sometimes, this interpretation is explicitly stated. Ordeshook and Shvetsova (1994), for instance, make the remarkable assertion that ethnic heterogeneity “is not a product of individual choice—rather, it is better portrayed as an exogenously determined social state” (p. 108). More often, it is buried in the assumptions informing the design of the study. It is conventional in time-series analyses, for instance, to treat the ELF index as a constant for the entire period, in sharp contrast to other variables. Take Przeworski et al. (2000) as one influential example. The authors are meticulous in constructing annual measures for democracy and on economic development over a 50-year period and taking the possibility of endogeneity between the two into account. But the effect of the ELF index on democracy is calculated and interpreted using the same data across this 50-year period.

According to the conceptualization of ethnic identity introduced here, some aspects of ethnic identity can indeed be treated as being constant in the short term. Ethnic structure, for instance, can certainly be treated as being exogenous in the short term, whereas ethnic practice can change endogenously. And even among the ethnic categories activated in practice, some may be stable for long periods, whereas others change. But for the reasons outlined above, we have no way of knowing whether our data on ethnic heterogeneity measure structure or practice. And if it is indeed the categories activated in practice that are counted, we have no way of knowing whether they are stable or not. Thus, we cannot support interpretations based on the claim or the assumption that ethnic heterogeneity is not a product of individual choice. It may be—but then again, it may not.

Similarly, the relationship between the ELF index and the outcome of interest is often interpreted as an indication of the relationship between cultural differences and that outcome. Fearon and Laitin’s (2003) work is simply one influential example of the body of work that treats ethnicity and culture as interchangeable concepts. But if we look at the identities that we *classify* as ethnic, they are often distinguished only by descent-based attributes—not by a common culture, however defined (Chandra, 2006). Indeed, many of those whom we put into the same ethnic category are put there despite their cultural differences with other members, not because of them. At the same time, many of the identities that we do not classify as being ethnic (e.g., class identities, professional identities) are often distinguished by a common culture (Chandra, 2006). Take, for instance, those categorized as Black and as White in New York. The cultural vocabularies of many of those categorized as Black are incomprehensible to one another (Malcolm X, 1964); yet, they would classify themselves and be classified by others as members

of the same ethnic category. And although a middle-class Black may have been able to perfectly comprehend a White neighbor from her or his class and educational background, this shared cultural vocabulary would hardly lead one or others to classify these two individuals as being part of the same ethnic category. We cannot therefore reasonably interpret a relationship between the ELF index and some outcome as an indicator of the effect of culture.

Interpretations that do not distinguish between the effect of features intrinsic to ethnic identity and contextual features. An example may help to illustrate this point. In an examination of the relationship between different types of identity in India and violence, Wilkinson (in press) found that religious identities in India were more likely to be associated with violence than linguistic identities. Based on what he knew about the data and the context, he interpreted the finding as one not about the effect of religion per se but about the effect of institutions. The institutional context in India provides legalized channels for linguistic mobilization but not religion-based mobilization. Consequently, religious identities were diverted into violent channels of protest. In a different institutional context, the argument implied, the same association might be found between class-based identities and violence, or language and violence, or ideological identities and violence, and so on.

This is an instance in which a correlation between a measure of an ethnic identity and the dependent variable—in this case, a religious identity—could not be straightforwardly interpreted as proof of the effect of a feature intrinsic to ethnic identity. Rather, it was the effect of the context in which that ethnic identity was institutionalized. However, much cross-national statistical work using ethno-fractionalization indices does not consider in practice—even if it does so in theory—the distinction between these two effects, automatically assuming that an association between ELF and the outcome of interest tells us something about the relationship between something intrinsic to ethnicity and that outcome.

Interpretations that treat the effect of the ELF index as an indicator of the effect of ethnicity. Ethnicity, we have argued throughout, is simply a label for many concepts. The ELF index can at best be seen as a measure of ethnic diversity (based on structure or practice) but not a proxy for the effect of ethnicity as a whole. Yet, that is how it is often interpreted. If the ELF index—or some other individual measure—does not matter, then we argue that ethnicity does not matter. And if the ELF index—or some other individual measure—is shown to matter, then we read the results as indicating that ethnicity matters.

Among these works, Fearon and Laitin (2003) are perhaps the most careful in the measures that they use and in interpreting their results. They use a battery of measures to operationalize several different concepts related to ethnic identity, including ethnic grievances, linguistic and religious discrimination by the state, and the size of the largest ethnic group. But even they sometimes assume that this handful of concepts stands in for ethnicity broadly defined. In their words, the fact that there is no association between the ELF index and civil war onset “runs contrary to a common view among journalists, policy makers, and academics, that holds *ethnically divided* [italics added] states to be especially conflict-prone due to ethnic tensions and antagonisms” (p. 75). In our view, a test of ethnic structure is not a test of the ethnic division hypothesis. Rejoinders to this work use the same general language. As one example, consider Cederman and Girardin (2007), who argue that their N^* index “has a strong effect which is highly significant, thus casting doubt on the tendency to ignore *ethnic politics* as an explanation of civil wars” (p. 173; italics added).

Using the ELF index or any other single measure as an all-purpose measure of the effect of ethnicity or ethnic divisions or ethnic politics is as illogical as using the index of effective number of parties—calculated in a similar way, using the proportion of the vote won by political parties rather than the proportion of the population made up by ethnic groups—to operationalize the effect of politics. Suppose that to measure the effect of politics on economic growth, we specified a model that examined the association between the effective number of parties and a growth measure. In evaluating this model, the reader would immediately ask—why is the effective number of parties the right concept and measure to use? And suppose that we found that the effective number of parties had no association with that growth measure. We would hardly accept this as evidence of the claim that politics does not matter for growth. We might be able to rule out the effect of the effective number of parties, but there are surely many other ways of measuring the effect of political competition on growth. By the same logic, the fact that the ELF index or any other individual measure matters or does not matter does not tell us whether ethnicity matters or does not matter.

ECI and EVOTE: Two Alternatives to the ELF index

In recent years, those interested in the effect of ethnicity have begun to develop new indices and concepts that measure some specific aspect of ethnicity. Proceeding in chronological order, the Minorities at Risk Database

now provides data on a range of concepts for over 200 ethnopolitical groups (see the project's Web site at <http://www.cidcm.umd.edu/mar/>). Reynal-Querol (2002) proposes a new index of ethnic polarization. Scarritt and Mozaffar (2003) propose an alternative measure of politically relevant groups that recognizes that there are multiple levels of ethnic identity operating simultaneously and thus makes precise distinctions between commonsensically real groups, politicized groups, and particized groups. Posner (2004) published an index of politically relevant ethnic groups for Africa, which was designed as an improvement over the ELF index when the variable of interest is the effect of political competition on growth. Cederman and Girardin (2007) recently developed N^* , a measure of ethnonationalist exclusiveness intended to replace the ELF index in "assessments of the role of ethnicity in internal conflicts" (p. 173). And Bossert, d'Ambrosio, and La Ferrara (2006) have recently developed the generalized index of fractionalization, an indicator that can simultaneously measure similarity and dissimilarity of multiple identities (class, income, ethnicity, race), depending on which ones we think are theoretically most important. None of these indices is a one-size-fits-all replacement for the ELF index, nor do any escape all the problems with it identified above. But each improves on the ELF index for some class of problems.⁹

In this section, we review two alternative cross-country measures from our own work that capture concepts related to ethnicity: First, ECI is a measure of the imbalance between the representation of ethnic groups in the army, bureaucracy, and civil services of colonial states. Second, EVOTE is a measure of the aggregate percentage of the vote obtained by ethnic parties across countries in 1996. Both ECI and EVOTE capture aspects of ethnic practice: the representation of ethnic groups in nonelectoral politics (in the case of the ethnic imbalance measure) and the explicit mobilization of voters by political parties (in the case of EVOTE). Returning to the diagram in Figure 1, note that ECI captures an aspect of how ethnic identities are activated in nonelectoral politics by looking at one institution—the military. EVOTE, by contrast, captures an aspect of how ethnic identities are activated in electoral politics by looking at the explicit behavior of political parties.

Ethnic Imbalance (ECI)

In the study of ethnic conflict, many scholars have focused on the relative degree of inequality among groups—especially in terms of groups' disproportional access to or control of the state, the military, the police, education, and the economy—as an important driver of conflict. The distinction

between backward and advanced groups along these dimensions, for instance, is central to Horowitz's influential theory of conflict (1985), which draws on several decades of research by social psychologists on group categorization and discrimination. Backward groups, Horowitz shows, "have frequently exhibited severe anxiety about threats emanating from other groups" (pp. 175-176). One form that this anxiety takes is that of apprehension about being dominated and turned into

hewers of wood and drawers of water. Conciliatory leaders are being depicted as being excessively generous in granting concessions to ethnic strangers, whose intentions are, allegedly, to take control of the country and subordinate the backward group. Every issue can then become a survival issue. (p. 179)

The imbalances between groups that see themselves as advantaged and disadvantaged and whose leaders regard their windows of opportunity to act as being relatively small before their groups face permanent exclusion from power form a constant theme in much of the comparative literature on ethnic conflict.

In what follows, we introduce one possible way to directly measure the imbalances among ethnic groups in key state institutions, derived from Wilkinson's ongoing study of the long-term impact of colonialism on ethnic conflict. Wilkinson is interested in measuring the extent to which the colonial state left a highly unequal and imbalanced ethnic distribution of power in government, education, business, the administration, and the army at the point of independence. It seems likely that directly paying attention to such ethnic imbalances will be much better than using the ELF index or another off-the-shelf ethnic data set in explaining why colonies with apparently similar inheritances in terms of political institution building and the presence of trained personnel (such as that in India and Pakistan) have done so differently in terms of their postindependence stability and levels of ethnic conflict.

The most important imbalance in terms of causing conflict, and the one we focus on here, was that of ethnic imbalance in the military. The overrepresentation of some groups in the army and the underrepresentation of others—even if the underrepresented groups were large and influential in politics or the economy—have been identified by many scholars as an important driver of ethnic violence, democratic instability, and civil wars in the postindependence era (Decalo, 1990; Gutteridge, 1962; Horowitz, 1985). Among the rank and file, this ethnic imbalance was often of long standing—for instance, that in Pakistan, where as a result of recruitment patterns set in the late 19th century, East Pakistanis (52% of the state's population) accounted for only 1% or less of the positions in the army and

higher administration at independence in 1947 whereas West Pakistanis (in particular, people from the province of Punjab) had a majority of positions in the army and administration. In Sudan, just to provide another example, the overwhelming proportion of positions in the administration and in the army at independence were occupied by Arabs from the north, rather than Southerners (Johnson, 1992; Shepherd, 1966).

Ethnic imbalances could also be severe among the officer corps, and the overrepresented ethnic groups among the officer corps were different from those groups overrepresented in the ranks. This was the result of the colonial states' rapid attempt to localize their previously European-dominated officer corps in the run-up to independence by employing well-educated recruits, who tended to come from different ethnic groups than those that dominated the rank and file (Gutteridge, 1962; Horowitz, 1985; Miners, 1971). In Nigeria, for instance, as in West Africa more generally, many of the new officers were educated men from the south and port cities, whereas the bulk of the enlisted men had long been from tribes living in interior districts in the north (Horowitz, 1985; Miners, 1971).

How might we measure such imbalances? The most appropriate measure in terms of not only reflecting the theoretical mechanisms in which we are interested but also simply being easy to calculate and understand is a slight modification of the ECI, developed by Gayl Ness (1967) in the 1960s to measure ethnic imbalance in Malaysia.¹⁰ As we can see in Table 2, to calculate a country's ECI, we simply sum the differences between each group's share of the population and its share in the army, government, or whatever sphere in which we are interested. A score close to zero indicates a state where there is little ethnic imbalance, whereas a country with an ECI score of 1.0 or more would be highly imbalanced. Table 2 shows how the ECI score is calculated: The first half depicts a relatively high-imbalance country, in which the ECI is 0.96, whereas the second shows a low-imbalance country, in which the imbalance is 0.10.

Although there are good reasons to think that ethnic imbalances in politics, the economy, the civil service, and the army are all significant for explaining the outcomes in which we are interested, reasonable data coverage is unfortunately available (and with some difficulty) for only the last two of these.¹¹ However, the army and civil service are clearly key institutions: They were often the major focus of ethnic competition during the colonial period, and severe imbalances in either have the potential to lead to much wider conflicts, as we have seen in Sri Lanka, Pakistan, Uganda, Kenya, Nigeria, and elsewhere. In fact, it is partly because of the key importance of these services that we have reasonable data on ethnic balances:

Table 2
Ethnic Concentration Index (ECI)

	Proportion of Population	Proportion of Occupation	Δ
High-imbalance country			
Group A	0.50	0.10	0.40
Group B	0.25	0.50	0.25
Group C	0.13	0.05	0.08
Group D	0.12	0.35	0.23
Sum Δ	1.00	1.00	ECI = 0.96
Low-imbalance country			
Group A	0.25	0.30	0.05
Group B	0.25	0.20	0.05
Group C	0.25	0.25	0.00
Group D	0.25	0.25	0.00
Sum Δ	1.00	1.00	ECI = 0.10

Note: After Ness (1967).

Colonial legislators asked for breakdowns to redress perceived ethnic balances; colonial administrations kept records of ethnic balances to make sure that they were recruiting from “loyal” groups; and postindependence scholars have, of course, paid attention to the ethnic breakdown of these services because of their continuing political importance.

One important issue, however, involves how we use data to calculate the index. How should we identify the relevant ethnic group classifications on the basis of which to count? Wilkinson uses the ethnic group and territorial classifications provided in the original colonial data on the military. Using the colonial classifications provides detailed information on group and intragroup identities made salient by colonial policy and boundaries that may be politically meaningful but may not match at all the ethnic categories that are used to generate indices such as the ELF index. They are likely to track the identities and imbalances that are actually related to conflict rather than those that are simply meaningful to anthropologists or linguists, even if these salient identities are also endogenous to state policy.¹²

Calculating the ECIs for this article provides some striking data on the extent of the ethnic imbalances in many countries’ armed forces at independence. In Kenya, for instance, as a result of the British recruitment from groups regarded as *martial*, the ECI at independence was quite high: 0.96. This reflected the heavy recruitment from groups such as the Samburu

Table 3
Ethnic Imbalance in the Colonial Iraqi Military

	Proportion of Population	Share in Military	Δ
Kurds	0.18	0.05	0.13
Sunni	0.16	0.90	0.75
Shia	0.63	0.05	0.58
Turcomans	0.02	0.00	0.02
Sum Δ	1.00	1.00	ECI = 1.46

Source: Wilkinson database (see Chandra, 2006, 2007, in press), including Tarbush (1982).

Note: ECI = Ethnic Concentration Index.

(11% of the population, 36% of the army) and Kalenjin (11% of the population, 26% of the army) and the very low levels of recruitment from the Kikuyu, Embu, and Meru (27% of the population, 3% of the army; Parsons, 1999).¹³ In Singapore, the imbalances were even more striking (ECI = 1.31) because of the heavy recruitment of the minority Malay community to the security forces, in which 20% of all Malay men were employed, compared to only 4% of the general population (Bedlington, 1981).

Another state that was profoundly imbalanced during the colonial period was Iraq (Table 3). When the British organized the Iraqi military in the 1920s, they decided to preserve the Sunni bias of the Turkish provincial forces from which many of the Iraqi soldiers and officers were drawn. As we can see from the table, which displays the composition of the Iraqi officer corps in the colonial period, the recent struggle between a Sunni-dominated military state and a non-Sunni majority is emphatically not just a product of the Saddam Hussein regime. Iraq's officer corps in the colonial period had an ECI of 1.46, reflecting the overwhelming Sunni dominance in the army. This massive imbalance in the officer corps was largely replicated in the ranks, prompting Shia protests throughout the later colonial period over discrimination against their community (Tarbush, 1982).

We provide some comparative statistics in Table 4 to show how the ECI rankings for various colonial armies at independence compare to those in the more widely known ELF index, as well as the improved index of ethnic fractionalization (EF) data set and index of cultural difference (CULDIF), developed by Fearon (2003; see also Fearon's Web site, <http://www.stanford.edu/~jfearon/>). The EF index uses improved data on group proportions to recalculate the EF data, and the CULDIF index is Fearon's effort to measure, using data on language, the degree of cultural difference in a population.

Table 4
A Comparison Between the Ethnic Concentration Index (ECI)
and Other Ethnic Fractionalization (EF) Indices

	ECI ^a	ELF ^b	CULDIF ^c	EF ^d
Australia	0.01	0.32	0.15	0.15
Nigeria	0.20	0.87	0.66	0.80
Malawi	0.42	0.62	0.29	0.83
Syria	0.58	0.22	0.24	0.58
Sri Lanka	0.59	0.47	0.39	0.43
Ghana	0.77	0.71	0.39	0.85
Kenya	0.96	0.83	0.60	0.85
India	0.99	0.89	0.67	0.81
Burundi	1.00	0.04	0.04	0.33
Lebanon	1.03	0.13	0.19	0.78
Burma	1.23	0.48	0.42	0.52
Pakistan	1.30	0.64	0.29	0.53
Singapore	1.31	0.42	0.39	0.39
Uganda	1.53	0.90	0.65	0.93

a. Ethnic imbalance, as measured by a modification of the ECI (Ness, 1967), based on Wilkinson's data (Chandra, 2006, 2007, in press).

b. Ethnolinguistic fractionalization, as measured by the ELF index (*Atlas Narodov Mira*; Miklukho-Maklai Ethnological Institute, 1964).

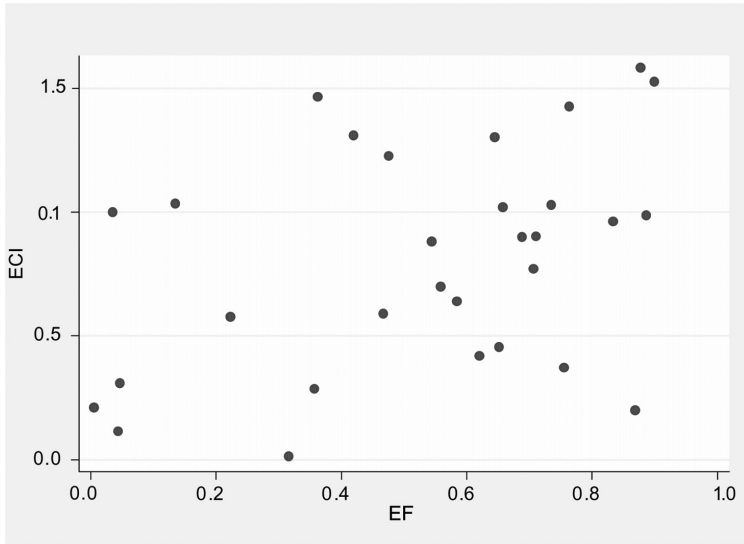
c. Cultural difference, as measured by Fearon's (2003) index of cultural difference.

d. For these data, based on the EF index, see Fearon's Web site: <http://www.stanford.edu/~jfearon/>.

The ECI and these other indexes are clearly measuring different things, as we can see from the scatter plot between ECI and EF in Figure 2, as well as from the correlation coefficients between ECI and these other indices: The correlation coefficient between the ECI data and the EF index is .43 and between ECI and CULDIF is $-.39$. The potential value of the ECI as a likely predictor of conflict is suggested when we compare Australia and Burundi, which are both at the low end of the range on the EF index (.14 and .33) and ELF index (.31 and .03)—though not on Fearon's CULDIF indicator—but which look completely different on the ECI data, with Australia at .16 and Burundi at a very high 1.00.

The comparison between India and Pakistan on these three measures is also revealing. Why has India done better than Pakistan since independence in terms of internal levels of conflict? The conventional measures do not help us here, given that India is more fractionalized and has a higher CULDIF level than does Pakistan. But ECI does seem to help explain the difference. In Pakistan (ECI = 1.30), the imbalance between the Punjabi-dominated east

Figure 2
Scatter Plot of the Indices of Ethnic Fractionalization (EF)
and Ethnic Concentration (ECI)



and Bengali west was at the heart of the Bangladeshi campaign for independence and the 1970-1971 civil war, as well as in subsequent conflicts in West Pakistan between Punjabis and the smaller ethnic groups: Rizvi (2000) points out that even today, although

Pakistan officially discarded the British concept of martial races for recruitment to the Army and somewhat expanded the recruitment base . . . ethnic imbalances persist in the Army. The Punjab continues to provide the bulk of officers and other ranks; unofficial estimates put its share as 65 and 70 per cent respectively. (p. 240)

India, however, had a considerably lower ECI figure at independence (0.988 compared to 1.303), a reflection of the fact that although India, like Pakistan, inherited a heavily Punjabi army (the state of East Punjab accounted for 42% of the Indian military in 1947), the Punjabi proportion was nowhere near a majority of the Indian army. Moreover, the category of Punjabi in the Indian army, unlike that in Pakistan, was cross-cut by an

important religious cleavage between Punjabi Hindus (19% of the army) and Punjabi Sikhs (23%) and by the fact that the Punjabi presence in the army was balanced by a broader representation of other states than that in Pakistan.¹⁴ The dominant groups in such major states as Uttar Pradesh and Bombay, for instance, were represented in the army more or less in proportion to their population.

In other countries too the ECI measure appears to make more sense than the other indices when we think about the relationship between ethnic practice and conflict. Malawi's low level of ethnic imbalance (ECI = 0.42) in its army helps to explain its relative stability since independence despite its high level of ethnic fractionalization (EF = 0.83). Burundi's, Sudan's, and Lebanon's high levels of ethnic imbalance in their armies at independence, however, may help us to understand why they have been so unstable since independence.

Perhaps the most puzzling cases in the above list—which go against the general expectation that conflict rises with ECI—are those of Singapore, Sri Lanka, and Nigeria. In Singapore, the explanation for low conflict despite a high imbalance at independence seems to be that the imbalance was immediately recognized as a potential major problem by the new, Chinese-dominated government, which rapidly reduced the Malay proportion in the Singapore security forces during the 1965-1971 breathing space provided by the staged British withdrawal from its military base in Singapore (Bedlington, 1981). Sri Lanka and Nigeria both have relatively low imbalances in the army at independence but high postindependence conflict. The explanation in both cases seems to be that the ECI, useful though it is, gets at only some of the most salient aspects of ethnic practice.

In Nigeria, the relatively mild northern imbalance in the rank and file fails to take account of the early dominance of southerners in the officer corps; in 1960, only 14% of the officers were from the north. It was a mismatch that led to several coup attempts in the 1960s as southerners sought (unsuccessfully) to take action to prevent the likelihood of northern dominance (Horowitz, 1980; Miners, 1971).

In Sri Lanka, some of the answer to the puzzle comes from the fact that the Tamils have a significant intragroup cleavage between Sri Lanka Tamils and the separate Indian Tamil community (11% at independence) who were brought over in the century before independence to work the plantation economies in the central highlands and who had virtually no representation in the army and civil services. By counting both groups as part of the single category *Tamils*, we somewhat underestimate the ECI for the Sri Lankan military. But the more important part of the puzzle is that

the imbalance in the military that Wilkinson is able to capture is only one part—though an important part—of the overall ethnic imbalances that drove the conflict. As Tambiah (1986) has pointed out, much of the conflict in Sri Lanka, whose education system has produced many graduates, has focused on the threat (from the Sinhalese perspective) of Tamil overrepresentation in medicine, administration, and the professions, not just in the military and civil services. It was discrimination against Tamils in government service and higher education in the 1960s and 1970s that created the environment in which the movement for Tamil independence flourished, especially among alienated Tamil high school and college graduates.

EVOTE: Aggregate Vote Obtained by Ethnic Parties Across Countries

EVOTE is a time-sensitive measure of the aggregate vote obtained by ethnic parties across countries. It is one of over a hundred variables from the Constructivist Dataset on Ethnicity and Institutions (CDEI) project, constructed by Chandra in collaboration with graduate students at MIT and New York University. The following discussion is based on a cross-sectional version of the data set, which includes that on EVOTE for over a 100 countries in the lower-house legislative election closest to but before 1996. We chose these 100 countries on the basis of whether they held competitive elections—measured by Keefer's (2005) index of legislative competitiveness—but are now expanding the coding across space to cover any country which held an election, competitive or not, and over time.

EVOTE is constructed as follows: First, we classify each political party in each country for which we can obtain data as *ethnic*, *multiethnic*, or *nonethnic* on the basis of its campaign in the legislative election closest to but before 1996. Then, we add up the percentage of votes obtained by all ethnic parties in a given country. Thus, EVOTE for Country A is constructed as follows: $EVOTE_{96}(\text{Country A}) = \text{Vote for Ethnic Party 1}(\text{Country A}) + \text{Vote for Ethnic Party 2}(\text{Country A}) + \text{Vote for Ethnic Party 3}(\text{Country A}) + \dots + \text{Vote for Ethnic Party } n(\text{Country A})$. In principle, the value of EVOTE can range between 0% (for countries with no ethnic parties) to 100% (for countries in which all votes are captured by ethnic parties). In reality, EVOTE for the year 1996 ranges from 0.00% (e.g., in Greece) to 85.63% (in Yugoslavia), with a mean value of 12.95%.

The classification of parties, which is the foundation of EVOTE, is based on the definitions proposed in Chandra (2004), according to which an ethnic political party is

a party that represents itself to voters as the champion of the interests of one ethnic category or set of categories to the exclusion of another or others, and makes such a representation central to its strategy of mobilizing voters. (p. 3)

The key aspects of this definition are as follows: *exclusion*, an ethnic party must make an appeal on behalf of some ethnic group (or groups) that excludes others; *explicitness*, the appeal must be open; and *centrality*, the appeal must be central to its mobilizing strategy. A multiethnic party also makes an open appeal related to ethnicity central to its mobilizing strategy but assumes a position of neutrality and equidistance toward all relevant groups. In other words, it differs from an ethnic party only in its inclusiveness. A nonethnic party is one that does not make an ethnic appeal central to its mobilizing strategy. Note that these definitions classify parties on the basis of their messages, and because messages can change across elections, they are time sensitive: A party classified as an ethnic party in one election need not be classified the same way in subsequent elections.

The concept of EVOTE attempts to measure an aspect of ethnicity related to the behavior of political parties: the use of an explicit ethnic appeal. It does not capture implicit behavior. By restricting EVOTE to explicit appeals, we are not taking the position that implicit appeals are unimportant—simply that they are different. Separating implicit and explicit appeals makes it possible to test for the effect of one on the other and of each on other outcomes, separately. Indeed, several other variables in CDEI, not discussed here, capture the implicit ethnic appeals made by political parties.

Even among explicit appeals, EVOTE is only one of the many ways of measuring the explicit behavior of political parties. Others in this family include variables that code the aggregate vote captured by nonethnic and multiethnic parties, names and sizes of the ethnic groups explicitly mobilized by political parties, the types of identities explicitly mobilized by political parties, the number of identity types mobilized in each country, the proportion of an explicitly mobilized ethnic group that votes for its own party, and so on. These other measures of explicit appeals are all separately coded in CDEI, but here we focus simply on EVOTE.

The coding of the parties is based on a content analysis of the election campaign of the party in question using four sources: the *Europa World Yearbook*, the *Political Handbook of the World*, news sources from the Foreign Broadcast Information Service, and Lexis-Nexis searches. For each party, we obtain a sample of campaign materials (speeches at election rallies, policy pronouncements, and so on) as reported in Foreign Broadcast Information Service and Lexis-Nexis for a period up to 3 months before the election date. These include reports from the international media and

translations of local news reports from newspapers, radio, and television. These samples have three advantages: First, they are primary sources that report what parties are actually saying to voters rather than what they print in their manifestos. Second, many of these sources are translations of what parties say to voters in local languages. Third, they are time-sensitive sources that report party statements for the year of election. Where the samples are too small to permit reliable codings, we turn to local newspapers and secondary sources as a last resort. These sources give us a sample of articles for the election platform of each party. We archive the materials for each party for each country after completing the coding. This archive, composed of a uniform set of source materials for each observation (country or party), makes it possible to doublecheck old variables as we proceed and to construct new variables as they become important.

The coding is based on a protocol that establishes rules for the identification of an appeal as being ethnic, explicit, or central. The content analysis is qualitative rather than quantitative. Thus, rather than establish centrality simply by counting the number of times that an issue is mentioned, the protocol identifies rules of interpretation for centrality—an ethnic appeal can be central on the basis of an issue's frequency but also the way in which an ethnic appeal is used. For instance, a party that associates an ethnic category with ownership of the state would be coded as an ethnic party even if statements to this effect were not frequent, on the basis of the reasoning that once such a statement is made, it colors the interpretation of other statements. If, given the content analysis, we find that a political party makes an open and exclusive appeal to some ethnic category or set of categories and that such an appeal is central to its campaign, we code it as an ethnic party. If we find that a political party makes an open and inclusive appeal to all ethnic categories that define a population and makes such an appeal central to its election campaign, we code it as a multiethnic party.¹⁵ And if we find that a political party does not make an open appeal or a central appeal to an ethnic category, whether exclusive or inclusive, we code it as a nonethnic party. We document each coding for each party on each country, compare it with codings in other data sets where available, check for consistency across coders, and assign a reliability score to the coding (1 = *high certainty*, 2 = *moderate certainty*, 3 = *low certainty*) based on the quality of information in the sample. Because countries can sometimes have over a hundred parties, each with a separate sample, this level of documentation adds considerably to the time required to construct this data set. But it is important if other researchers are to replicate our efforts and estimate the bias and error in the data.

Consider the case of India as an example of our coding procedures. Hundreds of parties competed in India in the 1991 parliamentary elections (the elections closest to but before 1996), but most of them obtained a miniscule percentage of the vote. We obtained disaggregated data on all parties that obtained at least 0.01% of the vote, thereby including 66 parties in our data set. We then coded each of these 66 parties on the basis of a content analysis of its party platform. Of the 66 parties, we coded 13 parties, accounting for 51.81% of the vote, as *nonethnic* and 18 parties (38.95% of the vote) as *ethnic*, but we were not able to find sufficient articles on election platform to code the remaining 35 parties, accounting for 10.24% of the vote (these were small parties, with a mean vote of 0.14%).

Our purpose in constructing EVOTE and related variables is to investigate two principal questions: First, what explains variation in the incidence and performance of ethnic parties across countries? Second, what is the relationship between the explicit political mobilization of ethnic identity and political stability? However, this measure is also of value to social scientists interested in exploring the effect of the politicization of ethnic identities, at least as measured through the party system, on a range of outcomes. One such set of questions is the following: What is the effect of the explicit politicization of ethnic divisions (measured using EVOTE) on some outcome of interest, including war, riots, economic growth, public policy, welfare spending, and so on? Replacing ELF with EVOTE in this body of work would be a meaningful test of whether one concept—namely, the degree to which ethnic identities are explicitly politicized in the party system at a particular point—matters in explaining any of these outcomes, while leaving open the possibility that some other aspect of ethnicity might also matter in ways not captured by EVOTE.

Other questions that can be addressed using EVOTE include the following: Is the politicization of particular types of ethnic divisions (e.g., region, religion, language, tribe) associated with particular types of outcomes? This would entail using the percentage of votes won by regional, religious, linguistic, or tribal parties across countries as measures of the political salience of these types of divisions across countries? What determines the size of the coalition that an ethnic party is likely to mobilize? To assess this, we would need to use the party as the unit of analysis and take the proportion of the ethnic party's target category as the dependent variable. Are we more likely to see the ethnification of politics in new democracies? This would entail treating EVOTE (or one of its substitutes) as the dependent variable and using the age of the democracy (measured in the Przeworski et al. data set [2000]) or the presence of founding elections as an independent variable. Is

there a link between colonial history and the degree of ethnic politicization? This would entail regressing EVOTE (or its substitutes) on the range of variables on colonial history being collected by Wilkinson and others.

EVOTE addresses several of the problems identified with the ELF index, and the data that inform it, in the following way: First, it codes variables on the basis of ethnic practice, consistently separating this from ethnic structure. Second, it has a clear criterion for how to identify the relevant categories and dimensions. In the realm of ethnic practice, the categories and dimensions counted in CDEI are those that are named by the parties in question. Thus, the CDEI count of explicitly activated ethnic categories in India is based on all the categories explicitly mobilized by political parties in the 1991 parliamentary election campaign (the national legislative election closest to but before 1996). Third, it does not impose any assumptions about mutual exclusiveness and exhaustiveness; rather, it allows us to observe such completeness in the data.

In countries in which all individuals activate ethnic identities in their voting behavior, the value of EVOTE would be 100%. In countries in which only some individuals activate ethnic identities in their voting behavior, the value of EVOTE would be less than 100%. In the Indian case, the value of EVOTE is .39. This proportion is unaffected by whether the ethnic parties in question mobilize mutually exclusive or overlapping categories. In India, it so happens that the parties activate overlapping categories. But the value of EVOTE would be the same even if the parties in question activated mutually exclusive categories. Similarly, EVOTE also does not impose the requirement that the categories activated by political parties be complete, because it is the votes won by the parties that mobilize each category that are added, not the proportion of the population made up by the categories themselves.

How is EVOTE related to ELF? There appears to be no relationship. Table 5 compares the data on some individual observations. As we see, countries with similar levels of EVOTE (Canada, India, and Guatemala) vary widely in their measures of ELF no matter which data set we use. And India and Sri Lanka, with a comparable value on ELF (at least using the Alesina et al. data [2003]) have different values on EVOTE.

Taking all observations together, the correlation between EVOTE and the ELF index based on the *Atlas Narodov Mira* data (Miklukho-Maklai Ethnological Institute, 1964) is only .07. The correlations between EVOTE and the ELF index, calculated from the Fearon data (2003) and the Alesina et al. data (2003) are similarly weak: respectively, .11 and .08. Figure 3 summarizes the relationship between the two measures. Figure 3 uses

Table 5
A Comparison Between EVOTE and Fractionalization Indices

	EVOTE	Year of Election for EVOTE	ELF ^a	ELF ^b	ELF ^c	CULDIF ^d
Canada	34.4	1993	.75	.71	.60	.50
Guatemala	41.9	1995	.64	.51	.49	.49
India	38.9	1991	.89	.42	.81	.67
Sri Lanka	6.9	1994	.47	.41	.43	.39

Note: ELF = ethnolinguistic fractionalization; CULDIF = cultural difference.

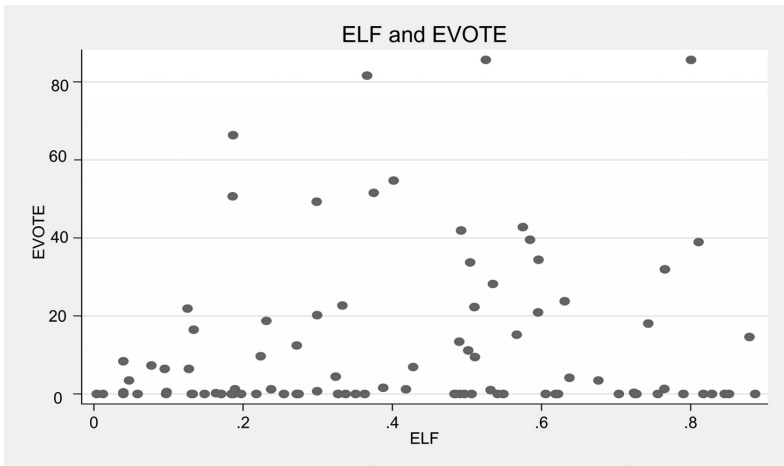
a. Index of ethnolinguistic fractionalization based on *Atlas Narodov Mira* (Miklukho-Maklai Ethnological Institute, 1964).

b. Index of ethnolinguistic fractionalization based on Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003).

c. Index of ethnolinguistic fractionalization based on Fearon (2003).

d. Index of cultural difference based on Fearon's data (see his Web site: <http://www.stanford.edu/~jfearon/>).

Figure 3
Scatter Plot of the Index of Ethnolinguistic Fractionalization (ELF) and EVOTE



the data on ethnic groups provided by Fearon (2003), for which there is the greatest overlap in the number of observations, but the pattern remains the same using the ELF indices based on the ELF measures using the other two sources of data.

What Might We Learn From ECI and EVOTE About the Effect of Ethnicity?

To illustrate, we revisit the argument made by Fearon and Laitin (2003) about the effect of ethnicity on the onset of civil war. The authors find that “it appears *not* to be true that a greater degree of ethnic or religious diversity—or indeed any particular cultural demography—by itself makes a country more prone to civil war” (p. 75). The principal piece of evidence supporting this claim is an empirical model demonstrating that there is no statistically significant relationship between the ELF index, calculated using Fearon’s (2003) data on ethnic groups, and the likelihood of civil war onset. They find a similar absence of association when they use three other measures for the same concept: the size of the largest ethnic group (using the same data), the number of distinct languages spoken by groups exceeding 1% of the country’s population (using data from the Ethnologue database), and a measure of religious fractionalization (using data from the same sources used to construct the data set on ethnic groups; e.g., *CIA World Factbook*). We conduct a comparison here between what we learn from adding ECI or EVOTE to the core model in Fearon and Laitin’s work for the limited observations for which we have data. We reproduce the database of Laitin and Fearon not because we differ with their central findings in the role of capacity in explaining civil wars but because they make a clear argument on the basis of tests using what we would call *ethnic structure variables*. More pragmatically, we also choose their 2003 article because they have been especially good at making their data easily available for others to replicate.¹⁶ The general points we make here would apply to any other cross-national empirical study using the ELF index.

In Table 6, we report the results of this preliminary comparison. Column 1 estimates the broad empirical results in Fearon and Laitin 2003 (Table 1, Model 1).¹⁷ With annual data on 160 countries between 1945 and 1999, the Fearon and Laitin model is based on a total of 6,327 country–year observations. Columns 2 and 3 compare this core model with a model that adds the ECI variable. Column 2 replicates the model in column 1 for the limited observations for which we have data on ECI. Because Wilkinson has data on ECI for less than 40 countries, this yields a truncated data set of 1,265 country–year observations over the same period. Column 3 adds ECI to the model in column 2. Column 4 replicates the model in column 1 for the 846 observations for which it is feasible to run the model using EVOTE. Column 5 adds EVOTE to the model in column 4. The observations that we use in these two columns include the 91 countries for which there was an

Table 6
New Measures of Ethnic Practice and Civil War Onset

	Original Model ^a	Adding ECI		Adding EVOTE	
	1	2	3	4	5
Prior war	-0.961 (0.315)***	-1.715 (0.635)***	-2.143 (0.646)***	-2.134 (1.151)*	-2.330 (1.195)*
Per capita income	-0.346 (0.072)***	-0.420 (0.156)***	-0.544 (0.206)***	-0.403 (0.211)*	-0.441 (0.226)*
Log (population)	0.265 (0.073)***	0.250 (0.173)	0.385 (0.184)**	0.315 (0.246)	0.239 (0.270)
Log (% mountains)	0.213 (0.084)**	0.496 (0.203)**	0.056 (0.229)	0.270 (0.261)	0.349 (0.283)
Noncontiguous state	0.438 (0.275)	-0.169 (0.550)	-0.549 (0.650)	1.218 (1.032)	1.020 (1.103)
Oil exporter	0.829 (0.275)***	1.330 (0.515)***	1.098 (0.528)**	1.771 (0.985)*	2.263 (1.100)**
New state	1.726 (0.338)***	-0.054 (0.821)	-0.190 (0.846)	3.107 (0.937)***	3.336 (1.003)***
Instability	0.616 (0.235)***	-0.081 (0.466)	-0.071 (0.474)	0.247 (0.722)	0.363 (0.720)
Democracy	0.020 (0.017)	0.045 (0.034)	0.054 (0.036)	0.054 (0.066)	0.062 (0.067)
ELF	0.219 (0.363)	-0.386 (0.747)	-2.097 (0.987)**	1.554 (1.445)	1.765 (1.501)
ECI			2.091 (0.680)***		
EVOTE					0.032 (0.016)**
Constant	-6.647 (0.722)***	-6.137 (1.675)***	-7.138 (1.809)***	-8.025 (2.420)***	-8.068 (2.534)***
Observations	6,327	1,265	1,265	846	846

Note: Standard errors in parentheses. ELF = ethnolinguistic fractionalization; ECI = ethnic concentration index (ethnic imbalance).

a. Fearon & Laitin's Model 1, excluding fractionalization variable.

* $p < .10$. ** $p < .05$. *** $p < .01$.

overlap between countries in the Fearon and Laitin data set and Chandra's data set for the years 1991 to 1999. Therefore, the sample is truncated by time in addition to space. We choose this period because EVOTE, unlike ECI and ELF, is a time-sensitive measure. It reports the total vote share won by ethnic parties in legislative elections between 1991 and 1996 in countries that held competitive elections. This may well change in different elections.

We restrict the sample to the years 1991-1999 on the reasoning that EVOTE can at best be seen as a measure of the political activation of ethnic identities in the years proximate to this election—but it would be a stretch to think of EVOTE as a measure of the political activation of ethnic identities in more distant periods.

As we see in the original Fearon and Laitin model (2003) reported in column 1 of Table 6, the ELF index is not significantly associated with the likelihood of onset of civil war in the complete data set. It continues not to be significant when we run a regression (column 2) using only the limited sample (1,265 observations) for which the ECI variable is available. But in column 3 we find that when we include the ECI variable in the regression, it is positive and significant in explaining civil war onset (regardless of whether we include ELF, as we do here, or exclude it). This association is not just the result of the much smaller number of observations, because ELF continues not to be significant for the same set of observations. Note too that in column 3 the sign on ELF changes direction and becomes significant. Similarly, in column 5, we find that when we include the EVOTE variable in the regression, it is positive and significant in explaining civil war onset (regardless of whether we include ELF, as we do here, or exclude it). Because the ELF index is not significant for the same set of observations in column 4, we know that this finding is not just the result of the much smaller number of observations.

Interpreting the Association

We cannot say whether these results will hold up with a more complete data set. But taking them as provisional data, we can indeed identify more about what we have learned and what we have not from these measures than we can from the ELF index. We discuss below a substantive interpretation of the effect associated with all three measures.

Fearon and Laitin (2003) interpret the lack of a statistical association between the ELF index and the likelihood of the onset of civil war as evidence that what they term *ethnic demography* (and what we term *ethnic structure*) does not have a direct effect on civil war. This is an especially careful interpretation in several ways. First, Fearon and Laitin link their measure with a concept: They note explicitly that the ELF index and the concept of ethnic diversity refer to ethnic structure and not ethnic practice. Second, it is based on several measures of ethnic diversity. Third, their interpretation of the lack of association between these measures and the onset of civil war is nuanced. They take this to mean only that there is no

direct relationship between ethnic structure and the onset of civil war, noting that there may well be an indirect relationship.

But given the lack of a conceptual justification guiding the design of the data sets and measures, even this careful interpretation is difficult to support. Take the effect associated with the ELF index. As we have argued in the body of this article, we simply do not know whether the data used to compute the ELF index reflect ethnic practice or ethnic structure. Similarly, we do not know what the index captures. The second measure—the size of the largest ethnic group—is more justifiable because it does not employ the assumptions that drive the ELF index. But here too, we have no way of knowing whether the data used in constructing this measure describe the largest group based on ethnic structure or ethnic practice. Third, take the measure of ethnic diversity based on linguistic distance. Without a discussion on how the boundaries between language groups are drawn, we do not know if these data are reliable measures for linguistic structure or linguistic practice for the countries in the data set. But ethnicity, as we note earlier, is not defined simply by language. Thus, even the most justifiable data set on linguistic structure cannot tell us what the effect of ethnic diversity is, broadly defined. The same points apply to the data and measures on religious fractionalization. Because we do not know what these data and measures mean, we also do not know how we might improve on them to generate more precise conclusions.

Consider now what we learn and what we do not learn from the use of ECI and EVOTE. ECI and EVOTE capture particular aspects of ethnic practice—the representation of ethnic groups in nonelectoral politics, in the case of the ethnic imbalance measure, and the explicit mobilization of voters by political parties, in the case of EVOTE. Furthermore, they capture ethnic practice at different points in time. ECI is a measure of ethnic practice at the time of independence from a colonial power. The imbalance reflected in the ECI measure may or may not be reflected in the period in which a civil war breaks out. As such, it is a measure of the long-term effects of historical ethnic practice. By contrast, EVOTE is a measure of political mobilization around ethnic identities in the period proximate to a civil war. In the model above, the election for which EVOTE is coded occurs just before, just after, or in the same year as the year in which a civil war broke out.

We can take these results to suggest that ethnic practice has some effect on the likelihood of violence. The precise effect remains to be elaborated, but we can map out an agenda for ascertaining the nature of this effect on the basis of how we conceptualize and measure these variables. Take ECI first: ECI can be taken as one measure of interethnic disparity presumed to

link ethnic diversity to the likelihood of violence. Suppose we continue to find an association between ECI and the onset of civil war when we have a more complete data set. We know that ECI is a measure of historical practice. To identify whether it does indeed increase the likelihood of civil war, we would have to theorize about and measure the link between historical interethnic imbalances and the actual outbreak of civil war. Might the shadow of colonial inequities affect the likelihood of civil war regardless of postindependence events? One way to test this hypothesis would be direct, using data on postindependence ethnic imbalances; but given the difficulty of collecting data on these, we could also simply reduce the magnitude of the ECI indicator as we move further away from the year of independence. Furthermore, we should find a link between ECI in some period and the activation of ethnic identities in the period proximate to violence. This could be EVOTE (to the extent that institutionalized activation increases the likelihood of violence) or a measure of noninstitutionalized ethnic activation (e.g., protests, riots, strikes). If we did not find a link between ECI and a measure of ethnic mobilization, we would have to identify a different chain of mechanisms leading to civil war onset.

Consider now what we learn from the association between EVOTE and the likelihood of civil war. The positive association between EVOTE and the onset of civil war for the cases for which we have data tells us that in countries with competitive elections, the mobilization of ethnic identities by political parties and the onset of civil war are positively related. But we cannot say at this point that one causes the other. In the model above, the election for which EVOTE is coded occurs just before, just after, or in the same year as the year in which a civil war broke out. We do not know if EVOTE was similarly strong in these countries in elections preceding civil war. Therefore, we cannot say that the activation of ethnic identities by political parties caused the civil war—it may well have been its product. Alternatively, and more likely, the explicit activation of ethnic identities by political parties and civil war (or lesser forms of violence) may well coevolve. A proper test would require time-series data on EVOTE on a broader set of countries (and not just those that hold highly competitive elections), which we are in the process of collecting.

Just as important, we can also clearly identify what these results do not suggest. They do not tell us anything about the relationship between ethnic structure and the likelihood of civil war. At this point, we cannot tell whether ethnic practice as measured by ECI or EVOTE is a product of ethnic structure or independent of it. Thus, we cannot tell if ECI and EVOTE have an effect on the likelihood of civil war or whether they are simply

intervening variables in a causal chain triggered by the presence of a particular form of ethnic structure. All that we do know is this: There is a weak bivariate association between ELF and ECI and between ELF and EVOTE (reported in the preceding section) and no statistically significant association between ELF and the likelihood of civil war. If we assume that ELF captures the concept of ethnic structure, we can interpret this to suggest that ethnic structure has a weak relation to ethnic practice. This weak association might lead us to conclude that ethnic practice affects the likelihood of civil war independently of ethnic structure. But what we know about ELF hardly warrants such an informative conclusion. A more reliable conclusion about the relationship between ethnic structure, ethnic practice, and the likelihood of civil war requires the collection of new data and more appropriate ways of measuring ethnic structure than the ELF index.

Even without a precise measure of ethnic structure, we suspect that, by looking closely at EVOTE and ECI in individual cases, it may not determine ethnic practice. Take the case of India. Although we do not have a measure, we might guess that there are at least 5 times as many ethnic identities embedded in India's ethnic structure as were actually explicitly activated by political parties in politics. We could also reasonably say that the ethnic structures of Canada, Guatemala, and India are distinct. The number and type of ethnic identity dimensions relevant in each country and the categories arrayed on each appear, at least at first glance, to be different. Yet, each country had an equivalent percentage of voters voting for ethnic parties our data: 35% in Canada, 39% in India, and 41% in Guatemala. Conversely, the ethnic structures of India and Sri Lanka are similar: Both countries, although of very different sizes, are divided by language, religion, region, and caste. But the degree to which explicit ethnic rhetoric is used in both countries—and the proportion of voters who are voting for ethnic parties in both countries—is quite distinct. Only 7% of voters voted for explicit ethnic parties in Sri Lanka, compared to 39% in India.

The same can be said of the gap between ethnic structure and ethnic imbalance. Take Pakistan and Burundi, countries of dramatically different sizes and structures. Pakistan's ethnic divisions are based on tribe, region, language, religion, and caste, with no majority group on any except the dimension of religion. Burundi, by contrast, appears to be a simpler ethnic structure, with one principle division based on tribe (for want of a better word), with a dominant majority and two smaller minorities. Yet both had very high ECIs at independence: 1.3 and 1.0, respectively.

Evaluating and Improving the Measures

Neither ECI nor EVOTE is free of bias and measurement error. In some cases, neither has been able to escape some of the problems that characterize the ELF index and previous data. In other cases, there are new biases and errors. But to the extent that these measures and the concepts that they capture are transparent, they help us in deciding how to assess the reliability of our findings and how to improve them.

Take the case first of ECI, which relies on census classifications for its count of ethnic categories. Our previous data sets also rely heavily on national censuses. But ethnic identity is, of course, a dependent variable as well as an independent variable, so the census data that underlie this indicator will often themselves be highly politicized. These data are likely to be particularly politicized and unreliable in high-conflict states where (a) governments seek to make groups appear larger or smaller than they are or to ignore some identities (e.g., religious data are not collected in the French census, linguistic data are not collected in the Belgian census); (b) individuals within the groups strategically declare themselves to be members of one group rather than another; and (c) conflict and violence can rapidly change the demographic composition of a population because of migration and deaths, as well as government preferences and individual preferences on group identity. Thus, we should not use ECI as an independent variable to predict the degree of interethnic conflict in a society without thinking about how to address this problem of endogeneity.

EVOTE has other biases and errors. The data reported above are characterized by a selection bias: As we note, they include only those countries that held competitive elections in the 1990s—based on Keefer's index of legislative competitiveness (2005). Autocracies and democracies in which elections were not judged as being highly competitive by the Keefer index are only now being added to this initial data set. But even when expanded over space and time, an important selection bias will remain. EVOTE can be collected only for countries that have elections (a measure of the vote would hardly be sensible for countries that do not have elections). The exclusion of countries that do not hold elections in each period for which we have data will have to be taken into account in an interpretation of association between EVOTE and the likelihood of civil war (and several other dependent variables). Furthermore, there is measurement error in the data that may well be a source of bias. One major source of measurement error comes from the quality of our sources. This error can be a source of bias for some purposes. For instance, we have better information for some regions

(Europe) than we do others (Africa), for larger parties than smaller parties, and for incumbent parties than opposition parties. Suppose now the activation of large numbers of small ethnic categories is less likely to produce violence than the activation of a small number of large categories. Suppose also that small parties are disproportionately likely to be associated with small ethnic categories. In this case, the true effect of EVOTE may be distorted by the systematic undercounting/error associated with small parties.

Several other instances of bias and error are documented in the coding protocol accompanying this variable. But the strength of the new measures that we have proposed here may lie precisely in the transparency of their weaknesses. Our principal critique of previous work has been not that the conclusions generated from that work are wrong but that we do not know what they mean, how we can assess their degree of reliability, and how we can improve on them. The advance in knowledge that we associate with the new data and measures that we have proposed—and several of the more recently developed alternatives—is not that they generate correct or even different conclusions but that we can interpret conclusions generated by them, estimate the level of uncertainty and the degree of bias in those conclusions, and formulate strategies for how to improve on them without having to start from a new foundation. This, ultimately, is what we need to progress in a cumulative way rather than go around in circles.

Notes

1. See, for instance, Easterly, Alesina, and Baqir (1999); Easterly and Levine (1997); Fearon and Laitin (2003); Miguel (2004); Miguel and Gugerty (2005); Neto and Cox (1997); Ordeshook and Shvetsova (1994); Posner (2004).

2. Horowitz (1985), for instance, counts Hindus and Muslims in India, Christians and Muslims in Lebanon, and Creoles and Indians in Guyana and Trinidad as ethnic categories even though they do not possess his primary defining characteristic of a myth of common ancestry. Fearon (2003) counts Hindi speakers in India, Blacks in the United States, and Mestizos in Nicaragua as ethnic groups, even though those classified as Hindi speakers in India do not meet his definitional criterion of having a distinct history as a group or a shared culture valued by the majority of members, even though those classified as Blacks in the United States do not often share the definitional criterion of having a shared culture, and even though many of those classified as Mestizos in Nicaragua do not share the definitional criterion of the descent rule. Chandra's previous work (2004) counted categories based on region as ethnic, even when it was not clear whether these groups met her definition of ethnic groups as ascriptive groups.

3. See Horowitz (1985) for a comparable but not identical list. The modifications of this list (e.g., the inclusion of identities based on dialect) are based on examining his actual classification of identities as ethnic.

4. Although several of the authors whom we discuss here use the word *group*, the entities that they call *groups* are simply categories. We cannot avoid the use of the word *group* here, because so many of those whose work we discuss use it. But we suggest interpreting the word *group* throughout as category because that would be the more accurate understanding of an author's usage.

5. In political science, one of the first books to raise the question of how we move from many ethnic categories to only a few salient political identities was Brass's *Language, Religion and Politics in North India* (1974). In it, Brass highlighted the role of political competition, institutional rules, and intrinsic aspects of the ethnic identities themselves in explaining why out of many thousands of ethnic groups in India, only a few dozen identities had become politically salient.

6. See Fearon (2003) and Posner (2004).

7. In addition to being cited to the aforementioned works, this statement is based on a representative sample of research to review. Using the social science citation index, we surveyed all articles with *ethn* in the title published since 1990 in the following journals: *American Journal of Political Science*, *American Political Science Review*, *Comparative Political Studies*, *Comparative Politics*, *International Organization*, *Journal of Conflict Resolution*, *Journal of Economic Growth*, *Quarterly Journal of Economics*, and *World Politics*.

8. We put to one side for the moment a topic that we return to at the end of this article: the basic reliability of the census statistics from which we typically derive these indicators. For instance, Fearon's total (2003) for Hindi speakers (.39) in constructing his index is presumably higher than the *Atlas Narodov Mira's* (.25; Miklukho-Maklai Ethnological Institute, 1964) because of a decision that the Indian government and census bureau took in the early 1970s to aggregate 48 categorical answers to the question "What language do you speak?" under the single heading of *Hindi* in the published census tables. This includes large language groups such as Bhojpuri (23 million speakers in 1971) and Chattisgarhi (10.6 million) that have generated quite substantial political movements at various times (personal communication to Wilkinson from former Indian census commissioner M. Vijayananni, March 27, 2003). For an excellent introduction to the many state, individual, and socioeconomic incentives that can affect the quality and reliability of census statistics, see Kertzer and Arel (2002).

9. For instance, N^* is a measure that does not require the assumption of exhaustiveness that the index of ethnolinguistic fractionalization does; thus, it can be used in instances where the activated ethnic categories in a polity do not sum to 100%.

10. Ness (1967) uses percentages so that his ethnic concentration index runs from 0 to 200 rather than 0.0 to 2.0, as it is here in the form of ethnic imbalance.

11. Cruise O'Brien (1980) is a rare exception, and her study of Kenya clearly measures the extent of the economic inequalities among that country's ethnic groups, inequalities that were important in driving conflicts in the 1960s and 1970s. She shows that two thirds of the businesses with more than 50 employees in 1961 were under the control of Kenya's 100,000-strong Asian minority, thereby creating a powerful incentive for postindependence leaders to target this advanced community to solidify their support.

12. An example of the partial endogeneity of ethnic identity to group categorizations used by the state in military recruitment comes from research on colonial Nyasaland (Malawi) by Parsons (1999). The use of ethnic criteria as an implicit indicator of recruits' reliability by colonial recruiters encouraged some strategic switching on the part of those who identified themselves as members of Group X to be eligible for recruitment in the colonial army. In Nyasaland, members of the nonmartial Lomwe identified themselves as Yao to enlist in the King's African Rifles. Colonial officers also categorized those who might have claimed several

identities in line with their ethnic stereotypes according to which groups were martial and which not so that “if a Southerner [in Nyasaland] refused to enlist or was a bad soldier, he was a Mpotola; if he did enlist and served with distinction, he was a Nguru or even a Yao” (p. 91).

13. See Table 2.1 in Parsons (2003).

14. These figures have been generated by an analysis of the ethnic composition of each regiment in the unified Indian army that was divided between India and Pakistan in August–September 1947. The basic data on the religious balance of the army are extrapolated from the table titled “Class Composition of Indian Infantry 1947” in Appendix 7 of Tukur (1950), supplemented with an analysis of regimental histories—where these are not clear from the name of the regiment—that establishes the religious and provincial origin of recruits.

15. This criterion is somewhat easier to operationalize than the definition by Horowitz (1985), who codes ethnic parties according to the distribution of their supporters, as well as by whether they serve the interests of an ethnic group or a cluster of groups.

16. The Fearon and Laitin data set used in their 2003 article is available at Fearon’s Web site: <http://www.stanford.edu/~jfearon/>.

17. Our results are slightly different because we do not include (because it was not included in the particular replication dataset we downloaded) the variable “religious fractionalization,” which was however not significant at the .05 level in any of Fearon & Laitin’s models.

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