CONFLICT AND DEVELOPMENT

Joan Esteban

IAE - CSIC

Debraj Ray

New York University

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Abstract

In this review, we examine the links between economic development and social conflict. By eco- nomic development, we refer broadly to aggregate changes in per-capita income and wealth, or in the distribution of that wealth. By social conflict, we refer to within-country unrest, ranging from peaceful demonstrations, processions and strikes to violent riots and civil war. We organize our review by critically examining three common perceptions: that conflict declines with ongo- ing economic growth; that conflict is principally organized along economic differences rather than similarities; and that conflict, most especially in developing countries, is driven by ethnic motives.

"No society is immune from the darkest impulses of
man."
Barack Obama, New Delhi, India 27
January 2015

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INTRODUCTION

In this paper, we examine the links between economic development and social conflict. By *economic development*, we refer broadly to aggregate changes in per-capita income and wealth, or in the distribution of that wealth. By *social conflict*, we refer to within-country unrest, ranging from peaceful demonstrations, processions and strikes to violent riots and civil war. In whatever form it might take, the key feature of social conflict is that it is *organized*: it involves groups and is rooted — in some way or form — in within-group identity and cross-group antagonism.[2]

Our review is organized around the critical examination of three common perceptions: that conflict declines with ongoing economic growth; that conflict is principally organized along economic differences rather than similarities; and that conflict, most especially in developing countries, is driven by ethnic motives. While these perceptions are not necessarily wrong, they are often held too closely for comfort; hence the qualification "critical" in our examination.

Within-country conflicts account for an enormous share of deaths and hardship in the world today. Since World War II there have been 22 inter-state conflicts with more than 25 battle-related deaths per year; 9 of them have killed at least 1000 over the entire history of conflict (Gleditsch et al,

[2]That is not to argue that individual instances of violence, such as (unorganized) homicide, rape or theft are unimportant, and indeed, some of the considerations discussed in this chapter potentially apply to individual violence as well. But social conflict has its own particularities; specifically, its need to appeal to and build on some form of group identity: religion, caste, kin, occupational, or economic class. In short, social conflict lives off both identity and alienation.

Armed Conflict by Type, 1946-2013

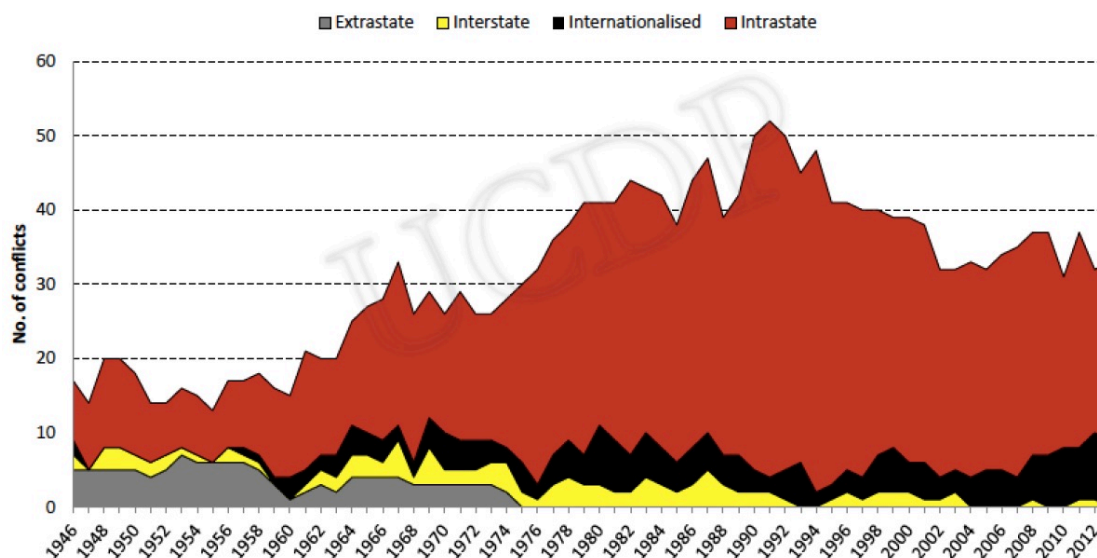■ Extrastate   □ Interstate   ■ Internationalised   ■ Intrastate

Figure 1. ARMED CONFLICTS BY TYPE. Sources: Data based on UCDP/PRIO armed conflict database. Conflicts include cases with at least twenty-five battle deaths in a single year.

2002). The total number of attendant battle deaths in these conflicts is estimated to be around 3 to 8 million (Bethany and Gleditsch, 2005). The very same period witnessed 240 civil conflicts with more than 25 battle-related deaths per year, and almost half of them killed more than 1000 (Gleditsch et al, 2002). Estimates of the total number of battle deaths are in the range of 5 to 10 million (Bethany and Gleditsch, 2005). To the direct count of battle deaths one would do well to add the mass assassination of up to 25 million non-combatant civilians (Political Instability Task Force, http://eventdata.parusanalytics.com/data.dir/atrocities.html) and indirect deaths due to disease and malnutrition which have been estimated to be at least four times as high as violent deaths (Global Burden of Armed Violence, 2008), not to mention the forced displacements of sixty million individuals by 2015 (http://www.unhcr.org/558193896.html).[3] In 2015 there were 29 ongoing conflicts that had killed 100 or more people in 2014, with cumulative deaths for many of them climbing into the tens of thousands. Figure 1 depicts global trends in inter- and intra-state conflict and Figure 2 their distribution over the world regions.

Of course, things were probably worse in the past. For instance, Steven Pinker's book, *The Better Angels of Our Nature*, is a delightfully gruesome romp through the centuries in an effort to show that violence of all forms has been on the decline. And he is undoubtedly correct: compared to utter mayhem that prevailed in the Middle Ages and certainly earlier, we are surely constrained — at least relatively speaking — by mutual tolerance, the institutionalized respect for cultures and religions, and by the increased economic interactions within and across societies. To this one must add the growth of States that seek to foster those interactions for the benefits of their citizens, and that internalize the understanding that violence — especially across symmetric participants —- ultimately leads nowhere.

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[3]Such displacement also have a high cost in lives due to endemic sicknesses the newly settled population is not immune to (see Montalvo and Reynal-Querol (2007) and Cervellati and Sunde (2005)).

Armed Conflict by Region, 1946-2013

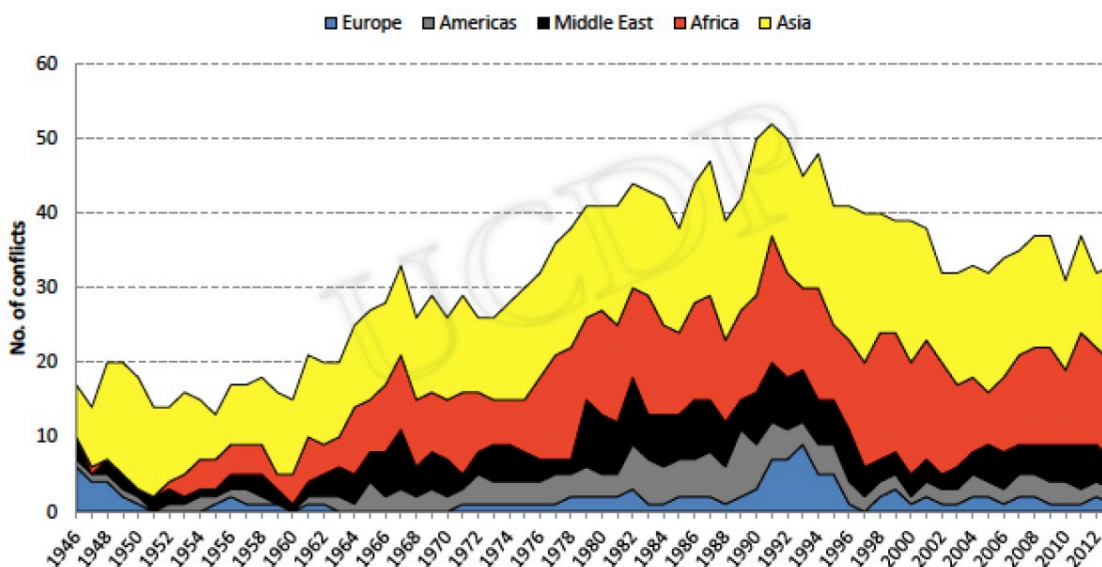■ Europe ■ Americas ■ Middle East ■ Africa □ Asia

Figure 2. ARMED CONFLICTS BY REGION. Sources: Data based on UCDP/PRIO armed conflict database. Conflicts include cases with at least twenty-five battle deaths in a single year.

And yet, it isn't hard to understand why this sort of long-run celebration seemingly flies in the face of the facts. We appear to live in an incredibly violent world. Not a day appears to go by when we do not hear of some new atrocity: individuals beheaded, planes shot from the sky, suicide bombings of all descriptions, mass killings, and calls to even more escalated violence. True: perspective is important: we did not live a century ago, nor in the Middle Ages, nor in the early days of Christendom. Nor did those eras have access to the internet where each act of savagery could be played on YouTube, or by media outlets specializing in breaking news. With the calm afforded by a longer, historical view, a perspective that Pinker correctly brings to the table, we can place our tumultuous present into context.

What today's violence does show, however, that there are limits to peace and civility as long as there are enormous perceived inequities in the world, and as we shall try and argue in this review, high on that list of perceived inequities are economic considerations. Even the most horrific conflicts, ones that seem entirely motivated by religious or ethnic intolerance or hatred, have that undercurrent of economic gain or loss that flows along with the violence, sometimes obscured by the more gruesome aspects of that violence, but never entirely absent. From the great religious struggles of the past to the civil wars and ethnic conflicts we see today, we can see — if we look hard enough — a battle for resources or economic gain: oil, land, business opportunities, or political power (and political power is, in the end, a question of control over economic resources).

This sort of economic determinism is unnecessarily narrow to some sensibilities, and perhaps it is. Perhaps conflict, in the end, is a "clash of civilizations" (Huntington, 1993), an outcome of simple ethnic hatred, or the unfortunate corollary of a religious or ideological dogma. Perhaps, but that sort of reasoning is incomplete. Is anti-Semitism a fundamental construct, or is racism just a primitive abhorrence of the Other, or is the caste system born from some primeval, intrinsic desire

to segregate human beings? In all of these queries, there is a grain of truth: anti-Semitism or racism or ethnic hatred is deeply ingrained in many people, perhaps by upbringing or social conditioning. Often, we can get quite far by simply using these as working explanations to predict the impact of a particular policy or change (and we will do so below). But stopping there prevents us from seeing a deeper common thread, that by creating and fostering such attitudes, there are gains to be had, and often those gains are economic. By following the economic trail, by asking *cui bono?*, we can get further insights into the origins of prejudice and violence that will — at the very least — supplement any non-economic understanding of conflict.

This contribution therefore asks the following questions:

1. How is economic prosperity (or its absence) related to conflict? What is the connection between economic development and conflict? Does economic growth dampen violence, or provoke it?

2. Is the main form of economic violence between the haves and the have-nots? Is conflict born of economic similarity or difference?

3. Is there evidence for the hypothesis that "ethnic divisions" — broadly defined to include race, linguistic divisions, and religious difference — are a potential driver of conflict? And if so, does this rule out economic motives as a central correlate of conflict?

1. Three Common Perceptions About Conflict

We organize the themes of this review around three common perceptions.

Perception 1: Conflict Declines with Per-Capita Income. Perhaps the most important finding of this literature is that per-capita income is systematically and *negatively* correlated with civil war, whether one studies "incidence" or "onset". This is a result that appears and reappears in the literature, especially in the large-scale cross-country studies of conflict; see, e.g., Collier and Hoeffler (1998, 2004a,b), Fearon and Laitin (2003a) or Hegre and Sambanis (2006).

Yet, even this seemingly robust finding is fraught with difficulties of interpretation. While there is no doubting the correlation between these two variables, there is also little doubt that countries with a history of active conflict are likely to be poor, or that there are several other omitted variables, such as the propping up of a dictatorship by international intervention or support, that lead to both conflict and poverty. There are also issues of conceptual interpretation that we will discuss in Section 2.

The argument we make is that economic development is intrinsically uneven. That tranquil paradigm on which generations of economists have been nurtured — balanced growth — must be replaced by one in which progress occurs in fits and starts: via processes in which one sector, then another, takes off, to be followed by the remaining sectors in a never-ending game of catchup. Thus it is often the case that overall growth is made up of two kinds of changes: one that creates a larger pot to fight over — and therefore raises conflict — and another that raises the opportunity cost to fighting, and so lowers it. Whether conflict is positively or negatively related to growth will therefore depend on the *type* of growth; specifically, how uneven it is across sectors or groups. Cross-country studies are too blunt to pick these effects up in any detail.

Perception 2: Conflict is Created by Economic Difference, Rather than Similarity. The great revolutions of the twentieth century were born of economic difference, of the realization that a relatively small elite reaped most of the rewards while a large, struggling proletariat suffered under a disproportionately small share of the pie. The traditional literature on crisis and revolution, in which the contributions of Karl Marx are central, focuses near-exclusively on class conflicts. More recently, the work of Piketty (2014) and co-authors has documented the rise of economic inequality in the second half of the twentieth century. Movements such as *Occupy* have re-highlighted the awareness of economic differences, and the connections between those differences and social unrest.

And yet, there are eerie lines along which conflict occurs across economically *similar*, rather than different groups. That conflict is over resources that are explicitly and directly contested: a limited pool of jobs (e.g., natives versus immigrants), the same customers (business rivalries across organized groups), or scarce land. Because the conflict is over the direct use of a resource, the groups are often remarkably similar in their economic characteristics, although there are exceptions.[4] The gains from conflict are immediate: the losing group can be excluded from the sector in which it directly competes with the winners.

This is the second theme of our article. It leads naturally to the view that ethnicity is possibly a marker for organizing similar individuals along opposing lines, and that takes us to our third and final topic.

Perception 3: Conflicts in Developing Countries Are Based on Ethnic Differences. Conflicts in post-colonial developing countries, while certainly not immune to the gravitational pull of class, have often been organized along *ethnic* lines. Specifically, many conflicts appear to be largely ethnic, geographical, and religious in nature, while outright economic class struggle is relatively rare. Indeed, as noted by Fearon (2006), 100 of 700 known ethnic groups participated in rebellions over 1945–1998. Observations such as these led Horowitz (1985), a leading researcher in the area of conflict, to remark that "in much of Asia and Africa, it is only modest hyperbole to assert that the Marxian prophecy has had an ethnic fulfillment."

This perception is the subtlest of all to analyze. The facts, as laid down by Horowitz and others, are certainly correct. But there are two puzzles to confront. First, if conflicts are ethnic, then "ethnic divisions" must somehow bear a strong statistical relationship to conflict. It turns out that the answer to this question is somewhat involved, and in part rests fundamentally on a proper conceptualization of what "ethnic divisions" entail. Second, if such a result were to indeed be true, how does one interpret it? One approach is based on the primordialist position that at the heart of all conflicts is intrinsic hatred; that conflict is a Huntingtonian "clash of civilizations." A second approach is instrumentalist: that noneconomic divisions can be and frequently are used to obtain economic or political gains by violent means, often through exclusion.

And this takes us back to Perception 2. Nothing dictates that the groups must be *economically* distinct. Indeed, we've argued the contrary. If two groups are very similar economically, it is more likely that they will intrude on each other's turf: the motives for exclusion and resource-grabbing — and therefore for violence — may be even higher. In such situations, organized violence will *necessitate* the instrumental use of markers based on kin, religion, geography and other possibly

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[4]For instance, the land acquisition debates in India feature very different groups, because buyers and (potential) sellers see the land as being put to very different uses.

Variable	[1] CIVIL WAR	[2] ETHNIC WAR	[3] CIVIL WAR	[4] CIVIL WAR (COW)
Prior war	**-0.954 (0.314)	*-0.849 (0.388)	**-0.916 (0.312)	-0.551 (0.374)
PC Income	***-0.344 (0.072)	***-0.379 (0.100)	***-0.318 (0.071)	***-0.309 (0.079)
log(Population)	***0.263 (0.073)	***0.389 (0.110)	***0.272 (0.074)	**0.223 (0.079)
log(% mountain)	**0.219 (0.085)	0.120 (0.106)	*0.199 (0.085)	***0.418 (0.103)
Noncontiguous state	0.443 (0.274)	0.481 (0.398)	0.426 (0.272)	-0.171 (0.328)
Oil Exporter	**0.858 (0.279)	*0.809 (0.352)	**0.751 (0.278)	***1.269 (0.297)
New State	***1.709 (0.339)	***1.777 (0.415)	***1.658 (0.342)	**1.147 (0.413)
Instability	**0.618 (0.235)	0.385 (0.316)	*0.513 (0.242)	*0.584 (0.268)
Democracy [Polity IV]	0.021 (0.017)	0.013 (0.022)		
Ethnic Fractionalisation	0.166 (0.373)	0.146 (0.584)	0.164 (0.368)	-0.119 (0.396)
Religious Fractionalisation	0.285 (0.509)	*1.533 (0.724)	0.326 (0.506)	*1.176 (0.563)
Anocracy			*0.521 (0.237)	*0.597 (0.261)
Democracy [Dychotomous]			0.127 (0.304)	0.219 (0.354)
Constant	***-6.731 (0.736)	***-8.450 (1.092)	***-7.019 (0.751)	***-7.503 (0.854)
Observations	6327	5186	6327	5378

Table 1. LOGIT ANLYSES OF DETERMINANTS OF CIVIL WAR ONSET, 1945-1999. Source: Fearon and Laitin (2003a), Table 1.

Note. The dependent variable is coded "1" for country years in which a civil war began and "0" in all others. Standard errors are in parentheses. Per capita income, population, instability, democracy, and anocracy are lagged one year. Income and population are in thousands.

observable differences; on ethnicity, in a word. In short, there is no contradiction between the use of noneconomic markers in conflict and the view that conflict may be driven by economic forces.

2. ECONOMIC DEVELOPMENT AND CONFLICT

Systematic empirical studies of conflict begin with the work of Collier and Hoeffler (1998, 2004) and Fearon and Laitin (2003a). These are cross-section studies, (presumably) aimed at establishing the correlates of civil war, though causal interpretations have all too readily been advanced. Perhaps the most important finding from this literature is that conflict is negatively related to per capita income. In this section we discuss alternative interpretations of this finding, but we also critically examine the finding itself.

2.1. **The Empirical Finding.** Collier and Hoeffler (1998, 2004) and Fearon and Laitin (2003a) observe that per-capita income and conflict are significantly and negatively correlated. Table 1 reproduces the central table in Fearon and Laitin (2003a). They study the onset of "civil war," a conflict that they define by (1) "fighting between agents of (or claimants to) a state and organized, nonstate groups," (2) a yearly average of at least 100 deaths, with a cumulative total of at least 1000 deaths, and (to rule out genocides or one-sided massacres) (3) at least 100 deaths on both sides. These criteria are similar to, though not identical with other criteria used in the literature, which principally vary in the size of the thresholds and generally lack criterion 3.

They conclude that "Per capita income . . . is strongly significant in both a statistical and a substantive sense: $1,000 less in per capita income is associated with 41% greater annual odds of civil war onset, on average . . . The income variable is not just a proxy for 'the West,' whose states might have low rates of civil war for reasons of culture or history that have little to do with income. The estimated coefficient . . . remains strongly significant."

One can discuss this finding at a number of levels, and we shall do so now.

2.1.1. *The Definition of Conflict.* We get an obvious preliminary consideration out of the way. There are conflicts, and there are conflicts. Whether threshold-like criteria involving substantial numbers of deaths are adequate depends on the type of question the analyst has in mind. Many types of organized unrest can lead to relatively low levels of deadly violence: demonstrations, strikes, coups, the detaining of political prisoners or even the growth of organized crime come to mind. Their costs might even exceed the costs imputed to civil wars. Indeed, one might argue that this type of social unrest corresponds more clearly to the Marxian notion of "class struggle,"[5] rather than a recurring state of armed civil war. The problem, of course, is that we do not have comprehensive data of this sort.

When violence *is* involved, it could have potent and long-lasting consequences for social tension, and yet have low numbers of fatalities attached to them. Think of the IRA movement in the UK, the Red Army Faction in West Germany in the late seventies, the Black Panther movement in the US, the permanent turmoil situation in Italy with either real or fabricated "extreme left" terrorist actions, the military coups in Greece and Turkey, the failed coups in France in 1958 and in Spain in 1981, as well as the ETA movement (again in Spain) since the early 1970s. One could add the many revolutionary movements and bloody military coups in Latin America, in countries with per-capita income well above that of many Asian or African countries. How can it be that this does not sufficiently show up in the empirical results? Is this because the number of deaths did not go beyond some arbitrary threshold of 50 or 100 yearly casualties?

More generally, we cannot discard the possibility that the empirical results capture more the explicit outbreaks of civil war, while in reality there could be active sources of discontent that don't always come to fruition in the form of multiple deaths and overt conflict. That is, the reasons for conflict could well be active at all economic levels, but poverty allows that conflict to fully express itself. A hypothesis compatible with this alternative interpretation is that richer countries have better state capacity to contain insurgencies than poor countries, a line of reasoning to which we return below.

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[5]However, note that the Marxian view is that conflict is precipitated by the development of the "productive forces" while what we observe is that higher GDP reduces the likelihood of conflict.

We do not wish to dwell excessively on this specific issue. There is not much more that can be done with the data we currently have. Our only point is that developed countries may have relatively more of the "quieter conflicts," leading to a bias in the observed correlation between per-capita income and conflict.

2.1.2. *Endogeneity.* The negative relationship between per-capita income and conflict must obviously be interpreted with a great deal of caution, rife as it is with endogeneity. Ongoing conflict will destroy productive capacity, leading to lower per-capita income. For instance, Hess (2003) estimates to cost of all civil wars to be 8% of world's GDP, and de Groot (2009) finds that global GDP in 2007 would have been 14.3% higher if there had not been any conflict since 1960.[6]

There are also important omitted variables to be contended with. Both low per-capita income and conflict could be the joint outcome of weak political institutions, as already mentioned. Djankov and Reynal-Querol (2010) argue that country-specific historical factors are highly significant in explaining both conflict and weak institutions and that they render non-significant the role of low per capita income. Besley and Reynal-Querol (2014) find that local conflicts over the past few centuries are highly significant in explaining today's civil wars, as well as today's development outcomes. Ashraf and Galor (2013) and Arbath, Ashraf and Galor (2015) argue that genetic diversity explains both the level of development and social conflict.

A good instrument for per-capita income would alleviate some of these concerns. Rainfall is potentially such an instrument, and this connection is exploited in Miguel, Satyanath and Sergenti (2004). Their analysis must rely, however, on regions in which rainfall significantly affects output, which explains their focus on sub-Saharan Africa. Specifically, a large fraction of output is agricultural, and irrigation is far from being widespread. Indeed, a first stage regression of income growth on weather shock works very well for sub-Saharan Africa. Yet this strategy is obviously limited. Rainfall shocks do not work well outside the sub-Saharan sample, and indeed, even over more recent time periods for sub-Saharan Africa.

Miguel, Satyanath and Sergenti (2004) work with a conflict database developed by the Peace Research Institute of Oslo (PRIO) in conjunction with the University of Uppsala. (We will return to this database below.) The specification they use is somewhat different from that employed by Collier-Hoeffler-Fearon-Laitin: they relate the incidence of civil conflict in sub-Saharan Africa (over 1981–1999) to the *growth rate* of per-capita GDP (and not its level). The relationship they uncover is strong: "a five-percentage-point drop in annual economic growth increases the likelihood of a civil conflict (at least 25 deaths per year) in the following year by over 12 percentage points, which amounts to an increase of more than one-half in the likelihood of civil war."

Table 2 reproduces the main results from this paper. Of particular interest are columns 5–7, which report the IV specifications and show how growth has a negative association with conflict. It is also noteworthy that the *level* of per-capita income plays no role, once growth rates are brought into the

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[6]Collier and Hoeffler (2004a,b) estimate the typical cost of a civil war to be around $50 billion and argue that this reduces the future growth rate by 2% points. The recent computations by Gates et al (2012) indicate that a medium-sized conflict with 2500 battle deaths increases undernutrition by an additional 3.3%, reduces life expectancy by about 1 year, increases infant mortality by 10%, and deprives an additional 1.8% of the population from access to potable water. Undoubtedly, that in turn affects per-capita income.On a rigorous methodology for computing the costs of conflict see Abadie and Gardeazabal (2010) . For an overview of the different quantitative cost estimates see de Groot (2009) and Lindgren (2004).

Variable	[1] PROBIT	[2] OLS	[3] OLS	[4] OLS	[5] IV-2SLS	[6] IV-2SLS
Economic growth t	-0.370 (0.260)	-0.330 (0.260)	-0.210 (0.200)	-0.210 (0.160)	-0.410 (1.480)	-1.130 (1.400)
Economic growth $t-1$	-0.140 (0.230)	-0.080 (0.240)	0.010 (0.200)	0.070 (0.160)	**-2.250 (1.070)	**-2.550 (1.100)
log(GDPpc 1979)	-0.067 (0.061)	-0.041 (0.050)	0.085 (0.084)		0.053 (0.098)	
Democracy [Polity IV $t-1$]	0.001 (0.005)	0.001 (0.005)	0.003 (0.006)		0.004 (0.006)	
Ethnolinguistic Frac	0.240 (0.260)	0.230 (0.270)	0.510 (0.400)		0.510 (0.390)	
Religious Frac	-0.290 (0.260)	-0.240 (0.240)	0.100 (0.420)		0.220 (0.440)	
Oil Exporter	0.020 (0.210)	0.050 (0.210)	-0.160 (0.200)		-0.100 (0.220)	
Log Mountainous	**0.077 (0.041)	*0.076 (0.039)	0.057 (0.060)		0.060 (0.058)	
Log Pupulation $t-1$	0.080 (0.051)	0.068 (0.051)	*0.182 (0.086)		*0.159 (0.093)	
R^2	-	0.13	0.53	0.71	-	-
Root mean square error	-	0.42	0.31	0.25	0.36	0.32
Observations	743	743	743	743	743	743

Table 2. ECONOMIC GROWTH AND CIVIL CONFLICT IN SUB-SAHARAN AFRICA. Source: Miguel, Satyanath and Sergenti (2004), Table 4.

Note. Dependent variable: Civil conflict ≥ 25 deaths. Huber robust standard error are in parentheses. Regression disturbance terms are clustered at the country level. Regression 1 presents marginal profit effects, evaluated at explanatory variable mean values. The instrumental variables for economic growth in regressions 5-7 are growth in rainfall, t and growth in rainfall, $t-1$. A country-specific year time trend is included in all specifications (coefficient estimates not reported), except for regretssions 1 and 2 where single linear time trend is included. Country fixed effects have been included in columns [4] and [6] and country-specific time trends in columns [3]-[6]

picture. This is not to say that the previous cross-sectional correlations are necessarily suspect, but that the exact nature of the relationship between income and conflict — questions of correlation and causation aside — is far from cast in stone. We will return to this issue below once we discuss matters of interpretation.

2.2. **Questions of Interpretation.**
If we tentatively buy the causal link from low income (or negative shocks to income) and conflict, there are two main interpretations to consider:

(a) *Opportunity cost*. Individuals allocate their time between productive work and conflictual activity to obtain resources. When the society is poor, the opportunity cost of engaging in conflict is lower.

(b) *Weak institutions*. States in poor societies are ill-equipped to handle the demands and pressures of conflicting groups and succumb more easily to open conflict.

The first of these interpretations is favored by Collier and Hoeffler (1998, 2004), the second by Fearon and Laitin (2003).

2.2.1. *Opportunity Cost.* The opportunity cost argument, going back to Becker (1968) and Ehrlich (1973), and echoed in Skaperdas (1992), Hirshleifer (1995), Grossman and Kim (1995), Dal Bó and Dal Bó (2011) and Miguel, Satyanath and Sergenti (2004),[7] emphasizes the fact that conflict and production are often alternative choices. In poorer societies, engaging in the alternative of productive labor has a low payoff. So there could be a greater incentive to participate in conflict.

The opportunity cost argument is *prima facie* reasonable, and we return to it below in a more nuanced way. But it is obviously inadequate as an explanation for the income-conflict correlation. True, the opportunity cost of conflict is lower, but so presumably are the gains from conflict in a poorer society: there is less to seize. The argument must connect the opportunity costs of conflict *relative* to the potential gains from conflict. But the movement of per-capita income up or down does not immediately affect this relative magnitude in any particular way. So even if considerations of opportunity cost are appropriate — and we believe they are — once nested into the context at hand, the explanation leaves something to be desired.

We note in passing that it is easier to buy the opportunity cost argument in the case of short-term income *shocks*, which is the leading case in Miguel, Satyanath and Sergenti (2004). For instance, if potential conflict is over oil resources held by a State, then a sudden change in, say, agricultural employment opportunities may well lead to more of it.

Conceptually, then, a change in economic fortunes can have two effects. One changes the cost of seizing a prize, the other affects the prize itself. It is this schizophrenic nature of economic change that generates really interesting predictions about conflict and development, but those predictions will need to be examined under a finer lens, and not through considerations of aggregate income alone. We return to this question below.

2.2.2. *Weak Institutions.* A second explanation for the prevalence of social conflict in poorer countries is one favored by Fearon and Laitin (2003a): the State is too weak, either to adequately solve the competing claims of different groups, or to effectively prevent conflict when it does break out. Their empirical findings, while similar to those of Collier and Hoeffler, are interpreted thus: "[T]he civil wars of the period have structural roots, in the combination of a simple, robust military technology and decolonization, which created an international system numerically dominated by fragile states with limited administrative control of their peripheries . . . [O]ur analysis suggests that while economic growth may correlate with fewer civil wars, the causal mechanism is more likely a well-financed and administratively competent government."

Just as in the case of the opportunity cost argument, the effect of a weak state on the likelihood of conflict must balance two forces in opposite directions. Weak states are easier to confront, true, but the payoff from victory is equally modest, if for no reason than the fact that victory can in turn be challenged (Mehlum and Moene, 2015). On the other hand, *all* prizes do not naturally scale up and down with per-capita income and state weakness. For instance, the discovery of natural resources, by suddenly increasing the rent controlled by a weak state, can become a destabilizing

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[7]For instance, Hirshleifer (1995) writes: "[R]ational behavior in a conflict interaction . . . is for the poorer side to specialize more in fighting, the richer side more in production."

factor, a "curse". Likewise, if there is intrinsic value (over and beyond economics) attached by a group to religious, cultural or political dominance, weak states can contribute to conflict.

So "state capacity" certainly matters. As defined by Skocpol (1985), this refers to the ability of a government to administer its territory effectively through four basic state capacities: the capacity to mobilize financial resources (extractive capacity); the capacity to guide national socioeconomic development (steering capacity); the capacity to dominate by using symbols and creating consensus (legitimation capacity); and the capacity to dominate by the use or threat of force (coercive capacity). Snyder (1990), who like Fearon and Laiton (2003a) links state capacity (or the lack thereof) to the likelihood of violent conflict, proposed to measure such capacity by the share of the government budget in aggregate GDP. This measure is now standard in the literature, and indeed, there are dramatic differences in this measure across rich and poor countries. Germany, France, or the UK have a budget/GDP ratio more than twice that of many African countries.

While we have already touched on issues of endogeneity, it bears reiteration that state capacity and conflict can jointly evolve in a self-reinforcing manner. For instance, countries that have undergone civil war experience a loss in capacity (see, e.g., Chowdhury and Mansoob, 2013), which makes the government less able to manage public affairs, to effectively confront future uprisings, or to generate growth. The recent contributions by Besley and Persson (2008, 2009, 2010, and 2011) and Mc Bride et al (2011) have not only popularized among economists the notion of "state capacity," but have developed a more nuanced theoretical basis for thinking about the intertwined connections between capacity and conflict.

2.3. **Development and Conflict Reconsidered.** So far, we have been somewhat skeptical about the observed cross-sectional relationship between per-capita income and social conflict. At the same time, we believe that the core conceptual arguments — based on opportunity cost or weak state capacity — have great merit and are capable of extension to more nuanced contexts. Such extensions may not yield a straightforward connection between development and conflict, but that does not make the exercise any less useful. We illustrate this now with the opportunity cost argument applied to societies that experience uneven growth.

Harmonious, orderly models of balanced growth notwithstanding, economic development is fundamentally an uneven process. Ongoing structural change, rapid technical progress and globalization all lead to situations in which economic growth is not uniform across the entire economy. Sometimes that growth can spur conflict, if the gains are viewed as loot to be seized. Or it can lower conflict by increasing the opportunity costs of engaging in unproductive, violent activity. Both outcomes are possible, in principle.

Dal Bó and Dal Bó (2011) formalize this idea in the context of a simple general equilibrium model. They consider an economy with several sectors: the productive sectors differ (as in the Heckscher-Ohlin framework) in the capital intensity of production, and there is in addition a sector that generates unproductive "appropriation" or conflict, with its participants essentially preying on the output of the productive sectors. Individuals freely sort themselves into the sectors; the equilibrium size of the "appropriation sector" is used as a measure of overall conflict.

Consider such an equilibrium, and suppose that the capital-intensive sector receives a positive shock. Then wealth increases all around, but because the sector that benefits is relatively capital-intensive, the *relative* prices move against labor. The resulting lowering of wages (relative to

other prices) permits the opportunity cost argument to come into its own: more labor flows into the appropriation sector, and conflict rises. (It can even be shown by example that the increase in conflict might overpower the positive shock that generated it in the first place, resulting in a negative outcome in the net.) Conversely, positive shocks to the labor-intensive sector (or policies that subsidize employment) will raise relative wages, implying this time that conflict declines. As for the net effect when the economy grows overall: who knows? It would depend on whether that growth is balanced or not, and if not, on the technological profile of the sectors that benefit from growth.

The Miguel, Satyanath and Sergenti (2004) finding fits well within this framework. A weather shock impinges on agriculture, which is labor intensive. Thus conflict is expected to rise with adverse shocks. This argument, while in no way negating the finding itself, calls into question the conceptual validity of the instrument as one which affects "overall growth." With a disaggregated view in mind, weather shocks can be seen as affecting particular segments of that economy — the labor-intensive agricultural sector, to be precise. Whether there is an overall negative causal relationship running from per-capita income to conflict is not, therefore, established by this particular choice of instrument.

Dube and Vargas (2013) explicitly cast their empirical study within the Dal Bó-Dal Bó setting. They study how internal conflicts in Colombia are affected by the movements of world prices for two commodities that are particularly pertinent to that country: oil and coffee. (Colombia is a major exporter of both products.) For each of these commodities, they interact its international price with the amount of that good produced in each municipality. When coffee prices rise, conflict falls more in coffee-producing municipalities. In sharp contrast, when oil prices rise, conflict *increases* in oil-producing municipalities. These observation are in line with the model described above. Coffee production is a relatively labor-intensive activity, so that a rise in coffee prices is likely to lead to an increase in wages relative to the overall price index. The opportunity cost argument then kicks in, reducing conflict. On the other hand, oil extraction and processing is capital-intensive, so that the opportunity cost argument runs in the opposite direction, with positive shocks generating conflict.

As it so happens, coffee prices fall by 68% over 1997–2003, while oil prices rise by 137% over 1998–2005. The Dube-Vargas estimates suggest that the former led to 18% more guerrilla attacks and 31% more paramilitary attacks in the average coffee municipality, relative to non-coffee municipalities. There is also evidence for the channel explored by Dal Bó and Dal Bó: wages and hours of work fall to a greater extent in the average coffee municipality. In contrast, the rise in oil prices appears to induce an additional increase of 14% in paramilitary attacks in the average oil municipality. Again, there is evidence of the channel: oil municipality tax revenue increases differentially, and so does the kidnapping of politicians and leaders.

In summary, theories of uneven growth demand that we keep track of the opportunity cost of engaging in conflict *relative* to the expected payoff from it. It may well be that the latter rises while the former increases less so, thereby making rebellion a more likely outcome.

3. Conflict Drivers: Difference or Similarity

Karl Marx justifiably stands at the apex of all studies on within-country conflict, and research on the subject has been dominated by the Marxist view that *class* is the only relevant social cleavage

and *class conflict* the fundamental source of social unrest. For Marx social conflict would pave the road to the ultimate downfall of capitalism, with workers seeing control of the means of production from the capitalists. So the struggle across economic classes has been viewed as focal, and often correctly so. Quite apart from the great revolutions of the early and mid-20th century, "class consciousness" continues in some shape or form to the present day: witness, for instance, the explicit awareness of and discontent over high inequality that followed on the heels of the financial crisis of 2008.[8] Class conflict, or the fear of it, is also at the heart of all taxation systems, which invariably keep an eye open for some degree of progressivity. The recent contribution by Piketty (2014) has played an important role in putting on the table the remarkable increase in income inequality in all the OECD countries. Is such an intuitive link between inequality and conflict backed by the data?

3.1. **Empirical Evidence on Social Conflict and Inequality.** On the whole, though, the relationship between inequality and social conflict appears to be far more nuanced, and in the stark form posited by Marx, tenuous at best. Researchers, mostly in political science, have tried for decades to find a convincing empirical connection; see, e.g., Nagel (1974), Midlarski (1988) and and Muller, Seligson and Fu (1989). Lichbach (1989) mentions forty three papers on the subject, according to him "some best forgotten." He concludes that the overall evidence obtained by all these works is thoroughly mixed. Some studies support each possible relationship between inequality and conflict, and others show no relationship at all. A recurrent observation is that under several measures of inequality including the Gini, conflict appears to be low *both* for low and for high values of inequality. Midlarsky (1988) remarks on the "fairly typical finding of a weak, barely significant relationship between inequality and political violence . . . rarely is there a robust relationship between the two variables."

While in the previous section we were critically questioning the validity and interpretation of an empirically robust correlation between income and conflict, here we have to confront the *lack* of confirmatory empirical evidence on the inequality-conflict nexus.

3.2. **Why We Don't Find a Clear Link Between Inequality and Conflict.** In the Introduction to his book, *On Economic Inequality*, Amartya Sen (1973) asserts that "the relation between inequality and rebellion is indeed a close one." Why, then, can we not see this relationship in the data? We are going to discuss a number of reasons for this failure.

In the first place, all recent empirical exercises have tried to link income inequality with civil war, with the same conceptual problems of defining conflict that we have already described above. Indeed, it is plausible that the "class struggle" envisioned by Marx takes the dominant form of social unrest — strikes, demonstrations and the like — rather than an armed civil war. Therefore, empirical work on this nexus should pay special attention to indicators of "lower voltage" social unrest.

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[8]Inequality made it to the headlines of articles in the popular press; e,g., *Time Magazine*, 1 April 2014, "There is a Class War Going On and the Poor are Getting their Butts Kicked," or *The Observer* 30 November 2014, "Class War is Back Again and British Politicians are Running Scared." *Cartier* boss Johann Rupert declared he could not sleep because of the fear that "rising inequality will spark class war," CNN/Money, June 2015. Earlier, in 2006, the bosses were a bit more bullish: "there is class warfare, all right, but it is my class, the rich class, that is making war, and we are winning" (Warren Buffett in *The New York Times*, 26 November 2006).

Secondly, all the contributions we are aware of lack a well-defined model that informs and shapes the empirical test. Maybe the Gini index is not suited to adequately capture social tensions and the notion of polarization (Esteban and Ray, 1994, Wolfson, 1994) should be employed instead. We might also be missing very relevant interactions that a model would help us to identify. We think that adequately modeling potential social conflict triggered by income differences is a priority research area. In Section 4, we do just this for ethnic conflict.

Our third point, somewhat related to the second, is that class conflict is often latent and inadequately expressed because, in a word, the rich have the means but not the motive, while the poor have the motive but lack the means. The experience of grassroots movements such as *Occupy* show how difficult it is to sustain a conflict on the basis of energy, enthusiasm and anger alone. Where class conflict has emerged into the open, it has been dependent on sustained financing as well as labor. Money and finance are synergistic in conflict. This is a line of argument that Esteban and Ray (2008, 2011) employ to explain the salience of non-class conflict, perhaps along religious or ethnic lines.[9]

Finally, we could challenge the Marxian position head-on. There are reasons to believe that economic similarity may be just as conflictual as economic inequality, and what is more, a fight between two economically similar groups is easier to sustain. This is the topic we turn to next.

3.3. **Social Conflict and Similarity.** Even if we could obtain empirical support for the argument that income inequality can generate social conflict, it is also undeniable that a situation of economic *similarity* can be conflictual in a direct way that no class confrontation can emulate. When employment, or land, or business resources are scarce, like is often pitted against like, invariably to the great disappointment of conventional Marxists. The self-described socialist Presidential candidate, Bernie Sanders, recently stated in an interview[10] that open borders posed a threat:

"Bring in all kinds of people, work for $2 or $3 an hour, that would be great for them. I don't believe in that . . . You know what youth unemployment is in the United States of America today? . . . You think we should open the borders and bring in a lot of low-wage workers, or do you think maybe we should try to get jobs for those kids?"

While the immigrant-native schism is the best known example of conflict caused by economic similarity, it is by no means the only one. For instance, in developing countries, and at the heart of all ostensibly ethnic or religious conflicts, the land-grab often plays a central role. A leading example is the Rwandan conflict, where economic desperation was clearly seen to play a major role in what appeared to be unreasoning ethnic hatred:

"[E]conomic desperation, blighting individuals' presents and their perceived futures, was a contributor to the willingness of many thousands of poor famers and urban dwellers (a) to fear the possibility of a Tutsi land-and-jobs grab under a victorious RPF regime, (b) to be tempted by more specific hopes for land and jobs, or, more crudely still, to participate in order to grab a share of the victims' property." (Austin, 1996, p.10, quoted in Andre and Plateau 1998).

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[9]There is also a literature that argues, both theoretically and empirically, that more unequal countries appear to carry out less redistributive policies, when a standard median voter argument would have perhaps suggested the opposite. See, e.g., Perotti (1996) and Benabou (2003). The main argument here is that the poor may be less active politically.

[10]See `http://www.vox.com/2015/7/28/9014491/bernie-sanders-vox-conversation`, *Vox*, July 16, 2015.

(a) Conflict and Muslim Expenditure

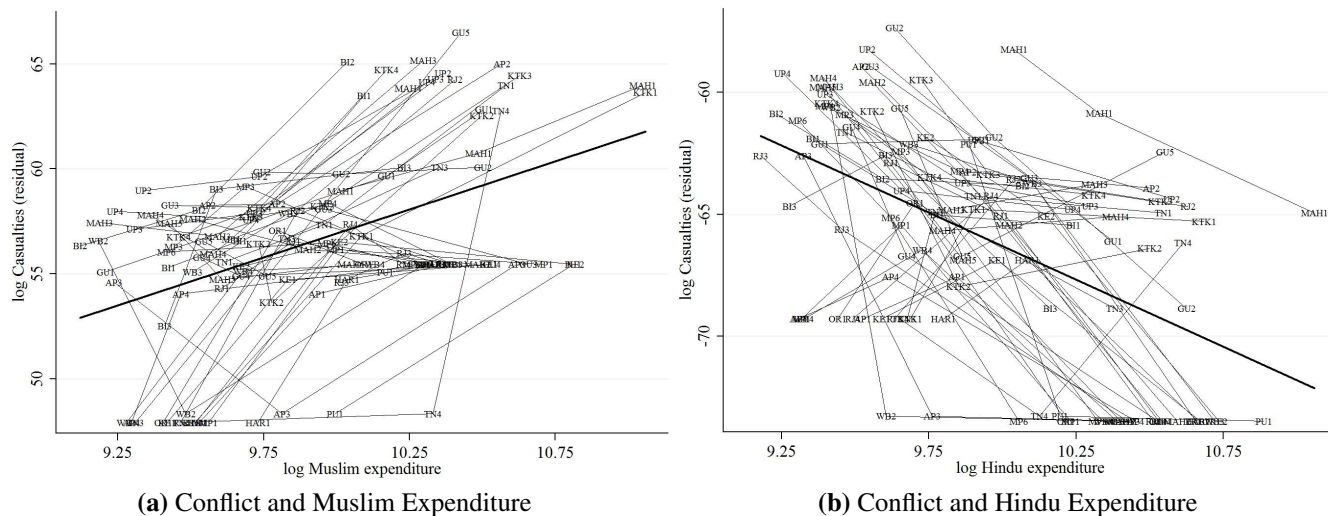**(b)** Conflict and Hindu Expenditure

Figure 3. CONFLICT AND PER-CAPITA EXPENDITURE. Each panel plots the residual of casualties after region and time effects have been removed, in the 5-year period following expenditures. Each line segment connects three data points for a region. Source: Mitra and Ray (2014, Figure 4).

Garth Austin's observations for Rwanda find supportive echoes in the studies of Prunier (1996), Andre and Platteau (1998), and many others, and in many contexts: Rwanda is far from being the only example of land conflicts disguised as ethnic hatred. Finally, labor and land do not exhaust the similarity interface: there are also business interests. For instance, ostensibly religious conflicts in India are laden with sinister economic undertones; witness, for instance, the systematic decimation of rival businesses during the anti-Sikh pogroms of 1984. Likewise, Hindu-Muslim conflicts are inextricably linked with economic motives. As Asgar Ali Engineer (1987) wrote of one of these episodes (in Meerut, India):

"If [religious zeal] is coupled with economic prosperity, as has happened in Meerut, it has a multiplying effect on the Hindu psyche. The ferocity with which business establishments have been destroyed in Meerut bears testimony to this observation. Entire rows of shops belonging to Muslims . . . were reduced to ashes."

Mitra and Ray (2014) study the determinants of the different waves of Hindu-Muslim violence. Accordingly with their work a clear pattern emerges: conflict appears to react significantly and positively to an increase in Muslim per-capita income, while the opposite reaction occurs to an increase in Hindu per-capita income: conflict declines. The very fact of a connection between changes in group relative incomes and subsequent conflict is of interest, as it suggests a clear instrumental basis for conflict.

Figure 3 summarizes the findings by Mitra and Ray. Each panel contains lines that connect a particular region of India over three rounds of the National Sample Survey, ordered by the (the logarithm of) Hindu and Muslim per-capita expenditure in those rounds. (The Survey uses expenditure as a proxy for income.) The vertical axis records the logarithm of total "casualties" — killed plus injured — in the five-year period starting immediately after the rounds. Region-specific and time-specific effects on conflict have been eliminated from the latter number: only the residuals are plotted. The line segments are generally upward-sloping in the first panel of the figure, and

downward-sloping in the second showing that indeed conflict follows an increase in Muslim per-capita income, while a decline occurs after an increase in Hindu per-capita income. Mitra and Ray argue that the rise in Hindu income represents an opportunity cost effect, much like the change in coffee prices in Colombia in the Dube-Vargas study. It serves to reduce Hindu-Muslim violence. The rise in Muslim incomes, on the other hand, is analogous to the change in oil revenues in the Dube-Vargas setting: it aggravates the desire to loot, or seek to retribution against an upstart community.

The work of Mitra and Ray illustrates the story of ethnic conflict that we have in mind. It is an instrumentalist view, and quite opposed in spirit to the notion espoused by Samuel Huntington (1996); that such violence is a "clash of civilizations." (The instrumentalist view, incidentally, is far from being our creation; see, e.g., Brubaker and Laitin 1994.) The argument runs in two steps. First, economic *similarity*, not difference, can breed tensions — indeed, such tensions, involving as they do the direct contestation of resources, can be extremely acute. Second, the resolution of such tensions involves the use of existing ethnic divisions or categories to create a sense of Us and Them, thereby accentuating the salience in those divisions. We recognize that such an instrumentalist view cannot survive entirely on its own: there must be some exploitable historical animosity embedded in those ethnic divisions. Nevertheless, on the surface, a conflict across economically similar groups that differ in caste, ethnicity or geography can be a profoundly economic conflict!

It is entirely reasonable that with uneven growth or globalization, some social or economic groups will benefit more than others, possibly on account of their fortuitous positioning.[11] This is certainly true of income groups, because higher incomes or wealths may permit individuals to incur the threshold costs that are needed for training and entry into a new occupation, or to incur the setup costs for entering a new business. But it is also true of ethnic groups whenever such groups exhibit some degree of geographical or sectoral specificity. Returning to the Hindu-Muslim example: it is certainly the case that the Gulf boom led to differential Hindu-Muslim gains. Rising oil prices resulted in a sizable increase in the demand for labor in the Gulf countries. Indian Muslims were more likely to emigrate there than Indian Hindus. In turn, this flow resulted in remittances back to India from the Gulf, affecting Muslim incomes and expenditures in India, often generating highly visible real estate improvements, and presumably improving the ability to enter new business sectors.

These changes in group-specific relative incomes can have deep effects on individual aspirations, and possibly on resulting frustrations; see Appadurai (2004), Ray (1998, 2006), and Genicot and Ray (2014). Albert Hirschman's parable (see Hirschman and Rothschild, 1973, and also Gurr 1968) of a multi-lane traffic jam comes to mind: you're in one of the lanes, stationary like all the cars around you. Now the cars in the other lane begin to move. Do you feel better or worse? Presumably, that depends on how long the other lane has been moving. Unevenness can be tolerated or even welcomed as it raises aspirations across the board, but it will be tolerated for only so long. In the flip side of this parable, uneven growth is frustrating rather than inspiring, and economic development may be threatened by violent means.

Notice that such frustration may not necessarily be tied to inter-group *inequality*. The improvement in the economic fortunes of a rival group may be viewed as a threat (or a source of frustration)

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[11]This is in line with the argument of Robert Bates (1983), who emphasizes the impact of uneven growth facilitating the emergence of an economic and cultural elite. They provide the leadership and the means for the escalation.

even when that rival group is poorer than your group. In the latter case, an increase in cross-group *equality* can be conflictual. The key word is *uneveness*, not inequality.[12] If two groups have disparate incomes, as in a caste-based or feudal society, cross-group interaction may be limited and pose little threat. But as the fortunes of the deprived group improve, the previously advantaged groups may feel threatened and react with violence (see, e.g., Olzak and Shanahan 1856). This argument echoes Tocqueville's study of the French Revolution, in which he repeatedly stressed the apparent paradox that the Revolution was the outcome of improvement, not impoverishment. It was precisely because the middle classes were becoming richer that they were more conscious of where they felt they *should* stand. In that new light, the privileges of aristocracy were unacceptable. And so it was that "[t]he French found their position insupportable, just where it had become better …" In summary, "[i]t is not always that from going from bad to worse that a society falls into revolution" (Tocqueville, 1856).

Observe that in the Olzak-Shanahan argument, it is the advantaged groups that have the upper hand in igniting a conflict — perhaps the disadvantaged groups are too poor or in a numerical minority. In the Tocqueville argument, it is the rising frustration of the relatively disadvantaged groups that leads to the proverbial storming of the Bastille (though not proverbially in his case). Just *which* group has this power to ignite a conflict may be deeply rooted in the history of the society.

3.4. **Difference and Similarity: A Summing Up.** The traditional economic view of conflict is that it is driven by large inequalities in income and wealth. Oddly enough, the empirical support for this assertion is mixed. We've discussed why this might be so. We need good theory to precipitate the form of the relationship, followed by empirical research that takes serious advantage of the theory. While there is little doubt — simply by looking at the world around us — that large inequalities create social tension and unrest, it is entirely unclear what the specific structure of that relationship should be; for instance, whether a measure of inequality or polarization should be used as an explanatory variable. The technology of conflict, such as the synergistic use of labor and financing, also requires careful study. It is only with such building blocks in place that we can begin to conjecture the particular relationship to be examined. We believe that this is a significant open area for research.

Our second point is that the possibility of class-driven conflict does not preclude the existence of other sources of social discontent. In the next section we shall examine the case of ethnic conflict. But the more basic observation is that similarity can be directly conflictual when resources are limited and economic change is unevenly distributed. This is possibly relevant even for developed countries, but it is a first-order consideration in developing countries. Non-class conflict is the outcome, and ethnicity is a convenient marker to categorize individuals on either side of some quasi-artifical divide. It isn't that the marker isn't real: it is certainly as real or more than the dividing line between, say, the viciously competitive supporters of rival soccer teams. It doesn't take much for people to fight. But additionally, more than a soccer match is at stake: it is the division of economic gains. From this point of view, conflict is more about the *change* in the relative income status of two similar groups rather than the overall level of inequality, a consideration that also appears in Stewart (2002). How group-based aspirations are formed, and how certain groups might react to frustrated aspirations remains an important and open research direction.

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[12]There is little evidence for the argument that the relative deprivation of a group, or indeed, overall economic inequality, is conflictual; see, for instance, Lichbach (1989) for cross-country studies, and Spilerman (1970, 1971, 1976), Wilson (1978), and Olzak and Shanahan (1996) on race riots in the urban United States.

4. CONFLICT AND ETHNIC DIVISIONS

We now turn to a particularly pernicious non-economic marker: ethnicity, broadly defined to include religious or ethnolinguistic differences. Suppose that we use the criterion, employed by the Peace Research Institute of Oslo, that a conflict is "ethnic" if it involves a clash against the State on behalf of some ethnic or religious group; see Fearon (2006). Under this criterion, more than half of the civil conflicts recorded since the end of World War II have been classified as ethnic by Political Instability Task Force (2012), and see also Fearon and Laitin (2003a,b). Such conflicts involved 14% of the 709 ethnic groups categorized worldwide; see Fearon (2003). R. Brubaker and D. Laitin (1998), examining the history of internal conflicts in the second half of the twentieth century, are led to remark on "the eclipse of the left-right ideological axis," and the "marked ethnicization of violent challenger-incumbent contests." D. Horowitz, (1985) author of a monumental treatise on the subject of ethnic conflict, observes that "[t]he Marxian concept of class as an inherited and determinative affiliation finds no support in [the] data. Marx's conception applies with far less distortion to ethnic groups."

While we do not mean to suggest that *all* conflicts between ethnic groups are those between economically similar groups, they often are. Ethnicity might serve as a convenient rallying cry to include and exclude. Of course, for that to happen, the society in question must have ethnic divisions to draw upon to begin with. That leads to the following hypothesis: "ethnically divided" societies are more likely to engage in conflict. Do we have evidence to support this view?

4.1. **Fractionalization, Polarization and Ethnic Conflict.** The first question is: just what does "ethnically divided" mean? There is a classical measure that attempts to get at this concept, and it was first introduced in the 1964 edition of the Soviet Atlas Narodov Mira. If $\{n_1, n_2, \ldots, n_m\}$ stand for the population shares of m ethnic groups, then the *fractionalization index* is given by

$$(1) \qquad F = \sum_{i=1}^{m} n_i(1 - n_i),$$

which can be interpreted as the probability that two individuals drawn at random from the society will belong to different groups.

Fractionalization is a famous index that has been put to work on several occasions. It has indeed been connected to low per-capita GDP (Alesina, Baqir and Easterly 1999), slow economic growth (Easterly and Levine 1997), underprovision of public goods (Alesina and La Ferrara 2005), or to poor governance (Mauro 1995). It is also closely connected to the Gini coefficient of economic inequality.[13]

Unsurprisingly, this is the measure that leading scholars initially used as a possible correlate of conflict; see Collier (2001), Collier and Hoffler (1998, 2004), and Fearon and Laitin (2003a). Yet, the verdict is surprisingly murky: there does not appear to be a strong relationship between conflict and ethnic fractionalization. Look again at the Fearon and Laitin (2003a) regression reproduced in Table 1. Ethnic fractionalization is marginally significant; that too, only in some specifications. The same lack of significance can be observed in the results by Miguel, Satyanath and Sergenti in

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[13]The Gini is proportional to $\sum_{i=1}^{m} \sum_{j=1}^{m} n_i n_j d_{ij}$, where d_{ij} is a measure of inter-group distance between i and j; usually, absolute income differences. The fractionalization measure F corresponds precisely to the case in which d is "binary": $d_{ij} = 1$ if $i \neq j$, and 0 otherwise.

Table 2, or in the studies by Collier and Hoeffler. Fearon and Laitin conclude that the observed "pattern is thus inconsistent with ... the common expectation that ethnic diversity is a major and direct cause of civil violence."

And yet, these statistical findings remain strangely at odds with the frightening ubiquity of within-country ethnic conflicts. We reiterate a now-familiar complaint below: this is in part because we are not using theory to inform the empirical specification at hand. The fact that fractionalization is an easily available index is not good enough reason to conclude that if *that* measure is uncorrelated with conflict, then ethnic divisions aren't conflictual. We need a theory that connects conflict to "ethnic divisions," and we must exploit that connection in the empirics.

The problem is (and this is true of empirical research more generally) that little discipline is often imposed on the specification of a conflict regression. Much of that research involves the kitchen-sink approach of including all variables that could possibly play a role in ethnic conflict. Such an approach is problematic on at least three counts. First, the number of plausible variables is unbounded, not just in principle but apparently also in practice: eighty five different variables have been used in the literature (Hegre and Sambanis, 2006). Trying them out in various hopeful combinations smacks uncomfortably of data-mining. Second, even if we could narrow down the set of contenders, there are many ways to specify the empirical equation that links those variables to conflict. Finally, the absence of a theory hinders the interpretation of the results.

Motivated by the need to capture how conflictual a society is, Esteban and Ray (1994)[14] introduced a measure of *polarization* based on the inter-group perceived distances d_{ij} as well as their size. A central index they derive is

$$
(2) \qquad P = \sum_{i=1}^{m} \sum_{j=1}^{m} n_i^2 n_j d_{ij}
$$

The polarization index P speaks to the existence of deep cleavages, not a "fractionalization" of society into many small and possibly inconsequential fissures. It deeply differs from fractionalization. For instance, P attains its maximum when the population is divided into two equally sized groups at some maximum distance from each other, while F attains its maximum when every individual is his or her own group, different from the rest. For a detailed discussion of the differences between polarization and fractionalization, see Esteban and Ray (2011).

We will, of course, need to think about how to implement d empirically. But there is a noteworthy special case in which d is "binary": $d_{ij} = 1$ if $i \neq j$, and 0 otherwise. That reduces P to the measure:

$$
(3) \qquad R = \sum_{i=1}^{m} \sum_{j=1}^{m} n_i^2 (1 - n_i)
$$

This is the index used by Montalvo and Reynal-Querol (2005) in their study of the determinants of intermediate and high-intensity civil war, as defined by PRIO. We discuss their results in Section 4.2.2. Before we do so, we describe how a theory of conflict can highlight the role of polarization and fractionalization in capturing "ethnic divisions."

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[14]Esteban and Ray (1994, 1999, 2011a), Wolfson (1994) and Duclos, Esteban and Ray (2004) all develop related measures of polarization that attempt to incorporate deep cleavages.

4.2. **Are Ethnic Divisions Conflictual?** If ethnicity is indeed salient, conflict should be related to the existence of "ethnic divisions" in society. How do we conceive of that connection?

4.2.1. *Theory.* From a statistical perspective, fractionalization and polarization are just two, seemingly equally reasonable, ways of measuring ethnic divisions. Which (if any) of these should matter in connecting ethnic "divisions" to conflict? This is the issue studied in Esteban and Ray (1999, 2011a), who introduce an explicit game-theoretic model of conflict.[15]

Consider m groups engaged in conflict. Think of two types of prizes at stake. One kind is "public," the individual payoff from which is undiluted by one's own group size. Examples include a norm or culture, a religious state, the abolition of certain rights or privileges, the repression of a language, the banning of certain organizations, the seizing of political power, or the satisfaction of seeing one's own group vindicated or previous defeats avenged. Let u_{ij} be the payoff experienced by an individual member of group i in the case in which group j wins and imposes its preferred policy; $u_{ii} > u_{ij}$ is true almost by definition. This induces a notion of "distance" across groups i and j: $d_{ij} \equiv u_{ii} - u_{ij}$, which can be interpreted as the loss to i of living under the policy implemented by j.

The other type of prize is "private". Examples include access to oil or other mineral deposits (or the revenue from them), or specific material benefits obtained from administrative or political positions, or just plain loot. In contrast to public prizes, a private prize is diluted by group size: the larger the group, the smaller is the return per capita. Moreover, there is no fine-tuned notion of intergroup distance with private prizes: either your group seizes the loot, or it doesn't.

Individuals in each group expend costly resources (time, effort, risk) to influence the probability of success. The winners get to make the decisions and enjoy the prize(s); the losers have to live with the policies chosen by the winners. A *conflict equilibrium* is just the induced Nash equilibrium of this game, with an extended payoff structure that includes both individual and group concerns (see Esteban and Ray, 2011a, for a detailed discussion of this point). Briefly, we assume that an individual will act selfishly, and to some extent he will act in the interest of the ethnic group.

Let us measure the intensity of conflict —call it C— by the money value of the average, per capita level of resources expended in conflict. In Esteban and Ray (2011a), we argue that the population-normalized intensity of conflict C is described by the approximate formula

(4) $$C \sim \big[\lambda P + (1 - \lambda)F\big],$$

for large populations, where λ is the relative degree to which the prize is public, and F and P are the polarization and fractionalization indices described earlier in (1) and (2), the former constructed binary distances, and the latter using inter-group distances d_{ij} derived from "public" payoff losses $u_{ii} - u_{ij}$. The constant of proportionality (not explicitly stated in (4)) will depend on the scale of the prize(s) as well the extent to which a typical individual places payoff weight on his group.

Note how the theory informs empirical specification. In particular, the publicness of the prize is naturally connected to polarization. With public payoffs group size counts twice; once, because the payoffs accrue to a larger number, and again, because a larger number of individuals internalize that accrual, and therefore contribute more to the conflict. Inter-group distances matter too: the precise policies interpreted by the eventual winner continue to be a cause of concern for the loser. Both

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[15]The following informal exposition draws heavily on Esteban, Mayoral and Ray (2012b).

these features — the "double emphasis" on group size and the use of distances — are captured by the polarization measure P. On the other hand, when groups fight for a private payoff — say money — one winner is as bad as another as long as my group doesn't win, and measures based on varying inter-group "distances" become irrelevant. Moreover, with private payoffs, group identification counts for less than it does with public payoffs, as group size erodes the per-capita gain from the prize.

In short, the theory tells us to obtain data on P and F, and combine them in a particular way. It informs us, moreover, that F alone is unlikely to be significant in explaining conflict, as the resulting omitted variable P would confound the effects laid bare in (4).

4.2.2. *Empirics.* Esteban, Mayoral and Ray (2012a, b; EMR henceforth) study 138 countries in five-year intervals over 1960–2008. They measure conflict intensity in two ways. The first is the death toll. In this review, we consider one such example: for every five-year period and every country, set conflict equal to 1 if that country has experienced more than 25 battle related deaths but less than 1000 in any of these years, to 2 if the country has experienced more than 1000 casualties in any those years, and to 0 otherwise.[16] The second approach is to use a non-death measure; they employ the *Index of Social Conflict*, computed by the Cross-National Time-Series Data Archive, Banks (2008). which embodies eight different manifestations of internal conflict, such as politically motivated assassinations, riots, guerrilla warfare, etc.

To compute P and F, EMR rely on Fearon (2003), who identifies over 800 "ethnic and ethno-religious" groups in 160 countries. For inter-group distances, they (Fearon, 2003, Laitin, 2000, and Desmet et al. , 2012)) employ the *linguistic distance* between two groups as a proxy for group "cultural" distances in the space of public policy; see EMR for details. Such a proxy undoubtedly takes EMR out on a limb, but reflects a common tradeoff. On the negative side, linguistic distances are at best an imperfect proxy for the unobserved "true distances". But something closer to the unobserved truth — say, answers to survey questions about the degree of intergroup antagonism, or perhaps a history of conflict — are deeply endogenous to the problem at hand. Whether the tradeoff is worth it is something that only a mixture of good intuition and final results can judge.

To obtain a relative publicness index by country, EMR use the value of oil reserves per-capita as a a proxy for privateness. They create an index of "publicness" by measuring the degree of power afforded to those who run the country, "more democratic" being regarded as correlated with "less power" and consequently a lower valuation of the public payoff to conflict. The latter indicator is multiplied by per-capita GDP to convert the "poor governance" variables into monetary equivalents. (The results are robust to the precise choice of this conversion factor; see EMR.) To obtain *relative publicness*, the two indices are converted into a single ratio.

Columns 1 and 2 in Table 3 record the results for each specification of the conflict intensity variable — the death-based outcome PRIO-C and the aggregated indicator of several conflict dimensions ISC. Ethnicity turns out to be a significant correlate of conflict, in sharp contrast to the findings of the previous studies mentioned above. Throughout, P is highly significant and positively related to conflict. F also has a positive and significant coefficient.

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[16]This index uses data from the jointly maintained database under the Uppsala Conflict Data Program and the Peace Research Institute of Oslo (UCDP/PRIO).

Variable	[1] PRIO-C	[2] ISC	[3] PRIO-C	[4] ISC
P	*** 5.16 (0.001)	***19.50 (0.002)	- 1.48 (0.606)	-16.33 (0.227)
F	* 0.93 (0.070)	* 3.56 (0.061)	0.76 (0.196)	0.31 (0.878)
$P\Lambda$			***11.174 (0.003)	***61.89 (0.001)
$F(1-\Lambda)$			* 1.19 (0.097)	***10.40 (0.000)
GDPPC	**- 0.34 (0.047)	***- 2.26 (0.004)	*- 0.36 (0.080)	***- 3.02 (0.001)
POP	*** 0.24 (0.000)	*** 1.14 (0.000)	*** 0.21 (0.001)	*** 1.30 (0.000)
NR	- 0.27 (0.178)	- 0.53 (0.497)	- 0.00 (0.570)	0.00 (0.432)
MOUNT	0.00 (0.537)	0.02 (0.186)	0.00 (0.362)	* 0.03 (0.061)
NCONT	*** 1.06 (0.001)	*** 4.55 (0.001)	** 0.77 (0.026)	*** 4.28 (0.001)
POLITICS	0.18 (0.498)	0.29 (0.789)	- 0.00 (0.328)	**- 0.00 (0.026)
LAG	*** 1.99 (0.000)	*** 0.46 (0.000)	*** 1.94 (0.000)	*** 0.44 (0.000)
CONST	-	0.90 (0.915)	-	9.19 (0.398)
(Pseudo)-R^2	0.35	0.43	0.36	0.44
Observations	1125	1111	1104	1090
Countries	138	138	138	138

Table 3. Ethnicity and Conflict.

Notes. All specifications employ region and time dummies, not shown explicitly. p-values are reported in brackets. Robust standard errors adjusted for clustering have been employed to compute z-statistics. Columns [1], [3] and [5] are estimated by maximum likelihood in an ordered logit specification, and columns [2], [4] and [6] by OLS. GDPPC: log of gross domestic product per-capita; POP: log of population; NR: a dummy for oil and/or diamonds in Columns 1 and 2 and oil reserves per-capita (OILRSVPC) for columns [3]–[6]; MOUNT: percentage of mountainous territory; NCONT: non-contiguous territory, see text; POLITICS is DEMOC in columns [1] and [2] and the index PUB times GDPPC (the numerator of λ) for the remaining columns; LAG: lagged conflict in previous five-year interval. Source: Esteban, Mayoral and Ray (2012b).

Quite apart from statistical significance, the effect of these variables is quantitatively important. Taking Column 1 as reference, if we move from the median polarized country (Germany) to the country in the 90th percentile of polarization (Niger), while changing no other institutional or economic variable in the process and evaluating those variables at their means, the predicted probability of experiencing conflict (i.e, the probability of observing strictly positive values of PRIO-C) rises from approximately 16% to 27%, which implies an increase of 69%. Performing the same exercise for F (countries at the median and at the 90th percentile of F are Morocco and Cameroon, respectively) takes us from 0.19% to 0.25% (an increase of 31%).

In Columns 3 and 4 in Table 3, the main independent variables are $P * \Lambda$ and $F * (1 - \Lambda)$, just as specified by the theory; where Λ is our estimated degree of relative publicness. Polarization

interacted with Λ is positive and highly significant, and the same is true of fractionalization interacted with $1 - \Lambda$. These results confirm the relevance of both polarization and fractionalization in predicting conflict once the variables are interacted with relative publicness in the way suggested by the theory.

Indeed, the level terms P and F are no longer significant on their own, *once* entered in interacted form with Λ. Assuming that the proxy for relative publicness accurately captures all the issues at stake, this is precisely what the model would predict. For instance, polarization should have no further effect over and beyond the "λ-channel": its influence should dip to zero when there are no public goods at stake. The fact that the estimate Λ happens to generate exactly this outcome is of interest. But the public component of that estimate is built solely on the basis of governance variables. If this wipes out all extraneous effects of polarization (as it indeed appears to do), it possibly suggests that primordial factors such as pure ethnic differences *per se* have little to do with ethnic conflict.

EMR (2012a,b) present a large number of variations on these regressions. We refer the reader to those papers for more details.

EMR are not the first to take polarization to the data. In an earlier contribution, Montalvo and Reynal-Querol (2005) study the determinants of intermediate and high-intensity civil war, as defined by PRIO. (They do not relate polarization to public prizes and fractionalization to private prizes.) In order to measure ethnic divisions they use the measure of ethnic polarization in (3). We summarize their main result by quoting them (p. 805):

> "The first column [in a multi-column table of regressions] shows that the index of ethnolinguistic fractionalization ...has no statistically significant effect on the incidence of civil wars. This result is consistent with Fearon and Laitin (2003a) and Collier and Hoeffler (1998). If we substitute the index of ethnic fractionalization by the RQ index of ethnic polarization [which is the measure in (3)] ...we find (column 2) a positive and statistically significant effect on the incidence of civil wars ...Column 3 checks the relative strength of the index of ethnic polarization versus fractionalization, and shows that the coefficient on ethnic fractionalization is not significantly different from zero, while the one on polarization is positive and significant."

4.3. **Ethnic Salience: A Summary and Research Directions.** A society has several potential cleavages, most of which lie dormant at any one point of time. After all, the term "ethnicity" covers a variety of traits that could have been invoked to substantiate a division: religion, language, regional identity, gender, and so forth. And overlying all these, though sometimes obscured, is the deep difference that comes from economic inequality. Just how these various factors interact, and exactly which cleavage is ignited in any given situation, are research topics of first-order importance. In particular, we need to understand why an ethnic division often acquires salience over an economic division.

From this perspective, the results on ethnic divisions and violence that we've just summarized really represent the tip of the iceberg. For instance, the fact that an ethnic division exists is not enough reason for it to be invoked. We also need to study the economic characteristics of each ethnic group, along with their demographics. The theoretical framework has to be enriched to allow

for a far more nuanced interaction of economics and ethnicity. It is only such a framework that can suggest the right specification to take to the data. Such a research project seems indispensable if we are to truly understand the fundamental connections between inequality, ethnicity, development and conflict.

Here are some specific arguments that bear on the salience of non-class markers in conflict, as well as the interaction between economic adn ethnic characteristics. We have already touched briefly on some of these issues in Sections 3.2 and 3.3, but they are worth additional elucidation in the specific context of ethnic conflict.

In the first place, economic demarcation across classes is a two-edged sword: while it breeds resentment, the very poverty of the have-nots separates them from the means for a successful insurrection. Conflict isn't always an outpouring of individuals onto the streets, though in times of extreme stress it may well be. Yet even such movements that are explicitly based on class — *Occupy* being a recent and visible example — require sustained organization and financing so as not to die out. That requires, in turn, a commitment to class struggle by some socially aware sub-segment of the wealthy, so that a large rebellion by the proletariat or the peasantry can be sustained in a viable way. This is rare, and in developing countries, where the problem of poverty are so endemic, it is rarer still.

Secondly, in conflicts across groups that are not demarcated by pure economic considerations, each group will have both poor and rich members, with the former supplying conflict labor and the latter supplying conflict finances. Esteban and Ray (2008) use this line of reasoning to argue for the salience of ethnic conflict. As an example, suppose that individuals can be rich or poor and of either of two ethnic identities. The issue is then whether individuals will prefer to form alliances using class or ethnicity. When effective activism requires both labor and finance, this will happen more easily with ethnic alliances. Therefore, controlling for the size of the prize, the poor of the majoritarian ethnic group will prefer an ethnic to a class alliance.

This argument suggests an interesting interaction between inequality and ethnicity, in which ethnic groups with a higher degree of within-group inequality will be more effective in conflict (Esteban and Ray, 2011b). Such a hypothesis is entirely distinct from — though complementary with — the notion of "horizontal inequality" across ethnic groups, in which economic differentials *across* ethnic groups is an important correlate of conflict (Sambanis, 2005, Stewart, 2002, Cederman, Weidmann and Gleditsch, 2011, Østby, 2008). Both hypotheses are worth testing. As an example, Huber and Mayoral (2014) compute the Gini index of the income distribution within ethnic groups as well as across groups for a number of countries. In their study, within-group inequality is highly significant in explaining violent conflict. Between-group horizontal inequality is not. Here again is an echo of the similarity-differences theme discussed earlier. More research on this theme is surely needed.

Third, the possibility of conflict across ethnic lines — while conceivably economic — *presumes* that there is some reason for there to be conflict across groups to begin with. There are two broad views on the ethnicity-conflict nexus; see, e.g., Brubaker and Laitin (1998), and Fearon (2006). The "primordialist" view , e.g. Huntington (1996) and Ignatieff (1993), takes the position that ethnic differences are ancestral, deep, and irreconcilable, and for these reasons invariably salient. In contrast, the "instrumental" approach pioneered by Bates (1983) and discussed in Brubaker and Laitin(1998) sees ethnicity as an strategic basis for coalitions that seek a larger share of economic

or political power. Under this view, ethnicity is a device for restricting the spoils to a smaller set of individuals. Certainly, the two views interact. Strategic ethnic conflict could be exacerbated by hatreds and resentments — perhaps ancestral, perhaps owing to a recent clash of interests — that are attached to the markers themselves. And there could be factors at play other than economic considerations or hatreds: for instance, exclusion might be easier if ethnic groups are geographically concentrated (see, e.g., Matuszeski and Schneider (2006) and Weidmann, Rød, and Cederman (2010)).

Fourth, it is worth taking note of the possible inadequacy of political institutions to solve the competing claims of the main social groups in developing countries. Developing countries were born from a process of decolonization — often a sudden and hasty decolonization — that left newborn governments especially vulnerable to a host of competing claims. As Mayoral and Ray (2015) have argued, post-colonial societies inherited certain institutions — progressive taxation, land reform, public provision of education or health care — that were designed to temper class conflict. That focus finds its echo even in scholarship. Noteworthy contributions such as Acemoglu, Johnson and Robinson (2001) or Glaeser (2002)) often invoke the implicit viewpoint of class to evaluate institutions. For instance, the differential effect of civil and common law is often viewed through the different treatment of private ownership. Our concern is different: ethnically divided countries (which are correlated with poorer countries) may be less equipped to make transfers across ethnic groups than across income classes. Such class-sensitive arrangements are no coincidence, as the colonizing countries from which these newcomers separated have had centuries of experience in developing those very institutions. But the divisions in the newly born countries are often *ethnic*, and there are few analogous institutions for the differing fiscal treatment of ethnic groups. Such countries are therefore more prone to conflictual challenges on ethnic fronts.

Thus, economic policy in developing countries must often adapt to the realities of ethnicity, religion or caste. After all, ethnic differences are often a (noisy) proxy for economic differences. The reason developing countries hone in on those ethnic differences — rather than focus on class *per se* — is that individual income is far harder to observe, and can be an inadequate basis for policy.[17] Ethnicity, while imperfect, is observable and is frequently used as a second-best solution.

Particularly in sub-Saharan Africa, but also in many other developing countries, income inequality is closely attached to ethnic identity. As an illustration, the Gini index of incomes can be decomposed into the inequality between ethnic groups (BGI), the inequality within groups (WGI), and a residual overlap term. In sub-Saharan Africa, the between-group term accounts for a large part of recorded inequality: it is more than 3 times as large as in the OECD countries and 50% larger than in other developing countries.[18] In addition, BGI is the component of the Gini decomposition that co-moves most with the Gini, with a correlation of almost 0.7. Hence, explaining income inequality in sub-Saharan Africa often goes hand in hand with explaining between group inequality.[19]

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[17]That isn't to say that discourse in developed countries is immune to playing the ethnic card. Perhaps the most visible recent example is Ta-Nehisi Coates' (2016) call for direct transfers ("reparations") to African-Americans.

[18]Data on ethnic inequality comes from Huber and Mayoral, 2014.

[19]Alesina et al. (2013) show that what mostly matters for development are economic differences between ethnic groups coexisting in the same country, rather than ethnic diversity per se or income inequality conventionally measured (i.e., independent of ethnicity). Easterly and Levine (1997) were the first to stress the key role of ethnic identity. See also Stewart (2002) and more recently Cederman et al (2011).

Given these correlations with economics, and given the general difficulty of observing individual incomes, it is hardly any surprise that public discourse often focuses on ethnicity. But it isn't just discourse, it is also public action. For instance, government expenditures are often biased towards group-specific investments or transfers, rather than universal public investment. This bias is specially remarkable in SSA countries where ethnic divisions are deeper. Public investments often have a strong ethnic undertone; see, e.g., the study of road-building in Kenya by Burgess et al (2015). Even explicit and formal policies have been formulated on the basis of non-economic markers. India has possibly the largest affirmative action program in the world; it is all based on a massive reservation of job, college admissions, and political positions *by caste*. While that reservation has been historically justified on the basis of past inequities perpetuated on the so-called Scheduled Castes, there is little doubt that it has served as a proxy for *economic* redistribution. Such policies, even when well-intentioned, create an enormous salience for claims that are ethnic-based, and generate ethnic violence when those claims are not acceded to. A particularly recent example comes from Gujarat, where the influential and generally well-heeled Patel community has demanded "their share" of quotas in government jobs. In August of 2015, violence broke out in Ahmedabad, leaving several dead.

At one level such violence is reminiscent of an absurdist drama. How does an economically better-off community ask for quotas? The answer becomes clear when one recognizes that such ethnic divisions correlate with income, but do not correlate perfectly. Therefore, there are always grievances that can be justified: a rich person from a Scheduled Caste can get a government-sanctioned advantage relative to a poor Patel. And yet, ethnicity — caste, in this case — needs to be retained because it is an observable, and all too often, income is not.

5. Concluding Remarks

We have presented a panoramic view of the literature on social conflict and economic development organised around three widely shared views: higher standards of living reduce the probability of conflict; inequality nurtures conflict; and most conflicts in developing countries are ethnic in nature. On the way, we have also described a number of research questions still open in each of these three broad fields. To briefly conclude, we summarize these research suggestions into three major lines of potential progress in our understanding of economic development and social conflict.

The first one takes seriously into consideration that economic growth is unbalanced by nature. The uneven impact of sector- or group-specific economic fortune upsets traditional social and economic rankings. We must think not just of high aspirations that serve to inspire, but high aspirations that can serve to frustrate. These considerations seem particularly relevant when differential economic growth takes place among previously similar groups.

The second major research problem is to better explain the link between income inequality and social conflict. The one thing we know is that that connection is far from straightforward, and at many points in this text we have emphasized both this fact and the possible reasons for it. There is much to be gained from a closer marriage between theory and empirics, similar to — but going well beyond — the analysis of the determinants of ethnic conflict just summarized in the last section. The discipline imposed by theory will tell us which are the relevant independent variables, and how they interact.

The third line of research is related to the second, but goes beyond it. We have a long and distinguished tradition in theories of social conflict based on economic *distance*. But a substantial share of social conflict can be attributed to economically *similar* groups. The dividing cleavage is then non-economic (though the conflict can still be over economic resources). Typically, that cleavage manifests itself along ethnic lines. For this reason, the interaction between ethnic identity and economic characteristics, and how such interaction might result in ethnic conflict, is a research topic of the highest importance.

In short, we need to broaden our horizons beyond economics, even for conflicts that are intrinsically economic in nature. We must explore more fully the interdependence between income distribution, ethnic identity, economic development and social conflict.

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