

Chapter 2.

Ethnic, Religious and Class-Based Civil Conflicts

The first part of this book focuses on national political exclusion and rebel support along identity lines, namely ethnic, religious or class-based ideological lines, as well as any combination thereof. In this context, **political exclusion** or **discrimination** refers to actions of the government that exempt certain parts of the population from the distribution of governmental resources. Political exclusion is the reverse of **political inclusion** or **favoritism**, referring to governmental actions that privilege and support certain parts of the population (Grim and Finke 2006, 15f.). Furthermore, following Chandra (2006, 400), I define **identity** as “any social category in which an individual is eligible to be a member.” The main research question is whether it is possible to predict the onset of civil conflict based upon exclusion patterns along all three lines of identity — ethnicity, religion and class-based ideology — as well as whether such a three-dimensional perspective on political exclusion is superior to a one- or two-dimensional perspective. The following two sections show that this research makes an important contribution to the literature, and that ample empirical case study evidence points to the necessity of considering all three — rather than only one or two — of the three identity dimensions of ethnicity, religion and class-based ideology.

2.1. The Literature

By focusing on the exclusion of large, multidimensional identity groups as a predictor for civil conflict, this book ties in with extant research on the relationship between demography and civil conflict (Esteban and Ray 1994; Alesina et al. 2003; Montalvo and Reynal-Querol 2005), as well as political exclusion and civil conflict (Horowitz 1985; Gurr 1993; Wimmer et al. 2009; Cederman et al. 2010). I add a theoretical foundation to existing studies and deviate from the focus on a single identity category by looking at three dimensions. This book also extends existing studies by conditioning the effect of exclusion on regime type: an aspect that has previously not been in

the focus. The following paragraphs show what we can learn from existing studies by sketching both the demographic and the exclusion perspective, as well as by briefly introducing findings on regime type and civil conflict.

As the examples in the introduction illustrate, civil conflicts run along ethnic, religious and class-based ideological lines as well as any combination thereof. Some consider these identity-based differences between rebel groups and governments as epiphenomenal (e.g. Collier and Hoeffler 2004). Others try to account for these differences assuming that groups of a different identity are more likely to experience grievances, more likely to become mobilized and thus more likely to clash (Gurr 2000). The question thus arises concerning whether it is possible to predict the onset of civil conflict with demographic measures of social identity. Given the prevalence of *ethnic* civil conflict, the most prominent demographic indicator has become the ethnic fractionalization index as a measure of ethnic diversity (Easterly and Levine 1997), which is assumed to increase the risk of civil conflict onset. However, quantitative support for this indicator has been absent or weak (Collier and Hoeffler 2004; Fearon and Laitin 2003; Miguel et al. 2004; Hegre and Sambanis 2006).¹

One deficiency of the fractionalization index is that the choice for fractionalization is rather arbitrary. While a higher number of mobilized ethnic groups may clash more often, an opposite stance holds that it is very difficult for small ethnic groups to mobilize in the first place, with the implication being that the risk of civil conflict should be highest if two almost equally sized groups — rather than many small groups — oppose each other (cp. Montalvo and Reynal-Querol 2005; Esteban and Schneider 2008). On top of that, it may be argued that very different, distant ethnic groups are more likely to clash than very similar groups, wherefore the distance between ethnic groups should also be taken into account (see Fearon 2003). That said, authors recommend using a measure of polarization — rather than fractionalization — although the support for this is likewise ambiguous (see Schneider and Wiesehomeier 2010). Given these opposing views, Esteban and Ray (2011) develop theoretical expectations about the conditions under which one or the other indicator — fractionalization or polarization — should apply in determining the severity of civil conflict (not its onset). With public goods, preferences with respect to the implemented policy and thus distances between groups are more relevant, thus privileging the polarization index. With private goods, when goods are distributed on

¹There is more support for the fractionalization index if non-linear effects or lower-level violence are taken into account (cp. Cederman et al. 2009; Hegre and Sambanis 2006).

an individual basis, relative group size matters most, thus privileging the fractionalization index (Esteban and Ray 2011, 1364). Empirical tests on conflict incidence (Esteban et al. 2012) provide support for this perspective.

A second weakness of demographic indices like the fractionalization index is that they neglect the institutional context, most importantly, patterns of political inclusion and exclusion (see Cederman and Girardin 2007; Fearon et al. 2007). In a seminal work, Horowitz (1985) most prominently introduces the idea that ethnic favoritism and its downside of ethnic political exclusion are the main causes of violent ethnic conflict, providing numerous case study examples for the assumed relationship (similarly Petersen 2002). From a theoretical perspective, political exclusion may lead to conflict via social-psychological mechanisms, whereby politically excluded groups develop low self-esteem and a feeling of subordination, which motivates them to violently reverse the status quo (cp. Tajfel and Turner 1979). Others like Caselli and Coleman II (2006) and Fearon (1999) provide a more strategically motivated underpinning for ethnic rebellion.

Empirically, the Minorities at Risk project sets out to test the grievance perspective by collecting systematic world-wide data on excluded ethnic minorities and their risk for rebellion, although it is often criticized for its selection bias (Fearon 2003, 196; Hug 2003). The most recent and elaborate data collection exercise comes from Cederman et al. (2010), with a dataset on supposedly all politically relevant ethnic groups in over 155 countries and their relative political status.² Quantitative analyses of these data show that the size of the excluded ethnic population (Wimmer et al. 2009) and the political exclusion of large ethnic groups (Cederman et al. 2010) significantly increase the risk of civil conflict onset. Theoretically, the authors informally refer to a mixture of social-psychological grievances and strategic calculations, which motivate individuals to resort to violent means (Wimmer et al. 2009, 321).

While the simultaneous consideration of demographic characteristics and political exclusion in more recent studies is desirable, one shortcoming is that these studies constrain their focus to ethnicity in the form of either ethnic demographics or ethnic exclusion. However, this exclusive focus on *ethnicity* has been criticized in recent years and is the main peg of this book. Although civil conflicts frequently run along ethnic lines, other identity lines — most prominently religion and class-based ideology — are also present, sometimes in isolation, sometimes in combination. However, if civil conflicts

²Ethnic groups are politically relevant if they are active in national politics or if they are discriminated against, according to expert codings.

are multidimensional, running not only along ethnic but also religious or class-based ideological lines, predicting civil conflict merely with an ethnic demographic indicator or ethnic exclusion falls short of the reality. Ignoring these other dimensions implies that we occasionally try to predict class-based or religious conflict with an ethnic demographic or exclusion pattern rather than the more plausible alternative of class-based or religious divisions. It also implies that we ignore all lines of identity that emerge in addition to ethnic divisions and that might be able to predict any kind of civil conflict, whether ethnic, religious or class-based ideological. Put differently, given that civil conflicts are often non-ethnic and multidimensional — as case studies show — shouldn't we take all relevant dimensions into account when predicting civil conflict? The literature thus far has only partly recognized this problem of the non-ethnic and multidimensional nature of civil conflict, providing tentative solutions, which I will recapitulate in turn.

One solution when acknowledging the multidimensional nature of civil conflict is to focus on ethnic conflict as a dependent variable rather than civil conflict per se (see Sambanis 2001; Fearon and Laitin 2003, 79), or to focus on ethnic groups as the unit of analysis (Cederman et al. 2010). The idea is that the ethnic demographic or exclusion variables should at least be able to predict the particular kind of ethnic civil conflict, if not all kinds of conflict. However, this approach is deficient in two respects: first, we are mainly interested in civil conflicts per se, and not in one very narrow kind; and second, adopting an exclusively ethnic lens obscures that divisions other than ethnic ones might influence and modify the demographic setup. Therefore, another common solution in the literature has been to include religious or class indicators alongside ethnic indicators to predict civil conflict as such. Most prominently, Collier and Hoeffler (2004) and Fearon and Laitin (2003) include the ethnic fractionalization index in addition to a religious fractionalization index and a Gini coefficient on income or land inequality, although they find little support for any of them. Alternatively, these and other authors construct multiplicative indices like the social fractionalization index³ (Collier and Hoeffler 2004), albeit they are likewise problematic (Stoll 2007). Therefore, the quest has been to ascertain a single variable that captures the multidimensional nature of social diversity to predict civil conflict, which might ultimately run along one dimension or

³According to Collier and Hoeffler (2004, 595), the social fractionalization index is “the product of the ethno-linguistic fractionalization and the religious fractionalization index plus the ethno-linguistic or the religious fractionalization index, whichever is the greater.”

many.

Selway is exceptional in that he follows the quest for a single multidimensional variable by constructing a two-dimensional index of overlappingness or crosscuttingness (cp. Selway 2011; Gubler and Selway 2012). His implicit assumption is that if two cleavages strongly overlap, the diverging identities of the respective groups and their opposing interests become more salient, whereby the groups are more likely to clash than if cleavages are cross-cutting. However, Selway's index is constrained to two dimensions, the issue of political inclusion and exclusion is completely neglected, and empirical support for the index has been inconclusive (see Selway 2011). Others (cp. Østby 2008) likewise recognize the importance of characteristics other than ethnic ones. However, they maintain the assumption about the predominance of ethnic groups and consider — in addition to the ethnic groups' political status — their economic endowments. The principal argument is similar to that of Selway: if ethnic groups not only differ in their political status, but also in their economic endowments, there are more reasons for antagonism, meaning that they are more likely to violently oppose each other. While these latter studies at least take patterns of inclusion and exclusion into account, class only serves as an attribute of ethnic groups, rather than an extra dimension. Moreover, religion is subsumed under the banner of ethnicity rather than being conceptualized as an extra dimension. This forfeits the possibility of predicting pure class-based ideological or religious conflicts.

Finally, previous studies on political exclusion have failed to account for an interaction between exclusion and regime type. Regime type has been examined — if at all — as an independent predictor of civil conflict onset yielding the following findings: regime type, measured with the Polity IV index, seemed to exert an inverse u-shape relationship on armed conflict onset, whereby regimes in the middle categories — often called anocratic regimes — were found to be more likely to experience armed conflict onset than autocratic or democratic regimes (cp. Hegre et al. 2001; Fearon and Laitin 2003). However, Vreeland (2008) convincingly showed that this effect was due to the fact that — by definition in Polity IV — anocratic regimes are often politically instable, thus making the alleged relationship between anocracy and conflict tautological. When removing this aspect of political instability for the coding of anocracies, anocratic regimes are no more likely and democratic regimes no less likely to experience armed conflict onset than other regimes (Hegre 2014), wherefore no specific regime type effect was found. This book contributes to the research on regime types in that it

shows that the effect of political exclusion and transnational links varies for non-competitive/autocratic and competitive/democratic regimes. One argument is that large excluded groups only have a strong incentive and a numerical advantage of violently mobilizing against the government in non-competitive regimes. By contrast, in competitive regimes, large excluded groups can realistically hope to win the next election. Small groups might miss out in both regimes alike, although it is unclear whether these groups — given their small size — have the potential to violently challenge the government without external help.

In sum, there is no study at present — to my knowledge — that makes an argument about a social diversity measure that accounts for all three dimensions of ethnicity, religion and class-based ideology and additionally takes the political exclusion patterns along all three lines into account, thereby being able to predict civil conflict of any kind, whether ethnic, religious or class-based ideological. Accordingly, this book intends to fill this gap. It conceptualizes ethnicity, religion and class-based ideology as three latent dimensions within one identity space, where political entrepreneurs — whether the government or the rebel group — can position themselves. The self-definition of the respective entrepreneurs comes along with certain patterns of political inclusion and exclusion, which give rise to rebellion in more than one dimension. However, the effect of exclusion on armed conflict crucially depends on the type of the political regime.

2.2. Patterns of Favoritism and Conflict

This section provides ample empirical case study evidence showing that both political favoritism and the violent mobilization of rebel groups run along all three lines of identity — ethnicity, religion and class-based ideology — often even exhibiting a combination of two or three dimensions. Furthermore, there is good reason to assume that there is neither a regional pattern nor a time trend in the latent dimensionality of political competition implying that ethnicity, religion and class-based ideology can be assumed to be latently present in all societies all the time. With these arguments being established in further detail in the following paragraphs, the subsequent chapter develops an informal strategic theoretical underpinning concerning why we observe these identity-based patterns of political favoritism and violent mobilization in the first place.

Favoritism and Conflict along Ethnic Lines

Favoritism and mobilization along *ethnic* lines has hugely been discussed in the African context (Arriola 2009; Padró i Miquel 2007; Posner 2005). Typically, one or more ethnic groups tend to be in power favoring co-ethnics via the distribution of governmental posts and assets, while other ethnic groups are politically excluded. Ethnic groups vie to get into office to secure and distribute governmental spoils (Bates 1983; Posner 2005; Fearon 1999; Caselli and Coleman II 2006), via either electoral (see Posner 2005; Chandra 2007), or non-electoral means (see Wimmer et al. 2009; Cederman et al. 2010). Private goods are distributed in the form of road development (Burgess et al. 2011), education and health policies (Franck and Rainer 2012; Kramon and Posner 2013) or — most importantly — via governmental posts (Padró i Miquel 2007). Prominent African examples of an ethnic group controlling the government and favoring co-ethnics are Bemba in Zambia, Ovimbundu in Angola, Kikuyus and Kalenjin in Kenya, Northern groups in Nigeria and Uganda, Tutsis in Burundi, M'Boshi in the Republic of the Congo and Tigreans in Ethiopia. However, ethnic favoritism is by no means constrained to the African continent. Examples of ethnic dominance outside Africa include Sinhalese in Sri Lanka, Pashtuns in Afghanistan, Bengali in Bangladesh, Burmese in Myanmar, Whites in Bolivia, Persians in Iran, Turks in Turkey and Serbs in Yugoslavia (cp. Chandra 2007, 84; Horowitz 1985).

While case study examples are abound, quantitative evidence that governments tend to be captured by ethnic groups and that these groups actually favor their co-ethnics is more scarce. Indirect evidence for ethnic favoritism has been found in early quantitative studies examining the relationship between ethnic diversity and economic and political performance like public goods provision, with highly diverse countries performing rather poorly (see Easterly and Levine 1997; La Porta et al. 1999; Alesina et al. 2003; Alesina and Ferrara 2005; Habyarimana et al. 2007; Desmet et al. 2009; Casey and Owen 2014). Direct evidence has been provided in recent studies with the availability of data on the identity of political leaders and the dominant ethnic composition of the executive. These studies explicitly show that ethnic groups actually benefit from having their co-ethnics in power: road development in Kenya seems to be driven by ethnic favoritism (Burgess et al. 2011), and an ethnic political leader positively affects primary education and infant health of its co-ethnics as examined for 18 Sub-Saharan Africa countries over the last 50 years (Franck and Rainer 2012; also see Kramon

and Posner 2012, 2013).⁴

Given the politics of ethnic favoritism, excluded ethnic groups have an incentive to challenge the ethnic group in power, via either electoral or non-electoral means.⁵ Ethnic clientelism during elections has been documented by Posner (2005), Chandra (2005), Wantchekon (2003), and Cederman et al. (2013). The mobilization of excluded ethnic groups in rebellions has been shown in various case studies (see Collier and Sambanis 2005 or the seminal study by Horowitz 1985). To relate back to the examples above, almost every ethnic group in power has experienced counter-mobilization by excluded ethnic groups like by Lozi in Zambia, Bakongo and Cabinda in Angola, Luo in Kenya, Southern groups in Nigeria and Uganda, Hutus in Burundi, Lari in the Republic of the Congo, Afars, Amhara, Oromo, and Somalis in Ethiopia, Tamils in Sri Lanka, Hazaras and Tajiks in Afghanistan, Biharis and Chittagong Hill Tribes in Bangladesh, Kachins, Karens, Mons and Shans in Myanmar, indigenous people in Bolivia, Kurds in Iran and Turkey and Bosniaks and Croats in Yugoslavia. Furthermore, quantitative examinations reveal that the size of the excluded ethnic population or the size of an ethnic group positively predict the onset of civil conflict (Wimmer et al. 2009; Cederman et al. 2010).

Favoritism and Conflict along Religious Lines

The importance of religion in politics has become an issue in recent years (Thomas 2000; cp. Philpott 2007), with the role of religion in conflict being one aspect of it, whereas religious favoritism has received much less

⁴Adverse results on ethnic favoritism can be found for taxation in Kasara (2007) and infant mortality in Guinea in Kudamatsu (2009). However, see the cautionary note by Kramon and Posner (2013) concerning any conclusions when focusing on one private good only.

⁵The question of whether governments are indeed captured by certain ethnic groups providing spoils to only the core constituency (see Cederman et al. 2010) does not yet seem to be completely settled. Francois et al. (2012) find evidence for a proportionate representation of ethnic groups. Alubo (2008, 51) considers ethnic favoritism to be mere window-dressing stating that “politicians refer to unequal representation, and seem to manipulate the view and use it strategically — in the end, however, both sit together and share the spoils”. By contrast, other authors attribute the presence of several ethnic groups in government to the cooptation of single individuals, but not to real group representation (see Faksh 1984, 147). Moreover, still others hold that the core constituency of one ethnic group can still be convinced that it receives most goods of all, despite the presence of other ethnic groups in government (see Chandra 2007, 101).

attention.⁶ In religious conflicts, often different religious groups oppose each other, but sometimes it is also a secular government versus a religious uprising, or liberals versus radicals of the same denomination (see Blaydes and Linzer 2012; Stewart 2009, 3).⁷ Religious favoritism seems to come in a similar vein as ethnic favoritism: political and military posts are distributed to co-religious groups and the government budget flows to co-religious people, often via regional development projects or education programs that favor the respective religious groups over the political exclusion of other religious groups (cp. Grim and Finke 2011, 49f., 51, 207; Toft 2006, 19; Akbaba 2009). Prominent examples of regimes of religious favoritism are the prevailing cases of the Alawite Syrian regime or the Sunni governments in Bahrain, Saudi Arabia or under Hussein in Iraq. Other examples include the Northern Muslim governments in Nigeria and Sudan, the Buddhist governments in Thailand and Sri Lanka, the Hindu government in India, the Jewish government in Israel or the Protestant Northern Ireland government.

In all these cases, absent other channels of political contestation, religious favoritism led to the violent uprising of the excluded religious groups, mobilizing the respective population with the explicit goal to control the government. Thus, Muslim Sunnis stood up in Syria, Shiites in Bahrain, Saudi Arabia, and Iraq, Southern Christians in Nigeria and Sudan, Southern Muslims in Thailand, Hindus in Sri Lanka, Muslims in India and Israel and Catholics in Northern Ireland. To take up the Syrian case as an example of religious favoritism and violence, the Alawite religious minority constituting 12 percent of the population, once excluded, succeeded in entering the Syrian army in substantial numbers in the 1960s. By staging a military coup, it was able to control the government and completely reshuffle the composition of the government, appointing a high number of Alawites in key political and military positions and channeling substantial resources to the Alawite region (King 2007, 454; Faksh 1984, 146). This preferential treatment triggered the opposition of the Sunni majority, leading to violent uprisings against the Alawite government during the 1970–1980s (Muslim Brotherhood), and again from 2011 onwards (Svensson 2013, 418, 422f.). Despite these numerous case study examples, quantitative data on religious

⁶One exception is Stewart (2009) analyzing the issue of religious favoritism under the terminology of political as well as social, economic and cultural horizontal inequalities. See also Henne (2012, 755f.).

⁷The relationship between state and religion is complicated by the fact that religious actors often emerge as a very potent, independent force, wherefore one has to understand the special relationship between the state and the church (cp. Finke and Martin 2012, 12).

favoritism is scarce (exceptional is Grim and Finke 2011, who code religious favoritism as one category in their data, but do not explore it in much detail), instead privileging the issue of religious discrimination (Grim and Finke 2006; Fox 2013).⁸ To my knowledge, there are no quantitative studies explicitly examining the relationship between religious discrimination/favoritism and the provision of public goods or armed conflict onset.⁹

Favoritism and Conflict along Class Lines

Class-based ideological favoritism in developing countries typically sets landowners and capitalists on one side against peasants and workers on the other side.¹⁰ Both groups seek to distribute state assets among themselves (cp. Scott 1969; Acemoglu and Robinson 2005).¹¹ If landowners/capitalists are in power, typically under the banner of right-wing politics, we generally observe politics dominated by a *landed elite* facing a huge impoverished and uneducated peasant population (very typical in Latin America like in Brazil, Nicaragua, El Salvador, Colombia, and Guatemala (see Booth 1991), as well as in the Philippines (Holden 2009; Quimpo 2005, 2009), Nepal (Misra 2002; Joshi and Mason 2011), and South Africa (Ntsebeza 2011)), or politics dominated by a *capitalist elite* colluding with state bureaucrats while exploiting the labor force of the urban poor (exemplary are Iran,

⁸Religious discrimination encompasses restrictions on buildings (most prominently, the construction of mosques and churches), punishment of conversion, mandatory education in the religion of the majority, confiscation of property, exclusion from the social pension system, etc. (see Fox 2013, 460ff.). Religious favoritism in Grim and Finke (2006) “refers to the actions of the state that provide one religion or a small group of religions special privileges, support, or favorable sanctions” (Grim and Finke 2006, 15f.).

⁹Many studies focus on the relative frequency of religious conflicts and their duration or intensity (see Lindberg 2008; Tuscisny 2004) or on the relationship between religious fragmentation/polarization and performance/conflict (see Montalvo and Reynal-Querol 2003; Akdede 2010; Dincer 2008). Religious conflicts have also been indirectly examined under the heading of ethno-religious violence.

¹⁰Matters are complicated by the often dependent relationship between landlords and peasants, whereby landlords have extensive monitoring and sanctioning capacities over peasants thereby preventing a violent uprising (see Joshi and Mason 2011; Schmidt et al. 1977, 305).

¹¹While rich people certainly do not have the same propensity for the provision of pork like poor people (“a dollar is not worth the same”, see Calvo and Murillo 2004; Magaloni et al. 2007, 187; Dixit and Londregan 1996), rich people still have a strong interest in government favoritism in the sense that their wealth is preserved. Moreover, direct payments have been documented at least for middle-income people in Mexico (see Magaloni et al. 2007).

with a period of accelerated capitalist industrialization between 1965 and 1975 (Petras et al. 1981, 60ff.), South Africa from 1958 onwards (Petras et al. 1981, 56ff.), or Brazil under the military dictatorship from 1964 to 1985 (Petras et al. 1981, 50ff.)). In either case, government resources are distributed among a small group of people based upon class criteria. By contrast, if peasants/workers come into power, typically under the banner of left-wing politics, we observe land distribution to the poor, micro credit programs, low interest loans, the nationalization of firms, the granting of labor rights and extensive public education programs — in actuality that assets are distributed among the rural and urban poor masses (exemplary are Chile under Allende (1970–1973), Brazil under Goulart (1961–1964), Iran under Mossadegh (1951–1953), Syria under Baath (1950–1960s) and Thailand under Thaksin (2001–2006)). Quantitative evidence for whether inequality impairs good governance is mixed (see Easterly 2007 vs. Casey and Owen 2014). However, the tendency of right-wing governments to favor a small wealthy elite and of left-wing governments to empower the poor is confirmed in quantitative analyses showing that inequalities tend to reduce under left-wing governments (Ha 2012), as exemplified in recent years for several leftist Latin American countries (Ortiz and Cummins 2011, 27).¹²

Depending on which side is in power, the other side tries to mobilize forces to topple the government: exemplary are the leftist insurgencies in Nicaragua (1977–1979), El Salvador (1979–1991), Colombia (1964–), Guatemala (1963–1995), the Philippines (1946–1954, 1969–), Nepal (1960–1962, 1996–2006), South Africa (1981–1988) and Iran (1979–2001) (see Booth 1991; Parsa 2000), as well as violent topplings and uprisings through rightists — often with the help of the military (see Sklar 1979, 540f.) or external forces, like in Chile (toppling of Allende in 1973), Brazil (toppling of Goulart in 1964), Iran (toppling of Mossadegh in 1953) and Thailand (toppling of Thaksin in 2006). Quantitative examinations of whether the uneven distribution of income or land increases the risk of rebellion long found only mixed support, probably due to poor data (see Russett 1964; Muller 1985; Weede 1986; Lichbach 1989; Collier and Hoeffler 2004; Fearon and Laitin 2003; Besançon 2005).¹³

¹²Once the poor get to power and have access to wealth, they sometimes tend to change their ‘ideology’ from leftist to rightist — see Mbeki in South Africa (Vale and Barrett 2009) — although this is not necessarily the case.

¹³For a skeptical view that vertical inequalities matter for conflict, see Esteban et al. (2012, 1310): “The clear economic demarcation across classes is a two-edged sword: while it breeds resentment, the very poverty of the have-nots militates against a successful insurrection, and even then the different skill and occupational niches occupied by capitalist and worker makes effective redistribution across classes a

However, a recent study by Bartusevičius (2014) uses much better data on income inequalities (Solt 2009) and educational inequalities (Benaabdelaali et al. 2012) and finds that inequality increases the risk of civil conflict.

Multidimensionality of Favoritism and Conflict

As documented thus far, case studies suggest and quantitative studies presuppose that ethnic groups in government face ethnic rebellions, religious groups face religious rebellions and rightist/leftist groups face leftist/rightist rebellions. While this ‘same-dimensionality’ of favoritism and rebellion might be true for some cases, it is not true for all cases. Different empirical examples show that government favoritism can run along one dimension and rebellion along another. Exemplary are the Islamic government in Iran (under Khomeini) opposed by a predominantly leftist insurgency (MEK) (Svensson 2013, 465), the Socialist regime in Ethiopia (under Derg) faced by several ethnic rebel groups (Oromo, Tigray, Amhara), or the ethnic regime of Acholi and Langi in Uganda (under Obote) opposed by a religious movement (NRA). Moreover, case studies suggest that different identity dimensions are frequently combined, in both favoritism and rebellion. Thus, the Syrian government defined itself not only in religious terms (Alawite) but also in class terms (Leftist), the Sri Lankan government not only referred to its distinct ethnic identity (Sinhalese) but also to its religion (Buddhist), and the Mexican government under the PRI was not only characterized by its ideological stance (Centrist) but also by its ethnicity (White). Rebel groups likewise refer to categories from different dimensions constituting themselves as ethnic-ideological (FAR/URNG in Guatemala), ethnic-religious (VRS in Bosnia) or religious-ideological (NSF in Romania).

Quantitative studies have long recognized that rebel groups can often not been coded along one dimension only, rather exhibiting the characteristics of many (Fearon and Laitin 2003; Fearon 2003). This book’s descriptive data likewise supports a multidimensional nature of favoritism and rebellion: of all non-democratic governments, 34% were ethnic, 9% ideological (left-right) and 54% were ethnic-ideological (the remainder being ethnic-religious, religious-ideological, or ethnic-religious-ideological). Of

more indirect and difficult prospect.” In a similar vein, some studies have turned to horizontal, instead of vertical inequalities, largely focusing on the inequality within and between ethnic groups (Baldwin and Huber 2010; Stewart 2009; Østby 2008; Cederman et al. 2011; Cederman et al. 2013; Kuhn and Weidmann 2013; Huber and Mayoral 2013).

all rebel movements, 28% were ethnic, 2% religious, 17% ideological, 5% ethnic-ideological, 26% ethnic-religious, 3% religious-ideological and 3% ethnic-religious-ideological (with 17% being nothing at all). Despite its prevalence, the multidimensionality of favoritism and conflict has almost completely been neglected in the formal, quantitative and analytical case study literature. Given this lack of attention, the contribution of this book is to take all three identity dimensions — ethnicity, religion and class-based ideology — simultaneously into account. My argument is that all three dimensions are *latent* in every society all the time and can be activated by political entrepreneurs instrumentally for political purposes thus rendering one or the other dimension *salient*. Trends in saliency might be due to differing underlying demographic characteristics or different outside support patterns. However, the assumption of three latent dimensions presupposes that there is neither a regional pattern nor a time trend: an objection one might raise at first sight, but that has to be declined upon further inspection, as the following paragraphs show.

Regional Pattern

One might object that certain dimensions are present in some regions, but not in others, with the class dimension being confined to Latin America, the ethnic dimension to Sub-Saharan Africa and the religious dimension to the Middle East. This possibly Western European perspective might arise from the fact that Latin America is associated with leftist revolutions and governments, Sub-Saharan Africa is known for its ethnic political competition and civil conflicts, and the Middle East attracts much attention for the role of religion in politics, like in the recent uprisings with a prominent role of the Muslim Brotherhood in Egypt, an Islamist government in Tunisia, Sunni Islamist violence in Libya and Syria, as well as Shia-Sunni struggles in Iraq, Saudi Arabia and Bahrain (Fox 2013, 408). However, I argue that this view is misleading. While it might be true that certain dimensions are more salient in some regions than others, this is not the case for the underlying *latent dimensions*. Thus, although many conflicts in Latin America definitely center on class issues, ethnicity and religion nevertheless serve as important additional dimensions. This becomes visible in the ethnic discrimination and mobilization of black, colored and indigenous people (Hooker 2005; Van Cott 2007, 129f.; Madrid 2005; do Valle Silva 2000), the debated role of the Catholic church during the military dictatorships (Gill 1994), or the recent steep rise in Protestant activism (Burdick 2005; Vásquez and

Williams 2005; Freston 2004). Likewise, it holds for Sub-Saharan Africa that despite the prevalence of civil conflicts along ethnic lines, class and religion are also important dimensions. During the Cold War, often due to superpower support, many African governments and movements took a socialist or a capitalist stance¹⁴ (cp. Bates 1981), while religion intermingles as a potent force in many of its conflicts (see Basedau et al. 2011), like in the recent case of the Ivory Coast (cp. Stewart 2009, 25).¹⁵ In the Middle East/North Africa (MENA) region, the prominence of religion unjustifiably hides ethnicity and class dimensions. For one, there are numerous well-organized *ethnic* groups in the MENA region like Kurds in Iraq and Iran, Baluchis in Iran, Palestinians in Israel, Lebanon, and Jordan, as well as Berbers in Algeria and Morocco, the political discrimination of which has been answered with revolt (Asal et al. 2012). For another, class has been a crucial force in the recent Arab Spring uprisings and before: starting in the 1970s, former socialist regimes slowly re-transformed into liberal, capitalist societies empowering economic and landed elites vis-à-vis the excluded lower and middle class (King 2007; Kandil 2012, 203f.). The severe rent-seeking behavior of a minor upper class — expressed in low price purchases of national firms, loans without guarantees, an upward distribution of land and the positioning of businessmen in political office (see King 2007, 439ff. for Egypt, Algeria, Tunisia) — resulted in severe inequalities and alienated huge parts of the population, certainly contributing a great deal to the recent uprisings (cp. Campante and Chor 2012). Disregarding this class aspect during the Arab Spring and before seems to be ‘indefensible’ (Kandil 2012, 198).¹⁶

¹⁴While the class issue became important with some retard, due to colonial politics fostering ethnic rather than class differences (see Posner 2005), during the Cold War, class came to divide Socialist regimes like Guinea, Guinea-Bissau, the Congo People’s Republic, Angola, Mozambique, Tanzania, Somalia and Ethiopia (see Sklar 1979) on the one hand, and Capitalist regimes with an emerging indigenous owning class like Nigeria (Kieh and Agbese 1993), Kenya (Gordon 1995), Uganda (Fattou 1988) and Zambia (Baylies and Szeftel 1982) on the other hand.

¹⁵Studies focusing on the prevalent instrumental purpose of ethnicity during elections nevertheless recognize the presence of class and religion as additional important dimensions (cp. Eifert et al. 2010).

¹⁶Note that from 1975 to 1980, only 7 per cent of armed conflicts in the MENA region had a religious incompatibility (see Svensson 2013, 419).

Time Trend

A second objection might be that certain dimensions are present at some time point but not at another. Thus, one might suspect that the left-right divide was prominent during the Cold War, ethnicity came to the forefront in the 1990s, and religion emerged in the 2000s (see Brubaker and Laitin 1998, 424f.; Huntington 1993, 26, 29; Gartzke and Gleditsch 2006, 55f.). Some stylized facts seem to support this view: indeed, many ideological conflicts were terminated with the end of the Cold War, like in Honduras in 1990 or Guatemala in 1996 (see Kalyvas and Balcells 2010; Kanet 2006, 342) or leftist movements became a right-ward shift like in South Africa (Bauer and Taylor 2011, 259f., 280f.), thus nurturing the view of declining leftist violence. Instead, numerous ethnic conflicts like in Rwanda, Congo, Yugoslavia, Kosovo, Chechnya, and Sri Lanka came to dominate the media in the 1990s, whereas religious favoritism and violence has received increasing attention with the recent Middle Eastern uprisings, as documented above. However, the perceived trend of religious violence replacing ethnic violence replacing ideological violence is most likely due to a changing focus in media and scholarly attention (see Fox 2013, 407f.; Fox 2004, 717f.). Ethnicity, religion and class have been present all the time, at least as underlying latent dimensions. Although it might be less beneficial nowadays to call oneself Marxist as a rebel group, class-based differences giving rise to leftist/rightist dichotomies remain as topical as ever. There remain leftist groups challenging governments like in Venezuela (1992: Chavez), Turkey (1991: Devrimci Sol), Russia (1993: leftist forces), Nepal (1996: Maoists) or Colombia (1946–: Farc, ELN, EPL, M-19), and the continued or even increasing inequalities between rich and poor do not suggest a reversing trend.¹⁷ Moreover, in-depth analyses of the Middle Eastern violence will certainly contribute to a revival of the class issue. Unsurprisingly, scholarly attention has very recently refocused on class-based differences as a potent mobilizing force (see Bartusevičius 2014).

While class thus continues to be a relevant dimension after 1990, ethnicity already played a crucial role well before 1990, as amply documented. Authors have found that the share of ethnic conflicts started to increase since the

¹⁷Although inequality in Sub-Saharan Africa and Latin America slightly decreased between 1990/2000 and 2008, it remains huge. In other regions, such as Eastern Europe and Asia, inequality is even increasing (see Ortiz and Cummins 2011). Note that conflict-ridden Colombia, Nepal, Russia and Zambia have the highest level of inequality.

1960s and even declined in the mid-1990s (see Gurr 1993), thus rendering the Cold War a “non-significant determinant of ethnic war onset” (Sambanis 2001, 275). Similarly, the political relevance of religion has not been constrained to the very recent years. Although there might be a “slight increase of people who believe in God” (Grim and Finke 2011, 203), religious conflicts existed well before 1990 (Fox 2004). Thus, if there was any time trend at all, this effect is certainly not due to shifting latent dimensions. At most, temporal changes in the *saliency* of dimensions might be due to changing governmental favoritism or changing transnational support patterns rendering one dimension more salient during one period than another (see Kanet 2006, 342).

To conclude, this chapter has established that ethnicity, religion and class-based ideology are the three latent political dimensions present in all regions of the world for all the time since 1945 (at least). The next chapter develops the argument that political entrepreneurs position themselves along these dimensions based upon strategic calculations, thus rendering one or the other — or even a combination of dimensions — salient.

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