Using Machine Learning To Predict Conflict: Toward an unified theory of civil conflict?

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Abstract: *Current approaches to understanding conflict ignore the possibility of out of sample prediction. This is a significant evidence gap in the study of civil conflict. A good scientific model should not only explain, but also predict conflict. We suggest that machine learning techniques can predict while avoiding many of the missing data, rare event, and endogeneity problems that plague parametric estimation. This paper shows how machine learning can help choose between different theoretical causal models of conflict by testing the predictive validity of a theoretical model. Thus machine-learning can help ascertain the public policy levers that are most predictive and therefore most effective. We find that rational choice models of bargaining failure, i.e. models focused on the strategic interaction between rational individuals (a distinctly methodologically individualistic approach) seem to be the best predictor of civil conflict.*

Introduction

Steven Pinker in his masterful "The Better Angels of Our Nature" makes the compelling argument that we may be living through some of the most peaceful times in human history and possibly prehistory. So, why this paper? Let us ignore the incessant and depressing notifications of violence from various news media outlets and "friends" on our wired devices for a moment and look at some stylized facts. The Center for Systemic Peace, a non-partisan think tank, has an informative chart on its website (Center for Systemic Peace, 2016) that shows global trends in armed civil conflicts from 1946 to 2015. During this time, interstate war declined steadily but societal warfare (civil war) increased steadily till the 1990's, then declined. Further, the incidence of civil war throughout this time was much higher than that of interstate war. At the same time, the most recent data on the website shows that at least 10% of all states are in some kind of civil conflict. The number of terrorist attacks also appears to be increasing since 2010, after showing steady declines since 1980 (Bang, Basuchoudhary, David, and Mitra, 2016). Moreover, the proportion of noncombatant deaths to combatant deaths remains quite high. Possibly as a consequence, close to 65.3 million people have been displaced from their homes. To put this in perspective, that's almost double the population of Canada. Thus civil conflict, Dr. Pinker's optimism notwithstanding, remains a peculiarly human tragedy.

Our goal is to be able to *predict* civil conflict so policy makers can reduce human suffering. We do not care to be caught in the many controversies over the precise definition of civil conflict: the finer distinctions between what constitutes a guerilla war and an insurrection, for example or civil conflict onset and persistence.[1] Therefore, we adopt a broad definition of civil conflict, one that includes (civil conflict-related) deaths from political wars, ethnic wars, regime change, genocides, and politicides, based on data from the Political Instability Task Force (PITF). According to the PITF, if any one of these events happened in a country in any given year, the country is in civil conflict. The PITF uses certain death thresholds to categorize whether a country is in civil conflict or not. However, they make no distinctions on whether, for example, those deaths were the result of an insurrection or a terrorist attack. In addition, PITF considers a country to be in civil conflict if there was an adverse regime change, or if the state targeted a group because of its communal or political characteristics. Thus, our definition of civil conflict includes almost all aspects of civil conflict, short of interstate war.

To further our goal of predicting civil conflict in order to prevent or mitigate its adverse effects, we need to understand the driving forces behind civil conflict. To this end, we assemble a dataset that includes a wide range of economic, political, social, demographic, and geographic variables comprising the "usual suspects" in the civil conflict literature. We try to find links within these data that gives us the best predictions. Machine Learning algorithms leverage this dataset to predict the likelihood of any civil conflict over a five-year period. For example, data in 1990 is used

[1] Though as we will note, civil conflict persistence plays a major role in civil conflict prediction.

to predict whether civil conflict occurs in the period 1991-1995. These give us the material to begin shaping a truly predictive theory of civil conflict.

There is a varied literature devoted to civil conflict. This literature, though vast, does not yield a unique theory of civil conflict. Of course it is possible that different civil conflicts happen for different reasons, so that there can be no one theory of civil conflict. Nevertheless, so far, no one has even asked whether there is a unifying theory of civil conflict, or even if there are a few "umbrella" theories. This paper addresses this question.

We introduce Machine Learning to help understand whether there can be a unified theory of civil conflict by helping to choose between competing models of civil conflict. At the most basic economic level, the question is one of production or predation. After all people can get what they need either by making what they need and trading with others, or by taking what they need. The former requires specialization and trade and has the potential to make humanity better off. The latter is more of a zero sum game in the short run, and may make everyone worse off in the medium to long term. Stated as baldly as this, war appears to be irrational. Nevertheless, it is not. Yet we do not have a unified theory explaining this crucial, apparent contradiction. A number of economic *models* and corresponding evidence suggest that war is quite rational. However, we do not have a *theory* of civil conflict that yields this conclusion. The reader will notice that we are making a distinction between *models* of civil conflict, and a *theory* of civil conflict. Economists explicitly use models as part of a toolkit to investigate how and why humans make decisions. This is standard procedure. This is certainly true for the civil conflict literature, probably more so than other areas of economic investigation given the complex nature of civil conflict. So what we have is an array of models of civil conflict. Which of these predict civil conflict the best? Investigators have hunches but do not know the answer in any validated or cohesive way. Of course, if no choice is possible then we are left with the conclusion that civil conflict may not have unifying principles. On the other hand, if we can identify some unifying principles of civil conflict by identifying a particular class of theoretical models as having greater predictive power than others, we can develop the basis for a causal and predictive theory of civil conflict. This is not just an academic issue: Policy makers also can benefit from predictive models of civil conflict. They are, above all, concerned about the effect of a policy change on the likelihood of civil conflict. It stands to reason that they would like to choose a policy lever that has the biggest impact on civil conflict.

This brings us to our focus on prediction: We want to identify *variables* that best *predict* civil conflict. We do not use the word prediction lightly. Most statistical models of civil conflict try to identify the correlates of civil conflict and, in some cases, try to tease out causal patterns. But none truly predict civil conflict by asking the following question – how well do the identified correlates (causeates?) of civil conflict perform in identifying civil conflicts that *have not yet happened*? A reasonable answer to this question would identify variables that ought to be part of a unified theory of civil conflict. Such an answer could therefore jumpstart investigations into the causal patterns among these factors. After all, a theory is no good unless it can predict. Moreover, policy makers want to know the impact of their policies. At worst, they would like to eliminate policy levers - variables - that do harm, and at best they would like to implement policies that have the biggest impact on reducing death and dislocation from civil conflict.

Obviously, the effect of a policy is felt in the future. Thus, from a policy-making perspective, predictability is a key feature. We therefore argue that prediction should be at the heart of any scientific investigation into identifying underlying principles of civil conflict. Predictability remains the true scientific test of a theoretical model and it is the key to good policy as well.

How do we get a sense of a good predictive model? After all, it is impossible to test for the validity of a model by comparing it with something that has not yet happened. We can, however, *simulate* the comparison by dividing any data set into two parts. We build an empirical model by training it to perform well in one part of the data. Then we see how well this trained model performs in the other data set. If the trained model does not perform well out-of-sample, then clearly it is not generalizable at all. And if it is not generalizable then it cannot be predictive. At that point, the variables used to build the empirical model are suspect, as are the variables in the theoretical models that suggest these variables matter. In short, this approach can help eliminate non-predictive variables and therefore potentially non-predictive theoretical models. The predictive variables that remain after this process then should be explored for causal links rooted in theoretical models. To summarize, if there is indeed any general theory of civil conflict, building theoretical model structures and empirically testing those structures should be a comparative process where predictability is the margin of comparison. This process can identify the best predictive theoretical models. These "best" models are cross validated because they predict well out-of-sample, i.e., they are generalizable. Only the most validated and therefore generalizable theoretical models can wear the mantle of "theory." We suggest that this process of cross-validation should become a scientific standard in the literature on civil conflict in particular, but more generally, in all data based social sciences.

The approach we suggest leaves us open to the charge of data mining. If the charge is that we let cross-validated evidence guide us toward a theory of civil conflict, we are guilty. So are Galileo and Copernicus. They started with an Aristotelian theoretical model of the universe. But that model did not predict the observed movement of the planets. Those observations changed the theory of earth's place in the universe – and history. We do not claim the hubris of changing history. We merely suggest that scientific cross-validation should guide civil conflict theory. In any case as scientists, Galileo and Copernicus are good company.

Our goal, then, is to identify the viable and consistent predictors of civil conflict. In the process, we hope to identify a unified theory of civil conflict that Policy makers can use to understand and predict civil conflict. In short, we want to predict civil conflict in order to prevent it. We use Machine Learning technology to meet these goals.

Existing cross-country analyses of civil conflict often provide civil conflicting results, even as the literature appears to converge on a few consistent correlates of war. These conflicting results are rooted in inconsistencies in the way variables are interpreted, in coding and measurement problems, as well as ad hoc specifications developed from competing theoretical models. Further, most of these empirical studies do not provide any evidence on which of these competing theories best explain civil conflict. Thus, clear theoretical causal links supported by evidence remain elusive. We use *theoretically agnostic* Machine Learning techniques to identify predictive patterns. Machine Learning identifies these predictive patterns by first identifying modeling technologies that are best at out-of-sample prediction. Some of these technologies can also identify the variables with the most predictive powers. In the process, they eliminate those that have weak or low predictive powers.

If we are to use a theoretically agnostic framework, how do we decide which variables to include? We choose an inclusionary method: we use all the variables that are in competing theoretical models. This has an added benefit, since Machine Learning techniques consistently eliminate some variables as being not very predictive, we should also eliminate the theoretical approaches that suggest those variables matter for civil conflict. To reiterate, in our telling, non-predictive theoretical models cannot really be part of any unified theory of civil conflict.

Civil conflict is rare. This may bias estimated marginal effects from traditional parametric estimation techniques. It may also make it difficult to interpret the meaning of the marginal effects when the explanatory variables are correlated with one another. For example, parametric models may be over-fitted and still return significant marginal effects for explaining civil conflict. Moreover, parametric estimates are also misleading because of the tangled causality between civil conflict and its correlates. This tangling happens for a couple of reasons. Often causality may run both ways: Low GDP may cause civil conflict but civil conflict may also result in low GDP. Further, there may be an omitted variable that has an effect on both GDP and civil conflict.

The civil conflict literature is particularly rife with such endogeneity problems, leading to biased and inconsistent parametric estimates of the marginal effects of explanatory variable on the likelihood of civil conflict. In fact, even advanced econometric techniques designed to mitigate these problems fail to solve them largely because the parametric estimates generated in these models are sensitive to model specifications. These parametric estimates therefore do not provide clear guidelines for deciding which variables matter for civil conflict. But efforts to identify the marginal effect of the change in one variable or another have spawned a veritable small cottage industry in academic papers. While this may be part of the "sausage making" process in science, it offers scant comfort to the policy maker. Policy makers cannot be confident of the effect of a policy change in one or the other variable on the likelihood of civil conflict. All this suggests the need for a mechanism to search for a more parsimonious list of explanatory variables.

Machine Learning techniques can develop parsimonious cross-validated lists of predictive variables. We delve into this variable-winnowing process at two levels. First we use factor analysis methods to extract the variation from different, correlated variables, so as to identify specific classes of institutional variables. For example, contract enforcing economic institutions and democratic accountability share the characteristic that together they make state institutions inclusive and credible. Factor analysis can extract these elements of commonality. At the second level, we use various Machine Learning technologies to identify the most predictive among these factors and other variables, and winnow out the non-predictive variables. Together, these address some of the overfitting problems and help policy makers identify *predictive* policy levers. At the same time, to the extent that we do not estimate parameters, the issue of biased or inconsistent parameters does not arise freeing us from some of the econometric problems raised by endogeneity and the rarity of civil conflict. Therefore, to the extent that these techniques develop a parsimonious list of *predictive* variables, we can offer the academic a cross-validated sense of which theoretical models predict well, and therefore light a path toward a potential unified theory of civil conflict.

Spotty data, particularly from civil conflict torn regions, plagues the civil conflict literature. Further a lot of the data has subjective roots and may suffer from measurement errors. But the predictive element of Machine Learning techniques helps address some of these concerns as well. We use sophisticated, and *validated* data imputation techniques to plug data gaps. This process allows us to retain much of the "good" information we would have thrown out because of missing observations. Moreover, poorly measured variables should not be able to predict well. So, if variables predict well, we should be confident about the lack of measurement errors.

Our discussion here may read like a breathless love affair with Machine Learning. We are very aware of the limitations of Machine Learning. First and foremost, predictive models are not the same as causal models. Our models say absolutely nothing about causation. What use, then, are our predictions? Even though we cannot identify or establish causality, our results help the investigator identify potential pathways for causality through *further* theoretical modeling and traditional econometrics. We suggest that Machine Learning ought to be part of an *iterative* process towards a unified predictive theory of civil conflict. But it is only one part of the process. In addition, the process needs theoretical modeling and traditional econometrics. In this paper, we describe a first step in this iterative process. We have assembled a dataset that includes, political, economic, institutional, demographic, and geographical variables that represent a wide array of the sorts of variables usually identified as correlates of civil conflict. In this paper, we will first rank the most important variables that predict civil conflict, and then explore what these identified variables imply for our understanding of civil conflict – can there be a unified theory of civil conflict?

We present our data in section 1 and methodology in section 2. In section 3 we discuss some of the seminal models of civil conflict. In section 4 we present our results to make our case for the variables and theoretical modeling approach that have the most predictive power. We conclude soon after.

Section 1. Data

In this section, we present and describe the variables in our model. All our variables are drawn from six databases, the 2014 Cross-National Time-Series data archives (CNTS), the 2012 Database of Political Institutions (DPI), the International Country Risk Guide (ICRG), the Political Instability (formerly, Civil conflict) Task Force (PITF), the Polity IV and the Standardized World Income Inequality Database (SWIID).

Our data set begins in 1950 and run through 2014. We use data on political and ethnic war, adverse regime changes, and genocide and politicide from the PITF database to define our dependent or target variable. Thus, Civil conflict = 'civil conflict' if there is civil conflict in the period t+1 to t+5, when the corresponding predictor variables are from year t. So, for example, if GDP per capita et al is from 1990, Civil conflict = 'civil conflict' if there was civil conflict in the period 1991-1995. Given the way we define our target (dependent) variable – whether there was conflict in any one of five "following" years – our dataset is effectively 1950-2009. Thus, the first period contains predictors for 1950-1954 to predict conflict in the 1955-1959 period; the last period contains predictors from 2005-2009 to predict conflict in the 2010-2014 period.

For most intents and purposes we have "up to" 202 political entities in our dataset. For the years 1991-2014, this number is pretty stable at around 194-202. Basically, we dropped a political entity if it did not exist according to BOTH the Polity dataset (which comes from the Center for Systematic Peace, which publishes the PITF conflict variables) AND the CNTS dataset (Databanks). We chose those two datasets for the dropping rule because they weren't as susceptible to the current World Bank country lists are other sources (PWT, WDI, ICRG). In any case this extensive data set gives us a list of independent variables described below.

It is useful to sort our fifty-eight independent variables into broad categories. In order not to preempt the findings, we sort them largely according to the sources from which they are drawn, but also according to certain broad cohesive categories. These categories are ones that capture civil conflict, those that speak to the quality of political institutions, and those that portray the stage of economic development of the country. Please note that these categories are not immutable, and re-sorting the variables across the categories, or choosing other categories, does not change our conclusions.

Let's look at our independent variables, starting with what we call 'domestic civil conflict variables.' These are events where entire or subsets of the populace express a systematic discontent with a government or government policy. An extreme form of this could be riots that break out or (attempts at) assassinations of government officials. There could be less violent expressions by way of general strikes and anti-government demonstrations. Civil conflict could also be initiated by the ruling government itself: for instance, if the government begins a systematic attack on the civil liberties, or even lives, of those individuals who oppose it. To capture such civil conflict, we use seven variables. These variables are all drawn from the CNTS database, unless either wise noted:

1) LagCivil conflict: This is civil conflict lagged by one year from the start of the five-year period for which Civil conflict = 'civil conflict'. To continue the example from the definition of Civil conflict above, if Civil conflict = 'civil conflict' because there was civil conflict in the period 1991-1995 (with the predictor variables such as GDP per capita et al from 1990), then LagCivil conflict = 1 if there was civil conflict in 1990. Data is from the PITF database.
2) Assassinations: This counts the number of times there is an attempt to murder, or an actual murder of, any important government official or politician;
3) Purges: This counts the number of times political opponents, whether part of the regime, or outside it, were systematically eliminated;
4) Strikes: This counts the number of times there were mass strikes by 1,000 or more industrial or service workers, across more than one employer, protesting a government policy;
5) Government crises: This counts the number of times there was a crisis that threatened the downfall of the government, other than a revolt specifically to that end;
6) Demonstrations: This counts the number of times there was peaceful protestations of government domestic policy by 100 or more people;
7) Riots: This counts the number of times there was a violent protest by 100 or more people;

To these we add two variables that are from Hendrix and Saleyhan (2002):

8) EthnicPolariz: This is the Reynal-Querol index of ethnic polarization. What matters for this index is not the number of different ethnic groups but their relative size.
9) ReligPolariz: This is the Reynal-Querol index of religious polarization and measures the extent of religious diversity. What matters for this index is not the number of different

religious groups, but how threatened one group feels by the presence of another group, the threat being measured by the size of the other group. So, for example, it reaches the maximum value when there are 2 groups of equal size.

Next, we turn to those variables that speak to the quality of the existing political institutions. These variables capture the transparency, inclusiveness and stability of political institutions. Some of these look at the quality of the legislature and the legislative process while others look at the government as a whole.

We include five variables from the ICRG that measures political stability insofar as the system is prone to the risk of instability:

10) Government stability (0 to 12) assesses "the government's ability to carry out its declared programs and ability to stay in office." The risk rating is the sum of ratings on three subcomponents, Government Unity, Legislative Strength and Popular Support, each with a maximum score of 4 points (very low risk) and a minimum score of 0 points (very high risk);
11) Democratic accountability index (0 to 6); on the assumption that a government that is less responsive to its people is more likely to fall ("peacefully in a democratic society, but possibly violently in a non-democratic one"), the index considers how responsive a government is to its people. The measure of such responsiveness is the system of government: one subject to elections with political opponents is more accountable, more responsive, and therefore less risky. For example, a system wherein the government does not serve more than two successive terms is more accountable than one where the government can/has served more than two consecutive terms, and therefore the former is assigned higher points. Similarly, a system with a varied and active opposition is assigned a higher score than one where such opposition is limited or restricted. This index also takes into

account evidence of checks and balances between the three branches of government, how independent the judiciary is, and evidence of the protection of personal liberties; [2]

12) The investment profile index (0 to 12) assesses the risk to private investment according to how effectively contractual agreements are enforced, ease of expropriation, how easy it is to repatriate profits and the whether there are payment delays. Countries with lower risk are higher in the index;
13) The index of bureaucratic quality (0 to 4) assesses the efficiency of the bureaucracy. Countries with bureaucracies that are efficient and relatively autonomous tend to maintain stable policies and services even when governments change. They therefore are assigned higher points.
14) Corruption; The corruption index (0 to 6) focuses on the kinds of corruption, such as nepotism, bribes, etc. that if revealed, may lead to political instability such as the overthrow of the government or the breakdown of law and order. Countries with less corruption receive higher scores.

Next we look at those variables from the CNTS that evaluate the legislative process:

15) Legislative Effectiveness (0 to 3): This is ordinal scaled and measures how independent the legislative is of the executive, and therefore how effective it is. It is coded 0 if no legislature exists, 1 if the legislature is ineffective, 2 if it is partially effective, and 3 if the legislature is considered effective. A legislature may be considered ineffective under three different circumstances: if the legislature is in effect a "rubber stamp", if, because of domestic unrest, legislation cannot be implemented, and finally, if the executive prevents or impedes the working of the legislature. The legislature is considered Partially Effective when the

[2] For the specific definitions of the types of governance, please see the ICRG codebook, page 6.

executive has considerable power over the legislature but cannot outweigh it completely. Finally, an Effective legislature is one that has significant autonomy, especially in matters of taxation, disbursement and the power to override executive vetoes of the legislature.

16) Cabinet Changes: This measures how many times either a new premier was named and/or the number of times new ministers replaced fifty percent of the cabinet positions. The fewer changes, the more stable the government;

17) Executive (0 to 3): This is the chief executive selection index. It codes the way in which control of executive power went to a new independent chief executive. If there were direct elections, the index is coded to 1, 2 if the election is Indirect, and 3 if it is considered nonelective. Direct Election is when the election of the effective executive is by popular vote or by delegates committed to executive selection. When the chief executive is elected by an elected assembly or by an elected but uncommitted Electoral College, this is considered an Indirect Election. This coding is also used when a legislature is called upon to make the selection in a plurality situation. Finally, if the chief executive is chosen neither by a direct or indirect mandate, it is coded as Nonelective.

18) ParliamentResp (0 to 3): Parliamentary Responsibility. This measures the degree to which a premier must depend on the support of a majority in the lower house of a legislature to remain in office. It is coded 0, Irrelevant, when the Office of premier or legislature does not exist, 1, Absent, when the Office of premier exists, but has no responsibility to the parliament, 2, Incomplete, when the premier's constitutionally responsibility to the legislature exists but is limited, and finally, 3, Complete, when the premier is constitutionally and effectively dependent on a legislative majority for continuance in office.

19) PartyCoalitions (0 to 3) measures the number of parties in a system, and whether there are coalitions. It is coded 3 when there exists more than one party with no coalitions, 2 when

there is more than one party, a government coalition, and an opposition, 1 when there is more than one party, government coalition, but no opposition, and 0 if there is only one party, or no parties.

20) PartyExclusion (0 to 3): This looks at how inclusive the political system is by coding whether and how political parties may be excluded from the political process. It is coded in the following way: (3) if no parties excluded, (2) if one or more minor or "extremist" parties are excluded, (1) if there is 'Significant' exclusion of parties, and (0) if all but dominant parties and satellites are excluded.

21) Change in Executive: Changes in the effective chief executive measures the number of times in a year that effective control of executive power went to a new independent chief executive.

To capture a sense of the transparency, stability and inclusivity of the political and election process, we include ten variables from the DPI dataset. They are:

22) Executive Electoral Competition measured by the executive index of electoral competitiveness, EIEC (1 to 7). The EIEC measures the degree to which the selection of the executive is decided by free and fair elections. In cases where the executive is elected directly by the people, or by an electoral college which itself is elected by the people with the sole purpose of electing an executive, the score is a 7 or 6, whereas if an executive unilaterally declares a second term, the system is awarded a 2 in the second term. This index is similar to the LIEC[3].

23) Legislative Electoral Competition measured by the legislative index of electoral competitiveness, LIEC (1 to 7). The LIEC measures the degree to which the selection of

[3] For details, please see page 13 of the DPI codebook.

the legislature is decided by elections. For example, an unelected legislature gets a 2 (no legislature is 1), while a system with multiple party elections where the largest party got less than 75% of the votes gets a 7[4];

24) Legislative fractionalization captures how politically diverse a system is by looking at the number of parties participating in a regime. It is measured by the likelihood that 2 deputies picked randomly from the legislature will be from different political parties. In cases where there is no parliament or no parties in the legislature, it is recorded as NA, and is blank if any government or opposition seats are blank;
25) Government polarization that takes values between 0 and 2 depending on the ideological distance between the legislative and the executive branches;
26) Executive years in office measures the number of years the current chief executive has served;
27) The government Herfindahl index measures the degree to which different parties share in the operation of the government. It is measured by the sum of the squared seat shares of all parties in the government;
28) Fraud, or Allegations of fraud in the last election, is measured by whether claims of fraud, boycott of the elections by important opposition parties, or candidate intimidation were recorded during the election process. For example, FRAUD is 0 in countries where the opposition is banned. Less fraudulent elections are ranked higher in the measure;
29) System (0 to 3): This depicts the system of government. Systems of government that are elected are rated higher than those that are not. A Parliamentary system is rated 2, an

[4] From DPI codebook, page 13: "**Legislative and Executive Indices of Electoral Competitiveness (criteria modified from the scale created by Ferree & Singh 1999) LIEC** Legislative IEC
Scale: No legislature: 1, Unelected legislature: 2, Elected, 1 candidate: 3, 1 party, multiple candidates: 4, multiple parties are legal but only one party won seats: 5, multiple parties DID win seats but the largest party received more than 75% of the seats: 6, largest party got less than 75%: 7"

assembly-elected President is rated 1, while a Presidential system is rated 0. For instance, a system with unelected executives get a 0, while systems with presidents who are elected directly, or by an electoral college (whose only function is to elect the president) is rated 4. In systems that have both a prime minister and a president, the power of the president versus that of the parliament and prime minister is considered in the rating. For example, in such a system, if the president has sufficient veto powers, or can appoint a prime minister, or can dissolve parliament, the system is rated higher.

30) Checks: Checks on Power is a categorical variable that increases incrementally by 1 the more checks there are in the government system. For example, a presidential system with two chambers of the legislature has a higher rating by one than a system wherein the president's party has a majority in one of the houses.
31) Changes in veto power measures: This measures the percent drop in the number of players who have veto powers. The fewer the number of veto players within the government, the less open the government. For example, a presidential system with two houses will have a veto number of 3 – the president and each house. If the president gains control of the legislature, veto power drops to 1;

Finally, we include two variables from the Polity IV project:

32) Polity index is the polity 2 democracy-autocracy index that measures the institutional regime in state. It ranges from -10, institutional autocracy to +10, institutional democracy; and
33) Regime durability: This is the number of years since the most recent change in regime or the end of a period of politically unstable institutions. The more stable the region, the higher the number

Finally, we include variables that give us a sense of the level of economic development. This list includes not only the usual measures of growth and wealth, but also the degree to which the country engages in trade, and the distribution of its population and level of education. These are mostly from the CNTS database, unless noted otherwise.

34) GDP per capita: This is a nation's Annual Gross Domestic Product Per Capita at factor cost.
35) Changes in Gross Domestic Product Per Capita: This is the change in per capita GDP.
36) Exchange: This is a nation's official exchange rate, at the end of the year in local currency per US dollar. If the official rate is inoperative after 1971 the effective rate (usually the IMF market or principal rate) is used.
37) Trade: This is from the World Bank's WDI database. Trade is the annual sum of exports and imports of goods and services measured as a share of GDP, with gaps filled in using random Forest imputation.
38) Terms of trade: This is from the World Bank's WDI database. The terms of trade effect is the capacity to import minus exports of goods and services in constant prices. Data are in constant local currency.
39) Changes in Imports Per Capita: This is the percentage change in per capita imports, measured in US dollars.
40) Changes in Exports Per Capita: This is the percentage change in per capita imports, measured in US dollars.
41) PrimCommodityExports: This is from the World Bank's WDI database. It is a measure of the exports of primary commodities as a percent of merchandise exports. It is calculated using fuel exports, agricultural exports, and ores and metals exports.
42) AidAssist: This is from the World Bank's WDI database and is measured as aid assistance as percentage of GDP.

43) Area: This is from the World Bank's WDI database. Measured in square kilometers, it is a country's total area, excluding area under inland water bodies, national claims to continental shelf, and exclusive economic zones. In most cases the definition of inland water bodies includes major rivers and lakes

44) DroughtIndex: This is the Palmer Drought Severity Index (PDSI) that uses available precipitation and temperature data to estimate relative dryness. It is standardized to zero, and spans the range from -10 (relative dryness) to +10, (relative wetness).

45) DroughtIndexRange: This is the difference between the maximum and minimum values spatially for the PDSI.

46) ElectricPC: This gives a country's per capita gross generation of electricity. The data include production for both public and private purposes, and cover both thermal and hydroelectric output.

47) Gini: This is the SWIID Gini index measure of income inequality.

48) Inflation: Inflation as measured by the annual growth rate of the GDP deflator. This is from the World Bank's WDI database.

49) Investment: This is from the World Bank's WDI database. It is a country's gross capital formation as a percentage of GDP.

Variables that capture the total population, as well as where they reside are:

50) PopDensity: Also from the World Bank's WDI database, it is the number of people per sq. km of land area.

51) Populationgrowth: This measures the annual population growth rate. For year t is the exponential rate of growth of midyear population from year t - 1 to t, expressed as a percentage.

52) Urban Population. This is from the World Bank's WDI database. Urban population refers to people living in urban areas as defined by national statistical offices. It is calculated using World Bank population estimates and urban ratios from the United Nations World Urbanization Prospects.

53) Urban population growth: This is the annual percentage change in the urban population.

54) RuralPop: This is from the World Bank's WDI database. Rural population refers to people living in rural areas as defined by national statistical offices. It is calculated as the difference between total population and urban population

55) RuralPopGrowth: This is the annual percentage growth in rural population.

56) FemalePop: This is from the World Bank's WDI database. It is the percentage of the population that is female.

We capture communication with:

57) PhonesPC: This is the number of Telephones per capita, including Cellular. It is largely drawn from telephone directory listings.

We have two variables that portray education rates:

58) Secondary School Enrollment Per Capita: This is CNTS data on the number of students enrolled in secondary school per 10,000 people.

59) ChangesSecEnrollPC: This is the annual percentage change in per capita secondary school enrollment.

There is a varied literature on the role of many of the institutional variables we report above. However, many of these are correlated and therefore may together capture some unique institutional dimension. Isolating these dimensions of institutional quality may not be entirely straightforward, and in fact each dimension may depend on multiple structural characteristics of a country's institutional structure. Note that if multiple dimensions of institutional quality matter, the first method may fail due to omitted variable bias. Yet even if this is not the case, the fact that these measures often in fact capture elements of multiple concepts of institutions exposes these measures to considerable measurement error.

We combine various elements of Jong-A-Pin (2009) and Bang and Mitra (2011) in that we combine a long list of measures of institutional quality and stability into an exploratory factor analysis (EFA) to develop a general view of institutions. Not surprisingly, our results mirror these two studies, and we find evidence for six dimensions of institutional quality and stability. We use these dimensions instead of the actual institutional measures described above. These dimensions and their components are represented in Figure 1. The cell is shaded if it is a dimension for the factor. For example, Government crises, Cabinet changes and Executive changes are dimensions of the factor for Regime Instability.

Section 2. Methodology

Econometric models that estimate the marginal effects of the independent variables are inadequate because they tend to focus on the impacts at the mean (Goldstone, et al., 2010). For example, the econometric model might find significant marginal effects for variables simply because the model is over-fitted. Goldstone et al. (2010), address this problem using complicated techniques that do not lend themselves to easy interpretation. Alternatively, some scholars (Fearon and Laitin, 2003) focus on regions where instability is common, thus reducing the generalizability of their results. Last, a key reason for adopting a new approach to explaining civil conflict is that, globally, such events are rare. King and Zeng (2001) argue that using logit models to analyze rare events underestimates their probabilities of occurring and suggest a technique that generates unbiased estimates. However, it is unclear how well King and Zeng's approach captures the interactions, nonlinearities, and context dependence that plague analyses of civil conflict (Beck et al., 2001).

Methods based on single classification trees as well as ensembles of multiple classification trees address some of these issues by using an iterative algorithm to divide the data into partitions (see Hand, 1997 for details). In a single classification tree, the algorithm considers every possible binary split on every candidate variable. It then uses this information to determine how well splitting the sample at discrete values of each explanatory variable improves the accuracy of the classification. Thus, the algorithm chooses variables and partitions that minimize the total amount of variation, or "impurity," in subsequent subsamples.[5] It then splits each subsample further using the same technique into purer subsamples with the goal of building the single and best possible tree to fit the data in the learning sample. The tree therefore picks out the variables that are most likely to explain the greatest variations in the dependent variable without *a priori* assumptions about the distribution of the variables or the functional form of the relationships between them (Ledolter, 2013. p. 170).

Ensemble tree methods also seek to minimize measures of impurity, but differ from single trees in that they produce many different trees. Ensemble methods do this either by randomly selecting a subset of observations that are included in the construction of each tree – as is done in the bagging predictor developed by Breiman (1996) – or by randomly selecting a subset of the variables that are considered in the split at each node – as is done in the boosting predictor developed by Freund and Schapire (1996) and the random forest predictor developed by Breiman (2001). Once a specified number of trees have been constructed, the trees "vote" for the predicted

[5] In the case of a categorical dependent variable such as civil conflict (classification trees), impurity is measured by "classification deviance" – usually either the Shannon entropy criterion or the Gini impurity measure; in the case of continuous dependent variable (regression trees), impurity is measured using mean square error.

classification (failure or no failure) of each observation. Hence, since ensemble methods randomize the choice of classification *even within the learning sample*, they should perform better out of sample.

Thus, tree-based methods are fundamentally different from parametric approaches because they look for patterns in the data rather than estimating the parameters of an assumed or theory-based model. Each partition merely represents a decision rule that identifies the levels of the explanatory variables that are associated with the dependent variable. Thus, tree-based methods reduce an investigator's worry about the rarity of civil conflict or issues of endogeneity – both significant problems in the civil conflict literature. In addition, the tree approach selects values for variable splits based on *relative importance*, rather than precision (as significance testing does). Thus, it is possible to identify those explanatory variables that are most important for predicting civil conflict. Moreover, if observations are missing on a variable the methodology skips over to find the next best variable that helps predict civil conflict.

Once we have identified the best predictor, we can test the scientific validity of the predictive power of the model by measuring how well they predict out-of-sample. This validating process can also help reduce the over-fitting problem. For example, we could eliminate an explanatory variable in the training sample that does not improve predictive accuracy (Ledolter, 2013, p. 170). Moreover, a variable that matters for explaining civil conflict should also be important for predicting it. We can therefore use this process to *eliminate* variables in causal econometric studies. If a covariate is helpful in predicting some variable then it may or may not cause it, but if it does *not* predict its causal value is probably limited. Moreover, if a variable predicts civil conflict we can estimate the causal impact of a policy *change* affecting that variable (Varian, 2014). Last, civil conflict-torn countries are notoriously poor record keepers. Thus, missing observations in most analyses of civil conflict lead to sample-selection bias. Our approach improves on parametric approaches because, rather than throwing out observations or variables because of a few missing data points, it uses optimally chosen "surrogate" information from other variables as proxies for the missing values.

Section 4. Seminal strands in the civil conflict literature.

Seminal civil conflict models suggest that civil conflict is an artifact of a lawless world where political processes are resolved with bullets rather than ballots. That covers a rather wide range of reasons for civil conflict. We will look at distinguishing features in these seminal theoretical approaches, and see if our empirical models can separate these theoretical models into classes that differ by their ability to predict civil conflict. We suggest that the class of theoretical models that predict civil conflict the best might be the most important approach to understanding civil conflict. A good general model should be able to predict civil conflict well.

One seminal strand of the civil conflict literature looks at the economic, sociological and psychological motivation behind civil conflict. Predatory leaders try to grab resources because they are greedy or have the opportunity to do so. These leaders capture natural resources like diamonds or oil, the proceeds from illegal trades, or straight out tax the populace (Collier and Hoeffler, 2007). Thus, rents from natural resources drive civil war. On the other hand, grievance may drive civil war. Typically, this grievance may stem from political structures that close off avenues through which certain groups can be heard or may better themselves. This kind of closure is easier if excluded groups are identifiable in particular ways like ethnic or religious differences (Fearon and Laitin, 2003 p. 76 of course suggest that these forces cannot be identified as specific correlates for civil conflict even if they do play a role). These differences, however, may play an important role in fomenting civil conflict when exogenous shocks make economic conditions particularly bad (Gurr, 1968 and Tellis, Szayna, and Winnefield, 1997, pp. 86–96). Sudden scarcity may exacerbate civil conflict between social groups while enhancing the importance of kinship ties (Hirshleifer, 1998). Thus, grievance–motivated civil conflicts should be more apparent in multiethnic countries faced with exogenous economic shocks.

Moreover, recent experimental work in behavioral economics shows that people care about fairness at a neuro–physiological level. Individuals seem to prefer punishing social norm violators even when punishment costs are non–trivial (Charness and Rabin, 2002). Indeed, Choi and Bowles (2007) argue that this sort of civil conflict helps group selection. Our country level data cannot directly test the validity of this class of grievance models. Nevertheless, presumably such measurable things like the unequal distribution of resources and income should stir the instinct for fairness and predict civil conflict robustly if a desire for justice, i.e., grievance, is the most important motivator of civil conflict.

Rational choice theory centers on another seminal source for civil conflict models, one in which individuals decide on peace of war, but do not interact. Contest models (originating with Haavelmo, 1954 and continuing with Hirshleifer, 1988, 1989 and Skaperdas, 1992 among others) suggest that leaders choose predation if the benefit of predation (which includes the opponent's economic production in addition to their own production) exceeds the costs (the diversion of productive resources towards guns). These models famously initiated the modern conversation on developing models of civil conflict that are rooted in rational choice. Grossman (1991) complicates this by adding the individual participation problem to the civil conflict calculus – leaders need to convince people to be soldiers. These models, however, like in the greed/grievance debate, largely ignore how agents interact with each other while choosing between predation and production, and the consequences of these interactions. Moreover, in these models, civil conflict is unlikely whatever the grievance, as long as the net benefit of civil conflict does not exceed the net benefit of peace.

This comparison of net benefits and net costs appear to be a simple thought in itself and perfectly in tune with standard economic rational choice models. Nevertheless, even at this level, things are complicated. For example, these models suggest that empirically, poverty and civil war should be correlated. Indeed, GDP is considered a fundamental correlate of civil conflict (Blattman and Miguel, 2010). Notice that the general structure of the models we cite here suggest that more national assets should create more war since at the very least the winner will get the right to tax. Further, people in poorer countries may be more easily persuaded to fight but they also have less to fight for. Thus, in equilibrium, a higher GDP should be correlated with more war (Garfinkel and Skaperdas, 2007). However, a lower GDP reduces the opportunity cost of war for individuals so that a lower GDP should be correlated with more war. Indeed, the two effects may cancel each other out, (Fearon, 2007) particularly if coordination costs are high. Thus, the theoretical problems pertaining to understanding civil conflict even at this fundamental level remains problematic.

This particular theoretical Gordian knot can be resolved only if we know whether GDP is an important predictor of civil conflict relative to other variables. Even if one ignores the rampant endogeneity that bedevils the civil conflict literature, standard econometric techniques have no methodology that can rank variable importance. Of course, endogeneity cannot be wished away either. Lower GDP may increase civil conflict (and remember even that is theoretically unclear) but increased civil conflict may lower GDP as well. Then there is always the possibility of a third variable that can influence both GDP and civil conflict. These problems would lead to parametric estimates that are inconsistent and biased – in short, meaningless. Thus, the relative magnitude of parametric estimates themselves may say nothing about the importance of GDP relative to other factors in promoting civil conflict.

Further, econometric attempts to control for this sort of endogeneity using instrumental variables have also been problematic. Instrumental variables can resolve the endogeneity problem only if, for example, there is an instrument for GDP that is correlated with GDP but not with civil conflict. Miguel, Satyanath, and Serengeti (2004) use annual rainfall as such an instrument. However, as Blattman and Miguel (2010, pp. 25) point out, the use of rainfall as an instrument is itself problematic. Crop failure because of low rainfall may reduce the opportunity cost of civil conflict for individuals. But it may increase the opportunity cost of civil conflict if people have to forage for food – armies famously do not march on empty stomachs. Then again, crop failure may reduce government revenues and state capacity (more on state capacity later). Oh what a tangled web.

Machine Learning provides a perspective that bypasses this web and ranks variables by out-of-sample validations of their predictive power. Notice, there are two issues here. First, if the opportunity cost rational choice model is a good theoretical model for understanding civil conflict, then at the very least, per capita GDP should predict civil conflict well. In fact, per capita GDP, and the conditions that increase per capita GDP, should be important predictors of civil conflict if there is causal link between the opportunity cost of civil conflict and civil conflict. Second, if we can show that that likelihood of civil conflict increases or decreases with higher per capita GDP, and the conditions that increase per capita GDP, then we can resolve some of the confusion above. We will investigate this latter idea in a later paper.

The two seminal approaches discussed above incorporate purely psychological and sociological explanations, as well as rational choice cost benefit analyses of civil conflict. But in these models people act alone – they are pure utility maximizing Homo Economicus. A warlord makes a rational decision on the benefit of war relative to the opportunity costs. If the benefit outweighs the cost, he (and warlords are usually "he") prosecutes war. The warlord may have to worry about side payments to his troops, or may even use grievance as the glue to keep his troops fighting, but he remains a unitary actor. The general miasma of grievance, on the other hand, may motivate people to start a rebellion. However, here too, the implicit assumption is that grievance helps individuals internalize the external benefits of rebellion. Thus, the idea that rebellion can redress injustice becomes part of the internal calculus of an individual's motivation to go to war. Grievance increases the benefits of rebellion relative to the costs. Once again, implicitly, the aggrieved person is Homo Economicus.

A second feature distinguishes these models. People in this model are already aggrieved or greedy to enjoy the spoils of war. Their only restraints are the costs of war. But what of peace? Arguably the benefits of peace enter into their calculus as an opportunity cost, if at all. But it is a lonely peace. Because actors here are unitary, they cannot understand that peace may, for example, change the opportunity cost of war if other people peacefully trade with them. In these models, therefore, war is the natural state for the unitary agent. Peace is a mere by product of the calculus for war. Apart from being a pretty depressing view of humanity, this ignores the reality that civil conflict is rare even in poor countries.

A third related strand in the civil conflict literature suggests that civil conflict may be a matter of state capacity. Weak states that cannot respond to grievances (as opposed to strong states that squelch grievances) can create the conditions for civil conflict. This may happen for a number of reasons. Rebels with grievances, for instance, may ignore a weak state and try to control resources to help their particular ethnic group. In this case, the weak state has nothing to offer rebels. Thus, weak states may provide the opportunity for civil conflict that may be motivated by either greed or grievance. Moreover, any deals with weak states may be problematic because they may not be able to insure against time inconsistency. For example, once a peace deal is struck the relative strength of the warring factions may change in the future. Factions will take this future into account as they make current decisions about war and peace. Any faction that feels that they may become relatively weaker in the future may be unlikely to sue for peace now. In equilibrium, rebels would be unlikely to make deals, particularly if the whole problem could be avoided by taking down the weak state in the current period rather than wait for an uncertain future. Unstable regimes may therefore be more likely to create the opportunity for civil conflict. Notice here that the focus is on instability rather than the nature of the state. Thus, both stable autocracies and stable democracies may not be conducive to civil conflict. Strong autocracies may squelch legitimate grievance while stable democracies may have mechanisms to voice grievances that channel and redress grievances positively. States in between – and particularly states in transition from one to the other – may practice both repression and be open enough to allow aggrieved groups to mobilize (Hegre, Ellingsen, Gates and Gleditsch, 2001, p. 33). This line of reasoning suggests that unstable regimes should be a good predictor of civil conflict.

A fourth modeling approach takes a rational choice approach in a *strategic* setting. In these models, bargaining failure between two individuals drives civil conflict. There is a notable distinction in this class of rational choice models. Here, individuals have a choice between production and predation – however, their choices interact. Bargaining implies something to bargain over. The size of this bargaining pie, therefore, matters, and depends on the interactions of agents in this model. For example, agents may choose to trade, and in the process, enjoy gains from trade; gains that may be lost with civil conflict. These models therefore capture a significant reality – if peace can provide clear benefits and war is costly, why do people choose civil conflict? After all, civil conflict can happen in rich or poor countries, even if they are more likely in poorer ones. And shouldn't civil conflict be even less likely in poor countries because people in these countries have most to gain from peace? Fearon (1995) suggests that bargaining failure has three reasons. First, leaders may be irrational or just make mistakes about the cost–benefit calculus. Second, the leaders may be rational, but the costs of civil conflict may be disproportionately borne by others. This would increase the leader's private net benefit from going to war even when peace is a viable option. Last, though, leaders may be rational and their private costs and benefits may reflect reality, they may face a problematic information structure. Agents may not bargain if they have private information about the costs and benefits of war. They may know they have the economic capacity for war while their opponent does not, making the bargaining table less attractive. Or, they may choose to lie about their capacity for war. Nevertheless, this approach is not very convincing because if asymmetric information leads to costly war then agents also have an incentive to invest in mechanisms to reduce the information asymmetry, such as spying.

A more promising mechanism for bargaining failure could be commitment failures. Commitment failures occur when one of the two agents engaged in the bargaining process find an advantage from reneging on bargained solution. Powell (2006), for example, suggests that commitment problems can lead to bargaining failure even when all parties have complete information. For example, say a state makes a peace–deal with rebels. Consequently, the state may get a peace dividend and gain economically in the future. At that point, the state may renege on the bargain. The expectation of reneging may reduce the rebel's desire to negotiate the peace deal to begin with. In any case these models explicitly model strategic interactions between agents who, unlike Homo Economicus, are no longer islands. In the process they also explain realistic civil conflict when the peace is an attractive alternative.

A fifth source of civil conflict may be what Fearon (1995) defines as part of his argument for civil conflict arising from bargaining failure. In his telling, religion is a subset of general bargaining failures that necessitate civil conflict. In our opinion though, religion may rank as a separate seminal source for theoretical civil conflict models because it does not quite fit the mold of the general tenor of the approaches being described here. Fearon (1995) himself notes that religion feeds bargaining failure because in some senses it is indivisible. That is one can fight for or against religion but not over it. After all it would be foolish to say that I have won a war by keeping 51% of my idea of God. Jackson and Morelli (2011) nevertheless point out that religious wars can be rational as well as long as leaders in these civil conflict choose and optimize by using religion to, for example, inspire followers to overcome differences and join a civil conflict. Fearon's religious leader might not optimize as long s/he thinks religion is indivisible, but Jackson and Morelli's leader is merely using religion as a tool to prosecute a rational cost–benefit war. The latter case fits the rational choice modeling approach but the former requires a category by itself. This indivisible religion approach to civil conflict suggests that religious polarization should increase the likelihood of war. The more polarized people are over religious factors, the less likely they will compromise since the very indivisibility of religion reduces the likelihood of compromise.

A final approach to civil conflict is psychological. Hardie, Johnson, and Dominic (2011) summarizes the literature on the psychological causes of war by focusing on three major kinds of biases that they suggest make war "irrational." We place the word "irrational" in quotes because there is a great deal of controversy about whether many of these experimentally verified biases may fit into the rational choice paradigm. Nevertheless, these approaches do suggest that psychological biases may cause agents to miscalculate the true costs of war. Thus, for example, fundamental attribution errors and social identity theories may help explain why certain groups of people may be viewed as enemies. Prospect theory may then help explain why agents may be more willing to gamble more when facing the certain losses from war with those enemies. At the end, these biases move them toward civil conflict.

Economic investigations of civil conflict, however, tend to be of the rational choice variety. This may be an artifact of the utility optimization approach to decision making preferred by economists. Cramer (2002) argues that as a result economists' empirical investigations tend to be limited to reinforcing economic interpretations. We have assembled a dataset that includes both economic and non–economic variables. We want to respond to Cramer by analyzing the results of the horse race between important variables. At the same time, we try to gain some insight into whether certain classes of models are better predictors of civil conflict. Eliminating certain classes of models may also help us develop a theory of civil conflict by shining a light on the channels of civil conflict that truly matter. So what does matter? Let us take look at these seminal models through a Machine Learning lens.

Section 5. Choosing the Most Predictive Theoretical Model.

The story goes that receiver operating characteristic originated when the British started using radar to identify enemy aircraft in WW2. As they increased radar sensitivity they began to pick out more enemy aircraft - but also more flocks of birds. Thus increasing sensitivity increased the error rate, i.e. the identifications became less specific. The ROC curve takes this problem into account. A model's true positive identification rate (sensitivity) is measured on the vertical axis while the model's false positive rate (lack of specificity) is measured on the horizontal axis. In more general statistical terms, specificity is (1 – Type 1 error), while sensitivity is (1 – Type 2 error).[6] The ROC curve then reflects how well the model trades off sensitivity and specificity, true positives and false positives. Notice that an ROC graph measures proportions along the axes. Since it is not possible to

[6] In Table 1, we report specificity as (1 – Type 1 error) and sensitivity as (1 – Type 2 error). Type 1 error happens if our model predicts conflict when the observed outcome was peace. A Type 2 error occurs if our model predicts peace when the observed outcome was conflict. A risk averse policy maker may be more concerned about missing a likely conflict. She may therefore wish to avoid type 1 errors – thereby maximizing specificity.

get more than 100% correct classifications (or for that matter misclassifications) the axes are limited to 0, 1 and the graph is basically a unit square. If the ROC is coincident with a 45-degree line in this graph, the model is no better than a coin toss as a predictive tool. The area under such an ROC would be 50% of the unit square. To be better than a coin toss, then, any predictor model must have more than 50% of the unit square under the ROC. A model with an AUC of 100% would be a perfect predictor.

How do our machine learning perform in terms of the AUCs or the c statistic? Table 1 in the Appendix reports the AUCs for the, Bagging, Boosting, Forest, and Single-Tree models as a c - Statistic. We look at the results of choosing a classical threshold. The classical threshold for classifying a new observation into a class assumes that all of the categories of the target (output) variable are equally likely. The class with the highest predicted probability above that threshold is the predicted class. In the case of a binary target (Conflict/No Conflict), an observation is classified as a "positive" outcome (Conflict) if the probability of the positive outcome (Conflict) is greater than 0.5.

Notice that our tree models predict remarkably well out of sample overall according to the c statistic. Nevertheless, overall their specificity scores tend to be much better than the sensitivity. Nevertheless, also note that the positive predicted value for all these models, i.e. the likelihood the model predicts conflict given that conflict actually happened is quite high. Even the negative predicted value, i.e. the likelihood that countries our model predicts peace given the country was actually at peace is also quite high.[7] Thus we have some confidence that our machine learning

[7] We should note that a model can achieve the ideal level of sensitivity and specificity without actually predicting positive outcomes well at all. Note that the specificity (or "true negative rate") is equal to P(Predicted Negative Outcome | Negative Observed Outcome), whereas the sensitivity (or "true positive rate") equals P(Predicted Positive Outcome | Positive Observed Outcome). In the extreme, as our model approaches predicting *zero* cases to be "positives" (failed states), the specificity and sensitivity both converge to one. However, one could hardly argue this to be "perfect prediction since the positive predicted value will be zero! Hence, all of the measures of predictive quality deserve some consideration.

models are reasonably good predictors. Given that outcome which variables contribute the most to this predictive quality?

We would like to bring the reader's attention to the top ten predictors of civil conflict in Table 2. The variables are ranked according to the *average* reduction in the Gini impurity index (distinct from the Gini inequality index) from the Tree class of models. Notice, though, that there is some variation in the ranking of the variables between the tree methods. Thus, religious polarization is ranked the 9th most important predictor of civil conflict on average. However, it is ranked 31st by the tree method, and 7th by the Forest and Boosting variations of the tree methodology respectively. The reader will also notice that this sort of variation is much smaller among the top five variables. We will focus on the top ten variables in this list as we discuss our model choices below. Nevertheless, we remain most confident about the top 5 among this list. Why the top ten? This is largely a judgment call. We would invite the reader to go further down the list. However, we think they will continue to find that the broad outlines of our story below will remain undisturbed.

Greed as an explanation of civil conflict – at least in the sense that rebel groups are motivated to capture rents – seems to be a loser in our horse race. Recall that if greed is a motivator of civil conflict then easily captured rents should facilitate civil conflict. Primary commodities are well known proxies for such easily captured rents. Primary commodity exports, however, do not even break into the top 20 most important variables on both the average ranking and in all the models except in the Single-tree model (where it ranks a low #13). Can such a poor predictor of civil conflict ever be an important cause for civil conflict? We think not. However, this variable represents the salience of greed as well. Collier, Hoeffler, and Roehner (2009) rename their greed variables as feasibility variables. This places them more in line with the opportunity cost reasoning for war approach where people choose war because of a decrease in the opportunity cost of war as a matter of rational choice. They find that rough terrain, proportions of young men in the population, along with a population's dependence on commodity exports, correlate with civil conflict. Our data matches theirs, except for the Primary Commodity Exports. Nevertheless, we find that area as a proxy for terrain, and the percentage of women in the population as proxy for the role of demographics, are not salient predictors of civil conflict. These findings suggest that greed – at least to the extent it implies resource grabbing – and some types of opportunity costs, ought not to be part of a validated model of civil conflict and therefore probably should not be featured in a theory of civil conflict. Of course, there are other kinds of rents to pursue – taxation revenues for one. We are not claiming that we have the definitive answer to the model selection problem here. We do however suggest that our methodology provides the means towards solving the model selection problem and in that sense help us gain insight in to a true theory of civil conflict.

What about grievance? Recall that in our discussion above, civil conflict motivated by grievance should at least be predicted by ethnic fractionalization, income inequality, and exogenous shocks. We do have income inequality (the Gini coefficient of income inequality) and ethnic fractionalization explicitly in our models. Unfortunately, none of these variables ranks particularly highly. If variables have a significant causal impact on civil conflict, they also should be important predictors of civil conflict. To the extent that our grievance variables (and these are grievance variables that are commonly used elsewhere in the literature) do not rise up to the challenge, could it be that a grievance based model of civil conflict is not a good contender for a theory of civil conflict?

Of course, one might argue that these variables do not capture the psychological factors that drive grievance. It may even be that these variables are capturing something other than grievance. For example, income inequality may be driving greed – "if X has more than me why shouldn't I have more?" Such a telling of the story nevertheless misses the holistic point. To quote Miguel and Blattman (2010, pp. 10–11):

"...the distribution of income and wealth – whether across individuals or sectors – is central in explaining the economic incentives for rebellion. Civil war seems more likely when state wealth is easily appropriated or divorced from the citizenry, as with some natural resource wealth and foreign aid flows."

Our validated methodology suggests that neither greed (or the updated "feasibility" redefinition of greed type variables) nor grievance predict civil conflict well. Incidentally, foreign aid flows, contrary to the emphasis placed on it by Miguel and Blattman (2010), either as a proxy for greed or grievance, is not an important predictor of civil conflict either. Friedman (2006) suggests that economic growth may fuel "greater opportunity, tolerance of diversity, social mobility, commitment to fairness, and dedication to democracy". The converse then can have the opposite effect. Friedman suggests that when we cannot compare our current economic situation to our past favorably, for example during an economic downturn, we are more likely to compare our economic situation with others. At this point, relative affluence becomes a source of grievance and potential civil conflict. The predictive salience of GDP per capita may capture some of this. Nevertheless, both the change in GDP per capita and the Gini coefficient have low predictive powers in our empirical model. Thus, while Friedman's argument for moral decay during bad economic times may be valid, it does not seem to be predictive of all out civil conflict.

Thus, a predictive theoretical model that looks to either greed or grievance may be deeply problematic. Note further that these models focus on the motivation behind civil conflict. On the greed side, unitary actors make a rational choice decision to choose war if the opportunity costs are low enough, and on the grievance side they choose war merely because they are aggrieved. In both cases though, civil conflict is an underlying feature and peace merely a hiatus from civil conflict. Our results therefore are quite important for both theory and policy. For one, it appears that focusing on the motivation for civil conflict without any concomitant sense of the motivation for peace does not make a good predictive theoretical model of civil conflict. As a matter of policy too, redistribution programs by themselves may not stave off civil conflict by redressing grievance. Moreover, we also seem to have put paid to the "greed" argument since the availability of resources and some of the other variables that according to Collier, Hoeffler, and Roehner (2009) influence the feasibility of civil conflict also appear to be poor predictors of civil conflict.

We do however note that religious polarization ranks higher than all the potential grievance variables in our models. This may be a reflection of grievance though it seems unlikely since in the holistic context the other greed/grievance variables don't seem to predict civil conflict very well. Religious polarization often fractures along ethnic lines. For example, in civil conflict torn Iraq, the Yazidi are ethnically and religiously different from the Shia (more Persian) and the Sunni (more Arab). Here, both ethnic fractionalization AND religious fractionalization should be important predictors of civil conflict. In fact, Reynal-Querol (2002) makes precisely this point. To the extent that religious fractionalization seems to predict civil conflict much better than ethnic fractionalization, natural resources, and income inequality, something other than the greed/grievance paradigm must be at play here. Moreover, religious civil conflict may be different from the state capacity models or the rational choice models if religion is indivisible and cannot be fought over. The salience of religious fractionalization in this context therefore suggests that a theory of civil conflict should somehow encompass the role of indivisible religion.

Let us look at another strand among the seminal models of civil conflict. Earlier, we noted that unstable regimes promote civil conflict because they cannot completely avoid repression nor can they repress enough to prevent the formation of rebel groups. This approach suggests that neither stable democracy nor strong autocracies are particularly conducive to civil conflict. In other words, both democracy and autocracy should strongly predict peace, i.e. the lack of civil conflict. In short, if the state capacity model is a good theory of civil conflict, unstable regimes, democracy, and autocracy together should be strong predictors of civil conflict.

Au contraire, we find that none of the political structure variables rank very highly as predictors of civil conflict. Party Exclusion is the highest ranking of the political structure variables. This variable measures whether and how political parties are excluded from the political process. This may hint at the importance of state capacity as a predictor of civil conflict. Nevertheless, it does not break the top ten. The two most direct measures of political instability and state capacity are Regime Instability and Within Instability. Regime Instability captures an institutional dimension that may indicate sources of instability stemming from fissures within the regime, specifically the frequency of cabinet changes and executive changes. Within instability, on the other hand, captures a broader political instability arising from the concentration of government and legislative fractionalization. Democracy - which captures the institutional dimensionality from democratic accountability, whether a regime is autocratic or not, the extent of electoral competition for the executive and the legislature - is ranked even lower. In short, the state capacity argument seems to be losing as a predictive theory as well.

Next we investigate whether psychological factors may help predict civil conflict follows a similar pattern. Hardie, Johnson, and Dominic (2011) suggest that psychological causes for war find expression in observable variables. They first suggest that while psychological biases are part of the human condition, their most perverse expressions are most likely in autocracies rather than democracies. Biases are more likely to result in civil conflict in an autocracy than in a democracy because there are no checks and balances in autocracies (Kowert, 2002). Even within democracies, biases may overwhelm checks and balances if regimes tend to be concentrated and efficient and information flows are controlled (Desch, 2008). However, our democracy variable, which captures the essential features of a democracy like democratic accountability, checks on power, government polarization, electoral competition etc., is not a predictor of civil conflict. If psychological biases are indeed more likely to result in civil conflict in autocracies, then the democracy variable should at least be a salient predictor of civil conflict. It is not.

Another way that psychological biases may emerge is when, within democracies, the decision making structure is centralized so that it is likely to transfer biases into policy, leading to civil conflict. Yet our "within instability" variable is *also* not a salient predictor of civil conflict. Recall that this variable captures the variation emanating from government concentration and legislative fractionalization. Thus, if certain psychological biases matter, they certainly do not seem to operate through the political structures suggested in the literature.

On the other hand, "Transparency" is a salient predictor. Transparency is sourced from variables like corruption, democratic accountability, and bureaucratic quality. This variable captures the sort of open decision making processes wherein different views are valued – a classic characteristic of democratic structures. More diverse decision-making groups are more likely to avoid biases because they are more likely to have agents playing devil's advocate, and have countervailing voices. Thus, if different people with different biases are represented, existing biases may cancel each other out. Hardie, Johnson, and Dominic (2011) make a similar point about the decision making process from a psychological perspective. Therefore, more competitive decision-making systems are less likely to be biased in any particular, overconfident, direction, and less likely to lead to war. Again, this suggests that more concentrated governments/autocracies are prone to war, while more competitive political systems are not.

So, even though Democracy and Within Stability are not important predictors of civil conflict, the broad point of this literature - that truly democratic processes may help place a check on psychological biases playing out into civil conflict - is captured in our Transparency variable. Thus, our results certainly do not rule out the causal and experimentally verified impact of psychological biases on decision-making. However, our results add nuance to this idea by bringing into question the role of the structures of democracy vis-à-vis democratic processes. In short, elections and multi–party democracy may not be enough to avert civil conflict if democratic processes emphasizing free and open discussion of policy making are clogged.

The saliency of Transparency has a further implication: a third expression of psychological bias, Urgency, may also have an effect on the likelihood of civil conflict. Psychological biases may be most relevant when there is no time to think things through. From a rational choice perspective, it may merely mean that our brain's capacity constraints kick in to emphasize the present over the future. Urgency may also ensure that even in even in democratic and competitive systems, debates and checks and balances of different biases may not find their way into a coherent policy. For example, civil conflict prone biases may become more salient if decision makers are under stress (Rosen 2004) and emotions cloud their judgment (McDermott 2004). We have no variable that tests this point directly and note that it may be hard to do in a cross country study. Nevertheless, the predictive salience of transparency in our empirical model is broadly consistent with the role of urgency. Urgency is less likely to lead to civil conflict if multiple brains are yoked together in adversarial ways in the decision making process, thus alleviating the resource constraint element of brain capacity, while emphasizing the effect of different types of biases.

Is there then any hope for rational choice approaches to civil conflict? Notice the variables in the top ten list. Of course, lag civil conflict is the elephant in the room. It is important enough to warrant a new look at the role of path dependency in conflict. In this paper though, we will merely note that the idea that past conflict predicts the likelihood of future conflict merely suggests that conflict is an equilibrium phenomenon. Indeed, in strategic settings such as a prisoner's (security) dilemma's or stag hunt like coordination games conflict is a rational equilibrium outcome. To that extent our finding is quite consistent with strategic rational choice game theoretic models well known to represent the predation production tradeoff in the public choice literature. Nevertheless, almost all the variables in this list *also* suggests a rational cost benefit analysis of civil conflict may be our best shot at a formal predictive theory of civil conflict.

First of all, we note that per capita GDP (as noted by the rational choice contest models) is the second most important predictor of civil conflict. Other important variables in this parsimonious list like Credibility, Transparency, education (secondary school enrollment), electricity, trade, dependency, all influence *economic* opportunity. *The salience of these variables, taken together, suggests the predictive primacy of rational choice models that focus on the opportunity cost of civil conflict over other theoretical modeling paradigms*.

This is not to say that grievance, state capacity, or psychological biases do not matter. We suggest that the sort of grievance that matters is centered on economic opportunity. Further, political structures that give us a sense of state capacity and stability may be important insofar as they facilitate economic opportunity. As a matter of policy, economic opportunity may thus be more important than political structures that give voice to grievances in reducing civil conflict. If state capacity matters, it is probably to the extent that the state allows a framework for economic opportunity to flourish and negate grievances. Indeed, the sorts of structures that support economic opportunity may also intercede in placing a check on the psychological biases that may lead to war. Religious polarization, as we have alluded to earlier, may be a slightly different animal. In any case, the effect of religiosity in predicting civil conflict is small relative to the combined predictive power of what we are calling the economic opportunity variables. A unique theory of civil conflict may then well be formulated around open governments and economic opportunity.

Conclusion

Why does identifying a *predictive* theoretical model matter apart from assuaging some academic need for intellectual coherence? It matters for policy. Policy makers implement a policy with the hope of some future impact. Theoretical models that perform well as predictors are therefore more useful to policy makers because these models highlight the possible salience of the effect of a policy. So, if people choose civil conflict by weighing its costs and benefits, rather than because of whether they feel repressed, policies designed to allow people a structure to air their grievance may fail if the goal is to reduce civil conflict. Elections and new parliament buildings may therefore be poor investments in civil conflict reduction while improvements in transparency and credibility may be better policies to reduce civil conflict.

Further, we often hear from political science "realists" that certain regions like the Middle East may avoid civil conflict if only dictators there have a firm grip on power. But this sort of policy is predicated on a sense that regime stability, whatever the sort of regime, is a bulwark against civil conflict. Our results suggest that variables like regime instability, or for that matter, political structure variables like executive selection, is not important predictors of civil conflict. Therefore, policies that target those variables may not be effective for reducing civil conflict. Propping up regional dictators like Saddam Hussein, or for that matter, the Shah of Iran are the mainstay of "realist" policy. But if the goal of such policy is to reduce the likelihood of civil conflict in the Middle East, then our technology suggests it is likely to fail.

Recall Cramer's (2002) criticism of economists focus on economic variables: It is a valid criticism in general. However, we may have lucked out while considering civil conflict. As a matter of *predictive* salience, among a wide array of variables that capture elements of the psychological, political structure, greed/grievance approaches to modeling civil conflict, those variables that capture economic activity and opportunity seem to matter the most.

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Appendix

Variable	Democracy	Transparency	Protest	Credibility	Within Instability	Regime Instability
Gov Stability	0.0038	0.1713	-0.1728	0.6894	0.0029	-0.0907
Investment Prof	0.1750	0.4171	-0.0656	0.6859	0.0225	-0.0407
Corruption	0.1773	0.7423	-0.0840	0.0259	0.0240	-0.0273
Dem Accountability	0.5419	0.5586	0.0051	0.1307	0.0685	0.0221
Bureaucratic Quality	0.2607	0.7813	0.0169	0.2429	0.0435	-0.0080
Polity2	0.7312	0.1463	0.0542	-0.0178	0.0322	0.0780
Reg Durability	0.1527	0.4118	0.1334	0.0470	-0.0427	-0.1325
Exec Years in Office	-0.3882	-0.0780	-0.0079	0.1056	-0.0932	-0.0514
Leg Electoral Competition	0.8735	-0.0201	0.0236	0.0603	-0.0586	-0.0171
Exec Electoral Competition	0.9258	0.0148	-0.0018	0.0218	-0.0675	-0.0127
Electoral Fraud	-0.0194	-0.3141	-0.0138	-0.0049	-0.0099	0.0006
Gov Herfindahl	-0.2905	-0.0981	0.0733	-0.0207	-0.6944	-0.0189
Leg Fractionalization	0.6651	0.0254	0.0233	0.0301	0.5507	0.0378
Checks on Power	0.7687	0.1746	0.0126	-0.0725	0.1931	0.0122
Changes in Vetos	0.1015	-0.0058	0.0317	-0.0775	0.0435	0.0710
Gov Polarization	0.5059	0.3097	-0.0074	-0.0738	0.3628	0.0031
Assassinations	0.0685	-0.0979	0.1580	-0.1345	0.0050	0.0737
Strikes	0.1002	-0.0230	0.3793	-0.2023	0.0093	0.1050
Government Crises	0.1009	-0.0592	0.1858	-0.2407	0.0494	0.3666
Purges	-0.0429	-0.0438	0.1521	-0.0728	0.0003	0.1135
Riots	0.0614	-0.0396	0.7189	-0.1067	-0.0352	0.0529
Demonstrations	0.0630	-0.0110	0.7134	-0.0777	-0.0228	0.0153
Effectiveness of Leg	0.5818	0.2206	0.0232	0.1032	-0.0664	0.0023
Cabinet Changes	0.0316	-0.0764	0.0547	-0.0988	0.0337	0.6186
Executive Changes	0.0860	-0.0064	0.0475	-0.0562	0.0099	0.6239

Figure 1. EFA Factors

Machine Learning Technologies	Single-Tree	Forest	Bagging	Boosting
c-statistic	0.844	0.883	0.850	0.882
Specificity (1-Type I error)	0.957	0.985	0.984	0.948
Sensitivity (1-Type II error)	0.565	0.548	0.598	0.619
Positive Predicted Value	0.749	0.894	0.891	0.731
Negative Predictive Value	0.907	0.906	0.915	0.917
Overall Error Rate	0.115	0.095	0.088	0.112

Table 1. Model Prediction Error Rates

% of Decrease in Gini					
	Single	Forest	Bagging	Boosting	Average
LagCivil conflict	64.553	33.601	81.159	34.427	53.435
GDPpc	3.814	3.884	1.492	12.244	5.359
RuralPop	3.982	5.105	2.608	4.391	4.021
Transparency	3.77	3.015	2.394	5.721	3.725
Credibility	1.909	4.257	0.606	3.104	2.469
SecEnrollPC	2.636	1.463	1.541	3.669	2.327
ElectricPC	1.932	4.017	0.326	1.738	2.003
Dependency	1.463	2.524	0.367	1.404	1.439
ReligPolariz	0.036	2.942	0.226	2.295	1.375
Trade	0.769	2.842	0.694	1.191	1.374
UrbanPopGrowth	1.875	1.696	0.558	1.035	1.291
Gini	1.395	2.794	0.405	0.457	1.263
PartyExclusion	0.842	1.317	0.565	1.836	1.14
Inflation	0.542	1.547	0.345	2.074	1.127
Area	1.658	1.724	0.384	0.568	1.084
RegimeInstab	0	1.081	0.607	2.201	0.972
PopGrowth	1.505	1.356	0.245	0.655	0.94
PopDensity	0.072	1.618	0.363	1.571	0.906
Regime	0.334	0.893	0.606	1.786	0.905
PhonesPC	0.623	1.502	0.209	1.245	0.895
Year	0.985	1.025	0.107	1.39	0.877
RuralPopGrowth	0	1.406	0.366	1.589	0.84
FemalePop	0.146	1.341	0.305	1.426	0.804
TermsOfTrade	0.719	0.921	0.104	1.46	0.801
EthnicPolariz	0.521	1.327	0.372	0.873	0.773
PrimCommodityExports	1.033	0.982	0.285	0.774	0.769
ExchangeRate	0.282	1.467	0.188	1.105	0.76
ParliamentRespons	0.367	0.987	0.173	1.16	0.672
AidAssist	0.295	1.265	0.396	0.702	0.665
System	0.987	0.401	0.034	0.568	0.498
ChangeGDPpc	0	0.6	0.201	1.1	0.475
WithinInstab	0.022	1.039	0.281	0.541	0.471
Democracy	0	0.963	0.197	0.558	0.429
Investment	0	1.275	0.211	0.116	0.4
Executive	0.367	0.863	0.219	0.152	0.4
PartyCoalitions	0.439	0.597	0.069	0.453	0.39
DroughtIndex	0	0.547	0.162	0.814	0.381
ChangePhonesPC	0	0.653	0.132	0.553	0.335
Protest	0.105	0.986	0.187	0.055	0.333
ChangeImportsPC	0	0.53	0.101	0.369	0.25
ChangeSecEnrollPC	0.022	0.707	0.09	0.135	0.239
DroughtIndexRange	0	0.473	0.035	0.36	0.217
ChangeExportsPC	0	0.47	0.083	0.132	0.171

Table 2. Variable Importances.