CRISE • Centre for Research on Inequality, Human Security and Ethnicity

Queen Elizabeth House, University of Oxford, Mansfield Rd, OX1 3TB, UK
Tel: +44 1865 281810; Fax: +44 1865 281801; http://www.crise.ox.ac.uk/

Dealing with time in the quantitative study of conflict

Graham Brown and Arnim Langer

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Abstract

This paper explores the ways in which the quantitative literature on the onset of violent conflict deals with the passage of time. We argue that current approaches are insufficient for dealing with the methodological challenges raised by this issue. In particular, we argue that quantitative innovations have focused on resolving problems of ‘relative time’, relating a given year observation to previous year observation within a single case. But this has not addressed the more problematic epistemological question about the passage of absolute time. The paper suggests a number of routes towards addressing this problem.

The authors

Graham Brown is Senior Lecturer in the Politics of Development, Department of Economics and International Development at the University of Bath.

Email: g.k.brown@bath.ac.uk

Arnim Langer is CRISE Research Officer in Economics and Politics, West Africa.

Email: arnim.langer@qeh.ox.ac.uk

Table of Contents

List of Tables and Figures

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By Graham Brown & Arnim Langer

1. Introduction

There is increasing awareness in the econometric literature on conflict that time represents a problem. Logit and other standard OLS regression analyses using pooled data based on 'country-year' observations are problematic because the underlying regression model is based on the assumption that observations are not interdependent whereas, quite clearly, they are.

In this paper, we discuss the ways in which problems of time have been addressed in quantitative analyses of civil conflict, and explore possible ways in which this line of analysis may be extended. We contend that the response to the problem of time has mainly been to find essentially technical ways of circumventing or at least minimizing the problem of inter-dependent observations. While a useful step forward, this leaves unaddressed fundamental epistemological questions which are raised by an examination of the role of time in quantitative conflict studies.

To make our case, we first need to distinguish between two different conceptualizations of the passage of time: absolute time and relative time. By absolute time, we mean the ways in which quantitative studies handle particular historical events or periods of time, that is to say variables that could be (partially) coded for on the basis of a variable for the date of occurrence. A typical example here is the 'Cold War dummy' that is included in many analyses (e.g. Collier and Hoeffler 2004), and which is typically coded as 1 for all years between 1945 and 1988, and 0 for 1989 and all subsequent years. By relative time, we are thankfully not suggesting that econometrics take into account Einsteinian physics of motion. Rather, in our terms, relative time refers to statistical techniques and variables that either relate country-year observations within a single country-case to each other, or that relate country-year observations to a common 'origin point' for that country-case, but without reference to the 'real' time in which these events occur. A count of 'peace years' prior to each observation (i.e. the number of year preceding the current year in which the country has <u>not</u> experienced armed conflict) is a common example of relative time (Urdal 2006).

But this leads us to a third issue related to time, namely when does the main dependent variable, a conflict 'event', start and when does it end? Essentially, our claim is that the 'event' of violent conflict or civil war which is tested for in econometric studies is not sufficiently well operationalized. And, by using a simple casualty per year definition of violent conflict or civil war, conflict datasets often artificially divide one and the same conflict into different time periods. While this is particularly problematic for quantitative studies on the duration of conflict or on conflict recurrence, it also poses potentially serious problems for quantitative studies into the causes of violent conflict.

To examine these issues, we make use of the extensive replication datasets available from previous studies through journal and personal websites. We make particularly extensive use of the Fearon and Laitin dataset (henceforth FL) developed for their 2003 APSR article on 'insurgency, ethnicity, and civil war' (Fearon and Laitin 2003a; Fearon and Laitin 2003b), which remains a touchstone for the quantitative analysis of civil war. Fearon and Laitin develop an 'insurgency' model of civil war, arguing that the bulk of civil war in the post-Second World War era has been constituted by 'insurgency or rural guerrilla warfare', which has been 'harnessed' to various political agendas. They contend that the determining factors that explain the incidence of insurgency are, rather, the underlying possibilities for insurgency, which they measure in geographical terms – the extent of mountainous and non-contiguous territory in the country – and political terms. Their empirical analysis, they conclude, shows that 'the conditions that favour insurgency – in particular, state weakness marked by poverty, a large population, and instability – are better predictors of which countries are at risk for civil war than are indicators of ethnic or religious diversity or measures of grievance such as economic inequality, lack of democracy or civil liberties, or state discrimination against minority religions or languages'. The FL dataset, which is one of the most extensive annualized conflict datasets and is available online (Fearon and Laitin 2003b), has subsequently been used as the basis for many other econometric studies that typically append one or more extra variables to the FL model to test their significance.

In using this dataset to make our arguments, we wish to make it clear that we do not intend this paper to be read as an attack on this article, which has made a valuable contribution to the literature on civil war. We use it rather because it is such an extensive dataset that it provides fertile ground for testing the impact of different approaches to operationalizing the passage of time. The paper proceeds as follows.

In the next section, we will deal with the issue of relative time, and Section 4 subsequently focuses on the issue of absolute time. The fifth section then deals with the issue of how violent conflicts and civil wars are usually operationalized and how this affects econometric results.

2. ***Plus ça change***: taking time relatively seriously

Econometric studies of the incidence of civil war typically use the "country-year" as the unit of analysis – that is to say, they comprise a dataset in which each row represents a given country in a given year, and which then also includes a variety of other data for that year thought to be likely to impact upon the incidence of civil war, such as GDP per capita, level of democracy, and so forth. Some or all of these variables are often lagged by one or more years to reduce problems of identifying causality – a low level of GDP per capita, for instance, is widely found to be a strong predictor of civil war, but war itself also usually reduces GDP per capita significantly. Hence, for instance, if war breaks out at the beginning of a country-year, this is likely to reduce GDP for that year, which would artificially increase the significance of low GDP in the regression. To obviate this problem, the previous year's data is frequently used, for instance the country-year observation "Ghana 1972" would contain the GDP data for Ghana in 1971. Various regression models, discussed further below, are then used to examine which variables are most strongly correlated with the likelihood of civil war onset, most commonly the pooled logit model, which is suitable for a dependent variable such as civil war onset that takes only two values: 0 (no civil war onset) and 1 (civil war onset). This has proved problematic, however, because one of the underlying assumptions of the logit model is that the individual observations making up the dataset are completely independent of each other. But, for instance, treating political instability and violent conflict in 'D.R. Congo in 2002' as a completely separate observation from 'D.R. Congo in 2003' seems highly unrealistic.

Until quite recently, the most common way of addressing, though not resolving completely, the issue of the interdependence of country-year observations within conflict panel data was through the introduction of a variable that measured, in effect, the passage of absolute time between the country-year observation in question and an imputed 'foundation point' of the country-case, usually indexed to the formation of the state in question (Fearon and Laitin 2003a), the most recent major regime transition (Gates, et al. 2006; Saideman, et al. 2002), or duration of peace preceding the country-year observation (Urdal 2006).

The passage of relative time is operationalized in a number of ways. The simplest is a range of dummy variables. With many conflict hypotheses suggesting that newly-formed states or states that have recently undergone regime transition are more vulnerable to internal conflict, a dummy variable that captures the 'newness' of the state or regime in question is common, such as Fearon and Laitin's 'New State' dummy variable, which takes the value 1 in the first and second years after a country gains independence and 0 in all other years. A more unusual use of a relative time dummy is Saideman et al (2002), who use an 'enduring regime' dummy, coded 1 for all observations where the regime has endured for 20 years or more and 0 otherwise. While, as we shall see shortly, such dummy variables are typically highly significant, they are clearly arbitrarily bounded – why is a 19-year old regime so different from a 20-year old regime?

Continuous measures of the passage of relative time are less obviously open to accusations of arbitrariness, and are widely used both to measure 'peace years' – the number of years for each country-year since the country-case last experienced conflict – or 'polity duration' – the equivalent number of years since a significant regime change (e.g. Hegre et al 2001; Urdal 2006), usually operationalized as a change in the Polity index score. Typically, this is transformed into a 'decay' function using the formula exp((0 - years in peace)/X)[1], where X is a variable representing the presumed 'half-life' of conflict/political instability legacy, i.e. the number of years after which the impact of a previous conflict halves. Although there is some degree of arbitrariness in the selection of a value for X – Østby (2008) uses a value of X=1, for instance, while Urdal (2006) uses X=4 – the different decay functions are highly correlated.

Table 1 replicates Urdal's study of the impact of demographic 'Youth Bulges' using different values for X. While the model as a whole remains broadly the same whatever value is given to X, some interesting trends emerge as X is increased. Firstly, as X increases, the significance of the peace-year decay function decreases, as does the overall fit of the model, although the peace-year function remains by far the most significant predictor in all models. As X increases, there are consistent

[1] This notation taken from Urdal 2006.

downward trends in the significance of two of the main explanatory variables Urdal invokes – the presence of demographic youth bulges and the overall population size – and a consistent increase in the significance of one of his control variables, the Polity-squared index. It is to Urdal's credit that he did not simply pick the value of X which would have given the strongest results: a value of X=1 would have produced a model with stronger z-statistics for his main explanatory variables and a pseudo-R^2 some two percentage points higher. But this nonetheless raises uncomfortable question about the impact of relative time on regression results, particularly as it comes out as the strongest predictor, since the results are dependent on the choice of an essentially arbitrary time period.

Table 1: Re-estimation of Urdal model with varying time decay function

	X=1		X=2		X=3		X=4		X=5		X=6	
Youth Bulge	0.057	***	0.052	***	0.049	***	0.047	***	0.046	**	0.045	**
	(3.00)		(2.75)		(2.62)		(2.53)		(2.45)		(2.38)	
Population (ln)	0.227	***	0.213	***	0.207	***	0.204	***	0.201	***	0.198	***
	(3.97)		(3.76)		(3.66)		(3.60)		(3.55)		(3.51)	
IMR	0.005	***	0.005	***	0.005	***	0.005	***	0.005	***	0.005	***
	(2.70)		(2.61)		(2.61)		(2.64)		(2.68)		(2.73)	
Polity	0.014		0.012		0.012		0.012		0.012		0.012	
	(1.02)		(0.88)		(0.84)		(0.84)		(0.84)		(0.86)	
Polity-sq.	-0.011	***	-0.011	***	-0.011	***	-0.011	***	-0.011	***	-0.011	***
	(-3.55)		(-3.72)		(-3.77)		(-3.78)		(-3.79)		(-3.79)	
Peace years	**3.966**	***	**2.644**	***	**2.161**	***	**1.920**	***	**1.776**	***	**1.678**	***
	(8.65)		**(7.47)**		**(6.80)**		**(6.49)**		**(6.31)**		**(6.20)**	
_cons	-7.345	***	-7.040	***	-6.934	***	-6.870	***	-6.824	***	-6.788	***
	(-8.26)		(-8.03)		(-7.92)		(-7.86)		(-7.81)		(-7.77)	
N	5331		5331		5331		5331		5331		5331	
Pseudo-R2	0.1072		0.0922		0.0880		0.0863		0.0855		0.0850	

Source: Authors' calculations, based on Urdal (2006) replication dataset. Notes: z-stat in brackets; X=4 replicates Urdal's results.

In fact, in many econometric studies, relative time variables are the strongest, or among the strongest, predictors that result from the analysis (Carey 2007; Fearon and Laitin 2003a; Urdal 2006). Moreover, the size of the coefficient is typically large. Figure 1 demonstrates this graphically, showing the significance of the 'New State' dummy to the FL model, which codes for the first two years of a state's existence, by graphing the post-estimation impact of country size (one of the other main explanatory variables in the FL model), on the predicted likelihood of conflict onset. The graph disaggregates by the New State dummy, holding all other variables at their mean value.[2] The difference in results is striking; even the smallest 'new states' have a predicted mean conflict onset likelihood of around 2%, whereas all but the

[2] Here and elsewhere, post-estimation graphs have been generated using the postrgr3 addition to Stata developed by Michael Mitchell of UCLA. Unless otherwise stated, these curves are all generated by holding all other variables at their mean value.

very largest non-'new states' have predicted conflict likelihood of less than 2%. Table 2 explores this impact further by re-testing the FL model for different specifications of the 'new state' dummy, where Y is the number of years post-independence for which the dummy is coded 1 (hence, the original FL specification equates to Y=2). As in the Urdal re-test above, the significance of this relative time dummy drops off as its legacy time is increased, but for the FL dataset the effect is much more rapid and drastic. In the original specification, Y=2, the new state dummy is the most significant predictor in the results, but as Y is increased, this drops off rapidly, and at Y=5, the variable is of only marginal significance.[3] This is particularly worth noting because typically theories of regime transition see the first election under a new regime, usually held after four to five years, as a key hurdle at which conflict often (re-)emerges (e.g. Gates, et al. 2006); we might thus expect a 'new state' dummy to retain its significance at least for five years.

Table 2: FL re-tests with varying definitions of 'new state'

	Y=2		Y=3		Y=4		Y=5		Y=6	
Ongoing war	-0.974	***	-1.002	***	-1.009	***	-1.004	***	-1.004	***
	(-3.04)		(-3.18)		(-3.21)		(-3.20)		(-3.19)	
GDP per capita (lag)	-0.344	***	-0.350	***	-0.356	***	-0.355	***	-0.355	***
	(-4.79)		(-0.49)		(-4.90)		(-4.88)		(-4.87)	
Population (log. and lag)	0.263	***	0.256	***	0.247	***	0.245	***	0.245	***
	(3.62)		(3.52)		(3.39)		(3.37)		(3.36)	
Mountainous Territory	0.219	**	0.218	**	0.219	**	0.221	**	0.221	**
	(2.58)		(2.58)		(2.59)		(2.61)		(2.61)	
Non-contiguous Territory	0.443		0.459		0.476		0.473		0.471	
	(1.62)		(1.67)		(1.73)		(1.71)		(1.70)	
Oil	0.858	***	0.859	***	0.873	***	0.876	***	0.877	***
	(3.07)		(3.09)		(3.14)		(3.15)		(3.16)	
New State	**1.709**	*******	**1.162**	*******	**0.772**	******	**0.645**	*****	**0.567**	
	(5.05)		**(3.54)**		**(2.38)**		**(2.11)**		**(1.95)**	
Instability	0.618	**	0.551	**	0.412	*	0.498	*	0.494	*
	(2.63)		(2.36)		(2.21)		(2.15)		(2.14)	
Polity	0.021		0.022		0.024		0.024		0.024	
	(1.24)		(1.34)		(1.43)		(1.44)		(1.46)	
Ethnic Frac.	0.166		0.196		0.200		0.199		0.194	
	(0.45)		(0.53)		(0.54)		(0.53)		(0.52)	
Religious Frac.	0.285		0.300		0.338		0.349		0.352	
	(0.56)		(0.59)		(0.67)		(0.69)		(0.70)	
Constant	-6.731	***	-6.633	***	-6.522	***	-6.509	***	-6.513	***
	(-9.15)		(-9.03)		(-8.88)		(-8.85)		(-8.82)	
N	6327		6327		6327		6327		6327	
Pseudo-R2	0.1080		0.0995		0.0947		0.0938		0.0933	

Source: Authors' calculations from FL replication dataset. Original FL model equates to Y=2

[3] Replacing the 'new state' dummy with a time decay function likewise does not substantially affect the significance of the main explanatory variables and the decay function itself is only marginally significant, whatever the value of X. Results are not reproduced here for space.

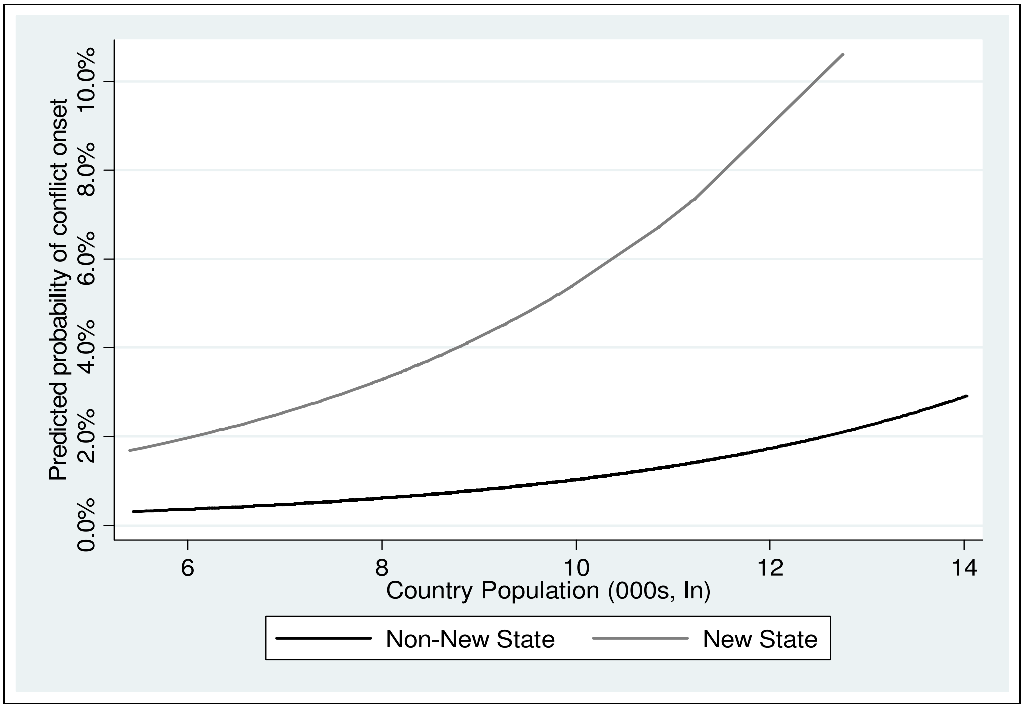

Figure 1: Post-estimation impact of population size on conflict incidence in FL model, disaggregated by 'New State' dummy
Source: Authors' calculations, based on FL replication dataset

An alternative approach to handling relative time, adopted in a number of recent studies, is to utilize a different regression equation, demonstrated for the FL model in Table 3. The most commonly used is the Cox proportional hazard model. The Cox hazard model, widely used in medical research to estimate survival times follow disease interventions, represents a useful advance in dealing with relative time because it links country-year observations within the country-case, and handles 'censored' cases – those which 'exit' the study without 'failure', but which may nonetheless fail in the future – more appropriately. While use of the Cox method represents a methodological improvement, however, it typically has remarkably little impact on model results (see Column 2 in Table 3).[4] Given the logit results discussed above, which suggest a very clear role for relative time, this may seem strange, but an answer can be found in contrasting the particular assumptions of the Cox model with these logit findings. Putting aside for a minute the issue of problems in the operationalization of the dependent variable, typical logit findings suggest a strongly significant but rapidly diminishing legacy of a 'foundational event', such as war or the creation of a new state, on the likelihood of subsequent conflict. But this is unlikely to show up in a Cox regression because one of the assumptions behind the

[4] Re-estimations using the Cox model were also performed on Urdal (2006) and Walter (2005). Both re-tests also produced no significant changes in outcome.

Cox hazard model is that the impact of the independent variables on the likelihood of survival does not vary over time (hence, the *proportional* hazard model). In other words, while the Cox hazard model usefully ties together successive years within country cases, it does so at the cost of assuming that the impact of independent variables does not vary across (relative) time. The medical analogy is useful here; if we consider democratization as an 'intervention' which may reduce the risk of conflict (assuming a simple binary value 1=democracy, 0=no democracy), the Cox model tests the strength of this intervention across time-linked within-country observations, but assumes that this impact is constant – if democracy reduces the likelihood of conflict by 5 per cent after 1 year, it will also do so after 2 years, 3 years, and so forth.

Alternative survival models are available that posit differential impacts over time between the independent variable and the likelihood of survival. Gates et al (2006) use a log-linear survival ratio function that assumes initially increasing and then decreasing risk. Here, however, one must specify in advance the expected relationship. Column 3 in Table 3 replicates the FL model using this function. Again, the model remains largely unchanged. The value of gamma (<1.0), estimated by the regression programme, indicates that the hazard function is indeed distributed log-linearly, initially increasing and then decreasing.

Table 3: FL Model Re-estimates

	Logit		Cox		Survival Ratio Model I	
Ongoing war	-0.974	***	1.856	*	-0.860	**
	(-3.04)		(2.14)		(-2.55)	
GDP per capita (lag)	-0.344	***	1.345	***	-0.338	***
	(-4.79)		(5.01)		(-4.55)	
Population (log. and lag)	0.263	***	0.798	***	0.238	***
	(3.62)		(-3.17)		(3.30)	
Mountainous Territory	0.219	**	0.834	**	0.214	**
	(2.58)		(-2.49)		(2.56)	
Non-contiguous Territory	0.443		0.845		0.360	
	(1.62)		(-0.66)		(1.29)	
Oil	0.858	***	0.464	***	0.797	***
	(3.07)		(-2.82)		(2.87)	
New State	1.709	***	0.163	***	1.561	***
	(5.05)		(-7.06)		(2.70)	
Instability	0.618	**	0.561	**	0.559	**
	(2.63)		(2.53)		(2.38)	
Polity	0.021		0.979		0.023	
	(1.24)		(-1.45)		(1.34)	
Ethnic Frac.	0.166		1.019		0.109	
	(0.45)		(0.06)		(0.29)	
Religious Frac.	0.285		0.805		0.281	
	(0.56)		(-0.50)		(0.56)	
Constant	-6.731	***				
	(-9.15)					
Gamma					0.654	***
					(-4.59)	
N	6327		6303		6303	
L.Likelihood	-480.4		-237.5		-474.6	

Source: Authors' calculations, based on FL replication dataset. Notes: Cox model give proportional hazards, and hence does not produce an overall constant; survival ratio model gives estimated time ratios for survival, such that coefficients greater than 1 indicated an increased chance of *survival* (i.e. a lower risk of war).

We have argued in this section, then, that recent attempts to control for the problem of passage of time and the interdependence of observations in econometric studies of civil war have utilized a variety of techniques that represent important methodological advances but that (fortunately enough) do not appear to impact significantly upon the results of the models tested. They do, however, point to the particular importance of the immediate legacy of the 'foundational event' for the country-case as a major determinant of conflict. We return to this issue in section 4. First, we will deal with a second problem – that all these techniques, while successfully incorporating the impact of relative time, do not address more fundamental epistemological problems relating to the passage of absolute time.

3. *Wie es eigentlich war*: taking time absolutely seriously

Taking absolute time seriously, we will suggest here, entails a much more radical shift in the underlying ontologies of the models we produce and test. We rarely actually address the ontological assumptions on which quantitative analyses are based. We can draw an analogy here with the debates over the postmodern 'attack' on history. Mary Fulbrook has argued that it is a common but misguided belief among practising historians to think it is possible to ignore these philosophical debates over the nature and reliability of historical inquiry in favour of just getting on with '"doing history", exploring the archives, trying to find out as best they can "what really happened" or "how it really was" [wie es eigentlich war, in Ranke's famous dictum]' (Fulbrook 2002: p.4). They are misguided, she argues, because they will, nonetheless, inevitably be 'working within bodies of assumptions of which they may be more or less aware' (Fulbrook 2002: p.4). One gets the impression that some quantitative studies of conflict proceed in a similar manner, "exploring the data" to find out "how it really is". Neither is this analogy to historiographical debate entirely in vain, as we want to suggest later that it may be more productive for quantitative analysis to be thought of as a form of historical inquiry.

So, how can we characterize the (implicit) ontology of conflict underlying mainstream quantitative analysis? Quantitative analyses of conflict incidence typically generate or test positivist 'law-like' propositions about the conditions that predispose towards conflict. Combining country-year observations into panel data over as extended a period as possible generates conclusions that are held to be valid for at least that time period as a whole. In reality, however, conclusions drawn are rarely expressed even with such limitations; they are expressed as general statements along the lines of 'variable X has been shown to increase/decrease the likelihood of civil war'. The fact that these propositions are expressed probabilistically – for instance, *ceteris paribus*, a lower GDP per capita will not *necessarily* lead to civil war, but does leave a country more vulnerable to civil war – renders them no less positivist.

Our intention here is not to launch a full-blooded attack on positivism in social science; this debate has been engaged with far more vigour and insight on both sides than we can muster here. Our plea, rather, is that we must at least take the acknowledged limitations of such an ontological stance seriously, and a key issue here relates precisely to the passage of time and the extent to which such 'law-like propositions' can be taken to hold over the *longue durée*. Few modern philosophers of social science would still defend Hempel's stand on the 'necessity' of 'extensive use of universal hypotheses' in social scientific research (Hempel 1942: p.48); modern sentiments seem, rather, more in line with Giddens' (1979) assertion that such generalizations as we can make in the social sciences are historically bounded.

Yet the implication of most econometric studies of civil war is precisely that there are such 'universal' time-independent causes of civil war, however probabilistic they may be. A notable exception here worth considering is Ted Gurr and Will Moore's early analysis of the Minorities At Risk dataset (Gurr and Moore 1997), which we discuss further below.

At this point, we should address a possible objection the claims made here. Surely, it might be claimed, many studies do indeed take the passage of absolute time into consideration, as witnessed by the 'Cold War dummy' we referred to earlier. Collier and Hoeffler (2004), for instance, include such a dummy – although, as we discuss below, giving it a rather counter-intuitive interpretation – and find it insignificant in explaining the incidence of civil war, while Bethany Lacina (2006) finds it a significant correlate of the *severity* rather than incidence of conflict. Gates et al (2006) conduct a *longue durée* study of regime duration that includes five different dummies for successive four-decade periods between 1800 and 2000, all of which yield significant results. However, the inclusion of absolute time dummies in other datasets appears to have little impact upon results, as shown in Table 4, which shows the FL model in their original specification (Model I) and re-run with the addition of a Cold War dummy (cw), coded 1 for the years 1945-1988, and 0 for the year 1989 and onwards (Model II). The Cold War dummy is marginally significant (P>|z|=0.036), and its inclusion has only minor impacts on the significance of two other variables: the dummy variable for geographic non-contiguity which creeps just within normally accepted bounds of significance (P>|z|=0.049), while the variable for political instability loses some significance. Figure 2 shows the post-estimation graphing of the impact of population size on conflict incidence broken down by the Cold War dummy.

Table 4: FL Replication, models 1-2

	I		II	
warl	-0.954	***	-1.038	***
	(0.314)		(0.318)	
gdpenl	-0.344	***	-0.363	***
	(0.072)		(0.074)	
lpopl1	0.263	***	0.258	***
	(0.073)		(0.073)	
lmtnest	0.219	**	0.219	***
	(0.085)		(0.084)	
ncontig	0.443		0.549	*
	(0.274)		(0.279)	
Oil	0.858	***	0.798	***
	(0.279)		(0.282)	
nwstate	1.709	***	1.720	***
	(0.339)		(0.337)	
instab	0.618	***	0.566	**
	(0.235)		(0.237)	
polity2l	0.021		0.015	
	(0.017)		(0.017)	
ethfrac	0.166		0.121	
	(0.373)		(0.374)	
relfrac	0.285		0.185	
	(0.509)		(0.511)	
cw			-0.492	*
			(0.235)	
_cons	-6.731	***	-6.227	
	(0.736)		(0.771)	
Pseudo R	0.108		0.112	
N	6327		6327	

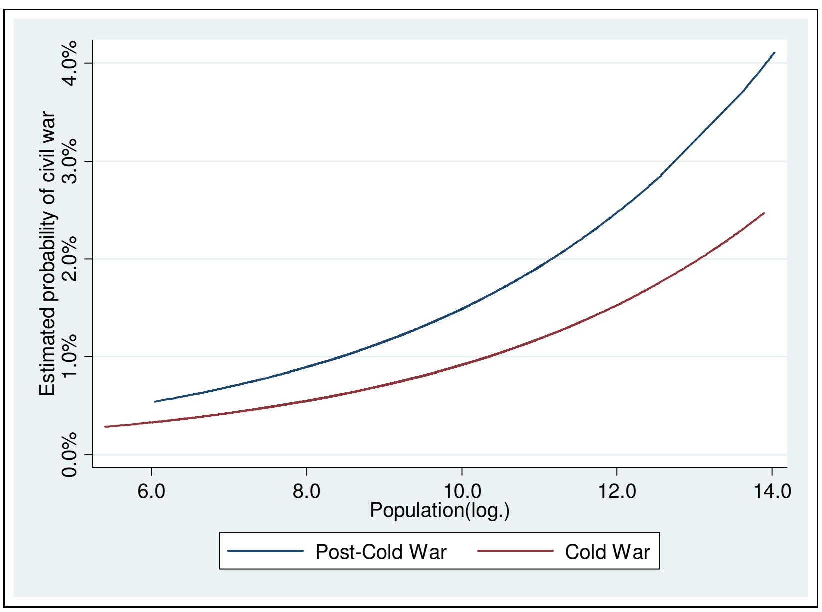

Figure 2: Post-estimation of Cold War dummy for FL model

If the inclusion of absolute time dummies typically has statistical significance without seriously affecting the significance of other variables in the model this, it might surely be claimed, is job done. Not quite, and here's why. Absolute time dummies such as the Cold War dummy simply introduce an extra variable or variables into the vector of predictors of conflict likelihood, which tell us whether there was a statistically significant 'extra' (positive or negative) chance of conflict, *ceteris paribus*, during the particular period.[5] For interpretation purposes, this is highly unsatisfactory; it does not give us any idea *why* the chances of conflict were higher (or lower) during this period, unless we indulge in speculative assignment of interpretations; Collier and Hoeffler, for instance, interpret their Cold War dummy as a proxy for the availability of outside funding to rebel organizations, but this is surely only one of many possible impacts of the Cold War – and not even, it seems to us, a particularly important or strong one. Indeed, if we are in the business of searching for generalizations invariant of absolute time, we would ideally find other independent variables that were correspondingly higher or lower during the Cold War and that, when entered into the regression, render the Cold War dummy insignificant. Put this way, within the context of a quest for absolute time-invariant generalizations, statistically significant absolute time dummies can be seen as circumstantial evidence of 'missing' variables.

But what if there was something about the Cold War period that impacted upon conflict incidence that defies disaggregation into country-year-level socio-economic observations – for instance the Realist 'bipolar' configuration of the international system? If this were the case, even the most exhaustive datasets would not be able to render the Cold War dummy insignificant. We might then simply take the Cold War dummy at face value, but give it a more theoretical interpretation – as, for instance, a 'bipolar international system dummy'. But this still strikes us as somewhat unsatisfactory, for two reasons. Firstly, as we have already alluded to, such absolute time dummies are open to a whole range of reasonable interpretations and the data do not really provide us with a way of preferring one interpretation to others. The Cold War dummy may seem intuitively interpretable, but what about such dummies as the Gates et al five-fold four-decade dummies? These are much harder to pin to a reasonable intuitive theoretical interpretation. Moreover, it seems unreasonable to attribute the significance of these dummies to the specific time-periods, given their arbitrary definitions. Surely, however, the same thing could be

[5] This 'extra' chance is, of course, subject to transformation by the regression equation as depicted in Figure 2.

said of the Cold War dummy. From a statistical perspective, all such dummies really tells us is that there was (or, if insignificant, wasn't) a statistically higher (or lower) chance of conflict onset between time t_1 and t_2.

Moreover, they don't allow for the interesting possibility that the passage of absolute time might not only be reflected in *ceteris paribus*, plus or minus, additional risks of conflict, but that the significance or even polarity of other independent variables may shift over time. What if, for instance, there was something about the configuration of the international system during the Cold War that meant that oil-exporting countries were more prone to civil war (say, in a plausible interpretation along Collier/Hoeffleresque lines, because the major powers competed for access to petrochemical resources by sponsoring rebel groups in resource-rich countries not politically affiliated to them) but the post-Cold War era rendered the same countries less prone to civil war (say, because after the end of the ideological conflict of the cold war, all major powers found it less risky to compete for access to resources simply by competing to provide institutional and developmental support to the incumbent government of resource-rich countries, whatever their political affiliation). This type of absolute time scenario would, *ceteris paribus*, be likely to produce diametrically opposed results for an oil exporting variable if tested separately on the Cold War and post-Cold War periods of absolute time, something which would be entirely missed if absolute time were only included through a Cold War dummy.

This leads us to our second point, which is that by adopting an (implicit) ontology that privileges time-invariant generalizations and thus providing such *post facto* 'theoretical' interpretations of statistically significant absolute time dummies, we are in fact closing ourselves off to potentially fruitful and interesting avenues of inquiry. The question of *why* there was a significantly lower chance of conflict during the Cold War is surely much more interesting than simply the satisfaction at having 'explained' the passage of absolute time through the use of a dummy variable. The prospect that even some of our strongest findings of correlates of conflict may not hold as valid now as they did in the past, or vice versa, should be tantalizing, rather than a matter of concern. This means taking the passage of absolute time much more seriously.

One way of getting a glimpse at the possible impact on the passage of absolute time on econometric results is simply to re-run existing models over a restricted period of time. Here, we replicate the FL model, using the basic logit configuration, on successive rolling 20-year periods, i.e. for the 20 years beginning 1945, for the 20 years beginning 1946, and so on up to the period 1980-1999. Figure 3 shows the resulting z-stats for four of the main independent variables which have a high level of significance in the FL model – oil dependency, GDP per capita, population size, and political instability. It is important to note that as the z-stat is a function of, among other things, the number of cases under observation, it would not be appropriate to compare these results directly with the z-stats for the original FL model, as the latter obviously includes many more observations – around 6,000 versus between 1,500 and 2,000 for each of the periodized re-tests. It seems reasonable, however, to compare across these time-bounded periodizations, as they consist of broadly similar numbers of observations and, indeed, largely overlap in the actual observations included.

GDP per capita, which is one of the strongest predictors of civil war in virtually all econometric models including FL (cf. Hegre and Sambanis 2006), retains the normal benchmark of statistical significance ($|z|>2$) in every period, with a brief exception for the 1950-1969 periodization. Moreover, there is a clear increasing trend; later periods see considerably greater significance for this variable than earlier periods. The population size variable remains fairly constant at a marginal rate of significance for the first half of the periodization, only gaining clear significance for periods starting after around 1965; the political instability variable never really attains significance. But the most startling result here is the oil dependency dummy, which for the first periodizations until around 1960-1989 comes out as very significant, but which then rapidly drops off to clear insignificance in later periods. Neither does this drastic change in the significance of an independent variable over different periods of absolute time appear that unusual. Figure 4 replicates this analysis for the main variable in Urdal's study of the impact of demographic youth bulges on the incidence of civil war. In this case, while the youth bulge measure is a consistent and strongly significant positive correlate of civil war incidence for all 20-year periods up to around 1970-1989, in subsequent periods the z-stat drops off sharply and by the last periods, the variable is nearing significance as a negative correlate.

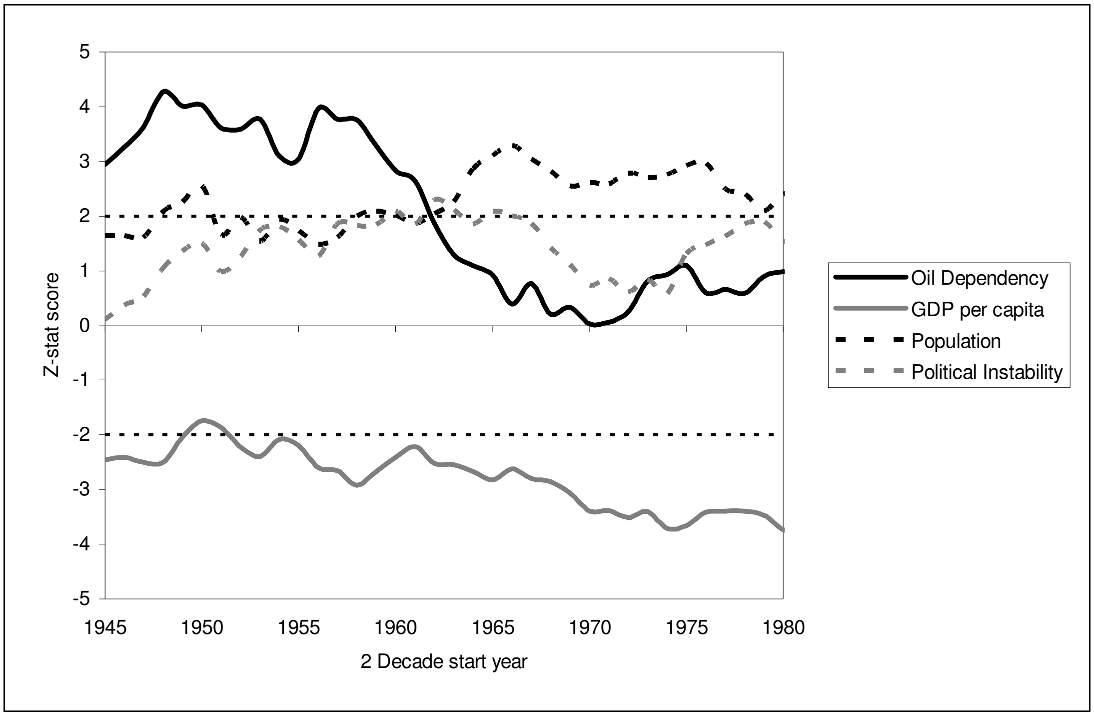

Figure 3: FL model by rolling 20-year periodization

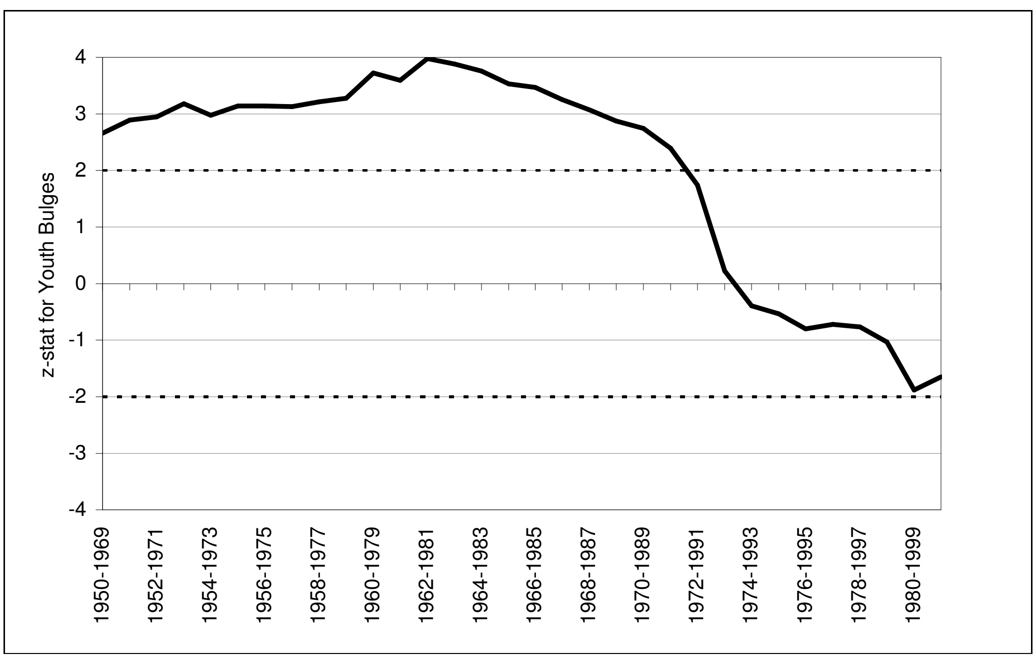

Figure 4: Urdal model by rolling 20-year periodization

Table 5 shows the complete results of a slightly different re-test of the Urdal model; the first column shows the original model, the second column the model restricted only to Cold War era observations and the last column restricted only to post-Cold War observations. There are clearly major differences in the model outcomes, not least of which is the main predictor of interest to Urdal, Youth Bulges, which – as we might expect given the previous results – is more strongly positively significant in the Cold War only model than in the overall model, but is negatively correlated at a weak level of significance in the post-Cold War era. Similarly, the very strong role for total population as a predictor vanishes in the post-Cold War era.

Table 5: Urdal re-test by Cold War periodization

	All Cases		Cold War		Post-Cold War	
Youth Bulges	0.474	**	0.120	***	-0.062	*
	(0.019)		(0.030)		(0.029)	
Total Population	0.204	***	0.285	***	0.162	
	(0.057)		(0.072)		(0.094)	
Infant Mortality Rate	0.004	***	0.007	***	0.018	***
	(0.001)		(0.002)		(0.004)	
Polity Index	0.012		0.036	*	-0.028	
	(0.014)		(0.017)		(0.024)	
Polity-squared	-0.011	***	-0.004		-0.021	***
	(0.003)		(0.003)		(0.005)	
Peace Years	1.920	***	1.672	***	1.531	***
	(0.296)		(0.406)		(0.448)	
Constant	-6.870	***	-10.616	***	-3.083	***
	(0.874)		(1.347)		(1.167)	
Pseudo R	0.086		0.098		0.129	
N	5331		3865		1466	

4. Implications: towards a pragmatic, historical empiricism

What should we make of these results? We do not want to suggest that they invalidate the methods or findings of quantitative analysis completely – the bathwater may get mucky with the passage of time, but the baby is still quite healthy. What we do want to suggest is that it compels us to be more careful in the way we think about quantitative studies, their epistemological underpinnings and their relationship to qualitative research. Earlier, we hinted that quantitative analysis may best be conceptualized as a form of historical research, as a kind of systematic, large-N comparative study which is located within a more nuanced understanding of the passage of time – of, to put it bluntly, history. This, we want to suggest, drives us both towards a more *pragmatic* (in the philosophical sense) epistemological understanding of quantitative studies, and a greater engagement with qualitative literature – something that David Laitin himself calls for but in a slightly different way (Laitin 2006).

In fact, this does not entail too radical a shift in our positions, and we think we can discern something of what we are advocating in Ted Gurr's work on Minorities at Risk, particularly his quantitative analysis with Will Moore alluded to above (Gurr and Moore 1997). In this article, Gurr and Moore generate a model of ethnic rebellion based on the Minorities at Risk data for the 1980s and use this to make 'predictions' about the groups most likely to rebel in the early 1990s. With the benefit of hindsight, they are then able to compare these predictions against the actual level of rebellion among these groups, and discuss reasons why particular groups 'predicted' to be likely rebels in the early 1990s did not actually rebel. This might be read as 'theory-saving' *post facto* excuses, but we suggest a more charitable reading in line with the kind of approach we wish to advocate here.

Gurr and Moore, we suggest, can be seen as operating within a fundamentally *pragmatic* social scientific philosophical orientation (Baert 2005; Rorty 1979), in which the purpose of the analysis is not to uncover or test 'universal' hypotheses about the causes of rebellion, but rather to identify time-bounded 'useful truths' – particular cases and variables based on the systematic analysis of previous experience, that may provide insights into potential problem areas in the future. Indeed, an imputable underlying philosophical pragmaticism is combined in their analysis with a very practical pragmatism about the uses of such forms of enquiry. Hence, Gurr and Moore conclude that a 'general statistical modelling approach cannot provide us with predictions of ethnopolitical violence, but coupled with monitoring resources, it identifies both risky cases and the variables that need to be observed'. Of course, many subsequent econometric analyses have been undertaken with policy implications in mind – Paul Collier's work, among others, has strong links to the World Bank – but the way in which these analyses are undertaken, and the generality of the conclusions that are thence derived, betray a far more universalist ontological underpinning, as is evidenced by numerous pronouncements in Collier's magisterial *Bottom Billion*.

Gurr and Moore engage with qualitative assessments of their model's 'failures', then, to demonstrate that such models can be *pragmatically* useful, even if they have acknowledged limitations in their scope of applicability. Similarly, Laitin (2006) shows how engagement between quantitative and qualitative analysis can shed new light on orthodox explanations of particular conflicts – in particular, he suggests a novel re-interpretation of the roots of the Sri Lankan civil war in the light of quantitative research on ethnic diversity and conflict. We find both of these useful approaches, but what we want to encourage here is that such engagement should run both ways. To do so, we return to the issue alluded to above of what, precisely, the conflict 'event' should be understood as – and hence coded for – in quantitative research.

Our travels through time have driven us to suggest that we need to take a closer look at the 'events' which constitute our dependent variable. These various ways of looking at this issue of relative time pointed to the initially unsurprising conclusion that various forms of political transition – whether the emergence of a new state, a sharp transition in polity type, or the end of a civil war – leave a country statistically more liable to experience civil war or violent internal conflict in successive years. What is perhaps more surprising from these results is the speed of the 'decay' of this impact, such that within a few years the effect all but evaporates, whatever measure one uses. This, at least, is the usual interpretation of such results. We want to suggest here another, more fundamental, re-interpretation, which can be gained through a return to the qualitative narration of the conflict cases underlying these datasets. In particular, these results have suggested that there might be something worth examining qualitatively in those cases of conflict 'onset' that occur relatively soon after the end of a previous conflict episode.

There are 15 conflict onsets in the FL dataset that occur when the 'New State' dummy is equal to 1. We do not have the space here, or the in-depth qualitative understanding, to consider all these cases. We will instead simply point to two particular cases that we are more familiar with and that raise the issues we are concerned with. Firstly, the outbreak of the civil war in the Southern Philippines. In the FL dataset this outbreak, coded for 1972, ranks as a confirming observation for the instability hypothesis, because 1972 coincided with Marcos' declaration of Marshal Law, which is coded in the Polity Index as a drastic change from 2 to -9. While the qualitative literature on the conflict certainly affirms that the transition to Marshal Law was a major springboard for the escalation of the conflict in the South, it is also clear from this literature that the conflict was already pronounced prior to the declaration of Marshal Law and, indeed, one of the major justifications given by Marcos for his authoritarian turn (aside from the impending end of his constitutionally final second term as President) was the instability in the South (McKenna 1998; Noble 1981). At least in the official chronology of the Marcos administration, Fearon and Laitin have got the causality wrong: the cause of the polity transition (read 'instability') was, in fact, the onset of conflict, which simply had not yet reached the battle death threshold for their dependent variable. Secondly, the 'onset' of a conflict in Indonesia in 1950 can hardly be treated as a separate event from the gaining of 'New State' status in 1949 and 1950, because the rebellion in question – the attempted secession of the Republik Maluku Selatan – was essentially a continuation of the war of independence by a group that had sided with the Dutch during the war and which hence refused to recognize the 'new' Indonesian state; it was, in Richard Chauvel's terms, 'not a revolution, but a counter-revolution' (Chauvel 1985).

These may seem like minor points – and indeed the omission of all new-state conflict onsets from the FL dataset does not affect the other predictors substantially. But what we want to suggest is that the engagement of qualitative and quantitative assessments of the passage of time in civil war analysis suggests that the quantitative coding for war is simply not sufficiently well operationalized. In particular, while a minimum threshold of battle deaths in one year may provide a useful proxy for the onset of a civil war, it is more problematic to see the (sometimes brief) absence of such levels of conflict as indicating the 'end' of the war, or at any rate, the 'known predisposition thereto', in Hobbes' terms. More prosaically, conflict 'events' which 'begin' relatively quickly after the cessation of a previous 'event' – whether a previous war or regime transition – may not, in fact, be separable 'events'. A similar case can be made with respect to the passage of absolute time, taking, for instance, the remarkable loss of significance for the Oil dummy in recent periodizations of the FL dataset. It will only be through engagement with qualitative literature, we suggest, that we will be able to understand this trend and feed it back into improved quantitative analysis.

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