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Relative Cohort Size and the Risk of Civil War, 1961 – 2001

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EXTENDED ABSTRACT: It has been suggested that countries with youthful populations are more civil war-prone (Davies 1969; Goldstone 1991; Hammel and Smith 2002; Huntington 1965; Mesquida and Wiener 1999), but a recent flurry of econometric studies of civil war have generally *not* found youth to be an important factor in conflict onset. I demonstrate that one likely reason for the contradictory evidence on youth and civil war is the poor conceptualization of the relationship between the two in prior research. To label the ratio of men aged 15 to 24 to adults a "youth bulge"—as some major studies have done (cf. Cincotta, Engelman, and Anastasion 2003; Goldstone, Gurr, and Harff 2000; O'Brien 2002; Urdal 2002)—is a misnomer. A bulge in the age pyramid refers to an age group larger than those above and below it. A better conceptualization of the linkage between youthful populations and civil war should include Easterlin's *relative cohort size* (RCS) hypothesis, which posits that the size of one's birth cohort is inversely related to one's opportunities in the job market, education, and marriage (1987).

Using a logistic model of civil war onset worldwide, I find that although several measures of youthful age structure are significant in the pooled cross-section analysis, first-differenced measures of youthful age structure involving RCS generally outperform other first-differenced measures of youth. In other words, changes in the ratio of young adults to a set group of older working adults is more closely linked to the onset of civil war than changes in the ratio of young people to the entire adult population. This finding is consistent with both the literal meaning of the term "youth bulge" and with the RCS hypothesis. The relationship between civil war onset and changes in RCS appears robust to a variety of controls. Hence contemporary research on civil wars could likely be improved by measures of RCS and by a more precise conceptualization of the link between age structure and conflict.

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[1] This study grew out of my efforts to replicate Henrik Urdal's results (2002) on age structure and civil war onset. I have substantially revised the methods and aims of the study but I remain indebted to Henrik Urdal for sharing his data and for discussing his methods with me. All errors are of course my own. I also wish to thank Ron Lee, Bernardo Lanza Quieroz, Warren Sanderson, and Bryan Sykes for methodological consultation; and Brian Arthur, Christian Dorsch, Jennifer Johnson-Hanks, Gene Hammel, Ron Lee, Isolde Prommer, and Vegard Skirbekk for useful feedback on earlier drafts.

I. Introduction

> *The majority of deadly human violence is, and seems always to have been, committed by young adult men and directed toward other men. Whenever and wherever there is violence, whether individual or collective, young and usually unmarried males are most often the perpetrators.* (Mesquida and Wiener 1999: 181)

Young men are most commonly the perpetrators (and the victims) of armed conflict worldwide. Whether for biological reasons such as evolution and hormones, gender-specific socialization, or the institutional demands of armies and militias, the relationship between young men and violence appears to have held throughout history. But does this imply that populations with higher concentrations of young men more prone to war? Scholars have occasionally proposed a link between youthful populations and war. Herbert Moller, for example, suggested that wars in pre-modern and present-day Europe, including the rise of the Nazi party in Germany, corresponded with surges in the proportion of young men (1968). Yale historian Paul Kennedy argued that revolutions occur more often in countries with large populations of "energetic, frustrated, young men" (1993: 34). Other authors have hypothesized a connection between youthful populations and the outbreak of violence on theoretical or quantitative grounds (Cincotta, Engelman, and Anastasion 2003; Goldstone 1991; Goldstone 2001; Hammel and Smith 2002; Mesquida and Wiener 1999; Urdal 2002).

Yet a recent flurry of econometric literature on civil wars has found few links between youth and the onset of civil war. Collier & Hoeffler's landmark work on greed and grievance in civil war (2000; 2001; 2002), along with work by Fearon and Laitin (2003) has found the opportunity costs of insurgency—based on dependence on primary commodity exports and a rough terrain which lends itself to guerilla movements—to be of key importance, while age structure mattered little in the onset of civil war. The State Failure Task Force, a group of scholars originally commissioned by

Vice President Al Gore, found a link between youth bulges and state failure but does not mention the finding in the text of their report (Goldstone, Gurr, and Harff 2000).

This paper is an attempt to unravel the contradictory assertions about the relationship between youth and violence by introducing a more precise measurement of youthful age structure: *relative cohort size* (RCS). RCS, a concept pioneered by Richard Easterlin (1978; 1968; 1987) refers to the relative size of two birth cohorts (for example the size of the generation born from 1947 to 1962 as divided by the size of the generation born from 1935 to 1946). I argue that Easterlin's relative cohort size hypothesis may provide the missing link between youthful populations and the economic and psychological frustrations that enable political instability and ultimately civil war. As a large relative cohort comes of age, the frustration and tension produced by lack of success in the job market and on the marriage market may—in the presence of other factors—render armed conflict a more appealing option.

Recent conflicts appear to be no exception to Mesquida and Wiener's observation at the start of this paper: anecdotal evidence certainly lends credence to the "excess young male" hypothesis in Rwanda. Gourevitch describes how Rwandan génocidaires were recruited from among the jobless young men who were "wasting in idleness and attendant resentments… Most of the men were motivated by the opportunity to drink, loot, murder, and enjoy higher living standards than they were previously accustomed to" (1998: 93). And in Sierra Leone, where young people comprised 95 percent of the fighting forces in a recent civil war, an NGO official explained that the youth are "a long-neglected cohort; they lack jobs and training, and it is easy to convince them to join the fight" (Mastny 2004: 19). Even though population growth has slowed worldwide and will likely end within the next century (Lutz, Sanderson, and Scherbov 2004), the lagged effects of fertility booms in Africa and the Middle East will continue to bring increasingly larger cohorts of young adults in the decades to come.

Two side notes are in order. First, it is important to separate proximate and distal causes of conflict. This paper does *not* claim that relative cohort size is the proximate or inciting cause of conflict. Linking political violence to any particular variable is difficult because political instability alone does not always result in violence. Rather, this paper seeks to explore the youthful demographic as an important factor that sets the stage for civil war. On the surface, civil wars (particularly in Africa) seem to be about ethnicity or resources or religious differences, yet the civil war literature has helped illuminate that this is rarely the true "cause" – merely a way in which populations have been mobilized (cf. Fearon and Laitin 2003; Leonard and Straus 2003). Hence a better understanding of distal factors leading to civil war may improve our ability to prevent it in the future.

Second, this paper is not an attempt to "blame" young men for war. Violence is also perpetrated by older men, women, and unforgettably child soldiers in many recent civil wars. Rather this paper is an attempt to *understand* how youthful dissatisfaction combined with deleterious economic opportunities and overwhelming frustration can make violence an appealing option for some young men who have few other opportunities.

Following this introductory section, section two introduces the concept of relative cohort size, section three discusses hypothesized linkages between relative cohort size and violence, section four describes my study and section five gives my results.

II. Relative Cohort Size

Nearly all empirical researchers of youth and conflict—including the most prominent and influential—have measured youth either as the ratio of men aged 15-24 to the entire male population (Collier and Hoeffler 2001; Fearon and Laitin 2003) or as the ratio of young people to adults (Choucri 1974; Cincotta, Engelman, and Anastasion 2003; Goldstone 2002; Goldstone, Gurr, and Harff 2000; Mesquida and Wiener 1999; O'Brien 2002; Urdal 2002). Although in some latter cases the authors chose a specific age range instead of the general group of "all adults," it was surprising how little thought was given to the measure of youth (with the obvious exception of Urdal, although even he settled on a ratio of youth ages 15 to 24 over all adults).

Huntington hypothesized that a critical threshold was reached when the proportion of young people aged 15-24 reaches 20% of the population in a country (Huntington 1996). Mesquida suggested extreme stress is reached when young people aged 15-29 reach 40% of the adult population.[2] But what do these thresholds mean? Do these ratios measure a "youth bulge," or some other quantity? Does it matter whether the remaining population is primarily comprised of slightly older adults, much older adults, or the elderly?

The notion of a "youth bulge" originally referred to the age pyramid's graphical illustration of a population by age and sex. When the cohort of youth is so large as to render its portion of the pyramid wider than the categories above and below it, we can say there is a youth bulge.[3] A youth bulge is usually caused by a sharp rise in fertility, such as the post-World War II baby boom, but can also be caused by a decrease in infant mortality rates, immigration of young adults, or the impact of HIV/AIDS on early adulthood mortality.

I propose that what we are really interested in is how well the government, schools, and the economy can accommodate the incoming cohort of teenagers. Thus, one useful estimate is to measure the current group of young adults (i.e. 15 to 24 years old) and compare them to the cohort of older working adults (25 to 34 year olds). According to Easterlin, RCS can be best approximated by the crude birth rate in the years surrounding the birth of the cohort. RCS is inversely proportional to the birth rate. However for the purposes of this paper I use the term RCS to refer to the ratio of two age groups in the population (for example the 15 – 24 year old population

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[2] See Appendix 2H of Cincotta, Richard P., Robert Engelman, et al. 2003. "The Security Demographic: Population and Civil Conflict After the Cold War." Population Action International, Washington, DC.

[3] Because a "youth bulge" should technically be larger than the categories above *and* below it in the age pyramid, I will use the term "youthful age structure" and/or RCS to describe my own findings.

divided by the 25 – 34 year old population).[4] Combining data on RCS with information on economic growth and development should give us a good indication of how well the economy can absorb the incoming cohort of youth.[5] In some countries, a surplus of youth can actually enhance economic growth, if the youth are able to be absorbed into the economy. Some economists, for example, have used a surge in youth to explain the East Asian economic "miracle" (Bloom, Canning, and Malaney 1999; Bloom and Williamson 1997) . Thus knowing the relative cohort size alone is likely not enough, we also need to get a sense of economic growth, secondary schooling, and other factors to give us an idea of what Hammel and Smith (2002) call the "demographically-induced unemployment rate."

Relative cohort size is an appealing measure of youthful age structure because if the cohorts comprise an equal number of birth years and we count them at age 0—for example the number of babies born from 1961 to 1970 compared to the number of babies born 1951 to 1960—we have an intuitive sense that the ratio should be 1.0 if there were the same number of births in both periods. In a population growing at a constant rate throughout the two decades, the ratio would obviously be higher than 1.0. If we measure the size of these two birth cohorts at a given point in time (when they are different ages), their ratio will also be affected by age-specific mortality schedules (likely making the younger group even bigger) and by immigration (which could affect the ratio either way). Thus the metric of RCS has an intuitive meaning, unlike youth as a proportion of all adults or as a proportion of the entire population.

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[4] Measuring the crude birth rates in the period when the cohort was born as Easterlin suggests has two clear advantages over my definition of RCS: first, it controls for differential mortality of age groups; second, it eliminates consideration of immigration. However, because of the poor quality of historical fertility data in the developing world, I use the more easily-obtainable ratio of age groups in the population.

[5] Immigration will also be an important factor but due to data availability is not considered here.

III. Theoretical Linkages

Ryder's classic work on the cohort as a cause of social change (1965) outlined the concept of "demographic metabolism," whereby a population is continually adding new members, each of whom move along the age pyramid until they perish or migrate outward. Birth cohorts move together through the age pyramid in unison, undergoing socialization and assimilation into society simultaneously, experiencing key historical events at the same ages. Large birth cohorts can strain the capacity of schools when they are young and the labor market when they are older, particularly in developing countries if the government and the labor market are unable to respond to the large influx of young people. A recent psychological study showed that members of larger cohorts have less civic knowledge than members of smaller cohorts even in a developed country (Hart, Atkins et al. 2004), indicating that larger cohorts experience less socialization than smaller cohorts. Socialization in and of itself is a far cry from civil war. However a lack of socialization could imply higher rates of psychological detachment, which in turn could make a cohort more prone to violence. This section explores four major theoretical linkages between RCS and civil war: relative male income, relative deprivation, age- and sex-specific factors, and the microfoundations of rebellion.

A. Relative Male Income

Relative male income refers to the standard of living a man's income can by relative to his father's standard of living. The notion of relative male income was also pioneered by Easterlin (1978; 1968; 1987), who hypothesized that it was inversely related to relative cohort size. Easterlin's work often focused on the labor market impact of the baby boom cohort in the United States. Because the baby boomers were a much larger birth cohort than their parents' cohort, later baby boomers experienced a much tighter entry-level job market than early or pre-boomers. In this way, one's birth and fortune were interlinked: members of smaller cohorts generally had an easier time finding jobs and education, whilst equally well-qualified members of larger cohorts struggled to achieve the same standard of living.

Hence it could be argued that the rise in female labor force participation during the 1960s and 1970s was because of low relative male income (Macunovich 1996; 2002). Male baby boomers earned less relative to their fathers because they competed in a tight labor market where employers could afford to pay less and still attract qualified candidates. The resulting economic squeeze on baby boomers prompted women to enter into the labor force and delay or limit fertility in order to achieve as high or higher standard of living than their parents.

Not every society may respond the same way to low relative male income, but in this day and age large birth cohorts in any country must be accommodated by schooling system and eventually by the labor market. In African countries where the real standard of living has declined over the last twenty years, this is immensely difficult. Positive population growth, which continues even now, means that the size of each successive birth cohort will be larger than the previous one. The government will be required to increase expenditures on "congestible" services, such as roads, schools, hospitals, etc. to accommodate each new cohort. In the absence of economic growth, when the large birth cohort gains adulthood, they will require more jobs than were vacated by previous cohorts.

Without enough jobs and facing more intense labor market competition, the young adult cohort will experience a declined standard of living compared to their parents (Easterlin, Schaeffer, and Macunovich 1993) and may simply remain idle and unemployed. A study by Korenman and Neumark (1997) of economically-advanced countries over the 1970s to the 1990s attempted to isolate the effects of youth cohort size on unemployment. The authors found that large youth cohorts face increased unemployment, with elasticities as high as 0.6. Further, Bloom et al. (1987) found that the entry of relatively large cohorts into the labor market resulted in a decline of earning in these cohorts relative to older, smaller cohorts . This also tended to produce increased unemployment (more so in Europe) and decreased earnings (more so in the United States).

In a recent National Academy of Sciences report on terrorism, Hammel and Smith (2002) suggest that countries with a more youthful age structure might be more highly prone to terrorism. They propose that the difference between cohort sizes is a "demographically-induced unemployment rate" that delays adulthood for many youths in traditional cultures, causing idleness, sexual frustration, and economic insecurity. However, they did not attempt to test this hypothesis; instead they focused on Muslim fertility rates in countries where they were majority or minority populations.

B. Relative Deprivation / Rising Expectations

The relative cohort theory described above can be thought of as an instance of the sociological theory of relative deprivation and rising expectations. The notion of "relative deprivation" suggests that when there is a significant gap between expected and achieved welfare, frustration and aggression result (Kelley and Galle 1984). Davies articulated this thesis 35 years ago:

> Revolution is most likely to take place when a prolonged period of rising expectations and rising gratifications if followed by a short period of sharp reversal, during which the gap between expectations and gratifications quickly widens and becomes intolerable. The frustration that develops, when it is intense and widespread in the society, seeks outlets in violent action (Davies 1969)

Declining economic opportunities or an expanding urban population can thus also provide a source of tension and conflict (Goldstone 2001), particularly when combined with a large relative cohort. Youth migrating to urban areas in the hopes of finding employment may be dissatisfied with available opportunities and have few other options to consider. The feeling that there are few other opportunities combined with the will of one's comrades can certainly be a powerful inducement for civil war.

Choucri (1974) was one of the first and most important researchers of age structure and political violence. Although she did not employ a measure of relative cohort size,[6] her careful case study method lent her much insight into the causes of wars. She studied 45 wars, including 40 from the 93 wars in Africa, Asia, and Latin America between 1945 and 1969. These were not strictly "civil wars," rather they often involved some degree of outside intervention or outside aggression. Choucri found that age structure was either a "background factor," a "minor irritant," or a "major irritant" in 10 of 45 cases of conflict (meaning for the most part the conflict still would have happened in the absence of youthful age structure, but that somehow it exacerbated the problem.). Unfortunately her study suffered from selection on the dependent variable, but her careful use of the case study method still lent her important insight into the relative cohort phenomenon:

> The higher proportion of youthful population and the greater the unemployment, the greater are the possibilities of dissatisfactions, instabilities, and violence… this proposition finds considerable support among cases of local conflict in developing areas. For many social and psychological reasons, young populations can generally be more easily disposed to radical politics and guerrilla warfare, although clearly age is not the only determining factor. …. Often the problem of inducting a large number of young people into the work force is pronounced; the failure to successfully do so may then lead to radical youth movements. (Choucri 1974: 184)

Among the examples of conflicts Choucri cites that were exacerbated by an unsuccessful attempt to integrate young people into the economy were the Arab-Israeli conflict, the Palestinian conflict, the Algerian revolution, and the war between Guyana and Venezuela.

There is also evidence to suggest that large cohorts strain the resources of families and the public sector. Choucri found that the drain on resources caused by a large cohort intensified the internal instabilities in the Dominican Republic-Haitian conflict, and in Venezuela (1974).

One major question about relative deprivation is whether education makes young men more or less likely to commit violence. On the one hand, it has been suggested that less educated young

[6] Choucri officially measured the ratio of men aged 20 to 40 to the entire population, but her examination of cases seemed to encompass the notion of relative cohort size.

men face a lower opportunity cost of participating in violence because they have less to lose, and fewer more attractive alternatives. On the other hand it has been suggested that educated young men are more likely to be frustrated by a lack of opportunities because they have reason to expect good jobs and a decent standard of living. Many rebellions and insurgencies may be orchestrated by more educated members, with less educated occupying lower positions in the hierarchy. Huntington argues that educated men are more politically risky because they have higher aspirations and will be more likely to act out. He advises that governments of developing countries attempt to reduce the number of university graduates until sufficient demand exists for them (Huntington 1965).

The difficulty in examining secondary school enrollment as a proxy for education is that being currently *in* school reduces the chance of participating in a rebellion. Fearon and Laitin (2003) and Collier and Hoeffler (2001) find that male secondary school enrollment is so highly correlated with per-capita income that it is impossible to disentangle the effects. Thus the question of education and relative deprivation may remain unanswered until more micro evidence of participants can be gathered.

C. Age- and Sex-Specific Factors

For a host of physical and/or social reasons, young men seem to be particularly prone to violence. It is unclear whether this results from hormones or socialization—but the likely answer involves both. Testosterone levels are correlated with aggression (Dabbs Jr., Carr et al. 1995); moreover, gender theorists have also argued for decades that in most societies, men are socially conditioned to express their frustrations through violence. Young men, particularly in traditional societies, are socialized to fill the role of 'breadwinner' in order to start a family—and in a strictly religious setting, even in order to become sexually active (Hammel and Smith 2002). Thus youthful hormones when combined with social expectations and gender-role training may combine to make young men—particularly young men whose income expectations remain unfulfilled—a volatile force to reckon with.

D. Microfoundations of Rebellion

Grievance alone is not enough for civil war: groups must also be able to form a coherent collective identity with which to challenge state authority, and they must also find opportunities for collective action (Diehl and Gleditsch 2001). As Gates (2002) maintains, a key consideration in understanding how civil war begins is the issue of how to start and maintain a rebel movement. Walter suggests that enlistment is only likely to be attractive "when two conditions hold. The first is a situation of individual hardship or severe dissatisfaction with one's current situation. The second is the absence of any nonviolent means for change" (Walter 2004). It is much easier to recruit combatants when there is a large pool of unemployed and idle young men, such as in a large relative cohort. Youth face lower opportunity costs to participating in an armed rebellion, such as the general lack of economic dependents and the less desirable alternatives that they have to pursue. Choucri's case study documented that in conflicts in Cyprus, Palestine, Algeria, and Laos, a youthful age structure increased the size of the potentially mobilizable population, which in turn appeared to influence the intensity of the conflicts (1974: 191)

Youthful alienation can be a powerful motivation to join a rebel movement. Rebel groups can provide a "gang" type of social system. As ironic as it seems, being part of an armed group can in some cases provide more safety than not, psychological security and gratification, and sense of camaraderie (Keen 2000). As Moller articulated:

> The purpose and direction that young people find in movements of rebellion helps many to overcome the insecurity and hopelessness of a futile existence. The feeling of being able to cope with hardship and danger, the enjoyment of comradeship, and the acceptance of their peers is basic to a sense of identity in the young. Even belonging to an anti-social and destructive movement can have a salutary effect on the personality formation of a boy or girl, especially in times of social dislocation. (1968: 259)

Once a country is engaged in warfare, life becomes less predictable and the scope for rent-seeking predation increases. Thus it becomes more and more tempting for additional young men to join the ranks of rebel (and government) armies, to release their aggressions against one another.

IV. My Study

My study grew out of an attempt to replicate Urdal's study (2002) on youth bulges and the risk of civil war onset.[7] I use conflict data from the International Peace Research Institute [herein PRIO] in Oslo (Strand, Wilhelmsen, and Gleditsch 2003), which classifies internal and external conflicts from the period from 1946 to 2001. Although there are many datasets of civil wars, PRIO's was the most appealing because its threshold of deaths (25 as opposed to 1,000 in other datasets) did not bias against small states,[8] and because it did not attempt to distinguish ethnic wars from border disputes or other types of civil warfare—wars which may have different proximate causes but similar underlying causes.

I employ a logistic model to predict the probability of a civil war[9] onset during a given country *five-years* of time.[10] The onset of civil war is a binary variable taking on the value of 1 if the country had a civil war onset during those five years and a 0 otherwise. The five-year period was

[7] Many thanks to Henrik Urdal for sharing his data and discussing his methods with me. As I replicated Urdal's study and examined his methods in detail, I realized that I disagreed with a number of his model decisions and decided to do an entire study of my own rather than simply extending his work. In addition to objections described elsewhere in the text, (1) Urdal created his own conflict dataset which differed from the Uppsala data and thus was difficult to recreate, particularly in terms of dependent states and territories; (2) His variable of previous conflicts seemed to inherently biases later years (and thus I created a variable to indicate whether there had been a conflict in the previous ten years); (3) Centering of the youth variable was done on the entire sample rather than the sample valid for the regression, such that un-centering the term gave a different result than reported and (4) I disagreed with his decision to impute missing values of GDP and regime using the average value for the entire dataset. To err on the conservative side, I did not impute any values for GDP and regime and instead used only the data that were available.

[8] If anything PRIO's low threshold of conflict deaths may bias against large states, which will have a higher probability of meeting the threshold than smaller ones, and hence I include the log of population in my regression models. Eventually I aim to include information on the severity of the conflict to help control for this as well.

[9] The dataset defines an *armed conflict* as a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths.

[10] Originally I made this decision because the UN age-structure data from its 1998 revision were only available in 5-year groups and it seemed unwise to impute annual values as Urdal appeared to have done. More recently I have switched to the 2002 UN revision which now has annual age-structure historical data but I am still using five-year intervals to avoid the overstatement of precision.

only assigned a 0 or a 1 if I had complete data on the country over the five-year period and if the country was not currently involved in a conflict at the beginning of the period.

The PRIO dataset distinguishes between internal armed conflict and internationalized armed conflict. According to the data codebook, internal armed conflict occurs between the government of a state and internal opposition groups without intervention from other states, while internationalized internal armed conflict occurs *with* intervention from other states. Although Urdal chooses to restrict civil wars to the former, I find that this disturbingly excludes conflicts such as the recent civil wars in the Democratic Republic of the Congo and Angola because they have had international intervention. I therefore define four types of onset in my research, which I will discuss and compare in my final draft of this paper.

Some studies, such as that done by the State Failure Task Force (Goldstone, Gurr, and Harff 2000) distinguish between ethnic conflict and other types of conflict. Given that the evidence from Collier & Hoeffler and Fearon & Laitin suggests that ethnic wars are often motivated by other underlying factors, I do *not* distinguish ethnic wars from other wars. Nor do I distinguish colonial wars of independence from non-colonial wars. Wars may be caused by varying proximate factors, but I am interested in investigating the background factors that help set the stage for war.[11]

Factors Considered

Building on previous research on civil war and state failure, I was able to determine a limited set of dependent variables that I would need to control for in my model. The plurality of literature on civil war suggests that development (either IMR or GDP per capita), political regime type, previous conflict, and logged population size are essential controls. In some cases—such as with population size—I examined whether to use the logged term or the linear term, and saw that the

[11] For the same reason, I do not include a variable for post-communist states, as Urdal does. Several armed broke out in the former Soviet Union and the former Yugoslavia during its dissolution, but I see no reason manipulate my results by artificially excluding these cases from my analysis. If I were to do so, I would compelled to review *all* types of wars and decide whether or not to include them or exclude them.

logged term was much more important. I also investigated but do not discuss secondary school enrollment or urbanization (due to their limited availability and high correlation with other development measures and population density (due to its unimportance).

My study includes the following independent variables:

LN Country Population – Natural log of the total population, averaged during the 5 years immediately prior to the period of interest. (United Nations 2003).

Infant Mortality Rate – number of deaths for every 1,000 children aged 0 to 1 averaged during the 5 years immediately prior to the period of interest. (United Nations 2003).

GDP growth – from the Penn World Tables based on the constant price chain index, averaged during the 5 years immediately prior to the period of interest.

Real GDP – from the Penn World Tables based on the constant price chain index, averaged during the 5 years immediately prior to the period of interest.

Political Regime - taken from the Polity IV dataset– measured as the average of the democracy score minus the autocracy score to result in a scaled regime score between -10 and +10 during the five years prior to conflict onset. -10 represents the most strongly autocratic and +10 is the most strongly democratic. Also squared value to produce **Regime Squared**, since the literature indicated that the squared value of regime was much more important than the term itself.

Previous Conflict in 10 years– A dichotomous variable taking the value of 1 if there was a civil war (of the given onset type) in the previous ten years prior to the period of interest. I experimented with measures of the number of prior wars, but this was highly correlated with time. I also tried using separate measures of previous conflict in the last 1-5 and 6-10 years, but this single combined measure proved to be just as good.

Year – the year that marks the beginning of the five-year period of interest. Note that because PRIO's dataset began in 1946, my years started in 1956 (thus marking the period from 1956 to 1960) in order to give all groups of country years an opportunity to have had a conflict in the past ten years. In some regressions the start year is 1961 (marking the period from 1961 to 1965) in order to record values of youth cohorts ten years earlier, since the United Nations demographic data began in 1950.

Youth - I test eight different measures of youth as shown in Table 1:

Table 1: Measures of Youth tested in the study
<u>Measures NOT involving Relative Cohort Size</u>
collyouth *Collier & Hoeffler (2001):* men 15-29 divided by all men
fearyouth *Fearon & Laitin (2003):* men 15-24 divided by all men;

<u>Quasi-Relative Cohort Size Measures</u>
mesyouth *Mesquida & Wiener (1999):* men 15-29 divided by men 30 +
urdyouth *Urdal (2002)*: population 15-24 divided by population 15+;
cinyouth *Cincotta et al. (2004)*: population 15-29 divided by population 15+

<u>Relative Cohort Size Measures</u>
golyouth *Goldstone et al. (2000), O'Brien (2002):* population 15-29 / 30-54
macyouth Macunovich 1 (2000): population 15-24 divided by pop 25-59
relcoh men 15-24/men 25-34

Table 2 gives descriptive information about each variable used.

[Table 2 here]

V. My Results

Table 3 gives correlation matrices of my variables. As Table 3a shows, urbanization, secondary school enrollment, GDP per capita, and the infant mortality rate are highly correlated and thus should not be used in the same model together. Table 3b demonstrates that although all 8 measures of youth are positively correlated, there is a noticeable difference between measures of youth involving RCS and those that do not.

[Table 3 here]

After running a variety of regressions using the independent variables described above, three independent variables proved to be the most important and to provide the most parsimonious model: the infant mortality rate, the log of population size, and regime squared. The relationship between IMR and log of population was positive (meaning the higher the infant mortality and the larger the population, the more likely conflict onset was to occur). Consistent with the literature on conflict, regime score mattered little while regime squared had a negative relationship to conflict onset. In other words, whether a regime leans toward democracy or autocracy is less important than how "absolute" it is. Strong democracies and strong autocracies seem to be better able to prevent (or to squelch) rebellions, while intermediate regimes were more vulnerable to civil war. The lagged dependent variable (conflict onset in the previous 10 years) was also measurable and important.

Table 4 shows regression results using various measures of youth combined with these three variables on the onset of conflict. Each regression is run with and without the lagged dependent variable, onset in past 10 years, to evaluate its robustness. Table 4a gives the baseline results without any youth measures. In Table 4b I show the results of non-RCS measures of youth. None of these are measurable or statistically significant, nor do they add to the explanatory power of the model. Table 4c gives the results of quasi-RCS measures of youth and table 4d gives the result of RCS measures of youth.

[Table 4 here]

Contrary to my initial hypothesis, some of the quasi-RCS measures of youth are larger and more significant than the RCS measures of youth. The AIC and pseudo-R^2s are not yet displayed in the table, but they also indicate that quasi-RCS measures add to the model's explanatory power more than RCS measures of youth. Yet in a pooled time-series model such as this, it is difficult to discern whether the quasi-RCS measures are better at fitting conflict *within* countries over time or *between* countries across time. An explanation of conflict that relies on between-country differences is likely to be picking up unobserved heterogeneity in the characteristics of the countries themselves rather than the importance of the variable itself.[12] To test whether this is the case, I took first differences of each RCS and quasi-RCS variable to see whether the change in youth affected conflict. My findings are given in Table 5.

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[12] Note: a fixed-effects model would be an ideal test of unobserved heterogeneity because it assigns an intercept to each country, but my fixed effect models have generally *not* found youth to be important. I believe that this is because there are 137 countries and only 900 valid country five-year periods. If I include an interaction term between youth and each country (hence giving each country a different slope for youth), a combined F-test of the slopes is measurable and statistically significant. I plan to pursue a grouping of countries that would allow me to pursue more meaningful fixed effects later this year.

[Table 5 here]

Table 5 does not yet include AICs, estimates of odds-ratios, and other useful interpretive information, but based on the full set of findings it appears that first-differences of quasi-RCS measures of youth (with the exception of Cincotta) do not explain the onset of conflict as well as RCS first-differences of youth (with the exception of Macunovich). Thus even though quasi-RCS measures of youth performed slightly better than RCS measures of youth in Table 4, they are explaining more of the *between-country* differences in conflict than the *within-country* differences in conflict. Likely this is due to the fact that quasi-RCS measures of youth include the elderly in the denominator, and countries at low levels of development with high levels of mortality have fewer elderly than other countries, and development is clearly associated with the risk of civil war onset.

Admittedly the difference in explanatory power between RCS and quasi-RCS measures of youth at the country level is rather small. But I believe that it is large enough to be important given that the stakes in civil war are so high. Moreover, RCS-based measures of youthful age structure offer an additional compelling reason to use them over other measures of youth: predictive power.

Comparing the ratio of the size of two birth cohorts can be done immediately prior to the period of interest, as is done here, but it can also be done earlier in time. For example, the cohort born from 1981 to 1990 is age 15 to 24 in the year 2005 and can be compared to the size of the 1971 to 1980 birth cohort (the 25 to 34 year old population in 2005). Suppose, however, that we wish to estimate in 1995 what the 2005 ratio will be. Even without using immigration and mortality rates to make a prediction, in 1995 we can easily measure the size of the 5 - 14 year-old population (the 1981 - 1990 birth cohort) and compare them to the size of the 15 – 24 year-old population (the 1971 to 1980 birth cohort). As the lagged measures in Table 3c through 3e illustrate, our 1995 measure would be highly correlated with our 2005 measure.

Regression results not yet presented here [future table 6] show that such lagged relative cohort measures are as good or better predictors of conflict onset than are contemporary relative cohort measures. In other words, relative cohort size can be easily measured up to 10 years in advance of the period of interest and still tell us almost as much about the onset of conflict as waiting ten years or trying to forecast future populations would. I say up to ten years because I am working with five-year time intervals and hence was only able to test it five, ten, and fifteen year measures of future cohort size. For all definitions of youth, five- and ten-year measures of future cohort size (such as current 5 to 14 year olds over current 15 to 24 year olds) were almost as good as or better than current measures. Fifteen-year future cohort measures were noticeably less useful, probably because the infant and early-childhood mortality rate in some parts of the world has a large effect on eventual cohort size.

[Table 6 to come!]

VI. Conclusion

Two months after the attacks of September 11, 2001 in the United States, a New York Times headline asked "Is the devil in the demographics?" (Sciolino 2001). The article asserted that political instability in the Middle East was associated with large populations of unemployed youth, and speculated about the hazards of future youth cohorts in the decades to come. Yet econometric models of the relationship between youthful populations and the onset of civil war have produced mixed results.

I have argued that one reason for conflicting results on youthful age structure and conflict may be the poor conceptualization of the linkage between youth and conflict in prior research. To measure a "youth bulge" as the percent of youth in a population or as the ratio of youth to all adults is not only inconsistent with the basic structure of age pyramids, it is also atheoretical. What is it about youthful populations that we might expect to influence the risk of conflict?

Easterlin's theory of relative cohort size provides us with a solid foundation for a theory of how youthful age structure and conflict are related. Large birth cohorts often strain the schooling system and labor market of a country as they age (particularly in a developing nation), which can result in massive frustration, unemployment, reduced wages, and dissatisfaction when the cohort reaches its young adult years.

Using a logistic regression of the onset of civil war worldwide in five-year periods from 1961 to 2001, I showed that strength of a country's political regime, its infant mortality rate, and the log of its population size prior to the period of interest were consistently significant and measurable factors in the conflict model. After establishing the baseline model, I tested eight definitions of youthful age structure. Two of them were "non-RCS measures": that is they take youth as a percentage of the entire population, without concern for whether the entire population is comprised of infants, elderly, or workers. Non-RCS definitions of youthful age structure were neither statistically significant nor measurable in the model of conflict onset.

Three of the measures are what I have called "quasi-RCS measures"; that is, they take youth as a percentage of the adult population. Although they are closer to the definition of RCS than the non-RCS measures, they still do not distinguish between elderly and working-age adults. The final three measures were "RCS" measures: that is, they compared young adults to a specific and finite group of working adults. Contrary to my hypothesis, the evidence that RCS was an important factor in the onset of civil war was not nearly as strong as the evidence that quasi-RCS measures were important. However, it was unclear whether this was because quasi-RCS measures were better at explaining *within*-country or *between*-country variation. First-differencing of the measures of youth showed that RCS measures fared slightly better than quasi-RCS measures at explaining the onset of civil war *within* a given country. It is likely that quasi-RCS measures of youth were picking up variations in the percent elderly between countries (a marker for the level of development in a country) as a risk factor for conflict, rather than differences in youthful age structure.

Based on these findings alone, it seems as though studies of civil war should take relative cohort size into consideration as a measure and as an explanation for the sequence of events leading to conflict. Even though RCS measures fare only slightly better than quasi-RCS measures of youth in a first-differenced regression, relative cohort size has the major advantage of being easy to observe up to ten years ahead, well before a cohort reaches its late teens. Measures of relative cohort size ten years prior to the period of interest (for example when the cohort of interest is aged 5 to 14 and the older cohort is 15 to 24) are *nearly as good or better* at predicting conflict than current measures of relative cohort size (when the cohort of interest is aged 15 to 24 and the older cohort is 25 to 34). These findings appear to be robust to a variety of different specifications.

Although a large relative youth cohort is neither a *sufficient* nor a *necessary* condition for civil war, relative cohort size may indeed be an important risk factor in the onset of internal armed conflict. Relative cohort size is unlikely to be an *immediate* cause of civil war—a successful rebellion requires not only motive but also an opportunity to recruit and mobilize. Yet the dissatisfaction generated by a large cohort reaching adulthood only to experience low wages and massive unemployment arguably creates a potential army of young men who could be easily recruited in a rebellion. Research on relative cohort size has shown that ones fortunes are indeed influenced by ones birth cohort: unemployment increases, wages decrease, and dissatisfaction tends to rise when large cohorts reach young adulthood. The present study has shown that relative cohort size is also strongly linked to the onset of civil war worldwide from 1961 to 2001, more so than other measures of youth at the country level. Contemporary research on the causes of civil war would be wise to incorporate relative cohort size into its research. Unraveling the background factors that put a country at risk for conflict is arguably more important than finding the immediate "sparks" of conflict; as sparks are much harder to prevent than a combustible atmosphere. Large relative cohorts cannot be prevented in the immediate future, but understanding the role of cohort size and planning wisely could help to reduce the probability of future civil wars.

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Table 2: Means and SDs of Variables used in the Study

******THIS TABLE IS NOT YET COMPLETE. In the final version of this table, I will also include descriptions of each variable.**

Variable	Description	Mean	Std Dev	Minimum	Maximum	N
DEPENDENT VARIABLES						
onset1	Conflict onset of Type 1	0.11567	0.31998	0	1	1072
onset2		0.14552	0.35279	0	1	1072
onset3		0.12127	0.32659	0	1	1072
onset4		0.15392	0.36104	0	1	1072
INDEPENDENT VARIABLES						
gdpg5		1.82191	3.58764	-14.46	27.1	912
imr		80.6693	55.9441	4	263	1126
regime		-0.8376	7.46244	-10	10	1118
regimsq		56.3397	32.0491	0	100	1118
lnpop		8.82665	1.65068	4.7552	13.9938	1195
collyouth		25.9927	2.50823	18.8174	40.1115	1126
fearyouth		18.3995	2.07537	9.17811	26.6756	1126
mesyouth		77.8623	20.7324	33.171	138.557	1123
urdyouth		29.976	6.06211	14.9652	44.9804	1126
golyouth		105.115	22.6458	43.1052	158.356	1119
cinyouth		42.2185	7.3905	24.4356	57.4755	1126
macyouth		52.2813	12.0965	18.1809	84.4769	1124
relcoh		132.498	23.9054	32.3773	221.019	1123
rellag5		131.887	24.5328	27.7851	269.225	1048
rellag10		131.193	20.7802	57.2241	211.511	964
mac2lag5		238.205	72.2133	84.4727	476.572	1048
mac2lag10		234.962	69.544	83.3333	442.729	964
gollag5		104.308	23.4461	43.3222	212.831	1048
gollag10		103.632	22.7405	36.2887	183.927	964
urddiff		-0.0261	1.62111	-7.42824	7.86249	1046
mesdiff		0.39751	5.53794	-24.9038	22.3025	1040
cindiff		0.04095	1.62903	-6.40145	7.65553	1045
goldiff		0.51008	6.96263	-24.0742	21.4795	1035
macdiff		0.14036	4.09419	-15.5187	19.8496	1044
relcdiff		-1.2104	13.7634	-54.1167	48.034	1036
reldiff		1.18E-09	1.02E-09	-1.84E-09	5.85E-09	1042

Table 3: Corellations of Independent Variables

******THIS TABLE IS NOT YET COMPLETE. In the final version of this table, I will format these more parsimoniously and better-explain variable descriptions**

Table 3a: Correlations between non-Youth Independent Variables

Pearson Correlation Coefficients
Number of Observations

	regimsq	urb	secenr	imr	gdpg5	GDPpc
regimsq	1	0.32434	0.39345	-0.32039	0.07813	0.525
	1004	935	632	939	785	804
urb	0.32434	1	0.74031	-0.7542	0.10686	0.76165
	935	1000	680	1000	786	801
secenr	0.39345	0.74031	1	-0.80858	0.16576	0.78528
	632	680	680	680	565	578
imr	-0.32039	-0.7542	-0.80858	1	-0.16242	-0.71703
	939	1000	680	1004	786	801
gdpg5	0.07813	0.10686	0.16576	-0.16242	1	0.14311
	785	786	565	786	825	825
GDPpc	0.525	0.76165	0.78528	-0.71703	0.14311	1
	804	801	578	801	825	846

Table 3b: Correlations between Youth Measures

Pearson Correlation Coefficients
Number of Observations

	collyouth	mesyouth	urdyouth	golyouth	cinyouth	macyouth	mac2youth	relcoh
collyouth	1	0.64521	0.56898	0.63475	0.64621	0.5415	0.62147	0.2639
	1004	1001	1004	997	1004	1002	1002	1001
mesyouth	0.64521	1	0.97007	0.97073	0.97925	0.9461	0.96581	0.6729
	1001	1001	1001	997	1001	1000	1000	999
urdyouth	0.56898	0.97007	1	0.97106	0.98034	0.98122	0.95591	0.7675
	1004	1001	1004	997	1004	1002	1002	1001
golyouth	0.63475	0.97073	0.97106	1	0.96376	0.97773	0.96627	0.7324
	997	997	997	997	997	997	997	996
cinyouth	0.64621	0.97925	0.98034	0.96376	1	0.9388	0.9671	0.6445
	1004	1001	1004	997	1004	1002	1002	1001
macyouth	0.5415	0.9461	0.98122	0.97773	0.9388	1	0.94043	0.8377
	1002	1000	1002	997	1002	1002	1000	1001
mac2youth	0.62147	0.96581	0.95591	0.96627	0.9671	0.94043	1	0.6391
	1002	1000	1002	997	1002	1000	1002	999
relcoh	0.26387	0.67292	0.76754	0.73238	0.64454	0.83771	0.63913	1
	1001	999	1001	996	1001	1001	999	1001

Table 3c: Correlations between Macunovich measures of Youth

Pearson Correlation Coefficients Number of Observations	macyouth	mac2youth	mac2lag5	mac2lag10	macdiff
macyouth	1	0.94043	0.95889	0.95047	0.31261
	1002	1000	926	846	922
mac2youth	0.94043	1	0.97557	0.96366	0.15099
	1000	1002	926	846	923
mac2lag5	0.95889	0.97557	1	0.99126	0.15261
	926	926	928	848	924
mac2lag10	0.95047	0.96366	0.99126	1	0.13024
	846	846	848	848	844
macdiff	0.31261	0.15099	0.15261	0.13024	1
	922	923	924	844	924

Table 3d: Correlations between Goldstone et al. & O'Brien Youth Measures

Pearson Correlation Coefficients Number of Observations	golyouth	goldiff	gollag5	gollag10
golyouth	1	0.296	0.97331	0.95727
	997	911	921	841
goldiff	0.296	1	0.29048	0.27773
	911	916	916	836
gollag5	0.97331	0.29048	1	0.9838
	921	916	928	848
gollag10	0.95727	0.27773	0.9838	1
	841	836	848	848

Table 3e: Correlations between my Relative Cohort Measure

Pearson Correlation Coefficients	relcoh	rellag5	rellag10	reldiff
relcoh	1	0.96383	0.89707	0.5569
	1001	925	845	919
rellag5	0.96383	1	0.91402	0.54035
	925	928	848	922
rellag10	0.89707	0.91402	1	0.63451
	845	848	848	843
reldiff	0.5569	0.54035	0.63451	1
	919	922	843	922

Table 3f: Correlations between Youth First Differences

Pearson Correlation Coefficients Number of Observations	urddiff	mesdiff	cindiff	goldiff	macdiff	relcdiff	reldiff
urddiff	1 926	0.7621 919	0.82912 924	0.80978 915	0.97734 924	0.78171 915	0.64694 920
mesdiff	0.7621 919	1 920	0.92336 920	0.90638 915	0.74202 918	0.41691 911	0.52652 916
cindiff	0.82912 924	0.92336 920	1 925	0.9398 916	0.77577 922	0.41376 913	0.60569 919
goldiff	0.80978 915	0.90638 915	0.9398 916	1 916	0.80343 914	0.47397 908	0.52991 912
macdiff	0.97734 924	0.74202 918	0.77577 922	0.80343 914	1 924	0.81459 915	0.59377 919
relcdiff	0.78171 915	0.41691 911	0.41376 913	0.47397 908	0.81459 915	1 916	0.47108 912
reldiff	0.64694 920	0.52652 916	0.60569 919	0.52991 912	0.59377 919	0.47108 912	1 922

Table 4: Results for Eight Youth Definitions

******THIS TABLE IS NOT YET COMPLETE. In the final version of this table, I will also include n values, AIC, pseudo-R2s, 95% confidence intervals for odds ratios, etc.**

Table 4a: Baseline Models [without Youth]

1. Baseline

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-4.4235	0.7131	38.4788	<.0001	
lnpop	0.2278	0.0667	11.6642	0.0006	0.1911
regimsq	-0.0103	0.00348	8.8542	0.0029	-0.1823
imr	0.00983	0.00191	26.5056	<.0001	0.3035

2. Baseline wit

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-3.7074	0.8284	20.0304	<.0001	
lnpop	0.1504	0.0791	3.6154	0.0572	0.1227
regimsq	-0.0135	0.00391	11.9557	0.0005	-0.2421
imr	0.00908	0.00221	16.8522	<.0001	0.2761
prev1	0.8097	0.2640	9.4061	0.0022	0.1586

Table 4b: Models of Youth not Involving RCS

1. Collier Yout

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-4.2057	1.5003	7.8579	0.0051	
collyouth	-0.00853	0.0518	0.0272	0.8691	-0.0117
lnpop	0.2285	0.0669	11.6553	0.0006	0.1917
regimsq	-0.0104	0.00351	8.8296	0.0030	-0.1837
imr	0.00985	0.00191	26.5045	<.0001	0.3039

2. Fearon Yout

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-5.6000	1.3547	17.0883	<.0001	

fearyouth	0.0649	0.0629	1.0633	0.3025	0.0734
lnpop	0.2236	0.0665	11.3164	0.0008	0.1876
regimsq	-0.00971	0.00354	7.5036	0.0062	-0.1711
imr	0.00959	0.00192	24.8729	<.0001	0.2961

3. Collier Yout

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-3.8514	1.7461	4.8651	0.0274	
collyouth	0.00546	0.0582	0.0088	0.9253	0.00769
lnpop	0.1504	0.0790	3.6232	0.0570	0.1227
regimsq	-0.0135	0.00394	11.6658	0.0006	-0.2413
imr	0.00907	0.00221	16.8502	<.0001	0.2760
prev1	0.8099	0.2640	9.4099	0.0022	0.1586

4. Fearon Yout

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-5.1459	1.5801	10.6066	0.0011	
fearyouth	0.0768	0.0712	1.1635	0.2807	0.0890
lnpop	0.1510	0.0785	3.7025	0.0543	0.1232
regimsq	-0.0128	0.00397	10.4335	0.0012	-0.2299
imr	0.00882	0.00222	15.8171	<.0001	0.2684
prev1	0.7994	0.2640	9.1699	0.0025	0.1565

Table 4c: Quasi-RCS Definitions of Youth

1. Mesquida &

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-6.2103	0.9596	41.8830	<.0001	
mesyouth	0.0202	0.00673	8.9799	0.0027	0.2327
lnpop	0.2473	0.0672	13.5527	0.0002	0.2076
regimsq	-0.00813	0.00361	5.0707	0.0243	-0.1432
imr	0.00810	0.00202	16.0232	<.0001	0.2500

2. Urdal Youth

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-7.2731	1.1488	40.0795	<.0001	
urdyouth	0.0874	0.0262	11.1586	0.0008	0.2940

lnpop	0.2466	0.0670	13.5342	0.0002	0.2069
regimsq	-0.00723	0.00364	3.9437	0.0470	-0.1274
imr	0.00742	0.00206	12.9424	0.0003	0.2291

3. Cincotta You

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-7.4411	1.2550	35.1573	<.0001	
cinyouth	0.0663	0.0218	9.2785	0.0023	0.2728
lnpop	0.2470	0.0668	13.6695	0.0002	0.2073
regimsq	-0.00746	0.00363	4.2193	0.0400	-0.1315
imr	0.00756	0.00206	13.5014	0.0002	0.2333

4. Mesquida &

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-5.4750	1.0983	24.8488	<.0001	
mesyouth	0.0196	0.00755	6.7339	0.0095	0.2304
lnpop	0.1773	0.0798	4.9392	0.0263	0.1447
regimsq	-0.0115	0.00403	8.1919	0.0042	-0.2063
imr	0.00729	0.00234	9.7205	0.0018	0.2219
prev1	0.7225	0.2659	7.3807	0.0066	0.1415

5. Urdal Youth

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-6.5720	1.3190	24.8240	<.0001	
urdyouth	0.0860	0.0296	8.4490	0.0037	0.2944
lnpop	0.1816	0.0796	5.2043	0.0225	0.1482
regimsq	-0.0108	0.00405	7.1331	0.0076	-0.1939
imr	0.00665	0.00238	7.7870	0.0053	0.2023
prev1	0.6840	0.2668	6.5731	0.0104	0.1339

6. Cincotta You

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-6.7331	1.4429	21.7754	<.0001	
cinyouth	0.0651	0.0246	6.9799	0.0082	0.2730
lnpop	0.1813	0.0795	5.2026	0.0226	0.1479
regimsq	-0.0110	0.00405	7.3845	0.0066	-0.1970

imr	0.00682	0.00237	8.2742	0.0040	0.2076
prev1	0.7023	0.2665	6.9473	0.0084	0.1375

Table 4d: RCS Definitions of Youth

1. Goldstone et

Analysis of Maximum Likelihood Estimates					
Parameter	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**	**Standardized Estimate**
Intercept	-6.6084	1.0463	39.8894	<.0001	
golyouth	0.0186	0.00612	9.2631	0.0023	0.2339
lnpop	0.2446	0.0670	13.3179	0.0003	0.2055
regimsq	-0.00803	0.00362	4.9192	0.0266	-0.1412
imr	0.00847	0.00200	17.9155	<.0001	0.2615

2. Macunovich

Analysis of Maximum Likelihood Estimates					
Parameter	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**	**Standardized Estimate**
Intercept	-6.6510	1.0258	42.0390	<.0001	
macyouth	0.0378	0.0117	10.4284	0.0012	0.2524
lnpop	0.2454	0.0672	13.3304	0.0003	0.2059
regimsq	-0.00761	0.00362	4.4226	0.0355	-0.1342
imr	0.00827	0.00201	16.8353	<.0001	0.2551

3. My Definition

Analysis of Maximum Likelihood Estimates					
Parameter	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**	**Standardized Estimate**
Intercept	-6.3335	1.0621	35.5589	<.0001	
relcoh	0.0139	0.00547	6.4315	0.0112	0.1809
lnpop	0.2320	0.0670	11.9923	0.0005	0.1947
regimsq	-0.00910	0.00354	6.6011	0.0102	-0.1603
imr	0.00891	0.00198	20.1940	<.0001	0.2750

4. Goldstone et

Analysis of Maximum Likelihood Estimates					
Parameter	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**	**Standardized Estimate**
Intercept	-5.9756	1.2062	24.5443	<.0001	
golyouth	0.0188	0.00690	7.4182	0.0065	0.2406
lnpop	0.1786	0.0797	5.0242	0.0250	0.1458
regimsq	-0.0114	0.00404	7.9453	0.0048	-0.2035
imr	0.00765	0.00231	10.9252	0.0009	0.2326
prev1	0.7071	0.2664	7.0424	0.0080	0.1385

5. Macunovich

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-6.0046	1.1801	25.8899	<.0001	
macyouth	0.0381	0.0132	8.2908	0.0040	0.2586
lnpop	0.1798	0.0797	5.0866	0.0241	0.1468
regimsq	-0.0112	0.00404	7.6677	0.0056	-0.2001
imr	0.00743	0.00233	10.1955	0.0014	0.2261
prev1	0.6971	0.2665	6.8417	0.0089	0.1365

6. My Measure

Analysis of Maximum Likelihood Estimates					
Parameter	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	-5.6906	1.2111	22.0764	<.0001	
relcoh	0.0143	0.00615	5.3768	0.0204	0.1899
lnpop	0.1608	0.0791	4.1279	0.0422	0.1312
regimsq	-0.0126	0.00396	10.1250	0.0015	-0.2258
imr	0.00801	0.00230	12.1584	0.0005	0.2437
prev1	0.7512	0.2646	8.0616	0.0045	0.1471

Table 5: Results for First-Differenced Measures of Youth

******THIS TABLE IS NOT YET COMPLETE. In the final version of this table, I will also include n values, AIC, pseudo-R2s, 95% confidence intervals for odds ratios, etc.**

Table 5a: First Differences of Quasi-RCS Measures of Youth

1. Urdal First Diff

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.1871	0.7555	30.7186	<.0001	
urddiff	1	0.1309	0.0765	2.9269	0.0871	0.1139
lnpop	1	0.2090	0.0702	8.8665	0.0029	0.1749
regimsq	1	-0.0106	0.00364	8.5285	0.0035	-0.1852
imr	1	0.00969	0.00209	21.5728	<.0001	0.2909

2. Mesquida & Wi

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.2292	0.7582	31.1168	<.0001	
mesdiff	1	0.0422	0.0223	3.5705	0.0588	0.1254
lnpop	1	0.2122	0.0705	9.0561	0.0026	0.1770
regimsq	1	-0.0105	0.00364	8.3795	0.0038	-0.1836
imr	1	0.00963	0.00209	21.1965	<.0001	0.2893

3. Cincotta First

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.1916	0.7553	30.7947	<.0001	
cindiff	1	0.1545	0.0772	4.0037	0.0454	0.1354
lnpop	1	0.2083	0.0700	8.8533	0.0029	0.1744
regimsq	1	-0.0105	0.00364	8.3181	0.0039	-0.1830
imr	1	0.00960	0.00209	21.0246	<.0001	0.2882

4. Urdal First Diff

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-3.3814	0.8755	14.9168	0.0001	
urddiff	1	0.1238	0.0872	2.0170	0.1555	0.1100
lnpop	1	0.1248	0.0832	2.2513	0.1335	0.1017
regimsq	1	-0.0141	0.00411	11.7935	0.0006	-0.2491
imr	1	0.00880	0.00244	13.0358	0.0003	0.2589
prev1	1	0.7506	0.2737	7.5200	0.0061	0.1481

5. Mesquida First

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-3.4849	0.8817	15.6220	<.0001	
mesdiff	1	0.0498	0.0258	3.7361	0.0532	0.1494
lnpop	1	0.1337	0.0839	2.5438	0.1107	0.1084
regimsq	1	-0.0139	0.00410	11.5723	0.0007	-0.2465
imr	1	0.00868	0.00245	12.5334	0.0004	0.2556
prev1	1	0.7370	0.2739	7.2400	0.0071	0.1457

6. Cincotta First

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-3.4226	0.8746	15.3141	<.0001	
cindiff	1	0.1778	0.0878	4.1007	0.0429	0.1592
lnpop	1	0.1286	0.0828	2.4090	0.1206	0.1048
regimsq	1	-0.0141	0.00411	11.8248	0.0006	-0.2499
imr	1	0.00865	0.00245	12.4564	0.0004	0.2546
prev1	1	0.7418	0.2738	7.3404	0.0067	0.1465

Table 5b: First Differences of RCS Measures of Youth

1. Goldman et al.

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.1509	0.7649	29.4496	<.0001	
goldiff	1	0.0366	0.0183	3.9935	0.0457	0.1373
lnpop	1	0.1989	0.0713	7.7945	0.0052	0.1655
regimsq	1	-0.0105	0.00365	8.2377	0.0041	-0.1827
imr	1	0.00984	0.00210	21.9650	<.0001	0.2956

2. Macunovich Fi

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.1938	0.7565	30.7345	<.0001	
macdiff	1	0.0464	0.0300	2.3897	0.1221	0.0993
lnpop	1	0.2078	0.0704	8.7228	0.0031	0.1736
regimsq	1	-0.0106	0.00364	8.4828	0.0036	-0.1846
imr	1	0.00982	0.00208	22.3358	<.0001	0.2949

3. Relative Cohor

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.8602	0.8070	36.2672	<.0001	
reldiff	1	3.332E10	1.222E10	7.4343	0.0064	0.1873
lnpop	1	0.2281	0.0704	10.4848	0.0012	0.1899
regimsq	1	-0.00981	0.00368	7.1114	0.0077	-0.1712
imr	1	0.0101	0.00213	22.2212	<.0001	0.3015

4. Goldman et al.

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-3.3130	0.8906	13.8371	0.0002	
goldiff	1	0.0385	0.0210	3.3709	0.0664	0.1453
lnpop	1	0.1112	0.0851	1.7073	0.1913	0.0899
regimsq	1	-0.0141	0.00413	11.6810	0.0006	-0.2496
imr	1	0.00891	0.00246	13.0841	0.0003	0.2623
prev1	1	0.7460	0.2751	7.3540	0.0067	0.1477

5. Macunovich Fi

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-3.3743	0.8769	14.8065	0.0001	
macdiff	1	0.0404	0.0340	1.4118	0.2348	0.0882
lnpop	1	0.1224	0.0835	2.1522	0.1424	0.0995
regimsq	1	-0.0140	0.00410	11.6851	0.0006	-0.2478
imr	1	0.00891	0.00243	13.4495	0.0002	0.2621
prev1	1	0.7531	0.2738	7.5676	0.0059	0.1487

6. My Relative Co

Analysis of Maximum Likelihood Estimates						
Parameter	DF	Estimate	Standard Error	Wald Chi-Square	Pr > ChiSq	Standardized Estimate
Intercept	1	-4.0255	0.9312	18.6876	<.0001	
reldiff	1	3.293E10	1.411E10	5.4488	0.0196	0.1834
lnpop	1	0.1435	0.0841	2.9096	0.0881	0.1162
regimsq	1	-0.0133	0.00414	10.3937	0.0013	-0.2359
imr	1	0.00904	0.00248	13.2798	0.0003	0.2655
prev1	1	0.7178	0.2741	6.8601	0.0088	0.1420