The Pen or the Sword: Determining the Effects of Different Types of Coups D'état on Income Inequality

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Abstract

Existing literature on the relationship between income inequality and coup d'états focus on how the former cause the latter. No research has yet been done on how coup d'états affect income inequality after their occurrence. This study uses cross-country panel data and fixed effects with instrumental variables models to examine the impact of successful armed coups, successful unarmed coups, failed armed coups and failed unarmed coups. I find that, on average, none of these coups have a significant impact on the Gini coefficient and the income share of the poorest quintile of a population relative to the richest quintile, save for successful armed coups when the sub-sample of data from 1991-2013 was used.

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Section 1: Introduction

Civil uprisings are tumultuous affairs that inflict enormous human and economic costs. The Chinese Civil war of 1927-1949 claimed at least 1.8 million lives (Valentino, 2005). The ongoing Kurdish rebellion against Ankara has cost the Turkish economy at least 400-450 billion USD, displaced over 3 million people, and ended the lives of at least 30,000 (International Crisis Group, 2012). Over 210,000 people have been killed since the start of the Syrian Revolution in 2011, with hundreds of thousands more injured or displaced, precipitating a refugee crisis in Europe (Al-Khalidi, 2015). Even if Syria were to have attained peace in 2014, the United Nations estimates that it would still take at least 3 decades for the economy to return to its pre-war levels (UNRWA, 2014). The horrors of civil wars are presumably well-known to their instigators. Yet, throughout the latter half of the twentieth century alone, dozens of rebellions have been started in countries all around the globe, plunging various nations repeatedly into crisis. To many of these would be revolutionaries, there is a goal that justifies the terrible means: overthrowing the current government, so as to establish a more equitable society. Yet, is inequality really eradicated by the sword, or do the fires of civil conflict and revolutionary fervor merely stoke it to greater heights? We owe it to the millions who have died in the name of achieving equality to answer this question.

The rich minority in a given society often tries to implement inequality-enhancing economic policies, be it through its economic power or through direct political control (Bertola, 1993). Whenever these attempts to enhance inequality are successful, as they often are (see Gilens, 2013 for an example), resentment against the ruling class builds up among the masses and certain disenfranchised elite members of society. This resentment may then erupt in an **uprising[1],** which is defined, in this paper, as an attempt to replace the ruling political elite with another group through means that are illegal, according to the laws of that state. A rebellion that forces the ruling political party out of power will be considered a **successful coup.** Such rebellions can be either **armed**, with its participants utilizing weapons, or **unarmed**, where

[1] The terms rebellion, uprising, coup and revolution will be used synonymously in this paper.

weapons were barely used. As the next section shall elucidate, most research conducted on the relationship between uprisings and inequality focuses on the role of the latter in precipitating uprisings. Research in the other direction is scarce. The few studies that do examine how uprisings affect inequality fail to distinguish between different types of coups, choosing instead to focus on either one particular revolution or just violent conflicts in general. In order to provide a better understanding of how different types of revolutions affect inequality, this paper classifies revolutions into four groups: I) Armed, successful coups, II) Armed, non-successful coups, III) Unarmed, successful coups and IV) Unarmed, non-successful coups. This paper sets out to answer two empirical questions. First, it aims to determine the magnitude and direction of the impact on inequality that each class of revolutions have. Second, it aims to determine which of these 4 classes of revolutions have the greatest impact on inequality, if the impacts differ at all.

Using fixed effects models with instrumental variables, I find that all 4 coup types have, on average, no discernible impact on the Gini coefficient and the income share of the poorest quintile of the population relative to the richest quintile. The only exception to this was successful armed coups, which narrowed inequality as measured by the Gini coefficient when looking at the sub-sample of data from 1991-2013. A preliminary investigation into the effect of coups across different time horizons finds that successful unarmed coups in particular reduce inequality as measured by both the Gini and relative income shares 11-20 years after the coup. However, the limitations of this study and the paucity of literature in this field mean that much more future research is needed to evaluate the effects of such coups on the income gap.

This paper will be organized in the following manner. Section 2 of the paper will provide a literature review on the topic. Section 3 will state the theoretical underpinnings of how uprisings may affect inequality, while Section 4 will introduce the data sets used in this study. Section 5 will explain the econometric methodology utilized, and Section 6 will detail the results of the various models used. Section 7 will discuss ways in which the results should be interpreted, potential limitations of the study, and directions for future research. Section 8 will summarize and conclude the paper.

Section 2: Literature Review

This section of the paper will provide an overview of current research available on the connection between revolutions and inequality. This research can be organized into four broad categories: I) How inequality precipitates civil unrest, II) How changes in government policies affect inequality, III) The impact of violent conflict on the economy in general and IV) The impact of revolutions on inequality in particular.

Section 2.1: How inequality precipitates civil unrest

Most of the available research focuses on how income inequality causes uprisings, instead of the other way around. Surprisingly, income inequality between individuals was not found to be a significant predictor of civil wars (Collier and Hoeffler, 2004). However, social and economic inequalities between groups that are differentiated by factors such as race, religion or language were found to be positively related to the outbreak of armed conflict (Ostby, 2008). This implies that grievances over horizontal inequality, rather than vertical inequality, are a cause of armed conflict. Justino (2008) also found that redistributive policies are an effective means to reduce civil unrest in India, implying a link between decreased inequality and reduced armed conflict. By empirically confirming the long-held folk-wisdom that vast inequality precipitates armed conflict, this body of research provides a crucial impetus for the writing of the present paper. If inequality precipitates conflict, then it seems safe to claim that one of the primary goals of a civil uprising is to cause a political change that would reduce differences in income to acceptable levels. This was indeed one of the stated primary goals of many notable revolutions, such as the French Revolution of 1789 and the Arab Spring revolutions that started in 2010. Given the enormous human and economic havoc that such uprisings usually wreak, it is crucial for us to determine whether such costs are at least partially justified by the attainment of lowered inequality after the revolution, and whether violent means really are better than non-violent methods for achieving this goal.

Section 2.2: How changes in government policy affect inequality

One of the primary goals of this paper is to determine whether revolutions that successfully replace ruling governments result in lower inequality compared with failed revolutions. This rests on the assumption that government policies have an impact on inequality; if they do not, then the level of inequality should not be dependent on which political entity currently holds power. Thankfully, the common-sense notion that government policies have a very significant effect on income inequality, at least for certain time horizons, is strongly supported by the academic literature. Jourmand et.al (2012) discusses the impacts of different tax and redistributive policies on inequality in OECD countries, finding that different policy choices have varying effects on inequality. Likewise, Bulir (2001) provides strong evidence to support the notion that fiscal redistribution significantly lowers the level of inequality. Government policies also impact levels of inequality through more indirect means, such as by creating a more developed financial system; for instance, Beck, Demirguc-Kunt and Levine (2007) show that higher levels of financial intermediation reduce income inequality by disproportionately boosting the incomes of the poor. Suffice to say, the evidence that governments affect inequality is compelling. There is thus ample reason to examine the effect on inequality levels of a forced change in government due to a civil uprising.

Section 2.3: The impact of violent conflict on the economy and society in general

The negative impact of violent conflict on various aspects of the economy is well-explored by academic research. Chen, Loayza and Reynal-Querol (2008) find that civil wars cause average GDP levels to fall significantly as compared with the pre-war period, with longer wars exacting a greater toll on the economy. Martin, Mayer and Thoenig (2008) observed a 25 percent drop in trade in the first year of an armed conflict, with the trade disruption worsening the longer the conflict drags on. Deininger (2003) and McKay and Loveridge (2005) identified the huge negative impact that war had on the agricultural sectors of Uganda and Rwanda respectively, a finding of particular importance since the agricultural sector is the dominant contributor to the economy in these countries. Wars also frequently lead to the destruction of health infrastructure and the loss of skilled healthcare personnel in the country, leading to poorer health outcomes (Iqbal, 2006). Decreasing school enrollment has also been observed in countries experiencing war (Lai and Thyne, 2007), severely crippling the future pool of human capital available to these countries. All in all, this research shows that armed conflict tends to have extremely detrimental effects on the economy and society.

Section 2.4: The impact of armed conflict on inequality

While the impacts of armed conflict on the economy, education levels and health are fairly well researched, the literature on the effect of uprisings on inequality is surprisingly scarce. It seems that the only major study published on the topic is by Bircan, Bruck and Vothknecht (2010), who, using data from 128 countries, found that inequality increases in the 5 years after the end of an armed conflict, with inequality falling back to pre-war levels 10 years after the fighters laid down their weapons. Kelly and Klein (1981) studied the 1956 Revolution in Bolivar in particular, finding that inequality **decreased** in the short run after the conflict, but gradually returned to pre-war levels as a subset of the previously oppressed peasants leveraged their newly obtained opportunities to forge a new elite class. With the literature on the effect of armed conflict on inequality being so sparse, more research on the topic is urgently needed. This paper seeks to meet part of that need.

Section 2.5: The implications of the literature for the current paper

This paper attempts to make two extensions to the literature. First, while previous research firmly supports the idea that inequality causes civil uprisings, research in the other direction, on how civil uprisings cause inequality, is extremely sparse, despite the relatively copious amounts of research on how uprisings affect other aspects of society, such as education and economic growth. What research has been done focuses on how armed conflict affects post-war inequality; yet, to the best of my knowledge, there has been no study that addresses the crucial question of whether an armed uprising leads to different impacts on inequality compared with its chief alternative: a non-violent civil uprising that demands the removal of the government. Second, while the literature on the importance of government policy to changing inequality levels is very well established, no research to the best of my knowledge addresses the question of whether an uprising that successfully replaces the government has a different impact on inequality as compared with an uprising that fails to achieve this aim. By addressing these two shortcomings in the literature, I hope to answer the important question of which kinds of revolutions, violent or non-violent, successful or non-successful, best attain the aim of lowering inequality in society.

Section 3: Theoretical Underpinnings

In this section, I will first examine the reasons behind why violence and the successful overthrow of regimes may lead to either an exacerbation of inequality or a narrowing of it, both in the short and long run. Due to the ambiguous direction of the effect of both success and violence on inequality, it is unclear *a priori* which of the four classes of rebellions examined in this paper would lower inequality the most, if they result in less inequality at all. The aforementioned dearth of academic literature on the subject further compounds the difficulty of attempting to make a decisive prediction of the impact on inequality by uprisings.

Section 3.1: Theoretical effects of successful and non-successful coups on post-coup inequality

A successful rebellion could install a government that merely favors a new elite instead of the old one, implementing policies that favor the victors at the expense of their defeated enemies. This changes *which* group benefits from inequality, but not the existence of the inequality itself. On the other hand, a successful rebellion may install a new government that dismantles previous power structures within society, implementing reforms that result in a more equitable distribution of wealth. For instance, the new government may enact potent redistributive policies and break down systems of nepotistic patronage that gave certain social groups enormous economic advantages over others, causing inequality to narrow. However, even on this charitable view of the new government, inequality may fall in the short term only to rise again in the long run. This is because the new government may not be able to keep forces that inexorably widen inequality in check. For example, the revolution may lead to greater economic freedom, allowing people who were denied certain opportunities such as good education or access to certain professional fields to exercise their talents in these areas. With this, certain more talented or privileged individuals could end up doing better than others, leading to the rise of a new meritocratic elite with a higher earning power than the rest of the population. This new elite could then use their wealth and connections to give their offspring economic advantages that are inaccessible to most people, such as access to expensive, top schools and prestigious internships. As such, a system of inherited privilege could arise (Kelly and Klein, 1981), causing inequality to reach or exceed pre-revolution levels in the long run.

Similarly, uprisings that fail to topple the existing government could goad the victors to exact vengeful economic policies against the already downtrodden revolutionaries, restricting economic opportunities for them even further and widening the wealth gap in both the short and long runs. Yet, it could also prompt the government to enact more redistributive policies and equalize economic opportunities in order to prevent a repeat of the turmoil, narrowing inequality. It is also possible that the government would enact *both* vengeful and reconciliatory economic policies in an attempt to balance the demands of its supporters with the concerns of the revolutionaries, making the impact on inequality even more uncertain. In short, successful and unsuccessful revolutions have the potential to increase or decrease inequality relative to before the revolution depending on factors such as the predilections of the government. It is thus difficult to make a theoretical prediction as to whether successful revolutions will have a larger impact on inequality than unsuccessful ones.

<u>Section 3.2: Theoretical effects of armed and unarmed coups on post-coup inequality</u>

As Bircan, Bruck and Vothknecht (2010) found, violent, armed conflict in general is likely to exacerbate inequality. Such conflict could create new commercial opportunities for the exploitation of assets, investment, services, marketing and welfare (Pugh, 2003). Warlords and others with the necessary connections could thus profiteer from the conflict while peasant livelihoods are being destroyed, widening inequality. Countries which economies are devastated by war will also have fewer resources available for redistributive projects, making the scourge of inequality harder to combat. However, it is possible that armed coups *in particular* may have more ambiguous impacts on inequality. Armed conflict *in general* can happen for a vast variety of reasons, and includes situations that unambiguously increase inequality, such as when the elite group persecutes an already downtrodden segment of society, as happened in Rwanda and Myanmar. Armed coups are defined as a subset of such conflicts that have the specific aim of overthrowing the ruling elite. As such, a violent rebellion could specifically aim to and succeed in destroying the physical and social capital on which the wealth of the elite depends, causing them to lose much of their net worth and thus, leading to a steep drop in inequality (at a heavy cost to economic growth). Moreover, the coup participants may be able to use their arms to seize wealth-producing resources from the elite such as oil wells and divert those resources to their own people (Odera & Maasho 2013), causing a redistribution of income **.** Thus, the effect of armed coups on inequality depends on whether the inequality-widening forces of armed conflict in general outweighs the inequality-narrowing factors that are more specific to armed coups. As such, the possibility that armed coups may be significantly different from armed conflicts in general bears a need for empirical investigation. On the other hand, a non-violent rebellion could exert sufficient political pressure to get the current or newly-installed government to implement inequality reducing policies. However, without the threat of force being applied, the government could simply ignore the rebel's demands, causing inequality to stay stagnant or continue increasing. Thus, it is difficult to predict whether non-violent uprisings will widen or narrow inequality.

Section 3.3: Summary of theoretical underpinnings

The above outlines just a few of the numerous possible reasons for why violence and success may widen or narrow the wealth gap relative to before the revolution. The turmoil of a civil war all but ensures that there will be many other factors at play that push inequality in different directions. The presence of such a number of often interrelated variables makes it difficult for theory alone to provide a definitive answer as to *which* of these factors would likely outweigh the rest and hence, whether inequality is likely to increase or decrease in the aftermath of an uprising. Due to this ambiguity, it is also difficult to predict which of the 4 classes of revolutions would have the largest impact on inequality. As such, empirical research is needed to resolve this theoretical conundrum and determine how, on average, the success, failure, violence or non-violence of a revolution affect inequality.

Section Four: Data

Section 4.1: Data on Coups

The data on coup events comes from the Coup D’état Project (CDP) Data collected by researchers at the Cline Center for Democracy, which is part of the University of Illinois at Urbana-Champaign. The data set was created by combining and reconciling data from 5 pre-existing sources, each of which covered specific periods of time between 1945 to 2005 (Cline Center for Democracy, 2013). Made available to the public in 2013, the CDP data contains information on 1119 coups which took place during the period of 1945-2005, and covers 165 countries. The CDP data contains information on whether each coup succeeded, failed during the attempt, or was foiled during the planning stages. It also details whether actors such as the military, rebel groups, the public and foreign nations were involved in the coup, the fate of the incumbent head of state and whether arms of various types were used during the conflict. As such, the CDP data is an extraordinarily detailed data set which contains all of the independent variables of interest for the studies in this paper. The long time period and large number of countries covered by the CDP data makes it more suitable for doing a cross-country study than other data sets on coups such as the data of the Government Change in Authoritarian Regimes project (Cline Center for Democracy, 2013), which cover either a much shorter period of time, a much smaller sample of coups or a smaller subset of countries. As the CDP data was published only in 2013, to the best of my knowledge, few if any studies have been conducted using it. Thus, using this new data may allow me to glean information which other older data sets that have been used by other researchers may not be able to provide. All in all, the CDP data is a data set that is serendipitously tailored to the needs of this paper.

However, the CDP data does have a couple of weaknesses. First, while it details when each coup ended, it does not state when they started. Thus, with only an end date as the reference point, the regressions utilized would be comparing post-coup inequality with an *average* of pre-revolution and during-revolution inequality. This is not necessarily a problem for interpreting the results of this study, as one might still be very interested in knowing whether the ultimate effect of a coup is to increase or decrease post-coup inequality over and beyond the changes that have already arose during the coup. However, due to this limitation, more sophisticated analyses that compare pre-coup, during-coup and post-coup inequality are impossible. Second, the CDP data does not indicate who replaced the government in the case of a successful coup. This could be potentially problematic, as the type of government that takes over will obviously affect inequality in drastic ways. A military government, for instance, will likely implement policies that differ dramatically from what a democratically elected leader would implement. The type of government that took over could be a confounding factor if it is a variable that is highly correlated with some of the explanatory variables, such as use of arms. The results of this study will have to be interpreted with this possibility in mind.

The CDP data includes details for 270 coups that were foiled in the planning stages. These "coup conspiracies" are unlikely to have any significant impact on inequality as they were not set in motion. As such, they were excluded from the analysis along with 6 observations that did not specify whether the coup succeeded or not. Table 1 below provides the summary statistics for the CDP data on coups that were executed. The "Frequency" column reports the number of each of the four types of coups, and the "Percentage of total events" column displays the percentage of the 849 total events of each variable. Each of the four types of coups forms a sizable proportion of the data set, with the least frequent type, "Succeed With Weapons", occurring 133 times and making up 15.56% of total events. The abundance of each variable provides me with the necessary data to draw statistically significant results.

Table 1: Summary Statistics for CDP Data

Variable	Frequency	Percentage of total events
Succeed With Weapons	133	15.67
Succeed Without Weapons	302	35.57
Failed With Weapons	145	17.08
Failed Without Weapons	269	31.68

Section 4.2: Data on Inequality

The data on inequality are taken from the UNU-WIDER World Income Inequality Database (WIID) version 3.3 (UNU-WIDER, 2015). This is the updated version of the data set used by Bircan, Bruck and Vothknecht(2010). The WIID is the most comprehensive data set on inequality available (Pirttilä & Addison 2014), containing 7054 observations of Gini coefficient data for over a hundred countries, spanning a time frame that ranges from 1954-2013 for some nations. Unfortunately, the data set does have significant limitations. It is compiled from numerous sources of varying reliability that measure inequality using different methodologies. There are also many years of missing data and multiple observations for each year for some countries. As such, the data had to undergo a significant cleaning process before it could be used for this study. This process is documented in the data appendix of this paper.

The curated WIID data contains 2693 observations for a total of 173 countries. Table 2 below provides the summary statistics for this dataset. Under the "Frequency" column, it reports the number of countries for which there are fewer than 10 observations, between 10 and 30 observations, between 30 and 50 observations, and more than 50 observations. It also reports the number of observations that fall within certain time periods, as well as the number of observations for countries that are of a certain income bracket. The "Percentage of total" shows what percentage of 173 countries or 2693 observations each variable makes up.

Table 2: Summary Statistics for WIID Data

Variable	Frequency	Percentage of total
Countries with obs. <= 10	85	49.13
Countries with 10 < obs. <= 30	60	34.68
Countries with 30 < obs. <= 50	25	14.45
Countries with obs. >50	3	1.73
Obs. for 1960<=x<= 1969	213	7.90
Obs. for 1970 <= x <= 1989	799	29.67
Obs. for 1990 <= x <= 1999	730	27.11
Obs. for 2000 <= x <= 2013	950	35.28
Obs. for high income	1210	44.93

Obs. for upper middle income	717	26.62
Obs. for lower middle income	480	17.82
Obs. for low income countries	166	6.16
Obs. for unclassified countries[2]	120	4.46

Table 2 highlights significant limitations of the WIID data. The first limitation is the scarcity of data for many countries. Out of the 173 countries represented, 49.13% of them have fewer than 10 observations. This means that there will likely be few to no observations of the Gini for those countries before or after a coup, making it difficult to compare pre-coup with post-coup inequality for at least 85 nations. However, since the other half of the countries represented has at least 10 observations each, there is still a sufficiently large sample of countries to conduct this study. The second limitation is the unequal distribution of observations across different time frames. Only 7.90% of the observations pertain to years before 1969, whereas there are over 4 times as many observations for years including and after 2000, and over twice as many observations for the other two time frames. The relative paucity of data for the years before 1969 makes it difficult to accurately measure the effect of coups that happen in those years or in the early 1970s. However, as the data for years after 1960 are relatively abundant, and as there are still many coups that happen after 1960, there is still sufficient data to conduct this study. The third limitation of the WIID data is the unequal sampling of countries by income brackets. 71.55% of the data pertains to countries that are classified as either high income or upper-middle income. This is potentially problematic as coups are far less likely to happen in such countries than in lower-middle or low income countries, as the former tend to have stronger institutions that facilitate legal transitions of power over coups, whereas the latter tend to lack such institutions. Some empirical

[2] Unclassified countries are mostly countries that no longer exist, such as the USSR and Yugoslavia

methodology will likely be needed to correct for this sampling bias; unfortunately, as will be discussed in Section 7, this is difficult to do. All in all, a study is only as good as the data that it utilizes. While the WIID data is still of sufficient quality to allow us to run and draw significant conclusions from a regression, the reader should interpret the results of this study with these 3 limitations and those elucidated in the data appendix in mind.

Section Five: Econometric Approach

Section 5.1: Independent Variables Used

Each observation in the WIID inequality data is marked with the following indicators:

C = 1 if a coup has occurred *on or before* this year, 0 otherwise.

S = 1 if C=1 and if the coup that occurred was successful, 0 otherwise.

A = 1 if C=1 and if weapons were used in the coup that occurred, 0 otherwise.

Using these indicators, 4 independent variables were created:

$\mathbf{CSA_e}$ = 1 if C=1, S=1 and V=1, 0 otherwise.

$\mathbf{CFA_e}$ = 1 if C=1, S=0 and V=1, 0 otherwise.

$\mathbf{CSU_e}$ = 1 if C=1, S=1 and V=0, 0 otherwise.

$\mathbf{CFU_e}$ = 1 if C=1, S=0 and V=0, 0 otherwise.

The subscript e can take a value of To10, 5, 10 or 20. This indicates that the observation took place within 0-10 years, 0-5 years, 6-10 years and 11-20 years after a certain coup type respectively. For instance, if CSA_{10} = 1 for the year 2000 of Country X, this means that Country X experienced a successful armed coup sometime between 1990 and 1994 inclusive. It is possible for more than one of these variables to be equal to 1 if more than one coup took place within the 20 years before a given observation. If all 4 of these dummy variables are equal to 0, this means that no coup took place in the 20 years before the year of that observation. Most of this study will focus on determining the effect of the "To10" variables, as doing so allows me to keep the number of endogenous variables at a minimum while still leaving a moderately long time period for the effects of multiple different coup types to be controlled for.

Section 5.2: Econometric Model

I use a Fixed Effects (FE) estimator to measure the impact of the independent variables on inequality. Let the following variables be defined as:

$\mathbf{Y_{ct}}$: The (Adjusted – See Data Appendix) Gini coefficient for country c measured at time t.

$\mathbf{N}_{cit}$: A vector containing binary variables indicating the decade in which an observation takes place in. There are 4 such dummies: for the decade of 1960-1969, 1970-1979, 1980-1989 and for 1990-1999 inclusive. If, for instance, for country c at time t, the year is 1978, then the indicator for 1970-1979 would be 1, whereas the rest of the indicator variables will be 0.

$\mathbf{X_{cit}}$: A vector containing CSA_e, CFA_e, CSU_e, CFU_e for country c at time t. The subscript e can take on multiple values, depending on the regression in question. The subscript i refers to a specific variable within this vector.

$\boldsymbol{\mu_c}$: Factors which are "constant" across time for country c that affect inequality.

$\boldsymbol{\varepsilon}_{ct}$: The error term of the regression for country c at time t. This captures the factors that vary across time for country c that affect inequality.

In order to build up the theoretical foundation of my FE model, I begin with a simple Pooled Ordinary Least Squares (OLS) model to predict inequality. The model is as follows:

Equation 1:

$$Y_{ct} = \beta_0 + \beta_{1i} * X_{cit} + \beta_{2i} * N_{cit} + \mu_c + \varepsilon_{ct}$$

The β terms are the regression coefficients for each of the variables. This OLS model, however, is not sufficient to establish the *causal* effect of the independent variables on Y_{ct}. This is as the independent variables may be correlated with μ_c or ε_{ct}. As such, the fixed effects term and the error term could contain information that acts as a confounding factor. In other words, the observed effect of the independent variables on the dependent variable could stem from hidden factors correlated with both the dependent variable and the independent variables, and not from the independent variables themselves.

In order to establish causality, a FE estimator is used. The first step in the FE estimator is to calculate the mean of the observations for each country. This gives us:

Equation 2:

$$y_c = \beta_0 + \beta_{1i} * x_{ci} + \beta_{2i} * n_{ci} + \mu_c + \varepsilon_c$$

Where y_c, $x_{ci,}$ n_{ci} and ε_c are the mean values of Y_{ct}, X_{cit}, A_{cit} , N_{cit} and ε_{ct} respectively. The average of μ_c is still just μ_c , as it is a constant. The second step is to subtract equation 2 from (1).

Equation 3:

$$Y_{ct} - y_c = \beta_{1i} (X_{cit} - x_{ci}) + \beta_{2i} (N_{cit} - n_{ci}) + \mu_c - \mu_c + (\varepsilon_{ct} - \varepsilon_c)$$

$$= \beta_{1i} (X_{cit} - x_{ci}) + \beta_{2i} (N_{cit} - n_{ci}) + (\varepsilon_{ct} - \varepsilon_c)$$

As can be seen from equation 3, this subtraction removes the μ_c term, thereby removing time-constant factors that could affect the dependent variable from the regression.

Section 5.3: Endogeneity

While the FE estimator removes time-constant factors that could be correlated with the dependent and independent variables from the equation, it may still not ensure consistent estimates. For the coefficient estimates to be consistent, the independent variables will theoretically have to be uncorrelated with the time-varying unobserved variables, $(\varepsilon_{ct} - \varepsilon_c)$, for *every* time period t.[3] In this sub-section, I will discuss how this condition may be violated. To

[3] Endogeneity is also the reason why no control variables other than the decade dummies are included in the model. This is as possible controls, such as the Gross Domestic Product of a country, are almost certainly going to be endogenous, causing all coup variables correlated with them to become biased as well.

make the discussion clearer, I shall use the hypothetical example of a country that experienced its one and only (successful and armed) coup in the year 1985, causing the CSATo10 coup indicator to be labelled 1 for that country for the years 1985-1995.

I posit that *in the context of this study*, the possibility of the coup variable being correlated with the time-varying unobserved variables for the years >=1985 does not concern us. This is because coups affect inequality through two main channels. The first is through effects that are uncorrelated with time-varying factors; for example, a coup could install a government that immediately implements a policy which redistributes income from the rich to the poor. The second is through creating a change in other time varying factors; for instance, the coup could lead to less trade with other nations in the years after it as compared to if there were no coup, with the decrease in trade affecting income inequality. It would be worthwhile to examine the impact of coups on inequality not just through the first channel, but through the first and second channels combined. This is as our primary question of interest is whether a coup affects inequality, regardless of *how* it does so. Therefore, even though the coefficient of the coup variable may also reflect the part of the coup that is correlated with time-varying factors in the years >=1985, that is not necessarily a huge concern insofar as we believe that the coup caused the change in those factors, and not the other way round. That is a safe assumption to make: it is difficult to think of how a change in the GDP share of consumption in 1990, for instance, caused a coup that happened in 1980.

However, the possibility of the coup variable being correlated with the time-varying unobserved variables for the years <1985 is of great concern to us. If this were true, it means that the coup-variable coefficient is capturing an unobserved effect from before the coup that affects income inequality *after* the coup *and* helped cause the coup. For instance, the government could have abolished a social welfare program that provided aid to the poor in 1980. This action would likely affect the Gini in 1986, as the reduction of the poor's income in 1980 would probably cause inequality in the country to widen over the next few years. Resentment as a result of this abolishment may also be a significant contributing factor to the coup happening in 1985. Thus, the abolishment of the welfare program is correlated with both the coup variable and the Gini observations after the coup. Since the removal of the welfare program is not explicitly controlled for in the model, the coup coefficient would represent not only the effect of the coup itself on inequality (through both mechanisms discussed in the previous paragraph) , but also part of the effect of the abolishment of the welfare program on inequality. In other words, if the coup variables are correlated with the unobserved time-varying errors for time periods *before* the onset of the coups, then there are confounding variables that drive changes in both the coup indicators and the Gini index, causing the estimated coup coefficients to inaccurately measure the impact of the coup on inequality. This can be represented in the following mathematical form:

Equation 4:

$$y_{it} = \beta_0 + (\alpha_{1it} + \alpha_{2it})X_{cit} + \beta_2 N_{cit} + \mu_{it}$$

Each of the variables in this model has already been demeaned, as in the fixed effects model. Again, X_{cit} refers to the list of demeaned coup variables and N_{cit} represents a list of demeaned exogenous covariates, namely the indicator variables for decade. As can be seen in this model, the coefficient for X_{cit} comprises two parts, α_{1it} and α_{2it}. α_{1it} represents the effect of the Coup variable on y_{it}. α_{2it} represents the effect of the confounding variable that is correlated with both X_{cit} and y_{it}. In order to obtain a consistent estimate of the effect of coups, I will have to estimate only α_{1it} instead of $\alpha_{1it} + \alpha_{2it}$.

Section 5.4: Correcting for endogeneity: Instrumental variables

Instrumental variables are commonly employed in econometrics to solve endogeneity issues. For this paper, an instrument Z is valid if it meets *all* of the following criteria:

I) Z is strongly correlated with α_{1it}. That is, the instrument is strongly correlated with the part of the part of the coup that affects inequality.

II) Z is not correlated with $\alpha_{2it,}$ That is, the instrument cannot be correlated with potential confounding variables from before the coup that caused both the coup and inequality after the coup to change.

III) Z is not correlated with μ_{it}. That is, the instrument can only have an impact on inequality through α_{1it}, meaning that the instrument affects inequality *only* through the mechanism of the coup itself.

I propose instruments for each type of coup that meet these criteria, and provide theoretical justifications for their validity. Empirical justifications will be provided in Section 6.

Instrument One: Incumbent was incapacitated

For each successful armed coup, I checked whether the incumbent was either: forced to resign, put under house arrest, forced to flee the country, expatriated from the country or jailed during or after the coup. If any of these conditions were met, I labelled the appropriate years after the coup with the indicator variable "IncapacitatedCSA"=1, with the indicator being 0 otherwise. The same was done for each successful unarmed coup, with the appropriate years after the coup labelled with the indicator "IncapacitatedCSU".

The Incapacitated instruments will be strongly correlated with their respective coups if these coups often lead to the then-incumbents suffering any of the fates iterated above. This is likely, as successful revolutionaries tend to inflict such punishments upon ousted incumbents as a form of retribution. The instruments will also be uncorrelated with α_{2it} if there are no pre-coup confounding variables which affect post-coup inequality *and* systematically lead to the incumbent being incapacitated in any of the stated ways. Theoretically, this is likely true as the fate of the incumbent is often affected by changing circumstances; one can plan before the coup to exile the incumbent, for instance, but compromises made during the coup may force the revolutionaries to accept the incumbent merely leaving power instead. Therefore, while it is highly possible that there are pre-coup effects that help cause a coup, it is unlikely that any such pre-coup effect can also systematically determine what sort of fate befalls the incumbent. The instruments are also unlikely to be correlated with μ_{it}. This is because once the incumbent is removed from power by a successful coup he is in most cases no longer able to affect change in the country. Therefore, the fate that befalls him after he is removed from office should not have any additional impact on inequality beyond the effect of simply having the incumbent removed from power. As will be seen in the results section, there is indeed very strong empirical evidence to support these claims.

Instrument Two: Incumbent was harmed

For each successful armed coup, I checked whether the incumbent was harmed and/or killed, as defined in the CDP data. I labelled the appropriate years after the coup with the indicator variable “HarmedCSA”=1, with the indicator being 0 otherwise.

The Harmed instruments will be strongly correlated with successful armed coups if these coups often lead to the incumbents being harmed. It should be immediately obvious how an armed successful coup would likely lead to the incumbent being hurt, as such coups tend to be violent. The harmed instruments are also likely to be uncorrelated with α_{2it}. This is as while there are pre-coup factors that could lead to a coup, it is unlikely that any such pre-coup effect can also determine whether the incumbent gets hurt or killed. This is as harm to the incumbent is theoretically likely to be determined more by chance and changing circumstances; a stray bullet may hurt the incumbent even though there was no plan to harm the incumbent, the revolutionaries’ best attempts to assassinate the incumbent may be foiled, or they may be forced to compromise with the incumbent to grant him his life even though they planned to execute him instead. Finally, the Harm instrument is also unlikely to be correlated with μ_{it} for the same reason as the Incapacitated instruments: the fate that befalls the incumbent after he is removed from office should not have any additional impact on inequality beyond the effect of simply having him removed from power. Again, as will be seen in the results section, there is indeed robust empirical evidence to support these claims.

Instrument Three: People other than the incumbent were arrested

The Coup D’etat Dataset contains a binary variable that indicates whether people other than the incumbent were arrested as a result of each coup. I labelled the appropriate years after a failed armed coup with the indicator “OthersArrestedCFA”=1 if people other than the incumbent were arrested, and 0 otherwise. I did the same for each failed unarmed coup, with the appropriate years after the coup labelled with the indicator “OthersArrestedCFU”.

The OthersArrested instruments will be strongly correlated with their respective coups if people are often arrested as a result of these coups. This is theoretically very plausible, as revolutionaries that fail to overthrow the government are often at the mercy of the incumbent and arrested for crimes against the state. The instruments are also likely to be uncorrelated with α_{2it}. This is because even if there are factors that help cause a failed armed or unarmed coup, the decision of whether to arrest anyone falls to the incumbent government. These decisions are made on a multitude of factors, such as whether the coup instigators managed to flee or were captured and whether the revolutionaries struck a bargain with the incumbent to stop the coup in exchange for legal immunity. In other words, whether someone was arrested is likely strongly influenced by circumstances that arise *during* each coup, making it extremely difficult for it to be systematically determined by factors *before* the coup. Moreover, the OthersArrested instruments are also unlikely to be correlated with μ_{it}, affecting inequality only through α_{1it}. This is because if people are arrested as a result of the coup, then by definition, any effect on inequality of the people being arrested is also an effect of the coup. Thus, the requirements for being a valid instrument are fulfilled.

Summary of instruments

All in all, there are two instruments for successful armed coups: whether the overthrown incumbent was harmed, and whether the overthrown incumbent was incapacitated by legal means. There is just one instrument for each of the remaining three types of coups: whether the overthrown incumbent was incapacitated by legal means for successful unarmed coups, and whether people other than the incumbent were arrested for failed armed coups and for failed unarmed coups. I thus have 5 instruments for 4 potentially endogenous variables, which, as Section Six of this paper will detail, allows me to test whether the instruments themselves are exogenous.

Section 5.5: Sub-sample analysis

Having discussed the instruments used to deal with endogeneity in this study, I now explain the ways in which I check whether the results hold true across different specifications of the model. In order to ensure that the results are robust, I will run a couple of variations of the primary fixed effects with instrumental variables model described above. By doing so, I hope to show that the results obtained are not due to the way the model is specified. Furthermore, I will run each model on the following sub-samples of the data: entries from 1960-1990 only, entries from 1991-2013 only and entries from non-high income countries only. This is as the effects of coups may differ across time or the type of country studied. If that is the case, the sub-sample analysis would allow us to identify effects specific to these sub-samples that may be obscured in regressions done on the full sample.

Section Six: Results and Discussion

Before discussing the results in detail, it is useful to first examine three important pieces of information. The first is how to interpret changes in the Gini. When the Gini value increases, it implies that inequality in the country has increased. A more detailed interpretation of this change is difficult, as the Gini does not tell us *why* inequality increased: it could be that the income accruing to the top 10% increased, or to the top 1%, or it could be that the wealth going to the bottom 20% decreased, or to the bottom 2%. Some scholars suggest that "A Gini coefficient of G per cent means that, if we take any 2 households from the population at random, the expected difference is 2G per cent of the mean (Taylor, 2014)." Hence, a rise in the Gini coefficient by 3.7 from 30 to 33.7 per cent implies that the expected difference in income of any 2 random households has gone up from 60 to 67.4 per cent of the mean.

The second important piece of information is the Kleibergen-Paap rk Wald F statistic. This F statistic calculates the strength of the correlation of the instrumental variables with the endogenous variable. The F statistic is then compared with the Stock-Yogo weak ID test critical values for a 10% maximal IV bias (Stock & Yogo, 2002). The Stock-Yogo critical values test for weak instruments. If the Kleibergen-Paap rk Wald F statistic has a smaller magnitude than the critical value for a 10% maximal IV bias, it means that the relative bias between the OLS and IV regressions is over 10%, implying that the IV regression does not produce completely consistent estimates. If the Kleibergen-Paap rk Wald F statistic has a larger magnitude than the critical value for a 10% maximal IV bias however, it means that the IV regression produces no more than 10% of the bias of the OLS regression. If that is the case, one would have a strong empirical reason to assume that the IV regression produces consistent estimates. Unfortunately, the Stock-Yogo 10% maximal IV bias values are not available for regressions with 4 endogenous variables. However, we can use the critical value of 13.43 for 2 endogenous variables as a benchmark; the critical value for 4 endogenous variables will likely be higher than 13.43.

The third important piece of information is the Sargan-Hansen overidentification test of instruments. One can only test for whether the instruments are truly endogenous – that is, whether they are uncorrelated with the error terms of all time periods – when there are more instruments than endogenous variables, which we do in this study. The Sargan-Hansen test predicts the residuals from the second stage IV regression and then regresses those residuals on the instruments and all exogenous variables. It then calculates the statistic nR^2, where n is the number of observations and R^2 is the R^2 value of the regression of the residuals on the exogenous variables. It then compares the statistic to a Chi-squared distribution with (m-n) degrees of freedom, where m is the number of instrumental variables and n is the number of endogenous variables. The null hypothesis is that the instrumental variables are exogenous; therefore, if the statistic nR^2 exceeds the 5% critical value of the Chi-squared distribution, it means that we reject the null hypothesis and thus conclude that the instrumental variables are *not* exogenous. Note that should the instrumental variables and the exogenous variables explain a large fraction of the variation in the residuals from the main IV regression, the R^2 value will be very high. Therefore, it will be much more likely that the value nR^2 exceeds the 5% critical value of the Chi-squared distribution, making us reject the null hypothesis of exogeneity.

Section 6.1: Results of Primary Model with the Gini Coefficient as the Dependent Variable

I first ran a pooled OLS model with clustered standard errors to serve as a baseline for analysis. A pooled OLS model assumes that the independent variables are uncorrelated not only with time-varying unobserved effects, but also with the time-constant fixed effect for each country as well. The results of this regression are provided in Table 3 below. Again, the CSATo10, CSUTo10, CFATo10 and CFUTo10 indicators are marked 1 for observations that take place within 0 to 10 years inclusive of a successful armed coup, successful unarmed coup, failed armed coup and failed unarmed coup respectively.

Table 3: Estimates of effects of different coup types on the Adjusted Gini coefficient 10 years after a coup under a pooled OLS model

Variable	Coefficient	95% Lower bound	95% Upper bound
CSATo10	0.5864431 (1.700304)	-2.769706	3.942593
CSUTo10	0.716404 (1.579748)	-2.401784	3.834592
CFATo10	**3.400901**** **(1.479468)**	0.4806497	6.321153
CFUTo10	**6.497605***** **(2.467091)**	1.627932	11.36728
1960<=Year<=1969	-1.257162 (1.875085)	-4.958303	2.443979
1970<=Year<=1979	**-2.617376*** **(1.347554)**	-5.277248	0.0424952
1980<=Year<=1989	**-5.004585***** **(0.9109459)**	-6.802657	-3.206512
1990<=Year<=1999	0.4616272 (0.5690737)	-0.6616402	1.584895
Intercept	**36.42232***** **(0.9063098)**	34.63339	38.21124

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

Since all the independent variables are binary, we interpret the regression results in relation to the excluded category. In this regression, the excluded category is an observation between the years 2000 and 2013 inclusive that does *not* take place within 10 years of any type of coup (i.e. there was no coup within the last 10 years). So, for instance, we see from the results that, on average, an observation from 1980-1989 that takes place within 10 years of a failed, unarmed coup has a Gini value that is different by -5 + 6.50 = 1.50 from an observation after the year 2000 that is not affected by any coup in the last 10 years. From this regression, we see that the coefficients for failed armed coups and failed unarmed coups are both positive and significant at the 5% level, implying that such coups lead to increased inequality. On the other hand, the coefficients for successful armed coups and successful unarmed coups are not significant at the 5% level, suggesting that these coups have no discernible impact on the Gini.

However, as aforementioned, a pooled OLS regression is likely to produce inconsistent estimates. As such, I next ran the fixed-effects with instrumental variables model to account for potential endogeneity of the independent variables. The Kleibergen-Paap rk Wald F statistic for the first stage of this regression is 23.664. While this exceeds the critical value of 13.43 for a 10% maximal IV bias, recall that the value of 13.43 is taken for a regression with only 2 endogenous variables. As such, there may be concerns that the instruments are not strongly correlated with the coup variables, causing the IV estimate to produce **more** than 10% of the bias of the OLS estimate. This concern will be addressed in the next regression. On the other hand, the Sargan-Hansen p-value is 0.9517, causing us to strongly *not* reject the null that the exogenous variables and instrumental variables are indeed, not correlated with the residuals of any time period. Table 4 below outlines the results of this regression. The table reports the coefficients of the coup and time variables under a fixed-effects instrumental variables model in the second column, and just a fixed effects model without instrumental variables in the fifth column. The clustered standard errors are provided in parentheses beneath each coefficient value. The 95% confidence interval for each variable is also detailed for both models.

Table 4: Estimates of effects of different coup types on the Adjusted Gini coefficient 10 years after a coup under a fixed effects model, with and without instruments

Variable	FE IV Coefficient	FE IV 95% Lower bound	FE IV 95% Upper bound	FE Coefficient	FE 95% Lower Bound	FE 95% Upper Bound

CSATo10	-0.7035968 (1.217329)	-3.089519	1.682325	0.0893501 (0.9933887)	-1.871452	2.050153
CSUTo10	1.482162 (1.707722)	-1.864911	4.829235	0.9667608 (0.9993438)	-1.005796	2.939318
CFATo10	0.5115767 (1.280157)	-1.997485	3.020638	1.108551 (1.050758)	-0.9654895	3.182591
CFUTo10	0.8063171 (3.136705)	-5.341513	6.954147	-0.2493397 (1.311859)	-2.838755	2.340076
1960<=Year<=1969	0.2997791 (1.680005)	-2.992971	3.592529	0.4201294 (1.666143)	-2.86859	3.708849
1970<=Year<=1979	**-2.63559*** (1.02042)**	-4.635576	-0.6356037	**-2.482875*** (0.9547383)**	-4.367387	-0.5983624
1980<=Year<=1989	**-4.176168*** (0.6561563)**	-5.462211	-2.890126	**-4.078705*** (0.6339165)**	-5.329963	-2.827448
1990<=Year<=1999	0.2324699 (0.370564)	-0.4938222	0.958762	0.2687447 (0.3743257)	-0.4701188	1.007608

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

The coefficient for successful, armed coups is negative, implying that such coups decrease inequality. On the other hand, the coefficients for the other three types of coups are positive, suggesting that they cause the income gap to widen. However, in stark contrast to the results of the pooled OLS regression, *none* of the coup coefficients are significant at even the 10% level. The difference between the results in table 4 and those in table 3 suggest that the coup variables are highly correlated with time-constant factors in each country that affect inequality, with these time-constant factors causing the estimates to be strongly biased positively away from 0. Moreover, the coefficients of the coup variables also differ moderately under the FEIV model and the FE model without instrumental variables. This suggests that when instrumental variables are not used, the coup variables may be endogenous, causing them to be biased, but not sufficiently so to make the results significant from 0. On the other hand, the time dummy variables for the decade of 1970 and 1980 are highly significant. This means that inequality in those decades is, on average, significantly lower than inequality in the years 2000-2013, which is the implicit basis for comparison in the model.

However, as aforementioned, the Kleibergen-Paap rk Wald F statistic for the first stage of this regression is only 23.664, prompting concerns that the instruments are not strongly correlated enough with the coup variables. From the results of table 4, the primary reason for this seems to be the variable CFUTo10, as it has by far the largest standard error. This suggests that the instrument "OthersArrestedCFUTo10" is not strongly correlated with CFUTo10. To correct this problem, I assume that failed, unarmed coups are exogenous, and thus include CFUTo10 as an exogenous variable in the regression (i.e. it is no longer instrumented for). Under this re-specification, the Kleibergen-Paap rk Wald F statistic for the first stage of the regression increases to 100.437, a value which should be large enough to put to rest any concerns about the weakness of the remaining instruments. Moreover, the Sargan-Hansen p-value remains high at 0.9283, causing us to strongly *not* reject the null that the exogenous variables and instrumental variables are indeed, not correlated with the residuals of any time period. The high p-value of the Sargan-Hansen test also validates the assumption that CFUTo10 is exogenous. Table 5 below reports the results of this re-specified FE IV model, with the results of the FE model without instrumental variables replicated again for comparison.

Table 5: Estimates of effects of different coup types on the Adjusted Gini coefficient 10 years after a coup under a fixed effects model, with and without instruments, with failed, unarmed coups assumed to be exogenous.

Variable	**FE IV Coefficients**	**FE IV 95% Lower Bound**	**FE IV 95% Upper Bound**	**FE Coefficients**	**FE 95% Lower Bound**	**FE 95% Upper Bound**
CSATo10	-0.4481512 (1.055467)	-2.516829	1.620526	0.0893501 (0.9933887)	-1.871452	2.050153
CSUTo10	1.684065 (1.682355)	-1.61329	4.981421	0.9667608 (0.9993438)	-1.005796	2.939318

CFATo10	0.7410001 (1.081416)	-1.378537	2.860538	1.108551 (1.050758)	-0.9654895	3.182591
CFUTo10	-0.258941 (1.420965)	-3.043981	2.526098	-0.2493397 (1.311859)	-2.838755	2.340076
1960<=Year<=1969	0.3610825 (1.671928)	-2.915836	3.638001	0.4201294 (1.666143)	-2.86859	3.708849
1970<=Year<=1979	**-2.525171*** (0.9665599)**	-4.419594	-0.6307487	**-2.482875*** (0.9547383)**	-4.367387	-0.5983624
1980<=Year<=1989	**-4.133651*** (0.6435665)**	-5.395018	-2.872283	**-4.078705*** (0.6339165)**	-5.329963	-2.827448
1990<=Year<=1999	0.2484123 (0.3781605)	-0.4927687	0.9895933	0.2687447 (0.3743257)	-0.4701188	1.007608

*******- Significant at the 1% level. ******-Significant at the 5% level *****-Significant at the 10% level.

The results of the re-specified model do not contradict those in table 4, with all 4 coup coefficients remaining insignificant, though that for CFUTo10 is now negative instead. A joint-test for the 4 coup variables gave a p-value of 0.7839, causing us to be unable to reject the null hypothesis that the 4 variables are indeed jointly insignificantly different from 0. I further ran both the FEIV and FE models under the following sub-samples of the data: entries from 1960-1990 only, entries from 1991-2013 only and entries from non-high income countries only. I also ran the models with the unadjusted Gini coefficient and the Gini coefficient adjusted only for coefficients that were significant in the Gini adjustment regression (See the data appendix) in order to test whether the results were due to the way the Gini coefficient was adjusted. The coup variables were still not significantly different from 0 in all of these regressions save for one exception: under the sub-sample for the years 1991-2013 with the Adjusted Gini coefficient as the dependent variable, the CSATo10 variable was significant at the 5% level using a FE IV model ,with a coefficient of -2.17. The robustness of this anomaly will be tested in section 6.3 of this paper. Thus, aside from this exception, the evidence so far suggests that on average, none of the four coup types significantly impact the Gini.

Section 6.2: Results of Primary Model with Quintile Shares as the dependent variable

As aforementioned, the Gini coefficient is notoriously difficult to interpret: even should the Gini increase, one does not know whether the increase is due to the poor getting poorer, the rich getting richer, or both. Moreover, the Gini may change very slowly over time, making variations between years hard to detect. As such, to alleviate concerns that the Gini may be a poor measure of inequality for this study, I also ran regressions with first (Q1) and fifth (Q5) quintile income shares as dependent variables. The first quintile income share refers to the total share of income that goes to the poorest fifth of the population: a Q1 value of 9, for instance, indicates that the poorest 20% of the population obtained 9% of the total income earned by the populace in that year. On the same note, the fifth quintile income share refers to the total share of income that goes to the *richest* 20% of the population.

Table 6 below outlines the results of running a FEIV and FE regressions with the first quintile income share as the dependent variable, assuming, again, that CFUTo10 is exogenous. The Kleibergen-Paap rk Wald F statistic for the first stage of the FEIV regression is 67.037, indicating that the instruments are relevant. The Sargan-Hansen p-value is 0.7551, providing strong evidence for the exogeneity of the instruments.

Table 6: Estimates of effects of coup types on the First Quintile Income Share 10 years after a coup under FEIV and FE models, with failed, unarmed coups assumed to be exogenous.

Variable	FE IV Coefficients	FE IV 95% Lower Bound	FE IV 95% Upper Bound	FE Coefficients	FE 95% Lower Bound	FE 95% Upper Bound
CSATo10	0.3394331 (0.3270097)	-0.3014941	0.9803603	0.3920341* (0.2133407)	-0.0291041	0.8131723
CSUTo10	-0.0752615 (0.2764385)	-0.617071	0.466548	0.137316 (0.1964171)	-0.2504146	0.5250466
CFATo10	-0.2438916 (0.2316504)	-0.6979181	0.2101348	-0.4984632** (0.2493936)	-0.9907703	-0.0061561

CFUTo10	0.0515795 (0.2747733)	-0.4869664	0.5901253	0.0258828 (0.2665636)	-0.5003182	0.5520838
1960<=Year<=1969	-0.100474 (0.2752716)	-0.6399965	0.4390485	-0.1397186 (0.2715471)	-0.6757572	0.39632
1970<=Year<=1979	-0.0868019 (0.2452027)	-0.5673905	0.3937866	-0.1155364 (0.2454289)	-0.6000172	0.3689443
1980<=Year<=1989	**0.3386642*** **(0.1840308)**	-0.0220294	0.6993579	**0.3182728*** **(0.1773449)**	-0.0318091	0.6683547
1990<=Year<=1999	**-0.2316132**** **(0.1131544)**	-0.4533918	-0.0098347	**-0.2410599**** **(0.1097523)**	-0.4577127	-0.024407

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

The FEIV and FE models provide different results. CSATo10 has a positive coefficient that is significant at the 10% level under the FE model, suggesting that successful, armed coups cause the poorest fifth of the population to claim a larger share of the income generated. The coefficient, however, is not significant under the FE IV model. CFATo10 has a negative coefficient that is significant at the 5% level under the FE model, implying that failed, armed coups cause the poorest fifth of the population to claim a *smaller* share of the income. Yet, the coefficient is not significant under the FEIV model. Due to concerns regarding endogeneity, I place more weight on the FEIV estimates. However, since the *magnitudes* of the coefficients under both models are fairly similar, the difference in significance is likely attributed to the fact that robust standard errors tend to be higher in models that use instrumental variables.

Table 7 below outlines the results of running a FEIV and FE regressions with the fifth quintile income share as the dependent variable, assuming, again, that CFUTo10 is exogenous. The Kleibergen-Paap rk Wald F statistic for the first stage of the FEIV regression is 68.753, indicating that the instruments are relevant. The Sargan-Hansen p-value is 0.6470, providing strong evidence for the exogeneity of the instruments.

Table 7: Estimates of effects of coup types on the Fifth Quintile Income Share 10 years after a coup under FEIV and FE models, with failed, unarmed coups assumed to be exogenous.

Variable	FE IV Coefficients	FE IV 95% Lower Bound	FE IV 95% Upper Bound	FE Coefficients	FE 95% Lower Bound	FE 95% Upper Bound
CSATo10	-0.8983935 (0.7272487)	-2.323775	0.5269876	-0.3105861 (0.5233918)	-1.34377	0.722598
CSUTo10	0.4870347 (0.9396993)	-1.354742	2.328812	-0.0955602 (0.5715909)	-1.22389	1.03277
CFATo10	**1.341773* (0.8057194)**	-0.2374077	2.920954	**1.723394** (0.7363332)**	0.2698604	3.176928
CFUTo10	0.0856214 (0.8821912)	-1.643442	1.814685	0.0892706 (0.8310733)	-1.551282	1.729823
1960<=Year<=1969	1.269289 (0.8450529)	-0.3869843	2.925562	1.343551 (0.8352953)	-0.3053356	2.992438
1970<=Year<=1979	0.2954594 (0.7105994)	-1.09729	1.688209	0.3557482 (0.7046807)	-1.035303	1.7468
1980<=Year<=1989	**-1.230734*** (0.4707257)**	-2.153339	-0.3081283	**-1.177897*** (0.4525618)**	-2.071261	-0.2845321
1990<=Year<=1999	**1.043895*** (0.3477796)**	0.3622599	1.725531	**1.071254*** (0.3402326)**	0.3996289	1.742879

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

The coefficient for CFATo10 is significant at the 10% level under the FEIV model, and at the 5% level under the FE model. The positive coefficient implies that after a failed armed coup happens, the share of income going to the richest fifth of the population increases by 1.34% (according to the FEIV estimate). Again, I place more weight on the FEIV estimate, and erring on the side of caution, reject significance at the 10% level.

The primary goal of this paper is to uncover the impact of coups on inequality. As such, I ran regressions with (Q1/Q5)*100 as the dependent variable. The value of (Q1/Q5)*100 refers to the share of income going to the poorest fifth of the population as a percentage of the share of income going to the richest fifth of the population. A large magnitude for (Q1/Q5)*100 would indicate that the poorest fifth and richest fifth of the population earn very similar shares of the national income, implying that inequality in the country is low. Table 8 below outlines the results of running a FEIV and FE regressions with this relative income share as the dependent variable, assuming, again, that CFUTo10 is exogenous. The Kleibergen-Paap rk Wald F statistic for the first stage of the FEIV regression is 68.753, and the Sargan-Hansen p-value is 0.8220.

Table 8: Estimates of effects of different coup types on relative income share 10 years after a coup under FEIV and FE models, with failed, unarmed coups assumed to be exogenous.

Variable	FE IV Coefficients	FE IV 95% Lower Bound	FE IV 95% Upper Bound	FE Coefficients	FE 95% Lower Bound	FE 95% Upper Bound
CSATo10	1.295112 (1.063619)	-0.7895434	3.379767	1.00099 (0.6421867)	-0.2666975	2.268677
CSUTo10	-0.8497311 (1.035955)	-2.880166	1.180704	0.1054577 (0.7652689)	-1.405196	1.616111
CFATo10	-0.9574347 (0.7761233)	-2.478609	0.563739	**-1.731151**** **(0.8425483)**	-3.394355	-0.0679462
CFUTo10	0.4327808 (1.020531)	-1.567423	2.432985	0.3414637 (0.961404)	-1.556364	2.239291
1960<=Year<=1969	-0.0288352 (0.9974707)	-1.983842	1.926171	-0.1789522 (0.9754413)	-2.104489	1.746585
1970<=Year<=1979	0.1907986 (0.8910589)	-1.555645	1.937242	0.0796043 (0.8882787)	-1.673873	1.833081
1980<=Year<=1989	**1.781213**** **(0.705069)**	0.3993033	3.163123	**1.687566**** **(0.6735403)**	0.3579863	3.017146
1990<=Year<=1999	-0.5572421 (0.3610238)	-1.264836	0.1503516	**-0.6036725*** **(0.3490417)**	-1.292687	0.0853416

***- Significant at the 1% level. **-Significant at the 5% level *-Significant at the 10% level.

From table 8, we again see that the coefficients for CSATo10, CSUTo10 and CFUTo10 are insignificant under both models. However, CFATo10 is significant at the 5% level under a FE model, but insignificant at even the 10% level under a FEIV model. Running further checks, I find that all the coup coefficients are insignificant at the 5% level under both the FEIV and FE models when looking at sub-samples of observations from 1960-1990 inclusive only and from 1991-2013 inclusive only. However, under the sub-sample of observations from non-high income countries only, CFATo10 is significant at the 10% level under a FEIV model with a coefficient of -1.222, and remains significant at the 5% level under a FE model, with a coefficient of -1.91. As such, the evidence for whether failed armed coups decreases the share of income going to the poorest fifth of the population relative to the share going to the richest fifth is mixed. Placing more weight on the FEIV estimates (again, due to endogeneity concerns) provides us with tenuous evidence that failed armed coups causes inequality to increase in non-high income countries only. However, even if we were to accept the FE estimates as more accurate, the effect of such coups is still relatively miniscule: even the coefficient with the largest magnitude, -1.91 (under a FE model for non-high income countries) means that Q1 as a percentage of Q5 decreases by less than 2% after a failed armed coup. Thus, on the whole, the evidence seems to suggest that all 4 types of coups have either no or, at most, a very small effect on inequality, in the case of failed armed coups.

Section 6.3: Results of Multi-level model

The independent variables in the models so far have all been binary variables. While this allows us to keep track of whether a particular observation takes place within 10 years of a given coup type, it does not allow us to note whether more than one of a given coup type took place within the last 10 years of that observation. To account for the possibility that successive coups of the same type may lead to different impacts on inequality, I ran regressions that allowed the coup variables to take on integer values greater than one. For instance, if a successful armed coup took place in both 1980 and 1985, then the observations for 1985 to 1990 for that country will be labelled CSATo10=2, whereas in the previous models, CSATo10 would only be equal to 1. I will refer to these FEIV and FE regressions that allow the coup variables to take on more integer values as “Multi-Level models”.

The multi-level models replicated the results of the binary models. When all the data was used, all 4 coup types were found to have insignificant coefficients at the 5% level under the FEIV multi-level models, regardless of whether the Gini, Q1, Q5 or (Q1/Q5)*100 were used as the dependent variable. The FE multi-level models replicated the results of the FE binary models as well: at the 5% level, CFATo10 was found to be positively significant when Q5 was the dependent variable. However, when relative income share was the dependent variable, CFATo10 ceases to be significant at the 5% level, in contrast with the result of the FE binary model, though it remains significant at the 10% level. However, due to endogeneity concerns, we still place more stock by the FEIV models, and conclude from those results that the 4 types of coups do not significantly impact on inequality when looking at the period from 1960-2013.

When the multi-level models were processed on sub-samples of the data, the results of the binary models were replicated as well. Save for one exception, all 4 coup types were found to have insignificant coefficients at the 5% level under the FEIV models for all dependent variables. The exception was the same as in the binary model: for the sub-sample of data from 1991-2013, successful armed coups were found to significantly decrease the Gini. The results of the multi-level FEIV and binary model are given in table 9 below. The Kleibergen-Paap rk Wald F statistic for the multi-level model is 96.352, and the Sargan-Hansen p-value is 0.8129.

Table 9: Estimates of effects of different coup types on the Adjusted Gini Coefficient 10 years after a coup under FEIV multi-level and binary models, with failed, unarmed coups assumed to be exogenous, using data from 1991-2013 only

Variable	FE IV Multi-Level Coefficients	FE IV 95% Lower Bound	FE IV 95% Upper Bound	FEIV Binary Model Coefficients	FE 95% Lower Bound	FE 95% Upper Bound
CSATo10	**-1.899213*** (0.6961714)**	-3.263684	-0.5347426	**-2.401544** (1.202049)**	-4.757517	-0.0455718

	0.3098369			0.2450033		
CSUTo10	(1.067494)	-1.782412	2.402086	(1.479202)	-2.654179	3.144186
	0.0114705			0.1459922		
CFATo10	(0.9332621)	-1.81769	1.840631	(1.214532)	-2.234447	2.526431
	1.192512	-		1.607374	-	
CFUTo10	(0.8494656)	0.4724095	2.857434	(1.1145)	0.5770051	3.791753
1991<=Year	**0.6622507***	-		**0.6900567***	-	
<=1999	**(0.3704354)**	0.0637892	1.388291	**(0.3703269)**	0.0357708	1.415884

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

The CSATo10 coefficient was found to be significant at the 5% level when the unadjusted Gini coefficient and the Significant-only adjusted Gini were used as the dependent variable as well. Thus, we need to explain three discrepancies. First, why is CSATo10 significant for the sub-sample of 1991-2013, and not when the sub-sample of 1960-1990 is used? Second and similarly, why is it not significant when the full sample of data is used? Third, why is it not significant when the sub-sample of 1991-2013 is used for the FEIV regression where relative income share is the dependent variable?

The first and second discrepancies may be explained by the changing nature of violence and warfare. The weapons used in coups after 1980 (1980 is the starting point because we are looking at 10 years after a coup) are likely more advanced than the weapons used before 1980, causing the impact of their use to be different. Moreover, social values may have changed over the years due to events such as the 1991 collapse of the Soviet Union, causing the ideologies that drove revolutions pre-1991 to be different from those after 1990. The spread of world trade and capitalism post 1990 may also have interacted with successful revolutions in ways that led to a significant reduction in inequality. These theoretical reasons may explain why the CSATo10 coefficient is significant for the 1991-2013 sub-sample, but not for the 1960-1990 sub-sample. When looking at the full sample of 1960-2013, the *average* impact of all the successful armed coups over this time period may be insignificant, obscuring the significance of such coups for the 1991-2013 sub-sample. The third discrepancy may be explained by the different way in which the Gini coefficient is calculated. It could be that while successful armed coups do not change the income of the poorest quintile relative to the richest quintile, it could affect the income of the entire population in such a way that the Gini decreases.

Section 6.4: Summary of FEIV results

Table 10 below provides a summary of the results of the FEIV models. The table details which of the coup variables are significant at the 5% level under FEIV models which differ by dependent variable or the sample used. Since the results of the multi-level model do not differ from those of the binary model, the table is representative of the results of both.

Table 10: Summary of results of coup types under different specifications of the FEIV model

	CSATo10	CSUTo10	CFATo10	CFUTo10
Adjusted Gini: Full Sample	Not Significant	Not Significant	Not Significant	Not Significant
Adjusted Gini: 1960-1990	Not Significant	Not Significant	Not Significant	Not Significant
Adjusted Gini: 1991-2013	**Significant: Negative**	Not Significant	Not significant	Not significant
Adjusted Gini: Non-high income countries	Not Significant	Not Significant	Not Significant	Not Significant
Relative income shares: Full sample	Not Significant	Not Significant	Not Significant	Not Significant
Relative income share: 1960-1990	Not Significant	Not Significant	Not Significant	Not Significant

Relative income share: 1991-2013	Not Significant	Not Significant	Not Significant	Not Significant
Relative income share: Non-high income countries	Not Significant	Not Significant	Not Significant	Not Significant

Section 6.5: Results of expanded model with different time horizons

The regressions so far have examined the impact of coups on inequality 10 years after the coup. However, the effect of coups on inequality may differ between the short, medium and long terms. As such, a more complex model is needed to determine the impact of each type of coup across different time horizons. To do so, I ran FE model with the variables X5, X10 and X20. "X" refers to each of the four types of coups: CSA, CSU, CFA and CFU. A X5 variable will be labelled 1 for an observation that takes place 0 to 5 years after a certain type of coup; if CFU5 is labelled 1, for instance, that means that that observation takes place within 5 years after a failed, unarmed coup. Similarly, a X10 variable will be labelled 1 for an observation taking place between 6 and 10 years inclusive after the respective coup, and a X20 variable will be true for observations taking place between 11 and 20 years inclusive after the coup.

This more complex model, which I shall refer to as the expanded model, has two main advantages. First, as aforementioned, it allows us to distinguish the impact of the coup across different time horizons. Second, it also allows us to better control for the impact of multiple coups. For instance, if a failed unarmed coup took place in 1980 and a successful armed coup took place in 1990, an observation in 2000 will be labelled both CFU20 and CSA10, allowing the regression to control for the effects of the failed unarmed coup while determining the effects of the latter and vice versa. There is, however, a huge downside to this model. With 12 potentially endogenous variables, instrumental variable estimation no longer performs well; the sheer number of instruments and endogenous variables involved result in the correlation between the instruments and coup variables being very weak, causing the Kleibergen-Paap rk Wald F statistic to be very small (16.950 when I ran the model). As such, only a fixed effects without instrumental variables model can be used, which likely leads to inconsistent estimators if the assumption of strict exogeneity is violated, as it likely is, as discussed in Section 5.3. It is thus important to interpret the results of this section with this severe limitation in mind.

When the expanded model was run with the full data from 1960-2013 and on the sub-samples of 1960-1990 and non-high income countries only, none of the coup coefficients were significant, regardless of whether the dependent variable was the Gini or relative income share and whether the multi-level or binary model was used. However, when the expanded model was run with the sub-sample of data from 1991-2013, several of the coup variables were consistently significant across different specifications of the model. Table 11 below details the results of the expanded model when run under a multi-level model and binary model with both the Adjusted Gini and (Q1/Q5)*100 as the dependent variables.

Table 11: Estimates of effects of different coup types on the Adjusted Gini Coefficient and Relative income shares for different time horizons after a coup under FE multi-level and binary models, for years 1991-2013.

Variables	**Adjusted Gini, Multi Level**	**Adjusted Gini, Binary**	**Relative income, Multi-level**	**Relative income, binary**
CSA5	-1.234347 (1.211723)	-1.175578 (1.589372)	1.329019 (1.641588)	0.962879 (1.62431)
CSU5	-0.4967367 (0.6814872)	-1.447025 (0.9750956)	**0.8011026*** **(0.472985)**	1.021226 (0.7057081)
CFA5	1.64398 (1.397462)	2.194765 (1.551913)	-0.5690392 (0.639158)	-0.5091316 (0.7722097)
CFU5	**1.3211*** **(0.7029912)**	**2.400641**** **(1.172991)**	**-1.11325*** **(0.6249821)**	-0.9675051 (0.7916272)
CSA10	0.0323965 (1.098091)	0.8997056 (1.384237)	0.4607642 (0.5424429)	-0.2597155 (0.8317365)

CSU10	0.021751	-1.025756	**1.334034****	**1.992598*****
	(0.6897487)	(1.064483)	**(0.5675789)**	**(0.7670329)**
CFA10	**2.112849****	2.054893	-1.104562	-1.10472
	(1.035987)	(1.267653)	(0.6764729)	(0.8494402)
CFU10	0.8632826	0.9591503	-0.3743098	-0.1431235
	(0.9727262)	(1.133383)	(0.419653)	(0.6156549)
CSA20	0.741544	1.140187	-0.4087968	-1.085795
	(0.8426299)	(1.276176)	(0.6514363)	(1.241509)
CSU20	**-0.9859134****	**-2.129073****	**0.7313603*****	**1.010298***
	(0.4281162)	**(0.8444994)**	**(0.2399161)**	**(0.5212099)**
CFA20	1.098577	0.784814	-0.3535263	0.3697791
	(0.7013022)	(0.8660433)	(0.4884916)	(0.6379756)
CFU20	-0.3624111	0.1522583	0.0707111	0.050298
	(0.6353143)	(1.079054)	(0.3096766)	(0.7332603)
Nineties	**0.7769706****	**0.7467765****	**-0.7842516****	**-0.7220953****
	(0.3599169)	**(0.3532414)**	**(0.3392037)**	**(0.33275)**

*****- Significant at the 1% level. ****-Significant at the 5% level ***-Significant at the 10% level.

Most noticeably, at the 5% level, successful peaceful coups seem to both decrease the Adjusted Gini (and the Significant-only Adjusted Gini and raw Gini) **and** increase the income share poorest quintile relative to the richest quintile 11-20 years after them, thereby reducing inequality. Results for the other coup types were mixed. Successful peaceful coups significantly increased the income share of the poorest quintile relative to the richest quintile at the 5% level, but do not seem to significantly impact the Gini. This discrepancy can, perhaps, again be explained by the fact that the Gini does not only measure inequality by comparing the relative incomes of the top and bottom quintiles. There also seems to be some evidence that failed peaceful coups may increase the Gini 5 years after their occurrence. One might attempt to provide reasons for these results. For instance, it is conceivable that successful unarmed coups tend to install governments committed to structural reform, causing inequality 11-20 years after the coup to be lowered once these reforms have had time to take effect. However, as mentioned in Section 3 of this paper, the theoretical impacts of coups on inequality are ambiguous. As such, without a richer theoretical and empirical model, it is difficult to explain *why* these results are the way they are. Moreover, as aforementioned, endogeneity is a serious concern with the variables in this model. Thus, the results in this section should serve merely as a preliminary exploration of this topic. Further theoretical and empirical research will be needed to better evaluate the impact of these coups types across different time horizons.

Section 7: Overall Discussion of study

In this section of the paper, I will tackle three issues. First, I will discuss the primary limitations of the study, and how these limitations may affect the results. Second, I will attempt to interpret the overall findings of the study, given these limitations. Third, I will note possible future avenues of research that could extend and improve on the research done in this paper.

Section 7.1: Primary limitations of the study

The first significant limitation of this study is sample selection. Unfortunately, as stated in Section 5, data on inequality is not uniformly available across all countries. The Gini and quintile income shares are not observed for many years for low-income countries, whereas high-income countries have comparatively few years of missing data. This imbalanced panel is likely to lead to inconsistent estimators if the data is not missing at random, with selection being determined instead by factors that are correlated with coups and the dependent variable (Wooldridge, 1995). This is, theoretically, very likely the case: the occurrence of a coup, for instance, would disrupt data collection within a country, making calculating a Gini coefficient or a quintile income share impossible in that year. Such a phenomenon also has the unfortunate effect of reducing the number of observations marked as having taken place in the few years after a coup, thereby decreasing the sample size and the power of the test. The most common method in the literature for correcting such a sample selection problem in panel data is Wooldridge's variant of the Heckman Selection Model. Unfortunately, the Wooldridge model requires an exogenous instrument for every time period that predicts selection that is uncorrelated with the measure of inequality (Wooldridge, 1995). It is difficult to conceive of an instrument that meets these criteria. Even should a potential instrument exist, data on that variable is usually scarce as well. The World Bank “Statistical Capacity” indicator (The World Bank, 2015) for one, meets the criteria for such an instrument, but is available only for years beyond 2004. Instruments used in previous studies, such as the size of the country’s population (Bircan, Bruck & Vothknecht 2010), are likely to be correlated with the Gini (Piketty, 2014). Thus, the inability to correct for sample selection will likely cause the estimates to be inconsistent.

The second limitation of this study is measurement error. As elucidated in the data appendix, the WIID data comprises observations that are measured in many different ways. The regression-based approach used to standardize the observations is a crude approach at best. Thus, one cannot rule out the possibility of significant measurement error in the Adjusted Gini coefficients, causing the estimates to be biased. However, as the results of the regressions are robust even when the unadjusted Gini and significant-only adjusted Gini are used as the dependent variables, the results of this study likely did not arise due to the specific way in which I adjusted the Gini. Still, measuring inequality is a difficult task in itself; if there are errors in the observed raw Gini values, the results of this study will be affected. Moreover, measurement error may also be present in the Coup data set; coups, for example, may be incorrectly classified as successful. Due to the sheer number of coups and the lack of a second database to corroborate the information with, it is difficult to identify such measurement errors. However, the documentation for the data set provides detailed cross-tabulation data that supports the notion that the Coup data set is largely error free, mitigating the potential harms of such errors. In short, while measurement error in both the dependent and independent variables is likely present, the implications of this for the results are likely to be limited.

Multicollinearity is the third possible major limitation of this paper. Unsurprisingly, the 4 different types of coups are moderately correlated with one another. This is likely because countries that have coups tend to have multiple coups happen in succession. Table 12 below depicts the correlation matrix of CSATo10, CSUTo10, CFVTo10 and CFUTo10.

Table 12: Correlation matrix of "To10" coup variables

	CSVTo10	CSPTo10	CFVTo10	CFPTo10
CSVTo10	1			
CSPTo10	0.2498	1		
CFVTo10	0.2685	0.2265	1	
CFPTo10	0.2951	0.3143	0.3081	1

As can be seen, years marked as having taken place 10 years after a certain coup type tend to also be marked as having taken place 10 years after other types of coups as well. The moderately high correlation between the variables could inflate the standard errors of the coefficients, making it difficult to determine if they are significant or not. However, this problem is mitigated by performing joint tests on the coup variables, which corroborate the finding that of all 4 coup variables are, jointly, insignificant in all cases save for the one exception noted in section 6.4.

Section 7.2: Interpretation of overall findings

The finding that all 4 types of coups do not significantly affect inequality 10 years after them (save for the exception noted in Section 6.4) may seem counterintuitive. Some might doubt the claim that such tumultuous events have no impact on income inequality in the countries in which they occur. Indeed, these doubts are likely to be correct. It is unlikely that these results are explained by almost every single instance of each type of coup having little to no impact on inequality. It is much more probable that individual coups *do* affect inequality in the short run; a coup that happened in Country A for instance, is likely to change the income distribution in that country. However, *on average across all countries*, each coup type has no impact on the Gini. This means that coups of the same type do not have a tendency to increase or decrease inequality; successful unarmed coups, for instance, do not, on average, increase inequality. Rather, each instance of a successful unarmed coup may, depending on its characteristics, increase or decrease inequality, such that when all instances of such coups are taken as a whole, they have, on average, no discernible impact on inequality. This distinction is crucial. The results of this study should *not* be interpreted as evidence that any single given instance of a coup will have no impact on inequality. Rather, they should be interpreted as support for the notion that the given coup may either increase or decrease inequality, with the coup's type alone not being indicative of which effect is more likely to occur.

As aforementioned, coups are also likely to have very different effects across different time-horizons. As such, examining the impact of coups on the 10 year block after them may obscure finer differences. It could be, for instance, that a certain coup type increases inequality in the first 3 years after the coup, but causes inequality to fall slowly back to pre-coup levels in the next 7 years, such that, on average, it appears that the coup had no impact on inequality 10 years after it. In order to uncover such nuances, a model such as the expanded model used in Section 6.5 that allows for different time horizons is needed. However, the model used must somehow account for a potentially huge number of potentially endogenous and mutually highly correlated variables. That is no trivial task, and should be a subject of much more research. In particular, the construction of equilibrium models which take into account pre-coup and post-coup inequality may help to create a better theoretical understanding of when coups are likely to occur, and how likely they are to affect inequality across different time horizons given certain characteristics of the coup. The provision of such a model would provide hypotheses for future research to test and refine, increasing our understanding of how such political events work.

Section 7.3: Suggestions for future research

Aside from the already discussed need for a better empirical analysis of the effect of coups across different time horizons, the present study could serve as a starting point for three more avenues of research. The first is the effect of the participation of different actors in a coup on inequality. Coups are undertaken by a multitude of different actors, such as the military, the general public or rebel groups. There are theoretical reasons to expect the impact of coups to differ depending on which groups participate in them. For instance, a successful military-led coup will more likely than not cause the income gap to widen. This is as most militaries are led by a few elite generals who tend to arrogate resources once they are in power, as the Junta in Myanmar did. The Coup D'etat Database includes data on whether certain actors participated in each coup. However, that information was not used in this study as its inclusion would inflate the number of endogenous variables, making it difficult to estimate consistent coefficients. Future research could make use of that data to determine whether the participation of certain actors affects post-coup inequality in particular ways. Such research would shed light on the present study as well; it could be, for instance, that certain coup types, such as successful armed coups, only have an impact on inequality if groups such as rebels participate in them.

The second potentially profitable area of research lies in analyzing other key aspects of coups to determine if those characteristics affect inequality. One such direction could be to use a more finely tuned definition of "violent". The present study examines the impact of armed or unarmed coups, implicitly assuming that coups in which weapons are used are likely to be violent. However, the *extent* of the violence could differ; some coups may take more lives than others, while others may destroy more property. As such, future research could examine the estimated amount of lives or property lost in successful or unsuccessful coups and attempt to determine whether coups that are more destructive widen or narrow the income gap. Similarly, future studies could focus on whether coups that install certain types of governments impact inequality. For instance, coups that install functioning democracies, socialist governments, non-functioning democracies and dictatorships are likely to have very different effects on society. Research on this front will be crucial to help answer the question of what sort of government successful revolutions should aim to install in order to narrow the income gap.

The third direction of research is to work with more nuanced definitions of inequality. More specifically, instead of focusing on income inequality between individuals, as this study does, future research could attempt to determine the impact of coups on horizontal inequality, that is, social and economic inequalities between groups that are differentiated by factors such as race, religion or language. Such research is crucial; even if the findings in this study are corroborated by other research and coups are found to have no effect on inequality between individuals, the socio-economic ramifications on a country are nevertheless going to be enormous if inequalities between groups drastically changes as a result of a coup. Research in this vein is particularly crucial for evaluating the ramifications of coups motivated by racial or religious inequalities as the eradication of these differences, rather than inequities between individuals, is likely going to be one of the main goals of the revolutionaries.

Section 8: Conclusion

This paper set out to answer two empirical questions. First, it aimed to determine the magnitude and direction of the impact on inequality that each of the four types of revolutions have. Second, it aimed to determine which of these 4 classes of revolutions have the greatest impact on inequality, if the impacts differ at all. Looking at the Gini coefficient and the relative income shares of the poorest quintile of the population to the richest quintile in the 10 years after a coup, this study finds that all four types of coups have no impact on these measures on inequality at the 5% level. The only exception to this was successful armed coups, which narrowed inequality as measured by the Gini coefficient when looking at the sub-sample of data from 1991-2013. The results of this study, however, should be interpreted with its significant limitations in mind, such as the lack of a correction for potential sample selection. Moreover, these results do not mean that *individual* coups have no impact on inequality, but that *on average*, no effect could be discerned. All in all, the relation between coups and post-coup inequality is a topic ripe for more research, and extensions such as those that attempt to uncover the impact of coups on inequality across different time-horizons are sorely needed. Should future research support the findings of this study however, one must then start to doubt whether revolutions really are a valid cure for the social ill of income inequality.

Data Appendix

The WIID data is comprised of data compiled from several sources. Each of those sources measure the Gini coefficient in different ways. For instance, some of the observations were measured using data on differences in income levels, whereas others were measured using data on differences in consumption levels. Due to this diversity in the way that the data were gathered, if I were to use only observations that meet a certain strict set of criteria, there would be very few data points retained. As such, this study is conducted on the assumption that imperfectly measured Gini data is still better than having no data at all, so long as the data is not considered blatantly unreliable by the data source. Hence, while the data cleaning process is structured such that observations that meet certain important criteria are used over others that do not as much as possible, so as to maximize the comparability of the estimates, it does not *require* every observation retained to meet all of these criteria, or indeed any of them at all. Thus, the cleaned data set will be composed of Gini coefficient observations that are not measured in exactly the same way. In order to meaningfully compare these Gini values, I use a regression-based approach to standardize the values as described in Step 1 below. While this method is commonly used to establish a basis of comparison for Gini coefficients (Dollar and Kraay, 2002; Lundberg and Squire, 2003; Grun and Klasen, 2001), the different ways in which the observations were gathered would likely still affect the comparability of the observations. Hence, the reader should interpret the results of this study with this limitation in mind.

Step 1: I regressed the Gini values on variables indicating how each value was measured.

Appendix Table 1: Results of regression of Gini values on measurement characteristics

Variable names	Coefficient Values	Standard Error	P-Value
Quality: Average	**-1.85538**	**0.7183347**	**0.011**
Quality: Low	0.006555	1.025353	0.995
Area Covered: Other	**-1.85894**	**0.624293**	**0.003**
Area Covered: Rural	1.839259	0.9938238	0.066
Population Covered: Not All	-1.574	1.098669	0.154

Measured by Expenditure	-1.4007	0.9064933	0.124
Measured by Income, Gross	5.331268	0.8676439	0
Measured by other methods	**4.475502**	**1.621128**	**0.006**
Unit of Analysis: Family	-1.34704	1.270569	0.291
Unit of Analysis: Household	2.978523	0.765628	0
Unit of Analysis: Tax Unit	**4.521324**	**1.949959**	**0.022**
Income Sharing Unit: Family	19.87512	0.6757911	0
Income Sharing Unit: Person	2.254713	1.922605	0.243
Income Sharing Unit: Tax unit	9.043432	1.157572	0
Standardization: Household Adult Equivalent	-0.75538	0.5089575	0.14
Standardization: Other	-1.67577	0.8731648	0.057
Standardization: Without Adjustment	**-2.3019**	**1.145082**	**0.046**
Intercept	36.17334	0.5468234	0

Bolded entries are significant at the 5% level.

I then standardized the Gini values using these coefficients, using entries that meet the following characteristics as the base: of high quality, covers the entire area of the country, covers the entire population of the country, measured using a disposable income concept, measured with the person as the unit of analysis, measured with the household as the income sharing unit and standardized according to household per capita. For instance, if an entry has a raw Gini value of 10 but is of low quality and is measured using the household as the unit of analysis, the adjusted Gini value would be 10 + 3.995485 -2.743708. Hence, the adjusted Gini value is what a given observation is estimated to be if it fulfills all of these criteria. None of the adjusted Gini values took on values that are less than 0 or more than 100. I also calculated a "Significant only" adjusted Gini. These are Gini values that are adjusted only by coefficients that are significant at the 5% level, as stated in Appendix Table 1.

Step 2: Each data point was assigned one of four quality ratings by the source. The ratings are "High", "Average", "Low" and "Not Known". 404 entries were assigned the lowest quality rating of "Not Known". As these entries are considered unreliable, they are excluded from my analysis, leaving me with 6739 data points. It should be noted that entries assigned an "Average" or "Low" quality rating are not necessarily unreliable, but simply "did not meet the rather high standards which [the data source] set" (UNU-WIDER, 2015).

Step 3: If a particular country had more than one observation for a particular year, the algorithm below was executed to determine which observation will be used for the analysis. The algorithm works to sieve out data points at each stage. Data points that meet the criterion at that stage are retained whereas those that do not are dropped. If **none** of the data points meet the criteria at a particular stage, then **all** of them are carried over the next stage, instead of dropping all of them. If, after any of the stages, there is only one observation for that country in that year left, then the algorithm terminates and that observation is used for the regression. This "sieving" algorithm was thus used to ensure that, should there be more than one data point for a particular year for a particular country, I will use the data point that best fits my specifications.

a) The quality ratings of the observations were compared with each other, and those with the highest relative quality were retained.
b) Observations that covered the entire area of the country (if any) are retained.
c) Observations that covered the entire population of the country (if any) are retained.
d) Observations that used an income concept (if any) to measure inequality, as opposed to a consumption concept, were retained. The income concept is used by most industrialized countries (Deaton and Zaid, 2002), and was preferred in order to

maintain as consistent a measuring concept as possible in order to maximize comparability of estimates.

e) Observations that used Person weights (if any) are retained, following the recommendations of the data source (UNU-WIDER, 2015).
f) Observations that were adjusted to take account of household size using per capita methods (if any) are retained, following the recommendations of the data source
g) Observations that used the household as the income sharing unit (if any) are retained, following the recommendations of the data source.
h) If more than one observation for that year still remains, the value for that year is calculated using the median of the remaining observations.

Step Four: Steps two and 3 were repeated to obtain a curated sample of data on the first and fifth quartile income shares of each country for years where both statistics are available.

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