Peace Diamonds: Combating Civil War with a Diamond Certification Scheme*

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Abstract

We estimate the effect of an international certification scheme for rough diamonds whose aim is to eliminate, from world markets, diamonds that are sold to finance violent conflict against legitimate governments. Using a difference-in-differences framework we find a 14.3% reduction in the probability of conflict in countries with diamond deposits that can be easily mined by rebel groups. The primary challenge to obtaining a causal estimate of the policy's effect is identifying a group of countries that accurately reflect how conflict would have evolved in countries with diamond deposits in the absence of the policy. We obtain such a counter-factual by constructing a synthetic control group which matches each country with diamond deposits to a weighted average of countries without diamonds. The weights are chosen so that the synthetic country matches the country with diamonds in terms of its covariates and history of conflict. In addition to estimating the effect of the policy on the probability of conflict we provide evidence that the policy worked to reduce conflict by reducing the duration of conflict and find no evidence of an effect on the onset of new conflict. The results suggest that certification schemes applied to natural resources used to finance conflict can be an effective means to reduce the duration of conflict. However, the results suggest that such policies are not sufficient to prevent conflict from initiating.

Keywords: conflict, the kimberley process, diamonds, conflict minerals, civil war
JEL Codes: D74, F51, F53, F55, O19

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1 Introduction

One and a half billion people live in countries affected by violent conflict (World Bank, 2011). With conflict comes poverty, famine, disease and human rights abuses (Blattman and Miguel, 2010). In addition to its humanitarian consequences, conflict contributes to human suffering through its effect on economic development. This is highlighted by the fact that no low-income conflict-affected country has achieved any of United Nations Millennium Development Goals (World Bank, 2011). For this reason the World Bank described the reduction of violence as the primary development challenge of our time. However, in the conflict afflicted countries, existing development policy and humanitarian assistance has been shown to be problematic, with the policies actually causing an increase in conflict in some instances.[1] The problematic nature of development policy in these countries highlights the importance of effectively designing policies that directly target the reduction of conflict. The great importance of this issue has led to a tremendous amount of research on the causes of conflict. Although our understanding of the causes has improved, little is known about the effectiveness of policies aimed at reducing the incidence of violent conflict.

An increasingly popular policy option for reducing conflict is to place sanctions on natural resources that are known to be financing conflict. In this paper we estimate the effect of one of the first and longest running policies to reduce conflict by restricting the sale of natural resources. In particular, we estimate the effect of an international certification scheme for rough diamonds, known as The Kimberly Process (KP), on the probability of civil war. We find that the introduction of the KP caused a decrease in the incidence of conflict by approximately 14.3% in countries effected by the policy.

The KP is a joint initiative between the United Nations and governments of both diamond importing and exporting countries and is essentially a club in which member countries commit to buying and selling rough diamonds exclusively from other members (Haufler, 2009). To gain entrance to the club, diamond producing countries must enact a system of laws ensuring that all diamonds exported are not used to finance violent conflict against a legitimate government. At a minimum, the system of laws must require all diamond exports to be shipped in tamper-resistant containers and to be accompanied by a government issued certificate of origin. The certificate of origin must be forgery-resistant and describe the shipments contents. To ensure the system of controls is effective, members must submit to regular third party audits. Should an audit reveal that the system of controls is unable to prevent diamonds that are funding conflict from entering world markets, that country is expelled from the KP and sanctions are imposed on all diamond

[1] Nunn and Qian (2014) show humanitarian assistance in the form of food aid caused an increase in conflict. Crost et al. (2014) show that large development projects in the Philippines caused an increase incidences of conflict in regions that received the aid

exports.

Using a panel of countries over the period 1998 to 2008 we employ a difference-in-differences approach to identify the effect of the KP on the probability of conflict in countries with diamond deposits. This strategy identifies the effect of the KP by comparing the change in conflict before and after its implementation in countries with diamond deposits to a control group of countries without diamond deposits. The primary assumption necessary to identify the causal effect of the KP is that conflict would have followed a similar time trend in the two groups of countries had the policy not been implemented. To gain insight into whether this assumption is satisfied we examine the time trends in average conflict prior to the introduction of the KP. In doing so we see that countries with diamond deposits experienced a relatively flat time trend while countries without diamonds were on a downward trend. Estimating the effect of the KP without accounting for these differential time trends will not provide a causal estimate of the KP's effect on the probability of conflict. We overcome this issue in two ways. First, we construct a synthetic control group (Abadie and Gardeazabal, 2003) which gives greater weight to countries in the control groups that are similar to the treated countries in terms of their history of conflict and pretreatment covariates. Second, we restrict the sample to countries in Sub-Saharan Africa. Doing so reduces the sample size but enables a comparison of countries that are more similar in terms of the values of their covariates and time trend in average conflict.

Without accounting for the differential time trends we find that the introduction of the KP reduced the incidence of conflict in countries with diamond deposits relative to the control group by approximately 9.6%. After correcting for the trends with the synthetic control the estimates increase to approximately 14.3%.

We explore whether this reduction is the result of a reduction in the duration of ongoing conflicts or a reduction in the onset of new conflict. the mechanism by which the KP reduced conflict by examining the effect of the KP on the onset and duration of conflicts. The intent of the KP is to reduce conflict by limiting the ability of rebel groups to finance their operations though the sale of rough diamonds. If access to financing is an important barrier to the formation of rebel groups, then the introduction of the KP should be associated with a reduction in the onset of new conflicts. On the other hand, because the KP does not address the underlying causes of conflict (e.g. political grievances) eliminating a potential source of financing may not effect the onset of conflict but might effect the duration as rebel groups are costly to operate and diamonds are a lucrative source of financing.

The enormous cost associated with operating a rebel group has lead some researchers to hypothesise that violent conflict will only occur in areas in which it is financially feasible (Collier et al., 2009). According to this theory a reduction in the ability to finance conflict would be associated with a reduction in the onset of conflict. Other research suggests that access to financing does not cause conflict but can intensify and prolong existing conflicts. Angrist and Kugler (2008) exploits an exogenous increase in coca cultivation in Colombia resulting from the bombing of trade routes used to import coca leaf from Bolivia and Peru to test if an increase in illicit resources has an effect on violence by increasing the resources available to insurgent groups. The results show that regions that experienced an increase in coca cultivation also experienced an increase in violence. However, the conflict in Colombia began long before the increased cultivation, leading the authors to state "Clearly, cocaine cannot be blamed for starting this conflict, though it may play a role in perpetuating it." (Angrist and Kugler, 2008) Berman et al. (2014) provides additional support for the idea that access to finance does not cause conflict but can prolong it by estimating the probability a conflict intensifies after a rebel group captures a region with an active mine. The authors find that the probability of conflict in non mining regions of a country is significantly higher when a rebel group was successful in conflict in a mining region in the previous year. This effect is particularly strong when mines are located in the homeland of a discriminated ethnic minority. In the context of diamonds, Richards (1996) argues that the diamond financed civil war in Sierra Leone initiated as a result of political grievances associated with government corruption. Although diamonds may not have caused the civil war they may have played a role in prolonging it as the chaos associated with the conflict allowed rebel leaders to make large profits smuggling diamonds out of the country (Keen, 2005).

Consistent with the view that access to finance does not cause conflict but can prolong existing conflicts, we provide evidence that the KP reduced the duration of conflict and find no evidence of an effect on the onset of new conflict. In particular, for the sample of Sub-Saharan African countries we find that the introduction of the KP caused a significant increase in the probability that a conflict would come to an end in the first three years the program is in effect.

These results complement a large literature attempting to identify the opportunity cost of conflict. Changes in access to financing can effect an individuals decision to join a rebel group as the source of financing likely effects the ability to pay combatants. Furthermore, having access to a lucrative source of financing can effect the probability a conflict is successful by effecting the quality of arms that the group is able to purchase and the number the soldiers the group can afford to pay. This makes joining a rebel group more attractive by increasing the expected payoff from joining the group. The majority of papers in this literature identify the opportunity cost by exploiting exogenous variation in income arising from shocks to productivity or world commodity prices. The idea being that a positive price or productivity shock to labour intensive agricultural commodities raises wages thereby reducing the incentive to participate in conflict.[2] Dube and Vargas (2013) provide within country evidence from Colombia that positive shocks to coffee prices, a labour intensive commodity, reduces the intensity of conflict in coffee growing regions. These results are consistent with Bazzi and Blattman (2014) who provide cross country evidence that rising agricultural commodity prices are associated with shorter conflicts and fewer battle deaths.

Although changes in income are associated with changes in the intensity of conflict, there is little evidence that changes in income effect the onset of new conflicts. For example, Bazzi and Blattman (2014) find no evidence of an effect of price shocks on the outbreak of new conflicts or coups. These results are consistent with ours in that we show the KP increased the probability a conflict will end but had no effect on the onset of new conflicts.

This paper is also related to a large literature on the effects of natural resource wealth on conflict. Of particular the relevance to this paper is the literature that considers the effect of the diamond wealth on conflict.[3] The results in this literature are mixed; some papers find positive correlations between diamond deposits and conflict and others find no relationship. However, unlike other commodities there is a large amount of variation in the quality and therefore the price of diamonds which makes researchers unable to exploit variations in price to estimate a causal effect of diamond wealth on conflict. We contribute to this literature as we our the first to exploit exogenous variation in the ability to finance conflict with diamond wealth.

A related paper evaluating the effect of a policy to reduce conflict through restrictions on the sale of natural resources is Parker and Vadheim (2014). The authors study the effect of US legislation requiring companies to report their source of minerals known to be financing conflict in the Democratic Republic of the Congo. Parker and Vadheim (2014) find that the policy caused an increase in rebel group attacks against civilians in regions that mined the regulated minerals. Furthermore, the policy caused an increase in conflict between rebel groups in regions with minerals that were not effected by the policy. These results are consistent with their theory in which rebel groups that lose their financing source seek new sources by looting citizens and challenging other rebel groups to acquire their source of financing. The case of the Democratic of Republic of the Congo is quite unique in that large areas of the country completely lack any formal government (Sanchez de la Sierra, 2013). As such Parker and Vadheim (2014) provides insight into how restriction on the sale of natural resources effect conflict in lawless states. Given the unique institutional environment it is questionable as to whether the results

[2]For papers that use variation in commodity prices see Bazzi and Blattman (2014) and Brückner and Ciccone (2010). For papers that use exogenous productivity shocks see Miguel et al. (2004) and Couttenier and Soubeyran (2014)

[3]See Ross (2006) for a review of the literature.

are applicable to countries with more developed institutions. In this paper we evaluate the effect of a policy restricting the sale of natural resources in numerous countries with different institutional settings.

2 The Kimberly Process Certification Scheme

The Kimberly process was developed in May of 2000 in Kimberly, South Africa when diamond producing nations together with industry representatives and non governmental organizations met to address the issue of the rough diamonds being sold to finance violent conflict against legitimate governments in Sub-Saharan Africa. The meeting resulted in United Nations resolution 55/56 which made the KP international law and in 2003 the process was put into practice.

The KP is a certification scheme requiring member countries to trade rough diamonds exclusively with other members. In order to gain entrance into the KP countries must pass into law a system of controls that ensures all diamond exports are not being used to finance conflict. The system of controls requires all diamond exports to be shipped in tamper resistant containers and be accompanied by a government issued certificate of origin. In addition, member countries must summit to regular third party audits of their system of their controls. Should an audit determine that the controls put in place are unable to prevent conflict diamonds from entering legitimate diamond markets, sanctions are imposed by the UN on all diamond exports from that country.

Given that all major diamond importing countries are members of the KP, any country with diamond deposits wishing to access legitimate markets is required to join the KP. This implies that all countries with diamond deposits are effected by the KP irrespective of whether they have formally joined the process. Consider a rebel group in a non-member country attempting to finance their operation with diamonds. This group will face the same difficulties exporting diamonds as a rebel group in a country that has not been issued a KP certificate as both groups are unable to access the formal diamond markets. Thus whether a country is effected by the KP is determined by the physical presence of diamond deposits. This eliminates concerns that countries of a certain type select into the KP as treatment is determined by the exogenous geological characteristics of the country.

Since its inception three countries have had sanctions imposed on diamond exports for non-compliance. In 2004 the Republic of Congo was removed from the process and sanctions were place on diamond exports after a discrepancy was noticed between the country's estimated diamond capacity the volume of exports. Following an audit it was found that the system of controls put in place to eliminate conflict diamonds was inadequate and poorly enforced (Global Witness, 2005). The Republic of Congo was reinstated to the KP in 2007 after implementing improvements to it's monitoring system (US Department of State, 2014). In 2005 Cote dIvoire was expelled from the KP and sanctions were placed on all diamond exports after it was found that illicit diamond sales were funding the rebel group "Forces Nouvelles" in the north of the country. Finally, in 2013 Central African Republic was temporarily suspended after it was discovered that both sides of the conflict were partially financing their operations through the sale of rough diamonds. Armed groups in the west of the country associated with the Anti-balaka movement were directly involved in artisanal diamond production while armed groups in the east (affiliated with the Séléka rebels) imposed taxes on aircraft transporting diamonds and collected security payments from diamond miners (United Nations, 2014b).

The Kimberly process is not without it's problems. The primary criticism leveled against the KP is that the sanctions are ineffective as diamonds can be easily smuggled into bordering countries and exported along with certified diamonds. A UN group of experts (United Nations, 2014c) found that while Côte d'Ivoire was under sanctions, rough diamonds were being smuggled into neighboring Guinea and Liberia where they were exported along with KP certified diamonds (United Nations, 2014c). Given these criticisms our estimates of the polcy's effect represents a lower bound on the effectiveness of such policies.

3 Data

Our sample consists of a balanced panel of 110 non-OECD countries for the period 1991-2008. Our primary outcome variable of interest is the incidence of an intrastate armed conflict, which we obtain from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002).[4] This variable takes a value of one if an internal armed conflict occurred in a given country in a given year. Armed conflict is defined as "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." (Gleditsch et al., 2002) A party can be either a governments or an opposition group where opposition groups are defined as: "Any non-governmental group of people having announced a name for their group and using armed force to influence the outcome of the stated incompatibility".(Gleditsch et al., 2002)

The treatment group consists of countries with diamond deposits that can be easily mined by rebel groups while the control group consists of all other countries. We classify deposits as easily extractable by rebel groups based by the geological form of the deposit. Diamond deposits come in two different geological forms: primary and secondary. The

[4]Since the KP was designed to minimize the conflict between rebel groups and governments we exclude all incidences of the interstate conflict for our analysis.

two types of deposits vary greatly in the amount of capital required for extraction and therefore in the ability of rebel groups to extract them to finance their operations. Primary, or kimberlite, diamonds are found deep beneath the earth's surface and require large capital investments to extract. Secondary, or alluvial diamond deposits are found in river beds and can be extracted by unskilled labour with minimal capital investment. The relative ease of extracting secondary diamonds has made them an attractive source of financing for rebel groups (Olsson, 2006). It is for this reason that we consider countries with secondary diamond deposits to be treated by the policy.

Data on the presence of secondary diamond deposits is obtained from Gilmore et al. (2005). This dataset categorizes diamond deposits as primary, secondary or unknown. Deposits of unknown type are further categorized as either "probable primary" or "probable secondary". From this dataset we construct a dummy variable that equals one if a country has at least one diamond deposit classified as either secondary or probable secondary. We drop all deposits for which it is unknown whether production has ever occurred at that site.[5]

4 Summary Statistics

Given the potential for secondary diamond deposits to provide rebel groups with a lucrative and easily accessible source of financing, one might suspect countries with secondary deposits to differ significantly from the rest of the world in terms of their propensity for conflict. Table 1 presents the pretreatment summary statistics for our measure of conflict, along with covariates that are commonly used in the conflict literature.[6] As seen in the table countries with diamond deposits are significantly more prone to conflict with 32.1% of country-year observations being in conflict relative to 18.1% for countries without diamond deposits. We also see a significant difference in GDP between the two groups of countries, with countries with diamond deposits being significantly poorer than the rest of the world.[7] We also find significant differences in income derived from metal extraction, ethnic fractionalization, and population. There is no significant difference in the quality of political institutions as measured by the Polity 2 index.[8]

Next we examine how conflict varies overtime in the two groups of countries. Figure 1 plots the average incidence of conflict in countries with and without secondary diamond deposits. The vertical line at 2002 indicates the year before the KP came into effect.

[5]Dropping mines with unknown production eliminates Columbia and Thailand as countries with secondary diamond deposits. After searching the country reports from the USGS we find no reports diamond mines existing in Columbia

[6]All statistics are presented for the five years prior to the introduction of the KP.

[7]Data on GDP comes from The Maddison-Project, http://www.ggdc.net/maddison/maddison-project/home.htm, 2013 version.

[8]The metal minerals variable is obtained from Haber and Menaldo (2011).

Table 1: Pretreatment Characteristics: 1991-2002

	Treatment	Control	Difference
Conflict	.321	.181	.139***
Polity	1.891	1.183	.708
GDP	2326.665	3873.751	- 1547.09***
Metals Income	39.145	18.289	20.856***
Fractionalization	.636	.488	.149***
Population (millions)	104	16.7	87.1***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As seen in the figure average conflict in countries with secondary diamonds deposits was relatively constant in the five years preceding the introduction of the KP. In the year the KP came into effect average conflict decreases substantially and remains low in the following years. The control group, countries without secondary diamond deposits, experienced a very different trend with conflict decreasing in the years preceding the introduction of the KP and remaining constant in the years following its introduction.

Analyzing the two trends it appears that the KP had a large and persistent effect on the incidence of conflict in countries with secondary diamond deposits. However, given that countries without secondary diamond deposits were following a different time trend they do not represent an ideal counterfactual for how conflict would have evolved in countries with secondary diamond deposits in the absence of the KP. Estimating the effect of the KP without correcting for the different time trends will provide biased results. To obtain a more accurate estimate of the effect of the KP we correct for the differential time trends in two ways. First, we construct a synthetic control country for each treated country which is a weighted average of countries in the control group where greater weight is given to countries with similar pretreatment covariates and history of conflict. Second we restrict the sample to countries in Sub-Saharan Africa as countries in this region are more similar in terms of their experience with conflict.

Figure 2 plots the average incidence of conflict in Sub-Saharan Africa for countries with and the without secondary diamond deposits. As seen in the figure the two groups of countries follow a similar time trend in the five years preceding the introduction of the KP suggesting that the control countries in this subsample represent a relevant counterfactual for the treated countries. However, differences remain between the two groups of countries in terms of their covariates as seen in Table 2. Countries with secondary deposits on average derived more income from metal production, have a higher level of ethnic fractionalization and larger populations. Because of these differences we also construct a synthetic control group for Sub-Saharan Africa.

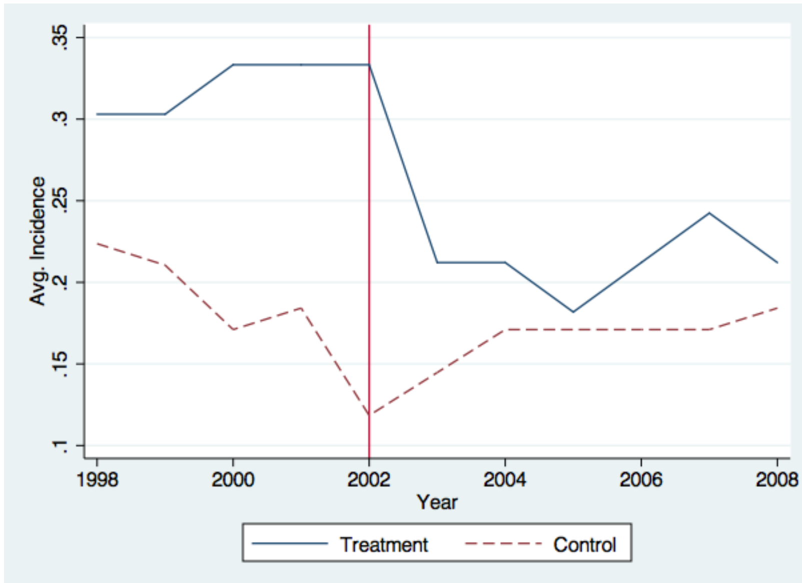

Figure 1: Average incidence of conflict in countries with and without secondary diamond deposits.

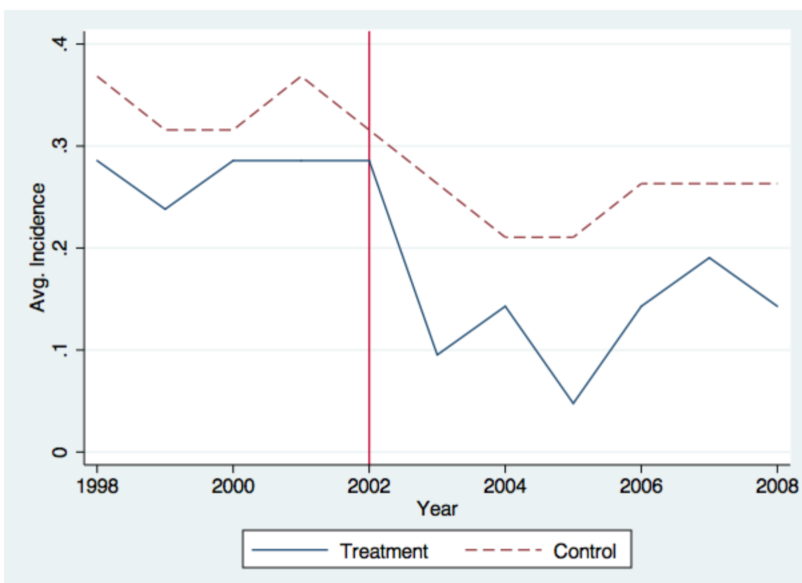

Figure 2: Average incidence of conflict in Sub-Saharan Africa for countries with and without secondary diamond deposits.

Table 2: Pretreatment Baseline Characteristics Sub-Saharan Africa: 1998-2002

	Treatment	Control	Difference
Conflict	.276	.32	-.061
Polity	1.038	.7	.353
GDP	1379.756	1605.001	-278.692
Metals Income	49.214	13.489	35.014***
Fractionalization	.743	.607	.103***
Population (millions)	19.5	13	5.8*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Identification Strategy

To identify the effect of the KP on the probability of conflict we employ difference-in-differences identification strategy. The difference-in-differences approach identifies the effect of the KP on the incidence of conflict by comparing the change in conflict before and after the introduction of the policy in the treatment and control groups Let Sec_i be an indicator variable equal to one if country i has secondary diamond deposits and zero otherwise (ie $Sec_i = 1$ if a country is in the treatment group). Let KP_t be a dummy variable that equals one if the KP is in effect in year t and zero otherwise. Countries are considered treated if they the have secondary deposits and the KP is in place ($Sec_i \times KP_t = 1$). Our outcome variable of interest is the incidence of conflict in country i in year t which we denote as c_{it}. Our main estimating equation is as follows:

$$c_{it} = \delta Sec_i \times KP_t + \gamma_i + \lambda_t + \epsilon_{it}, \tag{1}$$

where γ_i and λ_t are country and time fixed effects and ϵ_{it} is a random variable that measures idiosyncratic changes in conflict. Our coefficient of interest is δ which represents the average treatment effect of the KP on countries with secondary diamond deposits.

We also estimate a specification which allows the treatment effect to vary overtime by replacing KP_t with a series of indicator variables for each post treatment year. The estimating equation is as follows:

$$c_{it} = \sum_{t=2003}^{2008} \delta_t Sec_i \times I_t + \gamma_i + \lambda_t + \epsilon_{it}, \tag{2}$$

Where I_t is an indicator variable that equals one in year t. The coefficient δ_t captures the treatment effect of the KP in year t. If after losing diamonds as a source of financing rebel groups require sometime to acquire a new source of financing we would expect to see large and statistically significant effects of the KP in first few years following its introduction and smaller effects in later years. Alternatively, the KP may have had a delayed effect on the probability of conflict if rebel groups accumulated excess funds from diamond sales made prior to the introduction of the KP and used those funds to finance their operations in years immediately following the introduction of the KP. In this case we would expect the treatment effect to be small and not statistically significant in the first few years of the policy and greater in magnitude and statistically significant in later years. Equation 2 allows us to evaluates these competing conjectures by examining the treatment effect in each year the KP is in effect.

A necessary assumption to identify the treatment effect is that conflict would have followed a common time trend in the treatment and control groups had the KP not been implemented. However, as discussed in previous section this assumption is unlikely to hold for the full sample of countries. The downward tend in countries without secondary diamonds combined with the relatively constant trend in countries with secondary diamonds implies that our estimate of the effect of the KP will biased towards zero and we will therefore be estimating a lower bound on the effect of the KP on the incidence of conflict.

We account for this bias by constructing a synthetic control group (Abadie and Gardeazabal, 2003; Abadie et al., 2010). The synthetic control method constructs a control country for each treated country from a weighted average of the countries in the control group. The synthetic control method was originally developed for comparative case studies in which there is a single treated unit and multiple control units. Since we have multiple treated countries, we follow Severnini (2014) and create a synthetic control country for each treated country, then pool all of the treated and synthetic control countries together and estimate the treatment effect using the differences-in-differences framework (equations (1) and (2)). As each synthetic control country is intrinsically associated with it's treated counterpart we cluster the standard errors at the match level where a match consists of the treated country and its synthetic counterpart.

The weights given to each control country are chosen so that the synthetic control country closely resembles the treated country in term the pretreatment value of the covariates and history of conflict. More specifically, the weights are chosen to minimize the squared difference in the pretreatment average covariates between the treated and control countries. Let W be a $(J \times 1)$ vector of non-negative weights, w_j, assigned to each of the J control countries. The minimization problem that determines the weight given to each control country can be written as follows:

$$WMin(X_1 - X_0W)'V(X_1 - X_0W) \tag{3}$$

subject to: $w_j > 0, \forall j$ and $\sum_{j=1}^{J} w_j$

Where X_1 is a $(K \times 1)$ vector of pretreatment covariates for the treated group with K being the number of covariates. X_0 is $(K \times J)$ matrix of pretreatment covariates for the control groups. Finally, V is a $(K \times K)$ diagonal matrix, the elements of which reflect the relative importance of each covariate in predicting incidences of conflict.[9]

The covariates used to construct each synthetic control are: log gross domestic product per capita, the log of income resulting from metal extraction, the Polity IV index, ethnic fractualization and log population. We also include two years of lagged conflict (1998 and 2002). The weights given to each control country for the corresponding treated country can be found in Table 17 for full sample of countries and Table 18 for Sub-Saharan Africa in the Appendix.

Table 3 presents the pretreatment summary statistics for the treatment, control and synthetic control groups for both the full sample of countries and the sub-sample of Sub-Saharan African countries. The synthetic control group more closely matches the treatment group in terms average conflict and the two pretreatment values of conflict in both samples. In terms of covariates the synthetic control more closely matches the treatment group for most variables. The exceptions being the polity IV index in the full sample and the population in Sub-Saharan Africa.

Table 3: Pretreatment Characteristics: 1998-2002

	Full Sample			Sub-Saharan Africa		
	Treatment	Control	Synthetic	Treatment	Control	Synthetic
Conflict	.321	.181	.314	.276	.32	.269
Polity2	1.891	1.183	1.035	1.038	.7	.86
GDP	2326.665	3873.751	2590.041	1379.756	1605.001	1610.81
Metals Income	39.145	18.289	32.88	49.214	13.489	27.35
Fractionalization	.636	.488	.61	.743	.607	.67
Population (millions)	104	16.7	29.4	19.5	13	12.5
Con(2002)	.333	.118	.31	.286	.3	.277
Con(1998)	.303	.224	.32	.286	.35	.291

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figures 3 and 4 plot the time trends in average conflict in the treated and synthetic control countries for the full sample and Sub-Saharan Africa. The common trend assumption appears to be satisfied for both samples as the treatment and synthetic control group experience a relatively flat time trend in average conflict prior to the introduction of the KP. For the full sample of countries in particular synthetic control group appears to provides a better counterfactual for how conflict would have evolved in the treated group in absence of the KP. Thus estimates of the effect of the KP using the synthetic control group are likely to be unbiased.

A potential issue with identifying the treatment effect in any difference-in-differences setting arises if an unobserved factor that effects the outcome variable changes for the

[9]See Appendix for how V is derived.

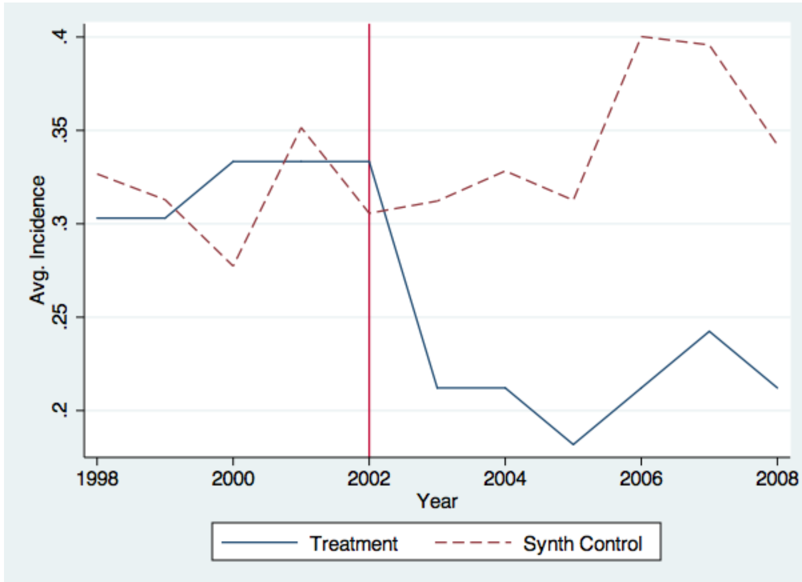

Figure 3: Average incidence of conflict in the countries with secondary diamond deposits and synthetic control countries.

treatment group at the same time that the treatment occurs. In this situation the treatment effect δ captures both the effect of the treatment and the change in the unobserved factor.

The only important policy change that occurred around the same time as the KP is the United Nations approach to peacekeeping. In 2003 the UN began increasing the number of peacekeeping troops it sent to countries in or at risk of conflict. The increase followed what the UN saw as a failure of the international community to prevent the tragedies in Rwanda and the Balkans in the 1990s. Following these tragedies the UN adopted a norm know as "Responsibility to Protect," which asserts that all governments have a responsibility to protect their population from genocide, war crimes, ethnic cleansing and crimes against humanity. Should a government be unwilling or unable to fulfill this responsibility, the responsibility falls on the international community (United Nations, 2014a). The policy change presents a problem for identifying the effect of the KP if the countries with secondary diamond deposits experienced a differential increase the UN troops relative to the control group.

To test if the number of UN troops differed in our treatment and control group following the introduction of KP we estimate equation (1) using the log number of the UN peacekeeping troops as the outcome variable. If the number of troops does not differed in countries with and without secondary diamonds after the KP was implemented then the coefficient δ should not be significant, implying that the troop increase does not present

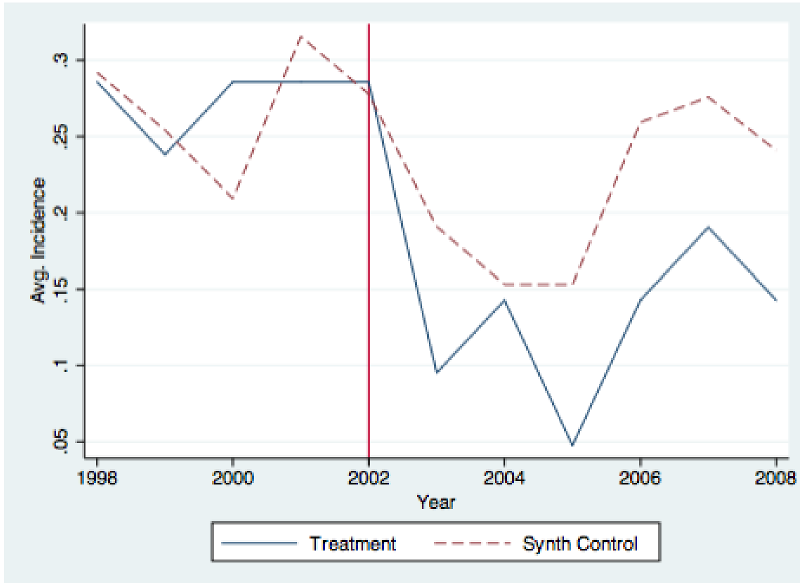

Figure 4: Average incidence of conflict in Sub-Saharan Africa in the countries with secondary diamond deposits and synthetic control countries.

an issue for identifying the effect of the KP. Data on the number of UN peacekeeping troops is obtained from the International Peace Institute's Providing for Peacekeeping data set. The data contains the number of UN peacekeeping troops in a given country and month. We aggregate the monthly data to the annual level by averaging the number of UN troops in all months that have a positive number of troops.

Table 4 presents the results for both the full sample and Sub-Saharan African countries. In both samples the coefficient on the interaction term of interest is not significant, indicating that the treatment and control groups were not effected differently by the troop increase. Thus the treatment effect δ captures only the effect of the KP.[10]

6 Results

Table 5 reports the difference-in-differences estimates from equations (1) and (2) for the full sample of countries. Columns (1) and (2) reports the results without correcting for the differential time trends while columns (3) and (4) report the results using the synthetic control. The result in first column indicates that the introduction of the KP reduced the probability of conflict in countries with secondary deposits by 9.6%. Recall that estimates without correcting for the downward trend in the control group the result is

[10]This implies that the effect of the troop increase will be accounted for when taking the second difference between the change in conflict in the two groups of countries.

Table 4: Results From Difference-in-Differences Using the numberof UN Troops as an Outcome

	Full Sample	Sub-Saharan Africa
KP_t	0.153	0.724
	(0.213)	(0.469)
Sec_i	-1.295***	2.865***
	(0.161)	(0.279)
$Sec_i \times KP_t$	0.741	0.673
	(0.483)	(0.838)
N	2049	738
Fixed effects	No	No

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

likely biased. Column (3) presents the result correcting for the downward trend using the synthetic control. The coefficient on our main interaction term becomes more negative and indicates that the KP caused a 14.3% reduction in the probability of conflict in countries with secondary diamond deposits, a result that is significant at the 1% level. Columns (2) and (4) present the results from equation (3) which allows the treatment effect to vary overtime. Both with and without the synthetic control the treatment effect is large and persistent across all years that the KP is in effect although without the synthetic control none of the coefficients are statistically significant. The treatment effect is particularly large when using the synthetic control group in the years 2006 and 2007. Overall the results in Table 5 suggest that restricting a rebel groups access to a lucrative source of financing has a large and persistent effect on the probability of conflict.

Table 6 presents the results for Sub-Saharan Africa. We find a much smaller treatment effect both with and without the synthetic control. When using the synthetic control we find the the KP caused a decrease in the probability of conflict by 9.1%, a result that is significant at the 10% level. In next section we provide evidence that this result is driven by the KP having a differential effect on the probability of a new conflict beginning and an ongoing conflict coming to an end.

To assess the plausibility of our results we compare the magnitude of our estimates to within country studies of natural resource financed conflict. Dube and Vargas (2013) estimate the effect of an increases in oil prices on the probability of conflict in oil producing regions in Columbia. The authors provide case study evidence that paramilitary groups in oil producing regions partially fund conflict by drilling holes in oil pipelines and selling the stolen oil on the black market. Fluctuations in the world price of oil represent changes in the funding available to paramilitary groups in oil producing regions relative to non-oil

Table 5: Results From Difference-in-Differences: Full Sample

	Unadjusted		Synthetic Control	
	(1)	(2)	(3)	(4)
$Sec_i \times KP_t$	-0.09637*		-0.14282***	
	(0.05206)		(0.0508156)	
$Sec_{2003} \times I_{2003}$		-0.07225		-0.10646
		(0.07064)		(0.0816938)
$Sec_{2004} \times I_{2004}$		-0.09856		-0.12255
		(0.08383)		(0.0839934)
$Sec_{2005} \times I_{2005}$		-0.12887**		-0.13719**
		(0.06078)		(0.0599876)
$Sec_{2006} \times I_{2006}$		-0.09856		-0.19452***
		(0.06644)		(0.0669992)
$Sec_{2007} \times I_{2007}$		-0.06826		-0.15973**
		(0.07089)		(0.0794617)
$Sec_{2008} \times I_{2008}$		-0.11172		-0.13649
		(0.07786)		(0.0831823)
N	1199	1199	726	726
Fixed effects	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Results From Difference-in-Differences: Sub-Saharan Africa

	Unadjusted		Synthetic Control	
	(1)	(2)	(3)	(4)
$Sec_i \times KP_t$	-0.05798		-0.09166*	
	(0.09116)		(0.04829)	
$Sec_{2003} \times I_{2003}$		-0.10727		-0.10220
		(0.12109)		(0.10139)
$Sec_{2004} \times I_{2004}$		-0.00702		-0.01672
		(0.12661)		(0.09055)
$Sec_{2005} \times I_{2005}$		-0.10226		-0.11196
		(0.09925)		(0.06523)
$Sec_{2006} \times I_{2006}$		-0.05965		-0.12287
		(0.10959)		(0.08392)
$Sec_{2007} \times I_{2007}$		-0.01203		-0.09182
		(0.13623)		(0.10537)
$Sec_{2008} \times I_{2008}$		-0.05965		-0.10439
		(0.14364)		(0.09054)
N	440	440	609	609
Fixed effects	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

producing regions. The authors find that a 10% increase in the price of oil resulted in a 1% increase attacks per year in oil producing regions. If the KP was completely effective, meaning all diamond that were financing conflict were eliminated from world markets then the 14.3% reduction in conflict (Table 5) appears reasonable when compared to a 1% decrease following a 10% reduction in the value of resources used to finance conflict.

6.1 Mechanisms

In this section we explore the mechanism by which the KP reduced the incidence of conflict by examining its effect on the probability of new conflicts beginning and existing conflicts coming to an end. As detailed in the introduction, the existing literature on natural resources and conflict suggests that access to financing plays an important role in determining a conflict's duration and has less of an effect on the onset of new conflicts. Anecdotal evidence suggests that a similar pattern holds for diamond financed conflict. For example, the conflict in Côte d'Ivoire, which was partly financed by diamond wealth, began as a result of issues surrounding migration, nationality and ethnicity. The conflict occurred between the largely Muslim and migrant population in the country's north and the Côte d'Ivoire government, which represented the southern Christian population. In the years leading up to the civil war, the northern Muslim population were seeing their political rights eroded. In 2000 the constitution was changed to require the parents of presidential candidates to have been born in Côte d'Ivoire thereby making the most popular candidate from the north ineligible. Political rights were further eroded when in 2001 the government instituted strict requirements to obtain a national identity card effectively disenfranchising a large fraction of the northern population. The violence began following a disputed election result in 2001 which marked the beginning of the civil war (Collier, 2011). The stated objective of the northern rebel group was to overthrow the government and hold inclusive elections (Bah, 2010). Although diamonds partly financed the northern rebels military operations it was issues surrounding citizenship that ignited the conflict. While, it is possible that these political grievances could have been resolved peacefully had the northern rebel group not had a lucrative and easily accessible source of financing it does not appear that diamond wealth itself caused the onset of the conflict.

We examine these issues in more detail by estimating the effect of the KP on the onset and duration of conflict. Estimating the probability of onset requires restricting the sample to include only years of ongoing peace and the initial incidence of conflict. Estimating equation (1) using onset as the outcome variable provides us with the effect that the KP has on the probability a peaceful period will come to an end. Similarly, estimating the effect of the KP on the duration (or offset) of conflict requires restricting the sample to include only years of ongoing conflict and the initial year of peace. Estimating equation (1) using offset as our outcome variable provides us with the effect that the KP had on the probability a conflict would end. Given the smaller sample size we estimate equation (1) with and without country and year fixed effects.

Note that the sample of countries for both outcome variables is different then in incidence regression. For example, when using offset as the outcome variable both the treatment and control groups consist of only countries that were in conflict in the previous year. This implies that we need to reassess whether the common trend assumption is satisfied for these outcome variables. Note that the number of countries in the sample changes each year as countries enter into and exit out of conflict. As a result, the average outcome variable may change because of either a change in the outcome variable itself or a change in the sample size. This makes a visually examination of the common trend assumption problematic as we are unable to assess whether the average outcome variable is changing as a result of a change in the sample size or a change in the outcome variable. As an alternative to the visual inspection of the time trends we test whether the common trend assumption is satisfied by conducting placebo tests in which we estimate

Table 7: Placebo Test 1998-2002: Onset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	0.09473				0.13327			
	(0.06851)				(0.11067)			
D_{it}^{2001}		0.04756				0.04245		
		(0.05441)				(0.11078)		
D_{it}^{2000}			0.07890				0.08915	
			(0.06320)				(0.11007)	
D_{it}^{1999}				0.05564				0.05605
				(0.07382)				(0.12991)
N	418	418	418	418	139	139	139	139
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the "effect" of the KP on pre treatment changes in conflict (Kline and Moretti, 2014). In particular, we run separate regressions on pretreatment data assuming the KP was implemented in each of the five years prior to actual introduction of the KP. If the two groups of countries are following a similar time trend we would expect the coefficients on the placebo interactions to be close to zero and not statistically significant. To ensure that any non significant results are because of a common trend and not the smaller sample size we conduct all analysis using both the sample period 1998-2002 and 1991-2002.[11] The results using the 1991-2002 are very similar to the 1998-2002 sample and can be found in the appendix.

Tables 7 and 8 presents the results for the placebo tests using onset and offset as the outcome variable. The variable D_{it}^{t} is our placebo treatment variable which is a dummy variable that takes a value of one for countries with secondary diamond deposits in all years greater than or equal to t. All other variables are defined as before. As seen in Tables 7 and 8 the coefficients are close to zero and none are statistically significant which indicates that the treatment and control groups are following similar trends for both onset and offset prior to the introductions of the KP. Similar results are obtained when including control variables and for the longer sample. (See Tables 11 to 16 in the Appendix).[12]

We now turn to our estimates of the actual treatment effect. Table 9 presents the results using onset as our outcome variable. As seen in Table 7, for both the full sample of countries and Sub-Saharan Africa we find no evidence of an effect of the KP on the probability of onset. When using offset as the outcome variable (Table 10) we find a more robust positive treatment effect. In the full sample of countries without fixed

[11] 1991 is the first year for which data on the number UN peacekeeping troops is available.

[12] For the sample 1991-2002 we find a significant placebo treatment effect when treatment begins in the 2000 and all other placebo treatment effects are not significant

Table 8: Placebo Test 1998-2002: Offset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	-0.17199				0.09508			
	(0.22113)				(0.36252)			
D_{it}^{2001}		-0.03819				0.17810		
		(0.15642)				(0.27981)		
D_{it}^{2000}			-0.03336				0.11337	
			(0.15920)				(0.23394)	
D_{it}^{1999}				-0.13844				-0.06583
				(0.17621)				(0.26748)
N	127	127	127	127	61	61	61	61
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effects the coefficient on our interaction term of interest is positive and significant at the 10% level. Including country and year fixed effects the coefficient is no longer statistically significant (column (4)). Columns (5) to (7) present the results for Sub-Saharan Africa. In columns (5) and (6) we find a large but not statistically significant effect of the KP on the probability a conflict will end. When we allow the treatment effect to vary over time, column (7), we find that the KP had a large and positive effect on the probability a conflict would come to an end in the first three years the policy was in effect. In the following three years (2006 to 2008) the treatment effect is small in magnitude and not statistically significant. Recall that we did not find a significant effect of the KP on the incidence of conflict in Sub-Saharan Africa. Given that incidence is a combination of onset and offset this result can be explained by the fact that we find no effect on onset and a positive effect on offset.

7 Conclusion

In this paper we provide evidence that the KP reduced the incidence of conflict and that this reduction came about through a reduction in the duration of conflicts. The results provide an optimistic view of how similar policies could applied to other natural resources known to be financing conflict. For example, oil is often used by rebel groups to finance conflict. As discussed in the introduction rebel groups in Colombia are known to drill hole in oil pipe lines and sell the siphoned oil on black market (Dube and Vargas, 2013). The problem is not unique to Colombia. At one point it was estimated that terrorist ISIL produces approximately 80,000 barrels of crude oil a day (Al-Khatteeb and Gordts, 2014). Although much of this crude oil is refined and and sold to the population ISIL controls, a portion of oil makes its way to world markets (Al-Khatteeb and Gordts,

Table 9: Results From Linear Probability Model: Onset

	Full Sample			Sub-Saharan Africa		
	(1)	(2)	(3)	(5)	(6)	(7)
Sec_i	0.02916			0.01546		
	(0.02461)			(0.04420)		
KP_t	0.00888			-0.01643		
	(0.01366)			(0.02973)		
$Sec_i \times KP_t$	-0.01820	-0.03323		0.00291	-0.02879	
	(0.03180)	(0.03066)		(0.04937)	(0.05147)	
D2003			-0.04108			-0.09735
			(0.03284)			(0.06047)
D2004			-0.05586			0.00442
			(0.06825)			(0.09099)
D2005			-0.04607			0.00151
			(0.04372)			(0.06849)
D2006			-0.00515			-0.02836
			(0.05731)			(0.10316)
D2007			-0.00300			-0.01858
			(0.05907)			(0.12006)
D2008			-0.05127			-0.04133
			(0.03556)			(0.05996)
N	954	954	954	331	331	331
Fixed Effects	No	Yes	Yes	No	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Results From Linear Probability Model: Offset

	Full Sample			Sub-Saharan Africa		
	(1)	(2)	(3)	(5)	(6)	(7)
Sec_i	-0.09795			0.08190		
	(0.06931)			(0.10288)		
KP_t	-0.06056			0.04741		
	(0.06142)			(0.08259)		
$Sec_i \times KP_t$	0.20604*	0.18218		0.27201	0.28398	
	(0.10686)	(0.12405)		(0.16957)	(0.22078)	
D2003			0.09163			0.28597
			(0.22172)			(0.29656)
D2004			0.24699			0.73920**
			(0.24061)			(0.32725)
D2005			0.34620			1.19446***
			(0.23505)			(0.33096)
D2006			0.29424			-0.02928
			(0.19369)			(0.08728)
D2007			0.00324			-0.09176
			(0.17021)			(0.32904)
D2008			0.18688			0.14148
			(0.20538)			(0.49947)
N	245	245	245	109	109	109
Fixed Effects	No	Yes	Yes	No	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2014). At one point the Turkish government confiscated 450,000 litres of fuel that had been smuggled over it's border with Syria (Faucon and Albayrak, 2014). The results in this paper suggest that a similar certification scheme that provides assurance that oil being sold on world markets is not financing conflict against legitimate governments could be effective at reducing conflict.

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8 Appendix

Table 11: Placebo Test 1998-2002: Onset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	0.09432				0.19856			
	(0.06532)				(0.12476)			
D_{it}^{2001}		0.04729				0.08294		
		(0.04931)				(0.10530)		
D_{it}^{2000}			0.07955				0.12071	
			(0.06000)				(0.10808)	
D_{it}^{1999}				0.05869				0.10644
				(0.07113)				(0.11450)
polity2	-0.02030	-0.02052	-0.02100	-0.02065	-0.03080	-0.03306	-0.03447*	-0.03427*
	(0.01560)	(0.01548)	(0.01552)	(0.01553)	(0.02066)	(0.01967)	(0.01984)	(0.01980)
lgdp	-0.17778	-0.17706	-0.15662	-0.16903	-0.74999	-0.69088	-0.64498	-0.63624
	(0.17497)	(0.17582)	(0.17422)	(0.17774)	(0.62196)	(0.60051)	(0.60089)	(0.61279)
lmetals	-0.00076	0.00035	-0.00077	0.00266	-0.02221	-0.01986	-0.01706	-0.01388
	(0.02707)	(0.02651)	(0.02763)	(0.02636)	(0.03344)	(0.04018)	(0.04008)	(0.03616)
lpop	0.37885	0.39521	0.35308	0.39441	3.67793	3.38919	3.50049	3.18442
	(0.63192)	(0.63248)	(0.60450)	(0.61760)	(2.78659)	(2.72032)	(2.63474)	(2.60836)
ltroops	-0.03578	-0.03509	-0.03534	-0.03571	-0.14443***	-0.13818***	-0.13695***	-0.14388***
	(0.04218)	(0.04167)	(0.04172)	(0.04258)	(0.00902)	(0.01028)	(0.01117)	(0.00918)
N	418	418	418	418	139	139	139	139
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.1 Deriving the Weights for the Synthetic Control

For a more complete discussion of how the diagonal matrix V is derived see Appenix B in Abadie and Gardeazabal (2003). The diagonal matrix V in equation 3 is is constructed so as to minimize pretreatment differences in the outcome variable between the treatment and synthetic control groups. The elements of V are determined by the following minimization problem.

$$\underset{V \in V}{Min}(Z_1 - Z_0 W^*(V))'(Z_1 - Z_0 W^*(V)) \tag{4}$$

Where V is the set of all non-negative diagonal matrices. Z_1 is vector of pretreatment outcome variables for the treatment group and Z_1 is a matrix containing the pretreatment outcome variables for each control country.

8.2 Robustness Checks

8.3 Weights Applied to The Control Countries

Table 12: Placebo Test 1998-2002: Offset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	-0.12342				0.14263			
	(0.18634)				(0.32357)			
D_{it}^{2001}		-0.02432				0.23021		
		(0.12798)				(0.23070)		
D_{it}^{2000}			0.00266				0.23432	
			(0.14833)				(0.19781)	
D_{it}^{1999}				-0.13085				0.03940
				(0.16710)				(0.32496)
polity2	0.02443	0.02383	0.02393	0.02489	0.00922	0.01692	0.02041	0.00519
	(0.01830)	(0.01848)	(0.01838)	(0.01796)	(0.03190)	(0.03057)	(0.02571)	(0.03246)
lgdp	0.49626	0.47657	0.47388	0.36833	0.89824	1.05404	1.30155	0.91950
	(0.69756)	(0.71404)	(0.72732)	(0.66468)	(1.09991)	(1.12900)	(1.10009)	(1.28856)
lmetals	0.20931	0.20421	0.19831	0.18566	0.39679	0.37024	0.46991	0.43305
	(0.26142)	(0.25207)	(0.24983)	(0.25307)	(0.40256)	(0.34786)	(0.40342)	(0.44189)
lpop	-0.77217	-0.65087	-0.60721	-0.73304	-0.36368	-0.44678	-0.38754	-0.36009
	(1.77829)	(1.86956)	(1.84484)	(1.77912)	(2.34996)	(2.12447)	(2.10033)	(2.41093)
ltroops	0.07386*	0.07439*	0.07439*	0.07408*	0.07562*	0.07652*	0.07664**	0.07428*
	(0.03665)	(0.03792)	(0.03756)	(0.03906)	(0.04016)	(0.03652)	(0.03288)	(0.03755)
N	125	125	125	125	61	61	61	61
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Placebo Test 1991-2002: Onset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	0.07362				0.10652			
	(0.06658)				(0.10729)			
D_{it}^{2001}		0.04369				0.04847		
		(0.04330)				(0.08551)		
D_{it}^{2000}			0.06737*				0.08795	
			(0.04047)				(0.07733)	
D_{it}^{1999}				0.04574				0.07840
				(0.03389)				(0.07288)
N	986	986	986	986	341	341	341	341
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Placebo Test 1991-2002: Onset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	0.07926				0.10827			
	(0.06372)				(0.09219)			
D_{it}^{2001}		0.04772				0.03885		
		(0.04355)				(0.07306)		
D_{it}^{2000}			0.07390*				0.08453	
			(0.04214)				(0.07309)	
D_{it}^{1999}				0.05285				0.08151
				(0.03754)				(0.06886)
polity2	-0.00402	-0.00405	-0.00417	-0.00416	-0.00718	-0.00732	-0.00747*	-0.00760*
	(0.00437)	(0.00433)	(0.00428)	(0.00433)	(0.00457)	(0.00451)	(0.00438)	(0.00441)
lgdp	0.07153	0.07217	0.08009	0.07776	-0.22882	-0.22869	-0.21126	-0.20716
	(0.08285)	(0.08344)	(0.08456)	(0.08521)	(0.22111)	(0.21749)	(0.22535)	(0.22899)
lmetals	0.00210	0.00204	0.00236	0.00306	0.03329	0.03370	0.03288	0.03328
	(0.01156)	(0.01148)	(0.01170)	(0.01148)	(0.03619)	(0.03558)	(0.03706)	(0.03693)
lpop	0.27188	0.26847	0.24251	0.24965	-0.24708	-0.29158	-0.19072	-0.20534
	(0.22977)	(0.23037)	(0.22781)	(0.23177)	(1.04739)	(1.02764)	(1.00213)	(0.97471)
ltroops	-0.01449	-0.01436	-0.01481	-0.01477	-0.05627**	-0.05574**	-0.05535**	-0.05603**
	(0.01788)	(0.01783)	(0.01785)	(0.01782)	(0.02632)	(0.02599)	(0.02609)	(0.02616)
N	972	972	972	972	341	341	341	341
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Placebo Test 1991-2002: Offset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	-0.15455				0.17577			
	(0.19232)				(0.27087)			
D_{it}^{2001}		-0.05329				0.22541		
		(0.13308)				(0.20261)		
D_{it}^{2000}			-0.07218				0.19180	
			(0.11578)				(0.15830)	
D_{it}^{1999}				-0.12815				0.13826
				(0.10518)				(0.13491)
N	323	323	323	323	139	139	139	139
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Placebo Test 1991-2002: Offset

	Full Sample				Sub-Saharan Africa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{it}^{2002}	-0.09963				0.21461			
	(0.16730)				(0.26031)			
D_{it}^{2001}		-0.03136				0.28086		
		(0.11680)				(0.18941)		
D_{it}^{2000}			-0.06582				0.23733	
			(0.11193)				(0.16023)	
D_{it}^{1999}				-0.14254				0.16599
				(0.10637)				(0.15185)
polity2	0.01171	0.01158	0.01130	0.01140	0.00817	0.01004	0.01084	0.00968
	(0.01071)	(0.01080)	(0.01062)	(0.00995)	(0.00798)	(0.00780)	(0.00814)	(0.00924)
lgdp	-0.06132	-0.06319	-0.07097	-0.09005	0.12275	0.17064	0.19343	0.18276
	(0.19692)	(0.19771)	(0.19988)	(0.20225)	(0.15168)	(0.14492)	(0.14728)	(0.15464)
lmetals	0.05266	0.05300	0.05322	0.05434	-0.02463	-0.02058	-0.02482	-0.02806
	(0.08371)	(0.08360)	(0.08289)	(0.08156)	(0.07656)	(0.07605)	(0.07535)	(0.07635)
lpop	-0.23305	-0.23318	-0.24439	-0.25051	-0.55303	-0.72222	-0.66000	-0.59229
	(0.65719)	(0.66400)	(0.64997)	(0.63991)	(0.97400)	(0.95728)	(0.93369)	(0.95074)
ltroops	0.03054	0.03051	0.03073	0.03162	0.03160	0.03023	0.02969	0.02980
	(0.01919)	(0.01940)	(0.01965)	(0.02005)	(0.02599)	(0.02462)	(0.02453)	(0.02552)
N	321	321	321	321	139	139	139	139
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Treated and synthetic control countries

Treated Countries	Control Countries (Weight)
Algeria	Colombia(.032), Egypt(.001), Kuwait(.001), Philippines(.116), Sudan(.8)
Angola	Sudan(1)
Bolivia	Argentina(.019), Bulgaria(.374), Ecuador(.242), Kazakhstan(.318), Trinidad and Tobago(.047)
Brazil	Argentina(.019), Bulgaria(.374), Ecuador(.242), Kazakhstan(.318), Trinidad and Tobago(.047)
Cameroon	Kenya(.396), Libya(.3), Madagascar(.305)
Central African Republic	Madagascar(.176), Nepal(.468), Somalia(.314), Uganda(.041)
Chad	Guinea-Bissau(.168), Nepal(.082), Somalia(.08), Sudan(.265), Uganda(.405)
China	Argentina(.108), Saudi Arabia(.177), Vietnam(.715)
Cote d'Ivoire	Benin(.461), Cuba(.282), Libya(.132), Somalia(.124), Sudan(.001)
Democratic Republic of the Congo	Iran(.343), Kenya(.022), Madagascar(.162), Thailand(.011), Uganda(.279), Yugoslavia(.18)
Gabon	Botswana(.118), Kazakhstan(.378), Mauritania(.128), Qatar(.376)
Guinea	Ethiopia(.2), Madagascar(.176), Mauritania(.624)
India	Colombia(.008), Iran(.001), Philippines(.987), Sudan(.005)
Indonesia	Colombia(.202), Iran(.186), Philippines(.113), Sudan(.193), Uzbekistan(.306)
Lesotho	Cambodia(.067), Costa Rica(.112), Guinea-Bissau(.467), Mauritius(.08), Mongolia(.192), Swaziland(.082)
Liberia	Botswana(.097), Colombia(.019), Rwanda(.273), Somalia(.612)
Malaysia	
Mali	Ethiopia(.06), Kenya(.073), Madagascar(.465), Mauritania(.039), Mongolia(.306), Peru(.056)
Mozambique	Costa Rica(.077), El Salvador(.139), Honduras(.001), Kenya(.514), Madagascar(.203), Sri Lanka(.066)
Myanmar	Sudan(1)
Namibia	Botswana(.792), Djibouti(.066), Macedonia(.042), Madagascar(.045), Mauritania(.055)
Nigeria	Kenya(.338), Thailand(.662)
Paraguay	Bangladesh(.003), Costa Rica(.581), Haiti(.192), Swaziland(.224)
Republic of the Congo	Colombia(.148), Guinea-Bissau(.403), Qatar(.113), Rwanda(.337)
Russia	Iran(.034), Kazakhstan(.106), Philippines(.478), Somalia(.382)
Sierra Leone	Somalia(.734), Cambodia(.008), Egypt(.002), Guinea-Bissau(.052), Peru(.134), Senegal(.009), Sri Lanka(.003), Tajikistan(.055), Yugoslavia(.004)
South Africa	Kazakhstan(.845), Mongolia(.155)
Tanzania	Kenya(.514), Madagascar(.191), Mauritania(.053), Vietnam(.243)
Ukraine	Albania(.264), Bulgaria(.247), Ecuador(.229), Kazakhstan(.189), Mongolia(.071)
Venezuela	Argentina(.36), Ecuador(.074), Iran(.124), Kazakhstan(.412), Trinidad and Tobago(.029)
Zambia	Madagascar(.588), Mauritania(.293), Mongolia(.119)
Zimbabwe	Kenya(.427), Mauritania(.437), Singapore(.077), Vietnam(.058)

Table 18: Treated and synthetic control countries: Sub-Saharan Africa

Treated Countries	Control Countries (Weight)
Angola	Sudan(1)
Cameroon	Benin (.055), Botswana (.016), Kenya (.682), Madagascar (.043), Mauritania (.024)
Central African Republic	Burundi (.08), Madagascar (.553), Mauritania (.015), Niger (.027), Somalia (.139), Uganda (.187)
Chad	Burundi (.346), Guinea-Bissau(.19), Rwanda (.012), Somalia (.064), Sudan (.262), Uganda (.126)
Cote dIvoire	Botswana (.019), Kenya (.625), Mauritania (.231), Somalia (.024), Sudan (.102)
Democratic Republic of the Congo	Kenya (.136), Madagascar (.292), Senegal (.171)
Gabon	Benin(.363), Botswana (.357), Mauritius (.28)
Ghana	Kenya (.662), Madagascar (.251), Mauritius (.122)
Guinea	Ethiopia (.2), Madagascar (.259), Mauritania (.541)
Lesotho	Guinea-Bissau(.462), Malawi (.02), Mauritius (.001), Senegal (.077), Swaziland (.44)
Liberia	Botswana (.01), Mauritius (.036), Rwanda (.318), Somalia (.636)
Mali	Botswana (.028), Burkina Faso (.067), Ethiopia (.017), Madagascar (.518), Niger (.172), Rwanda (.017)
Mozambique	Botswana (.138), Ethiopia (.059), Kenya (.303), Madagascar (.079), Malawi (.068), Niger (.353)
Namibia	Botswana (.42), Madagascar (.121), Mauritania (.163), Mauritius (.296)
Nigeria	Botswana (.029), Kenya (.89), Madagascar (.029), Mauritania (.052)
Republic of the Congo	Burundi (.011), Guinea-Bissau (.295), Mauritius (.102), Rwanda (.431), Sudan (.16)
Sierra Leone	Guinea-Bissau (.12), Niger (.179), Rwanda (.04), Senegal (.182), Uganda (.479)
South Africa	Uganda (.125), Kenya (.746), Swaziland (.128)
Tanzania	Benin (.125), Kenya (.746), Swaziland (.128)
Zambia	Botswana (.078), Madagascar (.601), Mauritania (.321)
Zimbabwe	Kenya (.743), Swaziland (.257)