The Impact of Peace: Evidence from Nigeria

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Abstract

This paper studies the consequences of peace – or conversely, conflict – on four outcomes of fundamental economic relevance: Education, health, self-employment income and household expenditures. While the empirical literature on the consequences of conflict involving cross-country regression studies may deliver suggestive big picture evidence on links between conflict and economic outcomes, establishing causation remains problematic. By contrast, my study builds on the rather recent micro-empirical literature and proposes to use a natural experiment in Nigeria to evaluate the consequences of a reduction of conflict. The amnesty policy implemented by the Nigerian government in the Niger Delta Region in 2009 is used as a policy shock to assess the effect of a conflict reduction on the outcomes of interest. Using a constructed synthetic control region from the states that are not part of the Niger Delta region and therefore unaffected by the policy as a within-country counterfactual to the Niger Delta region, the natural experiment setting enables me to interpret the results causally and estimate the peace benefits the amnesty policy generated. I find that peace through the amnesty policy generated an increase in education by 0.53 years of schooling, a 67% increase in self-employment income and a 19% increase in household expenditures four years later.

JEL classification: D12, D74, I15, I25, J31, O12

1. Introduction

Pax Europaea – the period of relative peace in Europe since World War II – is considered one of the most fundamental drivers of economic prosperity on the continent for the last 70 years or so. By contrast, many developing countries have experienced periods of war and civil conflict since the second half of the 20th century began. Although conflict seems to have such a destructive power for many key economic characteristics of the people involved and affected, economists have only started to study it towards the very end of the 20th century. Blattman & Miguel (2010) provide an excellent review of the literature. Early leading theories on the causes of conflict distinguished between roots around antagonisms, ethnic divisions and dissatisfaction ("grievances") and those around the opportunity for rebellion or insurgency and benefits linked to that ("greed"). More support is found for the latter in cross-country regression analyses (Collier & Hoeffler, 2004; Fearon & Laitin, 2003). On the consequences of conflict, the early literature has focused on the links between conflict and economic performance or growth that can be observed across countries (Alesina & Perotti, 1996; Barro, 1991; Collier, 1999). Much more recently, authors have made use of micro-data that has increasingly become available for many countries to perform within-country micro-empirical studies of the consequences of conflict, involving a broader range of outcomes. Such studies are more suitable to find and disentangle various causal channels. I contribute to this literature with evidence from Nigeria.

After independence from the British in 1960, a civil war in the late 1960s and almost thirty years of military rule following the civil war, Nigeria has been under the rule of democratically elected governments and presidents since 1999. However, different parts of Nigeria have still experienced various degrees of conflict after the democratisation. In particular, following tensions that date back a long time, resistance activism turned violent in the south-eastern Niger Delta region in 2006 and quickly escalated to higher levels of conflict. As a response, the Nigerian government implemented the so-called 'Presidential Amnesty Program' (PAP) in 2009, in the context of which ex-militants were granted pardon, received cash and joined a 'Disarmament, Demobilisation and Reintegration' (DDR) programme in return for surrendering their weapons. The policy was effective in reducing violence in the region and establishing a period of peace. Along with the fact that other parts of Nigeria experience conflict between 2009 and 2013, this provides a unique quasi-experimental setting for estimating the benefits of peace resulting from the amnesty

policy. For this analysis, I employ the synthetic control method developed by Abadie & Gardeazabal (2003). In my context, by matching key characteristics of the Niger Delta region and states outside it for 2009 in an optimisation procedure, this method generates a synthetic control region made up of a weighted average of four Nigerian states outside the Niger Delta region that are closely comparable to the Niger Delta region. Serving as a counterfactual that can successfully reproduce pre-2009 realisations of outcome variables that I consider, this allows me to estimate the peace dividend[1] that the amnesty policy generated by simply comparing the Niger Delta region and its synthetic control region four years later.

I consider four outcomes of fundamental economic relevance: Education, health, self-employment income and household expenditures, where the latter serves as a measure for overall economic performance. While the micro-empirical literature includes some studies estimating the consequences of conflict for different measures of education, health and economic performance, there is – to my knowledge – only one such study (in Mexico) for self-employment income as an additional measure of economic activity. Considering this outcome in addition to overall economic performance, however, allows me to observe the consequences on economic activity in a richer way that will be further elaborated on in this paper. Therefore, my inclusion of self-employment income as an outcome variable makes my study a valuable contribution to the existing literature. In addition, on the methodological side, I contribute by using the synthetic control method and can specifically test for some potential pitfalls. One result is that the pre-2009 trend can be matched by the synthetic control region while it is not parallel for *all* non-Niger Delta states in general, a key assumption which the commonly used difference-in-differences (DID) method in the literature would require. While some studies employing the synthetic control method have been carried out to evaluate the consequences on economic performance, none have examined health, education or self-employment income as an outcome variable – again, to my knowledge. Therefore, in particular for these outcomes, providing an additional method gives us further confidence that emerging patterns in various studies may have some degree of general validity.

Broadly in line with the range of results found in the literature, I find that peace generates 0.53 more years of schooling on average and increases household expenditure at the

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[1]The term 'peace dividend' is used in the literature to refer either generally to benefits of newly established peace or, in a different and much more specific context, the potential gains generated by shifting government resources from military expenditure to other uses. Throughout this paper, I refer to the first notion when using the expression.

median by 19% four years later. My estimate for an increase in self-employment income at the median is 67% which is considerably larger than what the only one comparable study finds in Mexico. In spite of a positive coefficient, my estimate on health is insignificant. As a general caveat, however, it is worth noting that my standard errors and other measures imply that there is a substantial degree of uncertainty around my average effects, so that none of my findings should be taken as very precise point estimates.

Section 2 will provide an overview of the existing literature with regards to the four outcomes considered in this study, section 3 will give some more contextual background to the Niger Delta amnesty policy, section 4 informally explores potential theoretical channels involved in the consequences of conflict for health, education, self-employment income and household expenditures while section 5 describes the data I use for these variables. In the main part of this paper, section 6 explains the empirical design of my study in more detail, section 7 presents the results and section 8 delivers some relevant robustness checks. Finally, section 9 concludes.

2. Related Literature

The literature on the economic consequences of conflict is quite sizeable.[2] Early approaches, but also some more recent papers, focused on the effects of conflict on overall economic performance or GDP and employed cross-country regression methods (Alesina & Perotti, 1996; Barro, 1991; Collier, 1999; Gaibulloev & Sandler, 2009; Gates et al., 2012; Murdoch & Sandler, 2002; Stewart et al., 2001). These studies typically show that conflict and GDP or GDP growth are negatively correlated. The magnitude of the correlation varies and depends on what the precise link, sample and magnitude is that the authors consider; for example, both Collier (1999) and Gaibulloev & Sandler (2009) find that GDP per capita *growth* is around 2 percentage points lower with conflict[3] while Gates et al. (2012) provide an overall estimate and find that a median-sized conflict is associated with a reduction of GDP per capita by 15%. Whereas these studies provide some evidence for the (unsurprisingly) negative association between conflict and may deliver some indication of how costly conflict can be, they suffer from obvious endogeneity problems which make it hard to interpret any of the estimates presented as the causal effect running from

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[2]In this review, I will focus on the empirical literature that is related to estimating the consequences of conflict on the four outcomes that I am estimating in this study: health, education, self-employment activity and household expenditures. Of course, this is not a comprehensive overview of the vast theoretical and empirical literature on the consequences of conflict in general.

[3]Similarly, in a summary paper, Bozzoli et al. (2010) claim that mass violent conflict is associated with a reduction of GDP growth lying between 1 and 3 percentage points.

conflict into economic performance. Even when some institutional and socio-economic control variables or fixed effects in panel regressions are included, it is quite likely that there are remaining time-varying confounders at the country level and that conflict is non-randomly placed.

One way to deal with this problem is to estimate the direct and indirect costs of conflict within a country specifically instead of relying on reduced form cross-country comparisons. Ali (2013) and Arunatilake et al. (2001) are two studies of this sort that evaluate the costs of conflict in Sudan and Sri Lanka, respectively. They estimate military expenditures as direct cost and consider roughly the same channels of indirect cost (lost income from foregone investment, reduced tourism, reduced labour and human capital, displacement, destroyed infrastructure, etc.) to come up with a measure of the total *annual* cost in terms of GDP. In the case of Sudan, this number amounts to 13% while it is 8-9% in the case of Sri Lanka according to these estimates. The advantage of this method is indeed to circumvent endogeneity problems and have a direct estimate of involved channels. However, these measures are probably quite inaccurate for two reasons. Firstly, in particular the indirect cost are probably measured with a non-negligible amount of measurement error. Secondly, it is close to impossible to comprehensively cover all the indirect channels and interactions between them involved.[4]

The more prominent approach to address endogeneity concerns has been the use of within-country variations in recently increasingly available micro-level data. Lopez & Wodon (2005) use methods of time series analysis to argue that the cost of the Rwandan genocide range between 25% and 30% of GDP. Other studies use measures of expenditures or income on the household level to capture overall economic performance. Also in Rwanda, Serneels & Verpoorten (2015) use variation in conflict intensity in an instrumental variable (IV) approach and also find large impacts on consumption expenditure: A one standard deviation increase in conflict intensity (measured as war excess mortality) decreases household consumption by 19% six years later. By contrast, Bellows & Miguel (2006) do not find a significant relationship between conflict and per capita expenditure in Sierra Leone a couple of years after the war. In the very long run (around 30 years later), Saing et al. (2017) (DID approach) and Miguel & Roland (2011) (IV approach) also fail to find significant effects of US bombing on economic performance measures in Cambodia and Vietnam, respectively. However, further studies employing the DID

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[4]Such an estimate would require knowledge of fiscal multipliers, production functions, different elasticities, etc.

(Akbulut-Yuksel, 2014; Galdo, 2013; Islam et al., 2016) or the synthetic control method (Abadie & Gardeazabal, 2003; Dorsett, 2013; Gong & Rao, 2016; Matta et al., 2016) find significant effects of conflict on economic performance.[5] The former authors use earnings as a measure and find effects ranging between -6% and -9% or a marginal reduction by 5% for a one standard deviation increase in conflict between 20 and 40 years later. The latter authors find substantial effects in the first couple of years following the onset of conflict ranging between -5% and -20% per year.[6]

With regards to the effects of conflict on education, on the other hand, a considerable literature of micro-empirical evidence has emerged recently that seems more conclusive. The majority of these studies employ essentially a DID approach using variation in conflict at the school age (or time) and geographic level within a country (Akbulut-Yuksel, 2014; Akresh & De Walque, 2011; Chamarbagwala & Morán, 2011; Islam et al., 2016; Kesternich et al., 2014; Leon, 2012; Márquez-Padilla et al., 2015; Merrouche, 2011; Pivovarova & Swee, 2015; Saing et al., 2017; Shemyakina, 2011; Swee, 2015; Valente, 2014). Some of these studies control for linear time trends or flexible time trends at a more aggregate geographical level to deal with potential endogeneity arising due to non-parallel trends. There are some more studies using different methods (Alderman et al., 2006; Blattman & Annan, 2010; Brown & Velásquez, 2017; Rodriguez & Sanchez, 2012), among which Blattman & Annan (2010) can make use of a quasi-experimental setting with plausibly random 'recruitment' abductions by rebels in Uganda. Ichino & Winter-Ebmer (2004) investigate the consequences of World War II for education in Austria and Germany by comparing the trend over time to Switzerland and Sweden as two unaffected countries. In spite of the great variety of countries[7] and contexts investigated in these studies, the results are reasonably similar and large. Those authors who estimate life-time accumulation effects find results ranging between a reduction by 0.2 and 1 year of schooling. To put this in context, it may be helpful to consider the study Duflo (2001) has carried out in Indonesia. She finds that each school built per 1000 children in the context of a massive

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[5]I will describe and discuss the synthetic control method in more detail in section 6.1.

[6]Indeed, the evidence on whether there is convergence in line with the logic of the Solow model or not in the very long run (at least 20 years after the end of conflict) is not conclusive. While some estimates suggest there is convergence, other studies find evidence for the opposite. In particular, some direct evidence on GDP (Chen et al., 2008; Gates et al., 2012; Miguel & Roland, 2011) and on city size (Brakman et al., 2004; Davis & Weinstein, 2002) suggest convergence. By contrast, other direct evidence on GDP (Akbulut-Yuksel, 2014; Galdo, 2013; Islam et al., 2016) and a range of studies on education and health outcomes (Akresh et al., 2012a; Alderman et al., 2006; Ichino & Winter-Ebmer, 2004; Kesternich et al., 2014; Merrouche, 2011, details see below) that should affect economic performance according to standard theory suggests that there is no convergence.

[7]The countries subject to analysis in these papers are Austria, Bosnia and Herzegovina, Cambodia, Colombia, Germany, Guatemala, Mexico, Nepal, Peru, Rwanda, Tajikistan and Uganda.

school construction programme leads to an increase by 0.12 to 0.19 years of schooling. Márquez-Padilla et al. (2015) consider enrolment as an outcome variables and are almost the only ones who do not find an effect in the context of a drug war in Mexico. The only other study not finding a significant effect is by Pivovarova & Swee (2015) who investigate the civil war in Nepal. However, Valente (2014) who analyses the same civil war even finds positive significant effects and gives some historical context explaining why it is not clear in which direction the effect would go.[8] Leon (2012) also estimates short-run effects and finds a reduction by almost one year of schooling (relative to a long-run effect of 0.31 fewer years of schooling) suggesting that there is a catch-up effect in the long-run. Shemyakina (2011) and Swee (2015) do not use years of schooling as an outcome variable, but the likelihood of finishing mandatory schooling and secondary school enrolment respectively. In line with the other research, the results are significantly negative.

In terms of the methodology, most studies analysing health effects are equivalent. Based on the idea that health shocks to children in utero or in early childhood have long-term consequences,[9] they use variation in birth cohorts during conflict and location of conflict for a DID approach (Akbulut-Yuksel, 2014; Akresh et al., 2012a,b, 2011; Bundervoet et al., 2009; Galdo, 2013; Islam et al., 2016; Kesternich et al., 2014; Minoiu & Shemyakina, 2014; Saing et al., 2017). In alternative approaches, Alderman et al. (2006), Camacho (2008) and Mansour & Rees (2012) manage to isolate plausibly exogenous variation by including mother fixed effects in their regression approaches, essentially comparing siblings that were exposed to war to different extents. The studies are not entirely consistent in the outcome variable chosen which makes them hard to compare. (Birth) weight, height-for-age scores, height, (infant) mortality and diseases show up as different outcome variables. However, almost all studies find significantly negative impacts on health.[10] The majority of studies considers the contemporaneous effect on children in utero or early childhood and all those studies find an effect which, however, is sometimes hard to interpret in terms of its severity. To this end, Camacho (2008) provides a handy back-of-the-envelope calculation to make her finding that conflict caused a 8.7 gram reduction in weight (which may seem a small effect) more meaningful. The effect translates into an expected number

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[8]The argument is basically that the conflict is minor and insurgents actually promoted education, in particular for girls, by reporting teacher absenteeism and explicitly opposing caste- and ethnicity-biased traditions as well as gender inequality in the access to it.

[9]This idea goes back to Barker (1998) who posited the fetal origins hypothesis. More recently, economists have found evidence for considerable long-run effects of various shocks while in utero or as an infant (see Almond, 2006; Almond & Currie, 2011a,b; Maccini & Yang, 2009, for some of this evidence and summaries).

[10]The authors choosing height-for-age z scores are indeed comparable. The effects found range between a reduction by 0.42 standard deviations to 1 standard deviation from exposure to conflict.

of 360 lives of newborns and infants saved in a non-conflict relative to a conflict area. Only two studies (Islam et al., 2016; Saing et al., 2017) considering the long-term consequences, in their context whether exposure to conflict as a child in utero or early childhood affects height as an adult, do not find a significant effect. Nevertheless, there is a greater number of studies finding an effect even in adulthood.

Taking all the findings on health and education together, they do seem to paint a fairly clear picture of the direction and in particular similar magnitude in various contexts. However, in light of the fact that the DID approach is predominantly used, it is not clear whether many of these studies suffer from systematic bias arising because of a potential violation of the parallel trend and Stable Unit Treatment Value Assumption (SUTVA). The parallel trend assumption would be violated if control cohorts in conflict areas (for education, the youngest cohort beyond school age is typically considered; for health, children above the age of five are considered in most specifications) are affected by conflict as well, i.e. the time trend between (older) control cohorts and (younger) treatment cohorts is not the same in conflict and non-conflict areas. In particular for health, it is not unreasonable to think that war exposure also affects older children. In fact, Domingues & Barre (2013) provides some evidence that this is the case when analysing older cohorts of children as well. The SUTVA would be violated if conflict has spillover effects in non-conflict areas. Again, this is not necessarily unlikely, in particular when very disaggregate measures of conflict exposure, for example at the district level, are considered. Migration as a response to conflict would be a typical driver of a SUTVA violation. Indeed, some studies consider linear trends or perform robustness checks, e.g. placebo tests and explicit considerations of migration, but this is not generally the case and few can convincingly alleviate all concerns.[11] By contrast, the synthetic control procedure in this study assesses the overall effect on education and health by using pre- and post-peace data for matching, not different birth cohorts within the same time period. The fact that I use the variation in actual time periods covering both conflict and peace enables me to much more clearly test the pre-trend on years before the peace-generating amnesty policy whereby the parallel trend cannot mechanically be violated if different cohorts are affected since I am not using cohort variation. Similarly to the existing literature, I address a potential SUTVA violation in a robustness check. Therefore, I am the first to

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[11]This is mainly due to the fact that it is very hard to address these concerns in the DID setting using birth cohort variation. For example, it is not clear whether time developments over different cohorts are linear, in particular if conflict affects control cohorts. In addition, if conflict affects different control cohorts in a similar way, placebo tests to this end will fail to show an effect.

use – to my knowledge – the synthetic control method in the context of the consequences of conflict for health and education which I consider a valuable and relevant contribution to the existing literature.

Regarding the effects on employment activity or income, the DID method does not seem to hold the methodological monopoly. This is not necessarily surprising since health effects on newborns and infants as well as education effects provide by definition a cohort variation that can be exploited while any self-employment outcomes affect a broader range of people. Therefore, there are two ways in which DID approaches could be and have been applied which both require more comprehensive data. Firstly, the cohort effect on earnings much later in life can be estimated which naturally requires data during the time of the conflict and decades later. This type of analysis captures effects via cohort exposure (e.g. health shocks in utero or early childhood or less education), but no total conflict effects, and has been carried out by a few authors in different contexts (Akbulut-Yuksel, 2014; Galdo, 2013; Islam et al., 2016; Nillesen, 2016; Saing et al., 2017). Secondly, and equivalently to my approach, an overall effect of the war on exposed people can be estimated, even only a few years later, when pre- and post-conflict data is available.[12] This approach has recently been adopted by two researchers in Burundi and the Basque country (Colino, 2013; Nillesen, 2016, respectively). Other studies exploit panel data and control for individual heterogeneity (Deininger, 2003; Velásquez, 2015) and/or IV approaches (Bozzoli et al., 2012; Camacho & Rodriguez, 2013; Collier & Duponchel, 2013; Kondylis, 2010). Blattman & Annan (2010) make use of a quasi-experimental setting and Guidolin & La Ferrara (2007) employ the synthetic control method. Apart from methodological differences, the studies also vary greatly in the outcomes considered. Few actually consider self-employment activity. Bozzoli et al. (2012) finds that an increase by 4 attacks (the interquartile range) decreased the rate of self-employment by 6.2 percentage points (on a base of 41%) in Colombia while Deininger (2003) estimates the reduction in the propensity to start a business through exposure to conflict to be almost 10% in Uganda. Velásquez (2015) finds a reduction in self-employment earnings for men by 15% in Mexico. Other outcomes considered include labour supply, coffee farming, firm size and exit, investment as well as total earnings (discussed above). The effects founds are in almost all cases significantly negative and substantial. Guidolin & La Ferrara (2007) are

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[12]Note that this may be a non-trivial requirement for longer conflicts. While the cohort variation in the context of a DID approach only requires data in one post-conflict period – and the massive recent increase in micro-level data has guaranteed availability for many countries – and data on geographical conflict intensity, pre-conflict data on outcome variables does not exist in many contexts.

the only exception who actually find positive effects of conflict on an Angolian portfolio holding diamond assets.[13] This paper shows that for specific industries, conflict may have positive income effects.

In the context of self-employment, my paper contributes to the literature in two ways. Firstly, it deploys the synthetic control method that has only been used by Guidolin & La Ferrara (2007) for specific employment activity outcomes so far.[14] Secondly, I use self-employment income as an outcome variable which sheds light on a slightly different aspect of employment activity than a measure indicating whether someone is engaged in a particular business or not. Average self-employment income captures both the extent to which more business owners induce more competition, crowd out low-productivity competitors and engender higher average income in more successful businesses and the extent to which already existing business owners put more time and effort into their business.

3. Context

The Niger Delta region is an oil rich region in the south eastern part of Nigeria. Since oil revenues account for a large part of the government's fiscal budget, it has traditionally been a very important region for policymakers (Abazie-Humphrey, 2014; Obi, 2014). As Obi (2014) explains, the government has taken over federal control over oil in the 1960s and since then sharply decreased revenue derivation, that is, the share flowing into regional budgets. This has created tensions in the Niger Delta region and resistance activists have become a key actor since the late 1990s. In particular, the 'Movement for the Emancipation of the Niger Delta' (MEND) has emerged and became violent from 2006, marking "an escalation from uncoordinated protests and conflicts into a trans-Delta insurgency." Their violent activities were characterised by oil worker abductions, attacks of government forces and oil installation sabotages which led to a conflict involving a rapid decrease in oil production and increase in fatalities.[15]

By the time Umaru Yar'Adua got elected as president in April 2007, the Niger Delta conflict has become a pressing issue for the government to solve due to its dependency on

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[13]In fact, they conversely estimate the effect of peace on the portfolio in an event study design and find that the portfolio value drops as the rebel leader dies suddenly.

[14]To my knowledge and excluding the few studies on total earnings discussed above and for which my measure of expenditures is the more appropriate equivalent.

[15]In fact, as can be seen below, the number of reported fatalities drastically escalates only in 2009. From 2006 to 2008, therefore, the conflict seems to have manifested itself rather in great uncertainty, pipeline explosions, and threat of terror.

oil revenues. He announced an amnesty programme on 25 June 2009 and established the 'Niger Delta Peace and Conflict Resolution Committee' (NDPCRC) in July. The amnesty programme targeted ex-militants and granted "unconditional pardon to all persons who have directly participated in the commission of offences associated with militant activities in the Niger Delta" in return for surrendering their weapons, renouncing the use of violence and taking part in a government-sponsored 'Disarmament, Demobilisation and Reintegration' (DDR) programme. In fact, the DDR programme was a key and hugely expensive element of the programme.[16] Ex-militants participating in the programme received a monthly stipend of 65,000 NGN (approx. 440 USD in 2009), support and vocational training to facilitate their reintegration into society. The amnesty programme took effect on 6 August 2009 (The Guardian, 2009), from which point militants had a period of 60 days to sign up. 20,192 rebels signed up during this period; however, further militants were still approved after the deadline which accumulated the total number of participants to 30,000 after all (Oluduro & Oluduro, 2012; Premium Times, 2012).

The statistics on post-2009 fatalities show and analysts generally agree that the programme was effective in establishing peace in the region, at least for a couple of years (Abazie-Humphrey, 2014; Obi, 2014; Oluduro & Oluduro, 2012; Oluwaniyi, 2011).[17] Abazie-Humphrey (2014) argues that peace in the region served as a condition for development and growth through a more stable business environment and the completion of infrastructure project. In a very simple analysis comparing mean outcomes for the artisan fishing industry, Achoja et al. (2013) find some suggestive evidence of an improved business situation. Therefore, it seems worthwhile to exploit the amnesty programme as a policy shock generating peace to investigate its effect on a range of outcomes.

4. Theoretical Considerations

For each of the outcomes I consider, there are multiple channels how peace may affect them. Since the direction of the effect is usually clear, I will resort to a brief verbal discussion of the possible mechanisms involved without a formal model. After all, the empirical

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[16]According to Abazie-Humphrey (2014) who refers to a press conference with Kingsley Kuku, the chairman of the amnesty programme, annual budgets for the programme up until 2014 amounted to more than 1.8 billion US dollar.

[17]These authors are, however, sceptical as to whether the policy is able to generate lasting peace. They generally criticise it for not addressing fundamental causes of the conflict in spite of the government's rhetoric viewing the policy as a pathway to sustainable peace. Indeed, some increases in conflict can be observed in the region from 2015, but in light of the Boko Haram insurgency affecting the whole country, it is difficult to causally link this back to a failed amnesty policy. In any case, my analysis focuses on outcomes in 2013 and the data shows a period of peace between 2009 and 2013.

exercise is mainly about the quantification of these effects. In addition, I will focus on a discussion of the converse effect, namely the channels how conflict affects my outcomes of interest, in order to stay in line with the vast majority of the literature regarding and estimating the issue from this perspective. The effect of peace then obviously just operates in the opposite direction.

As for GDP or general economic performance, Collier (1999) provides an overview of potential channels in his seminal paper. Conflict leads to the destruction of resources, physical and human capital as well as infrastructure, disruption of order, diversion of public expenditure from output-enhancing activities, dissaving, increased uncertainty and decreased foreign investment. From this macro perspective, such effects should all lead to a reduction in economic performance. Conversely, peace as a reversion of these effects should lead to increased economic performance, especially in the medium and long run. However, related to the idea of a rapacity effect as a cause of conflict (Dube & Vargas, 2013), if there is conflict over a valuable resource (which is the case with oil in the Niger Delta), rebels benefit from resource theft and revenues. Therefore, for those households involved in conflict, peace may mean a drop in their economic performance, at least in the short run.[18] Naturally, we may expect the reversion of the above effects as peace unfolds to operate on a larger scale, be relevant for all households and therefore dominate potential immediate economic losses from resource theft in the medium and long run.

The effect on self-employment activity or income is partly related to general economic performance effects. The obvious link is that any changes in households' income translate into demand changes. A demand reduction translates into contraction of businesses providing the goods demanded. Other than through this link, conflict also directly affects self-employment. Destruction of infrastructure and increased uncertainty mean higher cost of production.[19] In the specific case of conflict over a natural resource, engaging in militant activity and resource theft also becomes an alternative supply of labour and some households may shift some labour into this activity which also reduces labour supplied to self-employment activity. Taken together, a contraction of self-employment businesses and higher cost of operation should lead to a reduction in self-employment income. Con-

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[18]This is reflected in the findings by Guidolin & La Ferrara (2007).

[19]Seiermann (2012) and Ksoll et al. (2016) provide specific examples. The former explores the infrastructure channel explicitly in Peru. She argues that destroyed infrastructure, in particular roads, leads to higher cost of market access for self-employed people that need to get to the next market to sell their products. The latter consider flower exporters in Kenya during times of electoral violence. They find that firms had to face increased labour absenteeism and pay higher wages as a compensation for coming to work in times of conflict. Although they consider (larger) firms, the mechanism is clearly equivalent for self-employed businesses which employ labour.

versely, peace operates in the opposite direction of these effects and should lead to an increase in self-employment income.[20]

With regards to education, three channels are usually discussed in the literature. Firstly, demand side factors may reduce the amount of schooling obtained during conflict. If conflict reduces financial resources,[21] an income effect would lead to a reduction in (the consumption of) education since schooling is costly. Furthermore, especially in the context of developing countries and poor communities, if the reduction in financial resources pushes households below or close to the subsistence level, a substitution between education and income-generating work may kick in. Apart from this chosen education-labour substitution, involuntary education-labour substitution also occurs in incidences of child soldiering through forced recruiting or military draft. Secondly, supply side factors may reduce schooling. This includes the destruction of skills or conflict-related deaths of teachers. Thirdly, a general situation of increased uncertainty and insecurity means that it is more dangerous to leave the house to attend public institutions.[22] When peace starts, the reversion of all these effects should lead to an increase in education.

Apart from the mechanical effect of conflict-related deaths, there are several ways in which conflict typically impacts on health. Conflict may have a direct effect on food security through the destruction of livestock and burning of crops which clearly affects the health of those suffering from food insecurity. Conflict also leads to displacement which in particular increases children's exposure to and risk of water and vector-borne diseases (Verwimp et al., 2010). In addition, the destruction of health infrastructure in the course of conflict and lower financial resources reduce access to healthcare and the demand (ability to pay) for it, respectively. These channels all lead to worse health conditions during conflict or, conversely, improve health at peace. It is also noteworthy that a change in health conditions in turn translates into respective changes in work and education. In particular, better health conditions would lead to increased self-employment activity and education.[23]

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[20]Note, in particular, that self-employment activity may increase both on the intensive and extensive margin as a response to peace which both lead to higher self-employment income. Clearly, business owners who invest more time and effort into their existing businesses generate more income in so doing. On the extensive margin, more businesses being set up result in more competition and crowding-out of the least productive businesses which would also result in higher income on average.

[21]Although this is potentially not clear in the short run if there is massive involvement of households in resource theft as discussed above, this should at least be what we observe in the medium and longer run.

[22]Strictly speaking, this is a demand side factor as well but the focus here is on the substitution effect towards other goods arising from an increase in the cost of education. By contrast, the first channel focuses on the mere income effect from a reduction in financial resources. Therefore, authors like Shemyakina (2011) and others usually state this channel as a separate point.

[23]Miguel & Kremer (2004) show how impressive the effect of health on education can be in itself.

5. Data

5.1. Data Sources

I am using three different sources of data for this study. The first is the Nigerian General Household Survey (GHS) with socio-economic data on Nigerian households. I am using survey rounds 2006, 2007, 2008 and 2012 (National Bureau of Statistics Nigeria, 2012). These surveys provide measures for three of the four outcomes considered: education, self-employment income and household expenditures. The health measure that I will consider is indeed recorded in the GHS as well; however, sample sizes for the measure at the state level are unfortunately very small for most states so that I cannot assume representative outcomes. The data collection of GHS survey round in year y is actually carried out early in year $y + 1$, more precisely, between March and April of the following year. Therefore, the GHS round 2008 with data on early 2009 is used for the matching procedure to create the synthetic control region. The GHS rounds 2006 and 2007 serve to verify the validity of the synthetic control region and assess the common trend assumption in the context of a DID procedure carried out as a robustness check. Earlier rounds are not available. The GHS round 2012 provides data on outcome variables.

The second source is the Demographic & Health Survey (DHS) for Nigeria in rounds five and six covering 2008 and 2013, respectively (DHS Program, 2013). Since the DHS is only carried out very irregularly in Nigeria, these are the only two rounds of use for me. The DHS round five (2008) provides the health measure for the matching procedure and the DHS round six (2013) provides the health measure as outcome variable.

For measures of conflict, I am making use of data provided by the Armed Conflict Location & Event Data Project (ACLED, 2015). This is a very rich data source covering all conflict events with precise location data in many developing states from 1997. In particular, I am using the data on Nigeria from 2006 to 2009 for the matching procedure and verification of its validity as well as the common trend assumption for the DID approach. In addition, I am using data for the years between 2010 and early 2013 to assess the first stage of my investigation, i.e. whether the matching indeed produces a synthetic control that displays sustained post-2009 high levels of conflict relative to the Niger Delta region in which conflict reduces.

There are two main reasons why I consider the 2013 surveys for outcome variables. The first is practical in nature. DHS round six is the earliest DHS survey provided after the

policy change in 2009 which is why the earliest post-2009 health measure I consider is only available then. In order to uphold time consistency with other measures, I also consider the other outcomes in 2013. Secondly, the amnesty policy established a period of lasting peace in the Niger Delta region for at least a couple of years[24] and peace is likely to have medium- and long-run consequences. Since these consequences are more interesting to consider from a welfare perspective than short-term changes and the amnesty programme took at least a year to show seriously low levels of violent conflict, four years seem to capture at least medium-run consequences better than earlier outcomes.

5.2. Relevant Measures

As a measure for conflict, I consider fatalities in a conflict event. In particular, for the matching procedure and as an outcome variable, I use the sum of fatalities in violent events in a particular state and year as a measure of the degree of conflict. Following the definition of ACLED regarding political violence, I include the following event types: Battles ("Battle – No change of territory", "Battle – Non-state actor overtakes territory", "Battle – Government regains territory"), "Remote violence" and "Violence at civilians". The predominant reason for the inclusion of these events is that they involve violence and variation in the number of fatalities. However, even if other event types are included that are typically associated with no or very few fatalities (e.g. "Strategic development"), the results do not change dramatically. The main reason for using the sum of fatalities as opposed to other measures of conflict is that it captures both the *extent* and *intensity* of conflict. As a demonstrating example, consider the following two scenarios of conflict a state could undergo: Very few events involving enormous amounts of fatalities vs. an extremely large amount of events with relatively few fatalities each. In my simple categorisation, the former is rare, but quite intense conflict while the latter means extensive, but low-intensity conflict. It is unclear whether one scenario represents more conflict than the other or not. Taking the average number of fatalities in an event would, for example, capture only the intensity of conflict, but disregard the extent and give the first scenario a much higher score. Taking the number of incidents as a measure would, on the other hand, capture the extent, but disregard the intensity and give the second scenario a much higher conflict rating. Therefore, it seems reasonable to use the sum of fatalities as a

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[24]Conflict increased again to some extent in 2015/16 when the amnesty programme ended and Boko Haram became violently active. The Niger Delta region has not been as badly affected by Boko Haram attacks as regions in the North East of Nigeria. However, a few attacks have been carried out there as well.

measure in order to capture both elements.

As an education measure, I am employing a standard measure: Years of schooling. This is observed in great sample size at the state level. For health, I am taking life expectancy (at birth) as a measure.[25] The variable I am using to measure this is mean death age of household members in the 12 months preceding the survey interview. This is probably only imperfectly capturing life expectancy in its literal sense (the amount of years a newborn can expect to live) for two reasons. Firstly, in order to preclude a mechanical change in life expectancy due to less fatalities from reduced conflict and capture the route via the impact of conflict on general health conditions in a state, I do not take sudden deaths into account, such as dying in a conflict incident. I do this by only allowing for deaths that showed some signs of previous illness. In particular, it must be indicated that the deceased was sick at least 3 of the 12 months prior to death or had severe pain or experienced nausea in the preceding month.[26] Secondly, since households were interviewed in the surveys, deaths of long-time ill patients in hospitals or residents of homes for elderly who do not have an immediate connection to a household cannot be captured. However, for both sources of inaccuracy, it is reasonable to assume that they affect both the Niger Delta and non-Niger Delta region in the same way. The first inaccuracy is precisely designed to prevent a mechanical bias in my results (with the interpretation of conflict impacting on general health conditions). For the second one to generate a bias, it would need to evolve systematically differently over time in the two regions which seems quite implausible.

In order to capture overall economic capacity or welfare, I use a measure of household expenditures. Although this does not capture savings, I prefer this measure over household income for several reasons. Firstly, household income is imprecisely measured. For the GHS 2008 survey, the monthly income is only measured in 1000s of NGN (1000 NGN were approximately 6.80 USD in 2009) which – although this seems to be a fairly small inaccuracy in absolute terms – may have non-negligible implications given the fact that 25% of the population report to have an income below 6000 NGN. For the 2012 survey, the income question is on the last payment and not for a specific time period. In light

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[25]I am deviating from the most commonly used measures in the literature evaluating the impact of conflict on health, namely height or height-for-age z scores of children. The predominant reason for using my measure is that it is quite unclear how to assess the meaningfulness of effects on the height measures while it is straightforward to interpret changes in life expectancy. However, as a robustness check, I have also checked whether my results hold up when using height or height-for-age z scores from the DHS 2009 and 2013 survey as a measure for health. Indeed, for both measures, I do not find a significant effect either.

[26]These three indicator variables are available in the DHS rounds to capture the notion of a preceding process of illness.

of the high irregularity of income flows that is common for many people in developing countries, it is unclear how informative and accurate such a measure is. Secondly, key quantiles in the distribution of monthly income and expenditures in the GHS 2008 survey used for the matching procedure are very close. It doesn't seem to be the case that large amounts of constant saving disguise the true welfare of household. Thirdly, and relatedly, household expenditures also capture the ability of households to smooth out consumption. While income measures for the last month may be due to high degrees of fluctuation, expenditures are much more smooth and provide a clearer picture of the household's economic welfare situation. Indeed, the distribution of expenditures for the GHS 2008 survey looks considerably more smooth than the one for income.

As another measure of economic activity that may differently be affected by conflict than overall economic capacity or welfare, I consider self-employment income.[27] In particular in the developing country context, it seems reasonable to consider self-employment as an economic activity which includes many small and micro-businesses that a substantial share of households run relative to formal waged employment. Regarding *income* as a measure as opposed to a mere measure of whether household members own a business or not, this captures the extent of self-employment activity arguably better. It obviously captures any additional time and effort going into one's business as well as success through a more conducive environment. In addition, however, it also captures an increase in the number of businesses indirectly since this would translate into more competition, crowding-out of low-productivity businesses, and therefore higher income on average.

In order to capture the average in a meaningful way for both household expenditures and self-employment income, I am using the median as opposed to the mean for standard reasons. The mean is subject to high volatility for such measures coming from changes in the extreme top part of the distribution. This is a potentially particularly severe issue in my context where I only have data on monthly expenditures and self-employment income in the month preceding the survey interview.[28]

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[27]See section 4 for a small exploration of the different mechanisms at play.

[28]In fact, I verified that – if cutting the extreme 1% of the data to eliminate some degree of volatility – using the mean measure does not change the results dramatically.

6. Empirical Design

6.1. The Synthetic Control Method

In order to get a valid counterfactual region that the Niger Delta region can be compared to after the policy implementation, I employ the same method as Abadie & Gardeazabal (2003) for their investigation of the economic cost of conflict in the Basque region in Spain in the late 1960s. I am constructing a synthetic control region from the states that are outside the Niger Delta region and thus not affected. 28 states are outside the Niger Delta region (denote this number $N_{NND} = 28$) and remained unaffected by the policy. The idea is to use a weighted average of these 28 states that is comparable to the population weighted average of the nine Niger Delta states prior to the policy implementation. Since I am investigating the impact of the policy on education, health, household expenditures and self-employment activity, it seems appropriate to make the synthetic control region comparable to the Niger Delta region in these characteristics before the policy change. In addition, I am taking a measure of conflict before 2009 into account which is what the policy addresses. Therefore, the appropriate weights for the 28 states outside the Niger Delta region that construct the synthetic control regions arise as the solution to the following optimisation problem:

$$\min_{w} \quad (Xv - Yw)'(Xv - Yw)$$
$$\text{s. t.} \quad w_j \geq 0 \quad \forall j,$$
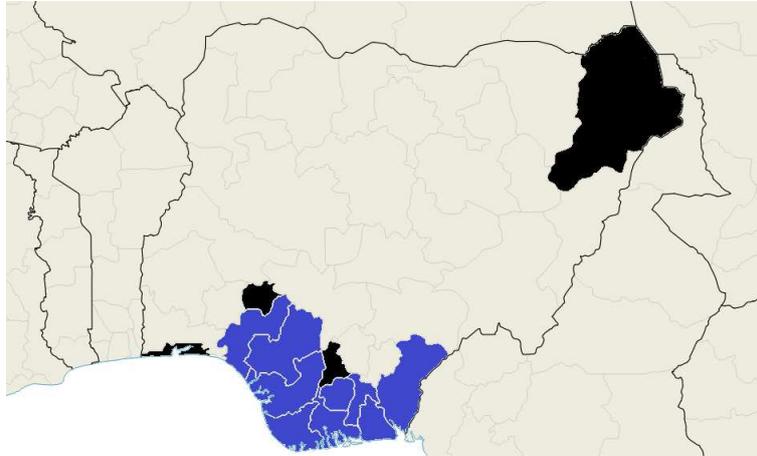$$\sum_{j=1}^{28} w_j = 1$$

where X is a (5×9) matrix of the early 2009 measures[29] of conflict, education, health, self-employment income and household expenditures in the nine Niger Delta states (denote this number $N_{ND} = 9$), Y is a (5×28) matrix of the same measures in the 28 non-Niger Delta states, v is a (9×1) vector of the population weights for the Niger Delta states and the minimand (28×1) vector w captures the weights on the non-Niger Delta states that are to be optimised.

The resulting optimal vector w^* has non-zero weights for only four of the 28 non-Niger Delta states (denote the number of states included in the synthetic control region

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[29]In order to guarantee comparability in the scale of the different measures, I am using constructed z scores of each measure by standardising on the 28 non-Niger Delta control states. However, I am reporting the non-standardised values for ease of interpretation in the following.

$N_{SC} = 4$): Anambra (24%), Borno (21%), Lagos (23%) and Ogun (32%). The Niger Delta and synthetic control states are displayed in Figure 1.[30]

Figure 1: Niger Delta and Synthetic Control Region

Note: The Niger Delta states are displayed in blue while the synthetic control states are displayed in black.

The synthetic control region overcomes the substantial gap between the population weighted average of Niger Delta states and a mere population weighted average of states outside the Niger Delta region before the policy change. A simple comparison between these two regions would be subject to selection bias and lead to massive overestimation of the effects since the non-Niger Delta states perform systematically worse than the Niger Delta states in all relevant characteristics. However, the synthetic control constructed using the weights w^* is comparable to the population weighted average of Niger Delta states. In addition, providing some confidence that w^* actually provides a valid counterfactual region, it can map the outcome variables in the Niger Delta region in years prior to 2009, for which data is available and that are *not* included in the matching procedure, reasonably well. Although data on conflict is available going back until 1997, violent activities in the Niger Delta region have only started in 2006 which is why the trend from 2006 is considered. For the education, expenditures and self-employment income previous GHS survey rounds with data on 2007 and 2008 can be used. Unfortunately, these rounds have too few observations on deaths in 2007 and 2008 to have meaningful representative statistics. Table 1 and Figures 2 to 5 display the available results.

What is particularly striking about the synthetic control region is that it does not fail to match the sharp upward conflict trend from 2006 culminating in high conflict levels in
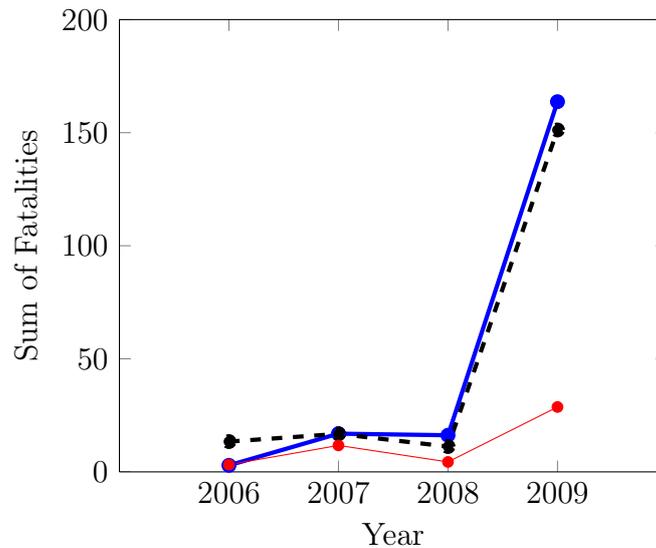
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[30]The shares are rounded to the nearest integer. All other states have a weight of zero rounded to the nearest integer. It is not unusual that only a minority of potential control states has a non-zero weight. Abadie & Gardeazabal (2003), Gong & Rao (2016) and Matta et al. (2016) similarly get that only fewer than five control states or countries out of a considerably larger set have non-zero weights. Three out of four states making up the synthetic control region are indeed spatially close to the Niger Delta region. Section 8.2 deals with potential spillover concerns.

Table 1: Matching Results

Variable	Niger Delta (ND)	non-ND (NND) (p-value ND-NND)	Synth. Control (SC) (p-value ND-SC)
Matched Variables			
Fatalities 2009	163.7	28.7 (n/a)	151.2 (n/a)
Yrs. of Schl. 2009	7.60	6.85 (0.00)	7.62 (0.76)
Death Age 2009	42.92	29.34 (0.00)	40.24 (0.54)
Emp. Income 2009	94.81	62.98 (0.00)	91.47 (0.44)
Expenditures 2009	100.30	89.00 (0.00)	101.95 (0.56)
Non-matched Trend			
Fatalities 2006	2.9	3.2 (n/a)	13.4 (n/a)
Fatalities 2007	16.9	11.7 (n/a)	16.8 (n/a)
Fatalities 2008	16.2	4.4 (n/a)	11.2 (n/a)
Yrs. of Schl. 2007	7.58	6.86 (0.00)	7.57 (0.92)
Yrs. of Schl. 2008	7.28	6.58 (0.00)	7.37 (0.36)
Emp. Income 2007	72.22	66.68 (0.21)	61.29 (0.08)
Emp. Income 2008	107.98	90.02 (0.04)	102.98 (0.67)
Expenditures 2007	94.96	69.22 (0.00)	73.05 (0.01)
Expenditures 2008	155.26	126.76 (0.00)	187.20 (0.03)

Note: The outcome variables considered are explained in section 5.2. Both self-employment income and expenditures are reported in USD. Standard errors for mean years of schooling and mean death age are based on standard deviations at the state level and computed according to the weighting. Standard errors for median self-employment income and median expenditures are based on boot-strapped standard deviations with 500 replications at the state level and computed according to the weighting. P-values are based on t statistics with $\min\{N_{ND}, N_{NND}\} - 1 = 8$ d.o.f. for the difference between ND and NND and $\min\{N_{ND}, N_{SC}\} - 1 = 3$ d.o.f. for the difference between ND and SC. Both are computed for two-sided tests. Since the measure for conflict is aggregate data on the universe of conflict, typical standard errors are not available. For more discussion of uncertainty with aggregate data when using the synthetic control method, see section 8.3.

Figure 2: Conflict Matching

Note: In these and all following figures, the blue thick line displays the Niger Delta values, the black thick and dashed line displays the synthetic control values and the red thin line displays the non-Niger Delta values.
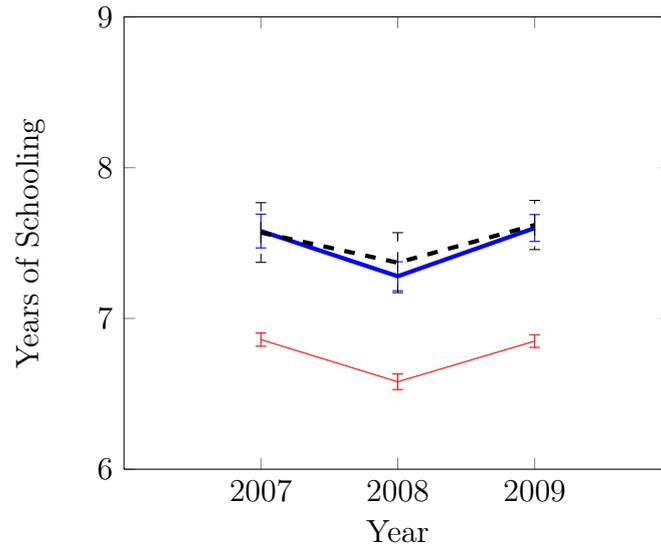
Figure 3: Education Matching
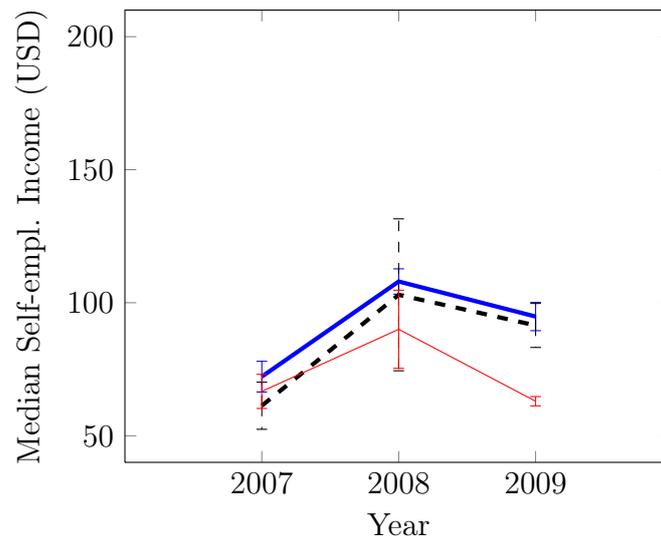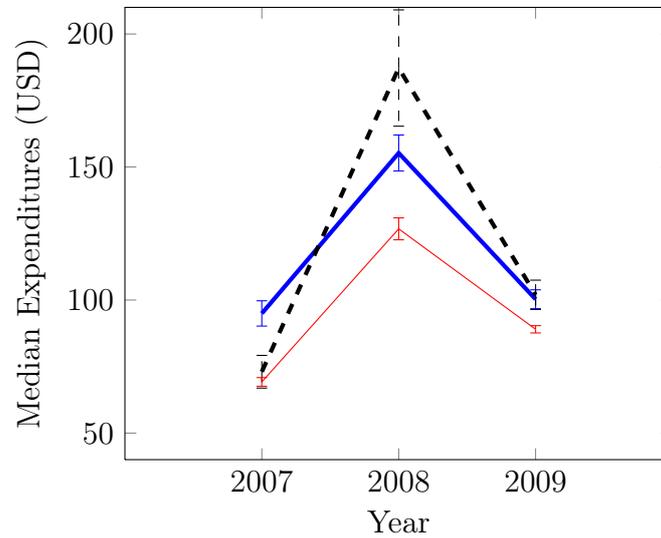

Figure 4: Employment Income Matching

Figure 5: Expenditures Matching

2009 in the Niger Delta region very well. The match for median household expenditures is not as close as for the conflict and education measure prior to 2009, but arguably still matches the Niger Delta region better than the population weighted average of non-Niger Delta states. In particular, the synthetic control underestimates in 2007 and overestimates in 2008; however, the average over the two years is very close to the Niger Delta region (130.13 USD in the synthetic control region and 125.11 USD in the Niger Delta region) while it is substantially lower in the 28 states outside the Niger Delta region (97.99 USD).[31]

Comparing the synthetic control method to standard regression techniques (involving in particular DID regression methods that are commonly used in the micro-empirical literature on the effects of conflict), three distinct advantages of the former method emerge. Firstly, while it can be shown that a regression-based estimator would essentially also produce a weighted average of potential control states with the weights summing up to one, the weights may lie outside the unit interval (Abadie et al., 2015). Therefore, regression techniques allow for extrapolation and hence potential extrapolation problems whereby the extent of extrapolation is typically unknown to the empirical analyst since the weights are usually not computed in practice. Secondly, and especially relevant to this multi-outcome analysis, a regression-based analysis would not produce the *same* counterfactual for all the outcomes while the synthetic control method – given its success in reproducing the pre-2009 trend in this case – may more convincingly deliver estimates of the causal effects of peace. Thirdly, the commonly used DID techniques do not allow to control for time-varying fixed effects in different regions while the synthetic control method does.[32] Abadie et al. (2010) prove that the synthetic control method is essentially a generalisation of the usual DID model in precisely the way that it allows unobserved

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[31]It also becomes very clear that there is no linear or uni-directional trend for education, employment income and expenditures. There are several important developments that may be reasons for the patterns observed: General elections in 2007, a sharp food price hike in 2008 and the financial crisis in 2008. Multiple channels are conceivable. Sharply increasing food prices from 2007 to 2008 (UNFAO, 2009) may have increased expenditure mechanically, but may also have resulted in substitution away from children's education towards income-generating activity through lower income. The crisis may have hit Nigeria throughout 2008 and resulted in worse economic performance in 2009, in particular in the Niger Delta region suffering from oil price drops as the Central Bank's governor suggested (CBN, 2009). The new president's policy may have had its own effect on education and economic performance. Irrespective of what combination of key events or developments may explain these patterns, however, it is more important for my identification that the synthetic control region matches the Niger Delta region in this trend.

[32]While it is possible to control specifically for potentially confounding observable variables – here, past realisations of outcome variables could for example be controlled for – and hence capture their time-varying effects within regions, this does not solve the problem entirely. First of all, there may still be *unobserved* characteristics that vary in time. In fact, including past outcome variables in the DID regressions that I carried out as robustness checks does not change the results substantially. In addition, this still imposes a linear relationship between the observed control variables that are included while the synthetic control method does not make such a parametric assumption. Finally, including past outcome variables as control variables engenders the standard problem of putting the assumption of no correlation between the error term and regressors at risk since any degree of auto-correlation in the error would result in a violation of the assumption.

characteristics to vary with time. The key common trend assumption in DID analyses which basically assumes away that unobserved characteristics in treatment and control units are time-varying can actually be verified in my data. Since my data allows me to observe how the treatment and control states evolve with regards to three of the outcomes of interest in the two years preceding the policy implementation in 2009, it can actually be observed how the common trend fails between 2008 and 2009 for self-employment income and household expenditures (see Figures 4 and 5). In fact, implementing a simple DID procedure as a robustness check, I show that the DID results deviate from the results coming out of the synthetic control method in exactly the direction of the violations of the common trend assumption (see section 8.1 for details).

6.2. Interpretation and Potential Concerns

It is instructive at this point to think about the interpretation of results coming out of the synthetic control method in the study context given that I take the unusual approach of evaluating peace in the Niger Delta region relative to continued conflict in other Nigerian states serving as counterfactual. Most studies conversely evaluate either ongoing conflict relative to peace or the consequences of a period of conflict some time after the conflict has ended (with greatly varying time horizons) by comparing areas that were affected by conflict to counterfactual areas that did not experience conflict in the past. I choose to evaluate peace relative to continued conflict because the amnesty policy in the Niger Delta region and conflict in other Nigerian states after 2009 provide a unique (quasi-experimental) setting for this estimation. Conflict in the period between 2009 and early 2013, especially the later part of this period, happens in a number of Nigerian states mainly – but not exclusively – in the North Eastern part of the country where Borno lies, one of the states making up the synthetic control region. This is partly driven by Boko Haram's activities in that region. In this sense, the results I am finding can also be interpreted as the contemporaneous impact of continued conflict in some parts of the Nigeria (represented by the synthetic control region) relative to peace in a comparable Niger Delta region. However, I prefer to interpret them conversely as the benefits of peace in the Niger Delta region relative to what would have happened counterfactually in the same region had the policy not been implemented. This is essentially an estimate of a peace dividend, an evaluation of the potential damage the amnesty policy prevented. To some degree, since the counterfactual displays (only) sustained levels of conflict very

comparable to the ones in 2009, this may even be a lower bound of the true peace dividend. The reason is that it is not clear whether conflict levels would have been sustained or further escalated in the absence of the amnesty policy. Given long underlying tensions and violent conflict that sparked shortly before 2009, it may not be unreasonable to assume that an escalation of conflict would provide a more realistic counterfactual.

Related to the interpretation and context, one may wonder why an amnesty policy was carried out as a response to conflict in the Niger Delta region and not in other states of Nigeria that experienced ongoing conflict and – more importantly – whether the underlying reasons pose a threat to identification. Clearly, the fact that the Niger Delta region is an oil rich region and that the oil production is a main determinant of the government's fiscal resources plays a role. However, I argue that this is not a concern for bias and internal validity and, if anything, affects external validity. First of all, any relevant influence of systematic differences in oil production between the Niger Delta region and the synthetic control region should show up in the pre-2009 trend which is actually well matched. Furthermore, oil production was decreasing in the Niger Delta region before 2009 (Abazie-Humphrey, 2014; OPEC, 2010). Any concern (in the sense of an omitted variable) postulating that oil production may have picked up before 2009 when the policy happened to be put in place and therefore observed effects are actually confounded with positive effects of already increasing oil production before 2009 is inconsistent with such a decrease. Much to the contrary, in fact, it seems likely that the policy was put in place precisely because of massive losses from decreased oil production due to conflict. However, this means that the effects of peace (partly) operate *through* increased oil production after 2009 which is not a source of endogeneity, but just a channel through which peace operates.[33] Therefore, my results just provide a reduced form effect of peace from all possible channels. Of course, because of the Niger Delta's oil richness, the region is particularly suitable and may be expected to generate large benefits from peace. This is an issue of external validity, related to the idea of "site selection" in randomised controlled trials (Allcott, 2015). As a consequence, and as usual, my results should first and foremost be interpreted as the effect generated in the specific context of conflict in the Niger Delta region. To assess to what extent external validity may be compromised, it is then instructive to regard them in light of findings from different settings.

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[33]Note that the synthetic counterfactual displays sustained levels of conflict reflecting a counterfactually sustained low oil production. Again, if anything, it seems reasonable to assume that conflict may actually have escalated further and induced further drops in fiscal revenues from oil production, in which case the results are lower bounds.

7. Results

7.1. The First Stage

While the sharp conflict trend was stopped and reversed in the Niger Delta region as a consequence of the policy from 2010, the synthetic control region continues to show high conflict levels after the policy implementation. Figure 6 depicts this first stage relationship very clearly. The synthetic control region maintains high conflict levels relative to both the Niger Delta region and the non-Niger Delta states after the amnesty policy implementation in 2009. Although there is a sharp drop in the synthetic control region in 2010, conflict levels are high again and comparable to the 2009 Niger Delta levels from 2011 to 2013. It is particularly convenient that conflict levels are very similar in the synthetic control in early 2013 and in the Niger Delta region in 2009 since the peace dividend estimation is based on outcomes in early 2013. By contrast, a population weighted average of all states outside the Niger Delta region does not match these conflict levels.[34]

7.2. Main Results

Comparing the Niger Delta region to its synthetic control region in early 2013, the effect of peace in the Niger Delta region on education, health, self-employment income and expenditures is considered in this section. Table 2 and Figures 7 to 10 summarise the main findings.
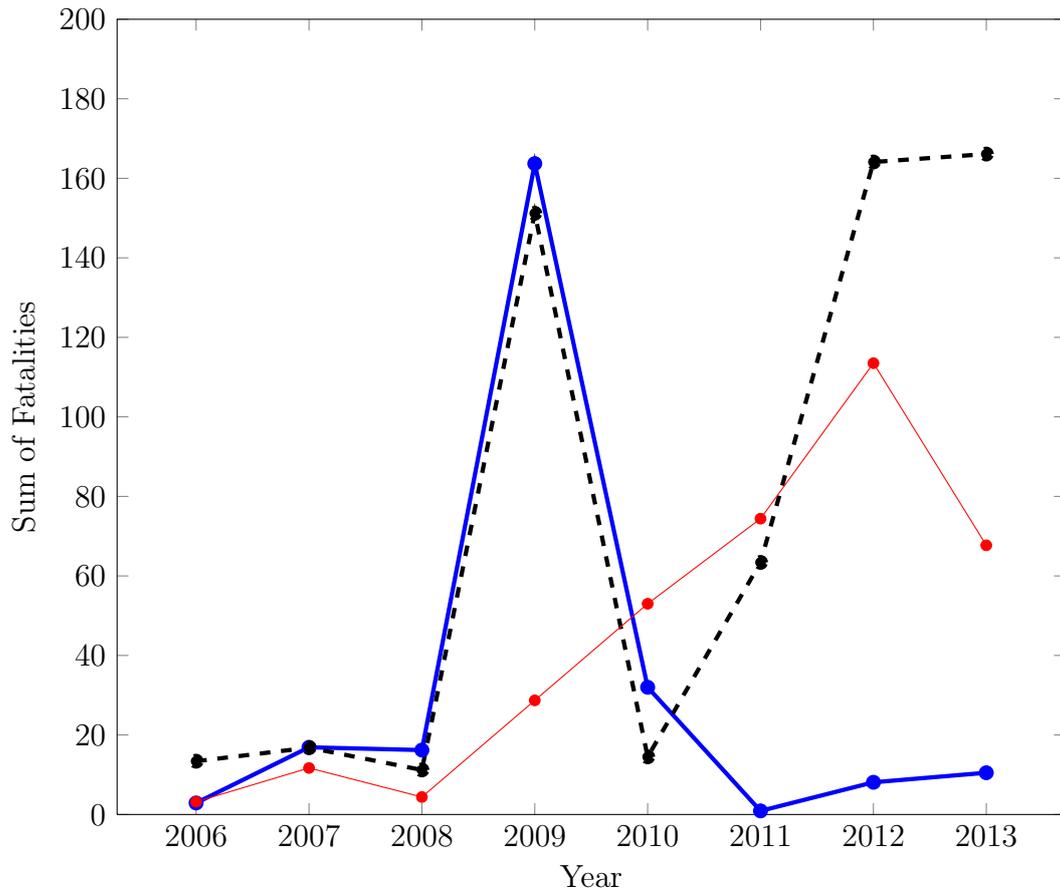
Table 2: Main Results

Variable	Niger Delta (ND)	non-ND (NND)	Synth. Control (SC)	ND - SC (Std. Err.)
Yrs. of Schl. 2013	8.56	7.21	8.03	0.53*** (0.11)
Death Age 2013	44.65	30.72	39.13	5.52 (3.85)
Emp. Income 2013	118.25	50.17	70.85	47.39** (12.87)
Expenditures 2013	193.47	140.00	163.11	30.35** (7.67)

Note: The outcome variables considered are explained in section 5.2. Both self-employment income and expenditures are reported in USD. Standard errors for mean years of schooling and mean death age are based on standard deviations at the state level and computed according to the weighting. Standard errors for median self-employment income and median expenditures are based on boot-strapped standard deviations with 500 replications at the state level and computed according to the weighting. Tests are computed based on t statistics with $\min\{N_{ND}, N_{SC}\} - 1 = 3$ d.o.f. and for one-sided tests (in line with the theoretical predictions). ***, ** and * denote significane at the 1%, 5% and 10% level, respectively.

The results represent a peace dividend over a period of four years. Except for the health effect, all results are significant at the 5% level. The substantial magnitude of

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[34]As a second piece of evidence for the first stage using simple regression analyses within the Niger Delta region only, see appendix A.

Figure 6: Conflict Levels Before and After 2009

Note: In these and all following figures, the blue thick line displays the Niger Delta values, the black thick and dashed line displays the synthetic control values and the red thin line displays the non-Niger Delta values. By 2013, the first six months of 2013 are meant since outcome variables in 2013 were collected between April and June.
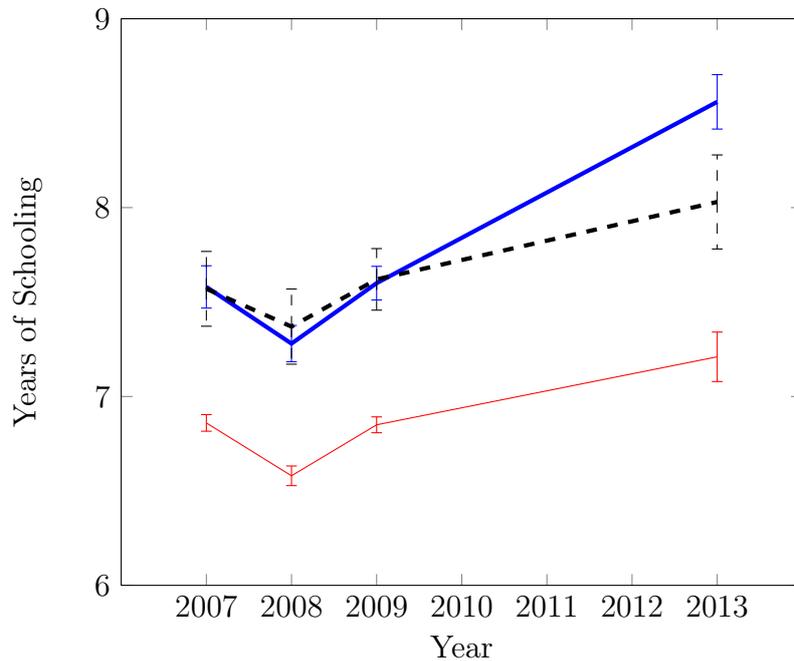
Figure 7: Education Results
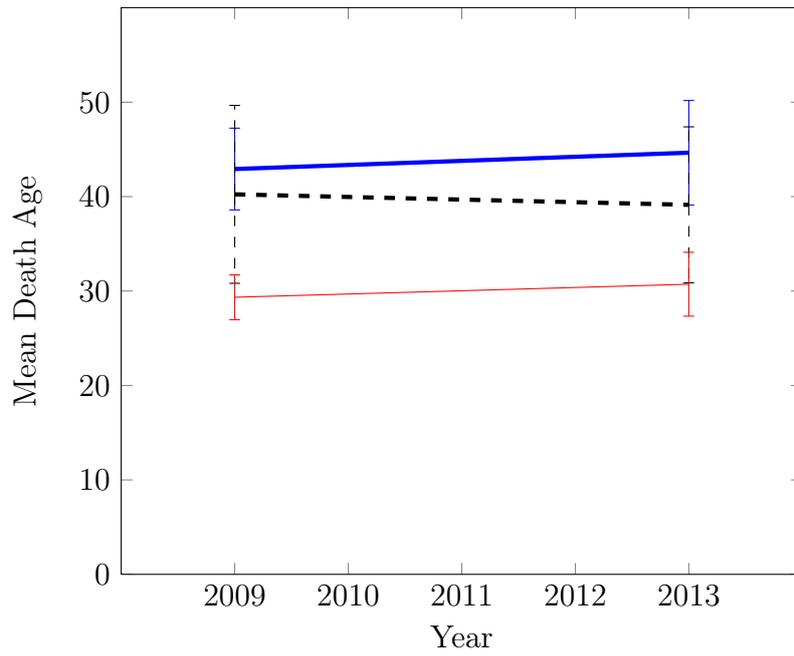
Figure 8: Health Results

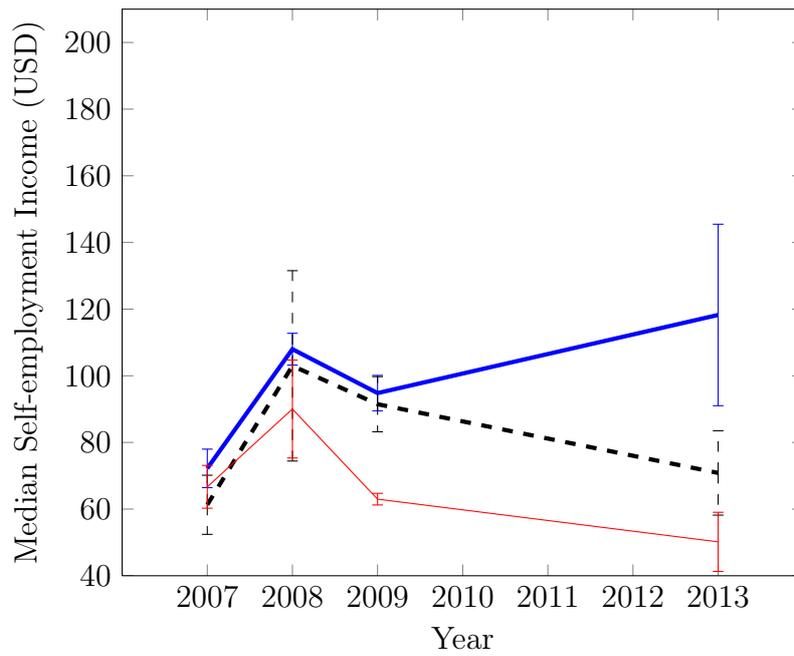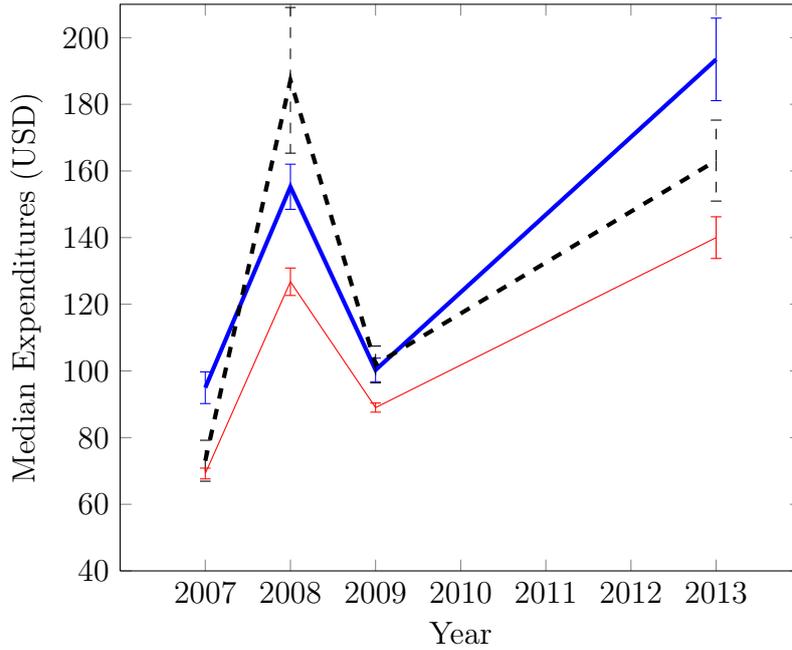

Figure 9: Employment Income Results

Figure 10: Expenditures Results

the results suggests that there are considerable benefits from peace or, conversely, severe consequences of conflict. Average education increases by 0.53 years of schooling. To put this in relation to the estimates from the existing literature, consider only children under the age of 18 which corresponds to the school age cohort that most authors have focused on. For this age group, the effect is even larger, namely an increase by 0.81 years of schooling which is in the range of what the literature finds in different contexts. According to estimates by Aromolaran (2006) and Schultz (2004), returns to primary and secondary schooling in Nigeria range between 2% and 7% and are even higher for post-secondary education (10%-15.5%).[35] Given these estimates, even if we were to ignore the potentially overestimated returns for post-secondary education which only about 10% of the Nigerian population enjoyed in 2013, an average increase by half a year of schooling has a meaningful long-term economic effect.

The effect on health is indeed positive, but not significantly different from zero. However, since this is not a precise zero estimate either, it is hard to conclude anything from the result. Given rather small sample sizes of deaths in many states, large standard errors were to be expected and I do not have enough statistical power to detect changes that are not extremely large.

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[35]Even though these authors merely run OLS regressions on household survey data, they argue that the potential upward bias from omitted socio-economic factors may very well be offset by downward bias from measurement error, referring to Card (1999) and Ashenfelter & Krueger (1994). In particular for primary and secondary education, the quality of education measurements in the household surveys would suggest that this is quite likely to be the case or that the bias from measurement error may even be larger in which case, if anything, their return estimates provide lower bounds.

With regards to both measures for economic activity, I find substantial effects. In the Niger Delta region, self-employment income is 47.39 USD higher than in the synthetic control region at the median and household expenditures exceed its counterfactual by 30.35 USD at the median. The relative effects are enormous. Given the counterfactual median values, these differences represent 67% and 19% increases, respectively. Since the match for household expenditures for years before 2009 is not perfect and there are significant differences between the Niger Delta and synthetic control region, one might be concerned about bias of the estimator. However, as discussed before, the averages over 2007 and 2008 in the Niger Delta and the synthetic control regions are extremely close since the match is just producing more extreme value in 2007 and 2008 (in 2007, the synthetic control is a bit too low and it is somewhat too high in 2008). In addition, given the fact that the fall between 2008 and 2009 is sharper in the synthetic control region, any DIDID procedure that would take this differential pre-trend into account would produce an even larger estimate of the effect of peace in 2013. In this sense, the estimate I am reporting may even be a lower bound.

In relation to the existing literature, the effect on self-employment is considerably larger than what the only comparable study in Mexico finds (Velásquez, 2015). This may hint at the external validity issue discussed above;[36] however, since there is only one comparison study, it is generally hard to draw any conclusions. In addition, my results on overall economic performance, here captured by household expenditure, are in line with what the most closely comparable literature finds. In any case, the large findings on self-employment income suggest that this may be an exciting channel to explore further in future research.

With regards to the theoretical considerations, the results are in line with what peace is expected to generate. In particular, the effect on household expenditure, taken as a measure of overall economic performance, is strongly positive. Clearly, the medium-run benefits of peace significantly dominate any potential immediate economic losses from diminished militant activity and oil theft. We can also observe that the increases in self-employment income are relatively larger than the increase in overall economic performance which may reflect that some labour supply shift from military activity and oil theft into self-employment activity has indeed taken place. Of course, alternative explanations may be that not all additional income is spent, but part of it is saved, or that self-employment

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[36]The fact that we may expect large economic benefits of peace in the oil-rich Niger Delta region may be due to both any spin-off businesses of the oil industry and the fact that oil or any derivative materials are key inputs in many businesses, e.g. for using generators or cars.

activity has increased relatively more strongly than waged labour supply as a response to peace.

8. Robustness

The fact that the synthetic control region can map the pre-2009 trend on the outcome variables on which it was *not* matched (i.e., this is not a mechanical result) reasonably well provides some confidence that the matching procedure generated an appropriate counterfactual. Building on that, I will present DID estimates and checks for spillovers as further robustness checks in this section. In addition, addressing both the issue of matching uncertainty and robustness against confounding forces, a placebo study is presented here.

8.1. DID Results

As a first robustness check, I will use a simple DID approach as an alternative method and present the results.[37] In particular, I can demonstrate that the DID results are in line with my previous results for outcomes for which the parallel trend assumption holds and deviate from the results in the expected direction for outcomes for which the pre-2009 trend for the non-Niger Delta states deviates from the Niger Delta and synthetic control region. Table 3 presents the results.

Comparing the results from the DID and synhetic counterfactual procedure, it must first of all be noted that the two estimators are not significantly different for any of the four outcome variables (the lowest p-value on the four difference tests is 0.32 – for median household expenditures). As for education and health, the difference between the two estimators is very small. In the case of the former outcome, we can observe the pre-trend and note that the parallel trend assumption strongly appears to be satisfied which is consistent with finding coinciding DID and synhetic counterfactual estimates. Graphically, Figure 7 demonstrates this clearly. Especially for the employment income

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[37]The simple comparison is essentially just a comparison of two differences in means. In regression format, the DID estimator would be the coefficient δ in:

$$y_{it} = \alpha + \beta \text{ND}_i + \gamma \text{T2013}_t + \delta(\text{ND}_i \times \text{T2013}_t) + u_{it}$$

where y_{it} and u_{it} are the outcome and error term in state i and year t respectively, ND is a dummy variable indicating whether state i is in the Niger Delta region or not and T2013 is a dummy taking the value 1 if the year is 2013. Most DID approaches are finer in the sense that they control for regional fixed effects by taking a dummy variable for each region instead of a coarser distinction between Niger Delta states and non-Niger Delta states only. However, which set of dummies is used does not make a difference for the magnitude of the DID estimator δ. The finer approach can only increase precision. For health, the only insignificant result that emerges with the simple DID means comparison, taking a set of state dummies still gives a large standard error and no significance.

Table 3: DID Results

Variable	ND	NND	DID (Std. Err.)	ND - SC (Std. Err.)	Trend Deviation (2009-2008)
Yrs. of Schl. 2013	8.56	7.21	0.59***	0.53***	0
Yrs. of Schl. 2009	7.60	6.85	(0.09)	(0.10)	
Death Age 2013	44.65	30.72	0.35	5.52	n/a
Death Age 2009	42.92	29.34	(3.70)	(3.85)	
Emp. Income 2013	118.25	50.17	36.24**	47.39**	+
Emp. Income 2009	94.81	62.98	(13.04)	(12.87)	
Expenditures 2013	193.47	140.00	42.17***	30.35**	-
Expenditures 2009	100.30	89.00	(6.36)	(7.67)	

Note: ND: Niger Delta region; NND: non-Niger Delta states; SC: Synthetic Control region. The outcome variables considered are explained in section 5.2. Both self-employment income and expenditures are reported in USD. The trend deviation (DID 2009-2008) for years of schooling is a quite precisely estimated zero (-0.07, p-value: 0.63). Standard errors for mean years of schooling and mean death age are based on standard deviations at the state level and computed according to the weighting. Standard errors for median self-employment income and median expenditures are based on bootstrapped standard deviations with 500 replications at the state level and computed according to the weighting. Tests are based on t statistics with $\min\{N_{ND}, N_{NND}\} - 1 = 8$ d.o.f. for the DID estimator and $\min\{N_{ND}, N_{SC}\} - 1 = 3$ d.o.f. for the synthetic counterfactual estimator. Both are computed for one-sided tests (in line with the theoretical predictions). ***, ** and * denote significane at the 1%, 5% and 10% level, respectively.

and expenditure measure, however, the fact that the two estimates are statistically indistinguishable may not necessarily be a result of the two estimators in fact coinciding, but of large standard errors. Nevertheless, considering the point estimates, the direction of the DID estimator's deviation is perfectly in line with pre-trend differences between the Niger Delta states and those outside the Niger Delta region. Figures 9 and 10 demonstrate this point graphically. It seems that the two regions follow a similar trend between 2007 and 2008 and have different trends between 2008 and 2009. Therefore, the direction of the common trend assumption's violation can be well captured by a simple DID between 2009 and 2008. As for median self-employment income, this trend deviation is positive. Therefore, we would expect the DID estimator to underestimate. In the case of median household expenditures, the trend deviation is negative and we would expect the DID estimator to overestimate. Relative to the estimator using the synthetic counterfactual, these are exactly the bias directions that we observe.[38] The fact that in precisely these two cases in which we observe non-negligible differences between the two estimators, they are in line with the direction of the common trend violation and that we do not observe differences when there are no or no observable such violations, should place some further

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[38]With regards to the magnitude of bias, the standard errors are extremely large which makes it impossible to make precise statements on whether the magnitude of the bias reflects the actual difference observed between the two different estimators. Indeed, the point estimates are close and statistically indistinguishable in both cases. For median self-employment profits, the estimator difference is 11.15 and the trend deviation is 13.87; for median household expenditures, the estimator difference is -11.82 and the trend deviation is -17.21.

confidence in the main results found using the synthetic counterfactual.

8.2. Spillover Effects

Since three of the four states that are part of the synthetic control region are quite close to the Niger Delta region, one might be concerned about spillover effects. Any effect of peace in the Niger Delta region on states outside it would form a violation of the Stable Unit Treatment Value Assumption (SUTVA) and bias the results. There are potential spillover effects in two directions that would bias the results in opposite ways. Firstly, there may be spillover effects from peace on other states that would form a positive impact on them, for example through increased trade with neighbouring states if economic activity improves in the Niger Delta region or through infrastructure extending into neighbouring states. Such effects would mean that states outside the Niger Delta region are also "treated" and their outcomes would show up more *positively* than the true counterfactual should. Therefore, these effects go against my results and would mean that I am underestimating and the true effect is even larger than what I find. Secondly, posing more reason for concern, peace in the region may attract people from other states into the Niger Delta region. In this case, outcomes of states outside the Niger Delta region would show up more *negatively* than the true counterfactual should and I would overestimate the true effect. Typically – and plausibly – these effects would be stronger for states that are closer to the treatment region which is what I am exploiting to test for spillover effects.

As a first piece of evidence against the presence (or dominance of) one type of spillovers, consider the above DID results again. The particular spillover concern with the synthetic control region is that three out of the four states making it up are very close to the Niger Delta region. The DID approach, however, takes all 28 non-Niger Delta states as part of the counterfactual. Since spillovers are expected to be stronger in states closer to the treatment region, the DID results should be systematically different from the main results using the synthetic control region in their presence. This is not the case. Not only are the DID results not significantly different, but also do different point estimates for median self-employment income and median expenditures go in *different* directions. They do not jointly indicate the prsesence or domanince of one type of spillover. Much rather, as discussed above, these differences can be explained by pre-trend deviations.

Migration flows, taken from the GHS 2012 survey, shall serve as a second piece of evidence against the presence (or dominance of) one type of spillovers. The particular

spillover concern in terms of migration flows is that there may be a massive inflow of (in particular highly economically contributing) people into the Niger Delta region from the synthetic control region relative to the opposite flow in times of peace or that peace generally attracts in-migration in which case states closer to the Niger Delta region should show more migration into that region than states further away. Table 4 demonstrates that both these concerns do not seem to apply. In fact, the migration flow of people from synthetic control states into the Niger Delta region is very small in absolute terms and considerably (as well as statistically significantly) lower than the opposite migration flow (the same result holds when comparing the *share* of migration from the synthetic control region into the Niger Delta region and vice versa). In addition, both the migration flow from states directly neighbouring the Niger Delta region (NS)[39] and from the synthetic control region into the Niger Delta region are very close (and indeed statistically indistinguishable) to the general migration flow from non-Niger Delta states into the Niger Delta region (again, the same result holds true when comparing migration *shares*). Therefore, the data does *not* suggest that the migration flow from states that are closer to the Niger Delta region is over-representative of the general migration flow into the Niger Delta region which is what we would expect to see if the second type of spillover effects was at play.

Table 4: Migration Flows

	Move from SC (Std. Err.)	Move from NS (Std. Err.)	Move from NND (Std. Err.)	Move from ND (Std. Err.)
Move to SC				1.95%
				(0.0024)
Move to NS				0%
				(0)
Move to ND	0.47%	0.40%	0.45%	13.02%
	(0.0019)	(0.0012)	(0.0006)	(0.0027)
Move to NND	10.59%	12.45%	7.75%	3.05%
	(0.0019)	(0.0012)	(0.0006)	(0.0027)
Total Migration	11.05%	12.84%	8.20%	16.06%
	(0.0053)	(0.0043)	(0.0018)	(0.0045)

Note: SC: Synthetic Control region; NS: States directly neighbouring the Niger Delta region; NND: non-Niger Delta states; ND: Niger Delta region.

8.3. Placebo Study

A typical disadvantage of synthetic control methods that work with aggregate data is that aggregate data does not provide standard errors indicating any uncertainty as to whether

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[39]These eight states have a common border with at least one of the Niger Delta states and are thus defined to be the neighbouring states: Anambra, Benue, Ebonyi, Ekiti, Enugu, Kogi, Ogun, Osun.

a sample is reflective of a population (Abadie et al., 2010). For example, studies that employ GDP data to produce a synthetic control for some treatment region (or country) from a set of potential control regions (or countries) essentially use the universe of available GDP observations in their context.[40] Standard inference uses standard errors reflecting the unavailability of aggregate data. However, not all uncertainty is due to lacking aggregate data. In studies employing the synthetic control methods, "an additional source of uncertainty derives from ignorance about the ability of the control group to reproduce the counterfactual of how the treated unit would have evolved in the absence of treatment" (Abadie et al., 2010). In this study, I actually use sample averages from household surveys for my outcome variables which is why I can produce classic standard errors reflecting the uncertainty around whether the sample is representative of the universe of households. In a way, this uncertainty also captures the extent to which the Niger Delta region and its synthetic counterfactual are comparable before the policy intervention and differ after it. The underlying assumption is essentially that households in the synthetic control region display the same relevant characteristics on average before the policy is implemented. The matching procedure is supposed to produce exactly such a counterfactual satisfying this assumption and considering the pre-2009 trend enables me to verify how reasonable the assumption is. Therefore, reporting the classic standard errors as a measure of uncertainty around whether the synthetic control is a viable counterfactual is meaningful.

Nevertheless, as an additional check, I provide the classic solution to the problem of working with aggregate data in the context of the synthetic control method: A placebo study. Much more directly, implementing the same method for control regions that are actually unaffected and verifying what change after 2009 can be observed, this gives us an idea of how precisely the obtained estimates are and whether they are larger than what "placebo coincidence" would produce. In addition – particularly considering states that are part of the synthetic control region for placebo checks – this allows us to verify whether there is systematic movement (in a particular direction) in the synthetic control region which would suggest endogeneity and render the region invalid as a counterfactual.
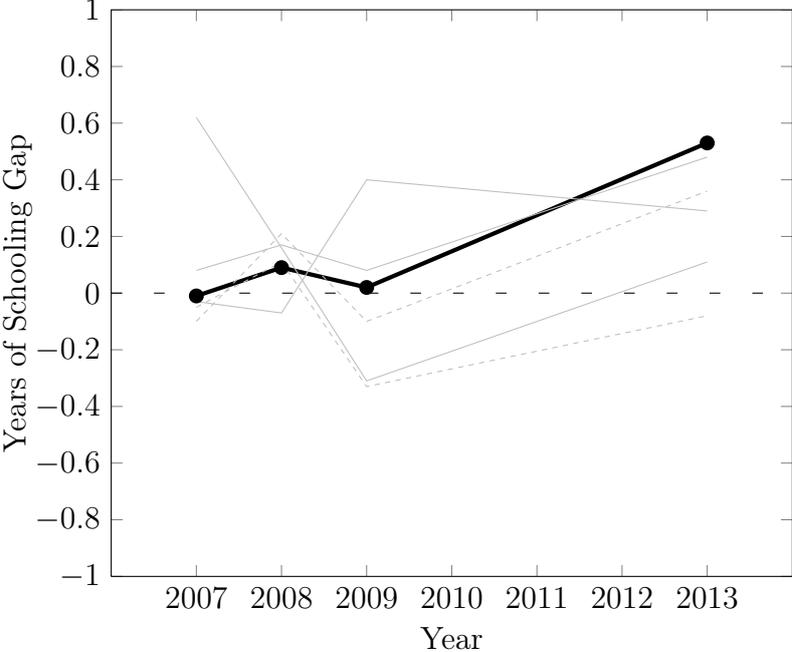
As a placebo study, I have carried out the matching procedure to produce a synthetic control region for each of the 28 non-Niger Delta studies.[41] For many states, this procedure

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[40]This is precisely the approach taken by Abadie & Gardeazabal (2003), Gong & Rao (2016) and Matta et al. (2016).

[41]As a first approach, I matched on the sum of fatalities and the realisations of the four outcome variables in 2009. However, the synthetic control regions this procedure generated were quite bad matches for the great majority of the 28 non-Niger Delta states. Therefore, I explicitly matched on realisations of the outcome variables before 2009 as well which produced synthetic control regions that were somewhat more congruent with their respective states.

unfortunately did not produce a good match. Placebo synthetic control regions for any of the non-Niger Delta states are of course only useful if they produce a reasonable match. In line with Abadie et al. (2010), I calculate the mean squared prediction error (MSPE) for all placebo synthetic control regions on the pre-2009 period to evaluate whether the match is good enough to provide a good placebo check. I present the results for the five non-Niger Delta states, the synthetic control region of which had an MSPE not exceeding thirty times the MSPE of my original synthetic control region in relation to the Niger Delta region.[42] The gap between the state to be matched and its synthetic control in the four outcome variables of interest are presented for the Niger Delta region (black) as well as the five admissible non-Niger Delta states (Anambra, Gombe, Kogi, Niger, Ogun) as placebos (grey) in Figures 11 to 14.

Figure 11: Education Placebo Results

The results for education and self-employment income show clearly that the estimates for the Niger Delta region are outside the interval of mere randomness. In both cases, the estimated difference between the Niger Delta region and its synthetic counterfactual in

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[42]I separately considered the MSPE for realisations of the four outcome variables and conflict and imposed the condition on all of them. For the outcome variables, I considered the MSPE for the pre-2009 trend while I considered in particular the post-2009 period from 2010 to early 2013 for the conflict realisation. The idea is that a valid placebo synthetic control region obviously would not experience any change in conflict during this period; therefore, the synthetic control region is supposed to provide a good match on conflict for this period. Note also that the factor of 30 relative to the original synthetic control's MSPE as a tolerance allows for substantial variation of the synthetic control regions around the actual realisations of their respective non-Niger Delta state to be reproduced. However, given that the majority of matches actually produces little congruent synthetic controls, this factor seemed to be the most reasonable number in the trade-off between having decent matches and having enough placebo studies to include.
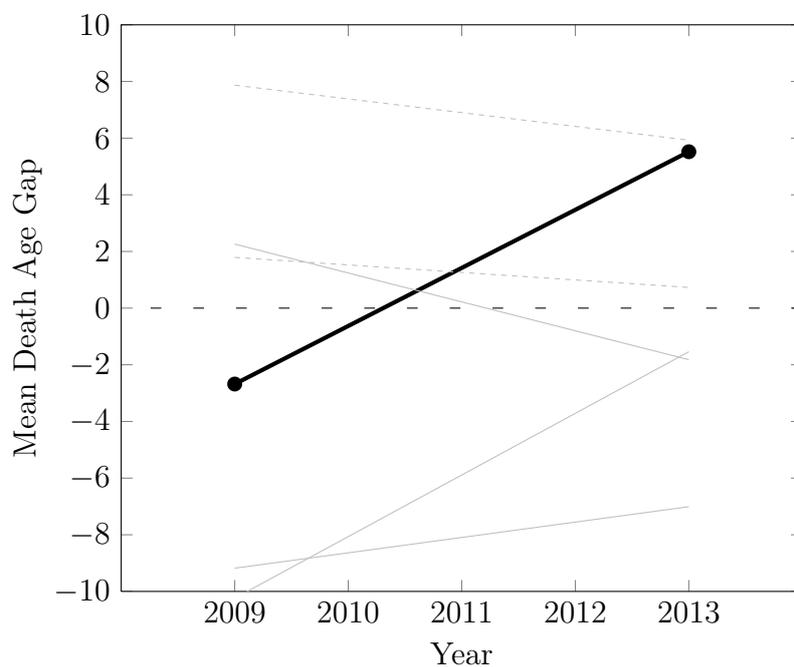
Figure 12: Health Placebo Results

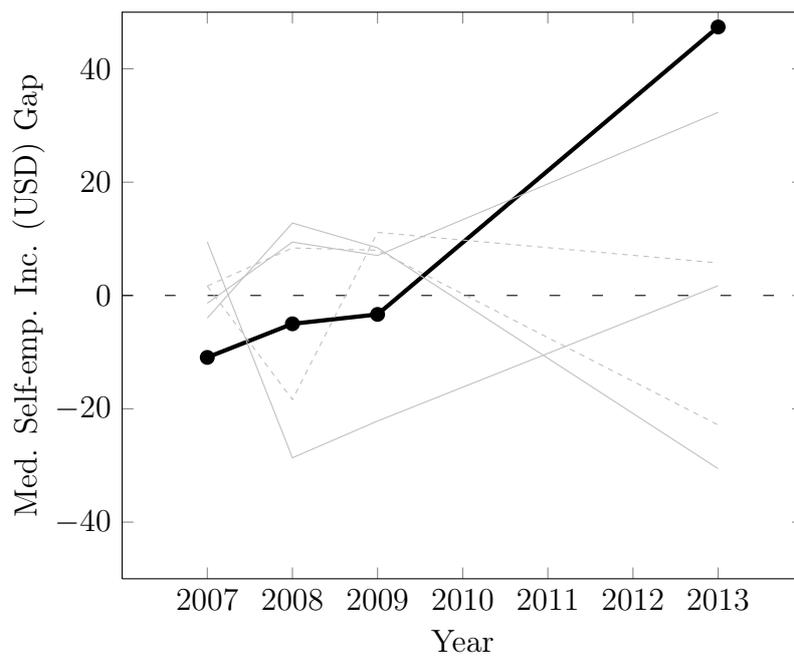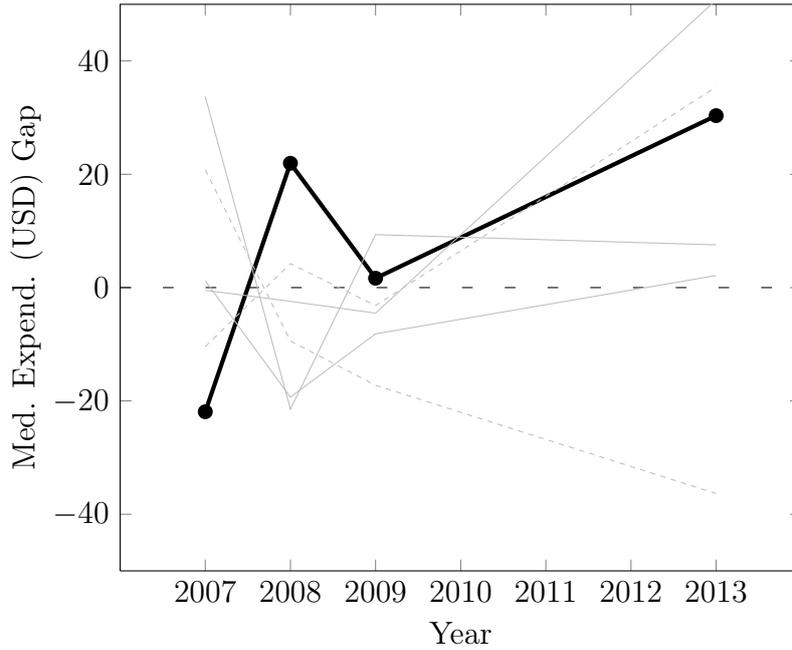Figure 13: Empl. Inc. Placebo Results

Figure 14: Expend. Placebo Results

2013 takes the largest value in comparison to the placebo studies. For health, the results are quite unclear which is in line with the fact that the main result gives an insignificant effect.[43] With regards to household expenditure, the Niger Delta estimate is not clearly outside the range of the placebo results. In spite of significance of the main results, this means that the results on household expenditure should be interpreted with a considerable degree of uncertainty. However, it is important to bear in mind that the placebo studies considered here include synthetic control regions that have non-negligible MSPEs on the pre-trend already, i.e. they are suboptimal matches. Therefore, it may not be surprising to see considerable gaps between the non-Niger Delta states and their respective synthetic control regions in 2013 as well. In addition, in spite of these substantial gaps, it is reassuring that this may to a large degree be actual randomness since there does not seem to be a systematic uni-directional "effect" that all placebo studies show jointly.

In particular, it is interesting to consider the placebo results for the two non-Niger Delta states that are part of the Niger Delta's synthetic control region (Anambra, Ogun; dashed grey lines). Although the gap between them and their respective placebo synthetic control region is large, it goes in opposite directions for the two and has approximately the same magnitude (-36.35 USD and 35.41 USD, respectively). In fact, for both the education and self-employment income placebo studies, the results on Anambra and Ogun also go

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[43]The measure for health employed here has great standard errors and does not have by far as large a sample size as the other measures. Therefore, it may not be very surprising that the Niger Delta estimate and placebo results display great variation and generally imperfect matches.

in opposite directions. This provides some confidence that deviations are not systematic and the Niger Delta's synthetic counterfactual is not subject to endogeneity.

9. Conclusion

Building on the existing micro-empirical literature on the consequences of conflict, this paper provides evidence from Nigeria. Regarding the issue from the converse perspective, I estimate the benefits of peace for education, health, self-employment income and household expenditures. For this estimation, I make use of the 'Presidential Amnesty Program' carried out by the Nigerian government in the Niger Delta region which granted ex-militants in the region pardon, cash and required them to join a 'Disarmament, Demobilisation and Reintegration' programme in return for surrendering their weapons. This policy shock led to a period of peace in the region and provides a unique setting to estimate a peace dividend by comparing the Niger Delta region to a synthetic within-country counterfactual in the spirit of Abadie & Gardeazabal (2003). The synthetic control region does not fail to reproduce the pre-2009 trend in the Niger Delta region and the estimation results are robust to several checks.

I find that peace in the Niger Delta region resulted in an average of 0.53 more years of schooling, a 67% increase in self-employment income at the median and a 19% increase in household expenditures at the median four years later. The estimated impact on health is insignificantly positive, but very imprecisely measured. These results are to be interpreted as the peace dividend generated by the amnesty policy in the Niger Delta region relative to a counterfactual of sustained conflict in the region. Although suffering from a considerable degree of uncertainty around the point estimates, they may even provide a lower bound to the true effects of peace if we were to assume (not unrealistically) that violent conflict in the Niger Delta region would not have remained constant, but further escalated in the absence of the amnesty policy.

The estimates give us an idea of how large and varied the effects of peace – or conversely, conflict – potentially are for different outcomes of economic importance and provoke two exciting questions for further research. Firstly, what are the consequences of conflict for different areas of economic activity beyond general economic performance? Given the extra-ordinarily large effect I find for self-employment income and that there is only one other paper exploring this channel, it seems worthwhile trying to dissect effects on overall economic performance in its various components. Secondly, do these large effects persist

long into the future and or do affected regions or countries converge back to their non-conflict counterfactual eventually? Although there is some literature exploring effects in the very long run, the results are inconclusive. More micro-empirical research employing methods that can plausibly establish causal links, such as the synthetic control method, and identify mechanisms between various outcomes and welfare seem necessary to find an answer to this question.

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Appendix

A. Conflict Reversal in the Niger Delta Region

As an additional regression peace of analysis on the first stage, consider the Niger Delta region only. A simple regression of fatalities from 2006 to 2015 on a dummy taking the value 1 from 2010 onwards (2010+ dummy) – essentially a mean comparison – shows a significant reduction in conflict. I consider an effect from 2010 only since the policy came officially in effect in August 2009 as a response to violent conflict in earlier years and particularly early 2009 itself. Therefore, conflict in 2009 is actually quite high and only reduces from 2010 onwards. This effect is particularly strong for more violent events, defined here as events involving at least five fatalities. In addition to this reduction in conflict, there is also a stark trend reversal. A further regression including a time regressor for years as time periods starting in 2006 and an interaction with the 2010+ dummy – again, for more violent events – shows a significant and substantial trend reversal. The results are shown in Table 5 and 6.

Table 5: First Stage in the Niger Delta Region

Variable	Coefficient	(Std. Err.)
2010+ dummy	-34.067***	(12.934)
Intercept	44.781***	(9.744)
N		74
R^2		0.088
F $_{(1,72)}$		6.938

Note: The dependent variable is fatalities in a violent conflict event involving at least five fatalities. The *2010+ dummy* takes the value 1 for 2010 and all following years and 0 otherwise. The regressor *time* is a normalised year variable taking the value 0 for 2006 and by one unit increasing values for following years. Normal standard errors are computed and reported, but heteroskedasticity-robust standard errors change none of the significances. ***, ** and * denote significane at the 1%, 5% and 10% level based on two-sided testing, respectively.

Table 6: First Stage Trend in the Niger Delta Region

Variable	Coefficient	(Std. Err.)
2010+ dummy	97.093**	(43.569)
time	36.664***	(8.156)
2010+ dummy × time	-40.087	(9.295)
Intercept	-59.481	(24.763)
N		74
R^2		0.297
F $_{(3,70)}$		9.849

Note: see Table 5.