Received: 4 July 2019 | Revised: 13 June 2021 | Accepted: 15 June 2021

DOI: 10.1111/ecpo.12197

ORIGINAL ARTICLE

ECONOMICS & POLITICS WILEY

The economic impact of political instability and mass civil protest

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Abstract

An extensive literature has examined the economic effects of non-violent political instability events. Nonetheless, the issue of whether economies react differently over time to such events remains largely unexplored. Using synthetic control methodology, which constructs a counterfactual in the absence of political instability, we estimate the output effect of 38 regime crises in the period 1970–2011. A crucial factor is whether crises are accompanied by mass civil protest. In the crises accompanied by mass civil protest, there is typically an immediate fall in output which is never recovered in the subsequent five years. In crises unaccompanied by protest, there are usually no significant output effects. It is unclear, however, whether mass civil protest causes the greater fall in output or is simply an indicator of a more severe political crisis.

KEYWORDS

civil protest, economic crisis, economic recovery, political instability, synthetic control method

1 | INTRODUCTION

There is consensus in the political economy literature that armed conflicts and politically motivated violence such as assassinations depress economic growth (Barro, 1991; Bleaney & Nishiyama, 2002). What about other, less drastic forms of political instability? Here, the evidence is somewhat less clear.

Aisen and Veiga (2013), Alesina et al., (1996) and Jong-A-Pin (2009) find a negative impact of political instability on growth, but with different measures of political instability and different econometric approaches. Leadership changes in less democratic regimes can also affect the growth rate, either positively or negatively (Collier & Hoeffler, 2015; Jones & Olken, 2005). Moreover, the issue of whether countries recover the output loss from such political instability events by catching up later, or whether the loss is permanent, remains largely unexamined. This is particularly relevant because research has shown that major "disruptive events" such as unusually deep recessions or currency crises tend to cause permanent output losses (Cerra et al., 2013; Cerra & Saxena, 2008; Hong & Tornell, 2005).

The contribution of the present paper is twofold. First, it makes use of the recently developed synthetic control methodology (SCM) to uncover, for the first time, the output recovery path after non-violent political instability events. Second, it shows that non-violent political regime crises have significant and permanent negative output effects and that these negative effects arise largely in cases where the crisis is accompanied by mass civil protest.

To obtain a robust measure of political instability, we draw on the work of Jong-A-Pin (2009), who has trawled cross-country data bases for no fewer than 25 different indicators of political instability and distilled them into a few major dimensions using exploratory factor analysis. These dimensions consist of (a) politically motivated violence (civil conflict, revolutions, and assassinations); (b) mass civil protest (riots, demonstrations or strikes); (c) structural instability of the political regime (factors such as ethnic diversity, frequency of elections or how often the largest party is out of office); and (d) regime crises (changes of chief executive or cabinet ministers, major changes to the constitution or to the political regime, coups d'état or major government crises). Jong-A-Pin finds that only (d), his factor for regime crises, has robust and significant negative effects on economic growth. We too focus on regime crises. Such events are often, but not always, accompanied by mass civil protest (e.g., the "Arab Spring" in Egypt and Tunisia in 2011); we distinguish between regime crises with and without mass civil protest.

We estimate the output effects of these disruptive events up to a five-year horizon using SCM, which yields estimates not only of the immediate impact but also of the cumulative output effects for each subsequent year for each country. The essential idea of SCM is to compare the postcrisis performance of a country with that of a synthetic alternative consisting of a weighted average of other countries, the weights having been chosen in such a way that the synthetic alternative closely tracks the economic performance of the country before the crisis. The estimated instability effect is then calculated as the postcrisis difference between the actual output performance of the country and that of its synthetic counterpart. As in difference-in-difference (DiD) estimation, the aim is to find an untreated matching country with which to compare each treated country. By allowing weighted averages of other countries to act as matches, which DiD does not, SCM expands the pool of possible comparators, which is valuable when the number of untreated countries is limited. Applying this technique to all countries that have experienced disruptive events of a particular type yields estimates not only of the average effect on real per capita GDP across countries but also of the dispersion about this mean, which is a significant advantage over the conventional panel econometric models used in the applied political economy literature (Aisen & Veiga, 2006, 2013; Jong-A-Pin, 2009). An attractive feature of SCM is that, like DiD, it should not be affected by global economic shocks that are common to all countries in either the pretreatment or the post-treatment period, unlike methods based on comparing the treated country just with its own past (Calvo et al., 2006; Cerra & Saxena, 2005; Reinhart & Rogoff, 2014) or with forecasts based on projections from its own past (ESCWA, 2016).

We are careful to discard cases where other disruptive events, such as armed conflicts or earthquakes, might have affected the result, which leaves us with 38 episodes of regime crises accompanied by mass civil protest, and a further 10 of regime crises without mass civil protest. We find striking

differences between these two cases. Regime crises accompanied by mass civil protest cause an immediate drop in output, which, on average, is not recovered in the following five years. In the absence of mass civil protest, by contrast, regime crises tend to have negligible adverse effects on the path of output. However, these results do not tell the whole story as our empirical findings also show that there is a great deal of diversity in the output effects of regime crises (either with or without mass civil protests) across countries. Moreover, further research is needed to establish whether mass civil protest is the critical factor or whether it just happens to be associated with other determinant factors, such as the severity of the political crisis.

The remainder of the paper is structured as follows. Section 2 provides a brief discussion of theory and a summary of existing literature on the economic effects of political instability. Section 3 discusses our data and our definition of political instability events. Section 4 describes the empirical method, including the covariates used to construct synthetic controls for each country's GDP per capita. Section 5 presents the findings, while section 6 reports the results for individual countries. Finally, section 7 concludes.

2 | THEORY AND LITERATURE REVIEW

There are several potential mechanisms through which mass civil protest might worsen the negative impact of regime crises. One possibility is that mass protest signals the risk of more fundamental political and economic changes in the future. Events that do not lead to protest may be "business as usual"—a changing of roles within the existing elites, but not a structural change to the day-to-day operations of businesses. However, when people go on to the streets, this might signal the threat of a major regime change—in the worst case, a revolution. Here, we are limited by our data, which record crises as simple binary events, with no measure of severity. A second mechanism is that protests and strikes may impose additional constraints on firms' operations as the cost of production indirectly increases due to transportation disruptions, workers taking unscheduled time off and delays to business meetings (Mas, 2008; Shonchoy & Tsubota, 2016). However, it is not obvious why such effects would persist for many years after the instigating event. Finally, mass civil protests are often associated with crime events such as theft, vandalism and robbery (Roberts, 2012; Roman, 2013; Tadjoeddin, 2013) which would have a direct negative effect on business confidence and investment. For instance, during and after the 2011 Tunisian revolution, political instability was a major constraint on firms located in the interior region of Tunisia which experienced the highest number of protests and strikes (Matta et al., 2018). Unfortunately, we cannot empirically investigate the validity of this channel as we do not have firm-level data that span across all countries and time periods.

There has been a considerable volume of research on the theory and empirics of civil conflict, which is relevant here because such conflict can be regarded as political instability that is raised to the level of serious violence. Fearon and Laitin (2009) identify 139 civil wars across the globe during the period 1945–2008, of which they count 79 as ethnic and 24 ambiguously so, which suggests that ethnic tensions often play a part in political instability. The main conclusion of empirical research is that countries with little ethnic diversity have less conflict, but conflict can be high in both highly fractionalized states (with many small ethnic groups) and highly polarized ones (with two large competing groups) (Bleaney & Dimico, 2011). Cederman et al., (2009) develop a new perspective, focusing not so much on the statistics of the ethnic composition of states, but on whether ethnic groups are part of the ruling coalition or not. They argue that conflict overwhelmingly tends to occur between the state and groups that are politically organized but are excluded from the ruling coalition. They then hypothesize that the excluded groups are more likely to engage in armed conflict when they are relatively

numerous, are more distant from the capital, and/or inhabit rougher terrain, and therefore have greater chances of success, and they find some supportive evidence in the data. Applying this idea to milder forms of political instability highlights the possibility that mass civil protest may not be entirely spontaneous but may be part of a strategic decision by opposition political groups. In short, the occurrence of mass political protest may sometimes reflect a deliberate decision to escalate a challenge for political power and therefore have greater effects on the economy than when such activity is absent.

However, it is still difficult to judge whether any statistical association between mass civil protest (MCP) and the likelihood of significant negative economic effects of political instability arises because MCP actually causes these economic effects. Perhaps it is just a symptom of some other factor or a particularly deep crisis, and shallower crises do not have similar effects. If the estimated MCP effect can be entirely explained by the intensity of the political crisis, that would tend to suggest that MCP is not causative. Unfortunately, the data do not provide any measure of the intensity of the political crisis, except in the very limited form of which or how many of the criteria used for identifying a political crisis are observed.

Early work on the economic impact of political instability used cross-country regressions to investigate the effect of violent events such as revolutions, coups, and assassinations (Barro, 1991; Fosu, 2001), although Alesina et al., (1996) also include constitutional government changes, provided that they represent a significant chance in ideology. Carmignani (2003) provides a useful survey of this literature. More recently researchers have attempted to explore the effect of other forms of political instability, making use of the rich Cross-National Time Series dataset (Banks and Wilson (2015); hereinafter CNTS), which was originally launched in 1979 by Arthur Banks.

However, political scientists have argued that instability is a latent and multidimensional concept that reflects different events: institutional change, political violence, armed conflicts, civil protest, riots, instability of the political regime, among other things (Hibbs, 1973; Rummel, 1963; Tanter, 1966). It is inevitable, therefore, that researchers have defined and measured this construct somewhat differently.

Jong-A-Pin (2009) has made the most comprehensive effort to address the multidimensionality of political instability by applying exploratory factor analysis (EFA) on 25 measures of instability previously used in the literature (see Table A.1 in the appendix for details).[1] He finds that political instability indicators can be grouped into four major categories: "politically motivated violence," "mass civil protest," "instability of the political regime" (structural factors such as ethnic diversity), and "instability within the regime" (various forms of regime crisis). We use this classification to examine the effects of mass civil protest and political regime crises. In particular, we investigate whether these events have an immediate cost in lost output and whether this loss is subsequently recovered to a significant degree.

Similar issues have been addressed in relation to other types of what might be called "disruptive events." Cerra and Saxena (2005, 2008) and Cerra et al., (2013) have investigated the long-term effects of deep recessions.[2] Kang and Meernik (2005) and Flores and Nooruddin (2009) have examined why certain economies recover faster than others in the aftermath of armed conflicts, while Hong and Tornell (2005) and Cavallo et al., (2013) do the same for currency crises and severe natural disasters, respectively. Only the last of these uses the synthetic control methodology (SCM) that we employ here. Matta et al., (2019) use SCM in a detailed analysis of the uprisings in Tunisia that initiated the Arab Spring, but that is only a single case.

[1]All tables and figures that start with an "A" are in the appendix.

[2]Deep recessions in this context are ones in which output actually falls in at least one year.

3 | DATA

Because the effects of politically motivated violence have already been extensively investigated by economists,[3] and also because such violence often lasts for a considerable length of time, thus making it less suitable for analysis by the method used here, we focus on other forms of political instability. Based on the findings of Jong-A-Pin (2009) discussed above, we consider two types:

(i) Regime crisis: there is a coup, a major constitutional change, and/or a major government crisis.[4]
(ii) Mass civil protest: one (or more) of the following three indicators pass a threshold value: strikes, riots and demonstrations.

Precise definitions are given in Table A.2 in the appendix. To identify all these events that happened between 1970 and 2011,[5] we use the 2015 version of the CNTS, which compiles political instability indicators based on the daily editions of the New York Times.[6] More often than not, a regime crisis occurred against the background of mass civil protest. Therefore, we analyze the output effects of such a regime crisis both with and without mass civil protest.

In certain cases, however, episodes of political instability occurred in the midst of large-scale natural disasters (floods, earthquakes, hurricanes). To accurately capture the economic impact of political instability and minimize potential biases, we exclude these episodes because it is hard to disentangle their economic impact from that of other large non-economic exogenous shocks that brought havoc to their respective economies (e.g., Guatemala in 2009). We also exclude Myanmar and Qatar because of data limitations: Myanmar did not have real GDP per capita data, while Qatar's GDP per capita was available only from 2000, five years after the coup has happened. Overall, we omit 28 episodes. Table A.3 in the appendix lists these events and provides a brief explanation of why we excluded them. Consequently, our final set of episodes of regime crisis consists of 48 events presented in Table A.4, of which 38 are accompanied by mass civil protest (panel I), while 10 are not (panel II).

4 | EMPIRICAL METHOD

Synthetic control methodology (SCM), which is considered by Athey and Imbens (2017) to be the most important innovation in the program evaluation literature over the last decade, has been widely used in various social science disciplines over the past few years. It has been used to evaluate the impact on economic

[3]For an extensive review, see Blattman and Miguel (2010).

[4]In contrast to Jong-A-Pin (2009), we d0 not include cabinet changes as part of the significant change in the regime crisis component because these events do not necessarily represent instability, since they occur frequently as part of normal political cycle. For instance, the standard government change variable includes "changes that do not involve substantial turnover of leadership" (Alesina et al., 1996, p. 193).

[5]We start from 1970 in order to allow for at least 10 years prior to any political instability event in order to construct a robust counterfactual for each country that was affected by political instability events. More details regarding the construction of counterfactuals will be presented in section 4. Our sample ends at 2011 because we set the minimum number of post-instability years to three, meaning that we exclude countries that were subjected to mass political instability after 2011 (more details in section 4).

[6]To ensure that the political instability events identified using the CNTS database are accurate, we cross-checked using information from international institutions (IMF and World Bank) and news outlets (BBC and CNN), among others.

activity of terrorism (Abadie & Gardeazabal, 2003), counterinsurgencies (Singhal & Nilakantan, 2016), civil wars (Bove et al., 2016), trade openness (Nannicini & Billmeier, 2011), economic liberalization (Billmeier & Nannicini, 2013), natural resource discoveries (Smith, 2015), inflation targeting (Lee, 2011), natural disasters (Cavallo et al., 2013), and fiscal consolidation (Kleis & Moessinger, 2016).

SCM is a generalization of the matching and difference-in-difference (DiD) techniques conventionally employed in large microeconomic datasets. It is particularly suited for macroeconomic applications where the cross-section dimension of the data is limited, so that credible untreated observations required by other matching methods are hard to find. The SCM alleviates this problem by allowing the use of weighted averages of other units as the counterfactual. This requires a reasonable number of pretreatment observations for each unit in order to select an appropriate counterfactual, but in macroeconomic applications, the time series is usually long enough for that. Moreover, this technique has several advantages over conventional panel econometric models. First, it captures the effects of time-changing unobservable variables, unlike DiD and fixed effects models which only accounts for time-invariant effects (Abadie et al., 2010; Billmeier & Nannicini, 2013). Second, it enables us to examine the causal impact of political instability on output over time, in contrast to standard econometric methods, which only allow for evaluating the average treatment effect for the whole sample. Third, it allows for a country-by-country assessment of the impact of a shock, a feature that is very useful in providing further insights into the heterogeneous effects of political instability.

In what follows we briefly describe the application of the synthetic control method in a general context. Let r denote a unit (country, state, or region) that was exposed to an exogenous treatment (in our case a political regime crisis) at time T, and $c \in C$ denote a potential control unit that was not exposed to the treatment. In addition, we indicate by $\boldsymbol{X}$ a $(x \times 1)$ vector of observed covariates that are likely to influence the outcome variable Y. Under certain assumptions (no anticipation, no interference, large pretreatment period, and structural similarity),[7] Abadie et al., (2010) have shown that the outcome and covariates of the treated unit can be approximately matched by a weighted average of control units, called synthetic control (or counterfactual), such that

$$Y_{r,t} = \sum_{c=1}^{C} w_c^* Y_{c;t} \quad \text{for } t < T \tag{1}$$

and

$$X_{r,t} = \sum_{c=1}^{C} w_c^* X_{c;t} \quad \text{for } t < T \tag{2}$$

In equations (1) and (2), $w^*_{c \in C}$ are the optimal weights assigned to each unit c in the constructed synthetic control. They satisfy the following conditions: (i) $w^*_{c \in C} \geq 0$ and (ii) $\sum_{c=1}^{C} w_c^* = 1$.

In our case, we apply SCM for each of the episodes listed in Table 1. Our outcome variable is real GDP per capita as reported in the April 2016 version of the World Development Indicators (WDI) published by the World Bank. The data period is 1960 to 2015. In our analysis, the set of potential control units includes the universe of economies. This selection keeps the weights assigned to each donor control as much data-driven as possible, hence ensuring the transparency of our study (Costalli et al., 2017). More importantly, and in contrast to Abadie et al., (2015) and Hope (2016), we cannot limit our controls to neighboring countries, which tend to have similar cultural, economic and social

[7]Matta et al., (2019) explain in detail the assumptions of SCM.

TABLE 1 Percentage difference in GDP by year—mass political instability events

	N	Mean	SD	Min	Max
$T-2$	32.0	0.1	2.6	−7.0	6.4
$T-1$	32.0	0.1	0.8	−1.4	3.8
T	32.0	−4.3***	4.7	−18.6	1.4
$T+1$	32.0	−6.7***	6.5	−23.6	3.0
$T+2$	32.0	−9.0***	7.6	−27.6	1.7
$T+3$	30.0	−10.6***	8.6	−30.3	3.9
$T+4$	29.0	−11.5***	9.3	−29.3	5.5
$T+5$	19.0	−13.5***	7.2	−25.9	−2.1

Source: Authors' own calculations. Notes: T denotes the year of the political instability event. We exclude Albania, Peru, Togo, Argentina, Venezuela, and Cote d'Ivoire from this sample because we could not find a reliable counterfactual. The values in the "Mean" column are the average of $Diff_{i,t}$ across the 32 countries. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

fundamentals to the treated one, because of spillover effects whereby political turmoil in a certain country might impact others within the same geographical region (Murdoch & Sandler, 2002).[8] We also exclude all control countries that experienced mass political turmoil and/or were impacted by an exogenous shock (natural disaster, war, etc.) during the corresponding post-treatment year.[9] For example, we omitted Arab Spring countries (Egypt, Tunisia, Libya, Yemen, etc.) as many Arab countries experienced some of the same political problems since 2011.[10] In addition, we impose a relatively long pretreatment period of 20 years, allowing us to have a robust synthetic control.[11] In many instances, countries initially impacted by political instability were later exposed to other distinct exogenous shocks. Hence, to avoid any double treatment problem, which will bias our results, our postpolitical instability assessment period will, in these cases, stop the year before the second exogenous shock has occurred.[12]

Our set of covariates consists of the following variables: investment, consumption, exports, imports (all as a ratio of GDP), which are the components of our variable of interest, real GDP per

[8]The cases of Bahrain and Ukraine are special as the structure of these economies are very similar to their neighbors (other GCC countries in the case of Bahrain and Russia in the case of Ukraine), meaning that we could not get reliable counterfactuals after omitting neighboring countries. However, the removal of Bahrain and Ukraine from our sample does not impact our main results.

[9]This is standard SCM procedure (Abadie et al., 2010). The reason for this is to get a pure estimate of the treatment effect that is not contaminated by any treatment of the control countries. The list of countries that were excluded from the set of controls corresponding to each political regime crisis that occurred with *(without)* mass civil protests can be found in Table OA.1.1 (Table OA.1.2) in the online appendix.

[10]This reflects the assumption of no interference between units, whereby the control group must not have experienced the event X (in our case political instability events), otherwise the results will be biased, as discussed by Abadie et al., (2010).

[11]In the cases of Chile, Haiti, Portugal, Spain, Estonia, and Ghana, we had to content ourselves with shorter pre-instability periods because the real GDP per capita series was not available for the whole 20 year-period time span.

[12]The case of Iran illustrates this point clearly. Iran was exposed, in 2009, to mass civil protests, in particular large demonstrations, in the wake of the presidential elections. Hence, we consider these events as a treatment in our analysis. However, three years later, the United States and the EU imposed additional sanctions on the financial and energy sectors which pushed the economy into a recession (International Monetary Fund, 2014). Consequently, if we apply SCM with 2009 being the treatment year without taking into account for the fact that in 2012 the Iranian economy was hit by another major shock, we would be overestimating the impact of the political instability. As a solution, we stop our postpolitical instability assessment period the year before the sanctions were imposed (i.e., 2011).

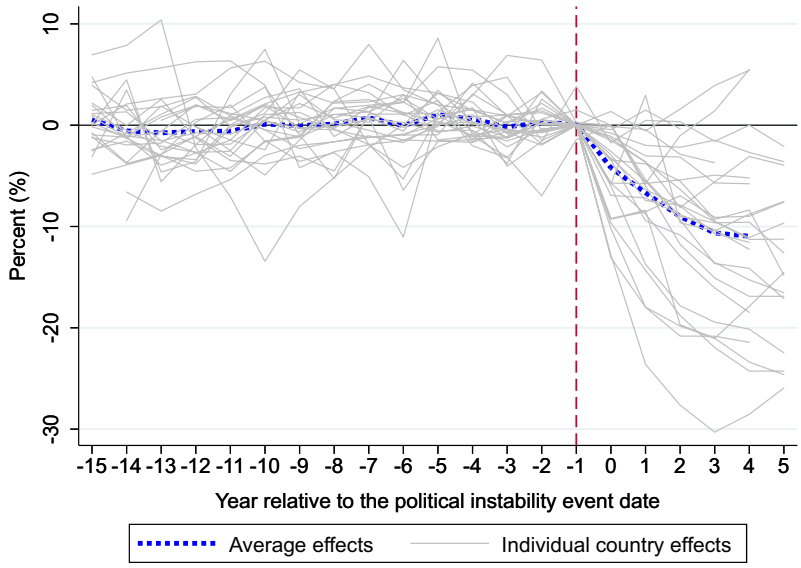

FIGURE 1 Percentage difference between actual and synthetic GDP per capita for countries that experienced regime crises with mass protests. Source: Authors' own calculations. Note: The gray lines represent the SCM results for individual countries that experienced regime crises with mass protests, while the blue dashed line represents the average results across all the countries

capita;[13] the percentage of secondary school enrollment, which is a key driver of economic growth (Barro, 1991); net fuel exports as a ratio of GDP, which captures a country's energy dependence (Matta et al., 2019);[14] money supply as a ratio of GDP, to control for the depth of the financial sector (Klein & Olivei, 2008); and the Polity2 score, to account for different institutional factors (Aidt & Leon, 2016; Huang, 2010) that might affect the economy (Acemoglu et al., 2001; Góes, 2016). For each of these variables, to avoid distortion from cyclical variation, we use ten-year average values rather than each annual observation.[15] We also add four-year averages of per capita GDP to our set of covariates. Nonetheless, because data for secondary school enrollment, net fuel exports, money supply and the polity2 index are not available for all countries at all periods, we do not include, in certain cases, all the covariates.[16] Table A.5 provides the definition, unit, and source of each variable.

[13]We also intended to control for fiscal policy using the fiscal balance, in addition to the variables that compose the supply side of GDP (industry, agriculture and services). However, data for these variables were missing for many countries in our dataset. In cases where data were available, we experimented with different sets of covariates and the results were almost unchanged.

[14]Net fuel exports (as a % of GDP) is the difference between fuel exports (as a % of GDP) and fuel imports (as a % of GDP). The definitions of these variables are available in Table A.4.

[15]In other words, we use the average value of years 1 to 10 for the first half of the pretreatment period, and the average of years 11 to 20 for the second half.

[16]This can be illustrated by the Haiti case which does not have data on net fuel exports; hence, we cannot include this variable in our set of covariates.

5 | ECONOMIC EFFECTS OF POLITICAL REGIME CRISES

5.1 | With mass civil protest

Figure 1 depicts the evolution of actual minus counterfactual real GDP per capita (as a percentage of the actual figure) in the 38 countries that experienced a regime crisis accompanied by mass civil protest (which is termed here a "mass political instability event"), together with the average across the 38 countries. The average is very close to zero for the pretreatment period, but turns markedly negative after the political crisis, with the gap increasing over time up to year $T + 5$, although with some signs of leveling off.[17] This implies not only that the immediate output loss is not recovered, but that output losses continue to accumulate over the subsequent five years.

While the lines plotted in the graphs are suggestive, our aim is to formally examine the impact of political turmoil on output, particularly whether the initial real GDP per capita losses are recuperated. Thus, to accurately measure economic recovery, we calculate the percentage difference (or output gap) between the actual and synthetic GDP per capita for each country as follows:

$$Diff_{i,t} = \left(\frac{A_{i,t} - S_{i,t}}{A_{i,t}} \right) \times 100 \text{ for } t \leq T + 5. \tag{3}$$

In equation (3), $A_{i,t}$ and $S_{i,t}$ represent respectively the actual and synthetic real GDP per capita for each country i, while t denotes the calendar year, and T is the event year during which political turmoil occurred. Table 1 reports the summary statistics of the percentage difference for each year from T - 2 to $T + 5$. In years T - 2 and T - 1, the mean percentage difference was small and statistically insignificant, which confirms the reliability of the constructed counterfactuals in mimicking per capita GDP of the actual countries prior to the mass political instability events. In the year of the event, the actual GDP per capita is, on average, 4.3 percent lower than its counterfactual.[18] Moreover, we find that during the next five years, the percentage difference increases gradually from 6.7 percent in year $T + 1$ to 13.5 percent in year $T + 5$. Although the gap appears to widen with time, we cannot reject the hypothesis that the difference stays constant over the years $T + 1$ to $T + 5$,[19] but even that would imply that the initial output loss was, on average, never recovered.

These findings are reminiscent of the output effect of other disruptive events such as currency crises, banking crises, and exceptionally deep recessions. Hong and Tornell (2005) find that, although GDP growth recovers to its "normal" rate on average by the second year after a currency crisis, the loss of output (relative to trend) in years T and $T + 1$ is never recovered. Cerra and Saxena (2008, p. 456) show that "the large output loss associated with financial crises and some types of political crises is highly persistent. Of the large negative shocks examined, a partial rebound in output is observed only for civil wars. Moreover, the magnitude of persistent output loss ranges from around 4 percent to 16 percent for the various shocks." Cerra et al., (2013) show that growth in the first year of recovery after a deep recession in which growth turns negative tends to be slower than after milder recessions and that this is particularly true of recessions associated with banking crises.

[17]For the graphs of the actual and synthetic GDP for each country, see Appendix Figure A.1.

[18]It should be noted, however, that the significance of *Diff* is exaggerated to the extent that it takes no account of uncertainty about the accuracy of the weights used in the counterfactual.

[19]The Wald test for joint significance of the recovery coefficients ($T + 1$, …, $T + 5$) had a p-value of 0.39.

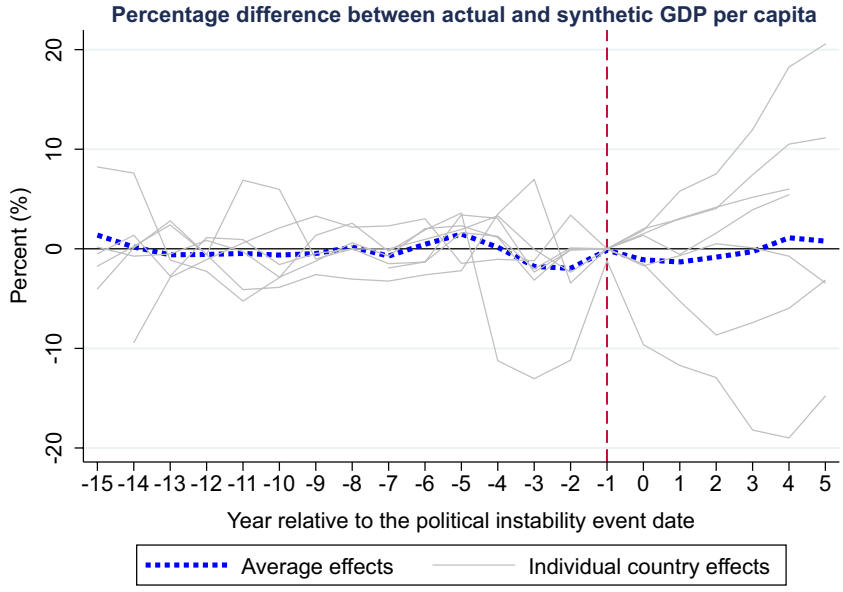

FIGURE 2 Percentage difference between actual and synthetic GDP per capita for countries that experienced regime crises without mass protests. Source: Authors' own calculations. Note: The gray lines represent the SCM results for individual countries that experienced regime crises with mass protests, while the blue dashed line represents the average results across all the countries

However, the differences in the means in Table 1 do not tell the whole story, as the trajectories of actual and synthetic GDP per capita are not uniform across countries. For instance, actual GDP per capita of Haiti fell sharply and then remained flat compared to its estimated counterfactual, whereas in Kyrgyzstan actual per capita GDP fell initially but bounced back to its counterfactual level four years later, and in Thailand, it was almost unchanged after the bloodless coup. Therefore, our methodology uncovers some degree of heterogeneity in economies' reactions to political instability events, hence improving on previous studies (Aisen & Veiga, 2013; Alesina & Perotti, 1996; Jong-A-Pin, 2009) that concealed these differences due to their use of panel regression techniques that only estimate the average treatment effect.

5.2 | Without mass civil protest

In this section, we test whether the combination of a regime crisis and mass civil protest is more economically damaging than when the regime crisis happens unaccompanied by mass protest. To test our conjecture, we apply the SCM used above on the set of nine countries listed in panel II of Table A.4 which, according to the CNTS database, experienced major changes in the political regime without being accompanied by mass civil protest. Figure A.2 below illustrates the path of the actual and counterfactual real GDP per capita for these countries, while Table A.7 in the appendix reports the covariate averages for the treated and synthetic countries together with the weights composing each counterfactual. It can be seen from Figures A.2 and A.3 that the estimated counterfactuals for Angola and Ghana do not closely mimic the corresponding actual per capita GDP prior to the treatment. Accordingly, we will exclude these two countries from the subsequent analysis to avoid possible biases.

A visual inspection of the graphs in Figure 2 indicates that the average impact of political regime crises not accompanied by mass civil protest is much more muted than when there is mass civil

TABLE 2 Percentage difference in GDP by year—regime crises without mass civil protest

	N	*Mean*	*SD*	*Min*	*Max*
$T-2$	8.0	−2.0	4.3	−11.2	3.4
$T-1$	8.0	−0.1	0.4	−1.2	0.0
T	8.0	−1.1	3.9	−9.6	2.0
$T+1$	8.0	0.1	3.7	−5.3	5.8
$T+2$	8.0	0.9	4.9	−8.7	7.5
$T+3$	8.0	2.3	6.4	−7.4	12.0
$T+4$	8.0	3.8	8.2	−6.0	18.2
$T+5$	6.0	3.7	10.3	−5.7	20.6

Source: Authors' own calculations. Notes: T denotes the year of the political instability event. We exclude Angola and Ghana from this sample because we could not find a reliable counterfactual. The values in the "Mean" column are the average of across the 8 countries.

protest. In particular, Table 2 confirms our observation as the estimated output gap between the actual and synthetic per capita GDP is, on average, only minus 1.1 percent during the event year but turns positive thereafter, in stark contrast with the 4.3 percent average drop in the case of mass political instability reported in Table 1. However, the large standard deviations suggest some degree of heterogeneity across countries that only experienced major changes in the political regime, similar to the result found in the cases of mass political instability.

We statistically test our proposition that political regime crises are more damaging to an economy when they are accompanied by mass civil protest (riots, strikes, or demonstrations) by simply regressing the estimated output gap ($Diff_{i,t}$) on a dummy variable that equals 1 if a country experienced a mass political instability event and 0 if it was subjected to a political regime crisis only. The results of that test are reported in Table 3. The estimated coefficient on the mass civil protest dummy is negative and highly significant, hence supporting our claim that in the absence of mass civil protest, the economic effects of a significant change in the political regime are much smaller.

6 | SIGNIFICANCE OF THE RESULTS

The SCM procedure generates a point estimate of the path of output of the counterfactual in each year, and not a confidence interval. However, it is possible to do some comparisons of the postcrisis evolution of the estimated percentage difference between actual and synthetic GDP ($Diff_{i,t}$) with the precrisis difference. For each country, we regress $Diff_{i,t}$ on two dummy variables: the first ($Event_{i,t}$) takes the value 1 only in the year of the political instability event (year T), while the second ($Post_{i,t}$) takes the value 1 in each of the subsequent five years only (years $T+1$ to $T+5$). In other years, both variables are zero. More specifically, we estimate the following regression for each of the countries that suffered from major political regime crises:[20]

$$Diff_{i,t} = c + \beta_1 . Event_{i,t} + \beta_2 . Post_{i,t} + \varepsilon_{it}. \quad (4)$$

[20]38 were accompanied by mass civil protest, while 10 were not. As mentioned earlier, we exclude from our sample Peru, Togo, Ghana, and Niger, because the SCM did not yield suitable counterfactuals that accurately measure how the real GDP per capita of these countries would have performed in the absence of political instability, hence not satisfying the parallel trend assumption of DiD.

The regression sample starts from the beginning of the dataset and ends five years after the political instability event in each case, so data after year $T + 5$ are not used. The intercept c is a measure of the average difference before the event. Our coefficients of interest are β_1 and β_2: the former can be interpreted as the economic impact of political turmoil during the instability year, while the latter captures the average effect in the next five years. In particular, a significantly negative β_1 implies that the economy lost output because of political instability, while a significantly negative β_2 suggests that, on average, the initial adverse impact is persistent over time (i.e., there is a less than full recovery compared to the pre-instability period). If β_2 is less negative than β_1, that indicates some recovery of the output lost in period T, whereas if it is more negative, there is further output loss after period T.

Table 4 reports the estimation results for each country: the point estimates of β_1 and β_2, their t-statistics and the number of observations. Panel I contains the findings for the countries that experienced major political crises with mass civil protest, and the findings for those without MCP are shown in Panel II. In Panel I, out of the 32 cases, 23 (71.9%) had a significantly negative β_1 coefficient, implying that these countries were considerably damaged by mass political instability during the event year. Moreover, in 25 cases (78.1%), the β_2 coefficients are significantly negative, indicating that the actual GDP per capita did not fully recover its initial output loss. In panel II, on the other hand, out of eight cases, only Australia, Ecuador, and The Gambia had significantly negative β_1 and only two (Australia and The Gambia) had significantly negative β_2. In summary, these findings imply that the negative effects of political regime crises estimated by SCM are typically statistically significant if accompanied by mass civil protest, whereas in the absence of such protests, the adverse effects are much more muted.

Our results differ from those of other authors, because of different measures of political instability and also because others do not use SCM. As discussed in Section Two, Jong-A-Pin (2009) uses four measures of political instability. He estimates a GMM model of growth rates of real per capita GDP, using six non-overlapping five-year periods from 1974 to 2003. He finds that political regime crises have a significant negative effect on growth, but that mass civil protests have no effect. Aisen and Veiga (2013) estimate a similar panel regression with five-year averages of growth over the period 1960–2004. Their principal measure of political instability is what they term "cabinet changes" (the number of times per year that the head of government or more than 50% of the cabinet changes). They find that such cabinet changes, as reported in the CNTS data base, have a significant negative effect on growth. They do not test for any effects of mass civil protest. These studies differ from ours in the following major ways: (1) by using an econometric method that includes country fixed effects, they are comparing growth only with the country's

TABLE 3 Regression of percentage difference in GDP on a mass civil protest dummy

	Dependent variable: $Diff_{i,t}$					
	T	$T+1$	$T+2$	$T+3$	$T+4$	$T+5$
Mass civil protest	−4.146*** (1.176)	−5.940*** (1.685)	−8.838*** (2.410)	−11.161*** (3.076)	−13.118*** (3.703)	−14.249*** (5.172)
Constant	−0.407 (0.731)	−0.282 (1.255)	1.243 (1.870)	2.330 (2.297)	3.798 (2.810)	3.674 (4.185)
Observations	35	35	35	33	32	22

Source: Author's own calculations. Notes: Bootstrapped standard errors in parentheses. *, **, *** indicate significance levels of 10%, 5%, and 1%, respectively.

own growth in other periods and not with comparable other countries in the same period; (2) they do not explicitly test for any lagged effects on growth, so they tell us nothing about the recovery process (in their models there is no difference between an immediate postcrisis period and a pre-crisis period); and (3) they do not make any adjustment for exactly when in a five-year period the instability starts; if it starts close to the end of the period, then much of the growth effect might appear in the subsequent five-year period rather than the current one. In an earlier paper, Alesina et al., (1996) find that growth is negatively correlated with significant changes of government, using annual data from 1960 to 1982. They do not use country fixed effects but they do include regional dummies. They also do not include a lag of their government change measure, so they cannot test whether the estimated growth effects are persistent or to some extent reversed in later periods.

We may use the data in Table A.4 to estimate whether different types of regime instability and MCP make a difference. Table 5 compares the estimated output effects of (1) no MCP; (2) MCP that is only demonstrations and/or riots, but not strikes; (3) MCP that includes strikes; (4) regime

TABLE 4 Time series regressions for individual countries

	β_1		β_2		Obs
Country	**Coeff.**	***t*-stat**	**Coeff.**	***t*-stat**	
		Panel I — with mass popular protests			
Bahrain	−2.18	−6.66***	0.58	0.5	23
Bolivia	−2.93	−6.67***	−9.14	−11.96***	27
Chile	−9.34	−12.99***	−16.43	−7.1***	19
Egypt, Arab Rep.	−2.47	−13.53***	−8.36	−7.44***	26
Fiji	−2.37	−2.74**	−1.20	−1.19	22
Georgia	2.01	0.6	−14.94	−2.35**	17
Guinea	−2.60	−8.6***	−11.98	−6.14***	27
Haiti	−9.59	−24.28***	−20.65	−10.36***	12
Honduras	1.37	1.44	2.61	1.67	19
Iran, Islamic Rep.	−0.44	−0.38	0.57	0.42	24
Italy	−0.70	−2.64**	−2.31	−5.33***	27
Jordan	−6.01	−10.51***	−14.35	−7.13***	26
Kenya	−4.28	−6.18***	−9.23	−4.75***	22
Kyrgyz Republic	−4.68	−3.25***	1.06	0.41	24
Morocco	−2.33	−4.28***	−5.64	−6.33***	26
Nigeria	0.62	0.41	−5.36	−2.67**	27
Pakistan	−2.68	−3.2***	−9.78	−5.96***	27
Paraguay	−6.19	−9.36***	−18.92	−12.36***	27
Philippines	−12.90	−33.33***	−27.29	−23.71***	27
Portugal	−7.78	−7.43***	−6.75	−6.01***	21
Spain	−3.28	−4.93***	−12.97	−9.05***	24

(Continues)

TABLE 4 (Continued)

Country	β_1		β_2		Obs
	Coeff.	*t*-stat	Coeff.	*t*-stat	
Thailand	0.70	0.53	−1.53	−0.97	25
Tunisia	−6.81	−25.03***	−9.53	−9.24***	26
Turkey	−1.50	−2.27**	−4.88	−4.48***	27
Ukraine	−1.96	−0.95	−4.09	−1.17	17
Yemen, Rep.	−18.78	−50.38***	−20.74	−53.75***	24
Bulgaria	−5.65	−8.57***	−6.55	−2.67**	23
Greece	−10.14	−12.58***	−20.20	−18.74***	26
Indonesia	−12.47	−21.61***	−20.77	−16.24***	26
Latvia	1.43	1.1	−3.89	−2.97***	19
South Africa	−0.35	−0.74	−13.67	−6.79***	26
Algeria	0.06	0.05	−9.18	−5.76***	18
Panel II — without mass popular protests					
Australia	−2.76	−4.36***	−4.80	−5.9***	21
Ecuador	−1.70	−5.39***	−0.84	−1.16	27
Estonia	1.73	1.89*	12.71	4.11***	13
Gambia, The	−1.86	−2.4**	−6.42	−5.44***	27
Malaysia	1.78	3.05***	6.99	4.19***	27
Niger	5.19	5.36***	11.37	5.48***	27
Slovak Republic	3.82	4.17***	6.91	6.35***	24
Ireland	1.37	1.44	2.61	1.67	19

Source: Author's own calculations. Notes: Robust standard errors are used. The Coeff. and *t*-stat are the estimated coefficients and *t*-statistics for β_1 and β_2 resulting from the regressions in equation (4). We remove Albania, Peru, Togo, Argentina, Venezuela, Cote d'Ivoire, Angola, and Ghana because they did not have good counterfactuals. ∗, ∗∗, ∗∗∗ indicate significance levels of 10%, 5%, and 1%, respectively.

instability that consists of only one element (usually government changes); and (5) regime instability that consists of at least two of government changes, constitutional changes, and coups. To take account of the differing degrees of uncertainty about the estimated output effect in each country, we weight each coefficient by the inverse of its standard error (in other words, we use the t-statistics rather than the point estimates of the coefficient). We also omit Yemen, which is a large outlier, giving us 39 countries in all.

The first row of Table 5 shows that with no mass civil protest, there is no significant fall in output, either in year T or subsequently. With MCP excluding strikes, the average t_1 is −6.95 and the average t_2 is nearly as large, at −6.29. With MCP including strikes, the average t_1 is even more negative, at −8.63, but the average t_2 is much the same as without strikes, at −6.19. Turning to different types of regime instability, we see from Table 5 that the output effects are slightly larger with two or three indicators of instability than just one (the t_1 average is −6.80 compared with −5.47, and the t_2 average is −4.73 compared with −4.60). It is clear from Table 5, therefore, that what is associated with a larger negative output effect is whether there is mass civil protest, rather than the type of protest or the type of regime instability.

TABLE 5 Average effects on output in year T and years $T + 1$ to $T + 5$

	Number of countries	t_1	t_2
No mass civil protest	8	0.47	0.22
MCP without strikes	19	−6.95	−6.29
MCP with strikes	12	−8.63	−6.19
Mild regime instability	25	−5.47	−4.60
Intense regime instability	14	−6.80	−4.73

Source: Author's own calculations. Notes. "t_1" is the coefficient of the dummy variable that is equal to 1 in year T, divided by its standard error, averaged over the number of countries in that category; "t_2" is the coefficient of the dummy variable that is equal to 1 in years $T + 1$ to $T + 5$, divided by its standard error, averaged over the number of countries in that category. The figures in Table 5 are based on the numbers in Table 4, with Yemen omitted. "Mild regime instability" means only one "yes" in Table A.4, and "intense regime instability" means more than one "yes."

7 | CONCLUSION

We have used synthetic control methodology to estimate the effects of certain types of political instability on the path of output up to a five-year horizon. In particular, we have focused on regime crises and found a significant difference between crises accompanied by mass civil protest and those where mass civil protest was absent. We identified these events based on the 2015 Cross-National Time database and Jong-A-Pin's (2009) classification of different dimensions of political instability, and we were careful to filter out cases where other types of disruptive events such as economic crises and armed conflicts occurred during the relevant period.

Our unambiguous finding is that regime crises accompanied by mass civil protest result, on average, in a significant fall in output relative to the synthetic counterfactual that is not only not recovered over the subsequent five years, but even tends to increase. This is similar to the output effects of other major disruptive events, such as an exceptionally deep recession or a currency crisis that have been found in previous research. Analysis of a somewhat smaller sample of regime crises where mass civil protest was absent tends to show that there are no such negative output effects in these cases. These results are substantially different from those previously obtained by different methodologies, such as panel growth regressions. More importantly, our empirical approach also uncovers a high degree of heterogeneity in economies' reactions to political regime crises (with or without mass civil protest).

There are several potential mechanisms through which mass civil protest makes the impact of regime crises worse. One possibility is that the protests themselves have direct negative effects (e.g., output loss due to strikes or closures of business). However, it is not obvious why such effects would persist over time. A second possibility is that mass protests tend to be associated with more severe and profound regime crises such as full-fledged revolutions. Here, we are limited by our data, which does not include a measure of severity of the regime crisis. A third related possibility is that mass protest tends to signify events that cause a more profound increase in political uncertainty. The prospect of people coming onto the streets again in the future may introduce a new, potentially uncontrollable element of uncertainty about a country's future direction and thus have a more negative effect on business confidence and investment. In the theory of Cederman et al., (2009), political conflict often occurs between ethnic groups in the governing coalition and excluded but politically organized ethnic groups who choose their strategy according to circumstances (for instance, smaller groups are less likely to initiate armed conflict because of their lower probability of success). To the extent that mass civil

protest is not purely spontaneous but is fostered by opposition groups, it may signal a more profound contest for political power. Further research is required to distinguish between these hypotheses, and more generally whether mass civil protest is a cause of greater output losses or merely a symptom of other causative factors such as the intensity of the political crisis.

ACKNOWLEDGMENT

The authors wish to thank the Editor, Pablo Pinto, and two anonymous referees for very helpful comments on a previous version. Any errors that remain are of course the authors' responsibility. The views expressed are those of the authors and should not be interpreted as those of the World Bank.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available in the supplementary material accompanying this article.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section.

How to cite this article: Matta, S., Bleaney, M., & Appleton, S. (2021). The economic impact of political instability and mass civil protest. *Economics & Politics*, 00, 1–18. https://doi.org/10.1111/ecpo.12197