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How does expropriation affect FDI? A synthetic control analysis of oil and gas sector nationalizations in South America

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Abstract

How do expropriations of foreign oil and gas assets affect the net inflow of FDI? We analyze political and legal developments which led to increased government control of natural resource extraction industries in South America in the early 2000s and discuss at which point in time foreign investors saw legislation as violating their legitimate property rights. We use synthetic control methods (SCM) to date expropriations and quantify their effect on FDI inflows in subsequent years. Strongly negative and statistically significant effects are found for Bolivia and Venezuela, with similar, but less conclusive evidence for Ecuador. SCM approaches which focus on structural characteristics and put little weight on pre-treatment outcomes are better equipped to detect the true “treatment” date than canonical SCMs. This is shown for Argentina where the 2012 nationalization of Repsol hardly affected FDI still down from the reputational damage inflicted by Argentina’s 2001 sovereign default.

Keywords: Expropriation, FDI, synthetic control method

JEL: F21, H13

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I. Introduction

Commercial extraction of natural resources like oil and gas is capital-intensive and requires advanced technological skills. Investment in such activities is risky, but, if successful, it can generate high and long-lasting economic rents. Against this background, there are two basic possibilities for the government of a country rich in reserve resources: Either grant licenses to private (and often foreign) corporations under a system of taxes and royalties or have resources extracted by state-owned companies under direct government control.

Exploitation by foreign companies may be politically controversial. Parts of the profits are repatriated which many voters may find inappropriate as they feel that the resources belong to the people and should be spent on improving their living conditions, cf. Rees (2017). Proponents of private investment, however, argue that domestic governments have more fiscal space for social expenditure or productive investment elsewhere in the economy if private companies finance the capital for resource extraction on their own account. They also emphasize that foreign firms bring an inflow of capital and technological knowledge to the country and that they bear considerable risk of losses against which a government should try to insulate itself, cf. Martin (1999).

Not surprisingly, the preferred way to manage natural resources may change over time. This can easily happen in democratic societies or when societies go from democratic to autocratic rule or vice versa. Over the course of the last century, Latin American countries, for instance, have gone through several episodes of nationalization and de-nationalization of major corporations in their oil and gas industries, see Berrios et al. (2011): These culminated in a tide of privatizations in the 1990s, but Argentina, Bolivia, Ecuador and Venezuela initiated measures to reverse these decisions once more in the early 2000s.

Important as it may be in many developing or middle-income countries, the natural resource sector is not the only sector in need of investment. Most governments – and often even those which strive for more control of their natural resource industries – aim to encourage foreign direct investment (FDI) in the non-resource private sector of their countries. But a decision to expropriate corporations active in resource extraction may shy away foreign investors. Not only may such investors worry about property rights, but they may also fear that nationalizations in the resource sector are indicative of more interventionist economic policies in general.

In this paper, we take Bolivia's experience with the 2006 nationalization of its oil and gas sector as a natural (i.e. quasi-) experiment. The sector accounts for about 8% of Bolivia's GDP. We study to what extent *economy-wide* FDI was affected by changes in national economic policies of which the expropriations in the hydrocarbon sector were the most prominent examples. For this purpose, we use the Rubin causal model (cf. Rubin (1974), Holland (1986)) and apply modern synthetic control methods (e.g. Abadie et al. (2010), Malo et al (2020)) to estimate the counterfactual development of total Bolivian FDI under unchanged government policies.

There is a vast literature on expropriations and their possible effects on economic welfare. We survey only a few, representative publications to put our work into perspective. Early papers by prominent economists are Bronfenbrenner (1955) who argues that expropriations may foster economic development and Tobin (1974) who sketches a theoretical model which allows for both beneficial and detrimental overall effects of expropriations. Neither author analyses retaliation in the form of reduced future capital inflows. Formal models by Garnick (1963) and Eaton and Gersovitz (1984) close this gap.

Geiger (1989) seems to be the first attempt to study the effects of expropriations on foreign capital flows. In his dummy variable analysis of Latin American countries, he finds few significant effects of major expropriations, but, unfortunately, his handling of time series data is seriously flawed. Later studies took a much more structural approach building on previous theoretical work which emphasized the objective function of the government (Picht and Stüven (1991)), quality of institutions (Tornell and Velasco (1992), Aguiar et al. (2009)), optimal contracts (Thomas and Worrall (1994), Semmler (1994), Engel and Fischer (2010), Rigobon (2010)) or political economy considerations (Manzano et al. (2008), Aguiar and Amador (2011)). Recently, Melek (2020) argued that nationalizations reduce labor productivity due to a withdrawal of skilled foreign labor which, in turn, may make future FDI less attractive.

But as Akhtaruzzaman et al. (2017) correctly point out, the large body of empirical work aimed at identifying the relative importance of such factors for external capital flows did not converge to any consensus view. In fact, it can be divided into two groups of papers with basically opposing conclusions. One group of studies, e.g. Brunetti and Weber (1998), Harms and Ursprung (2002), Li and Resnick (2003), Daude and Stein (2007), Ali et al. (2010), Asiedu and Lin (2011), Akhtaruzzaman et al. (2017), Azzimonti (2018), finds that the risk of expropriation is an important determinant of foreign capital flows. The other group, e.g. Asiedu (2002), Jensen (2003), Bénassy-Quéré et al. (2007), Busse and Hefeker (2007), Naudé and Krugell (2007), Alfaro et al. (2008), Sanchez-Martin et al. (2014), reports results which suggest that factors like democratic accountability, corruption or regulatory complexities are much more important for FDI than contract enforcement and property rights. Méon and Sekkat (2012) claim that the risk of expropriations is often not properly taken into account when there is a global wave of FDI.

One important reason for the conflicting results is certainly the covariance structure of the variables. We do not observe the world *ceteris paribus* – recording FDI and expropriations, all other things being equal. But given the inconclusive results from many large panel studies, precisely this would be desirable. This is why we resort to synthetic control analysis. To isolate the effect of expropriation, we define a counterfactual of the outcome variable FDI which differs from observed FDI after “treatment” (i.e. expropriation) only through the absence of expropriation.

The rest of the paper is organized as follows: In section II, we briefly describe the synthetic control approach and the method employed to estimate the counterfactual. In section III, we review the history of expropriations and privatizations of natural resource extraction in Bolivia (selected Latin American countries). Section IV describes the data, reports the estimation results, and discusses implications and limitations. Section V concludes.

II. The synthetic control approach

The synthetic control (SC) approach is widely known today and many papers exist which describe the methodology in detail and with great insight, e.g. Ferman and Pinto (2016), Athey and Imbens (2017). So, we will be brief here.

Suppose we are interested in a variable, e.g. FDI, for unit i in period t . There are two potential outcomes for this variable, depending on whether a certain treatment (e.g. expropriation in the resource sector) has been applied. Denote the potential outcome in the case of treatment by y_{it}^{Tr} and in the case of non-treatment by y_{it}^N . By definition, the potential outcomes differ only by the effect of treatment, hence

$$\tau_{it} := y_{it}^{Tr} - y_{it}^N$$

is the causal effect of treatment. For each unit i and period t we observe either $y_{it} = y_{it}^N$ or $y_{it} = y_{it}^{Tr}$.

Suppose further we observe $J+1$ units in $T = T_0 + T_1$ periods of time. Treatment occurs in period $T_0 + 1$ and only for unit 1. So, we have J (untreated) control units. For unit 1, we observe $y_{1t} = y_{1t}^N$ for $t \leq T_0$ and $y_{1t} = y_{1t}^{Tr}$ for $t > T_0$. Let us collect the pre-treatment observations for unit 1 in a $T_0 \times 1$ vector y_1^{pre} and the post-treatment observations in a $T_1 \times 1$ vector y_1^{post} . Analogously, define $T_0 \times J$ and $T_1 \times J$ matrices Y_0^{pre} and Y_0^{post} for the observed outcomes of the controls. Synthetic control analysis aims at approximating the counterfactual potential outcomes y_{1t}^N , $t > T_0$, as a linear combination of the observed outcomes for the controls in the same time period. Denoting the vector of post-treatment counterfactuals of unit 1 by $y_1^{N,post}$ we would need to find nonnegative weights w^* which satisfy

$$y_1^{N,post} \approx Y_0^{post} w^* \quad (1)$$

The weights are required to add up to one, i.e. $w^* \in \Delta_J := \left\{ w \in \mathbb{R}^J \mid \sum_{i=2}^{J+1} w_i = 1, w_i \geq 0 \ \forall i \right\}$.

Usually, we will observe not only outcomes but, generally, R variables z_i useful in predicting y_{it}^N . These variables are called ‘‘predictors’’ and must be unaffected by the treatment, e.g. because they were determined prior to treatment. The predictors may thus include pre-treatment outcomes or other variables with predictive power (covariates). Let z_1 be the predictors for unit 1 and collect the predictors for the control unit in the $R \times J$ matrix Z_0 .

Abadie, Diamond and Hainmueller (2010) (henceforth ADH) propose to estimate the optimal weights w^* by solving the following optimistic bilevel minimization problem:

$$\min_{v \in \Delta_R, w \in \Delta_J} L_{up}(v, w) := \frac{1}{T_0} (y_1^{pre} - Y_0^{pre} w)' (y_1^{pre} - Y_0^{pre} w) \quad (2)$$

s. t.

$$w \in \Psi(v) := \operatorname{argmin}_{w \in \Delta_J} L_{low}(v, w) := (z_1 - Z_0 w)' \operatorname{diag}(v) (z_1 - Z_0 w)$$

This formulation is due to Malo et al. (2020). It emphasizes that the optimal weights w^* and the weighting matrix $V := \operatorname{diag}(v)$ are determined jointly by the interaction of two minimization problems: The ‘‘lower problem’’ determines w as a minimizer of L_{low} for a given choice of V . Accordingly, the ‘‘upper problem’’ recognizes that w depends on V and minimizes L_{up} by an appropriate choice of V so that $w = w(V)$. To solve the problem, ADH provided a computer code ‘‘Synth’’ which became very popular in recent years.

Unfortunately, subsequent research has pointed out numerous problems with this approach. For example, Ferman and Pinto (2016) show that a key assumption necessary for consistent estimation of w does not hold asymptotically and Lucke (2022) shows that the estimator is not identified at the optimum. On the numerical front, Malo et al. (2020) and Kuosmanen et al. (2021) demonstrate that ADH's Synth-algorithm suffers from non-uniqueness of the optimal solution (the identification issue) and may often not converge to a global optimum. Alternative algorithms developed by these authors also suffer from severe problems so that Kuosmanen et al. (2021) advise against the joint determination of w and V . Rather, V is recommended to be set exogenously, e.g. by using regression methods to fix predictor weights before finding the optimal w .

Abadie and L'Hour (2021) have followed precisely this route. They treat V as exogenously given so that (2) simplifies to

$$\min_{w \in \Delta_J} L_{low} := (z_1 - Z_0 w)' V (z_1 - Z_0 w) \quad (3)$$

Note that the diagonal elements of V are the weights of the predictors and therefore determine which predictors are to be used and how much importance is assigned to them. For instance, whether pre-treatment outcomes are to be used as predictors depends on whether pre-treatment outcomes are assigned positive weights on the diagonal of V .

Again, however, the synthetic control estimator, i.e. the vector of weights $w^* \in \Delta_J$ which solves (3), may not be unique. Abadie and L'Hour therefore suggest a "penalized synthetic control estimator" as the solution of

$$\min_{w \in \Delta_J} L_{pen} := \mu (z_1 - Z_0 w)' V (z_1 - Z_0 w) + (1 - \mu) \sum_{j=2}^{J+1} w_j (z_1 - z_j)' V (z_1 - z_j), \quad (4)$$

where $\mu \in (0, 1)$. This penalized estimator combines the objective function of a synthetic control estimator (with weight μ) with the objective function of a nearest neighbor matching approach (with weight $1 - \mu$). Setting $\lambda := (1 - \mu) / \mu$ we can equivalently minimize $\mu^{-1} L_{pen}$ and interpret (4) as a problem in which solutions for the standard synthetic control estimator (corresponding to the minimum of the first term) are penalized by λ times a measure of how distant each control predictor is from the predictor of the treated unit.

Abadie and L'Hour prove that the solution of (4) is unique for any $\mu \in (0, 1)$. Hence, while the solution of (3) suffers from non-uniqueness, setting μ to a value close to but slightly smaller than one (i.e. a very small penalty term) results in a perfectly identified estimator which is an approximate solution to the pure synthetic control problem (3). For this reason, we will, in the following, always solve (4) with $\mu = 0.99$ rather than solve (3). For ease of language, we still call it the synthetic control estimator (SCE)².

² Note that Abadie and L'Hour take a different approach: They suggest two data-driven methods to determine λ . Therefore, their penalized SCE is not necessarily close to either the standard SCE or the nearest neighbor matching. This may also be a useful approach, but given the popularity of Synth and the SCM method, we prefer to present our results in a closely comparable setting.

III. Nationalizations and privatizations of natural resource extraction in Latin America

Hajzler (2012), building on earlier work by Minor (1994), documents expropriations across all developing and middle-income countries in the period 1989-2006. Except for Yemen, nationalizations in the oil and gas sector occurred only in Latin America and in countries which were formerly part of the Soviet Union. Since FDI in the latter group of states is highly irregular in the years after the breakdown of communist rule and external capital flows reflect special conditions of transition economies, data from these countries cannot be used as either treated units or controls. Hence, we confine our attention to expropriations in Latin America.

Four Latin American countries expropriated private companies active in oil and gas extraction since 1989 (and thereby reversed former privatizations): Argentina (2012), Bolivia (2006), Ecuador (2006) and Venezuela (2001). All four countries had a history of nationalizations of their hydrocarbon industries, Argentina also expropriated in 1922, 1924 and 1930, Bolivia in 1937 and 1969, Ecuador in 1972 and 1974, and Venezuela in 1976. Against this background, it is perhaps more remarkable that other Latin American countries which actively expropriated in the first half of the 20th century (Brazil, Chile, Colombia, Mexico, Uruguay), abstained from such action since 1953, cf. Berrios et al. (2010).

Note that “expropriation” does usually not mean that the government seizes the assets of a firm completely and without any compensation. We define “expropriation” as any set of legal measures initiated by a government to gain control (i.e. more than 50% of voting rights) of a company for a price less than what the government would have had to pay prior to changing the law. According to this definition, a mere tax or royalty increase on extraction activities would not be “expropriation”, as seizing a higher share of company profits would not yet put the government in control of the company. However, a sharp tax increase plus a law requiring the private owners to sell at least 50% plus one share to the government, would be “expropriation” even if eventually the private owners reach an agreement on compensation with the government and refrain from seeking arbitration. For in such a case (which is similar to what happened in Venezuela in 2001, Bolivia in 2006 and Argentina in 2012³) it is obvious that the government was seeking control of the company and left owners little prospect of any more favorable settlement than at the terms which the government offered after reducing the present value of future profits through tax legislation.

Presumably, the details of how expropriations are shaped are less important for FDI than the general sentiment of international investors that property rights are not fully respected in a certain country. It is the political risk (joint with weak institutions in defense of property rights) that may affect FDI. Observing any particular expropriation raises the prospect that the government of the host country may on other occasions, in other markets and with other means take actions which significantly and selectively reduce the monetary value of the investor’s assets.

Hence, timing matters. Argentina, Bolivia, Ecuador and Venezuela had not expropriated any extraction company since 1976 and all except Ecuador had significantly privatized their hydrocarbon sectors in the 1980s and 1990s⁴. In order to analyze how expropriations affect

³ There was no tax hike in Argentina. Rather, the government announced that the Spanish owner Repsol would be held liable for environmental damage caused by the extraction company YPF in various regions of the country. See Costamagna et al. (2014) for details.

⁴ Ecuador’s privatization efforts were ultimately unsuccessful due to multifaceted political resistance. The country always had a mix of private and state-owned extraction companies, though. The 2006 expropriation of US-based Occidental Petroleum, the largest extraction company active in the country, significantly increased government

foreign capital flows, it is essential to determine when investors may have learned about an intended policy reversal tied to a greatly increased risk of expropriations.

For Argentina, the timing is relatively easy. President Cristina Fernández de Kirchner gave a speech on April 16th, 2012, in which she announced that the Spanish company Repsol S.A. would be expropriated to the effect that 51% of the shares of Argentina's dominant energy company YPF S.A. would be owned by the government and provinces of Argentina. This reversed Repsol's 1998/99 acquisition of almost all YPF shares in the wake of former President Carlos Menem's 1992 privatization initiative. Although some signs of a harder government line had already surfaced a couple of months earlier, President Kirchner's speech took Repsol and much of Spain's business community by surprise (Costamagna et al. (2014)). So, in synthetic control language, the date of the "treatment" is 2012.

The case of Bolivia is more difficult. In 1985 and in response to one of the worst economic crises in Bolivian history, President Victor Paz imposed presidential decree No. 21060 as the foundation of what was later called the New Economic Policy (NEP). With this policy (for details see Malloy (1991)) Bolivia committed (amongst many other measures) to large-scale privatizations of state-owned enterprises. One of these enterprises was energy monopolist YPFB, S.A. (Yacimientos Petrolíferos Fiscales Bolivianos) whose upstream and downstream activities were organized in three companies (Chaco S.A., Andina S.A., Transredes S.A.). A majority of their shares was sold to foreign investors in 1996 while YPFB was reduced to its supervisory functions.

Social conflicts about economic policies, gas usage and coca eradication policies culminated in the Bolivian "gas war" in September 2003, followed by the imposition of martial law in the next month. President Carlos Mesa proposed that the issue of gas nationalization was to be decided in a referendum held in July 2004. Almost 90% of the voters were in favor of nationalization. However, the Hydrocarbon Laws passed by the Bolivian Congress in 2005 in at least partial compliance with the results of the referendum were widely criticized as insufficient to ensure nationalization, which, along with shortages in fuel supply, reignited the protests. These set the stage for the eventual resignation of President Carlos Mesa and the election of President Evo Morales who assumed office in January 2006. Four months later, Morales signed Supreme Decree 28701 which provided for the transfer of more than 50% of the shares of the private hydrocarbon extraction companies to state-owned YPFB.

From the point of view of foreign investors, the likelihood of such an event had continuously increased over the three years 2004–2006. This makes it hard to fix a single year for "treatment". At the latest, expropriation as defined above occurred with Decree 28701 in May 2006, for which drastic increases of royalties (part of the 2005 Hydrocarbon Laws) had prepared the ground. However, anticipation of such steps may well have existed already in 2004. In our analysis, we take the view that the results of the 2004 referendum were already of key significance to investors (and probably induced a wait-and-see attitude which contributed to the fuel shortages which sparked the protests in 2005). We will, however, also provide an analysis under the assumption that "treatment" occurred only in 2006.

In 1999, the government of Ecuador, through its national oil company PetroEcuador, granted US oil giant Occidental Petroleum Corp. the right to explore, develop and exploit Block 15 of the Amazon Rainforest. A year later, in an attempt to diversify risk and attract additional capital,

control of the sector. After arbitration by the International Center for Settlement of Investment Disputes, Ecuador agreed in 2016 to compensate Occidental with close to \$1 bn.

Occidental sold 40% of its economic rights to Alberta Energy Corp. (AEC), failing to obtain the necessary ministerial approval for this transaction. In subsequent years, Ecuadorian President Alfredo Palacio was pressured by indigenous groups to expel Occidental from the Amazon. In April 2006, Ecuador enacted Law 42 requiring all foreign oil companies to hand over 50% of their “windfall revenues” to the government. One month later (and about two weeks after Bolivia had nationalized its hydrocarbon industries), the government issued the “Caducidad Decree” which canceled the contract with Occidental and ordered the company to turn over to PetroEcuador all its assets in Block 15. The assets were seized by the government the next day.

In the ensuing legal tangles at the International Centre for Settlement of Investment Disputes (ICSID), Ecuador argued that its measures did not constitute expropriation but were the legitimate reaction to the failure of Occidental to obtain ministerial consent for the sale of the 40% interest to AEC six years earlier. However, in two rulings in 2012 and 2019 the ICSID dismissed this argument. It ruled that the government’s actions were not proportional and were tantamount to expropriation. We follow this opinion and set 2006 as the treatment date.

The last case to be considered is Venezuela. Like many others, this country had opened its oil sector to foreign investments in the 1990s, a policy which became known as the “Apertura Petrolera”. Hugo Chavez, in his 1998 presidential campaign, vowed to review national oil policies, but reassured foreign investors after his inauguration that his government would respect existing contracts. In 1999, President Chavez even enacted the Organic Gas Law which liberalized Venezuela’s gas sector and encouraged private investment. Also, a referendum in 1999 approved a new constitution which, while containing strong language on state ownership and government management rights for Venezuela’s hydrocarbon resources, was seen as more open to private investment than the previous constitution since the question of private associates to state-owned *Petróleos de Venezuela S. A. (PdVSA)* had been deferred to legislation, cf. Rentner (2004).

President Chavez, using extraordinary powers conferred on him by the National Assembly, enacted a new Organic Hydrocarbons Law (OHL) in 2001. Article 22 of this law requires that the state holds more than a 50% participation in all future oil projects. This provision effectively made private investment impossible since state-owned PdVSA lacked sufficient capital to fund its 50% shares in new oil and gas projects (Eljuri and Tejera Perez (2008)). Article 42 raised the royalties to be paid by existing producers from 16.7% to 30% of revenues. Clearly, the economic effect of Article 42 was a sizable loss in the net present value of private capital in the hydrocarbon sector, reinforced by a separate reform of the corporate income tax law which raised the de facto rate from 34% to 50%⁵.

The Organic Hydrocarbons Law entered into force in 2002. But it took the National Assembly four more years until, in March 2006, the terms for the creation and operation of joint ventures with public majority of capital were finalized. Soon after, in April 2006, a “Regularization Law” was passed which terminated all operating agreements signed with private investors under previous hydrocarbon laws. This law violated the constitutional principle of non-retroactivity of laws as it changed the legal rights of private investors established in agreements under other

⁵ Monaldi et al (2020) erroneously report that the corporate income tax rate paid by oil companies was “reduced to 50 percent to balance the impact of the increased royalty”. They are misled by the fact that prior to 2001 the de jure tax rates for oil corporations (67%) were not the de facto tax rates. Rather, oil corporations were taxed at the maximum rate for non-oil corporations (34%). While the tax reform did decrease the de jure rate from 67% to 50%, it also made this rate the de facto rate for oil corporations. See Vivoda (2008, ch. 4, footnote 50).

laws applicable at the time of signature. Private investors could continue their operations only if they agreed to sell parts of their equity to the state.

While the Regularization Law applied to the operation of existing oil fields, a “Migration Law” passed in February 2007 imposed similar conditions on oil exploration and profit-sharing agreements in the Orinoco Belt. Here a minimum state participation of 60% of capital reduced private interests even more. Most private investors acquiesced and negotiated compensations for surrendering the larger part of their participations to PdVSA. However, ExxonMobile and ConocoPhillips refused and sought arbitration at the International Center for Settlement of Investment Disputes. Their installed capital was fully nationalized joint with an offer of compensation at book value, but significantly below market value.

There can be little doubt that expropriation in the sense of the above definition occurred in Venezuela at the latest in 2006/2007 and we will therefore take 2006 as the time of “treatment”. But a point could be made that the course of President Chavez’s oil policies was already set with the entry into force of the new OHL and accompanying changes in royalties and tax laws in 2002. We will therefore consider 2002 as an alternative time of “treatment”.

IV. Data and empirical implementation⁶

We use annual time series data 1984-2019 from the World Bank’s World Development Indicators. The outcome variable is net inflow of foreign direct investment as a percentage of GDP (FDI). From the same data source, we choose several variables as possibly useful predictors of FDI: The level and the growth rate of real per capita GDP, the share of government consumption in GDP, the annual inflation rate in terms of the GDP deflator and the sum of exports and imports as a share of GDP. This variable is, of course, intended to measure trade openness (TO).

The latter variables are part of a huge set of “candidate predictors” chosen for the following reasons: First, while FDI is already scaled by GDP, it would be overly restrictive to implicitly assume a simple linear relationship with the level of GDP. Moreover, it is certainly possible that strong growth in GDP attracts more FDI relative to GDP than in normal times. Next, government interaction with economic activity may also affect FDI decisions – this is what the share of government consumption in GDP is supposed to capture. Finally, the annual inflation rate is a proxy for the government’s willingness to impose informal taxes on the domestic economy.

Similar determinants of FDI have been identified e. g. by Schneider and Frey (1985), Asiedu (2006) and Rozanski and Sekula (2016). We refer to these and the variables described below as “possibly useful predictors” because we will, as a preliminary first step, subsequently check their predictive power for FDI in a set of (panel) regressions. Actual predictors in the synthetic control approach will be only those which are found to be significant in these panel regressions.

While the aforementioned variables are standard explanatory variables which may (or may not) affect the net inflow of FDI, many studies (e. g. Schneider and Frey (1985), Egger and Winner (2005), Rozanski and Sekula (2016); McCloud et al. (2018)) have pointed out that FDI also

⁶ For reproduction, we make our programs and all data except the proprietary ICRG data available free of charge.

depends on several softer, harder-to-measure variables, cf. e. g. Schneider and Frey (1985)⁷. To account for this possibility, we add further possibly useful candidate predictors from the “International Country Risk Guide” (ICRG) data which are compiled and supplied to commercial customers by The PRS Group, Inc. The ICRG data consist of time series indices intended to measure government stability (GS), socioeconomic conditions (SEC), investment profile (IP)⁸, internal conflicts (IC), external conflicts (EC), corruption (C), military in politics (MP), religious tensions (RT), ethnic tensions (ET), law and order (LO), democratic accountability (DA) and bureaucratic quality (BQ). For all these series, higher values indicate better conditions and less country risk.

We do not claim that the ICRG data measure accurately what they are supposed to measure – this would be quite impossible. But we claim that private investors seem to find them informative when investing in foreign countries – as witnessed by the commercial success of The PRS Group in selling their data. Therefore, the ICRG data may have predictive power for FDI.

ICRG data are available for 143 countries, and World Development Indicators for 266 countries. We exclude all countries with missing data over 1984-2019 and we exclude all advanced Western-type economies in which expropriation of major industries was rarely an issue during this period⁹. We further exclude all countries which are listed in Table 1 of Hajzler (2012) as “expropriating countries” between 1989 and 2006, except for our “treated” countries Argentina, Bolivia, Ecuador and Venezuela¹⁰. All other countries serve as possible controls¹¹.

As a third group of possibly useful candidate predictors, we include reserves in natural resources (separately for oil and gas) as a percentage of known world resources. These variables are intended to control for geographic concentration effects, since foreign investment in natural resources may be more difficult to divert in response to increased expropriation risk if the resources are highly concentrated in a particular country. Moreover, use oil rents in percent of GDP as a candidate predictor to take into account the possibility that FDI also depends on how resource-reliant a country is¹².

In the notation of the model, the variables described above belong to the predictors z_j if they have predictive power for FDI in country j . Denote these R covariates by x_j , a $R \times T_0$ matrix for each country j . In addition, the predictors z_j may consist of pre-treatment outcomes y_j^{pre} (or functions thereof). The overarching goal of SCM is the construction of a synthetic control country which is similar to the treated country not only with respect to the pre-treatment

⁷ In turn, Cohle and Ortega (2022) have argued that FDI may be a determinant of these variables and may, in particular, increase internal political conflicts.

⁸ This is an indicator of investment risk related to contract viability/expropriation, profits repatriation and payments delays.

⁹ This may be about to change, as some Western governments have taken control of energy companies in Russian property after the invasion of Ukraine.

¹⁰ No data are available for Venezuela after 2014.

¹¹ This yields 59 controls: Algeria, Angola, Bangladesh, Botswana, Brazil, Burkina Faso, Cameroon, Chile, China, Colombia, Congo, Congo (DR), Costa Rica, Cote d'Ivoire, Dominican Republic, Egypt, El Salvador, Gabon, Gambia, Ghana, Haiti, Honduras, India, Indonesia, Iran, Jamaica, Jordan, Kenya, Madagascar, Malawi, Malaysia, Mali, Mexico, Mongolia, Morocco, Namibia, Nicaragua, Niger, Nigeria, Pakistan, Panama, Paraguay, Peru, Philippines, Saudi Arabia, Senegal, Singapore, South Africa, South Korea, Sri Lanka, Sudan, Tanzania, Thailand, Togo, Tunisia, Turkey, Uganda, Uruguay, Vietnam.

¹² We thank an anonymous referee for pointing out this effect. Data on natural resource reserves are taken from the BP Statistical Review 2022. Data on oil rents are from the World Bank.

outcomes y_1^{pre} of the treated country 1, but also similar with respect to the covariates x_1 which we think of capturing structural characteristics of the treated country.

Since “similarity” shall be achieved along several dimensions, weights must be chosen which reflect the relative importance of each predictor. It is standard to assign the same weight to all pre-treatment outcomes, but no a priori guidance exists on which covariate shall receive what kind of weight. However, it is intuitively clear that a particular covariate should receive high weight if it has high predictive power for FDI, i.e. for the observed outcomes. This is why covariate weights are often determined by a regression of pre-treatment outcomes on the covariates.

To identify covariates x_j with predictive power for FDI in country j we follow Kuosmanen et al. (2021) in running random effects panel regressions in pre-treatment time for each treated country¹³. The regression equation is

$$y_{jt}^{pre} = \beta_0 + x_{jt}'\beta + \gamma_j + \varepsilon_{jt}, \quad j = 0, \dots, J+1, \quad t = 1, \dots, T_0 \quad (5)$$

where y_{jt}^{pre} is FDI in country j and period t , x_{jt} is the corresponding vector of covariates and γ_j is the country-specific random effect. Since treatment dates differ for the four countries under investigation, we run separate regressions for each treated country and include in the cross-section of the panel the treated country ($j = 1$) and all controls ($j = 2, \dots, J+1$).

We use a general-to-specific approach in which we begin with a regression containing all possible covariates and then stepwise delete the regressor with the weakest (heteroskedasticity-consistent) t-statistic. In the end, only regressors that are significant at the 5% level survive. For the four treated countries, this rather mechanical procedure yields the results displayed in Table 1.

¹³ As a robustness check, we also ran fixed effects regressions. Synthetic controls based on the fixed effects results are very similar. Detailed results and graphs are available on request.

Table 1
Panel regression results *: Predictors for FDI

Treated country (treatment date)	Argentina (2012)	Bolivia (2004)	Ecuador (2006)	Venezuela (2002)
Covariate				
GDP growth	0.062 (2.63)	---	---	---
Trade openness (TO)	0.031 (5.53)	0.023 (2.97)	0.026 (3.38)	0.027 (4.14)
Inflation	-0.013 (-2.39)	-0.018 (-4.12)	-0.022 (-5.79)	-0.020 (-6.02)
Government stability (GS)	0.136 (3.05)	0.172 (3.64)	0.151 (3.32)	0.157 (3.21)
Investment profile (IP)	0.381 (5.33)	0.271 (3.94)	0.279 (4.30)	0.261 (3.40)
Oil rents (% of GDP)	---	-0.061 (-2.73)	---	---
R^2	0.49	0.45	0.49	0.46
observations	1204	860	946	846
countries	43	43	43	47
sample	1984-2011	1984-2003	1984-2003	1984-2001
* Coefficient estimates of random effects model. Robust t-statistics in parentheses, constants suppressed. Results for alternative treatment dates Bolivia (2006), Venezuela (2006) are similar.				

All coefficients have the expected sign: GDP growth (just for the Argentina regression), trade openness, government stability and property rights are associated with higher shares of FDI in GDP, and high inflation is detrimental to FDI. The results are similar and robust across specifications and R^2 is almost 50%. We, therefore, use these variables (five in the case of Argentina and four in all other cases) as covariates when estimating the synthetic control.

Note that reserves in natural resources as a percentage of known world reserves were insignificant (or even found to have the wrong sign). We did not find a single incidence of a significantly positive association between FDI shares and the shares of world reserves, neither for gas nor for oil. This indicates that the geographic concentration of resources does not seem to play a role in investment decisions affecting the four countries we study.

Given their reserve shares, this is not surprising. For instance, in the year 2000 Venezuela had 3.3% of known world gas reserves and 5.9% of known world oil reserves. For both resources, Argentina, Bolivia and Ecuador had less than 1%. In the same year, the highest shares of world reserves are found in Iran (18.4% of gas reserves) and Saudi Arabia (20.2% of oil reserves). Both countries are in our control group along with more than 50 countries which have reserves shares for oil and gas of comparable size as our treated countries, i.e. below 6%. It seems that investors who would shy away from our treated countries would have ample opportunity to invest in the same resources elsewhere in the world.

Before we turn to the determination of the predictor weights (in the V matrix), let us emphasize another important implication of the regression results: They indicate possible confounders in the causal analysis we aim at. Confounders may occur in any natural (quasi-)experiment where, by design, the political, economic and social environment is beyond the control of the

researchers. Hence, great care must be taken when claims about causal effects are made to ensure that the effects are not wholly or partially due to unaccounted confounders.

The cause we are interested in is “expropriation”. By definition, this means a degradation of the property rights of foreign investors for which we discussed several government-initiated measures directly targeting the status quo ante of foreign property, e.g. seizure of assets with insufficient compensation, forced sales at prices below fundamental value, discretionary tax or loyalty increases aimed at reducing profitability, legal impediments to repatriation of profits and so forth.

But decisions of foreign investors to reduce FDI may also be due to confounders, in particular policy changes in other areas than those directly related to property rights. For instance, a new government may decide to intervene more (or less) in the private economy, may try to improve the socio-economic conditions of workers by raising minimum wages or by increasing the legal rights of trade unions, it may have an impact – whatever the direction – on law and order and/or on the political influence of the army leadership, it may have an impact on the level of corruption or the extent of guerilla activities, other internal conflicts, ethnic or religious tensions, etc. All these decisions may discourage or encourage inward FDI, and they may occur at the same time at which the new government may be set to attack property rights.

Here, the key question is empirical: Do such confounding developments affect FDI just at the margin or do they do this sufficiently strong to explain sizable and systematic fluctuations in inward FDI? The regression results in Table 1 provide a partial answer, because many regressors, most of them from the ICRG database, capture possible confounders in terms of ICRG-defined indices: GC for degree of government activities in the private economy, SEC for socio-economic conditions, LO for law and order, MP for military in politics, a corruption index, IC, ET and RT for different sorts of tensions and conflicts and so forth.

Most of these regressors are insignificant, i.e. they do not have a systematic relationship to FDI in our panel of countries. From the 12 ICRG indicators, only government stability (GS) and investment profile (IP) were significant for FDI. In terms of IP the takeaway is that, indeed, taxation/expropriation/repatriation issues are of major importance for determining investment decisions.

This leaves the possibility that major changes in other significant regressors confound our evidence. Therefore, we first check - for each treated country - if major changes in GDP, inflation, trade openness or government stability occurred at or around the hypothesized treatment dates. The respective time series, normalized to be equal to one in 1990¹⁴, are shown in Figure 1. The evidence for confounders is clearly negative. At treatment dates, the fluctuations of potential confounders relevant for FDI are not greater (actually: mostly smaller) than in the approximately 15 years before or after treatment. Therefore, while these events may have an impact on FDI in some countries on some occasions, we conclude that this was not the case for our four Latin American countries at the points in time when property rights of foreign investors were under attack.

Second, we also check if the insignificant ICRG indicators display major changes at treatment time. We do so because the regression results merely inform us about the average effect such

¹⁴ Rather than in 1990, inflation is indexed to one in 1992 for Argentina and in 1994 for Ecuador to have reasonable scalings in the graphs.

potential confounders have had (across all countries) on FDI. While the average effect is insignificant, it may be that in a single (treated) country such a confounder did have an effect on FDI¹⁵. If this were the case, then observed changes in FDI after “treatment” might not only be attributable to expropriation, but also to a confounding development which took place at the same time.

The respective ICRG time series are shown in Figure 2, with indicators showing major changes plotted in colour. For Argentina, there is virtually no evidence of any confounding development at treatment time (2012). For Bolivia, the by far largest drop in ICRG indicators in 2004 and subsequent years is in the investment profile series. This reflects the treatment, the threat of expropriations after the 2004 referendum. We also see more internal conflicts (a decrease in the indicator) 2004-2006 and an increase in democratic accountability in 2006. The former reflects the reignition of opposition protests demanding more nationalization of natural resources than the Mesa administration had proposed and the second reflects the election of opposition leader Evo Morales who swiftly complied with these demands. Hence, we conclude that all three coloured ICRG indicators express the increased risk of expropriations, i. e. the treatment.

For Ecuador, we also see a large drop in the investment profile series, i. e. a deterioration in property rights. This, again, is the treatment. The other remarkable development is an increase in external conflicts (a decrease in the indicator) prior to and after treatment time 2006. This is due to tensions with northern neighbor Columbia, whose government fought guerilla bands which, in turn, frequently encroached into Ecuadorian territory. There, the guerilla also threatened regions in which major natural resource activities were taking place, forcing extortion payments from companies and local population. From the perspective of international investors, this is tantamount to expropriation and property rights violation. Hence, we interpret this as treatment, not as a confounding event.

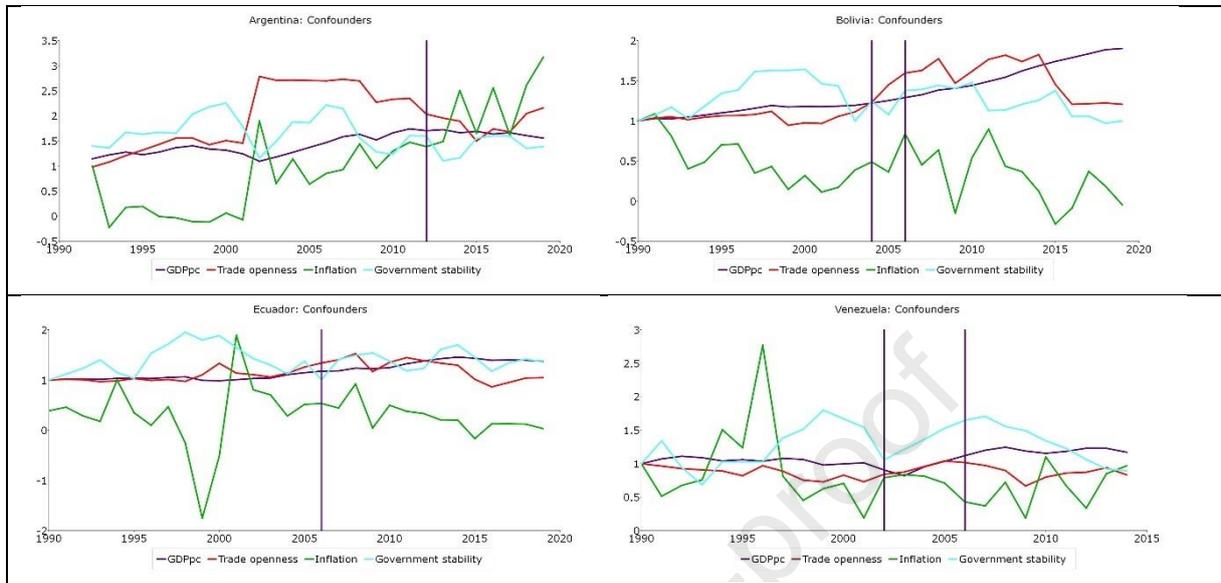
Venezuela, finally, is clearly a troubled country. Various ICRG indicators show strong declines starting already in the late 1990s: External conflicts, internal conflicts, corruption, military in politics, law and order. Much of this seems to be associated with the rise to power of Hugo Chavez who was elected President in 1998. Four years later, at the earliest hypothesized treatment date, most of these ICRG indices seem to have settled at rather low values. This may also be true for the internal conflicts, except for some short-term up-down-up-again movement 2001-2003. Law and order has deteriorated strongly prior to 2002, but the indicator reverts the downward trend in 2003 and on average stays approximately at the 2002 level between 2002 and 2010. Deteriorating conditions in these social unrest indicators may already have taken a toll on FDI in the years prior to 2002 and were, in any case, no suprising news for international investors in the treatment year.

The greatest and most persistent change in ICRG indicators at treatment time and in the next years is again in the investment profile, i. e. the variable capturing treatment. While there were certainly confounding events in Venezuela also in 2002 and subsequent years, the size of these events was mostly small compared to what investors had already experienced in the years preceding treatment. Moreover, the smaller confounding developments in 2002 and after often had no clear direction and partially offset each other in their presumed effect on FDI. Hence, with some caution, it seems reasonable to interpret a possible finding from synthetic control analysis as being largely due to the expropriation threat raised by Venezuela’s passage of the Organic Hydrocarbons Law in 2002.

¹⁵ We thank an anonymous referee for suggesting this additional check.

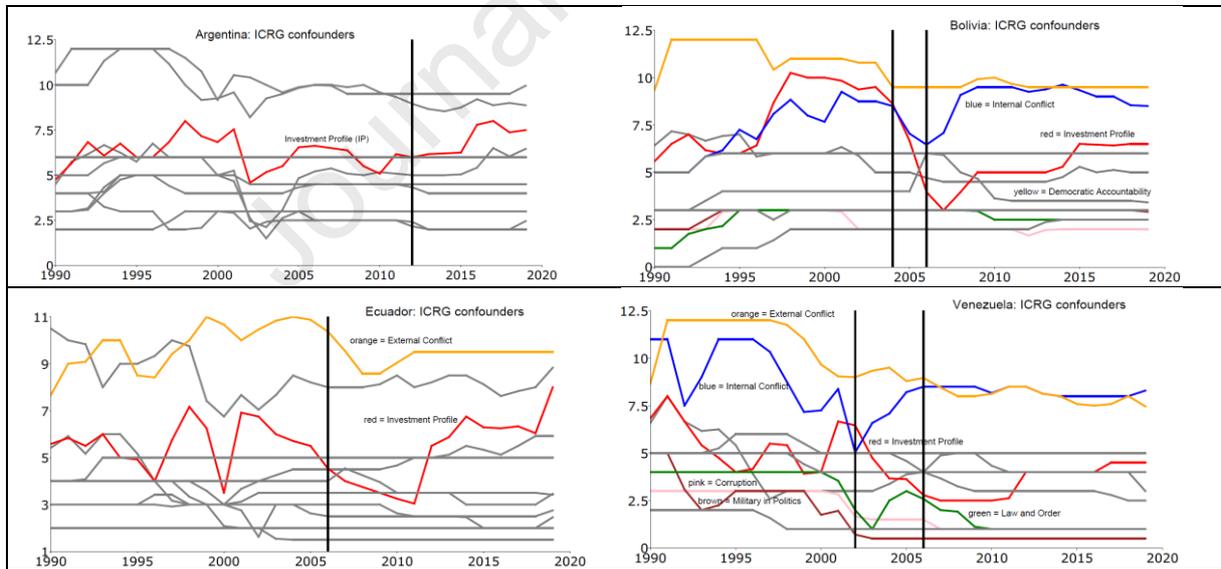
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Figure 1
Potential Confounders I: Significant regressors



Vertical bars at hypothesized treatment dates

Figure 2
Potential Confounders II: Other ICRG indicators



Vertical bars at hypothesized treatment dates

The final specifications in Table 1 indicate the predictive “importance” of each regressor through its t-statistic. Unlike other authors who assess “importance” for the estimated coefficients, we prefer their t-statistics since they are scale-invariant. Hence, we choose the predictor weights in the V -matrix proportional to the (absolute value) of each covariate’s t-statistic in the corresponding panel regression.

It is conventional to normalize the V -matrix such that the sum of its diagonal elements equals one. Predictors may be either covariates or pre-treatment outcomes. Let $\omega \in [0,1]$ be the sum of the predictor weights which refer to (functions of) pre-treatment outcomes. Hence, we normalize the t-statistics of the covariates such that the sum of their absolute values is $1 - \omega$.

To ensure our results are robust to changes in predictor choice, we will consider three cases:

Case 1: $\omega = 1$, i.e. predictors are all available pre-treatment outcomes.

Case 2: $\omega = 0.5$, i.e. pre-treatment outcomes receive overall the same weight as the covariates.

Case 3: $\omega = 0$, i.e. no pretreatment outcomes are used, only covariates.

Covariates measure structural characteristics of countries. Using covariates as predictors expresses the idea that countries which are structurally similar in pre-treatment time, would behave similar in post-treatment time if there were no treatment. The panel regressions show that some covariates have predictive power for FDI. Regressions use the first and second moments of regressors and dependent variables. This suggests that the predictors (covariates or pre-treatment outcomes) should also be expressed as means or standard deviations (over pre-treatment time).

But other choices are possible, too: For instance, the last observation of a predictor in pre-treatment time may be of particular importance. Or, for pre-treatment outcomes, many studies simply use all available observations as predictors.

Case 1, sometimes called “canonical” synthetic control approach, puts all predictor weights on pre-treatment outcomes. Case 3, by contrast, puts all predictor weights on structural characteristics (and does not try to match the time series of pre-treatment outcomes). Case 2 has 50% weight on pre-treatment outcomes and 50% on structural characteristics. Specifically, our choices are as follows:

Case 1: Predictors are all available pre-treatment outcomes, each with equal weight in the V -matrix.

Case 2: Predictors are just the mean, standard deviation and last pre-treatment observation of pre-treatment outcomes with weight $1/6$ each, i.e. a total of 50% of the V -weights. Further, for each structural covariate significant in the panel regression, we also use its mean, its standard deviation and the last pre-treatment observation with equal weights each such that all these weights add up to the remaining 50%.

Case 3: No information on pre-treatment outcomes is used. For each structural covariate significant in the panel regression, we use its mean, its standard deviation and the last pre-treatment observation with equal weights each, adding up to 100%.

Note that Case 1 will always reproduce pre-treatment outcomes better than Cases 2 and 3. This follows trivially from the fact that Case 1 is designed to optimize the fit for pre-treatment

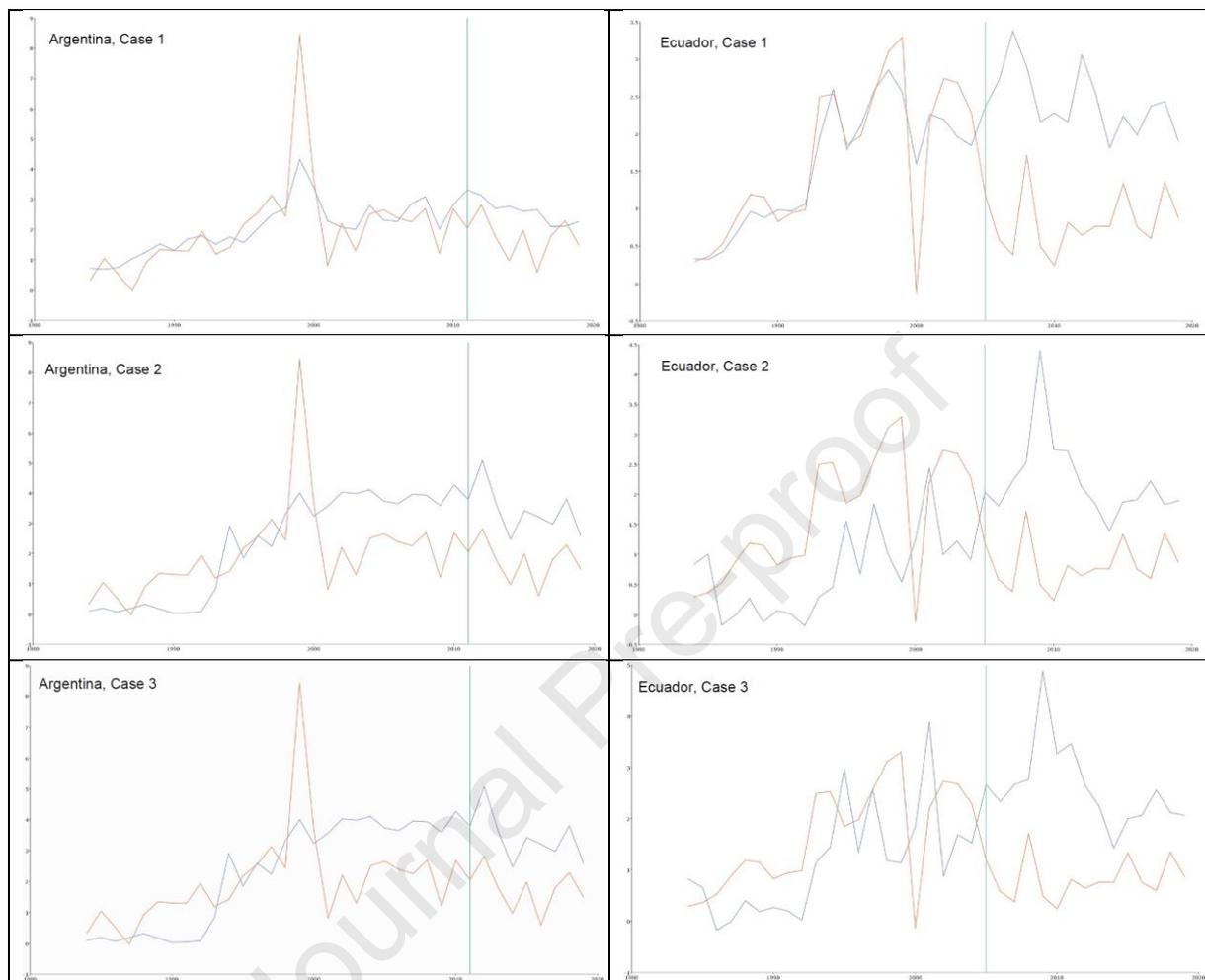
outcomes (and nothing else). But this does not at all imply that Case 1 estimates the counterfactual *post*-treatment outcomes optimally. Quite to the contrary, there is the problem of overfitting: Maximizing pre-treatment fit may result in assigning much weight to a control country that coincidentally was exposed to a similar shock at the same time as the treated country. Clearly, there is no guarantee that this will also be the case in post-treatment time – in particular, if the two pre-treatment shocks were unrelated.

In short: If the pre-treatment fit is to be maximized, a good fit for pre-treatment outcomes is almost uninformative and potentially misleading. This is why in Cases 2 and 3 we have deliberately refrained from reproducing pre-treatment outcomes and rather determine SC weights such that structural characteristics of the treated country are reproduced by the synthetic control. Note that even the seminal ADH (2010) paper has refrained from maximizing the pre-treatment fit of all outcomes for precisely this reason. In Case 2 we still have the last pre-treatment observations, as well as the average and the standard deviation of the outcomes over pre-treatment-time as predictors, so we do not yet decouple completely from pre-treatment outcomes. But, clearly, the SC-approach will then just try to match these three statistics and not the complete set of outcomes over pre-treatment time.

V. Results:

We first display results for Argentina, see Figure 3. Red lines are actual FDI shares of the treated country, blue lines depict the synthetic controls. Everything to the right of the vertical green line is treatment time.

Figure 3
Results for Argentina and Ecuador



Both Argentina and Ecuador have had one extreme FDI observation in pre-treatment time. Such an observation is typically not well matched by the synthetic control – an observation also made by Abadie (2021). Otherwise, the fit between the treated country and its synthetic control is fairly good in Case 1. In Cases 2 and 3, of course, no such fit is aimed for. At most, the synthetic control will mimic the mean and the standard deviation of outcomes in pre-treatment time. Note that the synthetic controls in Cases 2 and 3 are often quite similar, despite Case 2 having 50% predictor weight on the moments of pre-treatment outcomes. Still, the differences in country weights are usually rather small.

For Argentina, Case 1 suggests that the 2012 expropriation of Repsol, S. A., has had very little, if any, effect on net FDI. Quite different seem to be the results in Cases 2 and 3, which both suggest a sizable loss in FDI of 1.7% of GDP. But, interestingly, the gap between actual FDI and FDI of the synthetic controls opens much earlier than in 2012. In fact, it seems that the loss of FDI is due to Argentina's sovereign default in 2001 – and persists at roughly the same magnitude in the next almost twenty years. Under this interpretation, Cases 2 and 3 also suggest that the 2012 expropriation of Repsol, S. A., has had little effect on FDI. The damage to

Argentina's reputation among foreign investors may already have been done in 2001 and subsequent events may have had at most second-order effects.

We caution that this way of reading the data is challenged by an earlier study by Tomz and Wright (2010). These authors point out that, from a historical perspective going back to 1929, expropriations and sovereign defaults occur in waves that are typically not contemporaneous. Thus, they argue that defaults do not necessarily signal a risk of expropriation – if they did, they should be observed at approximately the same time. Their prime example is a wave of sovereign defaults which took place in the 1980s. No similarly broad wave of expropriations followed suit. Rather, it seems that expropriations in an equally large group of countries occurred about ten years earlier, in the 1970s.

While this evidence seems to be at odds with our interpretation, Tomz and Wright also note that on a cross-country level, expropriations correlate with sovereign defaults in the sense that about 70% of all countries either never engaged in either form of “sovereign theft” or made use of both forms – albeit not at the same time¹⁶. Under this perspective, Argentina's sovereign default would signal to foreign investors that Argentina is a high-risk country for FDI since it must be seen as likely to also resort to expropriations sooner or later. If this is the lesson foreign investors learned from Argentina's sovereign default, the loss in FDI documented by the synthetic control approach is perfectly in line with this reasoning. Moreover, Argentina's example reinforces Tomz and Wright's finding that default and expropriations are asynchronous and separated by a great number of years.

Methodologically, the analysis for Argentina shows that it is dangerous to maximize the pre-treatment fit of outcomes if the treatment date is wrongly specified. Focusing on structural similarities like in Cases 2 and 3 is much less error-prone because structural features of the treated country may not change by much when “treatment” occurs. This may then enable the researcher to identify the “true” treatment date.

For Ecuador, things are easier. While, by design, only Case 1 constructs a synthetic control which closely mimics actual FDI data in pre-treatment time, in all three cases there is a clear negative effect on FDI after Ecuador enacted the expropriating pieces of legislation in 2006. Quantitatively, the mean post-treatment effect on FDI is of similar size in all three cases, it varies between 1.4% and 1.8% of GDP. Whether these effects are “significant” remains to be seen when we run placebo studies in the next subsection. Note that the reduction in percentage points of GDP is quite similar in Ecuador (all cases) and Argentina (Cases 3 and 4).

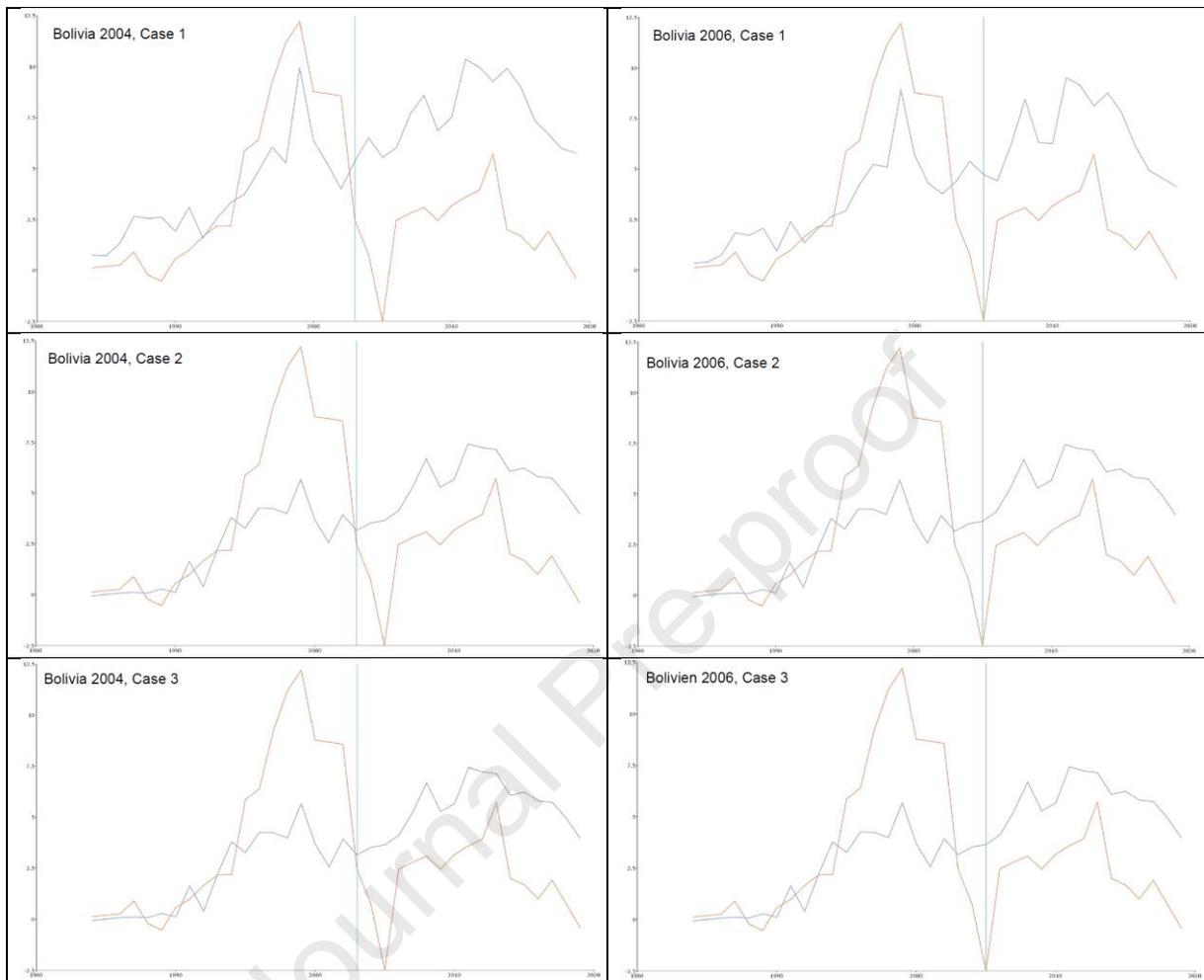
Figure 4 shows the results for Bolivia for the two hypothesized treatment dates 2004 and 2006. It turns out that in each of the three cases, the synthetic controls for treatment in 2004 and treatment in 2006 are very similar. Moreover, in all three cases, expropriation apparently has had a clearly negative effect on subsequent FDI, and this effect unfolds already in 2004 even if 2006 is the hypothesized treatment date. We may therefore conclude that foreign investors shied away from Bolivia already from 2004 onward, probably as a consequence of the 2003 gas war and the overwhelming majority in favor of nationalization in the 2004 referendum. President Evo Morales' legal measures which eventually enacted the expropriation of foreign investors seem to have had little additional effect. Quite likely, foreign investors had fully expected such legislation already in the wake of the 2003/2004 events.

¹⁶ We thank an anonymous referee for bringing this to our attention.

For Bolivia, we find much stronger effects in terms of FDI losses in terms of percentage points of GDP than for Argentina and Ecuador. For the 2004 treatment date, Case 1 suggests an FDI loss of 5.7 percent of GDP in post-treatment time, while Cases 2 and 3 find an FDI loss of 3.5%. For 2006, Case 1 yields a somewhat smaller estimate than for 2004 (a mean FDI loss of 4.3% of GDP), while Cases 2 and 3 come to almost the same result (3.4%) as for the 2004 treatment date. We tentatively interpret this as possible evidence for our conjecture that synthetic controls estimated on structural characteristics of countries (Cases 2 and 3) produce results more robust than SCs aimed at reproducing the pre-treatment time series optimally, i.e. Case 1. As already pointed out, Case 1 is prone to overfitting and may therefore have greater variance in estimated treatment effects. With this reasoning we consider an FDI loss of about 3.5% percent of GDP to be the more reliable estimate for Bolivia.

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Figure 4
Results for Bolivia, treatment assumed in 2004 or 2006



red=observed, blue=synthetic control

In stark contrast to Bolivia, results for Venezuela, cf. Figure 5, have very different synthetic controls for treatment in 2002 than those estimated for treatment in 2006. Moreover, in Case 1 the fit for pre-treatment outcomes is reasonably good if treatment is assumed for 2002 and very bad for the alternative treatment date 2006. Still, both scenarios indicate a large negative effect of expropriation: A mean loss in FDI of 4% (2002) or 2.8% (2006) of GDP in post-treatment time. Cases 2 and 3 (which do not try to find a good fit for pre-treatment outcomes) also come up with the conclusion of sizable mean losses in FDI for both possible treatment dates: 3.1% and 2.7% of GDP, respectively, for 2002, and 2.2% and 2.4% of GDP, respectively, for 2006. (Recall that these are post-treatment time averages and this time period is four years shorter if “treatment” is assumed to have occurred in 2006 rather than in 2002).

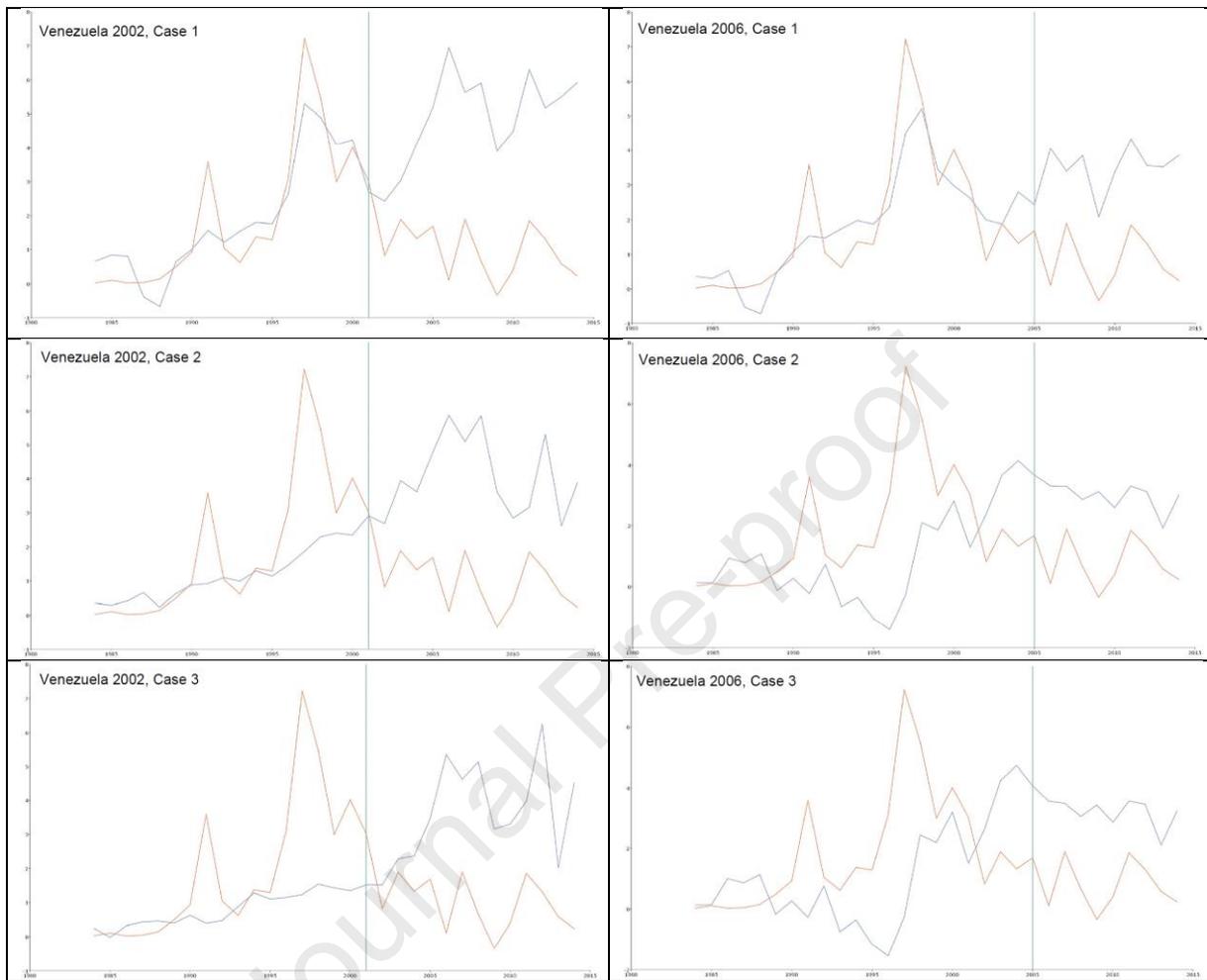
Which assumption on the date of treatment is appropriate? Again, as in the cases of Bolivia and Argentina, using either only the information on structural country characteristics (Case 3) or, additionally, selected “moments” of pre-treatment outcomes but not the full-time series (Case 2), helps find the correct treatment date, which, for Venezuela, seems to be the entry into force of the Organic Hydrocarbons Law and the increased royalties and taxes in 2002. For even if treatment is assumed for 2006, the year in which OHL was implemented and the Regularization Law was passed, the resulting synthetic controls suggest losses of FDI already in the preceding years and at a scale quite similar to what we observe from 2006 onwards. So, again, it seems

that foreign investors react fully already to early signs of nationalization such that legislation passed in subsequent years has little additional negative effect. This is in line with a reputation theory of FDI where a country's reputation among foreign investors is so severely damaged by the first attack on property rights that further events can hardly inflict any more reputational damage.

The estimated control weights for all four countries and all cases can be found in Tables A1-A3 in the appendix.

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Figure 5
Results for Venezuela, treatment assumed in 2002 or 2006



VI. Placebo Studies:

The visual inspection of observed data and counterfactual developments represented by synthetic controls strongly suggests sizable losses of net FDI inflows if a country contemplates expropriating foreign investors or otherwise violates property rights, e.g. the sovereign default in Argentina. But are the deviations between observed and synthetic control data “significant” or could they be explained as the consequence of other random shocks in post-treatment time unrelated to expropriations?

To check this question, we resort to so-called placebo-tests, see e.g. ADH (2010). Recall that we chose as controls, countries which did not enact nationalizations over the time period of our study. Hence, no effect should be visible if we experimentally handle one of the control countries as a “treated” country and estimate a synthetic control for this “placebo” country from the set of remaining control countries and by exactly the same method and treatment date as for one of our truly treated countries Argentina, Bolivia, Ecuador or Venezuela.

For each of the synthetic control analyses above we ran placebo studies in which each control unit was subject to a placebo treatment at the specified treatment time and a synthetic control was estimated from the remaining control countries (i.e. a leave-one-out donor pool) using the same method and settings (i.e. Cases 1, 2 or 3). We computed the mean treatment effect (over post-treatment time) for each placebo country and compared it to the same mean treatment effect of the country which had truly nationalized its oil or gas sector. Specifically, we computed from the empirical distribution of all corresponding placebo runs the empirical frequency (“P-value”) of a more extreme mean treatment effect than observed for Argentina, Bolivia, Ecuador or Venezuela. Somewhat loosely, we call these frequencies “significant” if they are below conventional significance levels such as 5% or 10%.

Table 2
Placebo tests:

Frequency of placebo mean treatment effects exceeding mean treatment effect of treated country

	Argentina	Bolivia 2004	Bolivia 2006	Ecuador	Venezuela 2002	Venezuela 2006
Case 1	0.238	0.000	0.000	0.071	0.000	0.000
Case 2	0.167	0.024	0.024	0.143	0.043	0.087
Case 3	0.143	0.024	0.024	0.167	0.087	0.109

Mean treatment effects for Argentina are not significant – probably a consequence of the misspecification of the timing of treatment. In fact, if we specify 2001, the date of Argentina’s sovereign default, as the treatment date, empirical P-values are much lower: 0.024, 0.095 and 0.119 in Cases 1 to 3. For Bolivia, the loss in FDI is significant at the 5% level throughout, even if the presumably wrong treatment date 2006 is specified. Hence, we may conclude that Bolivia’s oil and gas war and referendum result in 2003/2004 have indeed caused a major loss in subsequent net FDI inflows.

The evidence is less compelling for Ecuador. Only in Case 1 is the empirical P-value for the mean treatment effect below 10%, in Cases 2 and 3 the results are insignificant at conventional levels. Hence, the loss in FDI experienced by Ecuador after 2006 could also be a result of other factors than the controversial laws and seizures of foreign assets in 2006. This does not say that these events did not contribute to the reduced inflow of net FDI, but the statistical evidence is not quite hard enough to conclude with sufficient confidence that they did.

For the 2002 events in Venezuela, the opposite picture emerges. In Cases 1 and 2 our results are significant at the 5% level and in Case 3 the likelihood of the observed effect is still below 10%. With treatment specified too late (i.e. 2006), P-values are only slightly higher. We interpret this as indicating that foreign investors did not wait to see the rather radical measures enacted by President Chavez in the later years of his presidency. Already early in his tenure, when legal measures were still much more moderate than the President’s rhetoric, the latter seems to have caused a sizable and statistically significant reduction in net FDI inflows.

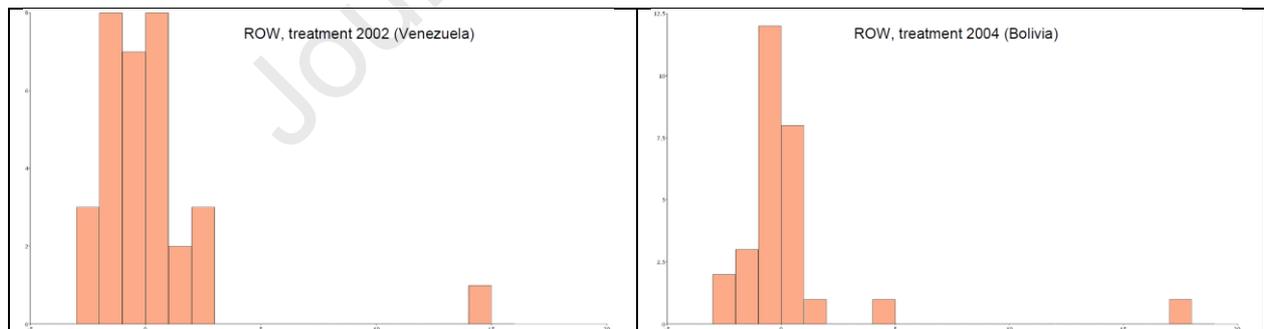
This said, we still must check a possible caveat: Were the control countries unaffected by the waves of nationalization moods which swept through some Latin American countries? Our results would *overestimate* the magnitude of FDI losses in the expropriating countries if foreign investors decided to just shift their funds to safer countries in the region, thereby increasing FDI

in some of the control countries. Our results would *underestimate* the magnitude of FDI losses if, conversely, expropriations in some countries scared away foreign investors also from investing in natural resource extraction in other countries in the region, fearing that political developments could be similar.

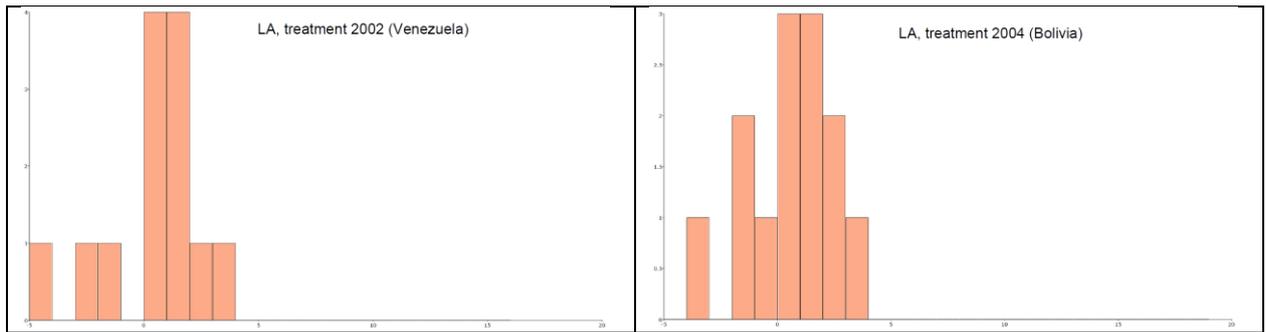
Spillover effects could work either way and it is difficult to assess which sign and which strength such effects may have had. Fortunately, the placebo studies open the way for an easy check: The control group consists of Latin American countries and countries elsewhere in the world. It is plausible to assume that whatever spillover effects took place mainly affected neighboring countries, i.e. countries in Latin America. Thus, we may compare the empirical distributions of the (placebo) mean treatment effects between the subgroups of Latin American (LA) controls and controls in the rest of the world (ROW).

To save space, we focus on the middle-ground Case 2 and study the supposed treatment years 2002, 2004, 2006 and 2012, corresponding to Venezuela, Bolivia, Ecuador and Argentina, respectively. Results are given in Figure 6, where, in each panel, the lower histogram shows the distribution of mean treatment effects for LA controls and the upper histogram shows the distribution for ROW controls. It is apparent that for all four treated countries the distribution of LA treatment effects has slightly more mass for treatment effects greater than zero, but the magnitude of these effects is well in the range of positive effects found in the empirical distribution of ROW countries and can therefore not be concluded to be evidence for positive spillover effects from Latin American countries with major expropriations of foreign asset holders¹⁷. Moreover, there are also negative effects of approximately equal magnitude so that no systematic bias in the donor pool is discernable.

Figure 6a
Empirical Distributions of Placebo Mean Treatment Effects for LA and ROW controls

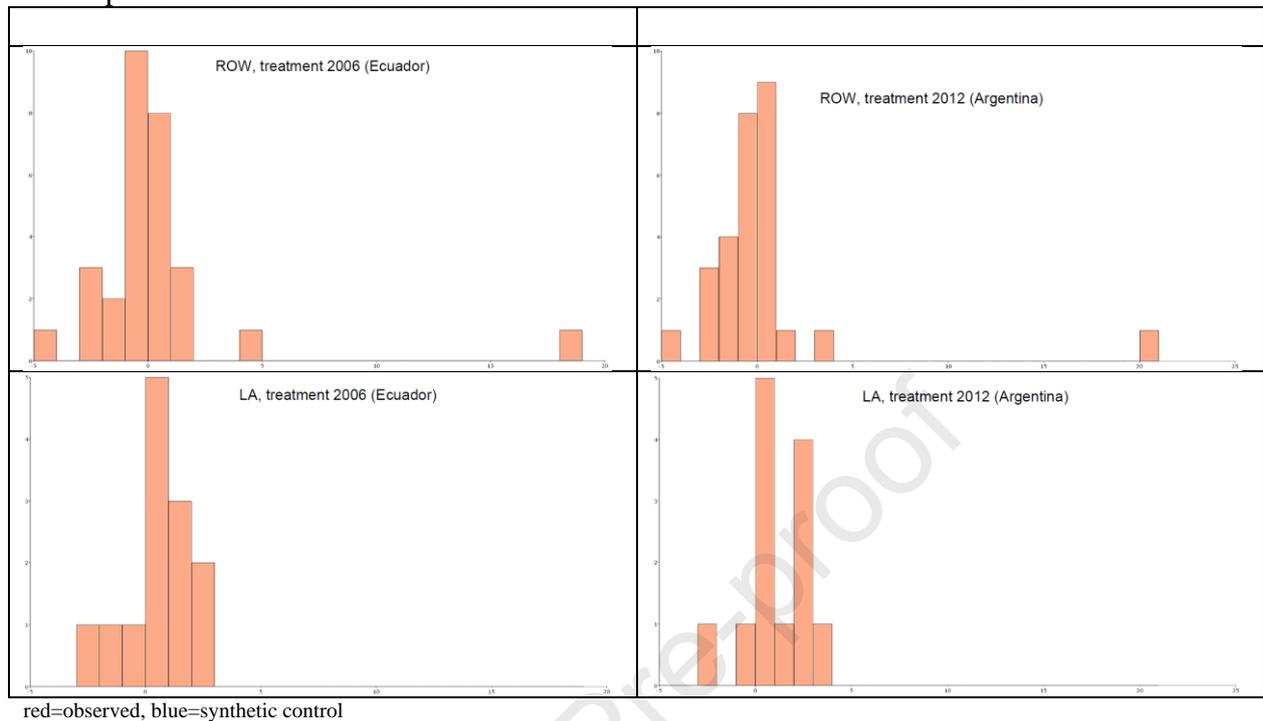


¹⁷ In the empirical distribution of ROW countries there is always one far right outlier. This is Singapore. Singapore's net FDI inflow is in most years much higher than for any other control country. Therefore, no good synthetic control can be constructed for Singapore – recall that synthetic control weights are always smaller than one, so there is no chance to match Singapore's FDI. Clearly, then, Singapore under placebo treatment will display strongly positive “treatment” effects – but only due to poor fit in pre-treatment time.



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Figure 6b
Empirical Distributions of Placebo Mean Treatment Effects for LA and ROW controls



VII. Conclusions:

Our analysis shows that attacks on property rights can have long-lasting negative effects on foreign direct investments and may therefore leave a heavy footprint on development and economic growth.

Synthetic control methods are a modern and appropriate instrument to assess by how much a policy measure, like expropriation, has caused unfavorable responses by world capital markets and for how long these persist. For the four countries we studied, we find that expropriations caused substantial losses in FDI inflows with no evidence of attenuation up to now. Neighboring countries which left their extraction activities in the hands of private companies would seem to be well advised to learn from these experiences and hence withstand public pressure towards nationalizations surfacing again these days.

Future research would need to study what the effect of new-style nationalizations in advanced Western economies will be. In the early 2000s, foreign investors who shied away from Latin American expropriations were presumably mostly capital owners from Western countries. Today, the situation may be reversed: Russian, Arabian or Chinese investors may worry whether their investments in Western economies are safe in a world of heightened confrontation between Western democracies and powerful or rich autocratic regimes elsewhere in the world. And for the Western economies, the key question is, of course, not predominantly what growth effects to expect if FDI from such countries is reduced, but much more importantly how severely their economies are affected if the autocratic countries shut down their exports of natural resources in response to a perception that their capital investments in Western democracies are at risk.

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Appendix:

Table A1
 Estimated SC-weights for Argentina and Ecuador
 (nonzero weights only)

Donors	Argentina Case 1	Argentina Case 2	Argentina Case 3	Ecuador Case 1	Ecuador Case 2	Ecuador Case 3
Bangladesh					0.158	0.112
Brazil	0.199	0.318	0.318			
Cameroon					0.219	0.110
Chile	0.128					
China						0.160
C. d'Ivoire					0.054	
Dom. Rep.				0.038		
El Salvad.				0.047		
India					0.086	
Indonesia				0.016		
Kenya				0.032		
Malaysia	0.145			0.026		
Mali					0.368	0.550
Morocco				0.086		
Peru		0.336	0.336	0.184		
Philippines				0.135		
Saudi-Arab					0.115	0.067
Senegal	0.078					
S. Korea	0.445					
Sri Lanka				0.312		
Sudan		0.346	0.346	0.008		
Thailand	0.006			0.115		

Table A2
Estimated SC-weights for Bolivia 2004 and Bolivia 2006
(nonzero weights only)

Donors	Bolivia 04 Case 1	Bolivia 04 Case 2	Bolivia 04 Case 3	Bolivia 06 Case 1	Bolivia 06 Case 2	Bolivia 06 Case 3
Brazil				0.053		
Chile	0.421			0.447		
El Salvad.	0.000					
Nicaragua	0.511	0.580	0.580	0.500	0.582	0.582
Peru		0.420	0.420		0.418	0.418
Singapore	0.068					

Table A3
Estimated SC-weights for Venezuela 2002 and Venezuela 2006
(nonzero weights only)

Donors	Venez. 02 Case 1	Venez. 02 Case 2	Venez. 02 Case 3	Venez. 06 Case 1	Venez. 06 Case 2	Venez. 06 Case 3
Colombia	0.191	0.128				
C. d'Ivoire	0.081			0.270		
Gabon					0.291	0.315
Indonesia	0.041					
Iran					0.208	0.155
Jamaica		0.191	0.025			
Jordan	0.092					
Malawi			0.069			
Malaysia				0.048		
Mali				0.001		
Nicaragua	0.290			0.166		
Pakistan		0.141	0.188			
Panama	0.132			0.132		
Paraguay	0.165		0.130	0.307		
Singapore	0.008					
S. Africa				0.077		
Sudan		0.122	0.000		0.361	0.448
Turkey					0.140	0.082
Uganda		0.146	0.151			
Uruguay		0.253	0.438			

Highlights:

Expropriations of oil and gas industries had negative impact on FDI in South America

Strongly negative effects of property rights violations on net FDI inflows in Bolivia and Venezuela, weaker for Ecuador.

Argentina still suffers from reputational damage of sovereign default.

Synthetic control methods allow dating of expropriation events and foreign investor anticipations.

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Declaration of interests

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests:

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