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GABRIELE LATTANZIO
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BY

Dr. William L. Megginson, Co-Chair

Dr. Lubomir P. Litov, Co-Chair

Dr. Gustavo Grullon

Dr. Scott C. Linn

Dr. Wayne B. Thomas

"…There is a new trend among authors to thank every famous people for inspiration, non-existent assistance, and/or some casual reference to the author's work. Authors do this to pump themselves up. So, on the off chance that this is helpful, I wish to thank the following people: the Emperor of Japan and the Queen of England for promoting literacy; William S. Cohen, former secretary of defense, for dropping me a note saying he liked my books, as did his boss, Bill Clinton; Bruce Willis, who called me one day and said, "Hey, you're a good writer"; Albert Einstein, who inspired me to write about nuclear weapons; General George Armstrong Custer, whose brashness at the Little Bighorn taught me a lesson on judgement; Mikhail Gorbachev, whose courageous actions indirectly led to my books being translated into Russian; Don DeLillo and Joan Didion, whose books are always before and after mine on bookshelves, and whose names always appear before and after mine in almanacs and many lists of American writers—thanks for being there, guys; Julius Caesar, for showing the world that illiterate barbarians can be beaten; Paris Hilton, whose family hotel chain carries my books in their gift shops; and last but not least, Albert II, King of the Belgians, who once waved to me in Brussels as the Royal Procession moved from the Palace to the Parliament Building, screwing up traffic for half an hour, thereby forcing me to kill time by thinking of a great plot to dethrone the King of the Belgians.

There are many more people I could thank, but time, space, and modesty compel me to stop here."

Nelson Demille

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Abstract

This dissertation comprises three essays. The first reports a macro-economic application of the synthetic control method developed in Abadie and Gardeazabal (2003) to assess the economic implications of the institutionalization of social norms within a country's legal framework. In the second essay I develop and applied the same methodology to assess the financial and social welfare implications of megamergers completed in the U.S. over the period 1979-2014. The last essay investigates whether the restructuring gains achieved by state owned enterprises (SOEs) over the three years preceding their attempted privatizations can be sustained absent the ownership transfer from political investors to profit maximizing investors.

The macroeconomic and macro finance literature have established that religious and cultural practices have major implications for a Country's economic performance. However, it is not clear if the institutionalization of these social norms within a country's legal system causes material economic effects. In Chapter 1 I show this to be the case. By employing a case study approach to mitigate endogeneity concerns, I show that the institutionalization of Sharia Law within a Muslim-majority country's legal system causes material economic costs. Results hold in different settings, confirming that the governmental enforcement of existing social norms constrain individuals' social and economic freedom, ultimately resulting in worsened economic outcomes.

In Chapter 2 I build upon the methodology described in Chapter 1 to assess the financial and social welfare implications of the megamergers completed in the U.S. over the period 1979-2014. It is indeed challenging to develop a plausible counterfactual for identifying merger synergies because the merged firm is fundamentally different from the two merging firms and targets' separate accounting information is no longer available. The synthetic controls method allows to address these issues and to assess the economic consequences of 383 domestic mega-deals. Merging firms'

sales-based performance improves following a merger, but find mixed results for their asset-based performance. This inconsistency originates from merged firms subsidizing their inefficiencies by charging higher markups. Consistent with the quiet life hypothesis, insiders capture most of the economic rents and consumers' welfare appears to be negatively affected.

In Chapter 3 I employ a novel, hand-collected sample of withdrawn and completed share issue privatizations (SIPs) to show that both groups undergo comparable restructuring processes over the three years preceding the event. By relying upon a matching procedure to explicitly control for the identified restructuring effect, I isolate the ultimate consequences of the ownership transfer from state to private investors on corporate policies and performance. I find that, absent the ownership transfer, most of the gains realized during the restructuring process are re-absorbed over the post-treatment period. Results are robust to the use of instrumental variables, indicating that the transition from state to private ownership represents a necessary condition for the long-term success of privatization programs..

Chapter 1: Beyond Religion and Culture: The Economic Consequences of the Institutionalization of Sharia Law

1. Introduction

Despite the existence of an extensive body of literature studying the effects of culture and of the rule of law on economic prosperity,[1] it is not clear whether the institutionalization of social norms within a country's legal system would cause material economic consequences. This question is particularly important for Muslim-majority countries, as the Quran offers an internally consistent legal framework which can co-exist alongside both common law and civil law systems (Esmaeili, 2011).

The demand for Sharia compliant legislations has increased exponentially in several historically secular countries over the last twenty years.[2] This pressure is surging especially in South and South-East Asia, and in the North-Eastern and sub-Saharan African regions, where an overwhelming majority of Muslims want Sharia Law to be the base of both their country's legal and judicial systems.[3] As this geo-political trend is becoming more prominent on a global base, the availability of robust evidence on the economic consequences of the institutionalization of

[1] Weber, 1930, La porta et al, 1998, Acemoglu et al., 2001, Beck et al., 2003, Stulz and Williamson, 2003, Barro and McCleary, 2005, La porta et al., 2008, Tabellini, 2010, Alesina and Giuliano, 2016, among others.

[2] The Sharia Law is derived from the Quran and from the teachings of Prophet Mohammed, called "Sunnah". These two sources have been subject to an extensive scholarly authentication, interpretation, and deduction, which has resulted in different schools of thought such as Hanafi, Malki, Shafie, and Hanbali. These schools vary in their interpretation of the Quran and Sunnah, and on their understanding of the general guiding principles (see Alzahrani and Megginson, 2017).

[3] In 2013, the Pew Research Center conducted a survey involving more than 38,000 people and covering 39 Muslim countries. They found that support for making Sharia law the core of a country's legal and judicial systems is highest in South Asia (median of 84%), followed by South East Asia (77%), the North-East African region and the Middle-East (74%), and sub-Saharan Africa (64%).

already existing religious practices within a legal framework is becoming of paramount importance.

To the best of my knowledge, this is the first study assessing this important question by analyzing the economic consequences of the institutionalization of Sharia Law within a Muslim-majority country's legal system.

The lack of studies in this area can be easily explained by pointing to the scarcity of disaggregated data and to the pervasive endogeneity severely weakening normal panel regression approaches. Furthermore, the introduction of Sharia Law within a legal system is generally surrounded by several changes in the country's political and economic environment, which might potentially contaminate the results. All in all, these constraints severely restrict the applicability of common large-sample quantitative methods to establish causal relationships (or at least reliable and meaningful estimates for conditional correlations) between the introduction of Sharia Law and economic prosperity.

In this paper, I overcome these limitations by applying a synthetic control analysis (Abadie et al. 2003; 2010; 2015) to a country (Mauritania) for which the introduction of Sharia Law within its legal and judicial systems was not surrounded by other major confounding events. This methodology allows me to construct a "synthetic" counterfactual which resembles relevant economic characteristics of "real" Mauritania before the introduction of Sharia Law in 1980. The post-treatment economic evolution of the synthetic control can therefore be interpreted as the economic growth that Mauritania would have had experienced if Sharia Law was not adopted.

Why should we expect the introduction of the Islamic Law within a country's legal system to affect its economic growth? First, financial development is widely considered as a critical determinant of economic growth (De Gregorio and Guidotti 1995; Levine, 1997; Levine and

Zervos 1998; Rajan and Zingales 1998; Beck and Levine 2004; Kroszner et al. 2007; among others). Consequently, the prohibition of the riba[4] might have a first order impact on a country's economic performance. Despite an extensive body of literature studies the effect of Islamic Finance and Banking on economic development, results appear to be inconclusive, mostly due to the intrinsic endogeneity characterizing this research question.[5] Second, a strand of the literature points to the strict restrictions that Islamic Law imposes on contracts structure, credit, insurance and corporate ownership (Kuran 2004). Another channel through which a country's economic growth might be affected by the introduction of Sharia compliant legislations comes from its effects on women's living standards. Several studies have investigated the effect of Islamic rule on women's life quality, identifying contrasting results[6]. Finally, the institutionalization of Islamic factors might strengthen the effects of culture on economic growth. For instance, Henderson and Kuncoro (2009) documents a reduction in corruption outcomes following increased representation of Islamic parties in Indonesia. All in all, the overall direction and magnitude of the effects of the introduction of Sharia Law on a country's economic growth remains an open empirical question. However, it is important to note that only Muslim-majority countries for which the above-mentioned social norms are already in place would consider framing the Islamic Law within their legal framework. Consequently, while the above-mentioned factors can be easily identified in the cross-section, there are unlikely to materially change in the time-series. Thus, any economic effects resulting from the institutionalization of religious and cultural practices should originate from their

[4] There are several different interpretations for this provision prohibiting the charge of interest (or, in broader terms, usury). El-Gamal (2001; 2003) provides a legal and economic analysis of its origins and consequences.
[5] For instance, Iqbal (1997) and Khan (2010) find support for Islamic finance having a growth-enhancing function; conversely, and Honhohan (2001) find results consistent with the hypothesis that Islamic Finance leads to limited financial and economic integration, and that it might fail to efficiently allocate available resources.
[6] Kristoff (2011) shows anecdotal evidence supporting the hypothesis that Islamic rule negatively affects women's living standards; conversely, Blayedes (2014) finds better health outcomes for women living in districts of Cairo controlled by radical Islamists, and Meyersson (2014) shows better education outcomes for poor Muslim women in Turkish regions governed by the Islamic Party Refah.

governmental enforcement, rather than from their sudden materialization. That is, any economic effects originated from the institutionalization of the Sharia Law should be ascribed to the resulting reduced personal and economic freedom of agents living in (or dealing with) the treated country.

By exploiting the synthetic control methodology formalized in Abadie and Gardeazabal (2003) and Abadie et al. (2010; 2015), I identify strong negative effects on a country's economic growth, measured as GDP per capita, caused by the introduction of Sharia Law within the existing legal and judicial systems. In particular, I show that the introduction of Sharia Law caused Mauritania's GDP per capita yearly growth to be 0.76% lower than its synthetic control's economic growth over the 10 years following its introduction.

As with all synthetic control analyses, results should be carefully interpreted, since this methodology does not allow one to easily claim external validity. In fact, comparative analysis methodologies exclusively allow study of the observed event in isolation, strongly limiting any inferences from the results. To assess whether the identified effects are case-specific, I overcome the unavailability of other clean settings to study the effects of Sharia Law on economic growth by analyzing marginal increases in its enforcement level. In particular, I study the effects of setting Sharia Law within a constitutional framework by identifying a specific instance (Saudi Arabia's Basic Law of Governance, 1992) in which the new constitution did not determine significant changes in the quality of existing institutions and form of government.[7] The applied synthetic control analysis identifies large negative effects associated with the studied marginal increase in Sharia Law enforcement level. In particular, framing Islamic Law within a constitutional setting caused Saudi Arabia' GDP per capita yearly growth to be 1.63% lower than its synthetic counterfactual's growth over the period 1992-2001.

[7] Persistency in the quality of institutions and in the form of government is proxied by stability in both the Polity IV and Durability Index around the analyzed event.

All in all, given the existence of significant economic, environmental, cultural and social differences between Mauritania and Saudi Arabia, the observed consistency in signs is suggestive that a strong, negative association between increases in governmental enforcement of social norms and a country's economic growth does indeed exist. However, the reported consistency is not sufficient to state conclusively that the identified results are universally generalizable. As for any case-study, policy-makers should exercise caution when extrapolating conclusions from internally valid results.

My results on how the institutionalization of already existing religious and cultural factors impacts a country's economic growth are related to three main strands of the literature. First, my paper contributes to the extensive literature on culture and economic growth by studying the economic effects of the institutionalization of cultural and religious factors within a legal framework. These novel results extend the existing literature, which has been mainly focusing on the effects of trust and other inherited (and therefore persistent) cultural and religious beliefs and values, by proposing a new setting to study the effects and channels through which culture might affects different economic variables. In particular, this paper add to the recent and limited literature on the effect of state religion provisions and of religious freedom on economic growth (Barro and McCleary, 2005; Gill and Owen IV, 2017). Second, this paper contributes to the legal literature studying constitutions including Sharia as a Source of Legislation Clauses (SSL Clause) (Al-Fahad 2005; Lombardi 2013; 2016; among others), by providing the first empirical results on the economic implications of this provision. Third, I contribute to the econometric literature by discussing weaknesses and strengths of the synthetic control methodology when countries selected in the donor group are characterized by high level of political and economic instability.[8] Finally,

[8] Or, generalizing, when units in the donor group are unstable.

this paper fits the current geopolitical debate on the role and future of political Islam, which has been extensively covered by media over the last few years.

2. Identification strategy

The estimation of the causal effects of the institutionalization of the Sharia Law on a country's economic growth is particularly challenging. Disaggregated data is not available for several African, Asian and Arab countries, and a government's choice of adopting Islamic Law compliant legislations is arguably endogenous and correlated with other country's characteristics. Consequently, a simple comparison of economic outcomes of countries with different degrees of exposure to the Sharia Law is likely to capture the effects of those several unobservable characteristics, rather than the effects of the treatment. Furthermore, the introduction of Sharia Law within a legal system is generally surrounded by several confounding events, such as dramatic transformations in a country's political and economic environment, which might potentially contaminate the results (Abadie and Gardeazabal 2003; Jones and Olken 2005). All these constraints severely restrict the applicability of common large-sample quantitative methods to establish causal relationships between the introduction of Sharia Law and economic prosperity, consequently requiring the use of a more sophisticated methodology.

To get consistent estimates of the treatment effect, we ideally want to observe how studied countries would have performed if Sharia Law was not introduced within their legal and judicial systems. As previously mentioned, the existence of several contemporaneous confounding events, the limited number of treated countries and the lack of disaggregated historical data make the use of a propensity score matching methodology unadvisable. Consequently, I apply the synthetic control method discussed in Abadie et al. (2003; 2010; 2015) to overcome these identification

issues. This particular form of comparative case study allows for the comparison of outcomes between 'treated units' and synthetically constructed counterfactuals.

Countries that introduced Sharia Law present several unique features, making it virtually impossible to identify a suitable single comparison unit (Collier et al. 2004; George and Bennett 2005; Gerring 2007). For this reason, the synthetic control method identifies a combination (weighted average[9]) of 'control' units, which has been shown to generally outperform any single comparison unit alone. Furthermore, the use of a convex combination of untreated units to construct the synthetic control precludes any form of model-dependent extrapolation,[10] significantly simplifying the interpretation of the results, and increasing its statistical reliability (King and Zeng 2006)[11].

The synthetic control methodology allows to identify an internally valid estimate of the causal effects of the treatment, provided that the studied unit is unaffected by other major shocks unrelated to the treatment over the analyzed time window. Table A1 shows that this assumption is arguably violated for most of the treated countries, for which a synthetic control (as well as any other large sample methodology) would consequently provide a severely biased estimate of the treatment effect.

Despite these limitations, in the next section I will argue that Mauritania provides a sufficiently clean setting for the application of the synthetic control methodology to estimate the causal effect of the institutionalization of the Sharia Law on economic growth.

[9] The use of a non-linear combination of untreated units would severely undermine our ability to provide an economic interpretation of the structure of the built synthetic control.
[10] Under the assumptions that weights attributed to each country belong to the interval [0 , 1] and sum to 1.
[11] Abadie et al. (2015) discuss that a normal regression estimator can be expressed as a weighted average of comparison units, with weights summing to one. Unfortunately, in this setting the weights would not be restricted to the interval [0 , 1], allowing for model-dependent extrapolation.

3. The political and economic history of Mauritania between 1960 and 1991

The 1958 Constitution marked the beginning of the French Fifth Republic and decolonization age. In this new, and long-awaited geopolitical scenario, Mauritania declared its independence on November 29, 1960. The new state was far from being structured as a modern country. Society was organized in tribes, and more than 50% of the population was nomad (Moore 1965). Government leaders were strictly politically connected with French authorities, which retained indirect control of the region until the early 1970s, when Mauritania's domestic and foreign policy started to diverge from France's interests. This increased degree of independence is well captured by Mauritania's decision to join the Arab League in 1973, strongly signaling its political independence from the former colonizer.[12] Despite the low quality of its political and economic institutions, Mauritania coped reasonably well the first half of the 1970s. Despite this relative stability, the Parti du People Mauritanien (the ruling party, PPM) decided to occupy and annex a portion of the former Spanish colony of Western Sahara, causing the beginning of a dramatic conflict with the Frente Popular para la Liberación de Seguia el-Hamra y Rio de Oro (hereinafter the Polisario Front) in 1975. This war had dramatic financial repercussions on Mauritania, leading to a bloodless coup aimed at changing the military leadership in 1978. The Comité Militaire de Salut National (CMSN), the political structure led by Colonel Mustapha Ould Mohamed Salek which substituted the deposed president Ould Daddah, reached a peace agreement with the Polisario Front in early 1979 (Pazzanita 1996). Despite several attempts to get back to a democratic regime, the CMSN retained power, de facto determining the survival of the strongly centralized system existing before the beginning of the war against the Polisario Front. In 1980 the government instituted the Islamic code, covering most civil and public law matters. This important

[12] In 1974, Mauritania nationalized foreign-owned iron ore mines.

institutional reform postulated the supremacy of Sharia law over the legislative power, and partially, over the judiciary power. The two existing bodies of judges (Sharia compliant judges and secular judges) were partially merged, and the Penal Code extended by formally including Sharia crimes, such as heresy, apostasy, atheism, refusal to pray, adultery and alcoholism, as well as punishments such as lapidation, amputation and flagellation.[13] Conversely, only limited restrictions against usury were institutionalized, ultimately generating marginal effects on Mauritania's underdeveloped financial system.

Although nearly all Mauritanians are Muslim, the introduction of Sharia Law within the Mauritanian legal system was followed by a mild opposition, mainly expressed by black citizens and women, who expected to be discriminated against under the new code in favor of the white, Moor, male population. Protesters obtained no results: Sharia was kept in place, and its enforcement strengthened over time. Importantly, In 1981 Mauritania became the last country in the world to abolish slavery.[14] Despite its symbolic importance, this reform had modest results, mainly due to the failure of the government in criminalizing this practice.[15] During the 1980's, Mauritania experienced a period of persistent instability; the domestic situation severely worsened in late 1990, when tensions between Moors and black Africans exploded, ending up in riots and fights, during which the military regime was accused of serious human rights violations (Pazzanita 1996)[16]. These internal conflicts and the deterioration of the relationship between Mauritania and Senegal due to illegal immigration issues caused a further weakening of Mauritania's economy,

[13] "Researching the Legal System and Laws of the Islamic Republic of Mauritania", Serge, Global Lex.
[14] Mauritania ratified in 1961 the convention against forced labor, but never enforced it, mainly because of several legal loopholes and lack in monitoring mechanisms.
[15] Mauritania criminalized slavery in 2007. See "Mauritania: Slavery's last stronghold", The CNN Freedom Project. Furthermore, note that the abolition of slavery is a Sharia compliant measure. Therefore, its economic consequences, if any, should be properly included in the average treatment effect.
[16] For details, see Africa Research Bulletin: political, social and cultural series (Exter), 27, 11, 1-30 November 1990, p. 9899, and "Mauritania: Slavery's Last Stronghold", The CNN Freedom Project.

leading to the end of the military regime in late 1991 and to the famous 15 April, 1991 speech by President Ould Tanya, announcing the organization of the first Mauritanian multi-party system and the establishment of an independent press.

3.1. Is Mauritania a feasible candidate for this study?

The synthetic control methodology allows me to identify an unbiased estimate of the treatment effect, conditional on two implicit assumptions. First, shocks unrelated to the treatment assignment and directly affecting the studied outcome variable[17] should impact both Mauritania and the constructed counterfactual[18] homogenously over the pre- and post-treatment periods. This assumption is arguably strong, especially when this methodology is applied to emerging or poor, politically unstable countries. The second implicit assumption captures the trade-off between having a long pre-treatment period incorporating several events, or a shorter, more stable 'matching' period. It can be shown that the estimation bias decreases in the number of pre-treatment periods (Abadie et al. 2015), conditional on that extending it does not affect the overall 'trend' of the time series for the outcome variable. This often overlooked condition implies that if we incorporate different shocks affecting the outcome variable over the pre-treatment period, the predictive power of the synthetic control would be low, independently from the length of the 'matching' period.

3.2. How does Mauritania behave with respect to these two restrictions?

The period preceding the introduction of Sharia Law within the Mauritanian legal system can be divided in two different sub-periods. During the 1960s, Mauritania started to build its political and economic infrastructure, slowly gaining full independence from France. The

[17] These shocks include natural disasters, such as earthquakes, tsunami, or floods, or international military actions.
[18] In this subsection I will exclusively address concerns related to Mauritania being a feasible candidate for this study. The validity of the constructed counterfactuals will be tested in Section VI through several robustness tests.

milestone event separating these two periods can be identified in Mauritania's decision to join the Arab League in 1973. Building the synthetic control over the complete time series would therefore result in an average of two different regimes, the first one characterized by high economic growth mainly due to the construction of a quasi-modern economic system, the second characterized by a stable economy, controlled by an independent government. Since Sharia Law was introduced in a political, economic, and social environment significantly closer to the latter period than the one characterizing Mauritania's first decade of independence, constraining the time-series to the post 1973 period allows to obtain an economically more meaningful counterfactual than the one that would have been obtained by exploiting the complete post-independence period. At the same time, shortening the 'matching' period might increase the relevance of other pre-treatment shocks. In this sense, the war between Mauritania and the Polisario Front might represent a major concern. The synthetic control would in fact incorporate Mauritania's characteristics during a period of military conflict, reducing its predictive power. In particular, since political instability negatively affects economic growth (Abadie et al. 2003; Collier and Duponchel 2013), the synthetic control would provide a conservative estimate of the GDP per capita of the ideal counterfactual for Mauritania. Consequently, as long as the constructed counterfactual systematically outperforms real Mauritania over the post-treatment period, this potential bias should not severely undermine the proposed identification strategy.

Finally, it is important to analyze whether the military coup occurred in 1978 introduces relevant noise in the model.[19] Interestingly, this bloodless coup led to no significant change in Mauritania's form of government, quality of existing institutions and democracy levels, resulting

[19] Jones and Olken (2005), Easterly and Pennings (2014) and Grier and Maynard (2016), among others, discuss whether or not political leadership has important implications for economic growth. This strand of the literature reports contrasting results.

exclusively in the end of the conflict against the Polisario Front. In particular, Figure A1 shows that both the Polity IV Index and the Durability Index are unaffected by the event, providing support for the argument that this change in leadership had at most marginal consequences on Mauritania's economic growth.[20]

With respect to the post-treatment period, a major concern is represented by Mauritania's decision to abolish slavery in 1981. If this important reform directly affected Mauritania's economic growth, it would be virtually impossible to disentangle its economic consequences from those caused by the introduction of Sharia compliant legislations.[21] Sadly, this reform had marginal effects on the population, due to the failure of the Government to criminalize slavery, and to the absence of any contextual economic measures aimed to allow former slaves to develop marketable skills and to achieve economic independence. In particular, different reports confirm that between 10% and 20% of the population was represented by slaves between 1970 and 1990, and that no major decline in this figure has been recorded over the last 30 years.[22] These facts support the hypothesis that this reform did not have a major impact on Mauritania's GDP level, at least over the studied period.

Finally, due to its geographical location, Mauritania is periodically hit by dramatic drought. Climate change might have increased the risk of extreme weather occurrence over time, resulting in lower aggregate production and more unpredictable harvests throughout the African continent (Boko et al. 2007; Rojas et al. 2011). If these changes have heterogeneously affected different countries, environmental factors might explain at least a part of any identified divergence between

[20] The Polity Index is a composite index proxying for a country's democracy "quality". It is based on a unified scale assuming values between +10 (strongly democratic country) and -10 (strongly autocratic country). The Durability Index "indicates the number of year since the most recent regime change [..], or the end of transition period defined by the lack of stable political institutions." For further details, see Marshall and Jaggers (2005), and Marshall (2011).
[21] Note that if this reform is consequential to the introduction of Sharia Law within the Mauritanian legal system, then it would not represent a concern for the proposed identification strategy.
[22] For further details and discussions, see Slow (1985), and Bales (2000), and Sutter (2012), among others.

the GDP per capita of real and synthetic Mauritania. Given the relatively short window covered by my analysis, and the absence of studies reporting significant changes in the probability of occurrence of extreme weather in Africa between the 1970s and the 1980s, this issue should not represent a major concern.

All in all, Mauritania can be defined as a persistently instable country, providing a sufficiently clean setting to evaluate the effects of Sharia Law on economic growth.

4. Data and Sample: Creating a Synthetic Control for Mauritania

The ideal control unit replicates how Mauritania's GDP per capita would have evolved if Sharia Law was not introduced within its legal and judicial systems. By following the methodology described in Abadie et al. (2003; 2010; 2015), I construct Mauritania's synthetic control as a weighted average of potential control states.[23] I restrict the weights to be non-negative and to sum to one, so that the obtained convex combination of unexposed countries does not allow for model-dependent extrapolation. Even if this methodology provides a good fit for the treated units, interpolation bias may still be large. To minimize this risk arising from interpolating across countries with economic, social, and cultural differences from Mauritania, I restrict the donor pool to units with similar characteristics to the treated country. In particular, I use exclusively the Organization of Islamic Cooperation members[24] and African countries in which Sharia Law has at most a residuary role, and for which data is available. This selection, leading to a final sample of

[23] Let X_1 represents the vector of used predictors for the studied unit, and Y_1 be the vector reporting pre-treatment values for the outcome variable. Analogously, let X_0 be the matrix of predictors for units in the donor group, and Y_0 the matrix reporting values of the outcome variable for all the potential controls. The weights are attributed to each unit in the donor group such that the distance function $D = (X_1 - X_0W)'V(X_1 - X_0W)$ is minimized. W is the vector of weights, and V is a positive-definite diagonal matrix. The methodology is discussed in detail in Abadie et al. (2003; 2010; 2015).

[24] OIC membership is commonly used in the literature to identify Islamic countries. Gutmann and Voigt (2015) propose a new Islamic State Index, but to avoid the arbitrary selection of a threshold level distinguishing Islamic and non-Islamic countries, I prefer to rely on this discrete binomial identifier.

18 potential units, should allow me to replicate Mauritania's cultural, geographic and economic characteristics.

As previously mentioned, the synthetic Mauritania is constructed as a weighted average of potential control units, where weights are chosen to reproduce the values of a set of predictors of the GDP per capita dynamic characterizing the treated country before the introduction of Sharia Law. Table A2 reports summary statistics for the available predictors.

Economic and demographic data are collected from the World Bank Indicators Database, and from the Penn World Table v9.0 (Feenstra, Inklaar, Timmer 2013). The Polity IV Index and the Durability Index are derived from the Polity IV Dataset (Marshall and Jaggers 2016), while education data come from Barro and Lee (2010). Finally, I hand collect religion data from the World Christian Encyclopedia (Barret et al., 2001).

It is important to consider that matching over the pre-treatment period on too many variables may lead to overfitting. Recognizing this issue, I follow Abadie et al. (2010) and Grier and Maynard (2016) in identifying 10 important predictors of a country's economic prosperity. First, I use the average GDP per capita, average population growth, average domestic credit to private sector[25], and average age dependency[26] levels over the identified pre-treatment period, 1973-1979. Furthermore, I use trade openness and population density levels, as observed in 1979, and the percentage of population with no education aged 15+, as observed in 1970 and 1975. Finally, I match on political stability level, proxied by the average Polity IV Index between 1973 and 1979, and on a dummy variable which equals one if the majority of the population is Muslim.

[25] This variable is used as a proxy for financial development.
[26] Age dependency is defined as the ratio between the dependent population (aged 0 to 14, or 65+), to the independent population (aged 14 to 65).

Applying the synthetic control methodology to this setting provides a synthetic counterfactual constructed as a portfolio composed of approximately 8% Algeria, 28% Kenya, 50% Democratic Republic of Congo, and 13% Mali, as described in Table A3. The quality of the constructed counterfactual is well captured by the low Root Mean Squared Prediction Error (RMSPE)[27], equal to 3.18%.

Table A4 reports the pre-Sharia Law values for the employed predictors. The average GDP per capita of the synthetic Mauritania differs by only 10 cents from Mauritania's actual GDP per capita, and most predictors match closely. The observed differences in population density are expected since Mauritania has one of the lowest level of population density in the world. All in all, the constructed counterfactual seems to provide a reliable representation of the pre-treatment economic performance of Mauritania.

5. Main Results

The constructed synthetic control replicates closely both Mauritania's actual GDP per capita dynamic and its predictors over the pre-treatment period. Consequently, any difference between the synthetic control and Mauritania over the post-treatment period can be interpreted as the causal effect of the institutionalization of the Sharia Law on Mauritania's economic growth (Rubin, 1974, 1977; Abadie et al., 2003).

Figure A2 plots real and synthetic Mauritania's GDP per capita over both the pre- and post-Sharia Law periods.

It can be observed that the counterfactual outperforms systematically the treated unit after the introduction of Sharia Law within the Mauritanian legal and judicial systems. The two time-

[27] $RMSPE = \sum_{t=1}^{T} \sqrt{\frac{(PE_t)^2}{N}}$

series diverge over time, consistently with time-increasing enforcement levels of Sharia compliance[28]. In particular, synthetic Mauritania has a GDP per capita 8.69% ($56.62) higher than real Mauritania's GDP per capita, as measured in 1991. This finding is consistent with the hypothesis that the governmental enforcement of social norms constraints people's social and economic freedom, ultimately resulting in worsened economic outcome.[29]

With respect to the statistical significance of the estimated effects, the post-treatment RMSPE is approximately twice as large as the pre-Sharia Law prediction error (5.59%).[30] This large difference in the model's predictive power between the two periods suggests that the institutionalization of Sharia Law within the Mauritanian legal system caused material effects. However, a wide array of robustness tests is needed to establish the statistical robustness of the identified results.

6. Robustness Tests

These findings are provocative, but their validity needs to be tested. A first important concern is related to the fact that the results might be driven by the specific weights attributed by the V-matrix to each selected predictor. This issue is particularly relevant with respect to the mismatching between the treated and control units in terms of average population density in 1979, and to the dummy variable indicating whether or not Islam is the major religion of the country.

The identified difference in population density is not surprising. Mauritania is characterized by one of the world's lowest population density levels and, therefore, matching on

[28] Note that the difference in trend between the treated and control units significantly increase after 1983. This is consistent with Mauritania significantly increasing its level of Sharia Law compliance between 1983 and 1985. See Pazzanita (1996) for further institutional details.
[29] Alternatively, it is possible to state that the synthetic control's GDP per capita diverge from Mauritania's GDP per capita at a 0.76% Yearly Compounded Divergence Rate (YCDR) over the period 1980-1991. YCDR is computed as the compounded rate of return that would generate the observed difference over the selected period.
[30] See Table A3.

this value is particularly problematic. Greater concerns might arise from the mismatching on the dummy variable "Islam major religion 1975". Even though the careful selection of the donor pool should significantly reduce concerns about possible interpolation bias, it is important to conduct a more rigorous robustness test to confirm baseline results' significance. To verify whether the selection of a particular set of predictors is driving my results, I run the synthetic control methodology on a different set of control variables. In particular, I directly use the percentage of Muslims in terms of a country's total population, as observed in 1975, and the average population density over the pre-treatment period as predictors of future GDP per capita. Results are qualitatively unchanged, and statistically stronger. The pre-treatment RMSPE increases slightly to 3.67%, confirming the high quality of the synthetic counterfactual, and the RMSPE ratio increases to 3.66. The difference in GDP per capita between real and synthetic Mauritania increases to 32.81%, as computed in 1991 (Figure A3).[31] All in all, this result provides support for that the identified negative effect of Sharia Law on a country's economic prosperity is not an artefact of the set of predictors used in the baseline model.

To detect whether the identified treatment effect is statistically significant, previous studies have relied on indicators of out-of-sample model accuracy to establish if the loss in predictive power of the synthetic control over the post-treatment period is meaningful. In the current setting, this test would arguably lead to over-rejection of the null hypothesis,[32] since the donor group includes several countries with high level of political instability which might be exposed to shocks affecting their future economic performance. Note that as long as a shock is endogenous to a

[31] This difference results in an YCDR of 2.31%. The difference in the magnitude of the identified effect between the baseline result and this robustness test is alarming. Running the synthetic control over different sets of predictors systematically lead to the identification of a significantly stronger negative effect than what identified by the baseline model, suggesting that the latter represents a prudential estimate of the treatment effect.

[32] The null hypothesis is defined as the RMSPE ratio being statistically indistinguishable from the mean RMSPE ratio for all the untreated units in the donor group. I am aware of no study formally testing the specification and power of tests commonly used in comparative studies.

country's characteristics and orthogonal to the treatment, its consequences do not represent a concern, since they would have potentially affected Mauritania's GDP per capita in the case Sharia Law would not have been introduced within its legal and judicial systems. Yet, it is important to analyze whether or not the negative effects identified in Section V can be interpreted as a statistical residual generated by changes in the predictability of post-treatment GDP per capita levels for countries in the donor group. To test this issue, I propose an alternative robustness test simultaneously exploiting the properties of placebo and jackknife tests. In particular, I run a synthetic control analysis on all the untreated countries, computing both their pre- and post-treatment RMSPEs. After dropping from the donor pool all the countries with an RMSPE ratio above the sample mean (median) plus one standard deviation, I rerun the baseline model on the remaining countries. If results are not driven by the identified outliers, this restricted model should yield qualitatively comparable results.

The donor group average (median) pre-treatment error is equal to 8.17% (4.25%), while in the post treatment period it equals 16.86% (12.62%). The distribution is clearly skewed, due to the presence of a few outliers. Among these extreme cases, a particular concern is represented by Guyana, whose RMSPE ratio equals 80.34. The lack of predictive power of the synthetic control in the post-treatment period is due to a severe economic crisis that hit Guyana during the second half of the '80s. This crisis was further exacerbated by the existence of severe political frictions between the government led by President Desmond Hoyte and the International Monetary Fund.[33] The important role played by the government in determining the overall outcome of the crisis and its independence from the treatment should reduce concerns about the relevance of noises eventually introduced in the baseline analysis by this event. In particular, given the nature of this

[33] In 1985 the IMF declared that Guyana was no longer eligible for further credit and loans.

shock, if a bias exists, it will be downward; that is the GPD per capita of the synthetic control should represent a conservative estimate of the GDP per capita of the ideal counterfactual. As previously discussed, I rerun the synthetic control on a restricted donor group exclusively including those countries with an RMSPE ratio smaller or equal than the sample mean RMSPE ratio, plus one standard deviation.[34] This combination of placebo and jackknife tests leads to the construction of a new synthetic control, described in Table A5.[35]

The pre-treatment RMSPE is now equal to 3.17%, indicating that the quality of the synthetic control remains high even after this conservative resampling process. The RMSPE ratio equals 1.93, emphasizing the existence of statistically and economically relevant differences between the pre- and post-treatment periods. These results provide further support for that baseline results are not donor group dependent (Figure A4).

As an additional robustness test, I pretend that Mauritania introduced Sharia Law within its legal system in 1976, and repeat the experiment. This simple "move the treatment" test allows me to verify whether the baseline results are due to the model's inability to predict Mauritania's GDP per capita in-sample, irrespectively from the existence of a shock. Table A6 reports the composition of this new synthetic control, which is characterized by a pre-treatment RMSPE of 1.80% and an RMSPE ratio of 0.61. Figure A5 shows that there is no significant difference between the synthetic control and real Mauritania's GDP per capita, neither in the pre-treatment nor in the post-treatment periods.

Despite these results provide support for my findings being not time-dependent, the short-period characterizing this placebo test severely undermines its statistical significance.

[34] I calculate mean, median, and standard deviation after truncating Guyana from the sample. This allows me to significantly mitigate the otherwise extreme positive skewness characterizing the sample RMSPE ratio.
[35] Replicating the same test by using the sample median RMSPE ratio plus one standard deviation as critical threshold leads to qualitatively similar results.

Consequently, at the cost of losing four control units, I rerun the synthetic control on the period 1960-1979, pretending that Sharia Law was introduced in 1966.[36] Column 3 in Table A6 reports the composition of this new synthetic control. Note that the RMSPE ratio converges towards one when I consider this longer window; consistently with that the model is able to replicate Mauritania's GDP per capita in-sample. As previously argued, using pre-1973 data introduce severe noise in the model, as testified by the increase in the pre-treatment period RMSPE, which equals 6.69%. Figure A6 plots the time series for this synthetic control and for real Mauritania over the period 1960-1979.[37]

All in all, these findings confirm the robustness of our initial analysis: the institutionalization of the Sharia Law within the Mauritanian legal system is, indeed, associated with non-negligible economic costs.

7. Are results case specific? An analyses of the Sharia as a Source of Legislation clauses

As with most case studies, limits to external validity imply that these findings need to be evaluated within the proper context. In particular, it is important to recognize that Sharia Law has been applied heterogeneously in different countries, and therefore the identified negative effects should be interpreted as specific to the Sharia Islamic Code approved in Mauritania in 1980. This code served as the law of the land in civil matters, with the exception of a few areas, such as nationality law and litigation involving corporations, automobiles, and aircrafts. Particularly relevant is the absence of references to the prohibition of the riba, which excludes that the observed

[36] This date is selected such that both the baseline model and the placebo test have the same number of pre-treatment periods.

[37] Country specific shocks might be driving the observed results. For instance, oil was discovered in the Democratic Republic of Congo in the late '70s. This discovery would result in an inflated economic performance for the constructed synthetic counterfactual. Importantly, this is not the case. Unreported tables show that our baseline analyses are robust to dropping each single country from the donor pool (Jackknife test).

negative effects are directly caused by the introduction of additional constraints or regulations slowing down Mauritania's financial development.[38]

While interesting per se, these results do not allow me to assess the economic costs that a representative country would face in case Sharia Law is adopted within its legal and judicial systems. As shown in Table A1, most treated countries introduced Sharia Law during periods of severe political and economic instability. Consequently, the application of a synthetic control methodology to these cases would provide an extremely unreliable estimate of the treatment effect.[39]

To overcome these limitations, I assess whether the previously reported estimates are case-specific by studying marginal increases in a country's level of Sharia Law compliance. In particular, I build on the recent literature studying the juridical relevance of "Sharia as a source of legislation" (SSL) clauses (Al-Fahad 2005; Lombardi 2013; among others) to analyze their economic consequences.[40] The approval of a new constitution offers a good setting to study the effects of a marginal increase in a country's level of Sharia Law compliance, provided that the new document generates no (or at least marginal) consequences on a country's institutional structure and form of government. To satisfy this "identification constraint", I rely on the extensive legal literature on ornamental constitutionalism[41] to argue that Saudi Arabia represents a proper

[38] If Sharia law affects banks or investors' risk aversion, then direct implications for financial development might arise. For instance, Adhikari and Agrawal (2016) shows that religiosity matters for bank risk taking behaviors.

[39] An unreported staggered difference in difference regression for which the treatment group includes all countries in Table A1, and the unmatched control group incorporates all the untreated countries for which data is available, identifies a strong, negative association between the institutionalization of Sharia Law and GDP per capita. These findings provide support for the generalizability of my baseline results, but they should be carefully interpreted due to potentially severe endogeneity issues.

[40] To the best of my knowledge, this is the first empirical study analyzing the economic consequences of SSL clauses. See Lombardi (2011) for an overview on the issue.

[41] Ornamental constitutions can be defined as an "Ornament with modest substance" (Al-Fahad, 2005), with virtually no impact on a country's democracy level and form of government.

setting to assess whether the degree of governmental enforcement of religious and cultural norms affect a country's economic growth.

On March 1, 1992, King Fahad announced the introduction of three new "fundamental laws" aimed to modernize Saudi Arabia's juridical framework. This reform, titled The Basic System of Governance (hereinafter the Basic System), represents a form of "codification of the status quo" (Al-Fahad 2005), simply acknowledging the existing political and institutional structure, while emphasizing Saudi Arabia's Islamic identity. Interestingly, this Islamic emphasis resulted in defining Sharia Law as the exclusive substantial constraint on the executive power. In particular, this new ornamental constitution stated that "governance […] derives its authority from the [Quran and Tradition] of the Prophet, both of which govern this Law and all the laws of the State".[42] Consequently, this new legal framework set Sharia Law as the core of Saudi Arabia's legal system, attenuating the relevance of the legislative power.[43] In particular, the Basic System codified the process of Islamic review aimed to invalid laws inconsistent with the Islamic jurisprudence by requiring courts to treat laws non-conforming to the Quran as void.[44,45] Consequently, the Basic System resulted in an at least marginal increase in Saudi Arabia's compliance to Sharia Law, while only marginally affecting other factors related to its institutional quality and form of government. This statement is confirmed by the absence of discrete jumps in

[42] See Al-Fahad (2005) for a technical analysis of Saudi Arabia's 1992 Constitution.
[43] "In addition to providing general constraints on government, Sharia operates as the legal system of the country. Accordingly, statutory laws are subordinated to Islamic law. Thus the concept of legislative authority is highly attenuated […]" (Al Fahad, 2005).
[44] Note that Saudi Arabia's constitution does not include an SSL clause, per se. However, it includes a strict process of Islamic review implying comparable juridical implications.
[45] Forms of Islamic Review were present in Saudi Arabia before the approval of the Basic System. However, this reform had "practical and sometimes profound implications. For example, holding to the view that bank interest is a form of prohibited usury, the judiciary would void most banking transactions, forcing the state to set up a special tribunal to adjudicate banking disputes. Even such a tribunal would not compel payment of interest; the most it would do is decline to recharacterize past interest payments as repayment of principal. The same dilemma long obtained with respect to the insurance industry. Some agencies were allowed to offer services but could not have recourse to courts, which viewed such contracts as void due to uncertainty. Only recently was a compromise reached that would legalize insurance of the mutual type. It remains to be seen if the courts will validate such insurance." (Al-Fahad, 2005).

Saudi Arabia's Polity IV Index and Durability Index around the year of adoption of this reform (Figure A7).

I exploit this shock to the degree of Sharia Law compliance to replicate the baseline analysis on Saudi Arabia.

It is necessary to acknowledge the existence of significant economic, social, environmental and cultural differences between Mauritania and Saudi Arabia in setting the synthetic control model. In particular, it is necessary to carefully review the selection of both predictors of future economic prosperity and countries included in the donor group. Since Saudi Arabia relies heavily on oil revenues, I add to the baseline model a 'control' for each country's yearly oil rent.[46] Furthermore, I constraint the donor group to members of the OIC and of the OPEC, for which Sharia Law is at most a residual source of legislation. Finally, I follow Grier and Maynard (2016) in including Norway and Canada in the donor group.[47] This selection results in a 'control' group of 21 countries and 11 predictors of future economic growth. For what concern the studied period, I focus on 1985-2001. This restriction allows me to exclude the years during which Saudi Arabia acted as a swing producer (1980-1985), as well as the post 9/11 period. Table A7 reports pre-treatment values for the selected controls for both real and synthetic Saudi Arabia, and Table A8 describes the composition of the constructed counterfactual.

The synthetic control replicates closely Saudi Arabia's economic characteristics over the pre-treatment period, 1985-1991. As shown in Table A8, the pre-treatment RMSPE is equal to

[46] Values for oil rent are collected from the World Bank Databases.

[47] I follow Grier and Maynard (2016) in including Canada and Norway in the donor group. These two countries are major oil exporters but are not OPEC members: including these two countries in the donor group allows to better capture Saudi Arabia's specific economic characteristics and to include potential "democratic outcomes" in the donor group.

3.28%, and the difference between real and synthetic Saudi Arabia's GDP per capita is extremely small ($0.54).

The results plotted in Figure A8 are provocative. After the introduction of the Basic System, real Saudi Arabia underperforms severely its synthetic counterfactual. In particular, the constructed control has a GDP per capita 26.49% higher than real Saudi Arabia's income per capita, as measured in 2001.[48] Furthermore, the RMSPE ratio equals 5.51, confirming the severe loss of predictive power over the post-treatment period.

This large negative effect cannot be fully explained by the partial mismatching between the treated unit and the synthetic counterfactual in terms of oil rent. The divergence between the two time-series takes place before the drop in oil price occurred in 1997. Furthermore, the synthetic control is built incorporating events and consequences of the Gulf War (1990-1991), therefore introducing a negative bias in the synthetic control's post-treatment GDP per capita. Nonetheless, further robustness tests are necessary to establish the statistical significance of these results.

The reported results are robust to several conservative resampling processes. In particular, the reported findings are robust to the separate exclusion from the donor group of Norway, Mali, Venezuela and Gabon, as well as to the exclusion of all countries for which a reliable counterfactual cannot be constructed.[49] Finally, it is necessary to point to the existence of a caveat that might invalidate the proposed identification strategy. The donor group includes no Arab country, and, consequently, it might be possible that the observed negative effects are nothing but an econometric residual generated by model-dependent interpolation. To address this concern, I extend the donor group to Sharia Law compliant countries that did not approve a constitutional reform introducing an SSL clause within their constitutional framework over the studied period.

[48] This is equivalent to an YCDR of 2.33%.
[49] Results are available upon request.

This extended sample includes Bahrain, Indonesia, Jordan, and UAE. Note that the inclusion of Sharia Law compliant countries should 'bias' the specification towards finding no results. In particular, if the Basic System caused no marginal changes in Saudi Arabia's Sharia Law compliance, then the RMSPE ratio should be close to one, and no effect should be identified. The constructed synthetic control includes 2.48% UAE, 50.7% Bahrain, and 46.82% Gabon. After this resampling process, results are qualitatively unchanged. The identified negative effects remain large in magnitude: Saudi Arabia's GDP per capita is 15.71% ($1,856.86) lower than its synthetic counterfactual's income per capita level, as measured in 2001.[50] Furthermore, the RMSPE ratio remains large, equal to 7.12. These results are plot in Figure A9.

This analysis confirms that baseline results are not driven by model-dependent interpolation, providing further support for their statistical robustness.

All in all, these robustness tests confirm the existence of large economic costs associated with a marginal increase in a country's degree of Sharia compliance.

The observed consistency in results for two significantly different countries, namely Mauritania and Saudi Arabia, and for two different settings is reassuring. However, extrapolating conclusions from internally valid results is a practice which should be exercise with extreme caution.

8. Conclusion and policy implications

This is the first study investigating the economic consequences of the institutionalization of pre-existing social norms within a country's legal system. In particular, I assess whether framing the Sharia Law within a country's legal framework induce material economic effects behind the

[50] This is equivalent to an YCDR of 1.84%.

direct and well know economic consequences that religious and cultural practice have on a country's economic performance.

To overcome the severe endogeneity characterizing this research question, I apply the synthetic control methodology discussed in Abadie and Gardeazabal (2003) to a country (Mauritania) in which the introduction of Sharia Law within its legal and judicial systems was not surrounded by other major confounding events. This setting allows me to identify large economic costs associated to the institutionalization of the Islamic Law within the Mauritanian legal and judicial systems. Synthetic Mauritania outperforms systematically the treated unit, reporting a GDP per capita 8.69% higher than real Mauritania's income per capita, as computed in 1991.

As with most case study methodologies, the synthetic control approach provides virtually exclusively internally valid results. To assess whether these findings are case-specific, I overcome the unavailability of other clean settings to study the effects of the introduction of Sharia Law within a country's legal and judicial systems by analyzing the economic consequences of a marginal increase in its enforcement level. By exploiting the introduction of an "ornamental constitution" in Saudi Arabia (1992), I show that qualitatively similar results can be identified in a significantly different setting. The identified consistency in sign provides support for that the governmental enforcement of cultural and religious practices materially reduce people's social and economic freedom, ultimately resulting in worsened economic conditions. However, the degree to which the estimated effects can be extrapolated and applied to radically different settings remains open for debate.

Chapter 2: The Worst of Both Worlds:

Megamergers are Anti-Competitive and Inefficient

1. Introduction

Megamergers can have massive implications for consumer welfare and for society as a whole. These large deals, such as the recent Bayer-Monsanto $66 billion merger, are always deeply scrutinized by anti-trust authority and policy makers, capturing media attention and reflecting consumers' concerns about their potential anti-competitive effects.[51] Yet, the emergence of a globalized economy has contributed to a massive increase in firms' minimum efficient scale (Doidge, Karoly and Stulz (2012), Larkin, Grullon and Michaely (2018), Lattanzio, Megginson and Sanati (2019)). These large transactions might thus be necessary to achieve and sustain operating efficiency on a global scale. However, despite survey evidence indicates that managers' main motivation for these deals is indeed to create value through operational synergies (e.g., Mukherjee, Kiymaz and Baker (2004)), it is still unclear whether they deliver these positive effects (Betton, Eckbo and Thorburn (2008)). Whereas some studies find improvements in operating performance or productivity after M&A transactions (Healy, Palepu, and Ruback (1992), Maksimovic and Phillips (2001), Devos, Kadapakkam and Krishnamurthy (2009)), others document the opposite effect (Herman and Lowenstein (1988), Ravenscraft and Scherer (1989), Gosh (2001)).

One of the main issues with assessing a merged-firm's performance is that the target firm's accounting information is no longer separately available. That makes synergies difficult to be

[51] "The $66 billion Bayer-Monsanto merger just got a major green light – but farmers are terrified", Business Insider, May 29th, 2018.

assessed in this setting (Fee, Hadlock and Pierce (2012)). Some authors match the acquirers (treated firms) to non-acquirers (control firms) to calculate abnormal levels of operating performance (Gosh (2001), Bloningen and Pierce (2017)), but this approach has several limitations. First, finding matching firms that closely resemble acquirers on several characteristics can be difficult and sometimes infeasible (Collier, Mahoney, Seawright (2004), Betton, Eckbo and Thorburn (2008)). This is particularly true when both the target and the acquirer are large, public corporations. Second, the traditional matching approach does not account for the impact of the unobservable contribution of the target to the merged firm performance. Consequently, even if the matching firms closely resemble the acquirers during the pre-merger period, the imbalance in firm characteristics between the treated and control firms will increase during the post-merger period. Finally, studies using a matching approach typically match on firm characteristics at one particular point in time (e.g., one year before the event), a procedure that cannot take into account important trends in the underlying variables. This issue is further exacerbated by the accounting consolidation of the acquiring and target firms, which might induce measurement errors in proxies for operating performance over the post-treatment period (Custodio (2014)). In sum, standard methods are poorly suited to take into account the economic and accounting consolidation of the acquirer and the target over the post-merger period.

We address these empirical limitations by using the synthetic control method (Abadie and Gardeazabal (2003) and Abadie, Dimond and Hainmueller (2010, 2015)) to construct counterfactuals for acquirers' post-merger operating performance. Under this approach, we form tracking portfolios of firms to create both synthetic target and acquirer firms. These synthetic firms closely mimic the pre-merger operating performance of the target and the acquirer, respectively, over the five years preceding the M&A transaction. We then "merge" the two

synthetic firms to form a post-merger counterfactual for the merged firm, thereby creating a "synthetic merger". We provide a detailed discussion of this methodological approach below.

This method provides a noticeably better counterfactual than traditional approaches do in this setting. Our analysis indicates that the synthetic control method generates lower root mean square prediction errors (RMSPE) and a larger sample of closely-matched observations than propensity score matching does. Furthermore, we find that the synthetic control method generates counterfactuals that display similar pre-merger trends in the outcome variables, such as return on assets and its components, as the M&A firms.

We begin our analysis by examining firms' post-merger operating performance. Consistent with extant literature, this initial assessment provides us with mixed evidence (Gosh (2001), Maksimovic, Phillips, and Yang (2013), Custodio (2014), Moatti, Ren, Anand and Dussauge (2015), among others). In particular, when we use return on assets (ROA) as a proxy for operating performance, we find limited evidence that M&A firms underperform the control firms.

These preliminary results suggests that megamergers might cause mild financial consequences. However, it is necessary to exercise caution in interpreting these findings. As most asset-based measures of operating performance, ROA is characterized by high sensitivity to both measures of operating efficiency and market power. To understand the competing forces driving our results, it is thus critical to decompose ROA into its main components: the Lerner Index, a proxy for a firm's market power, and Asset Utilization, a measure of a firm's operational efficiency (Aghion et al. (2005) and Grullon, Larkin and Michaely (2018)). Importantly, since our methodology allows us to develop a proxy for the unobservable target's post-transaction performance, we are able—for the first time to our knowledge— to study these two forces in isolation and at the firm level, rather than employing micro-settings or plant level data (McGuckin

and Nguyen (1995), Maksimovic and Phillips (2001), Gugler et al. (2003), and Bertrand and Zitouna (2008)). Indeed, absent a valid proxy for the target's post acquisition performance it would be impossible to isolate the competitive and efficiency effects caused by the combination of two previously independent firms.

We find that on average, the Lerner Index significantly increases after M&A transactions, suggesting mega-mergers increase an acquiring firm's markups,[52] and Asset Utilization falls, suggesting that merged firms become less efficient. These results show that, contrary to claims that mergers create operational synergies that benefit a merging firm's stakeholders (Calem, Dor and Rizo (1999), Chipty (2001), Sapienza (2002), Shahrur (2005), Erel (2011), Cho and Wang (2016), among others), instead they are worst-of-both-worlds, in the sense that merging firms are, on average, not only increasing their markups and profit margins, but also are doing so at the expense of operational efficiency.

To identify the potential channels driving our results, we split the sample in useful ways. Specifically, we find that concentrated industries have higher increases in markups than their more competitive counterparts. Likewise, we find that firms involved in larger deals experience larger increases in markups and also greater decreases in operational efficiency. These findings are consistent with theories that suggest that managers in less competitive markets are more likely to engage in empire building because they can offset their inefficiencies by exercising market power (Bertrand and Mullainathan (2003)).

[52] Our Lerner Index results are robust to the use of any sale-based measure of operating efficiency (e.g. return on sales (ROS), gross margin, and intangible asset adjusted return on capital (IAROC)). By construction, these sales based measures are more sensitive to a market power effect, than to changes in asset-based measures of operating efficiency. Furthermore, it is important to emphasize that these results are not driven by the reporting consequences of the use of fair value accounting (Custodio, 2014). To the extent to which is possible, we address these concerns in building our synthetic counterfactuals, as discusses in the following section of the paper.

Industry concentration levels have been increasing over the last two decades (see e.g., Grullon, Larkin and Michaely (2018)). Hence, if market power is one of the key economic channels behind our main findings, we should find stronger evidence of substitution between efficiency and markups during the latter part of our sample. We do. Consistent with this empirical prediction, we find that the decrease in operational efficiency after M&A transactions occurs exclusively after 2000, and the increase in markups are larger during this period.

Finally, we split the sample based on whether the acquisition is horizontal (same 2-digit SIC code for both the acquirer and the target) or diversifying. Consistent with the market power hypothesis, we find that the increase in the Lerner Index is significantly larger for horizontal acquisitions. Moreover, we find that diversifying acquisitions tend to generate larger inefficiencies, supporting the idea that conglomerates tend to destroy value by diluting their effort and attention across multiple industries (Macquieira, Megginson and Lance (1998), Scharfstein and Stein (2002), Burch and Nanda (2003), Buchuk et al. (2014)).

Further underscoring the idea that M&A firms subsidize their inefficiencies by charging higher markups, we find that there is a substitution effect. That is, firms that increase their markups also tend to experience significant declines in operational efficiency. This substitution effect remains economically and statistically significant even after controlling for other factors that affect operating performance.

We conclude our analysis by extending the recent findings reported in Blonigen and Pierce (2017). Using plant-level data from firms in the manufacturing industry, these authors find that, while markups significantly increase after M&A transactions, there are no efficiency effects. When we restrict our sample to only manufacturing firms, we confirm their results, providing external validation to our methodological approach. It is important to note that our paper expands their

results in two main ways. First, it shows that the effect of M&A transactions on markups is not restricted to manufacturing firms with plant-level data. Second, it demonstrates that M&As not only affect firm's plants, but also the entire firm.

2. Methodology, Key Variables and Sample Overview

2.1. Methodology

Estimating the treatment effect of an acquisition on firm performance is particularly challenging. First, the treatment assignment is non-random. Firms choose to be acquirers, and neither acquirers or targets are like the average firm in the population. Dealing with this self-selection issue is particularly challenging because predicting ex ante what firms will be an acquirer or be a target is difficult. Even the best predictive models cannot reliably identify future acquirers, with pseudo R2s ranging between 5% and 10% (Routledge, Sacchetto and Smith (2018)). More complicated still is the task of predicting acquisition targets (Betton, Eckbo and Thorburn (2008)), for which existing selection models report pseudo R2s below 5%. The lack of reliable predictive models has potential severe consequences on the effectiveness of a propensity score matching approach at achieving covariates balance. If the strong ignorability assumption stated in Rosenbaum and Rubin (1983a, 1983b) is violated, the low quality of the prediction model might exacerbate estimation biases, rather than mitigating them. This effect is particularly severe if predictors are measured at the event date (Ho, Imai, King and Stuart, 2007). Despite these limitations, propensity score matching is widely used in the literature (Heyman, Sjoholm and Tingvall (2007), Bertrand and Zitouna (2008), Arnold and Jovorcik (2009), Bandick and Gorg (2010), Fresard, Hoberg and Phillips (2013)), alongside with Heckman selection models as in Acharya (1988) and "truncated regression" specifications as in Ebcko, Maksimovic and Williams (1990), which, however, have similar limitations.

A second major challenge in identifying the treatment effect of an acquisition on firm performance is due to the financial and operating transformations induced by the successful completion of an M&A transaction. As shown in Figure B1, although one might be able to find a good match over the pre-treatment period, post-merger the new firm is likely to be fundamentally different from what the actual merging firms were before and also fundamentally different from any useful counterfactual.

We address these two major issues by using the synthetic control method developed in Abadie et al. (2003, 2010, 2015), and following studies. First, we deal with selection issues regarding the identification of a valid control unit for the acquiring firm. Given acquirers' distinctiveness, the identification of a suitable single comparison unit is challenging (Collier et al. (2004), George and Bennet (2005), Gerring (2007)). Furthermore, typical matching procedures treat acquirers' characteristics as observed at the end of the fiscal year preceding the transaction, without taking into account the pre-treatment dynamics characterizing the treated unit. The synthetic control method replaces a single matching control firm with a portfolio of control units that best replicates both level and trend of the treated unit's performance over the pre-treatment period, while simultaneously balancing a set of economic predictors of the selected dependent variable.[53] Absent any confounding events, the post-treatment performance of the synthetic acquirer can be interpreted as the performance the acquiring firm would have experienced absent the successful completion of the M&A transaction.

[53] Let X_1 represents the vector of used predictors for the studied unit, and Y_1 be the vector reporting pre-treatment values for the outcome variable. Analogously, let X_0 be the matrix of predictors for units in the donor group, and Y_0 the matrix reporting values of the outcome variable for all the potential controls. The weights are attributed to each unit in the donor group such that the distance function $D = (XX_1 - XX_0WW)'VV(XX_1 - XX_0WW)$ is minimized. W is the vector of weights, and V is a positive-definite diagonal matrix. The methodology is discussed in detail in Abadie et al. (2003, 2010, and 2015).

We repeat the procedure for the target firm. The resulting synthetic target closely replicates the acquired firm's pre-acquisition performance level and trend, furthermore allowing as to generate an internally consistent estimate for how the target would have performed if the M&A transaction would not have occurred. This otherwise unobservable time-series allow us to isolate the performance effect of the target on the merged firm.

We then build a synthetic merged firm by summing the two synthetic counterfactuals over the post-treatment period. This virtual consolidation allows us to build a time-series proxing for the merged firm's expected performance over the three year following the transactions, conditional on the M&A deal having no direct performance consequences. Furthermore, we inflate the synthetic acquirer's post-treatment assets by the deal-specific goodwill, and we adjust the synthetic target's post-merger assets by inflating them by its book-to-market ratio as at the end of the fiscal year preceding the transaction. [54] This process should allow us to mitigate concerns related to the eventual confounding effects of purchase accounting, which has been shown to result in deflated asset-based performance measures for the consolidated firm (Custodio (2014)).[55]

This strategy allows us to develop a valid counterfactual to isolate the performance effects of M&A transactions. In particular, our tests are based upon a sample in which each acquiring firm is matched with its synthetic counterfactual over the pre-treatment period, while the post-acquisition merged firm is matched with the synthetic merged firm over the post treatment period. Provided that both the synthetic acquirers and the synthetic targets replicate closely the treated unit, any difference in performance between the post-acquisition acquiring firms and the synthetic

[54] Results are robust to building the synthetic merger computing the value-weighted average of the post-treatment performance of the synthetic acquirer and of the synthetic target, where weights are based upon each firms' market capitalization as observed at the end of the fiscal year preceding the transaction.
[55] Results are not dependent upon this adjustment.

merged firm can be ultimately identified as the treatment effect of an acquisition on firm performance.

2.2. Data and Sample

2.2.1 M&A Data

To form our M&A sample, we collect all announced and completed U.S. M&A deals with announcement dates between January 1, 1979, and December 31, 2014 covered by the Mergers and Acquisitions database of Thomson Financial's SDC Database.[56] We identify all completed deals where the acquirer owns less than 50% of the target firm prior to the bid and gains control of more than 50% of the target firm after the deal completion. We retain only domestic deals, and we require both the acquirer and the target to be publicly traded to ensure the availability of accounting data. Finally, we require that (1) the deal size is larger than $100 million and (2) that neither the acquirer nor the target firm are financial firms (SIC 6000-6999). Since the efficiency of the synthetic control method increases in the length of the pre-treatment period (Abadie et al., 2015), we retain all deals for which all observations for the selected performance indicator are available over the 5 years preceding the event. As shown in Appendix A, this data selection procedure is extremely demanding. The resulting sample includes 383 mega-deals with an average size of approximately $1.4 billion, and representing about 15% of the universe of public to public domestic M&A deals completed in the U.S. between 1979 and 2014. While this evident selection issue imposes us to abstain from any extrapolation, the use of the synthetic control methodology allows us to get an internally valid estimate of the economic consequences of megamergers. Table B1 presents the temporal and industry distribution of the retained deals.

[56] Our sample ends in 2014 because we require the availability of three post-transaction fiscal years with complete observations for the observation to be included in our analysis.

2.2.2 Measuring Performance and Synergistic Gains

Following extant literature (Wang and Xie (2009), Custodio (2014), Qian and Zhu (2018), among others), we use return on assets (ROA) as a measure of profitability. ROA is unaffected by changes in capital structure or by the presence of unusual and non-recurring items, and it has been shown in simulations to be superior to any other commonly use proxy in detecting abnormal operating performance (Barber and Lyon, 1996).

Changes in profitability following an M&A transaction arise from either changes in post-merger efficiency or changes in markups. It is thus important to examine whether the empirical relation between the successful completion of an M&A transaction and the acquiring firm profitability stems from higher profits margins, higher operational efficiency, or both. To this end, we follow Aghion et al. (2005) and Grullon, Larkin and Michaely (2018) and decompose ROA into two components: the Lerner Index and the Asset Utilization ratio. The Lerner Index captures the extent to which equilibrium prices exceed marginal costs; the Asset Utilization ratio measures a firm's efficiency in exploiting its assets to generate sales. Consistent with Aghion et al. (2005) and Grullon, Larkin and Michaely (2018), we measure the Lerner Index as operating income before depreciation (OIBDP) divided by total sales (SALE).[57] Asset utilization is defined as total sales divided by total assets (AT). We complete the sample by collecting data on firm size, total sales, Tobin's Q, leverage, cash holdings, payout ratio, capital expenditures and R&D investments. Detail variables definition are provided in Appendix B.

Table B2 presents summary statistics for the sample. Consistent with extant literature, acquirers are significantly larger, have higher ROA, and have larger cash holdings than their

[57] We also use alternative sales-based measure of profitability to the existence of mark-up driven effects caused by the successful completion of an M&A deal. In particular, we use return on sales (ROS), the Intangibility Adjusted Return on Capital (IAROC hereinafter) developed in Peters and Taylor (2017) and in Ayyagari, Demirguc-Kunt and Maksimovic (2018) and the gross margin as alternative proxy for firms' sales-based performance.

targets. We identify no difference between target and acquirers in terms of leverage and operating efficiency (asset utilization), but targets have significantly lower Lerner Index. All in all, the identified sample does not differ materially from those used in other studies of mergers between public firms (Gaspar, Massa and Matos (2005), Harford, Jenter and Li (2011), Bena and Li (2014)).

2.2.3 Building synthetic counterfactuals

For each of the 383 mega-mergers we attempt to build a synthetic counterfactual for both the acquiring and the acquired firm. In order to construct an economically meaningful control unit, we restrict the sample of potential controls or donor pool to (1) all firms operating within the same two-digit SIC code industry, (2) for which the full time-series for the dependent variable is available over the period from five years preceding the transaction to three years after the successful completion of the deal, and (3) which are not involved in an M&A deal in the fiscal year in which the transaction takes place. Appendix C provides a detail example of this sampling procedure.

We use the five fiscal years preceding the transaction to form our synthetic controls. We build upon extant M&A literature in selecting five important covariates to constrain the optimization process. First, we condition the construction of the synthetic counterfactual upon matching on the mean size and Tobin's Q, as measured over the pre-treatment period (Bena, Li (2014)). Second, we take into consideration the treated firm's capital structure and financial flexibility by controlling for mean cash holding and leverage, as observed over the five years preceding the transaction (Betton, Eckbo and Thorburn (2008)). Finally, we control for the mean value of the selected performance indicator over the pre-treatment period and as the end of the

fiscal year preceding the successful completion of the M&A transaction, as suggested in Barber and Lyon (1996).[58]

The optimization model allows us to generate reliable counterfactuals for 281 deals when ROA, Asset Utilization, or the Lerner Index are the dependent variable, and for 270 deals when ROS is the dependent variable. To assess the quality of the produced matching, we compute for each treated firm the root mean squared prediction error (RMSPE hereinafter) over the five years preceding the transaction. The RMSPE reflects the average closeness of the synthetic counterfactual from the treated unit over the pre-transaction period. While the implied difference-in-difference setting does not require a zero RMSPE for providing internally valid estimates of the treatment effect, the lower the RMSPE the higher the economic and statistical reliability of the constructed counterfactual. Table B3 reports descriptive statistics for the quality of the originated matched sample over the pre-treatment period.

About 75% of the constructed counterfactuals have RMSPE below 10% for all the selected dependent variables. In order to understand whether this methodology generates better matches with respect to what it would be achieved by employing a propensity score matching based upon the same selected economic predictors, we match the 283 (270 for ROS) pairs of acquiring firms and acquired firms to their closest neighbor based upon a propensity score estimated at the industry-year level. The propensity score matched sample reports systematically higher RMSPE—20% to 80% worse on average, depending on the dependent variable—and it typically generates only 40% as many high quality counterfactuals (RMSPE below 2.5%) as the synthetic control

[58] Results are robust to further controlling for CAPEX, R&D activity, and payout ratio. However, as the number of the selected determinants increases, the feasible results space shrinks, causing the number of successfully matched units to decrease. Since size is the only systematically significant predictor of activity in the M&A market (Betton, Eckbo and Thorburn (2008)), we report baseline results based upon our conservative matching procedures.

method does. These limitations suggest that the use of the synthetic control method might indeed yield more accurate estimates of the treatment effect of an M&A transaction on firm performance.

To assess further the reliability of the generated sample, we follow Villalonga (2000) and Lattanzio and Megginson (2018) in assessing whether treated and control units have statistically indistinguishable trends over the pre-treatment period. In particular, we estimate the following regression model:

$$Y_{f,t} = \alpha + \beta_1 * M\&A + \sum_{t=-4}^{-1} \sum_{j=2}^{5} \beta_j * year_t + \sum_{t=-4}^{-1} \sum_{k=6}^{9} \beta_k * year_t * M\&A + \varepsilon_{f,t} \qquad (1)$$

where f indexes firms and t represents "relative" years, that is by how many years the observation precede a complete M&A deal. $Y_{f,t}$ is one of our performance indicator (ROA, ROS or IAROC); M&A is a dummy variable set equal to 1 for all the "real" units, 0 for their synthetic counterfactuals; $year_t$ is a dummy variable set equal to 1 if the observation precedes the deal by t years, 0 otherwise. We follow Berger, Butler, Hu, and Zekhnini (2018) and we bootstrap the standard errors to take into account the possibility that the same control unit might be included in multiple synthetic counterfactuals. Finding statistically significant β_j, with j ϵ [6 ; 9] would indicate the existence of statistically different pre-treatment trends. As reported in Table B4, no statistically different trend can be identified between the treated units and the matched synthetic controls, further validating the robustness of the proposed methodology.[59]

Next, we use the resulting matched sample to assess the treatment effect of a M&A on firm performance by running difference-in-difference specifications.

[59] To avoid redundancy, we report estimates for Model 1 for ROA, exclusively. Consistent results can be identified for Asset Utilization, the Lerner Index, and ROS, and these additional tables are available upon request.

3. Assessing acquirers' post-merger performance

Evidence reported in Section II provides support for the validity of the constructed counterfactuals. We can thus employ the resulting matched sample to assess and evaluate acquirers' operating performance over the post-merger period, ultimately isolating the eventual abnormal performance induced by the successful completion of an M&A transaction.

Following extant literature, we begin our analyses using Return On Assets (ROA) to measure operating performance. As described in Appendix B, ROA is defined as operating income before depreciation (OIBDP) divided by total assets (AT). We run a set of difference-in-differences regressions estimated over the 6 years surrounding the treatment date. Our estimates are reported in Table B5, Panel A.

We estimate this model for the full sample (Column I), and for subsamples including all deals for which both the acquirer and the target's synthetic counterfactual RMSPE is below 10% (Column II), 5% (Column III), and 2.5% (Column IV), respectively. Standard errors are bootstrapped to account for cross-sectional and time-series correlations induced by the possible inclusion of the same firm in multiple synthetic counterfactuals.[60] Like other results from the literature, our estimates for ROA are mixed. Our difference-in-differences estimates (Panel A, Column (1)) report a strong, negative association between the successful completion of an M&A deal and the acquiring firm's operating performance. However, as we refine our matched sample to include closely matched firms, exclusively, the estimated average treatment effect on the treated flips positive, but it becomes indistinguishable from zero. Estimates based upon our propensity score matched sample confirm the lack of robust results (Panel B), but, as one would have

[60] The resulting counterfactuals include 27 firms, on average. This feature is appealing for two reasons. First, it is unlikely that one untreated firm's idiosyncratic performance might drive the synthetic counterfactual's overall performance. Second, the resulting degree of diversification mitigates concern that eventual firm level shocks might invalidate the economic reliability of the counterfactual over the post-treatment period.

expected, the produced residuals are significantly larger than those obtained using our synthetic merger approach.

Results reported in Table B5 suggests that megamergers might cause mild financial consequences on acquiring firms. However, we need to exercise caution at interpreting these findings. First, Custodio (2014) shows that asset recognition under purchasing accounting results in deflated asset-based measures of operating performance over the post-acquisition period. However, as we do adjust synthetic acquirers' assets for the deal-specific goodwill and we inflate synthetic targets' assets using their book to market ratio as at the end of the year preceding the acquisition, our methodology should be able to mitigate any financial consequences induced by this reporting issue. Second, as with most asset-based measures of operating performance, ROA is highly sensitive to both changes in operating efficiency and market power. However, as a target's contribution to a merged firm's performance is unobservable, disentangling these two forces is extremely challenging. Our methodology overcomes this limitation. The synthetic control method provide us with a reliable proxy for the target firm's post-transaction performance, thus allowing us to disentangle the competitive and efficiency consequences of an M&A deal after controlling for the relative contribution of both the target and the acquirer to the merged firm's post-transaction performance.

To assess the relative contribution of these two economic forces, we build upon Aghion et al. (2005), Grullon, Larkin and Michaely (2018), and Bloningen and Pierce (2017) to decompose ROA in its two component: asset utilization, defined as the ratio of total sales (SALE) to total assets (AT), and an approximation of the Lerner Index, defined as operating income before depreciation (OIBD) to total sales (SALE). Asset utilization captures a firms operating efficiency,

whereas the Lerner Index represents the extent to which equilibrium prices exceed marginal costs, proxying for a firm's market power.

We replicate our analyses using these two components as the dependent variable of our difference-in-differences specification. Table B6, Panel A reports our estimates for the sample based upon acquirers and targets' asset utilization, while Table B6, Panel B reports results for the Lerner Index.

Our estimates confirm that the mixed ROA findings result from two counter-balancing forces. First, acquirers' operating efficiency (asset utilization, reported in Panel A) materially decreases over the post-treatment period, as compared to their synthetic counterfactuals. Second, acquirers exploit their increase market power to extract higher profit margins, achieving an increase in their Lerner Index (Panel B).[61]

The identified results are problematic for both consumers and policy makers as they provide evidence that contradicts the claim that megamergers create operational synergies that benefit all firm's stakeholders. The coexistence of increased profit margins (market power hypothesis) and decreased operating efficiency (quite life / empire building hypothesis) are indeed strongly suggestive that M&As are, on average, inefficient and anti-competitive. The average deal in our sample ultimately results in acquirers aggressively extracting rents from consumers, but being unable to fully internalizing the realized gains, which are partially captured by insiders.

To provide further support for our findings, we hypothesize that alternative measures of sales-based operating performance should significantly increase over the post-acquisition period.

[61] Replicating Table VI upon our propensity score matched sample delivers similar results. However, the estimated standard errors are extremely larger, and the resulted estimates are statistically significant at the 10% level for asset utilization, and marginally insignificant for the Lerner Index. These findings further confirm the increase in power that can be achieved by relying upon our synthetic control matched sample, rather than other commonly used methodology.

Indeed, these measures should display a higher sensitivity to a firm's profit margins and market power than ROA does. Consequently, for sales-based measures of profitability the previously identified market power effect should dominates the consequences of eventual merger-induced efficiency losses. We find this to be the case. As shown in Table B7, we identify robust evidence of an increase in post-merger operating performance when we use return on sales (ROS), the intangible adjusted return on capital (IAROC, see Peters and Taylor (2017) and Ayyagari, Demirguc-Kunt and Maksimovic (2018)), or the gross margin as the dependent variable.

Pairing these results with those reported in Table B5 and in Table B6, we can thus conclude that, on average, successfully completed megamergers are anti-competitive and cause material efficiency losses.

Before assessing the economic validity of our results, it is critical to further validate our econometric approach. To test whether our synthetic merger generates systematic bias, we randomly select pairs of acquirers and targets and we replicate our baseline tests. In particular, we test whether the sum of the randomly selected firms (placebo merger) and the sum of their synthetic counterfactuals (synthetic placebo merger) experience the same performance over the post-treatment period. As reported in Table B8, no statistically significant effect can be identified for any of the used performance indicators, suggesting the previously identified estimates are unlikely to be the result of systematic biased induced by the proposed methodology.

4. Subsample analyses and Robustness Tests

In order to validate our results and to develop a more comprehensive understanding of our findings, we replicate our analyses on several carefully selected subsamples. First, because operating inefficiencies are unsustainable in a highly competitive environment (Giroud and Mueller (2010)), we split our sample based upon the degree of competition faced by the acquiring

firm as at the end of the fiscal year preceding the successful completion of the M&A deal. In particular, we identify concentrated industries as those whose Herfindahl–Hirschman Index (HHI hereinafter) is greater or equal to 1,800 HHI points, which is the cutoff indicated by the U.S. Department of Justice for identifying highly concentrated industries. Using the Hoberg and Phillips (2015) measure of product market competition produces similar conclusions. Table B9 reports our analyses for both asset utilization (Panel A) and for the Lerner Index (Panel B).

We find that inefficiencies are more severe following transactions in which the acquirer operates in concentrated industries. Similarly, the increase in acquirers' profit margins (the Lerner Index) is higher when the buyer operates in a low competition environment. All in all, these findings are consistent with managers in less competitive markets being more likely to engage in empire building because they can offset the consequent inefficiencies by exercising market power (Bertrand and Mullainathan (2003)).

As changes in both market power and industry concentration increase in deal size, we should identify consistent results if we replicate Table B9 by splitting the studied deals by their transaction value, rather than by pre-deal HHI levels. We do. Results reported in Table B10 are consistent with our HHI-based analyses, indicating that operating efficiency decreases virtually exclusively when large deals are completed, and that the Lerner Index increases less following small M&A transactions.

Industry concentrations have been increasing over the last two decades. In particular, a massive consolidation took place in the late '90s, in the aftermath of the dot-com bubble (Moeller, Schlingemann and Stulz (2005), Grullon, Larkin and Michaely (2018), and Lattanzio, Megginson and Sanati (2019)). Hence, if market power is one of the key economic channels behind our main findings, we should find stronger evidence of substitution between efficiency and markups during

the latter part of our sample. Consistent with our hypothesis, Table B11 shows that the decrease in operational efficiency after M&A transactions occurs exclusively after 2000, while the increase in markups are significantly larger during this period.

Horizontal mergers, defined as M&A in which both the acquiring firm and the acquired firm operate in the same 2-digit SIC code industry, should induce a larger increase in market power than diversifying mergers. Similarly, diversifying acquisitions should be associated with greater efficiency destruction (Maquieira, Megginson and Nail (1998)). We test these hypotheses by splitting our sample based on whether the acquirer and target are competitors, and we re-estimate our difference-in-differences specifications for both subsamples. Results reported in Table B12 confirm our priors. Consistent with the market power hypothesis, we find that the increase in the Lerner Index is larger for horizontal acquisitions. Furthermore, we find that diversifying acquisitions generate larger inefficiencies, on average, supporting the hypothesis that conglomerates tend to destroy value by diluting their effort and attention across multiple industries (see Scharfstein and Stein (2002), Burch and Nanda (2003), Buchuk et al. (2014)).

Our subsample estimates validate our initial results, providing strong support that M&A deals induce material anti-competitive effects, while increasing operating inefficiencies. However, the reported results for asset utilization and for the Lerner Index capture the average treatment effects estimated in separate estimation processes. Thus, further analyses are necessary to assess whether a firm-level cross-subsidization is taking place. That is, are acquiring firms experiencing the largest drop in operating efficiency those which abuse the most from their increased market power, and vice versa?

We test this hypothesis by studying the determinants of acquirers' abnormal performance over the three years following the event date. Specifically, we estimate the following regression model:

$$\Delta Y_{f,t} = \alpha + \beta_1 X_{f,t} + \varepsilon_{f,t} \quad (2)$$

where $\Delta Y_{f,t}$ is the difference in performance (Asset Utilization and Lerner Index) between the merged firm and its synthetic counterfactual, and $X_{f,t}$ is a matrix including several acquiring firm level and deal-level characteristics. Namely, we control for firm size, the acquirer-target premium, defined as the difference in Tobin's Q between the two firms as measured at the end of the calendar year preceding the transaction, the acquiring firm Tobin's Q, leverage, cash holdings, the transaction value as a ratio to the acquirer's total assets, a dummy set equal to one if the acquirer is a serial acquirer, zero otherwise, a dummy set equal to one if the acquiring firm and the target operate in different industry, as defined using the 2-digit SIC code (Diversifying M&A), an indicator variable set equal to one if the consideration structure includes shares, exclusively. A detail definition for all the used controls is provided in Appendix B. As usual, standard errors are bootstrapped to account for the possible inclusion of the same control units in multiple synthetic counterfactuals.

Table B13, Panel A, reports our estimates for asset utilization. The decrease in operating efficiency is strongly negative associated to higher profit margins (Lerner Index), confirming a firm-level subsidization of inefficiencies through market power exploitation. Furthermore, we identify a positive association between asset utilization and Tobin's Q, and a negative association with book leverage, cash holdings and shares only acquisitions. The negative association between asset utilization and cash holding confirms that the rise of inefficiencies is consistent with an empire building / quite life story, in which insiders capture private benefits generated from the

successful competition of the M&A transaction (Bertrand, Mullainathan (2003)). To further validate our results supporting the hypothesis that insiders subsidies inefficiencies through higher markups, Table B13, Panel B replicates results in Panel A by employing the Lerner Index as dependent variable and asset utilization as a control variable. Again, we identify a strong, negative association between the Lerner Index and operating efficiencies, confirming that the results established in Table B6 are capturing a firm-level cross-subsidization of inefficiencies through rents extracted from consumers through the acquirers' increased markups.

Bloningen and Pierce (2017) use plant-level data from firms operating in the manufacturing industry to assess the operating efficiency and market power consequences of completed M&A transactions. In their study they find that while markups significantly increase after M&A transactions, there are no efficiency effects. We replicate their results at the firm level in an attempt to provide external validation to our methodological approach. In particular, we estimate our DiD specification over the period 1997-2007 for those acquisitions in which acquiring firms operate in the manufacturing industry, exclusively. Table B14 reports our analyses.

Our results confirm Bloningen and Pierce (2017)'s findings, providing strong support for that M&A transactions in the manufacturing industry induce limited operating efficiency effects, while causing an increase in acquirers' markups.

This successful quasi-replication provides an external validation to our methodology, further expanding Bloningen and Pierce (2017)'s results in three main ways. First, we show that the effects of M&A transactions on markups are not restricted to manufacturing firms. One main advantage of using M&A transactions from different industries is that it allows us to test the predictions of the market power hypothesis. Specifically, it allows us to examine how cross-sectional differences in concentration levels affect post-merger performance. Second, we confirm

that these results are not unique to plant-level analyses, but that they can be generalized to the firm level. Finally, to the best of our knowledge, our paper is the first one to document a significant substitution effect between measures of efficiency and markups after M&A transactions.

5. Conclusions

In this study we develop and use a novel identification strategy aimed at overcoming the major identification issues faced by scholars attempting to isolate synergistic gains (or losses) induced by the successful completion of a mega-merger. By building upon the synthetic control methodology developed in Abadie and Gardeazabal (2003) and Abadie, Dimond and Hainmueller (2010, 2015), we build tracking portfolios of firms to create both synthetic target and acquirer firms. We then "merge" the two synthetic firms to form a post-merger counterfactual for the merged firm, thereby creating a "synthetic merger".

After testing for whether the proposed methodology generates statistically more reliable counterfactual than other commonly used matching procedures, we use this novel identification strategy to study the performance effects caused by the successful completion of an M&A deal. Our results provide robust evidence that, on average, merging firms are subsidizing their inefficiencies by exploiting their increased market power. Results hold in several carefully selected sub-samples. Finally, we confirm results reported in Bloningen and Pierce (2017) based upon plant-level data for manufacturing firms. In particular, our estimates support that M&A transactions in the manufacturing industry induce limited operating efficiency effects, while causing a material increase in acquirers' market power.

While these findings are not directly generalizable to all successful mergers, they shed light on mega deals' overall economic consequences. As these deals are heavily scrutinized by regulators and as both the media and consumers often criticize these transactions, claiming that

they are likely to result in consumers' welfare destruction, studying this set of transactions is of paramount importance. Our results suggests that these fears are well-founded, furthermore showing that the resulting rents are not fully captured by the merging firms' shareholders, but rather by managers exploiting the increased market power to subsidize operating inefficiencies.

Finally, our paper provides corporate finance scholars with a novel methodology that can be broadly applied to answer several important questions for which, to date, a general consensus has not emerged yet due to the lack of a robust identification strategy.

Chapter 3: Can Restructuring Gains Be Sustained Without Ownership Changes? Evidence from Withdrawn Privatizations

"I don't make jokes. I just watch the government and report the facts"

Will Rogers (1925)

1. Introduction

Privatizations have reshaped the global economy. Since 1977, more than 100 governments in both emerging and developed countries have relied on this policy to enhance the competitiveness of their economic systems, raising approximately $3.5 trillion by successfully selling state-owned enterprises (SOEs) and assets to both domestic and international investors (Megginson [2017]). An extensive literature documents the economic consequences of these transactions, reporting often impressive performance improvements for the privatized companies, both in anticipation of divestment and after the share issue privatization (SIP) is executed.[62] Most studies examining both pre- and post-sale performance changes document improvements both before and after the SIP (Laurin and Bozec [2000], Wolf and Pollitt [2008] and, for China, Liao, Liu, and Wang [2014] and Tan et al. [2014]), but a few studies find that most or all of the recorded gains occur before the SIP transfers ownership from state to private investors (Martin and Parker [1995], Villalonga [2000], Dewenter and Malatesta [2001], Wolf [2009], and Goyal et al. [2018]).

[62] For recent surveys of the privatization literature, see Gupta, Schiller, Ma, and Tiongson (2001), Megginson and Netter (2001), Djankov and Murrell (2002), Estrin, Hanousek, Kòcenda, and Svejnar (2009), Fotak, Gao, and Megginson (2016), and Megginson (2017). The empirical evidence overwhelmingly indicates that even partial privatization is associated with significantly increasing revenue, profitability, efficiency, capital investment and investment efficiency, and dividend payments, and with declining financial leverage.

The existence of these endogenous pre-privatization trends suggests that a massive restructuring process takes place over the years immediately preceding these transactions. This raises a fundamental question concerning the causes of privatization programs long-term success: can a SOE lock in the reported restructuring gains absent successfully transitioning from state to private ownership?[63] That is, does the ownership transfer from political to private investors represent a necessary condition for the long-term success of privatization programs? The extant literature provides conflicting evidence: Boardman and Vining (1989), Ehrlich et al. (1994), Ramamurti (1997), Shleifer (1998), Shirley and Walsh (2000), Dewenter and Malatesta (2001), Karpoff (2002), O'Toole et al (2016) and Chen et al. (2017b) conclude that switching to private ownership is required to improve SOE performance, while Kole and Mulherin (1997) and Wolf and Pollitt (2008) show that SOEs can be competitive even without full privatization.

In this study we attempt to provide a rigorous answer to whether actual privatization is required to lock in pre-sale restructuring gains by studying a new, hand-collected sample of withdrawn privatizations. After governments file initial registration statements to sell shares in SOEs with the relevant national agency, a surprisingly large number of these share issue privatizations (SIPs) are ultimately withdrawn, which is consistent with findings reported in the emerging literature analyzing withdrawn private sector share issues in the United States. [64] Extant literature has largely ignored these failed privatizations, anecdotally considering them numerically and economically marginal. However, a detailed empirical analysis on the incidence of these events contradicts this idea: over the period from 1998 to 2016, 412 filed SIPs were ultimately

[63] Specific examples of privatization-related restructuring strategies are described in Ramamurti (1997), La Porta and Lopez-de-Silanes (1999), Dewenter and Malatesta (2001), Chong, Guillen, and Lopez-de-Silanes (2011), Santos (2016), and Subramanian and Megginson (2018), among others.

[64] See Mikkelson and Partch (1988), Dunbar (1998), Busaba, Benveniste, and Guo (2001), Busaba (2006), Dunbar and Foerster (2008), Lee and Masulis (2009), Boeh and Dunbar (2013). Helbing and Lucey (2017) is to date the only international study on withdrawn IPOs, focusing only on the main European markets: Britain, Germany, and France.

withdrawn, with governments consciously leaving on the table approximately $116 billion, globally. We hand-collect financial data for the seven years surrounding (three years before to three years after) 111 withdrawn and 166 successful SIPs occurring in 24 countries over 1998-2013.[65]

Since the reputational and financial costs of withdrawing a previously filed share issue are likely to be severe (Dunbar and Foerster [2008]), governments are unlikely to strategically register SIPs that are certain to fail. Consequently, all SOEs selected for privatizations are expected to be exposed to comparable restructuring process over the pre-transaction period. Consistent with the "anticipation" literature cited above, we confirm that the expectation of being exposed to the market discipline generates endogenous pre-privatization trends for all targeted firms. In particular, SOEs selected for privatization experience significant increases in profitability and operating efficiency, as well as a strong decline in labor intensity and capital expenditures over the three years preceding the event. No significant difference in trends over the pre-privatization period is identified between successful and withdrawn SIPs.

These findings indicate that we can explicitly control for both the non-random selection process governments use to select SOEs for privatization in a given year (Du and Liu [2015]), as well as for the previously overlooked restructuring effect by rigorously matching successful and withdrawn SIPs. This identification strategy allows us to robustly estimate the ultimate effect of the ownership transfer from political to private investors. However, the validity of the proposed setting hinges on our ability to successfully model the selection process leading governments to the final decision of withdrawing a previously filed SIP.

[65] Consistent with extant literature, we consider exclusively transactions for which at least one "post-event" year is available over the three fiscal years following the attempted privatization. This filter limits the sample of actual and attempted issuances to end in 2013. Before 1998 the number of available observations is extremely low, limiting our ability to analyze that period.

In order to shed light on the determinants of a government's decision to withdraw a previously filed privatization, we model a rational issuer's ex-ante probability to withdraw the SIPs included in our dataset. By employing linear as well as non-linear probability models, we find that both institutional and political factors play fundamental roles in explaining a government's decision to exercise this real option. Numerous studies highlight the role of politics in shaping privatization strategies (Biais and Perotti [1995], Gupta [2005], Belloc, Nicita, and Sepe [2014], and Piotroski and Zhang [2014, China]), but we are the first to show that governments' propensity to withdraw a previously filed SIP appears to be systematically larger in democratic countries. Consistent with Dinç and Gupta (2011) we identify the existence of a strong minority coalition party as the single most important political determinant of government's propensity to withdraw a SIP. This finding is consistent with political risk playing a major role in shaping privatization programs, ultimately determining their outcomes.

Since heavily indebted countries are more dependent on external capital markets and institutional investors, these governments need to carefully manage their reputation as reliable financial counterparties. Therefore, unsurprisingly, we find high levels of government debt to be associated with a lower propensity to withdraw a previously filed SIP. Similarly, high current deficit countries are more reluctant to withdraw a SIP, suggesting that public finance considerations play a non-negligible role in accelerating the successful completion of privatization programs. From a financial perspective, governments are less averse to withdrawing a SIP when the firm's operating efficiency is high. This is consistent with governments paying particular attention to those transactions involving their "crown jewels". Finally, in contrast with findings

reported in Busaba, Benvenise, and Guo (2001) and Bernstein (2015),[66] we identify no significant relationship between the decision to withdraw a previously filed SIP and stock market returns over the 30 trading days preceding the event, suggesting that market timing concerns have at most a marginal impact on a government's decision to withdraw a previously filed SIP. These results collectively call into question, first, whether governments manage SIP transactions for revenue maximization and, second, whether SIPs and share issues executed by privately owned firms are actually comparable, given their differing final objectives (Laffont and Tirole [1993], Shleifer [1998]) and sensitivities to market conditions.

The observed relevance of political factors in determining the ultimate success of an attempted privatization allows us to exploit them as a source of exogenous variation to quasi-randomize the treatment assignment. In particular, similarly to Dinç and Gupta (2011) we argue that a country's level of public debt and degree of political competitiveness represent valid instruments for the decision to withdraw a previously filed SIP.[67] The resulting propensity score matched samples allow us to isolate the ultimate effect of the ownership transfer from political to private investors from any confounding events taking place over the pre-treatment period. In particular, we estimate a set of difference in difference (DiD hereinafter) regressions to study the economic consequences of a SIP withdrawal on five major corporate values: profitability, measured as a firm's return on sales; operating efficiency, proxied by the ratio of net income to number of employees; labor intensity, defined as total number of employees divided by the firm's

[66] These papers focus on American privately-owned firms, identifying a strong negative relationship between stock market returns preceding the decision to withdraw a previously filed IPO and its economic consequences.

[67] The validity of the proposed instruments in discussed in detail in Section VI.

total assets; payout ratio, proxied by the ratio of total common dividends to total revenues; and capital expenditures, measured as the ratio of a firm's CAPEX to its total assets.[68]

Since roughly half of our sample of withdrawn and completed SIPs involve Chinese state-owned enterprises, we must explicitly account for the differences between share issue privatizations in China and elsewhere, beginning with the fact--as described in Sun and Tong (2003), Li et al. (2017) and Megginson (2017)—that Chinese SIPs are all primary capital-raising share offerings, whereas SIPs involve pure secondary offerings of government-owned shares almost everywhere else. Even more important for our purposes is the evidence that Chinese SOEs both over-invest (Liu and Siu [2011]) and invest very inefficiently (Chen et al. [2017a], Chen et al. [2017b]) relative to comparable private sector firms in China and elsewhere (Jaslowitzer, Megginson, and Rapp [2018]). This means that a privatization-related improvement in Chinese capital investment will manifest itself in a decline in SOE capital spending and an increase in capex efficiency. Additionally, there is less conclusive evidence that privatization improves Chinese SOE profitability (Sun and Tong (2003], Li et al. [2017]) than is true for most other countries, so profitability changes for both successful and withdrawn Chinese SIPs may be more muted than for the non-Chinese companies in our samples. As appropriate, we discuss the capex and profitability effects of SIP withdrawals separately for Chinese and non-Chinese companies.

Our tests provide strong support for the existence of state-ownership induced agency costs. Operating efficiency and the payout ratio decline dramatically post-withdrawal for those SOEs whose privatizations are not successfully completed, while their labor intensity increases compared with the selected counterfactuals. Capital expenditures decline for non-Chinese withdrawn SIPs post-event, but increase for withdrawn Chinese SIPs; both results suggest

[68] Variables are defined in Appendix D.

performance degradation after failed privatizations.[69] No major difference in profitability between successful and withdrawn SIPs is recorded over the post-treatment period for the full sample, but this is driven entirely by the static profitability effects for withdrawn Chinese SIPs; non-Chinese withdrawn SIPs experience significant profitability declines whereas successful non-Chinese SIPs see profitability increases post-sale. These findings strongly suggest that ownership transfer from political to private investors represents a necessary condition to sustain the economic gains cumulated by targeted firms over the pre-privatization period, and, therefore, to attain the long term success of privatization programs.

Despite being widely applied in academic research, the use of an "instrumented" propensity score matching procedure to infer causality has been criticized in the econometric literature (Wooldridge [2009], King and Nielsen [2016]). Therefore, it is very important to show that these results are not driven by a particular empirical strategy. Consequently, we employ the previously computed probability of withdrawal as an instrumental variable for the outcome of an attempted SIP to study the existence of systematic differences in growth rates and levels for the five investigated corporate policies over the post-treatment period. These two 3-stage least squares (hereinafter 3SLS) specifications are directly derived from Dinç and Gupta (2011) and Bernstein (2015), and they provide strong support for all the DiD results. Operating efficiency, capital expenditure and the payout ratio all decrease post-withdrawal, while labor intensity dramatically increases over the post-treatment period for the average withdrawn SIP, as compared to successfully privatized SOEs.

[69] Note that newly privatized Chinese firms' profitability might be depressed mechanically, since the offering involves only primary shares being sold. This yields an immediate increase in assets and equity, well before any compensatory rise in operating profits can be achieved.

All in all, as the expectation of being exposed to market discipline disappears state ownership induced agency costs reemerge, absorbing back the efficiency gains achieved by target SOEs over the pre-treatment period. Therefore, while the successful transfer of ownership and control to profit maximizing investors is not a sufficient condition for the long term attainment of firm-level efficiency gains, it appears to be a necessary condition for the long-term success of privatizations programs.

This paper contributes to the literature in two main ways. First, we present a novel, hand-collected sample of 111 withdrawn SIPs and 166 successful SIPs, which allows us to study the economic consequences of SIPs in a new setting. In particular, comparing successful and withdrawn privatizations offers new evidence for the importance of state-ownership induced agency costs, whose resolution cannot be achieved absent the actual transfer of control from government to private owners.[70] Second, this paper contributes to the growing empirical literature on the political economy of financial markets [Jones, Megginson, Nash, and Netter (1999), Clarke and Cull (2002), Brown and Dinç (2005), Dinç and Gupta (2011), Bortolotti, Fotak, and Megginson (2015), and Borisova, Fotak, Holland, and Megginson (2015)] in several ways. First, the political economy of finance literature builds on the idea that politicians are exposed to strong incentives that might lead governments to deviate from the ideal concept of the benevolent social planner. By supporting the proposition that privatization withdrawals are strongly motivated by political factors, this paper provides an indirect test for this underlying assumption and shows how politicians' incentives might shape the outcome of a proposed privatization program. Second, by modelling the decision to withdraw a previously filed SIP, we show that market conditions play

[70] Note that this result provides implicit support for the complementarity of privatization and liberalization programs, rather than for their substitutability. For a discussion, see Belloc, Nicita, and Sepe (2014).

an, at most, marginal role in explaining this important financial and political decision, raising further questions about the direct comparability of SIPs and share issues completed by privately owned firms, given their differential determinants and sensitivities to market conditions. Finally, these results complement findings reported in Dinç and Gupta (2011), suggesting that political measures may be used as instruments to correct for the intrinsic endogeneity characterizing the literature on privatization and liberalization programs.

2. Related literature

Only a few studies have investigated the incidence of share issues' withdrawal, primarily focusing on the American IPO market. Dunbar (1998) and Busaba, Benveniste, and Guo (2001) show that between the mid-1980s and mid-1990s almost 20% of proposed IPOs were ultimately withdrawn before completion. In a more recent study, Dunbar and Foerster (2008) confirm this figure, showing that the fraction of withdrawn IPOs increased significantly between the mid-1990s and 2008, reaching almost 50% of filed IPOs during 2000. Identifying similar statistics for seasoned equity offerings (SEOs) appears to be more challenging. Lee and Masulis (2009), identify a sample of 2,960 completed and 336 withdrawn SEOs by U.S. issuers over the period between 1990 and 2002, suggesting that a non-negligible number of SEOs are withdrawn before completion. A similar ratio of withdrawn to successful SEOs is reported in Mikkelson and Partch (1988), providing further support for the numerical and economic relevance of these "failed" transactions. Given this strong pattern observed for privately owned (American) firms, it would not be unreasonable to hypothesize similar figures for privatization. However, careful empirical analysis is needed, since the generalizability of these statistics outside the U.S. is far from proven.

In order to provide a detailed overview of this withdrawn share offering phenomenon across countries and over time, we carefully hand-collect and analyze data on the plans of both

successful and withdrawn firms to issue new common equity filed between 1998 and 2016 from SDC Platinum and Datastream.[71] Table C1 reports the time distribution and incidence of these failed transactions. Of the 5,797 SIPs attempted between 1998 and 2016, 412 (6.64%) were ultimately withdrawn.[72] This figure is slightly higher than the one recorded for privately owned firms: of the 145,442 attempted share issues, 7,121 (4.66%) were withdrawn over the same period. Consistent with results reported in Busaba, Benvenise, and Guo (2001), no strong time pattern is observed for the ratio of withdrawn to attempted share issues for either privately-owned or state-owned firms, as shown in Figure C1.

The correlation (0.5227) between the ratio of withdrawn to attempted share issues for privately owned firms and SOEs is significantly positive, but also significantly different from one.[73] This finding suggests that different determinants are likely driving the decision to withdraw a previously filed share issue for privately-owned and state-owned enterprises.[74]

SIPs' withdrawals are a global phenomenon. As reported in Figure C2, the incidence of these failed transactions ranges between 5% and 10% for most countries. As reported in Table C2, focusing exclusively on countries that experienced at least five SIP withdrawals over the period 1998-2016 shows that these failed transactions are rather common in some countries, reaching almost 10% in China and 24% in the Czech Republic. Furthermore, the incidence of SIP

[71] To cross-check the validity of the available information, we verify the available events with news information reported in Lexis-Nexis, Bloomberg and in *Privatization Barometer Reports* (2004 to 2016).

[72] Share issues of ADRs, closed-end funds, conversion of mutual institutions, or multiple-class are excluded from the sample. The authors note and address severe double-counting for data downloaded from SDC. The cleaned data set used in this section of the paper is available from the authors upon request.

[73] This result is not driven by the observed spike. Once we drop 2001 from the sample, the correlation between the two time series declines to 0.2539.

[74] We acknowledge that regulatory differences might cause the observed correlation to be significantly different from one, conditional on different countries representing different percentages of the number of yearly global issues executed by privately-owned and state-owned firms. This issue will be at least partially addressed in the next section through different matching procedures aimed at balancing the sample and within-group regressions.

withdrawals is systematically larger than the incidence of share issue withdrawals for privately owned firms, suggesting that the value of the option to withdraw a previously filed share issue is likely to be significantly higher for political agents due to their exposure to specific sources of political risk.

It is more challenging to assess the economic relevance of these failed transactions. Following Busaba, Benveniste, and Guo (2001), we compute the expected offer value as the number of shares offered times the midpoint of the price range specified in the filing prospectus and reported in SDC.[75]

Following this strategy allows us to recover the expected proceeds for approximately 42% of the withdrawn share issues executed by both privately owned and state owned firms. Given the large number of omitted observations it is difficult to make strong inferences from these values.[76] However, conditional on their availability, foregone revenues from withdrawn SIPs cumulate to a non-negligible $116 billion, representing approximately 5% of the $2.39 trillion raised by governments through SIPs over the study period, as shown in Table C1. This value is consistent with that observed for privately owned firms, for which the expected proceeds from withdrawn transactions cumulate to approximately $797 billion, which represent 6.14% of the $12.16 trillion raised by private investors. [77]

In order to get consistent and comparable accounting data, we exclude from the sample unit and multiple-class offerings, offerings of REITs, ADRs, and closed-end mutual funds. Furthermore, we also exclude offerings of financial institutions (SIC code 6000-6999), which

[75] Alternatively, we used the item "total proceeds in all markets" reported in SDC to proxy for the expected proceeds.
[76] Omitted observations are likely to be driven by both country-specific regulations and accounting standards and, consequently, they are unlikely to be randomly distributed. Therefore, any inference based on this sample should be carefully interpreted.
[77] Once we focus exclusively on IPOs, similar figures can be identified. Table and figures including exclusively IPOs are available upon request.

should be studied in isolation.[78] Following extant literature on privatization, we hand-collect from multiple sources data on targeted SOE's performance over the seven years surrounding the event.[79] Despite the fact that accounting data availability is extremely limited for withdrawn privatizations, we successfully collect observations for 111 withdrawn and 166 successful SIPs, which result in a weakly unbalanced panel including 1,524 firm-year observations. Due to the presence of extreme outliers for several financial variables which might be caused by reporting errors, we winsorize all accounting variables at the 1% level. Chinese SOEs account for roughly half the sample, but we study SOEs from 24 countries, so the sample remains quite global even when we exclude China. Table C3 reports summary statistics for the available variables.[80]

At a glance, Table C3 suggests that withdrawn SIPs involve significantly larger firms, and, unsurprisingly, they cluster during weak market conditions. However, the most striking differences between the two groups are observed with respect to political factors. Withdrawals are more common in democratic countries in which a strong minority coalition party is active and competitive, and they seems to cluster around elections. However, univariate statistics provide poor guidance to fully understand the factors leading to a SIP's withdrawal. Therefore, we attempt to shed light on the characteristics and consequences of any restructuring process taking place over the three years preceding an attempted privatization in the next section.

[78] We find accounting data for approximately 30 financial institution whose SIPs were withdrawn before completion.

[79] Accounting data are collected from Bloomberg, Compustat Global, and the share issue prospectus. We retain any observation for which we could identify at least two observations over the pre-treatment period, and one over the post-treatment period. This selection process allows us to study the presence of trends over the pre-privatization period, as well as studying changes in performance induced by the successful completion of the ownership transfer from state to private investors.

[80] Appendix E replicates Table C2 for the hand-collected sample. Appendix F provides representation of its global distribution.

3. Comparing Successful and Withdrawn SIPs over the Pre-Privatization Period

From an economic perspective, once an issuer files the required documents to issue shares on a regulated market, the reputational costs of withdrawing from this commitment are likely to be severe (Dunbar and Foerster [2008]). Therefore, it is unlikely that such a late withdrawal was strategically planned before the filing. If this were the case, then successful and withdrawn privatizations should be virtually indistinguishable before the treatment is assigned. In this section we formally test whether successful and withdrawn privatizations are ultimately comparable over the pre-treatment period. In particular, since the economic consequences of state-ownership on a firms' operating and financial performance are dynamic in nature (Villalonga [2000], Gupta [2005]), we investigate the existence of parallel conditional trends between the two groups by estimating the following regression model via OLS:

$$Y_{i,t} = \alpha + \sum_{i=1}^{2} \beta_i * year_i + \beta_3 * Withdrawn + \sum_{j=4}^{5} \sum_{i=1}^{2} \beta_{ij} * year_i * Withdrawn + \gamma_{fy} + \gamma_i + \varepsilon_{i,t}$$

Where $\propto$ represents the conditional mean; yeari is an indicator variable set equal to one if the observation precedes the event by i fiscal years, zero otherwise.[81] Withdrawn is a dummy set equal to one if firm i's SIP is ultimately withdrawn, zero otherwise. Finally, $\gamma\ fy\ and\ \gamma\ c$ are fiscal year and country fixed effects. The regression is run over the three years preceding the event date, and standard errors are clustered by country.

The two year dummies capture the existence of an eventual time trend over the pre-privatization period, while Withdrawn controls for the existence of any differences in the conditional mean for the dependent variable between the two groups. Finally, the two interaction

[81] All reported specifications do not include industry fixed effects due to the relatively small sample size and to the limited number of firms privatized in the same two-digit SIC industry code and country. Trends are robust to a specification including fiscal year and 2-digit SIC code industry fixed effects.

terms allow us to estimate differences in trends over the pre-treatment period between successful and withdrawn SIPs. Therefore, statistically significant $\beta 1$ and $\beta 2$ indicate the existence of pre-privatization trends for successful privatizations, while significant $\beta 4$ and $\beta 5$ are suggestive of differential dynamics for the two groups over the three years preceding the event.

We estimate this model to study the time-series behavior of five corporate policies that have been extensively studied in the privatization literature: profitability, labor intensity, operating efficiency, payout policy and capital expenditures. Table C4 reports our findings for the full sample. Strong, statistically significant pre-privatization trends emerge from this analysis. Consistent with Villalonga (2000), Dewenter and Malatesta (2001), and Wolf (2009), improvements in targeted SOEs' profitability, labor intensity and operating efficiency are identified over the pre-privatization period. We also document a reduction in capital expenditures for both groups.[82] No difference in trends is identified between the two groups, suggesting that all target SOEs undergo comparable restructuring processes over the three years preceding the planned privatization.

Despite the observed financial similarities between failed and successful SIPs, a government's decision to withdraw an attempted privatization is unlikely to be random. Since the direct comparability of the two samples hinges on our understanding of such a complex decision process, we attempt to model rational issuers' ex-ante expected probability of withdrawal in the next section.

[82] Findings in Jaslowitzer et al. (2018) and Chen et al. (2017a) suggest that the identified reduction in capital expenditures might be driven by Chinese SOEs. We will formally test for this consideration in the next section.

4. The Political and Financial Determinants of SIPs' Withdrawal

As discussed in Busaba, Benveniste, and Guo (2001) and Busaba (2006), the decision to withdraw a previously filed share issue hinges on the position of the issuer's reservation price relative to possible investors' valuation. Therefore, the economic and political factors affecting these two dimensions are likely to play a critical role in determining whether a government will ultimately withdraw a SIP. However, the empirical and theoretical frameworks presented in Busaba, Benveniste, and Guo (2001) and Busaba (2006) are likely to poorly reflect the situation regarding SOEs, given their particular nature. For instance, SOEs often face soft budget constraints [Kornai (1988), Laffont and Tirole (1993)], making financial constraint related considerations secondary for the decision to issue shares in a regulated market. Absent a clear theoretical model, we control for several firm- and country-level variables that might affect a government's decision to withdraw a previously filed SIP, and let the data speak for themselves.

4.1. The Decision to Withdraw a Previously Filed SIP

In Table C5, we estimate a rational issuer's ex-ante expected probability of withdrawal for all the SIPs included in our dataset. The decision to withdraw a previously filed SIP is modeled as a function of several firm-level financial factors, as observed at the end of the fiscal year preceding the attempted share issue, and of a set of political variables observed immediately before the proposed sale. We control for expectations concerning future macro-economic scenarios by including 2-year GDP forecasts as reported at the end of the fiscal year preceding the attempted SIP. In order account for country-level shocks, without sacrificing the possibility of studying the role played by highly persistent political factors in triggering the decision to withdraw a previously filed SIP, we employ country random effects (RE) in all five specifications.[83]

[83] Results are robust to the inclusion of 2-digit SIC code industry fixed effects. However, the resulting log-likelihood converge significantly towards zero, raising concerns of overfitting. Furthermore, the existence of industries including

Since the relation between the decision to exercise this real option and the set of variables reported in Table C5 can be either linear or non-linear, or both, we begin by running our baseline results by estimating a linear probability model via GLM (column 1). We then estimate both a probit and a logit model via GLM (columns 2 and 3, respectively).[84] The results do not appear to be model dependent, allowing us to rely on the logit estimates to infer the direction of the identified conditional correlations.[85] Interestingly, financial factors appear to play a secondary role. Operating efficiency (the ratio of net income to the number of employees) is the only factor whose coefficient is consistently (positively) significant at the 1% level of confidence. This indicates that governments' sensitivity to macro and political factors is highest when the privatization program includes that nation's "crown jewels." Furthermore, no support is provided for the idea that governments exercise their option to walk away in an attempt to time the market. Busaba, Benveniste, and Guo (2001) and Bernstein (2015) do find this for privately owned firms, raising further concerns about the direct comparability of share issues attempted by privately owned and state owned enterprises, given their different sensitivities to political and financial factors.

Political and institutional factors play an important role in determining the ultimate outcome of an attempted SIP. High current deficit countries have a substantially lower degree of financial flexibility, which results in a higher reluctance to withdraw a previously filed SIP. Similarly, heavily indebted countries are more dependent on external financial markets, and, consequently, they need to carefully manage their reputation as reliable financial counterparties. These governments also have a more pressing fiscal need to raise funds—and so are more reluctant

two – or one – observations, exclusively, call for prudence in interpreting these estimates. For this reason, reported results do not include industry fixed effects. This additional table is available upon request.

[84] All continuous variables have been normalized with mean 0 and standard deviation of 1.

[85] All results reported in Table C5 are robust to dropping Chinese SOEs from the main sample.

to abandon an offering once launched. These factors result in a lower propensity to withdraw a SIP for countries with a high stock of public debt. Finally, it is important to discuss the strong marginal effect of the Politi IV Index on the probability to withdraw a SIP. The Politi IV Index is a discrete measure which can take values between -10 and +10. The higher the score, the more democratic is a country. This index is an indirect function of all the other political variables included in Table C5, explaining the lack of significance for other arguably important factors, such as closeness to a political election. Thus, in column 4 we re-estimate the probit model after dropping the Politi IV Index. With this alternative specification we attempt to open the political "black box", shedding light on which political factors are ultimately driving the results. Consistent with findings reported in Dinç and Gupta (2011), the most important factor appears to be the strength of the minority coalition party (Political Competitiveness). In particular, the larger the difference in votes controlled in Parliament by the majority versus the minority coalition party, the lower the probability of withdrawing a previously filed SIP, indicating that strong minority coalition parties are more likely to cause the failure of an attempted privatization program.[86]

Results reported in Table C5 confirm that the decision to withdraw a previously filed SIP is ultimately endogenous. However, the primary role played by political variables in determining the ultimate outcome of governments' decision process allows us to mitigate self-selection concerns. In particular, we recognize that both a country's stock of public debt and its degree of political competitiveness as observed at the end of the fiscal year preceding the event are unlikely to have direct implications on a SOE's future performance,[87] while they appear to be fundamental

[86] Note that this finding can be either positive or negative in terms of its social welfare implications. Indeed, on the one hand, a strong opposition party can reduce the risk of tunneling or of political patronage. However, on the other hand, a stronger minority coalition party might have the power to freeze a government's attempts to finalize important economic reforms in order to damage the majority party from an electoral perspective.

[87] Both covariates are relatively highly persistent and both are pre-determined through a country history and political structure. In particular, a country's stock of debt results from the historical cumulation of governments' public finance

determinants of a government's decision to withdraw a previously filed SIP.[88] Consequently, we can employ the ex-ante expected probability of default estimated in Table C5, model 4 to propensity score match the two samples, since these political variables act as valid instruments, de facto quasi-randomizing the treatment assignment. In order to capture country specific factors which might drive the decision to exercise this real option, we match each withdrawn observation to its nearest neighbor, conditional on both SOEs lying on the common support, and on being incorporated in the same country. The resulting matched sample has 84 withdrawn and 84 successful SIPs.

Table C6, Panel A confirms that the previously identified trends (Table C4) hold in our matched sample of completed and withdrawn SIPs. In particular, we observe large increases in targeted SOEs' profitability and operating efficiency, as well as evidence of significant reductions in labor intensity and capital expenditures over the two years before the projected privatization date.[89] No significant difference in trends is observed between the two groups, confirming that all targeted SOEs undergo similar restructuring processes over the three years preceding the event.

Extant literature suggests that Chinese SOEs are exposed to a unique political and institutional environment, which might induce "abnormal" corporate behaviors compared to that observed in other countries. For instance, Chinese privatizations often result in no change in profitability and a net decline in capital expenditures (Megginson [2017], Chen et al. [2017a]), reversing the findings documented for non-Chinese transactions. Consequently, even if the

decisions, while the current degree of political competition depends on both historical considerations and on the outcome of the most recent election.

[88] Virtually no country in our sample experiences multiple withdrawal in the same fiscal year. This dispersion decreases concerns related to the lack of within country-year variation characterizing the proposed instruments.

[89] Results reported in Table C6 are generated by matching treated and control units with replacement. Consequently, we prudentially estimate the standard errors via bootstrapping them within each country.

numerical dominance of Chinese SIPs in our sample captures global trends in privatizations characterizing the last 20 years, it is important to address concerns about the extent to which the previously reported results are driven by Chinese attempted transactions. We replicate the same tests reported in Table C6, Panel A after excluding Chinese SOEs from the matched sample. Estimates based on the remaining 43 withdrawn and 43 successful non-Chinese privatizations are reported in Table C6, Panel B. Consistent with previous results, we report a strong increase in profitability and operating efficiency over the three years preceding the event for non-Chinese projected SIPs, as well as a strong decline in labor intensity. The greater performance improvements for planned non-Chinese SIPs are consistent with the weaker performance effect induced by Chinese privatizations, which dilutes the impressive gains achieved by targeted non-Chinese SOEs (Megginson, 2017). Similarly, we identify no evidence of a decline in capital expenditures for non-Chinese SOEs over the pre-treatment period induced by the expectation of being exposed to market discipline. As expected, over-investment issues are not observed in non-Chinese SOEs (Jaslowitzer et al. [2018], Chen et al. [2017a]).

All in all, the reported similarities between withdrawn and successful SIPs suggest that we might be able to successfully isolate the economic consequences of the ownership transfer from state to private investors by comparing these two groups. Indeed, the proposed instrumented matching strategy allows us to explicitly control for: (1) the economic consequences of the expectation of being privatized; (2) the selection of which SOE to target for privatization; and (3) the endogenous selection of which attempted SIP to withdraw before completion, thus ultimately isolating the marginal effect of a firm's privatization on its long term economic performance.

5. The Economic Consequences of SIPs' Withdrawal

As previously discussed, the propensity score matched sample allows us to isolate the ultimate effect of the ownership transfer from state to private investors after controlling for the existence of endogenous pre-treatment trends. Furthermore, results reported in model 6, Panel A of Table C6, are strongly suggestive that the parallel trend assumption is satisfied, allowing us to estimate the treatment effect by exploiting a DiD setting. Table C7, Panel A reports our findings.

Both the successful and unsuccessful privatization groups experience statistically significant declines in profitability (ROS) over the post-treatment period, with no relevant difference recorded between the two groups—though, as will be discussed below, this is driven completely by Chinese SIPs.[90] Completed privatizations experience significant increases in operating efficiency and payout, as well as a continuing significant decline in labor intensity post-sale. Conversely, withdrawn privatizations experience a strong relative decline in operating efficiency and payout after the privatization is withdrawn, plus a significant *increase* in labor intensity as compared to successful SIPs. These findings are consistent with the existence of state ownership induced agency costs that cannot be fully dealt with absent ownership and control transfer. Furthermore, consistent with the hypothesis of state ownership induced overinvestment, withdrawn SIPs' average capital expenditure increases significantly once the expectation of being exposed to the market discipline disappears—though this too is solely a Chinese SOE phenomenon. Finally, SOEs whose privatizations are not successfully completed have significantly lower payout ratios, consistent with private investors imposing dividend payments to

[90] It is possible that the profitability of newly privatized firms might suffer from a downward bias due to agency conflicts generated by the dispersed ownership resulting from the transaction itself, as suggested by Jensen and Meckling (1976) and Jensen (1989). This might explain the model's inability to identify statistically significant results. Furthermore, it is important to recall that Chinese SIPs are severely affected by the so called "listing effect", which ultimately results in weaker improvements in profitability and operating efficiency [Li, Megginson, Sun, and Shen (2017)].

mitigate agency costs which might be induced by governments retaining minority shares in targeted SOEs.[91]

The simultaneous increase in (1) capital expenditure and (2) labor intensity, and the reported (3) reduction in operating efficiency observed for withdrawn SIPs supports the hypothesis that state ownership induced agency costs do exist--and can potentially be severe. Furthermore, the lack of relative decline in profitability over the post-treatment period suggests that these agency costs might be transferred to consumers/taxpayers either directly, through a higher mark up for goods and services sold by the (remaining) SOE, or indirectly, through a suboptimal subsidization of certain industries or firms in order to maximize employment for political reasons, or both.

As previously discussed, it is important to study whether Chinese SOEs are driving the reported findings. Therefore, we drop China from the initial sample and we replicate our DiD regressions for the 86 remaining matched SOEs. These estimates are reported in Table C7, Panel B. Virtually all results are robust to this Jackknife test. However, two important differences are worth noting. First, as expected, the decrease in SOEs profitability after the completion of the SIP is a China-specific phenomenon: the estimated coefficient for the dummy variable indicating the post-event period turns positive and statistically significant for completed non-Chinese SIPs. Similarly, consistent with Jaslowitzer et al. (2018) and Chen et al. (2017a), SOEs' over-investment appears to be significantly more pronounced in China than in other countries. Indeed, no statistically significance difference is identified between withdrawn and successful SIPs with respect to their capital expenditures. These results confirm that absent the ownership transfer from political to private investors the large efficiency gains cumulated by SOEs over the pre-privatization period are not sustainable in the long term.

[91] Virtually no SIPs involve 100% of an SOE's equity. Governments generally divide privatizations in several tranches in an attempt to maximize revenues and to reduce market liquidity concerns. See, e.g. Nandini (2005).

The reliability of the reported results hinges on the validity of the proposed matching procedure. However, the use of an instrumental variable to estimate a firm's propensity to be treated is controversial, since it is based on untestable, strong econometric assumptions (Wooldridge [2009], King and Nielsen [2016]).[92] Therefore, it is of paramount importance to validate the identified results within a different framework.

In order to move in this direction, we estimate the 2SLS specification similar to the one proposed in Dinç and Gupta (2011). In particular, we follow Angrist and Pischke (2008) and employ the fitted values from the logit model estimated in Table C5, model 4, as an instrument for the decision to withdraw a previously filed SIP. As previously discussed, a country's stock of public debt and its degree of political competitiveness, as measured at the end of fiscal year preceding the privatization, provide the required exogenous variation to obtain consistent estimates of the treatment effect. These regressions include country and fiscal year fixed effects, as well as additional control variables similar to those employed in Dinç and Gupta (2011). These specifications are estimated on the full sample of targeted SOEs, and the dependent variables are changes in the five studied corporate policies over the three years following the event.[93] From the first-stage regression reported in Table C8, Panel B, we confirm that the estimated probability of withdrawal appears to be a relevant instrument for a government's decision to exercise this real option. The second-stage estimates are reported in Table C8, Panel A. All results are consistent with those reported in Table C7, strongly suggesting the existence of severe state ownership

[92] Propensity score matching (PSM) might result in increased imbalance, inefficiency, model dependence, and bias when covariates satisfying IV assumption are included in matching estimators.
[93] In order to compute changes over the post-treatment period we constrain the sample to the withdrawn and successful SIPs with at least two valid observations over the three years following the event.

induced agency costs which cannot be mitigated absent the successful ownership transfer from state to private investors.[94]

Similarly, Table C9 reports estimates for a 3SLS model, as in Bernstein (2015), in which the dependent variables represent the average level for each of the five studied corporate policies over the three years following the event, rather than their changes. As in Bernstein (2015), we control for the average level of the independent variable over the pre-treatment period, and we estimate these specifications over the full sample. Again, a country's stock of public debt and degree of political competition act as instrumental variables for a government's decision to withdraw a previously filed SIP. The relevance of the instrumented probability of withdrawal is confirmed in the first stage regressions reported in Table C9, Panel B. Second-stage estimates are reported in Table C9, Panel A, further suggesting that absent the successful completion of the ownership transfer state ownership induced agency costs reemerge, absorbing back the significant efficiency gains achieved by the targeted SOEs over the pre-privatization period. In particular, higher levels of labor intensity and capital expenditures, as well as lower levels of operating efficiency and payout ratio are identified for SOEs whose privatization failed.[95]

All in all, while extant literature supports the idea that an ownership transfer from political to private investors is insufficient, *per se,* to generate significant efficiency gains (Kole and Mulherin [1997]), the results reported here at least suggest that ownership and control transfer from state to private investor represents a necessary condition for the long term sustainability of any improvements achieved by targeted SOEs over the pre-privatization period.

[94] Virtually all results reported in Table C7 are robust to dropping Chinese SIPs from the sample. These estimates are reported in Table C8, Panel C and Panel D. Findings confirm that SOEs' over-investment appears to be more pronounced in China.

[95] Profitability, labor intensity and operating efficiency results are robust to dropping Chinese SIPs from the sample. However, no major difference in payout ratio and capital expenditures are identified in this sub-sample. These estimates are reported in Table C9, Panel C and Panel D.

6. Conclusion

This is the first study to empirically analyze the political and financial economics of withdrawn share issue privatizations. We document that approximately 7% of the SIPs filed over 1998-2013 are ultimately withdrawn, with governments leaving on the table $116 billion in unrealized proceeds. The decision to exercise this option appears to be both political and economic in nature, and all SOEs targeted for privatizations appear to undergo comparable restructuring processes over the three years preceding the attempted SIP.

By exploiting the reported similarities between withdrawn and successful privatizations, we provide support for the existence of severe state ownership induced agency costs. These agency conflicts materialize in a strong relative reduction in operating efficiency and payout ratio, while often resulting in sub-optimally high level of employment and, for Chinese SIPs, overinvestment. Furthermore, the small differences in profitability between successful and withdrawn privatizations over the post-treatment period is suggestive that these inefficiencies might be ultimately transferred to consumers/taxpayers either directly, via a potentially higher mark up for goods and services, or indirectly, through hidden subsidizations of inefficient SOEs, or both. Thus, while ownership transfer from government-related to private investors is not a sufficient condition to achieve the often impressive efficiency gains documented in the privatization literature, it appears to be a necessary condition for the long term sustainability of any gains achieved by targeted firms over the pre-privatization period.

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Appendixes

Appendix A. Sample construction

Appendix reports a detail description of the sample selection process. Number of Observations indicate the number of available deal-specific observations after applying the indicated filter. Total $ Value reports the cumulative deal value of the retained transactions in current U.S. Dollars.

Panel A: Pre-Matching Sample		
Filters	Number of Deals	Total $ Value
All M&A reported in SDC (1979-2014)	317,469	
Eliminate double counting	315,247	38,923,563
Only completed deal	247,173	26,271,347
Only acquisition in which at least 50% of the target is purchased (Drop if not reported)	206,149	23,272,851
Only domestic acquisitions	181,586	19,339,876
Only Public to Public deals	8,162	8,132,459
Only Deals > $100 mln.	4,030	8,008,559
Drop deals in which at least one party is a financial firm (SIC code 6000-6999)	2,863	6,300,926
Retain firms with full observations for ROS & ROA over the period (-5y ; +3y)	383	1,378,722
Panel B: Synthetic Counterfactual - ROA as dependent variable		
Retain deals for which the synthetic control method can build a synthetic control for both target and acquirer	281	834,218
Retain observations with an RMSPE below 10%	188	637,017
Retain observations with an RMSPE below 5%	124	493,215
Panel C: Synthetic Counterfactual - ROS as dependent variable		
Retain deals for which the synthetic control method can build a synthetic control for both target and acquirer	270	835,517
Retain observations with an RMSPE below 10%	212	637,849
Retain observations with an RMSPE below 5%	134	467,361

Appendix B. Variable definition

Industry Classification	Description
Agriculture, Forestry and Fishing	All firms operating in SIC codes from 0100 to 0999
Construction	All firms operating in SIC codes from 1500 to 1799
Manufacturing	All firms operating in SIC codes from 2000 to 3999
Mining	All firms operating in SIC codes from 1000 to 1499
Non-classifiable	All firms operating in SIC codes from 9900 to 9999
Retail Trade	All firms operating in SIC codes from 5200 to 5999
Services	All firms operating in SIC codes from 7000 to 8999
Transportation, Communications, Electric, Gas and Sanitary service	All firms operating in SIC codes from 4000 to 4999
Wholesale Trade	All firms operating in SIC codes from 5000 to 5199
Firm Level Variables	**Description**
Asset Utilization	Total Sales (sale) divided by total assets (at)
Cash	Cash and Short-Term investments (che)
Intangible Adjusted Return on Capital (IAROC)	Operating Income before depreciation (oibdp) plus R&D (xrd) and SGA expenditures (xsga) scald by invested capital, defined as in Peters and Taylor (2017).
Lerner Index	Operating Income before depreciation (oibdp) scaled by total sale (sale).
Leverage	Total debt (dltt+dlc) divided by stockholders' equity (seq)
Log Sale	The natural logarithm of the value of total sales (sale) in millions.
Payout Ratio	Common dividends (dvc) divided by Income before Extraordinary Items (ibcom)
R&D to Sale	Research and development expense (xrd) divided by the value of sales (sale).
ROA	Operating Income before depreciation (oibdp) divided by book value of assets (at).
ROS	Net Income (ni) divided by total sales (sale).
Size	The natural logarithm of the value of total book assets (at) in millions.
Tobin's Q	Market value of assets (at – book equity + market equity (prcc_f*csho)) divided by the book value of assets (at). Book equity and this measure, in general, follows Fama and French (1992).

Deal Level Variables	Description
Number of Transaction - Current Fiscal Year	Total number of completed transactions in the current fiscal year, as reported in SDC.
Cash Only Acquisition	Dummy variable set equal to 1 if the consideration structure includes cash only, 0 otherwise.
Diversification	Dummy variable set equal to 1 if the target and the acquirer operate in two different two-digit SIC industries.
Acquirer-Target Premium	The difference between the acquirer's Tobin's Q and the target Tobin's Q, as observed at the end of the fiscal year preceding the transaction.
Price per Share	Price per share at which the transaction is executed, as reported in SDC.
Serial Acquirer - Current Fiscal Year	Dummy variable set equal to one if the acquirer is involved in multiple acquisitions in the current fiscal year, 0 otherwise.
Shares Only Acquisition	Dummy variable set equal to 1 if the consideration structure includes equity only, 0 otherwise.
Transaction Value	Total deal value in millions, as reported in SDC.

Appendix C. Donor pool construction: a simple example

Appendix C explains the donor pool construction for a hypothetical deal occurred in 1995 in which the acquirer operates in the 2-digit SIC industry #13. Number of Observations indicate the number of available firm-year observations available after applying the indicated filter. The same process is applied for each target in our sample. Number of Observations indicate the number of available firm-year observations available after applying the indicated filter.

Filters	Number of Observations	SDC and Compustat Item
Compustat Universe (1979-2017)	195,136	
Drop firm-year observations completing a major deal in year t.	191,185	
Retain firms for which ROS is not missing.	134,808	*ROS=ni/sale*
Retain observations for the 8 years surrounding the event.	52,615	*Fiscal Year (Fyear)*
Retain observations for which the full time-series for ROS is available over the period (-5;+3).	22,716	
Retain observations for the specific 2-digit SIC industry.	927	*SIC code (SIC)*

Appendix D. Variable Definitions

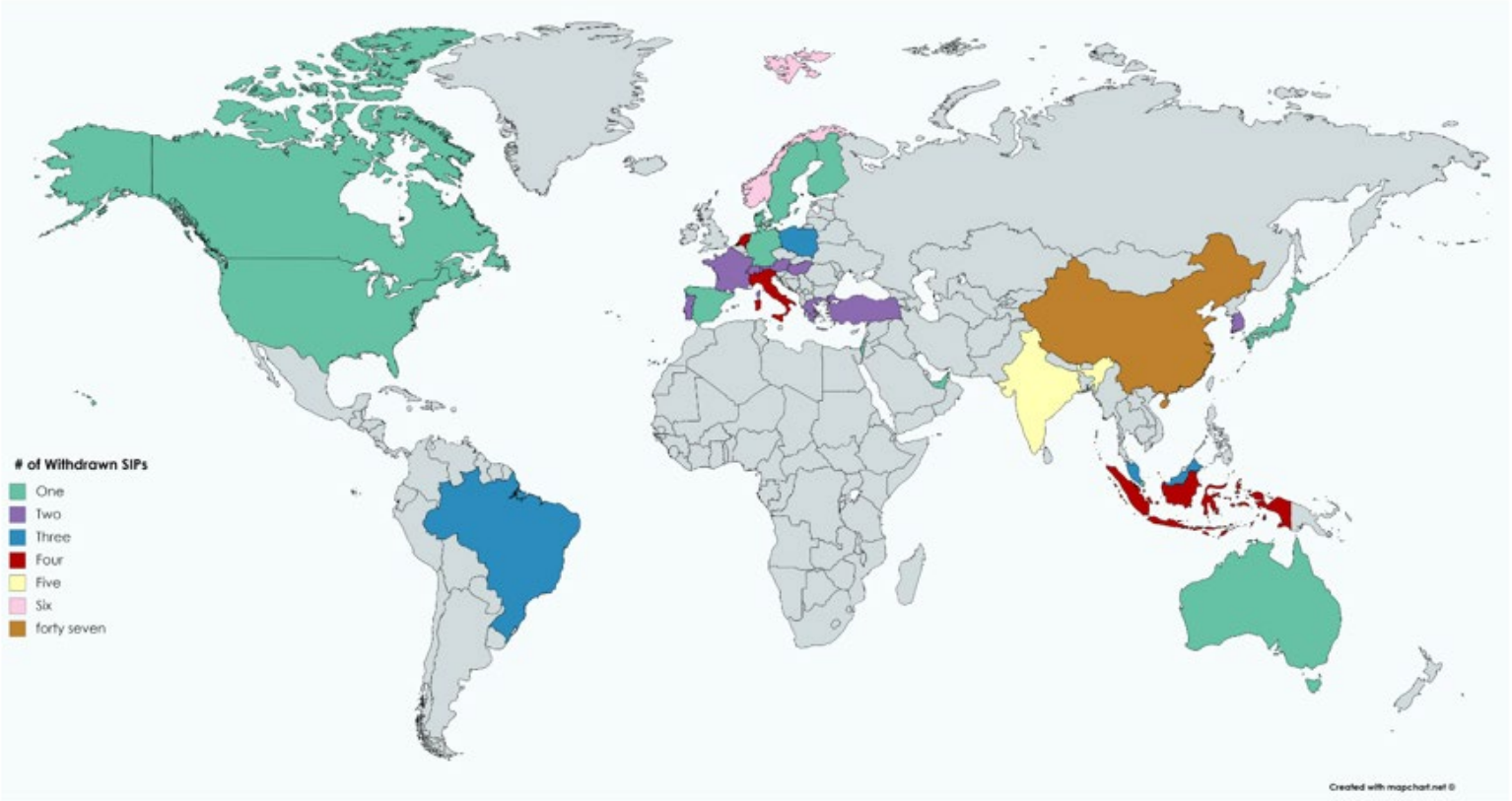
Panel A: Accounting Variables	
Variables	**Definitions**
Total Assets	The natural logarithm of the value of total book assets in millions, where assets are deflated to 2010 dollars.
Sales Growth	The natural logarithm of sales in 2010 dollar millions in year t divided by the value of sales in millions in year t-1.
ROS	Returns On Sales, measured as the ratio of net income to sales, both measured in millions.
Operating Efficiency	Net Income Efficiency, measured as the ratio of net income in millions to the number of employees.
Labor Intensity	The ratio of the number of employees to total book assets in millions, where assets are deflated to 2010 dollars.
Leverage	The ratio of total debt to total book assets.
Capital Expenditure	Capital Expenditures to Total Assets, measured as the ratio of CAPEX to total book assets, both in millions.
Accruals	Normalized Accruals, Cash Flow Method, as measured at the end of the fiscal year.
IPO	A dummy variable set equal to 1 if the share issue is an IPO, 0 otherwise.
1 month market returns	30-days' returns recorded for the main index of the stock exchange targeted for the share issue privatization.
# of IPO	Number of IPOs which occurred in the same country and in the same year the share issue privatization is supposed to occur.
Two-Year GDP forecast	Two-year, country specific GDP growth forecast published by the IMF at the end of the fiscal year preceding the attempted SIP.
GDP per Capita	GDP per capita is gross domestic product divided by midyear population. GDP is the sum of gross value added by all resident producers in the economy plus any product taxes and minus any subsidies not included in the value of the products. It is calculated without making deductions for depreciation of fabricated assets or for depletion and degradation of natural resources. Data are in current U.S. dollars.
Panel B: Political Variables	
Cumulative Proceeds from SIPs	The ratio of the proceeds in millions raised from share issue privatization by a government up to year t divided by its GDP per capita.
Public Debt	All general government, SOEs and central bank's liabilities that require future payment of interest and/or principal by the debtor to the creditor. This includes debt liabilities in the form of special drawing rights, currency, and deposits; debt securities; loans; insurance, pension, and standardized guarantee programs; and other accounts payable. (See the IMF's 2001 Government Finance Statistics Manual and Public Sector Debt Statistics Manual.)
Net Deficit	The year on year percentage growth of a country's public debt.
Left-wing Government	A dummy set equal to 1 if the government is defined as "left-wing" in the Database of political Institutions 2015.

Election year	A dummy set equal to 1 if a legislative or executive election is scheduled over the 12 months following an attempted share issue privatization, 0 otherwise.
Political Competitiveness	The absolute value of the difference between the number of seats controlled in the Parliament by the majority coalition party and those controlled by the main minority coalition party.
Politi IV Index	The Polity Index is a composite index proxying for a country's democracy "quality". It is based on a unified scale assuming values between +10 (strongly democratic country) and -10 (strongly autocratic country). For further details, see Marshall and Jaggers (2005), and Marshall (2011).

Appendix E. Time Distribution of Completed and Withdrawn Share Issues – Matched Sample

Appendix E reports the time distribution of completed and withdrawn share issues attempted by all state owned enterprises included in the matched sample. Proceeds for withdrawn transactions are defined as the product between the mid-price and the number of share issued, as reported in the filing documents. Reported proceeds are severely biased downward due to missing/omitted observations. Approximately 6.00% of the observations for proceeds raised by privately owned companies successfully completing a share issue are not reported in the original dataset. Analogously, 20.00% of the proceeds raised by governments from share issue privatizations are missing. Finally, we are unable to identify the proceeds associated with 60.00% of withdrawn share issues attempted by both private and state owned companies. All monetary values are reported in millions of U.S. dollars.

	Privatization - Successful		Privatization - Withdrawal	
Year	N	Proceeds	N	Proceeds
1998	0	$180.65	2	-
1999	1	$286.03	2	-
2000	1	$140.34	1	-
2001	1	$2,715.57	5	$ 3,388.63
2002	11	$5,787.60	9	$ 1,612.68
2003	7	$0.00	6	$ 1,748.46
2004	0	$2,090.00	1	$ 144.30
2005	11	$532.56	1	$ 125.50
2006	4	$205.63	2	$ 111.12
2007	1	$305.00	2	$ 261.10
2008	2	$635.44	3	$ 600.30
2009	8	$401.85	3	$ 272.37
2010	3	$3,331.20	6	$ 872.22
2011	10	$2,026.40	11	$ 2,563.00
2012	5	$3,304.86	12	$ 5,419.20
2013	19	$180.65	8	$ 1,796.48
TOTAL	84	$21,943.13	84	$18,915.36
YEARLY MEAN	15	$1,462.88	15	1,455.03



Appendix F: Global distribution of SIPs in the hand-collected sample. Source: SDC Platinum & Datastream.

Table A1. Introducing Sharia Law within a legal system

Table A1 reports countries that have adopted the Sharia Law as a non-residuary source of legislation. "Year of adoption" indicates the year in which Islamic Law was first introduced within the country's legal system. "Undefined" indicates that no salient date could be identified for the country. Data is collected from the GlobaLex – NYU database, BBC 'Timeline', and Lombardi (2013).

* North Yemen became an independent state in 1918. South Yemen's independence occurred in 1967.

Panel A: African Countries

Country	Year of independence	Year of adoption	Major Confounding Events & Characteristics
Comoros	1975	Undefined	Sharia Law was not supplanted during the French colonization
Eritrea	1991	Undefined	Sharia Law was introduced during the Italian colonization period, before the end of WW2.
Gambia, The	1964	Under discussion	The Gambia fashioned itself as an Islamic State in January 2016. Customary law will be probably substitute by Sharia Law soon.
Ghana	1957	Undefined	Sharia Law is considered to be a part of Customary law, which has never been codified
Mauritania	1963	1980	Civil war against Polisario Guerrillas in 1976-1978 resulting in financial strains and marginal changes in leadership. Slavery was abolished, but not criminalized, in 1981.
Morocco	1956	1956/2011	The 1956 constitution was based on the dominant Maliki school of Fiqh (Islamic Jurisprudence). The new 2011 constitution was surrounded by a dramatic escalation of terroristic events and violence.
Somalia	1960	2009	Exponential escalation of violence since 2006 leading Islamist to seize power in 2009. Several foreign interventions; The UN World Food program withdrew from the Al-Shabab areas in 2010.
Sudan	1956	1983	Sharia Law was first introduced in the 1968 constitution, but only enforced from 1983. In the same year, a dramatic civil war between North and South Sudan began.

[Continue]

Panel B: Asian Countries

Country	Year of independence	Year of adoption	Major Confounding Events & Characteristics
Indonesia	1945	2001	Aceh is the only region in Indonesia applying Sharia Law thanks to its special autonomy.
Afghanistan	1919	1976/1991	The introduction of the 1976 Criminal Law act was followed by riots and a long period of political instability.
Bahrain	1971	1973	Sharia was not supplanted during the British control. The 1973 constitution set Islamic Law within a constitutional framework, but it was suspended in 1975.
Brunei	1984	Under discussion	Since 2011, the Sultan Hassanal Bolkiah has committed its political action to fully incorporate and enforce Sharia Law within Brunei's legal and juridical system.
Nigeria	1960	1999	Nigeria is a federal country. Nine states have fully incorporated Sharia Law within their legal system. Furthermore, Kaduna State, Niger State and Gombe State have partially adopted it.
Iran, Islamic Republic of	1979	1979	The introduction of the 1979 constitution was preceded and followed by several riots and military actions. In particular, the Iran-Iraqi war began in 1980.
Iraq	1932	1958	The 1958 Civil Code identifies Islamic Law as a main source of legislation and it represents the end of the filo-British monarchic government.
Kuwait	1961	1962	Sharia was not suspended during the British control. The 1962 constitution set Islamic Law within a constitutional framework.
Maldives	1965	1961	Sharia Law was de facto in place when the Maldives were under British control. The 1961 Penal Code codified Sharia Law.
Oman	1749	1996	The 1996 ornamental constitution did not determine significant changes to Qatar's democracy level or form of Government. However, Sharia Law was already de facto in place before its approval.
Pakistan	1947	1978/1991	Sharia Law was already partially in place before the approval of the 1978 Islamic Law. The 1991 constitution was approved contextually to the lunch of a massive privatization program.
Qatar	1971	1972	The 1972 constitution was unusually autocratic, creating no justiciable requirement of state respect for the Islamic Law.
Saudi Arabia	1930	1992	The 1992 constitution is generally referred to as an ornamental constitution; Sharia Law was already in place before its approval.
Syria	1946	1950	The short-lived 1950 constitution introduced the Islamic figh (Islamic Jurisprudence) as the main source of legislation. In 1958 Syria joined with Egypt the United Arab Republic, adopting a new, quasi-secular constitution.
Yemen	1918-1967*	1962-1969	After the overthrow of the imamic monarchy in 1962, the Yemen approved a series of constitutions setting Sharia Law as the only source of legislation.

Table A2. Summary Statistics

Table A2 reports summary statistics for the available economic growth predictors for all the 23 countries in the donor group. Data is relative to the period 1973-1990, and they are expressed in 2005 Dollars. "Islam major religion" is a dummy set equal to one if Islam is the major religion of the country. "More than one religion" is a dummy set equal to one if there is more than one religion followed by at least 10% of the total population.

Variable	Mean	Standard Deviation	Max	Min
Wealth Level				
Avg GDP per capita 1973 - 1979	1345.84	1984.20	227.82	9284.73
Demographic Data				
Pop density 1973	39.48	59.51	1.22	205.09
Pop density 1979	45.71	68.19	1.45	226.28
Avg pop density 1973 - 1979	42.51	63.71	1.33	215.46
Avg pop growth 1973 - 1979	2.63	0.59	1.28	3.62
Trade Data				
Openness 1973	63.92	32.41	7.19	141.74
Openness 1979	70.89	31.69	11.90	147.48
Avg. openness 1973 -1979	84.09	11.57	56.84	123.71
Human Capital Data				
No education 1970	68.53	23.51	9.90	95.88
No education 1975	67.54	23.35	9.90	95.40
Tertiary education 1970	14.30	12.57	0.09	52.88
Tertiary education 1975	0.44	0.31	0.11	1.11
Age dependency 1973	90.77	10.05	72.57	111.69
Age dependency 1978	91.34	10.07	69.54	113.06
Avg age dependency 1973 - 1979	90.99	9.86	71.94	112.47
Financial Development Level				
Bank credit to GDP avg 1973-1979	119.03	68.22	41.63	362.36
Domestic credit to private sectors by banks (%GDP) avg 1973-1979	21.25	11.23	49.55	4.29
Political Stability				
Average Polity2 1973-1979	-4.42	5.82	-10.00	8.00
Culture				
Islam major religion 1975	0.33	0.48	1.00	0.00
Muslims (% Tot Population) 1975	36.70	37.84	99.40	0.00
More than one major religion 1975	0.83	0.38	1.00	0.00

Table A3. Estimated Synthetic Control Weights

Table A3 reports the weights assigned to each country in the synthetic control for Mauritania. Rounding errors may prevent columns from summing to 1. APE-to-mean ratio indicates the average pre-intervention prediction error divided by the average pre-intervention outcome value. YCDR is the Yearly Compounded Diverge Rate, and it represents the speed of divergence between the synthetic control and the treated unit over the post-treatment period.

Donor Group	Weight
Benin	0.00%
Cameroon	0.00%
Gabon	0.00%
Sierra Leone	0.00%
Algeria	7.77%
Guyana	0.00%
Kenya	27.96%
Malaysia	0.00%
Togo	0.00%
Botswana	0.00%
Congo, Dem. Rep.	50.33%
Niger	0.00%
Rwanda	0.00%
South Africa	0.05%
Zambia	0.00%
Tunisia	0.00%
Mali	13.44%
Swaziland	0.00%
Model fit pre-treatment	
RMSPE	3.18%
APE-to-mean ratio	-0.01%
SCM inference	
RMSPE ratio	1.84
YCDR	0.66%

Table A4. Indicator Fit & V- Matrix

Table A4 reports values for the selected indicator variables for actual and synthetic Mauritania. All the averages refer to the period 1973 - 1979, and all monetary values are expressed in 2005 Dollars. Population growth and education variables are in percentage points. The V-matrix column reports the diagonal entries in the V matrix (its value are normalized to the first element in the V-matrix [Average GDP per capita, 1973 – 1979]). All values are approximated to the second decimal digit.

Variables	Actual Mauritania	Synthetic Mauritania	V-matrix	African Countries average	OIC average
Avg. GDP per capita 1973 - 1979	751.98	751.88		1623.04	1492.34
Openness 1979	48.36	52.25	0.00	71.65	66.43
Population density 1979	1.45	14.40	160.03	31.21	53.15
Avg. Population growth 1973 - 1979	2.90	2.90	19.54	2.73	2.28
Avg. age dependency 1973 - 1979	94.20	96.34	3.09	93.56	87.28
No education 15+, 1975	63.26	63.24	11.91	67.67	62.08
No education 15+, 1970	65.27	76.14	0.00	74.97	69.71
Average Polity2 1973-1979	-7.00	-8.00	10.67	-6.32	-4.93
Islam major religion 1975	1.00	0.21	179.06	0.25	0.38
Avg. domestic credit to private sectors by banks (%GDP) 1973-1979	23.77	21.54	0.00	21.74	22.06

Table A5. Change in donor group: estimated synthetic control weights

Table AV reports the weights assigned to each country in the synthetic control for Mauritania. Rounding errors may prevent columns from summing to 1. "-" indicates that a country was dropped from the donor pool. APE-to-mean ratio indicates the average pre-intervention prediction error divided by the average pre-intervention outcome value. YCDR is the Yearly Compounded Diverge Rate, and it represents the speed of divergence between the synthetic control and the treated unit over the post-treatment period.

Donor Group	Weight
Benin	-
Cameroon	0.00%
Gabon	0.00%
Sierra Leone	0.00%
Algeria	8.65%
Guyana	-
Kenya	26.66%
Malaysia	0.00%
Togo	0.00%
Botswana	0.00%
Congo, Dem. Rep.	50.62%
Niger	0.00%
Rwanda	0.49%
South Africa	0.16%
Zambia	0.00%
Tunisia	-
Mali	13.41%
Swaziland	0.00%
Model fit pre-treatment	
RMSPE	3.17%
APE-to-mean ratio	-0.12%
SCM inference:	
RMSPE ratio	1.93
YCDR	0.71%

Table A6. Move the treatment test: estimated synthetic control weight

Table A6 reports the weights assigned to each country in the synthetic control for Mauritania. "-" indicates that the country has been removed from the donor group. Column (2) reports weights for the placebo test in which the treatment is applied in 1975. Column (3) reports weights for the placebo test in which the treatment is applied in 1966. Rounding errors may prevent columns from summing to 1.

Donor Group	short sample (1971 - 1979)	long sample (1960-1979)
Benin	0.00%	0.00%
Cameroon	0.00%	0.00%
Gabon	0.00%	0.00%
Sierra Leone	0.00%	0.00%
Algeria	0.00%	0.00%
Guyana	0.00%	0.00%
Kenya	2.94%	5.70%
Malaysia	0.00%	0.00%
Togo	0.00%	0.00%
Botswana	0.00%	0.00%
Congo, Dem. Rep.	85.56%	56.70%
Niger	0.00%	15.43%
Rwanda	0.00%	0.00%
South Africa	0.00%	2.15%
Zambia	0.00%	20.02%
Tunisia	0.00%	-
Mali	11.50%	-
Swaziland	0.00%	-
Model fit pre-treatment		
RMSPE	1.80%	6.69%
APE-to-mean ratio	0.10%	0.58%
SCM inference:		
RMSPE ratio	0.61	0.93

Table A7. Indicators Fit & V-Matrix

Table A7 reports values for the selected indicator variables for actual and synthetic Saudi Arabia. All the averages refer to the period 1985 - 1991, and all monetary values are expressed in 2005 dollars. Population growth and education variables are in percentage points. The V-matrix column reports the diagonal entries in the V matrix (its value are normalized relative to the first element in the V-Matrix [Average GDP per capita, 1985 – 1991]). All values are approximated to the second decimal digit.

Variables	Actual Saudi Arabia	Synthetic Saudi Arabia	V-matrix
Avg. GDP per capita 1985 - 1991	12129.72	12129.18	
Openness 1991	75.20	66.96	0.00
Population density 1991	7.86	9.45	0.15
Avg. Population growth 1985 - 1991	4.10	2.05	0.00
Avg. age dependency 1985 - 1991	81.97	81.97	17.21
No education 15+, 1990	35.75	39.14	0.00
No education 15+, 1985	43.69	43.47	0.04
Average Polity2 1985 - 1991	-10.00	-1.16	0.00
Islam major religion mid 1980	1.00	0.22	0.00
Domestic credit to private sectors by banks (%GDP) avg 1985 - 1991	19.00	25.69	0.01
Avg. oil rent (% GDP) 1985-1991	31.72	17.34	0.00

Table A8. Estimated Synthetic Control Weights

Table A8 reports the weights assigned to each country in the synthetic control for Saudi Arabia. Rounding errors may prevent columns from summing to 1. APE-to-mean ratio indicates the average pre-intervention prediction error divided by the average pre-intervention outcome value. YCDR is the Yearly Compounded Diverge Rate, and it represents the speed of divergence between the synthetic control and the treated unit over the post-treatment period.

Donor Group	Weight
Benin	0.00%
Cameroon	0.00%
Gabon	41.91%
Sierra Leone	0.00%
Algeria	0.00%
Guyana	0.00%
Kenya	0.00%
Malaysia	0.00%
Togo	0.00%
Senegal	0.00%
Turkey	0.00%
Niger	0.00%
Ecuador	0.00%
Venezuela	18.87%
Canada	0.00%
Tunisia	0.00%
Mali	21.81%
Norway	17.42%
Gambia	0.00%
Model fit pre-treatment	
RMSPE	3.28%
APE-to-mean ratio	0.00%
SCM inference:	
RMSPE ratio	5.51
YCDR	2.38%

Table B1. Summary statistics – M&A activity

Table B1 reports the number of corporate acquisitions selected for our study by year of completion (Panel A), by acquiring firm industry (Panel B), and by acquired firm industry (Panel C) over the period from January 1, 1979, to December 31, 2014. Average Deal Value is reported in current U.S. dollar. The sample selection is described in Appendix A. Industries are defined based upon the two digits SIC classification.

Panel A: Deals - Time Series Distribution

Year	Number of Deals	Average Deal Value
1979	2	$ 272.22
1980	2	$ 259.77
1981	5	$ 294.11
1982	4	$ 1,146.26
1983	6	$ 390.68
1984	11	$ 1,407.29
1985	17	$ 727.61
1986	15	$ 673.08
1987	15	$ 968.94
1988	8	$ 1,209.22
1989	5	$ 1,358.70
1990	8	$ 994.98
1991	7	$ 469.66
1992	6	$ 195.08
1993	10	$ 435.85
1994	8	$ 511.04
1995	14	$ 2,450.91
1996	18	$ 1,514.03
1997	22	$ 918.33
1998	20	$ 1,843.08
1999	31	$ 1,314.82
2000	21	$ 2,158.46
2001	13	$ 606.45
2002	6	$ 347.82
2003	8	$ 548.66
2004	12	$ 904.22
2005	7	$ 4,354.07
2006	13	$ 1,498.00
2007	13	$ 1,928.10
2008	7	$ 1,720.89
2009	7	$ 2,209.24
2010	12	$ 2,560.58
2011	6	$ 4,132.64
2012	9	$ 1,528.10
2013	8	$ 2,313.18
2014	7	$ 3,467.97
Full-Sample	383	$ 1,378.72

Panel B: Industry Distribution - Acquirers			
Industry	**N**	**Total Deal Value**	
Agriculture, Forestry and Fishing	1	$	251.00
Mining	32	$	1,550.09
Construction	2	$	932.52
Manufacturing	220	$	1,354.73
Transportation, Communications, Electric, Gas and Sanitary service	39	$	2,433.92
Wholesale Trade	14	$	596.52
Retail Trade	32	$	1,565.83
Services	38	$	920.20
Non-Classifiable	5	$	1,136.12
Panel C: Industry Distribution - Targets			
Industry	**N**	**Total Deal Value**	
Agriculture, Forestry and Fishing	3	$	2,792.65
Mining	29	$	1,834.27
Construction	2	$	932.52
Manufacturing	219	$	1,329.32
Transportation, Communications, Electric, Gas and Sanitary service	36	$	2,587.93
Wholesale Trade	19	$	621.38
Retail Trade	33	$	1,451.97
Services	47	$	907.00

Table B2. Summary statistics

Table B2 reports summary statistics for the selected sample of 383 M&A deals. Panel A reports accounting data for both target and acquirers, as observed at the end of the fiscal year preceding the transaction. Panel B reports summary statistics for deal-level variables. All indicators are defined in Appendix B. Values are approximated to the second decimal digit. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Firm Level Summary Statistics

	Targets		Acquirers		Tests for difference in means	
Variable	N	Mean	N	Mean	Difference	T-Test (p-value)
Size	383	5.98	383	7.57	-1.58	0.00
Log Sale	383	5.99	383	7.49	-1.49	0.00
Tobin's Q	383	1.69	383	1.93	-0.23	0.01
Leverage	383	0.54	383	0.56	-0.01	0.83
ROS	383	0.14	383	0.11	-0.05	0.08
ROA	383	0.04	383	0.06	-0.02	0.00
Asset Utilization	383	1.24	383	1.19	-0.05	0.39
Lerner Index	383	0.03	383	0.05	-0.02	0.00
Cash	383	91.29	383	794.94	-703.65	0.00
Capital Expenditures						
Payout Ratio	383	0.22	383	0.30	-0.084	0.41
R&D to Sale	219	0.10	224	0.07	0.038	0.25

Panel B: Deal Level Summary Statistics

Variable	N	Mean
Transaction Value	383	1419.81
Price per Share	366	32.31
Serial Acquirer - Current Fiscal Year	383	0.56
Number of Transaction - Current Fiscal Year	383	9.00
Diversification	383	0.28
Cash Only Acquisition	383	0.41
Shares Only Acquisition	383	0.25

Table B3. Synthetic control method and propensity score matching

Table B3 reports descriptive statistics for the quality of the matched sample generated through the synthetic control method and propensity score matching. RMSPE < X% indicates that both the acquiring firm and the acquired firms have counterfactuals whose root mean squared prediction error is lower than X%. Values are approximated to the fourth decimal digit. P-values indicate the statistical significance of the RMSPE between the synthetic control method matched sample and the propensity score matched sample.

	Dependent variable: ROS						
Performance Indicator	Synthetic Control: RMSPE			Propensity Score: RMSPE			
	N	Mean	St. Dev.	N	Mean	St. Dev.	P-value
RMSPE < 10%	212	0.0314	0.0416	192	0.0441	0.0458	0.0001
RMSPE < 5%	164	0.0196	0.0079	118	0.0281	0.0152	0.0001
RMSPE < 2.5%	111	0.0126	0.0042	43	0.0142	0.0061	0.2569
	Dependent variable: ROA						
Performance Indicator	Synthetic Control: RMSPE			Propensity Score: RMSPE			
	N	Mean	St. Dev.	N	Mean	St. Dev.	P-value
RMSPE < 10%	188	0.0437	0.0246	202	0.0526	0.0247	0.0010
RMSPE < 5%	124	0.0285	0.0119	121	0.0320	0.0123	0.0287
RMSPE < 2.5%	60	0.0146	0.0057	37	0.0166	0.0056	0.3343
	Dependent variable: Asset Utilization						
Performance Indicator	Synthetic Control: RMSPE			Propensity Score: RMSPE			
	N	Mean	St. Dev.	N	Mean	St. Dev.	P-value
RMSPE < 10%	234	0.0395	0.0215	222	0.0672	0.0285	0.0005
RMSPE < 5%	127	0.0267	0.0102	101	0.0401	0.0154	0.0001
RMSPE < 2.5%	45	0.0192	0.0081	19	0.0223	0.0128	0.0612
	Dependent variable: Lerner Index						
Performance Indicator	Synthetic Control: RMSPE			Propensity Score: RMSPE			
	N	Mean	St. Dev.	N	Mean	St. Dev.	P-value
RMSPE < 10%	222	0.0267	0.0226	227	0.0491	0.0236	0.0001
RMSPE < 5%	186	0.0184	0.0122	127	0.0318	0.0121	0.0001
RMSPE < 2.5%	138	0.0125	0.0067	41	0.0171	0.0057	0.0029

Table B4. Synthetic VS Real Units – Difference in trends

Table B4 reports linear models estimated via OLS over the 5 fiscal years preceding the M&A transaction. The dependent variable is ROA. Only real and synthetic acquirers are included in Panel A. Only real and synthetic targets are included in Panel B. M&A Dummy is a dummy variable set equal to 1 for the real unit, 0 otherwise. Year -t is a dummy variable set equal to 1 if the observation is recorded t years before the event, 0 otherwise. Model 1 includes the whole sample of 283 acquirers. Model 2, Model 3, and Model 4 include all deals for which both the acquirer and the target's synthetic counterfactual have a RMSPE is below 10%, 5%, and 2.5%, respectively. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Acquiring Firms

VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
M&A Dummy	-0.184***	-0.00809	-0.00122	0.000334
	(0.0342)	(0.00649)	(0.00699)	(0.00644)
year -4	0.00573	6.69e-05	5.74e-05	0.00250
	(0.00903)	(0.00563)	(0.00664)	(0.00677)
year -3	-0.00195	0.000782	-0.000929	0.00274
	(0.00991)	(0.00548)	(0.00575)	(0.00669)
year -2	0.00148	0.00188	0.00141	0.00558
	(0.00856)	(0.00472)	(0.00588)	(0.00596)
year -1	-0.00489	-0.00268	-0.000193	0.00247
	(0.0100)	(0.00521)	(0.00632)	(0.00788)
M&A x year -4	-0.0369	0.00616	0.00263	0.000477
	(0.0430)	(0.0104)	(0.00797)	(0.0105)
M&A x year -3	-0.0392	0.00870	0.00577	0.00291
	(0.0490)	(0.00950)	(0.00848)	(0.00905)
M&A x year -2	-0.0431	0.0113	0.00560	-0.000441
	(0.0420)	(0.00768)	(0.00857)	(0.00787)
M&A x year -1	-0.0180	0.0145*	0.00666	0.00586
	(0.0392)	(0.00876)	(0.00806)	(0.0108)
Constant	0.245***	0.0920***	0.0932***	0.0920***
	(0.0326)	(0.00517)	(0.00425)	(0.00465)
Observations	2,830	1880	1,240	600
Adjusted R-squared	0.001	0.009	-0.001	-0.006

Panel B: Acquired Firms				
VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
M&A Dummy	1.180***	1.050***	1.026***	1.020***
	(0.0960)	(0.0680)	(0.0855)	(0.0833)
year -4	0.00109	-0.000977	-0.000887	2.46e-05
	(0.00315)	(0.00340)	(0.00314)	(0.00473)
year -3	0.00140	-0.000300	-0.00114	0.000356
	(0.00310)	(0.00370)	(0.00296)	(0.00450)
year -2	0.00169	0.00136	0.00217	0.00350
	(0.00349)	(0.00336)	(0.00288)	(0.00430)
year -1	0.00124	0.000591	0.00229	0.00378
	(0.00372)	(0.00301)	(0.00252)	(0.00433)
M&A x year -4	0.0169	0.0395	0.0378	0.0643
	(0.126)	(0.0857)	(0.102)	(0.120)
M&A x year -3	0.0346	0.0740	0.0709	0.0658
	(0.126)	(0.0835)	(0.112)	(0.139)
M&A x year -2	-0.0122	0.0715	0.0918	0.0525
	(0.103)	(0.0932)	(0.105)	(0.121)
M&A x year -1	0.107	0.100	0.102	0.0965
	(0.126)	(0.0726)	(0.122)	(0.129)
Constant	0.0307***	0.0346***	0.0331***	0.0304***
	(0.00264)	(0.00247)	(0.00203)	(0.00309)
Observations	2,830	1880	1,240	600
Adjusted R-squared	-0.001	0.004	0.008	0.008

Table B5. Performance consequences of M&A transactions - ROA

Table B5, Panel A reports 4 difference-in-difference models estimated via OLS over the 6 years surrounding the studied M&A transaction (-3y ; 3y). The dependent variable is ROA. M&A Dummy is a dummy variable set equal to 1 for the real unit, 0 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model 1 is estimated over the full sample of 283 deals, while Model II, Model III and Model IV are based on observations for which the RMSPE originated from the synthetic matching is below 10%, 5% and 2.5%, respectively. Standard errors are bootstrapped. Table B5, Panel B replicates Panel A using our propensity score matched sample. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Synthetic Control Matched Sample

VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
M&A Dummy	0.157***	0.0162***	0.00535**	0.00143
	(0.0158)	(0.00307)	(0.00215)	(0.00306)
Post	-0.0102**	-0.0139***	-0.0149***	-0.0129***
	(0.00486)	(0.00327)	(0.00335)	(0.00387)
M&A Dummy x Post	-0.0712***	0.00225	0.00421	0.000891
	(0.0185)	(0.00546)	(0.00453)	(0.00582)
Constant	0.0605***	0.0840***	0.0920***	0.0950***
	(0.00306)	(0.00194)	(0.00183)	(0.00217)
Observations	3,920	3,008	1,984	800
Adjusted R-squared	0.049	0.026	0.018	0.025

Panel B: Propensity Score Matched Sample

VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
M&A Dummy	1.619***	0.00464	0.00467	0.00164
	(0.310)	(0.00283)	(0.00345)	(0.00375)
Post	0.0757	-0.0107***	-0.00769*	-0.0122***
	(0.607)	(0.00310)	(0.00409)	(0.00406)
M&A Dummy x Post	-0.0974	-0.0108**	-0.00403	0.000265
	(0.608)	(0.00540)	(0.00514)	(0.00696)
Constant	-1.508***	0.102***	0.0935***	0.0954***
	(0.309)	(0.00203)	(0.00263)	(0.00256)
Observations	5,648	3,672	1,944	592
Adjusted R-squared	0.004	0.011	0.007	0.013

Table B6. Market power & efficiency gains in M&A transactions

Table B6 reports 8 difference-in-difference models estimated via OLS over the 6 fiscal years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A Dummy is a dummy variable set equal to 1 for the real unit, 0 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model 1 is estimated over the full sample of 283 deals, while Model 2, Model 3 and Model 4 are estimated on observations for which the RMSPE originated from the synthetic matching is below 10%, 5% and 2.5%, respectively. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
VARIABLES	Full Sample	10% RMSPE	5% RMSPE	2.5% RMSPE
M&A Dummy	0.114***	0.146***	0.122***	0.140***
	(0.0293)	(0.0382)	(0.0357)	(0.0538)
Post	-0.0119	-0.0158	-0.0264	-0.0276
	(0.0337)	(0.0411)	(0.0399)	(0.0491)
M&A x Post	-0.147***	-0.157***	-0.152***	-0.160*
	(0.0436)	(0.0561)	(0.0575)	(0.0846)
Constant	1.103***	1.153***	1.226***	1.332***
	(0.0170)	(0.0235)	(0.0264)	(0.0279)
Observations	3,920	3,276	1,778	630
Adjusted R-squared	0.006	0.008	0.006	0.006

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
VARIABLES	Full Sample	10% RMSPE	5% RMSPE	2.5% RMSPE
M&A Dummy	-0.00414	-0.00240	-0.000253	-4.20e-05
	(0.00710)	(0.00329)	(0.00356)	(0.00328)
Post	-0.0886***	-0.0617***	-0.0579***	-0.0515***
	(0.0230)	(0.0119)	(0.0128)	(0.0110)
M&A x Post	0.0965***	0.0543***	0.0490***	0.0449***
	(0.0236)	(0.0117)	(0.0136)	(0.0109)
Constant	0.101***	0.122***	0.118***	0.108***
	(0.00590)	(0.00274)	(0.00251)	(0.00227)
Observations	3,920	3,108	2,562	1,932
Adjusted R-squared	0.012	0.024	0.024	0.024

Table B7. Sales-based measures of operating performance

Table B7 reports 3 difference-in-difference models estimated via OLS over the 6 years surrounding the studied M&A transaction (-3y ; 3y). The dependent variable is return on sales in Column (1), the industry adjusted return on capital in Column (2), and the gross margin in Column (3). M&A Dummy is a dummy variable set equal to 1 for the real unit, 0 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. All models are based on observations for which the RMSPE originated from the synthetic matching is below 5%. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Synthetic Control Matched Sample

	(1)	(2)	(3)
VARIABLES	ROS	IAROC	Gross Margin
M&A Dummy	-0.0013	0.0001	-0.0236***
	(0.00289)	(0.00574)	(0.0056)
Post	-0.0216***	-0.0138**	0.0097
	(0.00603)	(0.00666)	(0.0060)
M&A Dummy x Post	0.0493***	0.0242**	0.0153***
	(0.0126)	(0.00967)	(0.0012)
Constant	0.0616***	0.0820***	0.0976***
	(0.00188)	(0.00445)	(0.0033)
Observations	2,296	2,296	1,506
Adjusted R-squared	0.031	0.009	0.019

Table B8. Placebo tests

Table B8 reports 4 difference-in-difference models estimated via OLS over the 6 fiscal years surrounding a randomly assigned event date. The dependent variable are ROS (column 1), ROA (column 2), Asset utilization (Column 3), and the Lerner Index (Column 4). M&A Dummy is a dummy variable set equal to 1 for the real unit, 0 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. All models are estimated over the placebo sample for which the RMSPE originated from the synthetic matching is below 10%. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ROS	ROA	Asset Utilization	Lerner Index
M&A Dummy	0.0425**	0.0001	0.0912***	0.0066***
	(0.0056)	(0.0023)	(0.0456)	(0.0022)
Post	-0.0212	-0.0294***	-0.0753	-0.0092***
	(0.0150)	(0.0043)	(0.0273)	(0.0034)
M&A x Post	-0.0260	-0.0018	-0.0587	0.0065
	(0.0229)	(0.0538)	(0.0539)	(0.0050)
Constant	0.0379***	0.0889***	1.259***	0.0652
	(0.0035)	(0.0015)	(0.0200)	(0.0020)
Observations	2,974	3,112	3,466	3,218
Adjusted R-squared	0.021	0.008	0.006	0.006

Table B9. Subsample analyses: competition

Table B9 reports 8 difference-in-difference models estimated via OLS over the 6 years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A is a dummy variable set equal to 1 for real units, 0 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model I and Model III are estimated over the matched sample for which the obtained RMSPE from the synthetic control method matching is below 10%, while Model II and Model IV are based on observations for which the RMSPE originated from the synthetic matching is below 5%. Concentrated industries are defined as those with an HHI greater or equal to 1,800 points. The HHI is defined based on 3-digit SIC industry codes, using all firms included in the Compustat dataset in a given fiscal year. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
	Competitive Industries		Concentrated Industries	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	0.0901**	0.0655	0.285***	0.247***
	(0.0438)	(0.0516)	(0.0444)	(0.0548)
Post	-0.0163	-0.0290	-0.0144	-0.0207
	(0.0419)	(0.0577)	(0.0383)	(0.0437)
M&A x Post	-0.137**	-0.132	-0.208***	-0.198***
	(0.0579)	(0.0926)	(0.0775)	(0.0748)
Constant	1.151***	1.245***	1.156***	1.186***
	(0.0291)	(0.0306)	(0.0231)	(0.0273)
Observations	2,332	1,224	944	554
Adjusted R-squared	0.003	0.002	0.043	0.031

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
	Competitive Industries		Concentrated Industries	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	-0.00147	0.000145	-0.00472	-0.00113
	(0.00410)	(0.00377)	(0.00547)	(0.00468)
Post	-0.0619***	-0.0538***	-0.0613**	-0.0671***
	(0.0141)	(0.0110)	(0.0263)	(0.0232)
M&A x Post	0.0538***	0.0444***	0.0556**	0.0591**
	(0.0157)	(0.0118)	(0.0276)	(0.0241)
Constant	0.125***	0.122***	0.115***	0.111***
	(0.00280)	(0.00308)	(0.00342)	(0.00367)
Observations	2,212	1,763	896	799
Adjusted R-squared	0.023	0.022	0.022	0.026

Table B10. Subsample analyses: deal size

Table B10 reports 8 difference in difference models estimated via OLS over the 6 years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A is a dummy variable set equal to 1 if the unit is complete the transaction, 1 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model I and Model III are estimated over the matched sample for which the obtained RMSPE from the synthetic control method matching is below 10%, while Model II and Model IV are based on observations for which the RMSPE originated from the synthetic matching is below 5%. Deal size represents the dollar value of the completed M&A deals included in the studied sample. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
	Below Median Deal Size		Above Median Deal Size	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	0.145***	0.121	0.147***	0.123**
	(0.0536)	(0.0793)	(0.0450)	(0.0542)
Post	-0.0266	-0.0381	-0.00726	-0.0165
	(0.0632)	(0.0655)	(0.0438)	(0.0464)
M&A x Post	-0.155*	-0.149	-0.160**	-0.155*
	(0.0899)	(0.118)	(0.0764)	(0.0854)
Constant	1.206***	1.271***	1.110***	1.189***
	(0.0430)	(0.0447)	(0.0258)	(0.0290)
Observations	1,568	1,360	1,984	1,616
Adjusted R-squared	0.005	0.004	0.009	0.007

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
	Below Median Deal Size		Above Median Deal Size	
VARIABLES	10% RMSPE	5% RMSPE	5% RMSPE	10% RMSPE
M&A Dummy	-0.00335	0.00107	-0.00166	-0.00137
	(0.00447)	(0.00440)	(0.00499)	(0.00521)
Post	-0.0377***	-0.0418***	-0.0806***	-0.0715***
	(0.0134)	(0.0145)	(0.0192)	(0.0140)
M&A x Post	0.0364***	0.0346**	0.0685***	0.0611***
	(0.0135)	(0.0156)	(0.0203)	(0.0149)
Constant	0.116***	0.114***	0.127***	0.122***
	(0.00361)	(0.00393)	(0.00318)	(0.00337)
Observations	1,568	1,360	1,984	1,616
Adjusted R-squared	0.013	0.017	0.031	0.029

Table B11. Subsample analyses: pre & post 2000

Table B11 reports 8 difference in difference models estimated via OLS over the 6 years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A is a dummy variable set equal to 1 if the unit is complete the transaction, 1 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model I and Model III are estimated over the matched sample for which the obtained RMSPE from the synthetic control method matching is below 10%, while Model II and Model IV are based on observations for which the RMSPE originated from the synthetic matching is below 5%. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
	Period: 1979-2000		Period: 2001-2014	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	0.147***	0.125***	0.144**	0.114
	(0.0502)	(0.0385)	(0.0588)	(0.0724)
Post	0.00916	-0.00118	0.0349	0.0216
	(0.0506)	(0.0538)	(0.0467)	(0.0673)
M&A x Post	-0.133	-0.135	-0.186**	-0.170**
	(0.0909)	(0.0836)	(0.0821)	(0.0725)
Constant	1.203***	1.273***	1.010***	1.083***
	(0.0237)	(0.0243)	(0.0370)	(0.0624)
Observations	2,382	2,034	1,170	942
Adjusted R-squared	0.005	0.003	0.005	0.003

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
	Period: 1979-2000		Period: 2001-2014	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	-0.00298	-0.000662	-0.000758	0.000992
	(0.00347)	(0.00394)	(0.00623)	(0.00627)
Post	-0.0527***	-0.0522***	-0.0772***	-0.0708***
	(0.0146)	(0.0154)	(0.0181)	(0.0180)
M&A x Post	0.0493***	0.0473***	0.0595***	0.0505***
	(0.0157)	(0.0165)	(0.0196)	(0.0186)
Constant	0.119***	0.115***	0.130***	0.128***
	(0.00254)	(0.00270)	(0.00463)	(0.00569)
Observations	2,382	2,034	1,170	942
Adjusted R-squared	0.022	0.022	0.025	0.026

Table B12. Subsample analyses: horizontal VS diversifying acquisitions

Table B12 reports 8 difference in difference models estimated via OLS over the 6 years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A is a dummy variable set equal to 1 if the unit is complete the transaction, 1 otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Model I and Model III are estimated over the matched sample for which the obtained RMSPE from the synthetic control method matching is below 10%, while Model II and Model IV are based on observations for which the RMSPE originated from the synthetic matching is below 5%. Horizontal Acquisitions are those for which both the target and the acquirer operates in the same 2-digit SIC code industry. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
	Horizontal Acquisitions		Diversifying Acquisitions	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	0.142*	0.144**	0.149***	0.111**
	(0.0733)	(0.0653)	(0.0331)	(0.0488)
Post	-0.0189	-0.0264	-0.0144	-0.0264
	(0.0738)	(0.0910)	(0.0324)	(0.0380)
M&A x Post	-0.132	-0.137	-0.169***	-0.160**
	(0.109)	(0.107)	(0.0486)	(0.0687)
Constant	1.200***	1.254***	1.131***	1.213***
	(0.0432)	(0.0496)	(0.0224)	(0.0303)
Observations	1,104	992	2,448	1,984
Adjusted R-squared	0.004	0.004	0.008	0.006

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
	Horizontal Acquisitions		Diversifying Acquisitions	
VARIABLES	10% RMSPE	5% RMSPE	10% RMSPE	5% RMSPE
M&A Dummy	-0.00188	0.000224	-0.00264	-0.000491
	(0.00622)	(0.00495)	(0.00375)	(0.00413)
Post	-0.0803***	-0.0853***	-0.0533***	-0.0442***
	(0.0265)	(0.0282)	(0.0127)	(0.00918)
M&A x Post	0.0790***	0.0798***	0.0432***	0.0336***
	(0.0264)	(0.0286)	(0.0136)	(0.00932)
Constant	0.113***	0.110***	0.126***	0.123***
	(0.00466)	(0.00378)	(0.00262)	(0.00281)
Observations	1,104	992	2,448	1,984
Adjusted R-squared	0.025	0.027	0.023	0.025

Table B13. The determinants of the competitive gains

Table B13 reports panel regressions of the difference between the observed post-acquisition Asset Utilization (Panel A) or Lerner Index (Panel B) and their synthetically constructed value for the matched merged firm on several control variables. All covariates are defined in Table A.II, in the Appendix. Controls are simultaneous to the dependent variable, and the regression is estimated via OLS over the 3 years following the transaction. Standard errors are bootstrapped. *, **, and *** indicate statistical significance at the 1%, 5%, and 10%, respectively.

Panel A: Asset Utilization

VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
Size	-0.0488*	-0.0332	-0.0465*	-0.0102
	(0.0255)	(0.0255)	(0.0281)	(0.0310)
Acquirer-Target premium	-0.0691	-0.000800	-0.0109	0.0245
	(0.0541)	(0.0286)	(0.0331)	(0.0489)
Tobin's Q	0.127***	0.211***	0.234***	0.296***
	(0.0441)	(0.0326)	(0.0377)	(0.0520)
Leverage	-0.0647***	-0.0664***	-0.0686***	-0.0704***
	(0.0196)	(0.0174)	(0.0203)	(0.0252)
Cash	-0.137	-1.445***	-1.484***	-1.287***
	(0.342)	(0.266)	(0.279)	(0.380)
Lerner Index	-1.208***	-5.103***	-6.289***	-8.550***
	(0.301)	(0.513)	(0.651)	(1.007)
Transaction Value to Total Assets	-0.00155	0.0181	0.0179	0.0287
	(0.0204)	(0.0234)	(0.0241)	(0.0320)
Serial Acquirer	0.176**	0.0456	0.00253	-0.0234
	(0.0710)	(0.0635)	(0.0726)	(0.0799)
Diversification	-0.0846	-0.0640	-0.0187	0.0151
	(0.0694)	(0.0668)	(0.0738)	(0.0834)
Shares Only Acquisition	-0.274***	-0.290***	-0.326***	-0.286***
	(0.0674)	(0.0617)	(0.0692)	(0.0784)
HHI	0.992***	0.462**	0.178	-0.144
	(0.240)	(0.225)	(0.241)	(0.278)
Constant	1.368***	1.783***	2.062***	1.984***
	(0.218)	(0.235)	(0.259)	(0.308)
Observations	840	702	381	135
Adjusted R-squared	0.101	0.281	0.309	0.376

Panel B: Lerner Index				
VARIABLES	(1) Full Sample	(2) 10% RMSPE	(3) 5% RMSPE	(4) 2.5% RMSPE
Size	-0.0206	-0.00887	-0.0104	0.00546
	(0.0171)	(0.0116)	(0.0144)	(0.0112)
Acquirer-Target premium	-0.0902*	-0.0158	-0.0113	-0.0171
	(0.0539)	(0.0154)	(0.0180)	(0.0161)
Tobin's Q	0.0999***	0.0417***	0.0475***	0.0618**
	(0.0379)	(0.0150)	(0.0178)	(0.0258)
Leverage	0.0105	-0.00170	-0.00338	-0.00620
	(0.0105)	(0.00461)	(0.00463)	(0.00622)
Cash	0.761***	0.0838	0.0959	0.0613
	(0.287)	(0.0891)	(0.106)	(0.116)
Asset Utilization	-0.0364**	-0.0192**	-0.0169**	-0.0215**
	(0.0156)	(0.00783)	(0.00734)	(0.00921)
Transaction Value to Total Assets	0.00780	0.0235	0.0255	0.0361
	(0.0160)	(0.0214)	(0.0223)	(0.0313)
Serial Acquirer	0.0763	-0.0107	-0.00852	-0.0302
	(0.0474)	(0.0227)	(0.0271)	(0.0204)
Diversification	-0.0339	-0.0397	-0.0388	-0.0170
	(0.0391)	(0.0288)	(0.0294)	(0.0272)
Shares Only Acquisition	-0.0917**	-0.0438**	-0.0414**	-0.0426***
	(0.0447)	(0.0175)	(0.0168)	(0.0152)
HHI	-0.0682	-0.0111	0.0133	0.0242
	(0.0895)	(0.0586)	(0.0590)	(0.0646)
Constant	0.0907	0.109	0.102	-0.0451
	(0.109)	(0.0911)	(0.106)	(0.0639)
Observations	728	617	515	380
Adjusted R-squared	0.106	0.057	0.066	0.125

Table B14. Competitive Gains: only manufacturing firms

Table B14 reports 8 difference in difference-in-difference models estimated via OLS over the 6 years surrounding the studied M&A transactions. The dependent variable is Asset Utilization in Panel A, and the Lerner Index in Panel B. M&A is a dummy variable set equal to1 for real units, zero otherwise. Post is a dummy variable set equal to 1 if the firm-year observation occurs after the transaction, 0 otherwise. Only firms operating in the manufacturing industry are included. Model 1 is estimated over the full sample for the period 1997-2007. Model II, Model III and Model IV are estimated over the same period, but based on observations for which the RMSPE originated from the synthetic matching is below 10%, 5% and 2.5%, respectively. Standard errors are bootstrapped. *, **, *** indicate statistical significance at the 10%, 5% and 1% level of significance, respectively.

Panel A: Asset Utilization

	(1)	(2)	(3)	(4)
VARIABLES	Full Sample	10% RMSPE	5% RMSPE	2.5% RMSPE
M&A	0.0769	0.0258	0.0184	-0.0105
	(0.0598)	(0.0743)	(0.0787)	(0.0978)
Post	0.0814	0.0555	0.0189	0.0740
	(0.0571)	(0.0549)	(0.0764)	(0.0895)
M&A x Post	-0.157	-0.112	-0.123	-0.0583
	(0.0977)	(0.105)	(0.116)	(0.140)
Constant	0.981***	1.032***	1.131***	1.211***
	(0.0367)	(0.0430)	(0.0537)	(0.0634)
Observations	592	498	398	288
Adjusted R-squared	-0.000	-0.003	-0.002	-0.008

Panel B: Lerner Index

	(1)	(2)	(3)	(4)
VARIABLES	Full Sample	10% RMSPE	5% RMSPE	2.5% RMSPE
M&A	-0.00372	-0.00723	-0.00156	-0.000354
	(0.0122)	(0.00935)	(0.00775)	(0.00760)
Post	-0.0633***	-0.0604***	-0.0672***	-0.0671***
	(0.0224)	(0.0175)	(0.0171)	(0.0178)
M&A x Post	0.0710***	0.0568***	0.0448**	0.0374**
	(0.0263)	(0.0206)	(0.0194)	(0.0188)
Constant	0.108***	0.122***	0.132***	0.126***
	(0.00878)	(0.00615)	(0.00629)	(0.00663)
Observations	592	498	398	288
Adjusted R-squared	0.025	0.038	0.059	0.056

Table C1. Time Distribution of Completed and Withdrawn Share Issues

Table C1 reports the time distribution of completed and withdrawn share issues attempted by both privately owned and state owned enterprises. Proceeds for withdrawn transactions are defined as the product between the mid-price and the number of shares issued, as reported in the filing documents. Reported proceeds are severely biased downward due to missing/omitted observations. Approximately 6.00% of the observations for proceeds raised by privately owned companies successfully completing a share issue are not reported in the original dataset. Analogously, 20.00% of the proceeds raised by governments from share issue privatizations are missing. Finally, we are unable to identify the proceeds associated with 60.00% of withdrawn share issues attempted by both private and state owned companies. All monetary values are reported in millions of U.S. dollars.

	Private - Successful		Private – Withdrawal		Privatization - Successful		Privatization - Withdrawal	
Year	N	Proceeds	N	Proceeds	N	Proceeds	N	Proceeds
1998	4,621	$362,750.54	262	$1,318.17	572	$77,299.05	14	$200.00
1999	5,129	$472,126.79	181	$298.41	277	$129,799.51	26	$452.80
2000	5,773	$502,751.30	446	$964.42	162	$82,176.19	8	-
2001	4,343	$1,147,271.74	390	$655.65	146	$42,072.52	54	$2,536.70
2002	4,464	$292,073.99	243	$5,072.71	171	$48,914.93	39	$1,563.24
2003	5,015	$333,531.54	93	$9,056.47	187	$72,649.18	11	$1,199.22
2004	6,037	$411,638.10	215	$41,893.77	247	$121,506.31	10	$2,928.79
2005	6,125	$537,763.52	158	$20,348.49	173	$79,552.35	5	$1,196.77
2006	7,069	$715,607.55	199	$32,122.34	207	$109,366.59	5	$5,808.51
2007	9,990	$1,012,156.95	339	$49,657.29	308	$171,176.34	16	$6,869.33
2008	7,711	$722,469.02	537	$123,713.89	241	$102,477.39	16	$3,886.13
2009	10,008	$897,676.79	437	$78,624.48	289	$102,202.80	16	$6,877.38
2010	11,676	$868,453.92	609	$77,297.31	459	$217,779.61	34	$14,995.21
2011	10,060	$690,669.26	694	$86,030.44	358	$150,370.86	29	$14,283.92
2012	8,904	$579,095.18	551	$43,800.17	392	$143,849.80	22	$9,375.02
2013	9,143	$700,397.05	382	$33,431.11	382	$132,957.76	13	$3,984.21
2014	9,653	$ 767.237.92	449	$41,577.95	446	$187,916.19	23	$6,680.08
2015	7,935	$1,059,050.09	352	$89,825.47	307	$193,555.40	45	$24,483.49
2016	11,766	$863,753.70	584	$62,301.68	473	$229,634.50	62	$9,182.84
TOTAL	145,422	$12,169,237.03	7,121	$797,990.56	5,797	$2,395,257.97	412	$116,504.94
YEARLY MEAN	7,654	$676,069.28	375	$41,999.77	305	$126,066.84	22	$6,472.55

Table C2. Withdrawn Share Issues: A Country-Level Perspective

Table C2 reports the incidence of share issues' withdrawals for countries experiencing at least five SIP withdrawals. The reported frequencies are relative to the time period from 1998 to 2016. Percentage are rounded to the second decimal unit.

	Privately Owned Firms				State Own Enterprises			
country	Total	Successful	Withdrawal	% of withdrawal	Total	Successful	Withdrawal	% of withdrawal
Australia	18687	18387	300	1.61%	234	222	12	5.13%
Brazil	930	884	46	4.95%	89	81	8	8.99%
China	7772	6834	938	12.07%	1405	1250	155	11.03%
Czech Republic	50	43	7	14.00%	22	16	6	27.27%
France	2282	2196	86	3.77%	101	96	5	4.95%
Greece	571	525	46	8.06%	80	71	9	11.25%
Hong Kong	5512	5328	184	3.34%	318	311	7	2.20%
India	4815	4676	139	2.89%	388	370	18	4.64%
Italy	1005	892	113	11.24%	106	88	18	16.98%
Malaysia	2133	2052	81	3.80%	129	123	6	4.65%
Norway	860	826	34	3.95%	27	22	5	18.52%
Poland	1152	1127	25	2.17%	135	123	12	8.89%
Russian Fed	730	677	53	7.26%	224	210	14	6.25%
Singapore	2212	2149	63	2.85%	119	113	6	5.04%
South Korea	5630	5433	197	3.50%	176	169	7	3.98%
Taiwan	3748	3233	515	13.74%	55	50	5	9.09%
Turkey	569	513	56	9.84%	48	40	8	16.67%
United Kingdom	9722	9516	206	2.12%	128	122	6	4.69%
Vietnam	940	892	48	5.11%	238	223	15	6.30%

Table C3. Summary Statistics

Table C3 reports summary statistics for successful and withdrawn SIPs. Panel A reports accounting and financial variables. Panel B shows summary statistics for the available political variables. Variables' description is provided in Table A.I. All values are rounded to the second decimal unit. Monetary values are deflated to 2010 $. All values are winsorized at the 1% level. For non-U.S. firms, the yearly average exchange rates have been applied for conversion. For Countries that adopted the Euro in January, 1999, the exchange rate of the original country was used till the end of the fiscal year 1998. Bold values are statistically significant at the 5% level of confidence from those reported for successful privatizations. Statistical significance is assessed using the Wilcoxon Rank Sum tests for mean and median values.

Panel A: Accounting and Financial Variables						
	Successful Privatizations			Withdrawn Privatizations		
Variable	N	Mean	Median	N	Mean	Median
Log Size	979	8.81	7.14	545	**9.44**	**8.13**
# of Employees	979	13940.54	4128.00	545	**22102.70**	**5865.00**
Sales Growth	979	0.26	0.19	545	**0.18**	**0.12**
ROS	979	0.08	0.05	545	0.08	0.06
Operating efficiency	979	0.03	0.01	545	0.04	0.01
Labor Intensity	979	5.16	3.52	545	**3.86**	**2.18**
Capital Expenditures	979	0.08	0.06	545	0.07	0.06
Accruals	979	0.15	0.12	545	**0.09**	**0.07**
Payout	979	0.04	0.03	545	0.03	0.03
Leverage	979	0.30	0.28	545	0.32	**0.30**
IPO	979	0.23	0.00	545	0.25	0.00
1 month market returns	979	0.01	0.01	545	0.01	0.01
Time to action	783	215.76	182.00	482	233.06	**199.00**
# of IPOs	979	290.40	258.00	545	**146.59**	**60.00**
Two-Year GDP forecast	979	7.46	7.89	545	**5.34**	**5.18**
GDP per Capita	979	8.55	8.64	545	**9.15**	8.86
Panel B: Political Variables						
Cumulative Proceeds from SIPs	979	112.58	124.91	545	**66.13**	**9.68**
Public Debt	979	0.26	0.31	545	**0.49**	**0.34**
Left-Wing Government	979	0.74	1.00	545	**0.59**	**1.00**
Election Year	979	0.02	0.00	545	**0.17**	0.00
Political Competitiveness	979	0.81	1.00	545	**0.44**	**0.35**
Politi IV Index	979	-5.44	-7.00	545	**2.19**	**8.00**

Table C4. Difference in pre-privatization trends

Table C4 reports results for a test for difference in trends over the pre-privatization period between successful and withdrawn SIPs. Withdrawal is a dummy which equals one if the transaction is withdrawn, zero otherwise. Year2 is a dummy variable set equal to one if two observation precedes the treatment by two fiscal years. Year1 is a dummy variable set equal to one if the observation precedes the treatment by one fiscal year. Standard errors are clustered at a country level. *, **, *** indicate statistical significance at 10%, 5% and 1%, respectively.

VARIABLES	(1) ROS	(2) Labor Intensity	(3) Operating efficiency	(4) Payout	(5) Capital Expenditures
Withdrawn	0.00969	-1.701***	0.0102**	-0.0550**	0.00626
	(0.0125)	(0.555)	(0.00467)	(0.0241)	(0.00526)
Year2	0.0265**	-0.491*	0.000682*	0.0147	-0.00128
	(0.0100)	(0.300)	(0.00313)	(0.0129)	(0.00152)
Year1	0.0271**	-0.813**	0.000935*	-0.00631	-0.0145***
	(0.0110)	(0.372)	(0.00089)	(0.0185)	(0.00301)
Year1 x Withdrawn	-0.0180	0.505	0.00752	0.0570	0.00448
	(0.0124)	(0.339)	(0.00751)	(0.0391)	(0.00598)
Year2 x Withdrawn	-0.0248	0.817	0.0110	0.0184	0.0103
	(0.0167)	(0.512)	(0.00941)	(0.0527)	(0.00745)
Constant	0.422***	18.74***	0.00984	1.184***	0.228***
	(0.134)	(1.332)	(0.0668)	(0.150)	(0.0269)
Fiscal Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	787	787	787	737	787
Adjusted R-squared	0.098	0.095	0.091	0.107	0.087

Table C5. The decision to withdraw a SIP

Table C5 reports estimates for four in-sample probability models analyzing the determinants of the decision to withdraw a previously filed SIP. The dependent variable is set equal to one for withdrawn SIPs, zero otherwise. All variables are described in Table A.1. All continuous variables are standardized, with mean 0 and standard deviation 1. Model 1 is a linear probability model (LPM) estimated via GLM. Model 2 is a probit model; finally, model 3 and model 4 are probit regressions for which odds ratios are reported. Probit and logit models are estimated via maximum likelihood estimation (MLE). All models include a constant term. Additional controls include capital expenditures, number of employees, accruals, a dummy variable set equal to one if the share issue is an IPO, zero otherwise, and the number of IPOs occurred in the same country-fiscal year in which the event take place. Standard errors are clustered by country. *, **, *** indicate statistical significance at 10%, 5% and 1%, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	GLM	PROBIT	LOGIT	LOGIT
Log Size	-0.0242	-0.0619	-0.113	-0.0531
	(0.0564)	(0.180)	(0.404)	(0.361)
Sales Growth	-0.0105	-0.0431	-0.0620	-0.0879
	(0.0283)	(0.100)	(0.177)	(0.168)
Profitability	-0.0294	-0.101	-0.197	-0.221
	(0.0397)	(0.147)	(0.252)	(0.282)
Labor Intensity	-0.0194	-0.0754	-0.137	-0.143
	(0.0136)	(0.0481)	(0.0914)	(0.0921)
Leverage	0.0248	0.0961	0.173	0.128
	(0.0261)	(0.0917)	(0.181)	(0.133)
Operating Efficiency	0.0654***	0.219***	0.411***	0.407***
	(0.0224)	(0.0618)	(0.120)	(0.132)
GDP per Capita	0.0623*	0.224	0.525	0.621*
	(0.0435)	(0.154)	(0.400)	(0.349)
Two-Year GDP forecast	0.0342	0.137	0.275	0.0682
	(0.0329)	(0.137)	(0.308)	(0.277)
Net Deficit	-0.0325	-0.187	-0.221	-0.334
	(0.0439)	(0.154)	(0.280)	(0.274)
Public Debt	-0.0906**	-0.347**	-0.631**	-0.591**
	(0.0413)	(0.152)	(0.301)	(0.293)
Politi IV Index	0.205***	0.713***	1.206***	
	(0.0639)	(0.244)	(0.437)	
Left-Wing Government	-0.0984	-0.440	-0.680	-0.659
	(0.0856)	(0.355)	(0.663)	(0.647)
Election Year	-0.0221	-0.148	-0.309	-0.183
	(0.106)	(0.402)	(0.743)	(0.731)
Political Competitiveness	-0.0715	-0.167	-0.390	-1.239**
	(0.0618)	(0.281)	(0.552)	(0.500)
1 month market returns	-0.00143	0.00211	-0.0181	-0.0611
	(0.0153)	(0.0552)	(0.104)	(0.144)
Additional Controls	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes
Country	Random	Random	Random	Random
Number of Observations	277	277	277	277
Number of Countries	24	24	24	24

Table C6. Difference in pre-privatization trends -Matched Sample

Table C6 reports results for a test for difference in trends over the pre-privatization period betwee successful and withdrawn SIPs. Withdrawal is a dummy which equals one if the transaction is withdrawn zero otherwise. Year2 is a dummy variable set equal to one if two observation precedes the treatment b two fiscal years. Year1 is a dummy variable set equal to one if the observation precedes the treatment b one fiscal year. Standard errors are bootstrapped and stratified by country in Panel A and in Panel B. Th regressions are run over the three years preceding the successful/withdrawn SIP. All regressions includ country and fiscal year fixed effects. Panel A includes exclusively the 84 SOEs successfully matched vi PSM. Panel B includes exclusively the 43 non-Chinese SOEs successfully matched via PSM. *, **, ** indicate statistical significance at 10%, 5% and 1%, respectively.

Panel A: Matched Sample					
VARIABLES	(1) Return on Sales	(2) Labor Intensity	(3) Operating efficiency	(4) Payout	(5) Capital Expenditures
Withdrawn	0.0297	0.401	0.00899	0.00755	0.0192*
	(0.0268)	(1.020)	(0.0109)	(0.0537)	(0.0111)
Year2	0.0456*	-0.977*	0.0152*	0.0484	-0.0244**
	(0.0236)	(0.767)	(0.00884)	(0.0502)	(0.0122)
Year1	0.0481***	-2.099***	0.0254***	0.0349	-0.0247**
	(0.0175)	(0.813)	(0.00803)	(0.0594)	(0.0118)
Year2 x Withdrawn	-0.0403	0.873	-0.000418	0.0118	-0.0233
	(0.0315)	(1.157)	(0.0166)	(0.0625)	(0.0145)
Year1 x Withdrawn	-0.0421	1.221	0.0104	-0.0298	-0.0182
	(0.0341)	(1.319)	(0.0204)	(0.0732)	(0.0142)
Constant	-0.131	3.132***	-0.0188	0.355*	0.0885**
	(0.104)	(1.032)	(0.0161)	(0.202)	(0.0395)
Fiscal Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	464	464	464	429	464
Adjusted R-squared	0.097	0.036	0.074	0.138	0.028

Panel B: Matched Sample - Excluding Chinese SIPs					
VARIABLES	(1) Return on Sales	(2) Labor Intensity	(3) Operating Efficiency	(4) Payout	(5) Capital Expenditures
Withdrawn	0.0262	-0.149	0.00697	-0.110	0.0149
	(0.0295)	(1.702)	(0.0209)	(0.103)	(0.0143)
Year2	0.103***	-2.365**	0.0281*	0.103	-0.00751
	(0.0337)	(1.102)	(0.0165)	(0.0758)	(0.0168)
Year1	0.0879**	-3.612***	0.0213*	0.0451	-0.0109
	(0.0386)	(1.012)	(0.0245)	(0.0985)	(0.0173)
Year2 x Withdrawn	-0.101*	2.134	-0.0360	0.0329	0.0123
	(0.0445)	(1.803)	(0.0242)	(0.113)	(0.0189)
Year1 x Withdrawn	-0.0555	2.247	-0.00167	0.0250	0.0162
	(0.0510)	(2.095)	(0.0234)	(0.135)	(0.0173)
Constant	-0.127	3.682**	-0.0168	0.472*	0.0544
	(0.105)	(1.710)	(0.0249)	(0.268)	(0.0337)
Fiscal Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	226	226	226	203	226
Adjusted R-squared	0.217	0.181	0.175	0.102	0.110

Table C7. The economic consequences of privatization withdrawals: difference-in-difference estimation

Table C7 reports results for a difference in difference (DiD) model aimed to study the economic consequences of a SIP's withdrawal. Post is a dummy which equals one if the observation occurs after the transaction is completed, 0 otherwise. Withdrawal is set equal to one if the SIP is not successfully completed. The dependent variables are defined in Table A.I. Standard errors are bootstrapped and stratified by country. All regressions are run over the period from three years before the event, to three years after the event, for all observation successfully matched via PSM. In Panel A all models are estimated over the full matched sample. Panel B only includes the 86 matched withdrawn and successful SIPs. *,**,*** indicate statistical significance at 10%, 5% and 1%, respectively.

Panel A: Full Matched Sample					
VARIABLES	(1) ROS	(2) Labor Intensity	(3) Operating Efficiency	(4) Payout	(5) Capital Expenditures
Withdrawal	-0.00972	0.802***	0.0143*	-0.00865**	-0.00639
	(0.0111)	(0.287)	(0.00785)	(0.00362)	(0.00391)
Post	-0.0384**	-0.873*	0.0153***	0.0330***	-0.0221***
	(0.0182)	(0.449)	(0.00565)	(0.00733)	(0.00521)
Withdrawal x post	0.0292	0.305*	-0.0192**	-0.0298***	0.0170**
	(0.0191)	(0.220)	(0.00894)	(0.00751)	(0.00672)
Constant	-0.0914	2.731***	-0.0241*	0.0130***	0.0757**
	(0.104)	(0.363)	(0.0128)	(0.00445)	(0.0332)
Fiscal Year Effects	Yes	Yes	Yes	Yes	Yes
Country Effects	Matched	Matched	Matched	Matched	Matched
Observations	913	913	913	913	913
R-squared	0.127	0.039	0.078	0.148	0.060
Panel B: Matched Sample - Excluding Chinese SIPs					
VARIABLES	(1) ROS	(2) Labor Intensity	(3) Operating Efficiency	(4) Payout	(5) Capital Expenditures
Withdrawal	-0.0303**	0.983**	-0.00346	-0.0319***	0.0180***
	(0.0145)	(0.489)	(0.00911)	(0.00515)	(0.00527)
Post	0.0310*	-1.412**	0.00194	0.0281**	-0.0141**
	(0.0243)	(0.597)	(0.00679)	(0.0124)	(0.00598)
Withdrawal x post	-0.0120	0.368*	-0.0321**	-0.0347***	0.00361
	(0.0280)	(0.769)	(0.0138)	(0.0116)	(0.00729)
Constant	-0.0708	2.550***	-0.00638	0.0362***	0.0513
	(0.118)	(0.547)	(0.0164)	(0.00576)	(0.0425)
Fiscal Year Effects	Yes	Yes	Yes	Yes	Yes
Country Effects	Matched	Matched	Matched	Matched	Matched
Observations	491	491	491	491	491
R-squared	0.240	0.093	0.206	0.256	0.120

Table C8. The economic consequences of privatization withdrawals: 'Change' regressions

Table C8 presents results from a pooled instrumental variable regression. Panel A reports results from the second stage of 2SLS regressions over 5 corporate policies, defined in Table A.I. The dependent variables are measure as percentage change over the 3 years following the event. Withdrawal is a dummy variable set equal to one if the SIP is not successfully completed, 0 otherwise. This variable is instrumented by the fitted probabilities obtained in the probit model estimated in Table V, model 4. A country's degree of political competitiveness and stock of public debt act as instrument for the computed probability of withdrawal. Panel B reports the first stage of the regression. Standard errors are robust to clustering at the country level and all specifications include country and fiscal years fixed effects. Additional controls include log size, leverage, number of employees, GDP per capita, left wing government, election year, and a dummy set equal to one if the attempted SIP is an IPO, zero otherwise. Panel C and Panel D replicate Panel A and Panel B after excluding Chinese SIPs. *, **, *** indicate statistical significance at the 10%, 5% and 1%, respectively.

Panel A: Second Stage

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Change in ROS	Change in Labor Intensity	Change in Operating efficiency	Change in Payout Policy	Change in Capital Expenditures
Withdrawal	0.00946	0.277**	-0.163***	-0.0123*	0.0246***
	(0.0321)	(0.112)	(0.0144)	(0.0087)	(0.00442)
Constant	Yes	Yes	Yes	Yes	Yes
Country and Fiscal Year Effects	Fixed	Fixed	Fixed	Fixed	Fixed
Additional Control	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	218
R-squared	0.575	0.371	0.344	0.853	0.400

Panel B: First Stage

	(1)
VARIABLES	Withdrawn
Propensity to Withdraw	1.016***
	(0.160)
Constant	Yes
Country and Fiscal Year Effects	Fixed
Additional Control	Yes
Observations	218
R-squared	0.508

Panel C: Second Stage – Excluding Chinese SIPs					
VARIABLES	(1) Change in ROS	(2) Change in Labor Intensity	(3) Change in Operating efficiency	(4) Change in Payout Policy	(5) Change in Capital Expenditures
Withdrawal	-0.0013	0.243**	-0.131***	-0.0152***	0.0032
	(0.047)	(0.181)	(0.0113)	(0.001)	(0.0132)
Constant	Yes	Yes	Yes	Yes	Yes
Country and Fiscal Year Effects	Fixed	Fixed	Fixed	Fixed	Fixed
Additional Control	Yes	Yes	Yes	Yes	Yes
Observations	78	78	78	78	78
R-squared	0.583	0.392	0.326	0.890	0.378

Panel D: First Stage – Excluding Chinese SIPs	
VARIABLES	(1) Withdrawn
Propensity to Withdraw	1.203***
	(0.163)
Constant	Yes
Country and Fiscal Year Effects	Fixed
Additional Control	Yes
Observations	78
R-squared	0.765

Table C9. The economic consequences of privatization withdrawals: 'Level' regressions

Table C9 presents results from a pooled instrumental variable regression. Panel A reports results from the second stage of 2SLS regressions over 5 corporate policies, defined in Table A.I. The dependent variables are measure as the averages over the three years following the event. Withdrawal is a dummy variable set equal to one if the SIP is not successfully completed, 0 otherwise. This variable is instrumented by the fitted probabilities obtained in the probit model estimated in Table V, model 4. A country's degree of political competitiveness and stock of public debt act as instrument for the computed probability of withdrawal. Panel B reports the first stage of the regression. Standard errors are robust to clustering at the country level and all specifications include country and fiscal years fixed effects. Additional controls include log size, leverage, number of employees, GDP per capita, left wing government, election year, and a dummy set equal to one if the attempted SIP is an IPO, zero otherwise. Furthermore, all regressions include the average value of the dependent variable over the three years preceding the event as an additional control. Panel C and Panel D replicate Panel A and Panel B after excluding Chinese SIPs. *, **, *** indicate statistical significance at the 10%, 5% and 1%, respectively.

Panel A: Second Stage

VARIABLES	(1) ROS	(2) Labor Intensity	(3) Operating efficiency	(4) Payout	(5) Capital Expenditures
Withdrawal	-0.0283	0.547***	-0.0220***	-0.00319*	0.00392*
	(0.0172)	(0.150)	(0.00509)	(0.00414)	(0.00552)
Constant	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Country Effects	Yes	Yes	Yes	Yes	Yes
Fiscal Year Effects	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	218
R-squared	0.678	0.830	0.661	0.894	0.584

Panel B: First Stage

VARIABLES	(1) ROS	(2) Labor Intensity	(3) Operating efficiency	(4) Payout	(5) Capital Expenditures
	Withdrawal				
Propensity to Withdraw	1.023***	0.968***	0.955***	0.999***	1.007***
	(0.161)	(0.158)	(0.171)	(0.162)	(0.162)
Constant	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Country Effects	Yes	Yes	Yes	Yes	Yes
Fiscal Year Effects	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	218
R-squared	0.678	0.830	0.661	0.894	0.584

Panel C: Second Stage – Excluding Chinese SIPs					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	ROS	Labor Intensity	Operating efficiency	Payout Policy	Capital Expenditures
Withdrawal	-0.0313	0.632*	-0.0243***	-0.00342	0.00201
	(0.0172)	(0.350)	(0.00509)	(0.0414)	(0.00552)
Constant	Yes	Yes	Yes	Yes	Yes
Country and Fiscal Year Effects	Fixed	Fixed	Fixed	Fixed	Fixed
Additional Control	Yes	Yes	Yes	Yes	Yes
Observations	78	78	78	78	78
R-squared	0.685	0.812	0.678	0.832	0.613
Panel D: First Stage – Excluding Chinese SIPs					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	ROS	Labor Intensity	Operating efficiency	Payout	Capital Expenditures
	Withdrawal				
Propensity to Withdraw	1.017***	0.973***	0.961***	0.973***	1.012***
	(0.163)	(0.153)	(0.174)	(0.163)	(0.160)
Constant	Yes	Yes	Yes	Yes	Yes
Country and Fiscal Year Effects	Fixed	Fixed	Fixed	Fixed	Fixed
Additional Control	Yes	Yes	Yes	Yes	Yes
Observations	78	78	78	78	78
R-squared	0.713	0.822	0.632	0.914	0.601

Figures

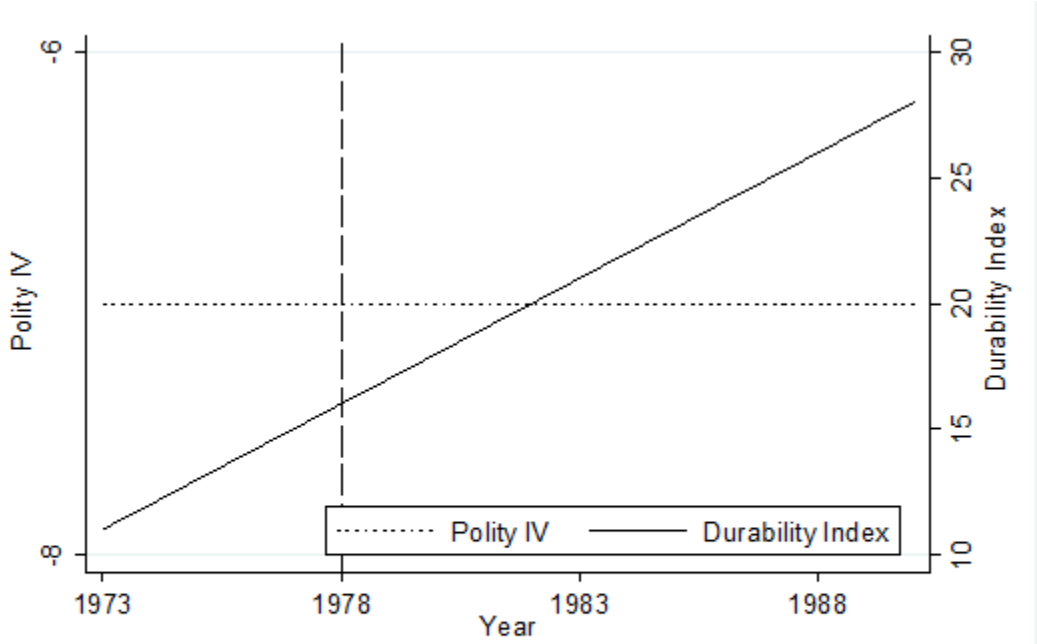


Figure A1. Political Stability in Mauritania between 1973 and 1990. The left, verti l axis reports the Politi IV index, whose time series is represented by the dotted line. T e right, vertical axis reports Mauritania's durability index, whose time series is represent d by the solid line. The vertical line for 1978 divides the pre- and post- coup d'etat perio

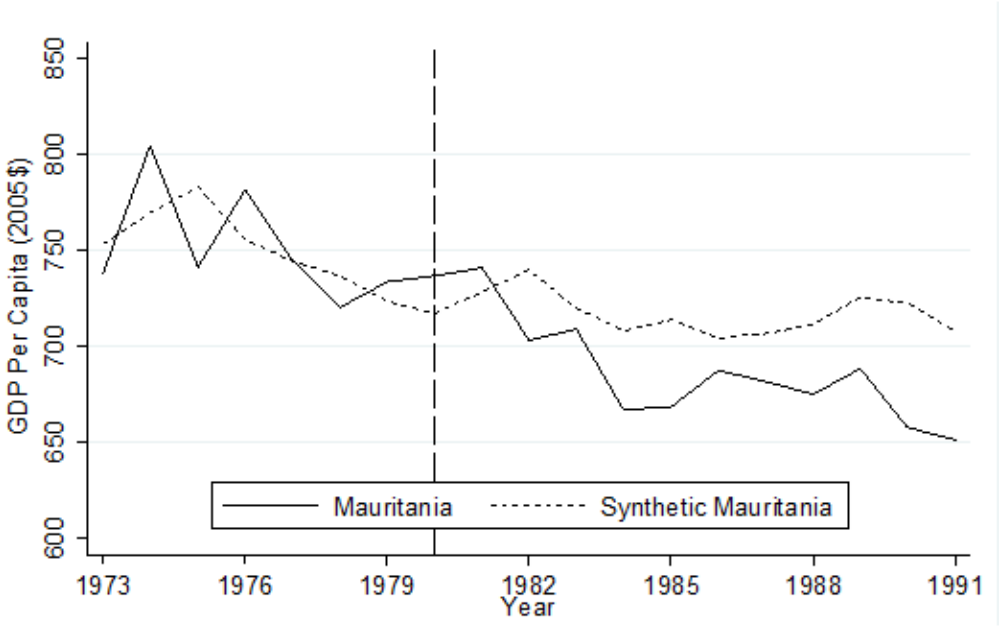


Figure A2. Income per Capita: Real and Synthetic Mauritania. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Mauritania. The dotted line represents the constructed syntehtic control. The vertical line is set on the year 1980 and it divides the pre- and post-treatment period. The chart is relative the period 1973-1991.

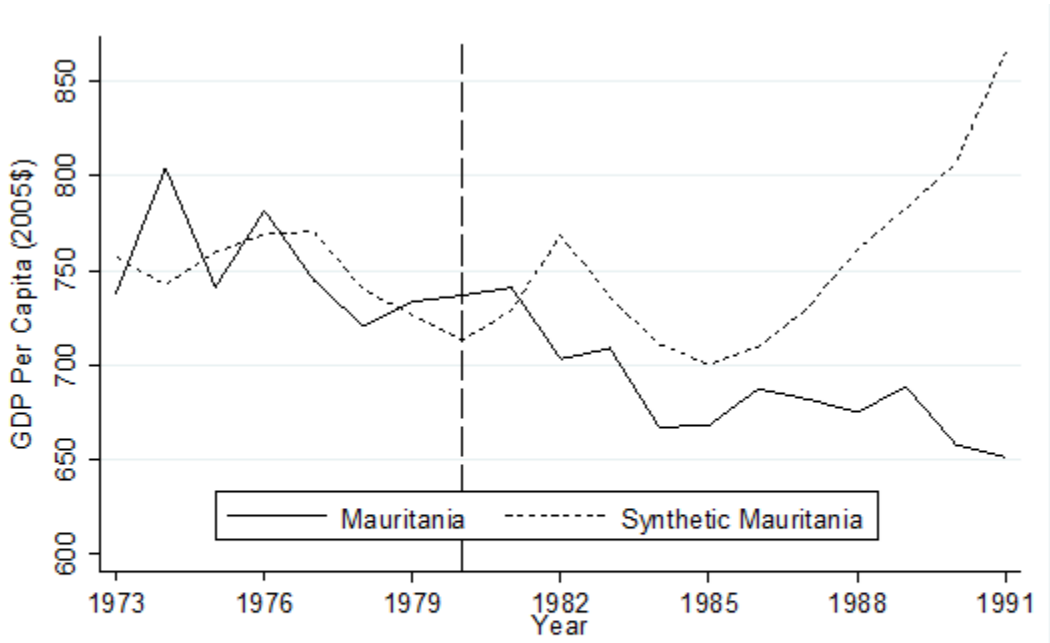


Figure A3. Robustness test: different predictors. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Mauritania. The dotted line represents the constructed syntehtic control, as built on a different set of predictors. The vertical line is set on the year 1980 and it divides the pre- and post-treatment period. The chart is relative the period 1973-1991.

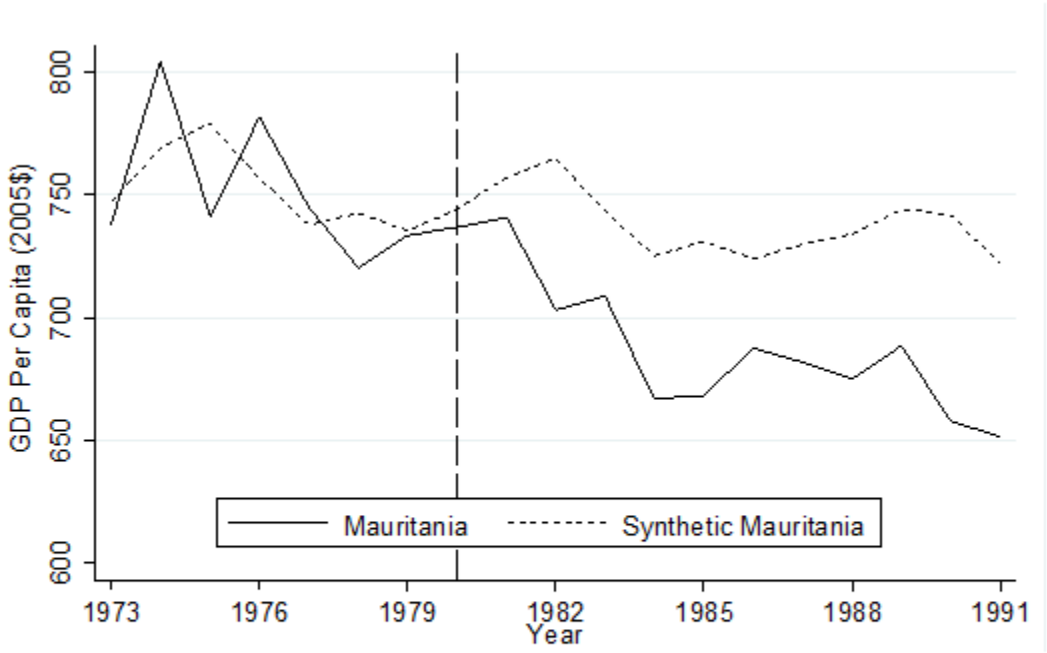


Figure A4. Robustness test: restricted donor group. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Mauritania. The dotted line represents the constructed syntehtic control built after a conservative Jackknife test. The vertical line is set on the year 1980 and it divides the pre- and post-treatment period. The chart is relative the period 1973-1991.

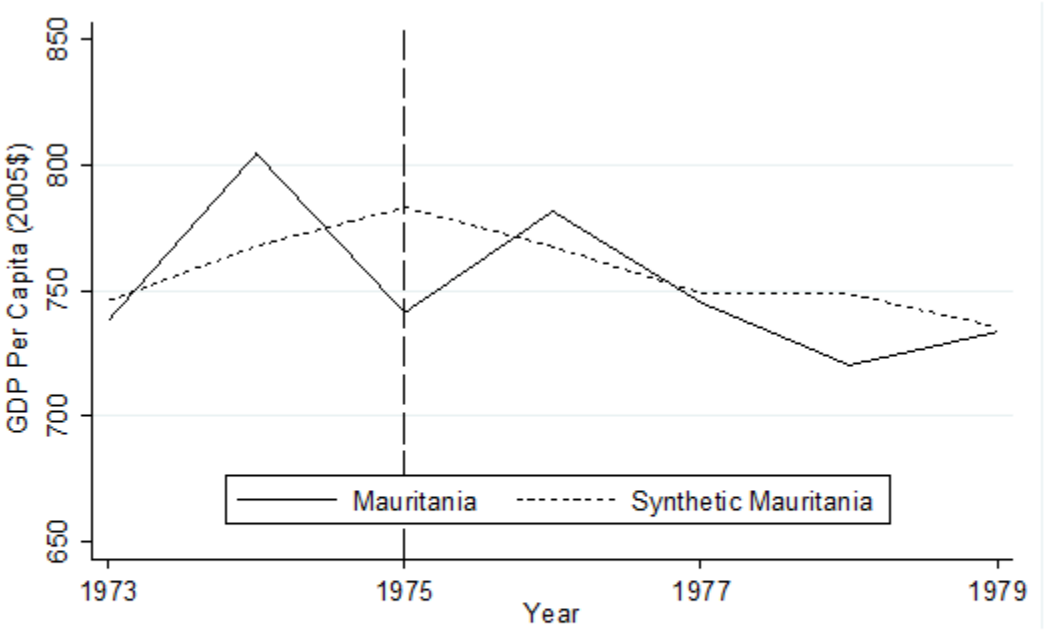


Figure A5. Robustness test: Move the treatment – Short Window. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Mauritania. The dotted line represents the constructed syntehtic control built for the "short term" placebo test. The vertical line is set on the year 1975, to which the false treatment is assigned. The chart is relative the period 1973-1979.

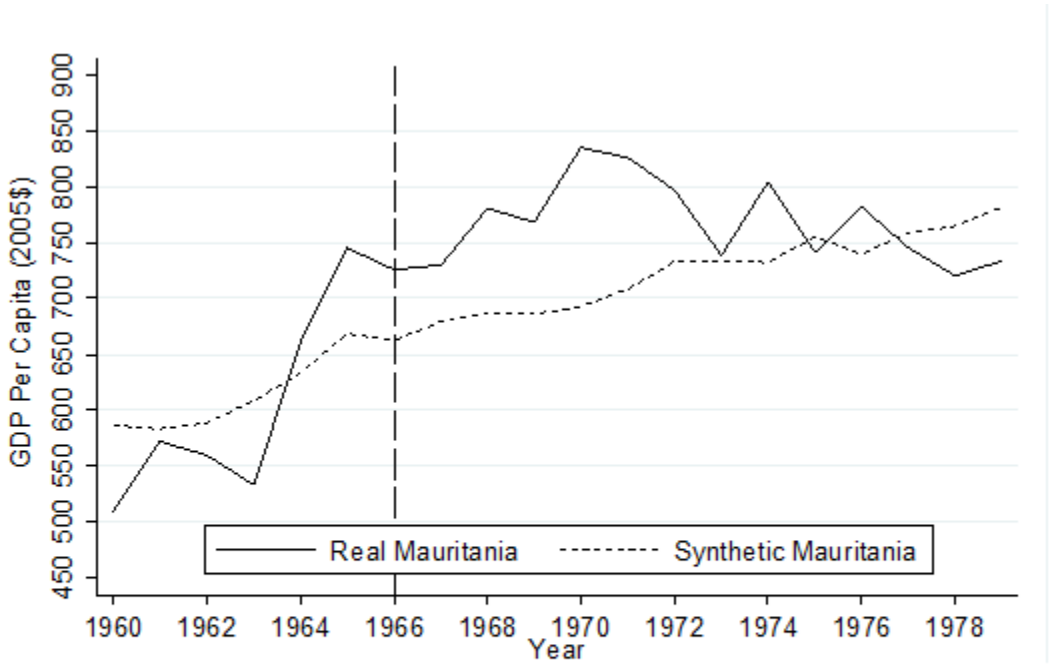


Figure A6. Robustness test: Move the treatment – Long Window. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Mauritania. The dotted line represents the constructed syntehtic control, as constructured for the "long term" placibo test. The vertical line is set on the year 1966, to which the false treatment is assigned. The chart is relative the period 1960-1979.

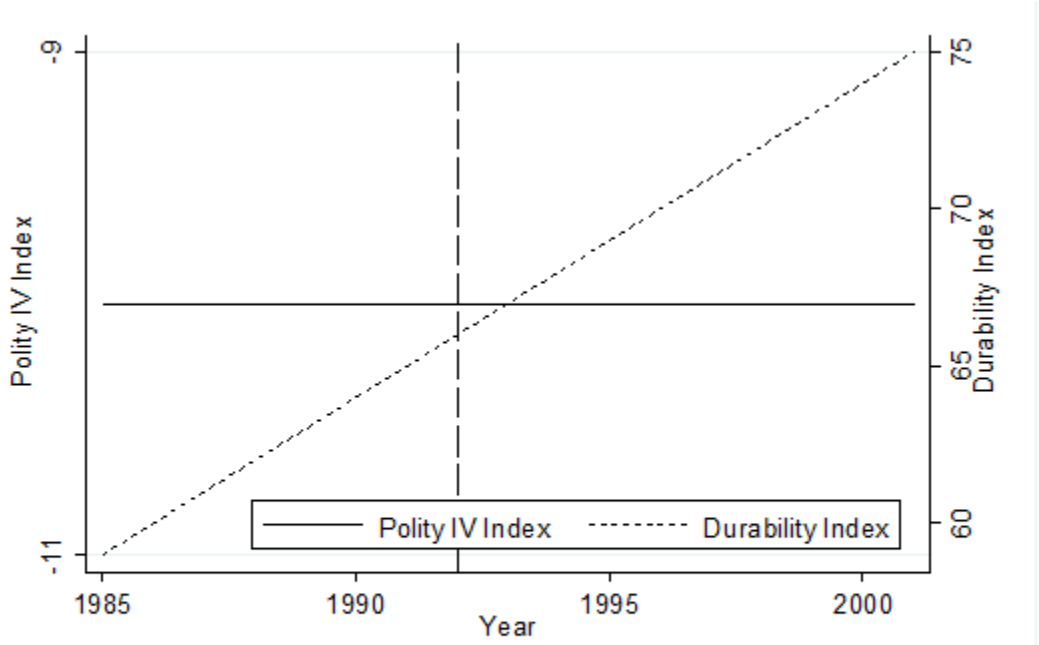


Figure A7. Political Stability in Saudi Arabia between 1985 and 2001. The left, vertical axis reports the Politi IV index, whose time series is represented by the dotted line. The right, vertical axis reports Saudi Arabia's durability index, whose time series is represented by the solid line. The vertical line for 1992 divides the pre- and post-treatment period.

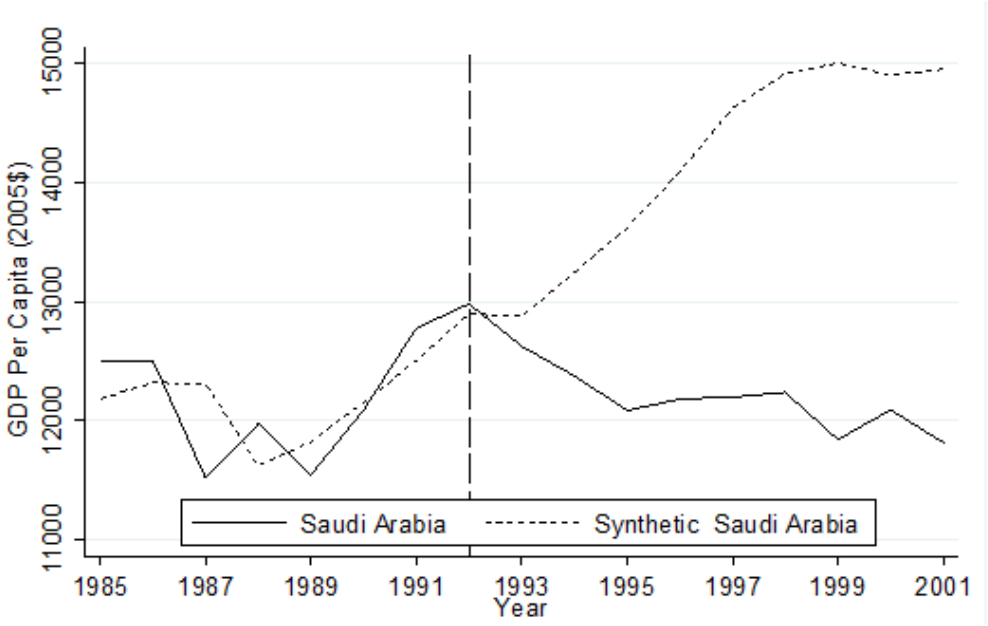


Figure A8. Income per Capita: Real and Synthetic Saudi Arabia. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Saudi Arabia. The dotted line represents the constructed syntehtic control. The vertical line is set on the year 1992 and it divides the pre- and post-treatment period. The chart is relative the period 1985-2001.

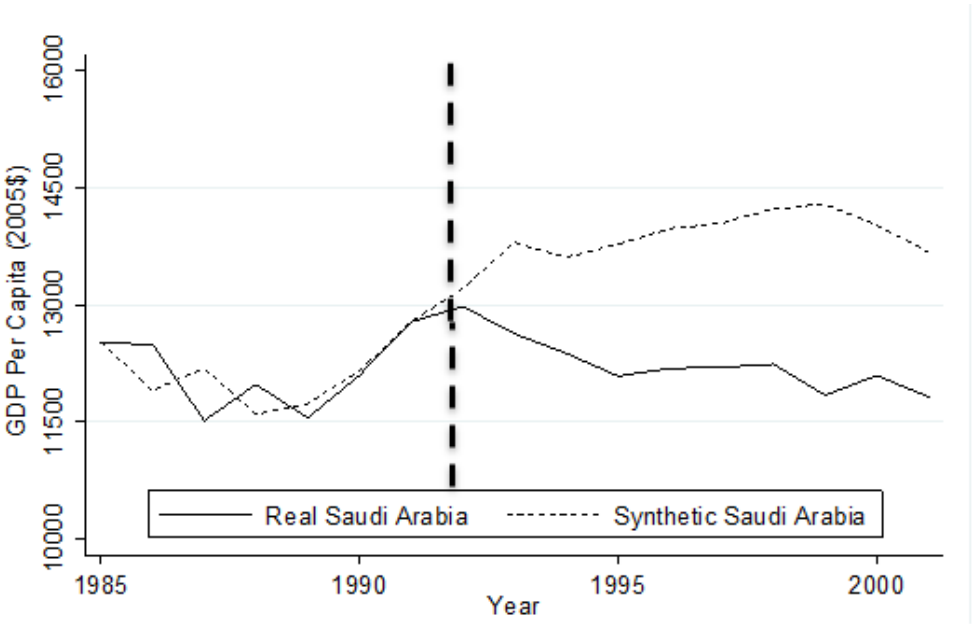


Figure A9. Robustness test: Interpolation bias test. The left, vertical axis reports GDP per Capita level, measured in 2005 Dollar. The solid line represents GDP per Capita level for "real" Saudi Arabia. The dotted line represents the constructed syntehtic control, as built after including Bahrain, Indonesia, Jordan, and the UAE in the donor pool. The vertical line is set on the year 1992 and it divides the pre- and post-treatment period. The chart is relative the period 1985-2001.

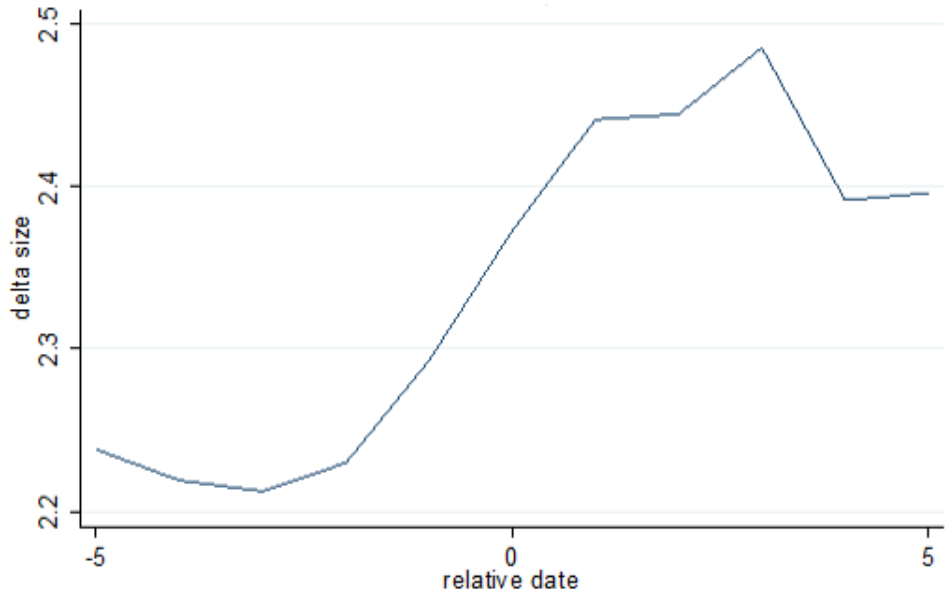


Figure B1. Difference in Size between acquirers and inactive firms. The left, vertical axis reports difference in Size between acquiring firms and firms that are not active in the M&A markets over the 10 years surrounding a successfully completed deal.

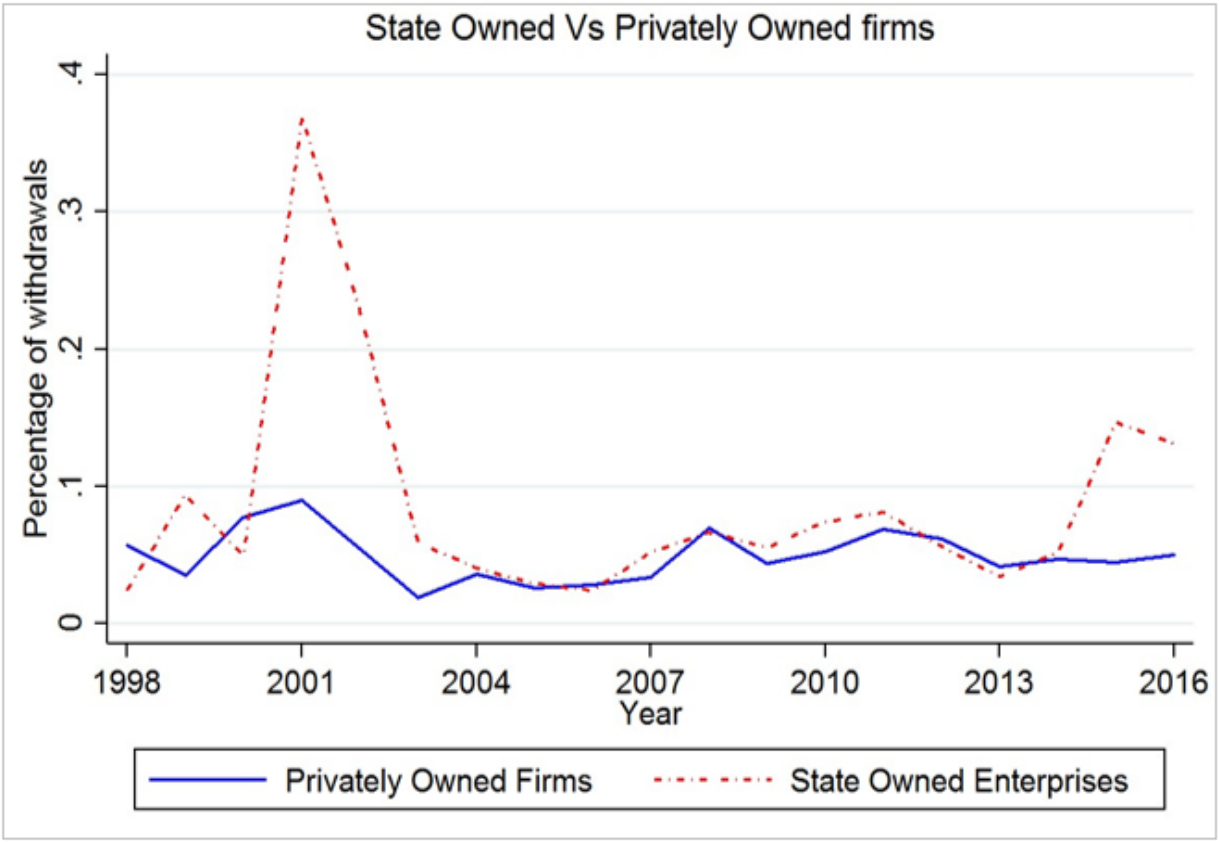


Figure C1: Incidence of share issues' withdrawals over the period 1998-2016. Source: SDC Platinum and Datastream.

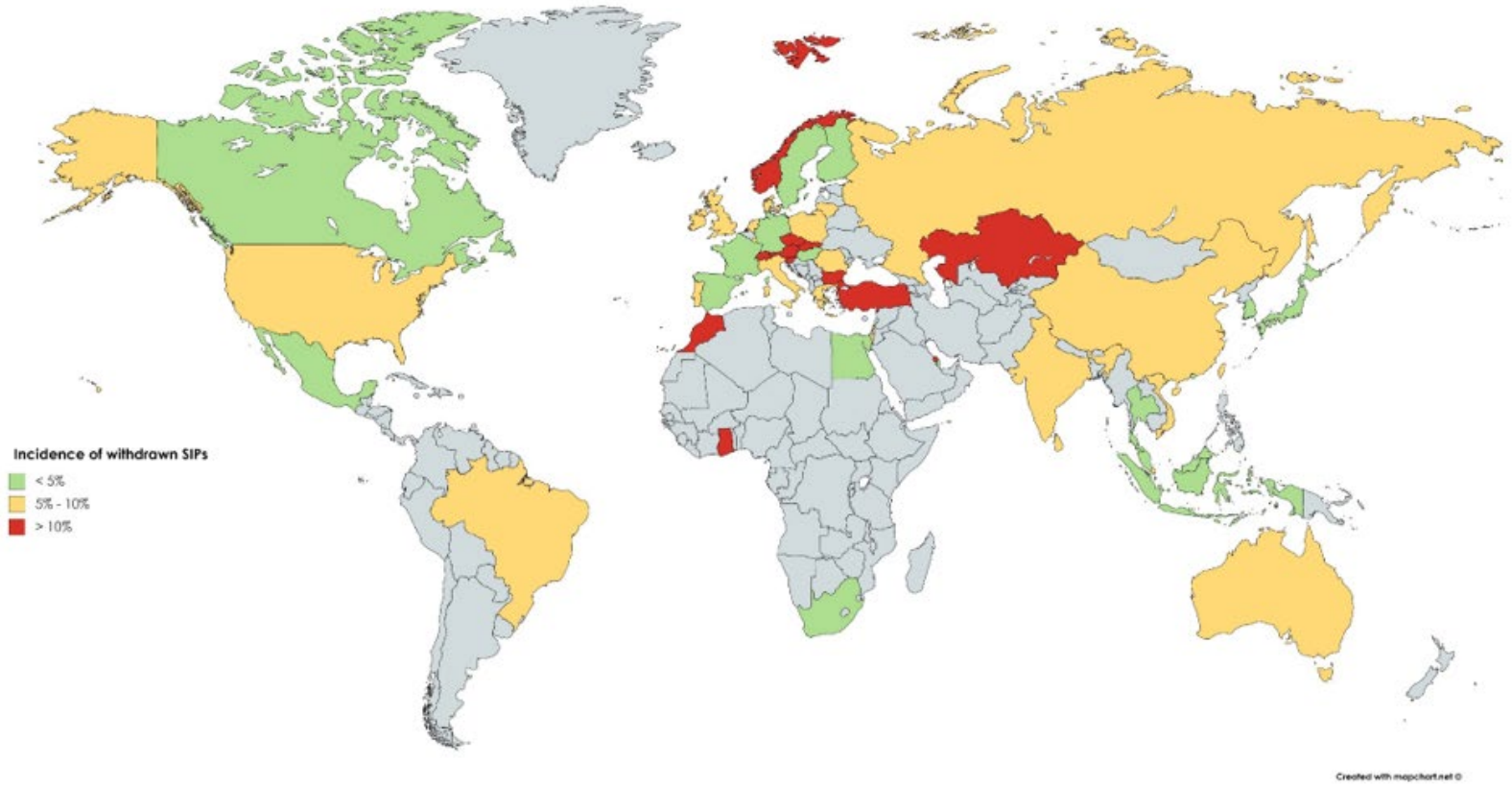


Figure C2: Incidence of withdrawn SIPs. Source: SDC Platinum & Datastream.