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Distributional Policy Effects with Many Treatment Outcomes*

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Abstract: Different segments of a population affected by the same policy intervention may have different responses. We study the role of equilibrium effects on explaining these differences. Our case study is the government's extension of guarantees during the Great Recession to certain debt issuers. We extend Athey and Imbens [2006] to a scenario of multiple outcome variables, and identify the counterfactual joint distribution. We find the intervention increased the funding for the treated segments, but at the cost of higher spreads. Finally, these equilibrium effects operate dissimilarly along the segments of the treated group, in the extreme, can produce undesired effects.
Keywords: Interventions, Stigma, Identification, Nonlinear Difference-In-Difference, Copulas
JEL Classification: G01, G23, G28, C24, C4

Resumen: Distintos segmentos de una población afectada por una misma intervención de política puede contar con distintas respuestas. Nosotros estudiamos el rol de los efectos de equilibrio en explicar estas diferencias. Nuestro caso de estudio es una extensión de garantías de parte del gobierno, durante la Gran Recesión, a ciertos emisores de deuda. Extendemos Athey e Imbens [2006] a un escenario de múltiples variables de desempeño e identificamos la distribución conjunta contrafactual. Encontramos que la intervención incrementó el fondeo recibido por el segmento de los tratados, pero con el costo de unos mayores spreads. Finalmente, estos efectos de equilibrio operan de forma disímil a lo largo del grupo de tratados, y en casos extremos, puede producir efectos indeseados.
Palabras Clave: Intervenciones, Estigma, Identificación, Diferencias en Diferencias no lineales, Copulas

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1 Introduction

Why different segments of a class of financial institutions, subject to a common shock (e.g. the Great Recession), can respond differently to the same policy intervention? Even if policy makers identify the class of institutions that the intervention should target, the response could vary across segments due to observed and unobserved factors. In this paper we focus on the role of equilibrium effects to explain such differences. By studying the impact of an intervention on several outcome variables, all jointly determined in equilibrium, we implement a novel econometric technique to quantify the differences across segments of the treated group.

The experience of Mexico during the Great Recession is of special interest because only a class of financial institutions received direct aid, in the form of guarantees on all issued debt securities, from the government[1], and because the net exposure of the rest of the financial system to them was low. Although the intervention was clearly targeted, we observe differences in the impact on the volume of funding and spreads across the treated group. In this paper we show that these differences can be explained by the equilibrium relationship that the volume of funding and the spreads have.

How should we assess the effects of the government guarantees on the market where these institutions offer the debt securities? First, the policy simultaneously affects several outcome variables that are jointly determined in equilibrium. We argue that the total impact of an intervention over each output variable can be divided between the direct impact on each variable, and the indirect impact they receive through the other output variables. The relevance of this distinction, direct versus indirect, relates to the ability of policy makers to design more efficient interventions in the future.

Second, it is important to quantify the indirect effects. For example, if the objective of

[1] The aid was a 65% guarantee on all short and long-term debt securities issued by the Sofomes and Sofoles from the mortgage sector, and it was administered by *Sociedad Hipotecaria Federal*. Later in the paper we explain in detail these institutions, their role at the industry, and how were they affected by the financial crisis.

a policy is to increase the volume of funding at a lending market, the policy maker would want to design the intervention such that simultaneously the funding volume increases and the spreads at the market do not spike too much. From Philippon and Skreta [2012] we know interventions may entail a stigma.[2] In our example the rise on the spreads captures the stigma. To assess the aggregate impact from the intervention, we need to calculate the direct impact plus the effect on the volume conditional on the spread, and viceversa.

Within the financial institutions that benefited from the guarantees on all debt securities, we find segments that undoubtedly needed the aid, but others that were in a better shape. In consequence, the total effect of the intervention, which we divide between direct and indirect, could differ within these institutions. On the one hand, the direct effect of the intervention on the volume of funding and on the spreads might be homogeneous across the treated group. That is, the volume could increase, and depending on how strong the stigma is, the spreads could either increase or decrease. On the other hand, with the indirect effect, the impact the volume of funding received through the spreads, and viceversa, might be more heterogeneous. The indirect effects could amplify or dampen the direct effect according to how intense the government aid was needed. All these questions will be addressed in the paper.

Several papers have evaluated the consequences of the latest Great Recession, and/or the impact of a policy on a particular market. For example, Chwieroth [2013] addresses the issue of the stigma on the treated group; Adrian et al. [2012] analyzes the effects of the Great Recession in the composition between granted credit loans and bond financing, Berger et al. [2013] focuses on the effects of a government intervention during that same period, and Cassola et al. [2013] studies a short-term funding market during the crisis. Still, we need to improve our understanding about how an intervention affects different variables jointly determined in equilibrium, and how these effects are distributed over the population.

[2] The intervention of a policy maker on a specific population imposes a stain relative to the members of that did not receive the aid.

In this paper, we propose a method that allows practitioners to assess the intervention on multiple output variables, and on different segments of the targeted population. Our case study is the intervention of the Mexican government on the Sofomes and Sofoles from the mortgage sector, a group of financial institutions that were heavily affected during the Great Recession. The objective of the government was to restore the functioning of the market in which these institutions fund their activities. We jointly evaluate the impact of the intervention, in the form of guarantees, over several output variables, e.g. volume and spread, and find qualitative differences on the indirect effects for distinct segments of the treated group.

Our methodology has two objectives. First, to identify, and then estimate, the counterfactual joint distribution of all outcome variables. To achieve this we obtain a novel identification result. And second, to quantify the indirect effects on a variable-per-variable basis, and ultimately calculate the distributional effects of the policy conditional on other outcome variables. With the latter, we can understand how the variables' equilibrium relationships shape the policy's effects.

The technical value added of the paper is twofold. First, we propose and implement a novel econometric technique, an extension of Athey and Imbens [2006] Changes-in-Changes model,[3] that allows us to identify and estimate the counterfactual joint distribution of all output variables. That is, we can identify the joint distribution of all output variables for the treated group in the hypothetical situation where no government guarantee was offered. Second, that same technique allows us to disentangle the total effect of the intervention into the direct and the indirect effects. Our method is entirely data driven.

The new methodology helps policymakers to understand better the following questions: *Are there fundamental differences in the way output variables affect each other across the population? The indirect effects of an intervention significantly influence the stigma? Why the participation of financial institutions on the provision of funding is desirable?*

[3] The extension to Bonhomme and Sauder [2011] remains an interesting topic for future research.

The indirect effects for the least attractive borrowers present qualitative differences as compared to those of the most attractive borrowers?

The paper has several contributions for the literature studying government interventions during the Great Recession. First, our estimates suggest that the intervention helped Sofomes and Sofoles to secure their funding, but the "cost", in the form of higher spreads, of receiving the aid outweighs the potential spread reduction due to the guarantees. The higher spread, which we interpret as a stigma, specially affected Sofomes and Sofoles with high volume of funding and low spreads. In other words, the highest spike on the spreads was suffered by the safest Sofomes and Sofoles. Also, we find that the intervention increased the share of debt held by financial institutions, but did not affect the share held by Mexican non-financial institutions. This may points towards the idea that sophisticated agents have a strategic advantage over other agents.

The second set of conclusions involves disentangling the total effect of the intervention into the direct and indirect effects. First, we find indirect effects are clearly non negligible, and mostly statistically significant. From this result follows that the estimates one obtains using standard Diff-In-Diff methods do not necessarily reflect the direct effect of the intervention, even if properly choosing the treated and control groups. Second, the indirect effects associated to different output variables are very informative. For example, while the volume of funding has important indirect effects, those effects associated to the spread are very low. In other words, the equilibrium effects are important for the former, and small for the latter. Finally, we find that indirect effects on most attractive borrowers amplify direct effects, but for the least attractive borrowers operate in the opposite direction. For example, for debt securities with high spreads and low volume, while the direct effect increases the volume and the spread, the indirect effect on the volume (spread) decreases (increases) the direct effect.

Related Literature. Our paper is related to the literature on identification of distributional treatment effects. Using the IV-LATE model from Imbens and Angrist [1994], in

Abadie [2002] and Abadie [2003] we can find one of the first identification results for distributional treatment effect for the compliers. Later, Firpo and Pinto [2011] and Firpo [2007] show that using the unconfoundedness assumption (see Rosenbaum and Rubin [1983] and Rosenbaum and Rubin [1984]) is also sufficient to identify the distributional treatment effect, a similar result is achieved by Chen et al. [2008]. The main difference of our paper is that we study identification assumptions on the joint counterfactual distribution output variables and not on their marginals.

Athey and Imbens [2006] showed it is possible to identify the counterfactual marginal distribution of outcome variables if one assumes that, within the group of treated or untreated, the treatment does not affect the distribution of unobservables. One important contribution of their paper is that practitioners do not need to worry if the *common trend assumption* is satisfied in either logs or levels. In other words, with the standard Difference-In-Difference (DID) model, if the *common trend assumption* is satisfied in levels, then it is not satisfied in logs, and viceversa. With the Changes-In-Changes (CIC) model, satisfying the *common trend assumption* in logs or in levels is no longer a problem.

Bonhomme and Sauder [2011] [4] build on Athey and Imbens [2006] to show it's possible to identify the distributional treatment effect, even when the treatment affects the distribution of unobservables, if we assume a parametric form for a production function that relates outcome variables and unobservables in absence of the treatment. The chosen parametric form though alleviates an important shortcoming of Athey and Imbens [2006], focusing only on the short run effect of a treatment, also limits the cases where this methodology can be used. This paper does not explore the identification of the counterfactual joint distribution of output variables affected by the treatment.

The model we propose uses the Changes-In-Changes model, and extends it to a multivariate setting by proposing additional identifying assumptions. In particular, we assume that, within each group the treatment does not affect the entire distribution

[4] Thuysbaert [2007], in a unpublished working paper, also addresses a way to move one step ahead of Athey and Imbens [2006].

of unobservables, which explains the output heterogeneity in absence of the treatment. Additionally, we propose a weaker identifying assumption. Namely, that within each group the treatment does not affect a particular characteristic of the distribution of unobservables, namely the diagonal section of their copula. We show that for a widely used family of copulas both identifying assumptions are identical. To our knowledge this is the first paper that studies the identification of the counterfactual joint distribution of outcome variables in the absence of the treatment.

This paper also contributes to three strands of empirical literature. The first strand, that studies the stigma created by policy interventions, is represented by Ennis and Weinberg [2009], Chwieroth [2013], and Armantier et al. [2011]. Our paper is related to them because we also discuss the stigma created by the intervention during the Great Recession, but is different in many aspects. One difference is that we identify and quantify the contribution of other output variables to the stigma without making any modelling assumption. Another difference is that we study how different segments of the treated group respond to the intervention. In addition, the data we use has a well defined and direct measure of the stigma, i.e. the spreads of the short-term debt securities issued by the Sofomes and Sofoles.

The second strand of literature, which quantifies the impact of events during the Great Recession at the firm or the bank level, is represented by Campello et al. [2010] and Adrian et al. [2012], that studies the effects of the Great Recession, as well as Kahle and Stulz [2010] and Berger et al. [2013], that study the effects of policy interventions during that period. Our paper is related to both groups of studies because we make a comprehensive analysis of Sofomes and Sofoles during the crisis, and because we quantify the effect of the policy intervention. The main difference relies again on quantifying the multiple indirect effects of output variables, which up to now were not properly addressed.

Finally, the last strand of literature, which studies the role of the government during a financial crisis over a market that suffers significant adverse selection problems, is represented by the theoretical papers of Philippon and Skreta [2012], and Tirole [2012], and the empirical paper of Cassola et al. [2013]. Our paper is related to all of them because we focus on a market severely affected during the Great Recession where Sofomes and Sofoles obtained their funding. Our contribution to the theoretical papers is that our formulation allows to measure the policy's impact on several dimensions, confirms some of their predictions, and identifies promising research areas. As Cassola et al. [2013], we study a short-term funding market during the financial crisis, and some of our results have the same flavor in the sense we also find heterogeneous effects along population segments. The main difference, is that our estimation technique is model-free, and allows us to simultaneously analyze several dimensions of the short-term funding market.

The remainder of the paper is structured as follows. In Section 2 we discuss some preliminary aspects of the industry, as well as how the intervention took place. In Section 3 we discuss the estimation method. Later, in Section 4 we present the data. Section 5 discusses the results. In Section 6 we make some final remarks. The last section concludes.

2 Preliminaries

Mexican policy makers have closely followed Sofomes and Sofoles (from hereon Sfm/Sfl) for several reasons. First, though their participation on the financial industry has declined, households and non-financial firms can use them to fund their activities. Second, Sfm/Sfl's delinquency rate, specially on those specialized on the mortgage sector, remained high throughout the observed period. And third, as acquiring information about them is costly, other financial institutions have incentives to exploit regulatory-based arbitrage opportunities.[5]

In this section we start by briefly describing the role of Sfm/Sfls at the financial

[5] Sofomes are divided in two, one small but important group of regulated entities, and another quite numerous that is not regulated. The second group of Sofomes are not compelled to provide the Mexican regulator with any information and hold a small fraction of the industry assets.

industry, then we continue explaining how they where affected by the Great Recession, and we conclude explaining how the government intervention was conceived and implemented.

2.1 The Industry

Sofomes and Sofoles were the Mexican financial institutions that suffered the most during 2007-09 financial crisis. Other important firms from the financial industry, among them the traditional commercial banks, did not suffer a major contagion because their net exposures were low. This lack of industry wide effects let us argue, without loss of generality, that any general equilibrium effects of the intervention do not represent a threat to our results.

Sfm/Sfls have several features. First, using Figure 1, that presents the evolution of their participation at the financial industry, we observe they represented less than 1% as of 2007, then they reached the peak by the end of 2009 with 3.5%, since then it declined and stabilized below 2%. Second, they fund particular sectors of the economy, namely, the mortgage, the non-mortgage private consumption, and the non-financial firms sectors. Finally, the main difference on their business model vis-a-vis commercial banks is that they do not receive cash deposits to fund their activities, instead they issue debt of different maturities among other financing instruments at the capital markets, e.g. mortgage Sfm/Sfls issue mortgage-backed securities (MBS).

The crisis affected Sfm/Sfls in two aspects, namely, their attractiveness vis-a-vis other players of the financial sector, and their business model. Their participation in the total assets of the industry decreased, and has not recovered from its pre-crisis level, see Figure 1. Their business model was also put into question. Mortgage Sfm/Sfls had to deal with a severe reduction of the demand of MBS (see Figure 2), and the financial Sfm/Sfls (that mainly serve non-financial firms) proved to be vulnerable to increases of the systemic risk[6] (see in Figure 3 the behavior of the spreads). In the following paragraphs we elaborate on these points.

[6] The systemic risk is a measure of the fragility of the financial system.

For mortgage Sfm/Sfls the access to capital markets became very expensive due to an increase in the risk aversion of market participants, as well as from a sharp increase on delinquency rates. By the end of 2008 the quality of the mortgages granted by these institutions decreased to the point that the demand for these securities disappeared. In addition, the crisis directly affected Sfm/Sfls balance sheets. They had to keep on their balance sheet the granted mortgages because the demand for MBS shrinked.[7] Overall, Sfm/Sfls quickly faced a solvency crisis.

The crisis affected the other Sfm/Sfls as well, in particular the financial Sfm/Sfls. These institutions also obtained their funding through commercial bank loans, and by issuing short and long-term debt securities at the capital market. They had to bear higher spreads, as we showed, because investors turned more risk averse as they did not know if the crisis could escalate. In addition, financial Sfm/Sfls were especially vulnerable in case non-financial firms suffered a credit crunch. Bottom line, by the beginning of 2009 financial Sfm/Sfls were close to facing a solvency crisis.

Mortgage and financial Sfm/Sfls had similarities at the dawn of the crisis. Although financial Sfm/Sfls did not use MBS as a source of funding, and mortgage Sfm/Sfls were not as exposed to a credit crunch on non-financial firms, both types of institutions were vulnerable to increases of the systemic risk. We posit in this paper that both institutions were likely to receive the same policy intervention. Later in the paper we construct a set of covariates to control for any remaining differences, and we show our results are robust to them.

From Figure 2 we observe that after October 2008 the share on total assets declined, then it increased during 2009 (probably due to the government aid), and then started to

[7] The decline for the MBS's demand was driven by a conjunction of two factors. First, the sub-prime crisis at the United States turned investors more risk averse. The crisis taught the market that investment vehicles initially conceived as safe could end up being the opposite. Mexican investors turned cautious and downsized their MBS demand. Second, starting 2008 the delinquency rate for Sfm/Sfls increased, specially for those at the mortgage sector. Consequently, after Lehman's bankruptcy Mexican investors understood the growth in the delinquency rate as a symptom of a sub-prime style crisis.

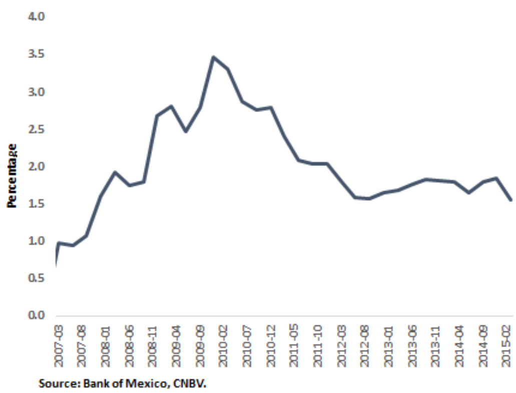

Figure 1: % on Financial Sector

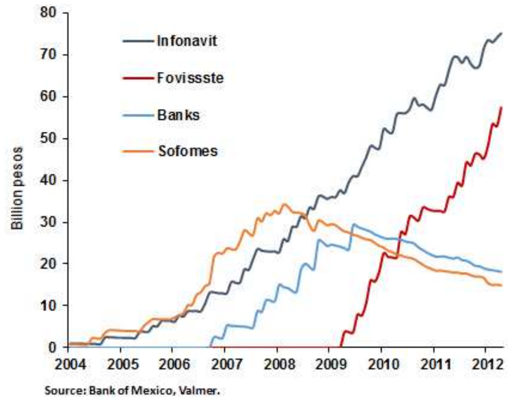

Figure 2: Mortgage-backed sec.

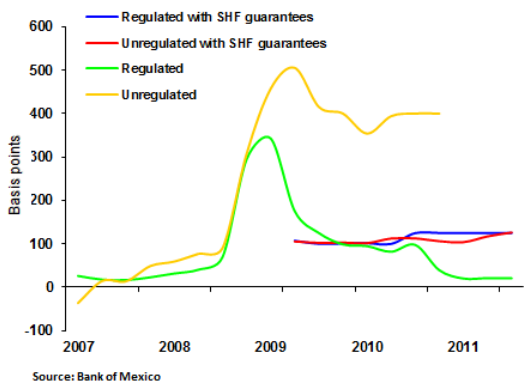

Figure 3: Spread ST debt - TIIE

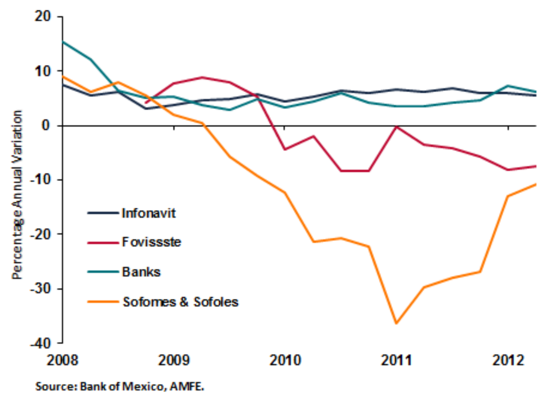

Figure 4: Mortgage Loans

decline until 2013. Figures 2 - 3 collect information from the mortgage Sfm/Sfls and show that effects of the crisis started during the last quarter of 2008. From Figure 2 we observe that these institutions had serious difficulties to fund through MBS after the crisis, as compared to their historical trend and to the trend of other financial institutions. We also observe, from Figure 3, that the spread on their debt remarkably spiked after the crisis. As a consequence, see Figure 4, the participation of mortgage Sfm/Sfl on all the mortgage loans supplied by the financial industry dropped on a yearly basis.

Mortgage Sfm/Sfls represent a significant fraction of the Sfm/Sfls sector, and also were the most severely affected by the crisis. Indeed, by 2011 they held 34.5% of all Sfm/Sfl's

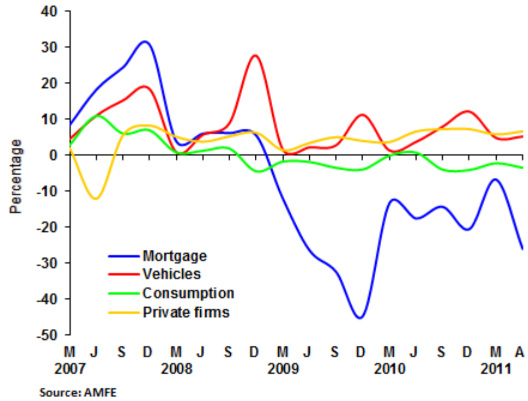

Figure 5: Return on equity for different Sfm/Sfls

assets, by 2012 this Figure slightly reduced to 32%.[8]

From Figure 5 we observe that indeed mortgage and financial Sfm/Sfls, e.g. the former in blue and the latter in green, suffered a heavy toll during the crisis.

2.2 The Intervention

The Great Recession significantly affected the market where Sfm/Sfls obtained their funding, specially for those specialized on the mortgage sector. The rest of the financial industry had a low net exposure to them, and thus suffered no contagion. We argue that the genesis of the intervention were the difficulties that Sfm/Sfls faced to obtain their funding, and the spike in the rates they had to pay.

By the beginning of 2009, mortgage Sfm/Sfls found themselves without demand for mortgage-backed securities, and relying exclusively on short-term debt to fund their activities. Likewise, financial Sfm/Sfls were fragile as capital market investors foresee

[8] At 2011 the Sfm/Sfls total assets were 329.6 billions of Mexican pesos, and the mortgage Sfm/Sfls had 113.9 billions of pesos, i.e. 34.5%. Now, the same calculation for Sofomes and Sofoles separately, shows that the former held 43%, and the latter 32.5% of the total assets. Source: Banco de Mexico's *Reporte sobre el Sistema Financiero*, www.banxico.org.mx

a reduction of the profitability of non-financial firms. At Figure 3 we observe that the spread of the short-term debt with respect to the TIIE28, which is the 28 days average rate of the market, suffered a sharp increase after the announcement of Lehman's bankruptcy.

At this stage, the Mexican government, through *Sociedad Hipotecaria Federal*[9] (*SHF*), decided to intervene the market where Sfm/Sfls obtained their funding. They decided to offer a 65% guarantee over all short and long-term debt securities starting May 2009, and until December 2012. In addition, as the SHF could also participate at the capital markets, Sfm/Sfls could receive direct funding from them. As expected, we observe that SHF's share on a particular debt security is inversely related to their spread. In the extreme, they end up being the only buyer for the riskiest debt securities issued by Sfm/Sfls.

The objective of the intervention was to restore the normal functioning of the market where Sfm/Sfls obtained their funding. On the one hand, this implied increasing the funding volume for Sfm/Sfls. On the other hand, the intervention needed to reduce the stigma assigned upon those receiving the aid. In our setup, the "benefits" of a government intervention are reflected on the volume. The "costs" are reflected on the spreads and will be interpreted as a stigma.

We believe the intervention can be conceived as exogenous. Sfm/Sfls indeed were affected by the first quarter of 2009, but at that time was not evident that policy makers would intervene only on them, and on the way they did it. The crisis was unfolding and it was unclear if the systemic risk could escalate even further. In case it did policy makers should have had to implement an aggressive intervention in the capital markets, extensive not only to Sfm/Sfls, and investors would have implemented a different investment strategy. So, by the first quarter of 2009, we strongly believe Sfm/Sfls knew policy makers would have to step in, but they did not know the intervention would only affect mortgage Sfm/Sfls in the form of guarantees.

[9] The mission of SHF www.shf.org.mx is to "foster the development of the primary and secondary market of mortgage loans, by providing guarantees designed to construct, acquire and ameliorate homes, preferably those for the poor" (own translation).

What should have been the consequences of an intervention? If it is successful the volume of funding should have increased, and their conditions should have improved, the latter will be measured by the maturity and spreads. Additionally, a successful implementation should also have encouraged the participation of private investors. A partially successful intervention should, at least, have increased the volume of funding. Every intervention faces the challenge of minimizing the stigma effect over the treated group. Clearly, following Ennis and Weinberg [2009], this was a central aspect of the FED during the previous financial crisis. The intervention we study is no different.

3 Empirical Strategy

3.1 Model

To simplify the exposition we focus on the bivariate case. The extension for a multivariate setting should follow straightforwardly.

Consider having a population of N individuals, e.g. $i = 1, 2, ..., N$, and that for every i we observe two variables directly affected by the treatment (from hereon *output variables*), say $\mathbf{Y}_i, \mathbf{X}_i$, at two different moments (e.g. $\mathbf{T}_i \in \{0, 1\}$ where zero stands for the moment when the treatment was not implemented), and also we observe the group which they belong to (e.g. $\mathbf{G}_i \in \{0, 1\}$ where zero stands for the untreated and one for the treated). Thus, $(\mathbf{Y}_i, \mathbf{X}_i, \mathbf{T}_i, \mathbf{G}_i)$ is a vector of random variables.

Denote Y_i^N and X_i^N the output levels for individual i if he receives no treatment, and Y_i^I and X_i^I the output levels for the same individual if he receives it. Thus, the realized outcomes for both output variables are $\mathbf{Y}_i = Y_i^N \cdot (1 - I_i) + I_i \cdot Y_i^I$ and $\mathbf{X}_i = X_i^N \cdot (1 - I_i) + I_i \cdot X_i^I$, where $I_i = \mathbf{G}_i \cdot \mathbf{T}_i$ is the treatment indicator.

In the absence of covariates, output heterogeneity will be explained by individual unobserved characteristics. In particular, denote U_i^y the unobserved characteristic attached to output variable $\mathbf{Y}_i$, and U_i^x the one attached to $\mathbf{X}_i$. From hereon we suppress the subscript i to simplify notation because we are using an iid sample from the population. We assume both unobserved skills are not necessarily independent but drawn from a common joint distribution.

Define Y_{gt} as the random variable with the same distribution of $\mathbf{Y} \mid \mathbf{G} = g, \mathbf{T} = t$. The cumulative distribution function (cdf) will be $F_{Y_{gt}}(y)$, and the probability distribution function (pdf) $f_{Y_{gt}}(y)$ will be strictly positive over its support $\mathbb{Y}_{gt}$, that is a compact subset of $\mathbf{R}$. Analogously, define the random variables Y_{gt}^N, Y_{gt}^I, X_{gt}, X_{gt}^N and X_{gt}^I.

We start identifying the joint distribution of output variables for the treated group at the hypothetical situation where they did not received any treatment, i.e. $F_{Y_{11}^N X_{11}^N}(y, x)$.

3.2 Identification Using Strong Invariance

The simplest, yet useful, situation one could imagine is where the policy does not affect, within each group, the dependence structure between the unobserved variables attached to each outcome variable. This special case can be understood as the short-run analysis of the policy because, as will be shown latter, the copula between all the unobserved variables, which by construction determine all the heterogeneity on the outcome variables, remains constant within each group.

We extend Athey and Imbens [2006]'s Changes-In-Changes (CIC) model to identify the joint cumulative distribution of Y_{11}^N and X_{11}^N.[10] CIC's main identifying assumption, that the treatment does not affect the marginal distribution of unobservables within the groups, will be preserved and we propose an additional identifying assumption that affects the copula of the unobservables.

Assumption 1. *$Y_{it}^N = h(U_{it}^y, t)$ and $X_{it}^N = g(U_{it}^x, t)$ where $t = 0, 1$*

Assumption 2. *$h(u^y, t) \nearrow u^y$ and $g(u^x, t) \nearrow u^x$ given any t*

[10] The CIC model generalizes previous models which guarantee the treatment does not affect the marginal distribution of unobservables. See the supplementary material (Cañón [2015]) for a detailed presentation.

Assumption 3. $\mathbf{U}_{1t}^y \subseteq \mathbf{U}_{0t}^y$ *and* $\mathbf{U}_{1t}^x \subseteq \mathbf{U}_{0t}^x$ *for* $t \in \{0, 1\}$

Assumption 4. $U^y \perp T \mid G$ *and* $U^x \perp T \mid G$

Assumption 5. $(U^y, U^x) \perp T \mid G$

Assumptions 1 - 4 are borrowed from CIC's model. Assumption 1 allows the unobserved component, of each output variable, to vary with time within each group. Assumption 2 argues that high output levels are correlated with high level of unobservables. This type of assumption is natural if we understand unobservables as skills, but becomes questionable if we allow for output's measurement errors. Assumption 3 is a standard support condition. Finally, Assumption 4 implies that, conditioning on the group, the marginal distributions of U_{g0}^y and U_{g0}^x are respectively identical to the marginal distribution of U_{g1}^y and U_{g1}^x.

The new identifying assumption, 5, implies that within each group the dependence structure of (U^y, U^x) is unaffected by the treatment. In particular, this restriction implies that the copulas of (U_{10}^y, U_{10}^x) and (U_{11}^y, U_{11}^x) are identical. As we mention at the beginning, with this assumption we are focusing on the short run effects of the policy. Later we will replace it with a milder assumption that allows the copulas of (U_{10}^y, U_{10}^x) and (U_{11}^y, U_{11}^x) to differ.

Given assumptions 1 and 2, there is a one-to-one relationship between the joint cumulative distribution of (Y_{gt}^N, X_{gt}^N) and the copula[11] of its corresponding unobservables. Indeed, these assumptions allow us to relate the joint cumulative distribution of Y_{gt}^N, X_{gt}^N with the joint cumulative distribution of U_{gt}^y, U_{gt}^x as,[12]

$$\begin{aligned} F_{Y_{gt}^N, X_{gt}^N}(y, x) &= Prob\{h(U_{gt}^y, t) \leq y, g(U_{gt}^x, t) \leq x\} \\ &= Prob\{U_{gt}^y \leq h^{-1}(y; t), U_{gt}^x \leq g^{-1}(x; t)\} \\ &= F_{U_{gt}^y, U_{gt}^x}(h^{-1}(y; t), g^{-1}(x; t)) \end{aligned}$$

[11] See the supplementary material (Cañón [2015]) for a brief summary of copulas.

[12] Using similar arguments we can establish a relationship between the cumulative distribution of Y_{gt} (X_{gt}) and the cumulative distribution of U_{gt}^y (U_{gt}^x), e.g. $F_{Y_{gt}}(y) = F_{U_{gt}^y}(h^{-1}(y; t))$ and $F_{X_{gt}}(x) = F_{U_{gt}^x}(g^{-1}(x; t))$.

Additionally, using Sklar's theorem, there is a unique copula that links the marginal distributions $F_{U^y_{gt}}(u^y) = l^y$ and $F_{U^x_{gt}}(u^x) = l^x$ with the joint distribution $F_{U^y_{gt}U^x_{gt}}(u^y, u^x)$, because both U^y_{gt} and U^x_{gt} are continuous random variables. Thus, $F_{U^y_{gt},U^x_{gt}}(h^{-1}(y;t), g^{-1}(x;t)) = C^{gt}(F_{U^y_{gt}}(u^y), F_{U^x_{gt}}(u^x))$ where $u = h^{-1}(y;t)$, $u^x = g^{-1}(x;t)$, so naturally,

$$F_{Y^N_{gt},X^N_{gt}}(y, x) = C^{gt}(F_{U^y_{gt}}(u^y), F_{U^x_{gt}}(u^x))$$

notice this joint distribution is identified from the data if $gt = 0$, for $g \in \{0, 1\}$ and $t \in \{0, 1\}$, because they correspond either to the outcomes of individual from the untreated group, or to the outcome from the treated group before the treatment was implemented. In these cases, where $gt = 0$, potential outcomes Y^N_{gt} are observed.

The main identification results is summarized in the next theorem.

Theorem 1. *Let assumptions 1 - 5 hold. Then we can identify the joint cumulative distribution of (Y^N_{11}, X^N_{11}), e.g. $F_{Y^N_{11},X^N_{11}}(y, x)$ by,*

$$F_{Y^N_{11}X^N_{11}}(y, x) \quad = \quad F_{Y_{10}X_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)), F^{-1}_{X_{00}}(F_{X_{01}}(x))) \tag{1}$$

Theorem 1 shows that we can identify the counterfactual joint distribution of outcomes variables for the treated group. This result is handy to assess a government intervention for at least two reasons. Firstly, analogous to the results found in Athey and Imbens [2006] and Bonhomme and Sauder [2011], most certainly the intervention had different effects along the quantiles of the joint distribution, and there is no compelling evidence to focus the analysis around the mean. Secondly, we can identify the counterfactual distribution of Y^N_{11} conditional on X^N_{11}, and viceversa, because we know the unconditional counterfactual distributions of all output variables.

The next corollary follows from the latter remark,

Corollary 1. *Let assumptions 1 - 5 hold. Then we can identify the marginal distribution of Y^N_{11} conditional on X^N_{11}, and viceversa.*

The previous corollary is relevant for practitioners because reduces their data demands. For example, if we want to estimate the counterfactual marginal distribution of Y_{11}^N conditional on X_{11}^N, we can't use instead the observed level of X_{11}^I and we must find a proxy for the latter.

3.3 Identification Using Relative Ranking Invariance

The next step is to propose another identifying assumption that weakens assumption 5, but that still has a clear economic intuition. We show that much can be learned if, within each group, instead of assuming that the policy does not affect the copula of unobservables, we assume it does not affect a particular feature of the copulas, known as the *diagonal section*. In this approach we depart from a short term analysis, and allow the policy to affect the dependence structure in a particular way which will be clear after a few paragraphs.

The diagonal section of a copula $C(l^y, l^x)$ is a function $\delta_C(l) : [0,1] \to [0,1]$, where l can be either l^y or l^x, that satisfies certain conditions.[13] The identification power of the assumptions over the diagonal section hinges on what we already know from the set of copulas that share the same diagonal. In simple words, given a diagonal δ, we can always find a sharp lower bound (e.g. the Bertino copula, $B_\delta(u,v)$), and if copulas are symmetric we can also find a sharp upper bound (e.g. the Diagonal copula, $K_\delta(u,v)$).[14] Moreover, we know that if two copulas from the Archimedean family share the same diagonal they must be identical. See Appendix A for a thorough exposition.

The diagonal section, in addition, has an interesting probabilistic interpretation. Define a new random variable $Z_{gt,g't'} = \max\{L_{gt}^y, L_{g't'}^x\}$, where $L_{gt}^y = F_{U_{gt}^y}(u^y), L_{g't'}^x = F_{U_{g't'}^x}(u^x)$. The diagonal section of copula $C^{gt,g't'}(\cdot,\cdot)$ is the c.d.f. of this new random

[13] In particular they must satisfy that (i) $\delta_C(0) = 0, \delta_C(1) = 1$, (ii) $0 \leq \delta_C(l_1) - \delta_C(l_2) \leq 2(l_2 - l_1)$, for all $l_1, l_2 \in [0,1]$ and $l_1 \leq l_2$, (iii) $\max\{2l-1, 0\} \leq \delta_C(l) \leq l$. For further details see Nelsen and Fredricks [1997a] and Nelsen and Fredricks [1997b].

[14] Even if we allow copulas to be asymmetric there still is a sharp upper bound which is copula $A_\delta(u,v)$.

variable,

$$\delta_{C^{gt,g't'}}(l) = Prob\{Z_{gt,g't'} \leq l\}$$

The new assumption we propose to replace assumption 5 is that, within each group, the diagonal section of the dependence structure of (U^y, U^x) is independent of the treatment. This restriction implies that the copulas of (U^y_{10}, U^x_{10}) and (U^y_{11}, U^x_{11}) belong to the set of copulas that share the same diagonal section. Formally, let $C^{gt,gt'}(l^y, l^x)$ be the unique copula between $(U^y_{gt}, U^x_{gt'})$ for $t, t' \in \{0, 1\}$, and define $\delta_{C^{gt,gt'}}(l) := C^{gt,g't'}(l, l)$ its *diagonal section*.

Assumption 6. $\delta_{C^{g0,g0}}(l) = \delta_{C^{g1,g1}}(l)$ *for* $l \in \{l^x, l^y\}$

Assumption 6 implies that the c.d.f. of a new continuous random variable $Z_{gt,gt'} = \max\{L^y_{gt}, L^x_{gt'}\}$, where $L^y = F_{U^y}(u^y)$ and $L^x = F_{U^x}(u^x)$, is treated as invariant. In other words, it implies that this new random variable $Z_{gt,gt'}$ is independent from the treatment within each group, i.e. $Z \perp T \mid G$.

The new identifying assumption has a concise interpretation. The new random variable $Z_{gt,gt}$ is the highest ranked unobserved variable for an individual from group g at time t, i.e. $\max\{u^x_{gt}, u^y_{gt}\}$. Continuing with the example, and given an individual from group g before the policy is implemented, if u^y_{g0} is ranked at the 75% percentile of the population, and u^x_{g0} at the 50% percentile of the population, then the highest ranked unobserved variable is U^y_{g0}. Assumption 6 implies that after the policy, and for that same individual, the percentile associated to u^y_{g1} it is still higher than the one associated to u^x_{g1}. In other words, assumption 6 does not affect the relative ranking of unobservables.

Identification under the Archimedean Family. Copulas from this family are extensively used in empirical literature (see Nelsen [2006], Trivedi and Zimmer [2007]) and prove very helpful for our particular problem. Indeed, as Sungur and Yang [1996] showed that all the information contained in a copula from this family is also contained in its diagonal section, if we assume two (or more) of these copulas share the same diagonal section, then copulas are identical. It is accurate to say that identifying assumptions 5 and 6 are equivalent.

We will show that it is still possible to point identify $F_{Y_{11}^N,X_{11}^N}(y,x)$ under the relative ranking assumption, and restraining ourselves to the Archimedean copula family.[15]

Theorem 2. *Let assumptions 1 - 4 and 6 hold, and restrict to the family of copulas to the Archimedean family. Then we can identify the joint cumulative distribution of (Y_{11}^N, X_{11}^N), e.g. $F_{Y_{11}^N,X_{11}^N}(y,x)$, as in Theorem 1*

Theorem 2 is important to practitioners for several reasons. Firstly, Archimedean copulas are already included in several statistical and econometric packages, consequently our estimator is easy to implement. Secondly, it is interesting to retain point identification with a weaker identifying restriction. Finally, as with corollary 1 we can still calculate the counterfactual distribution of Y_{11}^N conditional of X_{11}^N, and viceversa.

Identification for symmetric copulas. If we leave the Archimedean family and assume copulas are only symmetric, we will loose point identification, and the bounds we obtain will improve Fréchet-Hoeffding's.

Theorem 3. *Let assumptions 1 - 4, and 6 hold. (i) For any copula we can identify a sharp lower bound for the joint distribution of (Y_{11}^N, X_{11}^N), e.g. $F_{Y_{11}^N,X_{11}^N}(y,x) \geq \mathcal{B}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}})$, where the analytic expression of the bound is at the Appendix B. And (ii) if the copulas are symmetric, we can identify the sharp bounds for the joint distribution of (Y_{11}^N, X_{11}^N),*

$$\begin{aligned}\mathcal{K}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}}) &\geq F_{Y_{11}^N,X_{11}^N}(y,x)\\ &\geq \mathcal{B}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}})\end{aligned}$$

where the analytic expression of the bounds are at the Appendix B.

[15] On a deeper level, we posit that any assumption that imply any two copulas have *the same information* will guarantee point identification.

Theorem (3) tells us we can always find a sharp lower bound, but the upper bound is guaranteed for symmetric copulas. Our approach is general in several ways. First, we do not impose a parametric form to the production function, instead we only assume output in absence of the treatment is monotonically increasing in the unobserved characteristic of the individuals. Second, we impose a weaker restriction over the copula of unobservables that only require that their relative ranking must be unaffected by the treatment.

Remark 1. *What else could we learn?.* Within this approach there are other joint distributions we, as econometricians, observe but that are not used. That is, through Theorems 1 - 3 we only use the joint distribution of (Y_{10}^N, X_{10}^N), but we also observe (Y_{00}^N, X_{00}^N), (Y_{00}^N, X_{01}^N), (Y_{01}^N, X_{00}^N), and (Y_{01}^N, X_{01}^N). At the supplementary material, found online at the author's website, we show how can we use this extra information to achieve both point and set identification. Though the identifying assumptions will naturally change because we are dealing with more information, their spirit is closely related with the copula invariance assumption, and the relative ranking assumption. We opted not to present them because the new identifying assumptions are harder to interpret for the Mexican government intervention.

3.4 Estimation and Inference

Now we discuss the estimation of the distributional effects. The Matlab programs are available via email upon request.

We can conveniently reexpress equation (1) as

$$F_{Y_{11}^N X_{11}^N}(y,x) \quad = \quad F_{Y_{10}X_{10}}(F_{Y_{10}}^{-1}(F_{Y_{11}^N}(y)), F_{X_{10}}^{-1}(F_{X_{11}^N}(x))) \tag{2}$$

and obtain the point estimate using the sample counterparts. Using Athey and Imbens [2006], $\hat{F}_{Y_{11}^N}(y)$ and $\hat{F}_{X_{11}^N}(x)$ can be identified and point estimated, we can replace $F_{Y_{10}}, F_{X_{10}}$ by their empirical distributions, and lastly, we need to estimate the copula between $\hat{F}_{Y_{10}}$ and $\hat{F}_{X_{10}}$. There are multiple ways to achieve the latter, we briefly explain the semiparametric and the nonparametric approaches, in both cases consistency is guaranteed and we can establish the asymptotic distribution of the estimator.

The semiparametric approach has two stages. It requires first to estimate nonparametrically each marginal distribution, and then estimate the copula assuming it belongs to a particular family indexed by a parameter vector. Under suitable conditions, which many standard multivariate standard distributions satisfy, Genest et al. [1995] show the estimator is consistent, and the difference between it and the true vector of parameters is distributed asymptotically normal with zero mean and variance υ^2. The authors as well show the asymptotic variance can be consistently estimated.

At the nonparametric approach we exploit the assumption that Y_{gt}, X_{gt} are continuous random variables. Fermanian et al. [2004] show that if also the copula between $\hat{F}_{Y_{10}}$ and $\hat{F}_{X_{10}}$ has continuous partial derivatives, the empirical copula process, e.g. the difference between the empirical copula and the true copula, converges weakly at $\sqrt{N}$ to a particular gaussian process in $l^{\infty}([0,1]^2)$. Assuming the copula has continuous partial derivatives can be restrictive in some contexts. The authors provide a solution by using bootstrapping, under the assumption that Y_{gt}, X_{gt} are continuous random variables. They prove that the conditional empirical copula process we have under bootstrapping weakly converges to the same limiting Gaussian process as with the unconditional empirical copula process.

Until now we discussed consistency and inference under the Strong Invariance Assumption. With the Relative Ranking Assumption, in Theorem 3 we will prove that we can identify the lower bound to any copula between $(F(Y_{11}^N(y)), F(X_{11}^N(y)))$, and if we assume the copula is symmetric, we can also identify an upper bound. As we show at the Appendix B, we can express the lower bound as

$$
\begin{aligned}
F_{Y_{11}^N, X_{11}^N}(y,x) \;\; &\geq \;\; \min\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\} \\
&- \;\; \min_{z}\{z - F_{Y_{10}X_{10}}(F_{Y_{10}}^{-1}(z), F_{X_{10}}^{-1}(z)) \mid \\
&\qquad z \in [\min\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\}, \max\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\}]\}
\end{aligned}
$$

Following the same steps as before, we can use Athey and Imbens [2006] to identify and estimate $\hat{F}_{Y_{11}^N}(y)$ and $\hat{F}_{X_{11}^N}(x)$, and replace all other marginal distributions by their sample counterparts. Finally, we can estimate the copula between $(F(Y_{10}^N(y)), F(X_{10}^N(y)))$ either by a semiparametric or a nonparametric method. Consistency and inference equally follows.

4 Data

The data we use has several advantages. First, we have rich information about every issued short and long-term debt security. This point is particularly important because a central goal of the paper is to calculate the effect of the intervention on the volume and the stigma, but now accounting for the interdependences between them that arise in equilibrium, and without assuming a particular theoretical model to rationalize the data. Second, we have access to all the of short and long-term debt securities issued by Sfm/Sfls between 2006 and 2012. In particular, we have access to other sources of funding available to Sfm/Sfls. Third, we do not have any measurement error.[16]

The data source is Mexico's Central Bank.[17] For each debt security we observe several outcome variables: the traded volume at the capital market in millions of pesos, the spread of the rate respect to the 28 days average market rate (from hereon TIIE28), the maturity in days, the identities of all the holders of the securities, and how much each holder bought in millions of pesos at several points in time. In addition to the set of outcome variables we observe covariates that will be used later at the estimation phase.

The methodology we propose requires defining two groups and two periods. From Section 2 we know that the treated group are the Sfm/Sfls from the mortgage sector.

[16] This is important vis-a-vis the related literature that study the stigma created by policy interventions during the Great Recession.

[17] Mexico has regulated and unregulated Sfm/Sfls. The information we have is only about the regulated entities, but as we mentioned at Section 2, almost all of the assets held by the sector are held by regulated Sfm/Sfls.

For the control group we use the Sfm/Sfl from the financial sector because they were the second most affected group of institutions, yet some differences respect to the treated group remains. Likewise the treated group, they were significantly affected at their funding sources, but they did not had to keep in their balance sheets the mortgage-backed securities. We posit the government believed their business model was not in danger as with the treated group, and consequently, they could fund their activities.[18]

The natural date to divide the data is the moment when the SHF started providing the 65% guarantee, then the pre-intervention period starts January 2006 and finishes April 2009, and the after-intervention period starts in May 2009 until December 2012.

We consider five output variables: volume, spread, maturity, financial institutions (FI), and non-financial institutions (NFI). The volume measures how much money a Sfm/Sfl could raise, from FI or NFI at the capital market, from each debt security. With the spread we measure the risk capital market participants assigned to every debt security. This measure captures either idiosyncratic risks or common shocks that affect all Sfm/Sfls. The maturity has the usual interpretation. Finally, the last two variables capture the percentage held, out from the total debt, by financial institutions (FI) and from Mexican non-financial institutions (NFI) at different points in time.

To control for the differences between both groups we construct two types of covariates. On the one hand, we use a distance measure between the date each debt security was issued to the next day where the financial market faces a stress period; this covariate is labeled as *Emission*. We assume such stress episodes occur when the volatility of a leading indicator of the financial or economic activity is greater than one an a half times the historical volatility. As a robustness check we tried with different financial and economic indicators, and with different departures from the historical volatility.[19] On the other hand, we used

[18] In practice financial Sfm/Sfls did receive some help from the government but in a much smaller scale. We use covariates to control for these differences.

[19] We used four indexes to calculate the historical volatility. (1) EMBI, which is a financial index to measure country risk, (2) VIMEX, which is a volatility measure for the Mexican stock market, (3) CPI, which is the Mexican consumer price index, and (4) Exchange Rate of the Mexican Peso to the

another distance measure but now of the maturity date to the next day the financial market faced a stress period; this covariate is labeled as *Due*. We believe both distance measures are unaffected by the treatment because the vast majority of Sfm/Sfls' funding came from the short-term debt securities, and do capture any difference between both groups.

Now we discuss the summary statistics at Tables 2 - 3 at the Appendix B. For the treated group, and after the intervention, we observe a reduction of the maturity, and of NFI's share. In contrast, we observe the volume of funding, the spreads, and FI's share increased. On the other hand, for the control group, after the intervention we observer a decrease of the maturity, of the volume of funding, of the spread, and an increase of FI's share. Notably, NFI's share did not changed.

If we analyze as well what happened at different quantiles[20] additional insights arise. At Table 2 in the Appendix B we observe for the control group that, except for quantile 25^{th} which remain unchanged, the maturity decreased. For the treated group we observe a similar pattern, namely, it remain unchanged for quantiles 25^{th} and 50^{th}, but decreased for the rest. We find this surprising because one could expect that after the intervention, and for the treated group, investors at the capital market should believe Sfm/Sfls are in shape to repay their short-term debt, and consequently the effect on the maturity should be non decreasing.

As we mentioned before, the average volume of funding increased for the treated, but decreased for the control group. Looking at the quantiles, we observe, for the treated group, that the volume increased for quantiles equal to or above 50%, for the lower quantiles the volume decreased. The story for the control group is different. For them, the volume increased for the quantiles equal to or below 75%, but decreased for the highest quantiles. In sum, for the treated group, the increase in the volume for high quantiles

United States dollar. We decided to present the first two because they are financial market indicators, and because the results with the other two indicators are qualitatively similar. Data is available upon request.

[20] We decided to calculate the quantiles 25^{th}, 50^{th}, 75^{th}, and 90^{th}.

outweighed the decreased at the lower quantiles; and for the control group, the decrease in the volume for the highest quantiles outweighed the rise in the rest of the distribution.[21]

At Table 2 we observe that the behavior of the spread is quite different between groups. For the treated group it is clear it increased for all quantiles, specially for those equal to or below the 50%. For the control group, the spread decreased for all quantiles. This behaviour on the treated group is intriguing because it suggests a strong stigma reflected on the spreads.

The share of the debt held by financial and non-financial institutions, see Table 3, gives information about how attractive Sfm/Sfls were after the policy. For the FIs, we observe that after the intervention their share, on both groups, increased on the quantiles equal to or above 50%. For the NFIs, we observe that while their participation on the control group remained unchanged, it decreased at all quantiles for the treated group. This evidence suggests a crowding-out effect in favor of FI.

Table 4 present the summary statistics from the covariates *Emission* and *Due*. We observe a different pattern between both groups, while for the treated both distance measures decrease after the treatment, for the control group the opposite holds. This pattern is observed as well at every quantile.

To wrap up, the summary statistics from Sfm/Sfls between 2006 and 2012 suggest four phenomena. First, that SHF's intervention increases the attractiveness of the short-term debt securities issued by the Sfm/Sfls from the mortgage sector vis-a-vis those from the control group. Second, the intervention did not significantly changed the maturity. Third, we observe that after the intervention the spreads for the treated group increased for all quantiles. One explanation we posit is that the stigma associated to receiving the government aid outweighs the potential gains in terms of spread reduction. And finally,

[21] Sfm/Sfls are heterogeneous and respond differently to the same environment. Those at higher deciles are more attractive to investors compared as to those at lower deciles. High decile Sfm/Sfls' from the treated group could have increased their volume of funding because the government intervention made them more attractive relative to those at lower deciles. Later we argue that indirect effects through the spread help to explain these differences.

the FI's participation increased for the treated group.

5 Results

In this section we apply the methodology to decompose the government's intervention effect, on the volume and the spread, into the direct and indirect effects. Special attention will receive the indirect effects that each of these variables received from other output variables jointly determined in equilibrium.

This section is developed in two steps. First, we estimate the effect of the intervention on each of the output variables using the DID and CIC estimators, we also compare the effects across quantiles, and finally compare the effect of the intervention across different samples. Second, we calculate, using our methodology, the indirect effects of the intervention on volume and spread, and analyze how different segments of the treated group reacted to the intervention.

5.1 Total Effects

Using standard DID and CIC estimators we calculate the effect of SHF's intervention on each output variable. We will refer to this effect as the total effect.

Additionally, we repeat the same calculations using different samples. First, we start with the universe of short-term debt securities. Later, we split the sample between those securities where SHF is not a debt holder, i.e. *No SHF sample*, and the securities where SHF does hold a fraction of debt, i.e. *Only SHF sample*. The reason for splitting the sample is that any possible stigma will increase when the SHF decides to buy a fraction of the debt.

We want to test two main hypothesis. First, the intervention produced the expected effect on the volume of funding. Second, the intervention produced a stigma on the

Sfm/Sfls from the mortgage sector. Once we finish with the univariate analysis we will decompose the total effect between the direct and indirect effects.

Full Sample Analysis. Tables 5 - 6 at Appendix B show the total effect of the intervention on the *volume*, *maturity*, *spread*, *financial institutions* (FI), and *non-financial institutions* (NFI). At each of the Tables we calculate the total effect using a standard difference-in-difference (DID) methodology and with Changes-in-Changes (CIC) methodology. We concentrate on the latter because it makes fewer assumptions vis-a-vis the DID and because we can calculate the distributional effects of the intervention.

The effect on the volume is easy to interpret. At Table 5 we observe that the intervention increased the volume of funding in 91 million pesos, which is a large fraction compared to the average volume of funding for the treated group before the policy was implemented, e.g. 181 millions. Moreover, we find that this effect is significant. This pattern is replicated on every quantile.

One interesting result is the estimated total impact on the spreads. We find that the intervention increased in 74 basis points the spread for the treated group, we also find that this increase is statistically significant. If we analyze the results on the quantiles we observe a similar pattern. Additionally, we find that the increase was significantly higher for the lower quantiles. All this information is supporting the hypothesis that the intervention indeed created a stigma over all short-term debt securities, specially on those securities that needed the least the government aid.

The total effect on other outputs variables are equally informative.

The effect on the maturity has the expected sign but is not significant. At the first column of Table 5 we observe that the effect on the treated is about 9 days, but is not significant. Digging deeper into the effects on the quantiles we observe that while the effect is positive and significant for the quantile 50, it is negative and significant for higher quantiles. Analysing the total effect on the maturity we observe that the positive effect we expected to find for this type of intervention is concentrated on a small fraction of the population, as for another fraction we obtain the opposite effect.

Finally, the effect of the intervention on the share of debt held by financial institutions (FI), and Mexican non-financial institutions (NFI) suggest that FI had an strategic advantage over the NFI. At Tables 5 and 6 we observe that while the share of FI increased in 9%, the share of the NFI decreased in 21%. The analysis of the effects on the quantiles add no more insights.

Before continuing let us wrap-up. SHF's intervention produced the expected impact on the volume, additionally the effect is quite important and significant. Moreover, we obtained the expected sign on the maturity, but the estimates are not significant because different fractions of the population suffered from conflicting effects. Following this line, the intervention's main cost is found at the spreads. Our estimates suggest that the Sfm/Sfls from the mortgage sector suffered a stigma from receiving the policy. Finally, the intervention made the sector more attractive as the share of FIs increased at the expense of NFIs.

Sample Comparison. The presence of SHF as a debt holder might distort the analysis. To address this issue we compare the estimates using three samples, using the universe of all short-term debt securities, e.g. *Full Sample*, using only those where SHF did not appear as debt holder, e.g. *No SHF*, and using only those where SHF holds at least a fraction of the debt, e.g. *Only SHF*.

The estimates for the volume are shown at Table 7. We observe that the effect on the mean is greater on the *No SHF* sample. On the quantiles, we observe that the effect for higher quantiles is greater on the *No SHF* sample, conversely, for the lower quantiles is greater on the *Only SHF* sample.

The results from the spread are straightforward to interpret. In Table 8 we observe that the increase, which is statistically significant, on the spreads is higher for the short-term debt securities that belong to the *Only SHF* sample. A similar pattern is observed on the quantiles less than or equal to 50%. The results at higher quantiles are not statistically significant. We conclude that the estimates on the spreads support the stigma hypothesis because the securities that suffered the most, in terms of spread, from the intervention are those where SHF had to "intervene twice".

Role of Covariates. Estimates at Tables 5 - 8 do not control for the covariates, but at Table 9 we show that the differences between the conditional and unconditional estimates are very small overall.[22] We decided not to discuss the conditional estimates because the results, besides of being numerically similar, do not shed any extra insights. In addition, the methodology to compute them is more convoluted and requires using the method sketched at Section 3. The author will provide upon request the Matlab code and the data to replicate the results.

5.2 Decomposition

In the previous subsection we learned the total effect of the intervention increased the volume of funding and created a stigma on the treated group. But was the policy's direct effect the responsible for the total effect, or the indirect effect through other output variables played a mayor role?. To answer this question we use the methodology sketched at Section 3.

We present the estimates without controlling for additional covariates because at the previous subsection the results did not significantly changed once we use them. In other words, the differences between both the groups do not affect the estimates[23].

[22] To be precise, the conditional and unconditional estimates differ at some quantiles, for output variables *volume*, *FI* and *NFI*, in greater or lesser degree depending on the covariate we use. We observe the greatest differences for *FI* and *NFI*. In these particular cases the unconditional estimates at some quantiles are very small, and usually non-significant, so small numerical differences with respect to the conditional estimates yield a high percentage difference.

[23] If we use covariates we face an additional technical difficulty, not fully addressed in the copula literature, that could obscure the results. That is, we need to estimate a high dimensional copula, and our estimates will be subject of model specification error. The author will gladly provide Matlab code where the high

Direct effects. The main questions to be addressed are: *Do the total and direct effects of the intervention substantially differ?* and if the do, *they differ in the same way along all quantiles?*

Tables 10 - 11 present selected estimates of the direct effects of the intervention. Discarded estimates are based solely on their statistical significance and do not change the overall results.[24] We concentrate on the effect over the volume conditional on the spread (table 10), on the spread conditional on the volume (table 11), and on the share of financial institutions conditional on the spread.[25]

These Tables have the same format, let us explain the first one. Column 1 displays the quantile of the spread *until where* we are controlling. From columns 2 until 6 we present, respectively, the effect on all the short-term debt securities, and on the $25^{th}, 50^{th}, 75^{th}, 90^{th}$ quantiles. This table shows the *direct effect* of the intervention on the volume because we are controlling by the effect through the spread.[26] As an example, the estimate for quantile 60, at column 1, is the direct effect on the volume conditioning *until* the 60^{th} quantile of the spread.

The intervention's direct effect on the volume changes across population subgroups, and differs from the total effect. From table 10 we observe that the direct effect on all the population is smaller than the total effect, and all estimates are statistically significant. Now, if we focus on the effect over the quantiles, we observe that the direct effect is bigger than the total effect for the quantiles 25^{th}, 50^{th} and 90^{th}, and the opposite for the quantile 75^{th}. These result are robust to controlling by any other output variable.

Table 11 presents the intervention's effect on the spread conditional on the volume. We obtain that the direct effect is smaller than the total effect for all the population, for

dimensional copulas are estimated via R-vine or C-vine copulas.

[24] The author will gladly provide the Matlab code and the data in order to replicate all the results.

[25] The last Tables were not included to reduce the length of the paper. They are included at the companion paper with the supplementary material Cañón [2015].

[26] By construction the direct effect on the volume conditional on the 100^{th} quantile of spread must be quantitatively identical to the unconditional effect of the intervention on the volume.

the quantiles 25^{th} and 50^{th}, and the opposite holds for the quantile 75^{th}. The results for the last quantile, i.e. 90^{th}, are not significant. These results do not qualitatively change if instead we control by the maturity. On the other hand, if we control by FI or NFI, we observe that the direct effect is bigger than the total effect for all the sample, but if we analyze the quantiles the analysis is more complicated because the direct effect is U-shaped.

Wrapping-up, the evidence supports the hypothesis that the direct effect of the intervention differs from the total effect. For both output variables we obtain that the direct effect is smaller than the total effect as long as we control by the volume, the spread or the maturities. If we control by FI or NFI we obtain the same effect for the volume, but not for the spread. Finally, the results on the quantiles show that for securities with low spread and high volume the direct effect is lower than the total effect, while the opposite holds for securities with high spread and low volume.

Indirect effects. So far we have learned about the intervention's direct effects, now we will address the magnitudes of the indirect effects. The terms "amplification channels" and "indirect effects" will be interchangeably used, and describe the impact of the policy through other outcome variables. For example, the "volume channel" will reflect how the direct effect on the volume is amplified or reduced, through the impact of the intervention on another output variable, in order to reach the total effect.

The main questions to be addressed are: *Are the volume and spread channels symmetric?*, and if they are not, *Can we rank them according their strength?*.

We calculate the participation of the indirect effect, on the total effect, at quantile x, e.g. $\%IE_x$, as the ratio of the difference between the effect at quantile 100 and at quantile "x", e.g. $E_{100} - E_x$, and the effect conditional at quantile 100, e.g. E_{100}.

$$\%IE_x = 1 - \frac{E_x}{E_{100}}$$

By construction the indirect effects are equal to zero when we calculate it conditional at quantile 100. Moreover, it will be positive (negative) when the total effect is greater (lower) than the direct effect, e.g. $E_{100} \gtrless E_x$. For example, if $\%IE_x = 0.5$, the total effect of the policy at quantile x is 50% higher than the direct effect. This specification of an indirect effect will be useful to rank the magnitudes across all output variables.

In order to provide a visual insight, Figures 6 - 8 plot the share of indirect effects relative to the total effect on the vertical axis, and the quantile of the conditioning variable on the horizontal axis. The first Figure has a double vertical axis, with the debt held by the Financial Institutions (FI) at the right axis.

Figure 6 presents the indirect effects on the volume. We observe that the indirect effect associated to the participation of FI has the greatest impact. At Figure 7 we show the indirect effects on the spread. We observe indirect effects are non-monotonic, and the indirect effect through the volume is very low compared to the one of FIs. Finally, Figure 8 compares the indirect effect of volume through the spread, and viceversa. We observe the indirect effects are asymmetric.

To analyze the magnitude of indirect effects, and for a particular output variable, we average them out across all quantiles. For example, if we calculate the average indirect effect on volume through spread, we average the indirect effect across all quantiles of spread. This particular average indirect effect is equal to 6% of the total effect, and it is statistically significant. We will come back to this in a few paragraphs.

Indirect effects usually are non-negligible and statistically significant. From the table below we observe that the relationship between all output variables does significantly shape the total effect of the intervention. We find that the intervention's average indirect effect on the volume through the maturity and the spread accounts, respectively, to the 8% and 6% of the total effect. In other words, once we control for the indirect effect through the spread, the volume's direct effect represents in average 94% of the total effect.

Nonetheless the average indirect effects on the spread are significant, the effects are

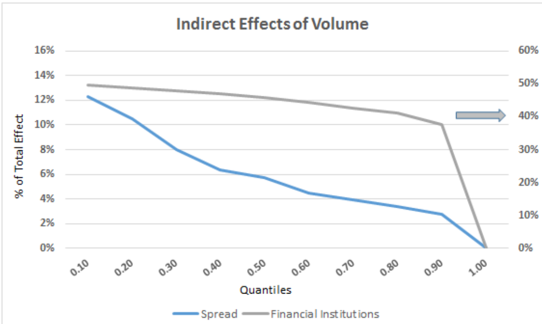

Figure 6: Indirect Effect on the Volume

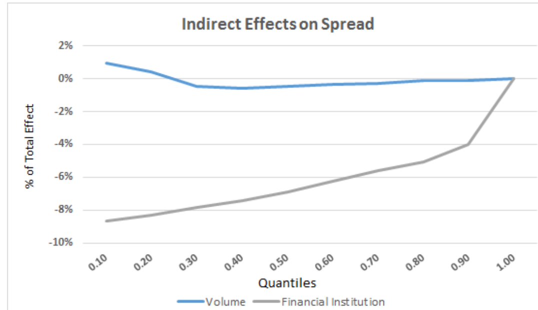

Figure 7: Indirect Effect on the Spread

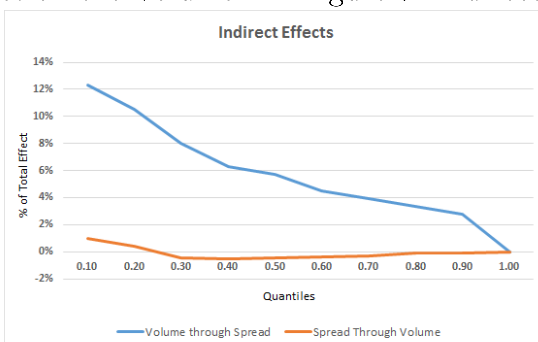

Figure 8: Indirect Effect

much smaller. From Table 1 we observe that after controlling by the maturity the direct effect account in average for 98% of the total effect. Moreover, if we control by the volume, the direct effect and the total effect are the same. This pattern, where the volume channel is stronger than the spread channel, will be recurrent.

The role of FI and NFI is of particular interest. On the one hand, we observe that the indirect effect on the maturity and the volume through FI and NFI is positive.[27] That means the intervention's direct effect is smaller than the total effect, and FIs and NFIs increase the initial direct effect. For example, once we control by FI, the direct effect on the volume only represent 55% of the total effect. On the other hand, we observe FIs and NFIs have an opposite effect on the spreads. Our estimates show that the average indirect effect on the spreads through FIs and NFIs are, respectively, -7% and -2%. That means,

[27] Estimates are not significant, but we attribute this in part to the fact we loose information when we merge the original dataset with those of FI and NFIs.

Table 1: Average of Indirect Effects Across Quantiles

	All Sample				
	Maturity	**Volume**	**Spread**	**FI**	**NFI**
Maturity		-0.12	-0.10	0.91	1.35
Volume	0.08 **		0.06 **	0.45	0.08
Spread	0.02 **	0.00 **		-0.07 **	-0.02 **
FI	0.09 **	0.11 **	0.02 **		
NFI	0.00	0.00	0.00		

*** indicate statistical significance at 1% level,** 5% level, and * 10% level.

for example, that the intervention's direct effect, once we control by FIs, is in average 7% higher than the total effect.

Now conversely, how are FI's indirect effects? Table 1 shows that FI's indirect effects through the maturity, the volume and the spread are positive, i.e. 9%, 11% and 2% respectively.

The volume and the spread interact with FIs in a concrete way. First, we obtain that the intervention's indirect effect on the volume through FIs is positive, and simultaneously, the indirect effect on FIs through the volume is also positive. Thus, output variables volume and FI reinforce between them the effect of the intervention. On the contrary, the relationship between FI and spread is different, while the effect through FIs decreases the spreads, the effect through spreads increases the FIs. To conclude, if the intervention induces FIs to buy short-term debt securities, this improves the volume of funding and decreases the spreads, and conversely, these effects will both increase FIs' participation.

We propose to rank the output variables according to the intensity of their indirect effects. Considering only the indirect effects on the volume and the spread we observe that the channels' intensity is ranked in that same order. Indeed, the indirect effects, in absolute terms, on the volume are of a greater magnitude than those on the spread. In addition, the amplification channels for the other output variables deserve a few words.

While the maturity channel is big relative to the other variables, the estimates turn out to be non-significant. The FIs channel is positive and significant, the NFIs channel is small and non-significant.

Let us wrap-up with the remark that among the channels that have statistically significant estimates, the spread channel is the weakest one. A priori we do not have neither theoretical results, nor empirical studies, pointing towards that direction. We believe this represents an interesting research topic in the future.

In the remaining of the section we focus on the spread and volume channels, and analyze the results of the quantiles, see Table12 at the Appendix B. We only consider those estimates that are statistically significant.

The pattern of the spread channel, which is in blue at the table, shows that the indirect effects are positive at quantiles 25^{th} and 50^{th}, but turns negative at quantile 75^{th}. This implies that below the quantile 50^{th} the intervention's direct effect is lower than the total effect, and the indirect effects increase the former up to the latter. Conversely, at quantile 75^{th}, the total effect is smaller than the direct effect, and the indirect effects decrease the former effect. These estimates suggest the spread channel, as expected, is more detrimental for the short-term debt securities with the lowest spreads.

The pattern for the volume channel, which is in red at the table, is the opposite. While below quantile 50^{th} the indirect effects are negative, at quantile 75^{th} they become positive. This implies that the direct effect is higher than the total effect, and at some point this inequality reverses. The role of indirect effects then is to decrease the direct effect, and at quantile 75^{th}, the opposite holds. Our estimates suggest that the volume channel is particularly strong on the short-term debt securities capable of attracting high levels of funding.

The analysis on the quantiles reveals how the intervention affected different subpopulations. For securities considered as "high quality", i.e. those with a small spread and high volume, the direct effect of the intervention increased the spread and the volume. The indirect effect, while plays against them with the spread as it increases the direct effect, helps with the volume because increases the direct effect. The story for securities considered as "low quality", i.e. those with a high spread and low volume, is analogous. As with the other subpopulation, the direct effect of the intervention increased the spread and the volume. The indirect effect, while plays in favor with the spread by decreasing the direct effect, plays against them with the volume as it decreases de direct effect.

6 Discussion

Interventions to restore market functioning due to a strong adverse selection phenomenon have been studied, using a mechanism design approach, by Tirole [2012] and Philippon and Skreta [2012]. Without discussing the details, we wish to highlight the main conclusions of both papers. First, the optimal intervention should not benefit all the agents, that is, the government should only help the segment of the "worst" agents, the rest should go and fund at the private capital markets. Second, it is costly to help each of the agents, in the extreme, the government will not even make monetary profits by helping "highest type" agents. As a consequence, the government has a trade-off to balance between the intervention cost and reducing the efficiency losses of adverse selection. Third, it is desirable that the government induces private investors to participate in capital markets. Finally, these interventions produce ex-ante moral hazard.

In our setup, the "benefits" of a government intervention are reflected on the volume, on the maturity, and on the willingness of financial and non-financial institutions to fund the Sfm/Sfls. Any government intervention has some "costs", but we know only a few things about their impact on the optimal intervention. Among the most prominent we have the administrative costs, the political costs, and the stigma of participation for the rescued entities. In our setup, the intervention's "costs" are reflected on the spreads and will be interpreted as a stigma. From Philippon and Skreta [2012] we know the rewarding rents the government has to give are not affected by the stigma, and from Tirole [2012] we know the optimal intervention is unaffected if per unit costs are small. Our paper provides evidence on the role of the stigma on the overall effect on the volume, the maturity, and the market thickness.

For policy makers it is crucial to understand the potential direct and indirect effects of the intervention. Several implications unfold.

Our estimates suggest it is critical to induce financial institutions to actively participate at the market suffering from adverse selection. Increasing the market thickness boosts the effect on the volume, e.g. volume channel, and at the same time decreases the negative impact on the spreads, e.g. the spread channel. Moreover, the role of the indirect effects varies across the population. While for those debt securities with a small spread and high volume, the indirect effect on the volume (spread) through the spread (volume) reinforces the direct effect of the policy, for those securities with high spread and low volume, the indirect effect reduces the direct effect.

Our paper runs in favor that policy makers, as long as it is feasible, should discriminate the policy across the population. We acknowledge that during a crisis it is difficult to come-up with a perfect intervention, but there is evidence suggesting that the policy's effects are different along the treated group.

7 Conclusions

As almost every policy program simultaneously affects several output variables, any policy maker is interested in identifying their counterfactual joint distribution. Up to our knowledge all the literature on program evaluation had focused on searching the set of assumptions that let us identify the counterfactual marginal distribution of each outcome variable. The identifying assumptions we find are placed at the marginal distribution level, and not at the level of the joint distribution. This paper uses Athey and Imbens [2006] Changes-In-Changes model to identify the counterfactual marginal distributions of all outcome variables, and studies which assumptions allow us to identify their counterfactual joint distribution.

We show that point identification can be achieved if, within each group, the treatment does not affect the copula between the unobservables. We name this as the Copula Invariance Assumption. Later, we relax this assumption and show that set identification can be achieved if the treatment does not affect the diagonal section of the copula between unobservables. We name this as the Relative Ranking Invariance Assumption. Both identifying assumptions are equivalent for Archimedean Copulas.

In our application we have studied the effects of a government intervention, in the form of guarantees, on the segment of Mexican financial institutions most severely affected by the 2008-09 financial crisis. Our analysis is comprehensive because we jointly consider several important dimensions that are usually analyzed in isolation. In particular, we calculated the effect of the intervention on the volume of funding the treated group received, as well as on the spreads.

We find evidence to conclude that while the government intervention did achieve the goal of increasing the funding relative to a situation without the government guarantees, it was less effective ameliorating the stigma created on the treated group which resulted in higher spreads. Also, we find the indirect effects of the intervention on each output variable are non-negligible and mostly statistically significant. Surprisingly, the indirect effects associated to the stigma are the smallest of all. Finally, we find the indirect effects on the "high quality" debt securities, i.e. those with low spread and high volume, operate in the opposite direction compared to the "low quality" debt securities, i.e. those with high spreads and low volume. This confirms the hypothesis that the intervention does not affect all members of the treated group in the same way.

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8 Appendix A

8.1 Diagonal Section of a Copula

Let δ be any diagonal, and $\mathcal{C}_\delta$ be the set of all copulas with diagonal δ. Literature on this field already showed (see Nelsen [2006], Nelsen and Fredricks [1997a] and Nelsen and Fredricks [1997b]) that given a diagonal δ, the set $\mathcal{C}_\delta$ is non-empty, and we can find sharper bounds than the Fréchet-Hoeffding bounds. R.B. et al. [2008] shows in Theorem 2 that for any diagonal δ, for copulas B_δ, K_δ and A_δ, and for every $(u,v) \in [0,1]^2$,

$$\begin{aligned}
B_\delta(u,v) &= \min(u,v) - \min_x \{x - \delta(x) \mid x \in [\min\{u,v\}, \max\{u,v\}]\} && (3)\\
K_\delta(u,v) &= \min\left(u, v, \frac{\delta(u)+\delta(v)}{2}\right) && (4)\\
A_\delta(u,v) &= \min\Big(u, v, \max(u,v) - \max_x \{x - \delta(x) \mid x \in [\min\{u,v\}, \max\{u,v\}]\}\Big) && (5)
\end{aligned}$$

the following statement holds: (i) Copulas $B_\delta, K_\delta \in \mathcal{C}_\delta$ and they are singular copulas; (ii) Copulas $B_\delta, K_\delta, A_\delta$ are symmetric and satisfy $B_\delta \le K_\delta \le A_\delta$ in the sense of first order stochastic dominance (FOSD); (iii) If all copulas from the set $\mathcal{C}_\delta$ are symmetric, then $\forall C \in \mathcal{C}_\delta \; C \le K_\delta$ also in the FOSD; (iv) $A_\delta \in \mathcal{C}_\delta$ iff $A_\delta = K_\delta$.

In simple words, given a diagonal δ, we can always find a sharp lower bound (e.g. the Bertino copula, $B_\delta(u,v)$), and if copulas are symmetric we can also find a sharp upper bound (e.g. the Diagonal copula, $K_\delta(u,v)$).[28]

If we restrict the family of copulas to the Archimedean family the identification power is greater. A copula C belongs to the Archimedean family[29] if we can represent it as $C(u,v) = \varphi^{[-1]}[\varphi(u)+\varphi(v)]$, where $\varphi : [0,1] \to [0,\infty]$ is continuous, convex, strictly decreasing, that satisfy $\varphi(1) = 0$. $\varphi^{[-1]}$ is the pseudo-inverse of φ.

For this family of copulas the diagonal section has an analytic solution that only depends on φ (called *generator* from hereon), i.e. $\delta_C(u) = \varphi^{[-1]}[2\varphi(u)]$. Moreover, Sungur and Yang [1996] showed that for a given diagonal δ_C we can pin down the generator as

$$\varphi(u) = \lim_{n\to\infty} 2^n \left(1 - \delta_C^{-n}(u)\right) \tag{6}$$

[28] Even if we allow copulas to be asymmetric there still is a sharp upper bound which is copula $A_\delta(u,v)$.
[29] See Trivedi and Zimmer [2007] and Nelsen [2006] for further details.

where $\delta_C^{-n} := \delta_C^{-1} \circ \delta_C^{-1} \circ \cdots \circ \delta_C^{-1}$ n-times. This implies in particular that if the dependence structure is restricted to the Archimedean family the set of copulas sharing the same diagonal section ($\mathcal{C}_\delta$) becomes a singleton.

9 Appendix B

9.1 Remarks & Proofs

Proof of Theorem (1)

Proof. Start with equation (3.2),

$$\begin{aligned} F_{Y_{11}^N X_{11}^N}(y,x) &= C^{11,11}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \\ &= C^{10,10}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \end{aligned}$$

where the second equality follows from assumption 5.

Using again equation (3.2) we have,

$$\begin{aligned} F_{Y_{11}^N X_{11}^N}(y,x) &= F_{Y_{10}^N X_{10}^N}(h(F^{-1}_{U_{10}^y}(F_{U_{11}^y}(h^{-1}(y;1))),0), g(F^{-1}_{U_{10}^x}(F_{U_{11}^x}(g^{-1}(x;1))),0)) \\ &= F_{Y_{10}^N X_{10}^N}(F^{-1}_{Y_{10}^N}(F_{Y_{11}^N}(y)), F^{-1}_{X_{10}^N}(F_{X_{11}^N}(x))) \end{aligned}$$

where the second equality express the equation in terms of the marginal of Y and X.

With assumptions 1 - 4 we can use Athey and Imbens [2006] to identify $F_{Y_{11}^N}(y)$ and $F_{X_{11}^N}(x)$. Replacing them at the previous equation we have,

$$\begin{aligned} F_{Y_{11}^N X_{11}^N}(y,x) &= F_{Y_{10} X_{10}}(F^{-1}_{Y_{10}}(F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)))), F^{-1}_{X_{10}}(F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x))))) \\ &= F_{Y_{10} X_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)), F^{-1}_{X_{00}}(F_{X_{01}}(x))) \end{aligned}$$

■

Proof of Theorem (2)

Proof. Assumption 6 applied to copulas from the Archimedean family implies that the copulas are equal as well, as with assumption 5. Then, the proof is the same as Theorem . ■

Proof of Theorem (3)

Proof. Using equation (3.2) and assumption 6 we establish an lower bound to the joint distribution of (Y_{11}^N, X_{11}^N) as,

$$\begin{aligned} F_{Y_{11}^N, X_{11}^N}(y,x) &= C^{11,11}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \\ &\geq B_{\delta_{C^{11,11}}}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \end{aligned}$$

Using the analytic expression for the Bertino Copula we have that,

$$\begin{aligned} F_{Y_{11}^N, X_{11}^N}(y,x) &\geq \min\{F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)\} \\ &- \min_z\{z - \delta_{C^{10,10}}(z) \mid z \in [\min\{F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)\}, \max\{F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)\}]\} \end{aligned}$$

Using equation (3.2) and replacing the marginal of the unobservables with the marginals of Y and X,

$$\begin{aligned} F_{Y_{11}^N, X_{11}^N}(y,x) &\geq \min\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\} \\ &- \min_z\{z - F_{Y_{10}X_{10}}(F_{Y_{10}}^{-1}(z), F_{X_{10}}^{-1}(z)) \mid z \in [\min\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\}, \\ &\max\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x)\}]\} \end{aligned}$$

Assumptions 1 - 4 allows us to identify $F_{Y_{11}^N}(y)$ and $F_{X_{11}^N}(x)$ using Athey and Imbens [2006] methodology, then

$$\begin{aligned} F_{Y_{11}^N, X_{11}^N}(y,x) &\geq \min\{F_{Y_{10}}(F_{Y_{00}}^{-1}(F_{Y_{01}}(y))), F_{X_{10}}(F_{X_{00}}^{-1}(F_{X_{01}}(x)))\} \\ &- \min_z\{z - F_{Y_{10}X_{10}}(F_{Y_{10}}^{-1}(z), F_{X_{10}}^{-1}(z)) \mid z \in [\min\{F_{Y_{10}}(F_{Y_{00}}^{-1}(F_{Y_{01}}(y))), F_{X_{10}}(F_{X_{00}}^{-1}(F_{X_{01}}(x)))\}, \\ &\max\{F_{Y_{10}}(F_{Y_{00}}^{-1}(F_{Y_{01}}(y))), F_{X_{10}}(F_{X_{00}}^{-1}(F_{X_{01}}(x)))\}]\} \\ &\equiv \mathcal{B}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}}) \end{aligned}$$

Now focus on copulas from the symmetric family. Let us start again using equation (3.2) and assumption 6 to determine the upper bound for the joint distribution of (Y_{11}^N, X_{11}^N) as,

$$\begin{aligned} F_{Y_{11}^N, X_{11}^N}(y,x) &= C^{11,11}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \\ &\leq K_{\delta_{C^{11,11}}}(F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x)) \end{aligned}$$

Replacing the analytic expression for the upper bound we obtain,

$$F_{Y_{11}^N,X_{11}^N}(y,x) \leq \min\{F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x), \frac{\delta_{C^{10,10}}(F_{U_{11}^y}(u^y))}{2} + \frac{\delta_{C^{10,10}}(F_{U_{11}^x}(u^x))}{2}\}$$

Using equation (3.2),

$$\begin{aligned} F_{Y_{11}^N,X_{11}^N}(y,x) &\leq \min\{F_{U_{11}^y}(u^y), F_{U_{11}^x}(u^x), \\ &\quad \frac{F_{Y_{10}X_{10}}(h(F^{-1}_{U_{10}^y}(F_{U_{11}^y}(h^{-1}(y;1))),0), g(F^{-1}_{U_{10}^x}(F_{U_{11}^y}(h^{-1}(y;1))),0))}{2} \\ &+ \frac{F_{Y_{10}X_{10}}(h(F^{-1}_{U_{10}^y}(F_{U_{11}^x}(g^{-1}(x;1))),0), g(F^{-1}_{U_{10}^x}(F_{U_{11}^x}(g^{-1}(x;1))),0))}{2}\} \end{aligned}$$

Replacing the marginal of the unobservables with the marginals of Y and X we have,

$$\begin{aligned} F_{Y_{11}^N,X_{11}^N}(y,x) &\leq \min\{F_{Y_{11}^N}(y), F_{X_{11}^N}(x), \\ &\quad \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{10}}(F_{Y_{11}^N}(y)), F^{-1}_{X_{10}}(F_{Y_{11}^N}(y)))}{2} \\ &+ \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{10}}(F_{X_{11}^N}(x)), F^{-1}_{X_{10}}(F_{X_{11}^N}(x)))}{2}\} \end{aligned}$$

With assumptions 1 - 4 we can use Athey and Imbens [2006] results to identify $F_{Y_{11}^N}(y)$ and $F_{X_{11}^N}(x)$. Replacing the marginal distributions of Y_{11}^N and X_{11}^N we obtain,

$$\begin{aligned} F_{Y_{11}^N,X_{11}^N}(y,x) &\leq \min\{F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y))), F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x))), \\ &\quad \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{10}}(F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)))), F^{-1}_{X_{10}}(F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)))))}{2} \\ &+ \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{10}}(F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x)))), F^{-1}_{X_{10}}(F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x)))))}{2}\} \\ &= \min\{F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y))), F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x))), \\ &\quad \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)), F^{-1}_{X_{10}}(F_{Y_{10}}(F^{-1}_{Y_{00}}(F_{Y_{01}}(y)))))}{2} \\ &+ \frac{F_{Y_{10}X_{10}}(F^{-1}_{Y_{10}}(F_{X_{10}}(F^{-1}_{X_{00}}(F_{X_{01}}(x)))), F^{-1}_{X_{00}}(F_{X_{01}}(x)))}{2}\} \\ &\equiv \mathcal{K}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}}) \end{aligned}$$

Thus we conclude that,

$$\begin{aligned} \mathcal{K}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}}) &\geq F_{Y_{11}^N,X_{11}^N}(y,x) \\ &\geq \mathcal{B}_{\delta_{C^{11,11}}}(F_{Y_{10}}, F_{Y_{00}}, F_{Y_{01}}, F_{X_{10}}, F_{X_{00}}, F_{X_{01}}) \end{aligned}$$

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9.2 Tables & Figures

Table 2: Summary Statistics

	Maturity								
	Mean	**Std**	**25th Q.**	**50th Q.**	**75th Q.**	**90th Q.**	**Min**	**Max**	**Obs**
Control, Before	103.48	101.22	28.00	56.00	168.00	336.00	7.00	364.00	489.00
Control, After	84.37	93.06	28.00	28.00	91.00	196.00	14.00	364.00	737.00
Treated, Before	111.57	103.78	28.00	84.00	168.00	336.00	6.00	364.00	965.00
Treated, After	77.78	53.39	28.00	84.00	84.00	112.00	7.00	340.00	432.00
	Volume								
	Mean	**Std**	**25th Q.**	**50th Q.**	**75th Q.**	**90th Q.**	**Min**	**Max**	**Obs**
Control, Before	2.53E+08	2.62E+08	7.33E+07	1.50E+08	3.00E+08	7.00E+08	0.00E+00	1.20E+09	489.00
Control, After	2.34E+08	1.91E+08	8.00E+07	1.67E+08	3.50E+08	5.40E+08	8.98E+06	8.50E+08	737.00
Treated, Before	1.83E+08	2.13E+08	5.76E+07	1.02E+08	2.18E+08	4.00E+08	1.50E+06	2.00E+09	965.00
Treated, After	2.66E+08	2.37E+08	1.12E+08	2.00E+08	3.65E+08	5.00E+08	6.11E+06	1.37E+09	432.00
	Spread								
	Mean	**Std**	**25th Q.**	**50th Q.**	**75th Q.**	**90th Q.**	**Min**	**Max**	**Obs**
Control, Before	87.61	134.34	-13.00	48.00	132.00	301.25	-42.00	631.50	489.00
Control, After	58.31	116.89	-10.25	4.00	55.00	250.50	-29.00	652.00	737.00
Treated, Before	87.78	160.52	-4.50	29.00	99.75	400.50	-340.75	595.50	965.00
Treated, After	131.35	115.30	97.18	109.50	125.25	405.50	19.60	595.25	432.00

Note: Source Banco de Mexico.

Table 3: Summary Statistics

	Financial Institutions								
	Mean	**Std**	**25th Q.**	**50th Q.**	**75th Q.**	**90th Q.**	**Min**	**Max**	**Obs**
Control, Before	0.08	0.15	0.00	0.00	0.09	0.29	0.00	1.00	240.00
Control, After	0.10	0.20	0.00	0.00	0.07	0.41	0.00	1.00	625.00
Treated, Before	0.21	0.31	0.00	0.02	0.32	0.77	0.00	1.00	417.00
Treated, After	0.34	0.39	0.00	0.04	0.78	0.90	0.00	1.00	395.00
	Mexican Nonfinancial Institutions								
	Mean	**Std**	**25th Q.**	**50th Q.**	**75th Q.**	**90th Q.**	**Min**	**Max**	**Obs**
Control, Before	0.89	0.20	0.84	1.00	1.00	1.00	0.00	1.00	240.00
Control, After	0.89	0.20	0.90	1.00	1.00	1.00	0.00	1.00	625.00
Treated, Before	0.75	0.33	0.58	0.91	1.00	1.00	0.00	1.00	417.00
Treated, After	0.54	0.41	0.09	0.63	0.96	1.00	0.00	1.00	395.00

Note: Source Banco de Mexico. The sample has fewer observations because we merged the original dataset with another one that includes the identities from all debt-holders.

Table 4: Summary Statistics - Covariates

	Emission - EMBI							
	Mean	**Std**	**25th. Q.**	**50th. Q.**	**75th. Q.**	**90th. Q.**	**Min**	**Max**
Control, Before	18.44	25.56	4.00	9.00	21.00	48.60	2.00	182.00
Control, After	19.20	24.30	4.00	9.00	22.00	55.00	2.00	112.00
Treated, Before	36.93	45.53	6.00	16.00	54.00	102.80	2.00	199.00
Treated, After	14.50	17.97	4.00	7.50	17.00	33.90	2.00	112.00

	Due - EMBI							
	Mean	**Std**	**25th. Q.**	**50th. Q.**	**75th. Q.**	**90th. Q.**	**Min**	**Max**
Control, Before	15.13	21.35	3.00	8.00	16.00	36.00	2.00	181.00
Control, After	21.80	25.58	5.00	11.00	29.00	58.00	2.00	138.00
Treated, Before	31.08	39.97	4.00	13.00	40.00	91.00	2.00	199.00
Treated, After	15.67	18.47	4.00	8.00	20.00	35.90	2.00	112.00

	Emission - VIMEX							
	Mean	**Std**	**25th. Q.**	**50th. Q.**	**75th. Q.**	**90th. Q.**	**Min**	**Max**
Control, Before	20.55	25.95	4.00	10.00	27.00	55.00	2.00	149.00
Control, After	22.09	24.69	6.00	14.00	29.00	52.00	2.00	166.00
Treated, Before	34.31	36.04	6.00	18.00	52.00	87.00	2.00	157.00
Treated, After	20.11	19.35	6.00	14.00	29.00	49.90	2.00	165.00

	Due - VIMEX							
	Mean	**Std**	**25th. Q.**	**50th. Q.**	**75th. Q.**	**90th. Q.**	**Min**	**Max**
Control, Before	16.42	20.49	4.00	8.00	18.00	46.20	2.00	149.00
Control, After	27.77	34.46	7.00	15.00	33.00	65.00	2.00	166.00
Treated, Before	30.38	35.03	5.00	14.00	46.00	80.00	2.00	157.00
Treated, After	22.45	23.83	5.00	14.00	32.00	52.00	2.00	165.00

Note: Source Banco de Mexico. This sample has the same number of observations as the original dataset.

Table 5: DID and CIC estimations

	Mean		25th quant.		50th quant.		75th quant.		90th quant.	
Maturity										
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
DID	-14.69	-14.69	19.10	-33.79	19.10	-5.79	-64.90	43.21	-204.90	106.21
	(7.67)	(7.35)	(6.05)	(4.44)	(9.57)	(12.85)	(5.94)	(18.75)	(9.90)	(52.18)
CIC	8.95	-17.54	0.00	0.00	56.00	56.00	-7.00	-7.00	-84.00	-84.00
	(8.44)	(4.69)	(1.28)	(0.00)	(1.79)	(8.93)	(0.00)	(0.77)	(43.88)	(39.29)
Volume										
	Mean		**25th quant.**		**50th quant.**		**75th quant.**		**90th quant.**	
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
DID	1.0E+08	1.0E+08	7.4E+07	7.7E+07	1.2E+08	6.6E+07	1.7E+08	3.3E+07	1.2E+08	2.4E+08
	(1.9E+07)	(1.9E+07)	(1.6E+07)	(1.7E+07)	(1.7E+07)	(2.5E+07)	(3.2E+07)	(3.3E+07)	(6.2E+07)	(5.3E+07)
CIC	9.1E+07	1.0E+08	3.7E+07	5.3E+07	5.1E+07	4.4E+07	1.2E+08	1.5E+08	8.8E+07	3.1E+08
	(1.7E+07)	(2.6E+07)	(1.2E+07)	(1.6E+07)	(2.2E+07)	(2.9E+07)	(4.5E+07)	(4.7E+07)	(6.6E+07)	(1.1E+08)
Spread										
	Mean		**25th quant.**		**50th quant.**		**75th quant.**		**90th quant.**	
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
DID	72.86	72.86	130.98	40.81	109.80	87.56	54.80	120.56	34.30	94.31
	(10.53)	(10.82)	(13.55)	(7.70)	(8.72)	(10.38)	(9.50)	(22.48)	(84.31)	(27.89)
CIC	74.25	53.82	103.18	60.75	107.50	115.50	106.25	70.50	59.49	-100.50
	(9.73)	(12.11)	(11.15)	(19.56)	(2.59)	(3.98)	(6.14)	(20.41)	(93.11)	(78.00)
Financial Institutions										
	Mean		**25th quant.**		**50th quant.**		**75th quant.**		**90th quant.**	
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
DID	0.10	0.10	-0.03	0.13	0.00	0.13	0.44	0.15	0.10	0.01
	(0.03)	(0.03)	(0.01)	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.07)	(0.06)
CIC	0.09	0.08	0.00	0.00	0.01	0.00	0.33	0.23	0.00	0.35
	(0.03)	(0.04)	(0.00)	(0.00)	(0.04)	(0.00)	(0.10)	(0.15)	(0.06)	(0.07)

Note: This table presents the estimated effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Numbers in parethesis represent standard errors.

Table 6: DID and CIC estimations

	Mean		25th quant.		50th quant.		75th quant.		90th quant.	
Mexican Nonfinancial Institutions										
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
DID	-0.21	-0.21	-0.50	-0.27	-0.29	-0.21	-0.04	-0.21	0.00	-0.21
	(0.03)	(0.03)	(0.06)	(0.05)	(0.11)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
CIC	-0.21	-0.20	-0.47	-0.53	-0.32	-0.05	-0.04	-0.06	0.00	-0.06
	(0.03)	(0.04)	(0.10)	(0.13)	(0.11)	(0.03)	(0.01)	(0.02)	(0.00)	(0.02)

Note: This table presents the estimated effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Numbers in parethesis represent standard errors.

Table 7: DID and CIC Estimations - Sample Comparison

Volume

	Mean			**25th quant.**			**50th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	2.6E+07	3.2E+07	2.2E+07	5.0E+06	-2.3E+07	2.9E+07	-4.1E+06	-4.6E+06	-1.2E+06
	(2.2E+07)	(2.4E+07)	(2.4E+07)	(1.9E+07)	(2.0E+07)	(2.0E+07)	(2.0E+07)	(2.5E+07)	(1.9E+07)
CIC	1.9E+07	2.5E+07	1.5E+07	2.5E+07	-5.0E+06	4.8E+07	-1.3E+07	-1.4E+07	-1.0E+07
	(2.1E+07)	(2.6E+07)	(2.2E+07)	(1.7E+07)	(1.9E+07)	(1.8E+07)	(2.2E+07)	(3.1E+07)	(2.4E+07)

	75th quant.			**90th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	6.1E+07	1.8E+08	-3.7E+07	1.5E+07	2.5E+07	1.5E+07
	(4.0E+07)	(5.0E+07)	(2.6E+07)	(6.4E+07)	(6.4E+07)	(1.0E+08)
CIC	3.5E+07	1.5E+08	-6.3E+07	-7.6E+07	-7.6E+07	-7.6E+07
	(6.3E+07)	(7.8E+07)	(6.2E+07)	(5.8E+07)	(5.7E+07)	(9.5E+07)

Note: This table presents the estimated effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Numbers in parethesis represent standard errors.

Table 8: DID and CIC Estimations - Sample Comparison

Spread

	Mean			**25th quant.**			**50th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	44.75	39.37	60.10	119.21	64.71	153.21	103.22	98.96	114.88
	(14.97)	(16.88)	(14.89)	(18.60)	(11.53)	(11.44)	(11.94)	(17.59)	(12.07)
CIC	42.49	35.43	57.85	87.50	33.00	121.50	110.01	99.25	121.67
	(16.37)	(15.76)	(16.67)	(13.78)	(1.79)	(3.30)	(3.06)	(8.12)	(2.78)

	75th quant.			**90th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	-123.79	-97.44	-123.79	-14.64	-12.82	-270.29
	(52.83)	(55.20)	(52.98)	(76.42)	(79.30)	(80.31)
CIC	-74.25	-27.40	-74.25	6.15	7.72	-249.50
	(67.44)	(60.08)	(68.37)	(78.60)	(87.42)	(129.21)

Financial Institutions

	Mean			**25th quant.**			**50th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	0.10	-0.09	0.33	-0.03	-0.03	0.13	0.00	-0.03	0.64
	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.10)	(0.04)	(0.02)	(0.05)
CIC	0.09	-0.11	0.32	0.00	0.00	0.16	0.01	-0.03	0.65
	(0.03)	(0.03)	(0.04)	(0.00)	(0.00)	(0.09)	(0.04)	(0.02)	(0.05)

	75th quant.			**90th quant.**		
	All	**No SHF**	**Only SHF**	**All**	**No SHF**	**Only SHF**
DID	0.44	-0.30	0.51	0.10	0.02	0.13
	(0.05)	(0.05)	(0.04)	(0.07)	(0.14)	(0.07)
CIC	0.33	-0.40	0.40	0.00	-0.11	0.02
	(0.10)	(0.10)	(0.10)	(0.06)	(0.12)	(0.07)

Note: This table presents the estimated effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Numbers in parethesis represent standard errors.

Table 9: % Difference Between Conditional and Unconditional CIC Estimates

VIMEX				
	All Sample		**25th. Q**	
	Emission	**Due**	**Emission**	**Due**
Diff.	-2.00	-2.00	0.00	0.00
Volume	3.00	2.0	2.0	2.00
Spread	0.00	0.00	4.00	3.00
FI	1.00	3.00	0.00	0.00
NFI	-2.00	-1.00	0.00	-1.00
	50th. Q		**75th. Q.**	
	Emission	**Due**	**Emission**	**Due**
Diff.	0.00	0.00	0.00	0.00
Volume	-89.00	-60.00	-2.00	2.00
Spread	0.00	0.00	-1.00	-1.00
FI	-22.00	-6.00	1.00	0.00
NFI	-5.00	0.00	-35.00	-10.00
	90th. Q			
	Emission	**Due**		
Diff.	0.00	0.00		
Volume	-9.00	-2.00		
Spread	-8.00	-8.00		
FI	-242.00	-196.00		
NFI	48.00	0.00		

EMBI				
	All Sample		**25th. Q**	
	Emission	**Due**	**Emission**	**Due**
Diff.	-0.02	-0.02	0.00	0.00
Volume	0.01	0.01	0.02	0.02
Spread	0.00	0.00	0.02	0.02
FI	1.00	3.00	0.00	0.00
NFI	-1.00	-1.00	0.00	-1.00
	50th. Q		**75th. Q.**	
	Emission	**Due**	**Emission**	**Due**
Diff.	0.00	0.00	0.00	0.00
Volume	-37.00	-43.00	2.00	2.00
Spread	0.00	0.00	-1.00	-1.00
FI	-11.00	28.00	1.00	0.00
NFI	-1.00	0.00	-24.00	-28.00
	90th. Q			
	Emission	**Due**		
Diff.	0.00	0.00		
Volume	-1.00	-1.00		
Spread	-3.00	-3.00		
FI	-60.00	-86.00		
NFI	0.00	0.00		

Table 10: Direct Effect Volume Conditional on Spread

Quantiles	All	Q25	Q50	Q75	Q90
0.10	79772339.97**	45000000.00**	91876600.00**	83649200.00**	105000000.00**
0.15	80846330.77**	50000000.00**	93906000.00**	83649200.00**	105000000.00**
0.20	81405447.52**	50000000.00**	94341000.00**	83649200.00**	105000000.00**
0.25	83357792.89**	51179800.00**	100528100.00**	86903100.00**	105000000.00**
0.30	83683709.11**	51179800.00**	100650000.00**	86903100.00**	105000000.00**
0.35	84452392.07**	32600000.00**	100650000.00**	93063000.00**	105000000.00**
0.40	85217937.57**	32816400.00**	101239400.00**	93063000.00**	105000000.00**
0.45	85517607.22**	32816400.00**	101239400.00**	100000000.00**	100000000.00**
0.50	85787595.14**	38000000.00**	101239400.00**	100000000.00**	100000000.00**
0.55	86359466.46**	38000000.00**	101988000.00**	100000000.00**	100000000.00**
0.60	86887332.03**	38000000.00**	101988000.00**	127960000.00**	100000000.00**
0.65	87114611.45**	38000000.00**	101988000.00**	134226000.00**	100000000.00
0.70	87409885.41**	38000000.00**	101988000.00**	134226000.00**	100000000.00
0.75	87634944.28**	38000000.00**	101988000.00**	134226000.00**	100000000.00
0.80	87896927.35**	40485000.00**	101988000.00**	138325700.00**	100000000.00
0.85	88052253.69**	40485000.00**	101988000.00**	138325700.00**	100000000.00
0.90	88474004.69**	40485000.00**	104341000.00**	97621000.00**	100000000.00
0.95	89004132.04**	37000000.00**	104567600.00**	112821700.00**	100000000.00
1.00	90966823.67**	38756800.00**	50000000.00**	115322700.00**	88000000.00

Note: This table presents the estimated direct effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Standard errors were calculated by bootstrap with 1000 iterations.
*** indicate statistical significance at 1% level,** 5% level, and * 10% level

Table 11: Direct Effect Spread Conditional on Volume

Quantiles	All	Q25	Q50	Q75	Q90
0.10	73.53**	44.00**	103.50**	114.00**	-98.00
0.15	73.81**	43.50**	104.65**	114.00**	-100.00
0.20	73.95**	62.50**	105.00**	114.00**	-100.00
0.25	74.51**	67.00**	105.79**	110.88**	-147.90
0.30	74.59**	67.00**	106.00**	110.00**	-147.90
0.35	74.62**	80.00**	106.00**	110.00**	-147.50
0.40	74.66**	84.51**	106.55**	110.00**	-147.50
0.45	74.68**	84.00**	107.85**	108.28**	-157.50
0.50	74.60**	83.50**	108.50**	108.00**	74.50
0.55	74.57**	96.00**	108.50**	108.01**	74.50
0.60	74.52**	96.87**	108.25**	108.11**	75.00
0.65	74.49**	96.87**	107.35**	108.11**	75.00
0.70	74.46**	96.87**	107.50**	108.11**	75.01
0.75	74.43**	96.87**	107.50**	108.11**	75.01
0.80	74.34**	97.50**	107.50**	107.28**	59.00
0.85	74.33**	103.18**	107.50**	107.28**	59.00
0.90	74.32**	103.18**	107.50**	107.28**	59.00
0.95	74.28**	103.18**	107.50**	107.10**	59.49
1.00	74.25**	104.50**	107.50**	106.25**	59.49

Note: This table presents the estimated direct effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Standard errors were calculated by bootstrap with 1000 iterations.
*** indicate statistical significance at 1% level,** 5% level, and * 10% level

Table 12: Average of Indirect Effects Across Quantiles

	All Sample					**25th. Q**				
	Maturity	**Volume**	**Spread**	**FI**	**NFI**	**Maturity**	**Volume**	**Spread**	**FI**	**NFI**
Maturity		-0.12	-0.10	0.91	1.35		0.00	0.00	0.75	0.00
Volume	0.08 **		0.06 **	0.45	0.08	-0.02 **		-0.05 **	-0.56	0.25
Spread	0.02 **	0.00 **		-0.07 **	-0.02 **	0.20 **	0.19 **		0.13 **	0.00 **
FI	0.09**	0.11**	0.02**			0.00	0.00	0.00		
NFI	0.00	0.00	0.00			0.03	0.04	0.00		
	50th. Q					**75th. Q**				
	Maturity	**Volume**	**Spread**	**FI**	**NFI**	**Maturity**	**Volume**	**Spread**	**FI**	**NFI**
Maturity		0.00 **	0.18 **	-32.42	0.00 **		0.00	2.26	0.87	0.00
Volume	-0.72 **		-0.96 **	0.33	-0.13	0.14 **		0.07 **	0.30	0.00
Spread	0.10 **	0.01 **		0.01 **	0.00 **	-0.04 **	-0.03 **		0.44	0.34
FI	-0.32	-0.32	-0.95			0.08**	-0.04**	-0.05**		
NFI	-0.06	-0.05	-0.03			-0.03	0.00	0.00		
	90th. Q									
	Maturity	**Volume**	**Spread**	**FI**	**NFI**					
Maturity		0.08	0.18	-0.26	0.00					
Volume	-0.32		-0.15	2.79	0.00					
Spread	0.90	1.27		0.27	0.05					
FI	-0.02	-0.05	-0.01							
NFI	0.00	0.00	0.00							

Note: This table presents the estimated average indirect effects of the intervention of Sociedad Hipotecaria Federal on the lending market Sofomes and Sofoles used to fund their activities. Standard errors were calculated by bootstrap with 1000 iterations.
*** indicate statistical significance at 1% level,** 5% level, and * 10% level