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# Does Public Financing Support Increase Exports? Evidence from a Quasi-Experiment at the US Export-Import Bank

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# Does Public Financing Support Increase Exports? Evidence from a Quasi-Experiment at the US Export-Import Bank

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**Abstract**

This paper estimates the causal effect of public financing support on exports using a large and plausibly exogenous shock to the supply of export financing support due to the shutdown of the U.S. Export-Import (ExIm) Bank in 2015. I utilize this unique quasi-experiment together with the synthetic control method to estimate that the average affected industry experienced a reduction in exports by 2.2%, or 56 cents per dollar of lost support. While these results suggest that support by the ExIm bank can be an effective policy tool to relax financing constraints and promote exports, the observed allocation of financing support across industries is suboptimal if the goal is employment growth as better targeting could create an additional 66,000 export-related jobs per year.

*JEL classification:* F13, F14, H81

*Keywords:* US Export-Import Bank, Synthetic Control Method, Export Promotion, Policy Evaluation

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# 1 Introduction

In 2020, the U.S. Export-Import (ExIm) Bank provided exporting firms with over $5.4 billion of public financing support in the form of loans, guarantees and insurance policies (ExIm, 2021). Similar official export credit agencies (ECAs) exist in countries all over the world, and together they support more than $160 billion in global trade flows annually.[1] It is, however, unclear whether government-backed export financing support is an effective policy to increase exports and promote high-paying export-related jobs, as alternative financing methods are available for exporters that do not involve ECAs (Antras & Foley, 2015), and the empirical evidence on the effectiveness of public financing support is surprisingly scarce.

This paper addresses the question of whether public financing support for exporters actually causes a significant increase in exports by using a large reduction in financing support after the shutdown of the ExIm bank in 2015 as a quasi-experiment. By comparing exports of industries that relied heavily on ExIm support to similar industries that never received any ExIm support and were thus arguably unaffected by the shutdown, I estimate that the average treated industry reduced its exports by 2.2% relative to similar but unaffected control industries. Thus, I find a statistically significant positive relationship between public financing support and exports. The associated multiplier effect of 56 cents in additional exports per dollar of authorized ExIm support is economically significant, but it is smaller than the multiplier found in previous studies that analyzed the effectiveness of public financing support.[2] I use this multiplier to estimate that the ExIm bank shutdown led to an annual loss of 13,800 export-related jobs.[3]

Importantly, without an exogenous shock to the supply of public financing support, any observed correlation between export financing and exports may be upward biased due to the mechanical relationship between exports and demand for export financing, which depends on potentially unobservable variables such as productivity or preference shocks. By using a large

[1]According to data from the 2021 annual report of the International Union of Credit and Investment Insurers ("Berne Union"), a group that includes ECAs from over 70 countries.

[2]The studies discussed below found multipliers between 1.09 and 2.8.

[3]Using industry-specific multipliers, I also show that more efficient targeting of financing support could help create an additional 60,000 jobs in industries where it is most effective.

export financing supply shock that is plausibly exogenous because it is the result of political gridlock in the U.S. Congress, I can estimate a causal treatment effect that is not biased by reverse causality or omitted variables. In contrast, previous studies on the effectiveness of export financing have used panel-data regressions based on the gravity approach and found sometimes large, positive correlations between public financing support and exports, without establishing a causal link between the two (see Egger & Url, 2006; Moser *et al.*, 2008 for Austria; Felbermayr & Yalcin, 2013, for Germany). Agarwal & Wang (2018), the only other authors looking at the effectiveness of the ExIm bank in promoting exports, find a small positive correlation between ExIm support and exports, but only for large businesses that are not in the aerospace industry.[4]

I also document two new stylized facts about financing support by the U.S. Export-Import Bank: (1) ExIm support is highly concentrated on a few industries at a narrowly defined NAICS-4 level; and (2) the industries that received the vast majority of ExIm support before the shutdown were, on average, not significantly more financially constrained than industries that received little or no support. While the first stylized fact is crucial to my identification strategy, which divides industries into those affected by the ExIm shutdown and those that are not, the second fact provides a possible explanation for why the estimated treatment effects of the ExIm shutdown are generally small and statistically insignificant for several industries. It also suggests that relaxing financing constraints might not be the main driver of either the demand or the supply of ExIm support, and that other variables such as market power, industrial policy or political capture might be important in determining which industries receive the most support as well.

This paper contributes to a large and growing literature that studies the effects of financial frictions and financing constraints on international trade flows.[5] Firms that engage in international trade have higher financing needs than firms that operate only domestically, as they face longer shipment lags, higher initial fixed costs to access foreign markets, and additional risks

[4]The results in this paper are qualitatively in line with Agarwal & Wang (2018), as my estimation strategy uses changes in financing support that affected mostly large businesses, and I also find no significant effect for the aerospace industry, which has been one of the largest beneficiaries of ExIm support.

[5]See Foley & Manova (2015) for a survey of this literature.

such as currency risks or political uncertainty. Chaney (2016) develops a heterogeneous-firm trade model in which potential exporters with high productivity but low assets can become collateral-constrained and are prevented from entering foreign markets. Leibovici (2021) analyzes a multi-industry general equilibrium trade model with financial frictions and shows that those frictions are more likely to become binding constraints in capital-intensive industries, which thus benefit most from financial development. Feenstra *et al.* (2014) and Ahn (2020) show how financing constraints can arise when private lenders have incomplete information about foreign borrowers and screening is costly. Empirically, it has been shown that financial constraints can reduce exports at the firm level (Manova *et al.*, 2015) and at the industry level (e.g. Manova, 2013), and that financial frictions have been an important factor in the "great trade collapse" after the financial crisis (e.g.; Amiti & Weinstein, 2011; Chor & Manova, 2012). More closely related to this paper, Niepmann & Schmidt-Eisenlohr (2017) analyze the effects of adverse shocks to letters of credit provided by commercial banks and find that they have a negative effect on country-level exports. My paper adds to this literature by providing empirical evidence of the effectiveness of a large government program that is designed to increase exports by reducing financial frictions.

This paper also adds to the large policy evaluation literature that uses quasi-experiments to estimate causal effects of economic policies (Athey & Imbens, 2017). For example, Heilmann (2016) estimates the impact of consumer boycotts on exports using the synthetic control method developed by Abadie & Gardeazabal (2003) and Abadie *et al.* (2010). I extend this method to multiple treated units, following the approach of Galiani & Quistorff (2017). The literature on synthetic controls is growing rapidly, and other recent extensions include the augmented synthetic control method (Ben-Michael *et al.*, 2021a; Ben-Michael *et al.*, 2021b), which accounts for imperfect pre-treatment fit between the treated units and the synthetic control and obtains de-biased treatment effect estimates, and synthetic control using lasso, which uses lasso

[6]Other extensions include the 'doubly robust' synthetic difference-in-differences estimator which adds a unit fixed effect to the standard synthetic control estimator (Arkhangelsky *et al.*, 2019) and the penalized synthetic control estimator which avoids multiple solutions to the optimal weights problem in high-dimensional settings (Abadie & L'Hour, 2021)

regression to automate covariate selection when the number of covariates is potentially large (Hollingsworth & Wing, 2020). I implement both of those extensions, and find that my main results are fairly robust to the choice of a particular synthetic control estimation method.[6]

Finally, by analyzing the effects of a government program that mostly benefits large exporters in just a few industries, this paper speaks to the renewed debate about the desirability of industrial policy (e.g., Rodrik, 2008; Aghion *et al.*, 2015). It also sheds new light on the potential economic consequences of rising political polarization and decreased bipartisanship in the U.S., which potentially led to the shutdown of a successful government program that used to have bipartisan support, ultimately resulting in a significant decrease in exports and a loss of export-related jobs. Thus, while Dorn *et al.* (2020) show that exposure to import competition can lead to increased political polarization, my results provide an example of how political polarization can also have significant effects on trade flows.

The remainder of the paper is structured as follows: Section 2 discusses institutional details about the ExIm Bank and the shutdown after a failed reauthorization in 2015. Section 3 explains the empirical methodology and gives an overview of the data, and Section 4 shows my main results, including heterogeneous treatment effects and implied job loss estimates due to the shutdown. Section 5 shows the results of various robustness checks and Section 6 concludes.

# 2 Background of the US Export-Import Bank

## 2.1 Structure, Goals and Instruments

Founded in 1934, the US Export-Import Bank (ExIm bank or ExIm) is the official export credit agency (ECA) of the United States. It is an independent, self-funding, government owned corporation that provides financing and insurance in order to facilitate the export of goods and services produced in the US and to support US jobs. The ExIm Bank is able to fund itself, and turn a profit, by charging interest rates, insurance premia and other fees on the services it provides.[7] The stated goal of the bank is to step in in cases where private lenders are unwilling or unable to provide financing for US exports, and in order to 'level the playing field' in the face of competition from foreign ECAs. In order to do this, ExIm offers four main services: insurance for exporters against buyer nonpayment, working capital loan guarantees for exporters, loan guarantees for foreign buyers of US exports and direct loans to foreign buyers. *Insurance* is the most common policy, and is mostly used by small businesses. When an exporting firm purchases an insurance policy, ExIm promises to cover up to 95% of the exporter's foreign receivables, in case a foreign buyer defaults. This added security allows U.S. exporters to sell on open account credit terms (instead of requiring cash-in-advance from their foreign buyers), which should make purchasing US goods more attractive to foreign customers. Insurance policies are almost always short term (one year), and can either be tied to a specific foreign customer, or can apply to foreign receivables from multiple countries.[8] Under a *working capital loan guarantee*, ExIm backs a working capital loan given by a commercial bank to a U.S. exporter. If the exporter cannot repay the loan, ExIm covers 90% of it and repays the lending bank. This added security for the lender should enable US exporters to secure working capital loans to better conditions (given the same amount of collateral), which then can be used to pay a variety of export-related costs (materials, equipment, supplies, labor, and other inputs). Working capital loan guarantees are usually short-term, are mostly given to small business exporters, and are never connected to a specific importer country. *Loan guarantees* are extended to foreign buyers for purchases of U.S. capital goods and services, and are not offered for purchases of consumer goods. A loan guarantee enables the foreign buyer to take out a loan in order to pay the US exporter at the time of shipment, and it guarantees the lender repayment of 85% of the loan in case the borrower defaults.[9] The foreign buyer can be a private or public sector company, and guaranteed loans can be medium or long-term (usually up to 10 years). Loan guarantees can be

[7] The ExIm Bank emphasizes that it has generated more than $9.5 billion for the U.S. Treasury for repayment of U.S. debt since 1992, and expects to "provide $228.0 million toward debt reduction" in fiscal year 2022 (ExIm, 2021).

[8] In my dataset spanning from 2007 to 2020, around one quarter of insurance policies is tied to a single importer, while the rest is insuring receivables from multiple importer countries.

[9] ExIm requires a 15% upfront down payment from the buyer.

purchased for exports to most countries, and ExIm charges an exposure fee based on importer country risk, in addition to the interest rate negotiated between borrower and lender. Exports to a small number of countries, and exports of military or defense products and services, are not eligible for this policy. Loan guarantees make up the vast majority of ExIm's portfolio exposure in terms of dollar value, and are most often benefitting large exporting firms. Finally, ExIm sometimes provides *direct loans* to foreign buyers of U.S. exports. The benefits of this policy for the exporting firm are very similar to those of loan guarantees (i.e. payment at the time of shipping), with the main difference being that no intermediate lender is involved. Direct loans are the only ExIm policy that does not use an intermediate lender or financial institution, and ExIm usually reports the interest rate it charges on those loans. Like loan guarantees, direct loans usually benefit large exporting firms, and are most often long-term. Both direct loans and loan guarantees usually require approval from the ExIm board of directors, and are only approved subject to certain economic impact policies and environmental effect policies, which do not generally apply to insurance policies and working capital loan guarantees.[10] The average contract length for ExIm support across all policies is 380 days.

## 2.2 The 2015 ExIm bank shutdown

As an independent government corporation, ExIm's operations are pursuant to a charter that needs to be periodically reauthorized by congress. The charter, among other things, specifies the maximum exposure cap for ExIm's portfolio in any given year.[11] Historically, most authorization periods lasted anywhere between 3 and 6 years (Akhtar, 2014). However, during a time of divided government and political gridlock, congress did not vote on ExIm reauthorization before its charter expired on July 1st 2015. This resulted in ExIm being completely closed for new business, until a new reauthorization agreement was reached on December 4th 2015.[12] In

[10]For example, economic impact policies dictate that ExIm should not finance foreign purchases of capital goods, if it would expand foreign production capacity to such an extend that the foreign customer would significantly increase competition for US firms in the same industry. Environmental impact policies direct ExIm to put a special focus on exports in the renewable energy sector, in particular to Sub-Saharan Africa.

[11]As shown in Akhtar (2014), the exposure cap is usually increased over time, but only has a very loose correlation with the actual exposure every year. At least since 1997, the exposure cap has never been binding.

[12]The 5 month closure was the longest in ExIm history, see Akhtar (2014).

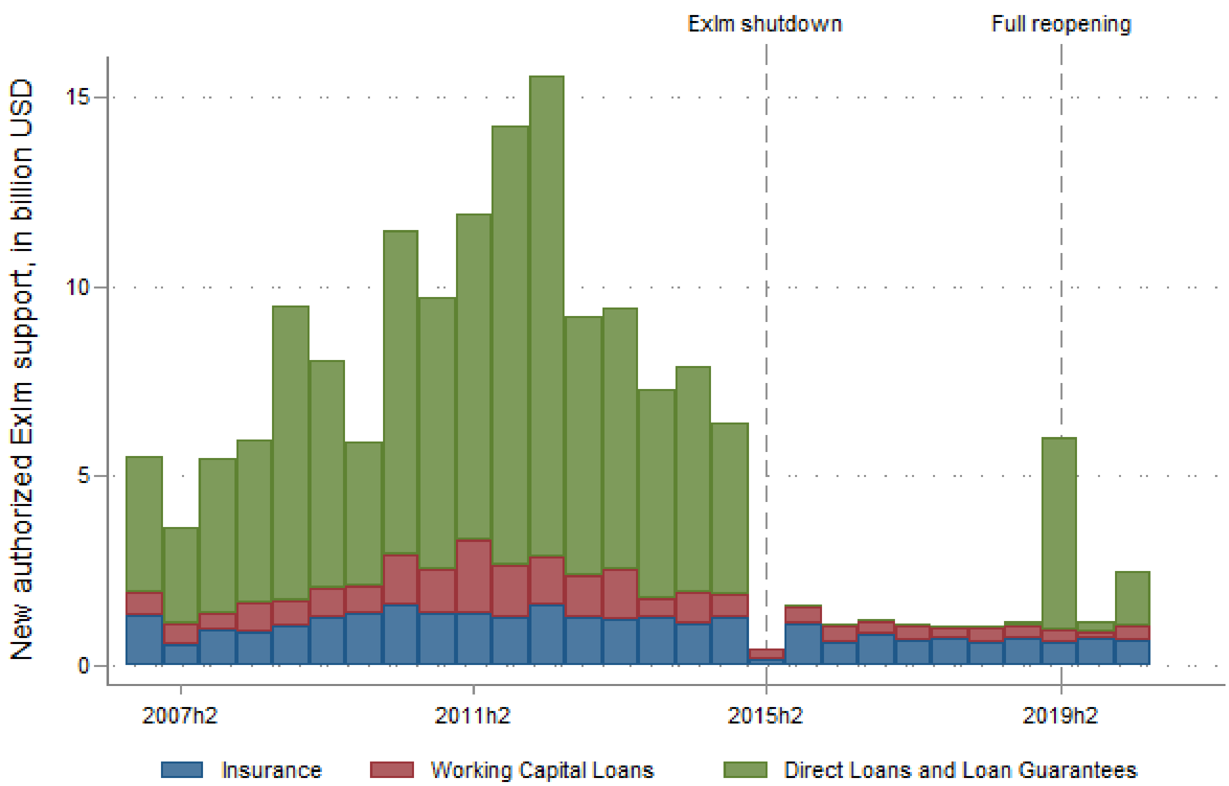
addition to the complete shutdown, the ExIm Bank also lost the three-person quorum on its five-person board of directors, when two board members' terms expired in June 2015 and no new nominees were being approved by the Senate. Among other things, the board quorum loss meant that ExIm was not able to provide long-term financing support for transactions worth more than $10 million, which need approval from the board of directors, and made up the vast majority of ExIm support in terms of dollar value before the shutdown. As a result of the shutdown and in particular the loss of quorum, financing support provided by ExIm, which covered around 2% of all US exports before 2015, decreased by more than 95% after 2015. Supporters of ExIm argued that this decline in support would reduce the competitiveness of American exporters, and ultimately lead to a loss of exports and export-related jobs (Cameron, 2015; Holland, 2015; Hopewell, 2017). Affected businesses, such as Boeing and General Electric, reported loosing large export contracts as a direct result of the Ex-Im shutdown, and threatened to shift production overseas (Radelat, 2015; Shalal, 2015). The ExIm Bank itself estimates that it supported over 100,000 fewer jobs in 2016 compared to 2014 (ExIm, 2016). On the other hand, opponents argued that the ExIm bank shutdown was unlikely to decrease overall exports, and that ExIm financing support is generally akin to 'picking winners' and is only redistributing profits and jobs to politically well-connected firms without affecting total exports(e.g. Katz, 2014).

The lack of quorum on the board of directors lasted until July 2019, when 3 new board members were confirmed by the Senate and the board met again for the first time in almost 5 years. The shutdown from July to December 2015, and in particular the lack of board quorum from July 2015 until July 2019, led to a dramatic decline in ExIm support for US exporters and their foreign customers, as shown in Figure 1.[13] While all forms of ExIm support were reduced after the shutdown, it can be seen that the majority of the decline is due to a reduction in direct loans and loan guarantees, as those financing instruments most often need approval by the board of directors. It is this decline in financing support shown in Figure 1 that I use as a

[13]To simplify the exposition, in the following I will loosely refer to both the actual shutdown that lasted from July 2015 to December 2015 and the lack of quorum that lasted from June 2015 to December 2019, as the 'ExIm shutdown'.

quasi-experiment in order to identify a causal effect of public financing support on exports.

**Figure 1:** ExIm support declined significantly after the shutdown, mostly for loans and loan guarantees




Note: New authorized ExIm support is the total value of the loan, either directly provided or guaranteed by ExIm bank (in case of working capital loans, direct loans and loan guarantees), or the value of the foreign receivables insured (in case of insurance). Each authorization is only counted once at the beginning of the authorization period. Data is aggregated over all industries for 6-month periods.

# 3 Methodology

## 3.1 Empirical Model

In order to analyze the effect of public financing support from the US Export-Import Bank on exports, the simplest approach would be to estimate the following empirical model (Agarwal &

Wang, 2018):

$$ln(Y_{it}) = \beta\ ln(S_{it}) + \gamma' X_{it} + \delta' Z_{it} + \mu_i + \mu_t + \epsilon_{it} \tag{1}$$

where $Y_{it}$ denotes the current US dollar value of exports and $S_{it}$ is the dollar value of authorized ExIm support in (4-digit NAICS) industry $i$ and quarter $t$. $X_{it}$ is a vector of potentially observable control variables, such as industry size, productivity, price levels and trade cost, while $Z_{it}$ is a vector of fundamentally unobservable variables that might affect exports, such as changes in preferences that change industry-level demand.[14] The $\mu's$ are industry and quarter-fixed effects that capture the impact of unobserved variables that are either time or industry-invariant, and $\epsilon_{it}$ is the error term. The main variable of interest is the coefficient $\beta$, which can be interpreted as the industry-level elasticity of exports with respect to credit support from the ExIm Bank.[15]

However, estimation of this standard gravity regression as done in previous studies is likely to lead to biased estimates of $\widehat{\beta}$ due to two related reasons: (i) omitted variables and (ii) reverse causality. From equation (1), it is apparent that not including the unobserved variables in $Z_{it}$ will lead to an omitted variable bias on $\beta$ if those variables are correlated both with exports ($X_{it}$) and with ExIm support ($S_{it}$), that is if $E[X'Z|X]\delta' \neq 0$. Two examples of such omitted variables might be industry-level productivity and industry-level demand. A positive shock to either variable would increase exports in a given year, while also potentially increasing the demand for credit support by the ExIm Bank for those new exports. Assuming that there is some positive probability that ExIm authorizes additional support, this would lead to an omitted variable bias as explained above. Relatedly, a second problem is the possibility of reverse causality, that is, the fact that higher exports lead to higher levels of ExIm support,

[14] In practice, due to data availability issues, some elements of $X_{it}$ such as prices or productivity might actually be unobserved and thus included in $Z_{it}$.

[15] The empirical model presented so far can also be derived from an industry level version of the standard gravity model that is very common in the trade literature (e.g. Anderson & Van Wincoop, 2003 and Head & Mayer, 2014), augmented with a term for ExIm financing support. Similar models have been used to analyze the effectiveness of some export credit agencies (see Egger & Url, 2006, Felbermayr & Yalcin, 2013, and Agarwal & Wang, 2018). In Appendix C, I analyze variants of the conventional gravity regression approach and show that the results are largely in line with the previous literature.

rather than vice versa. Thus, unless the supply of ExIm support is fully inelastic, an exogenous increase in exports would mechanically lead to an increase in ExIm support and thus a positive estimate $\widehat{\beta}$, but not because ExIm support causes exports. In both cases, the endogeneity bias that overestimates the effect of ExIm support on exports arises because the observed value of ExIm support is mostly driven by the demand from exporters, which naturally depends on the observed value of exports, rather than the supply that is set by the ExIm Bank itself.[16]

## 3.2 The Synthetic Control Method

To eliminate the endogeneity bias, my estimation strategy uses the ExIm reauthorization lapse in 2015 as a natural experiment to estimate the following empirical model instead of equation (1):

$$Y_{it} = \beta \ S_{it}^{treat} + \gamma' X_{it} + \delta' Z_{it} + \mu_t + \epsilon_{it} \tag{2}$$

where variables are defined as in (1), but $S_{it}^{treat}$ is a treatment indicator that equals 0 for all industries before the ExIm shutdown in the third quarter of 2015, and 1 afterwards for industries that relied disproportionally on ExIm support before 2015. The idea behind this specification is that industries that received more ExIm support prior in the pre-treatment period were "treated" by the reduction in ExIm support after 2015, while industries that never received any ExIm support (or only diminishingly small amounts) were unaffected, and thus serve as controls. Hence, if ExIm support has a positive effect on exports, I would expect the coefficient of interest $\beta$, which estimates the average treatment effect on the treated (ATT), to be negative. But since $Z_{it}$ is unobserved and time-varying, a simple difference-in-difference estimation of (2) might still yield biased results. I thus use the synthetic control method in order to construct a counterfactual time series of exports by choosing weights on observable controls and observable pre-treatment outcomes (i.e. exports) that minimize the difference between pre-treatment outcomes for the treatment and the control group. In a sense, this ensures that the

[16] As shown in Akhtar (2014), the maximum exposure cap for ExIm was not reached in any single year after 2007, indicating that the supply of ExIm support is in fact not the constraining factor.

"equal-pre-trends" assumption holds, and that any deviation in trends in the post-treatment period can be interpreted as the causal treatment effect.

Formally, the synthetic control estimator of the treatment effect for treated industry $i$ at time $t$, using industries from donor pool $C$, is calculated as

$$\widehat{\beta}_{it} = Y_{it} - \sum_{c\ \in C} w_i^* Y_{ct} \quad , \quad \forall t > 2015q2 \tag{3}$$

$$W_i^* = \arg\min_{W_i} \sqrt{(M_i - M_c W_i)' V_i (M_i - M_c W_i)} \tag{4}$$

where $M$ is a matrix that stacks pre-treatment outcomes $Y$ and observable control variables $X$, $W$ is a vector of donor weights that reflects how much weight is put on each control unit from the donor pool to construct the synthetic control unit, and $V$ is a matrix of predictor weights that reflects the relative importance of each predictor variable (including pre-treatment outcomes) in calculating the synthetic control. Because the ultimate goal of the synthetic control method is to predict a counterfactual trend of the outcome variable after the treatment period, I use a cross-validation approach to construct weights that perform well out-of-sample, as recommended by Hollingsworth & Wing (2020). More precisely, I divide the pre-treatment period into a training and a validation period, similar to popular machine learning methods. Then, for a given $V$ and predictor values in the training period, $W$ is chosen such that the weights minimize the deviations in outcome variables and predictors between the treated unit and the synthetic controls in the validation period. While the predictor weight matrix $V$ could in principle be provided by the researcher based on beliefs about the relative importance of each predictor, I follow the data-driven approach in Abadie *et al.* (2010) and find the $V$ that minimizes the root mean-squared prediction error (RMSPE) in the validation period.[17] It is important to note the traditional synthetic control method estimator restricts the elements of both weight matrices to be non-negative and sum up to equal one, which guarantees that there is no extrapolation beyond the support of the donor units. As argued in Abadie (2021), this

[17] I also consider the case where $V$ is set to 0 for each predictor variable except for previous outcomes, which is equivalent to using the Synthetic Control Method without covariates.

restriction increases the transparency of the synthetic control method, as the synthetic control unit can easily be interpreted as a weighted average of the control units in the donor pool. This is in contrast to regression-based approaches, which achieve a perfect pre-treatment fit by (implicitly) constructing weighted averages with possibly negative donor and predictor weights that lie outside of the support of the donor pool. This overfitting of the data is likely to lead to excellent in-sample performance at the cost of poor out-of-sample performance, which is why I focus on the traditional synthetic control estimator with non-negative weights below.[18]

As shown in Abadie & Gardeazabal (2003), when the number of pre-treatment periods is large, $\widehat{\beta}_{it}$ converges towards $\beta_{it}$, even if there are unobserved time-varying and industry specific shocks in $Z_{it}$. Formally, the identifying assumption for unbiased estimation of the treatment effect is "independence conditional on past outcomes,"

$$Y_{it}^{0} \perp S_{it}^{treat} | (X_{it}, Y_{it}^{0,pre}) \tag{5}$$

where $Y^0$ is the potential outcome in the absence of the treatment (see O'Neill *et al.*, 2016). One challenge with the synthetic control method is the inability to calculate standard errors used for statistical inference, since each treatment effect is only based on the difference between the outcomes for one treated unit and a single synthetic control. It is standard in the literature to conduct permutation-based inference by using placebo tests (Abadie *et al.*, 2015; Abadie, 2021). More specifically, after computing the actual treatment effect $\widehat{\beta}_{t}^{TR}$ for a treated industry and for each post-treatment period, I also compute placebo treatment effects $\widehat{\beta}_{it}^{PL}$ for each unit in the donor pool, where the weights of the respective synthetic control units are again optimally chosen as explained above. I then report the proportion of placebo treatment effects that are

[18] As robustness checks, I also consider regression based approaches to find the weight optimal weights, such as synthetic control using lasso (Hollingsworth & Wing, 2020) and augmented synthetic control using ridge regression (Ben-Michael *et al.*, 2021a). These approaches try to find the optimal trade-off between in-sample and out-of-sample performance by allowing for a limited degree of extrapolation, governed by a penalization parameter $\lambda$.

larger than the actual treatment effect as a p-value.[19]

$$p_t = Pr(\widehat{\beta}_{it}^{PL} > \widehat{\beta}_{t}^{TR}) \tag{6}$$

To take into account that some placebo estimates might have a bad pre-treatment fit and should thus be less reliable in terms of estimating the true treatment effect, I normalize the average treatment effect by the pre-treatment root mean squared prediction error (RMSPE), which measures the goodness-of-fit between the treated unit and the synthetic control. I then report p-values for this 'studentized' treatment effect following the same logic as above. Both the simple and the studentized p-values are time-specific. In order to calculate a summary statistic that measures the divergence between the treated unit and the control for the full post-treatment period, I follow Abadie *et al.* (2010) and also calculate the root mean squared prediction errors for the post-treatment period. As above, the p-value for this test statistic is the proportion of placebo tests that have a higher RMSPE ratio than the treated unit. However, in order to account for pre-treatment fit, I also compute the *standardized p-value* as the ratio of the post-treatment RMSPE to the pre-treatment RMSPE:

$$RMSPE_{post} = \left( \frac{1}{T - T_0} \sum_{t=T_0}^{T} \left(\widehat{\beta}_{it}\right)^2 \right)^{\frac{1}{2}} \tag{7}$$

$$RMSPE_{pre} = \left( \frac{1}{T_0} \sum_{t=0}^{T_0} \left(\widehat{\beta}_{it}\right)^2 \right)^{\frac{1}{2}} \tag{8}$$

$$p = Pr(RMSPE_{post}^{PL} > RMSPE_{post}^{TR}) \tag{9}$$

$$p^{std} = Pr\left( \frac{RMSPE_{post}^{PL}}{RMSPE_{pre}^{PL}} > \frac{RMSPE_{post}^{TR}}{RMSPE_{pre}^{TR}} \right) \tag{10}$$

Traditionally, the synthetic control method has been applied to the case of a single treated unit, but it is readily extended to the case of multiple treated units. I follow Cavallo *et al.* (2013) and Galiani & Quistorff (2017), and first calculate the treatment effect separately for all treated units and all time periods, after scaling exports for each industry so that it equals 1 in the last

[19] This is sometimes referred to as classical randomization inference. For example, a p-value below 0.05 would indicate that less than 5% of placebo treatment effects are larger than the actual treatment effect.

pre-treatment period. I then take the average effect across industries, and apply the inference procedure described above using permutations of 10 randomly selected placebo units from the donor pool.[20][21]

One potential shortcoming of the approach outlined above is that the pre-treatment fit between the treated units and the synthetic control may be less than perfect, in particular in a setting with relatively few pre-treatment periods and many predictor variables (or covariates), due to the 'curse of dimensionality' (Ferman *et al.*, 2020). Thus, in practice researchers often default to visual inspection of the pre-treatment fit to argue that the trends are 'close enough', and Abadie *et al.* (2015) recommend against using the synthetic control method when the pre-treatment fit is poor or the number of pre-treatment periods is small. However, recent work by Ben-Michael *et al.* (2021a) shows that it is possible to correct the bias that arises due to imperfect pre-treatment fit. Their augmented synthetic control method estimates the bias in the treatment effect estimate using ridge regression with a penalization parameter for extrapolation, and then uses those estimates to improve the pre-treatment fit and de-bias the SCM treatment effect estimates. Ben-Michael *et al.* (2021a) use Monte-Carlo simulations to show that their approach reduces bias for a variety of data-generating processes and outcome models, in particular in settings with additional covariates and poor pre-treatment fit.

## 3.3 Data

The ExIm Bank publishes a periodically updated transaction-level dataset, publicly available on `exim.gov`. The dataset contains all 42,522 support authorizations made between 2007 and 2020. The original dataset reports the industry classification of the exporting firm using either SIC or NAICS codes, and I convert the SIC codes to 4-digit NAICS codes using the crosswalk from Autor *et al.* (2013). I aggregate the dataset by quarter and 4-digit NAICS industry, and match it to trade flows (obtained from `https://usatrade.census.gov/`) and other control

[20] The actual number of possible placebo treatment effects is very large, since it equals the number of possible combinations of 10 units from a donor pool of 47 units. My inference is based on 1,000,000 random draws of 10 units from the donor pool.

[21] An alternative inference method for synthetic control estimates with multiple treated units is the rank-based inference suggested by Dube & Zipperer (2015)

**Table 1:** ExIm coverage rate for treated industries

| NAICS4 | Industry | Coverage Rate (in %) |
|---|---|---|
| 3324 | Boilers, Tanks & Shipping Containers | 14.1 |
| 3364 | Aerospace Products & Parts | 7.8 |
| 3365 | Railroad Rolling Stock | 4.1 |
| 3336 | Engines, Turbines & Power Transmission Equipment | 2.7 |
| 3212 | Veneer, Plywood & Engineered Wood Products | 2.6 |
| 3331 | Agricultural & Construction Machinery | 1.6 |
| 3332 | Industrial Machinery | 1.6 |
| 3342 | Communications Equipment | 1.6 |
| 3366 | Ships & Boats | 1.3 |
| 3339 | Other General Purpose Machinery | 1.1 |

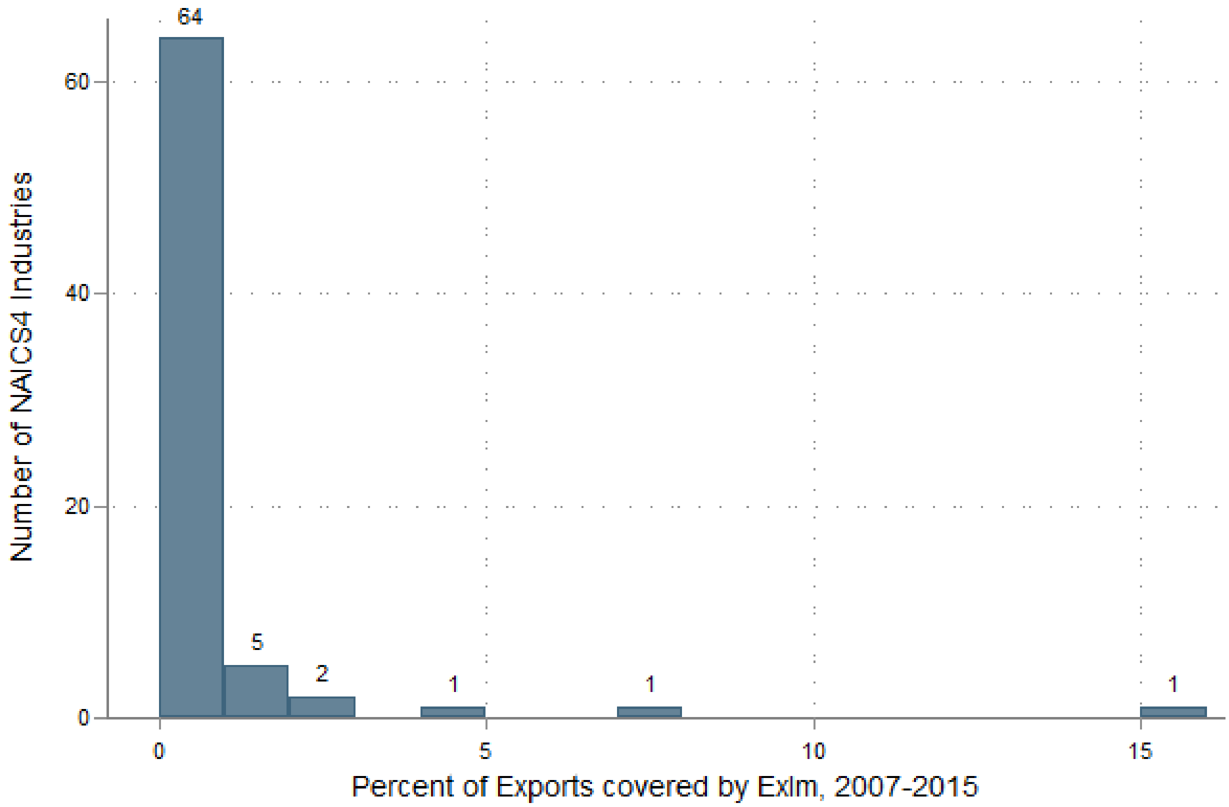
Note: Coverage rate is calculated as the total amount of ExIm support (authorized with board approval) received between 2007 and 2015, divided by total exports for the same time period. Treated industries are those industries with above-average coverage rate.

variables (obtained from CEPI (see Head *et al.*, 2010 and the WDI database). Importantly for my identification strategy, ExIm authorizations vary considerable by industry, and are highly concentrated on just a few 4-digit NAICS industries.[22] In Figure 2, I show that most industries received very little ExIm support, with coverage rates, i.e. the share of exports supported by ExIm, between 0% and 1%, while a few industries saw a larger percentage of their exports supported by ExIm. Table 1 shows the coverage rates for those industries that have an above average coverage rate. These 10 industries, which account for roughly 21% of all exports in the dataset, received over 94% of all ExIm support. While the average coverage rate for those industries is 3.9%, the average coverage rate for the 64 industries with below-average coverage is merely 0.05%.

The highly concentrated distribution of ExIm support is crucial for my identification strategy, as it allows me to divide the dataset into *treated* industries that previously received a significant amount of ExIm support, and thus were potentially affected by the ExIm shutdown, and *control* industries which never received any significant support, and thus were arguably not effected by the shutdown. However, as mentioned above, while the shutdown that lasted from July 2015 to December 2015 closed ExIm for all business, the loss of a board quorum that lasted until 2019

[22]When taking NAICS4 industries as the units of observation, the Herfindahl concentration index for ExIm support is 0.41, indicating a very high level of market concentration.

**Figure 2:** A few industries benefitted disproportionately from ExIm support, while most industries had ExIm coverage rates below 1%




Note: Percent of Exports covered by ExIm is calculated as the total amount of ExIm support (with board approval) received between 2007 and 2015, divided by total exports for the same time period. 47 industries have a coverage rate of 0%, and the average coverage rate is 0.9%.

only prevented ExIm from authorizing large loans and guarantees, that needed board approval, while smaller insurance policies were still being approved. To take this into account, I calculate the ExIm coverage rate, on which the treatment status is based, by using only the part of ExIm support that needed board authorization. More specifically, I define an industry as treated by the ExIm shutdown if this industry had an ExIm coverage rate (in terms of board-approved ExIm authorizations) in the period before the ExIm shutdown (2007-2015:Q2) that was higher than the average coverage rate of 0.9%. Conversely, I include all those industries that received no board-approved ExIm support before 2015 in the donor pool of untreated industries, from which the control units are collected. Finally, I discard industries that received below-average

**Table 2:** Balance table

| Variable | (1) Control | (2) Treated | (3) Difference | (4) P-value |
|---|---|---|---|---|
| Exports (mil. USD) | 2,037.385 | 5,570.881 | 3,533.496 | (0.006)*** |
| Importer credit (% of GDP) | 61.181 | 58.397 | -2.785 | (0.129) |
| Distance (km) | 8,837.884 | 8,980.234 | 142.351 | (0.214) |
| Importer GDP per capita | 16,268.550 | 15,585.271 | -683.279 | (0.186) |
| Asset tangibility | 27.127 | 22.247 | -4.880 | (0.341) |
| Labor productivity | 97.192 | 98.241 | 1.049 | (0.697) |
| Return on assets | 2.327 | 5.624 | 3.297 | (0.607) |
| Leverage | 39.802 | 43.262 | 3.460 | (0.758) |
| Employment | 153.435 | 166.496 | 13.061 | (0.793) |
| Small business share | 98.387 | 97.480 | -0.907 | (0.236) |
| Observations | 47 | 10 | 57 | |

Note: This table shows quarterly averages for the pre-treatment period (2007-2015), at the NAICS4 industry level. Treated industries are defined as industries with above-average ExIm coverage, and control industries are industries with no ExIm coverage.

ExIm support (with board approval), prior to 2015, as those are the industries most likely to switch from larger, board-approved authorizations to smaller authorizations after 2015, which would make them 'contaminated' control units. This leaves me with a dataset of 10 treated industries and 47 potential donor industries, as well as 17 potentially contaminated industries that are excluded from the dataset. I conduct extensive robustness checks to see whether this specific choice of treatment and donor groups affects my results, and the results are generally robust to this choice.[23]

How different are industries in the treated group from industries in the control group? Table 2 shows the pre-treatment means for exports and a number of other covariates that might be correlated with exports and financing constraints, for both the treated and the control group. The covariates I consider are: *employment* and *labor productivity* as measures for industry size and productivity, *distance* and *importer GDP per capita* as measures for trade cost and import demand, computed as output per worker). I also include *asset tangibility* (property, plant and equipment over total assets), *return on assets* (output over total assets) and *leverage* (debt

[23]Because close to 90% of the total dollar value of ExIm support was authorized with board approval, the results are very similar when the treatment status is based on all ExIm support. However, in this case, there are only very few industries which received no ExIm at all and would thus be included in the control group, while there would be many indsutries that received very small amounts of support prior to the shutdown.

over equity) as measures of industry-level credit constraints, as well as *Importer Credit* as measure for importer financial development. Finally, *Small business share* controls for the share of small businesses in an industry, since small businesses are less likely to be exporters, but conditional on exporting might be more likely to be targeted by ExIm. Importer-specific variables (*distance*, *importer GDP per capita*, and *Importer Credit*) are calculated as export-weighted averages using CEPI data, and other industry-specific variables are taken from the BEA and Compustat.[24]

While treated industries have significantly higher exports, for none of the control variables there is a statistically significant difference in means between the treated and the control group. However, the statistical power of the t-test is rather low, due to the small number of observations, and statically significant differences might be hard to detect. Disregarding statistical significance, it can be seen that industries in the treated group export to countries with lower levels of credit, and have themselves lower levels of asset tangibility, higher return on assets and more leverage, indicating that treated industries are potentially more financially constrained. Because most control variables are time-varying and I am interested in changes in the trend of exports, Table A3 in the appendix shows the same balance table as in Table 2, but for differences in mean growth rates instead of levels. Industries in the treatment and control group followed very similar trends in terms of all control variables, but treated industries had export growth rates that were on average 0.6 percentage points lower than industries in the control group, significant at the 10%-level. Thus, while this balance test should be taken with caution due to the small number of industries and accordingly large standard errors, it does indicate that there seems to be no large differences between the treated group and the control group in terms of pre-treatment trends and levels of observables.

### 3.4 Are ExIm-supported industries more financially constrained?

Given the highly skewed distribution of ExIm support across industries, and the theory that ExIm support might alleviate financial constraints, the question remains whether those in-

[24]Compustat variables are calculated as yearly industry medians, as in Manova (2013).

**Table 3:** Probit estimates for the probability of being treated

| | (1) Asset tan. | (2) Ext. Fin. | (3) Tobin's Q | (4) Leverage | (5) ROA | (6) Interest | (7) Assets/Emp. |
|---|---|---|---|---|---|---|---|
| | -1.902*** | -0.015 | -0.275 | -0.016 | 0.262 | -0.351 | -0.148 |
| | (0.702) | (0.017) | (0.190) | (0.018) | (0.596) | (1.017) | (0.127) |
| Observations | 694 | 694 | 690 | 694 | 694 | 693 | 693 |
| Pseudo $R^2$ | 0.110 | 0.084 | 0.088 | 0.081 | 0.082 | 0.082 | 0.085 |

Note: All columns include year fixed effects (not shown), and control for industry output and trade openness. The dependent variable is an indicator for being in the treated group. Clustered standard errors in parentheses.
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

dustries that received the majority of support (i.e. the *treated*) were in fact more financially constrained than those industries that receive little or no support. Even though the simple difference-in-means test above showed statistically insignificant results, this might be due to the low sample size or omitted variables in a simple t-test. Thus, in this section I further analyze whether higher levels of financial constraints make industries more likely to receive large amounts of ExIm support. Table 3 shows estimates from a Probit model where the dependent variable is an indicator that equals 1 if an industry has received ExIm support in a given quarter, and 0 otherwise.[25] The independent variables of interest are different indicators of financial constraints, and each specifications also controls for industry size, trade openness, and year fixed effects.

Out of the six possible indicators that I consider, only asset tangibility has a statistically significant effect on the probability of receiving ExIm support. Industries with a lower share of tangible assets are more likely to receive support, statistically significant at the 1%-level. This is presumably because exporters and importers in those industries can post less collateral when applying for loans, and are thus more financially constrained, and more likely to apply for ExIm loans and loan guarantees. However, overall, there is no strong relationship between the probability of receiving ExIm support and most indicators of financial constraints at the industry level.[26]

[25]For consistency, I only consider ExIm authorizations that were board-approved here, but the results are very similar when all authorizations are considered.

[26]It should be noted that industry-level indicators of financial constraints might be a bad proxy for the financial constraints faced by a specific firm that is supported by ExIm, if financial constraints are very heterogeneous within industries.

# 4 Results

## 4.1 Baseline synthetic control results

Table 4 shows the baseline results for different specifications of the synthetic control method approach, where the treatment period is set to be the third quarter of 2015, and the treated units are the 10 industries with an ExIm coverage rate above 1% prior to 2015. All specifications use the cross-validation method developed in Abadie *et al.* (2010), in which the first half of the pre-treatment period is used as a training period to construct the predictor weights $V$, and the second half is used as the validation period to find the donor weights $W$ that minimizes the mean-squared prediction error between the treated units and the synthetic controls, given $V$. The benefit of dividing the pre-treatment period into a training and a validation period is that it provides out-of-sample validation for the chosen weights, using actually observed data for both the treatment and the controls units. Columns 1-3 show results for specifications that include the full set of covariates or predictor variables (in addition to pre-treatment exports) when calculating the control unit weighting matrix $W$. Columns 4-6 only match on pre-treatment exports, as suggested by Ferman *et al.* (2020). Matching on pre-treatment covariates other than pre-treatment outcomes has the benefit of reducing the likelihood of overfitting exports by making the control unit weights more sparse (Abadie, 2021). At the same time, including additional covariates induces the risk of cherry-picking and specification searching, as there are no clear guidelines for choosing the 'right' set of covariates that should be included Ferman *et al.* (2020). I take a pragmatic data-driven approach by reporting results for the extreme cases of all covariates and no covariates, and later show robustness checks that include results for all possible combinations of my covariate list. It should also be noted that all of my specifications belong to the class of SCM estimators in which the number of pre-treatment outcomes converges to infinity when the number of pre-treatment period goes to infinity. As shown in Ferman *et al.* (2020), these specifications are asymptotically robust to specification searching. Furthermore, the specifications that do not include any covariates in Table 4, Columns 4-6, address, at least to some extent, the issue of the numerical instability of the synthetic control estimator with included covariates, that was raised by Kuosmanen *et al.* (2021). They show that the true synthetic control estimator often converges to a corner solution that puts all weight on a single predictor, rendering all other predictors redundant. It can be seen in Columns 4-6 that not including any covariates, which leaves no room for multiple solutions, does not signficantly change the results.

**Table 4:** Combined treatment effects for 10 treated industries

| | All Covariates | | | No Covariates | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Baseline | No Aerospace | Low treated | Baseline | No Aerospace | Low treated |
| ATT | $-0.022^{**}$ | $-0.024^{**}$ | $-0.021^{**}$ | $-0.024^{**}$ | $-0.026^{***}$ | $-0.026^{**}$ |
| P-value | (0.028) | (0.015) | (0.028) | (0.018) | (0.008) | (0.011) |
| RMSPE | 0.014 | 0.015 | 0.014 | 0.015 | 0.015 | 0.015 |

Note: The ATT is calculated as the average difference in exports between treated industries and their synthetic controls, after 2015: Q2. Exports are scaled to equal 1 in 2015: Q2. P-values are calculated as the percentage of placebo tests that has a higher ratio of post-RMSPE to pre-RMSPE, averaged over all treated units. Columns 2 and 4 exclude the aerospace industry, and columns 3 and 6 add industries with coverage rates below 1% but above the median to the treated group. Columns 1-3 include all 9 covariates listed in Table 2, while Columns 4-6 only match on lagged exports. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Because the aerospace industry might be considered an outlier which received very large amounts of ExIm support for political reasons (i.e. the subsidy conflict between Boeing and Airbus), in Columns 2 and 5 I report results that exclude this industry from the list of treated industries. This slightly increases the treatment effect estimate and lowers the p-value significantly, which indicates that the aerospace industry reduced exports by less than the average treated industry after 2015. Finally, because the 1% coverage rate cutoff for treated industries could be considered arbitrary, in Columns 3 and 6 I include industries with lower treatment intensity, that is, industries that had an ExIm coverage rate smaller than 1% but larger than the median industry in the group of treated industries.

Table 4 reports the average treatment effect, which is the simple average of the quarterly treatment effect (difference between treated and synthetic control unit outcome) for the entire post-treatment period, averaged over all treated industries.[27] The average quarterly treatment

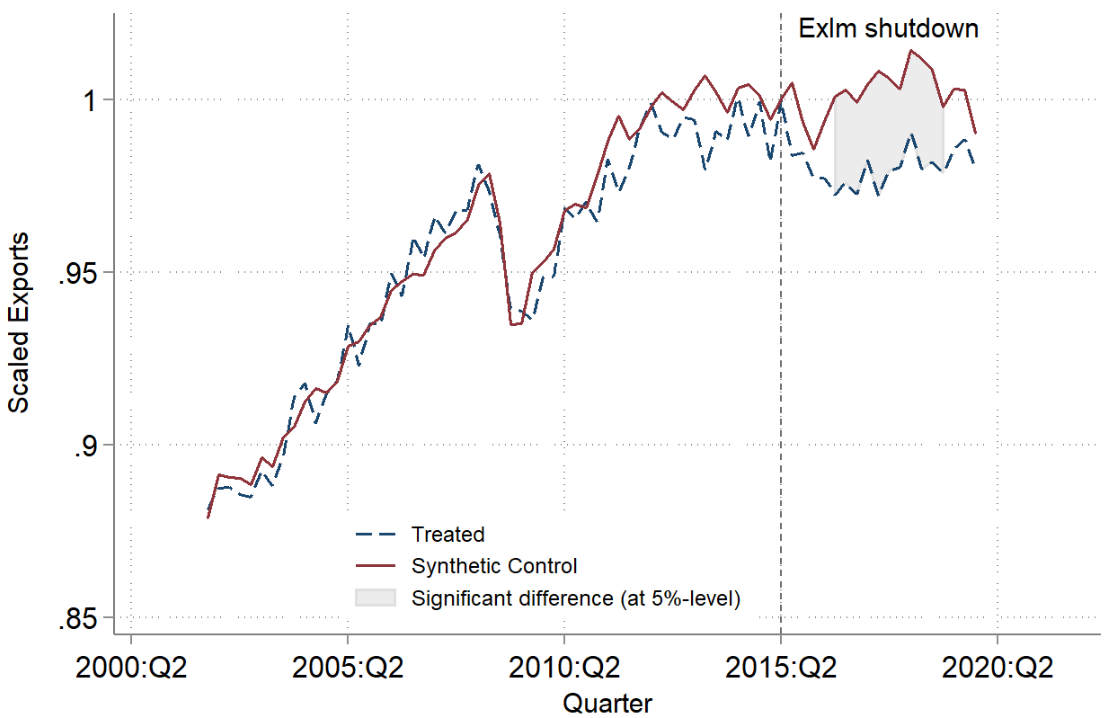
[27] To make the treatment effects comparable, I scale the outcomes to equal 1 in the last pre-treatment period for all treated and control units.

effect for the six baseline specifications is between -0.022 and -0.026, indicating that quarterly exports for the treated industries after the treatment (the ExIm reauthorization lapse) were on average between 2.2% and 2.6% lower than for the respective synthetic control industries. The generally small root mean squared prediction errors (RMSPEs) indicate a good pre-treatment fit between the treated industries and their synthetic controls. Table 4 also reports the standardized p-values, which are the proportion of placebo tests that resulted in a larger ratio of post-RMSPE over pre-RMSPE than the actually treated units. Overall, the p-values are significant at the 5%-level or lower for 4 out of the 6 specifications. Specifications without the aerospace industry have a slightly larger treatment effect and a higher significance level, indicating that the aerospace was less affected by the ExIm shutdown than the other treated industries. Including industries with low treatment intensity reduce the statistical significance of the treatment effect, which becomes insignificant at conventional levels in the specification without additional covariates (Column 6). This is not surprising, as it should be expected that industries which received very low levels of ExIm support prior to the reauthorization lapse would not see a significant decline in exports after 2015.

Figure 3 shows exports, averaged over the 10 treated industries and their respective synthetic controls, using the optimal weights from baseline specification 1 in Table 4). The figure visually confirms that there is a clear divergence in exports for the treated industries and their respective controls after the treatment period. The figure also shows the relatively close fit between the two groups in the pre-treatment period, although there is a slight divergence starting around 10 quarters prior to the treatment. There are two possible explanations for this divergence. First, as this is during the validation period, it might simply be random noise, indicating a less-than-perfect out-of-sample fit between the synthetic control and the treated units. Second, this decline might be due to anticipation of the treatment, and the general uncertainty surrounding ExIm reauthorization prior to 2015, as well as the reduction in ExIm support that already

[28] Figure B2 in the appendix shows a similar figure for imports instead of exports as a robustness check. Because US imports are not supported by ExIm, we would expect no significant treatment effect of the ExIm shutdown. The figure confirms that this is the case, and the average treatment effect on the treated is not significantly different from 0 for imports.

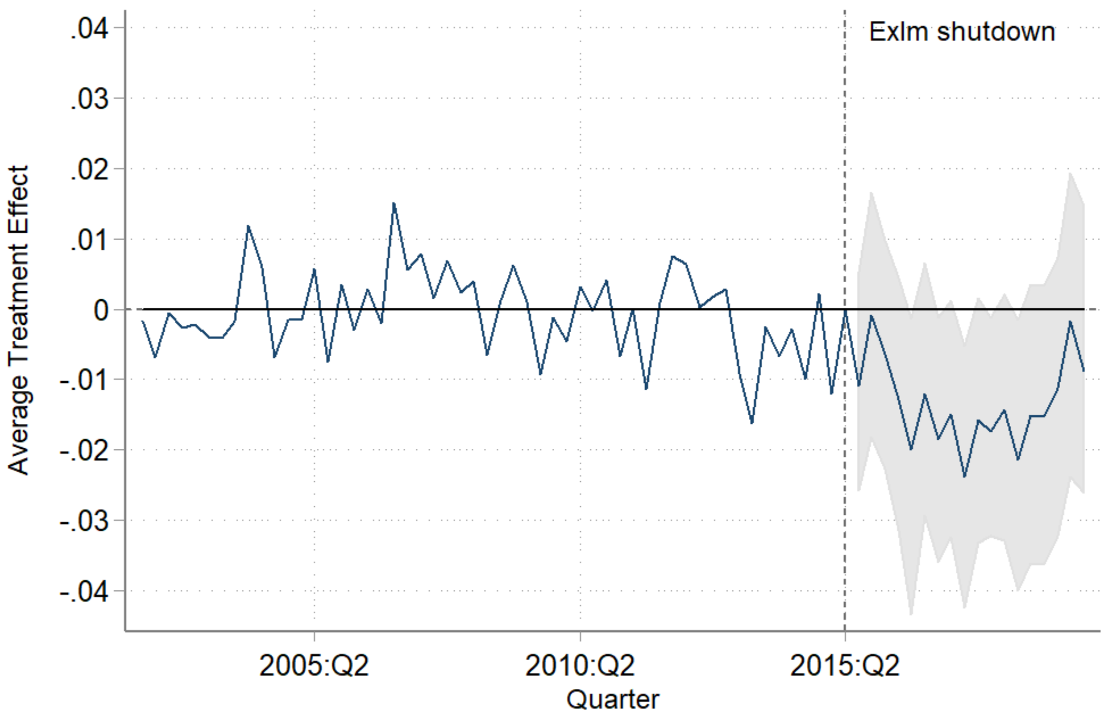
**Figure 3:** Exports for treated industries declined after the ExIm bank shutdown, relative to their synthetic controls




Note: Exports are normalized to equal one in the treatment period (2015:Q2). Both lines show simple averages for the 10 treated units and their respective synthetic controls, calculated according to specification 1 in Table 4. Statistical significance is calculated using placebo tests.

started around 2013. The average treatment effect becomes statistically significant at the 5%-level 5 quarters after the treatment, and remains highly significant for around 12 quarters.[28] The lagged treatment effect can be explained by the fact that after the shutdown, ExIm was still honoring contracts made before the shutdown, and the average contract length was around 380 days, as described earlier. Figure 4 shows the average difference in exports between the treated industries and their respective synthetic controls, as well as a 95% confidence interval for the treatment effect, calculated using the jackknife procedure.

**Figure 4:** Average difference between exports for treated industries and respective synthetic controls




Note: The average treatment effect is the average difference in scaled exports between each the 10 treated industries and their synthetic controls. The shaded are shows a 95% jackknife confidence interval.

It is considered good practice in the synthetic control literature to report both the donor weights $W$ and the predictor weights $V$ that were chosen to minimize the RMSPE between the treated units and the synthetic controls in the pre-treatment period (Abadie, 2021). Table A6 in the appendix reports the donor weights used to construct the synthetic controls for each of the 10 treated industries. The donor matrix is sparse, as each synthetic control is constructed of only

4-8 donor industries. Abadie (2021) argues that this sparsity reduces the risk of overfitting, and should thus lead to more reliable estimates. Furthermore, no donor industry received positive weights for more than half of the treated industries, and no donor industry received an average weight that is larger than 10%. Thus, it appears unlikely that a few influential donor industries are driving the results. Overall, 27 out of 47 industries in the donor pool received positive weights for at least one treated industry. In Table A4, I show the predictor weights for each of the treated industries. Exports in the pre-treatment period (the outcome variable) receive the largest weight for every treated industry, with an average weight that is close to 70%. Asset tangibility and leverage, both variables that proxy for an industry's borrowing capacity or financing constraints, receive relatively large weights, with averages of 14.8% and 7.7%, respectively. Importer country distance, as a proxy for trade costs, and importer country credit, a proxy for financial development, receive average weights around 2%. All other predictors receive weights that are much smaller. Finally, Table A5 shows the predictor balance, that is, the differences in predictor means between each treated industry and their respective synthetic controls. Small differences indicate a good fit, and increase the credibility of the synthetic control identification strategy, since this makes it more likely that both units are subjects to similar unobserved shocks. Table A5 shows that the treated industries and the synthetic controls are very balanced in terms of exports, as well as the traditional gravity variables distance and importer GDP. They are also very similar in terms of asset tangibility, with a median difference of less than 1 percentage point and a maximum difference of 6.64 percentage points, as well as in terms of total employment and the share of small businesses. For the other predictors, there is considerable variation in the match quality, although on average the differences between the treated industries and the respective synthetic controls are not very large.

While the 10 treated industries as a group saw their exports decline after 2015, relative to their synthetic controls, this might not be true for every treated industry individually. To test for heterogeneous treatment effects, I repeat the previous analysis separately for each treated

[29] I initially focus on specification 1 in Table 4 with all covariates, as this specification has one of the lowest RMSPEs and thus the best pre-treatment fit.

industry.[29] Figure B1 in the appendix shows that almost all of the treated industries saw a visible decline in exports after the ExIm shutdown in 2015, relative to their respective synthetic control units. In Table 5, I report the corresponding average treatment effects and the associated inference statistics for each treated industry. There is a large heterogeneity in estimated treatment effects across industries. The largest estimated treatment effect is -0.06 (a 6% reduction of exports) for "Railroad Rolling Stock" (NAICS4 = 3365) and "Ships & Boats" (NAICS4 = 3366), both significant at the 5%-level. Two out of the 10 treated industries ("Industrial Machinery", 3332 and "Communications Equipment", 3334) have slightly positive, but statistically insignificant, treatment effect estimates. Overall, the average reduction in exports is 2.14% and the median is 1.5%. Since the average ExIm coverage rate for treated industries was around 3.9%, a 2.14% decrease in exports implies an average ExIm multiplier of around 0.55. In other words, for each dollar of ExIm support that was lost due to the shutdown, exports decreased by 55 cents.[30]

Since the ExIm coverage rate cutoff for treated units was arbitrarily chosen at 1%, I also calculate the individual treatment effects for the 10 industries ranked 11-20 in terms of ExIm coverage ratio.[31] Those industries with ExIm coverage rates below 1% prior to 2015 had a treatment intensity that was much lower than for the 10 industries with the highest coverage ratio. For those industries, the mean ATT is 0.002, while the median is -0.012, and all but one of the estimates are statistically insignificant, implying that those industries did not see large reductions in exports after 2015 compared to the 10 treated industries, confirming the results of specifications 2 and 4 in Table 4.

## 4.2 Implied job losses due to the ExIm reauthorization lapse

One of the main goals of the Export-Import Bank is to help businesses create and maintain export-related jobs in the US, which usually pay higher wages than jobs that are not export-

[30] Using my baseline estimate for the combined treatment effect for all treated industries from Table 4, (Column 1), -0.022, yields a slightly larger ExIm multiplier of 0.022/3.9 = 0.56. I use the low multiplier in the following job loss calculations in order to be conservative.

[31] The results are available upon request.

**Table 5:** Single treatment effects for 10 industries with highest ExIm coverage (mean ATT = -0.0214, median ATT = -0.015)

| | Tanks & Containers | Aerospace & Parts | Railroad Mfg. | Engines & Turbines | Veneer & Wood |
|---|---|---|---|---|---|
| NAICS code | 3324 | 3364 | 3365 | 3336 | 3212 |
| ATT | -0.013 | -0.002 | $-0.060^{**}$ | -0.017 | -0.028 |
| P-value | (0.255) | (0.745) | (0.043) | (0.255) | (0.191) |
| RMSPE | 0.011 | 0.009 | 0.024 | 0.008 | 0.011 |
| Coverage rate | 0.141 | 0.078 | 0.041 | 0.027 | 0.026 |
| | **Agric. Machinery** | **Industrial Machinery** | **Comm. Equipment** | **Ships & Boats** | **Other Machinery** |
| NAICS code | 3331 | 3332 | 3342 | 3366 | 3339 |
| ATT | $-0.041^{*}$ | 0.018 | 0.001 | $-0.060^{**}$ | -0.012 |
| P-value | (0.085) | (0.234) | (0.915) | (0.021) | (0.468) |
| RMSPE | 0.010 | 0.016 | 0.007 | 0.042 | 0.005 |
| Coverage rate | 0.016 | 0.016 | 0.016 | 0.013 | 0.011 |

Note: The ATT is calculated as the average difference in exports between a treated industry and their synthetic controls, after the third quarter of 2015. P-values are calculated as the percentage of placebo tests that has a higher ratio of post-RMSPE to pre-RMSPE. Coverage rate is the value of ExIm support as a share of industry exports. P-values are based on placebo tests. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

related, as exporting firms are usually larger and more productive. In the following, I convert my treatment effect estimates for exports lost due to the ExIm Bank shutdown into job loss estimates, using two different back-of-the envelope calculations, assuming either a homogeneous or a heterogeneous multiplier effect. First, following the methodology used by ExIm to calculate the number of export-related jobs it support, I assume a homogeneous multiplier. But while ExIm implicitly assumes a multiplier of 1, meaning that one less dollar of ExIm support leads to a one dollar decrease in exports, the causal treatment effect estimate of -0.022 in my baseline specification implies a smaller multiplier of 0.55.[32] For the five years before the reauthorization lapse (2010-2014), ExIm estimates to have supported roughly 200,000 jobs per year, while in the last year it was fully operational (2014), it estimates 164,000 supported export-related jobs.[33] For the 4 full years after the reauthorization lapse, the average annual number of jobs supported declined to 40,000, which is a reduction of 160,000 jobs from the previous 5-year average, or

[32]See Government Accountability Office (2013, GAO-13-446).

[33]Taken from the ExIm Annual Reports 2010-2014.

a reduction of 124,000 jobs from 2014, due to the reauthorization lapse. Using the estimated 'causal' ExIm multiplier of 0.55 instead, the annual number of export-related jobs lost after 2015 would be between 68,200 and 88,000 jobs. Of course, this calculation assumes that the treatment effect is homogeneous and equal across all industries, which is clearly unrealistic as it is the average treatment effect on the treated industries (ATT). In fact, for the 10 treated industries with the highest ExIm coverage rate, the estimated treatment effect ranges from +2.4% to -6.2%, and is statistically insignificant at conventional significance levels in most cases. Disregarding statistical significance, I calculate the implied job losses in the 10 treated industries due to the reauthorization lapse, now allowing for industry-specific ExIm multipliers and treatment effects. More specifically, for each of the 10 industries, I first calculate the value of lost exports by multiplying industry exports in 2014 with the average treatment effect by industry from Table 5.[34] I then multiply this value by the industry-specific jobs ratios (for domestic jobs) taken from the Bureau of Labor Statistics' (BLS) employment requirement matrix for 2014 to get an estimate for the quarterly number of exported-related jobs lost due to the reauthorization lapse.[35] This leads to an estimated quarterly job loss of approximately 3,700 or annual job losses of 13,800.[36] This number is much smaller than the 68,200-88,000 lost jobs calculated in the homogeneous multiplier case above, due to two reasons. First, it only includes job losses that occurred in the 10 industries with the highest ExIm coverage rate, since all other industries were assumed to be untreated. However, those 10 industries accounted for 97% of all ExIm support in 2014, and and 98% of the reduction in support between 2014 and 2016, showing that the additional job losses due to a reduction in ExIm support for other industries should be minimal. Second and more importantly, the average jobs ratio for the treated industries is only 5.9, compared to approximately 8 for all industries in the dataset,

[34]This includes the small increases in exports for the 2 industries with a small positive ATT.

[35]The jobs ratio is defined as the number of workers needed to produce $1 million worth of output in a specific industry, and it includes both the workers in the same industry as well as workers needed to make intermediate inputs from other industries.

[36]Since the lack of quorum lasted for approximately 4 years, the total number of jobs lost in the 10 treated industries during the shutdown equals around 59,300. This also corresponds to a total loss of exports of $2.25 billion.

implying that industries supported by ExIm created disportionately fewer jobs.[37] Furthermore, among the treated industries, those with the largest decline in exports (i.e. the highest ATTs) tend to have relatively low export values as well as low jobs ratios, leading to a small number of lost jobs. For example, 'Railroad Rolling Stock' (NAICS 3365) with an ATT of -0.06 only had \$713 million average exports and a jobs ratio of 4.9, whereas 'Industrial Machinery' (NAICS 3332) with an ATT of 0.018 had exports of \$4074 million and a jobs ratio of 6.4. Thus, taking into account the heterogeneity of the treatment effect shows that most of the export losses after the reauthorization lapse occurred in industries with low total exports and low job ratios, leading to smaller implied job losses than in the case of homogeneous treatment effects.

It should be noted that preceeding calculations of the job impact of the ExIm bank shutdown disregard the fact that the industry-specific treatment effects in Table 5 are imprecisely estimated and often statistically insignificant, and should thus be taken with caution. As a second counterfactual, I calculate the additional numbers of jobs that could be supported by ExIm if it were to only support those industries that showed a significant reduction in exports after the shutdown. For this, I assume that the loans and loan guarantees extended to all industries between 2014 were instead equally distributed between the three industries with significant (at the 10%-level) treatment effects from Table 5). Using the industry-specific multipliers, job ratios and average levels of ExIm support before the shutdown, I calculate the number of jobs that would be supported by ExIm in those three industries, and compare them to the number of jobs that are currently supported. In this counterfactual, I find that ExIm would support around 66,000 more export-related jobs per year if it were to only support industries with a statistically significant ATT.[38]

[37]Note that because both treated and untreated industries have very similar labor productivities, this difference must be due to the labor intensiveness of the intermediate inputs used in those industries.

[38]Results available upon request.

# 5 Robustness checks

I conduct extensive robustness checks to test the sensitivity of my results when different assumptions are relaxed. In particular, there are two main dimensions which can be varied: the treatment period, and the units that belong in the treated and the donor group. First, in addition to running placebo tests that vary the treated industries, as was done in the standard inference procedure above, I also run placebo tests that vary the treatment period, following Abadie & Gardeazabal (2003).

Figure B3 in the appendix shows a histogram of standardized p-values for all possible treatment periods, using my preferred SCM specification with all covariates and half of the pre-treatment period as training period.[39] The overwhelming majority of treatment-period placebo tests have a p-value larger than that found in my baseline specification, and most are statistically insignificant at conventional levels. Out of the 50 placebo tests that assumed a quarter other than the third quarter of 2015 as the treatment period, only two result in a p-value that is smaller than 0.028.[40] As an example, Figure 5 and Figure 6 show the resulting trends for the treated units and synthetic controls when the treatment periods were assumed to be the fourth quarters of 2012 and 2016, respectively. In Figure 5, where the placebo treatment period coincides with the peak of ExIm support in 2002 (see Figure 1) there is no initial treatment effect, but the export trends of the treated industries and the synthetic controls still start to diverge around the actual treatment time, which strengthens the credibility of my previous results.[41] In Figure 6, which sets the treatment period to coincide with the final quarter of the Obama administration, no divergence in trends between the treated group and the synthetic control can be observed after the placebo treatment.

As another robustness check, in Table 6 I analyze whether a single treated industry is driving

[39] I excluded p-values for treatment periods that were within 2 quarters before or after the true treatment period (2015q3), since those might have been influenced by anticipation of the treatment or a lagged response to the treatment.

[40] Those two placebo tests set the treatment period as the first and second quarter of 2013. Their average treatment effects are -0.005 and -0.006, which is much lower than the effect found in the baseline specification.

[41] It should be stressed that the treatment period placebo tests provide no information about the actual treatment period.

**Figure 5:** Placebo treatment period 2012:Q4

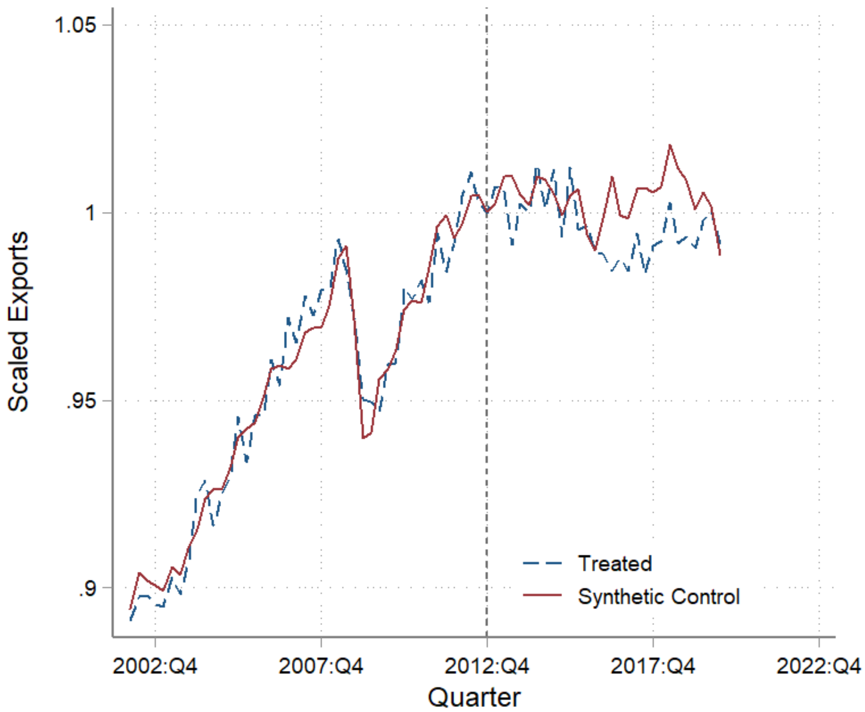
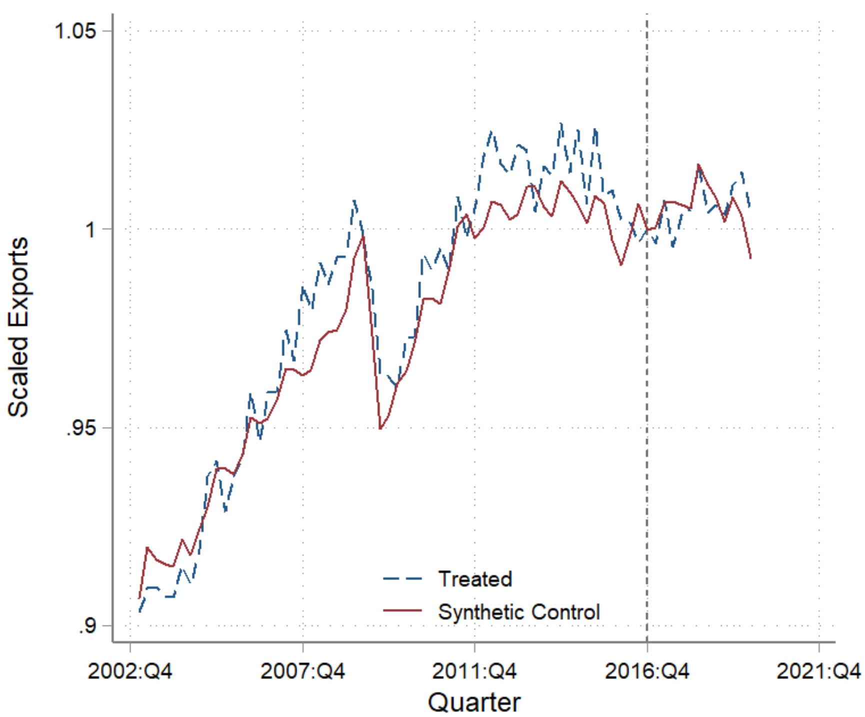



**Figure 6:** Placebo treatment period 2016:Q4




Note: Both figures show average normalized exports for the treated industries and their respective synthetic controls. Exports are normalized to equal 1 in the placebo treatment period, and synthetic control weights are calculated using data from before the placebo treatment period only.

the results. More specifically, I repeat my baseline specification while leaving out one of the 10 treated industries in each iteration, leaving a treated group of only 9 industries. The average treatment effect remains negative, and statistically significant at the 10% level in 9 cases (at the 5%-level in 8 cases), indicating that no single treated industry is driving the results.

**Table 6:** Leaving out one treated industry

| | 3324 | 3364 | 3365 | 3336 | 3212 | 3331 | 3332 | 3342 | 3366 | 3339 |
|---|---|---|---|---|---|---|---|---|---|---|
| ATT | $-0.023^{**}$ | $-0.024^{**}$ | $-0.017^{**}$ | $-0.022^{**}$ | $-0.021^{**}$ | -0.019 | $-0.026^{**}$ | $-0.024^{**}$ | $-0.017^{**}$ | $-0.023*$ |
| P-value | (0.026) | (0.015) | (0.049) | (0.044) | (0.048) | (0.112) | (0.022) | (0.014) | (0.027) | (0.052) |
| RMSPE | 0.015 | 0.015 | 0.013 | 0.015 | 0.015 | 0.015 | 0.014 | 0.015 | 0.011 | 0.015 |

Note: Each column drops one treated industry from the treated group, and repeats the analysis from specification 1 in Table 4. See Table 1 for NAICS4 code definitions. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

In Table 7 I follow the recommendation of Abadie (2021) and drop those industries from the donor pool that are too dissimilar to the treated industry, in terms of one of the covariates. More specifically for each treated unit (and each of the nine covariates), before choosing the optimal weights, I drop all those industries that have a mean value of the covariate that is more than two standard deviations below or above the mean for the treated industry. This preselection of donors restricts the donor pool from originally 47 industries to 33-45 industries, but it leaves the overall results qualitatively unchanged.

I further investigate whether my results rely only on a small number of donor industries (that might have disproportionately increased exports after 2015), by re-running my main specification after randomly excluding 5 potential donors for each single run. Figure 7 shows a histogram

**Table 7:** Dropping donors with large difference in covariates

| | GDP | Distance | Asset tan. | Lab.prod. | Leverage | ROA | Emp. | Small bus. | Credit |
|---|---|---|---|---|---|---|---|---|---|
| ATT | $-0.019^{**}$ | $-0.023^{***}$ | -0.018 | $-0.021^{**}$ | $-0.023^{**}$ | $-0.014^{**}$ | -0.012 | $-0.021^{**}$ | $-0.021^{*}$ |
| P-value | (0.022) | (0.007) | (0.108) | (0.042) | (0.016) | (0.018) | (0.116) | (0.042) | (0.072) |
| RMSPE | 0.016 | 0.018 | 0.015 | 0.014 | 0.015 | 0.015 | 0.014 | 0.015 | 0.015 |
| Donor pool | 33 | 35 | 42 | 45 | 44 | 39 | 41 | 43 | 44 |

Note: Each column repeats the analysis from specification 1 in Table 4 after dropping industries from the donor pool which have covariate values (column header) that are more than two standard deviations aport from the treated industry. Donor pool is the average number of industries left in the donor pool. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

of standardized p-values for 100 iterations of this robustness test. The majority of p-values is smaller than 0.05, and only very few results show p-values larger than 0.1. Thus, my results are not dependent on a small number of donor industries.

Finally, in order to address the specification-searching concerns that can arise by picking a certain set of covariates that should be matched by the synthetic control (see Ferman *et al.*, 2020), I also re-run my baseline specification using all possible combinations of my baseline covariates, starting from just including a single covariate, up to including all nine. The resulting p-values are plotted in Figure 8. The results are centered around a p-value of 0.015, with only a few outliers having p-values larger 0.05. This shows that the baseline synthetic control results are robust to a wide variety of covariate specifications, and are not dependent on a single covariate combination being chosen.

Overall, the robustness tests indicate that the statistical significance of the estimated treatment effect is highly sensitive to the choice of treatment period, but is not overly sensitive to the composition of the treated group or the donor group, meaning that the effect is not driven by a small number of treated or control industries, Furthermore, the estimates are similar when donors are restricted to those industries that are similar to the treated industries a priori.

# 6 Conclusion

This paper asks whether public financing support for exporters from the US Export-Import Bank actually increases exports. In order to determine causality, I analyze the effect of a large exogenous reduction in ExIm financing starting in 2015, using an event-study or quasi-experimental approach. I find that after the ExIm shutdown in 2015, industries that previously received the most support decreased there exports, relative to industries who were unaffected. The reduction in exports is heterogeneous across industries, but the average effect is a statistically significant 2.2% reduction in exports, and is robust to a variety of robustness checks and placebo tests. There are a few limitations to my results. The estimated treatment effect might be biased downwards, if firms anticipated the ExIm shutdown and obtained long-term

**Figure 7:** Robustness tests that randomly discard 5 donors

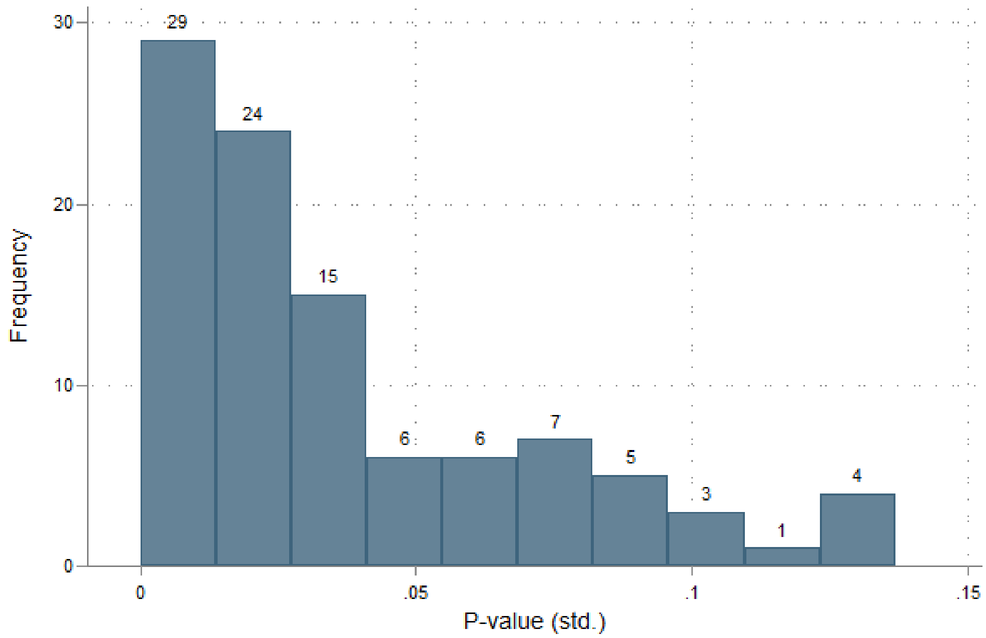
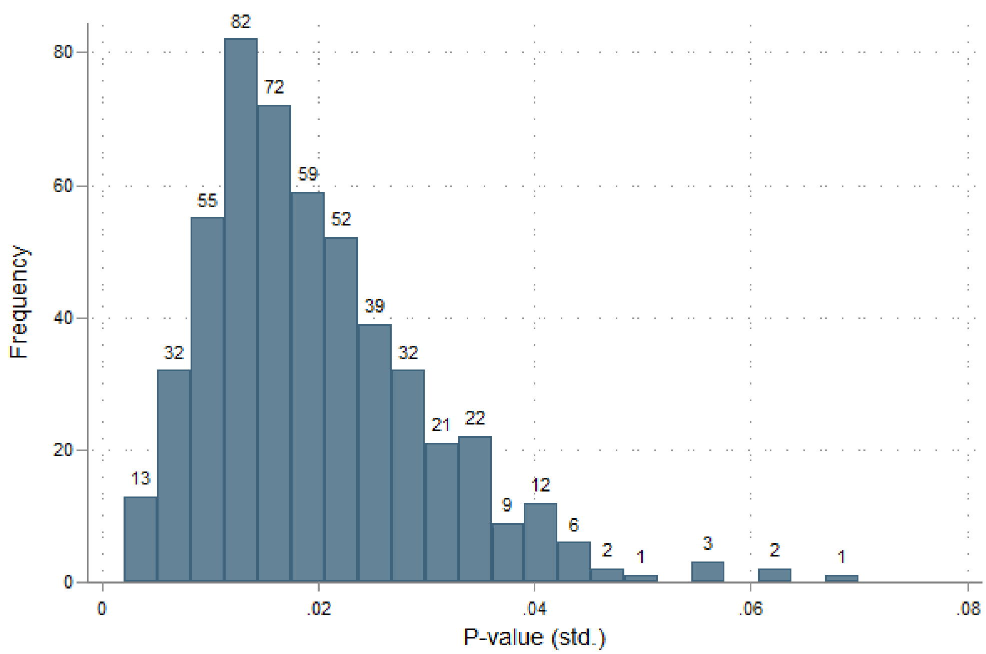



**Figure 8:** Robustness tests that randomly combine covariates




Note: Figure 7 shows the frequency distribution of p-values (based on placebo tests) for 100 iterations in which 5 randomly chosen donor industries are dropped from the donor pool. Figure 8 shows p-values for iterations of the baseline specification using all possible combinations of covariates.

financing support before the shutdown, thereby avoiding a reduction in exports. It is also possible that firms were able to substitute public export financing with private financing, due to generally low interest rates and high liquidity during the time of the ExIm bank shutdown. On the other hand, if ExIm increases exports not only through financing support, but also through non-monetary support (such as reducing information frictions), the estimated treatment effect could be biased upwards as well. Overall my results highlight the potential of public export financing to increase exports, and show how a large reduction in export financing support can reduce exports for many industries and lead to significant losses of export-related jobs.

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# APPENDIX

## A Tables

**Table A1:** Percentiles of Firm level support at the extensive and intensive margin

| | Total Support | | Support with Board Approval | |
|---|---|---|---|---|
| | Transactions | Mil. USD | Transactions | Mil. USD |
| min | 1 | .0019 | 1 | .5 |
| p5 | 1 | .18 | 1 | 10.18 |
| p10 | 1 | .28 | 1 | 15.57 |
| p25 | 1 | .6 | 1 | 33.75 |
| p50 | 3 | 1.5 | 3 | 75.63 |
| p75 | 5 | 4.4 | 4 | 350 |
| p90 | 8 | 12.04 | 9 | 931.036 |
| p95 | 9 | 25.65 | 17 | 2623.73 |
| max | 384 | 68844.14 | 383 | 68841.97 |
| $N$ | 7250 | | 147 | |

Note: This table shows percentiles for ExIm support, both at the extensive margin (number of transactions per firm) and the intensive margin (dollar value of supported export transactions). Support with board approval only includes authorizations that had to be signed of by the board of directors.

**Table A2:** Top 10 beneficiaries of ExIm support before and after the shutdown

| Before 2015: Q3 | | | After 2015: Q3 | | |
|---|---|---|---|---|---|
| Exporter | Support (mil. USD) | NAICS4 | Exporter | Support (mil. USD) | NAICS4 |
| Boeing Company | 68844 | 3364 | Anadarko Petroleum Corporation | 5000 | 2111 |
| Bechtel Corporation | 5175 | 5413 | Zeeco Domestic Holdings llc | 207 | 3323 |
| CBI Americas Limited | 3215 | 5413 | Delta Airlines Inc | 127.8 | 3364 |
| General Electric Energy Parts Inc | 3012 | 3336 | Air Tractor Inc | 123.9 | 3364 |
| Exxon Mobil Corp | 3005 | 3241 | Asi/Silica Machinery llc | 109.8 | 3332 |
| General Electric Intl. Operations | 3000 | 3339 | Montachem International Inc | 99.42 | 4246 |
| Solar Turbines Incorporated | 2721 | 3336 | General Electric Aviation | 93.85 | 3364 |
| Boeing Satellite Systems Inc | 2624 | 3342 | Ceca Supply & Services Inc | 88.42 | 4218 |
| Applied Materials Inc | 2127 | 3332 | Pacific Limited Corporation | 87 | 4226 |
| Diamond Offshore Drilling Services | 1900 | 2131 | Global Export Marketing Co Ltd | 86.38 | 4244 |

Note: This table shows the 10 firms with the largest total values of ExIm-supported exports, before and after the shutdown in 2015. *Support* is calculated as the total value (in million USD) of all exports that were supported by ExIm (with loans or loan guarantees) for each firm, before the shutdown (2007-2015) or after (2015-2019).

**Table A3:** Balance table for growth rates

| Variable | (1) Control | (2) Treated | (3) Difference | (4) P-value |
|---|---|---|---|---|
| Exports (mil. USD) | 0.013 | 0.008 | -0.006 | (0.082)* |
| Importer GDP per capita | -0.002 | -0.002 | -0.000 | (0.893) |
| Asset tangibility | -0.001 | -0.000 | 0.001 | (0.804) |
| Labor productivity | -0.001 | 0.000 | 0.001 | (0.390) |
| Return on assets | 0.000 | -0.004 | -0.004 | (0.733) |
| Leverage | -0.018 | -0.005 | 0.012 | (0.712) |
| Importer Credit (as % of GDP) | -0.015 | -0.012 | 0.003 | (0.451) |
| Employment | -0.004 | -0.003 | 0.002 | (0.290) |
| Small business share | 0.000 | 0.000 | -0.000 | (0.562) |
| Observations | 47 | 10 | 57 | |

Note: This table shows quarterly growth rate averages for the pre-treatment period (2007-2015), at the NAICS4 industry level. Treated industries are defined as industries with above-average ExIm coverage, and control industries are industries with no ExIm coverage.

**Table A4:** Predictor weights for treated industries

| Predictor \ Treated Industry | 3324 | 3364 | 3365 | 3336 | 3212 | 3331 | 3332 | 3342 | 3366 | 3339 | Average weight |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Exports | 74.20 | 68.30 | 71.00 | 69.02 | 70.06 | 69.76 | 68.67 | 68.55 | 68.22 | 69.56 | 69.73 |
| Importer GDP per capita | 0.93 | 0.95 | 0.96 | 0.97 | 0.90 | 1.10 | 0.84 | 0.95 | 1.22 | 0.94 | 0.98 |
| Distance | 2.06 | 1.74 | 1.88 | 2.06 | 1.84 | 2.91 | 1.82 | 1.76 | 1.81 | 1.89 | 1.98 |
| Asset tangibility | 12.53 | 15.86 | 14.20 | 15.20 | 14.41 | 14.82 | 15.91 | 15.96 | 14.44 | 14.99 | 14.83 |
| Labor productivity | 0.93 | 1.07 | 0.83 | 0.98 | 0.94 | 0.86 | 1.06 | 0.96 | 1.77 | 1.03 | 1.04 |
| Leverage | 5.47 | 8.16 | 7.48 | 8.29 | 7.99 | 8.07 | 8.22 | 8.08 | 6.97 | 8.06 | 7.68 |
| Return on assets | 0.84 | 0.63 | 0.62 | 0.58 | 0.63 | 0.47 | 0.55 | 0.64 | 0.84 | 0.62 | 0.64 |
| Employment | 0.68 | 0.64 | 0.74 | 0.67 | 0.64 | 0.66 | 0.62 | 0.65 | 0.64 | 0.64 | 0.66 |
| Small business share | 0.40 | 0.49 | 0.47 | 0.39 | 0.40 | 0.42 | 0.38 | 0.40 | 0.41 | 0.39 | 0.41 |
| Importer credit (as % of GDP) | 1.95 | 2.16 | 1.81 | 1.85 | 2.21 | 0.95 | 1.94 | 2.04 | 3.69 | 1.88 | 2.05 |

Note: This table shows the weights chosen for each predictor variable in the predictor vector $V$, by treated industry. Weights are restricted to be non-negative and sum up to one, and industry codes are defined as in Table 1.

**Table A5:** Predictor balance: Mean differences for predictor variables between treated industries and synthetic controls in the pre-treatment period

| Predictor \ Industry | 3324 | | 3364 | | 3365 | | 3336 | | 3212 | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Treated | Control | Treated | Control | Treated | Control | Treated | Control | Treated | Control |
| Exports | 0.92 | 0.92 | 0.95 | 0.95 | 0.91 | 0.91 | 0.97 | 0.97 | 0.99 | 0.98 |
| Importer GDP per capita | 10.20 | 10.35 | 10.28 | 10.35 | 10.08 | 10.24 | 10.19 | 10.20 | 10.34 | 10.07 |
| Distance | 8.71 | 8.65 | 9.09 | 8.88 | 8.71 | 8.64 | 8.78 | 8.77 | 8.43 | 8.71 |
| Asset tangibility | 23.23 | 29.87 | 13.50 | 15.16 | 27.63 | 28.86 | 18.78 | 18.88 | 55.42 | 52.41 |
| Labor productivity | 96.45 | 95.24 | 94.11 | 95.49 | 116.49 | 101.21 | 100.55 | 100.18 | 99.53 | 96.98 |
| Leverage | 125.86 | 82.43 | 42.68 | 39.73 | 73.56 | 62.97 | 27.69 | 28.30 | 74.21 | 67.98 |
| Return on assets | 7.73 | -4.56 | 6.64 | -2.85 | 6.73 | -0.29 | 1.66 | 1.41 | 5.38 | 3.45 |
| Employment | 4.54 | 5.14 | 6.18 | 5.55 | 3.21 | 4.49 | 4.59 | 4.66 | 4.47 | 4.56 |
| Small business share | 99.35 | 97.29 | 92.84 | 96.51 | 95.30 | 98.51 | 96.04 | 96.84 | 99.66 | 97.87 |
| Importer credit (as % of GDP) | 70.21 | 81.66 | 90.81 | 90.99 | 65.35 | 76.13 | 72.32 | 73.42 | 68.00 | 77.39 |

| Predictor \ Industry | 3331 | | 3332 | | 3342 | | 3366 | | 3339 | | Difference | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treated | Control | Treated | Control | Treated | Control | Treated | Control | Treated | Control | Median | Max |
| Exports | 0.99 | 0.99 | 0.95 | 0.95 | 0.94 | 0.94 | 0.92 | 0.92 | 0.98 | 0.98 | 0.00 | 0.00 |
| Importer GDP per capita | 10.14 | 10.16 | 10.01 | 10.17 | 10.13 | 10.23 | 10.30 | 10.28 | 10.16 | 10.18 | 0.09 | 0.27 |
| Distance | 8.91 | 8.83 | 8.91 | 8.83 | 8.87 | 8.80 | 8.82 | 8.61 | 8.82 | 8.79 | 0.08 | 0.28 |
| Asset tangibility | 17.67 | 17.72 | 11.93 | 11.95 | 6.80 | 7.74 | 34.67 | 33.63 | 12.83 | 13.72 | 0.99 | 6.64 |
| Labor productivity | 98.53 | 97.23 | 91.19 | 90.33 | 78.40 | 81.91 | 112.53 | 107.00 | 94.64 | 94.92 | 1.34 | 15.28 |
| Leverage | 42.89 | 42.38 | 6.64 | 11.97 | 3.01 | 3.85 | 2.23 | 17.00 | 33.86 | 31.37 | 4.14 | 43.42 |
| Return on assets | 8.15 | 7.56 | 3.32 | -1.66 | 0.52 | 3.40 | 8.13 | 2.59 | 7.98 | 6.14 | 3.93 | 12.29 |
| Employment | 5.41 | 4.77 | 4.76 | 4.61 | 4.83 | 3.93 | 4.96 | 3.56 | 5.57 | 5.15 | 0.61 | 1.40 |
| Small business share | 97.51 | 97.10 | 99.48 | 98.71 | 97.74 | 98.38 | 97.83 | 97.24 | 99.07 | 98.45 | 0.78 | 3.67 |
| Importer credit (as % of GDP) | 65.65 | 80.39 | 83.95 | 82.10 | 79.32 | 82.15 | 70.19 | 77.82 | 73.58 | 77.40 | 5.73 | 14.75 |

Note: This table shows the differences in predictor means between each treated industry and the respective synthetic control, as well as the median and maximum difference. Industry codes are defined as in Table 1.

**Table A6:** Donor weights for each treated industry

| Donor \ Treated | 3324 | 3364 | 3365 | 3336 | 3212 | 3331 | 3332 | 3342 | 3366 | 3339 | Average donor weight |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 2121 | 0 | 0 | 0 | 0 | 59.5 | 0 | 0 | 0 | 0 | 0 | 5.95 |
| 2122 | 0 | 0 | 0 | 5.4 | 0 | 0 | 0.8 | 0 | 35.8 | 0 | 4.2 |
| 2123 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3112 | 0 | 0 | 0 | 0 | 0 | 6.5 | 0 | 0 | 0 | 0 | 0.65 |
| 3113 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3114 | 65.7 | 32.2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 9.79 |
| 3116 | 0 | 9.1 | 0 | 0.2 | 0 | 10.2 | 0 | 0 | 0 | 0 | 1.95 |
| 3118 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3119 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3121 | 1.9 | 0 | 37.2 | 0 | 0 | 0 | 0 | 0 | 6.4 | 0 | 4.55 |
| 3122 | 0 | 2 | 0 | 6.4 | 0 | 16.3 | 0 | 0 | 0 | 7.6 | 3.23 |
| 3131 | 0 | 0 | 0 | 0 | 0 | 0 | 6 | 0 | 0.9 | 0 | 0.69 |
| 3132 | 0 | 0 | 0 | 0 | 27.2 | 0 | 0 | 0 | 0 | 0 | 2.72 |
| 3152 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 14.5 | 1.45 |
| 3162 | 0 | 0 | 3.2 | 0 | 0 | 0 | 0 | 0 | 44 | 0 | 4.72 |
| 3169 | 0 | 0 | 0 | 0 | 0 | 0 | 25.1 | 54.8 | 0 | 0 | 7.99 |
| 3211 | 12 | 0 | 8.7 | 0 | 3.7 | 0 | 0 | 0 | 0 | 0 | 2.44 |
| 3219 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3222 | 0 | 0 | 0 | 0 | 0.7 | 0 | 0 | 0 | 0 | 1.5 | 0.22 |
| 3231 | 0 | 0 | 0 | 0 | 8.8 | 9 | 0 | 0 | 0 | 0 | 1.78 |
| 3252 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 11.5 | 1.15 |
| 3253 | 0 | 0 | 0 | 13.3 | 0 | 0 | 0 | 0 | 0 | 0 | 1.33 |
| 3254 | 0 | 24 | 0 | 12.6 | 0 | 0 | 11.2 | 17.2 | 0 | 0 | 6.5 |
| 3255 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3256 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3259 | 0 | 0 | 0 | 0 | 0 | 27.7 | 15.9 | 0 | 0 | 0 | 4.36 |
| 3261 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3262 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3273 | 20.4 | 0 | 0.9 | 0 | 0 | 0 | 0 | 0 | 12.9 | 0 | 3.42 |
| 3279 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3312 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3313 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3314 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3325 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3326 | 0 | 0 | 37.1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3.71 |
| 3327 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3334 | 0 | 0 | 0 | 52.8 | 0 | 14.3 | 0 | 0 | 0 | 7.7 | 7.48 |
| 3341 | 0 | 0 | 0 | 5 | 0 | 16 | 14.6 | 24 | 0 | 24.5 | 8.41 |
| 3343 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3351 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3352 | 0 | 0 | 13 | 4.4 | 0 | 0 | 0 | 0 | 0 | 0 | 1.74 |
| 3353 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 4 | 0 | 3.6 | 0.76 |
| 3369 | 0 | 1.1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.11 |
| 3371 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3372 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3379 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3399 | 0 | 31.6 | 0 | 0 | 0 | 0 | 26.4 | 0 | 0 | 29.1 | 8.71 |
| Number of donors | 4 | 6 | 6 | 8 | 5 | 7 | 7 | 4 | 5 | 8 | 27 |

Note: This table shows the weights chosen for each donor industry in the donor unit vector $W$, by treated industry. Weights are restricted to be non-negative and sum up to one, and industry codes are defined as in Table 1.

**Table A7:** Single treatment effects for 10 industries with low ExIm coverage (mean ATT = 0.002, median ATT = -0.012)

| | 3361 | 3322 | 3323 | 3362 | 3335 | 2111 | 3221 | 3344 | 3359 | 3333 |
|---|---|---|---|---|---|---|---|---|---|---|
| ATT | -0.017 | -0.007 | -0.033 | 0.045 | -0.019** | 0.106 | -0.002 | -0.017 | -0.003 | -0.029 |
| P-value | (0.277) | (0.277) | (0.149) | (0.766) | (0.043) | (0.149) | (0.915) | (0.234) | (0.830) | (0.149) |
| RMSPE | 0.012 | 0.005 | 0.014 | 0.052 | 0.006 | 0.045 | 0.010 | 0.010 | 0.004 | 0.011 |
| Coverage rate | 0.007 | 0.007 | 0.006 | 0.004 | 0.002 | 0.001 | 0.001 | 0.001 | 0.001 | 0.000 |

Note: The ATT is calculated as the average difference in exports between a treated industry and their synthetic controls, after the third quarter of 2015. P-values are calculated as the percentage of placebo tests that has a higher ratio of post-RMSPE to pre-RMSPE. Coverage rate is the value of ExIm support as a share of industry exports. P-values are based on placebo tests. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

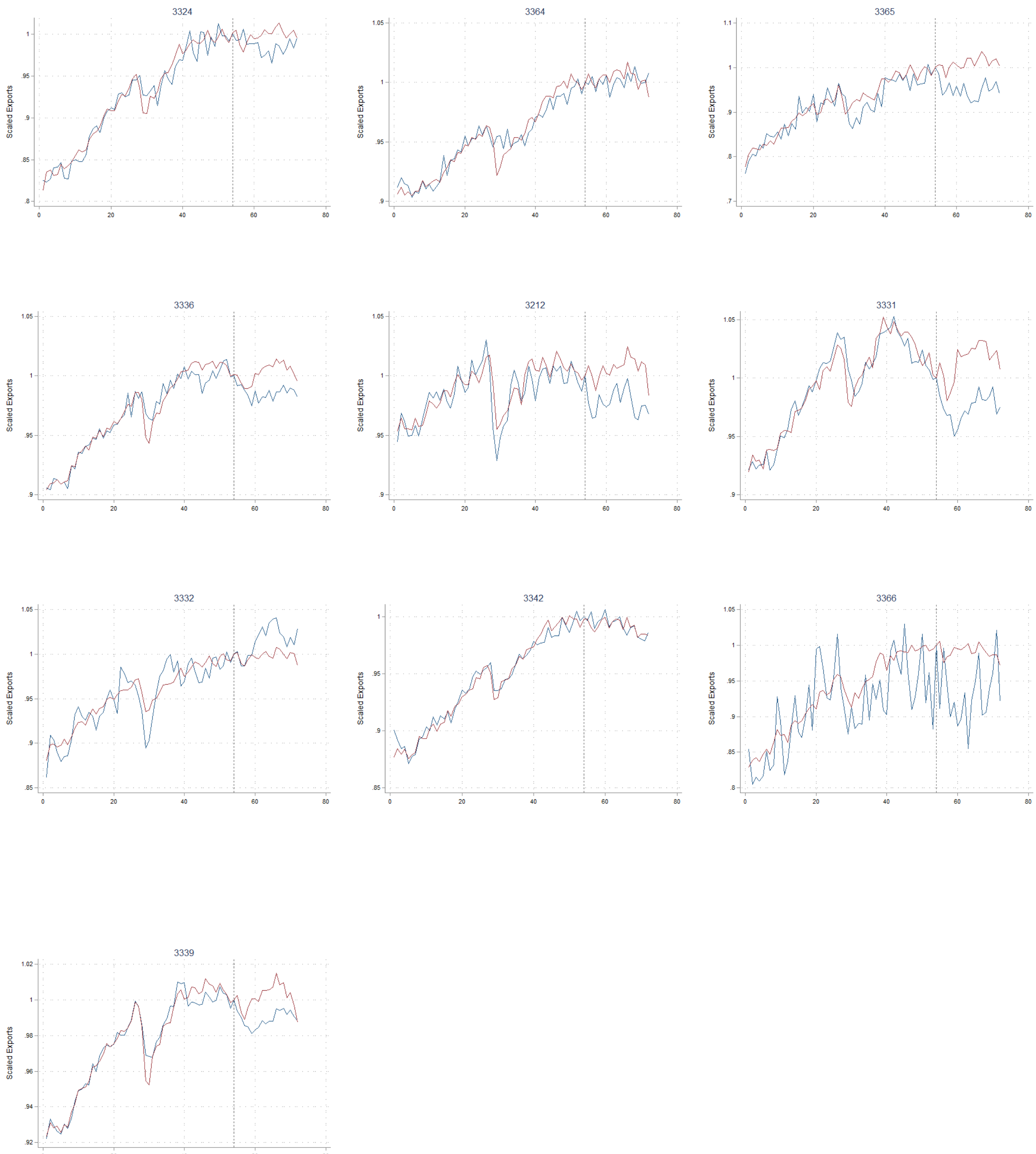
**Table A8:** Using different SCM estimators

| | Augmented SCM | | SCUL | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Baseline | No Aerospace | Baseline | No Aerospace |
| ATT | -0.013* | -0.015* | -0.023* | -0.024** |
| P-value | (0.077) | (0.081) | (0.060) | (0.018) |
| RMSPE | 0.012 | 0.011 | | |

Note: Columns 1 and 2 use the Augmented Synthetic Control Method (ASCM, Ben-Michael *et al.*, 2021a) and use Synthetic control using Lasso (SCUL, Hollingsworth & Wing, 2020). All specifications include the full set of covariates, as in Table 4, Column 1.

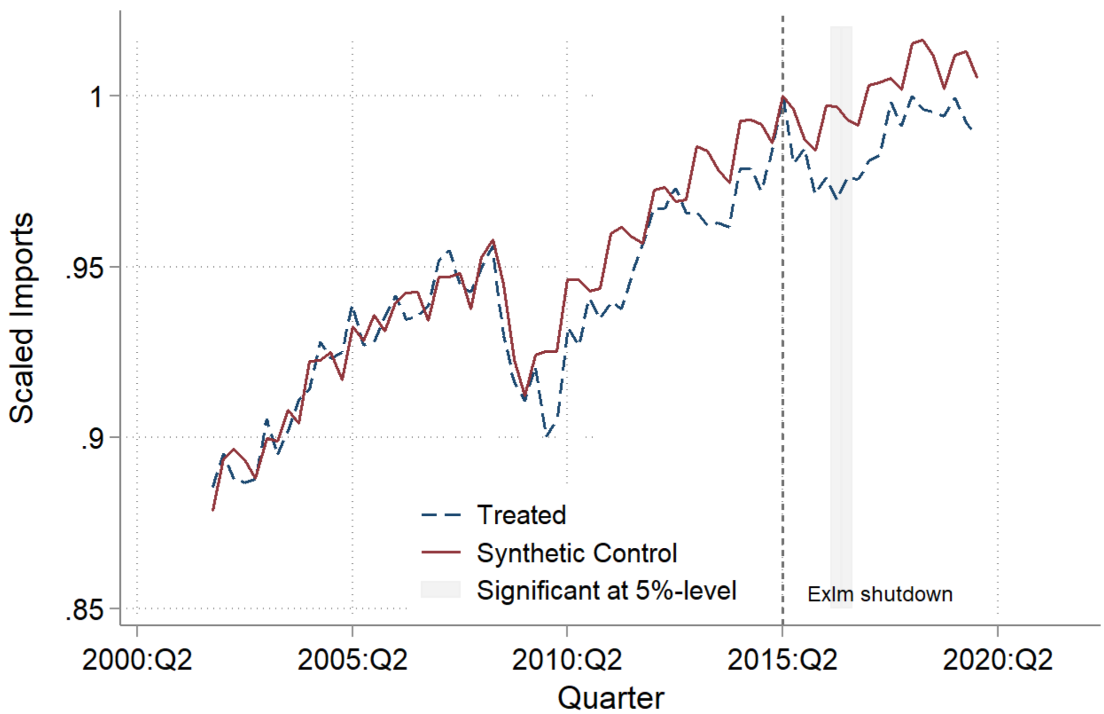
# B Figures

**Figure B1:** Exports of treated industries (blue) versus synthetic controls (red)



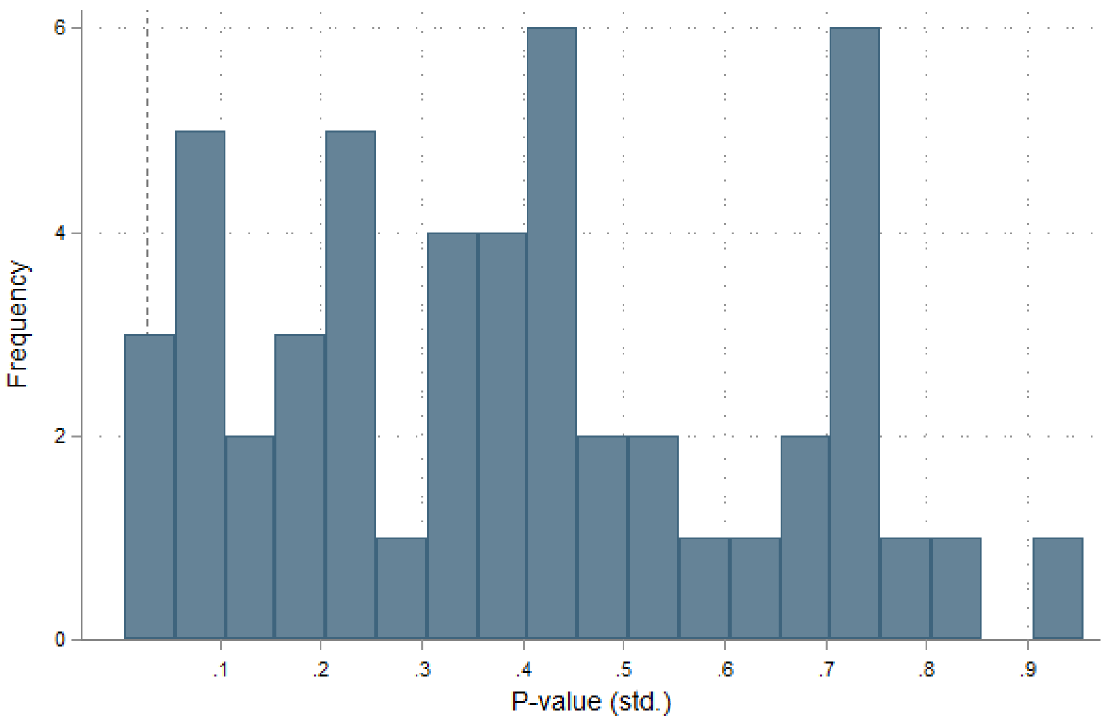

Note: This figure shows the trends in export for each industry and its respective synthetic control, corresponding to the estimates in Table 5. Industry codes are defined as in Table 1.

**Figure B2:** Imports for treated industries declined after the ExIm bank shutdown, relative to their synthetic controls




Note: Imports are normalized to equal one in the treatment period (2015:Q2). Both lines show simple averages for the 10 treated units and their respective synthetic controls, calculated according to specification 1 in Table 4. Statistical significance is calculated using placebo tests.

**Figure B3:** Treatment period placebo test p-values (actual p-value =0.028)




Note: Each observation is the standardized p-value of a placebo test, where the treatment period was defined to be a quarter other than the true treatment period (2015:Q3).

# C Gravity regression approach

Similar to previous studies, I estimate a gravity-type regression using a yearly panel dataset on US exports by 4-digit NAICS industry.[42] My baseline regression equation is given by the following reduced-form equation:

$$Y_{cit} = exp(\beta\ S_{cit} + \gamma' X_{cit} + \mu_{ci} + \mu_{ct} + \mu_{it} + \nu_{cit}) \tag{11}$$

where $c$ denotes the importer country, $i$ denotes the 4-digit NAICS industry, and $t$ denotes the year. $Y$ is the current US dollar value of exports to the respective market. $S$ is the dollar value of authorized ExIm support, and $X$ is a vector of gravity control variables that can vary at the country-industry-year level. The $\mu' s$ are country-industry, country-year and industry-year fixed effects, respectively. Those three-way fixed effects capture the impact of unobserved variables such as industry-level productivity and prices, industry-level multilateral resistance terms as in Anderson & Van Wincoop (2003) ($\mu_{st}$), country-level demand for US goods, country-level economic shocks and bilateral exchange rates ($\mu_{ct}$), and country-industry-level trade costs or industry preferences that do not vary over time ($\mu_{cs}$).[43] Finally, $\epsilon$ and $\nu$ are residuals.

As discussed in Silva & Tenreyro (2006), estimating a log-linear version of this gravity equations, as it has been done in Agarwal & Wang, 2018, will likely lead to biased coefficient estimates, due to the fact that the OLS moment condition in logarithmic form, $E[log(Exp) - log(\widehat{Exp})|\cdot] = 0$, does not necessarily imply $E[Exp - \widehat{Exp}|\cdot] = 0$ when the underlying error term $\varepsilon_{cit}$ is heteroskedastic.[44] To take this into account, I follow the current best practice of estimating my baseline regression in levels instead of logs, using the Poisson pseudo-maximum likelihood (PPML) estimator as my preferred gravity estimator.[45]

The results in Table C1 show that there is a positive correlation between ExIm support and exports, with an export elasticity of ExIm support between 1.5 and 4.3. In Table C2, I

[42]Previous studies estimated similar regressions for Germany (Moser *et al.* (2008) and Felbermayr & Yalcin (2013)), for Austria (Egger & Url (2006)) and for the US (Agarwal & Wang (2018)).

[43]These fixed effects also capture lower-level variation in only one dimension, such as distance from the US ($\mu_c$), world-wide economic shocks ($\mu_t$), or industry-level differences in financial constraints, such as average external finance dependence or asset tangibility ($\mu_s$).

[44]Silva & Tenreyro (2006) use bilateral trade data and show that the error terms are in fact heteroskedastic, and that the size of the bias increases with the degree of heteroskedasticity.

[45]See for example Eaton *et al.* (2012) and Larch *et al.* (2019). I include my dependent variable in levels instead of trade shares, which still leads to a coefficient estimate that is theoretically consistent with the standard gravity equation, although it weights observations differently (see Sotelo (2019)). In particular, using levels instead of shares induces the estimator "to try harder to fit the data for countries with larger spending."

**Table C1:** Gravity regressions results

| | (1) Exports (log) | (2) Exports (log+1) | (3) Exports (log) | (4) Exports | (5) Exports | (6) Exports |
|---|---|---|---|---|---|---|
| Ex-Im support (log) | 0.015** | | | 0.009 | | |
| | (0.007) | | | (0.006) | | |
| Ex-Im support (log+1) | | 0.043*** | | | 0.016*** | |
| | | (0.008) | | | (0.005) | |
| Ex-Im support (dummy) | | | 0.051*** | | | 0.010 |
| | | | (0.014) | | | (0.013) |
| Observations | 1434 | 231539 | 220450 | 1434 | 229831 | 229831 |
| Number of Clusters | 370 | 21065 | 20358 | 370 | 20771 | 20771 |
| Prob > F (Prob > $\chi^2$) | 0.029 | 0.000 | 0.000 | 0.120 | 0.000 | 0.430 |
| R-squared | 0.994 | 0.962 | 0.926 | | | |
| Pseudo-R-squared | | | | 0.998 | 0.969 | 0.969 |
| RESET p-value | .012 | 0 | 0 | .251 | .024 | .037 |

Columns 1-3 use log-linear regression, Columns 4-6 use PPML.

Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

All columns eliminate country-industry, country-year and industry-year fixed effects.

differentiate between ExIm support to large and small businesses, as well as the type of support. It can be seen that only loan guarantees to large businesses have a significant correlation with ExIm support.

**Table C2:** Differentiating by support type and business size, PPML

| | (1) Exports | (2) Exports | (3) Exports | (4) Exports |
|---|---|---|---|---|
| Ex-Im support (large bus., dummy) | 0.014 | | | |
| | (0.015) | | | |
| Ex-Im support (small bus., dummy) | -0.004 | | | |
| | (0.016) | | | |
| Ex-Im support (large bus.) (log+1) | | 0.016*** | | |
| | | (0.006) | | |
| Ex-Im support (small bus.) (log+1) | | -0.006 | | |
| | | (0.011) | | |
| Insurance (dummy) | | | -0.017 | |
| | | | (0.014) | |
| Loan (dummy) | | | -0.002 | |
| | | | (0.075) | |
| Guarantee (dummy) | | | 0.073*** | |
| | | | (0.020) | |
| Insurance (log+1) | | | | -0.011 |
| | | | | (0.010) |
| Loan (log+1) | | | | -0.007 |
| | | | | (0.016) |
| Guarantee (log+1) | | | | 0.023*** |
| | | | | (0.005) |
| Observations | 229831 | 229831 | 229831 | 229831 |
| Number of Clusters | 20771 | 20771 | 20771 | 20771 |
| Prob > $\chi^2$ | 0.631 | 0.005 | 0.002 | 0.000 |
| Pseudo-R-squared | 0.969 | 0.969 | 0.969 | 0.969 |

Clustered standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$
All columns eliminate country-industry, country-year and industry-year fixed effects.