The Costs of Sovereign Default: Evidence from Argentina*

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Abstract

We estimate the causal effect of sovereign default on the equity returns of Argentine firms. We identify this effect by exploiting changes in the probability of Argentine sovereign default induced by legal rulings in the case of *Republic of Argentina v. NML Capital*. Using a Rigobon (2003) heteroskedasticity-based identification strategy, we find that a 1% increase in the probability of default causes a 0.55% decline in the value of Argentine equities. We construct tracking portfolios for the present value of output growth, and estimate that the entire default episode caused a reduction in this measure of between 2.4% and 6%.

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1 Introduction

A fundamental question in international macroeconomics is why governments repay their debt to foreign creditors, given the limited recourse available to those creditors. The seminal paper of Eaton and Gersovitz (1981) argues that reputational concerns are sufficient to ensure that sovereigns repay their debt. In a famous critique, Bulow and Rogoff (1989b) demonstrate that reputation alone cannot sustain sovereign borrowing in equilibrium, without some other type of default cost or punishment. Following this critique, hundreds of papers have been written trying to find the source and measure the size of these costs. The fundamental identification challenge is that governments usually default in response to deteriorating economic conditions, which makes it hard to determine if the default itself caused further harm to the economy.

The case of *Republic of Argentina v. NML Capital* provides a natural experiment to disentangle the causal effect of sovereign default. Following Argentina's sovereign default in 2001, NML Capital, a subsidiary of Elliott Management Corporation, purchased defaulted bonds and refused to join other creditors in restructurings of the debt during 2005 and 2010. Instead, because the debt was issued under New York law, NML sued the Argentine government in US courts to receive full payment. To compel the Argentine government to repay the defaulted debt in full, the US courts blocked Argentina's ability to pay its restructured creditors, unless NML and the other holdout creditors also received payments. The Argentine government resisted paying the holdouts in full, even though the required payments would be small relative to the Argentine economy. As a result, legal rulings in favor of NML raised the probability that Argentina would default on its restructured bonds, while rulings in favor of Argentina lowered this probability.

Because the court rulings were not responding to private information about the underlying economic circumstances in Argentina, we can use them to examine the effect of changing default probabilities on Argentine firms. We use credit default swaps (CDS) to measure the change in the risk neutral probability of default. Compiling rulings from the United States District Court for the Southern District of New York, the Second Court of Appeals, and United States Supreme Court, we identify sixteen rulings[1] that potentially changed the probability of default. We find that, for every 1% increase in the 5-year cumulative default probability around these rulings, the US dollar value of an index of Argentine American Depository Receipts (ADRs) falls 0.55%.[2] Between January 3, 2011, when our data starts, and July 30, 2014, when Argentina defaulted, the risk-neutral 5-year default probability increased from roughly 40% to 100%. Our estimates imply that this episode reduced the value of the Argentine firms in the index by 33%.

We next attempt to translate our estimate of stock returns into a prediction about real economic

[1] We use sixteen rulings in our main specifications. The set of rulings we use differs slightly for some specifications, due to holidays and other issues.

[2] American Depository Receipts are shares in foreign firms that trade on US stock exchanges in US dollars.

activity. We use quarterly data from 2003 to 2014 to construct tracking portfolios that mimic news about real GDP growth. We then study the returns of these tracking portfolios around our legal rulings, and estimate that the present discounted value of expected future real GDP growth drops by between 0.04% and 0.10% for every 1% increase in the 5-year, risk-neutral cumulative default probability. Extrapolating our estimate to the recent sovereign default episode implies that the present value of future real GDP growth fell by between 2.4% and 6%. These estimates are large when compared to standard quantitative models of sovereign default (e.g. Aguiar and Gopinath (2006)), because those models assume that the loss in output caused by default is transitory. Our estimates can be rationalized by the presence of a persistent loss in output upon default.

To better understand how a sovereign default affects the economy, and in particular why the effects might persist, we examine which types of firms are harmed more or less by an increase in the probability of default. We sort firms along the dimensions suggested by the theoretical sovereign debt literature, as well as on some additional firm characteristics. We find suggestive evidence that exporters, banks, and foreign-owned firms are hurt more by increases in the probability of sovereign default than would be expected, given their "beta" to the Argentine market.[3] We argue that the poor returns for these firms are consistent with the theoretical sovereign debt literature.

Following Rigobon (2003) and Rigobon and Sack (2004), we identify the effect of changes in the default probability on equity returns through heteroskedasticity. We assume that during two-day periods in which US courts rule on *Republic of Argentina v. NML Capital* and related court cases, the variance of shocks to the probability of default is higher than during other two-day periods.[4] We describe our identification assumptions explicitly in section §4.

This paper contributes to a large literature examining the costs of sovereign default. The question of the cost of sovereign default is surveyed in Borensztein and Panizza (2008). Using quarterly time series, Yeyati and Panizza (2011) find that output generally falls in anticipation of a sovereign default and the default itself tends to mark the beginning of the recovery. Bulow and Rogoff (1989a) argue that default is costly because foreign lenders can disrupt trade, a channel for which Rose (2006), Borensztein and Panizza (2010), and Zymek (2012) find empirical support. Acharya et al. (2014b) examine the effect of the European sovereign debt crisis on syndicated loan supply and firm behavior. Gennaioli et al. (2014), Acharya et al. (2014a), Bocola (2013) and Perez (2014) present models of the disruptive effect of default on the financial system, and the consequences for macroeconomic activity. Mendoza and Yue (2012) present a general equilibrium strategic default model, building on the framework of Aguiar and Gopinath (2006) and Arellano (2008), where default is costly because it reduces the ability of domestic firms to import intermediate goods,

[3] Our point estimates are negative and economically significant for financial firms and foreign-owned firms, consistent with the theoretical literature, but our standard errors are too large to reject the hypothesis of no differential effects.

[4] In the appendix, B, we also explore a traditional event study approach, and find similar results.

reducing their productivity. Cole and Kehoe (1998) argue that a sovereign default causes the government to lose its reputation not just with respect to the repayment debt, but more generally. Arteta and Hale (2008) observe that during a sovereign default, external credit to the private sector is reduced. Schumacher et al. (2014) study sovereign debt litigation across a range of countries over the past 40 years. They find that creditor litigation is associated with a decline in international trade, sovereign exclusion from financial markets, and a longer time before the default is resolved. The Argentine case studied here differs from most of the cases studied in Schumacher et al. (2014), because the legal rulings we study also changed the probability of a new default, in addition to affecting the government's ability to resolve an ongoing default.

This paper is structured as follows: Section 2 discusses the case of *Republic of Argentina v. NML Capital*. Section 3 describes the data and presents summary statistics for the behavior of CDS and equity returns on event and non-event days. Section 4 presents our estimation framework, the identifying assumptions, and our results for firm equity returns. Section 5 discusses how we use tracking portfolios to estimate the impact of the legal rulings on output. Section 6 discusses industries and firm characteristics that are associated with larger responses to changes in the probability of sovereign default. Section 7 discusses institutional details and alternative interpretations of the results. Section 8 concludes.

2 Argentina's Sovereign Debt Saga

2.1 The Argentine Default of 2001 and the Restructurings of 2005 and 2010

Following decades of rampant inflation, in 1991 the Argentine government adopted the "convertibility plan," introducing a currency board in an attempt to irrevocably fix the peso-dollar exchange rate at one-to-one. This meant that the government legally committed itself not to print any currency that was not backed one-to-one by a US dollar in reserves. While inflation fell following the convertibility plan, the government continued to run a deficit, largely financed through external dollar borrowing. In 2001, Argentina entered a deep recession, with unemployment reaching 14.7% in the fourth quarter.[5] In December 2001, after borrowing heavily from the IMF, Argentina defaulted on over $100 billion in external sovereign debt and devalued the exchange rate by 75%.[6]

The Argentine government then spent three years in failed negotiations with the IMF, the Paris Club, and its private creditors. In January 2005, Argentina presented a unilateral offer to its private creditors, which was accepted by the holders of $62.3 billion of the defaulted debt.[7] To strengthen

[5] Data from Global Financial Data.

[6] Daseking et al. (2005).

[7] Hornbeck (2013).

its bargaining position, the Argentine legislature passed the "Lock Law," prohibiting the government from reopening the debt exchange or making any future offers on better terms.[8] After the first round of restructuring, holdout creditors were still owed $18.6 billion of principal, the Paris Club of official-sector creditors was owed $6.3 billion, and the IMF was owed $9.5 billion.[9] Despite the existence of the holdout creditors, S&P declared the end of the Argentine default in June 2005 and upgraded Argentina's long-term sovereign foreign currency credit rating to B-. In 2006, Argentina fully repaid the IMF, and Argentina reached an agreement with the Paris Club creditors in May 2014.[10]

In December 2010, Argentina offered another bond exchange to the holdout private creditors. Holdout private creditors who were owed $12.4 billion of principal agreed to the exchange. Following the exchange, on December 31, 2010, the remaining holdout creditors were owed an estimated $11.2 billion, split between $6.8 billion in principal and $4.4 billion in accumulated interest.[11] At this point, Argentina had restructured over 90% of the original face value of its debt.

2.2 Argentina vs. the "Vultures"

Following the 2010 debt exchange, the remaining holdout creditors, termed "vultures" by the Argentine government, continued their legal battle. One line of attack was on the Argentine government's reserve assets, with the creditors arguing the country's reserves, held at the Federal Reserve Bank of New York, should be subject to attachment. While a district court initially agreed with the creditors, in 2011 the appellate court overturned the ruling.[12] The second line of attack, focused on the *pari passu* clause, was the one that eventually culminated in Argentina's recent default on its restructured bond holders. The *pari passu* clause requires equal treatment of all bondholders. The creditors, led by NML Capital,[13] argued that the Argentine government breached this clause by paying the exchange bondholders and refusing to honor the claims of the holdouts. In addition, the holdouts asserted that the "Lock Law," by making explicit the government's policy of pledging not to re-open negotiations or pay any money, effectively subordinated them to the restructured bondholders.

The case took several years to work its way through the US courts, going from the United States District Court for the Southern District of New York ("Southern District"), to the United

[8] This Lock Law would feature prominently in Judge Griesa's interpretation of the *pari passu* clause, as evidence that holdouts were not on the same footing as the restructured creditors.

[9] Hornbeck (2013).

[10] http://www.reuters.com/article/2014/05/29/us-argentina-debt-parisclub-idUSKBN0E90JI20140529

[11] Hornbeck (2013).

[12] Hornbeck (2013)

[13] Elliott Management Corporation, the parent company of NML, has a long history in litigating against defaulting countries. See Gulati and Klee (2001) for a discussion of Elliot's litigation against Peru and Panizza et al. (2009) for an excellent literature review on the law and economics of sovereign default.

States Court of Appeals for the Second Circuit ("Second Circuit"), all the way to the United States Supreme Court. These three courts issued numerous rulings between December 2011, when Judge Thomas P. Griesa of the Southern District first ruled in favor of the holdouts on the *pari passu* issue,[14] and July 2014, when Argentina defaulted. For the purposes of this study, we view the various rulings as events that made it more or less likely that Argentina would be unable to pay the restructured bondholders, if it did not also repay the holdouts. Because of the Argentine government's unwillingness to pay the holdouts in full, rulings in favor of NML increased the probability of a default on the restructured bonds, while rulings in favor of Argentina reduced the probability of default.[15]

Following Griesa's initial ruling in December 2011, a year of legal wrangling ensued over what this ruling actually meant and how it would be enforced. Griesa clarified that Argentina was required to repay the holdouts as long as it was continuing to the pay the exchange bondholders (using a "ratable" payment formula). Argentina was not willing to comply with this ruling, and continued to pay the exchange bondholders without paying the holdouts. Griesa then ordered the financial intermediaries facilitating Argentina's payments to stop forwarding payments to the restructured bondholders, until Argentina also paid the holdouts. As a result, even if Argentina wanted to pay the restructured creditors, it could not do so without repaying the holdouts, as its trustee would not be allowed to disburse the funds. In late 2012, Griesa ordered Argentina to negotiate with the holdouts, but the holdouts and the courts rejected Argentina's offer of a deal comparable to the 2005 and 2010 bond exchanges. Argentina then twice appealed to the Supreme Court, with the Supreme Court declining to hear either appeal. Following the decline of the second appeal on June 16, 2014, Griesa's orders were implemented, and Argentina had only two weeks before a coupon to the restructured creditors was due. Against court orders, Argentina actually sent this coupon payment to the bond trustee, Bank of New York Mellon (BNYM), but due the court order, BNYM did not forward to the payment to the restructured bond holders. Argentina's restructured bonds did not receive a coupon payment on June 30, which began a 30-day grace period. Negotiations failed, and the International Swaps and Derivatives Association (ISDA) declared that a credit event had occurred for credit default swaps referencing Argentina's restructured bonds on August 1, 2014.

[14] See the decision at http://blogs.reuters.com/felix-salmon/files/2012/04/2011-12-07-Equal-Treatment-Liability-Order.pdf

[15] We use the term default to refer to a "credit event," as defined in the credit default swap contracts we study. Defaults come in many varieties, from a temporary cessation in payments to complete repudiation.

2.3 A Simple Interpretation

In the simplest interpretation of the unfolding events, Argentina was forced to default by the US court system. This was the interpretation offered by a number of commentators in the financial press.[16] Under this interpretation, Argentina could not pay its debts because the US courts forbade financial intermediaries from facilitating the coupon payment. As a result, the court rulings did nothing but change the probability of a default. This interpretation is convenient for us, and motivates our empirical strategy. However, there are other possible interpretations of these events, and we discuss them in section §7.

We also argue that these legal rulings did not reveal information about the underlying state of the economy (or other unobserved fundamentals), except insofar as they changed the probability of default. Our key assumption is that Judge Griesa (and the second circuit and Supreme Court) has no information advantage over the market with respect to the state of the Argentine economy.[17]

Under this interpretation, we can use credit default swaps to measure the market-implied changes in the probability of default following the court rulings. Any effect these rulings have on other variables, such as equity returns, is caused by the change in the probability of default. By comparing these two quantities (the change in default probability and the stock return), we can estimate the effect of default on the value of the firm. We look at the stock returns in windows around each of these events, and estimate how changes in the risk of sovereign default are related to changes in the valuation of Argentine firms. In the next two sections, we will describe the data and estimation procedures we employ.

3 Data and Summary Statistics

3.1 Stock Market and CDS Data

Our dataset consists of daily observations of financial variables from January 3, 2011 to July 29, 2014 (the day before Argentina most recently defaulted). We study the returns of US dollar-denominated ADRs issued by Argentine firms, which are traded in the United States, as well the Argentine peso-denominated equities traded in Argentina. The ADRs trade on the NYSE

[16]For instance, Matt O'Brien of the Washington Post wrote "Argentina was forced to default now, because it wouldn't pay the bonds it had defaulted on in 2001" (http://www.washingtonpost.com/blogs/wonkblog/wp/2014/08/03/everything-you-need-to-know-about-argentinas-weird-default/).

[17]In the event study literature that focuses on Federal Reserve monetary policy announcements, there is some concern that the Federal Reserve has more information than market participants about the state of the economy. These sorts of concerns are unlikely to apply in this paper.

and NASDAQ, are relatively liquid, and can be traded by a wide range of market participants.[18] However, using only the ADRs limits the number of firms that can be included in our analysis. To study the cross-sectional patterns of Argentine firms, we also examine the returns of firms traded only in Argentina. In order to ensure sufficient data quality, we limit our study of local Argentine equities to firms with a 2011 market capitalization at least 200 million pesos[19], and for which the equity price changes on at least half of all trading days in our sample. We exclude several firms that have neither headquarters or a large fraction of their revenues in Argentina, but are listed on the Argentine exchange for legacy reasons.[20] The full list firms included in our analysis, along with select firm characteristics, can be seen in Table 2.

The most commonly cited benchmark for Argentine ADRs is the MSCI Argentina index, an index of six Argentine ADRs. We also construct our own indices of ADRs, covering different sectors of the Argentine economy. We classify Argentine firms by whether they are a bank, a non-financial firm, or a real estate holding company. The industry classifications are based on the Fama-French 12 industry classification and are listed in table 2. We construct value-weighted indices for the entire market, banks, and non-financial firms.[21] The value-weighted indices we construct exclude YPF, the large oil company that was nationalized in 2012. They also include a 10% weight on US treasury bills, to ensure that their dividends are always positive.[22]

We use credit default swap (CDS) spreads to measure the market-implied risk-neutral probability of default. A CDS is a financial derivative where the seller of the swap agrees to insure the buyer against the possibility that the issuer defaults. Once a third party, the International Swaps and Derivatives Association (ISDA), declares a credit event, an auction occurs to determine the price of the defaulted debt. The CDS seller then pays the buyer the difference between the face and auction value of the debt. In appendix section §F, we provide details on how Markit, our data provider, imputes risk-neutral default probabilities from the term structure of CDS spreads using the ISDA Standard Model. We focus on the 5-year cumulative default probability, the risk-neutral probability that Argentina defaults within 5 years of the CDS contract initiation. In the figure below, we illustrate how CDS spreads and recovery assumptions can be used to infer hazard rates of default and cumulative default probabilities.

[Insert Figure 1 about here]

We use Markit's composite end-of-day spread, which we refer to as the "close." The composite

[18] Several market participants have told us that capital controls and related barriers are significant impediments to their participation in local Argentine equity markets.

[19] About $50mm USD at market exchange rates in 2011.

[20] See the appendix, section §D, for a discussion of these firms.

[21] We have also constructed equal-weighted indices, and found similar results.

[22] Positive dividends are useful in the analysis in section §5. In using portfolios of stocks and treasury bills for this purpose, we are following Vuolteenaho (2002).

end-of-day spread is gathered over a period of several hours from various market makers, and is the spread used by those market makers to value their own trading books. The composite end-of-day spread uses a survey of dealers to estimate the recovery rate. Markit uses a data cleaning process to ensure that the composite end-of-day quotes are reasonable approximations of market prices.

Because we want to capture the abnormal variation in Argentine CDS and equity returns caused by changes in the probability of default, we would like to account for other global factors that may affect both measures. To proxy for global risk aversion, we use the VIX index, the 30-day implied volatility on the S&P 500.[23] We use the S&P 500 to measure global equity returns and we use the MSCI Emerging Markets Asia ETF to proxy for factors affecting emerging markets generally. We use the Asian index to ensure that movements in the index are not directly caused by fluctuations in Argentine markets. To control for aggregate credit market conditions, we use the Markit CDX High Yield and Investment Grade CDS indices.[24] We also control for soybean (CBOT Soybean Futures Prices) and oil prices (West Texas Intermediate), two important commodities for the Argentine economy. Both commodity prices are from Global Financial Data. These controls are included in all specifications reported in this paper, although our results are qualitatively similar when using a subset of these factors, or no controls at all. In our discussion, we will assume that the legal rulings we study do not affect these controls; if this assumption were false, our estimates would measure the effect of the legal rulings on firms above and beyond what would be expected, given the effects on our control variables (see section §6 for details).

In order to examine the channels through which a sovereign default can affect domestic firms, in section 6 we will sort firms along a number of dimensions suggested by the theoretical literature. We group firms by industry, exporter status, imports of intermediate goods, whether the firm is a subsidiary of a foreign corporation, and whether the firm has an ADR traded in the United States. Our classification of firms as exporters and importers relies on industry-level, rather than firm-level, data. For details, see the appendix, section §A.

3.2 Definition of Events and Non-Events

We build a list of legal rulings issued by Judge Griesa, the Second Circuit, and the Supreme Court. We have created this list using articles in media (the Wall Street Journal, Bloomberg News, and the Financial Times), LexisNexis searches, and publicly available information from the website of a law firm (Shearman) that practices sovereign debt law.

In appendix section §L, we list all of these events and links to the relevant source material. Unfortunately, for many of the events, we are unable to determine precisely when the ruling was

[23] See Longstaff et. al. (2011) for discussion of VIX and variation in sovereign CDS spreads.

[24] We use the continuous on the run series from from Thomson Reuters Datastream. More information on these indices can be found at https://www.markit.com/news/Credit%20Indices%20Primer.pdf.

issued. We employ several methods to determine the timing of rulings. First, we examine news coverage of the rulings, using Bloomberg News, the Financial Times, and LexisNexis searches. Sometimes, contemporaneous news coverage specifically mentions when the ruling was released. Second, we use the date listed in the ruling (usually next to the judge's signature). Third, many of rulings are released in the PDF electronic format, and have a "creation time" and/or "modification time" listed in the meta-information of the PDF file. In appendix section §L, we list the information used to determine the approximate time of each ruling.

For our main analysis, we use two-day event windows. Consider the Supreme Court ruling on Monday, June 16th, 2014. The two-day event window, applied to this event, would use the CDS spread change from the close on Friday, June 13th to the close on Tuesday, June 17th. It would use stock returns (for both ADRs and local stocks) from 4pm EDT on Friday, June 13th to 4pm EDT on Tuesday, June 17th.[25]

For our two-day event windows, we choose our sample of non-events to be a set of two-day default probability changes and stock returns (based on closes), non-overlapping, at least two days away from any event, and at least two days away from any of the "excluded events." "Excluded events" are legal rulings that we do not use, but also exclude from our sample of "non-events." For three of the legal rulings, we could not find any contemporaneous media coverage, and are therefore unable to determine when the event was known to market participants. For one legal ruling, we could not find the ruling itself, only references to it in media coverage. One of the legal rulings was issued on the Friday in October 2012 shortly before "Superstorm Sandy" hit New York, and another the night before Thanksgiving.[26] Finally, one of the legal rulings was issued at the beginning of an oral argument, in which Argentina's lawyers may have revealed information about Argentina's intentions. We exclude this day because it violates our identification assumptions. For the heteroskedasticity-based identification strategy we employ, removing these legal rulings increases the validity of our identifying assumption that the variance of shocks induced by legal rulings is higher on event days than non-event days. However, our results are robust to including these days in the set of non-events.

3.3 Summary of Events and Non-Events

In figure 2, we plot the two-day change in the 5-year default probability and the two-day return of value-weighted index over our sample. Small data points in gray/light are non-events and the maroon/dark dots cover event windows in which a US court made a legal ruling regarding Argentina's debt. In most of our analysis, and in this plot, we use two-day return windows. As a

[25]For events occurring outside of daylight savings time in the eastern time zone, the local stocks close at 5pm ART (3pm EST), while the ADRs use 4pm EST. We make no attempt to correct for this.

[26]The ruling issued the night before Thanksgiving is problematic in several ways (see the appendix for details).

result, there is some risk that other shocks occurred during the event window. In figure 2, the event labeled “1” is affected by this issue. In our analysis that uses small window sizes, this event is no longer an outlier. The details on each event can be found in Appendix A. In figure 3, we construct the equivalent figure for the Mexican MSCI equity index and change in Argentina’s probability of default. Comparing the figure for the Argentine value-weighted equity index with the figure for the Mexican index, we see that on the non-event days, both stock indices co-move with our default probability measure. However, on the event days, only the Argentine equity index co-moves with the default probability measure. This observation suggests that omitted common factors might not be very important on our event days, consistent with our finding (shown below) that our event studies and heteroskedasticity-based identification strategy reach similar conclusions. In section §K, we present similar figures for the different sectors of the Argentine economy and measures of the exchange rate.

[Insert figure 2 here]

[Insert figure 3 here]

In the table below, we present summary statistics for the returns of the MSCI Argentina Index and the changes in 5-year risk-neutral default probabilities, during the two-day event and non-event windows.

[Insert table 1 here]

3.4 Exchange Rates

We are also interested in the effect of sovereign default on exchange rates. However, “the exchange rate” is difficult to measure in Argentina. Capital controls were imposed in 2002, strengthened in 2011,[27] and the official exchange rate has significantly diverged from the rate in other markets. We will consider three different measures of this parallel exchange rate, known as the Blue Dollar. All of them are based on the rate at which individuals could actually transact.

The first unofficial exchange rate that we consider is the one that Argentines can use to buy dollars from black market currency dealers. Dolarblue.net publishes this rate daily and this source is used by many Argentines as the reference for the exchange rate. Datastream also collects similar by polling the major currency brokers every day. This onshore rate is known as the Dolar Blue or the Informal dollar, among many other names.[28]

[27] http://www.economist.com/blogs/americasview/2014/01/currency-controls-argentina
[28] http://www.infodolar.com/cotizacion-dolar-blue.aspx

The other two measures of the unofficial exchange rate we will study come directly from market prices and provide a way for onshore currency dealers to secure dollars. Both rely on the fact that even though the Argentine peso is a non-convertible currency, securities can be purchased onshore in pesos and sold offshore in dollars. The first class of instruments for which this can be done are domestic law Argentine government bonds. By dividing the peso price of the government bond by the dollar price of the same bond, we arrive an exchange rate known as "blue-chip swap" rate. The mechanics of this transaction are outlined in Panel A of Figure 4. We can construct a similar measure of the exchange rate, known as the "ADR blue rate," by using equities rather than debt.[29] In Panel A of Figure 4, we demonstrate how to construct an exchange using local equities and ADRs. Our preferred measure for the asset-based blue rate is the blue-chip swap, because it seems to have substantially less noise than the ADR blue rate at the two-day frequency.

[Insert Figure 4 about here]

The official exchange rate is tightly controlled, so we would not expect to see any movement in the official rate in our two-day windows. However, there is a market in non-deliverable forwards (NDF), out to 1 year, that should capture the market's risk-adjusted expectation of a devaluation in the official rate. These contracts settle in dollars, based on the difference between the rate at which they are struck and the fixing rate, which reflects official rate at the time of settlement.[30]

[Insert Figure 5 about here]

In Figure 5, we plot all five of these exchange rates during our sample period. Throughout the period, the official rate is significantly below the unofficial rates. The ADR blue rate and the blue-chip swap rate are virtually indistinguishable during the period, and co-move with the Dolar Blue rate. We see that the 12 month NDF rate is much closer to the unofficial rates than to the official rate, even though its payoff is based on the official rate one year ahead. This implies that the unofficial rates may also reflect investor expectations about future official rates. This is useful for us, because the NDF market appears to be substantially less liquid than the bond and stock markets used to generate the other blue rates, and is therefore not a good fit for our identification strategy.

4 Framework

Our goal is to estimate the causal effect of sovereign default on equity returns. The key identification concerns are that stock returns might have an effect on default probabilities, and that

[29] Auguste et al. (2006) explore how the convertibility of ADRs provides a way around capital controls.
[30] For Argentina, the fixing rate is determined by the Emerging Market Trade Association (EMTA).

unobserved common shocks might affect both the market-implied probability of default and stock returns. In our context, one example of the former issue is that poor earnings by large Argentine firms might harm the fiscal position of the Argentine government, and therefore alter the probability of default. An example of the latter issue is a shock to the market price of risk, which could cause both CDS spreads and stock returns to change.

We consider these issues through the lens of a simultaneous equation model (following Rigobon and Sack (2004)). While our actual implementation uses multiple assets and controls for various market factors, for exposition we discuss only a single asset, r_t, and the change in the risk-neutral probability of default, ΔD_t, and ignore constants. For exposition, we will refer to this asset, r_t, as the equity market. The model we consider is

$$\Delta D_t = \gamma r_t + \kappa_D F_t + \varepsilon_t \tag{1}$$

$$r_t = \alpha \Delta D_t + \kappa F_t + \eta_t \tag{2}$$

where F_t is an unobserved factor that moves both the probability of default and equity returns, ε_t is a shock to the default probability, and η_t is a shock to the equity market return.[31] The goal is to estimate the parameter α, the impact of a change in the probability of default on equity market returns. If one were to simply run the regression in equation 2 using OLS, the coefficient estimate would be

$$\begin{aligned} \hat{\alpha} &= \frac{cov(\Delta D_t, r_t)}{var(\Delta D_t)} \\ &= \alpha + (1 - \alpha\gamma) \frac{\kappa(\kappa_D + \gamma\kappa)\sigma_F^2 + \gamma\sigma_\eta^2}{(\kappa_D + \gamma\kappa)^2 \sigma_F^2 + \gamma^2\sigma_\eta^2 + \sigma_\varepsilon^2} \end{aligned} \tag{3}$$

where σ_ε^2 is the variance of the default probability shock, σ_η^2 is the variance of equity return shock, and σ_F^2 is the variance of the common shock.[32] There are two sources of bias: simultaneity bias and omitted variable bias. The simultaneity bias exists if $\gamma \neq 0$ and $\sigma_\eta > 0$, and omitted variable bias exists if $\kappa \neq 0$, $\kappa_D \neq 0$, and $\sigma_F^2 > 0$. In order for the OLS regression to be unbiased, equity market returns must have no effect on default probabilities and there must be no omitted common shocks. These assumptions are implausible in our context, but we present this OLS regression in section 4.1 for comparison purposes.

We could rely on more plausible assumptions by adopting an event study framework (see, for instance, Kuttner (2001) or Bernanke and Kuttner (2005)). In this case, the identifying assumption would be that changes to Argentina's probability of default on during the event windows (time

[31] We assume these shocks and unobserved factors are independent.

[32] This expression is the one presented in Rigobon and Sack (2004).

periods in which a US court makes a ruling in the case of the *Republic of Argentina v. NML Capital*) are driven exclusively by those legal rulings, or other idiosyncratic default probability shocks (ε_t).[33] Under this assumption, we could directly estimate equation (2) using OLS on these ruling days. We will pursue this strategy in section 4.3 and in the appendix, section §B.

Our preferred specification uses a heteroskedasticity-based identification strategy, following Rigobon (2003) and Rigobon and Sack (2004). This does not require the complete absence of common and idiosyncratic shocks during event windows. This strategy instead relies on the weaker identifying assumption that the variances of the common shocks F_t and equity return shocks η_t are the same on non-event days and event days, whereas the variance of the shock to the probability of default ε_t is higher on event days than non-event days. The variance of ε_t is assumed to be higher because of the impact of the legal rulings, which are modeled as ε_t shocks. The assumption that the legal rulings cause shocks to ε_t is the exclusion restriction; we are are assuming that the legal rulings affect stock returns only through their effects on the probability of sovereign default, and not through other channels. Under this assumption, we can identify the parameter α by comparing the covariance matrices of abnormal returns and abnormal default probability changes on event days and non-event days. If the identification assumptions required for OLS or an event study hold, the heteroskedasticity-based strategy will also be valid, but the converse is not true.

In order to see how we can use this strategy to identify our key parameter of interest, we first solve for the reduced from of equations 1 and 2:

$$\begin{aligned} r_t &= \frac{1}{1-\alpha\gamma}\left((\alpha\kappa_D+\kappa)F_t+\eta_t+\alpha\varepsilon_t\right) \\ \Delta D_t &= \frac{1}{1-\alpha\gamma}\left((\kappa_D+\gamma\kappa)F_t+\gamma\eta_t+\varepsilon_t\right) \end{aligned}$$

We can then divide all days in our sample into two types of days, event (E) and non-event (N) days. For each of the two types of days $j \in \{E,N\}$, we can estimate the covariance matrix of $[r_t, \Delta D_t]$, denoted Ω_j :

$$\Omega_j = \begin{bmatrix} var_j(r_t) & cov_j(r_t,\Delta D_t) \\ cov_j(r_t,\Delta D_t) & var_j(\Delta D_t) \end{bmatrix}$$

Calculating these moments using the reduced form equations, we can then write the covariance matrix on day type j as

$$\Omega_j = \left(\frac{1}{1-\alpha\gamma}\right)^2 \begin{bmatrix} \alpha^2\sigma^2_{\varepsilon,j}+\sigma^2_\eta+(\alpha\kappa_D+\kappa)^2\sigma^2_F & \alpha\sigma^2_{\varepsilon,j}+\gamma\sigma^2_\eta+((\alpha\kappa_D+\kappa)(\gamma\kappa+\kappa_D))\sigma^2_F \\ \alpha\sigma^2_{\varepsilon,j}+\gamma\sigma^2_\eta+((\alpha\kappa_D+\kappa)(\gamma\kappa+\kappa_D))\sigma^2_F & \sigma^2_{\varepsilon,j}+\gamma^2\sigma^2_\eta+(\kappa_D+\gamma\kappa)^2\sigma^2_F \end{bmatrix}$$

[33] Rigobon and Sack (2004) demonstrate that the event study makes the identification assumption that on event days, the ratio of the default shock variance σ_ε to both the equity return shock σ_η and the common shock σ_F is infinite. If this assumption holds, we can see from equation 3 that $\hat{\alpha}$ is an unbiased estimator of α.

We can then define the difference in the covariance matrices on event and non-event days as $\Delta\Omega = \Omega_E - \Omega_N$, which simplifies to

$$\Delta\Omega \;=\; \lambda \begin{bmatrix} \alpha^2 & \alpha \\ \alpha & 1 \end{bmatrix} \tag{4}$$

where $\lambda = \left(\dfrac{\sigma^2_{\varepsilon,E} - \sigma^2_{\varepsilon,N}}{(1-\alpha\gamma)^2} \right)$. This provides us with a number of ways to estimate the coefficient of interest α that we will examine in section 4.5. Although we have described our framework where the only asset is the market, in the appendix, section I, we demonstrate how an equivalent system can be derived in a multi-asset framework.

We begin by presenting the OLS estimates, as point for comparison with our subsequent results.

4.1 OLS Estimates

In this section, we assume the OLS identifying assumption: $F_t = 0$ and $\gamma = 0$ in equations 1 and 2 above. The model can be written as

$$r_t = \alpha \Delta D_t + \eta_t$$

where α is the coefficient of interest, and $Cov(\Delta D_t, \eta_t) = 0$. We can estimate this equation with OLS.

In our actual implementation, we include a constant and the vector of controls X_t discussed in section 3.1. We estimate the OLS model for the returns of the MSCI Argentina Index, our value index and two value-weighted ADR industry groups, and our five measures of the exchange rate.

[Insert table 3a and table 4a here]

The results in table 1 imply that a 1% increase in the probability of default is is associated with a 0.46% fall in the MSCI Argentina Index. In Appendix Table A3, we see increases in the probability of an Argentine default are associated with increases in Brazilian and Mexican CDS spreads and declines in the Brazilian and Mexican equity markets. This correlation points to the importance of omitted common factors. In our heteroskedasticity-based estimates presented below and in the appendix, we show that the legal rulings have no measurable impact on Brazilian and Mexican CDS or equity markets. The method we use to construct standard errors and confidence intervals is discussed below in section 4.4. For the OLS estimates, it is essentially equivalent to using heteroskedasticity-robust standard errors in the typical fashion.

4.2 Case Study: Announcement

We begin our discussion of the event study approach with a single event. On June 16, 2014, the U.S. Supreme Court denied two appeals and a petition from the Republic of Argentina. This denial had several effects. First, it allowed the holdouts to pursue discovery against all of Argentina's foreign assets, not just those in the United States. Second, the court declined to review Judge Griesa's interpretation of the *pari passu* clause and his orders demanding equal treatment. The denial of Argentina's petition meant that Judge Griesa could prevent the Bank of New York, the payment agent on Argentina's restructured bonds, from paying the coupons on those bonds, unless Argentina also paid the holdouts. Because Argentina had previously expressed its unwillingness to pay the holdouts, this news meant that Argentina was more likely to default.

This event is ideal for our purposes because we are able to precisely determine the time the news was released. The Supreme Court announces multiple orders in a single public session, and simultaneously provides copies of those orders to the press. Prior to releasing the official opinion, the court announces the order. SCOTUSBlog, a well-known legal website that provides news coverage and analysis of the Supreme Court, had a "live blog" of the announcements on that day. At 9:33am EST, SCOTUSBlog reported that "Both of the Argentine bond cases have been denied. Sotomayor took no part." [34] At 10:09am, the live blog stated that Argentina's petition had been denied. At 10:11am, the live blog provided a link to the ruling. In figure 6, we plot the returns of the Argentine ADRs and the 5-year cumulative default probability, as measured by CDS. The ADRs begin trading in New York at 9:30am. The default probability is constructed from CDS spreads based on the Markit "sameday" data at 9:30am EST and 10:30am EST.

[Insert figure 6 here]

From 9:30am to 10:30am, the MSCI ADR index fell 6% and five-year cumulative implied default probability rose by 9.8%. When the Argentine stock market opened, the local stocks associated with the MSCI ADR index opened 6.2% lower than it closed the previous night. Under the standard event study assumptions, this implies that a 1% increase in the probability of default caused a 0.63% fall in ADR prices, and virtually no change in the ADR-based blue rate.

4.3 Event Study

We also present an "IV-style" event study. This study uses the two-day events and non-events described previously. The second stage equation we wish to estimate is equation (2), shown above. The instrument we use is $1(t \in E)\Delta D_t$ (and $1(t \in E)$), where E is the set of event days and $\mathbf{1}(\cdot)$ is

[34] See http://live.scotusblog.com/Event/Live_blog_of_orders_and_opinions__June_16_2014?Page=0

the indicator function. The first-stage regression is

$$\Delta D_t = \chi 1(t \in E)\Delta D_t + \rho 1(t \in E) + \mu_D + \omega_D^T X_t + \tau_t,$$

where τ_t is a composite of the three unobserved shocks (ε_t, F_t, ν_t) on the non-event days. Under the event study assumptions, the unobserved shocks ε_t and F_t (in the second stage) are not correlated with the change in the default probability on event days. The standard errors and confidence intervals for this approach are described in section 4.4, below.

[Insert table 3b and table 4b here]

Using this method, we find that a 1% increase in the probability of default causes a 0.56% fall in the MSCI Argentina Index, a 0.40% fall in our value index, a 0.55% fall in bank stocks, and a 0.34% fall in non-financial stocks. We also find that a 1% increase in the probability of default causes a 0.32% depreciation of the blue chip swap rate, a 0.14% depreciation (not statistically significant) of the dolar blue onshore rate, and has no effect on the official exchange rate.

4.4 Standard Errors and Confidence Intervals

To construct confidence intervals for our coefficient estimates, we employ the bootstrap procedure advocated by Horowitz (2001). The advantage of this procedure is that it offers "asymptotic refinements" for the coverage probabilities of tests, meaning that it is more likely to achieve the desired rejection probability under the null hypothesis. Our estimators (except for the OLS) are effectively based on a small number of the data points (the events), and therefore these refinements may provide significant improvements over first-order asymptotic approximations. As a practical matter, our confidence intervals are in almost all cases substantially wider than those based on first-order asymptotic approximations. Nevertheless, these "asymptotic refinements" are still based on asymptotic arguments, and there is no guarantee that they are accurate for our data. We also find (in unreported results) that our confidence intervals for our coefficient of interest, α, are similar to confidence intervals constructed under normal approximations, using a bootstrapped standard error.

We use 1000 repetitions of a stratified bootstrap, resampling with replacement from our set of events and non-events, separately, so that each bootstrap replication contains 16 events and 388 non-events.[35] In each bootstrap replication, we compute the (asymptotically pivotal) t-statistic $t_k = \frac{\hat{\alpha}_k - \hat{\alpha}}{\hat{\sigma}_k}$, where $\hat{\alpha}$ is the point estimate in our actual data sample, $\hat{\alpha}_k$ is the point estimate in bootstrap replication k, and $\hat{\sigma}_k$ is the heteroskedasticity-robust standard deviation estimate of $\hat{\alpha} - \alpha$

[35] The number of events and non-events listed apply to the ADRs. The exchange rates have a slightly different number of events and non-events, due to holidays, missing data, and related issues.

from bootstrap sample k. We then determine the 2.5th percentile and 97.5th percentile of t_k in the bootstrap replications, denoted $\hat{t}_{2.5}$ and $\hat{t}_{97.5}$, respectively. The reported 95% confidence interval for $\hat{\alpha}$ is $[\hat{t}_{2.5}\hat{\sigma}+\hat{\alpha}, \hat{t}_{97.5}\hat{\sigma}+\hat{\alpha}]$, where $\hat{\sigma}$ is the heteroskedasticity-robust standard deviation estimate of $\hat{\alpha}-\alpha$ from our original data sample. We construct 90% and 99% confidence intervals in a similar fashion, and use them to assign asterisks in our tables.[36] In the tables, we report the 95% confidence interval and the heteroskedasticity-robust standard error from our dataset ($\hat{\sigma}$).

4.5 Variance-based Analysis

Our final set of analysis is based on the difference between the covariance matrices in equation (4). There are several potential ways to estimate α based on $\Delta\Omega$. Two such estimators, which we call the CDS-IV and Returns-IV estimators, respectively, are defined as

$$\begin{aligned}
\hat{\alpha}_{CIV} &= \frac{\Delta\Omega_{1,2}}{\Delta\Omega_{2,2}} = \frac{\mathrm{cov}_E(\Delta D_t, r_t) - \mathrm{cov}_N(\Delta D_t, r_t)}{\mathrm{var}_E(\Delta D_t) - \mathrm{var}_N(\Delta D_t)} \\
\hat{\alpha}_{RIV} &= \frac{\Delta\Omega_{1,1}}{\Delta\Omega_{1,2}} = \frac{\mathrm{var}_E(r_t) - \mathrm{var}_N(r_t)}{\mathrm{cov}_E(\Delta D_t, r_t) - \mathrm{cov}_N(\Delta D_t, r_t)}
\end{aligned}$$

As shown in Rigobon and Sack (2004), these estimators can be implemented in an instrumental variables framework. More generally, equation (4) provides us with three moment conditions.

$$\begin{aligned}
\Delta\Omega_{1,1} - \lambda\alpha^2 &= 0, && (5) \\
\Delta\Omega_{1,2} - \lambda\alpha &= 0, && (6) \\
\Delta\Omega_{2,2} - \lambda &= 0. && (7)
\end{aligned}$$

The GMM estimator described by Rigobon (2003) uses all three moment conditions.

However, we use only the CDS-IV estimator. The Returns-IV estimator uses an "irrelevant instrument" under the null hypothesis that $\alpha = 0$. The estimator $\hat{\alpha}_{RIV}$ is the ratio of the sample estimates of $\Delta\Omega_{1,1}$ and $\Delta\Omega_{1,2}$, both of which are zero in expectation under the null hypothesis. The denominator, $\Delta\Omega_{1,2}$, is the covariance between the default probability, which is the variable being instrumented for, and the instrument. Under the null hypothesis, this covariance is zero, meaning that the instrument is irrelevant. As a result, the behavior of the $\hat{\alpha}_{RIV}$ estimator under the null hypothesis is not characterized by the standard IV asymptotics, and our confidence intervals will not have the correct coverage probabilities.[37] The CDS-IV estimator does not suffer from

[36] These asterisks represent an "equal-tailed" test that $\alpha \neq 0$.

[37] Under a different null hypothesis, that α is near, but not equal, to zero, weak identification asymptotics may be a better characterization of the sample distribution of $\hat{\alpha}_{RIV}$.

this issue. The estimator $\hat{\alpha}_{CIV}$ is based on the ratio of the sample estimates of $\Delta\Omega_{1,2}$ and $\Delta\Omega_{2,2}$. Under the null hypothesis that $\alpha = 0$ and $\lambda > 0$, the CDS-IV instrument is still relevant, and the standard asymptotics for $\hat{\alpha}_{CIV}$ apply. The GMM estimator, $\hat{\alpha}_{GMM}$, which uses all three moments, can be thought of as a geometric average of the CDS-IV and Returns-IV estimators. When $\alpha \neq 0$, using all three moments is advantageous because it takes advantage of all available information and makes over-identifying tests possible. However, under the null hypothesis that $\alpha = 0$, using the Returns-IV estimator in any way is problematic.[38] The two-step GMM procedure, implemented using standard asymptotics to estimate the optimal weighting matrix, would generally not correctly estimate the variances, because of the irrelevant instrument. As a result, the weight matrix might effectively place excessive weight on the Returns-IV estimator, relative to the CDS-IV estimator, and end up providing problematic results. For these reasons, we use the CDS-IV estimator as our estimation procedure.

The CDS-IV instrument is relevant under the assumption that $\lambda > 0$. We formally test this assumption using a one-sided F-test of the ratio of $(\Omega_E)_{22}$ to $(\Omega_N)_{22}$, which is the ratio of the variance of changes in the default probability on event days and non-event days. We test the alternate hypothesis that this ratio is greater than 1 (implying $\lambda > 0$) against the null hypothesis that it is equal to one. In our sample, this F-statistic is 11.78, well above the 99th percentile, one-sided, bootstrapped critical value of 1.98.[39] The relevance of the CDS-IV instrument is also suggested by the weak-identification F-test of Stock and Yogo (2005) (not to be confused with the F-test for $\lambda > 0$) shown in table 3c. In table 3c, we present the results of our CDS-IV estimation. The standard errors and confidence intervals use the bootstrap procedure described previously.

[Insert table 3c and table 4c here]

We find that increases in the 5-year risk-neutral default probability cause statistically and economically significant declines in the MSCI Argentina Index, bank ADRs, and non-financial ADRs. In contrast, we do not find a statistically significant effect on the ADRs of Argentine real-estate holding companies, although we cannot rule out economically significant effects. The point estimates in table 3c are very close to those reported in table 3b, with a 1% increase in the probability of default causing a 0.56% fall in the MSCI index, a 0.55% fall in bank stocks, a 0.33% fall in non-financial stocks, and 0.39% our value-weighted index. A 1% increases in the probability of default also causes a 0.30% depreciation in the blue chip swap rate. The increase in the risk-neutral default probability from 40% to 100%, which is roughly what Argentina experienced, would cause more than a 33% fall in the MSCI index, by our estimates.

[38]More formally, the Jacobian of the moment conditions with respect to the parameters does not have full column rank when $\alpha = 0$, and the identification assumption used to derive the standard GMM asymptotics does not hold.

[39]We use the bootstrapping procedure for pivotal statistics described by Horowitz (2001), and in our section on standard errors and confidence intervals.

Our results are consistent with the hypothesis that Argentina's default caused significant harms to Argentina's economy. In the next section, we attempt to translate this estimate into a result about output.

5 Output Costs of Default

The results in the previous section show that the stock prices of Argentine firms fall significantly in response to an exogenous increase the probability of sovereign default. In this section, we will attempt to translate these stock returns into changes in expected future output (real GDP). We pursue this line of inquiry for two reasons. First, an output-based measure brings us closer to understanding the welfare cost of default. We cannot treat the magnitude of the stock market declines as an estimate of the GDP loss; the Argentine economy performed poorly in the aftermath of the recent default, but real GDP certainly did not drop by 33%. Second, in theoretical models of sovereign default, such as Aguiar and Gopinath (2006) and Arellano (2008), the magnitude of the output loss conditional on default is a key model parameter. The size of this output loss plays a critical role in the government's decision to default, in the pricing of the sovereign debt, and in determining the quantity of debt that can be sustained in equilibrium. Yet there is very little direct evidence on the magnitude of this cost. Most authors treat the cost as a free parameter, and calibrate their models to match observed behavior.

Ideally, we would observe, at a daily frequency, estimates of future Argentine real GDP. If that data existed[40], we could use it with our heteroskedasticity-based identification strategy. Because it does not, we will use a tracking portfolio instead. The tracking portfolio[41] is the linear combination of financial assets (with returns data at high frequency) that best mimics news about real GDP (at the quarterly frequency). In practice, because of data limitations, our tracking portfolios will have only one or two assets– our value-weighted ADR index and the nominal exchange rate. We interpret the estimation exercises below as offering a "back-of-the-envelope" estimate for how one should convert the 33% estimated stock market decline into an output loss.

Let $N_{y,t}$ be the news, which we do not observe, about real GDP at time t (we will define exactly what we mean by real GDP news shortly). Suppose that we have a proxy for this news, $\tilde{N}_{y,t}$, that we do observe in low frequency data. This proxy is a noisy, possibly biased measure of real GDP

[40] In fact, as part of its bond restructuring, Argentina issued real GDP warrants, and these warrants trade in financial markets. However, they are very illiquid, have option-like features, and are affected by issues relating to the measurement of Argentine inflation. For these reasons, we did not find it feasible to use them to construct a high-frequency GDP forecast series.

[41] For an introduction to tracking portfolios, see Breeden et al. (1989). For the application of tracking portfolios to the forecasting of future macroeconomic variables, see Lamont (2001). For the use of a GDP-news tracking portfolio, see Vassalou (2003).

news:

$$N_{y,t} = \tilde{N}_{y,t} + \omega_t,$$

where ω_t is error associated with our proxy for news. The tracking portfolio is constructed by estimating a regression, on low frequency data:

$$\tilde{N}_{y,t} = \beta r_t + \nu_t, \tag{8}$$

where r_t is a vector of abnormal returns. Let $\hat{N}_{y,t} = \hat{\beta} r_t$ be the high-frequency estimate of real GDP news associated with this tracking portfolio.

Our strategy is to construct $\hat{N}_{y,t}$, and treat this tracking portfolio as an asset in our heteroskedasticity-based estimation procedure, as described in the previous section. Three conditions are sufficient for our estimates, constructed in this fashion, to be unbiased. As in equation (1), let ε_t denote the impact of the legal rulings. The first condition is that $E[\varepsilon_t \omega_t] = 0$. This condition requires that the legal rulings do not systematically affect the relationship between our GDP news proxy $\tilde{N}_{y,t}$ and actual GDP news. The second condition is that $E[\varepsilon_t \nu_t] = 0$. This condition requires that the legal shock does not systematically affect the GDP news proxy in a way that is not captured by the tracking portfolio. The third condition is that the variance of ω_t and ν_t is the same on event days and non-event days.

If the first condition was violated, but the second condition was not, our strategy would provide an unbiased estimate of the effect of the legal rulings on the proxy for GDP news, but not the actual GDP news. The second and third conditions are, in our view, an unavoidable requirement of using high-frequency data to proxy for a low-frequency variable. The assumptions become more plausible when one considers a richer set of proxy assets, but data limitations prevent us from using a large set of proxy assets.

We will define GDP news, the primary outcome variable we are interested in, as the discounted net present value of changes in the expectation of real GDP growth:

$$N_{y,t} = E_t[\sum_{j=0}^{\infty} \rho^j \Delta y_{t+j}] - E_{t-1}[\sum_{j=0}^{\infty} \rho^j \Delta y_{t+j}],$$

where $\rho \in (0,1)$ is a discount factor and Δy_t is real GDP growth from time $t-1$ to time t. We focus on this outcome for two reasons. First, in theoretical models of sovereign default, the government would take into account both the severity of the default punishment (reflected in Δy_τ, where τ is the time at which the government defaults) and the expected length of time that the default punishment will last (reflected in $\Delta y_{\tau+j}$ for $j > 0$). Second, defining real GDP news in this way allows us to clearly relate stock returns to real GDP news. As we will explain in detail in our section on VARs, by assuming that real GDP and real firm dividends are cointegrated, we can show that real GDP news is related to the cashflow news described by Campbell (1991).[42]

We choose the quarterly discount factor, ρ, following Campbell (1991):

$$\rho = \left(\frac{\exp(\bar{pd})}{1+\exp(\bar{pd})}\right)^{\frac{1}{4}},$$

where $\bar{pd}$ is the time-series average log of the (annual) price-dividend ratio for our stock index. In Appendix Table table A7, we present results for our main specifications with alternative values of ρ. Choosing ρ in this way is useful in the VAR analysis, because it allows us to relate real GDP news and firm cashflow news, and we use the same ρ when using forecast data for consistency.

In theory, we could regress realized GDP growth at time $t+j$ on time t returns (and a set of lagged predictor variables), and estimate β in equation (8). This procedure, advocated by Lamont (2001) for U.S. data, uses realized GDP growth as a proxy for expected GDP growth. However, our data on GDP and stock returns for Argentina goes back to 2003.[43] As a result, when we employ this procedure to forecast, for example, three-year-ahead GDP growth, there is a great deal of estimation uncertainty.[44]

To circumvent this issue, we study proxies for GDP news. We consider two different proxies. First, we will study survey expectations of real GDP growth. If survey expectations are equivalent to rational expectations, then this approach would reduce our estimation error, relative to the procedure described above, by avoiding the noise associated with the difference between expected and realized GDP growth. If survey expectations are not rational expectations, but are identical to market participants' expectations, then using survey expectations allows us to recover market participants' beliefs about the impact of default on GDP growth. Finally, if survey expectations are systematically biased, as in Coibion and Gorodnichenko (2010), then our estimates based on survey expectations will be systematically biased as well.

The second method for constructing a proxy for GDP news uses a VAR. We estimate a VAR on a set of variables, including real GDP growth, and then transform the coefficients of this VAR into an estimate for β. In this VAR, the legal shocks are a linear combination of the orthogonalized innovations, and this linear combination is identified by the high-frequency data. The VAR approach has the advantage that it is not subject to some of the potential biases associated with survey expectations. However, it has the disadvantage that it is estimated on a very short sample of data, and uses this data to extrapolate long-run outcomes.

[42] Many authors in the asset pricing literature (e.g. Hansen et al. (2008) and Bansal et al. (2005)) assume that consumption and dividends are cointegrated. Our approach is closely related.

[43] Some of our data series go back to the mid 1990s. However, there are very few ADRs traded during the mid 1990s (our "index" during that time is two or three stocks). Also, the large devaluation and depression in 2001-2002 coincided with a large break in the levels of many of our data series (GDP, inflation, exchange rates, stock prices, etc..). For these reasons, we choose to start our low frequency time series in 2003.

[44] Results available upon request.

In the next two subsections below, we describe these methods in detail, and will then present and interpret our results.

5.1 Survey Forecasts

In this section, we describe how we convert survey forecasts from Consensus Economics into a proxy for real GDP news. Consensus Economics surveys professional forecasters about real GDP growth and a variety other variables, for Argentina and many other countries. Twice a year, in April and October, they conduct a "long-term" forecast survey, in which they ask forecasters to predict real GDP growth for the next five calendar years, and their average forecast for the sixth through tenth calendar year ahead.

To construct our GDP news proxy variable, we use the year-over-year change in the April and October long-term forecasts.[45] To construct our measure, we define

$$\tilde{N}_{y,t} = \left(\sum_{j=0}^{9} \rho^{j} \Delta\tilde{y}_{t+j|t}\right) - \left(\sum_{k=1}^{10} \rho^{k-1} \Delta\tilde{y}_{t-1+k|t-1}\right),$$

where $\Delta\tilde{y}_{t+j|t}$ is the forecast made at time t for real GDP growth at time $t+j$. We have assumed that the forecast for each of the years $6-10$ is equal to the average. This measure, along with the news variables we describe in the next section, are presented in figure 7.

5.2 VAR/Cointegration

In this section, we describe a method of using a VAR, along with a cointegration assumption, to construct our tracking portfolio. We proceed in three steps. First, we estimate a cointegrating relationship between Argentine real GDP and the real dividends of our value-weighted index. Second, we estimate a VAR that includes Argentine real GDP growth, the cointegration residual, and several other variables. Finally, we use our VAR coefficients to construct weights for our tracking portfolio.

As we will discuss below, our results are driven primarily by our estimate of the cointegrating relationship between dividends and GDP. We estimate this equation as

$$y_t = \phi d_t + \delta t + x_t,$$

where x_t is the cointegration residual, y_t is real Argentine GDP, and d_t is the real (in Argentine goods) dividend of our value-weighted index. One might expect that the ratio of dividends to GDP

[45] Because the forecasts are done on a calendar-year basis, forecasts in April and October will have different levels of uncertainty about the various calendar-year GDP growth. We use April-to-April and October-to-October changes to avoid pooling these two types of forecasts.

is stationary, which would be equivalent to assuming $\phi = 1$. However, leverage[46] would cause a firm's dividends and share-repurchases, in a net-present-value sense, to be more volatile than GDP. Additionally, the firms in our sample are the largest firms in the Argentine economy, and not necessarily representative of all economic activity. Moreover, there may be a trend over time in this set of firms' share of economic activity ($\delta \neq 0$). Whether or not we allow for a time trend makes a small difference in the size of our estimates; we present results without the trend. We estimate the parameter ϕ (and δ, for specifications in which $\delta \neq 0$) using dynamic OLS (see Stock and Watson (1993)). We cannot test for the presence of a cointegrating relationship– our data sample runs from 2003 to 2014, and is far too short to reject the null of no cointegration with any power. Instead, we assume that dividends and GDP are cointegrated, and then estimate the coefficients of this cointegrating relationship.

For our VAR, we need a quarterly measure of Argentina's real GDP. Unfortunately, this is complicated by the fact that during this period the government has been reporting an official inflation rate that is well below actual inflation.[47] Because the nominal GDP numbers are less prone to manipulation, the official Argentine real GDP during this period is significantly overstated. To get around this issue, we combine the official nominal GDP data (downloaded from the IMF International Financial Statistics) with the inflation rate calculated by PriceStats following the work Cavallo (2013, 2015).[48] The PriceStats data scrapes prices from online retailers to construct an independent measure of the CPI. The PriceStats series is available beginning in December 2007, and we use this as our price index beginning then. Prior to December 2007, we use the official CPI index (downloaded from Global Financial Data). Our real GDP index is the official nominal GDP series deflated by our constructed CPI index. We do not attempt to account for the different between the GDP deflator and CPI index.

The market-based exchange rate series we used is the ADR blue rate series, which is available for the entire sample period. We impute the dividends for our value index by comparing the total return and price change for the index, following the procedure described in Bansal et al. (2005). This procedure treats the returns from treasury bills, which are 10% of the index, as dividends, ensuring that the level of dividends is always positive (Vuolteenaho (2002)).

After estimating our cointegration parameters, we consider the "companion-form" VAR, which includes real GDP growth Δy_t, the cointegration residual x_t, the official real exchange rate $orer_t$, the change in the log market (ADR-based) nominal exchange rate, Δe_t, and possibly other stationary variables, z_t. In our current implementation, z_t is the log price-dividend ratio of our value index. The official real exchange rate is defined as $orer_t = o_t - p_t + p_t^*$, where o_t is the log official nominal

[46]Here, we are referring to both financial leverage and "operating leverage," the idea that a firm's costs might be sticky relative to firm revenues.

[47]See, for instance, http://www.economist.com/blogs/americasview/2014/09/statistics-argentina.

[48]We downloaded the data from http://www.statestreet.com/ideas/pricestats.html.

exchange rate, p_t is the Argentine price index, and p_t^* is the U.S. price index. We include the official real exchange rate and the change in the log nominal exchange rate to account for the difference between real returns and dollar returns, as we will discuss shortly. This VAR can be written in AR(1) form, ignoring constants, as

$$(I - AL) \begin{bmatrix} \Delta y_t \\ x_t \\ orer_t \\ \Delta e_t \\ z_t \end{bmatrix} = \xi_t,$$

where ξ_t are the VAR innovations, I is the identity matrix, and A is the AR(1) representation of the VAR.

We can derive real GDP news as a function of the VAR innovations (a similar expression appears in Campbell (1991)):

$$\begin{aligned} N_{y,t} &= (E_t - E_{t-1}) \sum_{j=0} \rho^j \Delta y_{t+j} \\ &= e_1^T (I - \rho A)^{-1} \xi_t, \end{aligned}$$

where e_1^T selects the first element of the state vector. We can define dollar cashflows in a manner similar to our definition of real GDP news:

$$N_{d,t} = (E_t - E_{t-1}) \sum_{j=0} \rho^j (\Delta d_{t+j} - \Delta orer_{t+j} + \Delta p^*_{t+j})$$

This dollar cashflow news is the cashflow news denominated in Argentine goods, converted to pesos using the Argentine price index, then converted to dollars at the official nominal exchange rate. Using this definition, we can derive an expression for dollar cashflow news as a function of the VAR innovations and the real GDP news:

$$N_{d,t} = \phi^{-1} N_{yd,t} - \phi^{-1} (1-\rho) e_2^T (I - \rho A)^{-1} \xi_t - (1-\rho) e_3^T (I - \rho A)^{-1} \xi_t,$$

where e_2^T and e_3^T select the second and third element of the state vector, respectively. We have omitted news about U.S. inflation from this expression; we will effectively assume that our legal rulings have no impact on expectations about U.S. inflation, and in unreported results find that they do not predict movements in the "breakeven" inflation rate implied by nominal and real treasury bonds.

Our cointegration assumption is that x_t, the cointegration residual, and Δy_t are stationary. We have also assumed that the official real exchange rate is stationary. We believe the assumption of stationarity for the official real exchange rate is reasonable, for two reasons. First, we find that the market-based real exchange rate, which uses the ADR-based exchange rate, mean-reverts rapidly in our sample; we estimate a quarterly AR(1) coefficient of 0.8.[49] Second, the non-deliverable forward (NDF) rates, which are forward contracts on the official nominal exchange rate, track the ADR-based nominal exchange rate as seen in Figure figure 5. These two observations strongly suggest that the official real exchange rate is stationary. However, in our data sample period, the gap between the official nominal exchange rate and the market-based exchange rate has grown wider and wider. Based on this gap alone, there would be no reason to believe that the official real exchange exchange is stationary. However, both economic theory and the behavior of the NDF rates suggest that, in the long run, the official real exchange rate will be stationary. Note that in our VAR, if the official real exchange rate were estimated to be very persistent, our tracking portfolio could place significant weight on the nominal exchange rate to capture this effect.

The stationarity assumption for the cointegration residual, x_t, requires that $e_2^T(I-A)^{-1}\Sigma(I-A^T)^{-1}e_2$ is finite, where Σ is the covariance matrix of the VAR innovations, and similar expressions involving e_1 and e_3 follows from the stationarity of real GDP growth and the official real exchange rate. It follows that, as the discount factor ρ approaches 1, $\lim_{\rho\to 1^-} E[(N_{d,t}-\phi^{-1}N_{y,t})^2]=0$. That is, dollar dividend news converges, in a mean-square sense, to real GDP news, scaled by the constant ϕ^{-1}. Intuitively, if the discount factor ρ is very close to one, real GDP news and dollar cashflow news measure only nearly-permanent changes in the level of real GDP and dollar dividends, respectively. Because of the stationarity of the real exchange rate, nearly permanent changes in dollar cashflows are also nearly permanent changes in real cashflows. Nearly permanent changes in real cashflows and real GDP must be roughly proportional to each other; otherwise, the cointegration residual would not be stationary. In our application, the time series average of the log, annual price-dividend ratio is roughly 4, implying a 2% annual dividend yield. This translates to a quarterly value for ρ of 0.9955. As a result, dollar cashflow news and real GDP news are essentially equivalent, up to the scaling factor ϕ. In the appendix, table A7, we demonstrate that our results our robust to alternative values of ρ.

We next make the assumption that the dollar returns we estimate in response to the legal shocks are dollar cashflow news, and not discount rate news. We argue for this assumption on several grounds. First, the legal shocks to Argentina are an almost canonical example of idiosyncratic risk. It is very unlikely that US investors' stochastic discount factor is meaningfully affected by these legal rulings. Consistent with this argument, we find no evidence for an impact of these rulings on other emerging market CDS spread and stock indices (see the appendix, section §C). Second,

[49] Note, however, that our sample is too small to rule out (at a 5% confidence level) the possibility of a non-stationary market-based real exchange rate, using an augmented Dickey-Fuller test.

we control for the legal rulings' impact on a variety of proxies for the price of risk, including the S&P 500, VIX, credit indices, oil and soybean prices, and other emerging market stock indices. Consistent with the previous point, incorporating these controls makes little difference for our estimates.[50] Nevertheless, if the legal rulings did change investors' stochastic discount factor, we might expect it to be captured by the price changes in these assets. However, we cannot rule out the possibility that these legal rulings create a shortage of Argentina-specific expert capital, along the lines of Gabaix and Maggiori (2014).[51] This expert-specific capital would have to be specific to the firms we study, and not relevant for Argentine-listed multinationals, such as Tenaris and Petrobras Brazil; we find no statistically significant effects for these firms (see table A4 in the appendix).

Under the assumption that our returns (measured in dollars) are cashflow news, and the result that cashflow news is roughly proportional to real GDP news, we can simply scale our returns by the factor ϕ to estimate the real GDP news. Recall our earlier explanation, leverage, for why we estimate $\phi < 1$. Scaling our returns by ϕ is, in effect, "de-leveraging" our firms' returns. In the appendix, section §E, we present the results of a "crude" deleveraging. These results are similar to the results we get by scaling the index return by ϕ, suggesting that leverage is the primary reason that we estimate $\phi < 1$.

The discussion thus far has focused on the idea that the $O(1-\rho)$ terms in the above expressions are negligible. This would be true if ρ were literally one, but the ρ is our data is close to, but not equal, to one. As a result, if the real exchange rate or the cointegration residual were stationary but very persistent, those terms might be large, even though ρ is close to 1. We will present our results two ways. First, we will show results that neglect the $O(1-\rho)$ terms, assume that $N_{y,t} = \phi^{-1} r_t$, where r_t is the return on our value-weighted index.

Second, we will show results that include these terms. For these results, there is an additional step in the process– we construct a tracking portfolio for VAR-based real GDP news. We do this by transforming our estimated VAR coefficients, as if we were running a regression. Define the vector

$$r_t = \begin{bmatrix} N_{d,t} \\ \Delta e_t - E_{t-1}[\Delta e_t] \end{bmatrix},$$

which contains the surprise returns of our value index (which we assume are equal to dollar cashflow news) and exchange rates. We estimate the model $N_{y,t} = \beta^T r_t + \nu_t$ by computing the coeffi-

[50] Theoretically, the returns could also be caused by increases in the exposure of the ADRs to priced risk factors (an increase in "beta").

[51] If the returns we measure are discount rate news, then we should expect that our legal rulings predict future returns. We have run our heteroskedasticity-based estimator using two-day-ahead returns, rather than contemporaneous returns, as the outcome variable. We found no significant effects, but our standard errors are too large to rule out economically plausible return predictability.

cients β using the standard OLS formula,

$$\beta = E[r_t r_t^T]^{-1} E[r_t N_{y,t}],$$

where these expectations are a function of the parameters ϕ, A, and Σ. This is equivalent to generating time series for real GDP news, dollar cashflow news, and exchange rate surprises from the VAR innovations and parameters, and then running a regression on those generated time series.

After constructing the tracking portfolio, we can run our heteroskedasticity-based procedure to estimate the effect of changes in the default probability on real GDP news. However, before presenting our results, we will discuss how our standard errors account for the estimation error of the coefficients for the tracking portfolio.

5.3 Results and Interpretation

We begin by presenting our coefficient estimates for our three tracking portfolio strategies.

[Insert table 5 here]

The VAR and DOLS coefficient estimates are essentially identical, because the value of ρ we estimate is so close to one. The standard errors for the VAR, which do not account for the estimation error associated with ϕ (see the appendix, section §J), are substantially smaller than the DOLS standard errors. For this reason, the DOLS estimates are our preferred specification. The survey coefficients are not as precisely estimated, and the coefficient on the value index is substantially smaller in magnitude. These differences reflect the different time series properties of the GDP news we estimate through our VAR and the GDP news from the surveys. In the figure below, we present the GDP news time series extracted from our VAR and from the Consensus Economics forecasts. The two time series differ; one possible interpretation, consistent with the literature on survey forecasts (Coibion and Gorodnichenko (2010)), is that the survey forecasts react slowly to news, because respondents face information processing constraints. An alternative interpretation is that the survey respondents are generating their forecasts ahead of the survey date, or that the truncation of the consensus forecasts at the 10-year horizon impacts the results. Yet another interpretation is that our VAR is not correctly specified or poorly estimated.

[Insert figure 7 here]

The similarity between the VAR GDP news time series and the return on our value-weighted index, scaled by our estimate of ϕ, is not entirely mechanical. The cointegration assumption essentially imposes that GDP news and cashflow news are proportional. What the results of the

VAR show is, for our value-weighted index, a significant portion of the returns are caused by cashflow news, and not news about future returns. This is consistent with the existing literature that looks at the variance decomposition of returns for individual stocks (Vuolteenaho (2002)).

We next present results that apply the CDS-IV estimator to these tracking portfolios. In the table below, we list the CDS-IV estimator results for the value-weighted ADR index and dolar blue exchange rate, which are the two assets in our tracking portfolios. We then list the CDS-IV estimation results for our various GDP news tracking portfolios. The point estimates for the VAR and DOLS estimates are nearly identical, which is not surprising given the similarity of the tracking portfolio coefficients. The point estimates for the survey forecasts are statistically significant but roughly half the magnitude of the VAR/DOLS estimates. This is consistent with the findings of Coibion and Gorodnichenko (2010), who also use the Consensus Economics forecast data, but focus on developed countries, and find that survey forecasts incorporate information gradually.

[Insert table 7 here]

Our point estimates imply that, as Argentina moved from a 40% 5-year probability of default to a 100% probability, this caused a 6% (DOLS/VAR) or 2.4% (Survey) decline in the discounted present value of GDP growth. We next discuss how to interpret these results in the context of the existing sovereign default literature. We will use as our benchmark the calibration of Aguiar and Gopinath (2006). In that paper, the country in default loses 2% of its real GDP, and the loss persists until the country is "redeemed," which occurs with a 10% probability each quarter. Suppose that at time t, it was revealed that the country would default at time $t+\tau$ with certainty, and that previously the default probably was zero. The real GDP news would be

$$
\begin{aligned}
N_{y,t}^{AG} &= -2\% \times \rho^{\tau} + 2\% \times 10\% \times \sum_{j=0}^{\infty} \rho^{\tau+j+1}(1-10\%)^{j} \\
&= \rho^{\tau} \times 2\% \times (-1 + \frac{\rho \times 10\%}{1-\rho \times 90\%}).
\end{aligned}
$$

In the case of $\tau = 0$ and $\rho = 0.9956$, the real GDP growth news would be roughly -0.1%, almost two orders of magnitude smaller than our VAR/DOLS estimates. If we instead used $\rho = 0.95$, which would correspond to an extremely high dividend yield, the real GDP news in this model would be about -0.7%, still far below our estimates.

Why are our estimates so much larger than those implied by standard models? The key feature of the model that prevents it from having a large GDP news effect upon default is the assumption that the impact of default is transitory. In the calibration of Aguiar and Gopinath (2006), the average time until redemption is 10 quarters; this is based on the work of Gelos et al. (2011), who document that it typically takes about that much time for a defaulting sovereign to regain access to international capital markets.

The implicit assumption in this calibration is that the output costs of default are related to market access. Our results argue against this assumption. Although Argentina (the sovereign) was unable to issue foreign-law bonds both before and after the legal rulings we use for identification, Argentine firms have issued foreign debt both before and after these rulings. To argue that the output costs we measure are related to market access, one would need to believe that market access for the sovereign had a large impact on firms, even though those firms were themselves able to borrow, and that the cumulative effect of the legal rulings substantially increased the expected duration of the Argentine government's exclusion from foreign debt markets.

Instead, suppose that during the default period, the defaulting country experiences reduced real GDP growth by 1% per quarter, and then upon redemption, ceases to suffer a real GDP growth penalty, but never experiences elevated GDP growth. In this case, the real GDP news would be

$$\begin{aligned} N_{y,t}^{HS} &= -1\% \times \sum_{j=0}^{\infty} \rho^{\tau+j}(1-10\%)^{j} \\ &= \rho^{\tau} \times 1\% \times (\frac{1}{1-\rho \times 90\%}). \end{aligned}$$

In the case of $\tau = 0$ and $\rho = 0.9956$, the real GDP news would -9.58%, which is very close to our VAR/DOLS point estimate.

We next ask why the growth that is lost during the default period is never regained, even after market access is restored. In this respect, the effects of a sovereign default would be similar to other shocks in emerging market countries; Aguiar and Gopinath (2007) find that shocks to trend growth explain most of the fluctuations in emerging market countries. Aguiar and Gopinath (2007) attribute the persistence of shocks in emerging markets to the frequency of policy regime switches. We are sympathetic to this view, and believe it also offers a plausible explanation for why the effects of sovereign default could persist.

To explore this idea further, in the next section we study the effects of the default shocks on the cross-section of Argentine firms. If the hypothesis of Aguiar and Gopinath (2007) explains why defaults have persistent effects, then we would expect that firms that are particularly subject to policy regime changes would be most affected by our legal rulings. We will also attempt to test other mechanisms that have been posited in the literature.

6 Cross-Sectional Evidence

In this section, we examine which firm characteristics are associated with larger or smaller responses to the default shocks. The cross-sectional pattern of responses across firms can help shed light on the mechanism by which sovereign default affects the economy. First, we examine how different industries respond to default shocks. Second, we examine the heterogeneous firm responses to an increase in the probability of default, through the lens of different theories on the channel by which sovereign default affects the broader economy.

In their seminal contribution, Eaton and Gersovitz (1981) argue that the reason governments repay their debt is to maintain their reputation and ensure continued access to international bond markets. Because this access allows governments to smooth income fluctuations, it is valuable and is generally sufficient to guarantee repayment.[52] Because of the threat of attachment from outstanding creditors, Argentina had not issued a new international bond in thirteen years and was unlikely to do so soon. This suggests that the effect of default that we measure is different than the reputational mechanism posited in Eaton and Gersovitz (1981).[53] Instead, this points to the importance of alternative theories of sovereign default costs, examined in the literature following Bulow and Rogoff (1989b). We will attempt to examine the empirical relevance these hypothesized costs of sovereign default by examining whether four groups of firms are particularly affected by default: exporters, importers, banks, and foreign-owned companies.

First, motivated by Bulow and Rogoff (1989a), we will examine whether or not firms that are reliant on exports are particularly hurt. Bulow and Rogoff (1989a) argue that in the event of a sovereign default, foreign creditors can interfere with a country's exports. We would therefore expect exporters to underperform in response to increases in the probability of default. Using aggregate data, Rose (2006) and others have found support for this channel. Second, motivated by Mendoza and Yue (2012), we will examine whether or not firms that are reliant on imported intermediate goods are particularly hurt by default. Mendoza and Yue (2012) argue that a sovereign default reduces aggregate output because firms cannot secure financing to import goods needed for production, and so are forced to use domestic intermediate goods, which are imperfect substitutes. This would lead us to expect firms that are relatively more reliant on imported intermediate goods would underperform in response to a default shock. Third, motivated by Gennaioli et al. (2014), Acharya et al. (2014a), Bolton and Jeanne (2011), Bocola (2013) and Perez (2014), we will examine whether financial firms are more adversely affected. While these papers are not explicitly about whether banks are hurt more than other firms, they posit that the aggregate decline in output

[52] Tomz (2007) provides a historical account to argue in favor of the reputational model of sovereign debt. English (1996) argues that the experience of US states in the 1840s provides evidence in favor of the reputational model of sovereign default by arguing that no direct sanctions were available to creditors. The Eleventh Amendment prevents foreign creditors from suing US states to receive payments on defaulted debt, constitutionally guaranteed interstate free trade prevents foreign creditors from locking defaulters out of trade markets, and the US federal government prevents foreign creditors from using force to collect on the debt. English demonstrates that defaulting states are unable to borrow again for a number of years, concluding that the concern for maintaining a reputation for repayment is therefore the only explanation for continued repayment.

[53] We are not providing evidence against the importance of the the type of reputational concerns in Eaton and Gersovitz (1981), but rather arguing that this particular default is not likely to be affected by such concerns.

following a sovereign default occurs because of the default's effect on bank balance sheets. This leads to a reduction in financial intermediation and a reduction in aggregate production. If this argument or something like it were correct, we would expect banks to be hurt disproportionately by an increase in the probability of default. Finally, motivated by Cole and Kehoe (1998) and Aguiar and Gopinath (2007), we examine whether foreign-owned firms underperform following an increase in the probability sovereign default. Cole and Kehoe (1998) argue that even if the loss of a reputation for repayment alone is not sufficient to motivate countries to repay their debt, if their "general reputation" is lost by defaulting on sovereign debt, foreigners would become less willing to trust the defaulting government. This theory would lead us to expect increases in the risk of sovereign default to cause foreign-owned firms to underperform, as foreigners perceive a higher risk that Argentina will act disreputably in other arenas, such as investment protection. Aguiar and Gopinath (2007) attribute the persistent of shocks in emerging markets to frequent and significant policy changes in those countries. The theory of Cole and Kehoe (1998) suggests that foreign-owned firms might be particularly affected by these policy changes.

Our empirical approach is similar to several papers in the literature studying the cross-section of firms' responses to identified monetary policy shocks, using an event study for identification. Bernanke and Kuttner (2005) study U.S. stock market data and find that the response of various industry portfolios to a monetary policy shock is proportional to that industry's CAPM beta. Put differently, the ensemble of shocks that generate returns outside of the event windows have a similar cross-sectional pattern of returns to the monetary policy shock. Gorodnichenko and Weber (2013) find that a measure of the stickiness of firms' prices is correlated with the squared magnitude of firms' response to squared monetary policy shocks. We apply similar strategies to our context. First, we explore the abnormal returns for various industries in response to a default probability shock, controlling for the abnormal return of the Argentine market. Second, we form portfolios based on firm characteristics suggested by theory and then study the abnormal returns of those portfolios, again controlling for the abnormal return of the Argentine market.

Our procedures are motivated by a modified version of the model in equation (2) and equation (1). We derive both models from a single underlying system of equations, presented in the appendix, section §I. The modified version of the those equations has the return of the Argentine market index, $r_{m,t}$, on the right-hand side, in addition to the observable factors X_t and unobservable factors F_t. We denote the return of a particular stock or portfolio as $r_{i,t}$:

$$\Delta D_t = \mu_D + \omega_D X_t + \gamma r_{i,t} + \gamma_m r_{m,t} + \kappa_D F_t + \varepsilon_t \tag{9}$$

$$r_{i,t} = \mu_i + \omega_i X_t + (\alpha_i - \beta_i \alpha_m)\Delta D_t + \beta_i r_{m,t} + \kappa_i F_t + \eta_{i,t}. \tag{10}$$

The parameter α_m is the response of the Argentine market index, $r_{m,t}$, to the default shock. For the purposes of our study, this two equation system has exactly the same form as the system described in section §4. The Argentine market return, $r_{m,t}$, is an observable common factor, no different from the S&P 500 or other observable factors in X_t. The Rigobon (2003) procedure, applied to this system, identifies the coefficient $(\alpha_i - \beta_i \alpha_m)$, which can be interpreted as the excess sensitivity of the portfolio to the default shock, above and beyond what would be expected from the Argentine market's exposure to the default shock, and the sensitivity of the portfolio to the Argentine market. In this sense, our approach generalizes the CAPM-inspired analysis of Bernanke and Kuttner (2005) to a model with multiple exogenous shocks.

We begin by studying the response of industry portfolios to default shocks, controlling for the response of the Argentine market. To increase our sample size of firms, we use local Argentine stock returns, rather than ADRs. We convert the local stock returns, denominated in pesos, into dollars using the ADR-based blue rate described previously. For stocks with ADRs, the converted return will be nearly identical to the ADR return.[54] The use of the ADR-based exchange rate requires that both the New York and Buenos Aires stock markets be open, which reduces the size of our sample. However, the events in our sample remain the same, with one exception.

We group these firms into equal-weighted industry portfolios, using the industry definitions described in section 3.1. We also construct an equal-weighted index of all of the firms in our sample, which is restricted to firms passing a data quality test also described in section 3.1. We use this equal-weighted index as our measure of the Argentine market return. In Figure 8 and Table 8 below, we display estimates of the excess sensitivity of the industry portfolios to the default shock, using the CDS-IV estimator and the bootstrapped confidence intervals described in the previous sections.

[Insert figure 8 here]

[Insert table 8 here]

Four industries (banks, telecoms, real estate, and utilities) stand out as over- or under-sensitive to default shocks. However, care must be taken when interpreting the results. First, the confidence intervals around these estimates are very wide. Our point estimates suggest that a 10% increase in the probability of default would cause bank and telecom stocks to fall by roughly 1% more than would be expected, given their beta to the Argentine index, and would cause real estate stocks and utilities to fall by 2% less than would be expected. However, the standard deviation of these estimates is almost 1%, and only the telecom's under-performance is significant at the 95% confidence interval. The uncertainty around our point estimates is driven by the small number of events

[54] As mentioned previously, the implied exchange rate between various stock-ADR pairs does not vary substantially across firms.

we study, and the idiosyncratic variation in stocks' response to the different legal announcements. Second, our confidence intervals have not been adjusted for multiple testing; the fact that one industry has significant over- or under-performance at the 95% confidence level is not surprising, given the number of tests being performed.

That said, our point estimates are economically large. Taken at face value, our results suggest that as Argentina went from a 40% to 100% probability of defaulting, its banks' value fell by 6% more (in dollar terms) than would have been expected, given a 38% fall in the dollar value of the equal-weighted index. The excessive sensitivity of bank stocks to default risk is consistent with the theories of Gennaioli et al. (2013, 2014), Bocola (2013), and Bolton and Jeanne (2011). We interpret our data as providing suggestive evidence for these theories.[55]

We next consider which characteristics of non-financial firms are associated with over- or under-performance in response to default shocks. As discussed in section 3.1, we form zero-cost, long-short portfolios of non-financial firms based on the export intensity of their primary industry, the import intensity of their primary industry, whether they are a listed subsidiary of a foreign firm, and whether they have an associated ADR. An import-intensive industry is not the opposite of an export-intensive one; some industries are classified as neither import nor export intensive, whereas others are both import and export intensive.[56] Finally, we compare firms with and without ADRs.

In these portfolios, we equally weight firms within the "long" and "short" groups. For example, we classify 12 of our 26 non-financial firms[57] as high export intensity, and 14 of 26 as low export intensity. We equally weight these firms, so that the "long" portfolio has a 1/12 weight on each high export intensity firm, and the short portfolio has a 1/14 weight on each low export intensity firm. We then form the long-short portfolio, and determine whether the portfolio over- or under-performs after a default shock, using the CDS-IV estimator and bootstrapped confidence intervals discussed previously.

[Insert figure 9 here]

[Insert table 9 here]

In Figure 9 and Table 9, we find that firms whose primary industry is export-intensive under-perform, while firms whose primary industry is import intensive over-perform expectations, given

[55] Regarding the outperformance of utilities, one market participant suggested to us that pressure on the Argentine government's foreign reserves, exacerbated by the default, might lead them to liberalize utility prices. In the status quo, underpriced electricity (for example) leads to over-consumption, which results in excessive importation of utilities' inputs. Excessive imports reduce the Argentine government's foreign reserves position, and their inability to borrow makes it difficult to replenish these reserves. This story is one possible explanation for why utility companies could indirectly benefit from a sovereign default, relative to other companies.

[56] The correlation is our sample of non-financial firms is 0.16.

[57] We in fact have 27 non-financial firms, but one is a technology firm. The technology firm's industry classification did not exist when the input/output table we use to construct the data was generated.

their exposure to the equal-weighted index and the index's response to the default probability shock. We find that our long-short exporter portfolio underperforms 0.25% more for each 1% increase in the risk-neutral probability of default than would be expected given the portfolio's loading on the market index, and our long-short importer portfolio outperforms by about the same amount. However, our results about import intensive firms are not robust to changes in the portfolio formation threshold. In unreported results, we find that using a 4% threshold for import intensity, instead of 3%, results in all of the utilities being reclassified from high import intensity to low import intensity. Because the utilities responded far less to default shocks than their beta would predict, their reclassification is sufficient to change the sign of the results. In contrast, we find that the results for exporters are qualitatively robust to variations in the threshold.

The over- or under-performance of the export and import portfolios is not an ideal test of the theories. In the context of the Bulow and Rogoff (1989a) theory, if we do not observe that exporting firms under-perform, it may be because the firms we observe are not the ones whose exports would be seized, or because our export-intensive and non-export-intensive firms also differ on some other characteristic that predicts over- or under-performance.[58] The reverse is also true; a significant result does not necessarily validate the theory, but might instead be found because of a correlation across firms between exporting and some other firm characteristic.

We also find that foreign subsidiaries, of which there are ten, underperform relative to non-financial firms that are not foreign subsidiaries. The long-short portfolio falls 0.19% more in response to a 1% increase in the risk-neutral probability of default than would be expected given the portfolio's loading on the index. This result is consistent with the general reputation theory of Cole and Kehoe (1998), implying that policy changes become more likely and foreign investors become more reluctant to invest, although there are many other possible interpretations. However, this result is not statistically significant. We do not find that firms with an ADR substantially under- or out-perform firms without ADRs.

We interpret this cross-sectional analysis as lending modest support to several of the theories in the existing literature that try to understand the costs of sovereign default. The theories are not exclusive; sovereign default may harm the financial system, impede trade, and weaken a country's reputation in many areas. Our estimates are insufficiently precise to reject any of these theories, or speak to their quantitative importance. Nevertheless, our approach does have the advantage over the existing literature that we can pinpoint the direction of causality, from sovereign default to performance, in a way that would be very difficult using aggregate or annual data.

[58] Essentially, an omitted variables problem

7 Interpretation

We begin by describing an imaginary "ideal experiment" to identify the causal effect of default on economic activity. We will then discuss the ways in which our research design does and does not approach this ideal. We will offer alternative interpretations of the effect of the legal rulings, and discuss their implications for the interpretation of our results. We also discuss several additional aspects of Argentina's situation that are relevant.[59]

The ideal experiment would randomly induce one of two otherwise-identical groups of countries to default on their debt. These groups of countries would have characteristics similar to those of typical sovereign borrowers. The treatment (default) would be randomly assigned, so that it would be uncorrelated with the underlying state of the countries' economies. The treatment would induce a country to default, but would otherwise neither encourage nor impair other actions by that country's government, firms, or households. The null hypothesis in this experiment is that default does not affect economic activity. The alternative hypothesis is that default impairs economic activity, through some unspecified channel.

We emphasize the idea of "inducing" a country to default because we view default as a choice of the government. For the purposes of understanding why sovereign borrowers repay their debts, we would like to understand the consequences of them choosing not to repay. These consequences include the effects of whatever mitigating actions a country might take after having decided to default. These consequences also include the effects of firms, households, and other agents changing their behavior as a result of the default. The government's actions could include renegotiating with creditors, finding other means to borrow, balancing budgets via taxes or reduced spending, and taking actions that affect the convertibility of the currency, among other actions. When we refer to the causal of effects of sovereign default, we include the anticipated effects of whatever policies the government is expected to employ as a result of having defaulted.

Our research design differs from this ideal experiment in a variety of ways. First, we study Argentina, a country whose experience with sovereign debt is very different from most other countries. Argentina is in some sense in default for the entirety of our sample, depending on the definition of "default." It has an unusual currency regime, and defaulted for convoluted legal reasons. Additionally, the way in which Argentina acts to mitigate the consequences of its default might be different from the way other countries would respond in similar circumstances. Second, there is the issue of whether the default is exogenous to Argentina's economic circumstances. Third, these legal rulings might have effects on firms' stock prices, through channels other than changes in the likelihood of default (the exclusion restriction may not hold). If the legal rulings compelled Argentina to repay a large amount of money, relative to its economy or foreign reserves, then firms'

[59] Alfaro (2014) examines the implications of the legal rulings on future sovereign debt restructurings.

stock prices might fall due to the burdens of debt repayment and associated reduction in economic activity, rather than through any default-related effects.

In the reminder of this section, we will discuss each of these issues in more detail.

7.1 The Options Available to Argentina

It is not clear that Argentina was forced to default. Prior to these legal rulings, Argentina had several feasible courses of action with respect to its restructured debt and the holdouts. It could maintain the status quo, in which it was subject to attachment orders and other actions by the holdouts, while it continued to pay its restructured creditors. It could attempt to negotiate with the holdouts, and completely resolve its default. Finally, it could choose to default on its restructured creditors.

The cumulative effect of these legal rulings changed the menu of options available to Argentina. The status quo option, in which Argentina continued to pay its restructured bondholders without paying the holdouts, became infeasible.[60] Instead, Argentina could make payments on its debt, which would be divided between the restructured bondholders and the holdouts according to the "ratable payment" formula devised by Judge Griesa. Alternatively, it could attempt to negotiate with the holdouts, to avoid defaulting on its restructured bondholders. Finally, it could default on the restructured bondholders.

Argentina effectively chose the third option (default). It made a payment to the Bank of New York Mellon (BNYM), the trustee for its restructured bonds, that was sufficient to pay the restructured bondholders, without paying anything to the holdouts. Judge Griesa's order prohibited BNYM from forwarding this payment to the restructured bondholders, and Argentina missed a coupon payment. After the 30-day grace period, Argentina was declared in default.

As of this writing, how the situation will be resolved is unclear. One recent proposal involves replacing BNYM with another, non-U.S. trustee, who would not be subject to the U.S. courts' orders, and could continue to pay the restructured bondholders. Another complication concerns the treatment of euro-denominated bondholders, whose coupon payments are included in the amount held by BNYM. These bondholders have argued that BNYM acted contrary to Belgian and U.K. law, and that they should continue to be paid.

The cumulative effect of the legal rulings raised the probability of default on the restructured bonds and/or payment of the holdouts, relative to the probability that the status quo would continue. If Argentine firms would be affected by payment of the holdouts, holding default or no default fixed, then the exclusion restriction of our experiment would not hold.

One possibility is that the legal rulings might change the probability or size of a settlement

[60] This infeasibility might be temporary or permanent– it is not clear as of this writing.

with the holdouts, and this could affect the firms. Under the null hypothesis, if the government somehow repaid the holdouts without fiscal consequences (say, using a gift from abroad), there would be no effect on firms. In reality, because the government would need to raise taxes, cut spending, or borrow to repay the holdouts, an increase in the probability or size of a settlement with the holdouts could harm firms.

To get a sense of whether this is reasonable, we consider the extent to which the bonds owned by the holdouts appreciated, on our event days. We believe that the increase in the expected value of the holdout bonds is dwarfed by the cumulative losses of the Argentine firms.[61] This suggests that if, in expectation, the entirety of the burden of repayment fell on these firms, that would only explain a small part of the stock market declines. A very large "multiplier" for the loss of equity value associated with the debt burden would be required for this argument to apply. In Appendix table A8, we report the effect of increases in the probability of default on the price of the defaulted bonds held by the holdouts, the restructured bonds that Argentina eventually defaulted on in July 2014, and domestic-law dollar debt.

More generally, the legal rulings could have had other effects. However, Argentine corporations are legally independent from the Argentine government, and their assets cannot be attached by the holdouts.[62] The ruling affects them only to the extent that it changes the behavior of the Argentine government or other actors. This still leaves open several possible effects. The legal rulings could have provoked the government of Argentina into a sequence of actions unrelated to sovereign default. They could have influenced the probability that the current government of Argentina stays in power in the next election, for reasons unrelated to the default. The legal rulings could have changed the law regarding sovereign debt generally.

We can muster evidence against this last effect. In the appendix, section §C, we show that the stock markets and sovereign CDS spreads of Brazil and Mexico did not respond to these legal rulings (our estimates are close to zero, and relatively precise). This is in contrast to the OLS estimates, which show that those financial variables are correlated with the Argentine risk-neutral probability of default, presumably due to common shocks affecting Latin America or emerging markets more generally. This evidence suggests that, whatever changes to sovereign debt law occurred as the result of these rulings, they did not materially impact other Latin American countries that issue debt in New York.

However, we cannot rule out every possible channel through which these rulings might have affected firms, other than via sovereign default. Ex-post, it appears that the primary response of the Argentine government to these rulings was default. We are unaware of any direct consequences for

[61] These calculations are available upon request.

[62] There was litigation regarding whether the Argentine central bank qualified as independent from a legal perspective, but no such litigation for any of the companies listed in the stock index.

Argentine firms. Consistent with this interpretation, S&P did not downgrade any Argentine firms immediately upon the sovereign's default (Standard and Poor's 2014a). However, it subsequently downgraded a variety of firms, arguing that deteriorating economic conditions reduced those firms' credit quality (Standard and Poor's 2014b).

7.2 How Much Would Argentina Have to Repay?

To meet the precise demands of the courts, Argentina needed to pay its litigating creditors only \$1.5 billion. However, the \$1.5 billion owed to the litigating creditors was only around 10% of the estimated \$15 billion holdout debt outstanding.[63] Presumably, if Argentina paid NML and its co-litigants in full, the other holdout creditors would demand repayment on similar terms. Even if we assume that Argentina would need to pay the full \$15 billion, that represented only 3% of GDP,[64] and 45% of foreign currency reserves.[65]

However, it is possible that if Argentina did indeed pay the holdouts in full, it would then owe the restructured creditors a large payment as well. During its 2004-2005 debt restructuring, Argentina sought to convince its creditors that the unilateral offer it made was the best offer they would ever receive. Argentina included a "Rights Upon Future Offers" (RUFO) clause in the bond prospectus of the restructured debt.[66] If Argentina made an offer to the holdouts that was better than what the restructured creditors received, the restructured creditors would be entitled to the better deal, provided the offer occurred before December 31, 2014. Argentina claimed that this RUFO clause meant that it could not pay NML the \$1.5 billion owed without incurring hundreds of billions in additional liabilities. There is one crucial word in the RUFO that makes the whole matter more complicated: *voluntarily*. If Argentina offered the holdouts a better deal because US courts would otherwise have blocked its payments to the restructured bondholders, would that be voluntary or involuntary? Indeed, some observers noted that Argentina's counsel told the Second Circuit Court of Appeals that Argentina "would not voluntarily obey" court rulings to pay the holdouts in full.[67] In addition, other commenters noted that the RUFO appeared to have some loopholes, allowing Argentina to potentially settle with the holdouts without triggering the clause.[68] Finally, exchange bondholders could waive their right to exercise the RUFO, and because it takes 25% of exchange bondholders to trigger the clause, the whole issue could have been rendered moot

[63] See Gelpern (2014a).

[64] The CIA World Factbook reports Argentina's 2013 GDP as \$484.6 billion. However, this calculation uses the official exchange rate, which may overstate the size of Argentina's economy.

[65] The CIA World Factbook reports Argentina's foreign exchange and gold reserves at \$33.65 billion as of December 31, 2013.

[66] Olivares-Caminal (2013) refers to this as the "most favored creditor clause."

[67] http://ftalphaville.ft.com/2013/03/06/1411442/raising-the-rufo-in-argentine-bonds/

[68] See the comment's from Barclay's reported in FT Alphaville: http://ftalphaville.ft.com/2013/03/06/1411442/raising-the-rufo-in-argentine-bonds/

if the exchange bondholders could be persuaded that this was preferable to having their coupon payments blocked.[69] Of course, this possibility assumes Argentina would have paid any amount to the holdouts, a questionable proposition given the domestic politics surrounding the holdouts.[70]

For the purposes of interpreting our results, the RUFO clause complicates matters in two ways. First, if the RUFO clause was binding and could not have been easily circumvented, negotiation with the holdouts may not have been feasible. In this case, it would be correct to say that the legal rulings forced Argentina to default (the simple interpretation offered previously). Second, if the RUFO clause was binding, but the legal rulings compelled Argentina to involuntarily pay the holdouts (and therefore circumvented the RUFO clause), they might have made renegotiation feasible when it was not previously feasible. Finally, if the RUFO clause was not binding, it does not alter the interpretation of the rulings discussed above.

The RUFO clause expired on December 31, 2014 but, as of the time of this writing (September 2015), a settlement has not yet been reached.

7.3 Are the Legal Rulings Exogenous?

We argue that the rulings of the courts are not influenced by news about the Argentine economy. Formally, the interpretation of the laws in question does not depend on the state of the Argentine economy. Substantively, because the amount required to repay the holdouts in full was small relative to the Argentine economy, news about the Argentine economy's prospects would not materially change their ability to pay. Moreover, even if the judges were responding to economic fundamentals, under the null hypothesis that default does not affect fundamentals, the judges would have no information advantage over market participants. It follows that the effects of economic news on the judges' rulings would be anticipated by the market prior to those rulings, and any response by the market to the judges' rulings would not reflect news about fundamentals.

More subtle interactions between the state of the Argentine economy and the legal rulings might complicate the interpretation of our analysis. For example, if bad news about the Argentine economy causes the market response to the legal rulings to be larger than it otherwise would have been, our estimates will reflect some sort of average effect, where the averaging occurs over states of the economy. Relatedly, the underlying state of the economy might influence the Argentine government's decision about whether to negotiate with the holdouts or default, and therefore interact with the legal rulings to determine the extent to which the default probability and stock prices change. These issues emphasize that our estimates should be considered average treatment effects, relevant to Argentina.

[69] See Gelpern (2014b).
[70] See Gelpern (2014b).

It is important that our event study avoid announcements by the Argentine government, because such announcements might be responding to news about fundamentals, or affect corporations in ways other than through default. In the case of the Supreme Court decision discussed earlier, the Argentine government did not respond immediately to the ruling.[71] More generally, we include as events only orders by a judge or judges. We exclude orders that were issued during oral arguments, because those events also include opportunities for lawyers representing Argentina to reveal information.

Our identifying assumption is that the variance of "legal shocks" is higher on days when a US court rules on the dispute between NML and Argentina while the variance of all other shocks remain the same. However, if in addition to shocks to economic fundamentals, and legal shocks, we imagine that there are "political shocks" which move the probability of Argentina defaulting on its debt, then it could be that the variance of these shocks are higher on event days because the government is more likely to make a pronouncement revealing how likely it is to default following a ruling by Judge Griesa.[72]

7.4 Was Argentina Already in Default?

Although the debt exchanges of 2005 and 2010 eventually achieved a participation rate of 91.3%, above the level generally needed by a sovereign to resolve a default and reenter capital markets,[73] Argentina remained unable to borrow internationally. This is because the ongoing creditor litigation had resulted in an attachment order, which would allow the holdouts to confiscate the proceeds from a new bond issuance.[74] However, ratings agencies took a different view, and on June 1, 2005, S&P declared the end of the Argentine default and gave them a sovereign foreign currency credit rating of B-.

There are several complications arising from Argentina's ambiguous international standing. If the costs of default for Argentina were lower than that of a typical sovereign debtor, because Argentina was already unable to borrow in international markets, then our estimates understate the costs for the typical sovereign. On the other hand, because Argentina chose to default despite an ability to pay, the costs might be higher than is typical. Complicating the story further is

[71] See the following Bloomberg story: Link.

[72] Based on news stories, we believe that such "political shocks" are no more likely on event days than non-event days. Even if such political events are more likely one event days, our research design is valid. In this case, we would be identifying the causal effect of the rulings on default, inclusive of both the ruling's direct effect and the Argentine government's endogenous response. However, if the political events were more likely on event days but unrelated to the issue of default, or affected firms through some mechanism other than default, our identification would fail. We see no apparent reason for political events to be more likely on event days, unless they are related primarily to the legal rulings.

[73] Hornbeck (2013).

[74] Hornbeck (2013).

that the Argentine government was still able to borrow in local markets, and via inter-country loans. In the aggregate, the country of Argentina was able to run a current account deficit, because its households, firms, and even local governments were able to borrow internationally, despite the inability of its federal government to do so. Therefore, even if the federal government of Argentina was in default for our entire sample, it is not clear that (as a country) it was locked out of international markets, before or after the latest default. These complications emphasize the uniqueness of Argentina's circumstances.

8 Conclusion

For several decades, one of the most important questions in international macroeconomics has been "why do governments repay their debts?" Using an identification strategy that exploits the timing of legal rulings in the case of *Republic of Argentina v. NML Capital, we* present evidence that a sovereign default significantly reduces the value of domestic firms, and the expected level of future output. We provide suggestive evidence that exporters, foreign-owned firms, and banks are particularly hurt by sovereign default.

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Tables and Figures

Table 1: Summary Statistics

Window Type	Event	Non-Event
Mean ΔD_t (%)	0.84	-0.02
SD ΔD_t (%)	6.03	1.80
Mean Equity Return (%)	0.15	0.03
Equity Return SD (%)	3.37	2.58
$\mathrm{Cov}(\Delta D_t, r_t)$	-14.99	-2.01
Number of two-day windows	16	388

Notes: This table reports the mean default probability change, the standard deviation of default probability changes, the mean value-weighted index return, the standard deviation of that return, and the covariance of default probability changes and that return during events and non-events. The underlying data is based on the two-day event windows and non-events described in the text.

Table 2: Firms Included in Analysis

Company	Ticker	Industry	Exports	Imports	Market Cap (2011)	ADR	ADR Sample	Foreign
Aluar	ALU	Manufacturing	19.4	13.1	9443.0			
Banco Santander Rio	BIO	Banks	1.5	0.4	12786.1	Y		Y
Boldt	BOL	Telecoms			1537.5			
Banco Patagonia	BPT	Banks	1.5	0.4	3488.4	Y		Y
Banco Macro Bansud	BSU	Banks	1.5	0.4	9379.2	Y	Y	
Carlos Casado	CAO	Real Estate	0.0	0.0	378.1	Y		
Endesa Costanera	CEC	Utilities	0.1	3.2	702.6			Y
Celulosa	CEL	Chemicals	11.2	19.2	760.3			
Central Puerto Rights	CEP	Utilities	0.1	3.2	1814.4			
Grupo Clarin	CLR	Telecoms	0.4	1.3	5633.4	Y		
Sociedad Comercial Del Plata	COM	Diverse	1.5	0.4	212.3			
Capex	CPX	Utilities	0.1	3.2	1087.8			
Cresud	CRE	Non-Durables	14.5	2.1	3495.9	Y	Y	
Edenor	EDC	Utilities	0.1	3.2	1894.5	Y	Y	
BBVA Banco Frances	FRA	Banks	1.5	0.4	7723.6	Y	Y	Y
Pampa Energia	FRG	Utilities	0.1	3.2	3417.2	Y	Y	
Gp Finance Galicia	GGA	Banks	1.5	0.4	7125.7	Y	Y	
Hipotecario Naci	HPD	Banks	1.5	0.4	3540.0	Y		
Solvay Indupa	IND	Chemicals	11.2	19.2	1218.0			Y
IRSA	IRS	Real Estate	0.0	0.0	3350.5	Y	Y	
Juan Minetti	JMI	Manufacturing	3.6	5.3	1633.5			Y
Ledesma	LED	Non-Durables	14.5	2.1	4004.0			
Metrogas	MET	Utilities	0.1	3.2	677.3	Y		
Mirgor	MIR	Manufacturing	10.1	11.2	512.0	Y		
Molinos Rio De La Plata	MOL	Non-Durables	19.5	2.8	8014.4			
Quickfood	PAY	Non-Durables	19.5	2.8	641.9			Y
Petrobras Energia	PER	Energy	25.5	2.1	8228.4	Y	Y	Y
IRSA Propiedades Commerciales	SAM	Real Estate	0.0	0.0	2960.1	Y	Y	
Moli Juan Semino	SEI	Non-Durables	19.5	2.8	325.5			
Siderar	SID	Manufacturing	19.4	13.1	10893.1			Y
Sa San Miguel	SMG	Non-Durables	19.5	2.8	491.1			
Telecom Argentina	TEC	Telecoms	2.7	8.1	21754.8	Y	Y	Y
Transportadora De Gas Del Sur	TGS	Energy	25.5	2.1	2558.3	Y	Y	
Transportadores De Gas Del Norte	TN4	Utilities	0.1	3.2	540.4			
Transener	TRA	Utilities	0.1	3.2	640.3			
YPF	YPF	Energy	14.2	8.4	74532.8	Y	Y	

Notes: This table lists the 35 firms used in the analysis of local equities, and one firm (ticker SAM) whose ADR is included in our ADR sample, but whose local stock returns do not pass our data quality requirement. Ticker indicates the company's ticker in Datastream. Exports denotes the ratio (in percentage terms) of exports to total output for the firm's primary industry. Imports denotes the ratio (in percentage terms) of intermediate imports to total output for the firm's primary industry. Both Exports and Imports are calculated by classifying the firm into one of the 37 industries in the OECD STAN Input-Output Table according the SIC code of the firm's primary industry. Market Cap (2011) is the firm's average end-of-quarter market capitalization in 2011 from Bloomberg, measured in Argentine pesos. ADR is an indicator for whether the firm currently has an American depository receipt, either exchange-traded or over-the-counter. ADR Sample indicates whether the ADR is included in our sample of ADRs. To be included, the ADR must be exchange-traded and have existed for our entire sample. Foreign is an indicator for whether the firm is owned by a non-Argentine parent company.

Table 3: Equity Results

(a) OLS

	(1) MSCI Index	(2) Value Index	(3) Bank Index	(4) Non-Financial Index
ΔD	-45.88***	-34.55***	-45.19***	-30.82***
Robust SE	(8.567)	(5.870)	(7.203)	(6.434)
95% CI	[-62.8,-27.0]	[-47.5,-23.3]	[-60.4,-31.3]	[-44.8,-18.1]
Observations	404	404	404	404
R-squared	0.389	0.417	0.337	0.384

(b) IV-Style Event Study

	(1) MSCI Index	(2) Value Index	(3) Bank Index	(4) Non-Financial Index
ΔD	-56.30***	-39.63***	-54.78***	-34.17**
SE	(11.80)	(7.955)	(10.40)	(10.27)
95% CI	[-88.3,-24.2]	[-63.9,-22.6]	[-84.8,-32.9]	[-62.3,-6.5]
Obs.	404	404	404	404

(c) CDS-IV

	(1) MSCI Index	(2) Value Index	(3) Bank Index	(4) Non-Financial Index
ΔD	-55.89**	-39.35***	-55.30***	-33.29**
SE	(13.57)	(9.443)	(12.13)	(11.24)
95% CI	[-90.9,-25.6]	[-69.4,-20.9]	[-89.4,-27.5]	[-64.7,-8.4]
Obs.	404	404	404	404

Notes: This table reports the results for the OLS (a), IV-style event study (b), and CDS-IV (c) estimators of the effect of changes in the risk-neutral default probability (ΔD) on the MSCI Index, the Value-Weighted index, the Value-Weighted Bank Index and the Value-Weighted Non-Financial Index. All indices are composed of ADRs. The index weighting is described in the text. The coefficient on ΔD is the effect on the percentage returns of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Exchange Rate Results

(a) OLS

	(1) Official	(2) Dolar Blue	(3) ADR Blue	(4) Blue-Chip Swap	(5) NDF - 12M
ΔD	3.784	10.49**	24.89***	29.84***	4.530
SE	(3.284)	(4.228)	(5.654)	(3.775)	(4.825)
95% CI	[-34.6,8.5]	[0.9,19.3]	[13.4,38.0]	[22.6,37.8]	[-6.9,14.7]
Obs.	404	358	356	359	237
R-squared	0.079	0.033	0.161	0.174	0.048

(b) IV-Style Event Study

	(1) Official	(2) Dolar Blue	(3) ADR Blue	(4) Blue-Chip Swap	(5) NDF - 12M
ΔD	-0.272	14.11	23.84	31.88***	1.824
SE	(0.881)	(5.214)	(9.709)	(3.288)	(4.622)
95% CI	[-2.2,1.5]	[-18.0,28.6]	[-12.5,57.7]	[26.1,39.8]	[-10.1,13.5]
Obs.	404	358	356	359	237

(c) CDS-IV

	(1) Official	(2) Dolar Blue	(3) ADR Blue	(4) Blue-Chip Swap	(5) NDF - 12M
ΔD	-0.656	14.80	23.77	29.97***	1.905
SE	(0.960)	(5.958)	(11.18)	(4.328)	(5.774)
95% CI	[-2.3,1.3]	[-17.7,31.1]	[-20.0,58.1]	[23.7,40.3]	[-10.8,16.0]
Obs.	404	358	356	359	237

Notes: This table reports the results for the OLS (a), IV-style event study (b), and CDS-IV (c) estimators of the effect of changes in the risk-neutral default probability (ΔD) on measures of the Argentine exchange rate. Official is the government's official exchange rate. Dolar Blue is the onshore unofficial exchange rate from dolarblue.net. ADR Blue is the ADR Blue Rate constructed by comparing the ADR share price in dollars with the underlying local stock price in pesos, as described in Section 3. Blue-Chip Swap is constructed by comparing the ARS price of domestic Argentine sovereign debt with the dollar price of the same bond, as described in Section 3. NDF - 12M is the 12 month non-deliverable forward rate. The coefficient on ΔD is the effect on the percentage depreciation of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Coefficients for Tracking Portfolios

	(1) $N_{g,t}^{DOLS}$	(2) $N_{g,t}^{VAR}$	(3) $N_{g,t}^{Survey}$
Value Index	0.253***	0.257***	0.064***
SE	(0.033)	(0.0034)	(0.019)
FX (ADR Blue)	0	0.013	-0.124*
SE	-	(0.011)	(0.064)
Obs.	39	45	19

Notes: The DOLS estimates are computed with dynamic OLS, with 4 quarters of leads and lags, and Newey-West standard errors, with 4 quarters of lags. The VAR estimates are computed using a single lag VAR, assuming homoskedastic, uncorrelated innovations. The VAR uses the DOLS coefficient estimate for ϕ. The standard errors on the coefficients, which are a transformation of the VAR parameters, are computed using the delta method. For both the DOLS and VAR, the sample is 2003-2014, quarterly. For the survey, the sample is overlapping 1-year changes, from 2003-2012, in the forecast. The regression is run on abnormal index returns and exchange rate changes. These abnormal returns are estimated from a daily frequency sample, with our set of high-frequency controls. This linear model, estimated on the high-frequency data, is then used to generate yearly abnormal returns. The standard errors are Newey-West, with 4 lags (2 years), and do not account for the estimation error associated with the model of abnormal returns.

Table 6: Default Probability, Other Countries

(a) OLS

Country	ΔD	Country	ΔD	Country	ΔD
Austria	.715	Indonesia	1.71	Portugal	.094
Belgium	1.44	Ireland	-.373	Romania	2.19***
Bahrain	.576	Italy	-.226	Russia	5.3***
Brazil	4.14***	Japan	.506	South Africa	3.25***
Chile	2.2***	Kazakhstan	3.17***	Spain	-.142
China	.926	South Korea	.398	Thailand	.663
Colombia	3.23***	Malaysia	.338	Turkey	4.43***
Croatia	2.45**	Mexico	3.44***	Ukraine	9.62***
Cyprus	6.44	Morocco	-.455	Venezuela	16.6***
Egypt	1.53	Panama	3.14***	Vietnam	-.544
France	1.96**	Peru	3.11***		
Iceland	-.727	Philippines	1.67*		

(b) CDS-IV

Country	ΔD	Country	ΔD	Country	ΔD
Austria	-.219	Indonesia	-.612	Portugal	-.517
Belgium	1.24	Ireland	-1.41	Romania	-.119
Bahrain	-.322	Italy	-1.91	Russia	4.05
Brazil	1.5	Japan	-.321	South Africa	1.52
Chile	.348	Kazakhstan	1.23	Spain	-.865
China	.0566	South Korea	-.893	Thailand	-.504
Colombia	.729	Malaysia	-.395	Turkey	.858
Croatia	-1.76	Mexico	.737	Ukraine	10.9
Cyprus	7.43	Morocco	-.577	Venezuela	5.87
Egypt	-.824	Panama	.304	Vietnam	-4.26
France	-.063	Peru	.402		
Iceland	.156	Philippines	-.288		

Notes: This table reports the results for the OLS (a) and CDS-IV (b) estimators of the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on the five-year risk-neutral default probability for other countries. The default probability measure use for other countries is derived from the credit triangle approximation described in Appendix F. The coefficient on ΔD is the effect on the other country's five-year risk neutral default probability of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Default and the PV of GDP Growth

	(1) $N_{g,t}^{DOLS}$	(2) $N_{g,t}^{VAR}$	(3) $N_{g,t}^{Survey}$
ΔD	-9.953***	-10.00***	-4.172***
SE	(2.722)	(2.589)	(1.205)
95% CI	[-27.9,-5.4]	[-29.7,-5.4]	[-12.4,-2.0]
Obs.	404	358	358

Notes: This table reports the results the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on three measures of the present value of Argentine real GDP growth. The coefficient on ΔD is the effect on the present value of Argentine real GDP growth of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. $N_{g,t}^{DOLS}$ is the DOLS estimate of the cointegration coefficient ϕ multiplied by the dollar returns on the value-weighted index. $N_{g,t}^{VAR}$ is the real GDP news implied by the VAR estimates described in section 5.2. $N_{g,t}^{Survey}$ is the real GDP news measure derived from survey forecast as described in Section 5.1. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Cross-Section: Industry Returns, CDS-IV

	(1) Index	(2) Banks	(3) Chemicals	(4) Diverse
ΔD	-47.53*	-10.67	-2.929	9.148
	(12.62)	(10.79)	(13.26)	(14.79)
95% CI	[-97.9,0.6]	[-39.8,21.2]	[-36.5,25.4]	[-33.5,45.8]
Index Beta	-	1.089	0.907	0.962
Observations	356	358	358	358

	(5) Energy	(6) Manufacturing	(7) Non-Durables	(8) Non-Financial
ΔD	-6.236	-7.738	-11.78	1.000
	(11.32)	(6.270)	(6.410)	(4.356)
95% CI	[-37.7,34.6]	[-19.4,8.8]	[-30.1,3.9]	[-14.4,16.4]
Index Beta	0.909	0.767	0.756	1.005
Observations	358	358	358	358

	(9) Real Estate	(10) Telecommunications	(11) Utilities
ΔD	22.31	-13.08**	24.45
	(13.22)	(7.173)	(12.08)
95% CI	[-22.5,51.2]	[-29.3,-0.5]	[-13.4,60.7]
Index Beta	0.675	0.799	1.560
Observations	358	358	358

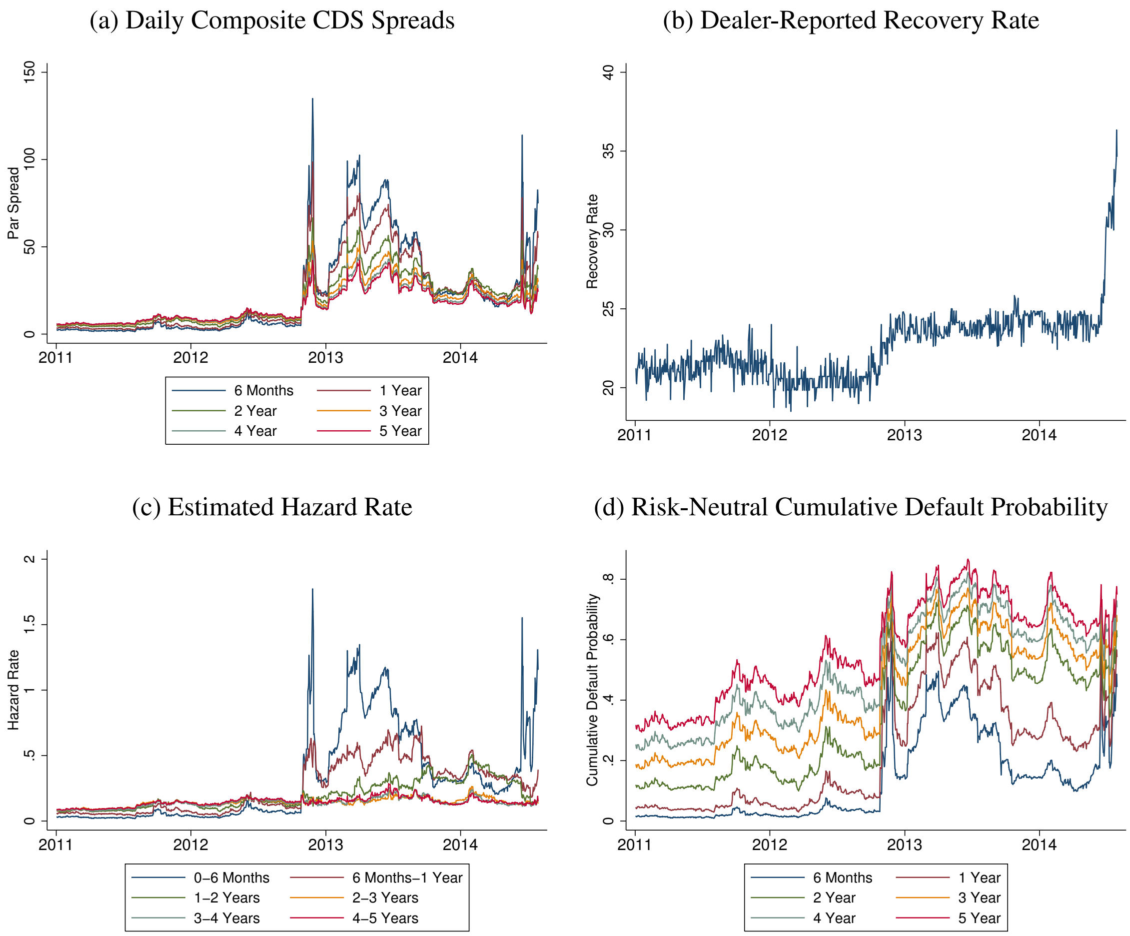
Notes: This table reports the results for the "CDS-IV" estimator. The column headings denote the outcome variable. Index is the equal-weighted index of local equities in Table 2. The industry classifications are based on Fama-French with modifications described in Section 3.1. The coefficient on ΔD is the effect on the percentage returns of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Index beta is the coefficient on the equal-weighted index of Argentine local equities, as described in Section 6. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Cross-Section: Long-Short Portfolios, CDS-IV

	(1) Exporter	(2) Importer	(3) Financial	(4) Foreign-Owned	5) ADR
ΔD	-24.77*	21.13***	-11.67	-19.30	6.487
	(9.426)	(5.659)	(14.91)	(11.07)	(9.598)
95% CI	[-55.1,0.3]	[7.5,34.0]	[-54.7,37.3]	[-52.5,8.4]	[-20.0,40.5]
Index Beta	-0.483	0.351	0.0836	-0.206	0.0757
Observations	356	356	356	356	356

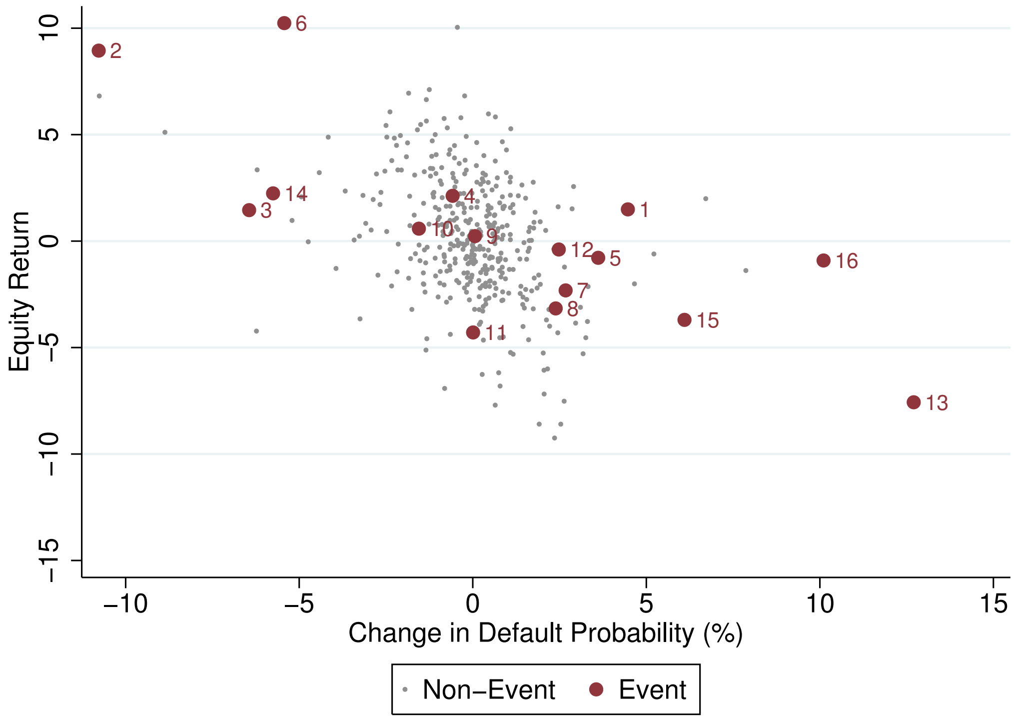
Notes: This table reports the results for the "CDS-IV" estimator. The column headings denote the outcome variable. Each column is a zero-cost long short portfolio. Exporter is a portfolio going long export-intensive non-financial firms and short non-export-intensive non-financial firms. Importer is defined equivalently for importers. Financial goes long banks and short non-financial firms. Foreign-owned firms goes long non-financial firms with a foreign parent and short domestically-owned non-financial firms. ADR goes long non-financial firms with an American Depository Receipt and short non-financial firms without one. The data sources are described in Section 3.1. The coefficient on ΔD is the effect on the percentage returns of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Index beta is the coefficient on the equal-weighted index of Argentine local equities, as described in Section 6. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Figure 1: From CDS Spreads to Default Probabilities



Notes: Panel (a) plots the daily Composite CDS spreads from Markit. Panel (b) plots the the average of all recovery rates of Markit contributors whose CDS curves are used to calculate the Markit CDS End of Day composite curve. Panel (c) plots the default hazard rates estimated using the ISDA Standard model. 0-6 Months indicates the estimated constant hazard rate from initiation to 6 months, 6 Months - 1 Year indicates the implied estimated constant hazard rate from 6 months after initiation to 1 year after initiation, and so on. Panel (d) converts the estimated hazard rates in Panel (c) into cumulative risk-neutral default probabilities. 6 Months indicates the probability the government defaults in the next 6 months, 1 Year indicates the probability of default in the next year, and so on. The data and ISDA Standard model are discussed in Sections 3.1 and F.

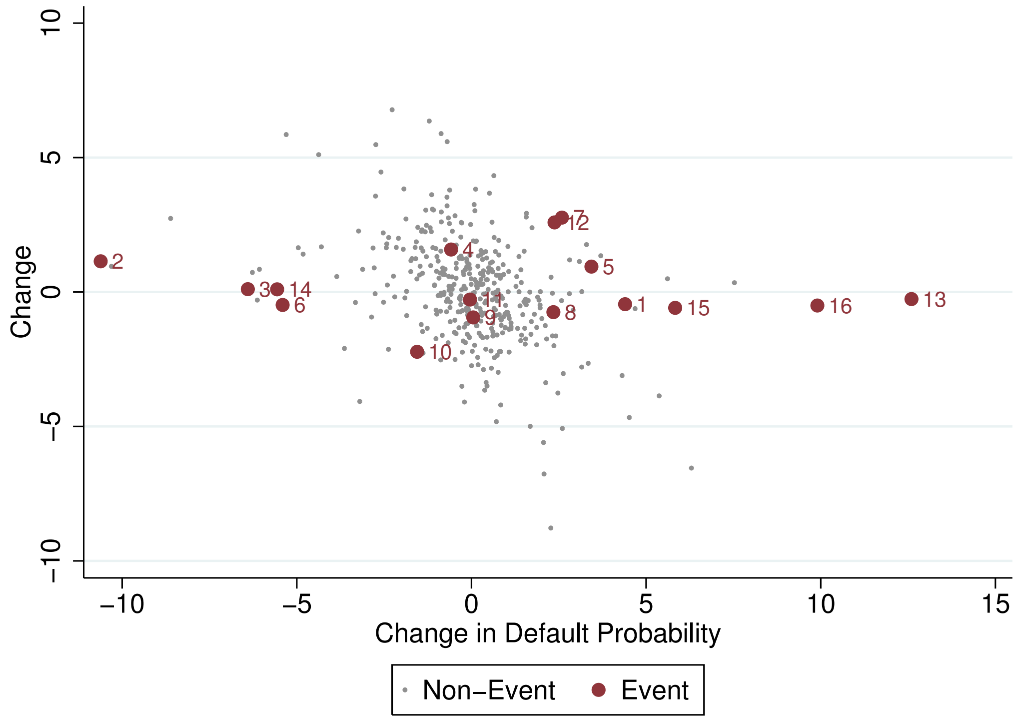
Figure 2: Value-Weighted Equity Index Return and Argentine Default Probability



Event Number	Two-Day Window End Date	ΔD (%)	Equity Return (%)
1	November 27, 2012	4.40	2.64
2	November 29, 2012	-10.61	6.27
3	December 05, 2012	-6.40	2.65
4	December 07, 2012	-0.58	0.84
5	January 11, 2013	3.44	-0.30
6	March 04, 2013	-5.41	6.00
7	March 27, 2013	2.59	-2.09
8	August 26, 2013	2.35	-2.81
9	October 04, 2013	0.05	-2.27
10	October 08, 2013	-1.56	2.35
11	November 19, 2013	-0.04	-4.00
12	January 13, 2014	2.38	-0.70
13	June 17, 2014	12.60	-5.78
14	June 24, 2014	-5.56	2.04
15	June 27, 2014	5.83	-1.99
16	July 29, 2014	9.90	-0.40

Notes: This figure plots the change in change in the risk-neutral probability of default and returns on the Value-Weighted Index on event and non-event two-day windows. Each event and non-event day is a two-day event or non-event as described in the text. The numbers next to each maroon/dark dot references each event-day in the Table below the figure. The procedure for classifying events and non-events is described in the text.

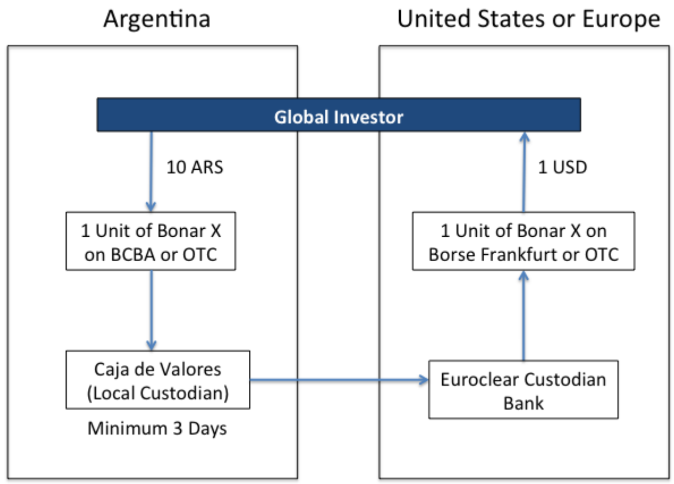
Figure 3: Mexico Equity Index Return and Argentine Default Probability



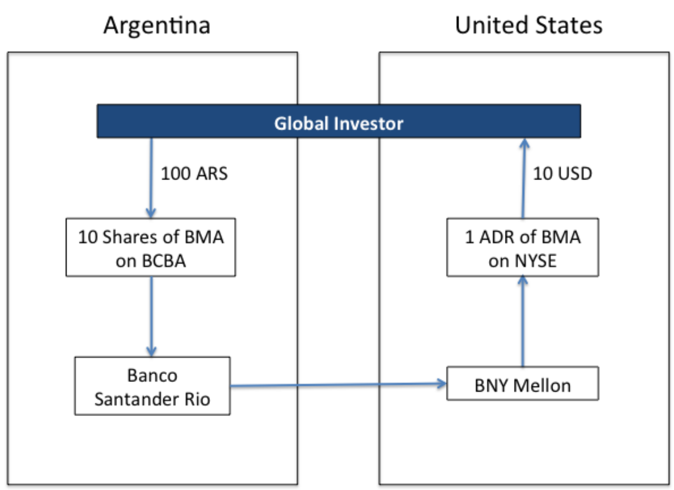
Notes: This figure plots the change in change in the risk-neutral probability of an Argentine sovereign default and returns on the MSCI Mexico Index on event and non-event two-day windows. Each event and non-event day is a two-day event or non-event as described in the text. The numbers next to each maroon/dark dot references each event-day in the Table below the figure. The procedure for classifying events and non-events is described in the text.

Figure 4: Blue Rate Construction

(a) Blue-Chip Swap

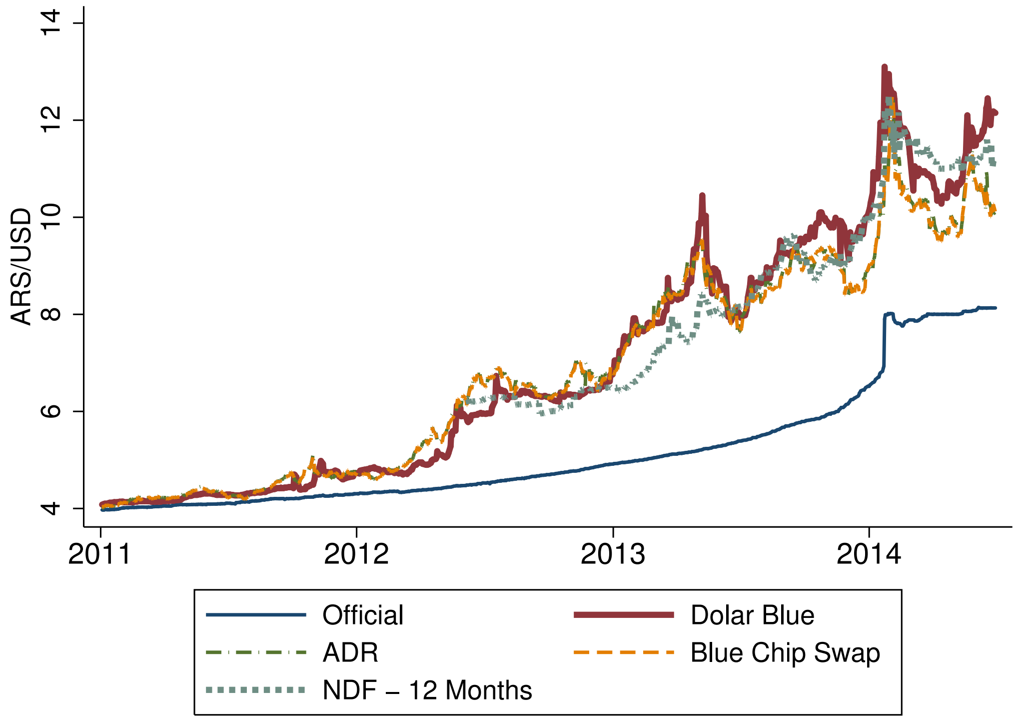


(b) ADR Blue Rate



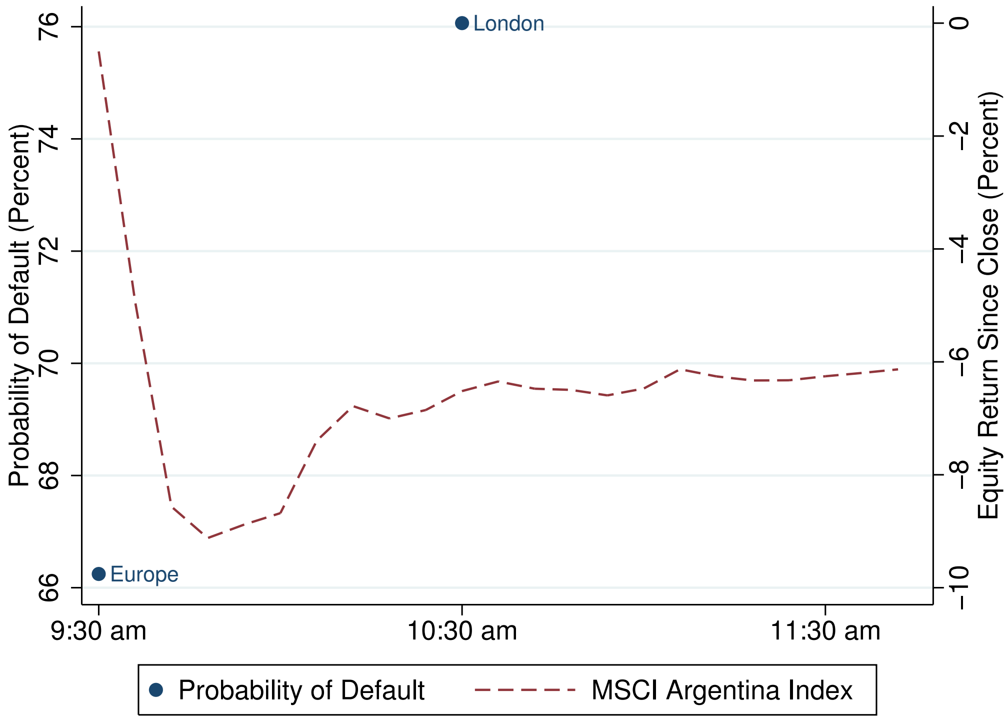
Panel A demonstrates how an investor would convert Argentine pesos into US dollars by buying a domestic sovereign bond in ARS and selling the bond offshore in USD. This transaction defines an unofficial exchange rate known as the Blue-Chip Swap rate. Panel B demonstrates how an investor would convert Argentine pesos into US dollars by buying shares of Banco Macro onshore and selling an ADR in New York. The transaction defines an unofficial exchange rate known as the ADR Blue Rate.

Figure 5: Exchange Rates



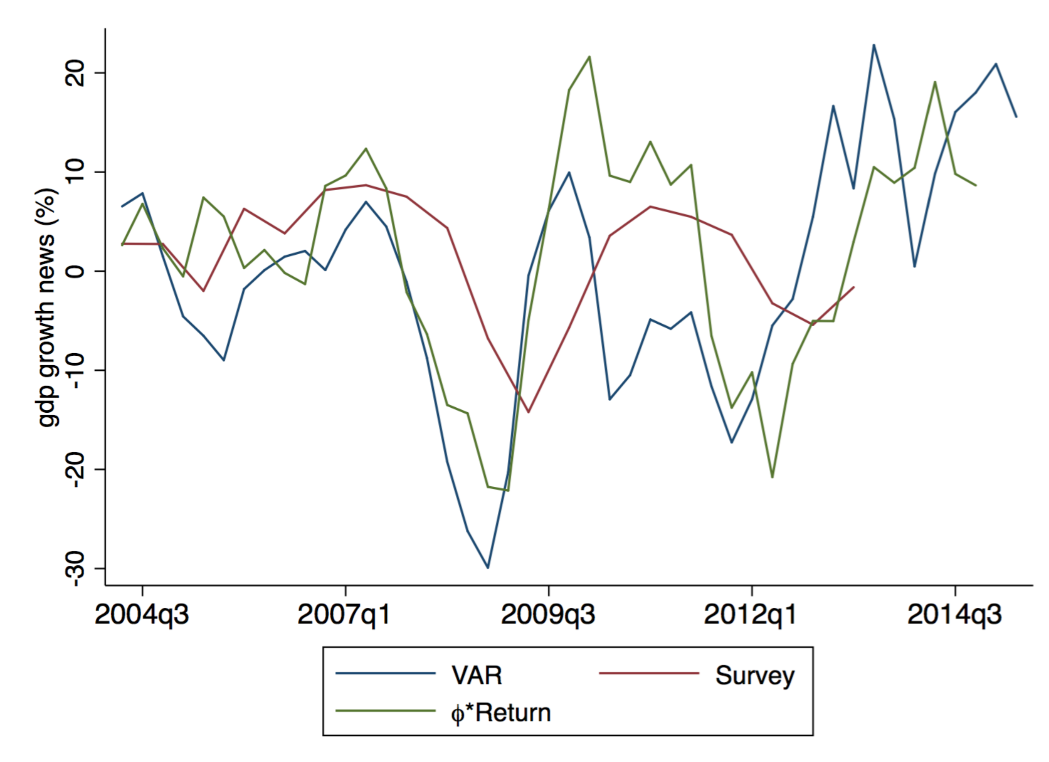
Notes: This figure plots the four versions of the ARS/USD exchange rate and the 12 month non-deliverable forward rate. Official is the government's official exchange rate. Dolar Blue is the onshore unofficial exchange rate from dolarblue.net. ADR is the ADR Blue Rate constructed by comparing the ADR share price in dollars with the underlying local stock price in pesos, as described in Section 3. Blue Chip Swap is constructed by comparing the ARS price of domestic Argentine sovereign debt with the dollar price of the same bond, as described in Section 3. NDF - 12 Months is the 12 month non-deliverable forward rate.

Figure 6: Event Data from June 16, 2014, 9:30-11:30am EST



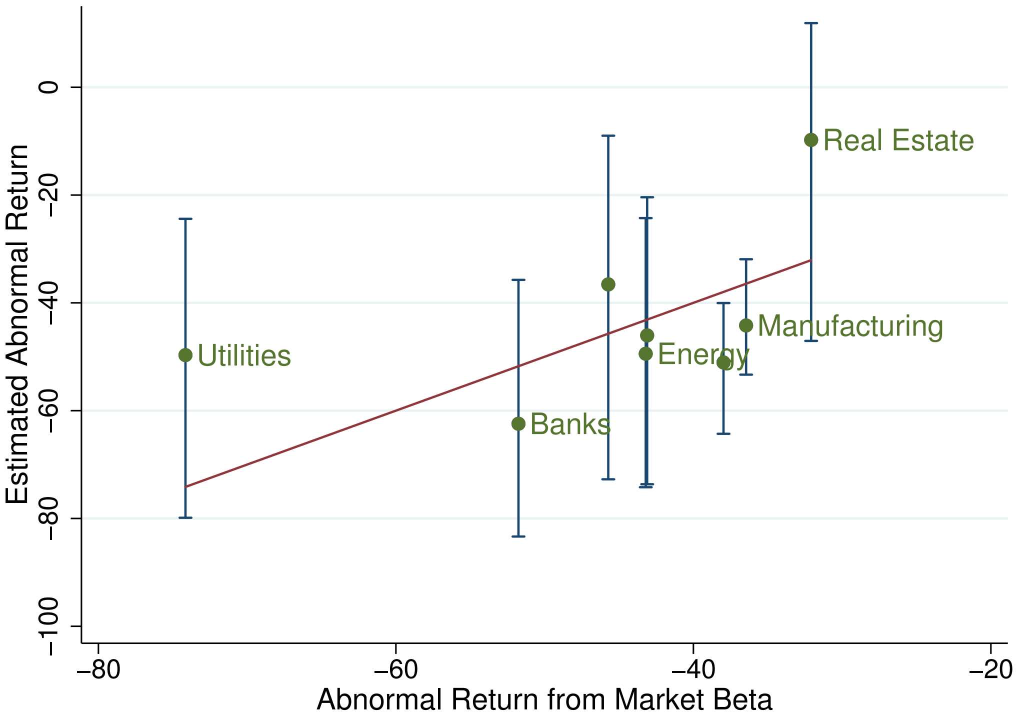
Notes: This figure plots the 5-year risk-neutral probability of default ("Probability of Default (Percent)", left axis), the change in the price of the MSCI Argentina Index against the previous night's close ("Equity Return Since Close (Percent)", right axis). The default probability points are labeled with the name of the reporting market, with European markets reporting at 9:30am EST and London Markets reporting at 10:30am EST. The Supreme Court ruling was released at 9:33 am EST.

Figure 7: Measures of GDP News (4Q Rolling Sum)



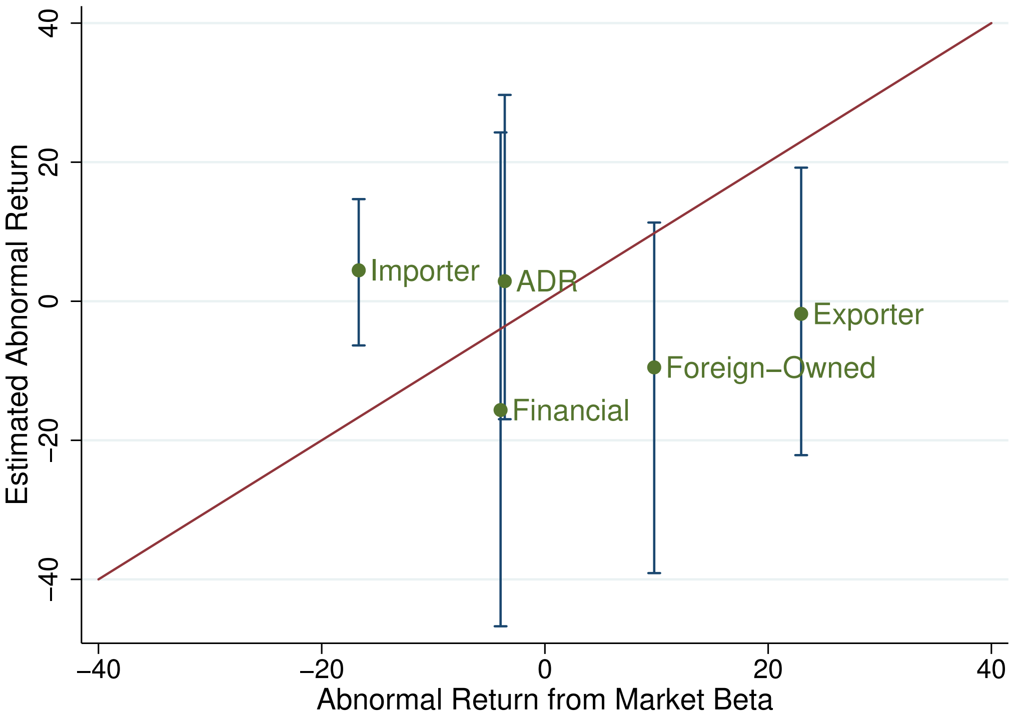
Notes: This figure plots the time series of real GDP news implied by our VAR estimates and the Consensus economics surveys. The VAR news series is the 4-quarter cumulative sum of the GDP news, and the data begins in 2003q1. The Consensus economics survey is twice-yearly (April and October), and we compare year-over-year changes. We have also plotted our DOLS estimate of the cointegration coefficient, ϕ, multiplied by the dollar returns on our value-weighted ADR index.

Figure 8: Estimated Response to Default Shocks: Industries



Notes: Industry classifications are based on the Fama-French 12 industry categories with the modifications described in Section 3.1. The labels for chemical firms, diverse firms, non-durables producers, and telecoms are suppressed. On the the x-axis, we plot the expected abnormal return for each portfolio, calculated as the beta of each long-short portfolio on the index times α_M, the effect of an increase in the probability of default in the abnormal return of the index. On the y-axis, we plot the sum of the expected abnormal return and $(\alpha_i - \beta_i \alpha_M)$, the additional sensitivity of each portfolio to an increase in the probability of default. Values above the line indicates that the portfolio overperformed following increases in the probability of default, relative to what would be implied by the portfolio's market beta. Values below the line indicate underperformance. The ranges indicate bootstrapped 90% confidence intervals.

Figure 9: Estimated Response to Default Shocks: Long-Short



Notes: Each label denotes a zero-cost long short portfolio. Exporter is a portfolio going long export-intensive non-financial firms and short non-export-intensive non-financial firms. Importer is defined equivalently for importers. Financial goes long banks and short non-financial firms. Foreign-owned firms goes long firms with a foreign parent and short domestically-owned firms. ADR goes long firms with an American Depository Receipt and short firms without one. The data sources are described in Section 3.1. On the the x-axis, we plot the expected abnormal return for each portfolio, calculated as the beta of each long-short portfolio on the index times α_M, the effect of an increase in the probability of default in the index. On the y-axis, we plot the sum of the expected abnormal return and $(\alpha_i - \beta_i \alpha_M)$, the additional sensitivity of each portfolio to an increase in the probability of default. Values above the line indicates that the portfolio over-performed following increases in the probability of default, relative to the abnormal return implied by the portfolio's market beta. Values below the line indicate underperformance. The ranges indicate bootstrapped 90% confidence intervals.

A Data Construction Details

In this section, we provide additional details about our data construction.

A.1 Firm Classifications

We classify firms according to their Fama-French industry classifications available on Kenneth French's website.[75] We sort firms into their corresponding Fama-French industries according the SIC code of their primary industry, available from Datastream. After this initial sort, we only have one firm, Boldt, classified as Business Equipment, and so we combine it with the telecommunications firms. The "Finance" Fama-French 12 industry classification is also too broad for our purposes, as it combines banks, holding companies, and real estate firms. We therefore split the nine firms initially classified as "Finance" according to their Fama-French 49 industry classification. This gives us six banks, two real estate firms, and one "Trading" firm, Sociedad Comercial del Plata. Because Sociedad Comercial del Plata is a diversified holding company, and is the only company in the Fama-French 49 industry classification of "Trading," we rename its industry "Diversified", and do not merge it with any other industry classification. After these modifications, we end up with six banks, two chemical firms, one diversified firm, three energy firms, four manufacturing firms, six non-durables firms, two real estate firms, three telecoms and eight utilities. These industries are listed in table 2.

We also sort firms by their exporter status. Unfortunately, this task is complicated by the fact that publicly available data sources do not comprehensively report firm-level exports. We instead rely on industry-level measures. We use the OECD STAN Input-Output Tables for Argentina to calculate what share of each industry group's output is exported. The Input-Output Table covers 37 industries, each of which covers at least one two-digit ISIC industry, and some of which, such as "Agriculture, hunting, forestry and fishing", cover up to five two-digit ISICs. After we calculate the share of exports for each of these 37 industries, we classify our 35 firms into one of these industries according to the SIC code of its primary output. Unfortunately, the most recent Input-Output Table for Argentina uses data from 1995, so our export analysis assumes that the relative tradability of different products has not changed too much over the past 20 years.[76] When we construct a zero-cost long-short portfolio, going long exporters and short non-exporters, we will classify firms as exporters if exports accounted for at least 10% of their primary industries' revenues in our Input-Output table, and non-exporters otherwise. The exporter threshold is set at 10% because there are

[75] Classifications available here. We use the versions formatted by Dexin Zhou.

[76] For those firms that report data on revenue from exports, there is a strong correlation between reported exports as a share of sales and the imputed share of exports from the 1995 input-output table. These results are available upon request.

no firms with an export share between 3.6% and 10.1%.

We also calculate the share of intermediate inputs imported for each industry. We again use the OECD STAN Input-Output Tables to calculate the reliance on imported intermediate goods for 37 industries, and then match each of our firms to these industries using their primary SIC code. As with exports, we rely on the 1995 Input-Output Table. For portfolio construction, we classify firms as non-importers if imported intermediates are less than 3% of total sales in their primary industry, and as importers otherwise. The importer threshold gives us roughly the same number of firms above and below the cutoff.

The next cut of the data divides firms among those that are subsidiaries of foreign corporations and those that are not. We classify firms as foreign-owned if the headquarters of their ultimate parent is any country other than Argentina in Bloomberg (Field ULT_PARENT_CNTRY_DOMICILE). We use the most recent (as of our data construction) version of this variable and cannot account for the possibility that an Argentine firm was only recently purchased by a foreign parent.

The final variable we use to classify our local equities is an indicator for whether or not the firms have an ADR that is traded in the US. This includes some firms with ADRs that trade over-the-counter, and are therefore not included in our analysis of the ADRs.

A.2 Exchange Rate Construction

We calculate the blue chip swap rate using the two of the most liquid available debt instruments, the Bonar X and the Boden 15.[77] To calculate this blue chip swap rate, we search for the bonds on Bloomberg, use <ALLQ> to find the list of all available pricing sources for the bonds, and then download the full available history of closing prices for every provider in ARS and USD.[78] Each day, we generally have around 5 closing price quotes per bond in ARS and USD. We keep the median price for each bond every day by currency and then construct the implicit exchange rate by dividing the median peso price by the median dollar price. This gives us a blue-chip swap rate for each our two bonds, and we construct the Blue-Chip Swap rate by taking the average of the two. Despite these bonds being classified as domestic debt, many of these instruments have ISINs and are accepted on Euroclear or Clearstream. This makes it relatively easy for foreign investors to use to get money on or offshore to circumvent Argentina's capital controls.[79] However, it is important to remember that although we calculate the exchange rate using simultaneous prices, an investor implementing this transaction is required to hold the bond for at least 3 days at an

[77] The ISIN for the Bonar X is ARARGE03F441 and the ISIN for the Boden 15 is ARARGE03F144.

[78] We drop pricing sources with less than 300 days of data and sources where more than 5% of the daily observations record no price change.

[79] Indeed, dolarblue.net offers a simple guide for how to buy and sell dollars http://blog.dolarblue.net/2014/09/como-adquirir-o-vender-dolares.html

Argentine custodian bank, and therefore bears some price risk when acquiring dollars.[80] Despite being domestic debt instruments, both of these bonds became entangled in the legal proceedings we focus on in this paper.[81]

For the ADR blue rate, we follow the methodology outlined on dolarblue.net[82]. We collect daily open and close price data on the ADR and local equity for eight firms trading from Bloomberg.[83] We then calculate the daily implicit exchange rate for each firm, drop the high and low price among the eight firms, and construct our measure as the mean of the remaining six equities. We find (in unreported results) that the implied exchange rate computed using ADR and local stock market prices does not vary significantly across firms. The average difference between the maximum and minimum firm-level exchange rate is 3.6% of the level the ADR Blue Rate. Together, the ADR Blue Rate and the Blue-Chip Swap rate may be known as the dolar contado con liquidación, dolar fuga, or the dolar gris.[84]

B Event Studies

Following the discussion in section §4, we present the results of two additional event studies. The first event study uses two-day windows around events.

Our event study methodology follows the one described in Campbell et al. (1997). Let N denote the set of non-event days, and let $L1 = |N|$. We first estimate the factor model on the non-event days,

$$r_{i,t} = \mu_i + \omega_i^T X_t + \nu_{i,t},$$

and generate a time series of abnormal returns, $\hat{r}_{i,t} = r_{i,t} - \hat{u}_i - \hat{\omega}_i^T X_t$, where X_t is the vector of controls discussed in section 3.1. We also estimate the variance of the abnormal returns associated with the factor model (assuming homoskedastic errors), $\hat{\sigma}_i^2 = \frac{1}{L1}\sum_{t\in N}\hat{\nu}_{i,t}^2$. We next estimate a factor model for the change in the probability of default, ΔD_t, and create a time series of abnormal default probability changes, $\hat{d}_t$. We then classify our event days into three categories, based on the abnormal default probability change during the event window. Let σ_d denote the standard deviation of the abnormal default probability changes. If the probability increases by at least σ_d,

[80] Chodos and Arsenin (2012).

[81] Excellent coverage of turmoil around the domestic debt was provided by Joseph Coterill of FT Alphaville. See, for instance, http://ftalphaville.ft.com/2015/04/23/2127218/the-great-bonar-caper/ or http://ftalphaville.ft.com/2015/02/26/2120454/bonar-turns-into-subpoena/.

[82] http://blog.dolarblue.net/p/calculo.html

[83] Grupo Financiero Galicia (ADR Ticker: GGAL, Local Ticker: GGAL), Tenaris (TS, TS), BBVA Banco Frances (BFR, FRAN), Banco Macro (BMA, BMA), Pampa Energia (PAM, PAMP), Petrobras Argentina (PZE, PESA), Petroleo Brasileiro (PBR, APBR), and Telecom Argentina (TEO, TECO2).

[84] http://www.infodolar.com/cotizacion-dolar-contado-con-liquidacion.aspx

we label that day as an "higher default" event. If the probability decreases by at least σ_d, we label that event as a "lower default" event. If the default probability change is less, in absolute value, than σ_d, we label that as a "no news" event.

For each type of event, we report the cumulative abnormal return and cumulative abnormal default probability change over all events of that type (higher default, lower default, no news). We also report two statistics that are described in Campbell et al. (1997). In this event study (but not the next one we discuss), which does not aggregate returns across different ADRs, the two statistics are identical, up to a small sample size correction. Define $E_{\{h,l,n\}}$ as the set of event days of each type. The first statistic, $J1$, is computed, for event type j and ADR i, as

$$J1_{ij} = \frac{\sum_{t \in E_j} \hat{r}_{i,t}}{\sqrt{|E_j| \hat{\sigma}_i^2}}.$$

Under the null hypothesis that the events have no effect on the stock returns, $J1_{ij}$ is asymptotically distributed as a standard normal. However, because we have so few events in each category, asymptotic normality will be a poor approximation, if the abnormal returns are themselves far from normal. This is one reason we prefer the variance-based estimators discussed in the next section.

The second statistic, $J2$, is nearly identical to $J1$ for this event study (they will be different in the next event study we describe). For each event, we can define a standardized cumulative abnormal return,

$$z_{i,t} = \sqrt{\frac{|E_j| - 4}{|E_j| - 2}} \frac{\hat{r}_{i,t}}{\sqrt{\hat{\sigma}_i^2}},$$

where the first term represents a small-sample correction. The statistic $J2$ is defined as

$$J2_{ij} = \frac{\sum_{t \in E_j} z_{i,t}}{\sqrt{|E_j|}}.$$

This statistic is also asymptotically standard normal under the null hypothesis, subject to the same caveat about return normality. In the table A1, we present these two statistics for the MSCI Argentina Index.

Table A1: Standard Event Study: Index

Shock Type	# Events	CAR (%)	ΔD (%)	J_1	J_2
Higher Default	8	-18.89	43.22	-2.54	-2.53
No News	3	-2.98	-0.32	-0.66	-0.65
Lower Default	5	24.05	-29.10	4.09	4.08

Notes: CAR indicates cumulative abnormal return over the event windows, ΔD is the change in the risk-neutral probability of default, and the test statistics J_1 and J_2 are described in the text and in Campbell et al. (1997), pp. 162. A shock type of higher default indicates that this event raised the default probability by more than one two-day standard deviation, a shock type of lower default indicates that this event lowered the default probability by more than one two-day standard deviation, and a shock type of no news indicates a day with a legal ruling in which the default probability did not move at least one two-day standard deviation in either direction. The underlying data is based on the two-day event windows and non-events described in the text. The p-values are the p-values for a two-sided hypothesis test assuming normality. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

The results of this event study are broadly similar to the OLS estimates. In the 8 event days where the default probability significantly increased, the cumulative increase in the default probability was 43.22% and the stock market experienced a cumulative abnormal return of -18.89%. Assuming a linear relationship between default probabilities and equity returns, this implies that a 1% increase in the probability of default causes a 0.44% fall in the stock market. During the 5 days where the default probability significantly declined, the cumulative fall in the default probability was 29.18% with a cumulative abnormal return of 24.05%. This implies a 1% fall in the probability of default causes an 0.83% rise in the stock market. Treating the movements symmetrically and adding together the absolute value of the change in default probability and cumulative abnormal returns, we find that a 1% increase in the probability of default causes a 0.59% fall in the equity market. While the large window sizes used in this study raise concerns about the validity of the identification assumptions, we will see that this estimate is very close to the results we find from our heteroskedasticity-based estimates.

The next event study we present uses four different window sizes. To construct these narrower windows, we also use a "sameday" CDS spread from Markit, which is as of 9:30 am EST. We refer to this as the "open," and is it in addition to the "close" defined in the main text. The sameday spread is built under the assumption that the expected recovery rate has not changed from the previous day's close. We convert the open and close CDS spreads into default probabilities ourselves for this analysis, rather than use probabilities provided by Markit, because Markit does not compute "open" default probabilities, only closing ones. Similarly, we report results for our value index, rather than the MSCI ADR index, because opening data is not available for the MSCI ADR index.

We classify events into several types: close-to-close, open-to-open, close-to-open, and open-to-close. For the Supreme Court ruling on June 16th, 2014, the event occurred in the morning of the 16th, after the U.S. stock market opened. We classify this ruling as "open-to-close" meaning that we will use the CDS spread change from 9:30am EDT on Monday the 16th to roughly 4pm EST on Monday the 16th, and the ADR returns from 9:30am EDT on Monday the 16th to 4pm EDT on Monday the 16th. If we had instead classified the event as "close-to-close," we would compare the 4pm EDT close on Friday the 13th to the 4pm EDT close on Monday the 16th. The "close-to-open" and "open-to-open" windows are defined in a similar way. We use the narrower window sizes (close-to-open and open-to-close) when possible, and the wider window sizes (close-to-close and open-to-open) when we do not have precise information about the event time.

The heterogenous-window-size event study approach does have one advantage over the heteroskedasticity approach (as we have implemented it). For the heteroskedasticity approach, we use two-day event days, because those are the smallest uniformly-sized windows that all of our events can fit into. If the identification assumptions required for the heterogenous-window-size event study hold, this approach may have more power than the heteroskedasticity-based approach.

Our data set includes one additional event (17 instead of 16), because one of the two-day windows in fact contained two separate legal rulings on consecutive days. Conceptually, the event study is almost identical, except that we must study each type of event (higher default, lower default, no news) for each window size. That is, we separately estimate abnormal returns and abnormal default probability changes for each window size $s \in S$, the set of window sizes. We classify events based on the standard deviation of abnormal default probability changes for the associated window size. Let E_{js} denote an event of type j (higher default, lower default, no news) with window size s (close-to-close, open-to-open, close-to-open, and open-to-close). The abnormal return $\hat{r}_{i,t,s}$ is the abnormal return for ADR i at time t with window size s, and $\hat{\sigma}^2_{is}$ is the variance of the abnormal returns for that window size. The $J1$ statistic is computed as

$$J1_{ij} = \frac{\sum_{s \in S} \sum_{t \in E_{js}} \hat{r}_{i,t,s}}{\sqrt{\sum_{s \in S} |E_{js}| \hat{\sigma}^2_{is}}}.$$

Asymptotically, subject to the same caveats mentioned previously, this statistic is distributed as a standard normal. The second statistic, $J2$, is constructed in a similar fashion. However, the standardized cumulative abnormal returns are now defined with respect to the event window size,

$$z_{i,t,s} = \sqrt{\frac{|E_{js}| - 4}{|E_{js}| - 2}} \frac{\hat{r}_{i,t,s}}{\sqrt{\hat{\sigma}^2_{is}}},$$

and the $J2$ statistic is

$$J2_{ij} = \frac{\sum_{s \in S} \sum_{t \in E_{js}} z_{i,t,s}}{\sqrt{\sum_{s \in S} |E_{js}|}}.$$

This statistic is also, subject to the same caveats, asymptotically standard normal. It is not the same as the $J1$ statistic, because of the heterogeneity in window size. If the cumulative abnormal returns occur mostly in narrower windows (which have smaller variance of abnormal returns), the $J2$ statistic will be larger in absolute value than the $J1$ statistic. If the reverse is true, the $J1$ statistic will be larger. The size of the window may depend in part on the court releasing the opinion, the urgency with which the opinion was required, and other endogenous factors. It is not obvious whether the $J1$ or $J2$ statistic should be preferred. Fortunately, the results presented in table A2 using the two statistics are similar.

Table A2: Heterogenous-Window Event Study: Index

Shock Type	# Events	CAR (%)	ΔD (%)	J_1	J_2
Higher Default	5	-10.41	14.68	-3.58	-3.14
No News	7	-4.48	4.93	-1.14	-1.08
Lower Default	5	9.22	-28.46	3.49	3.07

Notes: CAR indicates cumulative abnormal return over the event window, ΔD is the change in the risk-neutral probability of default, and the test statistics J_1 and J_2 are described in the text and in Campbell et al. (1997), pp. 162. This study pools events across different window sizes (open-open, open-close, close-open, close-close). A shock type of higher default indicates that this event raised the default probability by more than one standard deviation, where the standard deviation is defined for non-events with the same window size. A shock type of lower default indicates that this event lowered the default probability by more than one standard deviation, and a shock type of no news indicates a day with a legal ruling in which the default probability did not move at least one standard deviation in either direction. The underlying data is based on the event windows and non-events described in the text, and uses the narrowest windows possible with our data and uncertainty about event times. The p-values are the p-values for a two-sided hypothesis test assuming normality. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

In the 5 event days where the default probability significantly increased, the cumulative probability of default rose 14.68% and the stock market had a cumulative abnormal return of -10.41%. This estimate implies that a 1% increase in the probability of default causes a 0.71% fall in equity returns. During the 5 days where the default probability significantly declined, the cumulative fall in the default probability was 28.46% with a cumulative abnormal equity return of 9.22 %. This implies a 1% fall in the probability of default causes an 0.32% rise in the stock market. When we again treat up and down movements symmetrically, we find that a 1% increase in the probability of default causes a 0.45% fall in the equity market.

Compared with these event studies, the IV-style event study described in the main text has the advantage of offering an interpretable coefficient, $\hat{\alpha}$, that estimates the change in stock prices given a change in the default probability. It also takes into account the magnitude of the default probability changes on each event day, whereas the event studies discussed above treat each event in a category equally. However, it is not *a priori* clear that the impact of the default probability on stock returns should be linear, and therefore not obvious that this approach is superior to the two-day event study. The similarity of the two results suggests linearity is not a bad assumption. Additionally, because the IV-style event study uses two-day event windows, it requires stronger identification assumptions than the heterogenous-window event study.

C Mexico and Brazil

Table A3: Regressions for Brazil and Mexico

	(2) Brazil MSCI Index	(4) Mexico MSCI Index
OLS ΔD	-12.21***	-5.978**
Robust SE	(3.823)	(3.150)
95% CI	[-18.40,-6.50]	[-11.18,-0.98]
Event IVΔD	-3.035	0.634
Robust SE	(6.592)	(5.426)
95% CI	[-15.04,9.30]	[-4.14,7.93]
CDS-IVΔD	-1.098	1.669
Robust SE	(7.075)	(5.812)
95% CI	[-9.84,10.18]	[-3.22,8.73]

Notes: This table reports the results for the OLS, IV-style event study, and CDS-IV estimators of the effect of changes in the risk-neutral default probability (ΔD) on the stock market indices of Brazil and Mexico. The coefficient on ΔD is the effect on the percentage returns (of stocks) and change in the 5-year CDS spread (in bps) of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

D Multinational Firms

In this section, we discuss several firms that could be considered Argentine, but were excluded from our analysis. Techint is a privately held multinational conglomerate that controls, among other companies, Tenaris and Ternium. Tenaris is a steel pipe company, headquartered in Luxembourg, that conducts most of its business outside of Argentina. Tenaris is listed on the Buenos Aires stock exchange and has an ADR on the NYSE. Ternium is a steel company, also headquartered in Luxembourg, that is listed only on the NYSE, but owns a subsidiary, Siderar, that is listed on the Buenos Aires stock exchange, and that subsidiary conducts a substantial part of its business in Argentina. We include Siderar (ticker SID) in our data for local stocks, and do not include Tenaris in either our local stock or ADR datasets. Petróleo Brasileiro (Petrobras) is the state oil company of Brazil. The Argentine subsidiary of Petrobras, Petrobras Argentina (ticker PZE) is included in our dataset, but its parent is not. We also exclude Arcos Dorados ("Golden Arches"), an Argentina-headquartered McDonald's franchisee that has operations across Latin America and is listed only on the NYSE, and not in Argentina. We present results for the ADRs of Tenaris and Petrobras, and the stock of Arcos Dorados, below.

Table A4: Regressions for Tenaris, Petrobras, and Arcos Dorados

	(1) Tenaris ADR	(2) Petrobras ADR	(3) Arcos Dorados
OLS ΔD	-3.844	-17.62***	-5.198
Robust SE	(4.491)	(5.835)	(8.259)
95% CI	[-11.9,5.9]	[-29.3,-6.7]	[-20.4,10.4]
Event IVΔD	3.057	-9.345	19.89
Robust SE	(5.272)	(6.682)	(9.486)
95% CI	[-10.8,13.7]	[-27.9,5.5]	[-10.1,46.2]
CDS-IVΔD	3.064	-9.417	25.15*
Robust SE	(4.689)	(7.922)	(11.87)
95% CI	[-8.8,10.9]	[-28.2,6.2]	[-7.0,56.0]

Notes: This table reports the results for the OLS, IV-style event study, and CDS-IV estimators of the effect of changes in the risk-neutral default probability (ΔD) on the ADRs of Tenaris and Petrobras, and the stock of Arcos Dorados. These companies are multinationals that conduct a small portion of their business in Argentina, but are listed on the Argentine stock exchange (Tenaris and Petrobras) or headquartered in Argentina but listed on the NYSE (Arcos Dorados). The coefficient on ΔD is the effect on the percentage returns of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

E Delevered Portfolios

In table A5 below, we also present results with a "crude" deleveraging. We form an index composed of firm's ADRs and US treasury bills. We weight each firm by the previous year's book value of assets, and then assume that the firm has debt equal to the difference between that book value of assets and the previous quarter's market value of common equity. For each firm, we include in the index a mixture of treasury bills and ADRs, in proportion to the firms' mix of debt and equity. We then apply the CDS-IV estimation procedure to these indices.

Table A5: Delevered Indices, CDS-IV

	(1) Value Index	(2) Bank Index	(3) Non-Financial Index
ΔD	-8.631**	-7.052***	-12.65*
SE	(2.482)	(1.365)	(4.936)
95% CI	[-17.9,-1.4]	[-11.3,-3.4]	[-29.3,2.9]
Obs.	404	404	404

All regressions have controls for VIX, S&P, EEMA, soybean/oil prices, and CDX indices. Confidence intervals for value index and FX calculated using a stratified bootstrap following Horowitz (2001). Confidence intervals for the tracking portfolios calculated using a hybrid bootstrap method, in which the coefficients for the portfolio weights are sampled from their asymptotic distribution, then the high frequency data is bootstrapped using the stratified bootstrap procedure described in the text.

F Risk-Neutral Default Probabilities

We convert CDS spreads into risk-neutral default probabilities to provide a clearer sense of the magnitude of the estimated coefficients. We emphasize that we work with risk-neutral probabilities and do not attempt to convert them to physical probabilities. Pan and Singleton (2008) and Longstaff et al. (2011) impose additional structure to estimate the physical default probabilities.

In our baseline results, we will use the five-year cumulative risk-neutral default probability estimated by Markit using the ISDA standard model. This calculation begins with begin with data from Markit on CDS par spreads and the dealer reported recovery rates, as well as a zero-coupon discounting curve.[85] The par spread is the coupon payment that a buyer of CDS protections pays to the seller of the contract such that the CDS contract has zero cost at initiation. Because the seller of a CDS insures the buyer of a CDS against credit losses throughout the duration of the contract, pricing the contract involves calculating the term structure of credit risk on the bond. The recovery rate we use is the average of the recovery rates reported by dealers contributing prices to Markit. In robustness checks, we also consider a case with a constant recovery rate equal to the realized recovery of 39.5%.[86]

The market standard for pricing CDS is a reduced form model that models time-varying credit risk as a time-varying hazard rate of default.[87] Because we use the risk-neutral default probabilities calculated by Markit, our exposition will exactly follow Markit (2012). The par spread is the spread that equates the present value of payments from buyer of protection to the seller of protection (Fee Leg) equals the value of the from the seller to the buyer upon default (Contingent Leg). We can write the equation equating the present value of fee leg to the present value of the contingent leg as

$$S_n \sum_{i=1}^{n} \Delta_t P_{S(t)} Df_t + AD = (1-R) \cdot \sum_{i=1}^{N} \left(P_{S(t-1)} - P_{S(t)}\right) Df_t \tag{A11}$$

where

[85] Details on the discounting curve can be found at http://www.cdsmodel.com/cdsmodel/documentation.html. In the robustness checks where we estimate the risk-neutral default probability rather than using the data provided by Markit, we will use the US zero-coupon Treasury curve calculated in Gürkaynak et al. (2007) as our discount curve. As Longstaff et al. (2011) point out, changing from the Treasury curve to a zero-coupon curve extracted from Libor and swap rates would have very little effect on the results. Our estimation is performed using the Matlab function *cdsbootstrap*.

[86] See http://www.creditfixings.com/CreditEventAuctions/holdings.jsp?auctionId=9073 for details on the auction to calculate the recovery rate.

[87] White (2013) provides a very thorough discussion of the ISDA standard model.

$$
\begin{aligned}
S_n &= \text{Spread for protection to period } n \\
\Delta_t &= \text{Length of Period} \\
P_{Si} &= \text{Probability of survival to time } i \\
Df_i &= \text{Discount factor to time } i \\
R &= \text{Recovery Rate} \\
AD &= \text{Accrual on Default}
\end{aligned}
$$

White (2013) provides a detailed explanation of the calculation of accrual on default and we will omit the details here for brevity. If we assume that the default hazard rate is constant between CDS nodes (tenors for which CDS contracts are traded), the survival probabilities map exactly to the hazard rates. For example, if the shortest tenor CDS traded is 6 months, and the hazard rate of default is λ_{6m} from time 0 to 6 months, then the survival probability is equal to $\exp\left(-\lambda_{6M}\cdot\left(\frac{1}{2}\right)\right)$. Given a 6 month par spread, a discounting curve to 6 months, and an assumption on the recovery rate, λ_{6m} can be calculated directly from Equation A11. Once this hazard rate, and therefore the survival probability, has been calculated for the 6 month tenor, the hazard rate between the next node of the CDS curve, 6 months and 1 year, can be calculated in the same way. In this way, the hazard rate curve is bootstrapped until we have calculated the hazard rates between every CDS node. We can then use our estimate hazard rates to calculate the risk-neutral default probabilities for various horizons:

$$
\begin{aligned}
Pr\left(D \leq 6M\right) &= 1-\exp\left(-\lambda_{6M}\cdot\left(\frac{1}{2}\right)\right) \\
Pr\left(D \leq 1Y\right) &= 1-\exp\left(-\lambda_{6M}\cdot\left(\frac{1}{2}\right)-\lambda_{1Y}\cdot\left(\frac{1}{2}\right)\right) \\
&\vdots \\
Pr\left(D \leq 5Y\right) &= 1-\exp\left(-\lambda_{6M}\cdot\left(\frac{1}{2}\right)-\lambda_{1Y}\cdot\left(\frac{1}{2}\right)-\lambda_{2Y}-\lambda_{3Y}-\lambda_{4Y}-\lambda_{5Y}\right)
\end{aligned}
$$

The final equation, the probability that the government defaults in the next 5 years, is the measure we use for the default probability in our baseline analysis. For the calculation of the default probabilities of the other sovereigns in Section C, we approximate the default probability by using the credit triangle relationship. As shown in White (2013), if we assume the premium leg were paid instantly and the hazard rate were equal to a constant λ, then we would have

$$
\begin{aligned}
S &= (1-R)\lambda \\
\lambda &= \frac{S}{1-R} \\
Pr(D<5Y) &= 1-\exp(-5\lambda).
\end{aligned}
$$

G Robustness Checks

G.1 Alternate Measures of Default Probability

In this section, we will discuss how our results are affected by using different measures for the probability of default. In particular, we will change two features of our baseline default probability: the horizon and the assumed recovery rate. In order baseline specification we look at the cumulative default probability over five years and here we will also consider the one, three and seven year horizons. While we have data on CDS spreads out to 30 years, we are reluctant to use them because these longer tenors tend to be traded much less frequently. These are the first set of "Markit" results in Table A6.

The second change we will consider regards the recovery rate. In our baseline specification, we use the average dealer-reported recovery rate. While this series does vary, and in particular increase dramatically towards the eventual actual recovery rate as Argentina approached its eventual default, we cannot be sure how representative the earlier reported quotes are of market expectations. Therefore, as an alternative to the dealer reported recovery rates, we will set the recovery rate equal to 39.5%, the rate at which the CDS auction eventually settled. We estimate the risk-neutral default probability under this assumption using the Matlab command CDS bootstrap and use the US Treasury zero coupon curve as the discounting curve. These results are labeled "Constant R." in Table A6.

Finally, we will also consider the raw par spreads and points upfront as alternative measures of the default probability. This approach has the drawback that the coefficients are more difficult to interpret, but does come with the benefit that it uses market prices directly rather than relying on a model. The results are labeled "Constant R." in Table A6. The final set of results we include looks at the effect of changes in the quoted Points Upfront. The way that CDS generally trade today is not actually with the par spread. Instead, the buyer agrees to pay the seller a fixed coupon (5% for Argentine CDS) and "Points Upfront", the percentage of the notional that the buyer pays the seller upon initiation of the CDS. There is a one-to-one mapping between the par spread and points upfront. The results are labeled "Points Upfront" in Table A6.

Table A6: Alternate Default Probability Measures

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Exchange Rates			Indices			Delevered Indices		
Measure	Tenor	Dolar Blue	ADR Blue	Blue-Chip	Value	Banks	Non-Fin.	Value	Banks	Non-Fin.
Markit	1Y	9.067	9.575	15.65***	-21.42***	-29.78**	-18.41***	-4.976***	-3.988***	-7.424**
Markit	3Y	13.05	18.65	25.34***	-33.63***	-47.02***	-28.59***	-7.492***	-6.042***	-11.06*
Markit	**5Y**	**14.80**	**23.77**	**29.97*****	**-39.35*****	**-55.30*****	**-33.29****	**-8.631****	**-7.052*****	**-12.65***
Markit	7Y	17.55	28.99	35.93***	-46.43***	-65.31***	-39.14**	-10.19**	-8.339***	-14.91*
Constant R.	1Y	9.140	9.019	15.17***	-21.18***	-29.58**	-18.07***	-4.953***	-3.975***	-7.354**
Constant R.	3Y	14.25	21.22	27.97***	-37.10***	-52.09***	-31.32***	-8.264**	-6.663***	-12.14*
Constant R.	5Y	17.04	29.00	35.10***	-45.75***	-64.54***	-38.47**	-10.02**	-8.190***	-14.63
Constant R.	7Y	21.40	37.77	44.67***	-56.48**	-79.77***	-47.30*	-12.40**	-10.15***	-18.09
Par Spread	1Y	0.0454	0.0425	0.0773***	-0.106***	-0.151***	-0.0891**	-0.0236*	-0.0190***	-0.0350
Par Spread	3Y	0.0944	0.104	0.171***	-0.239***	-0.340***	-0.201**	-0.0510*	-0.0409***	-0.0755
Par Spread	5Y	0.121	0.141	0.224***	-0.316***	-0.449***	-0.266**	-0.0662*	-0.0532**	-0.0978
Par Spread	7Y	0.137	0.164	0.256***	-0.363**	-0.515***	-0.306**	-0.0753*	-0.0605**	-0.111
Points Upfront	1Y	0.113	0.105	0.190***	-0.269***	-0.375***	-0.229***	-0.0619***	-0.0493***	-0.0922**
Points Upfront	3Y	0.139	0.168	0.257***	-0.358***	-0.505***	-0.301***	-0.0794***	-0.0635***	-0.117*
Points Upfront	5Y	0.140	0.184	0.266***	-0.370***	-0.524***	-0.310**	-0.0810***	-0.0653***	-0.119*
Points Upfront	7Y	0.143	0.191	0.273***	-0.378***	-0.536***	-0.315**	-0.0826***	-0.0667***	-0.121*

Notes: Measure "Markit" indicates that these are the risk-neutral default probabilities computed by Markit. "Constant R." uses our estimation of the risk-neutral default probability under the assumption that the recovery rate is equal to its realized rate of 39.%. "Par Spread" directly uses the Composite par spread from Markit and "Points Upfront" uses the points upfront data from Markit. Dolar Blue is the onshore unofficial exchange rate from dolarblue.net. ADR Blue is the ADR Blue Rate constructed by comparing the ADR share price in dollars with the underlying local stock price in pesos, as described in Section 3. Blue-Chip Swap is constructed by comparing the ARS price of domestic Argentine sovereign debt with the dollar price of the same bond, as described in Section 3. The Value-Weighted index, the Value-Weighted Bank Index and the Value-Weighted Non-Financial Index are referred to as "Value," "Banks", and "Non-Fin.", respectively, and are included in their standard form and delevered. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

G.2 Alternate ρ

Table A7: Default and the PV of GDP Growth: Alternate ρ

	(1)	(2)	(3)	(4)
	Quarterly $\rho = .95$^(1/4)		Quarterly $\rho = .8$^(1/4)	
	$N_{g,t}^{VAR}$	$N_{g,t}^{Survey}$	$N_{g,t}^{VAR}$	$N_{g,t}^{Survey}$
ΔD	-10.31***	-4.019***	-8.700*	-3.061***
SE	(2.774)	(1.156)	(3.690)	(0.878)
95% CI	[-35.1,-6.6]	[-12.5,-2.0]	[-29.7,2.0]	[-9.4,-1.3]
Obs.	358	358	358	358

Notes: This table reports the results the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on two measures of the present value of Argentine real GDP growth. The coefficient on ΔD is the effect on the present value of Argentine real GDP growth of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. $N_{g,t}^{VAR}$ is the real GDP news implied by the VAR estimates described in section 5.2. $N_{g,t}^{Survey}$ is the real GDP news measure derived from survey forecast as described in Section 5.1. The first two columns assume an annual ρ of 0.95 and the last two assume an annual ρ of 0.8. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the text. The underlying data is based on the two-day event windows and non-events described in the text. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, soybean and oil prices. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

H Individual Bond Prices

Table A8: Bond Level Analysis

(a) OLS

	(1) Holdout	(2) Restructured: Disc.	(3) Restructured: Par	(4) Domestic: Boden 15	(5) Domestic: Bonar X
ΔD	-15.74	-83.44***	-51.91***	-46.34***	-40.41***
SE	(15.70)	(8.659)	(10.28)	(6.168)	(4.979)
95% CI	[-44.7,20.7]	[-102.4,-66.2]	[-75.5,-30.9]	[-61.9,-32.1]	[-51.0,-25.9]
Obs.	261	400	404	404	404

(b) CDS-IV

	(1) Holdout	(4) Restructured: Disc.	(5) Restructured: Par	(2) Domestic: Boden 15	(3) Domestic: Bonar X
ΔD	-29.79	-100.0***	-34.06	-56.23**	-35.77
SE	(33.80)	(17.59)	(19.65)	(13.12)	(10.06)
95% CI	[-91.3,107.9]	[-165.5,-65.4]	[-106.5,58.6]	[-101.3,-4.1]	[-62.3,11.0]
Obs.	261	400	404	404	404

Notes: This table reports the results the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on the price of Argentine government bonds. The coefficient on ΔD is the effect on the bond price of an increase in the 5-year risk-neutral default probability from 0% to 100%, implied by the Argentine CDS curve. "Holdout" is a euro-denominated bond issued in May 1999 and defaulted bond on in 2001. Its ISIN is DE0003045357. "Restructured: Disc." is a dollar-denominated discount bond issued as part of the 2010 restructuring with an ISIN of XS0501194756. "Restructured: Par" is a dollar-denominated par bond issued as part of the 2005 restructuring with an ISIN of US040114GK09. The data on Restructured: Disc. is far less stale than Restructured: Par and the Par results should be interpreted with caution. "Domestic: Boden 15" is domestic-law fixed coupon dollar debt maturing in 2015 with an ISIN ARARGE03F144. "Domestic: Bonar X" is domestic-law fixed coupon dollar debt maturing in 2017 with an ISIN ARARGE03F441.

I Econometric Model

The model we use is

$$\begin{aligned} \Delta D_t &= \mu_d + \omega_D^T X_t + \gamma^T r_t + \beta_D F_t + \varepsilon_t \\ r_t &= \mu + \Omega X_t + \alpha \Delta D_t + \beta F_t + \eta_t, \end{aligned}$$

where r_t is a vector of returns, ΔD_t is the change in the default probability, X_t is a set of global factors (S&P 500, etc...), F_t is an unobserved factor, and ε_t is the idiosyncratic default probability shock, and η_t is a vector of return shocks that do not directly affect the probability of default. Through some algebra, we show that this is equivalent to the systems described in equations 1 and 2, used in most of our analysis, and equations 9 and 10 used in the cross-sectional analysis.

We begin by separating the equation governing the vector of returns r_t into the return of asset i, $r_{i,t}$, which is the asset of interest, and the returns of some other assets, denoted $r_{-i,t}$. We separate the various coefficient vectors and matrices, $\mu, \Omega, \alpha, \beta, \gamma$, and shocks η_t, into versions for asset i, μ_i, ω_i^T, etc..., and versions for the other assets, μ_{-i}, Ω_{-i}, etc... This system can be written as

$$\begin{aligned}
\Delta D_t &= \mu_d + \omega_D^T X_t + \gamma_i^T r_{i,t} + \gamma_{-i}^T r_{-i,t} + \beta_D F_t + \varepsilon_t \\
r_{i,t} &= \mu_i + \omega_i^T X_t + \alpha_i \Delta D_t + \beta_i F_t + \eta_{i,t} \\
r_{-i,t} &= \mu_{-i} + \Omega_{-i} X_t + \alpha_{-i} \Delta D_t + \beta_{-i} F_t + \eta_{-i,t}.
\end{aligned}$$

Most of our analysis considers only a single asset, $r_{i,t}$, and the default probably change ΔD_t. Substituting the returns $r_{-i,t}$ into the ΔD_t equation,

$$\begin{aligned}
\Delta D_t &= \frac{\mu_d + \gamma_{-i}^T \mu_{-i}}{1 - \gamma_{-i}^T \alpha_{-i}} + \frac{\omega_D^T + \beta_{-i}^T \Omega_{-i}}{1 - \gamma_{-i}^T \alpha_{-i}} X_t + \frac{\gamma_i^T r_{i,t}}{1 - \gamma_{-i}^T \alpha_{-i}} + \\
&\quad \frac{\beta_D + \gamma_{-i}^T \beta_{-i}}{1 - \gamma_{-i}^T \alpha_{-i}} F_t + \frac{1}{1 - \gamma_{-i}^T \alpha_{-i}} (\gamma_{-i}^T \eta_{-i,t} + \varepsilon_t) \\
r_{i,t} &= \mu_i + \omega_i^T X_t + \alpha_i \Delta D_t + \beta_i F_t + \eta_{i,t}.
\end{aligned}$$

This system, for the two assets, is equivalent to the one in equations 1 and 2, except that is has two shocks, $\gamma_{-i}^T \eta_{-i,t}$ and ε_t, that directly affect ΔD_t without affecting $r_{i,t}$, and includes constants and observable controls X_t. Neither of these differences substantially alter the identification assumptions or analysis. The event study and Rigobon (2003) approach both identify the coefficient α_i, under their identifying assumptions, which is the coefficient of interest.

Next, we discuss a version of this system with the market return. Let the market return be a weighted version of the return vector, $r_{m,t} = w^T r_t$. Separating the vectorized version of the system into four equations,

$$\begin{aligned}
\Delta D_t &= \mu_d + \omega_D^T X_t + \gamma_i^T r_{i,t} + \gamma_{-i}^T r_{-i,t} + \beta_D F_t + \varepsilon_t \\
r_{i,t} &= \mu_i + \omega_i^T X_t + \alpha_i \Delta D_t + \beta_i F_t + \eta_{i,t} \\
r_{-i,t} &= \mu_{-i} + \Omega_{-i} X_t + \alpha_{-i} \Delta D_t + \beta_{-i} F_t + \eta_{-i,t} \\
r_{m,t} &= \mu_m + \omega_m^T X_t + \alpha_m \Delta D_t + F_t + w^T \eta_t,
\end{aligned}$$

where $\mu_m = w^T \mu$, $\omega_m^T = w^T \Omega$, and so on. We have assumed that $w^T \beta = 1$, which is a normalization. Substituting out $r_{-i,t}$,

$$
\begin{aligned}
\Delta D_t &= \frac{\mu_d+\gamma_{-i}^T\mu_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}+\frac{\omega_D^T+\beta_{-i}^T\Omega_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}X_t+\frac{\gamma_i^T r_{i,t}}{1-\gamma_{-i}^T\alpha_{-i}}+ \\
&\quad \frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}F_t+\frac{1}{1-\gamma_{-i}^T\alpha_{-i}}(\gamma_{-i}^T\eta_{-i,t}+\varepsilon_t) \\
r_{i,t} &= \mu_i+\omega_i^T X_t+\alpha_i\Delta D_t+\beta_i F_t+\eta_{i,t} \\
r_{m,t} &= \mu_m+\omega_m^T X_t+\alpha_m\Delta D_t+F_t+w^T\eta_t,
\end{aligned}
$$

as above. Next, we solve for F_t using the market return equation:

$$F_t = r_{m,t}-\mu_m-\omega_m^T X_t-\alpha_m\Delta D_t-w^T\eta_t.$$

Plugging this into our system of equations,

$$
\begin{aligned}
(1+\alpha_m\frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}})\Delta D_t &= (\frac{\mu_d+\gamma_{-i}^T\mu_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}-\frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}\mu_m)+(\frac{\omega_D^T+\beta_{-i}^T\Omega_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}-\frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}\omega_m^T)X_t+ \\
&\quad \frac{\gamma_i^T r_{i,t}}{1-\gamma_{-i}^T\alpha_{-i}}+\frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}r_{m,t}+(\frac{\gamma_{-i}^T}{1-\gamma_{-i}^T\alpha_{-i}}-\frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}w_{-i}^T)\eta_{-i,t}+ \\
&\quad \frac{\beta_D+\gamma_{-i}^T\beta_{-i}}{1-\gamma_{-i}^T\alpha_{-i}}w_i\eta_{i,t}+\frac{1}{1-\gamma_{-i}^T\alpha_{-i}}\varepsilon_t \\
r_{i,t} &= (\mu_i-\beta_i\mu_m)+(\omega_i^T-\beta_i\omega_m^T)X_t+(\alpha_i-\beta_i\alpha_m)\Delta D_t \\
&\quad +\beta_i r_{m,t}+(1-w_i\beta_i)\eta_{i,t}+w_{-i}^T\eta_{-i,t}.
\end{aligned}
$$

This system is equivalent to the one presented in equations 9 and 10, except that there are multiple common factors ($\eta_{i,t}$ and $\eta_{-i,t}$) and no idiosyncratic return shocks. The event study and Rigobon (2003) approach both identify the coefficient ($\alpha_i-\beta_i\alpha_m$), under their identifying assumptions, which is the coefficient of interest.

J Standard Errors with Tracking Portfolios

In this section, we describe how we incorporate the estimation error associated with our tracking portfolio coefficients into our confidence intervals for our heteroskedasticity-based analysis. This procedure will also apply to the event study that uses our bootstrapped standard errors. For the event studies that use the J_1 and J_2 statistics, we ignore the estimation error associated with the tracking portfolio weights $\hat{\beta}$.

Suppose that our estimate of $\hat{\beta}$ has the standard asymptotic distribution,

$$\sqrt{T}(\hat{\beta}-\beta) \rightarrow^p N(0,V),$$

where T is the number of quarters in our quarterly data set and V is a covariance matrix. The matrix V will be constructed differently, depending on which of our tracking portfolios is being considered.

We assume that our estimation error for $\hat{\beta}-\beta$ is independent of our estimation error in the heteroskedasticity-based analysis. This assumption could be justified by truncating our low frequency data prior to the sample period for our event study, and asserting that the abnormal returns and default probability changes we observe are unpredictable. As a practical matter, we are reluctant to discard any data from our relatively short quarterly data set, and present results with a dataset that runs from 2003 to 2014.

Next, consider the first-order asymptotic standard errors associated with the heteroskedasticity-based estimator. Let α_r be vector of coefficients of interest for the assets in our tracking portfolio. The standard errors for our estimates of these coefficients are described by

$$\sqrt{M}(\hat{\alpha}_r-\alpha_r) \rightarrow^p N(0,\Omega),$$

where M is the size of our high-frequency data sample and Ω is a covariance matrix.

Our estimate of α_y, the true coefficient of interest for real GDP news, is

$$\hat{\alpha}_y = \hat{\beta}^T \hat{\alpha}_r.$$

Because the CDS-IV analysis is linear, one can arrive at this estimator either by explicitly constructing a time series for the tracking portfolio, using $\hat{\beta}$ as the weights, and then running the analysis, or by running the analysis on all of the assets to generate $\hat{\alpha}_r$, and then computing the weighted sum of the coefficients. This linearity does not hold for the Returns-IV or GMM estimators, and we do not run our tracking portfolios when we apply those methods.

Let $m = MT^{-1}$ be the ratio of the size of the two datasets (the high-frequency and the quarterly data sets). In the limit in which both data sets grow to infinite size, while the ratio m stays the same, we have

$$\sqrt{T}(\hat{\alpha}_y-\alpha_y) \rightarrow^p N(0, \alpha_r^T V \alpha_r + m^{-1}\beta^T \Omega \beta),$$

by the independence assumption. Given feasible, consistent estimates of V and Ω, we could use these standard errors to compute confidence intervals. However, consistent with the spirit of the procedure described in section 4.4, we prefer to use a bootstrap procedure for our high-frequency data.

Under these asymptotics, the t-statistic

$$\hat{t} = \sqrt{T}\frac{\hat{\alpha}_y - \alpha_y}{\sqrt{\hat{\alpha}_r^T \hat{V} \hat{\alpha}_r + m^{-1} \hat{\beta}^T \hat{\Omega} \hat{\beta}}}$$

is asymptotically standard normal. We employ a sort of hybrid bootstrap procedure for this t-statistic. For the high-frequency data, we create bootstrap replication k by drawing events and non-events, with replacement, and computing $\alpha_{r,k}$ and Ω_k, as described previously. For the estimates of β_k in this replication, we draw from the multivariate normal distribution $N(\hat{\beta}, T^{-1}\hat{V})$. We use the actual sample estimate of the covariance matrix $\hat{V}$ in each replication. The t-statistic t_k for the bootstrap replication is

$$t_k = \sqrt{T}\frac{\beta_k^T \alpha_k - \hat{\alpha}_y}{\sqrt{\alpha_{r,k}^T \hat{V} \alpha_{r,k} + m^{-1} \beta_k^T \hat{\Omega}_k \beta_k}}.$$

We use the bootstrap sample distribution for t_k to construct confidence intervals, as described in section 4.4. This procedure has the virtue that, if the replicating portfolio weights were known with certainty ($\hat{V} = 0$), the results for a particular portfolio would be identical to the results described in section 4.5.

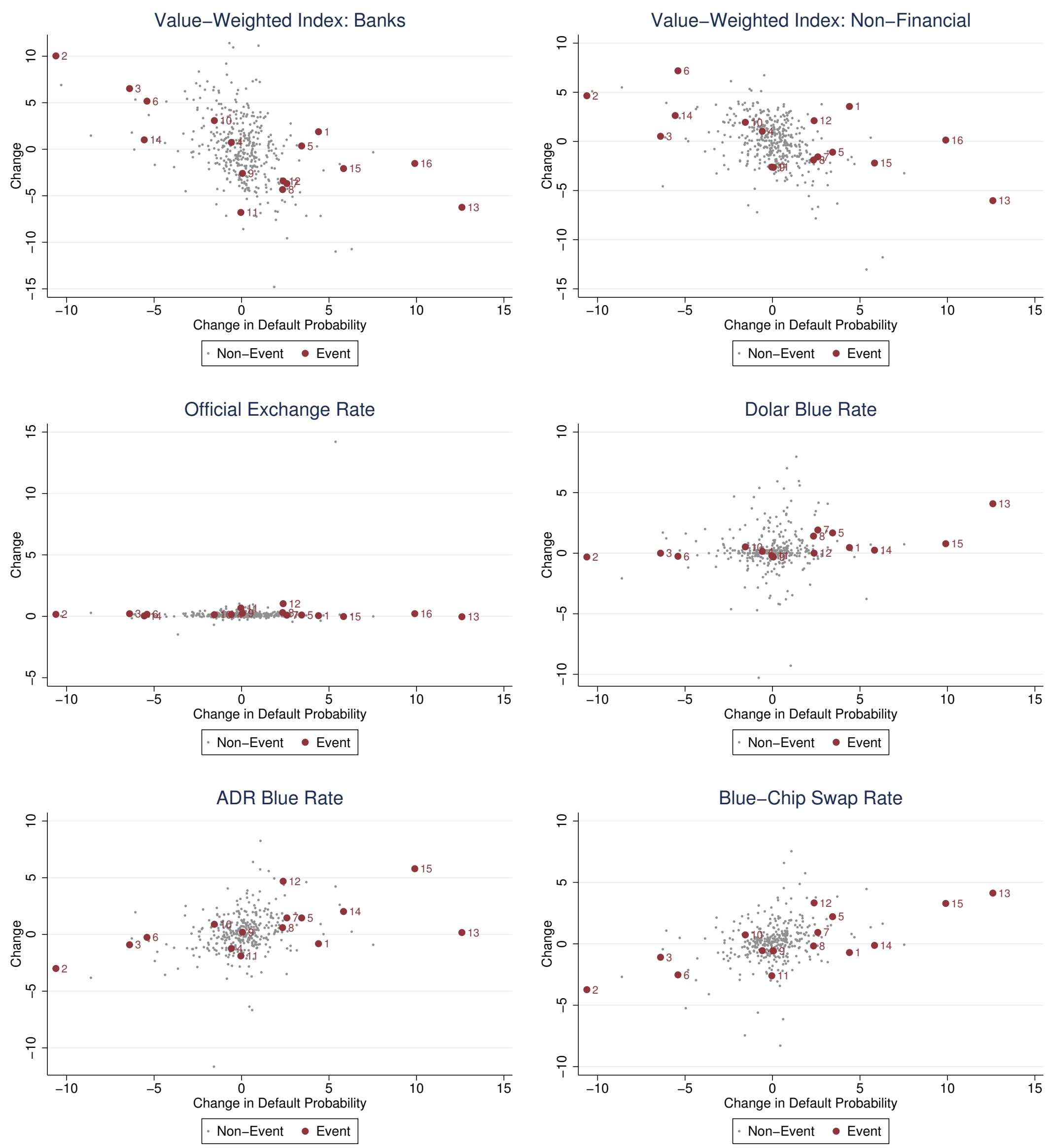
Having described the general procedure, we will now discuss how we implement it in three different cases: the forecast models, the VAR model where we neglect $O(1-\rho)$ terms, and the VAR model in which we do not neglect those terms. The simplest case is the forecast models, because the tracking portfolio is constructed by running a standard OLS regression. The covariance matrix V can be estimated with a standard heteroskedasticity and auto-correlation robust estimator, such as the Newey-West estimator (Newey and West (1987)).

The full VAR case is slightly more complicated. The $O(1-\rho)$ terms, which are a function of the estimated VAR matrix A and covariance matrix Σ, have estimation errors that are $O(T^{-\frac{1}{2}})$, and therefore asymptotically dominate the estimation error of ϕ, which is "super-consistent" (Stock and Watson (1993)). For this case, we neglect the estimation error in ϕ, and construct standards errors for β using the delta method.

The dynamic OLS case, where we neglect the VAR terms and just use our estimate of ϕ, is more complicated. The estimate for the coefficient ϕ is not asymptotically normal. However, the estimate of the variance of $\hat{\phi} - \phi$ from the OLS estimator is still valid (see pg. 608-610 of Hamilton (1994)). We can adapt the above argument to this case by assuming that the ratio $m = MT^{-2}$ is constant as both data sets grow large, so that the first-order errors from the estimation using the high-frequency data and the low frequency data are of the same asymptotic order.

K Summary Figures

Figure A1: Change in Default Probability and other Financial Variables on Event and Non-Event Days



Notes: This figure plots the change in the risk-neutral probability of default and returns on the Value-Weighted Bank and Non-Financial Index and four measures of the exchange rate, on event and non-event days. Official is the government's official exchange rate. Dolar Blue is the onshore unofficial exchange rate from dolarblue.net. ADR Blue is the ADR Blue Rate constructed by comparing the ADR share price in dollars with the underlying local stock price in pesos, as described in Section 3. Blue-Chip Swap is constructed by comparing the ARS price of domestic Argentine sovereign debt with the dollar price of the same bond, as described in Section 3. NDF - 12M is the 12 month non-deliverable forward rate.Each event and non-event day is a two-day event or non-event as described in the text. The numbers next to each maroon dot references each event-day in the table below Figure 2. The procedure for classifying events and non-events is described in the text.

L Event and Excluded Dates

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
7Dec11	Excluded	Original ruling by Judge Griesa with regards to Pari Passu clause.	7Dec11, 12:55pm	Decision	Missing	Missing	There was very little contemporaneous news coverage, and we are unable to determine when the ruling became public. The first story we found about the ruling is based on an article in "La Nacion" published on 5Mar12.
23Feb12	Excluded	Order by Judge Griesa requiring "ratable payment."	Missing	Order	Missing	Missing	See above.
05Mar12	Excluded	Stay granted by Judge Griesa, pending appeal.	Missing	Stay	05Mar12, 7:11am	Bloomberg	See above.
26Oct12	Excluded	Appeals court upholds Judge Griesa's ruling that the Pari Passu clause requires equal treatment of restructured bondholders and holdouts.	25Oct12, 12:43pm	Decision	26Oct12, 2:14pm	Bloomberg	The appeals court releases opinions during the middle of the day. Unfortunately, the closing marks on this day are questionable, given the impending impact of "Superstorm Sandy."

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
23Nov12	Excluded	Judge Griesa removes the stay on his order that Argentina immediately pay the holdouts, if they also pay the exchange bondholders.	Missing	Order	22Nov12, 5:33am.	Business News Americas	Nov 22 was Thanksgiving in the United States, and all CDS marks on that date and the morning of the 23rd appear in our data to be the same as on the 21st. The opinion was filed by Judge Griesa on the night of the 21st, but was embargoed until the 23rd. On the 22nd, the Argentine market fell a lot, but bounced back on the 23rd. We cannot observe this in the ADR data, so we exclude this event.
27Nov12	Open-to-Open, 26Nov12 to 27Nov12	Judge Griesa denies the exchange bondholders request for a stay. The bondholders immediately appealed.	26Nov12, 3:43pm	Denial	27Nov12, 5:00am.	New York Post	The denial occurred on the 26th, and both the government of Argentina and the exchange bondholders immediately appealed. We compare the open on the 27th to the open on the 26th. The 26th is an Argentine holiday, so the ADR Blue Rate is missing (for the open-to-open, but not the two day window).
29Nov12	Close-to-Open, 28Nov12 to 29Nov12	Appeals court grants emergency stay of Judge Griesa's order.	28Nov12, 5:04pm[88]	Stay	29Nov12, 8:24am.	Bloomberg	
05Dec12	Open-to-Close, 04Dec12	Appeals court denies request of holdouts to force Argentina to post security against the payments owed.	04Dec12, 1:15pm.[89]	Denial	04Dec12, 1:46pm.	Bloomberg	

[88]This order has a 9pm "creation time" and a 5pm "modification time."
[89]This "creation time" of this PDF is actually at 4pm, 3 hours after the "modification time".

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
07Dec12	Close-to-Close, 05Dec12 to 06Dec12	Appeals court allows the Bank of New York (custodian of the exchange bonds) and the Euro bondholders to appear as interested parties.	05Dec12, 10:13pm.	Order	06Dec12, 11:47am	Bloomberg	
11Jan13	Close-to-Open, 10Jan13 to 11Jan13	Appeals court denies certification for exchange bondholders to appeal to NY state court for interpretation on Pari Passu clause.	10Jan13, 4:10pm	Order	11Jan13, 12:01am	Bloomberg	The ruling was written immediately after the closes on the 10th.
28Feb13	Excluded	Appeals court denies request for en-banc hearing of appeal.	28Feb13, 3:27pm.	Decision	Missing	Shearman	The denial occurred at the beginning of a hearing, during which lawyers for both sides argued various issues. Lawyers from Argentina may have changed their arguments in response to expectations about the Argentine economy, violating the exclusion restriction.
04Mar13	Open-to-Open, 01Mar13 to 04Mar13	Appeals court asked Argentina for a payment formula.	01Mar13, 11:49am.	Order	01Mar13, 4:46pm	Financial Times	The FT story is the earliest we could find. Most other coverage is from the following day (a Saturday).
27Mar13	Open-to-Open, 27Mar13 to 26Mar13	Appeals court denies Argentina's request for en-banc rehearing.	26Mar13, 11:58am	Order	26Mar13, 2:35pm	Bloomberg	The Bloomberg story specifically mentions a 374bp increase in the 5yr CDS spread, which does not appear in our data until after the NY close at 3:30pm. We use the one day window to ensure we are capturing the event.

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
01Apr13	Non-Event (neither event or ex-cluded)	Argentina files payment plan. Offer roughly 1/6 of Judge Griesa ordered.	N/A	N/A	30Mar13, 12:05pm	Bloomberg	Argentina filed just before midnight on 28Mar13. Actions by Argentina are endogenous. This neither an event nor excluded.
22Apr13	Non-Event (neither event or ex-cluded)	Holdouts reject Argentina's payment plan.	19Apr13, 5:20pm	Reply	20Apr13, 12:01am	Bloomberg	Holdouts reject Argentina's payment plan. Also conceivably endogenous. The rejection was filed after business hours on Friday, 19Apr13. This is also neither an event nor excluded.
26Aug13	Close-to-Close, 22Aug13 to 23Aug13	Appeals court upholds Griesa's decision.	22Aug13, 4:21pm	Decision	23Aug13, 4:02pm	Bloomberg	The appeals court announces decisions during the business day. The modification date of the PDF is 10:17am.
11Sep13	Non-Event	Supreme court schedules hearing to consider Argentina's appeal.	Missing	Docket Info.	11Sep13, 2:35pm.	Bloomberg	The supreme court distributed case materials related to Argentina's petition. We were advised that this is routine and not "news," so we do not count is as a ruling.
26Sep13	Excluded	Holdouts had petitioned Griesa to consider the Argentine central bank liable for the defaulted debt. Argentina motioned to dismiss, and Griesa rejected Argentina's motion.	Missing	Missing	25Sep13, 5:40pm.	Bloomberg	We were not able to find Griesa's ruling, so we exclude this event.

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
04Oct13	Open-to-Open, 03Oct12 to 04Oct13	Griesa bars Argentina from swapping the exchange bonds into Argentine-law bonds.	03Oct13, 2:43pm.	Order	03Oct13, 6:27pm.	Bloomberg	
08Oct13	Open-to-Close, 07Oct13	Supreme court denies Argentina's first petition.	N/A	Order	07Oct13, 11:45am	SCOTUS Blog	The stock market opens (9:30am EST) before the Supreme court issues decisions (9:30am or 10:00am EST).
19Nov13	Open-to-Open, 18Nov13 to 19Nov13	Appeals court denies Argentina's request for an en-banc hearing.	18Nov13, 11:04am	Denial	19Nov13, 12:01am	Bloomberg	The modification time of the orders is 4:53pm.
13Jan14	Open-to-Close, 10Jan14	Supreme court agrees to hear Argentina case.	10Jan14, 2:42pm	Order	10Jan14, 2:48pm	SCOTUS Blog	The supreme court usually announces orders at 10am. The document was likely posted afterwards.
17Jun14	Open-to-Close, 16Jun14	Supreme court denies Argentina's second petition.	See Text.	Denial	See Text.	SCOTUS Blog	The text discusses this event in detail.
23Jun14	Close-to-Open, 20Jun14 to 23Jun14	Griesa prohibits debt swap of exchange bonds to Argentine law bonds.	20Jun14, 2:17pm	Order			20Jun14 is an Argentine holiday, so the local stocks are missing. During day of the 20th, the Argentine president made a market-moving speech, which we do not want to include, so we start this event only at the close of the 20th.
24Jun14	Open-to-Open, 23Jun14 to 24Jun14	Griesa appoints special master to oversee negotiations.	23Jun14, 12:36pm	Order	23Jun14, 7:35pm	Bloomberg	The modification date for the order is 1:05pm. With two-day windows, this event is pooled with the previous event.

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
27Jun14	Open-to-Close, 26Jun14	Griesa rejects Argentina's application for a stay, pending negotiations.	26Jun14, 11:40am	Order	26Jun14, 2:05pm	Bloomberg	
30Jun14	Non-Event	Argentina misses a coupon payment					
29Jul14	Open-to-Open, 28Jul14 to 29Jul14	Griesa allows Citi to pay Repsol bonds for one month.	28Jul14, 3:51pm	Order	28Jul14, 12:01am	Bloomberg	The modification time on the order is 5:07. This event almost certainly occurred post-close, but we use the one day window to be safe.
30Jul14		The 30-day grace period for the missed payment expires.				Bloomberg	We end our dataset on 29Jul14.