The Costs of Sovereign Default: Evidence from Argentina*

Benjamin Hébert †
Stanford University

Jesse Schreger ‡
Princeton University
Harvard Business School

December 2015

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*. 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 an entirely unexpected sovereign default would cause a decline in this measure of between 5.9% and 10.9%.

*We thank Laura Alfaro, Luigi Bocola, John Campbell, Jeff Frieden, Gita Gopinath, Ed Glaeser, Paul Goldsmith-Pinkham, Eduardo Levy Yeyati, Matteo Maggiori, Monika Piazzesi, Mikkel Plagborg-Møller, Ken Rogoff, Alex Roth, Fernando Alvarez, Christoph Trebesch, Victoria Vanasco, Wenxin Du, Joaquin Blaum, and Mary Bryce Millet Steinberg for helpful conversations. We thank Vivian Yue for an excellent discussion. We thank Alberto Cavallo and Brent Neiman for feedback and for generously sharing data. We thank seminar audiences at the NBER IFM Spring Meetings, the SED Annual Meeting, NYU Stern, Princeton, Stanford, Northwestern, Berkeley, UNC Kenan-Flagler, Michigan Ross, the Chicago International Macro-Finance Conference, the Boston Area Juniors Macro Group, and DIW Berlin. We thank Stephen King, Vivek Anand, and Tom Adney of Markit for useful discussions about the data. All errors are our own. Source code available at https://github.com/bhebert/CostOfSovereignDefault

†Hébert: Graduate School of Business and Department of Economics, Stanford University.
Email: bhebert@stanford.edu

‡Schreger: Princeton University and Harvard Business School.
Email: schreger@princeton.edu.

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 identify the causal effect of sovereign default. Following Argentina's sovereign default in 2001, NML Capital, a hedge fund, purchased defaulted bonds and refused to join other creditors in restructurings of the debt that occurred in 2005 and 2010. Instead, because the defaulted 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.

We argue that these legal rulings are exogenous shocks to the risk-neutral probability of default that allow us to identify the causal effect of sovereign default on the market value of Argentine firms. Our key assumption is that the information revealed to market participants by these legal rulings affects firms' stock returns only through the effect on the sovereign's risk-neutral probability of default. This assumption requires that the judges in U.S. courts making these rulings do not have private information about the Argentine economy. This assumption also requires that the firms are not directly affected by the rulings. Consistent with this assumption, Argentine firms are legally separate from the federal government of Argentina and are not subject to attachment of their assets by creditors of the sovereign.[1]

We use credit default swaps (CDS) to measure the change in the risk neutral probability of default.

[1]We discuss other potential challenges to our identifying assumption in more detail in section 7.

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 isolate fifteen rulings that potentially changed the probability of default. We identify the effect of changes in the default probability on equity returns through heteroskedasticity, following Rigobon (2003) and Rigobon and Sack (2004).[2] We describe this procedure and our identification assumptions explicitly in section 4. We find that, for every 1% increase in the 5-year cumulative default probability around these rulings, the US dollar value of a value-weighted index of Argentine American Depository Receipts (ADRs) falls 0.55%.[3] 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 28%. The most direct interpretation of these negative stock returns is that they reflect a decline in the expected value of the stream of dividends that will be paid by the firms. However, alternative interpretations are possible, and we discuss them in section 7.

We next translate our estimate of the effect on stock returns into a prediction about real economic activity. Using quarterly data from 2003 to 2014, we estimate a relationship between stock returns and real GDP growth. We then use this relationship to form GDP tracking portfolios, and estimate the effect of our default probability shocks on these tracking portfolios. We find that the present discounted value of expected future real GDP growth, as measured by our tracking portfolios, drops by between 0.059% and 0.109% for every 1% increase in the 5-year, risk-neutral cumulative default probability. These estimates are large when compared to standard quantitative models of sovereign default (e.g. Aguiar and Gopinath (2006) and Arellano (2008)) 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, similar to the persistent decline in output following other sovereign defaults, documented by Gornemann (2014), and following financial crises, documented by Cerra and Saxena (2008) and Reinhart and Rogoff (2009). We argue that anticipated changes in government policy, conditional on default, could explain why output losses would persist. This explanation is consistent with the story that Aguiar and Gopinath (2007) use to explain the persistence of innovations in the Solow residual for emerging markets, and narratives about policy in Latin America (Dornbusch and Edwards (1991)).

To better understand how this sovereign default was expected to affect the economy, and in particular

[2] In the appendix, section D, we run a traditional event study, and find similar results.

[3] ADRs are shares in foreign firms that trade on US stock exchanges in US dollars.

why the effect 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 foreign-owned firms, exporters, banks[4], and large firms are hurt more by increases in the probability of sovereign default than would be expected, given their "beta" to the Argentine market and exchange rate. We hypothesize that these types of firms are particularly exposed to government policies that might have been expected to change upon default.

This paper contributes to a large literature examining the costs of sovereign default, surveyed in Borensztein and Panizza (2009). Using quarterly time series, Levy Yeyati and Panizza (2011) find that output generally falls in anticipation of a sovereign default and that 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) finds 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), in which default is costly because it reduces the ability of domestic firms to import intermediate goods, 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. Methodologically, our paper uses a natural experiment to try to estimate the causal effect of sovereign default. Fuchs-Schundeln and Hassan (2015) survey the literature on natural experiments in macroeconomics.

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. Section 4 presents our estimation framework and results. Section 5 discusses how we use tracking portfolios to estimate the impact of default on output. Section 6 discusses firm characteristics that are associated with larger responses to changes in the probability of default. Section 7 discusses institutional details and alternative interpretations of the results.

[4]Our point estimates are negative and economically significant for banks, consistent with the theoretical literature, but our standard errors are too large to reject the hypothesis of no differential effects.

Section 8 concludes.

2 Argentina's Sovereign Debt Saga

In 2001, Argentina entered a deep recession, with unemployment reaching 14.7% in the fourth quarter. 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%.[5]

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. 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.

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.[6] At this point, Argentina had restructured over 90% of the original face value of its debt.

Following the 2010 debt exchange, the remaining holdout creditors, termed "vultures" by the Argentine government, continued their legal battle. This litigation eventually culminated in Argentina's 2014 default on its restructured bond holders. The creditors, led by NML Capital,[7] argued that the Argentine government breached the *pari passu* clause, which requires equal treatment of all bondholders, by paying the restructured bondholders and refusing to honor the claims of the holdouts.

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 States Court of Appeals for the Second Circuit ("Second Circuit"), all the way to the United States Supreme Court. These three courts

[5]Data and facts cited in this section from Global Financial Data, Daseking et al. (2005), Hornbeck (2013), and Thomas and Marsh (2014).

[6] The interest on the defaulted debt has continued to accumulate since 2010, with the total amount owed reaching $15 billion in 2014 (Gelpern (2014a)).

[7]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 a literature review on the law and economics of sovereign default.

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, and July 2014, when Argentina defaulted.[8]

Following Griesa's initial ruling in December 2011, years 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 continued 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. Griesa also 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 sent this coupon payment to the bond trustee, Bank of New York Mellon (BNYM), but due to 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. On September 3, 2014, the auction associated with the settlement of the CDS contracts was held, and it resulted in a recovery rate of 39.5 cents on the dollar.[9]

The cumulative effect of these legal rulings was to change 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. Instead, Argentina could attempt to settle with the holdouts, to avoid defaulting on its restructured bondholders, or it could default on the restructured bondholders.

Argentina effectively chose to default. In the simplest interpretation of these events, making the required payments was not possible, and Argentina was forced to default by the US court system. This was the interpretation offered by a number of commentators in the financial press (e.g. O'Brien (2014)). However, if a settlement was possible, the rulings might have also raised the probability of a settlement. If Argentine

[8]The litigation is ongoing, and neither the holdouts or restructured creditors are receiving payments, as of November 2015.

[9]The low recovery rate reflects both the decline in the price of Argentina's bonds since 2011 and the fact that some (the "cheapest to deliver") of Argentina's bonds had relatively low coupons, and therefore low prices, when they were issued.

firms would be affected by a settlement, through some channel other than the avoidance of a sovereign default, then the exclusion restriction of our experiment would not hold. We argue that such channels are not plausible, because the amount of money required for a settlement was small, in section 7.

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 returns of Argentine peso-denominated equities traded in Argentina. The ADRs trade on the NYSE and NASDAQ, are relatively liquid, and can be traded by a wide range of market participants.[10] However, using only the ADRs limits our analysis to twelve firms that have exchange-traded ADRs. To study the cross-sectional patterns of Argentine firms, we also examine the returns of firms traded only in Argentina. The full list firms included in our analysis, along with select firm characteristics, can be seen in appendix table A2.

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 A2. We construct value-weighted indices for the entire market each of these industries, except real estate.[11] The value-weighted indices we construct exclude YPF, the large oil company that was nationalized in 2012. The market index also includes a 10% weight on US treasury bills, to ensure that its dividends are always positive.[12]

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

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

[11]We do not include a value-weighted real estate index in the results because there are only two closely-related firms in this sector. We have also constructed equal-weighted indices, and found similar results.

[12]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).

(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 K, 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.[13]

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. Controlling for these factors is not necessary, under our identification assumptions, but can reduce the magnitude of our standard errors. To proxy for global risk aversion, we use the VIX index, the 30-day implied volatility on the S&P 500.[14] 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. We also control for oil prices (West Texas Intermediate). 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).

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 the financial press (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.

[13] We prefer the 5-year cumulative default probability measure because we believe that shocks which move default probability from (for example) one year ahead to two years ahead, without altering the cumulative default probability over those two years, should have only a minimal impact on stock valuations. Our results are qualitatively robust to using the 1- or 3-year cumulative default probability, and other CDS-based measures, subject to the caveat that the 1- and 3-year cumulative default probabilities are more volatile than the 5-year measure. Tenors longer than five years are not traded frequently. See appendix table A11 for details.

[14] See Longstaff et. al. (2011) for discussion of the relationship between the VIX and sovereign CDS spreads.

In appendix section Q, 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 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. 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 Q, 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 Thursday, June 12th to the close on Monday, June 16th. It would use stock returns (for both ADRs and local stocks) from 4pm EDT on Thursday, June 12th to 4pm EDT on Monday, June 16th.[15]

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.[16] 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 2a, 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 light gray are non-events and the maroon/dark dots cover event windows in which a US court ruling was released. The details on each event can be found in the

[15]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 do not correct for this. For some events, we are certain about the day of the event. For these events, we place the event on the first day of the two-day window; however, our results are robust to placing the event on the second day of the two-day window instead.

[16]For three of the rulings, we could not find any contemporaneous media coverage. For one ruling, we could not find the ruling itself. One of the rulings was issued on the Friday in October 2012 shortly before "Superstorm Sandy" hit New York, and another the night before Thanksgiving. 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. Finally, we exclude the ruling made on July 28th, 2014, because this ruling was made very close to the formal default date, and news articles on that day focused on the last-minute negotiations, not the ruling. Our results are qualitatively similar when we include this ruling.

appendix, section Q. In figure 2b, 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 Argentine default probability measure. However, on the event days, only the Argentine equity index co-moves with the Argentine default probability measure. This observation suggests that omitted common factors might not be very important on our event days, consistent with the result that our event studies and heteroskedasticity-based identification strategy reach similar conclusions. In appendix section B, we present similar figures for the different sectors of the Argentine economy and measures of the exchange rate.

[Insert figure 2 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,[17] 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. This onshore rate is known as the Dolar Blue or the Informal dollar, among many other names. This is our preferred measure of Argentina's market exchange rate.

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

[17] EconomistMagazine (2014).

in dollars. The first class of instruments for which this can be done are domestic law Argentine government bonds, and the exchange rate associated with this transaction is known as the "blue-chip swap" rate. We can construct a similar measure of the exchange rate, known as the "ADR blue rate," by using equities rather than debt.[18]

[Insert Figure 3 about here]

In Figure 3, we plot all four of these exchange rates during our sample period. Throughout this period, the official rate is significantly below the unofficial rates. The ADR blue rate and the blue-chip swap rate are virtually indistinguishable (at low frequencies) during this period, and co-move with the Dolar Blue rate.

3.5 Case Study: Supreme Court Announcement

For one of our events, we are able to precisely determine when the event occurred. On June 16, 2014, the U.S. Supreme Court denied two appeals and a petition from the Republic of Argentina. 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.[19]

The Supreme Court announces multiple orders in a single public session, and simultaneously provides copies of those orders to the press. SCOTUSBlog, a well-known legal website that provides news coverage and analysis of the Supreme Court, had a "live blog" of the announcements on June 16th, 2014. At 9:33am EST, SCOTUSBlog reported that "Both of the Argentine bond cases have been denied. Sotomayor took no part" (Howe (2014)). 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 1, 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.

[18] Auguste et al. (2006) explore how the convertibility of ADRs provides a way around capital controls. Both of our measures rely on Argentine local markets, which are illiquid, and therefore can be quite noisy at high frequencies. Pasquariello (2008) documents that, for countries with convertible currencies, ADR parity does not always hold, and that the violations of ADR parity are more common around financial crises. As a result, we should not necessarily expect our ADR-based measure and the dolar blue to respond identically to the legal rulings.

[19] On the same day, the Supreme Court also allowed the holdouts to pursue discovery against all of Argentina's foreign assets, not just those in the United States.

[Insert figure 1 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. Assuming that no other news affected the markets during this hour, 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 Framework and Results

In this section, we estimate the causal effect of sovereign default on equity returns using all of the events in our sample, which requires using two-day event windows. The key identification concerns are that stock returns might have an effect on default probabilities, and that 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. 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 the log return of a single asset (the equity index, for example), r_t, and the change in the risk-neutral probability of default, ΔD_t, and ignore constants.[20] 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 a single unobserved factor that moves both the probability of default and equity returns, ε_t is a shock to the default probability, η_t is a shock to the equity market return, and all of these shocks are uncorrelated with each other and over time. The goal is to estimate the parameter α, the impact of a change in the probability of default on equity market returns.

Our key identifying assumption is that the information revealed to market participants by the legal rul-

[20] In the appendix, section N, we demonstrate how an equivalent system can be derived in a multi-asset framework.

ings affects firms' stock returns only through the effect on the sovereign's risk-neutral probability of default. This assumption is equivalent to asserting that the legal rulings are idiosyncratic default probability shocks (ε_t) in the framework above. The assumption embeds both the requirement that the legal rulings be exogenous (the ε_t shocks are not correlated with the other shocks) and that the exclusion restriction is satisfied (the ε_t shocks affect returns only by affecting default probabilities).

If one were to simply run the regression in equation 2 using OLS, the coefficient estimate could be biased. There are two potential sources of bias: simultaneity bias (stock returns affect default probabilities) and omitted variable bias (unobserved common factors).[21] In order for the OLS regression to be unbiased, equity market returns must not affect default probabilities and there must be no omitted common shocks. These assumptions are implausible in our context, but we present OLS results 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 periods in which a US court makes a legal ruling) are driven exclusively by those legal rulings, or other idiosyncratic default probability shocks (ε_t). Under this assumption, we could directly estimate equation (2) using OLS on these ruling days. We present these results, and the details of the event studies, in the appendix, section D.

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 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 (because of the effects of the legal rulings, which we have assumed are ε_t shocks). 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.

We 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 :

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

[21] Rigobon and Sack (2004) discusses these biases in the context of this framework.

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[22]

$$\Delta\Omega = \lambda \begin{bmatrix} \alpha^2 & \alpha \\ \alpha & 1 \end{bmatrix} \tag{3}$$

where $\lambda = \left(\frac{\sigma^2_{\varepsilon,E} - \sigma^2_{\varepsilon,N}}{(1-\alpha\gamma)^2} \right)$.

There are several potential ways to estimate α based on $\Delta\Omega$. Our preferred estimator, which we call the CDS-IV estimator, is defined as

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

As shown in Rigobon and Sack (2004), this estimator can be implemented in an instrumental variables framework.[23]

The CDS-IV instrument is relevant under the assumption that $\lambda > 0$. We can reject the hypothesis that $\lambda = 0$ using a test for equality of variances, which is described in the appendix, section E. The relevance of the CDS-IV instrument is also suggested by the weak-identification F-test of Stock and Yogo (2005), also reported in the appendix, section E. In table 3, we present the results of our CDS-IV estimation. The standard errors and confidence intervals use the bootstrap procedure described in the appendix, section C.1.

[Insert table 3]

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, as well as a depreciation in the dolar blue rate. An increase in the risk-neutral default probability from 40% to 100%, which is roughly what Argentina experienced, would cause around a 28% fall in the value-weighted index, by our estimates. Our results are consistent with the hypothesis that Argentina's default caused significant harm to the value of Argentine firms.

In table 2, we present estimates for the magnitudes of the losses caused by default. The columns labeled "Estimate (60%)" and "Estimate (100%)" report the estimated losses caused by increasing the probability

[22] Algebraic details can be found in Rigobon (2003).

[23] Alternate estimators, and the issues with them, are discussed in appendix section F.

of default by 60% and 100%, respectively. The 60% is relevant for Argentina because this is approximately the amount the five-year risk neutral default probability increased during the period of our study. The 100% column is relevant because it is closer to the concept of the cost of default in the literature.

In the first three lines of the table, we present estimates for the firms that have ADRs. These estimates are calculated by multiplying the sum of the market values of all the firms in 2011 by the point estimate for the value index in table 3, converted from log to arithmetic returns and adjusting for the treasuries included in the index. The losses for firms with ADRs are comparable to the full face value of the claims of all holdout bonds, and an order of magnitude larger than the amount NML was demanding. In the fourth and fifth rows, we also include the losses experienced by locally traded firms. We assume that these firms experience losses at the same rate as the firms with ADRs. When considering the losses on these two broader classes of firms, the direct reduction in the market value of these firms as a result of default significantly exceeds the face value of all holdout claims.

In the second set of results, we now attempt to extrapolate what these reductions in firm value would imply if they were experienced by the broader economy. According to the World Bank, the stock market capitalization to GDP ratio of Argentina was 13.8% at the end of 2010. The price-to-annual-earnings ratio of firms with ADRs at the end of 2010 was 14.87. We calculate the "Aggregate Loss" in billions of dollars by dividing the loss in "All equities" by the 13.8% stock market/GDP ratio, and multiplying it by the P/E ratio. This calculation extrapolates the losses experienced by firms with ADRs to the net present value of Argentina's future output, by scaling those losses by the ratio of firm earnings to GDP. This delivers an estimate of the reduction in the market value of future Argentine output of $1.8 trillion for a 60% increase in the default probability. This is equivalent to 324% of Argentina's 2011 GDP. Using the present value concept described below in section 5, we calculate the permanent reduction in the level of output necessary to generate this large of a loss. Our estimates correspond to a permanent 9.4% reduction in output, for a change in the default probability from zero to 100%. In the next section, we will present other methods of extrapolating from stock market losses to output losses, and find similar results.

[Insert table 2 here]

5 Output Costs of Default

In this section, we will attempt to translate the stock returns we observe 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 28%. Second, in quantitative 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.

Ideally, we would observe, at a daily frequency, estimates of future Argentine real GDP. If that data existed[24], we could use it with our heteroskedasticity-based identification strategy. Because it does not, we will use a tracking portfolio instead. The tracking portfolio[25] 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 stock market decline into an output loss.

We define real 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 cost and the expected length of time that the default cost will last. Second, defining real GDP news in this way allows us to clearly relate stock returns to real GDP news. As we will explain below, by assuming that real GDP and real firm dividends are cointegrated, we can show

[24] In fact, Argentina has issued real GDP warrants. However, they are very illiquid, have option-like features, and are affected by the measurement of Argentine inflation. We did not find it feasible to use them to construct a high-frequency GDP forecast series.

[25] 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).

that real GDP news is related to the cashflow news described by Campbell (1991). We choose the quarterly discount factor, $\rho = 0.9956$, based on the dividend-price ratio, following Campbell (1991).[26]

We cannot observe real GDP news directly, so we consider two different proxies. First, we will study survey expectations of real GDP growth. Second, we construct a proxy for GDP news using a VAR. We estimate the VAR on a set of variables, including real GDP growth, and then transform the coefficients of this VAR into an estimate of the tracking portfolio coefficients. 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 requires additional assumptions.

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 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 survey-based GDP news proxy variable, we use the year-over-year change in the April and October long-term forecasts.[27] 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 the appendix, figure A3.

We construct our VAR-based measure in three steps. First, we estimate a cointegrating relationship between Argentine real GDP and the real dividends of our value-weighted index.[28] 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.

Our results are driven primarily by our estimate of the cointegrating relationship between dividends and

[26]The discount factor based on the price-dividend ratio may be substantially higher than the subjective discount factor of the Argentine government or households.

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

[28]We construct real GDP by combining official data on nominal GDP with inflation measures from Cavallo (2013). See appendix G for details.

GDP.[29] 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 is stationary, which would be equivalent to assuming $\phi = 1$. However, leverage would cause a firm's dividends and share-repurchases, in a net-present-value sense, to be more volatile than GDP.[30] 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 (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.

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 the log price-dividend ratio of our value index, z_t. We describe this VAR in the appendix, section G. The details of this VAR do not alter our estimation results very much; because of the cointegration assumption and the fact that ρ is close to 1, the relationship between real GDP news and real dividend (cashflow) news that we estimate is determined almost entirely by the coefficient ϕ.

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 that this assumption is reasonable, and discuss alternative possibilities, in section 7. Assuming that the returns we estimate in the high-frequency data are cashflow news, we can estimate the effect of the legal rulings on real GDP news using the relationship between real GDP news and cashflow news implied by the VAR.

[29] 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, and follows these authors in assuming that the cointegrating relationship does not necessarily have a unit coefficient.

[30] In our short data sample, we do not observe significant share repurchase or issuance activity. Presumably, if GDP growth was high or low for a long time, firms would eventually adjust their capital structure.

We present two sets of results for the VAR/Cointegration approach. In one, which we label the "DOLS" estimates, we simply rescale our estimates for stock returns by the coefficient ϕ. In the second approach, the "VAR" approach, we use the full VAR to estimate the tracking portfolio.[31]

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

[Insert table 4 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 C.2), 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.

We next present results that apply the CDS-IV estimator to these tracking portfolios, in the table below. 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 (2015), who also use the Consensus Economics forecast data, but focus on developed countries, and find that survey forecasts incorporate information gradually.

[Insert table 5 here]

Our point estimates imply that, a sovereign default (going from 0% to 100%) causes a 10.9% (DOLS/VAR) or 5.9% (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

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

[31] In the appendix, section J, we also present the results of a simple deleveraging, in which we rescale each firms' stock returns by its leverage before forming our index. The results of this deleveraging are similar to our VAR/Cointegration results.

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.[32]

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.[33]

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

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

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

Our output costs are fairly similar to those estimated looking at cross-country panel data. Gornemann (2014) finds that real GDP is roughly six percentage points below trend a decade after default. Uribe and Schmitt-Grohe (2015), based on Borensztein and Panizza (2009), find that around a default the growth rate falls and gradually returns to its pre-trend trajectory. However, output permanently remains 5.5% below its pre-default trajectory. Both these estimates are nearly identical to our survey-based estimates. Another way to interpret the size of these default cost estimates is to compare them to the historical shocks experienced by Argentina and other emerging markets. Aguiar and Gopinath (2007) argue that trend shocks explain a large amount of the output volatility in emerging markets. In Aguiar and Gopinath (2006), using estimates from Aguiar and Gopinath (2007), those authors calibrate the quarterly volatility of trend shocks to be

[32] If we instead used $\rho = 0.95$, which would correspond to an extremely high dividend yield, the real GDP news in this model would still be far below our estimates (see the appendix, table A12).

[33] 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.

3% with a quarterly persistence of 0.17. The annual standard deviation of real GDP growth news implied by this calibration is 5.1%. Our estimates for the output cost of default are between 1.1 (survey) and 2.1 (VAR/DOLS) annual standard deviations.

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, 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. 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. 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 following a sovereign default occurs because of the default's effect on bank balance sheets. 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 of sovereign default. Cole and Kehoe (1998) argue that "general reputation," rather than a specific reputation for repayment, is lost by defaulting on sovereign debt. This theory would lead us to expect increases in the risk of sovereign default to cause foreign-owned firms to underperform, due to a higher risk that Argentina will act disreputably in other arenas, such as investment protection.

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, such as Bernanke and Kuttner (2005) and Gorodnichenko and Weber (2013). We test whether certain types of firms experience returns around our legal rulings that are larger or smaller than would be expected, given those firms' betas to the Argentine equity markets and exchange rate. In effect, we are testing whether the ensemble of shocks that generate returns outside of the event windows have a similar cross-sectional pattern of returns to the default probability shock.

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 N. The modified version of the those equations has the return of the Argentine market index and the exchange rate on the right-hand side. We show that the heteroskedasticity-based estimation procedure identifies the coefficient $(\alpha_i - \beta_i^T \alpha_m)$, where α_i is the response of this portfolio to the default shock, α_m is the response of the market index and exchange rate to the default shock, and β_i are the coefficients of a regression of the returns of portfolio i on the market index and ADR blue rate. This coefficient 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 and exchange rate's exposure to the default shock, and the sensitivity of the portfolio to the Argentine market and exchange rate. In this sense, our approach generalizes the CAPM-inspired analysis of Bernanke and Kuttner (2005).

To increase our sample size of firms, we use local Argentine stock returns, rather than ADRs. The use of the local stocks and CDS data requires that both the New York and Buenos Aires markets be open, which reduces the size of our sample. However, all but one of the legal rulings remain in our sample.

We study which characteristics of firms are associated with over- or under-performance in response to default shocks. We form zero-cost, long-short portfolios[34] based on the export intensity of their primary industry (for non-financial firms), the import intensity of individual non-financial firms in 2007 and 2008 using data from Gopinath and Neiman (2014), whether they are a listed subsidiary of a foreign firm, firm size, and whether they have an associated ADR. For the exporter, importer, and firm size portfolios, we group firms based on whether they are above or below the median value in our sample. An import-intensive firm is not the opposite of an export-intensive one; some firms are classified as neither import nor export intensive, whereas others are both import and export intensive.

In these portfolios, we equally weight firms within the "long" and "short" groups. For example, we classify 12 of our 26 non-financial firms[35] as high export intensity, and 14 of 26 as low export intensity. We

[34]Because we form long-short portfolios, the nominal exchange rate does not directly impact the portfolio's return, except to the extent that it differentially affects the firms.

[35]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.

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. The local equity index that we use as a control is an equal-weighted index of all of the local stocks in our data sample.

The over- or under-performance of the portfolios is not an ideal test of the theories. For example, if we do not observe that importing firms under-perform, it may be because the firms we observe are not the ones who would have difficulties, or because our import-intensive and non-import-intensive firms also differ on some other characteristic that predicts over- or under-performance (essentially an omitted variables problem). 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 importing and some other firm characteristic.

[Insert table 6 here]

In table 6, we find that firms whose primary industry is export-intensive under-perform given their exposure to the equal-weighted index and exchange rates, and those assets' response to the default probability shock, while the long-short importer portfolio over-performs by a statistically insignificant amount.[36] We find that foreign subsidiaries, of which there are nine, underperform relative to non-financial firms that are not foreign subsidiaries. This result is consistent with the general reputation theory of Cole and Kehoe (1998), which implies that default makes policy changes more likely and that foreign investors become reluctant to invest. We also find that larger firms (market capitalization in 2011 above median) significantly underperform relative to smaller firms; however, this may reflect the relative illiquidity of smaller firms' stocks, rather than a difference in real outcomes. We do not find that firms with an ADR substantially under- or out-perform firms without ADRs.

We estimate economically large, but not statistically significant, underperformance for banks. 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). However, we find that a "de-levered" portfolio of bank stocks (see appendix J) outperforms a de-levered portfolio of non-financial firms, which suggests that the assets held by these Argentine banks are not substantially impaired by the sovereign default. This result is not necessarily surprising– Argentina did not default on its local law, locally owned debt.

[36]We display these results graphically in appendix figure A5.

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.

7 Identification and Interpretation

In this section, we discuss challenges to our identification assumption and alternative interpretations of our stock market results, as well as the external validity of our results. We begin by discussing the exogeneity of our shocks, and then discuss channels through which the rulings might have affected firms, other than by changing the default probability.

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 litigating the holdouts in full was small relative to the Argentine economy (more on this below), 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.[37]

It is important that our 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 (Russo and Porzecanski (2014)). 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.

We also argue that the rulings did not directly impact these firms, except by changing the probability of default. One potential issue is that the legal rulings might have changed the probability or size of a settlement with the holdouts, and this could affect the firms. 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 (Gelpern (2014a)). Presumably, if Argentina paid NML and its co-litigants in full, the other holdout creditors would

[37] 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.

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, and 45% of foreign currency reserves.[38]

This issue is complicated, however, by the presence of a "Rights Upon Future Offers" (RUFO) clause in the restructured bond contracts. 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? 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 (Cotterill (2013)). 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.[39] 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 if the exchange bondholders could be persuaded that this was preferable to having their coupon payments blocked. Of course, this possibility assumes Argentina would have paid any amount to the holdouts, a questionable proposition given the domestic politics surrounding the holdouts (Gelpern (2014b)). Notably, when the RUFO clause expired at the end 2014, no progress in settlement talks between the holdouts and Argentina was reported. Nevertheless, suppose the RUFO clause was binding, and settlement with the holdouts was not possible. In this case, the legal rulings caused Argentina to default, and our identification assumption holds.[40]

In table 2, we show that the losses to firms were of similar magnitude to the amount owed to all of the holdouts, not just the litigants. To believe that the prospect of a settlement was driving the losses we observe, one would need to believe that all of holdouts would be paid in full and that the entirety of the burden of

[38] The CIA World Factbook reports Argentina's 2013 GDP as $484.6 billion, and its exchange and gold reserves at $33.65 billion as of December 31, 2013. However, the GDP calculation uses the official exchange rate, which may overstate the size of Argentina's economy.

[39] See the comment's from Barclay's reported in Cotterill (2013).

[40] If the RUFO clause was binding, and nevertheless a settlement was possible, one would have expected rulings in favor of NML to *raise* the value of the restructured bonds. In fact, we observe that restructured bond prices decline along with the stock returns. In appendix table A13, 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.

repayment would fall on firms with ADRs, even though those firms are small part of the economy.

For our identification strategy, we would be concerned about any effect the ruling has on the value of Argentine firms that does not operate through its impact on the probability of default. There is no direct effect on Argentine firms because they are legally independent from the Argentine government, and their assets cannot be attached by the holdouts.[41] The ruling affects them only to the extent that it changes the behavior of the Argentine government or other actors. One potential channel not operating through the probability of default is the possibility that the legal rulings changed the law regarding sovereign debt generally. We muster evidence against this in the appendix.[42] Another possible channel that would violate our exclusion restriction, which we cannot test, is that the rulings act as a sort of coordination device. For instance, the legal rulings could have provoked the government of Argentina into a sequence of actions unrelated to sovereign default, or changed the probability that the Peronist government of Argentina stayed in power, for reasons unrelated to the default. It is important to remember, however, that our costs of default are inclusive of the effects of expected government policy changes and political fortunes, if these changes occur because of the default. Our exclusion restriction is only violated if these changes are unrelated to sovereign default.

7.1 Interpretation of the Stock Returns

We argue that our stock returns are likely to measure cash flow news, and not news about future returns, 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 spreads and stock indices (see the appendix, section H). Second, we control for the legal rulings' impact on a variety of proxies for the price of risk. Consistent with the previous point, incorporating these controls makes little difference for our estimates. 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, it is possible

[41] 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.

[42] In the appendix, section H, we show that the stock markets of Brazil and Mexico and the risk-neutral default probabilities of more than 30 countries 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.

that these legal rulings create a shortage of Argentina-specific expert capital, along the lines of Gabaix and Maggiori (2015).[43] We muster evidence against this by showing that Argentine-listed multinationals, such as Tenaris and Petrobras Brazil are unaffected by the default shocks (see table A9 in the appendix). This expert-specific capital would therefore have to defined more narrowly than firms trading on the Buenos Aires Stock Exchange.

Alternatively, the returns could be caused by an increase in the exposure of the dividends of the ADRs to priced risk factors (an increase in "beta", rather than a change in the mean value of the dividends). This would explain a decline in the value of the firms, as valued by the market. If we extrapolate and assume that Argentine GDP also became more exposed to these priced risk factors, this would imply that the market value of the future output of Argentina also declined.[44]

7.1.1 Transfer and Convertibility Risk

Even if we assume that the negative returns we observe represent cashflow news, there is still the question of whether news about these ADRs is representative of Argentina's economy. As mentioned previously, the earnings of firms with ADRs are a small fraction of the Argentine economy. Moreover, it is possible that, conditional on default, it would become difficult for firms to make payments on their ADR dividends. In other words, default might cause the government to adjust its capital controls.

In this case, we could expect to see a significant difference between firms' local (peso) stock performance and their ADR performance. However, to compare the performance of local stocks and ADRs, we need a measure of the exchange rate that would not be affected by these capital controls. Unfortunately, all of our market exchange rate measures (the ADR blue, the blue chip swap, and the dolar blue) would likely be affected by changes in the capital control regime. We do not find any evidence that there is a different effect across these three exchange rates. This suggests that, if changes in capital controls conditional on default are anticipated, these changes will be equally applied to bonds, stocks, and other means by which Argentines can acquire dollars.

[43] 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.

[44] The implications of this story for Argentine welfare are less clear.

7.2 External Validity

Our estimates of the cost of default include the consequences of whatever policies the government is expected to employ, conditional on default. These costs also include the effects of firms, households, and other agents changing their behavior as a result of the default. For the government, these policies 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. The external validity of our results depends on the extent to which other defaulting countries would behave similarly to Argentina in the aftermath of a default.

One potential cost of default is exclusion from markets. 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, the government of Argentina remained unable to issue international law bonds. 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 (Hornbeck (2013)). However, prior to these legal rulings, the government of Argentina was able to issue local-law, dollar-denominated bonds, and some of those bonds were purchased by foreigners. These bonds were affected by the legal rulings, and it may have become more difficult for Argentina to borrow as a result of a rulings.

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. 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. We extrapolate this result to conclude that the default caused a persistent decline in expected future output, and we provide suggestive evidence that exporters and foreign-owned firms are particularly hurt by sovereign default.

References

Acharya, Viral, Itamar Drechsler, and Philipp Schnabl, "A Pyrrhic Victory? Bank Bailouts and Sovereign Credit Risk," *The Journal of Finance*, 2014, *69* (6), 2689–2739.

Acharya, Viral V, Tim Eisert, Christian Eufinger, and Christian W Hirsch, "Real Effects of the Sovereign Debt Crisis in Europe: Evidence from Syndicated Loans," 2014.

Aguiar, Mark and Gita Gopinath, "Defaultable debt, Interest Rates and the Current Account," *Journal of International Economics*, 2006, *69* (1), 64–83.

— and — , "Emerging Market Business Cycles: The Cycle Is the Trend," *Journal of Political Economy*, 2007, *115* (1), 69–102.

Arellano, Cristina, "Default Risk and Income Fluctuations in Emerging Economies," *American Economic Review*, 2008, *98* (3), 690–712.

Arteta, Carlos and Galina Hale, "Sovereign Debt Crises and Credit to the Private Sector," *Journal of International Economics*, 2008, *74* (1), 53–69.

Auguste, Sebastian, Kathryn ME Dominguez, Herman Kamil, and Linda L Tesar, "Cross-Border Trading as a Mechanism for Implicit Capital Flight: ADRs and the Argentine Crisis," *Journal of Monetary Economics*, 2006, *53* (7), 1259–1295.

Bansal, Ravi, Robert F Dittmar, and Christian T Lundblad, "Consumption, Dividends, and the Cross Section of Equity Returns," *The Journal of Finance*, 2005, *60* (4), 1639–1672.

Bernanke, Ben S and Kenneth N Kuttner, "What Explains the Stock Market's Reaction to Federal Reserve Policy?," *The Journal of Finance*, 2005, *60* (3), 1221–1257.

Bocola, Luigi, "The Pass-Through of Sovereign Risk," *Manuscript, University of Pennsylvania*, 2013.

Bolton, Patrick and Olivier Jeanne, "Sovereign Default Risk and Bank Fragility in Financially Integrated Economies," *IMF Economic Review*, 2011, *59* (2), 162–194.

Borensztein, Eduardo and Ugo Panizza, "The Costs of Sovereign Default," *IMF Staff Papers*, 2009, *56* (683-741).

Breeden, Douglas T., Michael R. Gibbons, and Robert H. Litzenberger, "Empirical Tests of the Consumption-Oriented CAPM," *The Journal of Finance*, 1989, *44* (2), 231–262.

Brown, Morton B and Alan B Forsythe, "Robust tests for the equality of variances," *Journal of the American Statistical Association*, 1974, *69* (346), 364–367.

Bulow, Jeremy and Kenneth Rogoff, "A Constant Recontracting Model of Sovereign Debt," *The Journal of Political Economy*, 1989, *97.*

— and — , "Sovereign Debt: Is to Forgive to Forget?," *The American Economic Review*, 1989, pp. 43–50.

Campbell, John Y., "A Variance Decomposition for Stock Returns," *Economic Journal*, 1991, *101* (405), 157–79.

— , Andrew W. Lo, and Craig MacKinlay, *The Econometrics of Financial Markets*, Princeton University press, 1997.

Cavallo, Alberto, "Online and Official Price Indexes: Measuring Argentina's Inflation," *Journal of Monetary Economics*, 2013, *60* (2), 152–165.

— , "Scraped Data and Sticky Prices," *NBER Working Paper*, August 2015.

— and Brent Neiman, "Real Exchange Rate Behavior: Evidence from Online Retailers in Eight Countries," *Working Paper*, 2015.

Cerra, Valerie and Sweta Chaman Saxena, "Growth Dynamics: The Myth of Economic Recovery," *American Economic Review*, 2008, *98* (1), 439–57.

Chodos, Daniel and Igor Arsenin, "Argentina: NDFs are a good hedge for the Blue Chip swap (only) in "blow-up" scenarios," *Credit Suisse Fixed Income Research*, April 2012.

Coibion, Olivier and Yuriy Gorodnichenko, "Information Rigidity and the Expectations Formation Process: A Simple Framework and New Facts," *American Economic Review*, 2015, *105* (8), 2644–2678.

Cole, Harold L. and Patrick J. Kehoe, "Models of Sovereign Debt: Partial versus General Reputations," *International Economic Review*, 1998, *39* (1), 55–70.

Cotterill, Joseph, "Raising the RUFO in Argentine bonds," 2013. `http://ftalphaville.ft.com/2013/03/06/1411442/raising-the-rufo-in-argentine-bonds/` [Accessed: 10/22/2015].

Daseking, Christina, Atish R Ghosh, Timothy D Lane, and Alun H Thomas, *Lessons from the Crisis in Argentina*, International monetary fund, 2005.

Dornbusch, Rudiger and Sebastian Edwards, "The Macroeconomics of Populism," in "The Macroeconomics of Populism in Latin America," University of Chicago Press, 1991, pp. 7–13.

Eaton, Jonathan and Mark Gersovitz, "Debt with potential repudiation: Theoretical and empirical analysis," *The Review of Economic Studies*, 1981, pp. 289–309.

EconomistMagazine, "Relaxation therapy," 2014. http://www.economist.com/blogs/americasview/2014/01/currency-controls-argentina [Accessed: 10/22/2015].

Foley-Fisher, Nathan and Bernardo Guimaraes, "US real interest rates and default risk in emerging economies," *Journal of Money, Credit and Banking*, 2013, *45* (5), 967–975.

Fuchs-Schundeln, Nicola and Tarek A. Hassan, "Natural Experiments in Macroeconomics," *Available at SSRN 2583876*, 2015.

Gabaix, Xavier and Matteo Maggiori, "International Liquidity and Exchange Rate Dynamics," *Quarterly Journal of Economics*, 2015, *130* (3), 1369–1420.

Gelos, Gaston, Ratna Sahay, and Guido Sandleris, "Sovereign Borrowing by Developing Countries: What Determines Market Access?," *Journal of International Economics*, 2011, *83* (2), 243–254.

Gelpern, Anna, "After Argentina," Credit Slips Blog June 2014.

__ , "Argentina: The RUFO Crazy," Credit Slips Blog July 2014.

Gennaioli, Nicola, Alberto Martin, and Stefano Rossi, "Banks, Government Bonds, and Default: What do the Data Say?," 2013.

__ , __ , and __ , "Sovereign Default, Domestic Banks, and Financial Institutions," *The Journal of Finance*, 2014, *69* (2), 819–866.

Gopinath, Gita and Brent Neiman, "Trade Adjustment and Productivity in Large Crises," *American Economic Review*, 2014, *104* (3), 793–831.

Gornemann, Nils, "Sovereign Default, Private Investment, and Economic Growth," 2014.

Gorodnichenko, Yuriy and Michael Weber, "Are Sticky Prices Costly? Evidence from the Stock Market," 2013.

Gulati, Mitu and Kenneth N. Klee, "Sovereign Piracy," *The Business Lawyer*, 2001, pp. 635–651.

Gürkaynak, Refet S, Brian Sack, and Jonathan H Wright, "The US Treasury Yield Curve: 1961 to the Present," *Journal of Monetary Economics*, 2007, *54* (8), 2291–2304.

Hamilton, James Douglas, *Time Series Analysis*, Vol. 2, Princeton University Press, 1994.

Hansen, Lars Peter, John C Heaton, and Nan Li, "Consumption Strikes Back? Measuring Long-run Risk," *Journal of Political Economy*, 2008, *116* (2), 260–302.

Hornbeck, J.F., "Argentina's Defaulted Sovereign Debt: Dealing with the 'Holdouts'," *Congressional Research Service*, 2013.

Horowitz, Joel L, "The Bootstrap," *Handbook of Econometrics*, 2001, *5*, 3159–3228.

Howe, Amy, "Live blog of orders and opinions | June 16, 2014," 2014. http://live.scotusblog.com/Event/Live_blog_of_orders_and_opinions__June_16_2014?Page=0 [Accessed: 10/22/2015].

Kuttner, Kenneth N, "Monetary Policy Surprises and Interest Rates: Evidence from the Fed Funds Futures Market," *Journal of Monetary Economics*, 2001, *47* (3), 523–544.

Lamont, Owen A, "Economic Tracking Portfolios," *Journal of Econometrics*, 2001, *105* (1), 161–184.

Levene, Howard, "Robust tests for equality of variances," *Contributions to probability and statistics: Essays in honor of Harold Hotelling*, 1960, *2*, 278–292.

Longstaff, Francis A, Jun Pan, Lasse Heje Pedersen, and Kenneth J Singleton, "How Sovereign Is Sovereign Credit Risk?," *American Economic Journal: Macroeconomics*, 2011, *3* (2), 75–103.

Markit, "Markit CDS Sensitivities Methodology," January 2012.

Mendoza, Enrique G and Vivian Z Yue, "A General Equilibrium Model of Sovereign Default and Business Cycles," *Quarterly Journal of Economics*, 2012, *127*, 889–946.

Nakamura, Emi and Jón Steinsson, "High frequency identification of monetary non-neutrality," Technical Report, National Bureau of Economic Research 2013.

Newey, Whitney K and Kenneth D West, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica*, 1987, *55* (3), 703–708.

O'Brien, Matt, "Everything you need to know about Argentina's weird default," 2014. http://www.washingtonpost.com/news/wonkblog/wp/2014/08/03/everything-you-need-to-know-about-argentinas-weird-default/ [Accessed: 10/22/2015].

Pan, Jun and Kenneth J Singleton, "Default and Recovery Implicit in the Term Structure of Sovereign CDS Spreads," *The Journal of Finance*, 2008, *63* (5), 2345–2384.

Panizza, Ugo, Federico Sturzenegger, and Jeromin Zettelmeyer, "The Economics and Law of Sovereign Debt and Default," *Journal of Economic Literature*, 2009, pp. 651–698.

Pasquariello, Paolo, "The anatomy of financial crises: Evidence from the emerging ADR market," *Journal of International Economics*, 2008, *76* (2), 193–207.

Perez, Diego J, "Sovereign Debt, Domestic Banks and the Provision Of Public Liquidity," *Manuscript, Stanford University*, 2014.

Reinhart, Carmen M and Kenneth Rogoff, *This Time is Different: Eight Centuries of Financial Folly*, Princeton University Press, 2009.

Rigobon, Roberto, "Identification through Heteroskedasticity," *Review of Economics and Statistics*, 2003, *85* (4), 777–792.

— **and Brian Sack**, “The Impact of Monetary Policy on Asset Prices,” *Journal of Monetary Economics*, 2004, *51* (8), 1553–1575.

Rose, Andrew K, “Size Really doesn’t Matter: In Search of a National Scale Effect,” *Journal of the Japanese and International Economies*, 2006, *20* (4), 482–507.

Russo, Camila and Katia Porzecanski, “Argentine Bonds Plunge After U.S. Court Rejects Apeal,” 2014. http://www.bloomberg.com/news/articles/2014-06-16/argentine-bonds-plunge-after-u-s-supreme-court-rejects-appeal [Accessed: 10/22/2015].

Schumacher, Julian, Christoph Trebesch, and Henrik Enderlein, “Sovereign Defaults in Court,” *Available at SSRN 2189997*, 2014.

Stock, James H and Mark W Watson, “A Simple Estimator of Cointegrating Vectors in Higher Order Integrated Systems,” *Econometrica: Journal of the Econometric Society*, 1993, pp. 783–820.

— **and Motohiro Yogo**, “Testing for Weak Instruments in Linear IV Regression,” *Identification and Inference for Econometric Models: Essays in honor of Thomas Rothenberg*, 2005, *1*.

Thomas, Leigh and Sarah Marsh, “Argentina clinches landmark debt repayment deal with Paris Club,” 2014. http://www.reuters.com/article/2014/05/29/us-argentina-debt-parisclub-idUSKBN0E90JI20140529 [Accessed: 10/22/2015].

Uribe, Martin and Stephanie Schmitt-Grohe, *Open Economy Macroeconomics* 2015.

Vassalou, Maria, “News related to future GDP growth as a risk factor in equity returns,” *Journal of financial economics*, 2003, *68* (1), 47–73.

Vuolteenaho, Tuomo, “What Drives Firm-Level Stock Returns?,” *Journal of Finance*, 2002, *57* (1).

White, Richard, “The Pricing and Risk Management of Credit Default Swaps, with a Focus on the ISDA Model,” *OpenGamma Quantitative Research*, 2013, (16).

Yeyati, Eduardo Levy and Ugo Panizza, “The elusive costs of sovereign defaults,” *Journal of Development Economics*, 2011, *94* (1), 95–105.

Tables and Figures

Table 1: Summary Statistics

Window Type	Event	Non-Event
Mean ΔD_t (%)	-0.09	-0.01
SD ΔD_t (%)	5.06	1.79
Mean Equity Log Return (%)	0.26	0.04
Equity Log Return SD (%)	3.64	2.49
$\text{Cov}(\Delta D_t, r_t)$	-13.86	-1.98
Number of two-day windows	15	386

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: Estimates of the Cost of Default

Measure	Estimate (60%)	Estimate (100%)	Unit
ADRs (ex. YPF)	-4.77	-7.95	\$B
YPF	-6.59	-10.98	\$B
ADRs	-11.36	-18.93	\$B
All equities in dataset	-15.65	-26.08	\$B
All equities	-16.75	-27.92	\$B
Aggregate Loss	-1804.95	-3008.25	\$B
Aggregate Loss	-322.31	-537.19	% GDP (2011)
Aggregate Loss	-5.64	-9.39	PV ΔY

Notes: The first line, "ADRs (ex.YPF)" reports the imputed loss of market value all firms included in our sample of ADRs experienced, excluding YPF. It is calculated by multiplying the sum of the market values of all the firms in 2011 by point estimate on the Value Index in Table 3. The second row, "YPF", reports the same calculation for YPF. The third row, "ADRs," is the sum of the first two. The fourth row, "All equities in dataset", is the loss by locally traded firms that are included in the analysis of Section 6. The fifth row, "All equities," includes all Argentine firms with equities, even those that do not meet the data quality standards to be included in Section 6. "Aggregate Loss" is calculated as (Stock Market Loss - All) $\cdot$ (P/E Ratio) / (Stock Market Capitalization/GDP), reported as billions of dollars, a % of GDP, and the size of a permanent reduction in output needed to generate a loss of this size.

Table 3: Equity and Exchange Rate Results

	(1) MSCI	(2) Value	(3) Bank	(4) Non-Fin.	(5) YPF	(6) Official	(7) Dolar Blue	(8) ADR Blue	(9) BCS
OLS									
ΔD	-50.44***	-38.27***	-53.89***	-41.32***	-55.13**	4.183	9.689**	20.91***	24.72***
SE	(9.641)	(6.423)	(8.829)	(8.192)	(16.91)	(3.540)	(3.994)	(6.366)	(6.316)
95% CI	[-70.5,-30.6]	[-52.7,-24.7]	[-72.7,-36.1]	[-59.6,-24.3]	[-88.5,-15.8]	[-39.3,9.3]	[2.0,17.3]	[8.9,37.7]	[13.2,40.1]
CDS-IV									
ΔD	-79.44***	-54.89***	-83.16***	-58.63**	-93.69***	-0.716	10.37**	10.71	11.01
SE	(16.20)	(12.00)	(12.91)	(19.76)	(22.09)	(1.430)	(3.325)	(15.95)	(18.02)
95% CI	[-110.3,-41.0]	[-81.6,-32.8]	[-112.2,-61.0]	[-103.4,-9.2]	[-141.1,-39.1]	[-3.2,1.9]	[3.0,17.0]	[-29.4,86.6]	[-33.3,106.1]
Events	15	15	15	15	15	15	14	14	14
Obs.	401	401	401	401	401	401	355	353	356

Notes: This table reports the results for the OLS and CDS-IV estimators of the effect of changes in the risk-neutral default probability (ΔD) on several equity indices and exchanges rates. The equity indices are the MSCI Index, the Value-Weighted index, the Value-Weighted Bank Index, the Value-Weighted Non-Financial Index, and YPF. All indices are composed of ADRs. The index weighting is described in the text. For exchange rates, 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. BCS is the 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 coefficient on ΔD is the effect on the percentage log 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 appendix, section C.2. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Coefficients for Tracking Portfolios

	(1) $N_{g,t}^{DOLS}$	(2) $N_{g,t}^{VAR}$	(3) $N_{g,t}^{Survey}$
Value Index	0.199***	0.209***	0.103***
SE	(0.0377)	(0.0276)	(0.0324)
FX (ADR Blue)	0	0.0113	-0.0287
SE	-	(0.0128)	(0.0650)
Obs.	43	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 in the forecast, from 2003-2012. 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 5: Default and the PV of GDP Growth

	(1) $N_{g,t}^{DOLS}$	(2) $N_{g,t}^{VAR}$	(3) $N_{g,t}^{Survey}$
ΔD	-10.92***	-11.27***	-5.893***
SE	(3.158)	(3.055)	(1.871)
95% CI	[-28.4,-6.0]	[-31.5,-6.4]	[-16.3,-3.0]
Events	15	14	14
Obs.	401	355	355

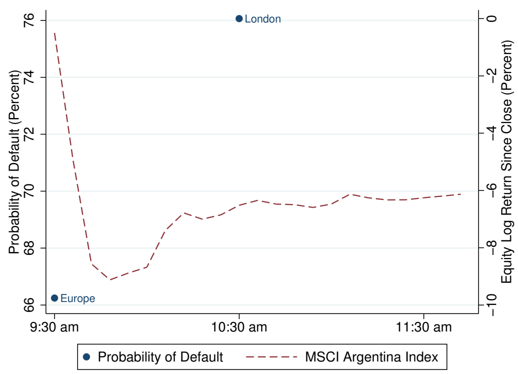
Notes: This table reports the results the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on three measures of the present value of Argentine real GDP growth. The coefficient on ΔD is the effect on the present value of Argentine real GDP growth of an increase in the 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. $N_{g,t}^{Survey}$ is the real GDP news measure derived from survey forecast as described in Section 5. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the appendix, section C.1. 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, and oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Cross-Section: Long-Short Portfolios, CDS-IV

	(1) Foreign	(2) Financial	(3) Exporter	(4) Importer	(5) Size	(6) ADR
ΔD	-27.96***	-34.36	-39.47***	2.722	-33.72**	-12.81
	(9.538)	(16.51)	(9.867)	(8.147)	(10.20)	(12.46)
95% CI	[-51.0,-10.1]	[-78.2,13.7]	[-62.8,-20.6]	[-17.1,18.4]	[-56.9,-9.8]	[-46.9,23.4]
Index β	-0.341	0.0198	-0.682	-0.124	-0.396	0.0679
FX β	-0.0888	-0.0252	-0.351	-0.184	0.0497	0.139
Events	14	14	14	14	14	14
Obs.	353	353	353	353	353	353

Notes: This table reports the results for the "CDS-IV" estimator. The column headings denote the outcome variable, a zero-cost long short portfolio. "Foreign" goes long firms with a foreign parent and short domestically-owned firms. "Financial" goes long banks and short non-financial firms. "Exporter" goes long export-intensive non-financial firms and short non-export-intensive non-financial firms. "Importer" is defined equivalently for importers. "Size" goes long firms with above-median market capitalization in 2011, and short firms with below-median market cap. "ADR" goes long firms with an American Depository Receipt and short firms without one. The coefficient on ΔD is the effect on the percentage log 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, and FX beta is the beta to the ADR blue rate. Standard errors and confidence intervals are computed using the stratified bootstrap procedure described in the appendix, section C.2. 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: Event Data from June 16, 2014, 9:30-11:30am EST

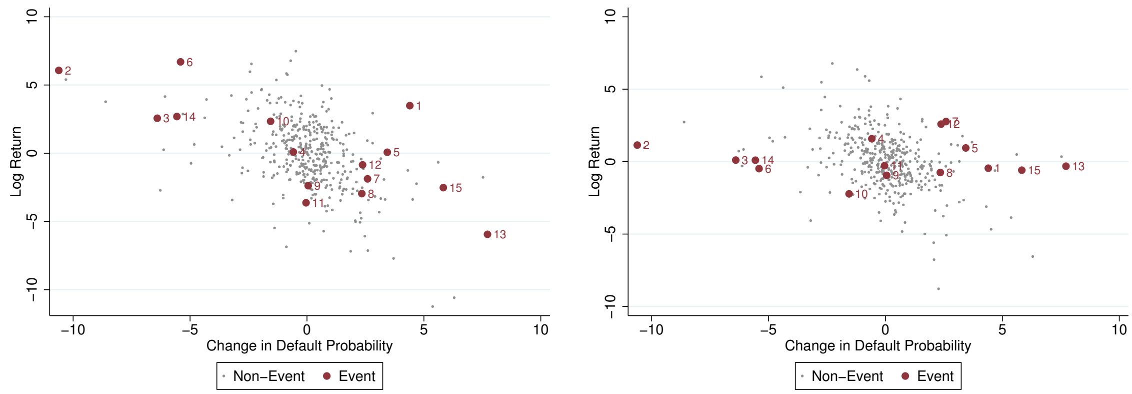


Notes: This figure plots the 5-year risk-neutral probability of default ("Probability of Default (Percent)", left axis), the change in the price of the MSCI Argentina Index against the previous night's close ("Equity Return Since Close (Percent)", right axis). The default probability points are labeled 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 order was released at 9:33 am EST.

Figure 2: Equity Returns and Argentine Default Probability

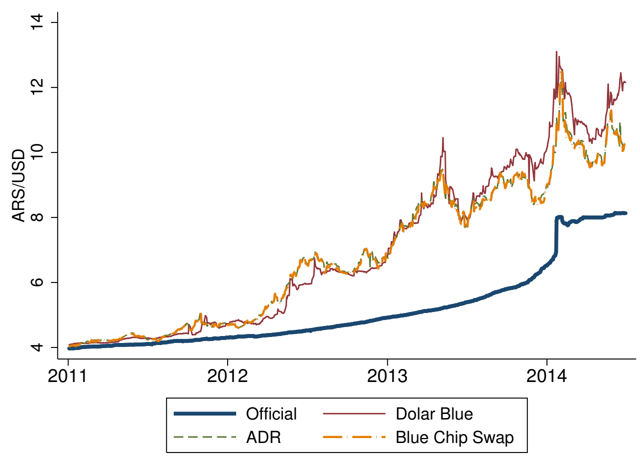
(a) Argentine Equity Index

(b) Mexican Equity Index



Notes: This figure plots the change in change in the risk-neutral probability of default and returns on the Argentine Value-Weighted Index (left panel) and Mexican MSCI index (right panel) on event and non-event two-day windows. Each event and non-event window is a two-day event or non-event as described in the text. The numbers next to each maroon/dark dot references each event window in appendix table A15. The procedure for classifying events and non-events is described in the text.

Figure 3: Exchange Rates



Notes: This figure plots the four versions of the ARS/USD exchange 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.

FOR ONLINE PUBLICATION

A Data Construction Details

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

A.1 Data Sources

In the table below, we list the data sources used in the paper. The data source for the credit default swap prices is Markit, a financial information services company. We use Markit's composite end-of-day spread, which we refer to as the "close." The composite 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.

We have experimented with alternative providers of CDS data, such as Bloomberg, but found significant discrepancies between these data sources and Markit.

Table A1: Data Sources

Data	Data Source
Prices and returns for ADRs	CRSP
Prices and returns for local equities	Bloomberg
VIX	CBOE
S&P	Global Financial Data
EEMA	Datastream
High Yield and IG Bond Index	Datastream
Soybean and Oil Prices	Global Financial Data
Industry Exports	OECD-STAN IO Tables
Firm Imports	Gopinath and Neiman (2014)
Firm Revenue	Compustat Global
Market Capitalization	Bloomberg
Foreign Ownership	Bloomberg
Industry Classification	Fama-French, formatted by Dexin Zhou
Bond Prices for BCS construction	Bloomberg
Dolar Blue Rate	dolarblue.net
Official nominal exchange rate	Datastream
Nominal GDP	Global Financial Data
Inflation (Official)	IFS
Inflation (Unofficial)	Cavallo, PriceStats
CDS spreads/Recovery Rate/Default Probability	Markit
Argentine Sovereign Bond Prices	Bloomberg
Survey Forecasts	Consensus Economics
Treasury Bill Yields	St. Louis Fed. FRED
US Inflation Rate	Global Financial Data

A.2 Firm Classifications

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[45], have returns during at least ten of our event windows, 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.[46]

We classify firms according to their Fama-French industry classifications available on Kenneth French's website.[47] 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

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

[46] See the appendix, section I, for a discussion of these firms.

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

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 A2.

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

To calculate each firm's import intensity, we use firm level data from Gopinath and Neiman (2014). The most recent available import data is for 2007 and 2008 (through October), and we compute the ratio of imports to firm revenue using data from Compustat global. Our measure of import intensity is the average ratio of imports to revenue in 2007 and 2008. The importer threshold is set to the median ratio 0.6%.

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.

[48]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.

Table A2: Firms Included in Analysis

Company	Ticker	Industry	Exports	Imports	Market Cap	Foreign	ADR
Aluar	ALU	Manufacturing	19.4	9.1	9443.0		
Banco Santander Rio	BIO	Banks			12786.1	Y	Y
Boldt	BOL	Telecoms		1.8	1537.5		
Banco Patagonia	BPT	Banks			3488.4	Y	Y*
Banco Macro Bansud	BSU	Banks			9379.2		Y
Carlos Casado	CAO	Real Estate			378.1		Y*
Celulosa	CEL	Chemicals	11.2	1.3	760.3		
Central Puerto Rights	CEP	Utilities	0.1	0.4	1814.4		
Sociedad Comercial Del Plata	COM	Diverse	1.5		212.3		
Capex	CPX	Utilities	0.1	0.9	1087.8		
Cresud	CRE	Non-Durables	14.5	0.0	3495.9		Y
Edenor	EDC	Utilities	0.1	0.1	1894.5		Y
Bbva Banco Frances	FRA	Banks			7723.6	Y	Y
Pampa Energia	FRG	Utilities	0.1	0.1	3417.2		Y
Gp Finance Galicia	GGA	Banks			7125.7		Y
Hipotecario Naci	HPD	Banks			3540.0		Y*
Solvay Indupa	IND	Chemicals	11.2	0.6	1218.0	Y	
IRSA	IRS	Real Estate			3350.5		Y
Juan Minetti	JMI	Manufacturing	3.6	2.1	1633.5	Y	
Ledesma	LED	Non-Durables	14.5	1.0	4004.0		
Metrogas	MET	Utilities	0.1	0.0	677.3		Y*
Mirgor	MIR	Manufacturing	10.1	11.8	512.0		Y*
Molinos Rio De La Plata	MOL	Non-Durables	19.5	0.4	8014.4		
Quickfood	PAY	Non-Durables	19.5	0.5	641.9	Y	
Petrobras Argentina	PER	Energy	25.5	3.8	8228.4	Y	Y
IRSA Propiedades Commerciales	SAM	Real Estate			2960.1		Y
Moli Juan Semino	SEI	Non-Durables	19.5	0.1	325.5		
Siderar	SID	Manufacturing	19.4	0.0	10893.1	Y	
SA San Miguel	SMG	Non-Durables	19.5	0.6	491.1		
Telecom Argentina	TEC	Telecoms	2.7	0.3	21754.8	Y	Y
Transportadora De Gas Del Sur	TGS	Energy	25.5	0.9	2558.3		Y
Transportadores De Gas Del Norte	TN4	Utilities	0.1	3.3	540.4		
Transener	TRA	Utilities	0.1	2.1	640.3		
YPF	YPF	Energy	14.2	2.2	74532.8		Y

Notes: This table lists the 33 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. Exports 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. Imports denotes the ratio (in percentage terms) of imports to firm revenue in 2007 and 2008. The import data is from Gopinath and Neiman (2014). Market Cap. 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. "Y*" indicates that the firm has an OTC-traded ADR and is not included in our sample of ADRs. . To be included in our ADR sample, 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.

A.3 Exchange Rate Construction

The blue chip swap rate is constructed by dividing the peso price of the government bond by the dollar price of the same bond. The mechanics of this transaction are outlined in Panel A of Figure A1. In Panel B of Figure A1, 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 less noise than the ADR blue rate at the two-day frequency.

We calculate the blue chip swap rate using the two of the most liquid available debt instruments, the Bonar X and the Boden 15.[49] 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.[50] 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.[51] 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 Argentine custodian bank, and therefore bears some price risk when acquiring dollars.[52] Despite being domestic law debt instruments, both of these bonds became entangled in the legal proceedings we focus on in this paper.[53]

For the ADR blue rate, we follow the methodology outlined on dolarblue.net.[54] We collect daily open and close price data on the ADR and local equity for eight firms trading from Bloomberg.[55] 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. The average difference between the maximum and minimum firm-level exchange rate is 3.6% of the level the ADR Blue Rate. This difference could reflect differences in the closing times of the NYSE/NASDAQ and Buenos Aires stock exchanges, bid-offer spreads, and other forms of illiquidity. Generally speaking, it is very costly for foreign investors to participate in local Argentine markets, which makes the ADR blue rate arbitrage difficult to execute for them. Together, the ADR Blue Rate and the Blue-Chip Swap rate may be known as the dolar contado con

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

[50] 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.

[51] 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

[52] Chodos and Arsenin (2012).

[53] 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/.

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

[55] 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).

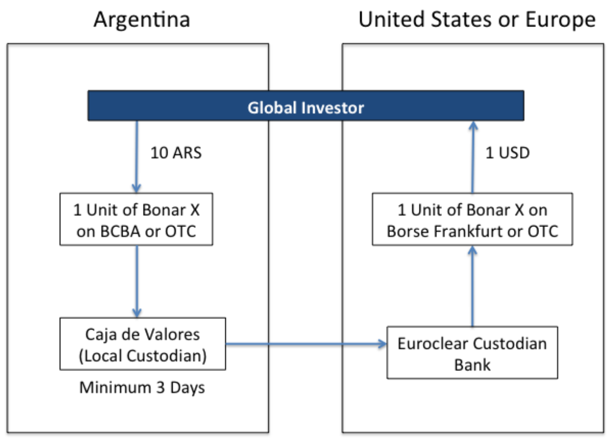
liquidación, dolar fuga, or the dolar gris.[56]

While the CDS-IV results in Table 3 report similar point estimates of the effect of default on the Dolar Blue, ADR Blue and Blue-Chip Swap Rate, the standard errors and confidence interval for the Dolar Blue are significantly tighter. The reason for this is that the behavior of the ADR Blue rate and Blue-Chip swap rate on the day with the largest increase in the probability of default, the Supreme Court ruling day on June 16, 2014, is a significant outlier. On that day, these measures of the exchange rate significantly appreciated. This is in stark contrast to the Dolar Blue rate, which has a significant depreciation. Mechanically, the reason for the appreciation of the ADR Blue and Blue-Chip Swap rates is that the value of domestically traded securities priced in ARS fell significantly more than those traded by foreign investors in dollars. Based on conversations with market participants, we believe that the ruling caused a major disruption in local trading. If we expand the window size around this ruling, it ceases to be an outlier, consistent with the trading disruption hypothesis. However, a major speech was made by the President of Argentina in the evening following the ruling, so we cannot be certain that this pattern is due to a disruption in trading. We also find that, if that event is excluded, the effect on the exchange rate approximately doubles and is relatively precisely estimated. The importance of this outlier (Event 13) can be clearly seen in Figure A2.

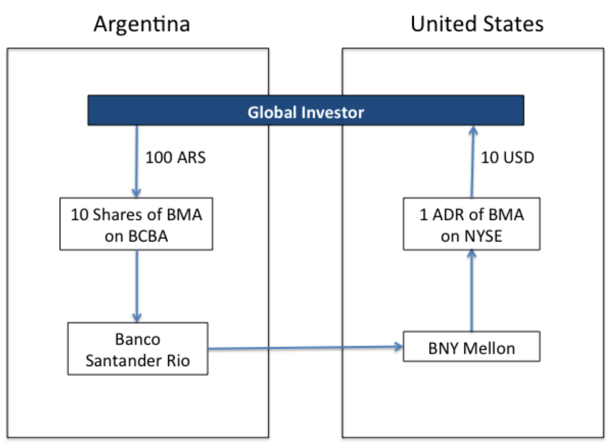
[56] http://www.infodolar.com/cotizacion-dolar-contado-con-liquidacion.aspx

Figure A1: Blue Rate Construction

(a) Blue-Chip Swap



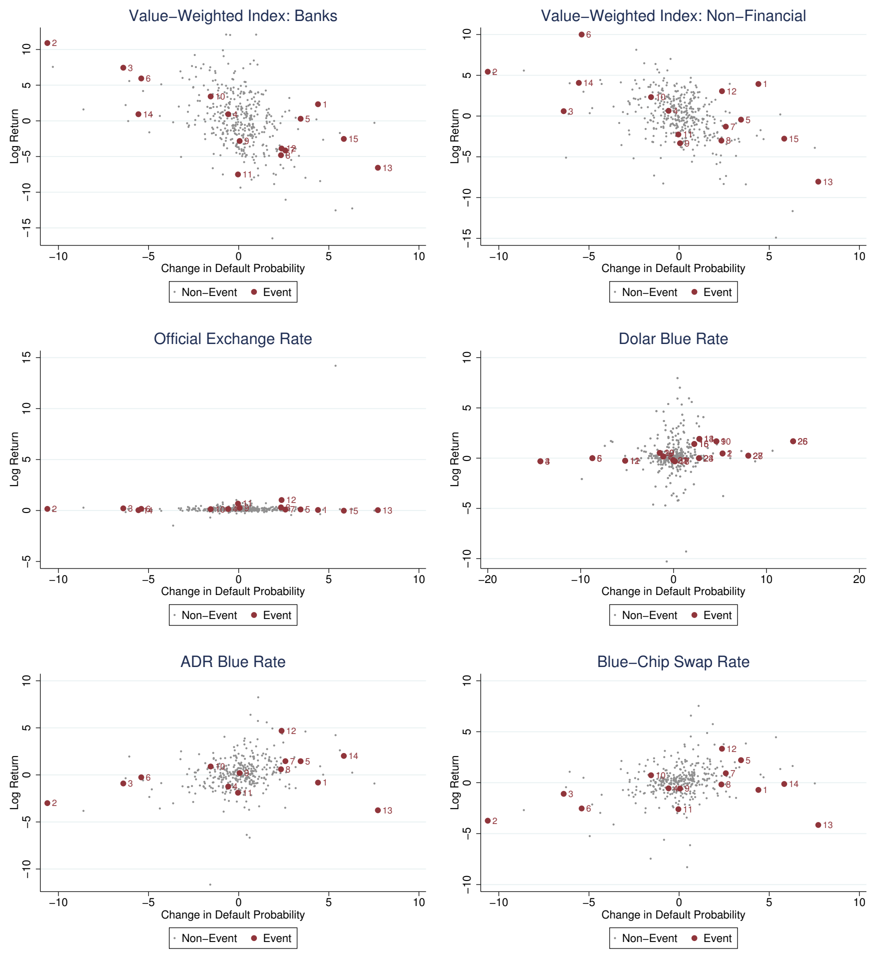
(b) ADR Blue Rate



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.

B Summary Figures

Figure A2: 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. 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 2a. The procedure for classifying events and non-events is described in the text.

C Standard Errors

C.1 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 15 events and 386 non-events.[57] 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 $\hat{\alpha} - \alpha$ 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.[58] In the tables, we report the 95% confidence interval and the heteroskedasticity-robust standard error from our dataset ($\hat{\sigma}$).

C.2 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.

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

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

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.

[57]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.

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

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

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

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.

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

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

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 C.1, 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 C.1. 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.

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.

D Event Studies

D.1 IV-Style Event Study

We present an "IV-style" event study in this section. This study uses the two-day events and non-events described previously. The second stage equation we wish to estimate is equation (2) in the text. 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 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, and X_t are the observable controls. 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 C.1.

Table A3: Equity and Exchange Rate Results, IV-Style Event Study

	(1) MSCI	(2) Value	(3) Bank	(4) Non-Fin.	(5) YPF
ΔD	-75.27***	-52.49***	-79.05***	-56.01**	-88.14***
SE	(14.36)	(10.66)	(11.97)	(17.81)	(19.67)
95% CI	[-107.3,-37.0]	[-78.2,-30.9]	[-109.8,-56.0]	[-105.6,-7.7]	[-136.1,-36.3]
Events	15	15	15	15	15
Obs.	401	401	401	401	401
	(6) Official	(7) Dolar Blue	(8) ADR Blue	(9) BCS	
ΔD	-0.00539	10.17**	12.39	13.95	
SE	(1.236)	(2.665)	(13.21)	(12.56)	
95% CI	[-3.2,2.1]	[3.3,16.4]	[-23.7,76.1]	[-14.7,66.7]	
Events	15	14	14	14	
Obs.	401	355	353	356	

Notes: This table reports the results for the IV-Style Event Study estimator of the effect of changes in the risk-neutral default probability (ΔD) on several equity indices and exchanges rates. The equity indices are the MSCI Index, the Value-Weighted index, the Value-Weighted Bank Index, the Value-Weighted Non-Financial Index, and YPF. All indices are composed of ADRs. The index weighting is described in the text. For exchange rates, 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. BCS is the 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 coefficient on ΔD is the effect on the percentage log 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 appendix, section C.2. All regressions contain controls for VIX, S&P, EEMA, high-yield and investment grade bond indices, oil prices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

D.2 Standard Event Studies

We also present the results of two additional event studies that use the methodology described in Campbell et al. (1997). The first event study uses two-day windows around events.

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 similar 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, 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.

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 A4, we present these two statistics for the value-weighted index.

Table A4: Standard Event Study: Index

Shock Type	# Events	CAR (%)	ΔD (%)	J_1	J_2
Higher Default	7	-11.92	28.40	-2.25**	-2.24**
No News	3	-6.96	-0.51	-2.01**	-2.00**
Lower Default	5	20.77	-29.38	4.64***	4.63***

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 variance-based estimates. In the 7 event days where the default probability significantly increased, the cumulative increase in the default probability was 28.40% and the stock market experienced a cumulative abnormal return of -11.92%. Assuming a linear relationship between default probabilities and equity returns, this implies that a 1% increase in the probability of default causes a 0.42% fall in the stock market. During the 5 days where the default probability significantly declined, the cumulative fall in the default probability was 29.38% with a cumulative abnormal return of 20.77%. This implies a 1% fall in the probability of default causes an 0.71% rise in the stock 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.

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 (16 instead of 15), 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 $\hat{\sigma}_{is}^2$ is the variance of the abnormal returns for that window size. The $J1$ statistic is computed as

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

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 A5 using the two statistics are similar.

Table A5: Heterogenous-Window Event Study: Index

Shock Type	# Events	CAR (%)	ΔD (%)	J_1	J_2
Higher Default	5	-9.86	14.73	-3.50***	-3.10***
No News	6	-0.42	3.91	-0.12	-0.09
Lower Default	5	10.78	-28.40	4.15***	3.50***

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 14.7% and the stock market had a cumulative abnormal return of -9.9%. This estimate implies that a 1% increase in the probability of default causes a 0.67% fall in equity returns. During the 5 days where the default probability significantly declined, the cumulative fall in the default probability was 28.4% with a cumulative abnormal equity return of 10.8%. This implies a 1% fall in the probability of default causes an 0.38% 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 0.48% fall in the equity market.

Compared with these event studies, the IV-style event study described previously 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.

E Tests of Differences in Variances

We conduct two tests to verify that the variance of the default probability changes during our event windows is significantly higher than the variance during non-event windows. Following Foley-Fisher and Guimaraes (2013), we conduct a formal test of hypothesis that $(\Omega_E)_{22} = (\Omega_N)_{22}$ using the method developed by Brown and Forsythe (1974) and Levene (1960). We use the sample associated with our value index (recall that for the exchange rates, the sample is slightly smaller). We strongly reject the hypothesis of equal variances. We also report the first-stage F-statistic of the CDS-IV estimator for the value index, as advocated by Stock and Yogo (2005). For the CDS-IV estimator, this first-stage F-statistic is closely related to the difference in the variance of the default probability during the event and non-event windows.

Table A6: Tests of Differences in Variance

Test	F-statistic	p-value
Levene	53.7***	0.0000
Brown-Forsythe trimmed mean	53.0***	0.0000
Brown-Forsythe median	52.6***	0.0000
First-Stage F-stat	351.2	

Notes: "Test" describe the F-statistic being computed. The Levene test for unequal variances is described in Levene (1960). The Brown-Forsythe tests are described in Brown and Forsythe (1974). These tests all formally test the hypothesis that the variance of the changes in the 5-year cumulative default probability is equal on event days and non-event days. The sample associated with these tests is the sample we used to compute the results for our value index, and involves 15 events and 386 non-events. The first-stage F-stat is the first-stage F-statistic from the two-stage least squares IV implementation of the CDS-IV estimator, on the same sample. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

An alternative to pre-testing for differences in variance is weak-identification-robust inference. A procedure for this type of inference in a similar context is described and implemented by Nakamura and Steinsson (2013). The strength of our rejection of the hypothesis of equal variances suggests that this approach is unnecessary for our application.

F Irrelevant Instruments

We use the CDS-IV estimator because the alternative estimators use an "irrelevant instrument" under the null hypothesis that $\alpha = 0$. As can be seen in equation (3), the coefficient of interest can be identified as the ratio of the first element of matrix to an off-diagonal:

$$\hat{\alpha}_{RIV} = \frac{\Delta\Omega_{1,1}}{\Delta\Omega_{1,2}} = \frac{\text{var}_E(r_t) - \text{var}_N(r_t)}{\text{cov}_E(\Delta D_t, r_t) - \text{cov}_N(\Delta D_t, r_t)}$$

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.[59]

The CDS-IV estimator does not suffer from 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, 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. 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. 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.

G Tracking Portfolios

G.1 General Method

Let $N_{y,t}$ be the news, which we do not observe, about real GDP at time t. 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 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:

$$\tilde{N}_{y,t} = \beta r_t + \nu_t, \tag{A4}$$

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-

[59] 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 $\hat{\alpha}_{RIV}$.

based estimation procedure, as described in the text. 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.

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 (A4). 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.[60] 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.[61]

The formula for ρ is

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

where $\bar{pd}$ is the time-series average log of the (annual) price-dividend ratio for our stock index. In appendix table table A12, 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 survey forecast data for consistency.

G.2 Survey Forecasts

Survey forecasts have advantages and disadvantages, relative to the standard tracking portfolio methodology. If survey expectations are equivalent to rational expectations, then this approach would reduce our estimation error, relative to the standard procedure, 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 (2015), then our estimates based on survey expectations will be systematically biased as well. The survey data ends in 2013.

[60] 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.

[61] Results available upon request.

G.3 VAR and Cointegration

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.[62] 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 of Cavallo (2013, 2015).[63] 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. Results using the official real GDP measure are similar and available upon request.

The market-based exchange rate series we use is the ADR blue rate series, which is available for the entire sample period. The official real exchange rate is defined as $orer_t = o_t - p_t + p_t^*$, where o_t is the log official nominal 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. 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)).

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)):

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

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

[63] We thank Alberto Cavallo for sharing his data.

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:

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

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.[64] Using data from online retailers, Cavallo and Neiman (2015) find much shorter real exchange rate half-lives for Argentina (and other countries) than what the PPP puzzle suggests. Second, the non-deliverable forward (NDF) rates, which are forward contracts on the official nominal exchange rate, track the ADR-based nominal exchange rate.[65] 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. 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,

[64] 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.

[65] Results available upon request.

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 0.9956. As a result, dollar cashflow news and real GDP news are essentially equivalent, up to the scaling factor ϕ. In the appendix, table A12, we demonstrate that our results our robust to alternative values of ρ.

For the results that do not assume $\rho = 1$, 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 coefficients β using the standard OLS formula,

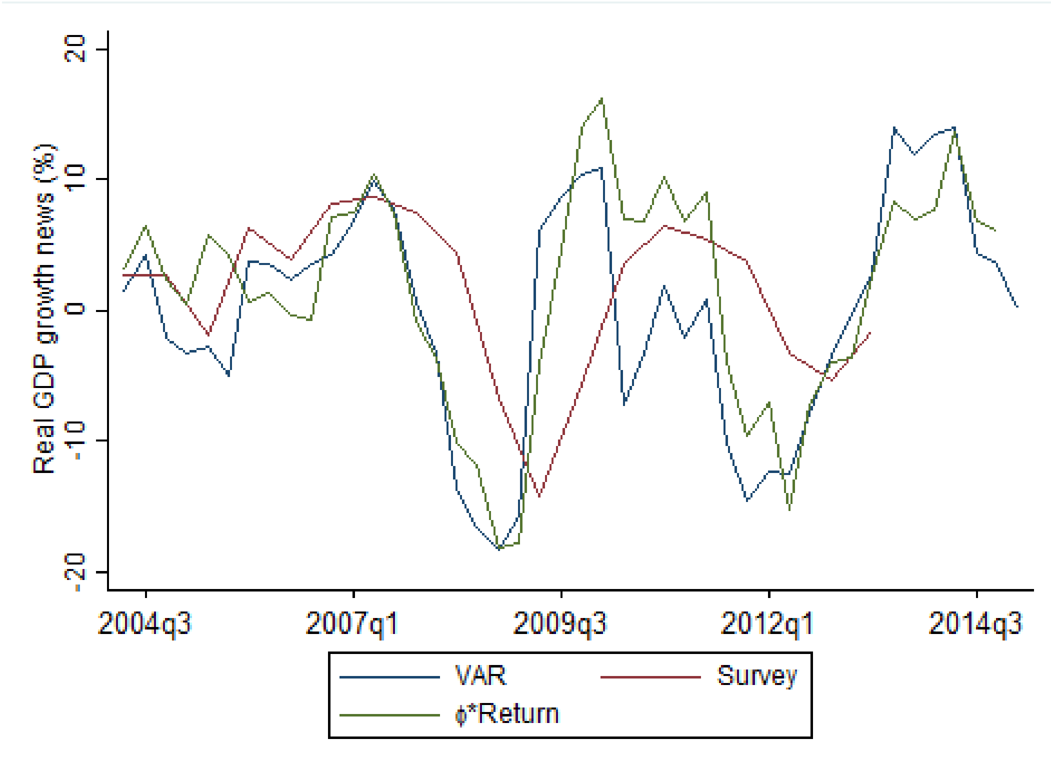
$$\beta = E[r_t r_t^T]^{-1} E[r_t N_{y,t}],$$

where these expectations are a function of the parameters ϕ, A, and Σ (we derive these formulas in a subsection below). 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.

G.4 Time Series of Estimated Real GDP Growth News

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 (2015)), 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.

Figure A3: 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.

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)).

G.5 Formulas for β and the Delta Method

We begin by the defining a matrix S, which depends on the VAR coefficient matrix A,

$$S = \begin{bmatrix} \phi^{-1}(e_1^T - (1-\rho)\phi^{-1}e_2^T - (1-\rho)e_3^T)(I-\rho A)^{-1} \\ e_1^T(I-\rho A)^{-1} \\ e_3^T \\ e_4^T \\ e_5^T \end{bmatrix}$$

and note that $N_{d,t} = e_1^T S \xi_t$ and $N_{y,t} = e_2^T S \xi_t$. It follows that

$$r_t = \begin{bmatrix} N_{d,t} \\ \Delta e_t - E_{t-1}[\Delta e_t] \end{bmatrix} = \begin{bmatrix} e_1^T \\ e_4^T \end{bmatrix} S \xi_t.$$

The "regression" that we compute is

$$\begin{aligned} \beta &= E[r_t r_t^T]^{-1} E[r_t N_{y,t}] \\ &= \left(\begin{bmatrix} e_1^T \\ e_4^T \end{bmatrix} S \Sigma S^T \begin{bmatrix} e_1 & e_4 \end{bmatrix} \right)^{-1} \begin{bmatrix} e_1^T \\ e_4^T \end{bmatrix} S \Sigma S^T e_2, \end{aligned}$$

which is a function of ϕ, ρ, A, Σ. Replacing each of these with their estimated versions results in our estimate of β.

To compute the standard errors for the estimate of β, we use the delta method. That is, we compute the derivative of β with respect to each element of A, evaluate it at our estimate, and then use the VAR estimator's variance-covariance matrix for the elements of A to compute standard errors for β. As mentioned in the text, this procedure does not incorporate the estimation error associated with the coefficients ϕ and ρ. Explicit formulas for these derivatives are available upon request.

H Mexico, Brazil, and Other Countries

In this section, we present the results of OLS and CDS-IV regressions for non-Argentine countries' equity indices and default probabilities. With OLS, we find that the Argentine risk-neutral default probability co-moves with other emerging market equity indices and sovereign default probabilities (as measured by those countries' CDS). With the CDS-IV estimator, we find no significant causal effect. We interpret these results as suggesting that their are common factors in the pricing of emerging market debt and equity, consistent with the findings of Pan and Singleton (2008), but the legal rulings we study did not affect these common factors. Moreover, our results suggest that the legal rulings did not have significant effects on other sovereign debtors. We interpret these results as consistent with the uniqueness of Argentina's circumstances, and the limited applicability of these legal rulings to future cases.

Table A7: Regressions for Brazil and Mexico

	(2) Brazil MSCI Index	(4) Mexico MSCI Index
OLS ΔD	-11.40***	-6.646**
Robust SE	(3.464)	(2.857)
95% CI	[-17.5,-4.6]	[-12.7,-1.1]
Event IVΔD	1.967	1.932
Robust SE	(5.360)	(3.655)
95% CI	[-11.5,13.8]	[-8.1,7.6]
CDS-IVΔD	3.989	3.286
Robust SE	(4.941)	(4.258)
95% CI	[-8.4,11.5]	[-10.6,9.3]

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 log 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.

Table A8: Default Probability, Other Countries

(a) OLS

Country	ΔD	Country	ΔD	Country	ΔD
Argentina	1.199***	Iceland	-0.00515	Philippines	0.0178
Austria	0.00829	Indonesia	0.0194	Portugal	0.00369
Belgium	0.0154	Ireland	-0.00555	Romania	0.0249**
Bahrain	0.00776	Italy	-0.00231	Russia	0.0558***
Brazil	0.0427***	Japan	0.00616	South Africa	0.0364***
Chile	0.0235***	Kazakhstan	0.0358***	Spain	-0.00267
China	0.00941	South Korea	0.00425	Thailand	0.00681
Colombia	0.0349***	Malaysia	0.00123	Turkey	0.0500***
Croatia	0.0290**	Mexico	0.0367***	Ukraine	0.105**
Cyprus	0.0729	Morocco	-0.00643	Venezuela	0.172***
Egypt	0.0158	Panama	0.0335***	Vietnam	-0.00470
France	0.0224**	Peru	0.0333***		

(b) CDS-IV

Country	ΔD	Country	ΔD	Country	ΔD
Argentina	1.384***	Iceland	0.0171	Philippines	-0.0122
Austria	-0.00547	Indonesia	-0.0158	Portugal	0.00355
Belgium	0.0108	Ireland	-0.0238	Romania	-0.00370
Bahrain	-0.00337	Italy	-0.0271	Russia	0.0331
Brazil	0.00191	Japan	-0.00399	South Africa	0.0197
Chile	-0.00376	Kazakhstan	0.0151	Spain	-0.0162
China	-0.00752	South Korea	-0.0170	Thailand	-0.0136
Colombia	0.00157	Malaysia	-0.0220*	Turkey	0.0112
Croatia	-0.0239	Mexico	-0.000665	Ukraine	0.128
Cyprus	0.119	Morocco	-0.0156	Venezuela	0.0240
Egypt	-0.0227	Panama	-0.00605	Vietnam	-0.0646
France	-0.00211	Peru	-0.00359		

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 on the five-year risk-neutral default probability for the country listed. The default probability measure used for the outcome variable is derived from the credit triangle approximation described in appendix K, which explains why the coefficient on Argentina is not exactly one. The coefficient 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.

I 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 A9: Regressions for Tenaris, Petrobras, and Arcos Dorados

	(1) Tenaris ADR	(2) Petrobras ADR	(3) Arcos Dorados
OLS ΔD	-5.620	-14.44**	-11.27
Robust SE	(5.163)	(7.191)	(7.775)
95% CI	[-15.3,6.1]	[-30.6,-0.5]	[-24.9,7.0]
Event IVΔD	-0.404	4.271	12.13
Robust SE	(6.775)	(9.612)	(10.20)
95% CI	[-18.5,12.9]	[-26.4,27.5]	[-25.4,44.1]
CDS-IVΔD	0.621	7.457	16.68
Robust SE	(7.314)	(10.86)	(13.75)
95% CI	[-18.9,12.7]	[-27.3,33.0]	[-26.9,45.8]

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 log 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.

J Delevered Portfolios

In table A10 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 A10: Delevered Indices, CDS-IV

	(1) Value Index	(2) Bank Index	(3) Non-Financial Index
ΔD	-16.00**	-10.79***	-27.98*
SE	(4.253)	(2.259)	(10.18)
95% CI	[-31.5,-4.2]	[-16.2,-4.4]	[-57.0,3.5]
Events	15	15	15
Obs.	401	401	401

All regressions have controls for VIX, S&P, EEMA, 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.

K 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 data from Markit on CDS par spreads and the dealer reported recovery rates, as well as a zero-coupon discounting curve.[66] 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%.[67]

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.[68] 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

[66] 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*.

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

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

$$S_n \sum_{i=1}^{n} \Delta_t P_{S(t)} Df_t + AD = (1-R) \cdot \sum_{i=1}^{N} \left(P_{S(t-1)} - P_{S(t)}\right) Df_t \tag{A5}$$

where

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

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 A5. 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:

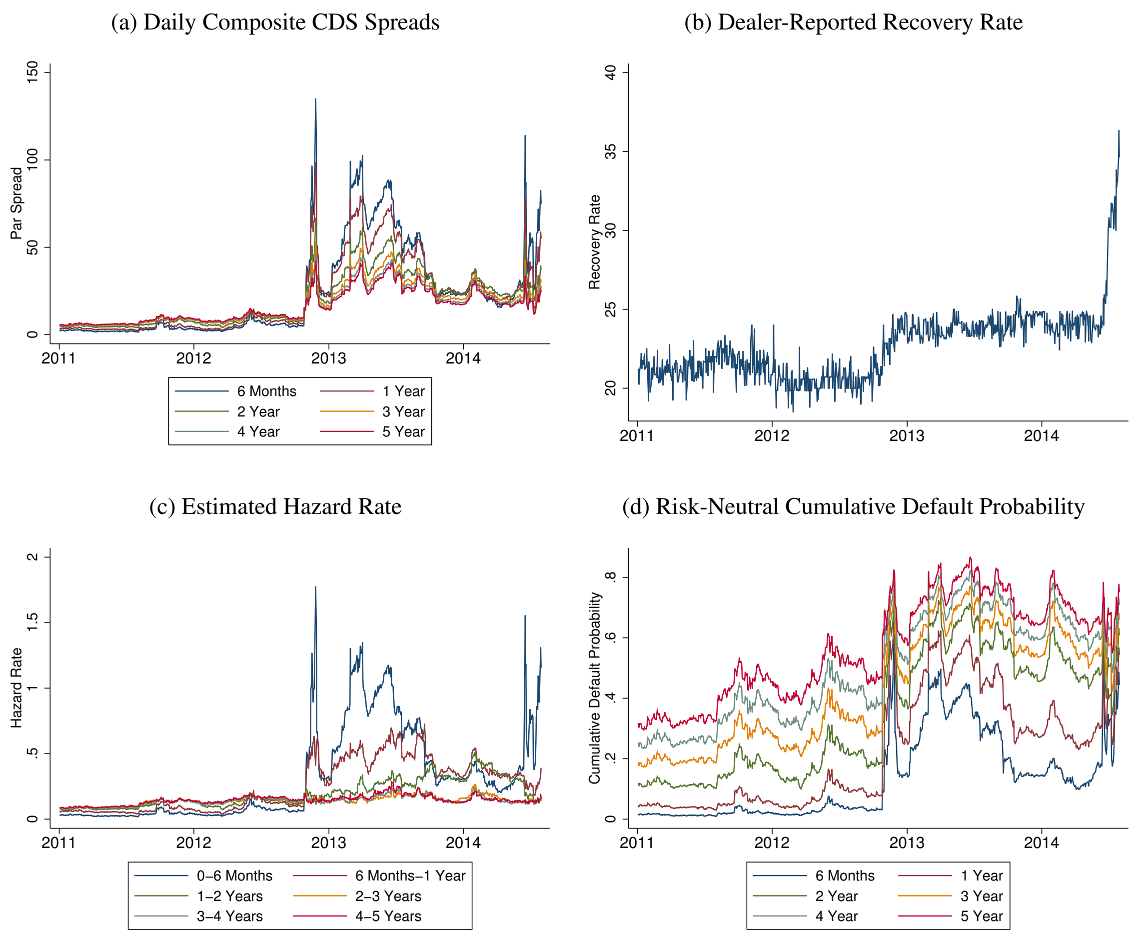
$$\begin{aligned}
Pr(D \leq 6M) &= 1 - \exp\left(-\lambda_{6M} \cdot \left(\frac{1}{2}\right)\right) \\
Pr(D \leq 1Y) &= 1 - \exp\left(-\lambda_{6M} \cdot \left(\frac{1}{2}\right) - \lambda_{1Y} \cdot \left(\frac{1}{2}\right)\right) \\
&\vdots \\
Pr(D \leq 5Y) &= 1 - \exp\left(-\lambda_{6M} \cdot \left(\frac{1}{2}\right)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

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 H, 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

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

In the figure below, we chart the CDS spreads and recovery assumptions that we use to infer hazard rates of default and cumulative default probabilities. In all regressions in the body of the paper, we use Markit's risk-neutral default probability calculations rather than our own calculations.

Figure A4: 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 K.

L Robustness Checks

L.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 our 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 A11.

The second change we will consider concerns the recovery rate. In our baseline specification, we use the average dealer-reported recovery rate. While this series does vary, and in particular increases 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 Recovery" in table A11.

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 A11. 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 A11.

Table A11: Alternate Default Probability Measures

Measure		Indices			Exchange Rates		
		Value	Banks	Non-Fin.	Blue	ADR	BCS
Markit	1Y	-27.72***	-42.45***	-30.42**	5.716***	0.910	1.025
Markit	3Y	-44.47***	-67.40***	-47.51**	8.580**	8.245	8.336
Markit	**5Y**	-54.89***	-83.16***	-58.63**	10.37**	10.71	11.01
Constant Recovery	1Y	-26.35***	-40.63***	-28.84***	5.701***	0.283	0.209
Constant Recovery	3Y	-46.96***	-71.16***	-50.04**	9.044**	9.387	8.942
Constant Recovery	5Y	-61.97***	-93.80***	-66.24**	11.67**	13.02	12.56
Par Spread	1Y	-0.122**	-0.199***	-0.134*	0.0283**	0.00817	0.0151
Par Spread	3Y	-0.273**	-0.441***	-0.295	0.0606**	0.0448	0.0631
Par Spread	5Y	-0.360**	-0.579***	-0.391	0.0790**	0.0643	0.0911
Points Upfront	1Y	-0.318***	-0.494***	-0.350**	0.0668***	0.00744	0.0121
Points Upfront	3Y	-0.417***	-0.645***	-0.448**	0.0832**	0.0637	0.0697
Points Upfront	5Y	-0.433***	-0.671***	-0.466**	0.0852**	0.0688	0.0759

Notes: Measure “Markit” indicates that these are the risk-neutral default probabilities computed by Markit. “Constant Recovery” 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.

L.2 Alternate ρ

Table A12: 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	-11.24***	-5.402***	-5.285	-3.953***
SE	(4.286)	(1.725)	(3.862)	(1.289)
95% CI	[-34.0,-3.8]	[-15.4,-2.7]	[-21.5,2.2]	[-11.2,-2.0]
Events	14	14	14	14
Obs.	355	355	355	355

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. $N_{g,t}^{Survey}$ is the real GDP news measure derived from survey forecast as described in Section 5. 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 appendix. 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.

M Individual Bond Prices

As discussed in section 7, one potential complication in interpreting our results is the RUFO clause in the restructured bond contracts. In particular, we might be concerned that as the probability of default increases, the expected payout on the restructured bonds also increases, raising the amount Argentina is expected to repay creditors. In this case, the effect of the legal rulings on bond prices is ambiguous. The effect on CDS-implied probability of default is not ambiguous; if the recovery assumption is updated correctly, then the default probability is correct.

In the table below, we observe that increases in the CDS-implied default probability lead to significant declines in the value of the restructured bonds. If the default probability is measured correctly, this is not consistent with the story that increases in the default probability coincided with increases in the probability of a settlement that offered improved terms to restructured bond holders. That is, either the RUFO clause would be circumvented, or the RUFO clause was binding and a settlement was not likely.

We also investigate the performance of the holdout bonds around the legal rulings. The bonds owned by the holdouts are very illiquid, but we were able to find some prices from Bloomberg. We are uncertain as to the quality of these prices and therefore interpret the results cautiously. Consistent with this, we find large standard errors in our estimation. This could reflect the poor quality of the data, but also has another interpretation. Several rulings coincided with significant increases in the holdout bond prices, while others did not. One possible interpretation of this fact is that some rulings raised the probability of a settlement, while others lowered that probability, but this was largely uncorrelated with whether the rulings raised or lowered the probability of default.

The restructured bond we examine in the table below is a dollar-denominated discount bond issued as part of the 2010 restructuring. The holdout bond we examine is a dollar-denominated bond maturing in 2030 that court documents show NML Capital owned in 2003. For completeness, we also show a domestic-law fixed coupon dollar bond maturing in 2017.

Table A13: Bond Level Analysis: CDS-IV

	(1) Restructured	(2) Holdout	(3) Domestic
ΔD	-124.8***	1.206	-48.56*
SE	(11.93)	(40.35)	(10.68)
95% CI	[-146.2,-93.5]	[-63.3,103.1]	[-75.0,2.0]
Events	15	15	15
Obs.	397	258	401

Notes: This table reports the effect of changes in the five-year risk-neutral Argentine default probability (ΔD) on the price (percentage log price return) 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 USD-denominated bond maturing in 2030. "Restructured" is a dollar-denominated discount bond issued as part of the 2010 restructuring with an ISIN of XS0501194756. "Domestic" is domestic-law fixed coupon dollar debt maturing in 2017 with an ISIN ARARGE03F441.

N 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 the equations 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:

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

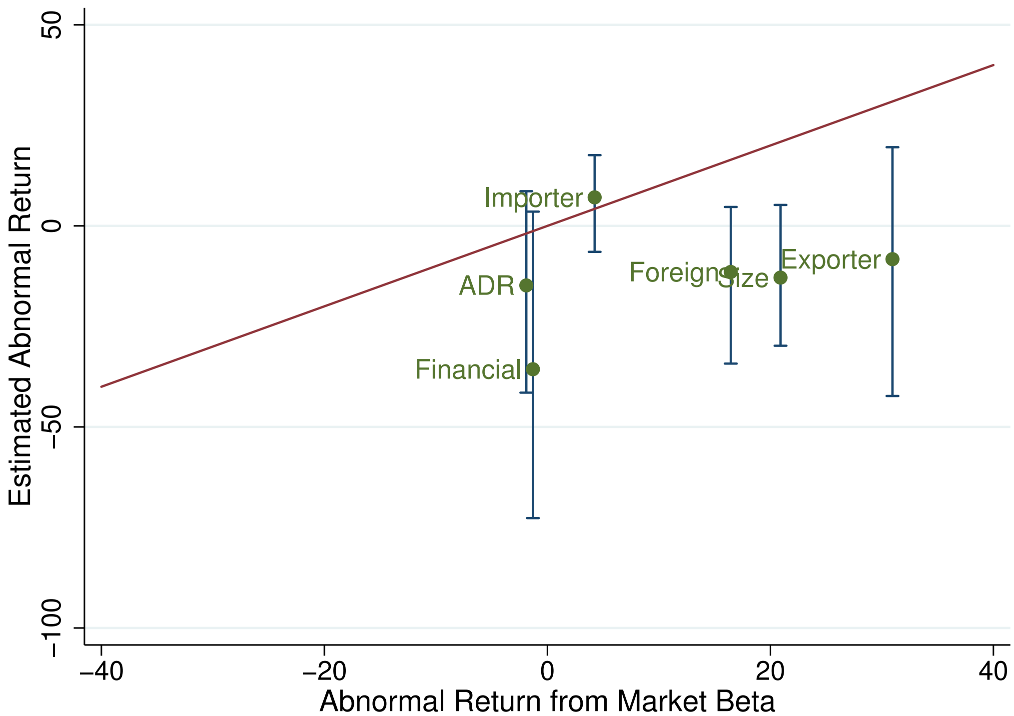
Plugging this into our system of equations,

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

From these equations, it follows that the event study approach and Rigobon (2003) approach both identify the coefficient $(\alpha_i-\beta_i\alpha_m)$, under their identifying assumptions, which is the coefficient of interest.

O Portfolio Figure

Figure A5: 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 (NFFs) and short non-export-intensive NFFs. "Importer" is defined equivalently for importers. "Financial" goes long banks and short NFFs. "Foreign" goes long firms with a foreign parent and short domestically-owned firms. "Size" goes long firms with above-median market capitalization in 2011, and short firms with below-median market cap. "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 (below) the line indicates that the portfolio over-performed (under-performed) following increases in the probability of default, relative to the abnormal return implied by the portfolio's market beta. The ranges indicate bootstrapped 90% confidence intervals.

P Results by Industry

We study the response of industry portfolios to default shocks, controlling for the response of the Argentine market. 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 A6 and Table A14 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.

Table A14: Cross-Section: Industry Returns, CDS-IV

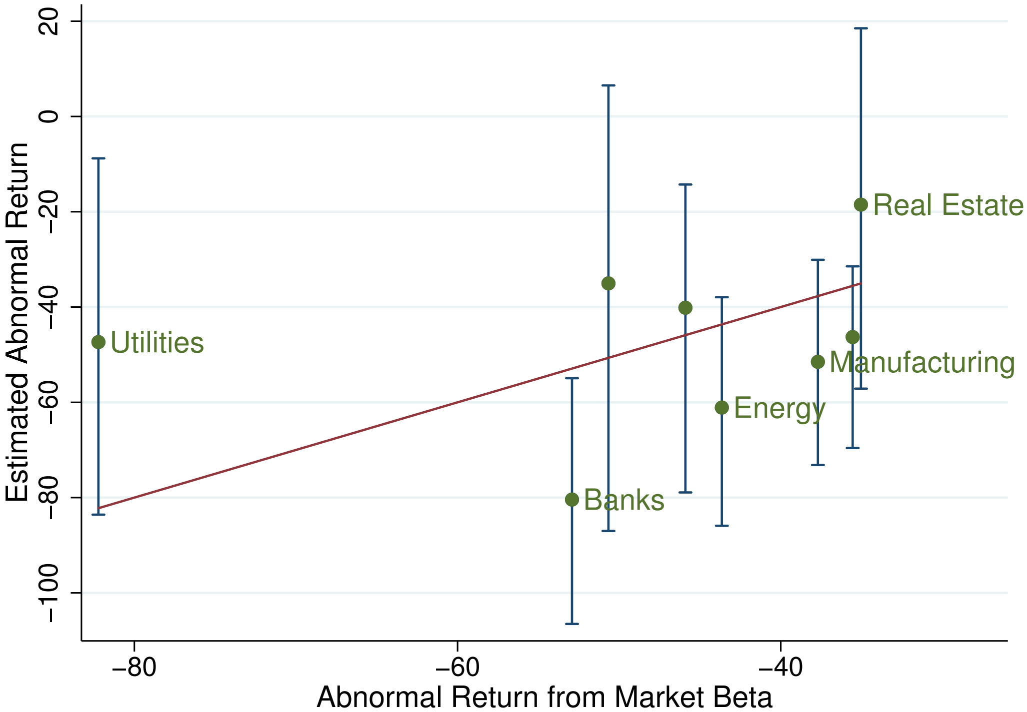
	(1) Index	(2) Banks	(3) Chemicals	(4) Diverse
ΔD	-51.23*	-27.49*	5.756	15.63
	(13.61)	(12.45)	(16.78)	(18.69)
95% CI	0	1.033	0.896	0.989
Index β	[-92.2,0.4]	[-61.0,4.7]	[-38.3,37.2]	[-45.2,66.5]
Events	14	14	14	14
Obs.	353	353	351	353

	(5) Energy	(6) Manufacturing	(7) Non-Durables	(8) Non-Financial
ΔD	-17.45	-13.79	-5.701	6.438
	(12.96)	(10.02)	(7.501)	(4.336)
95% CI	0.852	0.736	0.750	1.015
Index β	[-47.4,15.7]	[-42.1,11.1]	[-23.1,6.0]	[-10.3,20.3]
Events	14	14	14	14
Obs.	353	353	353	353

	(9) Real Estate	(10) Telecommunications	(11) Utilities
ΔD	16.54	-10.74	34.87
	(18.03)	(10.27)	(15.79)
95% CI	0.684	0.694	1.605
Index β	[-28.1,71.6]	[-37.9,6.7]	[-13.7,88.8]
Events	14	14	14
Obs.	338	353	353

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 A2. 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.

Figure A6: 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 over-performed 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.

Q Event and Excluded Dates

Table A15: Default Probability Changes and Returns during Event Windows

Event Number	Two-Day Window End Date	ΔD (%)	Equity Return (%)
1	November 27, 2012	4.40	3.49
2	November 29, 2012	-10.61	6.07
3	December 5, 2012	-6.40	2.56
4	December 7, 2012	-0.58	0.09
5	January 11, 2013	3.44	0.07
6	March 4, 2013	-5.41	6.70
7	March 27, 2013	2.59	-1.88
8	August 26, 2013	2.35	-2.96
9	October 4, 2013	0.05	-2.38
10	October 8, 2013	-1.56	2.34
11	November 19, 2013	-0.04	-3.63
12	January 13, 2014	2.38	-0.85
13	June 16, 2014	7.72	-5.94
14	June 24, 2014	-5.56	2.69
15	June 27, 2014	5.83	-2.52

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."
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).

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
29Nov12	Close-to-Open, 28Nov12 to 29Nov12	Appeals court grants emergency stay of Judge Griesa's order.	28Nov12, 5:04pm[69]	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.[70]	Denial	04Dec12, 1:46pm.	Bloomberg	
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).

[69] This order has a 9pm "creation time" and a 5pm "modification time."
[70] 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
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.
01Apr13	Non-Event (neither event or excluded)	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 excluded)	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.
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	

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
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.
16Jun14	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.
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					

Two-Day Window End	Event Type	Description	PDF Time (EST)	Decision Link	News Time (EST)	News Link	Comments
29Jul14	Non-Event	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. Excluded because the main news during this period was government statements and news about negotiations, not the ruling.
30Jul14		The 30-day grace period for the missed payment expires.				Bloomberg	We end our dataset on 29Jul14.