

The Costs of Sovereign Default: Evidence from Argentina

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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 10 percent increase in the probability of default causes a 6 percent decline in the value of Argentine equities and a 1 percent depreciation of a measure of the exchange rate. We examine the channels through which a sovereign default may affect the economy.

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. The size and features of this cost also play a key role in quantitative models of sovereign debt, beginning with Aguiar and Gopinath (2006) and Arellano (2008). Numerous empirical 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 some of the 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 U.S. courts to receive full payment. To compel the Argentine government to repay the defaulted debt, the U.S. courts blocked Argentina's ability to pay its restructured creditors, unless NML and the other holdout creditors also received payments. The Argen-

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tine government resisted paying the holdouts, 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 default¹ that allow us to identify the causal effect of sovereign default on the market value of Argentine firms. Our key identifying 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 we study 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. We discuss other potential challenges to our identifying assumption in more detail in section V.

We use credit default swaps (CDS) to measure the change in the risk-neutral probability of default. Compiling rulings from the United States District Court for the Southern District of New York, the Second Court of Appeals, and United States Supreme Court, we isolate fifteen rulings that potentially changed the risk-neutral probability of default. We identify the effect of changes in this default probability on equity returns through heteroskedasticity, following Rigobon (2003) and Rigobon and Sack (2004).² We describe this procedure and our identification assumptions in detail in section III. We find that, for every 10 percent increase in the five-year cumulative risk-neutral default probability around these rulings, the U.S. dollar value of a value-weighted index of Argentine American Depository Receipts (ADRs³) falls roughly 6 percent. Between January 3, 2011, when our data starts, and July 30, 2014, when Argentina defaulted, the risk-neutral five-year default probability increased from roughly 40 percent to 100 percent. Linearly extrapolating our estimate of the causal effect on log returns implies that this episode reduced the value of the Argentine firms in the index by 30 percent. We also find that a 10 percent increase in the five-year cumulative risk-neutral default probability causes a 1 percent depreciation in our preferred measure of the Argentine unofficial exchange rate.⁴ The most direct interpretation of the risk-neutral default probability changes and stock returns we study is that they reflect changes in the actual probability of default and the expected value of the stream of dividends paid by the firms. However, alternative interpretations are possible, and we discuss them in section V.

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

¹In our context, the "risk-neutral probability of default" is the probability that a risk-neutral agent would have to assign to Argentina defaulting within the next five years to be indifferent between buying and selling a credit default swap on Argentina. The actual, or physical, default probability could differ from the risk-neutral probability if market participants are risk-averse with respect to Argentine default. Computing the risk-neutral probability of default from credit default swap prices involves several assumptions (see appendix Section A.4).

²In appendix Section D, we run a traditional event study, and find similar results.

³ADRs are shares in foreign firms that trade on U.S. stock exchanges in U.S. dollars.

⁴We focus on unofficial measures of the exchange rate because Argentina had capital controls during this period. These measures are discussed in section II.D.

literature,⁵ as well as on some additional firm characteristics. We find suggestive evidence that foreign-owned firms, exporters, banks,⁶ 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.

This paper contributes to a large empirical literature examining the costs of sovereign default, surveyed by Borensztein and Panizza (2009) and Tomz and Wright (2013). 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. Rose (2005) documents that bilateral trade between creditor countries and debtor countries falls after a default. Acharya et al. (2014) examine the effect of the European sovereign debt crisis on syndicated loan supply and firm behavior. Arteta and Hale (2008) observe that during a sovereign default, external credit to the private sector is reduced. Schumacher, Trebesch and Enderlein (2014) study sovereign debt litigation across a range of countries over the past forty years. Using cross-country panel data, Gornemann (2014) finds a persistent drop in output following a sovereign default. Methodologically, our paper uses a natural experiment to estimate the causal effect of sovereign default. Fuchs-Schundeln and Hassan (2015) survey the literature on natural experiments in macroeconomics. Our cross-sectional analysis is methodologically related to Bernanke and Kuttner (2005) and Gorodnichenko and Weber (2016).

This paper is also related to the literature on quantitative models of sovereign debt, initiated by Aguiar and Gopinath (2006) and Arellano (2008). This literature attempts to simultaneously explain sovereign default decisions, debt levels, credit spreads, and business cycle facts for emerging market countries. In many of these papers, the magnitude of the exogenous output cost of default is one of the key parameters that determines the equilibrium quantity of debt issued and the incidence of default. This literature also emphasizes the cyclical properties of the output cost (Arellano (2008), Chatterjee and Eyigungor (2012)) necessary to explain the observed facts, and develops theories to explain the source of these costs (Mendoza and Yue (2012), Gornemann (2014), Perez (2014), Bocola (Forthcoming)). Our evidence speaks to both the size and source of these costs, but further work is needed to relate our findings to this class of models.

This paper is structured as follows: Section I discusses the case of *Republic of Argentina v. NML Capital*. Section II describes the data and presents summary statistics. Section III presents our estimation framework and results. Section IV discusses the firm characteristics that are associated with larger responses to changes in the probability of default. Section V discusses institutional details and alternative interpretations of the results. Section VI concludes.

⁵Bulow and Rogoff (1989a), Mendoza and Yue (2012), and Cole and Kehoe (1998) motivate us to study exporting firms, importing firms, and foreign-owned firms, respectively. Numerous authors (e.g. Bolton and Jeanne (2011), Acharya, Drechsler and Schnabl (2014), Gennaioli, Martin and Rossi (2014), Perez (2014), and Bocola (Forthcoming)) have offered explanations for why banks would be particularly harmed by sovereign default.

⁶Our 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.

I. Argentina's Sovereign Debt Saga

In 2001, Argentina entered a deep recession, with unemployment reaching 14.7 percent in the fourth quarter of that year. In December 2001, after borrowing heavily from the International Monetary Fund (IMF), Argentina defaulted on over \$100 billion in external sovereign debt and devalued the exchange rate by 75 percent.⁷

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 remaining holdout creditors. Holdout private creditors who were owed \$12.4 billion of principal agreed to the exchange. Following this 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.⁸ At this point, Argentina had restructured over 90 percent 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 bondholders. The creditors, led by NML Capital,⁹ 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 U.S. 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 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.

Following Griesa's initial ruling in December 2011, years of legal wrangling ensued over what this ruling meant and how it would be enforced. Griesa clarified that Argentina was required to repay the holdouts as long as it continued to pay the restructured bondholders (using a “ratable” payment formula). Argentina was not willing to comply with this ruling, and continued to pay the restructured 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

⁷Data and facts cited in this section are from Global Financial Data, Daseking et al. (2005), Hornbeck (2013), and Thomas and Marsh (2014).

⁸The interest on the defaulted debt has continued to accumulate since 2010, with the total amount owed reaching \$15 billion in 2014 (Gelpern (2014a)). However, some of these claims may never be repaid, due to issues related to the statute of limitations (Millian and Bartenstein (2016)).

⁹Elliott 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 Elliott's litigation against Peru, and Panizza, Sturzenegger and Zettelmeyer (2009) for a literature review on the law and economics of sovereign default.

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, as ordered by the court, BNYM did not forward the payment to the restructured bondholders. 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.¹⁰

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 and 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 U.S. 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 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. In section V, we argue that such channels are not plausible, because the amount of money required for a settlement was small.

In November 2015, Argentina held a presidential election in which both of the candidates in the run-off advocated a settlement with the holdouts. In April 2016, Argentina reached a settlement with most of the holdouts, and the injunction preventing payments to the restructured bond holders was lifted (Platt and Mander (2016)). The settlement was reported as being worth roughly 75 percent of the full value of the judgements obtained by the litigating holdouts (Politi and Yuk (2016)), and will cost around \$12.5 billion if it is eventually accepted by all of the holdouts (Mander and Moore (2016)). Around the same time, Argentina issued \$16.5 billion of new debt, for the stated purpose of repaying the holdouts, making the restructured bonds current, and increasing Argentina's dollar reserves.

¹⁰The low recovery rate reflects both the decline in the price of Argentina's bonds from 2011 to 2014, 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.

II. Data and Summary Statistics

A. 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 ADRs (U.S. dollar-denominated claims on underlying local equities) issued by Argentine firms and 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 are traded by a wide range of market participants.¹¹ 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 of firms included in our analysis, along with select firm characteristics, is available in appendix Table 2.

The most commonly cited benchmark for Argentine ADRs is the MSCI Argentina Index, an index of six Argentine ADRs. We also construct our own indices of ADRs, covering different sectors of the Argentine economy. We classify Argentine firms by whether they are a bank, a non-financial firm, or a real estate holding company. The industry classifications are based on the Fama-French 12 industry classification and are listed in appendix Table 2. We construct value-weighted indices for the entire market and each of these industries, except real estate.¹² The value-weighted indices we construct exclude YPF, the large oil company that was nationalized in 2012.

We use credit default swap (CDS) spreads to measure the risk-neutral probability of default. A CDS is a financial contract in which the seller of the swap agrees to insure the buyer against the possibility that a reference entity (in this case, the Republic of Argentina) defaults on a particular set of bonds issued by that entity. Once a third party, the International Swaps and Derivatives Association (ISDA), declares a credit event, an auction occurs to determine the price of the defaulted debt. The CDS seller then pays the buyer the difference between the face and auction value of the debt. In appendix Section A.4, 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 five-year cumulative default probability, the risk-neutral probability that Argentina defaults within five 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

¹¹Edison and Warnock (2004) document that ADRs that trade on the NYSE and NASDAQ are incorporated into U.S. investors' portfolios at float-adjusted market weights; that is, they are not subject to the "home bias" that causes U.S. investors to underweight foreign stocks more generally. In contrast, several market participants have told us that capital controls and related barriers were significant impediments to their participation in local Argentine equity markets during the time period we study.

¹²We 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.

¹³We prefer the five-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 one- or three-year cumulative default probability, and other CDS-based measures, subject to the caveat that the one- and three-year cumulative default probabilities are more volatile than the five-year measure. Tenors longer than five years are not traded frequently. See appendix Table 7 for details.

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 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 equity returns and we use the MSCI Emerging Markets Asia ETF to proxy for factors affecting emerging markets generally. We use the Asia 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 crude 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 IV for details).

B. 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 Shearman, a law firm that practices sovereign debt law.

In appendix Section J, 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 mentions roughly when the ruling was released. Second, we use the date listed in the ruling. Third, many of rulings are posted in the PDF electronic format, and have a “creation time” and/or “modification time” listed in the meta-information of the PDF file, which provides suggestive but not definitive evidence about when the ruling was released. In appendix Section J, 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 risk-neutral default probability changes and stock returns, non-overlapping, at least two

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

¹⁵For 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 (see appendix Section E.1). We also report our results using one-day windows in appendix Section E.1.

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.

C. *Summary of Events and Non-Events*

In figure 3a, we plot the two-day change in the five-year risk-neutral 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 U.S. court ruling was released. The details on each event can be found in appendix Section J. In figure 3b, we construct the equivalent figure for the MSCI Mexico equity index and the 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 3 here]

In the table below, we present summary statistics for the returns of our value-weighted ADR index and the changes in five-year risk-neutral default probabilities, during the two-day event and non-event windows.

[Insert table 1 here]

D. *Exchange Rates*

We also study the effect of sovereign default on exchange rates. However, “the exchange rate” was difficult to measure during this period. Argentina imposed capital controls in 2002, strengthened them in 2011, and then relaxed them at the end of 2015, after our sample ends. During the 2011—2014 period, the official exchange rate diverged from the exchange rates implied by other markets. We will consider three different measures of these parallel exchange rates, known as the Blue Dollar. All of them are based on the rate at which individuals could actually transact, subject to various transaction costs.

¹⁶We exclude three rulings for which 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.

The first unofficial exchange rate that we consider is the one that Argentines could use to buy dollars from black market currency dealers. Dolarblue.net published this rate daily and was the source many Argentines used as a 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 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 was a non-convertible currency, securities can be purchased onshore in pesos and sold offshore in dollars. The first class of instruments for which this can be done are domestic law Argentine government bonds, 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. We describe the construction of both measures in appendix Section A.3.¹⁷

[Insert Figure 1 here]

In Figure 1, 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.

The three parallel exchange rates we consider are not equivalent to a (hypothetical) freely floating exchange rate. All three depend on local markets that were difficult for foreigners to access, and were potentially subject to government intervention. We found news articles stating that, during the 2011–2014 period, the Argentine government occasionally intervened in local (peso-denominated) bond and stock markets to manage these parallel exchange rates. Moreover, the premium demanded by black market currency dealers for dollars would fluctuate based on the vigor with which the government prosecuted those dealers (Parks and Natarajan (2013)).

E. 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 Mellon, 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.¹⁸

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

¹⁷Auguste 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.

¹⁸On 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.

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 2, we plot the returns of the Argentine ADRs and the five-year cumulative default probability, as measured by CDS. The ADRs begin trading in New York at 9:30am. The default probability is constructed from CDS spreads based on the Markit “sameday” data at 9:30am EST and 10:30am EST.

[Insert figure 2 here]

From 9:30am to 10:30am, the MSCI Argentina Index fell 6 percent and five-year cumulative risk-neutral default probability rose by 9.8 percent. When the Argentine stock market opened, the local stocks associated with the MSCI Argentina Index opened 6.2 percent lower than it closed the previous night, implying virtually no change in the ADR-based blue rate.

III. 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 and 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.¹⁹ The model we consider is

$$\begin{aligned} (1) \quad \Delta D_t &= \gamma r_t + \kappa_D F_t + \varepsilon_t \\ (2) \quad r_t &= \alpha \Delta D_t + \kappa F_t + \eta_t \end{aligned}$$

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 rulings affects firms’ stock returns only through the effect on the sovereign’s

¹⁹In appendix Section I, we demonstrate how an equivalent system can be derived in a multi-asset framework.

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).²⁰ 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 risk-neutral probability of default during the event windows (time periods in which a U.S. 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 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 two-day periods in our sample into two types, events (E) and non-events (N). For each of the two types of two-day windows, $j \in \{E, N\}$, we estimate the covariance matrix of $[r_t, \Delta D_t]$, denoted Ω_j :

$$(3) \quad \Omega_j = \begin{bmatrix} \text{var}_j(r_t) & \text{cov}_j(r_t, \Delta D_t) \\ \text{cov}_j(r_t, \Delta D_t) & \text{var}_j(\Delta D_t) \end{bmatrix}$$

We can then define the difference in the covariance matrices during events and non-events as $\Delta\Omega = \Omega_E - \Omega_N$, which simplifies to²¹

$$(4) \quad \Delta\Omega = \lambda \begin{bmatrix} \alpha^2 & \alpha \\ \alpha & 1 \end{bmatrix}$$

²⁰Rigobon and Sack (2004) discusses these biases in the context of this framework.

²¹Algebraic details can be found in Rigobon (2003).

where

$$(5) \quad \lambda = \left(\frac{\sigma_{\varepsilon,E}^2 - \sigma_{\varepsilon,N}^2}{(1 - \alpha\gamma)^2} \right).$$

The estimator we employ, which we call the CDS-IV estimator, is defined as

$$(6) \quad \hat{\alpha}_{CIV} = \frac{\Delta\Omega_{1,2}}{\Delta\Omega_{2,2}} = \frac{\text{cov}_E(\Delta D_t, r_t) - \text{cov}_N(\Delta D_t, r_t)}{\text{var}_E(\Delta D_t) - \text{var}_N(\Delta D_t)}$$

As shown in Rigobon and Sack (2004), this estimator can be implemented in an instrumental variables framework. While at first glance there may appear to be alternative estimators to estimate α based on $\Delta\Omega$, we believe they are not appropriate given our null hypothesis that $\alpha = 0$.²²

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 appendix Section F. The relevance of the CDS-IV instrument is also suggested by the weak-identification F-test of Stock and Yogo (2005), also reported in appendix Section F. In table 2, we present the results of our CDS-IV estimation. The standard errors and confidence intervals use the bootstrap procedure described in appendix Section C.

[Insert table 2]

A. Equity Returns

In the first five columns of Table 2, we report the effects of increases in the probability of default on different classes of Argentine equities. We focus on our preferred CDS-IV specification in the lower panel and report the OLS results in the upper panel for comparison purposes. We find that a 10 percent increase in the five-year cumulative risk-neutral default probability causes a negative 6.043 percent log-return for our value-weighted ADR index. We also find that increases in the cumulative risk-neutral probability of default cause statistically and economically significant declines in the MSCI Argentina Index, value-weighted bank ADR index, value-weighted non-financial ADR index, and the YPF ADR. The MSCI Argentina Index falls by substantially more than our value-weighted index because the MSCI Argentina Index consists mostly of YPF and bank ADRs, which fall by more than the ADRs of non-financial and real estate firms. We linearly extrapolate the log-return to find that an increase in the risk-neutral default probability from 40 percent to 100 percent, which is roughly what Argentina experienced, would cause around a 30 percent fall in the value-weighted index, by our estimates. This increase also caused a 39 percent fall in the value of banks, a 30 percent decline in non-financials, and a 43 percent fall for YPF, by our estimates. Linearly extrapolating the log-return, we find that a completely unexpected default (a change from 0 percent to 100 percent in the risk-neutral default probability) would cause a 45 percent fall in the value index. Our results are consistent with the hypothesis that Argentina's default caused significant harm to the value of Argentine firms.

²²These issues are discussed in more detail in appendix Section F.2.

B. Exchange Rates

We next discuss our results for the various exchange rate measures we study. The last four columns of Table 2 report our estimate of the effects of increases in the probability of default on the four exchange rate measures. As with equities, we focus on the CDS-IV results and report the OLS results for comparison. Unsurprisingly, given the Argentine government's exchange rate policy, we find no contemporaneous effect of increases in the probability of default on the official exchange rate. We find that a 10 percent increase in the risk-neutral probability of default causes a 1 percent depreciation in all three measures of the parallel exchange rates. The results for the Dolar Blue (black market exchange rate) are statistically significant; the results for the ADR blue rate and blue-chip swap rate are not.

In appendix Figure 3, we display the raw data behind these results. The ADR blue rate and blue-chip swap results are both substantially influenced by a single outlier event (the Supreme Court announcement described in section II.E). We discuss this outlier in appendix Section A.3. For our other events, the ADR blue rate and blue-chip swap rate exhibit a consistent pattern of depreciation during events in which the risk-neutral probability of default increases, and appreciation during events in which that default probability decreases. Our preferred interpretation of these results is that they are consistent with the empirical coincidence of devaluation and default documented by Reinhart (2002), and with models in which a government finds it optimal to simultaneously default and devalue, such as Na et al. (2014). However, we must emphasize that the exchange rates we measure are not the freely convertible exchange rates studied and modeled by those authors.

C. Magnitudes

In Table 3, we present estimates for the magnitude of the losses caused by default. The columns labeled "Estimate (60 percent)" and "Estimate (100 percent)" report the estimated losses caused by increasing the probability of default by 60 percent and 100 percent, respectively. The 60 percent 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 percent column is relevant because it is closer to the concept of the cost of default in the literature.

In the first three rows of the table, we present estimates for the firms that have ADRs. The first-row estimates are calculated by multiplying the sum of the 2011 market values of all non-YPF firms in the value index by the point estimate for the value index in Table 2, converted from log to arithmetic returns. The second-row estimates follow the same procedure, for YPF, and the third row is the sum of the first two rows. The losses for firms with ADRs are comparable to the ultimate cost of the 2016 settlement. 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.

For the firms with ADRs, including YPF, the average yearly earnings from the first quar-

ter of 2009 through the second quarter of 2011 was roughly \$2.4 billion.²³ We estimate that, in response to a completely unanticipated default, the market value of these firms would decline by roughly eight years of annual earnings. For reference, in the quantitative sovereign debt model of Aguiar and Gopinath (2006), a country loses 2 percent of its output upon default, and is “redeemed” with a 10 percent chance each quarter. If a firm’s ADR dividends followed the same process after a default, the “cashflow news” (Campbell (1991)) associated with default would represent roughly 4–5 percent of the firm’s annual earnings. Accounting for leverage explains part but not all of the difference (see appendix Section G.3). One (speculative) explanation for our results is that default causes very persistent or permanent output losses, or equivalently a decline in growth rates, and this is reflected in firm earnings. This would be consistent with the findings of Gornemann (2014), who uses a panel dataset to estimate the impact of default on growth rates. It would also be consistent with the findings of Aguiar and Gopinath (2007), who argue that emerging market countries experience shocks to their trend growth rate more generally.²⁴ However, we must emphasize that this is not the only possible explanation for our results, and that further research on this topic is necessary.

[Insert table 3 here]

IV. 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 (1989*a*), we examine whether or not firms that are reliant on exports are particularly hurt. Bulow and Rogoff (1989*a*) 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 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 Bolton and Jeanne (2011), Acharya, Drechsler and Schnabl (2014), Gennaioli, Martin and Rossi (2014), Perez (2014), and Bocola (Forthcoming), we 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), 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

²³We calculate annual earnings from 2009–2011 because of the availability of earnings data in CRSP for all of the firms with ADRs. Unfortunately, that time period coincides with a recession in many developed countries. Using our preferred measure of Argentine real GDP, growth was low but positive over this period.

²⁴The view that emerging market countries experience permanent productivity shocks is controversial (see, for example, García-Cicco, Pancrazi and Uribe (2010)).

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 (2016). 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 appendix Section I. 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 equity market's and exchange rate's exposures to the default shock, and the sensitivity of the portfolio to the Argentine equity 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 now 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 portfolios²⁵ 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 firms²⁶ as high export intensity, and 14 of 26

²⁵Because 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.

²⁶We actually 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.

as low export intensity. We equally weight these firms, so that the “long” portfolio has a 1/12 weight on each high export intensity firm, and the short portfolio has a 1/14 weight on each low export intensity firm. We then form the long-short portfolio, and determine whether the portfolio over- or underperforms 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. In appendix Section G.4, we apply our CDS-IV estimator to the returns of the local stock index, converted to dollars at the ADR blue rate.

The over- or underperformance of the portfolios is not an ideal test of the theories. For example, if we do not observe that importing firms underperform, 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 underperformance (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 4 here]

In Table 4, we find that firms whose primary industry is export-intensive underperform 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 underperforms by a statistically insignificant amount.²⁷ 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 (defined as above-median market capitalization in 2011) 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 over- or underperform 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 Bolton and Jeanne (2011), Gennaioli, Martin and Rossi (2013, 2014), and Bocola (Forthcoming). However, we find that a “de-levered” portfolio of bank stocks (see appendix Section G.3) 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.

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.

V. 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.

²⁷We display these results graphically in appendix Figure 4.

We first discuss 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 the litigating 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 the Argentine government's 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.²⁸

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 the firms we study, 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 percent 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 have demanded repayment on similar terms; indeed, "me too" claims caused the size of the 2016 settlement with litigating creditors to grow to \$9.3 billion. Even if we assume that Argentina will eventually need to pay the full \$15 billion, that represented only 3 percent of GDP, and 45 percent of foreign currency reserves, as of 2013.²⁹ As seen in Table 3, the actual repayment was less than the loss of market value experienced by publicly traded firms. Because only a small fraction of Argentine firms are publicly traded, and there is no reason to expect the tax burden to repay the debt to fall exclusively on these firms,³⁰ the direct repayment costs could only account for a tiny fraction of the loss of market value experienced by these firms.

This issue is complicated, however, by the presence of a "Rights Upon Future Offers" (RUFO) clause in the restructured bond contracts. If Argentina voluntarily made an offer to the holdouts that was better than what the restructured creditors received, the restructured

²⁸More 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.

²⁹The CIA World Factbook reports Argentina's 2013 GDP as \$484.6 billion, and its exchange and gold reserves at \$33.7 billion as of December 31, 2013. However, the GDP calculation uses the official exchange rate, which may overstate the size of Argentina's economy.

³⁰Consistent with this argument, in past settlements (with the IMF and Paris club creditors), and in the eventual settlement of the default in 2016, the costs of the settlement were paid out of general government revenues, funded in part by issuing new government debt, and not borne by the particular firms we study.

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 U.S. courts would otherwise have blocked its payments to the restructured bondholders, would that offer 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 clause appeared to have some loopholes, allowing Argentina to potentially settle with the holdouts without triggering the clause.³¹ Moreover, the restructured bondholders could waive their right to exercise the RUFO clause, because it takes 25 percent of exchange bondholders to trigger the clause, and the whole issue could have been rendered moot if the exchange bondholders could be persuaded that waiving the clause 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 (2014*b*)). Notably, when the RUFO clause expired at the end of 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.³²

Given our identification strategy, we would be concerned about any effect the rulings had on the value of Argentine firms that did not operate through the rulings’ 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.³³ In fact, eleven of the twelve ADR firms issued debt internationally between 2002 and 2014, when the federal government of Argentina was excluded from international debt markets.³⁴ 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.³⁵ Another possible channel that would violate our exclusion restriction, which we cannot test, is that

³¹See the comments from Barclay’s reported in Cotterill (2013).

³²If 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 Section G.5, 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.

³³There 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.

³⁴Data from Bloomberg. The twelfth firm, Pampa Energia, issued debt internationally through a subsidiary.

³⁵In appendix Section G.1, 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.

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 on 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.

A. *Interpreting Returns*

In this subsection, we discuss how to interpret the stock returns and risk-neutral default probability changes we study. First, we argue that U.S. investors' stochastic discount factor is the relevant one for pricing the ADRs and CDS. As shown by Edison and Warnock (2004), the ADRs we study are held by U.S. investors in proportion to their market weight, and are not affected by "home bias." In appendix Section H.1, we document that the ADRs we study are held by large, diversified financial institutions (who are also the type of institutions that trade CDS). In appendix Section H.2, we show that the average turnover of ADRs is significantly higher than the average turnover of the underlying local equities, providing further evidence for the relative importance of foreign investors in pricing Argentine firms.

Second, we argue that the stock returns and risk-neutral default probability changes we study measure Argentina-specific news. The legal shocks are an almost canonical example of idiosyncratic risk, and it is very unlikely that U.S. 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 appendix Section G.1). We also 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.

However, it is possible that these legal rulings create a shortage of Argentina-specific (as opposed to emerging-market specific) expert capital, along the lines of Gabaix and Maggiori (2015). 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 11 in the appendix). This expert-specific capital would therefore have to be defined more narrowly than firms trading on the Buenos Aires Stock Exchange, and this shortage of expert capital would have to occur within the large financial institutions that own the ADRs we study.

Assuming the shocks we study are Argentina-specific news, there are two potential (and not exclusive) explanations for these returns. The most direct interpretation is that the returns represent changes in the expected cash flows from the ADRs. 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 the returns we measure are caused by this 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. Additionally, for some purposes, it may not matter whether the value

of Argentine firms fell because they were expected to be less profitable as a result of the default or because they became riskier as a result of the default.

The issues discussed in this section with regards to stock returns also apply with regards to the risk-neutral probability of default, as measured by credit default swaps. The most straightforward interpretation of the changes in default probabilities we study is that they are changes in the physical default probability. However, it is theoretically possible that the legal rulings induced changes in the covariance between Argentine default or recovery rates and priced risk factors, and this caused part of the change in risk-neutral probabilities that we observe.

One concern we can muster evidence against is that Argentine CDS markets are thinly traded. According to data from the Depository Trust and Clearing Corporate (DTCC), from the first quarter of 2011 until the second quarter of 2014, the Argentine sovereign was the 15th most commonly traded sovereign CDS and the 48th most commonly single-name CDS overall. In appendix Section H.3, we compare the liquidity of Argentine sovereign CDS to the liquidity of CDS on other emerging market governments, major financial institutions, and non-financial corporations.

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 broader economy. Ideally, we would study the returns of assets whose cashflows exactly matched Argentine GDP.³⁶ However, as mentioned previously, the earnings of firms with ADRs are a small fraction of the Argentine economy. Market participants may have anticipated that, conditional on default, it would become difficult for firms to make payments on their ADR dividends. In other words, default might have caused the government to adjust its capital controls.

Ex-post, we know that this did not occur, and we are not aware of any evidence suggesting it was ever likely. However, suppose investors were concerned about this possibility. In this case, we might 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 were anticipated, the anticipated changes would have applied equally to bonds, stocks, and other means by which Argentines can acquire dollars.

B. 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

³⁶In fact, Argentina has issued real GDP warrants. However, they are very illiquid, have option-like features, may have been "pari passu" with the holdout's debt claims, and are affected by the measurement of Argentine inflation. In Section G.6 of the appendix, we discuss these issues and present results for the GDP warrants.

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 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 percent—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. Ongoing creditor litigation had resulted in an attachment order, which would allow the holdouts to confiscate the proceeds from any 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. Some of these local-law bonds were affected by the legal rulings, and it may have become more difficult for Argentina to borrow as a result of the 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.

VI. 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 provide suggestive evidence that exporters and foreign-owned firms are particularly hurt by sovereign default.

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VIII. Tables and Figures

Table 1—: Summary Statistics

Window Type	Event	Non-Event
Mean ΔD_t (percent)	-0.09	-0.01
SD ΔD_t (percent)	5.06	1.79
Mean Equity Log Return (percent)	0.30	0.04
Equity Log Return SD (percent)	4.02	2.77
$\text{Cov}(\Delta D_t, r_t)$	-15.27	-2.21
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 3—: Magnitudes

Measure	Estimate (60 percent)	Estimate (100 percent)
ADRs (ex. YPF)	-4.7 \$B	-7.9 \$B
YPF	-6.6 \$B	-11.0 \$B
ADRs	-11.3 \$B	-18.9 \$B
All equities in dataset	-15.6 \$B	-26.0 \$B
All equities	-16.7 \$B	-27.8 \$B
Actual Repayment	12.5 \$B (Estimated)	

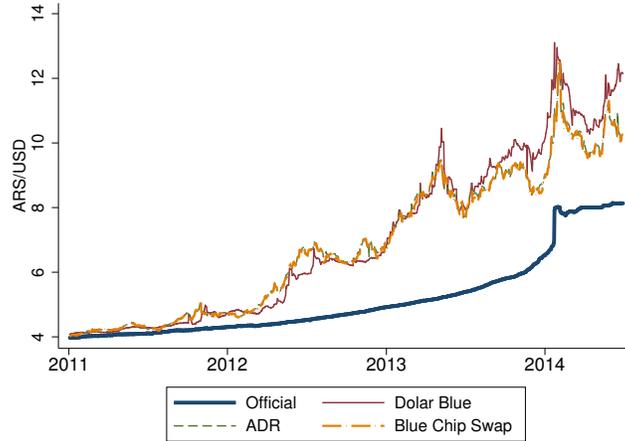
Notes: The first line, “ADRs (ex. YPF)” reports the imputed loss of market value all firms included in our sample of ADRs experienced, excluding YPF. It is calculated by multiplying the sum of the market values of all the firms in 2011 by point estimate on the Value Index in Table 2. The second row, “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 IV. It is computed by extrapolating the losses of the value index to these stocks. The fifth row, “All equities,” includes all Argentine firms with listed equities, even those that do not meet the data quality standards to be included in Section IV, and is also computed by extrapolation. “Actual Repayment” is the cost to the government of Argentina of the recently agreed-upon settlement with the major holdouts, extrapolated to cover all holdouts.

Table 4—: Cross Section: Long-Short Portfolios, CDS-IV

	(1)	(2)	(3)	(4)	(5)	(6)
	Foreign	Financial	Exporter	Importer	Size	ADR
ΔD	-27.96	-34.36	-39.47	-6.199	-33.72	-12.81
	(9.538)	(16.51)	(9.867)	(8.350)	(10.20)	(12.46)
95 percent CI	[-51.0,-10.1]	[-78.2,13.7]	[-62.8,-20.6]	[-24.7,12.5]	[-56.9,-9.8]	[-46.9,23.4]
Index β	-0.341	0.0198	-0.682	-0.169	-0.396	0.0679
FX β	-0.0888	-0.0252	-0.351	-0.141	0.0497	0.139
Events	14	14	14	14	14	14
Obs.	353	353	353	353	353	353

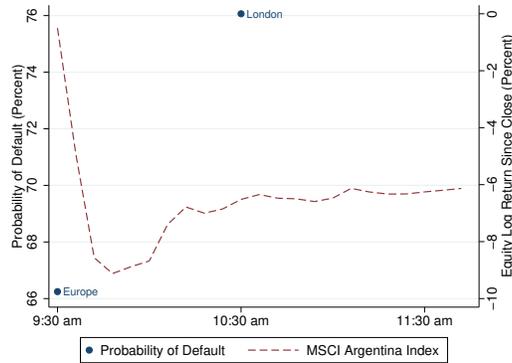
Notes: This table reports the results for the “CDS-IV” estimator. The column headings denote the outcome variable, a zero-cost long short portfolio. “Foreign” goes long firms with a foreign parent and short domestically owned firms. “Financial” goes long banks and short non-financial firms. “Exporter” 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 five-year risk-neutral default probability from 0 percent to 100 percent, implied by the Argentine CDS curve. Index beta is the coefficient on the equal-weighted index of Argentine local equities, as described in Section IV, 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 appendix Section C. 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.

Figure 1. : Exchange Rates



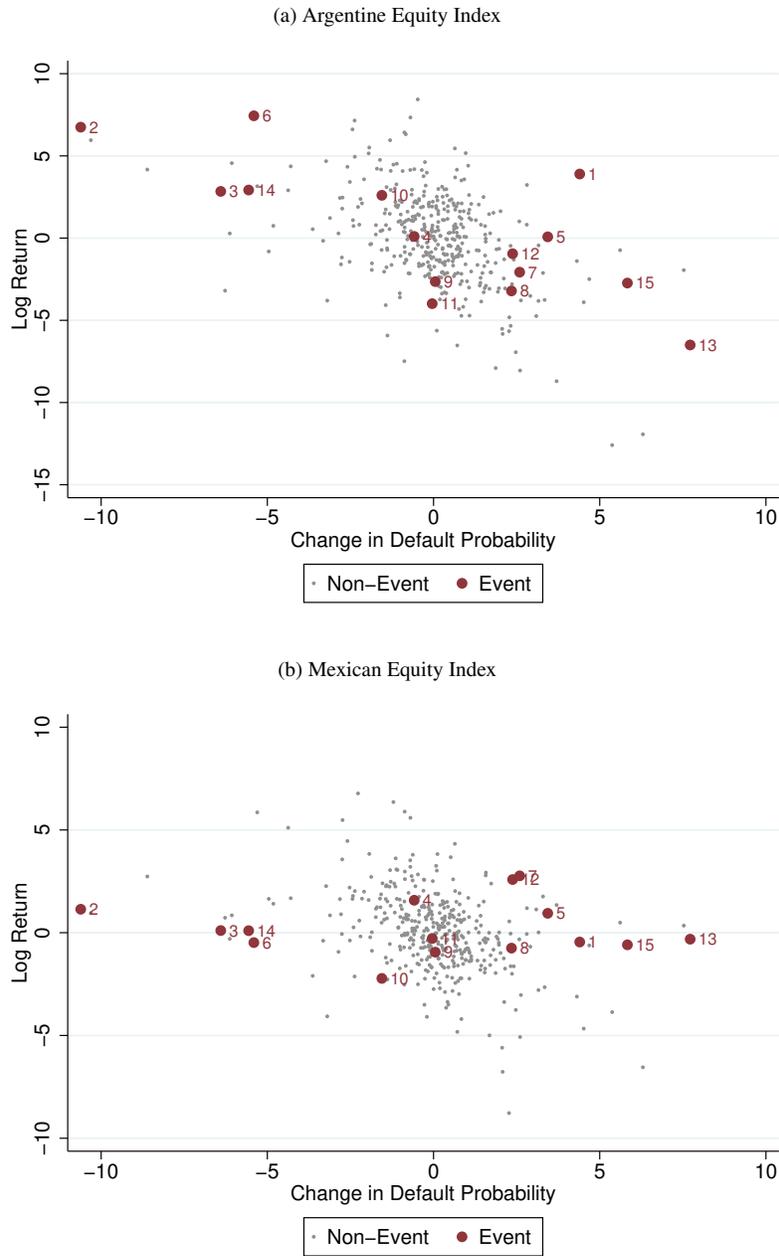
Notes: This figure plots the four versions of the ARS/USD exchange rate. Official is the government’s official exchange rate. Dolar Blue is the onshore unofficial exchange rate from dolarblue.net. ADR is the ADR Blue Rate constructed by comparing the ADR share price in dollars with the underlying local stock price in pesos, as described in Section II. 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 II.

Figure 2. : Event Data from June 16, 2014, 9:30–11:30am EST



Notes: This figure plots the five-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 3. : Equity Returns and Argentine Default Probability



Notes: This figure plots the change in the risk-neutral probability of default and returns on the Argentine value-weighted Index (top panel) and MSCI Mexico Index (bottom 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/large dot reference an event window in appendix Table 19. The procedure for classifying events and non-events is described in the text.