

Can International Courts Deter? Financial Markets Say Yes

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Abstract

Can international courts deter? Much scholarship relies on the answer being yes. Verdicts of international courts are thought to have spillover effects beyond a given case, changing global expectations about the viability of the measures ruled on. Yet public international law explicitly denies the possibility of such spillovers. Little empirical work assesses these competing claims and their underlying assumptions. This article takes a novel approach to the task, by looking to financial markets. We ask whether markets update their beliefs in reaction to international legal disputes at the World Trade Organization (WTO). Specifically, do investors downgrade a firm when a policy similar to the one it benefits from has been found in violation in *another country*? Our findings provide measured empirical support for the spillover effects of international legal verdicts: we find that Indian markets react to rulings against Canadian measures similar to those Indian firms benefit from. In a second test, looking at commodities markets, deterrence effects also appear across different industries: a ruling against US cotton subsidies significantly affects the valuation of other commodities benefiting from similar subsidies. The spillover effects of international rulings may be a matter of scholarly contention, but their existence is something that financial markets are betting on.

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1 Introduction

Can international courts deter? Much of the international law literature relies on the answer being yes. For example, Kim and Sikkink (2010) argue that international criminal prosecutions increase the perceived odds of enforcement, and thus change the calculations of other foreign leaders. They posit a “deterrence across borders” effect, whereby proceedings against a former head of state like Pinochet have effects beyond the confines of Chile on the calculations of neighboring leaders. However, not everyone agrees that such spillovers occur. Many observers maintain that given the nature of international law, such hopes are “fanciful” and amount to a “cruel joke” (Bolton, 2001, 176). Even international criminal prosecutors themselves have described the prospect of deterrence as “hopelessly idealistic”.¹

The question of deterrence matters. As Wippman (1999, 474) claims in the context of the international criminal regime, deterrence is the central goal of the legal system—more so than “considerations of justice, respect for international law, retribution, [and] avoidance of personal vengeance.” In fact, deterrence is arguably the unstated motive of most international courts. Litigation and prosecution are inefficient means of resolving conflict. A well-functioning court, it is said, cannot limit itself to resolving individual disagreements (Hudec, 1993). The aspiration is for courts to clarify the meaning of the rules; to converge expectations on what behavior is proscribed; and to convey that enforcement is likely when violations occur. In fact, one reading of the literature suggests that the *main* benefit of legal rulings may rest in their spillovers, rather than in their direct effect on the particular matter being ruled on.

At the international level, however, the wish to imbue court rulings with deterrence power runs into considerable resistance. It is no coincidence that, strictly speaking, deterrence is all but written out of international rules. As designers of treaties, countries have worked hard to ensure that rulings occur in isolation one from another, generating no spillovers outside the case and the parties to it. States fear that they stand to lose control of the regime if rulings are allowed to have effects beyond the case at hand. Deterrence implies an unprecedented level of delegation: if courts are swayed by other verdicts on similar matters, it implies that the drafters of rulings

¹ICTY Prosecutor Richard Goldstone, Letter to the Editor, Wall St. J., July 7, 2000, at A13.

have considerable power in shaping how rules are interpreted in the future. Exercising this power amounts to “making law”, a frightening prospect to sovereign countries uneasy about delegating power—particularly power over the exact meaning of countries’ legal obligations—to a group of unelected foreign judges.

The result of these competing considerations is ambiguous. On the one hand, students of courts, especially in normatively charged areas such as human rights and international criminal law, claim that the deterrence power of courts is their very *raison d’être*. On the other hand, a strict reading of international law, and states’ own incentives as designers of treaties, suggests otherwise. The question is, how can we evaluate the deterrence effect of international institutions? To what extent do international enforcement efforts have effects beyond the case at hand? Can one ruling meaningfully change expectations over similar measures elsewhere?

We address these questions in a setting that provides us with singular empirical traction: the international trade regime. The World Trade Organization’s (WTO) dispute settlement understanding is widely considered the backbone of the institution. It allows states to bring trade conflicts before a panel of judges who rule on the legality of the defendant’s policy in view of WTO law. In light of the binding nature of WTO decisions, the question of spillovers from rulings has generated an intense debate.

Anecdotal evidence suggests that the decision to file is often motivated by factors beyond the specific policy at issue. Firms demanding legal action from their governments often seem to have something akin to deterrence in mind. Davis and Shirato (2007) note how Japanese steelmakers viewed a series of steel disputes launched by Japan as a means of preventing “other countries” from putting up similar trade barriers in the future. Officials in interviews even went so far as to say that a Japanese dispute against the Ukraine was meant as a signal to all emerging economies.² Yet Davis (2012) herself also acknowledges that “WTO members are not obligated to change policies in light of new information from a ruling against another country.” In fact, the WTO’s texts largely echo the ICJ Statute in this respect, noting that “recommendations and rulings [...] cannot add to or diminish the rights and obligations provided in the covered agreements.”³ In other words, WTO

²Interview with METI legal counsel, on file with Authors. Tokyo, November 2013.

³WTO DSU Article 3.2.

rulings are not allowed to affect countries' commitments beyond the confines of the case. Thus, trade law begs the very question raised in other legal settings: do rulings have effects beyond the case at hand?

An ideal research design would consider every foreign measure related to every WTO ruling, and test whether a finding of violation makes measures similar to the one found at fault more likely to be removed, or less likely to appear. Of course, no such data exist. We do have anecdotal evidence suggesting governments can be deterred by verdicts against others: when the WTO denounced the practice of zeroing (a murky means of calculating dumping margins) by the US, both the EU and Canada, who relied on zeroing, ceased doing so. However, such anecdotal evidence is by itself unsatisfactory. Reform is more observable than the absence of expected reform, leading to potential bias, and isolated examples offer no means of answering the question, do rulings have deterrent effects on average?

Our solution is to turn to financial markets. Markets are unmoved by scholarly debates over the legal status of international verdicts. Yet the sum of their actions lends support to one view of this debate over the other. The analytical advantage of examining deterrence in the laboratory of the trade regime is that the gains and losses imposed by its legal system ultimately register on firms and prices. Markets thus find themselves interpreting the meaning of events like judicial rulings for the future prospects of firms. Our approach amounts to asking whether markets believe that deterrence operates in international law. Specifically, do markets bet that policies similar to those shown to be illegal in one country will be removed in another, despite the fact that, as Davis notes, countries face strictly no legal obligation to do so? And because markets make their bets on reform before reform has a chance to happen, market reactions are unencumbered by the exogenous factors that affect government reform. In other words, observing market reactions in the immediate aftermath of rulings lends considerable external validity to the findings.

The premise behind our approach is that firms benefit from protectionist policies that hold foreign competition at bay. The possibility that such protection might be removed should be enough to affect the valuation of a firm, or the trading price of a commodity. In fact, some firms may be entirely unable to compete absent trade protection. If a judicial ruling makes the removal

of such protection more likely, the effect should register on firms' market valuation. Yet this relationship should only be visible if markets believe that a ruling makes similar measures in other countries less likely to survive, all things equal.

We rely on event studies, a widely used method in the field of financial econometrics, to isolate the market reaction to verdicts. We first look to a recent dispute, a complaint by Japan and the EU against Canada's measures to promote solar energy manufacturing. We choose this case because the policy challenged in Canada, a "feed-in tariff", happens to be nearly identical to that implemented by India to promote *its* solar industry, making this an ideal test case for deterrence. Anecdotal evidence shows that observers and the media equated the two countries' policies. The legal matter at issue in the dispute was a complex one, and the uncertainty over its legal outcome meant the ruling came as a surprise. The question is, did the legal findings against Canada affect the valuation of solar firms in India?

Our findings support the spillover hypothesis. First, Canadian firms' prices declined after the panel's finding of violation: markets adjusted their valuations of firms on the losing side of the dispute, whose insulation from foreign competition was threatened by the decision. More importantly, the case affected the traded value of firms in India. Specifically, while the valuation of firms did not react to the panel ruling, we find a strong negative effect in the days following the ruling by the Appellate Body, the WTO's higher court. Markets reflected the concern that the AB ruling against Canada threatened continued government support of solar energy in India.

While the solar case considers deterrence across borders, our second case tests for deterrence effects across different *products*. We ask whether the ruling against American cotton subsidies in *US—Upland Cotton* affected futures prices of a different commodity, wheat, which benefits from similar subsidies to those found in violation in the cotton case. Commodities markets may provide a cleaner test than the stock market, since cotton futures concern only cotton, whereas traded firms may engage in a number of economic activities beyond producing solar panels.

Here too, the findings offer support for the deterrence hypothesis: wheat futures prices jump in reaction to the cotton ruling. Strikingly, we observe an identical pattern to the solar case in regards to the effects of panel vs. AB rulings. In both cases, the panel ruling has the greatest effect

on the disputed country or commodity, while the AB ruling is what appears to generate spillovers beyond the case at hand. This concords with the view of the Appellate Body, a standing body of judges as opposed to *ad hoc* panelists, as the creator of jurisprudence in the trade regime.

Our outcome of interest is government behavior. Yet faced with the inability of examining it directly, we study a set of actors that have an interest in anticipating this behavior. Our approach reveals a particular ontological premise: in the face of ambiguity over the existence of a social phenomenon, such as the deterrence power of international courts, one can turn to actors with a stake in the answer and ask which side of the debate their behavior is consistent with. If actors behave *as if* a social phenomenon existed, then we can conclude that for analytical purposes, that phenomenon exists—even if international law explicitly maintains otherwise.

Overall, rulings in trade law appear to have deterrent effects “across borders” and “across products”, as evidenced by market reactions. The rules may deny the existence of such effects. Yet it appears that financial markets are willing to bet that international courts’ decisions will matter beyond the case at hand. In sum, the impact of international rulings appears to exceed the intent of the regime’s designers.

2 Argument

Until recently, scholars were asking whether international courts could affect government behavior at all. Given the anarchic international realm, the debate was whether sovereign governments on the losing end of a verdict would ever follow the court’s recommendations. After all, international courts have “no jailhouse, no bail bondsmen, no blue helmets, no truncheons or tear gas” (Bello, 1996). International law is based on consent, up to and including the decision to comply with a ruling. It is a sign of the changing nature of international governance that in the span of two decades, the debate has shifted to the question of whether the effects of international courts are limited to the cases they rule on. As the institutionalist literature began demonstrating how institutions could take on “a life of their own”, expanding beyond their initial mandate to shape the expectations of the governments that established them in the first place, the study of courts has evolved accordingly. Contributing to this shift, the proliferation of international courts, each

with a growing case load—the European Court of Human Rights (ECHR) rendered verdicts in 3659 cases in 2013 alone; the WTO has handled 484 inter-state disputes since 1995⁴—has led scholars to consider how the sum of these verdicts might amount to a coherent jurisprudence. Suddenly, courts could function as the masters as well as the servants of the governments that appointed them (Alter, 1998). Not only were they able to deliver politically inconvenient rulings to sovereign states, but they could also exert lasting effects on the understanding of the rules themselves through their legal interpretations.

Of the possible spillover effects of rulings, the most attention has been devoted to the question of deterrence. This is especially true for the study of courts in normatively charged areas, such as human rights and international criminal law. As Kim and Sikkink (2010) claim in reference to prosecutions of leaders on human rights grounds, “the purpose [of trials] has not been only to punish perpetrators, but also to use accountability to deter future violations.” In view of this objective, their findings are bracing. Kim and Sikkink (2010) find that human rights prosecutions have both material and normative deterrent effects, and most interesting of all, that these effects can be felt across borders. Borrowing from the diffusion literature, they find that even a country with no prosecution activity of its own can be deterred from repression if neighboring countries have prosecuted leaders.

Others disagree. Writing about the international criminal regime, Bolton (2001) claims that “the most basic error is the belief that the ICC will have a substantial, indeed decisive, deterrent effect against the possible perpetration of heinous crimes against humanity.”⁵ Even scholars less skeptical of international law’s hold over states claim that “actual experience with efforts at deterrence is not encouraging” (Wippman, 1999).⁶

⁴http://www.echr.coe.int/Documents/Stats_annual_2013_ENG.pdf
http://www.wto.org/english/tratop_e/dispu_e/dispu_status_e.htm

⁵A more measured appraisal is found in Damaska (2008, 339): “In the adolescence of ad hoc tribunals, the cardinal importance of general deterrence was frequently invoked. The exaltation of this goal flowed from the hope that the mere threat of punishment would produce a moderating effect on the brutalities of conflicts. But as the threat failed to prevent horrendous atrocities, initial optimism surrounding this objective abated. Despite this disappointment, however, general deterrence, along with retribution, is still assigned a prominent place in discussions about the goals of international punishment.”

⁶A third camp has highlighted concerns about whether prosecutions, far from having a deterrence effect, may actually exacerbate violations. In the human rights context, the question is whether leaders are more likely to hang on to power knowing that the cost of losing power has gone up with the prospect of prosecution. In the trade context, the idea may be that governments might opt for every murkier protection (Kono 2008) as the odds of legal challenge increases. If anything, these scholars agree that courts are powerful, and affect behavior. They worry, instead, that

Beyond the empirical record, what we know about countries' incentives should warrant further skepticism over a possible deterrence effect. Sovereign states are wary of delegating power to international courts—consider the large literature seeking to explain why states would ever do this in the first place (Simmons, 2009; Simmons and Danner, 2010; Elsig, 2013). Allowing judicial decisions to bear on matters beyond a given case constitutes a significant further step in the direction of delegation. International judges are most often foreign nationals; they are not elected, and are not directly accountable to member governments. Consequently the prospect of allowing such individuals to modify the meaning of countries' obligations appears politically unpalatable. In the WTO as in the rest of public international law, the driving fear is that “the terms of the negotiated agreements could ‘evolve’ into something that none of the original parties to the agreements ever anticipated” (Ragosta, Joneja and Zeldovich, 2003). The price to pay for ‘deterrence across borders’ is that governments may progressively lose control of the institution. Therefore, states have an incentive to limit the perceived deterrent effect of rulings, even if in any given instance, some states stand to gain from such deterrence.

Little wonder, then, that the drafters of treaties have all but written deterrence out of international rules. Article 59 of the International Court of Justice (ICJ) Statute is representative of this stance: “a decision of the ICJ has no binding force except between the parties and in respect of the particular case.” This statement is widely seen as guiding all modern international courts, including the WTO, where both judges and litigants refer to it. The WTO itself includes a number of further provisions to similar effect. Article 3.2 of the Dispute Settlement Understanding holds that verdicts “cannot add to or diminish” the rights and obligations of countries. And lest judges' interpretations be perceived as changing the shared meaning of the legal texts, Article IX:2 of the WTO Agreement reads that “[t]he Ministerial Conference and the General Council shall have the exclusive authority to adopt interpretations”.

Yet much as in the case of human rights law, the actors who actually engage in trade litigation often seem to act on the premise that WTO rulings *do* have spillover effects. Pelc (2014) shows that governments appear to initiate some commercially unimportant disputes primarily as a means

this power may have unintended consequences. Our analysis allows us to speak to this concern in part, by capturing the direction of the spillover effect.

of setting a precedent to exploit in subsequent filings. Davis and Shirato (2007) show evidence suggesting that were it not for a belief in the indirect effects of litigation related to “deterrence of future protectionism”, Japan may not have filed some of its recent disputes.

Similarly, when the WTO sided with the US in a challenge against Japan’s ban on apples in 2003, the ruling was seen as having effects far beyond Japan. A number of countries were using a measure similar to Japan’s, a health standard that claimed to prevent the contagion of a fruit disease called fire blight, to ban foreign apples. That measure was found in violation, and countries like New Zealand, whose apple exports were also being blocked from other countries by these standards, celebrated the ruling. As New Zealand’s Trade and Agriculture Minister, Jim Sutton, claimed, “We will also be looking for better access to other markets, such as South Korea, which also restricts access because of fire blight.” In fact, New Zealand hoped the spillover effect of the ruling against Japan would be so strong that no further litigation would be needed to make other countries repeal their own bans. In the same statement, Sutton said that “there was no need to take a dispute case against Australia [...] as the WTO had already ruled comprehensively on the issue in a case between Japan and the United States.”⁷ Reasonable as it sounded, the trade minister’s claim was at odds with international law. Indeed, even amidst this optimistic public sentiment, apple producers themselves were acutely aware of the limits of deterrence under international law: as the chairman of Pipfruit, a major New Zealand apple producer, conceded, “It’s a great pity that this decision is not binding on other countries such as Australia.”⁸

In sum, there remains ambiguity over whether rulings deter. In many issue areas, including trade, the law is clear: international verdicts are independent, one-off events, and have no bearing beyond the matter and the parties at hand. This is hardly surprising, given sovereign countries’ fears that allowing rulings to have spillover effects might lead to a loss of control over one’s own international obligations. And yet, there is anecdotal evidence that the actors involved in international litigation think in terms of deterrence. To evaluate the potential for spillovers from rulings in international law, we require a more profound theoretical understanding of deterrence.

⁷New Zealand Government Press Release, Office of the Ministry of Agriculture, *Work Continuing on Apple Access*, 15 July, 2005. Last accessed October 31st, 2014. <http://www.beehive.govt.nz/node/23833>.

⁸“Reaction to Japan Apple Access Announcement”, Friday, 2 September 2005, Scoop Independent News, <http://www.scoop.co.nz/stories/BU0509/S00025/reaction-to-japan-apple-access-announcement.htm>.

2.1 How Does Deterrence Operate?

What does it mean to say that a legal ruling has a deterrent effect—one that reaches “across borders”, as per Kim and Sikkink (2010)? Most plainly, deterrence means that a government is less likely to enact or maintain a measure if a similar measure is ruled against elsewhere. At issue might be a policy of government repression, as in the case of human rights prosecutions, or a ban on apples, as in the aforementioned WTO example. In either case, deterrence means that, on observing a finding of violation, even those countries not directly involved in the dispute become less likely to enact or maintain similar policies. The effect is an intuitive one. The sight of a policeman handing a speeding ticket to one driver on the highway may lead other passing cars to slow down. But how might deterrence operate in the international realm? Here, we outline two related channels through which rulings deter: domestic mobilization and updated beliefs about subsequent legal outcomes. We distinguish these from a third mechanism, which relies on updated beliefs about a given country’s willingness to file (costly) legal challenges.

DOMESTIC MOBILIZATION

In any decentralized enforcement regime, where enforcement relies on challenges brought by the actors affected, deterrence is likely to operate in part through its effect on mobilization at the domestic level. This is the story often told in the case of human rights, where a finding of violation by a court can embolden a group affected by a similar measure to challenge it in turn (Simmons, 2009). This may be why, even in legal settings devoid of *stare decisis*, actors not directly concerned by a dispute often have strong feelings about the outcome. The ICJ’s ruling on Kosovo, and the possible spillover effects of the findings on secessionist movements outside of Kosovo, led many countries to forcefully take sides on the legality of Kosovo’s declaration of independence. Most countries also attempted to limit any spillover by painting the trial as a “special case”, or “an exception”.⁹

Given the decentralized nature of enforcement at the WTO, the initiation of a dispute is largely a demand-driven phenomenon. Disputes arise when industries successfully mobilize to demand en-

⁹The Nordic countries, for instance, agreed that the Kosovo ruling could not constitute a precedent, since it represented a “unique” case.

forcement. The odds of mobilization on the part of industry, in turn, should be increasing in a case's perceived legal merit. The greater the odds of winning an eventual ruling, the more likely an industry is to overcome a collective action problem and lobby for enforcement. Rulings may thus embolden actors that stand to gain if a similar ruling were applied to their situation. In the case of the dispute against Japan's ban on apples, New Zealand witnessed a sudden mobilization of its apple growers demanding a similar take-down of the ban in Australia. When the government did not immediately act on these demands, the Foreign Affairs and Trade spokesman for the opposition party sought to capitalize on the inaction: "Labour has no more excuses for failing to get action on apple access to Australia after the WTO ruling on the case between the United States and Japan... Labour owes it to our apple growers."¹⁰ The mechanism of mobilization operates when one ruling spurs demands by domestic groups to obtain similar legal outcomes in the matter that concerns them. For this to take place, however, domestic groups must believe that there is a link between a recent ruling and their odds of triumphing in their own case.

UPDATED BELIEFS ABOUT SUBSEQUENT RULINGS

International law itself is often ambiguous. The result of this ambiguity is uncertainty over the likely direction of rulings. Observing one ruling might lead observers to update their priors on what a subsequent ruling may look like, even if these are not causally related. At times, such ambiguity exists by design, serving as a second-best solution in a situation where the designers are either unable to agree on specific language, or wary of imposing overly rigid rules on a rapidly changing area of law.¹¹ As Steinberg (2004) puts it, "WTO judicial law-making has two dimensions, filling gaps and clarifying ambiguities, sometimes in areas that had been subject to diplomatic deadlock."

Steinberg offers the example of WTO judges in the *US-Shrimp Turtle* dispute striking a balance

¹⁰No More Apple Excuses For Labour, Friday, 24 June 2005, Scoop Independent News, <http://www.scoop.co.nz/stories/PA0506/S00612/no-more-apple-excuses-for-labour.htm>. The administration took up the demand: "The New Zealand Government, in consultation with industry, made a submission to Biosecurity Australia, which emphasised the WTO's finding in the Japan-apples dispute" ("Apple access or WTO censure: Australia faces a choice", The National Business Review. <http://www.nbr.co.nz/article/apple-access-or-wto-censure-australia-faces-a-choice-1>).

¹¹Fears over judicial lawmaking notwithstanding, in some cases government may want to delegate sensitive decisions to judges, rather than spend political capital reaching a compromise. As (Trachtman, 1999, 3) puts it, "We must also recognize that today dispute resolution often works in tandem with legislation in that dispute resolution tribunals function in part as agents of legislatures."

between trade and environment issues through an interpretation of GATT Article XX(g), a matter over which WTO members had long been deadlocked. Parts of this ruling had “no textual lineage”, meaning the judges filled the gap on their own. Yet the solution the judges arrived at is invoked to this day; the ruling resulted in an observable spillover on all trade measures concerning the protection of natural resources.¹²

The potential that rulings can lead to governments updating their priors grows more likely if we give credence to the recent literature on binding precedent, or what is known as *stare decisis*. Despite the lack of formal *stare decisis* in public international law, scholars generally maintain that something akin to binding precedent nonetheless operates in the trade regime (Bhala, 1998-1999). Precedential reasoning might be such an inherent part of legal reasoning (Kratochwil, 1991) that it is likely to operate in international law despite the denial of its formal authority. Insofar as there is reason to believe that judges’ training leads them to be constrained by prior rulings, then governments may re-evaluate the odds of a trade measure being found in violation strictly based on the constraining effect of a recent ruling.

In sum, updated beliefs about the likely outcome of a similar subsequent case are a likely source of deterrence power. This is what human rights scholars hope for when they argue that e.g. ICC prosecution increases the expected cost of repression by dictators. This belief also receives anecdotal support in the trade regime. When the WTO Appellate Body ruled against the practice of zeroing in the calculation of dumping margins, both the EU and Canada reigned in their own antidumping bureaucracies to get rid of the practice. Two potential explanations are possible. These countries may have been genuinely unclear on the legality of the practice. Or they thought that the new precedent made it likely that their own measure could now be more easily condemned in court. Tellingly, the main country at issue, the US, fought to maintain its practice of zeroing through multiple subsequent disputes. In doing so, its legal defense consisted largely of denying

¹²Yet the formal limits on the mandate of international courts mean that such clarification does not always survive. Countries can push back, reminding the judges that their mandate prohibits definitive interpretations of the rules. When the WTO’s Appellate Body (AB), its higher court, decided to accept *amicus briefs*, submissions by non-governmental entities taking a position in a trade dispute, developing country members protested vehemently (Ragosta, Joneja and Zeldovich, 2003). Judges have been reticent to uphold the AB finding ever since (Steinberg, 2004). However, insofar as violations exist because of a misunderstanding over the implications of the rules, a ruling can serve to dispel such confusion.

that prior rulings on zeroing had any bearing on the current dispute.¹³

CREDIBILITY OF ENFORCEMENT

Finally, legal rulings may deter by confirming actors' willingness to file challenges in the first place. International disputes are costly affairs, in both financial and political terms. In a decentralized enforcement system such as the WTO, especially, countries eager to deter violations that affect them thus face a credibility problem. As was reported in the run-up to the dispute over the EU's ban on seal products from Canada, "sources have questioned how committed Canada is to bringing an expensive WTO legal case on behalf of the relatively small sealing industry."¹⁴ Once Canada decided to take on these costs and file the seals case, observers may cease questioning Canada's commitment to upholding its rights to cultural protection—an effect that may well explain Canada's decision to file the dispute in the first place.¹⁵ Effects on countries' perceived resolve may also be the best reading of the finding in Blonigen and Bown (2003), where countries that frequently challenge antidumping remedies are shown to be less likely of being targeted by them in the first place.

This particular credibility problem is not unique to trade, or to decentralized enforcement systems more generally. Take the ICC, which has long faced a similar issue: a widespread belief held that the court would not dare to threaten state sovereignty by going after sitting heads of state. Consistent with the high hopes for the deterrence power of courts, this was thought to perpetuate a "culture of impunity" among dictators. When the ICC confounded these beliefs by issuing an arrest warrant in 2009 for Omar al-Bashir, the first sitting head of state to be indicted, this may have led other leaders to update their priors about the likelihood of similar challenges against them.

Similarly, governments may be more wary of erecting a measure that they have good reason to believe will be challenged by a trade partner that has demonstrated resolve. Such effects are

¹³As the US held before the court in *US—Zeroing*, "the Panel is not bound to follow the reasoning of any prior report. According to the United States, the Panel is charged with making its own objective assessment of the matter before it." (*US—Zeroing*, WT/DS402/R, para 7.8).

¹⁴*Inside U.S. Trade*, May 8 2009.

¹⁵The commercial interests represented by seal products exports were dwarfed by the legal costs, not to mention the political costs of antagonizing the EU at a time when a Canadian European Trade Agreement was being negotiated.

best understood through the lens of the literature on reputation and credibility in international security.¹⁶ Deterrence through updated beliefs about the odds of a challenge in the first place is less about the effect of the ruling on shared expectations over the meaning of the law, and more about the complainant’s willingness to take on enforcement costs. When Davis and Shirato (2007) note how Japanese steelmakers viewed a series of disputes launched by Japan as a means of deterring “other countries” from putting up future trade barriers, the implication is that Japan meant to convey its resolve to go after violations that affected it. In contrast, when Davis and Shirato (2007) later add that the Japanese saw even a failed case a success, on account of how the one claim upheld by the court had a deterrent effect on other countries, we are back to the type of legal deterrence discussed above. The distinction is important: when deterrence operates through domestic mobilization and updated beliefs about subsequent rulings, it is attached to the legal issue; when it operates by increasing the credibility of a challenge, its effect is attached to the challenging country. Both theory and empirics are chiefly concerned with the former.

2.2 How Can We Measure Deterrence?

How might we know deterrence when we see it? Ideally, one could observe all measures that bear a resemblance to every policy struck down in every WTO ruling, and test whether findings of violation increase the odds that these related measures are removed, or less likely to appear. Yet collecting such data is unfeasible. Indeed, the very design of the WTO reflects the difficulty of spotting trade violations: it is in part why WTO enforcement relies on “fire alarms” over “police patrols” (McCubbins and Schwartz, 1984), as centralized enforcement would require an outsized bureaucracy to identify violations. The upshot is that we cannot test whether legal rulings reduce similar unchallenged violations elsewhere, since there is no good measure of the latter.

This difficulty explains why few attempts have been made to measure deterrence in trade. The one stand-out is Davis (2012), whose research design is premised on the idea that effective deterrence would entail that fewer subsequent disputes need to be filed over time. Indeed, if judges’ rulings truly have a deterrent effect, then other countries should grow less likely of maintaining or erecting measures similar to the one found at fault, and the motives for legal challenges should

¹⁶See, e.g. Schultz (2001).

decrease in turn. If upon seeing a policeman hand a ticket to a speeding driver, every other driver slows down, then fewer tickets need to be written. As Davis shows, we do indeed observe a reduction in the frequency of disputes within most of the WTO's legal issues. The number of disputes alleging violations of both new agreements covering areas like intellectual property and agreements covering standards appears to decrease over time somewhat.

And while this is as close as trade scholars have come to testing the legal deterrence hypothesis, it is also imperfect, as Davis readily admits. Filing frequency is a blunt measure, and given the small number of cases in any given legal issue, it is difficult to distinguish the effect of past cases from exogenous factors. Moreover, one prevalent view of the WTO claims that governments may rely on unfavorable rulings for political cover, as a means of decreasing the political costs of denying protection to powerful interest groups. In such a case, disputes may still need to be fought, and lost, before governments comply. Therefore, successful deterrence need not entail a decrease in the frequency of litigation. The case of apples and fire blight is instructive in this regard: despite the hopes of New Zealand's trade ministry and its apple producers, Australia did not voluntarily take down its ban: New Zealand had to take the matter to court. It won on all counts, and the ruling borrowed heavily from the prior case against Japan.¹⁷

FINANCIAL MARKETS AS INDICATORS

Faced with the problems of measuring aggregate deterrence, we turn to a novel approach. The distinctive quality of international trade law is that the effects of rulings ultimately register on firms, some of which are publicly traded, and on market prices. Fluctuations in share prices and commodity prices provide us with the variation we use to test our expectations.

Firms lobby the government for protection in the form of trade barriers, or support in the form of subsidies. If an unfavorable finding against a similar discriminatory measure elsewhere makes the removal of such protection more likely, the effect should register on firms' stock market valuation. In an extreme case, a firm may be uncompetitive absent protection. Markets should be interested in the odds of such protection being maintained. Financial markets thus find themselves in the

¹⁷Additionally, there is reason to believe that the increase in legal merit may have ambiguous effects on the odds of disputes arising: as Gilligan, Johns and Rosendorff (2010) demonstrate, stronger cases may actually be more likely to be litigated, insofar as legal merit may lead complainants to ask for more from defendants.

position of assessing the odds of a cross-border deterrence effect. If rulings are truly independent events, there would be no reason for markets to update their priors about the future performance of firms based on rulings against other countries. If, conversely, a ruling has effects on the expected longevity of similar measures elsewhere, then one should expect a sell-off of the firms benefiting from the threatened measure.

In this way, markets provide us with revealed aggregate beliefs. Our approach amounts to a survey of individuals who are betting on the link (or lack thereof) between an international legal verdict and the future earnings of a foreign firm. Given the ambiguity of deterrence from a legal standpoint, our tests ask: do markets behave *as if* there were a deterrence effect? If so, then we can conclude that in considering the sum total of international law, judicial behavior, and domestic mobilization, a majority of global investors perceive the deterrent effect as weighing more heavily than the forces that would impede it.

Our focus on market reactions is a measure not of individual beliefs, but of their expectations in the aggregate. Similar approaches have been gaining support elsewhere. Recent findings, for instance, suggest that polls of voter expectations consistently yield more accurate forecasts than polls of voter *intentions* (Rothschild and Wolfers, 2010).¹⁸ The implication in our context is that it may be preferable to examine individuals' revealed beliefs about the likelihood of spillovers than to conduct a survey of a thousand trade lawyers asking: "Do trade disputes carry spillover effects?"

Focusing on market reactions rather than government behavior itself carries another analytical benefit. Because market reactions amount to *ex ante* bets on reform, before reform has a chance to occur, they carry greater external validity than if we were to simply test whether a given government reformed in reaction to a given ruling. Indeed, the actual occurrence of reform depends on a host of exogenous factors. This stochastic component (representing events like electoral upsets, or exogenous economic shocks leading to increased calls for protection) is absent from the evaluation of markets, who make an *ex ante* call on the likelihood of policy reform immediately after the announcement of an unfavorable ruling elsewhere. In sum, market reactions offer a less context-

¹⁸Similarly, polls asking individuals about their predictions for the 2014 Scottish sovereignty referendum fared significantly better than polls asking individuals what their own voting intent was. Our approach amounts to the former. ("Scotland's 'No' Vote: A Loss for Pollsters and a Win for Betting Markets". Justin Wolfers. *New York Times*. Sept 19, 2014.)

dependent estimate of the likelihood of deterrence-driven reform. They give us a better sense of the probability distribution of government behavior than examining a few draws from that distribution.

Until now, the few studies that have relied on stock price event studies to examine the effect of legal outcomes have focused on the direct effect of rulings. Desai and Hines (2008) examine the share prices of American exporters when the subsidies they benefited from were found to be illegal in the *US—Foreign Sales Corporations* dispute.¹⁹ As expected, share prices were negatively affected. Similarly, Jensen (2007) finds that the shares of US steel firms fell abnormally when the safeguards that President Bush imposed in 2002 were found to be in violation of WTO rules.

These earlier studies demonstrate how the question, until recently, was whether markets expected WTO rulings to have any effect at all on the policy being targeted by the dispute. There was sufficient doubt about odds of government compliance, and markets' ability to act on legal signals, that such studies were much needed tests of the binding effect of international law. Scholars were interested in whether markets expected defendant countries to modify their policies in any way in response to rulings. While we rely on similar methods, our objective is to see whether the share prices of *foreign* firms, in countries not directly implicated in the ruling, are also negatively affected. We also ask whether courts can deter across different commodities. If so, then we may conclude that markets are willing to bet on a deterrence across borders effect.

We conduct two distinct tests, looking at the effects of two distinct rulings. Our first test considers the prices of solar energy companies across several members in reaction to a ruling against Canada's solar subsidies. This tests for a cross-border, within-product deterrence effect. Our second test uses the same quantitative case study approach to look for a deterrence effect across different commodities. There, we are interested in whether rulings against US cotton subsidies affected the prices of another commodity, wheat, benefiting from similar subsidies as cotton.

CASE 1: SOLAR ENERGY AT THE WTO

Consider a typical WTO dispute. In August 2011, Japan, later joined by the EU, challenged Canada's domestic content requirements program, as implemented by the province of Ontario. It charged that Ontario's "feed-in tariff" (FiT) program unfairly discriminated against foreign

¹⁹See *US—FSC*, DS108.

providers and offered a WTO-illegal subsidy to attract investment and protect domestic firms.²⁰

Under the FiT program, solar energy producers were able to sell their electricity into the Ontario grid at up to six times the price of conventional energy.²¹ Industry sources suggested that in filing its complaint, Japan was motivated not only by the desire to take down Ontario's solar program, "but also by the worry that similar measures that condition renewable energy incentives on the use of domestic content *are proliferating around the world.*"²² The same source pointed out that "countries such as India, Italy and Malaysia have adopted similar programs."²³

Of these countries, India's program is the most ambitious. Its solar program, which also relied on a FiT, was highly similar to Ontario's. It, too, put in a content requirement, and it provided subsidies through guaranteed prices on condition of meeting those requirements. Foreign firms viewed both programs in a similar fashion. They considered investing and setting up in the country to benefit from the national schemes. As a lawyer representing US Solar companies claimed, "some U.S. solar companies may be willing to modify their business plans to tap the Indian market, as they have in Ontario, Canada, which has similar domestic manufacturing requirements."²⁴ The salience of the Indian solar industry, and the extent to which the Indian measures resembled the Canadian support scheme, makes it an ideal case on which to test our expectations.

The first ruling took place in December 2012. There were two main sets of claims, over (i) the content requirement, and (ii) the government subsidy. The panel found the content requirement in violation of Canada's WTO obligations, and it refused to rule on the EU's claim that Ontario's subsidy provided a "benefit" within the meaning of the Agreement on Subsidies. The ruling came as a surprise. The case was perceived as a complex one. In a rare conclusion, the panel found itself unable to rule on the question of "benefit", since it reasoned there was no true market price for energy against which to base a calculation of whether a benefit was conferred. As evidence for the fact that judges could well have ruled differently, one of the three panelists issued a dissenting

²⁰See *Canada—Renewable Energy* (DS412) and *Canada—Feed-In Tariff Program* (DS426).

²¹USTR Cautious On Japan's WTO Challenge Of Canadian Green Energy Law. Inside US Trade. June 24, 2011.

²²ibid, emphasis added. As Japan noted in the press release announcing the start of litigation, "Japan is seriously concerned about possible proliferation of such protectionist measures all over the world." (Request for the establishment of a Panel on Certain Measures Affecting the Renewable Energy Generation Sector in Ontario, Canada. METI Press Release. June 1, 2011. Available at http://www.meti.go.jp/english/press/2011/0601_01.html).

²³ibid

²⁴India's Local Manufacturing Rule Causes U.S. Solar Firms To Think Twice. Inside US Trade. February 25, 2011

opinion, a rare outcome in the WTO, occurring in about one percent of cases. The reason this is relevant from our standpoint is that it suggests that markets would have been unable to foresee the likely direction of the ruling ahead of its announcement.

Both parties appealed different parts of the verdict, as happens in a majority of cases, and the AB offered its ruling in May 2013. The AB agreed with the panel in finding Ontario's content requirement in violation (though it reversed some of the reasoning). It also reversed the panel's reasoning on the subsidy claim, finding that the panel's decision not to reach a conclusion on the question of "benefit conferred" was unjustified. Yet the AB but found itself similarly unable to arrive at a ruling, given how it the AB has no fact-finding ability. In sum, net of appeal, Ontario's program was ruled illegal on the basis of its content requirement. And while no ruling was delivered on the issue of benefit conferred, the AB ruling suggested that a subsequent panel could not similarly duck the matter. Canada stated it was disappointed with the result, but pledged to amend its solar program and come into compliance.

The question is, do market reactions reflect expectations of a deterrence effect, as per Japan's own hopes as it filed the dispute? Global media were quick to suggest that the ruling in Canada might embolden other countries to follow suit. As the *Financial Post* put it, "Canada's defeat may spur more WTO disputes by countries which are desperate for economic growth and suspect their firms are being illegally locked out of infrastructure projects abroad."²⁵ The initial ruling against Canada seemed to spur domestic mobilization among solar firms in the US. The US Department of Commerce began fielding petitions for enforcement of similar programs elsewhere. In fact, in the wake of the panel ruling against Canada, in February 2013, the US initiated a dispute against India's solar program.

Throughout the dispute against Canada, legal observers poured over the panel and AB reasoning on the local content and subsidy questions. They discussed implications for subsequent disputes of similar green energy initiatives. Yet was anyone willing to bet on these links having a causal effect? Was there a perception that other countries would be deterred from maintaining

²⁵"Canada loses WTO appeal over Ontario's green incentives that discriminate against foreign firms". *Financial Post*. May 6, 2013. Available at: http://business.financialpost.com/2013/05/06/canada-loses-wto-appeal-over-ontarios-green-incentives-that-discriminate-against-foreign-firms/?_1sa=8ac1-381d.

similar programs?

CASE 2: US—COTTON AND COMMODITY PRICES

In September 2002, Brazil initiated a dispute against the US, claiming that American cotton subsidies violated WTO rules, since they decreased American costs, increased production, and depressed world prices, causing injury to Brazil. The panel ruling, in September 2004, supported all of Brazil’s claims. Specifically, judges established a causal link between the American measures and world prices, finding that market loss assistance payments, a measure widespread in many countries, represented “significant price suppression” within the meaning of Article 6.3 of the Agreement on Subsidies, leading to prejudice to Brazil within the meaning of Article 5. The panel also argued that the subsidies were illegal because payments to US farmers depended on the crop grown, another feature of many farm support schemes in the US and the EU. The ruling was ground-breaking, both as an instance of a poor country prevailing over the superpower, and in the novel legal interpretations it yielded. Trade scholars began re-examining the legality of other agricultural support schemes “in light of the recent WTO panel report on upland cotton” (Swinbank and Tranter, 2005), readily referring to the “precedent of Upland Cotton”,²⁶ while recalling in the same breath that “future panels are not bound by the deliberations of past panels”.²⁷ The dispute yielded surprising legal findings, charting new waters in how to assess whether a subsidy caused injury, and thus whether it was allowed under trade rules. When the US appealed, the AB upheld all of the panel’s claims.

In short order, the mainstream media began asking what these rulings meant for other products benefiting from similar schemes. The finger was pointed most of all at wheat, which captured the lion’s share of US farm payments. Newspaper articles appeared with headlines like “WTO Ruling Threatens Wheat Subsidies”.²⁸ Observers spoke of “ripple effects” on other commodities, with others saying that “the decision might prompt the U.S. to consider changing its policy on other

²⁶In an indication of the continuing ambiguity of the status of past rulings at the WTO, trade scholars usually refrain from using the word “precedent”.

²⁷Swinbank and Tranter (2005), p. 52 and 54, respectively.

²⁸“WTO ruling threatens wheat subsidies”, Alwyn Scott. *The Seattle Times*. March 4, 2005. Available at http://seattletimes.com/html/business/technology/2002196338_wtofarmers04.html, last accessed September 30, 2014.

crops, to bring it into compliance with the cotton ruling.”²⁹ Roberto Azevedo, who was to become the WTO’s Director General, but was then advisor to Brazil’s foreign ministry, declared bluntly “This is a precedent (...) This is a war that must continue”, with *The Economist* commenting that “other countries may now join Brazil in this war; there are, after all, plenty of other subsidies to take on.”³⁰ In short, the legal ruling was seen as spurring on additional challenges.³¹

In both the solar energy and the cotton subsidies dispute, the question is, would markets be willing to bet on there being a deterrent on governments, despite international law itself suggesting otherwise? In the case of solar energy, would a ruling against Canada be seen as threatening continued support to the Indian solar industry? And in the case of cotton, would a legal finding on one product render support of an entirely different commodity, benefiting from a similar, but distinct program, less likely in the future? We turn to financial markets to find out.

3 Data and Research Design

We begin by examining reactions to the solar energy case, the more complex of our two cases. While the cotton case considers spillovers on a single commodity, our analysis in the solar case considers a number of firms across three different countries. We thus begin by collecting data on the daily share prices of publicly traded solar companies in Canada, the US, and India. The resulting dataset features one row per company i on trading day t . To collect a list of relevant firms, we drew from WTO documentation, the membership directories of industry associations, and policy reports from the renewable energy sector.³² We limit our sample to only those companies that have direct involvement in solar panel manufacturing or the provision of solar energy.³³ Our

²⁹ibid.

³⁰“Unpicking Cotton Subsidies”. *The Economist*. April 30th 2004. <http://www.economist.com/node/2626900>.

³¹“Brazil’s success in the cotton case could also lead Brazil or other countries to file additional cases against US domestic support programs, focusing on programs benefiting producers of commodities other than cotton” (Mercier, 2004).

³²Most usefully, the US request for consultations in its dispute against India provided a list of firms that benefited from the measures at issue. We included all publicly traded companies on that list. On the Canadian side, the Canadian Solar Industries Association (CanSIA) provides a comprehensive list of their North American membership. Groups like the Renewable Energy Policy Network for the 21st Century (REN21) also provide useful lists of the largest companies in the sector in their “Renewables Global Status Report.” Available at <http://www.ren21.net/> (accessed February 16, 2014).”

³³Some of these sources include law firms and consulting companies that work closely with the solar industry. We exclude such firms from the analysis since they will be affected less directly by the dispute outcome.

search yields 8 firms in Canada, 6 in the US, and 11 in India (see Table 1).³⁴

The data include daily stock prices for all 25 firms from November 2011 to December 2013. This time span ensures that there are enough observations on either side of our key events to test the durability of our results using a variety of estimation and event windows. We also include the daily price for the relevant index in each country: the Toronto Stock Exchange (TSX) for Canada; the New York Stock Exchange (NYSE) or National Association of Securities Dealers Automated Quotations (NASDAQ) for the US; and the Bombay Stock Exchange (BSE) for India. We rely on these broad indices in the analysis reported below. In our robustness tests, we also rerun our analysis using more focused industry-specific indices and find consistent results,³⁵ suggesting our choice of index is not driving the results.

RESEARCH DESIGN

In our primary analysis, we conduct an event study to identify abnormalities in the prices of traded solar companies (Wilf, 2013; Desai and Hines, 2008; Fama et al., 1969). We start with the observed share price of each company i on trading day t . The estimation window is a span of days $t < 0$ and the event window is a period $t = N$, where $N \geq 1$. Abnormal prices ($a_{i,t}$) are calculated as the difference between the observed price ($p_{i,t}$) and the expected price ($e_{i,t}$), or:

$$\text{Abnormal Price } (a_{i,t}) = \text{Expected Price } (e_{i,t}) - \text{Observed Price } (p_{i,t})$$

For event windows larger than one day, the cumulative abnormal prices ($c_{i,N}$) are the sum of $a_{i,t}$ over the event window ($t = 0, 1, \dots, N$), or:

$$c_{i,N} = \sum_{t=0}^N a_{i,t}$$

We estimate expected prices ($e_{i,t}$) by regressing the observed price ($p_{i,t}$) for each company on

³⁴Note that a large number of firms in the industry are private or state-run enterprises, rather than publicly listed ones.

³⁵The indices related more closely to the renewables/energy sector are the S&P/TSX Renewable Energy and Clean Technology Index (TXCT) in Canada, the NASDAQ Clean Edge Green Energy Index (CELS) in the US, and the S&P BSE Power index in India.

the daily index value over the specified estimation window. There is no absolute rule about the required length of estimation window. The idea is to track the correlated values of individual firms and a larger index over a sufficiently long period, so as to develop accurate predictions of what a firm’s price ought to be in the future, absent the event. We use 50-day estimation windows in our reported results.³⁶

Consistent with existing studies, we also control for the daily *Exchange Rate* $e_{j,t}$ in each country (Desai and Hines, 2008; Jensen, 2007), since this may affect share prices. To have a consistent reference point, we use the rate of each country j ’s currency relative to the Euro.

From these regression estimates, we predict the expected prices ($e_{i,t}$) for each day of the event window. We report the 10-day period after the circulation of the panel/appeal report.³⁷ We then sum the abnormal prices over the 10-day event window to get the cumulative abnormal prices ($c_{i,N}$) for each firm. To measure the significance of the abnormality, we calculate a test statistic using the following formula:

$$Test_{i,N} = \frac{1}{\sqrt{N}} * \frac{c_{i,N}}{sd_{i,N}}$$

where $sd_{i,N}$ is the standard deviation of the abnormal prices. The resulting test statistic tells us whether each firm’s prices are significantly different from the expected price absent the event. Assuming a normal distribution, absolute values of $Test_{i,N}$ larger than 1.96 indicate significant abnormality at the 5% level.

As we report below, most firms’ values exhibit significant responses to the circulation of the panel/appeal reports in the Canada dispute. However, since we are interested mainly in industry-wide effects at the country-level, we conduct a more conservative test of the aggregate abnormality across all companies per country. We do this by regressing the cumulative abnormal prices ($c_{i,N}$) of all firms on a constant. Those estimates tell us whether the abnormal prices, taken as a whole, are statistically different from zero — i.e., whether the companies *as a group* experienced a significant movement in prices. This stricter test is reported for each model, and it gives us a better indication of the industry-wide effects of panel/appeal reports.

Since our approach relies on the event being a surprise, it is worth repeating how unexpected the

³⁶The duration of the estimation window—whether it’s 50, 80, or 120 days—does not change our results.

³⁷The results do not change when looking at the single day on which the report is circulated.

ruling against Canada was. There was much ambiguity over the legality of Canada’s program, as evidenced by the rare split decision, with one of the panelists dissenting from the majority opinion. The nature of the ruling serves our analysis, which rests on the assumption that the markets were unable to anticipate the content of the ruling. Markets are aware that there is a dispute, but they cannot know the direction of the ruling until the report is circulated. As a result, there is little reason to suspect that the court’s decision can be anticipated by the markets.

Our approach is not the only way to gauge abnormal changes in stock prices. In robustness tests, we implement the procedure used in Jensen (2007). His study of how stock markets respond to a ruling against US steel safeguards uses a series of regressions with Newey-West standard error corrections, which account for heteroskedasticity and autocorrelation in the error term. The benefit of this approach is producing unbiased estimates while allowing for temporal correlation in the errors—a trait common to economic series like stock prices. We run a comparable procedure, which we describe below, and find results consistent with our event study analysis. The consistency in the results gives us confidence that our findings do not depend on our choice of estimation/event windows, reference index, or estimation technique.

4 Analysis and Results

If the WTO’s rulings exert a ‘deterrence across borders’ effect, then markets should take this into account in their valuation of foreign firms. If investors perceive that the earnings prospects of firms are harmed by adverse WTO decisions, we ought to see downward movement in share prices of companies in countries on the losing side of the dispute *and* in countries that have a comparable interest in the policy that was struck down. Using the event study approach described above, we find evidence that markets do react to WTO decisions in ways that point to a deterrence effect.

CANADA. Our main research question asks whether WTO rulings have consequences that ripple through the market to countries beyond the litigants. However, we first establish whether investors respond to rulings in the more likely case: firms in the country on the losing side. We begin by testing the impact of the panel and AB ruling on Canadian firms. To reiterate, all of the models

reported use a 10-day event window, a 50-day estimation window, and rely on the aggregate stock exchange index. However, the results do not change when using 1-day event windows, 80- or 120-day estimation windows, industry-specific indices, or combinations of these parameters.

Note that, through the analysis, the models provide a good overall fit to the data. Event studies depend on how reliably the model generates predictions of firms' future prices. Doing so requires that the model do a good job explaining variation in prices during the estimation window.³⁸ The r-squared statistics from our models of each firm give us confidence in the approach; our models explain at least 50% of the variation for 5 of our 8 Canadian firms, or 42% across the entire sample. Figure 1 provides a useful visualization. We observe quite a close marriage between observed and predicted prices over the estimation window ($t < 0$). This gives us greater confidence that the differences we see over the event window ($t > 0$) are not simply an artifact of poor predictions.

Table 2 reports the prices of Canadian firms after the announcement of the panel ruling (Column 1) and the AB ruling (Column 2). The top cell for each firm reports the cumulative abnormal price (over the 10-day event period) and the bottom cell is the test statistic $Test_{i,N}$. According to these results, the prices of 7 out of 8 Canadian firms traded below expectations 10 trading days after the panel report was circulated. Thus, nearly all the Canadian solar producers/providers were “under-performing” after the panel ruling. Again, Figure 1 provides an illustration of the difference.

We do not have strong theoretical priors about any specific firm *within* countries. Rather, we are interested in whether the 8 companies exhibited abnormal prices as a group. The statistic at the bottom of each column is the coefficient and standard error produced by regressing $c_{i,N}$ on a constant. The significant, negative coefficient confirms that, on average, observed prices were well below expectations. This suggests that the market responded significantly and consistently to the news that Canada had lost DS412 according to the panel report.

To probe the durability of this result, we employed a “jackknife” sampling approach comparable to the one used in Wilf (2013). We drop one firm at a time from the sample and test whether the collective significance disappears. This process confirms that no single firm is driving the overall

³⁸Models that poorly explain variation in observed prices will generate poor predictions of future prices, potentially leading to false inferences.

result; Canadian firms still collectively under-perform when dropping any individual company.

Interestingly, the results are more mixed following the appeal ruling. Only 3 of the 8 firms had prices below expectations, and while they were highly significant individually, a test of collective significance reveals no unified market response. The circulation of the appellate report therefore appears to have had little additional effect.

UNITED STATES. For comparison, and as a test of our methods, we now run the same analysis on a sample of solar firms in a “bystander” country, the US. The US, like India, joined as a third party in the dispute against Canada, suggesting that it had an interest in the outcome of the dispute. The US does have domestic support programs for solar energy, yet they do not take the form of a FiT scheme. Overall, US firms might stand to gain if competitors in Canada lost state support, yet any positive effect would be highly diluted across all firms competing with Canada, whether in the US or elsewhere.

The prices of US firms are reported in Table 3. Column 1 again shows the prices after the panel and Column 2 shows the prices after appeal. Most of the 6 firms in our sample enjoyed positive prices after both events. However, in the aggregate, we can reject the null hypothesis that these prices are statistically significant from zero. When taken as a whole, US firms appear to slightly outperform expectations absent the ruling, but the prices are ultimately not statistically abnormal following either the panel or the AB ruling.

INDIA. Next, we turn to our main question. Do markets sell off the shares of Indian firms upon hearing of the rulings against Canada? We run the same exercise as above on 11 publicly traded solar companies in India. The effect of the panel ruling on share prices are ambiguous (Column 1 in Table 4). A majority of the firms in the sample (7 of 11) did underperform, according to the model. However, when considered as a group, the share prices of firms in the Indian solar industry fall short of a statistically significant level of abnormality.

By contrast, the announcement of the Appellate Body ruling appears to have had a highly significant, negative effect on prices across the solar industry in India (Column 2 in Table 4). All

but one firm (10 of 11) was trading at values far below the predicted price in the wake of the appeal decision, and taken as a group, the industry as a whole shows statistically significant abnormal prices. Here again, the observed prices of Indian firms map well onto the model predictions over the estimation period. Figure 2 shows the close movement between the two series. Moreover, Figure 2 shows the departure of the observed prices from the predicted ones after the appeal decision, where we expect to see divergence.

Why might the AB effect on markets have been stronger than the panel ruling’s effect, as opposed to the case in Canada? We see two possible explanations. First, Appellate Body rulings, which frequently overturn panel reports, are widely seen as having broader authority than panel rulings, which have comparatively little precedential power. Insofar as one of these bodies might have spillover effects beyond a given ruling, it is the AB (Pauwelyn 2014). It follows that markets may draw no conclusions about the fate of Indian firms until the AB ruling is announced. Conversely, Canadian markets need not ponder any jurisprudential impact on subsequent cases to see that the panel ruling calls Canada’s scheme a violation of international law. A second possibility is that the US filing against India, which occurs between the announcement of the panel and AB report, played a priming role. It may be that the US filing was necessary to draw the market’s attention to the effects of other related rulings. In other words, a verdict against Canada may not have serious enough implications on the longevity of Indian measures to generate a market reaction, unless a legal challenge already exists that can capitalize on that ruling.

What remains certain is that the AB ruling against Canada generates an unambiguous reaction in Indian markets. Nonetheless, the absence of as strong a reaction following the panel ruling should be kept in mind when drawing broader conclusions. This discrepancy between the two rulings is in part what leads us to perform a second test set of tests, below, examining the effects of the rulings in *US—Cotton*. Our results there suggest that our first explanation of the distinction between AB and panel effects—highlighting the AB’s unique jurisprudential impact—may be the correct one.

4.1 Solar Energy Robustness Tests

The results presented in Tables 2, 3, and 4 are robust to a variety of estimation and event windows, as well as to the use of alternative indices to which one can compare the share price of individual firms. To probe the durability of the results further, we employ a related technique used in Jensen’s (2007) study of steel companies. Jensen runs OLS models with Newey-West standard error corrections on a time series of firms’ share prices. His variables are constructed as the day-to-day difference in logged prices, or:

$$\Delta Price_{i,t} = \log(p_{i,t}) - \log(p_{i,t-1})$$

The daily index prices are differenced in the same manner and the exchange rate is measured as a simple day-to-day change. The resulting model specification is:

$$\Delta Price_{i,t} = \beta_0 + \beta_1 * \Delta Index_t + \beta_2 * \Delta Exchange Rate_t + \beta_3 Panel Date_t + Appeal Date_t + \mu_{i,t}$$

where the two “dates” refer to dichotomous indicators of the panel and appeal dates, and where $\mu_{i,t}$ is the error term. As mentioned above, the virtue of this approach is building corrections for heteroskedasticity and autocorrelation into the model—traits common to economic series. Newey estimators require that users specify a lag and, in keeping with Jensen (2007), we use a lag of 10.³⁹

Table 5 reports the estimates for Newey models for firms in each country, taken as a group—Canada in column 1, the US in column 2, and India in column 3. Looking at the estimates, we see results highly consistent with the those found in the baseline analysis. The downward effects on Canada are even stronger here, showing that Canadian companies’ prices dropped unambiguously upon the circulation of both the panel and appeal rulings. Conversely, companies in the US gained on both days, suggesting that US firms were perceived to gain from the rulings. As in the event analysis, Indian companies are seen taking a significant negative hit following the appellate decision. And we observe a similar discrepancy between the panel and the AB rulings. In fact, Indian firms’ prices actually rise in the wake of the original panel report. It may be that the panel’s inability

³⁹The results are highly robust to different lags and we found no evidence that using 10 affected the estimates in any significant way.

to rule on the issue of “benefit conferred”, which is the claim that India would have been most concerned about, was greeted as good news for India’s own FiT program, while the AB ruling dispelled the notion that a subsequent panel could use the same reasoning to avoid ruling on the matter. In sum, the Appellate ruling against Canada has an unambiguous negative effect on Indian firms, yet we are still left with this discrepancy between the two rulings. In part to try and address it, we examine market reactions to another dispute.

5 The Cotton Dispute and US Commodity Prices

Our solar energy tests get at “cross-border” deterrence, examining the spillover effect of a ruling against one country on the same industry in another country. Would we observe the same market reaction if the spillover concerned an entirely different product? It could be that in the solar energy case, markets reacted to a perceived condemnation of all publicly supported solar energy schemes. By comparison, spillovers across different industries would require a more nuanced inference, one that takes a careful look at a ruling’s legal implications. To address this possibility, we examine the effects of another dispute: *US—Upland Cotton*.

We turn once again to financial markets, this time looking at commodities exchanges. This move comes with considerable analytical advantages. Firms invariably engage in a number of activities. In the solar case, some of the firms examined produce only solar panels, while others also e.g. process oil, muddying empirical expectations. By comparison, commodities represent a cleaner test: cotton futures traded on commodities exchanges concern only cotton.

Expectations also differ from the analysis considering stock prices. Whereas spillover effects of the ruling against Canada were expected to result in a *drop* in the valuation of Indian solar firms, the opposite expectation holds in *US—Cotton*: the threat of a removal of subsidies should lead to an *increase* in prices. Indeed, Brazil’s claim was precisely that the US measures depressed prices. It follows that if these measures were removed, prices should increase.

We use data from the Chicago Board of Trade and the United States Intercontinental Exchange to obtain spot-month futures prices calculated on a continuous daily basis for both cotton and wheat. To predict these prices in the estimation window, we use the daily US/Euro exchange

rate, gold prices, oil prices, and the S&P500 index. Taken together, these appear apt predictors of both cotton and wheat prices, explaining almost 70% of the daily variation during the estimation period.⁴⁰ As before, we use this estimated relationship to predict what the futures prices should be during the event period, were it not for the WTO rulings. We then calculate the difference between the predicted and the actual price, and test the statistical significance of this difference to look for “abnormality”.

The results, presented in Table 12, provide additional support for the theory. Strikingly, not only do the findings support the view of the Cotton ruling affecting wheat prices, but we observe a similar pattern to the solar energy case. Futures prices for the disputed product (cotton) show a highly significant abnormal increase in the wake of the panel ruling. In other words, investors predicted that following the ruling, the removal of the subsidy might drive the suppressed price up to market price levels. And while the valuation of wheat futures also increased abnormally following the panel report, this increase is only significant at the 10% level.

Conversely, following the Appellate Body report, which did little but uphold the panel’s findings, both commodities saw an abnormal price increase, but the increase for the spillover commodity (wheat) was statistically and substantively more significant than for the disputed commodity (cotton). Figure 3 illustrates this relationship. Note that the observed and predicted prices of wheat are closely related in the estimation window, again giving us confidence in the model predictions.⁴¹ In the wake of the appeal announcement, there is a sharp divergence from the predicted price absent the event, as wheat prices climb steadily, reflecting the bet that subsidies may be threatened. When we test these effects using a Newey estimation, as above, we obtain highly consistent results.

In other words, the same picture emerges as in the solar energy case: the panel ruling affects the underlying country/industry, while the AB ruling is what registers with the spillover country/industry. This should serve to bolster our explanation, in view of the solar findings, of panel vs. AB rulings. It may be that while markets draw conclusions about the disputed country/product from the panel report, spillovers to other cases are contingent on the AB report: markets do not apply the ruling elsewhere until they observe the AB’s verdict. In this view, panel reports by

⁴⁰The r^2 for the estimation window regression for the panel report is 0.64 for cotton and 0.70 for wheat.

⁴¹The r-squared from the model predicting wheat is 0.35 and from cotton it is 0.37.

themselves carry little precedential impact, formal or otherwise. Pushing this reasoning further, perhaps it is precisely the existence of a standing body of judges such as the AB that allows for spillovers to operate. After all, the related notion that *de facto* binding precedent operates in the trade regime only emerges with the creation of the Appellate Body.⁴²

Overall, the tests on *US—Cotton* support the view that WTO rulings have spillovers, as inferred from the behavior of both stock markets and commodity exchanges. In this case, investors bet that if wheat production benefits from a scheme similar to the disputed cotton subsidies, it may eventually lose part of its governmental support. As a result, investors start betting on an increase in wheat prices.

6 Conclusion

The promise of international courts depends in large measure on their ability to have effects beyond a given case and the parties to it. This hope is visible in much of the international legal literature. Human rights scholars, especially, have championed the creation in recent decades of a number of international courts prosecuting human rights violations. The premise is that such courts can not only provide retribution in the cases they prosecute, but also go some way in preventing subsequent violations from occurring.

This premise comes up against considerable resistance among some legal observers, as well as international legal texts themselves. Countries wary of delegating too much control to unelected judges over the meaning of international law have consciously sought to limit the reach of verdicts. The result, looking at formal rules, is considerable ambiguity over whether courts have any means of deterring violations beyond the case at hand.

This article addresses the question of deterrence in the issue-area of international trade, where there exists a similar scholarly debate over spillovers from rulings. International trade presents several analytical benefits over other legal settings. Since the outcomes of trade rulings ultimately register on firms and prices, we consider the reactions of financial markets to the announcements of trade rulings. We ask, do investors bet that governments grow less likely to maintain policies

⁴²See e.g. Bhala (1998-1999).

shown to be in violation elsewhere?

We first consider a dispute targeting government support schemes of solar energy projects in Canada. We find that financial markets in India, where solar firms benefit from a support scheme highly similar to Canada's, sell off shares of these firms when the WTO finds the Canadian support scheme to be noncompliant. In other words, Indian markets bet on the WTO ruling against Canada jeopardizing government support offered to firms in India. We then perform a similar exercise on a different case, asking whether a ruling against US cotton leads investors to bet that similar subsidies on wheat may be threatened. In both cases, we observe investors betting that a dispute has effects beyond its formal confines.

The analysis also reveals systematic variation in the effects of lower vs. higher court rulings. Specifically, in both the solar energy and the cotton cases, Appellate Body rulings appear to carry a stronger deterrent effect than panel rulings. This may carry implications for the scope conditions for deterrence. The unique feature of the AB is that it is a standing body of judges, similar to a domestic supreme court. This differs from human rights courts, and especially investment tribunals, which tend to be far more *ad hoc*. The findings thus suggest caution in readily applying the lessons from trade directly to other regimes. The lesson from our analysis, rather, is that spillovers from international legal rulings can result even in the face of rules that explicitly claim otherwise.

Finally, financial markets provide us with an overlooked means of gauging aggregate expectations in cases where observers disagree on the presence of an effect. One can ask whether markets believe sufficiently in a causal effect—in this case, the effect of a legal ruling on trade measures in a foreign country or a different industry—to put money on it. In regards to the question of whether international courts can deter, financial markets suggest the answer is a cautious yes.

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Table 1: Companies Included in Sample

Company Name	Index
Canadian Firms	
Aecon Group	Toronto (TSX)
Alterra	Toronto (TSX)
Boralex	Toronto (TSX)
Capstone	Toronto (TSX)
Carmanah	Toronto (TSX)
Hammond Power	Toronto (TSX)
Innergex	Toronto (TSX)
Northland Power	Toronto (TSX)
US Firms	
Ascent Solar	New York (NASDAQ)
First Solar	New York (NASDAQ)
Green Plains	New York (NASDAQ)
Real Goods Solar	New York (NASDAQ)
Sun Edison	New York (NYSE)
SunPower	New York (NASDAQ)
Indian Firms	
Bharat Heavy Electricals	Bombay (BSE)
HBL Power	Bombay (BSE)
Indian Oil Company	Bombay (BSE)
Indosolar	Bombay (BSE)
Jain Irrigation	Bombay (BSE)
Lanco Infratech	Bombay (BSE)
Maharashtra Seamless	Bombay (BSE)
Punji Lloyd	Bombay (BSE)
Solar Industries India	Bombay (BSE)
Sujana Towers	Bombay (BSE)
Tata Power	Bombay (BSE)

Table 2: Prices for Canadian Firms

Model	(1) Panel	(2) Appeal
Est. Window / Event Window Index	50 / 10 Toronto	50 / 10 Toronto
Aecon Group	-3.588 (-2.675)	-14.149 (-11.707)
Alterra	-0.155 (-2.604)	0.368 (13.100)
Boralex	-8.530 (-19.437)	1.870 (1.717)
Capstone	-0.345 (-1.709)	-2.542 (-7.595)
Carmanah	-0.422 (-10.260)	0.218 (6.285)
Hammond Power	-1.594 (-7.180)	-20.005 (-27.684)
Innergex	-2.594 (-6.909)	3.861 (8.027)
Northland Power	-2.976 (-5.619)	8.418 (13.611)
Test of significance	-2.244** (0.903)	-2.440 (2.988)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * $p < 0.100$ and ** $p < 0.050$.

Figure 1: Canadian Solar Firm Prices after Panel Ruling: Average Prices Across Industry

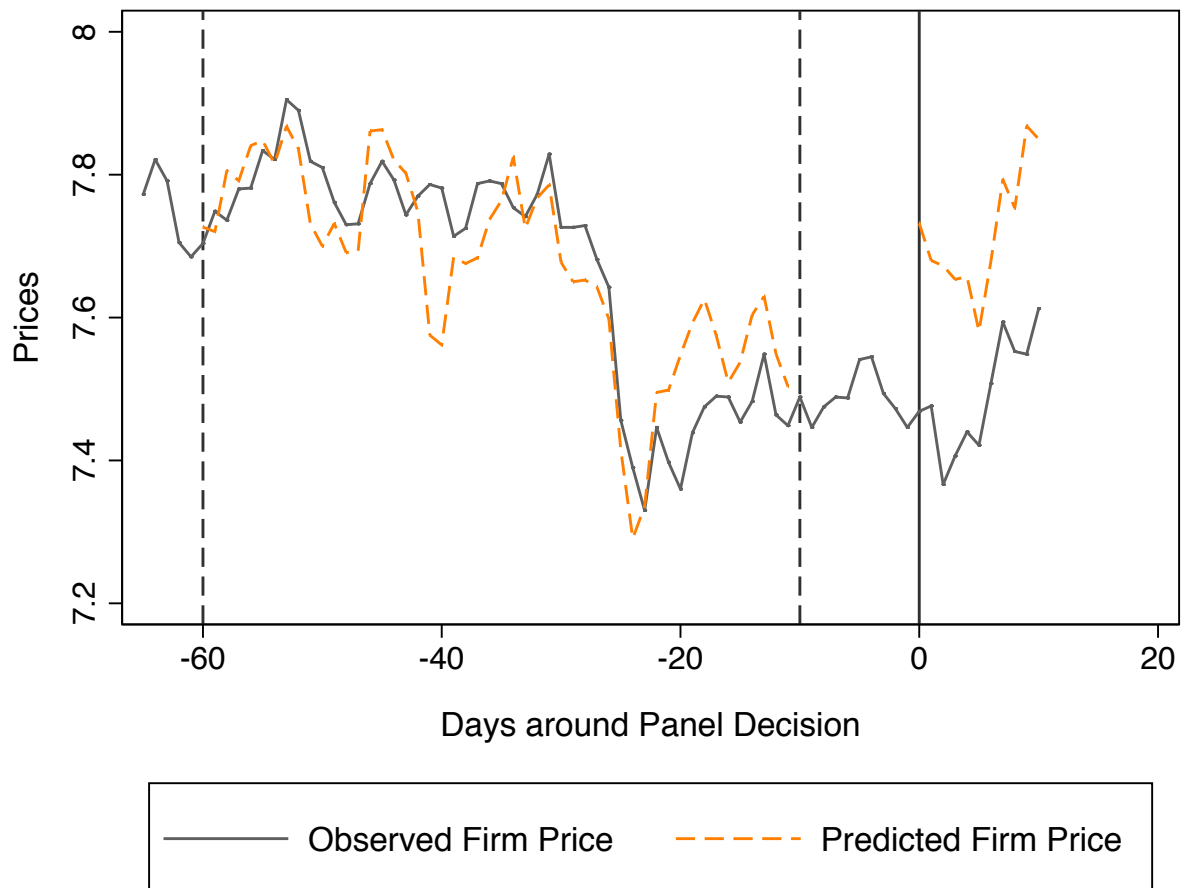


Table 3: Prices for US Firms

Model	(3) Panel	(4) Appeal
Est. Window / Event Window Index	50 / 10 New York	50 / 10 New York
Ascent Solar	-2.805 (-12.062)	2.414 (10.045)
First Solar	90.057 (13.417)	223.635 (20.432)
Green Plains	14.980 (9.505)	-5.776 (-3.396)
Real Goods Solar	2.375 (8.381)	12.366 (2.622)
Sun Edison	6.636 (14.479)	15.846 (7.925)
SunPower	18.634 (4.316)	75.969 (9.346)
Test of significance	18.553 (12.276)	46.350 (31.315)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * $p < 0.100$ and ** $p < 0.050$.

Table 4: Prices for Indian Firms

Model	(5) Panel	(6) Appeal
Est. Window / Event Window Index	50 / 10 Mumbai	50 / 10 Mumbai
Bharat Electricals	-200.177 (-27.353)	-276.647 (-16.255)
HBL Power	10.499 (7.758)	-53.370 (-21.429)
Indian Oil Company	9.320 (0.598)	-303.764 (-17.534)
Indosolar	-3.336 (-9.337)	-18.623 (-24.020)
Jain Irrigation	41.345 (9.516)	-18.150 (-3.094)
Lanco Infratech	-13.064 (-7.319)	-18.702 (-16.516)
Maharashtra Seamless	-507.713 (-58.707)	-74.384 (-10.736)
Punji Lloyd	32.168 (10.864)	85.967 (24.619)
Solar Industries India	-297.225 (-9.145)	-683.926 (-4.310)
Sujana Towers	-11.593 (-0.616)	-335.754 (27.187)
Tata Power	-8.980 (-2.125)	-60.193 (25.425)
Test of significance	-86.250 (52.889)	-159.776** (66.667)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * $p < 0.100$ and ** $p < 0.050$.

Figure 2: Indian Solar Firm Prices after Appellate Ruling: Average Prices Across Industry



Table 5: Newey Estimates

	(7) Canada	(8) US	(9) India
$\Delta Index_t$	0.373*** (0.048)	1.787*** (0.138)	0.544*** (0.046)
$\Delta Exchange Rate_t$	-8.517 (5.690)	44.778* (26.006)	-0.115 (0.102)
$Panel_t$	-0.179*** (0.037)	2.966*** (0.168)	0.311*** (0.049)
$Appeal_t$	-0.142*** (0.031)	2.785*** (0.177)	-0.414*** (0.049)
Constant	0.022 (0.027)	-0.070 (0.155)	-0.059 (0.038)
N	479	479	479

Newey-West standard errors in parentheses * $p < 0.100$, ** $p < 0.050$, *** $p < 0.001$

Table 6: Returns for Canadian Firms Across Different Windows (Panel Date)

Est. Window / Event. Window Index	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	50 / 10 Toronto	80 / 10 Toronto	50 / 10 Toronto	80 / 10 Toronto	50 / 10 Toronto	80 / 10 Toronto	50 / 10 Industry	80 / 10 Industry	50 / 10 Industry	80 / 10 Industry	50 / 10 Industry	80 / 10 Industry	50 / 10 Industry	80 / 10 Industry	50 / 10 Industry	80 / 10 Industry
Aecon Group	-3.588 (-2.675)	-1.635 (-1.181)	-0.852 (-2.682)	-0.575 (-1.633)	-5.009 (-3.026)	-12.427 (-11.783)	-0.898 (-3.466)	-2.091 (-33.543)								
Alterra	-0.155 (-2.604)	-0.293 (-4.567)	-0.026 (-1.383)	-0.050 (-2.556)	-0.187 (-2.910)	-0.701 (-16.180)	-0.026 (-1.540)	-0.107 (-12.687)								
Boralex	-8.530 (-19.437)	-7.119 (-13.623)	-1.405 (-14.138)	-1.166 (-10.544)	-8.409 (-18.547)	-5.826 (-10.495)	-1.383 (-13.406)	-0.988 (-11.127)								
Capstone	-0.345 (-1.709)	-0.359 (-1.827)	-0.238 (-5.922)	-0.243 (-7.902)	-0.559 (-3.188)	-1.837 (-10.228)	-0.241 (-8.031)	-0.450 (-37.524)								
Carmanah	-0.422 (-10.260)	0.031 (0.523)	-0.108 (-40.757)	-0.039 (-3.088)	-0.446 (-11.150)	-0.185 (-3.153)	-0.111 (-42.364)	-0.068 (-6.396)								
Hammond Power	-1.594 (-7.180)	-1.212 (-6.260)	-0.289 (-25.617)	-0.238 (-5.892)	-1.308 (-4.731)	-1.680 (-10.126)	-0.254 (-25.460)	-0.304 (-10.985)								
Innergex	-2.594 (-6.909)	-2.870 (-8.022)	-0.399 (-4.126)	-0.445 (-5.151)	-2.750 (-7.631)	-4.124 (-13.721)	-0.402 (-4.504)	-0.621 (-11.781)								
Northland Power	-2.976 (-5.619)	-0.231 (-0.401)	-0.417 (-8.197)	0.023 (0.599)	-3.076 (-6.609)	-1.464 (-2.265)	-0.394 (-5.768)	-0.153 (-1.927)								
Test of significance	-2.244** (0.903)	-1.521* (0.768)	-0.414** (0.151)	-0.303** (0.128)	-2.416** (0.932)	-3.138** (1.321)	-0.412** (0.151)	-0.531** (0.221)								

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * p<0.100 and ** p<0.050.

Table 7: Returns for Canadian Firms Across Different Windows (Appeal Date)

Est. Window / Event. Window Index	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	50 / 10 Toronto	80 / 10 Toronto	50 / 1 Toronto	80 / 1 Toronto	50 / 10 Industry	80 / 10 Industry	50 / 1 Industry	80 / 1 Industry
Aecon Group	-14.149 (-11.707)	-4.804 (-4.823)	-1.288 (-10.144)	0.161 (1.391)	-14.334 (-11.604)	-6.259 (-6.469)	-1.284 (-10.012)	-0.146 (-1.279)
Alterra	0.368 (13.100)	-0.278 (-12.163)	0.052 (138.008)	-0.044 (-79.331)	0.418 (11.740)	-0.071 (-1.903)	0.059 (142.903)	-0.007 (-12.799)
Boralex	1.870 (1.717)	8.634 (7.733)	0.219 (16.882)	1.236 (55.591)	1.568 (1.359)	6.900 (5.402)	0.132 (9.125)	0.860 (34.387)
Capstone	-2.542 (-7.595)	-2.117 (-6.461)	-0.386 (-7.694)	-0.307 (-6.112)	-2.462 (-7.504)	-1.812 (-5.653)	-0.376 (-7.501)	-0.273 (-5.532)
Carmanah	0.218 (6.285)	0.188 (5.536)	0.014 (52.778)	0.010 (30.784)	0.247 (7.201)	0.266 (7.560)	0.017 (81.290)	0.019 (127.034)
Hammond Power	-20.005 (-27.684)	-8.881 (-18.765)	-3.055 (-47.012)	-1.381 (-17.265)	-20.053 (-27.272)	-10.933 (-22.151)	-3.037 (-47.445)	-1.790 (-21.856)
Innergex	3.861 (8.027)	1.835 (4.680)	0.537 (16.368)	0.246 (6.790)	4.178 (8.214)	2.783 (6.232)	0.577 (17.794)	0.395 (11.109)
Northland Power	8.418 (13.611)	7.558 (11.647)	1.817 (258.903)	1.727 (179.857)	8.362 (12.981)	8.105 (11.819)	1.864 (203.764)	1.876 (160.988)
Test of significance	-2.440 (2.988)	0.273 (1.829)	-0.232 (0.445)	0.183 (0.183)	-2.453 (3.003)	-0.113 (1.982)	-0.227 (0.447)	0.103 (0.325)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * p<0.100 and ** p<0.050.

Table 8: Returns for US Firms Across Different Windows (Panel Date)

Est. Window / Event. Window Index	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	50 / 10 New York	80 / 10 New York	50 / 1 New York	80 / 1 New York	50 / 10 Industry	80 / 10 Industry	50 / 1 Industry	80 / 1 Industry
Ascent Solar	-2.805 (-12.062)	-5.442 (-11.394)	-0.434 (-21.917)	-0.949 (-35.781)	-5.577 (-18.305)	-11.695 (-14.393)	-0.900 (-63.626)	-1.997 (-141.190)
First Solar	90.057 (13.417)	92.963 (14.242)	18.658 (36.805)	19.159 (37.420)	85.139 (17.379)	100.090 (16.887)	17.538 (31.799)	20.224 (36.668)
Green Plains	14.980 (9.505)	20.582 (11.970)	3.338 (46.409)	4.376 (57.811)	19.096 (19.612)	34.657 (15.866)	3.916 (92.296)	6.707 (158.091)
Real Goods Solar	2.375 (8.381)	1.347 (5.324)	0.434 (156.980)	0.241 (63.471)	0.760 (3.220)	-1.125 (-6.409)	0.160 (10.305)	-0.179 (17.400)
Sun Edison	6.636 (14.479)	5.476 (10.476)	1.247 (156.922)	1.039 (64.688)	1.606 (9.143)	3.007 (15.166)	0.364 (10.305)	0.615 (17.400)
SunPower	18.634 (4.316)	18.476 (4.299)	1.768 (31.524)	1.735 (31.382)	12.730 (3.191)	15.596 (3.717)	0.752 (11.814)	1.266 (19.888)
Test of significance	18.553 (12.276)	19.057 (12.841)	3.572 (2.558)	3.657 (2.662)	16.250 (11.913)	20.075 (14.465)	3.118 (2.470)	3.805 (2.922)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * p<0.100 and ** p<0.050.

Table 9: Returns for US Firms Across Different Windows (Appeal Date)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Est. Window / Event. Window Index	50 / 10 New York	80 / 10 New York	50 / 1 New York	80 / 1 New York	50 / 10 Industry	80 / 10 Industry	50 / 1 Industry	80 / 1 Industry
Ascent Solar	2.414 (10.045)	1.216 (5.848)	0.368 (31.343)	0.197 (15.196)	0.847 (3.992)	0.987 (4.478)	0.125 (6.472)	0.138 (6.714)
First Solar	223.635 (20.432)	212.611 (20.343)	33.280 (11.129)	31.747 (10.573)	58.232 (9.342)	190.922 (20.398)	13.531 (6.714)	28.779 (9.763)
Green Plains	-5.776 (-3.396)	-3.925 (-2.296)	-0.347 (-4.103)	0.048 (0.576)	2.223 (1.090)	-13.778 (-4.264)	1.676 (4.693)	-0.012 (-0.024)
Real Goods Solar	12.366 (2.622)	-2.654 (-0.635)	0.833 (14.362)	-1.238 (-16.425)	5.183 (1.168)	-10.058 (-2.674)	-0.146 (-2.814)	-1.853 (-30.939)
Sun Edison	15.846 (7.925)	3.573 (2.466)	2.000 (38.634)	0.270 (1.664)	-1.321 (-1.375)	-7.958 (-8.892)	-0.084 (-4.446)	-0.840 (-12.722)
SumPower	75.969 (9.346)	29.243 (5.184)	8.304 (26.384)	1.855 (5.031)	37.103 (8.008)	2.019 (1.123)	3.889 (213.275)	-0.025 (-0.105)
Test of significance	46.350 (31.315)	34.295 (30.025)	6.348 (4.627)	4.697 (4.521)	14.610 (8.874)	23.152 (28.054)	2.713 (1.887)	3.741 (4.181)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * p<0.100 and ** p<0.050.

Table 10: Returns for Indian Firms Across Different Windows (Panel Date)

	(1)	(2)	(3)	(4)
Est. Window / Event. Window Index	50 / 10 Bombay	80 / 10 Bombay	50 / 1 Bombay	80 / 1 Bombay
Bharat Electricals	-200.177 (-27.353)	-200.423 (-29.330)	-33.608 (-52.181)	-33.700 (-55.748)
HBL Power	10.499 (7.758)	7.005 (5.206)	1.120 (1.958)	0.466 (0.794)
Indian Oil Company	9.320 (0.598)	3.842 (0.252)	-5.603 (-4.759)	-6.635 (-5.784)
Indosolar	-3.336 (-9.337)	-2.991 (-8.577)	-0.405 (-16.413)	-0.341 (-13.028)
Jain Irrigation	41.345 (9.516)	88.427 (26.170)	9.327 (30.871)	18.105 (153.449)
Lanco Infratech	-13.064 (-7.319)	-12.258 (-6.795)	-4.001 (-76.392)	-3.852 (-76.664)
Maharashtra Seamless	-507.713 (-58.707)	-530.580 (-60.635)	-94.000 (-93.146)	-98.238 (-91.199)
Punji Lloyd	32.168 (10.864)	42.203 (12.371)	5.441 (6.454)	7.315 (9.128)
Solar Industries India	-297.225 (-9.145)	-151.234 (-4.537)	-34.135 (-12.298)	-6.872 (-2.032)
Sujana Towers	-11.593 (-0.616)	-87.286 (-4.017)	14.598 (21.182)	0.457 (1.237)
Tata Power	-8.980 (-2.125)	17.840 (3.906)	-4.887 (-6.534)	0.103 (0.159)
Test of significance	-86.250 (52.889)	-75.041 (52.256)	-13.287 (9.337)	-11.199 (9.484)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * $p < 0.100$ and ** $p < 0.050$.

Table 11: Returns for Indian Firms Across Different Windows (Appeal Date)

	(1)	(2)	(3)	(4)
Est. Window / Event. Window Index	50 / 10 Bombay	80 / 10 Bombay	50 / 1 Bombay	80 / 1 Bombay
Bharat Electricals	-276.647 (-16.255)	-417.213 (-17.890)	-38.572 (-20.307)	-60.176 (-17.623)
HBL Power	-53.370 (-21.429)	-79.132 (-20.716)	-8.176 (-15.773)	-12.138 (-15.275)
Indian Oil Company	-303.764 (-17.534)	-105.215 (-5.821)	-59.424 (-12.099)	-28.340 (-9.482)
Indosolar	-18.623 (-24.020)	-18.753 (-25.272)	-3.014 (-72.296)	-3.056 (-88.418)
Jain Irrigation	-18.150 (-3.094)	-111.395 (-11.066)	2.295 (136.198)	-11.864 (-10.906)
Lanco Infratech	-18.702 (-16.516)	-31.197 (-17.997)	-2.303 (-25.257)	-4.214 (-18.299)
Maharashtra Seamless	-74.384 (-10.736)	-345.289 (-23.832)	-16.282 (-4.364)	-58.064 (-8.803)
Punji Lloyd	85.967 (24.619)	3.702 (0.835)	14.793 (9.963)	2.234 (3.953)
Solar Industries India	-683.926 (-4.310)	-547.485 (-3.521)	-36.164 (-32.499)	-14.309 (-6.374)
Sujana Towers	-335.754 (27.187)	-456.044 (24.985)	-56.605 (27.907)	-75.238 (23.013)
Tata Power	-60.193 (25.425)	-137.954 (21.559)	-9.399 (34.389)	-21.234 (18.365)
Test of significance	-159.776** (66.667)	-204.189** (59.736)	-19.350** (7.429)	-26.036** (7.963)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level. The last row reports the coefficient and standard error from a regression testing whether the abnormal performance of the firms taken as a group is significantly different from zero. Here, * $p < 0.100$ and ** $p < 0.050$.

Table 12: Commodity Futures Prices, *US—Upland Cotton*

Model	(1) Panel	(2) Appeal
Est. Window / Event Window	50 / 10	50 / 10
Cotton	20.86 (3.14)	13.71 (4.39)
Wheat	63.12 (1.65)	243.04 (4.71)

Test statistics in parenthesis. Absolute values greater than 1.96 represent a significant difference from zero at the 5% level.

Figure 3: Wheat Prices after Appeal Decision in *US—Cotton*

