How Effective is Trade Conditionality? Economic Coercion in the Generalized System of Preferences

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June 2022

Word Count: 9,697

Abstract

The Generalized System of Preferences (GSP) exemplifies the ways in which international economic linkages can become conduits of political influence. The program offers beneficiary developing countries the opportunity to export a wide variety of goods duty free to the United States, but eligibility is conditional on labor and intellectual property rights protections. How effective are programs like the GSP at causing states to change behavior by raising the risk that market access will be revoked? The challenge of detecting economic coercion in programs like the GSP is identifying the program's influence in states where the conditionality is never enforced. The paper applies a new conceptual approach to show that GSP beneficiaries change policy to reduce the risk of being threatened with expulsion from the program. An implication of the findings is that the political consequences of economic linkages could be far more widespread than previously thought.

Introduction

In an address at the Minneapolis Fair in 1901, then Vice President Theodore Roosevelt deployed an aphorism to describe his foreign policy vision: "Speak softly and carry a big stick." Roosevelt meant that powerful states are better served by making threats quietly than by publicly and haphazardly issuing ultimatums. Applied to the domain of economic coercion, the principle suggests that states may prefer

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to insinuate that their trading partners must adhere to certain criteria before they directly threaten any particular state with economic sanctions. Indeed, states do sometimes strongly imply that punishment is possible without threatening any particular state. A modern example is the United States' Generalized System of Preferences (GSP), which offers beneficiary developing states the opportunity to export thousands of products duty free to the US as long as they protect labor and intellectual property rights. How effective are programs of economic coercion like the GSP at causing states to change political behavior by raising the risk of market access being revoked?

The article finds that the GSP is effective at promoting rights protections in states that are neither under a formal compliance review nor actually expelled from membership. To reach this conclusion, the paper develops a conceptual framework that distinguishes between general threats to revoke market access from any noncompliant state and immediate threats to revoke market access from a particular noncompliant state. Acknowledging both types of threats is important because states typically only receive immediate threats after violating a general threat. This study constructs a research design representing the GSP as a general threat: a commitment to expel from membership any developing state which cannot demonstrate sufficient rights protections. The results of the empirical analysis suggest that the program causes states to change behavior even before being directly threatened, which could mean that trade conditionality is far more effective than previously thought.

The core challenge of studying the effectiveness of the GSP is similar to a well known difficulty in the study of economic sanctions. Many scholars of economic sanctions have reckoned with the difficulty of strategic selection, or the idea that sanctions are only ever imposed when the threat of sanctions was ineffective at changing behavior (Nooruddin 2002; Drezner 2003; Bapat and Kwon 2015). Strategic selection also affects the GSP because states are only expelled from the program if the threat of expulsion was not enough to motivate compliance. This article contends that the strategic selection problem in both cases is not a methodological challenge at all but actually a theoretical one. More specifically, strategic selection is an artifact of how economic coercion has been studied as a series of episodic events. Datasets that define observations as either definite threats to revoke market access or as the actual imposition of sanctions are necessarily incomplete. They definitionally exclude cases where states might increase compliance to reduce the risk of losing access to important markets without being directly threatened. In other words, these datasets necessarily miss the cases where target states take steps to meet the softly spoken conditionality of a sender state wielding its markets as a big stick.

The research design in this article is capable of estimating the effect of the GSP on states that are never directly threatened with expulsion. Estimating the effectiveness of the GSP in this group means comparing the compliance of state-years that benefitted more from GSP membership with those that benefitted less. For a source of exogenous variation in the value of GSP membership the study relies on changes in the GSP preference margin following multilateral trade negotiations in the early and mid 1990s (Martin, Winters, and Winters 1996; Finger, Ingco, and Reincke 1996; Hathaway, Ingco, and others 1996; Harrison, Rutherford, and Tarr 1997). As tariffs fall the benefits to GSP membership also fall (Alexandraki and Lankes 2004; Francois, Hoekman, and Manchin 2006; Amiti and Romalis 2007). The exogeneity of this measure depends on product level variation in the depth of tariff cuts at multilateral negotiations. By comparing states that trade in products that faced steeper cuts with those that did not it is possible to estimate plausibly exogenous variation in the value of GSP membership (Angrist and Lavy 1999; Angrist and Pischke 2008, 2010).

The distinction between general and immediate sanctions threats also illuminates why economic coercion is not always perceived as credible. General sanctions threats are issued because it is difficult to predict which specific states will be noncompliant in the future. When the scope of general threats include a large number of potential targets some may question whether the threat applies to them specifically – for example, the sender state may hesitate before imposing penalties on close trading partners. Immediate threats issued without a general threat might also lack credibility – would the sender act in a discriminatory manner against one state without acknowledging the behavior of other states? But an immediate threat issued in the context of an existing general threat is subject to less ambiguity because they inform a particular target state about an impending penalty. Thus, there are three types of target states: those that comply with the general threat because they found it credible; those that spurn the general threat but come into compliance following an immediate threat; and finally those that are unbothered by the economic consequences of ignoring both threats. The allotment of states to each category is an empirical question.

While the GSP has been previously studied as a tool for promoting development, relatively few studies have examined its ability to change the behavior of its beneficiary states.[1] The most closely related paper is Hafner-Burton, Mosley, and Galantucci (2018). The authors find that the United States enforces its conditionality sincerely, but stop short of asking whether the program changes the behavior of beneficiaries. Virtually the entire quantitative literature on sanctions defines observations as episodes but a few scholars studying economic coercion outside of sanctions have taken other approaches. Carnegie (2014) searches for evidence of "political hold-up problems" by examining trade flows at the dyadic level. The paper whose methodology comes closest to following the protocol developed in this paper is Carnegie and Marinov (2017), which uses quasi-experimental variation in the European Union's development aid allocations to identify its effect on human rights and democracy promotion. This paper advances the literature by using a case study of the GSP to study how states react to the risk of economic coercion even before being directly threatened.

Background on the Generalized System of Preferences

The remainder of this article will demonstrate the value and feasibility of the above protocol with an application to the United States's Generalized System of Preferences (GSP). More than 130 states are eligible for the US GSP which has been conditional on policy choices since 1984.[2] Thousands of products can be imported tariff free from eligible beneficiary states. Many developing states depend on the program for access to US markets while others benefit relatively little. The left panel of Figure 1 shows that there is always a significant number of states exporting more than 30% of their total exports to the US under the GSP. The right panel of Figure 1 illustrates the number of states exporting whose GSP

[1] Shushanik Hakobyan has done the most systematic work analyzing the program's general effectiveness, finding that beneficiary states export significantly more under the program and that the program is most valuable when the preference margin and the share of value added in output are high (Hakobyan 2015, 2017; Blanchard and Hakobyan 2015). Blanchard and Matschke (2015) provide evidence that the GSP also stimulates offshoring and increased trade through foreign direct investment.

[2] The program has been authorized since the late 1970s but it has only been conditional on respect for both labor and intellectual property rights since 1984. For more details on the history and administration of the program see Office of the United States Trade Representative (2017), United Nations Conference on Trade and Development (2016), Stamberger (2003), and Compa and Vogt (2000).

exports to the US account for meaningful fractions of their gross domestic product. These numbers can be significant because even small percentages of GDP can be very politically important depending on the degree of industry concentration. Even while the GSP is a small fraction of total US imports it is likely that certain industries face stiff import competition under the program.[3]

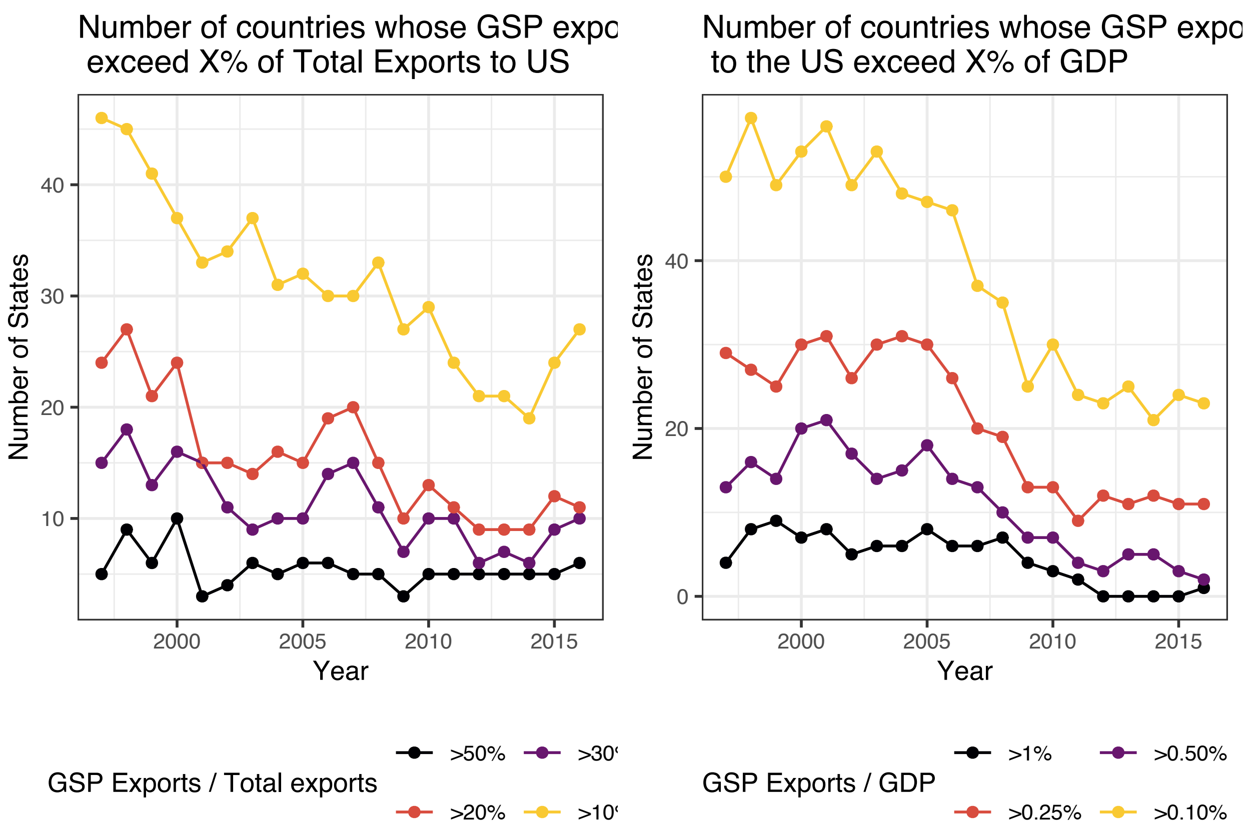

Figure 1: Data from Schott. Left panel: Note the logarithmic scale. The fraction of total US imports covered by the GSP is consistently small. Right panel: The number of states exporting at least a certain fraction of their total exports to the US under the GSP is shown over time. For example, in 2000 there were 30 states for which at least 10% of their total exports to the US crossed the border under the program.

If a state fails to meet the conditionality requirements it is not immediately removed from the GSP. Watchdog groups like the AFL-CIO can file a petition with the USTR alleging the noncompliance of a specific beneficiary. If the USTR accepts the petition a review process evaluating the compliance of the beneficiary will begin. This review process involves public hearings where representatives from the

[3]A US manufacturer of sleeping bags lobbied then Senator Jeff Sessions in 2010 to remove from eligibility certain sleeping bags from Bangladesh because of import competition faced by their firm. The entire program was temporarily suspended over the issue in 2011.

petitioner and the beneficiary argue their respective cases before the GSP Subcommittee, which is the policymaking authority for the program. The GSP Subcommittee, which is an interagency group with representatives from multiple departments, will make a recommendation to the USTR regarding the beneficiary's continued GSP eligibility. The USTR then relays the recommendation to the President for final approval. Only in the rarest of cases does the recommendation of the GSP Subcommittee not ultimately become policy.

Theory and Concepts

The core challenge to evaluating the effectiveness of the GSP is strategic selection: the states that are expelled from the GSP are precisely those that were indifferent to the threat of expulsion. Even if states rarely increase compliance after expulsion from membership the program could still be quite effective at motivating current members to maintain strong compliance. Any states that are motivated to change political behavior by the GSP would probably take steps to increase compliance if faced with a formal review. Indeed, the challenge of strategic selection also applies to the threat stage: states that maintain rights protections because of the GSP incentives might never be formally reviewed. These issues are not unique to the GSP. In fact, the literature on sanctions has engaged with similar strategic selection problems. This section will develop a conceptual framework that properly accounts for strategic selection at the threat and enforcement stages. The framework can be applied to trade conditionality programs like the GSP as well as programs of economic sanctions.

Strategic selection is the specter lurking beneath virtually all studies of economic coercion. The problem is that punishments are only observable when the *threat* of punishment did not produce the desired outcome. Thus, datasets of sanctions include only the cases where the target is least likely to make concessions. Cases where concessions are made after punishment must be interpreted in a completely different strategic context. For example, the target state might have decided to force the sender to prove their resolve to impose sanctions due to poor initial information about the sender's intentions. Under that model, punishments actually applied are best understood as a consequence of asymmetric information about the relative resolve of the states to endure the costs of punishment. Understanding the strategic context of economic coercion is absolutely essential to understanding its role in the globalized economy.

The strategic selection problem is well known in the literature (Baldwin 1985; Martin 1994; Morgan and Miers 1999; Drezner 2003; Lacy and Niou 2004; Bapat and Morgan 2009; Morgan, Bapat, and Krustev 2009; Morgan, Bapat, and Kobayashi 2014) Strategic selection and its consequences for inference in the context of sanctions was first formalized by Eaton and Engers (1992) and Smith (1995) but were also recognized by Baldwin (1985). Early studies interpreted evidence that sanctions were unable to achieve their goals as evidence that sanctions were ineffective or effective only under narrow conditions (Hufbauer and Schott 1985; Lindsay 1986; Hufbauer, Schott, and Elliott 1990; Pape 1997, 1998; Elliott 1998). Drezner (2003) and Lacy and Niou (2004) called on scholars to mitigate the problem by studying threats to impose sanctions. The call was answered by Morgan, Bapat, and Krustev (2009) and Morgan, Bapat, and Kobayashi (2014) who produced the Threat and Imposition of Economic Sanctions (TIES) dataset which has served as the most important resource for sanctions studies ever since. Perhaps the culmination of this research program is represented by Whang, McLean, and Kuberski (2013) which combines a model similar to that of Lacy and Niou (2004) with the TIES data to structurally estimate the importance of the sanctions threat stage.[4]

I propose that the strategic selection effect is not inherent to the nature of economic coercion but rather a consequence of research design choices driven by incomplete theoretical development. Specifically, it is an artifact of how economic coercion is conceptualized as an episodic phenomenon. Regardless of whether episodes are defined to begin with the threat or the punishment itself, datasets of sanctions episodes are a necessarily incomplete picture of the sanctions process. No reasonable definition of a sanctions episode could include cases where states changed behavior because of the risk of economic coercion, which is the origin of the selection problem. This section first introduces the concepts of general and immediate sanctions threats and explains how they relate to the selection problem. Then, it stipulates how studies can sidestep issues of strategic selection by accounting for

[4]Other important studies using the TIES data to account for immediate sanctions threats include Bapat et al. (2013), Bapat and Kwon (2015), Grauvogel, Licht, and Soest (2017), and Morgan and Kobayashi (2021). The recently released Global Sanctions Database described by Felbermayr et al. (2020) has an episodic structure but does not include sanctions threats and is therefore not appropriate to the study of the strategic selection problem.

general sanctions threats.

General and Immediate Sanctions Threats

Strategic selection problems, while particularly problematic to the study of economic coercion, are not unique to this domain. Fearon (2002) studied strategic selection in the context of general and immediate deterrent military threats.[5] His analysis describes an interaction between general deterrent threats, or the threat to attack any state that violates the status quo, and immediate deterrent threats, or the specific threat issued to a particular state following a challenge to the status quo. He argued that immediate deterrent threats would be more likely to fail precisely when they are credible. Fearon's argument begins with the observation that the only states receiving immediate deterrent threats are states that were undeterred by the general threat. These states are likely to be highly motivated to challenge the status quo and are therefore relatively unlikely to be deterred by an immediate threat. Thus, the states that choose to issue an immediate threat anyway are doing so credibly because they anticipate conflict but prefer it to backing down. The reason for this surprising result is strategic selection: the states that receive immediate threats are least likely to be deterred. Importantly for the literature on deterrence, Fearon's argument implies that the risk of war could be constantly shaping state behavior even if most credible deterrent threats fail to change the behavior of the target.

A direct application of Fearon (2002) to the logic of economic coercion reveals the importance of general and immediate threats in this context as well. An immediate corrollary is that evidence on the effectiveness of immediate sanctions threats has little bearing on the effectiveness of general sanctions threats. Although the literature is aware of strategic selection in sanctions cases, it has not grappled with strategic selection in immediate threats. Many studies of economic coercion are vulnerable to this criticism even if they are about mechanisms rather than overall effectiveness. For example, many studies find that multilateral sanctions are more effective while others find that unilateral sanctions are more effective (Drezner 2000; Miers and Morgan 2002; Bapat and Morgan 2009; McLean and Whang

[5]It is worth noting that the puzzle of war itself as studied by Fearon (1995) also exhibits strategic selection. Every war implies an antecedent failure to bargain peaceably. Thus, empirical analysis of the effectiveness of diplomacy and bargaining at achieving their aims is also subject to a strategic selection effect.

2010). Both of these conclusions are sensitive to the strategic context of a general threat. Evidence from threatened or imposed sanctions that supports one type's relative effectiveness could mean that it really is more effective. But it could also mean that general threats of the other type are so successful they rarely enter the dataset, thus creating an illusion of an effectiveness advantage only among the observed cases.

Existing literature rarely if ever considers the possibility that general threats could reverse the direction of the results. The neglect of general threats is consequential for the literature. A number of existing papers on multilateral sanctions, including Kaempfer and Lowenberg (1999), Drezner (2000), and Miers and Morgan (2002) are premised on a particular interpretation of the direction of most empirical results. The problem also affects the TIES dataset created by Morgan, Bapat, and Kobayashi (2014), the most significant attempt to study strategic selection in the context of sanctions, which supplements existing datasets on the imposition of sanctions with data about sanctions threats. Their effort, while representing a major step forward in the literature, includes only immediate threats. The acknowledgement of immediate threats alone does not construct the proper strategic context for sanctions because the states that receive immediate threats are the least likely to be responsive to economic incentives. In Fearon's words: "Selection effects introduce systematic bias, so that relationships that may be true for general deterrence will appear exactly reversed for immediate deterrence" (Fearon 2002, 7).

Choosing Between Threat Types

There are at least three major circumstances that would lead states to prefer issuing general sanctions threats. First, states are most likely to deploy a general threat when they cannot predict future noncompliance. If noncompliance were predictable the state could simply issue immediate sanctions threats to the targets at highest risk. The broader scope of the general sanctions threat ensures that all potential targets are incentivized to increase compliance regardless of space and time.

Second, there could also be uncertainty about which targets would be most responsive to economic coercion. The complexity and dynamism of the global economy makes it very difficult to predict the precise economic consequences of interrupting trade. It could be that certain states value trade in certain products highly without the sender state being aware. A general threat can potentially discover states that depend on trade more than previously thought. Thus, general threats require less information about the global economy to be effective.

Third, states with large markets may promote a reputation of benevolence by issuing general threats targeting behaviors rather than by issuing immediate threats to particular states. General threats send a message that the sender state is more concerned about the behavior than about which state is the perpetrator. For example, if the United States threatens additional sanctions on North Korea for internet censorship, the North Korean government may wonder why they were singled out. But if the United States makes it clear that it intends to sanction any state that censors the internet then North Korea may take immediate threats more seriously. Other states may interpret the US threat as compatible with its commitment to a rules based order over a discriminatory regime. By changing the context of an immediate threat, general threats can heighten their significance and credibility.

While general threats can enhance the credibility of immediate threats, they are typically difficult to make credible when used in isolation. The broad scope of general threats could be interpreted by potential targets as a mark of unseriousness. If the scope is broad enough to include states of very different economic sizes then they can probably expect to be treated differently because it is much more costly to the sender state to terminate trade with an important economy. The lack of differentiation between targets of general threats is therefore both a strategic advantage and weakness.

Much can be learned about a state's beliefs by observing their reactions to general and immediate threats. States that comply with general threats are unwilling to risk the advantages of trade to implement a noncompliant policy. They do not even attempt to challenge the condionality. States that suspect they might receive special treatment may push the envelope of a general threat. But when confronted with an immediate threat making it clear that the possibility of lost trade is real they may choose to comply. Finally, states that spurn both the general and immediate threats are simply more interested in choosing a noncompliant policy.

The issuance of immediate and general threats may also create strategic spillovers. For example, when a sender issues an immediate sanctions threat other potential targets may infer that they could be next if they behave similarly. In this way, an immediate sanctions threat could establish a more general threat. However, it could also be that immediate sanctions threats are perceived as applying narrowly to only the targeted state. The reputation of the sender could influence whether states perceive immediate sanctions threats as narrow or as evidence of a more general threat.

Studies of general sanctions threats could reveal that economic coercion is far more consequential than previously thought. Because general sanctions threats could incentivize many states simultaneously they can potentially change the behavior of more countries. Furthermore, economic coercion should be most effective in states that change behavior without ever receiving an immediate sanctions threat. As international economic linkages become deeper the opportunities for general sanctions threats to affect state behavior continue to multiply.

Varieties of Threats and the Unit of Analysis

The existing large-N sanctions literature studies economic coercion using datasets of sanctions episodes. The roots of this concept in the literature are very deep. Older literature on economic sanctions focused on just one or two prominent cases and generally concluded that sanctions were ineffective (Galtung 1967; Baer 1973; Schreiber 1973; Olson 1979; Von Amerongen 1980). In the 1980s scholars became concerned that studies of individual cases were not painting a representative picture of the phenomenon. Hufbauer, Schott, and Elliott (1990) addressed the problem by collecting information from a large number of cases into a dataset that could be processed with statistical analysis. Quantitative scholarship on sanctions since then has proceeded by augmenting their work. The choice to study economic coercion as a series of cases is attractive because it enables scholars to draw conclusions that transcend the particulars of any individual event by using information from different institutions, times, and places.

However, episodes are not the right unit of analysis for studying the most important questions in the literature. Many scholars are interested in understanding whether and under what circumstances sanctions can motivate states to change behavior, which is surely an important objective worthy of study. Notice that this question is fundamentally about a counterfactual: would the target have behaved differently in the absence of economic coercion? The standard approach to answering this question in the literature has been to 1) identify occasions where sanctions were either threatened or imposed and 2) count the cases where the sender's stated objectives were achieved. There are two main reasons why this design cannot answer the question posed. First, the dataset of sanctions episodes contains no information about how states behave when economic coercion is unlikely, making it impossible to construct a proper control group. Second, datasets of sanctions episodes also omit general sanctions threats, making it impossible to interpret results in the correct strategic context.

Answering the question requires a comparison of what happened under sanctions with what might have happened without sanctions. But the typical design never attempts to construct the target's behavior in the absence of sanctions.[6] Statistical inference about how states might have behaved in the absence of conditionality can be made by constructing a control group of states not subject to the conditionality. Of course, constructing a control group alone is not sufficient. It is important to take steps to ensure that the two groups are actually comparable – for example, the research design might need to account for policy differences due to wealth if the states that are likely to be targeted with economic coercion are relatively less wealthy. But a sophisticated statistics literature exists to provide methods which can control for observed and unobserved confounding variables. Without information about how states behave in the absence of conditionality it is impossible to infer how the conditionality changed behavior.

Second, sanctions episodes introduce strategic selection by omitting general sanctions threats. Unlike the control group above, this group includes states that perceive a risk of economic coercion without being immediately threatened. As discussed above, excluding states that might react to economic coercion creates the strategic selection problem. Resolving the problem means including all states that could potentially react to the sanctions threat. This is impossible to do using sanctions episodes, which require that a state be immediately threatened with sanctions or that sanctions be imposed.

If the sanctions episode is not the right unit of analysis, what would be better? First, consider the nature of the dependent variable: the target state's behavior. Sanctions target a wide variety of behaviors including human rights violations, intellectual property rights protections, security investments, and

[6]It is worth pointing out that the empirical section of Fearon (2002) also makes no attempt to construct counterfactuals.

more. Although these behaviors are quite different from each other they are all functions of policies chosen by states at particular times. Thus, the best unit of analysis is not the sanctions episode but the state-year. This analysis unveils a second way in which the datasets of sanctions episodes are inadequate: they contain information about the dependent variable only for the duration of the sanctions episode. The absence of information about the average variability of these choices limits the ability of scholars to determine whether any observed policy changes during a sanctions episode were consistent with normal variation or were a consequence of the sanctions. By contrast, data on the dependent variable at the state-year level provide rich information about how state behavior changes over time. It enables scholars to use within-state variation to test hypotheses about sanctions, a critical advantage because of how idiosyncratic state level characteristics could affect the results.

Choosing the state-year as the unit of analysis leaves an important question unanswered: How should sanctions be conceptualized if not as episodes? Sanctions are a commitment of a sender state to conditional punishment. In other words, a sanction is both 1) a commitment to punish a target state that does not fulfill particular criteria and 2) a commitment not to punish a target state that complies. The sender is not necessarily targeting any particular state with these commitments. For example, sanctions could be a general statement that "we will not trade with any state that violates human rights." What matters for the target's incentives is whether the sender has committed to conditional punishment that could apply to them at a given time.

From Concepts to Research Design

Reconceptualization of sanctions as commitments to conditionality suggests how scholars can account for the strategic context of general threats. Scholars should focus attention on attempts to commit to conditionality. A commitment to withhold trade from any state that does not meet prespecified criteria is a clear example of a general sanctions threat. By identifying the general threat at the outset scholars can study the set of states that are potentially subject to the general threat and the subset of states that challenge it. The strategic context of the general threat is reflected in the dataset because all states that are *potentially* affected are included. The dataset can then identify the effect of economic coercion separately for states that never challenge the general threat, states that challenge the general threat but then back down after an immediate threat, and states that are ultimately sanctioned. This procedure effectively studies economic coercion by studying the case studies of individual sanctions programs.

There are advantages and disadvantages to studying sanctions programs individually. The primary advantage is a clear sense of what general sanctions threat is being studied and which states are potentially affected. There are ancillary benefits as well. One major challenge facing previous studies has been finding a research design that can convincingly compare compliance across a wide range of issue domains. Choosing a single sanctions program ensures a degree of uniformity in the outcome measures because few sanctions programs frequently change their objectives. Another benefit is an opportunity to hold the sender state constant. It is easier to identify whether variation in compliance is attributable to characteristics of the target or the sender if one of these is fixed in the data.

The main disadvantage of studying sanctions programs individually beginning with a general threat is that the approach is not always available. There are certainly cases where an immediate sanctions threat was issued without a clear general sanctions threat. Because declaring a general sanctions threat is itself a choice there is now a new strategic selection effect: senders might issue general sanctions threats only under special circumstances. A cynic could complain that this research design merely replaces one strategic selection effect with another. But there are reasons to prefer this form of strategic selection. A research design that neglects the distinction between general and immediate sanctions threats risks misinterpreting the sign of the results and underestimates the effectiveness of economic coercion. These are both serious challenges that threaten the ability of the study to answer the central question. Selection in the decision to issue a general sanctions threat does not clearly cause either of these problems. Furthermore, this selection effect would affect external validity of the studies but not internal validity. Thus, the results could still be valuable for understanding particular sanctions programs.[7]

The final ingredient for the research design is a source of exogenous variation appropriate for con-

[7] A second disadvantage is the loss of the broad perspective that Hufbauer, Schott, and Elliott (1990) originally sought when they collected a cross-case dataset. This is a regrettable loss that cannot be readily remedied. The vital cross-case perspective could be recovered by meta analysis of multiple studies on sanctions, each of which focus on a particular general threat. But it will take time for the literature to accumulate the necessary body of work.

structing a counterfactual. The research question calls on researchers to evaluate a causal claim: did economic coercion cause target states to change their behavior? Questions about causality are particularly difficult to answer because counterfactuals are fundamentally unobservable – it is impossible to know with certainty how any particular state would have behaved in the absence of economic coercion. However, it might be reasonable to infer how states could have behaved on average using information about other states and times that were less affected by economic coercion. Research over the last thirty years on statistical identification has concluded that average counterfactual behavior can be inferred using exogenous variation in the independent variable – that is, variation that can only be correlated with the dependent variable through the hypothesized channel. In the context of economic coercion it is necessary to identify a source of variation that can only effect the behavior of target states by varying their exposure to sanctions. This variation can be used to compare average outcomes for groups of states that differ only in their incentives to comply.

The strategic selection effect that has caused much consternation in the literature on economic coercion is not one of the ineffable inference problems in social science. It is a consequence of research design choices and it can be remedied by making different choices. Designs using data describing whether the imposition of sanctions or immediate threats to impose sanctions achieved their stated objectives will always be subject to a pernicious strategic selection effect because they never include cases where general sanctions threats were successful. Designs adhering to the following protocol are not subject to that criticism:

1. Identify a general sanctions threat, meaning an attempt to commit to conditionally withholding trade from states not behaving in a certain way.
2. Identify the set of potential target states.
3. Measure outcome variables at the state-year level.
4. Identify a source of variation in the incentive to comply that is exogenous to the outcome variable except through sanctions.
5. Use the exogenous variation to estimate how states would have behaved in the absence of sanctions.

This protocol is immune to the classic strategic selection problem because of the first two steps. By including all states that might be affected by a particular general sanctions threat the dataset includes information about states that might have complied before an immediate threat needed to be issued. Measurement at the state-year level ensures that variation in the outcome variable is treated appropriately. The use of exogenous variation in sanctions incentives enables researchers to construct counterfactuals that are appropriate to answering a fundamentally causal question.

Research Design

The GSP offers a general sanctions threat for developing states. Although not directed towards any one particular state, most developing states are eligible for the program as long as they respect the conditionality. Thus, the set of potentially affected states includes most developing states.[8] Some GSP beneficiary states also experience immediate sanctions threats in the form of a GSP review. Thus, the GSP offers an opportunity to compare general and immediate sanctions threats.

What is an appropriate outcome variable to measure compliance with the conditionality? Interviews with GSP Subcommittee members in December 2018 illuminated the process by which the Subcommittee evaluates the compliance of a beneficiary state. The Subcommittee has no formal criteria and always considers issues on a case-by-case basis. Nonetheless, two criteria are informally prioritized: First, has the beneficiary implemented a law that would prohibit the alleged conduct? Second, is the law consistently enforced? The outcome measures are selected to approximate the US perception of compliance along these two dimensions to the extent possible. The GSP applies conditionality in two issue areas: labor rights and intellectual property rights.

The protocol requires that outcome variables be measured at the state-year level, but there are few sources of panel data on these issues. The US State Department issues an annual report on human and labor rights which has previously been coded by researchers studying human rights. A general measure of labor rights was compiled by Cingranelli, Richard, and Clay (2014) from the State Department

[8] Some states, including China, have been excluded by statute from eligibility since the beginning of the program. Other states can lose their eligibility when their incomes rise enough. Finally, states that have signed a trade agreement with the United States are not eligible. All three groups of states are not considered potential beneficiaries.

Human Rights Reports. This measure is coarse and only measures rights protections on a three point scale. Recently, Cordell et al. (2019) has used machine learning and text analysis to detect evidence of rights violations in the Human Rights Reports.[9] Their measure is coded as `rights_violations` in the analysis.

Intellectual property rights protections are measured using data on software piracy. A variable called `piracy.rate` is supplied by the BSA | Software Alliance, which is an advocacy organization representing the software industry. The alliance calculates piracy values and rates by comparing software usage figures from consumer surveys with proprietary sales figures drawn from the data of its members. They produce these data on an annual basis at the state level. Presumably, states that enforce their intellectual property rights laws more stringently have lower piracy rates.

Finally, the protocol calls for a source of plausibly exogenous variation that can used to estimate how states would have behaved without the incentives of conditionality. In the case of the GSP, this amounts to finding a variable that affects the value of GSP membership but not labor rights and intellectual property rights protections through any other channel. The benefit of GSP membership is having access to US markets without needing to pay tariffs, meaning that the value of membership is smaller when US tariffs are low. The US reduced its tariffs dramatically throughout the 1990s and early 2000s due to multilateral trade negotiations including the Tokyo Round and the Uruguay Round. Thus, the value of GSP membership fell dramatically as these tariff declines were phased in.[10] The research design will test whether actual compliance fell as the GSP's incentives to comply declined.

More specifically, the following variable was constructed to measure changes in the value of GSP membership. First, for each state-year, the mean tariff among products actually exported by the state in that year was calculated. The benefit of membership in that year was found by subtracting the mean tariff if the state were a member of the GSP and faced zero tariffs on GSP eligible products. Finally, to capture the total meaning of the tariffs for each state, the value of exports in GSP eligible products for each state were multiplied by the difference in tariffs. Economic theory would imply that this value is

[9] See also Cordell et al. (2020).

[10] The decline in the value of preference programs due to multilateral trade negotiations has attracted some attention in the economic development literature where the phenomenon is called "preference erosion." To the author's knowledge, the implications of preference erosion for compliance with conditionality have not previously been studied.

a lower bound for the value of GSP membership because it does not account for the behavioral effects of lowering tariffs.

Is it possible that the tariff declines are correlated with labor rights and intellectual property rights compliance through channels other than the value of GSP membership? Because tariffs fell during this period as a result of complex negotiations, scholars may worry that the GSP beneficiary states more likely to protect rights would somehow also be less resistant to tariff decreases. Alternatively, scholars may worry that falling tariffs could reduce rights protections by some mechanism outside the GSP, e.g. by exacerbating a race to the bottom in rights protections. The nature of the negotiation process makes it likely that the variation is exogenous to rights protections in target states. There are two major reasons to believe this variation is unrelated to rights protections in target states: the exogeneity of pre-negotiation tariff rates and the dominance of developed states during negotiations.

The pre-negotiation tariff rates are unlikely to be correlated with rights protections abroad. The initial tariff rates are an important determinant of the tariff declines because no tariff can decline below zero. Thus, tariffs that begin relatively low cannot have large declines. But these initial values are unlikely to be correlated with rights protections. Before the multilateral negotiations US tariffs were determined either as a result of trade agreements or tariff bills. As described by Schattschneider and others (1935), these the rates in tariff bills were heavily influenced by log rolling in Congress. Representatives traded support for tariffs that protected industries in each others' districts. They were mostly responsive to domestic pressures and thus unlikely to have any relationship to rights protections abroad.

Second, the developing states that were eligible for the GSP had little influence over the determination of the tariff declines during negotiations in this period. A positive relationship between tariffs and rights protections could occur if the states most likely to protect rights were also somehow better able than other states to keep tariffs high on GSP eligible products important to their economies. However, this selection mechanism is unlikely for the simple reason that the multilateral negotiations mostly reflected the interests of wealthy states. Indeed, the influence of the wealthy states was much lamented by developing states at the time. Ultimately, the Doha Development Round was a concession by the WTO that the interests of developing states were not always reflected in the results of previous agreements. The influence of developing states in the multilateral negotiations undercuts the chances that the pattern of tariff declines reflects any pattern of rights protections in developing states.

The main results are estimated from a two-way fixed effects regression of each outcome variable on the measure of GSP program value. This estimator is commonly employed to estimate average treatment effects in designs where treated and control units can be assumed to follow parallel trends (Angrist and Pischke 2008). In this context, the "treated" units are states whose benefits of GSP membership eroded relatively more. "Control" units consist of two types: 1) GSP eligible states whose benefits declined by less and 2) states that were not eligible for the GSP for various reasons. The most common reason a state might not be eligible for the program is wealth – as a trade for development program high income states are graduated out of eligibility.

Specifically, the regression being fit is

$$DV_{it} = \alpha_i + \gamma_t + \tau(\text{GSP Value})_{it-k} + X_{it}\beta + u_{it}$$

where DV_{it} is a measure of rights violations, α_i and γ_t are state and year fixed effects, GSP Value is the measure of the value of GSP membership (which might be lagged by k periods), X_{it} is a matrix of control variables, and u_{it} is an error term. Crucially, the control variables included in X_{it} are not necessary to identify the causal effect of program membership because the measure is chosen to be exogenous. Their primary purpose is to decrease variability in the dependent variables.

The coefficient of interest is τ and it should be negative under the hypothesis that the GSP program conditionality meaningfully increases rights protections. The coefficient τ can be interpreted as a causal estimate because of the exogeneity of the GSP Value measure. However, the measure does not include all possible sources of economic value from the program by design. For example, economic theory would expect that lowering tariffs would increase the trade volume, which would increase the value of the relationship. By focusing on aspects of the GSP value that are directly related to the tariffs the research design increases the plausibility of exogeneity at the cost of understating the true value of the program. This choice potentially attenuates the measured effect of the program.

Results

Table 1: Value of GSP Membership and Compliance.

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=8)	-0.214^{***}	-0.264^{***}	-0.0005^{***}	-0.0005^{***}
	(0.067)	(0.100)	(0.0001)	(0.0001)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	No	Yes	No	Yes
Covariates	No	Yes	No	Yes
Observations	2,295	1,314	821	775
Note:				*p<0.1; **p<0.05; ***p<0.01

The main results are reported in Table 1 and show support for the theory. The analysis indicates that each $1 million of GSP value causes personal integrity rights violations to decrease by about 0.26 for an average state-year. Importantly, this estimate is likely smaller than the true value because the independent variable does not include all possible benefits of GSP membership. Nonetheless, the results are statistically significant using standard errors clustered by year and state. Clustering is important to account for potential correlation in the error terms at the state and year levels (e.g. changes in the outcome due to an idiosyncratic event affecting one state that persist for multiple years). As described above, covariates are unnecessary to include for purposes of identification but may improve the estimation properties of the regressions.[11] As shown in Table 1, including covariates changes very little about the results. The direction and significance of the results support the hypothesis that the GSP's general threats affect state behavior.

The impact of the GSP's general threats appears to occur after a fairly long time. The results in Table 1 show the results where the value of GSP membership has been lagged by 8 years. Results for other lags are shown in Figure 2, which indicates that the effects are similar in size and statistical significance

[11]Detailed information about these covariates are available in Appendix .

in neighboring periods. These results are suggestive about how the general risk of economic coercion shapes the behavior of states. In the absence of an immediate threat of GSP exclusion, states may not immediately change policies protecting rights when the benefits of GSP membership fall. The first order effect of reducing the benefits of GSP membership is to undercut the influence of rights advocates in GSP eligible states. As their influence wanes, rights violations become less likely to be prioritized by the state. It could be years before the gradual erosion of preferences has accumulated enough to create rights violations that would be observable in a cross national dataset.

There are other possible explanations for the long delay before the effects become measurable. There is a possibility that rights protections in GSP-eligible states are following a different trend than rights protections in states that are not eligible for the program. If rights protections tend to fall over time in states that are eligible for the GSP at a faster rate than others the analysis could produce a statistically significant negative coefficient over a long horizon. If the results are explained by differential trends then it is less clear that rights protections are actually changing in response to the GSP's incentives rather than following their previous trends. However, the results are robust to the inclusion of fixed effects at the GSP eligibility-year level. Including these group-time fixed effects is a highly flexible way of accounting for potentially differential trends in a two way fixed effects regression (Wooldridge 2021). Results from specifications including these extra fixed effects are also reported in Table 1. Because controls for differential trends do not explain the results, it is more likely that the effects take a long time to manifest because of political reasons.

The structure of the GSP makes it possible to directly observe the benefits of studying general sanctions threats relative to immediate sanctions threats. States are rarely excluded from GSP eligibility but some are threatened with exclusion during a formal review. As described above, a review is initiated when a petition from an interest group in the United States is accepted by the USTR. Table A1 shows the results when using petitions – a measure of immediate sanctions threats – as the independent variable. The results are not precisely estimated and the coefficients have the wrong sign. Because relatively few states are ever named in a petition it is difficult to conclude that the petitions cause no changes in state behavior from this evidence alone. Even if the estimates were more precise it is possible that petitions and rights violations might still be positively correlated. States that receive petitions must have tolerated rights violations which were known to potentially lead to expulsion from the GSP. Thus, the states that eventually receive petitions might be precisely those which are already inclined to surrender GSP eligibility.

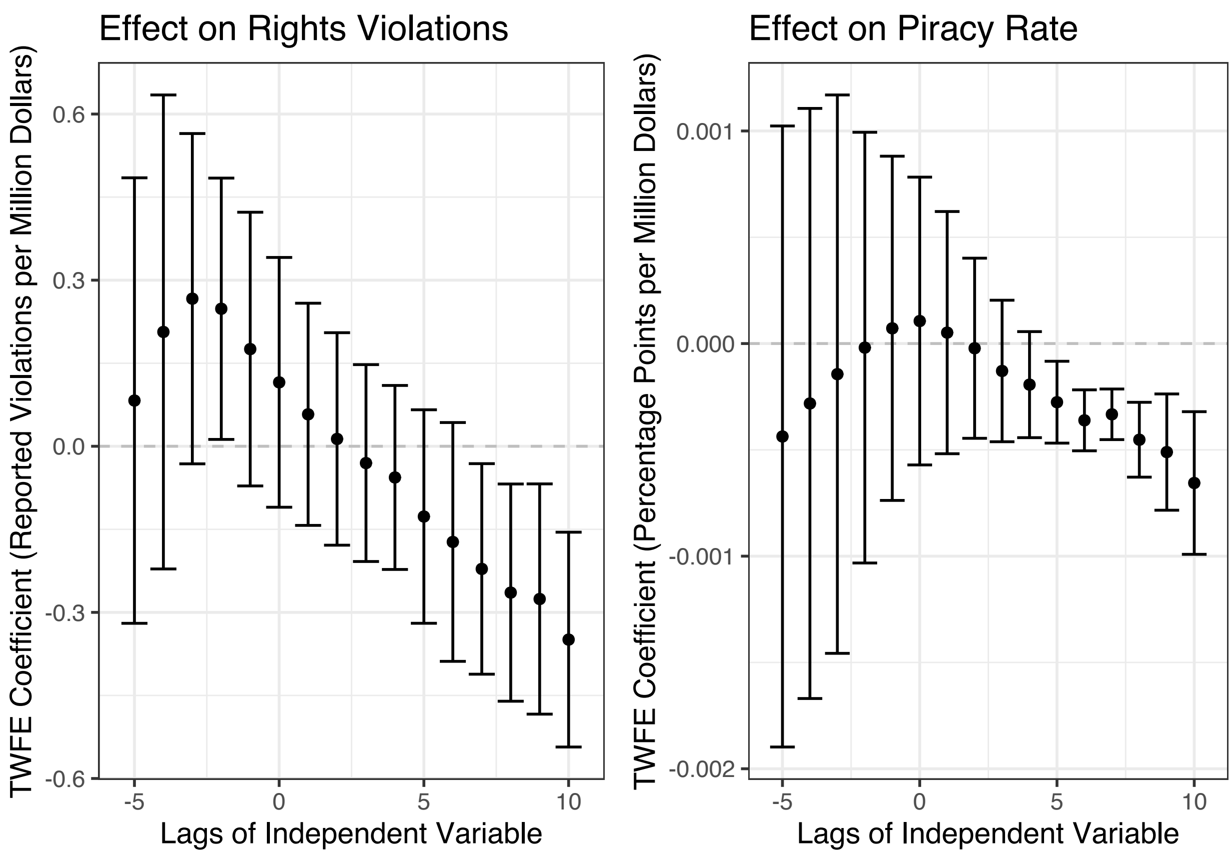

Figure 2: Results from main specification.

Robustness of the Results

A number of robustness checks intended to rule out specific confounders and inference problems are included in the Appendix. Tables A3 and A4 add covariates one at a time to demonstrate that the results are not sensitive to the inclusion of any single covariate. To rule out the possibility of post treatment bias, Table A8 reproduces the main results but also lags all covariates by two additional periods. Figure A3 analyzes two alternative measures of the dependent variables to demonstrate that the results are not contingent on particular measures. Most of the declines in tariff rates in GSP eligible products occurs in the first few years of the sample. To ensure the results are not being driven by this unusual decline, Table A4 reproduces the analysis after limiting to observations after 2002. Finally, the main results are reproduced after dropping all non-GSP eligible states in Table A7 to allay concerns that the effects are a consequence of pooling GSP eligible and ineligible states in the control group.

More robustness on differential trends is also available. Tables A5 and A6 shows the results when controlling for possibly differential trends among various groupings of states by other measures. Table A8 adds fixed effects allowing for even more flexibility in controlling for differential trends that may additionally differ by the control variables. To further confirm that the results are not driven by pre-trends, Figure 2 is reproduced with additional leads of the independent variable in Figure A2.

Conclusion

This article proposes a reconceptualization of economic sanctions that both acknowledges the possibility of general sanctions threats and completely eliminates the challenge of inference under strategic selection. The central proposition is that sanctions are commitments to conditional market access. Unlike the standard concept of "sanctions episodes", this definition includes general sanctions threats that may or may not target a particular state and refuses to constrain the analysis to a fixed target and time period. The theory constructs a five step protocol drawing on the revised definition of sanctions that will produce research immune to the strategic selection concerns.

The article demonstrates how this conceptual approach shapes empirical research by deploying the protocol to estimate the importance of implied threats in the context of the GSP. The results demonstrate that beneficiary states of the US Generalized System of Preferences (GSP) do react to the program's conditionality even without being directly confronted. It would not be possible to reach this conclusion by studying the cases of states being "sanctioned" by being threatened with immediate exclusion from the program. Datasets that define observations as either definite threats to impose sanctions or as the actual imposition of sanctions are necessarily incomplete. They definitionally exclude cases where states might increase compliance to reduce the risk of losing access to important markets. In other words, these datasets necessarily miss the cases where states take steps to meet the softly spoken conditionality of a state wielding its markets as a big stick.

Although the empirical results are from the GSP as a single case study, the evidence presented here has broader implications for the literature on economic coercion. Previously, scholars have studied instances of applied or threatened economic coercion. The framework developed by this paper illuminates the importance of distinguishing between general and immediate sanctions threats. It is likely that many existing estimates of the effectiveness of economic coercion are attenuated because they examine only immediate sanctions threats. Future research designed using the protocol outlined in the theory can determine the extent to which previous scholarship has understated the importance of economic coercion.

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How Effective is Trade Conditionality? Economic Coercion in the Generalized System of Preferences
ONLINE APPENDIX

June 2022

Appendix Table of Contents

Control Variables

Table summarizes the standard control variables and their sources used in the analysis. As described in the text, these control variables are not intended to facillitate the identification of the GSP's causal effect on compliance. The identification problem is adddressed by a research design using exogenous variation in membership incentives. Rather, the purpose of these control variables is to increase the precision of the estimates by accounting for other factors that also affect compliance. The `polity` variable accounts for democracy and proxies for rights protections due to domestic pressure. The State Department Terror Index accounts for any political favoritism (or its opposite) due to US objectives in the Global War on Terror. Total imports accounts for the importance of trade with that state to the United States. The GDP accounts for the size of the economy, which has been shown to be a very important variable in explaining bilateral trade flows (Head and Mayer 2014). Finally, `military.deployment` accounts for US military interests abroad, which might affect US perceptions of rights abuses in that state.

variable	description	Source	Purpose
pts_stdpt	State Department Terror Index	Hafner-Burton, Mosely, and Galantucci (2018)	Control for GWOT significance
polity	Polity	Hafner-Burton, Mosely, and Galantucci (2018)	Control for regime type
totalimportslog	Total Value of US Exports (log)	Hafner-Burton, Mosely, and Galantucci (2018)	Control for general trade importance
any_export	Number of Exported Products	Schott Data	Control for breadth of trade
log(gdp)	GDP (log)	World Bank	Control for economy size
military.deployment	US Military Deployment	Kane (2006, 2016)	Control for US strategic interest

Petitions as Immediate Threats

As described in Section , domestic interest groups in the United States can petition for a GSP beneficiary's compliance to be formally reviewed. These petitions may or may not be accepted by the US government. If a petition is accepted the State Deparment holds hearings to evaluate the merits of the accusations and to hear explanations from the "defendent" state.

The main text argues that formal reviews should be conceptualized as immediate sanctions threats because they are a clear warning to a specific state that their market access under the GSP is at risk. However, it is not clear whether immediate threats should be more or less effective than the general sanctions threat. If the general threat is effective then it could be that the states which choose to violate it also intend to violate any subsequent immediate threats. If the general threat does not have credibility then states might be more willing to comply after receiving an immediate threat.

Tables A1 and A2 test these effects in two ways. Table A1 tests whether a petition being filed in the last three years has an effect on compliance according to the two main outcome measures. Table A2 tests whether the *acceptance* of a petition effects compliance. The regression specification is identical to that used to make Table 1 with the independent variable replaced. Even though the specification is similar, it should be noted that these regressions should not be directly compared to Table 1 because petitions and reviews are not exogenous.

Due to the relative rarity of the petitions, effects are not statistically significant. However, the estimates have signs that are consistent with an increase in compliance following both petitions and reviews. Although the estimates are not directly comparable with those in Table 1, it is interesting to note that the coefficients are larger in magnitude. These results are consistent with petitions and reviews increasing compliance in some cases, but the rulings occur infrequently enough to make the effect difficult to detect statistically.

Figure A1 reproduces Figure 2 but using sanctions reviews as the independent variable. The results are consistent with Table A2 and indicate that they are not sensitive to the number of lags.

Table A1: Petition as an independent variable

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Petition in Last 3 Years	0.675	1.543	−0.004	−0.007
	(3.204)	(2.975)	(0.011)	(0.009)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	No	Yes	No	Yes
Covariates	No	Yes	No	Yes
Observations	2,872	2,184	1,486	1,395
Note:	*p<0.1; **p<0.05; ***p<0.01			

Table A2: Reviews as an independent variable (lag=0)

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Review Conducted	−3.487	−0.349	−0.017	−0.010
	(6.774)	(7.576)	(0.020)	(0.014)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	No	Yes	No	Yes
Covariates	No	Yes	No	Yes
Observations	2,872	2,184	1,486	1,395
Note:	*p<0.1; **p<0.05; ***p<0.01			

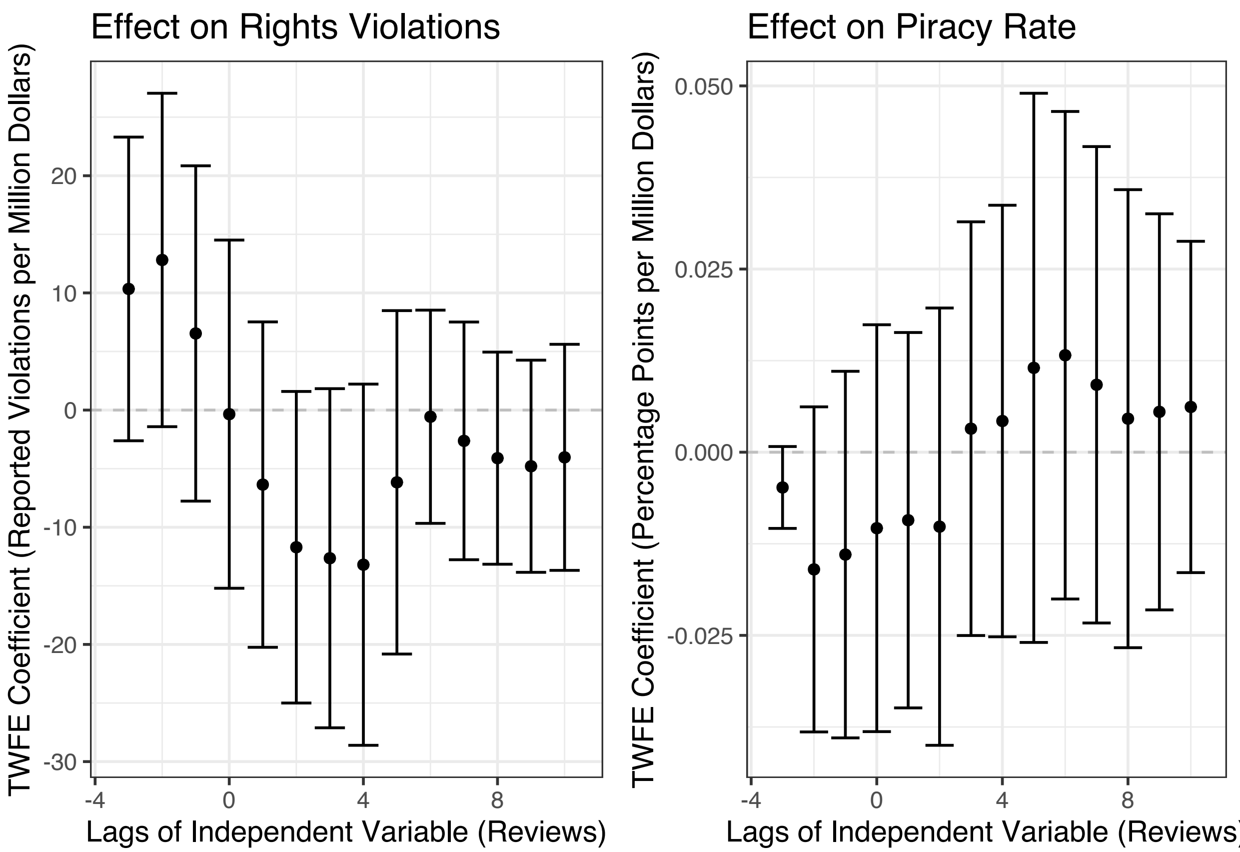

Figure A1: Plot of Effects at Long Lags Using Reviews

Robustness of Main Results

Adding Covariates in Sequence

To demonstrate that no single control variable is driving the results, the control variables from the main specification are added to the regression sequentially in Tables A3 and A4. The coefficients on the main independent variable barely change at all regardless of the set of control variables. This result is consistent with the claim that the main independent variable is exogenous to the control variables.

Table A3: Sensitivity to Covariates

	Dependent variable:						
	Rights Violations						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Value of GSP Membership (lag=8)	-0.291^{**} (0.113)	-0.291^{***} (0.104)	-0.305^{***} (0.107)	-0.289^{***} (0.109)	-0.294^{***} (0.104)	-0.290^{***} (0.105)	-0.264^{***} (0.100)
State Department Terror Index	6.469^{***} (1.457)						6.842^{***} (1.611)
Polity		-0.861^{*} (0.489)					-0.561 (0.442)
Total Imports (Log)			1.693 (1.484)				3.311^{**} (1.350)
Ever Export under GSP				-0.013 (0.010)			-0.020 (0.018)
GDP (log)					-5.653 (3.775)		-4.303 (3.979)
Military Deployment						0.00002 (0.00003)	-0.00003 (0.00003)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,567	1,425	1,686	1,687	1,650	1,463	1,314

Note: *p<0.1; **p<0.05; ***p<0.01

Table A4: Sensitivity to Covariates

	Dependent variable:						
	Piracy Rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Value of GSP Membership (lag=8)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)
State Department Terror Index	-0.001 (0.002)						0.0001 (0.002)
Polity		0.001 (0.001)					0.001 (0.001)
Total Imports (Log)			-0.005^{**} (0.002)				-0.005^{***} (0.002)
Ever Export under GSP				-0.0001 (0.00005)			-0.0001 (0.0001)
GDP (log)					-0.017^{**} (0.008)		-0.019^{***} (0.007)
Military Deployment						-0.00000^{***} (0.00000)	-0.00000^{***} (0.00000)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	820	791	821	821	813	813	775

Note: *p<0.1; **p<0.05; ***p<0.01

Differential Trends

To demonstrate the robustness of the specification, different versions of the main results with various fixed effects are reported in Tables A5 and A6. The difference-in-differences research design could be confounded if the dependent variable were trending differently for subgroups of states. Differential trends for a particular subgroup can be accounted for by including group-year fixed effects in the regression.

The tables indicate that the results are virtually unchanged when including fixed effects for four different groups. The first group includes "All GSP" meaning all states that have ever been members of the GSP program. The second group includes "Potential GSP" meaning all states that could become a member of the GSP in the future (some states graduate out of the program due to their level of development or sign trade agreements that supercede the GSP membership). The third group includes "Current GSP", meaning states that are currently members of the program. The fourth group includes trends by World Bank region. The results are similar across all these specifications, indicating that differential trends by subgroup is unlikely to be driving the results.

Table A5: Sensitivity to Differential Trends

	Dependent variable:			
	Rights Violations			
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=8)	-0.264^{***}	-0.255^{***}	-0.249^{**}	-0.236^{*}
	(0.100)	(0.098)	(0.098)	(0.141)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Fixed Effect for Trends	Any GSP	Potential GSP	Current GSP	Region
Observations	1,314	1,314	1,314	1,314
Note:	*p<0.1; **p<0.05; ***p<0.01			

Table A6: Sensitivity to Differential Trends

	Dependent variable:			
	Piracy Rate			
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=8)	-0.0005^{***}	-0.001^{***}	-0.001^{***}	-0.0004^{**}
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Fixed Effect for Trends	Any GSP	Potential GSP	Current GSP	Region
Observations	775	775	775	775
Note:	*p<0.1; **p<0.05; ***p<0.01			

Longer Leads Plots of the Regressions

Figure A2 extends Figure 2 to include coefficients from regressions of more leads of the indepdent variable. These regressions indicate that there is little evidence of differential pre-treatment trends when the dependent variable is rights violations. There is some indication of non-parallel trends when examining the piracy rate, but only at very long leads and the evidence is marginal. These concerns are partially mitigated by the alternative measures presented in Figure A3.

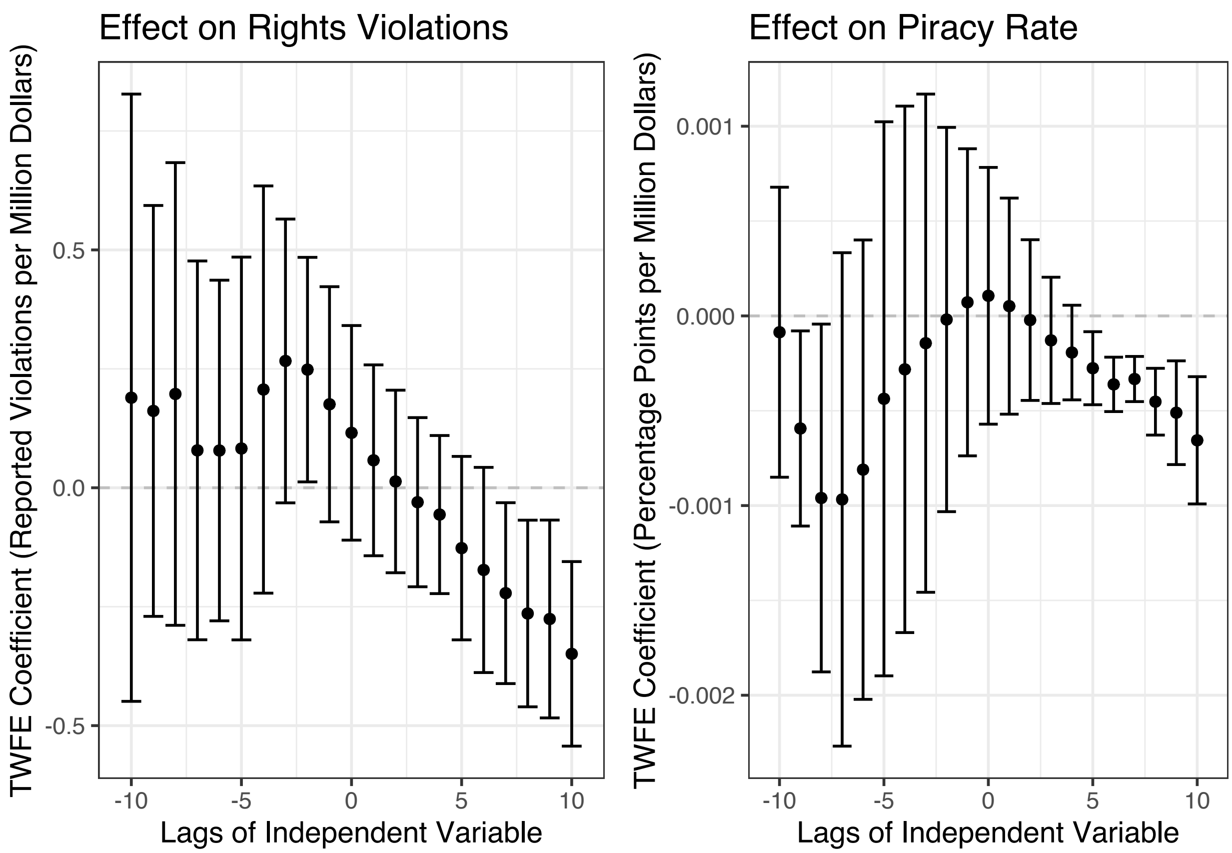

Figure A2: Plot of Effects at Long Lags

Alternative Outcomes

Given that compliance is difficult to observe, it is reasonable to wonder whether alternative measures of compliance would yield the same results. Figure A3 shows the results with alternative outcome measures. The variable `WORKER` is a trinary measure of worker's rights produced by expert evaluation of

State Department reports on human rights (Cingranelli, Richard, and Clay 2014). Higher values indicate better protections. The measure `piracy.value` indicates the value of pirated software estimated by the BSA | Software Alliance. These two measures show similar results to the main measures. In particular, 1) there is little evidence of deviations from parallel trends and 2) there are effects consistent with increased compliance at long lags. These results should increase confidence in the main table and figure.

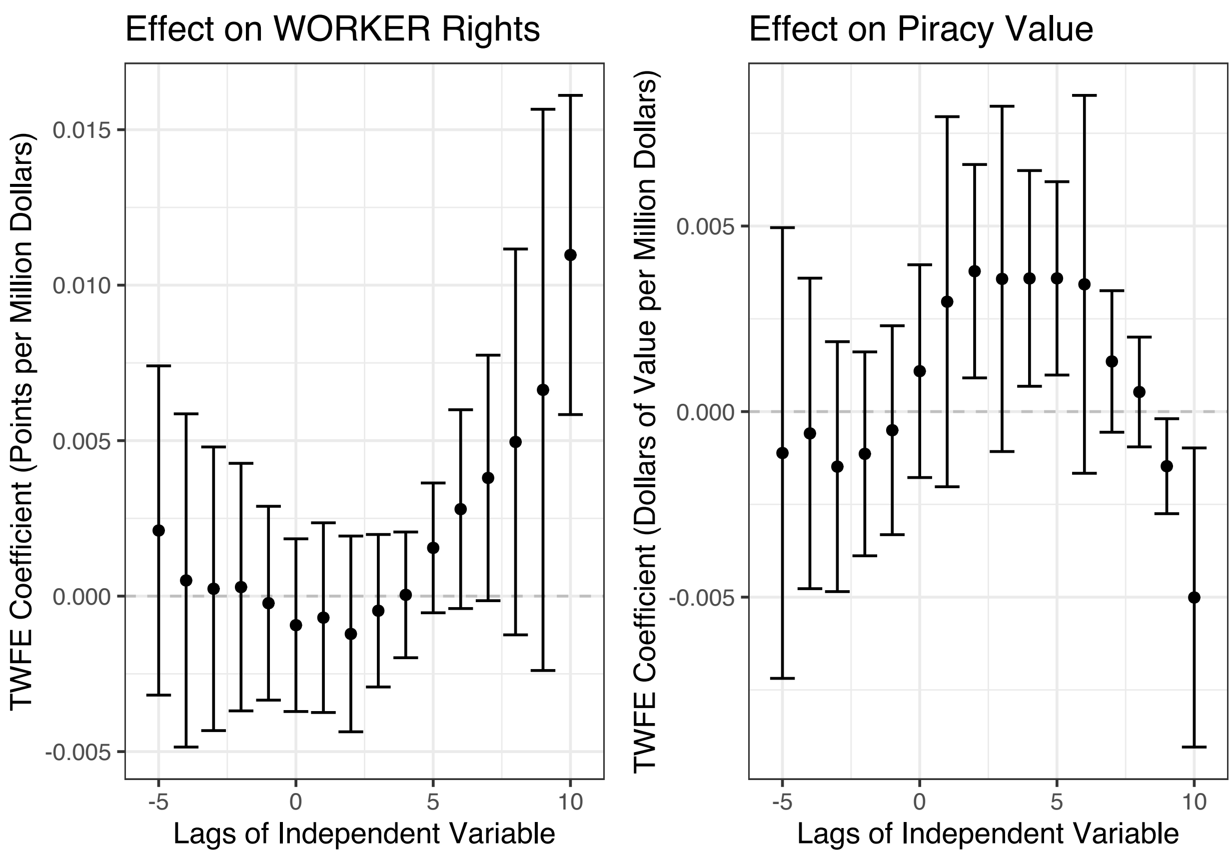

Figure A3: Effects on Alternative Outcomes

Limit to Post 2002

Most of the variation in preference erosion occurs during the 1990s. There were many important global political events in the 1990s that could theoretically affect compliance. To demonstrate that the effects are not dependent in this particular time period, Figure A4 reproduces the analysis after dropping all observations before 2002. The figure shows that the results are robust to this exclusion, demonstrating that the effects are not concentrated in the 1990s.

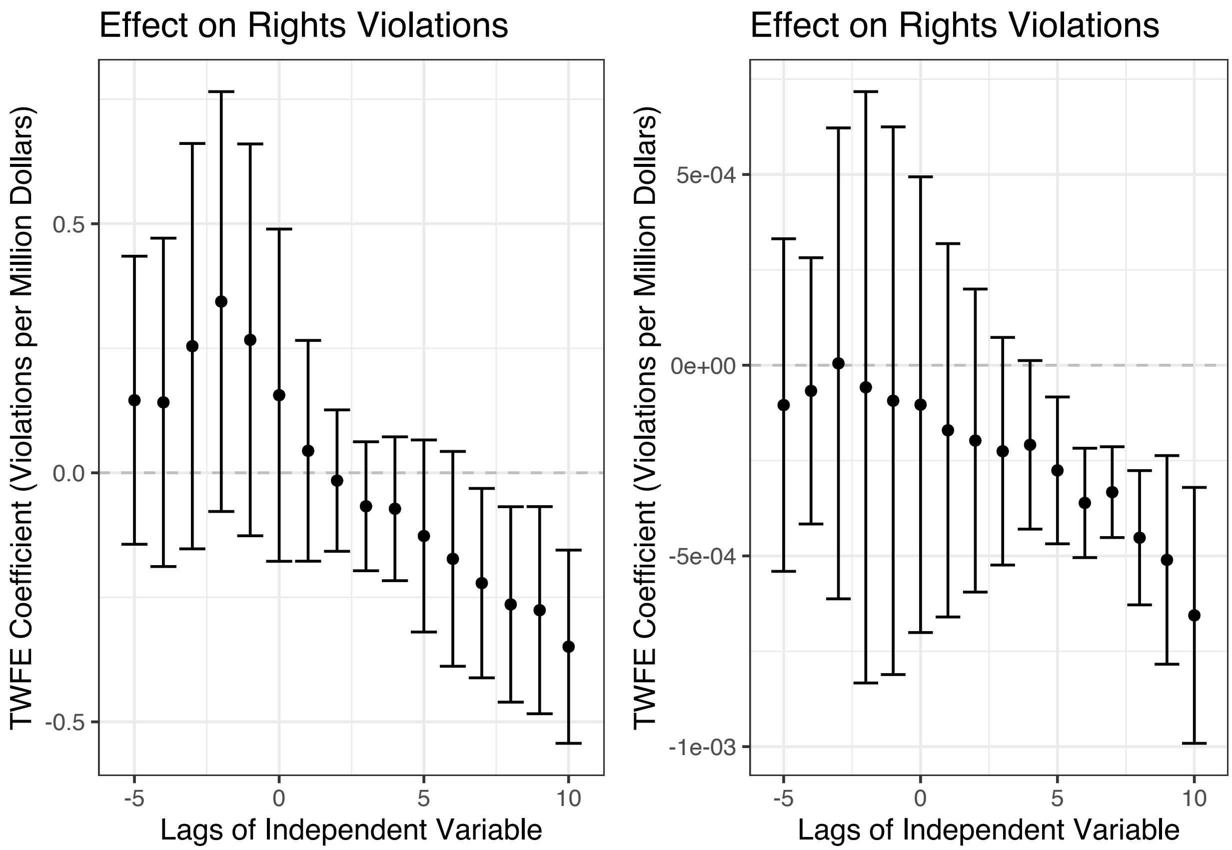

Figure A4: Results when limiting to years post 2002.

Drop Non GSP States

The main specification should be robust to including states that never enter the GSP. These states should always be counted as "control" units and should not affect the results other than by increasing their precision. To demonstrate that their inclusion in the analysis does not contaminate the results, the main table is reproduced in Table A7 after dropping all states that never enter the GSP. In these regressions, the control units are the states that are not in the GSP in that period. Results are very similar to the main specification demonstrating that no contamination occurred.

Table A7: Drop all states that are ineligible for GSP

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=8)	-0.302^{***}	-0.258^{***}	-0.0005^{***}	-0.0005^{***}
	(0.104)	(0.099)	(0.0001)	(0.0001)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes
Observations	1,345	1,044	601	571
Note:	*p<0.1; **p<0.05; ***p<0.01			

Include Additional Lags of Covariates

There are methodological concerns about "post-treatment bias" when variables that could be affected by treatment are included as controls in regressions. The construction of the independent variable makes it unlikely that any of the control variables will induce this bias. To ensure that no control variables are affecting the estimate of the treatment effect, all control variables are lagged by an additional 2 years in Table A8. The results are qualitatively similar to the main results.

Table A8: Value of GSP Membership and Compliance with Additional Lags.

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=8)	-0.273^{**}	-0.273^{***}	-0.0004^{***}	-0.0002^{*}
	(0.112)	(0.084)	(0.0001)	(0.0001)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Year * Any GSP Fixed Effects	Yes	Yes	Yes	Yes
Covariates Lagged	No	2 periods	No	2 periods
Observations	1,298	1,004	765	581
Note:	*p<0.1; **p<0.05; ***p<0.01			