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

Michael-David Mangini

March 2024
Word Count: 8,648

Abstract

States often receive threats of economic coercion only after committing some transgression. While attention has been given to strategic selection in the imposition of sanctions – the notion that sanctions are imposed only after the threat of sanctions has failed to elicit compliance – selection also occurs at the threat stage. Does the possibility of economic coercion incentivize states to stay in line before they are directly threatened? This article makes two contributions. First, it presents a theory showing how the interaction between different types of threats can alter the strategic context of economic coercion in ways that potentially reverse the results of empirical analysis. It explains that states are likely to issue general threats, or threats targeting the behavior of a group of countries, when sanctions effectiveness is unknown or the identity of noncompliers is difficult to predict. Second, using quasi-experimental techniques, this article demonstrates that states actively manage the risk of losing access to a trade preference program by altering their policies. The article indicates the political consequences of economic linkages may be more extensive than previously believed.

1 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 to insinuate that their trading partners had better behave themselves before resorting to direct threats of economic coercion against any particular state.[1] Indeed, states do sometimes strongly imply that punishment is possible without threatening a specific 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. Does the risk of losing market access to programs like the GSP incentivize states to stay in line before they are directly threatened?

One important challenge of studying the effectiveness of the GSP and any other program of economic coercion is properly accounting for the strategic environment. Scholars have previously recognized that punishments are only imposed when the threat of punishment was insufficient to change behavior (Nooruddin 2002; Drezner 2003; Bapat and Kwon 2015). Comparably little attention has been paid to strategic selection at the threat stage of economic coercion. And yet, states may consider the risk of reprisals before engaging in behavior that might draw the ire of important trading partners. To study strategic selection at the threat stage, the paper distinguishes between general and specific threats. General threats are threats to revoke market access from any noncompliant state while specific threats only apply to a particular noncompliant state. The GSP is a general threat to remove trade prefer-

---

[1]Economic coercion is defined as a commitment to conditional market access for any state behaving in a compliant manner.

ences from any state that does not comply with conditionality in the areas of labor rights and intellectual property rights. The effect of this general threat is estimated using state-year level variation in the magnitude of the economic incentives to maintain membership in the GSP. The results of the empirical analysis suggest that the program causes states to change behavior even before being specifically threatened. An implication of the results is that trade conditionality could be far more effective than previously thought.

The distinction between general and specific threats also illuminates why economic coercion is not always perceived as credible, even when the sender state is resolved to endure the costs of sanctions. General threats issued without ever being backed up by specific threats might be questioned by potential targets – would the sender be as willing to penalize a close trading partner as a distant one? Specific threats issued without a general threat might also lack credibility – would the sender act in a discriminatory manner against one state without acknowledging that other states are also in violation? But a specific 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. When both types of threats are present the behavior of target states falls into one of three categories: those that comply with the general threat because they found it credible; those that spurn the general threat but come into compliance following a specific 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.

This paper uses a case study of the GSP to study how states react to the risk of economic coercion even before being directly threatened. 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.[2] The most

---

[2]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

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. Carnegie (2014) searches for evidence of "political hold-up problems" by examining trade flows at the dyadic level while Carnegie and Marinov (2017) uses quasi-experimental variation in the European Union's development aid allocations to identify its effect on human rights and democracy promotion. Miller (2014) shows how the threat of sanctions has discouraged states from pursuing nuclear weapons even before sanctions were imposed and Marinov (2005) studies the way that sanctions might "invisibly" deter noncompliance by threatening to destabilize target regimes. Other related work including Hafner-Burton (2005) has studied how human rights conditionality in preferential trade agreements has improved human rights protections.

2 Case Selection and Background on the Generalized System of Preferences

More than 130 states are eligible for the United States's Generalized System of Preferences (GSP) which has been conditional on policy choices since 1984.[3] 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

---

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.

[3]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). Note that the legality of conditionality in trade preference programs was established by the Enabling Clause of the General Agreement on Tariffs and Trade in 1979 (Stamberger 2003). The program expired on December 31, 2020 and efforts to renew the program are ongoing as of the time of writing.

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 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.[4]

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 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.

It is necessary to comment on the choice of the GSP as a setting for studying economic coercion. To be sure, the GSP program and other preference programs around the world are seen as development aids available to states that have respect for certain rights. The reader might be hesitant to study a tool of aid as an example of coercion. As David A. Baldwin (1971) makes clear, positive sanctions are an im-

---

[4]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.
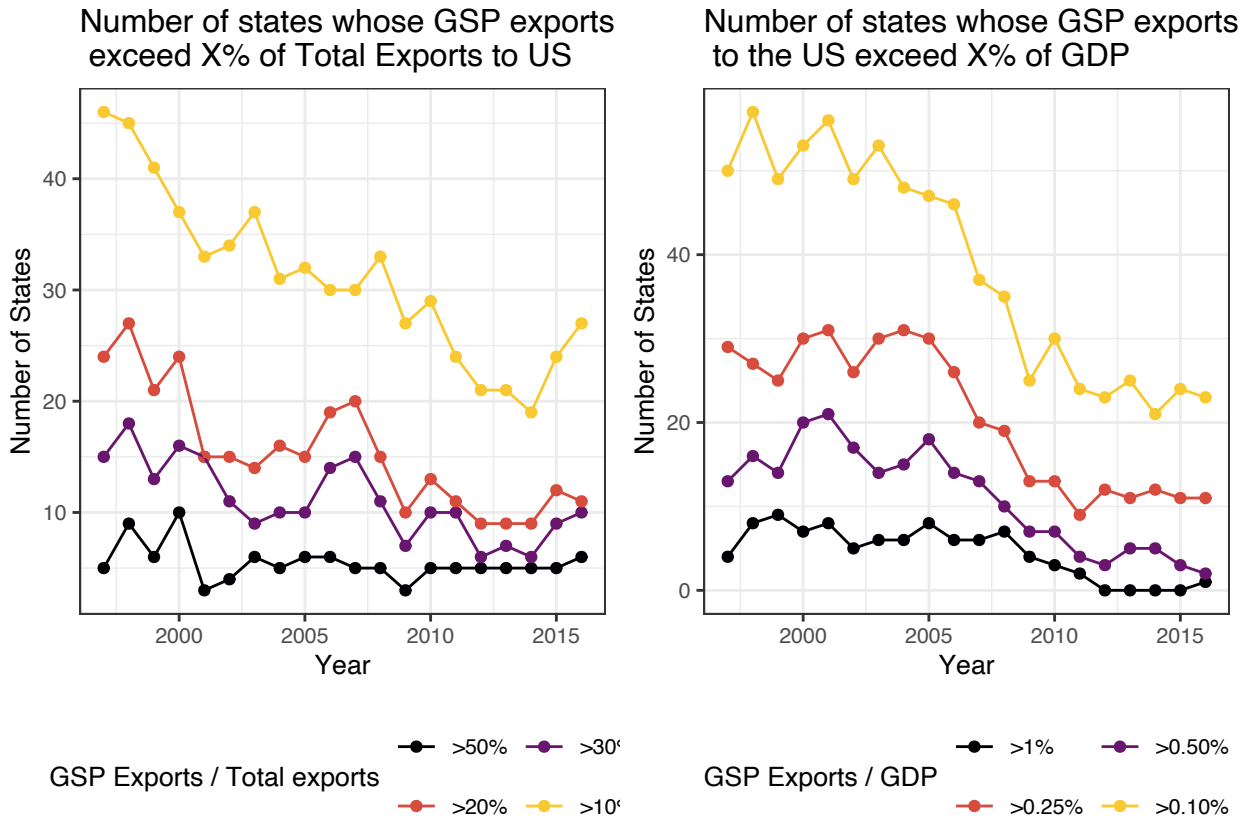
Figure 1: Data from Schott. The number of states exporting at least a certain fraction of their total exports to the US under the GSP is shown over time. For example, in 2000 there were 30 states for which at least 10% of their total exports to the US crossed the border under the program. Exports relative to GDP are smaller and both are declining over time, reflecting the declining value of program membership.

portant tool of economic statecraft in their own right. But just because the program is designed to help certain developing states does not mean that it cannot be used as an instrument of economic coercion. Indeed, the GSP meets the criteria for economic coercion including Baldwin's definition of negative sanctions – it threatens to withdraw material benefits from states that do not behave in a particular way.[5] Finally, like other forms of aid conditionality, the GSP has been previously studied as an example of economic coercion (Elliott 1998; Drezner 2003).[6]

3   Strategic Choice of Threat Type: Theory and Concepts

Strategic selection is the specter lurking beneath most 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.[7] The literature has acknowledged this problem and addressed it by searching for cases where punishments were threatened but not imposed (David Allen Baldwin 1985; Eaton and Engers 1992; Smith 1995; 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). Drezner (2003) and Lacy and Niou (2004) called on scholars to mitigate the problem by studying threats to impose sanctions.[8] But

---

[5]The GSP uses economic coercion to promote respect for rights abroad. The nobility of a program's aims does not insulate it from the label of "economic coercion." Other states may use similar programs to less virtuous ends.

[6]There is a large and growing literature on aid conditionality. See, for example, Svensson (2000) and Drazen (2002). There is also a large literature on the politics of IMF lending and conditionality including Stone (2008) and Dreher, Sturm, and Vreeland (2015).

[7]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).

[8]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 an important resource for sanctions studies ever since. Whang, McLean, and Kuberski (2013) combines a model similar to that of Lacy and Niou (2004) with the TIES data to structurally

threats of punishment are subject to a similar selection problem. For example, states that maintain rights protections because of the GSP incentives might never be formally threatened with expulsion from the program. Because general threats could incentivize many states simultaneously they can potentially change the behavior of more countries than specific threats used in isolation. Furthermore, economic coercion should be most effective in states that change behavior without ever receiving a specific sanctions threat.

3.1 General and Specific Threats

This section introduces the concepts of general and specific threats and explains how they relate to the selection problems. General threats consist of 1) a group of two or more potential target states to whom conditionality could be applied and 2) the conditionality itself, which is a commitment to conditional market access for any state behaving in a compliant manner. A specific threat informs a particular state that they are not in compliance and could be denied market access.[9] The presence or absence of a general threat is critical to understanding the behavior of a potential target when faced with a specific threat. 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 2010). Both of these conclusions are sensitive to the strategic context of a general threat. For example, consider the possibility that multilateral sanctions are typically imposed in the context of a general threat while unilateral

---

estimate the importance of the sanctions threat stage. Other important studies using the TIES data to account for specific 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) is also a significant contribution.

[9]Earlier work on selection effects in the context of deterrence includes Huth and Russett (1984) and Fearon (2002). Most related, Fearon describes a similar layered strategic selection problem in the context of deterrence and military conflict. This article uses the terms "general" and "specific" differently from Fearon, who used "general" and "immediate" to distinguish between initial and subsequent challenges to the status quo.

sanctions are more often imposed without a general threat. In that case, states receiving specific threats from multilateral coalitions are probably quite different from states receiving specific threats from single senders. The states facing sanctions from a multilateral coalition are more likely to have already chosen to violate the general threat and therefore might be inclined to also violate the specific threat. Targets facing a specific threat from a single sender without having previously been aware of a general threat might be considering the possibility of punishment for the first time, and might quickly change their behavior.[10] These results matter 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 these exact empirical results.

As illustrated in Figure 2, the presence of general threats alters the strategic context in ways that can actually reverse the interpretation of empirical results.[11] The figure shows stages of economic coercion and the probability of transition between each stage as represented by the thickness of the lines. Case I shows a program of economic coercion where specific threats are issued without a prior general threat. As drawn, the figure shows an ineffective sanctions program where few targets react to the threat of sanctions or to the sanctions themselves. Case II illustrates a program of economic coercion with exactly the same transition probabilities as Case I but with one major difference: a previous general threat gives targets the opportunity to comply before receiving a specific threat. In this case, the sanctions are effective because most states would rather comply than risk the possibility of sanctions, and so never take actions that would lead to a specific threat. The reason this

---

[10]This example scenario describes results where unilateral sanctions are more effective, but could be adjusted to produce the opposite result. The point is that conclusions about effectiveness are sensitive to the strategic context of a general threat.

[11]Fearon (2002) makes a similar point in the context of deterrence. This article extends and adapts the point from military conflict to economic coercion. Doing so requires discussing whether and why senders might choose to issue a general threat, how these threats can be made credibly in the context of economic coercion, and how these threats can be identified.

Case I: Threats are ineffective. States are noncompliant after threats and sanctions.

Specific Threat

Sanctions

Compliance Noncompliance
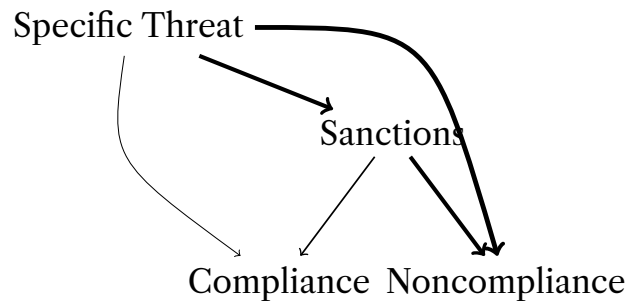
Case II: Threats are effective. The only states who receive specific threats previously violated the general threat.

General Threat

Specific Threat

"Speak softly and carry a big stick"
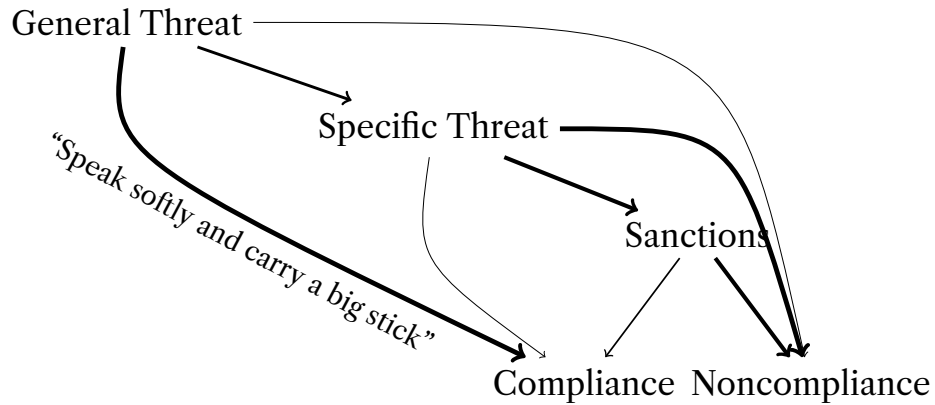
Sanctions

Compliance Noncompliance

Figure 2: Diagram illustrating the advantage of considering general threats. The thickness of each line represents a transition probability between nodes of the diagram.

distinction matters is that Cases I and II, which are only distinguished by the general threat, lead to opposite conclusions about the effectiveness of sanctions. 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).

How should general threats be identified? Must they be written as laws or might they take other forms? Other than the GSP, examples of general threats that are codified in law include the Global Magnitsky Act of 2016, which allows the US government to sanction "any foreign person" who is responsible for extrajudicial killings, torture, or other gross violations of internationally recognized human rights, among other similar abuses.[12] Section 307 of the Tariff of 1930 prohibits the importation of all goods made "in any foreign country by convict labor", establishing a general threat. Section 307 is rarely invoked while the Global Magnitsky Act has been deployed against individuals from 42 countries (Casey 2023; Ferullo and Smith 2023). Whether either law causes potential targets to increase compliance even before being threatened is an empirical matter. Another example, this time from outside the realm of sanctions and from outside the United States, are the Maastricht criteria necessary to adopt the euro currency. These economic criteria constitute a general "threat" because they indicate that any European state adhering to five conditions can receive membership in the monetary union. A specific "threat" in this context would be a notification to an existing member of the monetary union that they were not in compliance with the criteria and were at risk of discharge from the union.

Although the GSP has been codified in law, not all general threats will be so institutionalized. Consider the expectation, described by Miller (2014), that states pursuing nuclear weapons will be subject to sanctions. While there is no institution which formally establishes this expectation, there is evidence to suggest that states find this

---

[12]The Global Magnitsky Act is implemented as Subtitle F of the National Defense Authorization Act for Fiscal Year 2017 which became law on December 23, 2017.

to be a credible general threat.

3.2 Choosing a General Threat: Uncertainty and Credibility

There are at least three strategic circumstances that would lead states to choose to issue general sanctions threats. First, states can use general threats to mitigate uncertainty about the nature of potential targets. There are at least two sources of uncertainty about the effectiveness of a specific threat. First, the sender may not be able to predict which potential targets are at risk of noncompliance across regions and time. It could be that a coup or other unforeseen event dramatically changes the disposition of a potential target that could threaten compliance. Under such circumstances, the sender might be able to maintain the target's compliance by virtue of an existing general threat. Second, the sender may not perceive the political importance of its trade in every potential target. It could be that the sender is trading a particularly important good with a target without knowing it, and this trade is therefore a powerful incentive for the potential target to generate compliance. A general threat can discover such wells of influence without requiring the sender to obtain detailed knowledge about the internal domestic politics of every potential target.

Second, states wishing to adhere to the rules based order may prefer general threats. One purpose of a rules based international order is to encourage smaller states to participate in the global economy without fear of being subject to economic coercion by larger states (Carnegie 2014; Ikenberry 2001). One way to bolster the rules based international order is for large states to proscribe behaviors without targeting individual states. By orienting towards behaviors, unilateral action might be perceived as less threatening by smaller states who understand that a general threat is not necessarily directed at any one of them.

Third, the issuance of specific and general threats may also create strategic spillovers. Using both in tandem could produce more compliance than either in

isolation. A specific threat directed at one state could enhance the credibility for an existing general threat. For example, the specific sanctions threat against Iran's nuclear program bolstered the general threat that any state pursuing nuclear weapons would face consequences. States might even choose to follow through on specific threats for the sole purpose of bolstering the credibility of general threats. Scholars have argued that states may remain committed to sanctions, even when the sanctions are unlikely to succeed, in order to protect their reputation for resolve (Kertzer 2016; Jervis, Yarhi-Milo, and Casler 2021). Not all specific threats necessarily bolster the credibility of a general threat – some specific threats could be perceived as being too idiosyncratic to apply more generally. The reputation of the sender could influence whether states perceive specific threats as narrow or as evidence of a more general threat.

What determines whether specific threats enhance or undermine the credibility of a general threat? The broad scope of general threats could be interpreted by potential targets as a mark of unseriousness. For example, larger states whose trade with the sender is more valuable might reasonably wonder whether the general threat truly applies to them. The effect of specific threats on the credibility of the general threat depends on how potential targets update their beliefs about the sender's resolve. Specific threats directed at one larger state might convince other large states that the general threat really does apply to them. However, if the only specific threats are issued to small states, the large states might decide that the general threat will not be enforced against them. The sender may choose to issue specific threats for the purpose of bolstering the beliefs of similar potential targets that the general threat really does apply to them.

Not all states will deploy general threats in every instance. It may be that the sender really does want to limit the scope of a particular threat to just one specific target state. It could also be costly to issue general threats that must be enforced fre-

quently. For example, a sender state that issues a general threat to withhold market access from any state that does not extend reciprocal duty free access to its firms would be at risk of denying itself access to foreign markets – a very high cost. However, issuing a specific threat to withhold a degree of market access from one specific state as part of a trade negotiation could be more effective.

4   Research Design

The GSP constitutes a general threat for over 130 developing states. Most developing states are eligible for the program as long as they respect the conditionality.[13] As described in Section 2, 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. Thus, the GSP offers an opportunity to compare general and specific sanctions threats.

It is not clear whether specific threats should be more or less effective than the general 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 specific threats. If the general threat does not have credibility then states might be more willing to comply after receiving an specific threat.

4.1   Data and Sample

Given the theoretical emphasis on strategic selection, the sample for the empirical testing must be carefully chosen. To capture the idea of general threats the sample must include all states that could potentially be subject to expulsion from the

---

[13]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.

GSP – not only those which were specifically threatened or those which were actually expelled. Thus, the unit of analysis will be the state-year. The objective of the empirical exercise is to determine whether states that had bigger incentives to maintain GSP membership also maintained better compliance with the program's conditionality.

What are appropriate outcome variables 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.[14]

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 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.[15] 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,

---

[14]There are some other criteria, including most notably a market access condition, but these are much less prominently discussed.

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

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.

4.2 Identification

The effect of the general threat on compliance must be estimated using a source of plausibly exogenous variation. It is important that this variation be plausibly exogenous to determine how states would have behaved if the threat were absent. 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.[16] 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

---

[16]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" (Alexandraki and Lankes 2004; Francois, Hoekman, and Manchin 2006; Amiti and Romalis 2007). To the author's knowledge, the implications of preference erosion for compliance with conditionality have not previously been studied.

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 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? One concrete concern could be that preference erosion might correlate with rights abuses because products made in places without secure rights were so cheap that US industries demanded tariff protection before the negotiations.[17] It is not possible to rule out such unobserved confounding completely. However, it is unlikely for at least three reasons that cause the severity of preference erosion to be largely outside the control of both the United States and the beneficiary countries.

First, the pre-negotiation tariff rates were often chosen without giving any consideration to GSP beneficiaries. The initial tariff rates are an important consideration because they mechanically limit the amount of preference erosion – tariff rates cannot decline below zero, so the initial rates affect the total possible decline. Three reasons support the exogeneity of the pre-negotiation rates. First, the MFN tariff rates were not protecting US industries against competition from GSP beneficiaries. Membership in the GSP meant that the products could be imported duty free. Therefore, no US interest group would lobby for an MFN tariff on the basis of competition from a GSP beneficiary. Second, the pre-negotiation rates were often determined decades before the multilateral negotiations. Acosta and Cox (2024) document tariff persistence in the US tariff code. Third, the tariff rates were often almost completely determined by domestic US politics. As described by Schattschneider (1935), the rates in tariff bills were heavily influenced by log rolling in Congress.

---

[17]I would like to thank an anonymous reviewer for drawing attention to this point.

Representatives traded support for tariffs that protected industries in each others' districts. Therefore, the pre-negotiation rates depended on US domestic political considerations from, in many cases, decades prior to the sample period. These pre-negotiation rates were generally applied broadly at the product level, not to particular states, further boosting their exogeneity.

Second, the developing states that were eligible for the GSP had little direct influence over the determination of the tariff declines during negotiations in this period. 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. Of course, the nature of multilateral negotiations encourages consensus, including consensus from developing states. However, the influence of any single developing state in the complex multilateral negotiations was quite limited. Even GSP beneficiary states that might have had influence over the negotiations had little incentive to use it to lobby for lower tariffs in GSP products. Members of the GSP already had the option to export most products duty free to the United States, so there was no direct need to expend political capital to lower the MFN tariffs on those same products.

Third, the product mixture exported by each country to the United States is unique. The incentive to maintain GSP membership depends on the sum total of benefits across product categories. Because different developing states export different products to the United States, and these products are unevenly benefited by the GSP, the degree of preference erosion at the state level varies significantly. An important determinant of the export profile is comparative advantage. Variation in the export profile of any particular developing state due to its factor endowments and geography can also boost the plausibility of exogeneity of preference erosion.

4.3 Estimation

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; Wooldridge 2021). 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.[18] The coefficient of interest, τ, should be negative under the hypothesis that the GSP program conditionality meaningfully increases rights protections. Due to the continuous nature of treatment and the staggered nature of treatment timing, estimation follows Wooldridge (2021) which uses extra fixed effects to improve robustness.

Results regarding petitions as specific threats in the main text are estimated using the Callaway and Sant'Anna (2021) estimator. This estimator is appropriate in this instance because the petitions are a discrete treatment. The Callaway and Sant'Anna

---

[18]Detailed information about the covariates are available in Appendix A1. The controls include political variables including regime type and relationship with the United States, as well as economic variables like GDP size and trade.

([2021](#)) estimator is robust to heterogeneous effects, an important property in this setting because specific threats could be differently effective at different times, depending on trade flows and political context. The same dependent variables and control variable as are used in these regressions as in the main analysis on general threats.

5 Results

5.1 Petitions as Specific Threats

Table 1: Average treatment effect on the treated estimated via Callaway and Sant'Anna (2021) and aggregated by 'group' using the R package did. The outcome variable 'WORKER' is from Cingranelli and Richards (2014) and is described in the Appendix. Standard errors are bootstrapped and the control group is the never treated units.

	Dependent Variable	ATT	Covariates
1	Rights Violations	-4.039 (4.449)	No
2	Rights Violations	2.862 (6.266)	Yes
3	WORKER	-0.163^{*} (0.098)	No
4	WORKER	-0.18^{**} (0.088)	Yes
5	Piracy Rate	0.053^{***} (0.016)	No
6	Piracy Rate	0.061^{***} (0.018)	Yes

The structure of the GSP makes it possible to directly observe the benefits of studying general sanctions threats relative to specific 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 1 shows the results when using petitions – a measure of specific threats – as the independent variable. The primary measure of respect for labor rights, "Rights Violations", shows no significant effect in response to petitions. An alternate measure, described in more detail in the appendix, shows a negative and statistically significant relationship. The piracy rate also has a positive and statistically significant relationship with petitions. These two results indicate worsening respect for labor and intellectual property rights in response to petitions. This result is not explained by pre-treatment trends because the estimator compares the effect with the trajectory of a control group. Other specifications, which are less precisely estimated, are available in the Appendix.

Table 1 illustrates a positive relationship between rights violations and petitions for GSP review. However, as discussed in the theory section, these results do not necessarily indicate that the GSP is ineffective at promoting respect for rights. States that receive petitions are tolerating 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. Evaluating the effectiveness of the program requires an evaluation of the general threat to ascertain whether states that do not receive petitions are complying because of the GSP's incentives.

5.2 The GSP as a General Threat

The main results are reported in Table 2 and show support for the notion that the GSP is an effective general threat. The analysis indicates that each $1 million of GSP value reduces personal integrity rights violations 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.

Table 2: 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: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Nonetheless, the results are statistically significant using standard errors clustered by year and state. Clustering can 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 shown in Table 2, 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 2 show the results where the value of GSP membership has been lagged by 8 years. Results for other lags are shown in Figure 3, which indicates that the effects are similar in size and statistical significance in neighboring periods. These results are suggestive about how the general risk of economic coercion shapes the behavior of states. In the absence of a specific 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. These mechanisms are consistent with the modest success of trade agreements that successfully promote human rights: the GSP has a clear mechanism of enforcement that can promote the interests of domestic rights activists (Hafner-Burton 2010, chap. 6).

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 2. 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.

5.3 Robustness of the Results

A number of robustness checks intended to rule out specific confounders and inference problems are included in the Appendix. Tables A4 and A5 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 A10 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
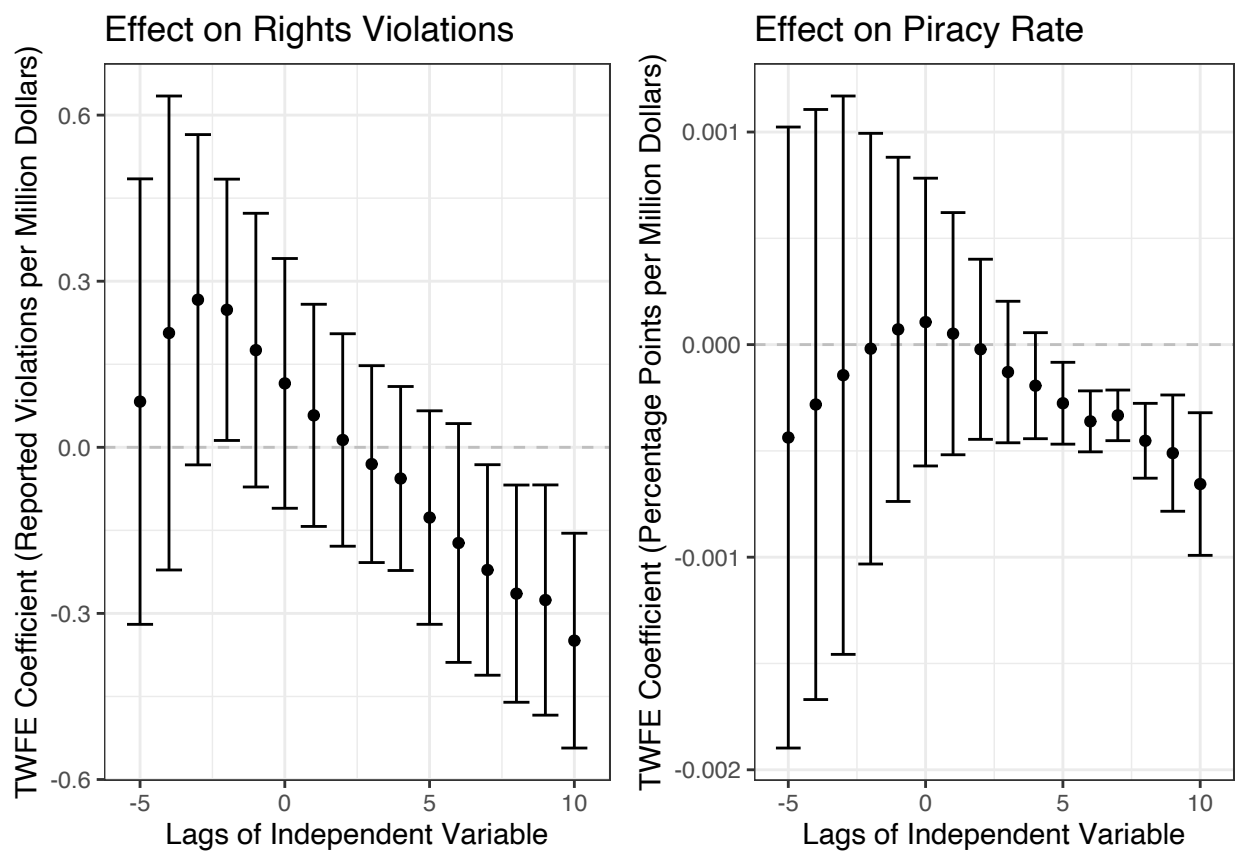
Figure 3: Results from main specification.

results are not contingent on particular measures. Figure A5 shows results are similar using an alternative independent variable inspired by the Bartik instrument which is arguably more robust to concerns about endogeneity (Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2022). The alternative independent variable interacts each country's share of US imports at the product level (exposure weights) with the preference margin (shocks). Table A8 shows additional results for the alternative independent variable. 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. This robustness check also ensures that the results are not due to changes in the sample when estimating for different lags. for how the sample varies when Finally, the main results are reproduced after dropping all non-GSP eligible states in Table A9 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 A6 and A7 shows the results when controlling for possibly differential trends among various groupings of states by other measures. Table A10 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 3 is reproduced with additional leads of the independent variable in Figure A2.

6 Conclusion

This article proposes a that specific and general threats exhibit strategic interaction that profoundly affects the interpretation of empirical data. States are more likely to defy a specific threat if they have previously defied a general threat. Failure to acknowledge the distinction can lead to the erroneous conclusion that a program

of economic coercion is ineffective when it is profoundly effective in reality. In an empirical application of the theory, the article shows that the GSP is an example of one such program. States that receive petitions seem likely to continue violating the conditionality, but states that have the highest incentives to comply with the conditionality typically do even without being directly confronted.

Although the scope of the empirical analysis only includes the GSP, the evidence presented here has broader implications for the literature on economic coercion. Previously, scholars have studied instances of applied or specifically threatened economic coercion. The framework developed by this paper illuminates the importance of distinguishing between general and specific threats. It is likely that many existing estimates of the effectiveness of economic coercion are attenuated because they examine only specific sanctions threats. Future research can determine the extent to which previous scholarship has understated the importance of economic coercion.

7 References

Acosta, Miguel, and Lydia Cox. 2024. "The Regressive Nature of the US Tariff Code: Origins and Implications."

Alexandraki, Katerina, and Hans Peter Lankes. 2004. "The Impact of Preference Erosion on Middle-Income Developing Countries."

Amiti, Mary, and John Romalis. 2007. "Will the Doha Round Lead to Preference Erosion?" *IMF Staff Papers* 54 (2): 338–84.

Angrist, Joshua D, and Jörn-Steffen Pischke. 2008. *Mostly Harmless Econometrics*. Princeton university press.

Baldwin, David A. 1971. "The Power of Positive Sanctions." *World Politics* 24 (1): 19–38.

Baldwin, David Allen. 1985. *Economic Statecraft*. Princeton University Press.

Bapat, Navin A, Tobias Heinrich, Yoshiharu Kobayashi, and T Clifton Morgan. 2013. "Determinants of Sanctions Effectiveness: Sensitivity Analysis Using New Data." *International Interactions* 39 (1): 79–98.

Bapat, Navin A, and Bo Ram Kwon. 2015. "When Are Sanctions Effective? A Bargaining and Enforcement Framework." *International Organization* 69 (1): 131–

62.

Bapat, Navin A, and T Clifton Morgan. 2009. "Multilateral Versus Unilateral Sanctions Reconsidered: A Test Using New Data." *International Studies Quarterly* 53 (4): 1075–94.

Blanchard, Emily, and Shushanik Hakobyan. 2015. "The US Generalised System of Preferences in Principle and Practice." *The World Economy* 38 (3): 399–424.

Blanchard, Emily, and Xenia Matschke. 2015. "US Multinationals and Preferential Market Access." *Review of Economics and Statistics* 97 (4): 839–54.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2022. "Quasi-Experimental Shift-Share Research Designs." *The Review of Economic Studies* 89 (1): 181–213.

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225 (2): 200–230.

Carnegie, Allison. 2014. "States Held Hostage: Political Hold-up Problems and the Effects of International Institutions." *American Political Science Review* 108 (1): 54–70.

Carnegie, Allison, and Nikolay Marinov. 2017. "Foreign Aid, Human Rights, and Democracy Promotion: Evidence from a Natural Experiment." *American Journal of Political Science* 61 (3): 671–83.

Casey, Christopher. 2023. "Section 307 and Imports Produced by Forced Labor." Washington, DC, USA: CRS Report No. IF11360. https://crsreports.congress.gov/product/pdf/IF/IF11360.

Cingranelli, David L., David L. Richard, and K. Chad Clay. 2014. "The CIRI Human Rights Dataset."

Compa, Lance, and Jeffrey S Vogt. 2000. "Labor Rights in the Generalized System of Preferences: A 20-Year Review." *Comp. Lab. L. & Pol'y J.* 22: 199.

Cordell, Rebecca, C Clay, Christopher J Fariss, Reed M Wood, and Thorin Wright. 2019. "Recording Repression over Space and Time: Identifying Allegations in Annual Country Human Rights Reports." Research report.

Cordell, Rebecca, K Chad Clay, Christopher J Fariss, Reed M Wood, and Thorin M Wright. 2020. "Changing Standards or Political Whim? Evaluating Changes in the Content of US State Department Human Rights Reports Following Presidential Transitions." *Journal of Human Rights* 19 (1): 3–18.

Drazen, Allan. 2002. "Conditionality and Ownership in IMF Lending: A Political Economy Approach." *IMF Staff Papers* 49 (Suppl 1): 36–67.

Dreher, Axel, Jan-Egbert Sturm, and James Raymond Vreeland. 2015. "Politics and IMF Conditionality." *Journal of Conflict Resolution* 59 (1): 120–48.

Drezner, Daniel W. 2000. "Bargaining, Enforcement, and Multilateral Sanctions: When Is Cooperation Counterproductive?" *International Organization* 54 (1): 73–102.

———. 2003. "The Hidden Hand of Economic Coercion." *International Organization* 57 (3): 643–59.

Eaton, Jonathan, and Maxim Engers. 1992. "Sanctions." *Journal of Political Economy* 100 (5): 899–928.

Elliott, Kimberly Ann. 1998. "The Sanctions Glass: Half Full or Completely Empty?" *International Security* 23 (1): 50–65.

Fearon, James D. 2002. "Selection Effects and Deterrence." *International Interactions* 28 (1): 5–29.

Felbermayr, Gabriel, Aleksandra Kirilakha, Constantinos Syropoulos, Erdal Yalcin, and Yoto V Yotov. 2020. "The Global Sanctions Data Base." *European Economic Review* 129: 103561.

Ferullo, Mark, and Lucie Smith. 2023. "GloMag Sanctions Database: Baseline Data on Human Rights Sanctions." Center for Human Rights; Humanitarian Studies at Brown University.

Francois, Joseph, Bernard Hoekman, and Miriam Manchin. 2006. "Preference Erosion and Multilateral Trade Liberalization." *The World Bank Economic Review* 20 (2): 197–216.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review* 110 (8): 2586–2624.

Grauvogel, Julia, Amanda A Licht, and Christian von Soest. 2017. "Sanctions and Signals: How International Sanction Threats Trigger Domestic Protest in Targeted Regimes." *International Studies Quarterly* 61 (1): 86–97.

Hafner-Burton, Emilie M. 2005. "Trading Human Rights: How Preferential Trade Agreements Influence Government Repression." *International Organization* 59 (3): 593–629.

———. 2010. "Forced to Be Good." In *Forced to Be Good*. Cornell University Press.

Hafner-Burton, Emilie M, Layna Mosley, and Robert Galantucci. 2018. "Protecting Workers Abroad and Industries at Home: Rights-Based Conditionality in Trade Preference Programs." *Journal of Conflict Resolution* 1: 30.

Hakobyan, Shushanik. 2015. "Accounting for Underutilization of Trade Preference Programs: The US Generalized System of Preferences." *Canadian Journal of Economics/Revue Canadienne d'économique* 48 (2): 408–36.

———. 2017. "Export Competitiveness of Developing Countries and US Trade Policy." *The World Economy* 40 (7): 1405–29.

Head, Keith, and Thierry Mayer. 2014. "Gravity Equations: Workhorse, Toolkit, and Cookbook." In *Handbook of International Economics*, 4:131–95. Elsevier.

Hufbauer, Gary Clyde, and Jeffrey J Schott. 1985. "Economic Sanctions and US Foreign Policy." *PS: Political Science & Politics* 18 (4): 727–35.

Hufbauer, Gary Clyde, Jeffrey J Schott, and Kimberly Ann Elliott. 1990. *Economic Sanctions Reconsidered: History and Current Policy*. Vol. 1. Peterson Institute.

Huth, Paul, and Bruce Russett. 1984. "What Makes Deterrence Work? Cases from 1900 to 1980." *World Politics* 36 (4): 496–526.

Ikenberry, G. John. 2001. *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order After Major Wars, New Edition-New Edition*. Princeton University Press.

Jervis, Robert, Keren Yarhi-Milo, and Don Casler. 2021. "Redefining the Debate over Reputation and Credibility in International Security: Promises and Limits of New Scholarship." *World Politics* 73 (1): 167–203.

Kaempfer, William H, and Anton D Lowenberg. 1999. "Unilateral Versus Multilateral International Sanctions: A Public Choice Perspective." *International Studies*

Quarterly 43 (1): 37–58.

Kertzer, Joshua. 2016. *Resolve in International Politics*. Vol. 2. Princeton University Press.

Lacy, Dean, and Emerson MS Niou. 2004. "A Theory of Economic Sanctions and Issue Linkage: The Roles of Preferences, Information, and Threats." *The Journal of Politics* 66 (1): 25–42.

Lindsay, James M. 1986. "Trade Sanctions as Policy Instruments: A Re-Examination." *International Studies Quarterly* 30 (2): 153–73.

Marinov, Nikolay. 2005. "Do Economic Sanctions Destabilize Country Leaders?" *American Journal of Political Science* 49 (3): 564–76.

Martin, Lisa L. 1994. *Coercive Cooperation: Explaining Multilateral Economic Sanctions*. Princeton University Press.

McLean, Elena V, and Taehee Whang. 2010. "Friends or Foes? Major Trading Partners and the Success of Economic Sanctions." *International Studies Quarterly* 54 (2): 427–47.

Miers, Anne, and T Morgan. 2002. "Multilateral Sanctions and Foreign Policy Success: Can Too Many Cooks Spoil the Broth?" *International Interactions* 28 (2): 117–36.

Miller, Nicholas L. 2014. "The Secret Success of Nonproliferation Sanctions." *International Organization* 68 (4): 913–44.

Morgan, T Clifton, Navin Bapat, and Yoshiharu Kobayashi. 2014. "Threat and Imposition of Economic Sanctions 1945–2005: Updating the TIES Dataset." *Conflict Management and Peace Science* 31 (5): 541–58.

Morgan, T Clifton, Navin Bapat, and Valentin Krustev. 2009. "The Threat and Imposition of Economic Sanctions, 1971—2000." *Conflict Management and Peace Science* 26 (1): 92–110.

Morgan, T Clifton, and Yoshiharu Kobayashi. 2021. "Talking to the Hand: Bargaining, Strategic Interaction, and Economic Sanctions." *European Economic Review* 134: 103685.

Morgan, T Clifton, and Anne C Miers. 1999. "When Threats Succeed: A Formal Model of the Threat and Use of Economic Sanctions." In *Annual Meeting of the American Political Science Association, Atlanta, GA*. Vol. 264.

Nooruddin, Irfan. 2002. "Modeling Selection Bias in Studies of Sanctions Efficacy." *International Interactions* 28 (1): 59–75.

Office of the United States Trade Representative. 2017. "U.s. Generalized System of Preferences Guidebook." Executive Office of the President.

Pape, Robert A. 1997. "Why Economic Sanctions Do Not Work." *International Security* 22 (2): 90–136.

———. 1998. "Why Economic Sanctions Still Do Not Work." *International Security* 23 (1): 66–77.

Schattschneider, Elmer Eric. 1935. "Politics, Pressures and the Tariff."

Smith, Alastair. 1995. "The Success and Use of Economic Sanctions." *International Interactions* 21 (3): 229–45.

Stamberger, Jennifer L. 2003. "The Legality of Conditional Preferences to Developing Countries Under the GATT Enabling Clause." *Chi. J. Int'l L.* 4: 607.

Stone, Randall W. 2008. "The Scope of IMF Conditionality." *International Organization* 62 (4): 589–620.

Svensson, Jakob. 2000. "When Is Foreign Aid Policy Credible? Aid Dependence and Conditionality." *Journal of Development Economics* 61 (1): 61–84.

United Nations Conference on Trade and Development. 2016. "Handbook on the Scheme of the United States of America." United Nations.

Whang, Taehee, Elena V McLean, and Douglas W Kuberski. 2013. "Coercion, Information, and the Success of Sanction Threats." *American Journal of Political Science* 57 (1): 65–81.

Wooldridge, Jeff. 2021. "Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators." *Available at SSRN 3906345*.

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

June 2022

Appendix Table of Contents

A Control Variables

Table A1 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.

name	mean	median	min	max	description	Source	Purpose
rights_violations_topcode	44.19	29.00	0.00	148.65	Rights Violations (Topcoded)	Cordell et al. (2019)	Main Dependent Variable
WORKER	0.92	1.00	0.00	2.00	Labor Rights	Cingranelli et al. (2014)	Alternate Dependent Variable
piracy.rate	0.62	0.65	0.19	1.00	Piracy Rate	Collected by Author	Main Dependent Variable
pts_stdpt	2.39	2.00	1.00	5.00	State Department Terror Index	Hafner-Burton, Mosely, and Galantucci (2018)	Control for GWOT significance
polity	0.89	1.00	-10.00	10.00	Polity	Hafner-Burton, Mosely, and Galantucci (2018)	Control for regime type
totalimportslog	7.79	8.14	0.00	11.65	Total Value of US Exports (log)	Hafner-Burton, Mosely, and Galantucci (2018)	Control for general trade importance
any_export	2254.33	1019.00	4.00	10214.00	Number of Exported Products	Schott Data	Control for breadth of trade
gdp_log	23.20	23.06	16.40	30.56	GDP (log)	World Bank	Control for economy size
military.deployment	1767.39	12.00	0.00	218506.00	US Military Deployment	Kane (2006, 2016)	Control for US strategic interest

Table A1: Table of Variables

B Petitions as Specific Threats

Tables A2 and A3 test the effect of petitions in two ways. Table A2 tests whether a petition being filed in the last three years has an effect on compliance according to the two main outcome measures. Table A3 tests whether the *acceptance* of a petition effects compliance. The regression specification is identical to that used to make Table 2 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 2 because petitions and reviews are not exogenous and because the sample size is much smaller.

Due to the relative rarity of the petitions, effects in this specification 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 2, 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.

Table A2: 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 A3: 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 reproduces Figure 3 but using sanctions reviews as the independent variable. The results are consistent with Table A3 and indicate that they are not sensitive to the number of lags.

C   Robustness of Main Results

C.1   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 A4 and A5. 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.
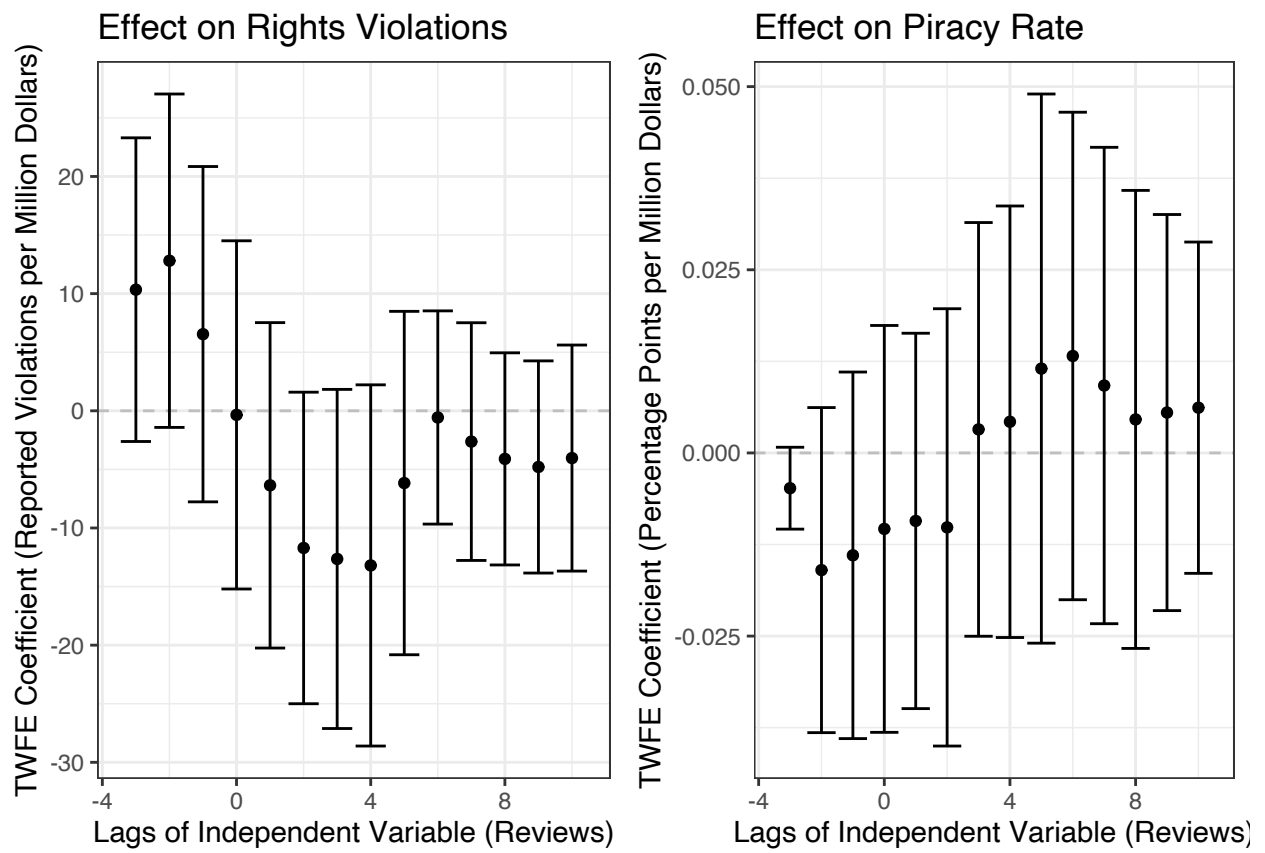
Figure A1: Plot of Effects at Long Lags Using Reviews

Table A4: 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 A5: 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

C.2 Differential Trends

To demonstrate the robustness of the specification, different versions of the main results with various fixed effects are reported in Tables A6 and A7. 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 A6: 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 A7: 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

C.3 Longer Leads Plots of the Regressions

Figure A2 extends Figure 3 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.
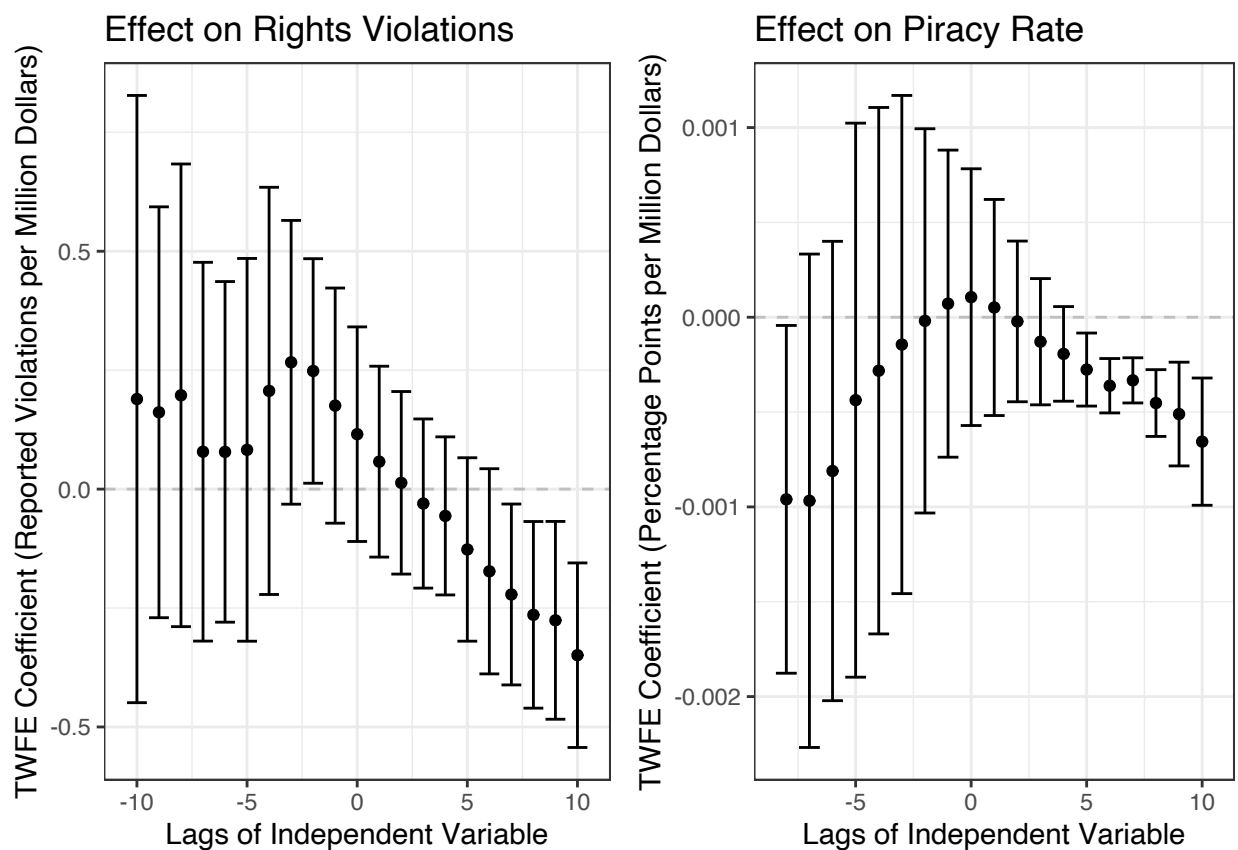

Figure A2: Plot of Effects at Long Lags

C.4 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.
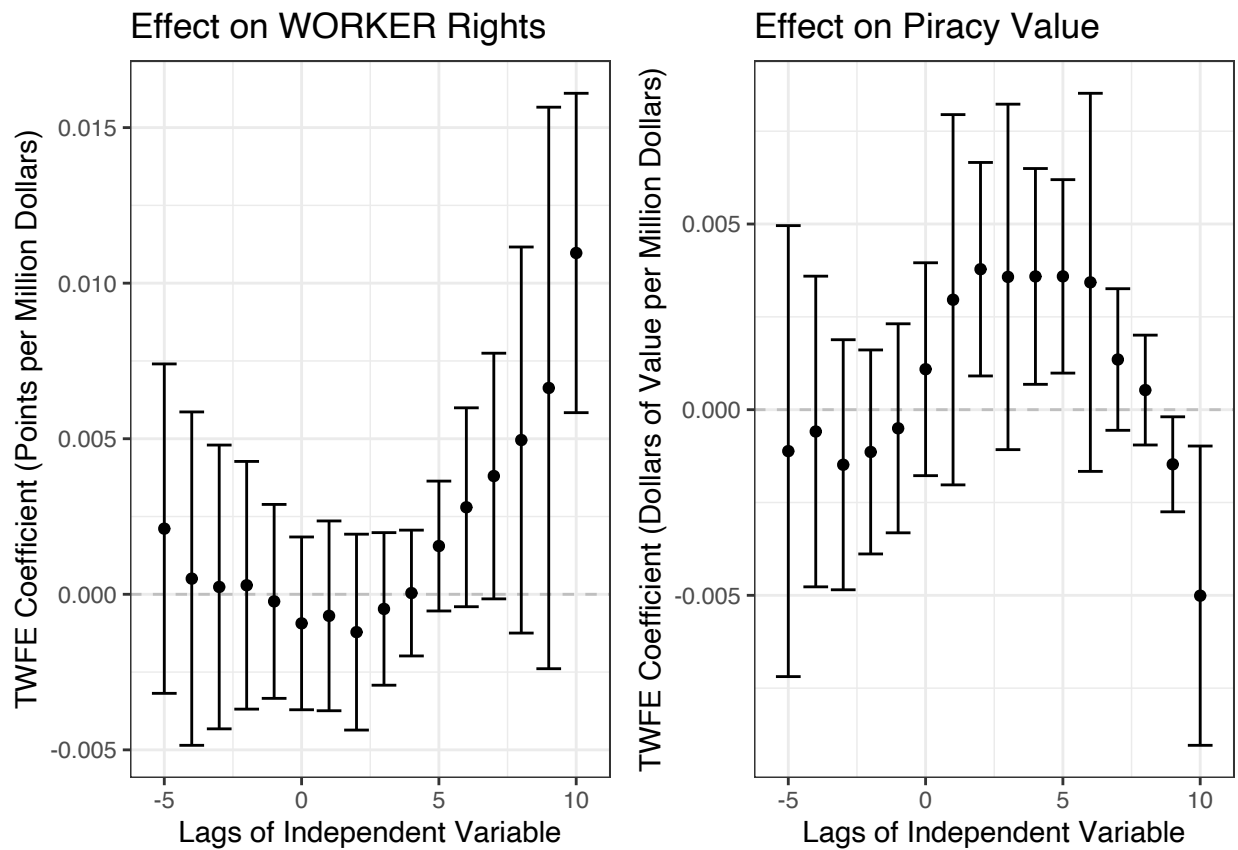

Figure A3: Effects on Alternative Outcomes

C.5 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.
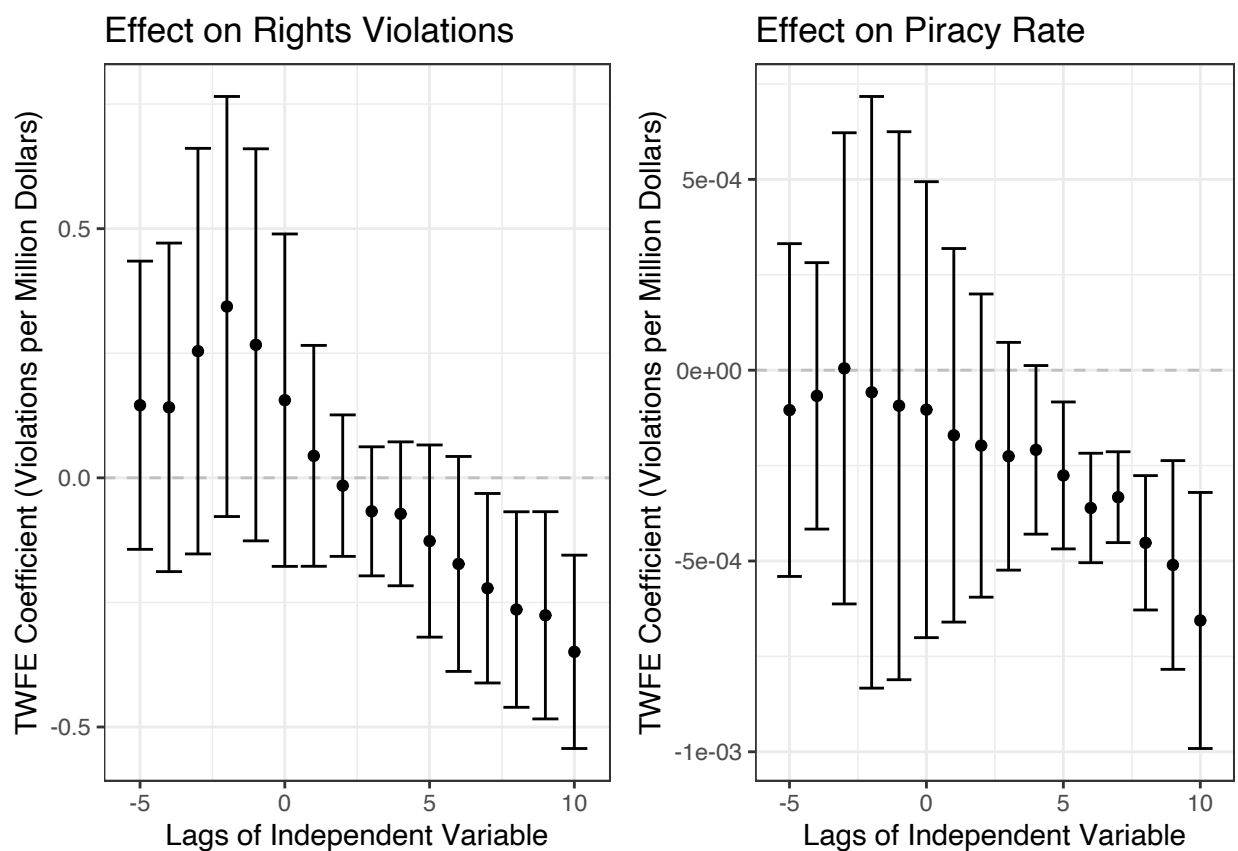

Figure A4: Results when limiting to years post 2002.

C.6 Alternate Independent Variable: Bartik

This section shows robustness for an alternative independent variable inspired by the Bartik instrument. The Bartik instrument, or "shift-share" instrument, is an average of shock magnitude weighted by exposure to the shock (Borusyak, Hull, and Jaravel 2022). In this case, the shock magnitude is the preference margin at the product level and the exposure weight is the country's share of all US imports of a given good at the product level. Exogeneity is driven by the preference margins and the exposure weights increase the relevance of the variable. The analysis in Table A8 replicates the main results using the alternative measure. Note that the results do not use the alternate independent variable as an instrument and the regression remains a diff-in-diff type analysis. Figure A5 also shows a wider range of lags and leads.

Table A8: Value of GSP Membership and Compliance via Bartik. Shares are relative to US market for imports.

	Dependent variable:			
	Rights Violations		Piracy Rate	
	(1)	(2)	(3)	(4)
Value of GSP Membership (lag=9, Bartik)	−250.460	−709.179**	−0.629**	−0.687**
	(180.519)	(279.940)	(0.279)	(0.284)
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	2,102	1,167	725	684

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

C.7 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
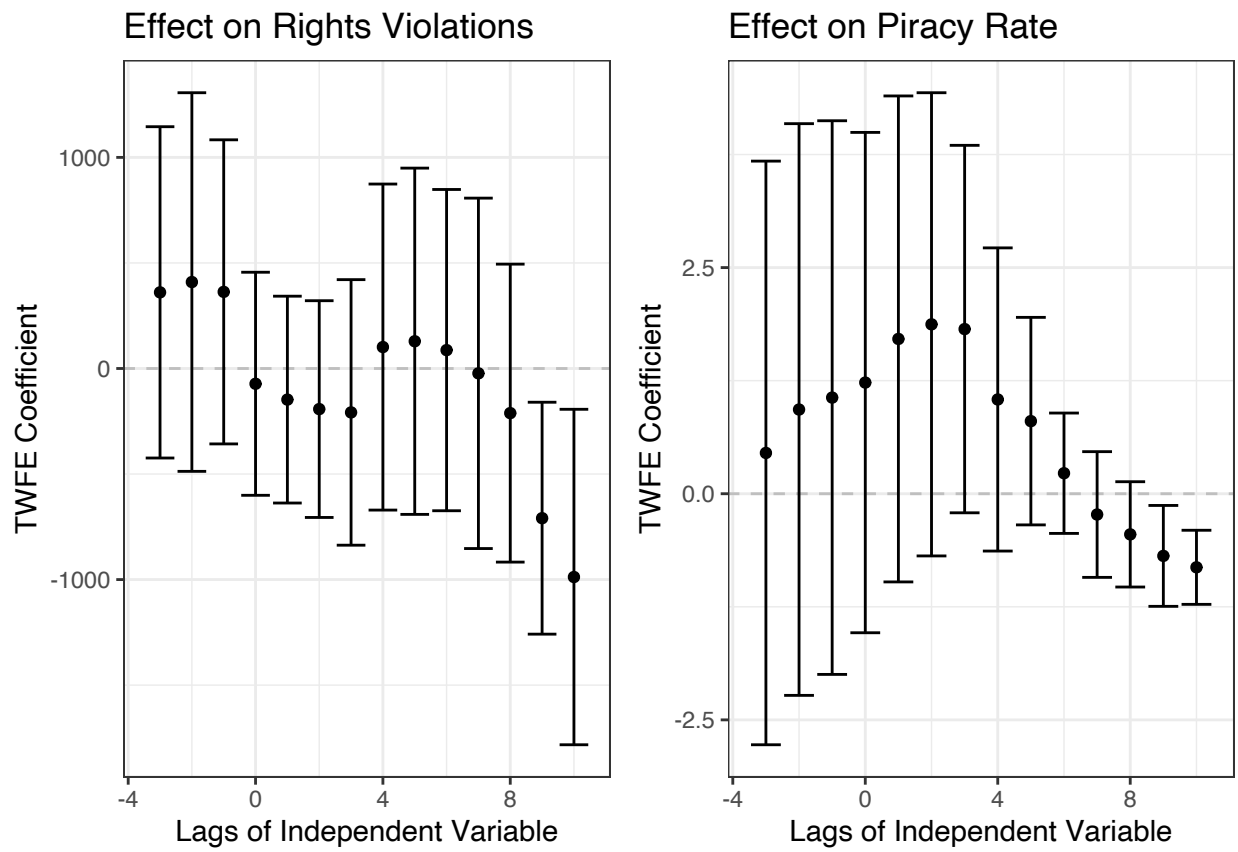
Figure A5: Results when using an alternate independent variable inspired by the Bartik instrument.

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 A9 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 A9: 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	

C.8 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 A10. The results are qualitatively similar to the main results.

Table A10: 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	