THE EFFECTIVENESS OF ENVIRONMENTAL PROVISIONS IN REGIONAL TRADE AGREEMENTS

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Abstract

Trade liberalization can spur environmental degradation. Concerns over these adverse impacts have led to a debate over the need for environmental provisions in regional trade agreements (RTAs); however, the effectiveness of such provisions is unknown. In this paper, we provide new plausibly causal evidence that environmental provisions are effective in limiting deforestation following the entry into force of RTAs. We exploit high resolution, satellite-derived estimates of deforestation, and identify the content of RTAs using a new dataset with detailed information on individual provisions. Accounting for the potential endogeneity of environmental provisions in RTAs, we find that the inclusion of specific provisions aimed at protecting forests and/or biodiversity almost entirely offsets the net increases in forest loss observed in similar RTAs without such provisions. The effects are particularly strong in tropical, developing countries with greater biodiversity. The inclusion of these provisions limits agricultural expansion and agricultural trade. (JEL: F14, F18, Q23, Q27, Q28, Q56, Q17)

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1. Introduction

The past 30 years have seen an unprecedented push for trade liberalization with 262 regional trade agreements (RTAs) involving 188 countries entering into force over this period. While the reduction in trade barriers offers tremendous potential for economic growth and productivity gains, the impacts of trade liberalization may not be universally positive. In particular, there is mounting evidence suggesting that increased global trade may lead to environmental degradation (e.g., Leblois, Damette, and Wolfersberger 2017; Zhang et al. 2017; Abman and Lundberg 2020), which has attracted attention from policy makers, scholars, and the public alike. Indeed, the potential for increased deforestation in Brazil has become a critical stumbling block that has all but halted the ongoing ratification process of the EU-Mercosur trade agreement. Concern over potential adverse impacts of opening trade has stimulated a debate on whether trade agreements should include specific provisions to limit the negative consequences thereof. While some see provisions in trade agreements targeting the environment as a form of thinly veiled protectionism, others perceive them as an important tool for mitigating potential harm from opening trade and a commitment device for environmental policy reform (Frankel 2009).[1] Despite negotiations and policy discussions around the inclusion of such provisions, there has been little in the way of careful, rigorous work exploring whether these provisions actually function as designed.

Assessing whether environmental provisions in trade agreements work is as difficult as it is important. First, until recently, there was a scarcity of detailed information on the specific provisions included in trade agreements. Second, from a methodological point of view, carefully assessing the impact of environmental provisions on environmental outcomes presents a number of econometric challenges that stem from the non-random inclusion of such provisions.

In this paper, we study the effectiveness of environmental provisions in trade agreements in mitigating environmental harm. We use high-resolution, satellite-derived estimates of deforestation as a measure of environmental damage. Deforestation is one of the most pressing environmental challenges of the modern era, both in its threat to biodiversity through the destruction of sensitive habitat as well as its prominent role in global climate change through associated greenhouse gas emissions. The extent of forest loss in the past 30 years has been unprecedented: On net, the world lost approximately 178 million hectares (nearly 440 million acres) of forest area between 1990 and 2020 (FAO 2020). In addition to an important environmental outcome in its own right, satellite-derived deforestation measures are also spatially explicit, local measures of environmental harm that circumvent many limitations associated with administrative data on environmental damage.

1. Environmental provisions in trade agreements represent the type of "issue linkages", discussed in Maggi (2016), in which different policy areas are intertwined through international agreements. Ederington (2010), in particular, provides a survey of the literature on the linkages between trade and environmental policies in international agreements.

We test whether the inclusion of provisions in RTAs aimed at protecting forests and/or preserving biodiversity mitigate the ecological impacts of trade liberalization. We use a novel dataset with detailed information on the content of all RTAs in force and notified to the WTO between 1958 and 2018 (Monteiro and Trachtman 2020) and combine these data with satellite-derived estimates of annual forest loss for 193 countries. We estimate the predicted probability that an RTA includes a forest and/or biodiversity provision via the use of machine-learning techniques and a variety of RTA and group-level characteristics informed by a reduced-form political economy framework. We then use these predicted probabilities to match RTAs that include these provisions to similar RTAs without them to create an appropriate set of counterfactuals. We construct a panel dataset on total forest loss for all countries that join a given RTA and, using a triple-difference model, test whether aggregate increases in forest loss associated with RTA enactment are lower for RTAs with environmental provisions than those without. We validate these primary findings using an alternative identification strategy based on a multiple event study model at the country level that features different identifying variation. This alternative approach yields similar qualitative and quantitative findings and provides supporting evidence that our results describe a common, underlying causal response to the inclusion of environmental provisions in RTAs.

We find large, significant net increases in annual forest loss following the enactment of RTAs without provisions (23%) and that the inclusion of these provisions almost entirely offsets this increase. This effect is largely driven by changes to forest loss in tropical, developing countries with high levels of biodiversity—the locations where deforestation is of greatest concern. We then investigate the mechanisms through which forestry and biodiversity provisions in RTAs mitigate environmental damage. Analysis using the same matched triple-difference approach indicates that environmental provisions limit agricultural expansion that otherwise occurs following the entry into force of trade agreements. RTAs without these environmental provisions lead to increases in agricultural production and agricultural exports in high-risk regions following entry into force. The provisions appear to mitigate or reverse these gains to agricultural production and trade. We find corresponding evidence that government interventions in agricultural and forestry markets—as evidenced by expenditures in these sectors—rise following trade liberalization but that the inclusion of environmental provisions limits such increases. We find that separate, supplemental dispute settlement mechanisms unique to these environmental provisions are not any more effective in mitigating forest loss than general-scope dispute settlement mechanisms in RTAs.

Our primary contribution with this paper is to provide the first plausibly causal evidence on the effectiveness of environmental provisions in RTAs. In particular, we show that the inclusion of forest-related provisions has mitigated forest loss resulting from trade liberalization. Furthermore, to our knowledge, this work is some of the first to develop a strategy to identify causal effects of RTA content more broadly.

Our work contributes to the large literature studying the effects of trade agreements. This literature has consistently found that trade agreements significantly increase trade flows between member countries.[2] The more recent strand of this work has emphasized the importance of the varying content of RTAs in assessing their trade effects. Baier, Bergstrand, and Feng (2014) find evidence of differential trade effects from different types of agreements (e.g., partial scope agreements, free trade agreements, or custom unions), while Mattoo, Mulabdic, and Ruta (2017) show that "deep" agreements (i.e., RTAs that cover a larger number of policy areas beyond tariff reduction) are associated with a stronger trade impact. The literature on trade agreements that focuses on non-trade issues such as labor and the environment is mostly theoretical and focuses on the conditions under which issue linkage in trade agreements is beneficial (e.g., Ederington 2002; Limão 2007; Maggi 2016). Notable exceptions include Abman and Lundberg (2020), who study the impact of RTAs on deforestation, Baghdadi, Martinez-Zarzoso, and Zitouna (2013), who study the impact of RTAs with environmental clauses on emissions, and Brandi et al. (2020), who investigate whether environmental provisions in RTAs make exports from developing countries greener.

This paper is closely related to two papers in the literature that consider the environmental impact of RTAs. Abman and Lundberg (2020) study the impacts of RTAs on country-year forest loss in an overlapping event study framework—focusing on plausibly causal identification of deforestation dynamics around trade liberalization. Abman and Lundberg (2020) find that deforestation significantly increases following entry into force of RTAs with evidence suggesting that this is driven by agricultural land expansion in tropical developing countries. While our research setting is similar to Abman and Lundberg (2020), the research question and econometric approach in the present paper differ in important ways. We study whether non-tariff provisions in RTAs affect non-trade outcomes. As the content of trade agreements is endogenously formed at the RTA-level, rather than at the country level, we develop an entirely new framework relative to Abman and Lundberg (2020), who allow us to directly tackle the issue of endogenous RTA content.

This paper is also closely related to Baghdadi, Martinez-Zarzoso, and Zitouna (2013), which considers the impact of environmental content in RTAs—very broadly defined—on emissions gaps between signatory countries. The trade literature has developed a work-horse framework to address endogeneity arising from two countries non-randomly entering into an RTA with each other. Saturated three-way fixed effects Poisson regressions that include country-pair fixed effects are able to control for the pairwise likelihood that any two countries sign a trade agreement. Baghdadi, Martinez-Zarzoso, and Zitouna (2013) adopt a similar approach in their consideration of outcomes y_{ijt} at the country-pair, year level (emissions gap between countries i and j in year t). While the FE approach used in Baghdadi, Martinez-Zarzoso, and Zitouna (2013) controls for endogeneity in selection into an RTA between i and j that may be correlated with their emissions gap (as in Baier and Bergstrand 2007b, etc.), it does not directly address endogeneity in the *content* of the trade

2. Seminal works include Baier and Bergstrand (2007a); Egger et al. (2011); Bergstrand, Larch, and Yotov (2015). See Limão (2016) for a survey.

agreements. Instead, they estimate "provision effects" by comparing a subset of the sample of RTAs they consider to only those that contain environmental content broadly defined and then reestimate their main model specification. Our paper substantively differs from Baghdadi, Martinez-Zarzoso, and Zitouna (2013) along two important dimensions. First, our outcomes are not bilateral in nature—requiring a very different econometric framework. Second, to answer our primary research question, we develop an identification strategy that explicitly addresses this source of endogeneity.

This paper also contributes to the literature considering the relationship between trade and the environment. Much of this literature studies the effects of trade on pollution (Antweiler, Copeland, and Taylor 2001; Frankel and Rose 2005; Managi et al. 2009; Kreickemeier and Richter 2014; Cherniwchan 2017), while other work has considered the relationship between trade and renewable resource management (Brander and Taylor 1998; Hotte, Van Long, and Tian 2000; Bulte and Barbier 2005; Copeland and Taylor 2009; Taylor 2011; Erhardt 2018). A subset of this literature has specifically studied the effects of trade on deforestation (Barbier and Rauscher 1994; Sohngen, Mendelsohn, and Sedjo 1999; Hannesson 2000; Barbier and Burgess 2001; López and Galinato 2005; Barbier, Damania, and Léonard 2005; Leblois, Damette, and Wolfersberger 2017; Abman and Lundberg 2020). Very little of the literature has established causal evidence of the impacts of trade on the environment. Most of these papers rely on either cross-sectional variation or within-country variation in observed trade volumes or trade measures to study this relationship using either pooled ordinary least squares or fixed-effects regressions. Erhardt (2018) and Leblois, Damette, and Wolfersberger (2017) examine trade and environmental outcomes using fixed-effects regressions with lagged trade variables. Abman and Lundberg (2020) and Alix-Garcia et al. (2018) provide causal estimates of the impacts of trade liberalization on forest cover change in a modern, global sample RTAs, and the historical context of the Austro-Hungarian customs union, respectively. Harstad (2020) studies the role that trade liberalization can play in deforestation via a theoretical model of North–South trade and argues that a conservation-sensitive agreement can reduce the negative environmental consequences from RTAs.

We also contribute to the literature on issue linkages in international agreements. This literature is primarily theoretical (see, e.g., Maggi 2016; Ederington 2001, 2002, 2010; Limão 2005) and, to our knowledge, our paper is the first work to empirically test the effectiveness of issue linkages between trade and environmental policies. Indeed, we believe this paper to be one of the first, if not the first, to use quasi-experimental methods to empirically investigate the impacts of trade-policy linkages on non-trade outcomes.[3]

Our work also makes two methodological contributions. First, we develop a framework to plausibly identify causal impacts of provisions in trade agreements. While the literature considering trade agreement content is still nascent, the availability

3. The limited empirical findings on issue linkages have focused on trade-related outcomes rather than linked non-trade outcomes (e.g., Limão 2007)

of new detailed data on trade agreement provisions will open many avenues for research in this area (Mattoo, Rocha, and Ruta 2020). The central challenge to identification when assessing the impacts of individual provisions arises from the non-random nature underlying the content of RTAs. Previous empirical work has addressed the non-random nature of RTA enactment on the extensive margin by matching country pairs that enact RTAs to similar country pairs that do not (Baier and Bergstrand 2004, 2007a; Egger, Egger, and Greenaway 2008). Such a framework may not be appropriate when evaluating individual provisions as it does not address the non-random nature of the content of RTAs. Our framework aggregates outcomes to the RTA-bloc level and utilizes a combination of panel data methods and propensity score matching to create appropriate counterfactuals for RTAs that include provisions of interest—similar RTAs that do not include such provisions. This approach could be relevant to assess the impact of individual provisions in RTAs well beyond the specific issue studied in this paper.

Our second methodological contribution is to the literature on cluster-robust covariance matrices (see, e.g., Cameron and Miller 2015; Abadie et al. 2017). We introduce a modification of the cluster-robust covariance matrix that allows for sparse cross-cluster correlation. Cross-cluster correlation arises in our empirical setting because our panel of trade agreements—our clustering unit—feature overlapping country membership. We leverage information on the structure of the cluster overlap to create a weighting function that restricts most cross-cluster correlations to be zero—as in the standard clustering approach—but allows for limited cross-cluster correlation between clusters with overlapping membership.

The remainder of the paper proceeds as follows: In Section 2, we discuss RTAs and detail the data sources used in our analysis. In Section 3, we present the methodology for our agreement level, matched panel approach. Section 4 presents the results from our primary, agreement-level analysis and explores potential mechanisms that could explain our findings. In Section 6, we present an alternative empirical framework—namely, a flexible country-level overlapping event study approach that allows us to estimate differential dynamics in forest loss around RTA enactment for RTAs that contain environmental provisions and those that do not. Finally, we conclude with a discussion of the implications of our findings.

2. Background and Data

2.1. Regional Trade Agreements and Agreement Provisions

Regional trade agreements are a technical umbrella term defined by the World Trade Organization to include free trade agreements, customs unions, partial scope agreements, and economic integration agreements.[4] RTAs have proliferated in the past

4. Under WTO and GATS definitions, free trade agreements establish a group of countries in which goods are tariff-free within the group, but member countries set their own tariffs on imports from outside

three decades. The number of RTAs, which had remained low and stable since the 1950s, increased from 50 in the early 1990s to roughly 300 by 2020. Over this same time period, RTAs have also expanded their scope. While the average RTA in the 1950s covered 8 policy areas, in recent years they have averaged 17 (Hofmann, Osnago, and Ruta 2017). Trade agreements that used to regulate areas such as tariffs, customs and trade remedies, increasingly cover non-trade areas such as competition policy, investment or intellectual property rights. The number of commitments that countries undertake in the context of RTAs has also increased considerably—most notably since 2000—and these deepening commitments have been accompanied by an increase in regulatory requirements, namely on enforcement. The changing nature of RTAs is documented by a new database by the World Bank informing on the detailed content of 295 RTAs signed between 1958 and 2018—i.e., all RTAs in force and notified to the WTO (Mattoo, Rocha, and Ruta 2020).

The literature on issue linkage provides an economic rationale for the inclusion of non-trade provisions such as environmental protection, labor and human rights, or provisions concerning national security in trade agreements. The literature highlights at least three reasons why provisions aimed at protecting the environment might be included in trade agreements. Foremost is the presence of interdependencies between trade policy and environmental outcomes—provisions aimed at protecting the environment might limit possible negative environmental outcomes that may result from opening trade.[5] Second, environmental provisions in trade agreements introduce the possibility of trade retaliation to enforce environmental commitments, which would otherwise be difficult to enforce. Third, linking trade policy and environmental policy through RTAs introduces the opportunity to offer trade policy concessions in exchange for more stringent commitments on environmental protection as part of the trade agreement[6] (or, equivalently, to induce the participation of a trade partner to an international environmental agreement). In equilibrium, the extent of issue linkage is determined by the relevance of these gains and the contracting costs created by the increasing dimensionality of the trade agreement (Horn and Mavroidis 2014).

Our data on environmental provisions in RTAs have been collected as part of the broader World Bank project on the content of trade agreements and are described in

the group. Customs unions are similar to free trade agreements but with a single external tariff rate for goods originating outside the group. Partial scope agreements similarly feature tariff reductions but for a limited scope of goods. Economic integration agreements are similar to free trade agreements but cover trade in services.

5. In a strict sense, the interdependency between issues is not per se a reason for linkage. Governments could negotiate two separate agreements on trade and the environment taking into account the externalities if they had correct expectations on the policy commitments that would emerge from both agreements. In practice, this is unlikely, which motivates our consideration of interdependencies as a rationale for issue linkage between trade and the environment.

6. In Limão (2007), non-trade issues are modeled as regional public goods. A large country offers preferential tariff reductions in an RTA with a smaller trade partner in exchange for the partner's cooperation on the non-trade issue. As a result, welfare of the two countries is higher under a trade agreement that links both trade and non-trade issues.

detail in Monteiro and Trachtman (2020). This is the most extensive effort to date to document environmental provisions in trade agreements. First, the coding covers environmental provisions included in the environmental chapter or in other chapters of trade treaties and, when present, in side agreements on the environment.[7] Second, the environmental provisions coded include environmental goals, specific commitments, compliance with multilateral environmental agreements, enforcement mechanisms, and external assistance and collaboration. Within this set of environmental provisions, we select two that are specifically important to protect forest resources:

- Does the agreement require measures to prevent deforestation and/or require sustainable trade practices in forest products?
- Does the agreement require states to promote and protect biodiversity?

While the relevance of first type of provision is clear, the second type of provision focusing on biodiversity protection is also relevant for forest outcomes. Tropical forests are home to more than 60% of all *global* species (Giam 2017) and forests—tropical forests in particular—are frequently the target of biodiversity conservation efforts. In forest ecosystems, preserving biodiversity and protecting forests are effectively synonymous. Figure 1 provides a summary view of the evolution over time of the environmental content in RTAs, and specifically the inclusion of forest and biodiversity provisions. Panel (a) shows that the inclusion of environmental provisions in RTAs is not a recent phenomenon, as it dates back to the founding treaty of the European Economic Community in 1958, nor uncommon as over 90% of RTAs in the sample have at least one environmental provision—broadly defined. Panels (b) to (d) show, however, that focusing on overall environmental provisions can be deceiving. Prior to the 1990s, environmental provisions in RTAs did not establish any obligation of environmental protection. Rather, these provisions took the form of environmental exception clauses to trade policy commitments such as those to protect the conservation of natural resources. This progressively changed in the 1990s and—with much stronger emphasis—in the late 2000s, when RTAs increasingly included commitments to environmental protection. Panels (b)–(d) document this shift for deforestation and biodiversity provisions. For the remainder of the article, we proceed by referring to these two narrowly defined forest-related provisions as "environmental provisions."

There are overall 51 agreements that include these provisions that have been notified to the WTO as of 2018, 78% of which were signed after 2005. The largest share of these agreements is between a developed and a developing country (e.g., EU–Algeria, the United States–Peru, Japan–Mongolia), with fewer cases of agreements between developed countries (e.g., EU–Korea) and between developing countries (e.g., the Pacific Alliance). The trade agreements with deforestation and biodiversity provisions vary widely in terms of the other environmental provisions covered by the

7. An example of a side agreement is the North American Agreement on Environmental Cooperation (NAAEC) that promotes sustainable development among the signatories of the North American Free Trade Agreement (NAFTA). An important caveat is that the World Bank coding does not cover other agreements that are not explicitly referenced in the RTA or secondary law that can emanate from the agreement.

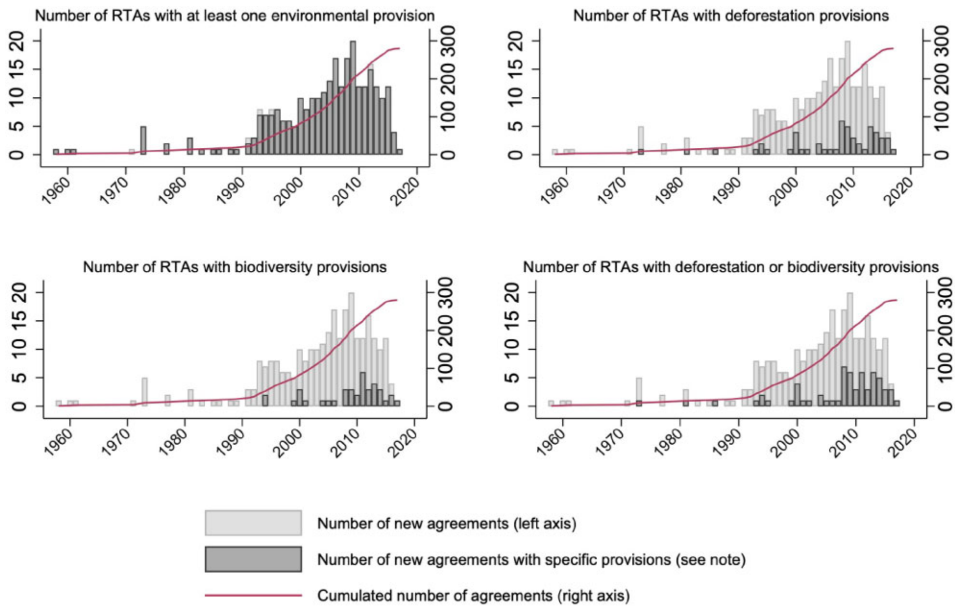

FIGURE 1. Regional trade agreements and environmental provisions over time. The graph plots the number of new RTAs entering in force over the period 1958–2017. It disentangles RTAs including at least one provision on the environment (upper-left), RTAs requiring states to prevent deforestation (upper-right), RTAs protecting biodiversity (bottom-left), and RTAs preventing deforestation or protecting biodiversity (bottom-right). The solid line shows the cumulative number of RTAs enacted by each year. Source: Mattoo, Rocha, and Ruta (2020).

RTA, ranging between 5 provisions for European Free Trade Area-Peru and Australia–Malaysia, and 38 provisions for the Comprehensive and Progressive Agreement for Trans-Pacific Partnership. Agreements that include forestry and biodiversity provisions generally have strong enforcement mechanisms. RTAs may require states to maintain judicial or administrative proceedings for enforcement of environmental regulation or they may subject environmental provisions to general state to state dispute settlement. In the late 2000s, a growing number of RTAs introduced a special environmental state-to-state dispute settlement.

The scope of provisions that aim at protecting forest resources varies significantly across agreements.[8] RTAs can include more general language to promote the conservation and sustainable use of biological diversity and sustainable forestry management. In addition, they can commit members to specific actions such as measures to combat illegal logging and related trade. For instance, the RTA between the United States and Peru contains an annex on forest sector governance setting out detailed commitments promoting sustainable management of forest resources and combating trade associated with illegal logging, among other things. The agreement

8. See Monteiro (2016) for details.

has specific requirements such as the identification within the Peruvian government of a focal point, with appropriate and sufficient authority and staff to investigate violations of laws and regulations for forest sector governance. The environmental provisions in the United States–Peru also featured well-developed cooperative mechanisms that can improve the effectiveness of existing domestic regulations through technical assistance. For example, the US government is reported to have spent an estimated $90 million USD on training of Peruvian personnel in support of forest-related commitments in the United States–Peru RTA, including environmental prosecutors, law enforcement, national park security, and so forth.[9] The RTA between the EU and Cameroon contains similar, although not as detailed, provisions on forestry governance and trade in timber and forest products. Like the United States–Peru agreement, the EU–Cameroon RTA also not only features technical assistance through cooperative mechanisms but also appears to have played a critical role in Cameroon's 2011 accession to the EU-FAO's Forest Law Enforcement, Governance and Trade (FLEGT) Programme following shortly after the signature of the EU–Cameroon agreement in 2009. This, in turn, promoted legislation on transparency aimed at establishing a legal timber sector in the country.[10]

To measure the impact of forest loss arising from RTAs, we utilize the Global Forest Change dataset by Hansen et al. (2013). These data provide high-resolution estimates of year 2000 forest cover and annual estimates of forest loss for the entire terrestrial surface of the earth. Following Abman and Lundberg (2020) and Leblois, Damette, and Wolfersberger (2017) (among many others), we aggregate these spatially explicit estimates of forest loss to the country-level to create a panel dataset of estimated annual forest loss for 193 countries from 2001 until 2018. While high-resolution satellite image-derived estimates represent a dramatic improvement over previously used data for country-level forest loss estimates (Steininger et al. 2001; DeFries et al. 2002), there are notable limitations that warrant some discussion.

First, the data only provide estimates of forest *loss* at the annual level, not forest gain. Because tree growth is a slower process than tree clearing, the data only report estimates of forest gain over a 12-year period (from 2001 to 2012). Consequently, our empirical work only considers forest loss, not net forest change, from RTAs. While RTAs may lead to shifts in the distribution of forested land over the long run (on the order of decades, at a minimum), we argue that differences in the short-run dynamics of deforestation and afforestation will likely still create acute environmental externalities. Hence, given the nature of our setting and question of study, we believe that annual forest loss is the important metric to study. Second, the data do not distinguish between

9. As noted in Claussen (2021) and Peña (2023), domestic laws protecting the environment already existed in Peru before the RTA with the United States entered into force in 2009. However, these mechanisms were considered mostly ineffective. The environmental provisions in the the United States–Peru RTA offered a tool to facilitate the enforcement of domestic Peruvian environmental law, especially in the case of actions by the congress or by subnational governments that breached this law.

10. Food and Agriculture Organization. 2017. Cameroon strengthens its legal timber sector. News item July 26, 2017, accessed at: https://www.fao.org/in-action/eu-fao-flegt-programme/news-events/news-details/en/c/1051517/.

different types of forest loss. The classification of tree cover is any vegetation greater than 5 m in height. Thus, this classification does not allow us to separately examine deforestation in primary forests from harvesting trees from timber plantations (for example). Finally, the detection algorithm used in our version of the data (version 1.6) changed slightly in 2011 relative to the earlier years. At the time of writing this paper, Version 2 of the dataset, which intends to provide the entire sample period with a consistent detection algorithm, is not yet available. We believe this is only a minor issue and the use of year-fixed effects should account for year-to-year changes in detection common to the entire sample.[11]

2.2. *Potential Transmission Mechanisms*

To frame our empirical investigation into the effectiveness of environmental provisions in trade agreements, we first introduce a broad discussion of theoretical mechanisms through which trade agreement content might affect deforestation. Above all, our understanding of provision effectiveness centers around the enforcement linkages between environmental commitments in trade agreements and the threat of tariff retaliation. By linking environmental commitments to trade agreement dispute settlement mechanisms, these commitments become plausibly enforceable. This is true regardless, if the environmental provisions are included as a mutually agreed upon commitment mechanism or if they are unilaterally imposed on signatories from a party with disproportionate bargaining power in trade negotiations. In both cases, legal recourse established in the trade agreement covers these environmental commitments and failure to meet these commitments exposes countries to the threat of retaliatory tariffs or other dispute remedies.

While linking environmental commitments to trade agreement dispute settlement mechanisms introduces enforceability to environmental content in trade agreements, it is not clear *ex ante* the domestic policy tools a country might use to meet these commitments. For the forest-related provisions, we focus on in this paper, governments may limit deforestation arising from trade liberalization by limiting legal and illegal timber harvest through reduced logging permits and concessions and/or increasing monitoring efforts aimed at illegal logging. Governments might also focus on limiting agricultural land expansion into forestland, which the literature has found to be the

11. Alix-Garcia and Millimet (2023) argue that satellite-derived estimates of forest loss may suffer from non-classical measurement error in some settings. In Alix-Garcia and Millimet (2023), such measurement error biases estimates of treatment effects of plot-level participation in a Mexican forest conservation program because cross-sectional differences in treatment status are correlated with factors driving measurement error (e.g., rugged terrain, persistent cloud cover). Such remote sensing measurement errors do not threaten identification in our setting for two main reasons: Any factors driving such non-classical measurement error (land topography, cloud cover, etc.) will be independent of treatment either because they are time invariant—as in the case of land topography and latitude—or changes will be orthogonal to RTA treatment timing—as in the case of changes in cloud cover. Our aggregation of forest loss to the treatment-unit level in conjunction with treatment-unit fixed effects (rather than at the plot level treatment with administrative division fixed effects as in Alix-Garcia and Millimet (2023)) further mitigates concerns about non-classical measurement error biasing our estimates.

dominant driver of forest loss around the globe (e.g., DeFries et al. 2010; Gibbs et al. 2010; Busch and Ferretti-Gallon 2017; Curtis et al. 2018). In particular, RTAs affect the relative prices of agricultural goods and associated land values, and can induce agricultural expansion into forestland (Abman and Lundberg 2020). Countries may seek to limit deforestation arising from trade liberalization by discouraging agricultural land expansion. In some settings, this might be achieved by directly prohibiting agricultural land expansion, however, property rights—or the absence thereof—may create difficulties in implementing this approach in practice. For example, market-based agricultural policies and private property rights may limit the government's ability to dictate private landholder decisions. Likewise, the absence of property rights may create challenges in establishing binding land use prohibitions. Instead, agriculture-driven deforestation may be avoided by reducing land-based production subsidies or reducing agricultural input subsidies that increase the marginal value of prospective agricultural land (e.g., fertilizers, agricultural machinery). Similarly, countries may facilitate or subsidize agricultural innovation and investment in land-sparing technology to encourage production gains at the intensive margin.[12] The hypotheses above imply changes in government intervention and expenditures in response to environmental commitments in RTAs.

3. Econometric Approach

We develop a matched *agreement-level* approach that aims at controlling for the potential endogeneity of environmental provisions. By aggregating to the level at which treatment occurs—the RTA group—we are better positioned to address such endogeneity and recover plausibly causal treatment effects. We create an agreement-level panel by aggregating outcome variables of interest across RTA signatories by year. For example, we create an annual time series of deforestation associated with the United States–Peru trade agreement by adding forest loss in Peru and forest loss in the United States by year. Therefore, for the group of countries G that are signatories to RTA g, we compute

$$y_{gt} = \sum_{i=1}^{n} y_{it} \mathbb{1}[i \in G], \tag{1}$$

where i indexes individual countries and $\mathbb{1}[i \in G] = 1$, if country i is a signatory to RTA g. We do this for all RTAs and all years in our sample and include a policy indicator that equals 1 when the RTA enters into force, and 0 for all years prior. Year-to-year changes in y_{gt} will reflect net changes in the outcome variable of interest among RTA signatories at the aggregate, agreement level. If entry into force shifts economic activity, for example, deforestation, from one subset of signatories to a different subset

12. The idea that agricultural intensification may limit land conversion and deforestation is known as "Borlaug's Hypothesis" (Borlaug 2007).

of signatories such that activity falls in some member countries but rises in others, our RTA group-level aggregation will measure the net changes arising from the entry into force of the agreement.

We identify the effects of environmental provisions with the following triple-difference model on this RTA-level panel:

$$y_{gt} = \beta_1 \mathbb{1}[Post\_RTA_{gt}] + \beta_2 \mathbb{1}[Post\_RTA_{gt}] \times \mathbb{1}[Enviro\_RTA_g] + \alpha_g + \gamma_t + \varepsilon_{gt}, \tag{2}$$

where y_{gt} is the RTA-aggregated outcome of interest, $\mathbb{1}[Post\_RTA_{gt}] = 1$, if year t is later than the year that RTA g enters into force and zero prior, and $\mathbb{1}[Enviro\_RTA_g] = 1$ if RTA g includes environmental provisions. We include agreement-level fixed effects α_g to control for time-invariant agreement group characteristics that may affect the outcome variables under consideration. Group characteristics such as baseline average deforestation across member countries, number of signatories, baseline agricultural production, and so on, are accounted for via the inclusion of α_g. The α_g fixed effects will also control for different idiosyncratic outcome levels across agreements. Some RTAs feature two small counterparties (e.g., Nicaragua–Taiwan) and will have mechanically low aggregated outcome levels, while other RTAs feature large countries like the United States or large numbers of signatories as with EU agreements and, hence, mechanically higher aggregated outcome levels. We also include year fixed effects γ_t to control for common year-to-year shocks, for example, changes in international commodity prices, that may likewise impact outcomes of interest.[13]

In this model, β_1 captures the net changes in overall group outcomes after the enactment of the RTA. β_2 captures the differential effect from RTAs with relevant environmental provisions on the outcomes under study. Thus, entry into force of RTAs that include environmental provisions will lead to an estimated $\beta_1 + \beta_2$ increase in the outcome variable.[14]

The central challenge to identification when assessing the impacts of individual provisions arises from the non-random nature underlying the content of RTAs.[15] *Ex ante,* forest-related provisions are most likely to appear in RTAs where deforestation is a concern. Estimates from a sample comparing all RTAs with and without

13. We do not explicitly incorporate control variables in our triple-difference models but they are used in the matching process discussed in Section 3.1. Control variables for the determinants of forest loss would only be necessary in as much as they are correlated with treatment selection or treatment timing. Our matching approach developed in Section 3.1 mitigates concerns around the former issue, while the plausibly exogenous timing of treatment mitigates concerns around the latter issue. Instead, such orthogonal determinants of forest loss will only affect our precision but will not introduce bias.

14. Here, β_1 is the traditional "difference-in-differences" parameter, which identifies how the difference between treated and untreated units (the first "difference") changes after entry into force of an RTA (the second "difference"). β_2 describes the difference in these difference-in-differences (the "third" difference) with the presence an additional treatment dimension (environmental provisions).

15. As argued by Abman and Lundberg (2020), the multilateral ratification required to enact RTAs mitigates concerns that endogeneity arises due to timing of enactment. No one country can unilaterally choose when an RTA enters into force. This process leads to long delays and a high degree of heterogeneity in the time between signing an RTA and when it enters into force.

environmental provisions will likely be biased. β_1, the estimated impact of RTAs without provisions, may suffer from attenuation bias because RTAs for which little forest loss might occur will tend not to include the provisions—for example, RTAs between countries with little or no standing forest. In contrast β_2, the estimated impact of environmental provisions, may suffer from positive bias due to the selection problem mentioned—provisions are likely to appear in agreements where deforestation would be higher *ceteris paribus*. This upward bias could be strong enough to result in positive estimated effects of provision inclusion. By carefully selecting a counterfactual group of RTAs without provisions that look as similar as possible to RTA groups with provisions, we mitigate the potential biases that arise via selection on observable characteristics.

3.1. Reduced-form Empirical Political Economy Model of Provision Inclusion

We address the endogeneity of environmental provisions with a propensity score matching exercise. We estimate propensity scores for the inclusion of environmental provisions by a cross-sectional logistic regression at the RTA level for agreements that entered into force in our sample period (2001–2018). However, the trade literature has not established a unified theory for trade agreement content formation that might inform our model specification. Absent a clear theoretical framework on which to base our specification, we take a broad, data-driven approach to our logit estimation that we view as a reduced-form, empirical political economy approach. We estimate the propensity scores using a least absolute shrinkage and selection operator (LASSO), which allows us to incorporate a wide range of potential characteristics that might capture important aspects of the political economy behind trade agreement content and the existence (or absence) of the relevant environmental provisions. The LASSO model then selects over these candidate characteristics for those that ultimately explain the inclusion of provisions in the RTA. We include a number of candidate variables that capture important characteristics relevant to the inclusion of such provisions, which we discuss below.

Based on the issue linkage literature, we expect forest provisions to appear in trade agreements when such a provision is relevant to the geographies of the parties. Thus, RTAs with member countries that contain significant forest resources, countries in the tropics, or countries with high degrees of biodiversity are more likely to have forest provisions than RTAs with member countries in regions with low forest stocks and limited biodiversity. We account for this relevance dimension by including as candidate covariates, the maximum and average biodiversity indices across agreement signatories,[16] the number of tropical signatories and an indicator for whether *any* signatories are in the tropics, the total forest cover of signatories in the year 2000, the total land area of signatories, the total percent of land area among signatories covered by forest in 2000, and finally a set of regional categories that include an indicator for

16. Our biodiversity index comes from UNEP (2007).

any signatories belonging to the region as well as the numbers of members in the region (e.g., North America, Central and South America).

The literature also suggests a number of country-specific factors such as sectoral composition, the strength of industry lobbies, terms of trade, and national preferences over the environment that may promote or forestall the inclusion of these provisions (Morin et al. 2018). The LASSO model allows us to include individual country indicators taking values of unity, if the country is a signatory to the RTA and zero otherwise, which capture such country-specific factors above, as well as any other country-level factors related to the political economy of trade negotiations.[17] We also include indicators for the presence of different combinations of developed and developing counterparties. If environmental provisions are more likely in trade agreements that include developing economies with weaker institutions, and thus weaker mechanisms to enforce international or domestic commitments to protect forests then these indicators should capture this relationship. Furthermore, these indicators may also proxy for bargaining asymmetries between the parties, as for the case when large, advanced economies such as the EU and the United States negotiate trade policy concessions with developing partners in exchange for commitments on environmental protection. We also account for potential contracting costs by including variables for the number of parties to a trade agreement, as well as indicators capturing "template effects", meaning a party to the current RTA has previously been party to another RTA with these relevant provisions. Finally, we account for changing international norms in trade negotiation and policy linkages by including as candidate variables the year and year squared that the agreement was signed.

Because the list of candidate variables described above is too large to include in a standard logit regression, we use the Logit LASSO—a form of penalized maximum likelihood estimation where the penalty function is the ℓ_1-norm of regression coefficients in the logit model—as a form of data-driven model selection. The penalty functions as a model selection criterion as extraneous covariates will be dropped from the model to avoid incurring the penalty. Formally, LASSO solves the following optimization problem:

$$\max_{\beta_0,\boldsymbol{\beta}} \left\{ \frac{1}{n} \sum_{g=1}^{n} \ell(\mathbb{1}[\mathit{Enviro\_RTA}_g], \beta_0 + \boldsymbol{\beta}\boldsymbol{X_g} \mid \beta_0, \boldsymbol{\beta}) - \lambda \|\boldsymbol{\beta}\|_1 \right\}, \tag{3}$$

where $\ell(\cdot)$ is the logit log-likelihood function, $\boldsymbol{\beta} = \{\beta_1, \beta_2, \ldots, \beta_k\}$ and $\|\boldsymbol{\beta}\|_1$ denotes the ℓ_1-norm of $\boldsymbol{\beta}$ (i.e., $\sum_{j=1}^{k} |\beta_j|$), and $\boldsymbol{X_g}$ is a set of *candidate* regressors from which the LASSO model selection procedure draws from. As discussed above, we include a wide variety of potentially relevant candidate regressors in $\boldsymbol{X_g}$, that is, informed by the issue linkage literature and more broadly by our so-called reduced-form empirical political economy approach to provision inclusion.

17. Many countries are already members of common markets over our sample period and, hence, always sign the same agreements. We drop these perfectly multicollinear country indicators, only keeping one indicator per trading bloc.

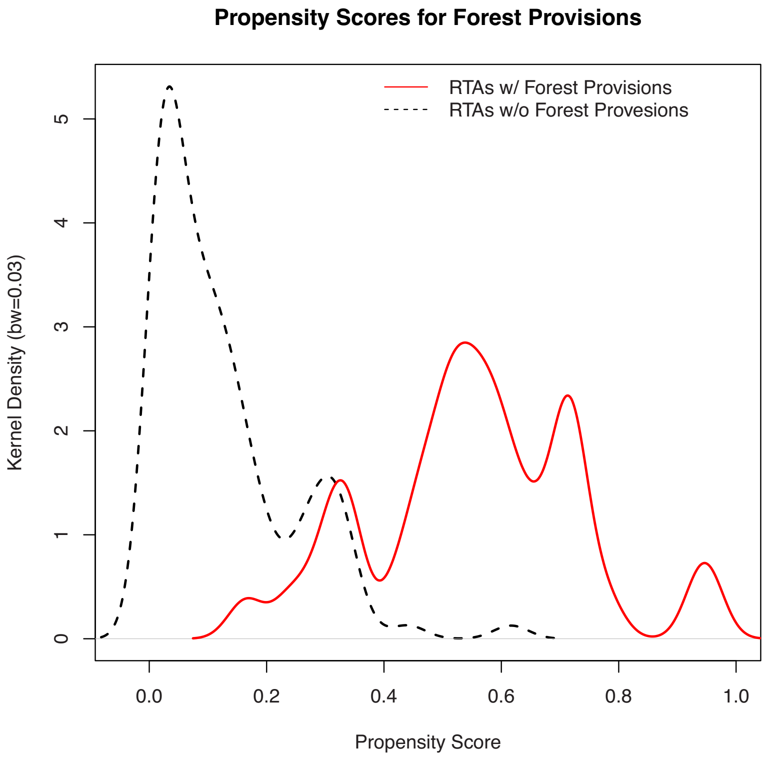

FIGURE 2. Fitted probabilities of inclusion of forest-related provisions in RTAs. This figure compares the fitted probability of the inclusion of forest-related environmental provisions in RTAs across the agreements that ex-post include the provisions and those that do not. Fitted probabilities are from a cross-sectional LASSO logit model estimated on all RTAs notified to the WTO that were signed after 2003. Note that fitted probabilities lie in the [0, 1] interval—apparent mass outside of this interval comes from kernel smoothing.

λ in equation (3) is the so-called regularization parameter that controls the penalty on the ℓ_1-norm of $\boldsymbol{\beta}$ and the corresponding shrinkage. In principle, λ is chosen by the researcher; however, we adopt an automated approach to λ selection. Our preferred λ minimizes the mean k-fold cross-validation error,[18] which provides a good balance of model parsimony (from the LASSO procedure) and model fit (necessary for subsequent propensity score matching). We also conduct sensitivity analyses on the effects of λ on our matching and subsequent results in Section 4.5 below. Figure 2 provides kernel density plots for the fitted probabilities from our LASSO logit.

18. k-fold cross validation involves randomly partitioning the sample into k disjoint subsamples. $k-1$ of these partitions are used as the "training" data to estimate the LASSO logit model, while the omitted partition is used as "out of sample" validation data from which so-called cross validation errors—the difference between the predicted and observed outcomes in the validation set—are obtained. This estimation-validation procedure is repeated k times, with each of the partitions used as the validation set exactly once. This process is iterated over different values of λ with our preferred choice of λ being that which achieves the smallest average cross-validation error.

Online Appendix Table A.2 details the selected candidate covariates across a range of λ values.[19] While the penalty parameter value does affect covariate selection to some extent, we observe some consistent patterns across all values of λ. Environmental provisions are more likely to appear in more recent RTAs. They are more likely to appear in RTAs with both developed and developing country signatories than RTAs with all developed country signatories (but less likely to appear in RTAs with all developing country signatories). Greater share of forest cover across all signatories is associated with increased likelihood of provision inclusion as is the value of the maximum biodiversity index among signatory countries. Templating effects appear to be relevant as RTAs with any country previously party to an RTA with these environmental provisions increases the probability of provision inclusion. RTAs with any members from Central or South America are also more likely to include these environmental provisions. Furthermore, a variety of individual country/bloc indicators are selected by the model (EU, Canada, Japan, e.g.) but many are only selected for some λ values, not all. Our analysis proceeds with $\lambda = 0.02$—the value that minimizes the mean cross-validation error.

After estimating propensity scores for our sample of trade agreements, we create a matched panel by matching treated agreements—those that include relevant environmental provisions—to untreated units—those that do not. We match with replacement to ensure that treated units are matched with their most similar control units, which leads to bias reduction at the expense of efficiency (Dehejia and Wahba 2002). Our data cover years 2001–2018; however, we restrict our sample of RTAs to those that enter into force after 2004 and before 2015 to ensure that there are at least 4 years of data before and after entry of force of every RTA in our sample to credibly identify treatment effects and fully identify all coefficients in event study specifications. This sample construction criteria leave us with a matched sample that includes 36 treated agreements (with the provisions) and 8 control agreements (those without) where matched controls are weighted based on the number of treated units to which they are matched. The list of trade agreements that require measures to prevent deforestation or to protect biodiversity is in Online Appendix Table A.1 along with matched control RTAs that are likely to have the provisions but do not. Table 1 presents summary statistics for our full sample and the matched sample.

We then estimate equation (2) on this matched panel of agreements to identify the effects of environmental provisions—the triple-difference coefficient (β_2 in equation (2)). Our matching approach yields an interpretation of β_2 as the average treatment effect on the treated (ATT)—a causal parameter. In particular, β_1 will measure the effect of RTA enactment for the selected (matched) counterfactual agreements and β_2 will measure the impact of the provisions on the selected outcomes for the agreements in which they were included (ATT). This causal interpretation is predicated on the identifying assumption that, upon controlling for the characteristics

19. Online Appendix Table A.2 also presents the sign of the estimated marginal impacts of the selected variables on the probability of environmental provisions appearing in an RTA.

TABLE 1. Summary statistics.

Panel A: Full sample (141 RTAs)			
	Observations	Mean	Standard deviation
Loss (m ha)	2,538	0.674	0.965
Loss rate	2,538	0.004	0.003
Ag (m Ha)	2,538	128.373	158.335
Ag (m Ton)	2,538	963.850	1,157.411
Ag Yield (m T/Ha)	2,538	0.490	0.790
Ag Exp (m USD)	2,538	1,408.003	2,300.481
Ag Exp ($/T)	2,538	16.034	36.037
Timber (m m^3)	2,538	260.617	280.916
Forest Exp (m USD)	2,538	17.633	31.456
Ag Recurring	2,538	2,262.584	7,229.383
Ag RD	2,538	54.702	108.787
Ag Capital	2,538	34.694	267.179
Forest Recurring	2,538	38.657	200.769
Forest Capital	2,538	5.965	17.778
Environment provisions	2,538	0.255	0.436
Panel B: Matched Sample (72 RTAs, 44 Unique)			
	Obs	Mean	St. Dev.
Loss (m ha)	792	0.937	1.043
Loss rate	792	0.005	0.003
Ag (m Ha)	792	101.253	101.956
Ag (m Ton)	792	861.279	864.045
Ag Yield (m T/Ha)	792	0.738	0.941
Ag Exp (m USD)	792	2,085.831	2,663.187
Ag Exp ($/T)	792	46.737	65.740
Timber (m m^3)	792	278.375	273.916
Forest Exp (m USD)	792	26.340	37.246
Ag Recurring	792	1,450.192	4,969.961
Ag RD	792	90.778	162.884
Ag Capital	792	40.141	379.906
Forest Recurring	792	52.507	225.352
Forest Capital	792	9.377	22.836
Environment provisions	792	0.500	0.500

included in our propensity score model, there are no other time-varying unobservable factors that drive changes in trading-bloc level forest loss that are correlated with the inclusion of these provisions in an RTA. While this is a fundamentally untestable assumption, the inclusion of group fixed effects accounts for concerns that could arise due to time invariant group-level characteristics.

3.2. Standard Error Corrections with Sparse Cross-cluster Correlations

While our RTA-level analysis developed above allows us to tackle identification of the causal impacts of environmental provisions, it introduces some complications to inference. We cluster standard errors at the agreement level to account for within-agreement correlation. In particular, this allows correlation in residuals within countries. However, countries enter into multiple trade agreements in our sample. Our agreement-level aggregation of outcomes introduces cross-cluster correlations due to overlapping country memberships. This cross-cluster correlation violates standard clustering assumptions.

We develop an extension to the standard approach to clustered covariance matrices. We allow for sparse cross-cluster correlation in the covariance matrix induced by overlapping cluster membership. Leveraging the structural information about the presence and degree of cluster overlap, we develop a covariance matrix that allows for within-country correlations in our RTA-level triple-difference model. Consider the standard matrix representation of equation (2)

$$Y = X\beta + \varepsilon, \tag{4}$$

then the covariance matrix of the vector of coefficient estimates $\hat{\beta}$ is given by

$$\mathrm{Var}[\hat{\beta}] = (X'X)^{-1}\widehat{V}(X'X)^{-1}, \tag{5}$$

where $\widehat{V}$ is the cluster-robust covariance matrix with sparse cross-cluster correlations, which we define as

$$\widehat{V} = \sum_{g=1}^{n}\sum_{h=1}^{n} w_{g,h} X_g' \hat{\varepsilon}_g \hat{\varepsilon}_h' X_h, \tag{6}$$

where g and h index agreements, n is the number of agreements in our matched sample, and $w_{g,h}$ is a weighting function that allows for sparse cross-cluster correlation. Let n_g and n_h denote the number of countries that are party to trade agreements g and h, respectively, and G and H denote the set of parties to each respective agreement. Then, our weight function is given by

$$w_{g,h} = \frac{1}{n_g}\sum_{k\in G} \mathbb{1}[k \in H], \tag{7}$$

where $\mathbb{1}[k \in G] = 1$ if country k is a party to agreement g. If there is no membership overlap between agreements g and h, $w_{g,h} = 0$, and we restrict the cross-cluster correlation between cluster g and h to be zero. If there is overlap, we allow for cross-cluster correlation but weight it by the degree of overlap, that is, the share of total members to agreement g that are also members to agreement h. Note that our cluster-robust covariance matrix with sparse cross-cluster correlations is a generalization of the standard cluster robust covariance matrix and nests the standard approach—if there is no overlapping agreement membership, $w_{g,h} = 1$, if $g = h$ and $w_{g,h} = 0$ for $g \neq h$.

The structural source of cross-cluster correlation in our experimental setting—cluster membership overlap—suggests that these cross-cluster correlations will be positive. Consequently, our approach should yield larger standard errors and, hence, represents a more conservative approach to inference relative to standard cluster-robust methods.

4. Empirical Results

We present our empirical findings below. We discuss our main findings in Section 4.1 followed by a variety of extensions and robustness checks. Section 4.2 considers treatment effect heterogeneity based on high-risk areas and ecosystems. Section 4.3 explores potential transmission mechanisms and Section 4.4 considers provision-specific dispute settlement remedies in RTAs. We also consider the sensitivity of our main findings to the LASSO penalty parameter (Section 4.5) as well as the composition of our matched control groups (Section 5).

4.1. Main Results

In Table 2, we present estimates of the triple-difference model from equation (2) on the annual log of aggregate forest loss in columns (1) and (2), as well as the deforestation rate in columns (3) and (4).[20] To emphasize the importance of addressing the endogeneity of provision inclusion, we present estimates from our matched sample as well as the full sample of agreements. In column (1), we find that net annual forest loss increases by approximately 23% following the entry into force of an RTA, which does not have provisions aimed at protecting forest and/or biodiversity. The inclusion of forest-related provisions substantively mitigates this negative environmental impact of trade liberalization—with reductions in forest loss of approximately 20% relative to agreements without provisions. This effect is statistically significant at the 1% level. This inference is based on our standard errors that are clustered at the agreement level and account for cross-cluster correlation as signatories may belong to multiple RTA groupings and is conservative relative to simple RTA-level clustering. Hence, agreements that include forest-related provisions, do not lead to any measurable increase in forest loss following entry into force. Measuring forest loss using the deforestation rate yields similar findings—in column (3), the deforestation rate increases approximately 0.04 percentage points following entry into force of agreements without provisions. This effect is partially mitigated by the inclusion of the provisions. However, the evidence is much weaker here—these effects are not statistically significant at conventional levels.[21]

While the data we have available do not allow us to observe the implementation of RTA rules, the econometric findings suggest that these types of environmental provisions provide a mechanism to defray the environmental costs that can arise as a result of international trade integration. While we concede that the inclusion of

20. We measure the deforestation rate as the percentage of aggregate baseline forest cover from the year 2000 that is lost annually across agreement signatories. Estimating these models using the inverse hyperbolic sine transformation of forest loss in place of our log forest loss measure yields nearly identical estimates everywhere. Consequently, we proceed presenting only estimates on log forest loss.

21. We note that in these models, as well as in many others we present, the R^2 statistics are high, well above 0.9. Due to our aggregation of outcomes across RTA group members, the majority of variation we observe in our outcomes is cross-sectional and explained by the inclusion of RTA-group fixed effects, α_g.

TABLE 2. Aggregate forest loss.

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	(1)	(2)	(3)	(4)
Post-RTA	0.227***	−0.002	0.001	−0.00004
	(0.059)	(0.031)	(0.001)	(0.0001)
Post × Environment RTA	−0.203***	0.062	−0.0003	0.001**
	(0.072)	(0.050)	(0.001)	(0.0003)
Mean	937203	673854	0.0046	0.0043
Observations	792	2,538	792	2,538
R^2	0.980	0.983	0.816	0.794
Matched	✓	–	✓	–

Notes: FE triple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). Annual forest loss for these blocs is computed as the sum across member countries by year. All models include individual (i.e., RTA) and year fixed effects. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching with replacement. Observations for the matched samples reflect unique RTA-year observations. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. We also include statistical significance at the 10% level from from one-sided t tests denoted by $^{\dagger}p < 0.1$.

such provisions may incur some bargaining costs in the negotiation phase of trade agreements, they appear to provide an institutional framework that allows member countries to commit to policies that encourage more sustainable patterns of trade integration.

Our matching approach, conditional on the identifying assumptions discussed above, reduces or eliminates selection bias arising from the potential endogeneity of provision inclusion, yielding a causal interpretation of our findings above. We illustrate the effects of such selection bias in columns (2) and (4) of Table 2, which estimate our triple-difference model on the full sample of trade agreements. Our findings from this model underscore the importance of accounting for the endogeneity of provision inclusion when attempting to estimate causal effects of provisions. In column (2), we find a *positive* increase in forest loss of approximately 6% associated with the inclusion of forest-related provisions in this full sample, which we interpret as clear evidence of the selection bias problem. In column (4), we see a similar positive increase in the deforestation rate associated with provision inclusion. As forest provisions are more likely to be included in RTAs in which deforestation may be a relevant concern, this selection bias yields an estimated increase in forest loss associated with such provisions and no effect on RTAs without provisions. Indeed, summary statistics in Table 1 illustrate much larger average levels of forest loss in our matched sample when compared with the full sample of RTAs.

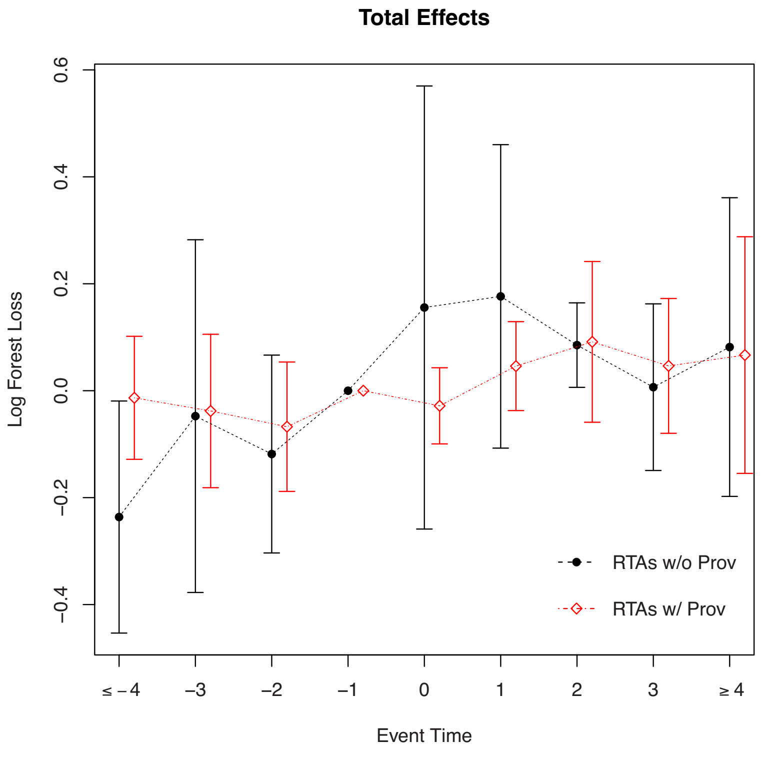

FIGURE 3. Matched RTA-level event study—log forest loss. This figure illustrates matched sample event study total effects along with 95% confidence intervals from our panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). The matched sample is created by matching with replacement on LASSO logit estimated propensity scores. Annual forest loss for these blocs is computed as the sum across member countries by year. All models include individual (i.e., RTA) and year fixed effects. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries.

Embedded in our triple-difference identification strategy is a parallel trends assumption. While fundamentally untestable, we present evidence that trends in forest loss prior to the entry into force of the RTA did not look different for our treated RTAs compared with our matched counterfactual group. To do this, we estimate the dynamics around entry into force of RTAs with and without the provisions using event time indicator variables. We include indicators from 3 years prior to 3 years after entry into force. We omit the indicator for year prior to entry into force as the reference group and we include pooled indicator variables for long-run leads—4 or more years before entry into force—and long-run lags—4 or more years after entry into force. We then interact this full set of leads and lags with our main forest provision variable. We present the estimates of these dynamic treatment effects in Figure 3. Leading up to the entry into force, we see no apparent trends in either group or any significant differences between the groups. We estimate a large increase in forest loss for the year of and year following the entry into force for the RTAs without forest provisions, while the provisions appear to mitigate this transitory increase in forest loss. Of note, the transitory dynamics in RTAs without provisions seen in Figure 3 are consistent with the main findings in Abman and Lundberg (2020). Importantly, the lack of differences in pre-period trends in forest loss supports our identifying assumptions.

We acknowledge that casual interpretations of our estimates also hinge on the so-called stable unit treatment value assumption (SUTVA). Notably, treatment spillovers to untreated units violate SUTVA. In the context of our identification strategy, shifts in deforestation activity among RTA signatories would not violate SUTVA, but potential displacement of deforestation outside an RTA trading bloc would constitute a threat to SUTVA. While SUTVA is essentially an untestable identifying assumption, we offer a brief discussion of features in our experimental setting that we believe partially mitigate potential SUTVA threats.

The literature has shown that agricultural land expansion is the operative driver of deforestation around the globe. In particular, Abman and Lundberg (2020) find transitory increases in forest loss after entry into force of RTAs and accompanying increases in agricultural land area. Presumably, shocks to the relative prices of agricultural goods induce production increases at the extensive margin. Transitory increases in forest loss correspond to increases in the stock of agricultural land. Spillovers in agricultural trade from RTAs might occur, if lower tariffs within an RTA group cause substitution away from RTA non-member agricultural products. While such spillovers in agricultural trade might threaten causal identification of the impacts of RTAs on trade flows, our study focuses first and foremost on deforestation. The asymmetric nature of forest loss from land conversion (in contrast to timber harvest) means that possible spillovers in agricultural trade flows would not correspond to SUTVA violations on deforestation. Agricultural spillovers might reduce or eliminate pressure for land conversion in RTA non-member countries, but this reduced pressure would not correspond to measurable changes—and, hence, violations of SUTVA—in forest loss. Agricultural land may be abandoned or fallowed, but it would not correspond to *negative* deforestation.[22]

Another potential source of SUTVA violation might arise from spillovers in timber trade. If tariff reductions in forest products within an RTA group lead to import substitution away from nonmembers, nonmembers may reduce forest harvests (and corresponding annual forest loss levels) in response to lower export demand. However, we believe this scenario is unlikely for a number of reasons. First, the literature has not found compelling evidence that forest product exports respond to trade liberalization (e.g., Dai, Yotov, and Zylkin 2014; Abman and Lundberg 2020). Second, while RTAs might cause the import prices of non-member timber exports to rise *relative* to member countries (potentially affecting trade flows), they should not affect the actual export *price levels* that timber producers in non-member countries will respond to, that is,

22. See discussion of our deforestation measures in Section 2 for details.

forest managers in non-member countries are unlikely to see a drop in timber prices that might induce them to adjust their harvest patterns. Additionally, timber products have a relatively high degree of geographic differentiation and products derived from many tree species in one country are not closely substitutable for products derived from species in others (e.g., softwood lumber and tropical hardwoods).

Finally, we concede that our main identification strategy involves aggregation at the RTA-level, which could lead to potential SUTVA violations due to overlapping country membership. However, in as much as this membership overlap is large and, hence, a substantive threat to SUTVA, overlap would induce spillovers that are positively correlated with the treatment effect leading to causal estimates that are biased toward 0, that is, conservative estimates of causal parameters of interest.

4.2. High-Risk Areas and Ecosystems

Because our identification strategy above measures net outcome variables at the RTA level, it can obscure the areas and ecosystems that are driving our results. To explore this idea further, we reconstruct our panel dataset of agreement-level deforestation using only a subset of country-level forest loss across agreement signatories. We consider tropical and non-tropical forest loss, forest loss in developed and developing countries, and forest loss in countries within the upper quartile of global biodiversity. Along each of these dimensions, we aggregate forest loss only from RTA signatories that belong to the subgroup under consideration. All other model specifications follow our main approach. We present this subsample analysis in Table 3. Panel A presents subsets we consider "high-risk"—tropical, developing, and biodiversity-rich countries. Panel B presents lower-risk subsets—the complements of the subsets in Panel A. Our main qualitative findings are consistent across all subsets: forest loss increases following entry into force of RTAs without environmental provisions, while the inclusion of the provisions partially or entirely offsets increases in forest loss. However, we note several interesting patterns across our subsamples. First, the tropical and developing country subsamples in Panel A exhibit larger magnitude effects compared with the non-tropical and developed subsamples in Panel B. Second, environmental provisions do not entirely offset deforestation in high-risk ecosystems—there are still net increases in forest loss following trade liberalization, even with the provisions. In contrast, the provisions entirely offset trade-induced deforestation in lower risk ecosystems, with suggestive evidence of net declines in forest loss following trade liberalization that includes the provisions in non-tropical countries.

4.3. Exploring Potential Mechanisms

We explore potential mechanisms through which trade agreement provisions protecting biodiversity and forestland might effectively mitigate forest loss arising from trade liberalization. In particular, we consider production and trade in forest products and agricultural commodities in addition to changes in government spending in these sectors.

TABLE 3. Forest loss effects by country type.

Panel A: Log Forest Loss in Higher Risk Ecosystems

	Tropical (1)	Developing (2)	High biodiversity (3)
Post-RTA	0.238**	0.225**	0.227*
	(0.108)	(0.097)	(0.118)
Post × Environment RTA	−0.186*	−0.195*	−0.201†
	(0.112)	(0.100)	(0.145)
Mean	532,039	545,065	514,190
Observations	792	792	792
R^2	0.997	0.989	0.998
Matched	✓	✓	✓

Panel B: Log Forest Loss in Lower Risk Ecosystems

	Non tropical (1)	Developed (2)	Lower biodiversity (3)
Post-RTA	0.049	0.080	0.129
	(0.122)	(0.135)	(0.217)
Post × Enviro RTA	−0.081	−0.091	−0.142
	(0.180)	(0.179)	(0.277)
Mean	405,163	392,138	423,013
Observations	792	792	792
R^2	0.998	0.997	0.995
Matched	✓	✓	✓

Notes: FE triple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). Outcome variables for these models are computed as the sum across member countries by year and subset. Panel A (top) presents estimates of log forest loss only aggregated among the given "high-risk" subsamples of RTA signatories. *Tropical* sums forest loss across RTA member countries that are in the tropics. *Developing* sums forest loss across RTA member countries that not advanced economies according to the IMF. *Biodiv Rich* sums forest loss across RTA member countries in the fourth quartile of biodiversity indices. Panel B (bottom) likewise presents estimates from models using log forest loss only aggregated among lower risk subsamples of RTA signatories, which are the complement to those in Panel A. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching without replacement. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

The international timber trade, especially in illegally harvested timber, is a clear threat to standing forest. If trade agreements include provisions protecting standing forest stock and requiring legal harvest and proper certification, this may support sustainable timber trade and harvest. We measure aggregate forest product activity using data from the UN Food and Agricultural Organization (UNFAO) including total forest-product exports and the total production of raw or minimally processed forest-derived commodities including, for example, round logs, sawn lumber, and so on. The trade data, in particular, corresponds to aggregate exports across all trading partners, not exclusively signatories to a particular trade agreement. We believe this is the appropriate measure to consider as trade liberalization may lead to substitution effects in bilateral trade flows that extend beyond the limited signatories of a trade agreement. As with our forest loss data, we aggregate these measures across all signatories to a trade agreement by year to construct an agreement-level panel of forest product activity.

Agricultural land expansion is one of the leading drivers of forest loss across the globe (Gibbs et al. 2010; DeFries et al. 2010; Busch and Ferretti-Gallon 2017; Curtis et al. 2018). At the same time, the literature has found that agricultural trade, in particular, responds strongly to RTAs (Grant and Lambert 2008; Sun and Reed 2010; Jean and Bureau 2016). Trade liberalization may lead to increases in agricultural production at the extensive margin, resulting in deforestation, as forestland is converted into agriculture. However, trade liberalization might also facilitate agricultural intensification through increases in agricultural capital or technology which, in turn, may reduce pressures on agricultural land expansion and forest loss. If trade agreements include provisions protecting standing forest stock and/or protecting biodiversity, this may limit conversion from forestland into agricultural production, which might be reflected in lower observable increases in agricultural production and agricultural trade, relative to agreements that do not include such provisions. However, these environmental provisions may also support the process of agricultural intensification, by creating incentives to invest in agricultural capital and technology, as they limit opportunities for agricultural land expansion. The overall impact of RTAs with environmental provisions on agricultural output and exports might thus reflect a combination of these two effects.

We measure agricultural activity using data from UNFAO that includes total agricultural exports, the total land area harvested, and the total weight of agricultural output. The land area harvested will capture land-use change at the extensive margin, while the total weight of agricultural output will reflect changes in both the extensive and intensive production margins. Our trade measures aggregate bilateral trade flows across all trading partners, which we likewise argue is the relevant measure to consider in our setting. Because our forest loss results are driven by responses higher risk ecosystems, we likewise focus on outcomes aggregated over developing countries and tropical countries.

In Table 4, we report estimates of our triple-difference model on agricultural and forest product outcomes in higher risk ecosystems. In Panel A, we consider these outcomes aggregated across developing country signatories, and in Panel B, we consider outcomes aggregated across tropical signatories, with similar results across both subsamples. First, we find evidence that environmental provisions limit agricultural expansion. In column (1) of Panel A, we find that land area harvested increases approximately 4.6% following entry into force of an RTA without environmental provisions. The inclusion of such provisions more than offsets these

TABLE 4. Agriculture and forest products in high-risk countries.

Panel A: Developing signatories

	Ag (Ha) (1)	Ag (Ton) (2)	Ag Yield (3)	Ag Exp ($) (4)	Ag Exp ($/t) (5)	Timber (6)	Forest Exports (7)
Post-RTA	0.046**	0.066**	0.009	0.102†	0.017	−0.044	−0.236†
	(0.022)	(0.026)	(0.026)	(0.066)	(0.084)	(0.056)	(0.168)
Post × Enviro RTA	−0.056**	−0.084***	−0.008	−0.104	0.122	0.066	0.309
	(0.023)	(0.029)	(0.042)	(0.104)	(0.135)	(0.087)	(0.255)
Mean	58.36	415.81	0.22	325.8	28.1	93.86	4.13
Observations	792	792	792	792	792	792	792
R^2	0.999	0.999	0.999	0.996	0.943	0.999	0.991
Matched	✓	✓	✓	✓	✓	✓	✓

Panel B: Tropical Signatories

	Ag (Ha) (1)	Ag (Ton) (2)	Ag yield (3)	Ag exports ($) (4)	Ag exports ($/t) (5)	Timber (6)	Forest exports (7)
Post-RTA	0.024	0.061**	0.010	0.101*	−0.045	−0.003	−0.184
	(0.023)	(0.030)	(0.017)	(0.053)	(0.092)	(0.043)	(0.180)
Post × Environment RTA	−0.034†	−0.076*	−0.011	−0.089	0.186†	0.043	0.264
	(0.026)	(0.043)	(0.023)	(0.166)	(0.113)	(0.065)	(0.246)
Mean	46.36	338.35	0.15	229.06	27.6	73.03	2.89
Observations	792	792	792	792	792	792	792
R^2	0.999	0.999	0.999	0.999	0.963	1.000	0.997
Matched	✓	✓	✓	✓	✓	✓	✓

Notes: FE triple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). Outcome variables for these models are computed as the sum across member countries by year and subset. Panel A (top) presents estimates on outcomes only aggregated across developing-country signatories. Panel B (bottom) presents estimates on outcomes only aggregated across tropical signatories. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching without replacement. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

increases, with a 5.6% decline in area harvested (a net decrease in harvest area of 1%). Results are qualitatively similar but slightly smaller for tropical signatories in column (1) of Panel B. Likewise, entry into force of RTAs without environmental provisions leads to an approximately 6.1%–6.6% increase in the weight of agricultural output, with even stronger offsetting effects from environmental provisions—7.6%–8.4% relative reductions across Panels A and B. We consider the impacts on agricultural yields in column (3), but our estimates are quite noisy and provide no clear insights. For developing countries, RTAs with or without provisions appear to drive yields higher, while in tropical countries, the provisions appear to have an offsetting effect. However, none of our yield estimates are statistically significant in either Panel. Agricultural exports exhibit patterns similar to our agricultural production measures with increases of nearly 10% following entry into force of RTAs without provisions and 9%–10% relative declines with the provisions. The preponderance of evidence suggests that the provisions effectively mitigate forest loss from trade liberalization by limiting the expansion of agricultural activity that otherwise arises. As found in the previous literature, we do not find evidence that trade liberalization or the effects of the provisions operate through forest product markets. Indeed, we find suggestive evidence that the production and exports of forest products fall following RTAs without the provisions. The provisions appear to increase forest product activity, corroborating the view that environmental provisions in RTAs affect deforestation primarily through their impact on agriculture trade rather than through trade in forest products.

We explore how RTA member governments might operate on the mechanisms discussed above to meet environmental commitments. We consider government expenditures in agricultural and forestry sectors using longitudinal survey data from the UNFAO that was jointly developed with the International Monetary Fund (IMF) based on the IMF's Classification of Functions of Government methodology used in Government Finance Statistics. However, the UNFAO data further disaggregate expenditures related to agriculture and forestry. In particular, the data report recurring government expenditures in both agriculture and forestry, expenditure on agricultural research and development (R&D), and net government capital expenditures in both agriculture and forestry. Recurring expenditure reflects net expenses, while capital expenditures reflect net investment in non-financial assets.[23] Unfortunately for our application, the data do not distinguish between expenses and investments related to conservation and expenses and investments related to agricultural production or forest harvest. An increase in expenses might reflect increased expenditure on agricultural input subsidies or an increase in forest conservation programs, for example. Because we cannot differentiate expenditures that might encourage deforestation from expenditures that aim at mitigating forest loss, we interpret the data as reflecting how *active* the government is in these sectors—an increase in expenditure corresponds to greater government interventions in the sector, while a decrease corresponds to less government intervention.

As in previous tables, Panel A of Table 5 reports outcomes aggregated over developing country signatories, while Panel B aggregates over tropical signatories. Our findings are qualitatively similar across both subsamples. We find suggestive evidence that aggregate net capital expenditures in both agriculture and forestry increase after entry into force of an RTA. This is consistent with increased government intervention in agricultural and forestry sectors following trade liberalization. However, the inclusion of environmental provisions partially offsets these increases. We also find suggestive evidence that recurring expenditures in both forestry and agriculture are lower following RTAs that include forest-related provisions compared with RTAs that do not include them. Following the entry into force, recurring agricultural and forest expenditures appear to rise in RTAs that do not include environmental provisions and fall in RTAs that do. Similarly, agricultural R&D expenditure appears to fall

23. See https://www.fao.org/faostat/en/ for additional details and documentation.

TABLE 5. Government expenditure on agriculture and forestry in high-risk countries.

Panel A: Developing signatories

	Ag recurring (1)	Ag R&D (2)	Ag capital (3)	Forest recurring (4)	Forest capital (5)
Post-RTA	0.882	−1.076	0.882	0.515	0.649
	(0.779)	(0.918)	(0.812)	(0.551)	(0.732)
Post × Environment RTA	−0.797	0.654	−0.728	−0.315	−0.301
	(1.186)	(0.668)	(0.902)	(0.730)	(0.610)
Mean	446.19	11.13	−14.34	12.71	7.14
Observations	792	792	792	792	792
R^2	0.668	0.531	0.651	0.639	0.606
Matched	✓	✓	✓	✓	✓

Panel B: Tropical Signatories

	Ag Recurring (1)	Ag R&D (2)	Ag Capital (3)	Forest Recur (4)	Forest Capital (5)
Post-RTA	0.945	−0.747	0.617	0.590	0.655
	(0.952)	(0.887)	(0.674)	(0.654)	(0.618)
Post × Environment RTA	−0.437	0.675	−0.479	−0.122	−0.533
	(0.964)	(0.732)	(0.871)	(0.619)	(0.545)
Mean	72.15	8.91	41.48	7.93	7.46
Observations	792	792	792	792	792
R^2	0.689	0.577	0.704	0.603	0.655
Matched	✓	✓	✓	✓	✓

Notes: FE triple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). Government expenditure variables are computed as the sum across member countries by year. "Recurring" variables are net fiscal expenditures. "Capital" variables are net investments in non-financial assets. "Ag R&D" is research and development spending in the agricultural sector. All models include individual (i.e., RTA) and year fixed effects. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data features sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching with replacement. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. We also include statistical significance at the 10% level from from one-sided t tests denoted by $^{\dagger}p < 0.1$.

following RTAs that do not include environmental provisions and rise after RTAs that do. However, we reiterate that these findings are suggestive, rather than definitive, given a general lack of statistical significance at standard levels.

The evidence above suggests that government interventions in agriculture and forestry markets rise following trade agreements without environmental provisions but decline slightly after entry into force of RTAs that do. Taken with our findings on deforestation, agricultural production, and agricultural exports, these results suggest that trade liberalization encourages government involvement in agricultural markets to facilitate trade and production at the expense of the environment. However, the inclusion of provisions aimed at protecting forests and biodiversity—and the threat of trade retaliation from failing to adhere to these commitments—appears to mitigate government distortions to agricultural markets that encourage land conversion. Indeed, our findings suggest that not only do these provisions limit increases in distortionary spending, but they may also lead to net reductions in government involvement in agriculture and forestry markets. Nevertheless, such conclusions should be viewed cautiously since—as we concede—the data do not distinguish between conservation spending and spending that may encourage agricultural extensification.

4.4. Provision-Specific Dispute Settlement Mechanisms

Environmental provisions in RTAs can be subjected to dispute under the general dispute settlement mechanism of the agreement or through a specialized dispute settlement Monteiro and Trachtman (2020). While the latter may have the advantage of having more specialized judges and thus more persuasive decisions, it could have two drawbacks. First, specialized mechanisms are generally "softer" in the sense that the possibilities for bringing cases are limited, respondents may have some flexibility in compliance or extra time to come into compliance. Second, the trade remedies are more limited as violations of environmental provisions under specialized dispute settlement may not be eligible for trade retaliation. This is different from areas such as intellectual property (IP) protection in many trade agreements signed by the United States and EU, among others, that intentionally allow trade retaliation for IP violations.[24]

Here, we explore the marginal effectiveness of these specialized dispute mechanisms for environmental provisions to further understand the conditions under which the inclusion of such provisions in RTAs is effective. Using data from Monteiro and Trachtman (2020), we distinguish between RTAs that subject forest and biodiversity provisions to the general dispute settlement mechanism and those RTAs that include an auxiliary environment-specific dispute settlement channel. We then estimate our main triple-difference model in equation (2) with the addition of a *fourth* differencing dimension: whether the provision includes the "special" dispute settlement for environmental provisions.

We present these findings in Table 6. The environmental provision coefficient now captures the marginal effect of provision inclusion when such provisions rely on general enforcement mechanisms, while the coefficient on the auxiliary environment-specific dispute mechanism describes the marginal impact of the "special" dispute mechanism. We find no evidence that environment-specific dispute settlement mechanisms offer any additional reduction in forest loss beyond that

24. For instance, the legal analysis by Bronckers and Gruni (2021) of EU's Free Trade Agreements (FTAs) concludes: "For the time being, the arrangements for settling disputes arising under the EU FTAs' sustainability chapters are separated from the disputes arising under the FTAs' other chapters, notably covering trade liberalization and intellectual property protection. There is a growing consensus, at least in scholarship, that this is undesirable. It weakens the credibility of the sustainability standards."

TABLE 6. Provision-specific dispute settlement mechanisms and aggregate forest loss.

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	(1)	(2)	(3)	(4)
Post-RTA	0.226***	−0.001	0.001	−0.00004
	(0.059)	(0.031)	(0.001)	(0.0001)
Post × Environment RTA	−0.232***	0.026	−0.0002	0.001†
	(0.072)	(0.053)	(0.001)	(0.0004)
Post × Environment RTA × Dispute	0.067	0.084	−0.0002	−0.0001
	(0.089)	(0.068)	(0.001)	(0.0005)
Mean	937,203	673,854	0.0046	0.0043
Observations	792	2,538	792	2,538
R^2	0.980	0.983	0.816	0.794
Matched	✓	–	✓	–

Notes: Fixed effects quadruple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level). The triple difference is as above, with an additional interaction indicating that there is a provision-specific dispute settlement on the relevant environmental provision. Annual forest loss for these blocs is computed as the sum across member countries by year. All models include individual (i.e., RTA) and year fixed effects. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching with replacement. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Our null hypothesis is that environmental provisions reduce forest loss.

achieved by general enforcement mechanisms. These findings support the view that the combination of enforceable language and general dispute settlement is required to achieve the intended environmental protection and “special” dispute settlement mechanisms for these provisions do not appear to support enforcement of environmental commitments.

4.5. LASSO Penalty Sensitivity

Although our LASSO approach to model selection and propensity score estimation is designed to guard against subjective specification choices unduly driving our results, we recognize that the LASSO penalty parameter can have a substantive effect on variable selection and, hence, estimated propensity scores. Our main results presented above use an objective criteria for penalty parameter choice based on the value that minimizes the mean cross-validation error. We explore the sensitivity of our results to alternative values of this penalty parameter in Figure 4. On the far right, we plot the coefficients from the triple-difference model from equation (2) estimated on the full, unmatched panel of RTAs (column 2 of Table 2). Moving leftward along the horizontal axis from the unmatched regression estimates, we report triple-difference

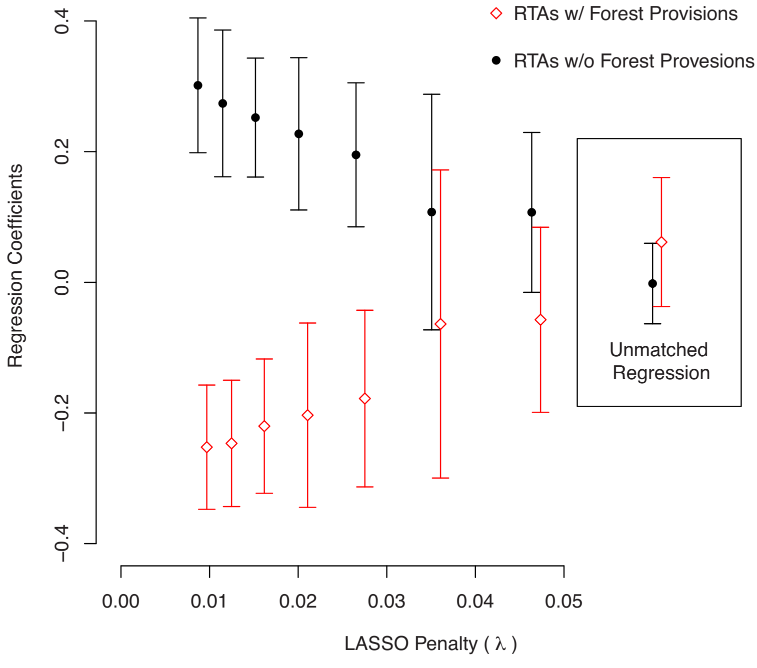

FIGURE 4. Sensitivity analysis of LASSO penalty (λ). This figure presents coefficient estimates from our main triple-difference model of RTA-level net log forest loss as functions of LASSO penalty parameters. Lines with filled circles correspond to estimates of β_1 and lines with diamond outlines correspond to estimates of β_2 in equation (2). Varying LASSO penalties yield different fitted propensity scores and hence, a different matched sample. Annual forest loss for these blocs is computed as the sum across member countries by year. All models include individual (i.e., RTA) and year fixed effects.

coefficients and 95% confidence intervals for sequentially lower values of the LASSO penalty parameter value, which correspond to sequentially more inclusive logit models (i.e., they include more of the candidate covariates). Figure 4 illustrates that relatively high values of the penalty parameter do not provide enough latitude to accurately fit the logit model and, hence, the estimated propensity scores are a poor reflection of the underlying probability of environmental provision inclusion. As a result, matching does very little to control the endogeneity bias. As the value of the penalty parameter declines, more inclusive logit models provide better model fits of provision inclusion and the subsequent matching procedures lead to more appropriate counterfactual control RTAs, thereby reducing the endogeneity bias.

5. Robustness to Control Group Composition

We explore the sensitivity of our matched results to control group composition by considering the two control agreements that we repeatedly match to—the Japan-Peru RTA and the ASEAN-Korea RTA. To ensure that our main findings are not

TABLE 7. Aggregate forest loss—Omitting ASEAN–Korea and Japan–Peru RTAs.

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	(1)	(2)	(3)	(4)
Post-RTA	0.225**	0.172***	0.0002	0.001***
	(0.091)	(0.061)	(0.0002)	(0.0004)
Post × Environment RTA	−0.185*	−0.174**	0.0004	−0.002**
	(0.109)	(0.074)	(0.0004)	(0.001)
Mean	663,700	1,635,080	0.004	0.006
Observations	792	774	792	774
R^2	0.976	0.980	0.818	0.806
Matched	✓	✓	✓	✓
Omit	JPN-PER	ASEAN-KOR	JPN-PER	ASEAN-KOR

Notes: FE triple-difference regressions on a panel of RTA-level trading blocs (i.e., observations are at the RTA and year level) matched after omitting the Japan–Peru RTA (columns 1 and 3) and the ASEAN–Korea RTA (columns 2 and 4) from the candidate control agreements. Annual forest loss for these blocs is computed as the sum across member countries by year. All models include individual (i.e., RTA) and year fixed effects. We cluster standard errors at the RTA level, however, because countries are signatories to multiple trade agreements, our data feature sparse correlation between clusters. Using RTA signatory information, we allow for cross-cluster correlation between clusters that have overlapping membership and weight cross-cluster correlations by the percentage overlap in RTA member countries. Matched samples are created by propensity score matching with replacement. Statistical significance from two-sided t tests are denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

exclusively driven by heavily weighting these control agreements, we create two new matched samples after omitting each of these RTAs from our set of candidate control agreements. We present triple-difference estimates of equation (2) in Table 7 on these modified matched samples and find that our main results persist.

We generalize this approach and consider the robustness of our results to control group composition more broadly. We do so by sequentially omitting all potential control RTAs from our matching approach and estimating the triple-difference model in equation (2). Our main findings are robust to this "leave-one-out" sensitivity exercise. We present results from this robustness exercise in the Online Appendix where Figure A.1 plots the distribution of the estimated total effects of RTA entry into force for agreements with and without environmental provisions ($\beta_1 + \beta_2$ and β_1 from equation (2), respectively).

6. Country-Level Analysis

The matched sample analysis presented above accounts for the potential endogeneiety in RTA-level content, but our matching approach relies on the use of observable characteristics and uses relatively few unique RTAs without provisions as controls. We provide additional evidence supporting our main findings using an entirely different identification strategy based on a multiple overlapping event study using a country-level panel dataset. This approach allows for the inclusion of all countries and all RTAs enacted in our sample window and estimates differential dynamics around RTA enactment for agreements with and without relevant environmental provisions. This identification strategy is not without its own limitations—foremost among these is that the country-level overlapping event study does not directly address the endogeneity of environmental provisions. The credibility of this identification strategy hinges on the set of countries that enter into both trade agreements without environmental provisions and agreements that include such provisions. Hence, our estimated provision effects are identified from differences between a country's forest loss around entry into force of RTAs without environmental provisions and RTAs that do include such provisions. We detail this approach below.

6.1. Multiple Event Study Framework

We extend the event study framework in Abman and Lundberg (2020) to allow the dynamic effects of RTA enactment on net forest loss to differ by whether or not the RTA under study includes relevant environmental provisions with the following model:

$$
\begin{aligned}
y_{it} = {} & \delta_{LR-} \mathbb{1}[RTA_{(<-3),it}] + \sum_{\substack{s=-3,\\ s\neq -1}}^{3} \delta_s \mathbb{1}[RTA_{s,it}] + \delta_{LR+} \mathbb{1}[RTA_{(>3),it}] \\
& + \xi_{LR-} \mathbb{1}[enviro_{(<-3),it}] + \sum_{\substack{s=-3,\\ s\neq -1}}^{3} \xi_s \mathbb{1}[enviro_{s,it}] \\
& + \xi_{LR+} \mathbb{1}[enviro_{(>3),it}] + \alpha_i + \gamma_t + \varepsilon_{it}, \qquad (8)
\end{aligned}
$$

where y_{it} is the outcome of interest for country i in year t. α_i and γ_t control for time-invariant, cross-country differences in outcomes and year-to-year common changes to outcomes, respectively. The indicator variables $\mathbb{1}[RTA_{s,it}]$ measure time-since-enactment s of any RTA,[25] while $\mathbb{1}[enviro_{s,it}]$ measure time-since-enactment s of an RTA that includes environmental provisions under consideration—hence, δ_s will measure the dynamic effects of the entry into force of RTAs without environmental provisions, $\delta_s + \xi_s$ will measure the dynamic effects of RTAs with environmental provisions, and ξ_s will capture the marginal impacts of provision inclusion. RTAs with and without provisions enter into force at different times for different countries allowing us to separately identify the δ_s and ξ_s coefficients from the year fixed effects, as well as from each other. Countries may enact more than one RTA in our sample

25. For example, if an RTA was enacted in $t-1$, $\mathbb{1}[RTA_{1,it}] = 1$. If an RTA is enacted in $t+2$, then $\mathbb{1}[RTA_{-2,it}] = 1$.

period, which differentiates our event study framework from the RTA-level triple-difference model above as well as other common applied microeconomic settings in which an event only occurs once per individual within the sample window. We allow for overlapping events in our framework and normalize all estimates to the period before enactment by omitting the corresponding indicator variable.[26] We also include long-run RTA indicators that correspond to 4 or more years before/after entry into force of an RTA and allow these long-run indicators to vary by the presence of relevant environmental provisions. Notably, our RTA data constrains the sample coverage with this multiple overlapping event study. Credible identification of leading coefficients requires information about entry into force timing of future RTAs. Because our RTA data only runs through 2018, our overlapping event study sample covers 2001–2014, beyond which we lack data on the timing and content of post-2018 RTAs.

We briefly contrast this country-level approach to our agreement-level approach outlined in Section 3. Because countries enact more than one RTA in the sample, the triple-difference approach is not viable in this setting. In the RTA-level approach, individual countries may enter into multiple RTAs with different trading partners at different times, but the entire RTA group enacts an RTA only once in the study window. Because of this, our country-level approach estimates the dynamics around RTA enactments—a so-called "overlapping" or "multiple" event study because treatment events can overlap in timing—and allows them to differ based on the inclusion of relevant provisions. As in our RTA-level analysis, the coefficient estimates still measure net forest loss among all counterparties; an increase in one country's annual forest loss can be offset by a decrease in annual forest loss in another signatory country. As we do not estimate post-enactment average annual estimates (but rather leads and lags around enactment), we present estimates of 3-year cumulative net forest loss after RTA enactment by presence or absence of provisions as an analog to our triple-difference approach at the RTA-level. We also present our full event study graph in the text with coefficients tabled in the Appendix.

Although the multiple-event study identification strategy is not explicitly designed to address the endogeneity of environmental provisions like our matched sample approach above, there are several features of the model that account for a great deal of the underlying endogeneity. First, and perhaps most importantly, countries enter into multiple RTAs over our sample period—some with provisions and some without. Our use of country fixed effects creates a within-country interpretation to our coefficient estimates that yields arguably the most relevant observable counterfactual to provision inclusion. Second, as argued in Abman and Lundberg (2020), the *timing* of entry into force of RTAs—both those that include provisions and those that do not—is plausibly exogenous due to the multilateral ratification process. This exogeneity in

26. While there are a few options to dealing with multiple events that may occur in the same window, Sandler and Sandler (2014) argue that the approach employed here (allowing multiple indicators to be ones at the same time) is preferred to other approaches dealing with multiple events (i.e., duplicating observations, limiting treatment to the first event). Other approaches may incorrectly generate pre-treatment or post-treatment trends.

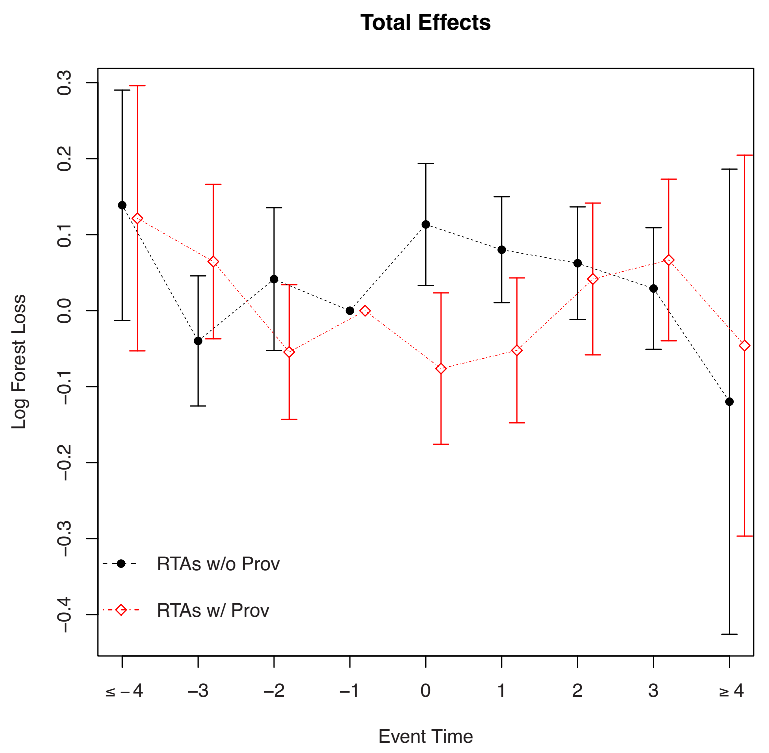

FIGURE 5. Country-level event study coefficients of RTA enactment on log forest loss. This figure presents event study coefficients for log forest loss before and after RTA enactment. Individual coefficients are presented in Table 8. Event time is relative to the year of RTA enactment and −4 and +4 coefficients represent the combined long run leads and lags, respectively. Filled dots represent the estimates for RTAs without forest or biodiversity provisions, diamond outlines correspond to RTAs with such provisions. The error bars represent 95% confidence intervals.

treatment timing creates credible identification of the dynamics around entry into force of both RTAs that do not include the provisions as well as those that do that is largely uncomplicated by potential endogeneity of provision inclusion.

6.2. Country-Level Results

We report country-level event study coefficients from equation (8) in Figure 5. The coefficients that correspond to "+/– 4" in event time are long-run leads/lags—δ_{LR+} and δ_{LR-} in equation (8)—and do not hold the same interpretation as the other event study coefficients. Consistent with Abman and Lundberg (2020), we find that RTAs lead to net increases in log forest loss upon enactment and the subsequent 3 years (black lines) relative to the year prior to enactment. The absence of significant leading coefficients provides evidence against effects being driven by diverging trends in forest

TABLE 8. Cumulative effects of RTAs on forest lost 3 years after entry into force.

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	No provisions	provisions	No provisions	provisions
Net 3-year forest loss	0.278*** (0.100)	−0.045 (0.135)	0.0017** (0.0008)	0.0010 (0.0013)
Relative to no provisions	–	−0.322** (0.148)	–	−0.0006 (0.0008)

Notes: Cumulative effects are computed as the sum of the event study coefficients at lags 0–3 from Figure 5 with appropriately transformed standard errors clustered at the country level. Models include country and year FEs. Sample is from 2001 to 2014 for proper identification of all coefficients. Statistical significance is denoted by $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

loss prior to RTA enactment. This increase is offset by the inclusion of the forest and biodiversity provisions. The red lines correspond to the event study coefficients for RTAs with environmental provisions. These provisions dampen the observed increases the year-of-and year after RTA enactment with later years indicating insignificant increases in forest loss. While some leading coefficients for RTAs with provisions appear to differ from leading coefficients without provisions, neither group has leading coefficients that differ from 0, nor is there evidence of differential trends prior to enactment.

We evaluate the cumulative effects 3 years after enactment on forest loss for RTAs with and without conservation provisions in Table 8. These estimates come from the sum of coefficients for the year of and 3 years following RTA enactment with the appropriately adjusted standard errors. For both forest loss measures, RTA enactment leads to significant increases in cumulative forest loss for agreements without conservation provisions, whereas increases in cumulative forest loss from RTAs with conservation provisions are small and statistically indistinguishable from zero. The lower panel presents the cumulative differences between those without and those with provisions. Using our log forest loss measure, the reduction in forest loss is sizable and statistically significant at the 5% level. In our rate of forest loss measure, the reduction associated with provisions is nearly half of the total increase from RTAs without provisions, but this difference is not statistically significant.

The results from our country-level analysis are qualitatively consistent with our agreement-level approach and, indeed, these results are quantitatively quite similar as well. As we discuss above, the country-level panel with country-level fixed effects, exogenous timing of entry into force, and dynamic effects together at least partly mitigate the selection bias issue of provision inclusion. The stability and persistence of our main findings across an entirely different identification strategy suggest that our results are not driven by a single empirical approach but represent a common, underlying causal response to the inclusion of environmental provisions in RTAs.

7. Concluding Remarks

In this paper, we evaluate the effectiveness of forest-related RTA provisions at limiting deforestation arising from trade liberalization. We find that provisions aimed at protecting forests and/or biodiversity mostly offset increases in net annual deforestation that follow entry into force of trade agreements without provisions, that is, provisions reduce forest loss relative to RTAs that do not include them. Back-of-the-envelope calculations from our triple-difference estimates indicate that the forest and biodiversity provisions studied in this paper prevented nearly 74,000 km^2 of deforestation between 2001 and 2018. We find evidence that reductions are driven by countries with more sensitive ecosystems. Our results indicate that this effect is at least partially attributable to (relative) reductions in agricultural expansion following RTA enactment. Finally, the impact of environmental provisions does not appear to hinge on the inclusion of "special" dispute settlement channels for the environmental provisions, but rather on general enforceability and broad-scope dispute settlement mechanisms. Our identification strategy addresses the potential endogeneity of environmental provision inclusion in trade agreements, yielding causal interpretations of our findings. Our treatment of the selection bias issue provides a roadmap for future studies on the effects of trade agreement content. We also address an artifact of our empirical approach by developing an extension to standard clustered covariance matrices that allows for sparse cross-cluster correlation.

This paper is the first to empirically investigate the effectiveness of environmental provisions in trade agreements and an additional future research is warranted on this topic—particularly work that address some limitations of the current study. First, we utilize a relatively coarse classification of trade agreement content. The language and scope of environmental protection may vary from agreement to agreement beyond what is captured by the available data, and such variations may have important implications for their subsequent effects. Second, as discussed above, our data only allow us to observe forest *loss* at the annual level, not forest gain. We acknowledge that this disparity may lead us to miss offsetting dynamics. Despite this limitation, we believe this is less of an issue due to the long-time horizons associated with afforestation. RTAs may lead to different long-run forest cover distributions, but the environmental consequences and forest dynamics around liberalization should be captured in the data we have. Third, we focus on the provisions most relevant to protection against forest loss. Given the high dimensionality of the RTAs, it is possible there are other provisions that are also important for forest conservation or other provisions that may help or hinder the effectiveness of the provisions we examine. We believe further exploration of these other provisions to be a fruitful avenue for future work. Finally, while we develop a useful framework that attempts to account for the endogeneity of trade agreement content, our propensity score matching approach fundamentally relies on the assumption that treatment selection (provision inclusion) occurs on observable characteristics of trade agreements, not unobservable factors that may also impact subsequent changes in net forest loss after enactment. We argue that our framework provides an improvement to existing literature in this arena, but we concede that the non-random nature of the content of RTAs will always raise challenges for causal identification. This motivates our country-level overlapping event study that at least partially circumvents this limitation.

Our work provides critical insights into the effectiveness of environmental provisions in mitigating forest loss arising from trade liberalization. The inclusion of such provisions on average *offsets* forest loss increases observed in trade agreements without environmental provisions. While environmental provisions appear to be an effective tool for mitigating environmental harm arising from trade liberalization, the costs and benefits are not born equally by all signatories. In particular, we find evidence that these provisions function, at least partially, by mitigating growth in agricultural production and trade. These findings suggest potential avenues forward in negotiation of future trade agreements to encourage sustainable growth.

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Supplementarty Data

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