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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. This paper provides new causal evidence that environmental provisions are effective in limiting deforestation following the entry into force of RTAs. It exploits 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, the paper finds that the inclusion of specific provisions aimed at protecting forests and/or biodiversity entirely offsets the net increases in forest loss observed in similar RTAs without such provisions. The inclusion of these provisions limits agricultural land expansion, but does not completely offset increases in total agricultural production. The effects are concentrated in tropical, developing countries with greater biodiversity.

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The Effectiveness of Environmental Provisions in Regional Trade Agreements*

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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. Abman and Lundberg, 2020; Zhang *et al.*, 2017; Leblois *et al.*, 2017) 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). 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 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 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.

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. From the set of RTAs that enter into force from 2004 and 2014, 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. 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 find large, significant net increases in annual forest loss following RTAs without provisions (23%) and that the inclusion of these provisions entirely offsets the rise in forest loss observed in the counterfactual RTA groups after enactment. 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 land expansion that otherwise occurs following the entry into force of trade agreements. RTAs without these environmental provisions lead to a 5% increase in the annual land area harvested on average while there is no increase in agricultural extensification following RTAs that include these provisions. Trade liberalization also leads to increases in agricultural output (as measured in tonnes harvested) that is partially, but not completely, offset by the inclusion of these environmental provisions. This suggests that provisions may limit agricultural land expansion, but not intensification. Net increases in agricultural exports are also lower in RTAs with environmental provisions, but not entirely offset. We find little evidence that production of timber products of forest exports are significantly affected. 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. We validate our 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.

Our primary contribution with this paper is to provide the first causal evidence on the effectiveness of environmental provisions in regional trade agreements.[1] In particular, we show that the inclusion of forest-related provisions has mitigated forest loss resulting from trade liberalization. Relative to Baghdadi *et al.* (2013), an early attempt to evaluate the impact of environmental provisions in RTAs on emissions, our empirical framework accounts for the potential endogeneity of RTA content, yielding policy-relevant insights into the causal impacts of such measures.[2]

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.[3] The more recent strand of this work has emphasized the importance of the varying content of RTAs in assessing their trade effects. Baier *et al.* (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 *et al.* (2017) show that "deep" agreements (i.e. RTAs that cover a larger number of policy areas beyond tariff reduction) are associated with a stronger

[1]Furthermore, to our knowledge this work is the first to develop a strategy to identify causal effects of RTA content more broadly.

[2]The treatment of endogeneity in Baghdadi *et al.* (2013) addresses endogenous bilateral selection into trade agreements as in Baier and Bergstrand (2004, 2007); Egger *et al.* (2008), but does not discuss selection bias in environmental provisions.

[3]Seminal works include Baier and Bergstrand (2007); Egger *et al.* (2011); Bergstrand *et al.* (2015). See Limão (2016) for a survey.

trade impact. The literature on trade agreements that focuses on non-trade issues such as labor and the environment is mostly theoretical (e.g. Limão, 2007; Maggi, 2016). Notable exceptions include Abman and Lundberg (2020) who study the impact of RTAs on deforestation, Baghdadi *et al.* (2013) that study the impact of RTAs with environmental clauses on emissions, and Brandi *et al.* (2020) that investigate whether environmental provisions in RTAs make exports from developing countries greener.

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 *et al.*, 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 *et al.*, 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 (Abman and Lundberg, 2020; Barbier and Rauscher, 1994; Sohngen *et al.*, 1999; Hannesson, 2000; Leblois *et al.*, 2017; López and Galinato, 2005; Barbier *et al.*, 2005; Barbier and Burgess, 2001). 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 *et al.* (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.

Our work also makes two methodological contributions. First, we develop a framework to identify causal impacts of provisions in trade agreements. While the literature considering trade agreement content is still nascent, the availability of new detailed data on trade agreement provisions will open many avenues for research in this area (Mattoo *et al.*, 2020). The central challenge to identification when assessing the impacts of individual provisions arises from the nonrandom nature underlying the content of RTAs. Previous empirical work has addressed the nonrandom 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, 2007; Egger *et al.*, 2008). Such a framework may not be appropriate when evaluating individual provisions as it does not address the nonrandom 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 regional trade agreements and detail the data sources used in our analysis. In Section 3, we present our 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 5, 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 have proliferated in the past 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 *et al.*, 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 the 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 *et al.*, 2020).

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 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.[4] 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?

[4] 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.

- Does the agreement require states to promote and protect biodiversity?

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 percent 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) to (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."

The list of trade agreements that require measures to prevent deforestation or to protect biodiversity is in Appendix Table A1. There are overall 51 agreements that include these provisions, 78 percent of which were signed after 2005. The largest share of these agreements is between a developed and a developing country (e.g. EU-Algeria, US-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 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 (CPTPP). 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.[5] RTAs can include more general language to promote the conservation and sustainable use of biological diversity and sustainable forestry management. Or 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 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 RTA between the EU and Cameroon contains similar, although not as detailed, provisions on forestry governance and trade in timber and forest products. Both agreements also include references to other international instruments that will support the objective of improved conservation, such as members' participation to multilateral environmental agreements (MEAs). Finally, both agreements foresee provisions on technical assistance and capacity building. In the case of the EU-Peru RTA, the agreement recommended a voluntary partnership agreement under the EU's action plan on forest law enforcement, governance and trade (FLEGT), which Cameroon signed shortly after the entry into force of the RTA.

2.2 Deforestation

Forest loss around the globe is driven by agricultural land expansion (both for small holder agriculture as well as commercial production), forestry practices, fires, and urban expansion (Curtis *et al.*, 2018). Tropical deforestation is overwhelmingly driven by agricultural expansion (Gibbs *et al.*, 2010), and accounts for a large share of global greenhouse gas emissions (Baccini *et al.*, 2012).

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 *et al.* (2017) (among many others), we aggregate these

[5] See Monteiro (2016) for details.

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 2014. 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 reports estimates of forest gain over a 12 year period (from 2001 - 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 different types of forest loss. The classification of tree cover is any vegetation greater than 5 meters 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 1.2) changes slightly in 2011 - 2014 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.

3 Methodology

We develop a matched *agreement-level* approach that aims to control for the potential endogeneity of environmental provisions. 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 US-Peru trade agreement by adding forest loss in Peru and forest loss in US 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 zero 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, e.g. deforestation, from one subset of signatories to a different subset 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, etc. are accounted for via the inclusion of α_g. We also include year fixed effects γ_t to control for common year-to-year shocks, e.g. changes in international commodity prices, that may likewise impact outcomes of interest.

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.

The central challenge to identification when assessing the impacts of individual provisions arises from the nonrandom nature underlying the content of RTAs. *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 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—e.g. 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 Estimating the likelihood of environmental provisions

We estimate propensity scores for the inclusion of environmental provisions by a cross-sectional logistic regression at the RTA level for agreements that are signed after 2003.[6] As above, for RTA g:

$$\mathbb{1}[Enviro\_RTA_g] = F\left(\beta_0 + \boldsymbol{\beta X_g}\right) \tag{3}$$

where $\mathbb{1}[Enviro\_RTA_g] = 1$ if agreement g includes environmental provisions, $F(\cdot)$ is the logistic link function, and $\boldsymbol{X_g}$ is a set of covariates. $\boldsymbol{X_g}$ includes a categorical variable describing development status of signatories (whether all parties are developed economies, developing economies, or if there are both developed and developing signatories), the number of signatories, linear and quadratic year of signature, maximum and average biodiversity indices across agreement signatories, the number of tropical signatories and an indicator for positive number thereof, the total forest cover of signatories in the year 2000, the total land area of signatories, the total percent of land

[6]Because our data on forest loss begins in 2001, we required a valid pre-treatement period for identification of treatment effects.

area among signatories covered by forest in 2000, the number of signatories that previously signed agreements with environmental provisions and an indicator for a positive numbers thereof, numbers and indicators of signatories in Africa, North America, Latin America, Europe, East or Southeast Asia, Central or South Asia, and Australasia and the Pacific, and finally, country indicators for agreement membership.[7] Because of the high dimensionality of this set of covariates (e.g. there are more than 100 country indicators alone), straightforward logistic estimation is not feasible. However, any *ad hoc* variable selection performed *ex ante* opens up our experimental design to concerns about selection bias in our matched sample arising from our specification choices.

We attempt to remove ourselves from the model selection via the use of a least absolute shrinkage and selection operator (LASSO) for our logit model selection and estimation. Logit LASSO estimation is a form of penalized maximum likelihood estimation where the penalty function is the ℓ_1-norm of $\boldsymbol{\beta}$ coefficients in Equation (3). This 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:

$$\max_{\beta_0,\boldsymbol{\beta}} \left\{ \frac{1}{n} \sum_{g=1}^{n} \ell(\mathbb{1}[Enviro\_RTA_g], \beta_0 + \boldsymbol{\beta}\boldsymbol{X_i} \mid \beta_0, \boldsymbol{\beta}) - \lambda \|\boldsymbol{\beta}\|_1 \right\} \tag{4}$$

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. $\sum_{j=1}^{k} |\beta_j|$). λ 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 cross-validation error which provides a good balance of model parsimony (from the LASSO procedure) and model fit (necessary for subsequent propensity score matching).[8] We also conduct sensitivity analyses on

[7] Our biodiversity index comes from UNEP (2007). Furthermore, 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.

[8] Table A2 presents the covariates selected (and the coefficient sign in the logit model) across a range of values for λ. This table includes only covariates that are ever chosen in the Lasso procedure in any of the penalty values and does not provide an exhaustive list of all candidate covariates. Selected covariates notably include the (maximum) biodiversity index among signatories, the percent of signatory land area that is forested, whether any signatories have previously signed an RTA with environmental provisions, whether any signatory is from Latin America, the number of North American signatories, the date that the agreement is signed, and finally, a variety of country indicators. Our analysis proceeds with $\lambda = 0.26$—the value that minimizes the mean cross-validation error.

the effects of λ on our matching and subsequent results in Section 4.4 below. Figure 2 provides kernel density plots for the fitted probabilities from our LASSO logit.

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 matched sample includes 35 treated agreements (with the provisions) and 10 control agreements (those without) that are weighted based on the number of treated units to which they are matched. As mentioned, since our panel covers 2001–2014, we restrict our sample of RTAs to those that enter into force after 2003.[9]

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 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.We acknowledge that unobservable factors that might differentially affect trading group blocs that have provisions relative to those that do not, and are correlated with the timing of RTA enactment, threaten this causal interpretation.

[9]We do so to ensure that there is at least three years of data in the period before RTAs enter into force. This pre-period is essential to credibly identifying the post-period effects of entry into force.

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):

$$Y = X\beta + \varepsilon \tag{5}$$

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

$$Var[\hat{\beta}] = (X'X)^{-1}\hat{V}(X'X)^{-1} \tag{6}$$

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

$$\hat{V} = \sum_{i=1}^{n}\sum_{j=1}^{n} w_{i,j} X_i'\hat{\varepsilon}_i\hat{\varepsilon}_j' X_j \tag{7}$$

where i and j index agreements, n is the number of agreements in our matched sample, and $w_{i,j}$ is a weighting function that allows for sparse cross-cluster correlation. Let n_i and n_j denote the number of countries that are party to trade agreements i and j, respectively, and I and J denote the set of parties to each respective agreements. Then our weight function is given by

$$w_{i,j} = \frac{1}{n_i} \sum_{k \in I} \mathbb{1}[k \in J] \tag{8}$$

where $\mathbb{1}[k \in J] = 1$ if country k is a party to agreement j. If there is no membership overlap between agreements i and j, $w_{i,j} = 0$ and we restrict the cross-cluster correlation between cluster i and j 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 i that are also members to agreement j. 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, $w_{i,j} = 1$ if $i = j$ and $w_{i,j} = 0$ for $i \neq j$.

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 Main Results

In Table 1 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).[10] 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 a regional trade agreement that does not have provisions aimed at protecting forest and/or biodiversity. Inclusion of forest-related provisions nearly eliminates this negative environmental impact of trade liberalization—with reductions in forest loss of approximately 23% relative to

[10] 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. As such, we proceed presenting only estimates on log forest loss.

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 0.1 percentage points following entry into force of agreements without provisions and does not increase for agreements that do include the provisions. However, the statistical evidence is weaker here—the effects of provision inclusion are only significant at the 10% level based on a one-sided hypothesis test and our conservative standard errors.[11]

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 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 1 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 7% 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–and statistically significant–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

[11] We believe this is a reasonable test as our primary hypothesis of interest is whether these provisions reduce forest loss.

an estimated increase in forest loss associated with such provisions and no effect on RTAs without provisions.

4.1 Exploring Potential Mechanisms

We explore potential mechanisms through which trade agreement provisions protecting biodiversity and forest might effectively mitigate forest loss arising from trade liberalization. In particular, we consider production and trade in forest products and agricultural commodities.

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 e.g. round logs, sawn lumber, etc. 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 this 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. But 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. But 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. As above, our trade measures aggregate bilateral trade flows across all trading partners which we likewise argue is the relevant measure to consider in our setting.

In Table 2 we report estimates of our triple-difference model on agricultural and forest product outcomes. We find strong evidence that environmental provisions limit agricultural extensification. In Column (1) we find that land area harvested increases approximately 6% following entry into force of an RTA without environmental provisions. The inclusion of such provisions offsets these increases, with 6% lower area harvested that is statistically significant at the 5% level. For RTAs that include these provisions, entry into force does not lead to any discernible changes in agricultural harvest area. Likewise, entry into force of RTAs without environmental provisions leads to an approximately 6% increase in the weight of agricultural output, however inclusion of environmental provisions does not totally offset this increase and the effect is not statistically differentiable from zero. Taken together, we interpret these findings as evidence that environmental provisions mitigate agricultural land expansion at the extensive margin while apparently still leading to output increases at the intensive margin–e.g. through increased yields. Agricultural exports exhibit similar patterns, with an increase of approximately 14% following entry into force for agreements without environmental provisions that are partially offset by provision inclusion, with approximately 10% lower increase in exports. We conclude that these provisions limit agricultural trade but note that agricultural exports still appear to increase following entry into force of agreements with environmental provisions (although this increase is not statistically significant). Forest-related production and exports exhibit qualitatively similar patterns–increased output and trade with RTA entry into force that is offset by inclusion of environmental provisions—but none of our forest-related estimates are statistically significant. These findings corroborate the view that environmental provisions in RTAs affect deforestation primarily through their impact on agricultural trade and production.

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 nontropical forest loss, forest loss in developed and developing countries, and forest loss in countries within the upper quartile of global biodiversity and the countries within the 75th percentile of global biodiversity. Along each of these dimensions, we only aggregate forest loss 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 and provision inclusion largely offsets these increases. However, the difference in the magnitudes of these effects is quite dramatic, with our findings driven by tropical deforestation rather than non-tropical forest loss. Likewise, our results are driven by forest loss in developing countries rather than developed economies. The only dimension along which our estimated magnitudes are similar is biodiversity, however the statistical significance is concentrated in high-biodiversity countries.

Taken together, our results suggest that forest loss arising from trade liberalization, as well as mitigating effects from inclusion of environmental provisions, is concentrated in relatively higher-risk countries.

4.3 Provision-specific dispute settlement mechanisms

All RTAs in our sample feature broad scope dispute settlement mechanisms that cover the entire trade agreement. However, some RTAs feature additional dispute settlement mechanisms unique to particular provisions that supplement the broad-scope, agreement-level mechanisms. We explore the marginal effectiveness of these supplemental dispute mechanisms to further understand how environmental provisions are effective. As discussed in Section 2, environmental provisions in RTAs may vary along several dimensions. Using data from Monteiro and Trachtman (2020), we distinguish between RTAs that subject forest and biodiversity provisions to a general dispute settlement mechanism and those RTAs that also include auxiliary environment-specific dispute settlement channels. We then estimate our main triple-difference model in Equation (2) with the addition of a *fourth* differencing dimension: whether the provision also includes the "special" dispute settlement for environmental provisions.

We present these findings in Table 4. 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 achieved by general enforcement mechanisms. While the coefficients on the interaction with enforcement provision in column (1) and (3) are indeed negative, they are not statistically differentiable from zero. This evidence supports the view that the combination of enforceable language and general dispute settlement is sufficient to achieve the intended environmental protection and "special" supplemental dispute settlement mechanisms for these provisions do not appear necessary.

4.4 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 3. On the far right, we plot the coefficients from the triple difference model from Equation (2) estimated on the full, unmatched panel of RTAs. Relatively high values of the penalty parameter do not provide enough latitude to accurately fit the logit model and hence 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 (e.g. a penalty parameter of approximately 0.06). As the value of the penalty parameter declines, the logit model provides a better fit and the matching procedure leads to more appropriate counterfactual control RTAs, therefore reducing the endogeneity bias.

4.5 Robustness to control group composition

A common practice in difference-in-difference or triple-difference models is to estimate an event study to investigate the presence of pre-trends or potential violations of the parallel trends assumption. We present such an event study for our main findings in Figure 4. Although the dynamics of agreements with the provisions do not appear to exhibit pre-trends, the figure raises some concerns that trends in forest loss among our matched control group may confound our results. Indeed, one of the control agreements that is repeatedly matched to treated agreements is the RTA between the Association of Southeast Asian Nations (ASEAN) and South Korea. ASEAN countries—including Indonesia, Cambodia, Vietnam—have struggled to contain rampant deforestation over our sample period. These patterns may contribute to the event study estimates and, in turn, affect our main triple-difference results. To explore this possibility, we create a new matched sample after omitting the ASEAN-Korea RTA from our set of candidate control agreements. We present triple-difference estimates of Equation (2) in Table 5 and a corresponding event study in Figure 5. These findings suggest that our main results are not an artifact of matching to the ASEAN-Korea RTA. The qualitative impact of provision inclusion remains the same and, indeed, we find a similar 24% reduction in forest loss when environmental provisions are included relative to agreements without the provisions. Furthermore, the event study in Figure 5 no longer exhibits trends in forest loss in our matched control group that might raise concerns about our findings.

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 appendix where Figure A1 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).

5 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. We detail this approach below.

5.1 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:

$$
\begin{aligned}
y_{it} = {} & \delta_{LR-}\mathbb{1}[RTA_{(<-3),it}] + \sum_{\substack{s=-3,\\ s\neq -1}}^{3} \delta_s \mathbb{1}[RTA_{s,it}] + \delta_{LR+}\mathbb{1}[RTA_{(>3),it}] \\
& + \xi_{LR-}\mathbb{1}[enviro_{(<-3),it}] + \sum_{\substack{s=-3,\\ s\neq -1}}^{3} \xi_s \mathbb{1}[enviro_{s,it}] + \xi_{LR+}\mathbb{1}[enviro_{(>3),it}] + \alpha_i + \gamma_t + \varepsilon_{it} \qquad (9)
\end{aligned}
$$

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[12] 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 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.[13] We also include long-run RTA indicators that correspond to four 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.

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

[12]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 $\mathbb{1}[RTA_{-2,it}] = 1$.

[13]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 first event, etc.). Other approaches may incorrectly generate pre-treatment or post-treatment trends.

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 three-year cumulative net forest loss after RTA enactment by presence or absence of provisions as an analogue 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 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.

5.2 Country-level Results

We report country-level event study coefficients from Equation (9) in Figure 6 with the full set of coefficients and standard errors reported in Appendix Table A3. The coefficients that correspond to '+/- 4' in event time are long-run leads/lags—δ_{LR+} and δ_{LR-} in Equation (9)—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 three years (black lines) relative to the year prior to enactment. We see no significant leading coefficients which provides evidence against effects being driven by diverging trends in forest 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 environmnetal 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 zero, nor is there evidence of differential trends prior to enactment. The estimates using the rate of forest loss as an outcome variable are qualitatively similar and presented along side our estimates on log loss in Appendix Table A3.

We evaluate the cumulative effects three years after enactment on forest loss for RTAs with and without conservation provisions in Table 6. These estimates come from the sum of coefficients for the year of and three years following RTA enactment from Appendix Table A3, 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 percent 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.

6 Concluding Remarks

In this paper we evaluate the effectiveness of forest-related RTA provisions at limiting deforestation arising from trade liberalization. We find no changes in net annual deforestation following implementation of agreements that include provisions aimed at protecting forest and/or biodiversity while agreements without these provisions see substantial increases in net forest loss, i.e. 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 approximately 7,500 square kilometers of deforestation from 2003–2014. 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 land expansion following RTA enactment, while we find no evidence that these provisions operate through timber product markets. 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.

We acknowledge several limitations of our work. 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 of 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 nonrandom 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 land area. Encouragingly, provision inclusion does not appear to preclude agricultural gains from trade at the intensive margin. These findings suggest potential avenues forward in negotiation of future trade agreements to encourage sustainable growth.

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Tables and Figures

Table 1: Aggregate Forest Loss

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	(1)	(2)	(3)	(4)
Post RTA	0.236***	0.019	0.001***	0.00004
	(0.046)	(0.044)	(0.0004)	(0.0001)
Post × Enviro RTA	−0.230***	0.067	−0.001†	0.001**
	(0.048)	(0.045)	(0.001)	(0.0003)
Observations	630	1,918	630	1,918
R^2	0.983	0.990	0.823	0.768
Matched	✓	–	✓	–

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 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. Our null hypothesis is that environmental provisions reduce forest loss. Hence, we also include statistical significance at the 10% level from from one-sided t tests on our triple difference parameters denoted by †p<0.1

Table 2: Trade and Production of Agricultural Output and Forest Products

	Dependent variable:				
	Ag (Ha)	Ag (Ton)	Ag Exports	Timber	Forest Exports
	(1)	(2)	(3)	(4)	(5)
Post RTA	0.055^{**}	0.061^{*}	0.137^{**}	0.037	0.070
	(0.022)	(0.036)	(0.054)	(0.027)	(0.089)
Post × Enviro RTA	-0.055^{**}	-0.043	$-0.100^{\dagger}$	-0.030	-0.107
	(0.025)	(0.054)	(0.074)	(0.035)	(0.086)
Observations	616	616	616	602	616
R^2	0.999	0.998	0.995	0.997	0.988

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 blocs are computed as the sum across member countries by year. Ag (Ha) is the area harvested in hectares. Ag exports are the are the export value of all agricultural products. Timber is measured in m^3 and includes round and sawn wood, wood chips, and processed boards (e.g. MDF, plywood, etc.). Forest exports are the export value of all forest-derived product. 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. Our null hypothesis is that environmental provisions reduce forest loss. Hence, we also include statistical significance at the 10% level from from one-sided t tests on our triple difference parameters denoted by †p<0.1

Table 3: Forest loss effects by country type

Panel A: Log Forest Loss in Higher Risk Ecosystems

	Tropical	Developing	High Biodiv
	(1)	(2)	(3)
Post RTA	0.257***	0.233***	0.228***
	(0.062)	(0.065)	(0.079)
Post × Enviro RTA	−0.188***	−0.212***	−0.212**
	(0.057)	(0.057)	(0.090)
Observations	630	630	630
R^2	0.997	0.994	0.998

Panel B: Log Forest Loss in Lower Risk Ecosystems

	Non tropical	Developed	Lower Biodiv
	(1)	(2)	(3)
Post RTA	0.048	0.094	0.219
	(0.111)	(0.125)	(0.216)
Post × Enviro RTA	−0.092	−0.099	−0.268
	(0.151)	(0.150)	(0.286)
Observations	630	630	630
R^2	0.998	0.997	0.996

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 4th 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 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 without replacement. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table 4: Provision-specific Dispute Settlement Mechanisms and Aggregate Forest Loss

	Dependent variable:			
	Log Forest Loss		Deforestation Rate	
	(1)	(2)	(3)	(4)
Post RTA	0.236***	0.019	0.001***	0.00004
	(0.046)	(0.044)	(0.0004)	(0.0001)
Post × Enviro RTA	−0.230***	0.064	−0.001	0.001*
	(0.051)	(0.059)	(0.001)	(0.0004)
Post × Enviro RTA × Dispute	−0.001	0.009	−0.001	−0.001
	(0.089)	(0.130)	(0.0005)	(0.001)
Observations	630	1,918	630	1,918
R^2	0.983	0.990	0.832	0.809
Matched	✓	–	✓	–

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 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. Our null hypothesis is that environmental provisions reduce forest loss. Hence, we also include statistical significance at the 10% level from from one-sided t tests on our triple difference parameters denoted by †p<0.1

Table 5: Aggregate Forest Loss – Omitting ASEAN-Korea RTA

	Dependent variable:	
	Log Forest Loss	Deforestation Rate
	(1)	(2)
Post RTA	0.179*	0.00002
	(0.093)	(0.0005)
Post Enviro RTA	−0.244***	−0.0003†
	(0.069)	(0.0005)
Observations	616	616
R^2	0.975	0.811

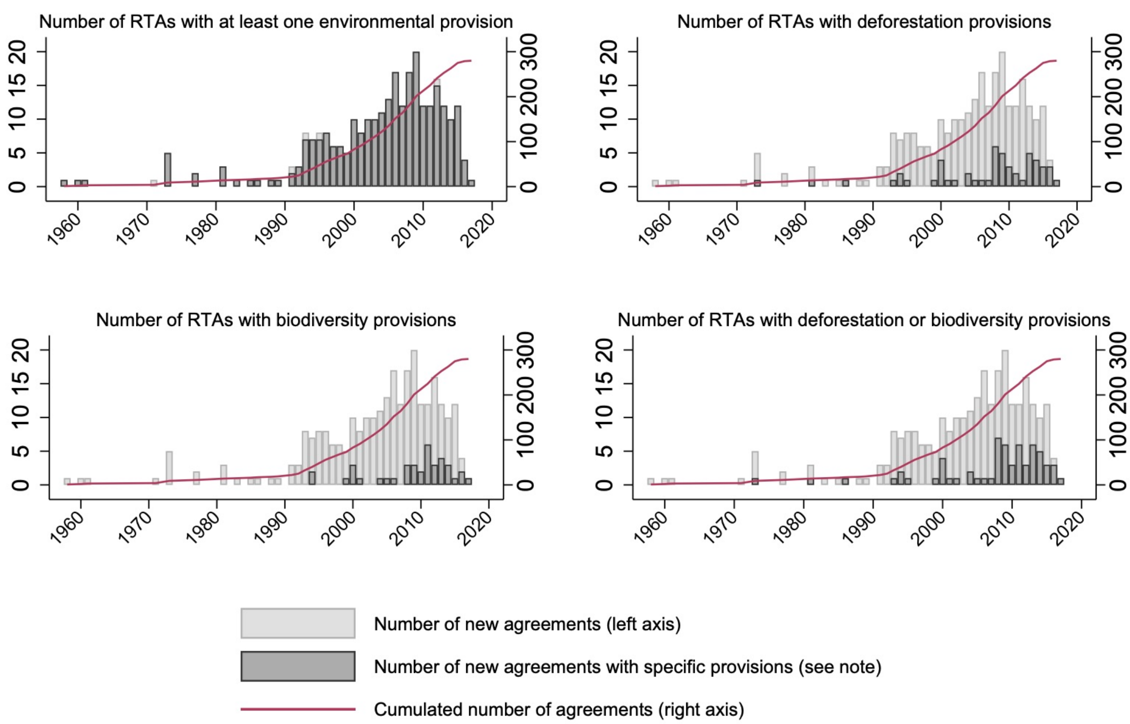
FE triple-difference regressions on a panel of RTA-level trading blocs (i.e. observations are at the RTA and year level) matched after omitting the ASEAN-Korea RTA from the candidate control agreements. Annual forest loss for these blocs is computed as the sum across member countries by year. All models 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 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. Our null hypothesis is that environmental provisions reduce forest loss. Hence, we also include statistical significance at the 10% level from from one-sided t tests on our triple difference parameters denoted by †p<0.1

Table 6: Cumulative effects of RTAs on forest lost three years after entry into force

	Dependent variable:			
	log(1 + loss)		loss_rate	
	no prov	prov	no prov	prov
Net 3-year forest loss	0.373***	0.039	0.002***	0.001
	(0.094)	(0.127)	(0.001)	(0.001)
Relative to no prov	—	-0.334**	—	-0.0007
		(0.137)		(0.0006)

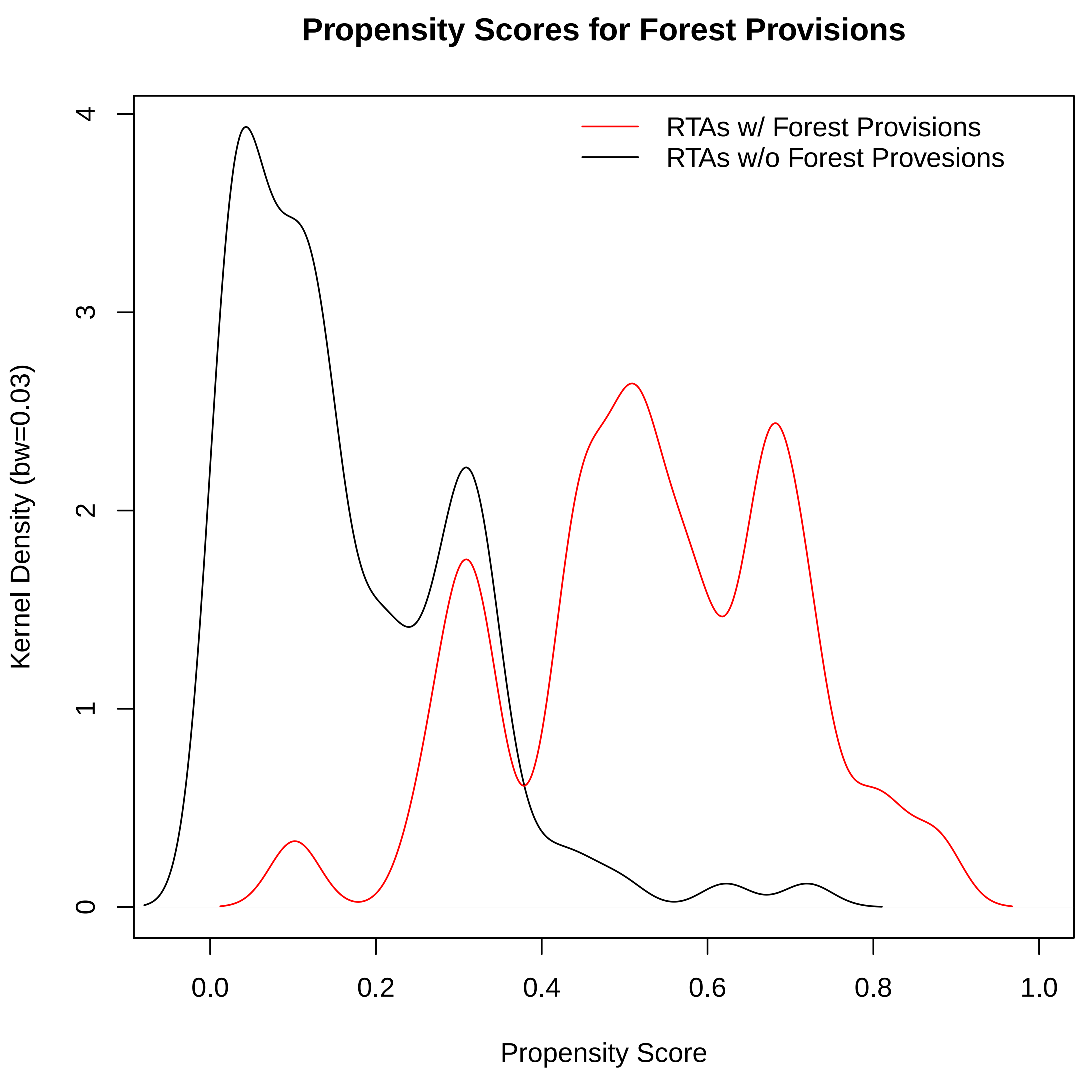
Cumulative effects are computed as the sum of the event study coefficients at lags 0–3 from Table A3 with appropriately transformed standard errors clustered at the country level. Models include country and year FEs. Sample is from 2001–2014 for proper identification of all coefficients. Statistical significance is denoted by *p<0.1; **p<0.05; ***p<0.01

Figure 1: Regional trade agreements and environmental provisions over time



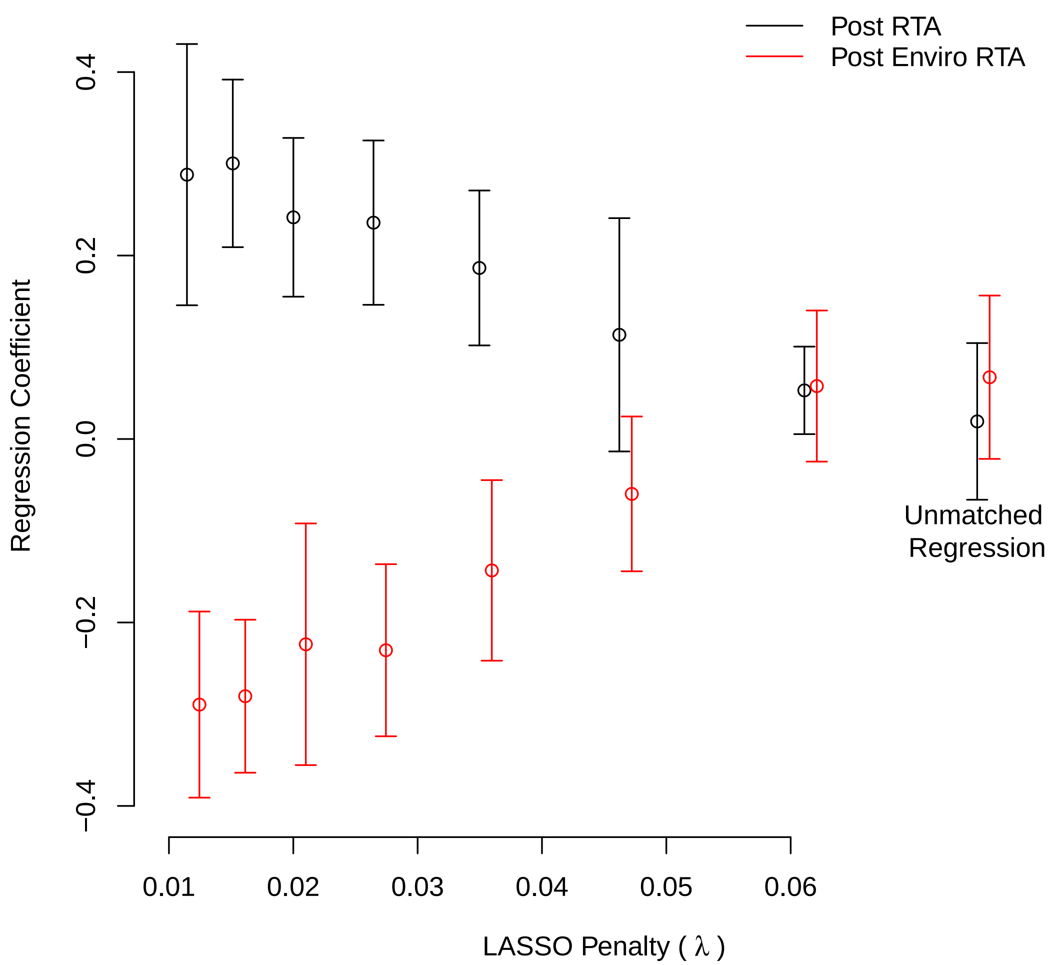
The graph plots the number of new RTAs entering in force over the period 1958-2017. It disentangles RTAs including at least one provision on the environment (upper-left), RTAs requiring states to prevent deforestation (upper-right), RTAs protecting biodiversity (bottom-left) and RTAs preventing deforestation or protecting biodiversity (bottom-right). The red line shows the cumulative number of RTAs enacted by each year. Source: Mattoo *et al.* (2020).

Figure 2: Fitted Probabilities of Inclusion of Forest-related Provisions in RTAs



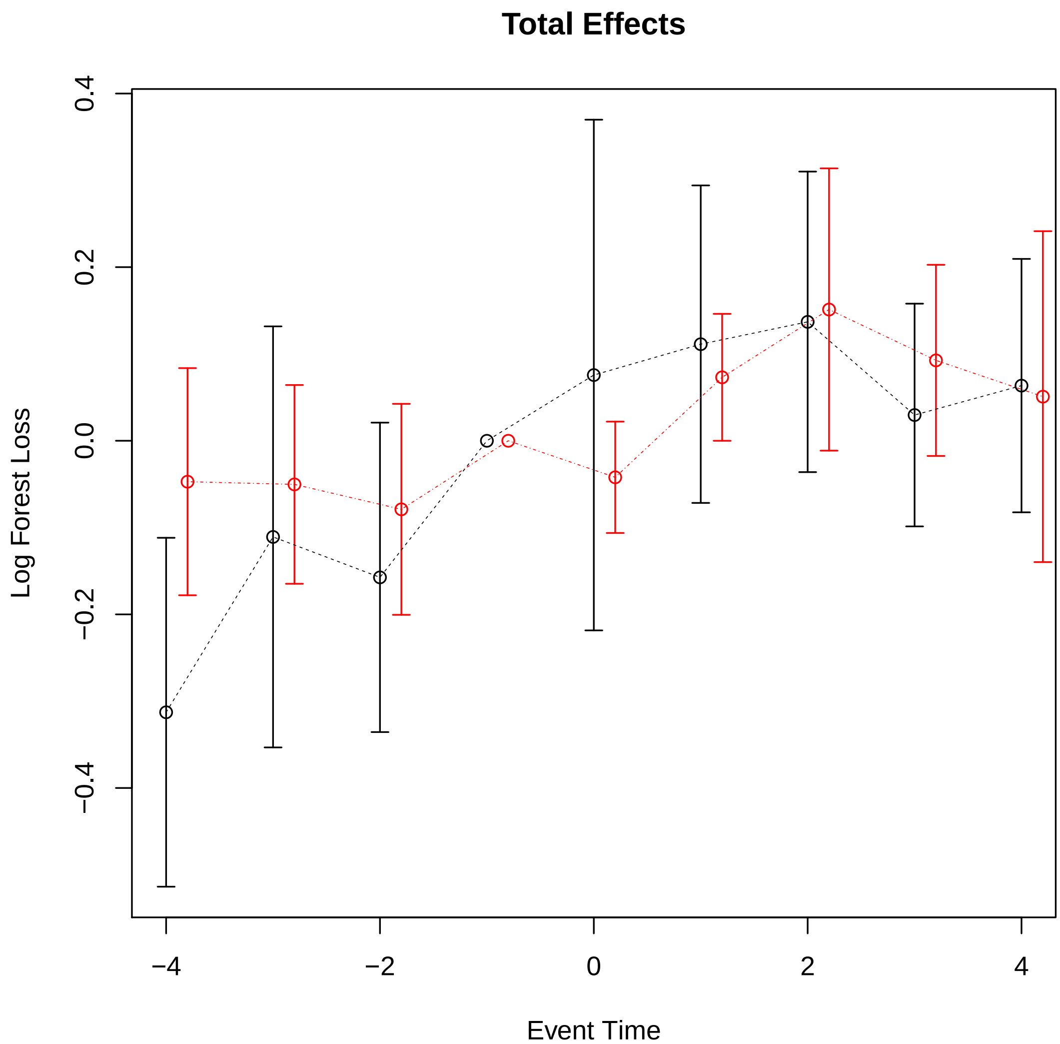
This figure compares the fitted probability of the inclusion of forest-related environmental provisions in RTAs across the agreements that ex-post include the provisions and those that do not. Fitted probabilities are from a cross-sectional LASSO logit model estimated on all RTAs notified to the WTO 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.

Figure 3: Sensitivity Analysis of LASSO Penalty (λ)



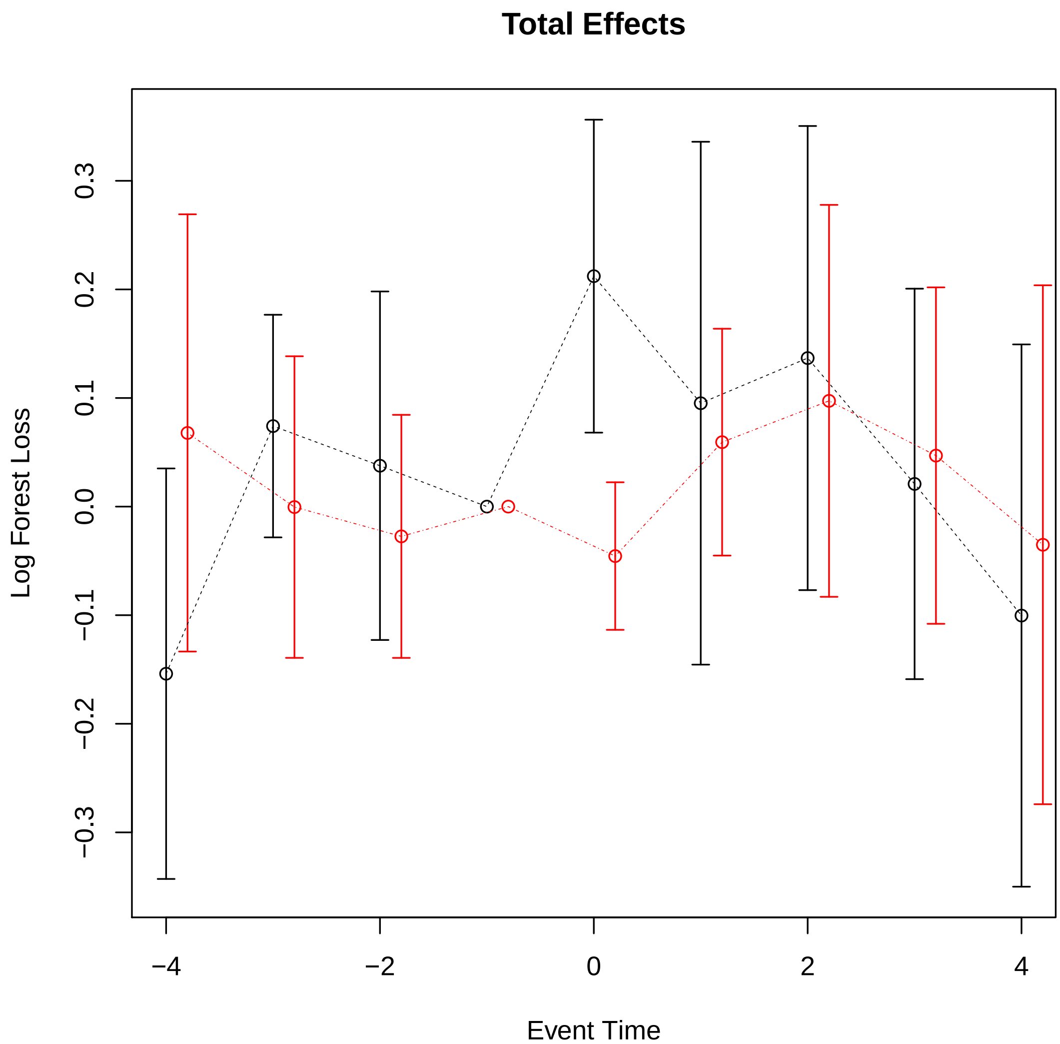
This figure presents coefficient estimates from our main triple-difference model of RTA-level net log forest loss as functions of LASSO penalty parameters. Black lines correspond to estimates of β_1 and red lines correspond to estimates of β_2 in equation (2). Varying LASSO penalties yield different 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.

Figure 4: Matched RTA-level event study – total effects



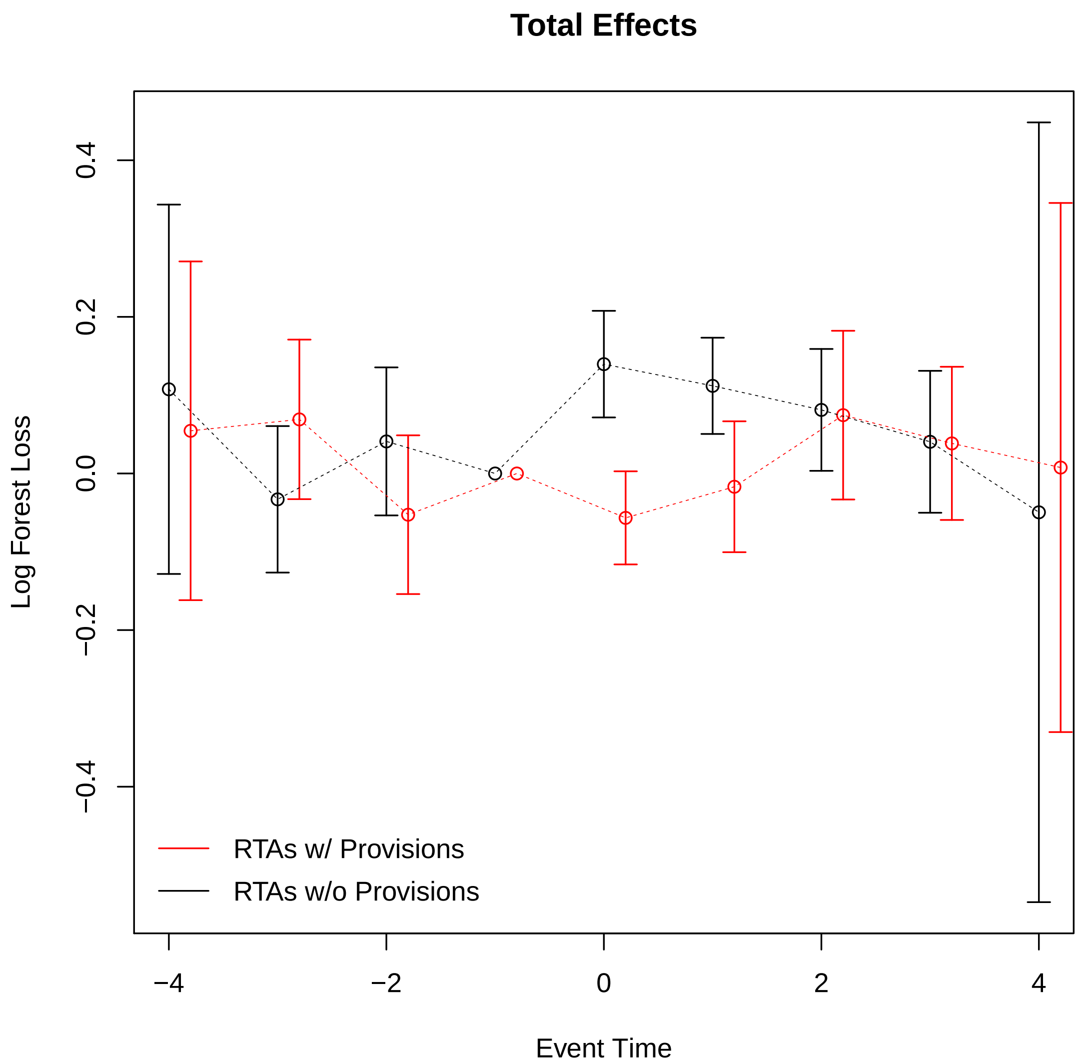
This figure illustrates matched sample event study total effects along with 95% confidence intervals from our panel of RTA-level trading blocs (i.e. observations are at the RTA and year level). The matched sample is created by matching with replacement on LASSO logit estimated propensity scores. Annual 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 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.

Figure 5: Matched RTA-level event study omitting ASEAN-Korea RTA – total effects



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 omitting the ASEAN-Korea RTA from the set of 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 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.

Figure 6: 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 the Appendix Table A3. Event time is relative to the year of RTA enactment and -4 and +4 coefficients represent the combined long run leads and lags, respectively. Black dots represent the estimates for RTAs without forest or biodiversity provisions, red dots correspond to RTAs with such provisions. The error bars represent 95 percent confidence intervals.

A Supplemental Tables and Figures

Table A1: List of RTAs requiring states to prevent deforestation or protect biodiversity (or both)

Agreement	Entry in force Year	Supplemental Dispute Settlement
EC-South Africa	2000	1
East African Community (EAC)	2000	1
EC-FYR Macedonia	2001	0
EU-San Marino	2002	0
EC-Algeria	2005	1
Japan-Malaysia	2006	0
Brunei Darussalam - Japan	2008	1
EC-CARIFORUM	2008	0
El Salvador - Honduras - Chinese Taipei	2008	1
Japan-ASEAN	2008	0
Japan-Indonesia	2008	1
Japan-Philippines	2008	1
Nicaragua - Chinese Taipei	2008	0
Canada-Peru	2009	1
Chile - Colombia	2009	0
EC-Cameroon	2009	0
Japan-Viet Nam	2009	1
US-Peru	2009	1
China-Peru	2010	1
New Zealand - Malaysia	2010	0
Canada - Colombia	2011	0
EFTA - Colombia	2011	0
EFTA - Peru	2011	1
EU - Korea, Republic of	2011	1
Peru - Korea, Republic of	2011	0
Turkey - Chile	2011	0
Chile - Malaysia	2012	0
EU - Eastern and Southern Africa States Interim EPA	2012	1
US - Colombia	2012	0
Canada - Panama	2013	0
EU - Central America	2013	0
EU - Colombia and Peru	2013	0
Korea, Republic of - Turkey	2013	1
Malaysia - Australia	2013	0
Canada - Honduras	2014	0
EFTA - Central America (Costa Rica and Panama)	2014	0
EU - Georgia	2014	1
EU - Republic of Moldova	2014	0
EU Ukraine	2014	1
Korea, Republic of - Australia	2014	0

The table lists trade agreements requiring measures to prevent deforestation and/or measures to protect biodiversity by the year they enter into force. The right-most column indicates the existence of supplemental dispute settlement mechanisms on the relevant environmental provisions.

Table A2: LASSO Logit Variable Selection

	LASSO Penalty (λ)						
	0.061	0.046	0.035	0.026	0.02	0.015	0.011
Year signed	+	+	+	+	+	+	+
Year signed^2		+					
Developed-Developing	+	+	+	+	+	+	+
Developing-Developing	-	-	-	-	-	-	-
Parties							+
Tropical Any					+	+	+
Forest Total %	+	+	+	+	+	+	+
Max BioDiv	+	+	+	+	+	+	+
Template Any	+	+	+	+	+	+	+
Template Number	+						
N Amer Number		+	+	+	+	+	+
Lat Amer Any	+	+	+	+	+	+	+
AUT		+	+	+	+	+	+
GTM					-	-	-
HND					+	+	+
NIC					+	+	+
IND		-	-	-	-	-	-
KOR		+	+	+	+	+	+
PNG				-	-	-	-
FJI				-		-	-
NZL						+	+
CHL					-	-	-
COL	+	+	+	+	+	+	+
MEX			-	-	-	-	-
PER				+	+	+	+
THA			-	-	-	-	-
TUR					+	+	+
BRN						+	+
MYS		+	+	+	+	+	+
SGP		-	-	-	-	-	-
VNM						-	-
CAN	+	+	+	+	+	+	+
COD				-	-	-	-
MDG						+	+
SWZ			-	-	-	-	-
MDA				+	+	+	+
CMR		+	+	+	+	+	+
CIV				-	-	-	-
ZAF			-	-	-	-	-
GEO				+	+	+	+
JPN	+	+	+	+	+	+	+
BIH				-	-	-	-
AGO						-	-
BWA					-	-	-
PAN						-	-
ALB						-	-
MNE						-	-
SRB					-	-	-
TWN					+	+	+
SMR		+	+	+	+	+	+
MNG			+	+	+	+	+

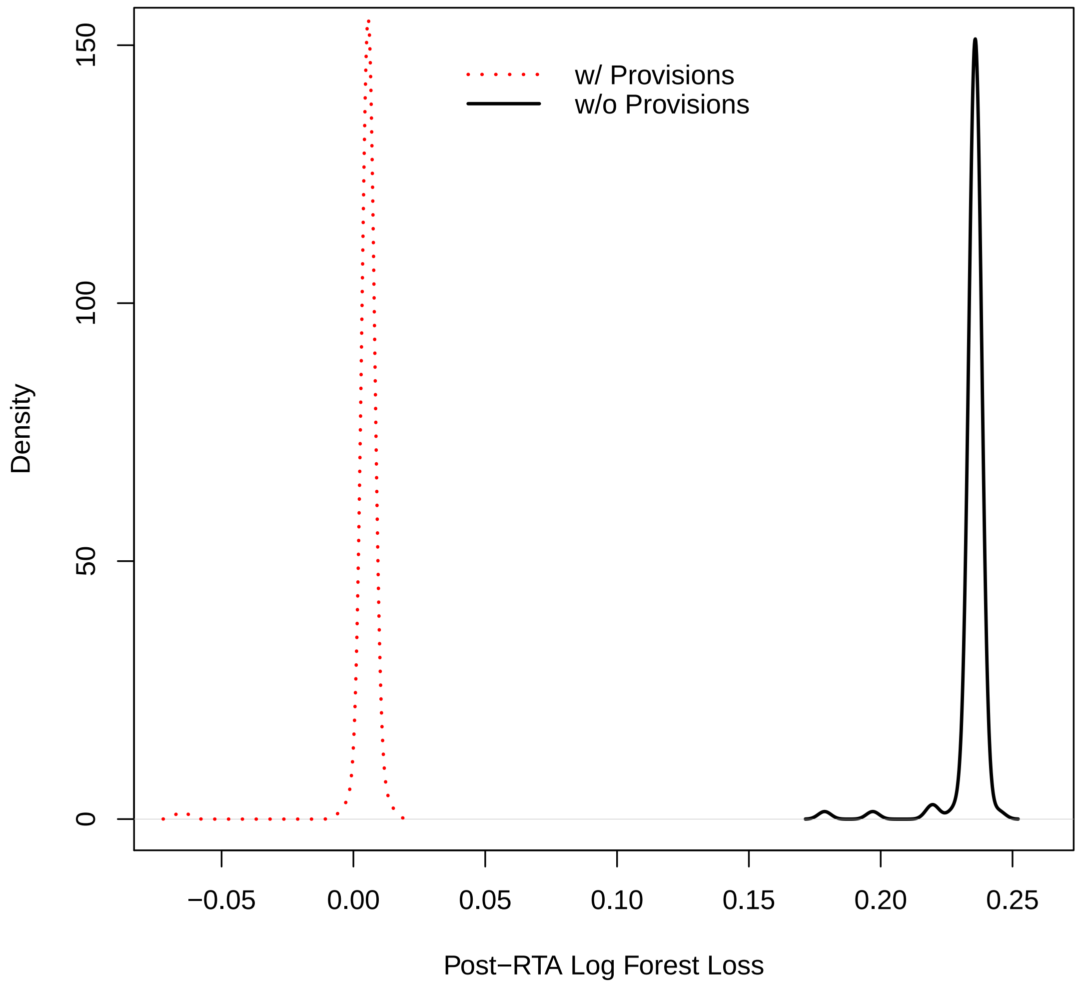
This table presents variables selected from the LASSO model across different values of the penalty parameter and indicates their sign in predicting the presence of environmental provisions in RTAs. The middle column ($\lambda = 0.026$) is the specification used in the main analysis.

Table A3: Country-level Event Study of RTA Enactment

	Dependent variable:	
	Log Forest Loss	Deforestation Rate
	(1)	(2)
RTA^{np}_{LR-}	0.108 (0.073)	−0.001 (0.001)
RTA^{np}_{t-3}	−0.033 (0.038)	−0.001 (0.0004)
RTA^{np}_{t-2}	0.041 (0.041)	0.0001 (0.0004)
RTA^{np}_{t-1}	...	...
RTA^{np}_{t}	0.140*** (0.037)	0.0003* (0.0002)
RTA^{np}_{t+1}	0.112*** (0.034)	0.0003* (0.0002)
RTA^{np}_{t+2}	0.081** (0.034)	0.001* (0.0004)
RTA^{np}_{t+3}	0.040 (0.036)	0.0003* (0.0002)
RTA^{np}_{LR+}	−0.050 (0.125)	0.0005 (0.002)
RTA^{prov}_{LR-}	0.054 (0.074)	−0.001 (0.001)
RTA^{prov}_{t-3}	0.069 (0.046)	0.001 (0.001)
RTA^{prov}_{t-2}	−0.053 (0.046)	0.0002 (0.0004)
RTA^{prov}_{t-1}	...	...
RTA^{prov}_{t}	−0.057 (0.045)	−0.001** (0.0002)
RTA^{prov}_{t+1}	−0.017 (0.049)	−0.0002 (0.0004)
RTA^{prov}_{t+2}	0.074 (0.051)	0.001 (0.001)
RTA^{prov}_{t+3}	0.038 (0.049)	0.001 (0.001)
RTA^{prov}_{LR+}	0.008 (0.105)	0.002 (0.002)
Observations	2,702	2,702
R^2	0.98	0.14

All models include country and year FEs. Sample is from 2001–2014 for proper identification of all coefficients. RTA^{np}_{t-i} denotes i periods before an RTA with *no provisions* (forest-related environmental provisions) while *prov* denotes RTAs with environmental provisions. Reported coefficients for RTAs with environmental provisions are total effects (i.e. $\delta_s + \xi_s$ from Equation (9)). Standard errors are clustered at the country level and reported in parentheses. Statistical significance is denoted by *p<0.1; **p<0.05; ***p<0.01

Figure A1: Sensitivity Analysis of Matching Estimates



In this graph, we present the distribution of estimates of our post-enactment forest loss effects for RTAs without provisions (β_1) and those with provisions ($\beta_1 + \beta_2$) as we iteratively omit each RTA in our matched control group. The narrow distribution indicates that our findings are not driven by any individual matched control RTA group.