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# Tragedy, property rights, and the commons: investigating the causal relationship from institutions to ecosystem collapse\*

Elisabeth Thuestad Isaksen<sup>†‡</sup>      Andries Richter<sup>§¶</sup>

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## Abstract

Do private property rights mitigate overexploitation of common pool resources, and if so, under which circumstances? In this paper, we examine the effects of private property rights on the status of marine fisheries by combining data on ecological, economic and institutional characteristics into a panel data set, spanning over 50 years, 170 exclusive economic zones and 800 species. To address the inherent endogeneity problem of policy implementation, we employ both a difference-in-differences (DiD) and instrumental variable (IV) strategy. Results from both estimations suggest that property rights lower the probability of a fish stock collapsing, but the effect varies with country and species characteristics. Specifically, we find evidence suggesting that property rights are more effective when ownership is transferable, the general level of ownership protection is strong, trade openness is high, the regenerative capacity of the resource is high, and the species value is high.

**Keywords:** common pool resource, property rights, catch shares, fisheries, endogenous institutions, instrumental variable

**JEL codes:** C33, C36, Q22, Q28

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# 1 Introduction

One of the key properties of social-ecological systems is that social and natural processes are intrinsically linked and mutually influence each other. While formal institutions are typically designed to prevent undesirable outcomes, such as economic losses or ecological collapse, their actual implementation usually depends on how the system has performed in the past. This potential reverse causality makes it difficult to evaluate causal effects of implemented policies. Marine fisheries are prime examples of systems where economic, biological, and governance processes are deeply intertwined (Webster, 2015; Grainger and Parker, 2013), and evaluating the effectiveness of institutions such as private property rights is therefore challenging. In addition to establishing causality, understanding under which conditions property rights are effective, and why, is crucial for successful resource management. Have current rights-based policies mitigated problems of overuse, and if so, what characterizes policy success? Such insights are sorely needed in the case of marine fisheries, where overexploitation and depletion is widespread (Costello et al., 2016).

In this paper, we investigate the relationship between overexploitation of marine resources and private property rights (PPRs) by exploiting a global panel dataset on ecological, economic and institutional variables, spanning over 5 decades. The first goal of the paper is to recover a *causal* estimate of policy implementation on ecological outcome variables to see if property rights can help to mitigate the tragedy of the commons in ocean fisheries. The second goal of the paper is to investigate under *which conditions* PPRs are likely to be successful. Are there key characteristics of countries or the resource itself that are likely to strengthen the effect of PPRs, and are there specific policy design elements that are associated with better performance? Such insights may shed light on the potential effectiveness of PPRs if implemented in settings where resource and country characteristics differ. As the far majority of stocks around the world are still subject to open-access, learning from past experience can provide important insights for policy-makers on the effectiveness of private property regimes.

The seminal paper on the effect of PPRs on the state of marine ecosystems is Costello et al. (2008), showing that fisheries that have adopted individual transferable quotas (ITQs) are less prone to collapse. While their study took the literature a step forward by combining a global dataset with a difference-in-differences (DID) strategy, it also has several limitations. First, their estimation strategy does not account for potential reverse causality and time-varying omitted variable biases, calling into question their ability to make causal claims. Second, they are not able to distinguish the effect of tradable quotas from non-tradable quotas, or from having a total allowable catch (TAC) - although a follow-up study (Costello et al., 2010) provides suggestive evidence that quota systems do have an effect beyond the assigned TAC.<sup>1</sup> Third, while they find a favorable effect

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<sup>1</sup>A TAC limits the total allowable catch, and is an essential part of a quota system.

on average, their estimate might conceal considerable heterogeneity across countries and species. This makes it hard to draw policy recommendations on potential expansions to other geographical, institutional and ecological settings. Fourth, they infer the state of the stock through catches, rather than biomass, which limits understanding of ecological outcomes, such as a stock collapse.

While later studies have tried to address some of these issues (see e.g., [Costello et al., 2010](#); [Essington, 2010](#); [Melnychuk et al., 2012](#)), the inherent endogeneity problem of policy implementation is not addressed. As a consequence, the findings might merely reflect correlations and/or potential feedback effects from the environment to the regulatory regime. Without establishing causality, we cannot be sure that implementing a PPR regime will have any effect on collapse.

In this study, we address several shortcomings of the previous literature. First, our study is one of few global empirical analyses on property rights and ecological outcomes, covering over 170 exclusive economic zones (EEZs) and 800 species over a 50 year period. Second, we address potential problems of reverse causality and omitted variable bias by combining a DiD estimator with several time-varying control variables. Furthermore, by estimating a DiD with leads and lags we can examine how treatment effects unfold over time, as well as explicitly test if pre-treatment trends are similar for the treatment and control group. To further strengthen the credibility of our results, we employ a novel instrumental variable strategy. The instrument we propose is the implementation of quota systems in other environmental domains, like forest, land, water, hunting, and pollution. We hypothesize that implementation of quota systems in other domains partly reflect an underlying preference for market-based solutions to deal with common pool resources, and in particular a preference towards assigning property rights. By exploiting variation in an instrumental variable, as well as including several time-varying control variables, we hope to mitigate potential problems of reverse causality and unobserved characteristics driving the results. While other studies in environmental economics have successfully employed an IV strategy to estimate a causal relationship between PPRs and resource use (see e.g., [Liscow, 2013](#); [Aichele and Felbermayr, 2012](#)), we are, as far as we are aware of, the first global study to employ an IV strategy to estimate a causal effect of private property rights on overexploitation of marine resources.

Third, in contrast to [Costello et al. \(2008\)](#), we examine the effects of a broader group of quota systems and not only ITQs. This allows us to test for potential differential effects across quota systems with different properties, such as the transferability of quotas. Furthermore, as our instrumental variable is meant to capture a preference for market-based solutions, it helps to identify effects of quota systems that go beyond assigning a TAC. Fourth, while we use exploitation status inferred from catch data in the main analysis, we do a separate analysis where we infer exploitation status from biomass for (i) a small subsample where biomass data is available, and (ii) simulated biomass data for

the entire sample from catch data following the method developed by [Martell and Froese \(2013\)](#).

Lastly, and importantly, we examine how effects of PPRs interact with different country and species characteristics, such as the strength of ownership protection, trade openness, species value, and the regenerative capacity of the resource. In countries with uncertain ownership rights, a tradable quota system might de facto resemble an open access regime ([Grafton, 2000](#); [Fischer and Laxminarayan, 2010](#); [Grainger and Costello, 2014](#); [Copeland and Taylor, 2009](#); [Costello and Grainger, 2018](#)). While a firm may decide to overexploit a resource if property rights are not secure ([Long, 1975](#)), insecure property rights may also discourage a firm from making the necessary investments in the first place, potentially leading to underexploitation ([Laurent-Lucchetti and Santugini, 2012](#)). Empirically, the ambiguous nature of property rights on exploitation have been established by [Bohn and Deacon \(2000\)](#). Further, while [Chichilnisky \(1994\)](#) argues that trade fuels overexploitation in countries with weak property rights, [Brander and Taylor \(1997\)](#) show that trade openness can also work in the opposite direction. [Copeland and Taylor \(2009\)](#) consider in a theoretical model the case where property rights are endogenous. Upon opening for trade, a country may make a transition from (de facto) open access to PPRs, depending on various factors, such as the enforcement power of a government and the intrinsic growth rate of a species. Understanding how various country and species characteristics influence the success of PPRs also enables a discussion of where PPRs would likely be effective if implemented.

Results from the DiD specification with leads and lags show that the pre-treatment development in ecological outcomes is not parallel for PPR and non-PPR fisheries. Failing to account for violation of the common trend assumption will lead to biased estimates. By including several time-varying covariates in the regression, we mitigate the problem of different pre-treatment trends. Furthermore, by coupling the DiD with an instrumental variable strategy, we hope to further strengthen the credibility of our results. Using both a DiD with time varying covariates and an IV-DiD specification, we find that property rights have a positive effect on ecological outcomes by preventing collapse of the fish stock. On average, PPRs lower the probability of a collapse by around 7-8% when using the DiD estimate.<sup>2</sup> Assuming a linear treatment effect over time, this translates into an annual effect of around 0.6%, which is slightly larger than what has been found in previous studies. By allowing the treatment effect to vary in a non-linear way over time, we find that effects of the quota systems do not materialize until a decade after implementation, suggesting that it takes time for stocks to rebuild. After 15 years, a PPR-fishery is about 7% less likely of being collapsed compared to a non-PPR fishery. After 20 and 26 years

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<sup>2</sup>The estimated favorable effect is robust to a battery of robustness checks, including adding various covariates, restricting the sample to treated species or treated EEZs only, and assigning collapse status based on simulated biomass data. We also run placebo tests where we randomly assign treatment status to non-PPR fisheries within treated EEZs, and find no favorable effect.

the estimated probabilities increase to 11% and 21% respectively. While we find a similar pattern when inferring exploitation status from biomass data in a small subsample, large standard errors give statistically insignificant results. Using simulated biomass data on the whole sample corroborates our findings, suggesting that our results carry over to inferring stock status through biomass.

Exploring heterogeneous effects, we find that transferable quotas have a stronger favorable effect than non-transferable quotas. We further find that PPRs tend to be more effective for very high levels of ownership protection, a high degree of trade openness, a high species value, and a high species growth rate. For the latter, we find that the treatment effect materializes soon after implementation, suggesting that high-growing species respond faster to the policy. As warmer regions, such as Southeast Asia and Central America have a high share of fast-growing species, our results suggest that a shift from open-access to PPRs might have a larger impact in these areas compared to colder regions with a large share of low-growing species, such as Europe or North America. This is, obviously, only one dimension that will matter for the success of PPRs. As ownership protection and rule of law are institutional characteristics that vary between countries, we also find that PPRs may be less effective in regions with weaker institutions, such as Western Africa and Latin America. Thus, we find that many fisheries have either favorable ecological conditions or institutional conditions to introduce PPRs, but rarely both combined.

The remainder of this paper is organized as follows: we first give an overview of the literature on the relationship between PPRs and ecosystem sustainability (Section 2), followed by a description of the data (Section 3) and the empirical strategy (Section 4). We then present the estimated effect of PPRs on the probability of a stock collapsing (Section 5), supplemented by a subsection where we allow the effect of the policy to vary along different country and species characteristics (Section 5.3). We further discuss where the introduction of PPRs may be most effective (Section 6). Lastly, we provide concluding remarks (Section 7).

## 2 Property rights and ecosystem sustainability

A prominent way of overcoming the problems pervasive in common pool resources has been to assign private property rights (PPRs) (Coase, 1960). In fisheries, this is typically achieved with a system of tradable transferable quotas, which assigns a tradable right to harvest a certain share of the total allowable catch (TAC) to the quota holder (Grafton, 2000; Hannesson, 2004). Especially in the last decades we have seen a policy shift towards rights-based management in many regions of the world (Costello et al., 2010). Among the different types of property rights we find individual quotas (IQ), individual transferable quotas (ITQs), individual vessel quotas (IVQs), territorial user rights (TURFs) and

fisheries cooperatives.

Implementing a quota system requires a country to have the authority to set and enforce limits on the total catch, which was largely facilitated by the 200-mile exclusive economic zone (EEZ).<sup>3</sup> However, it is important to note that rights-based management, like different quota systems, are not primarily tools for conservation and rebuilding stocks, but to achieve economic efficiency. Obviously, the fate of a fish population depends above all on the size of the total allowable catch (TAC) and what is controlled by the TAC. In most real systems that are governed with a TAC and some form of PPRs, there are still uncontrolled dimensions, such as the age or spatial structure of a stock that prevent a first-best outcome (Smith, 2012). In addition, quota systems could for instance create additional market failures, like an increased incentive to discard (Arnason, 2012).

The political economy is another potential channel through which a tradable quota system may improve the state of the stock, as formalized by Costello and Grainger (2018). By providing ownership of the resource to users, tradable quotas may create an incentive to lobby for long-term sustainable use of the resource as this will be reflected in the value of the quota that a quota owner holds. For similar reasons, tradable quotas may provide incentives for good stewardship, for example by avoiding destructive ways of fishing or reporting misconduct of peers (van Putten et al., 2014). Relatedly, assigning individual property rights may also strengthen quota holders' incentives to make sure the total allowable catch (TAC) is enforced. Interestingly, a tradable quota system typically leads to a concentration of quota which is often perceived to be politically undesirable, but may actually increase the chances to achieve cooperation among users (Ostrom, 2008). While it is certainly plausible that individual quotas could induce a sense of ownership, it is also possible that market mechanisms actually destroy stewardship motives as suggested by the crowding out theory (Frey and Jegen, 2001). This seems especially likely if the actual fishers have to lease their quota from "absentee landlords" (Branch, 2009). Hence, the question to what extent private property right influence marine ecosystems is essentially an empirical one.

Analyzing the same type of ecological data as in our study, Costello et al. (2008) find that each additional year an ITQ system is in place, the probability of a stock collapse decreases by about 0.4 %. While the paper makes an important first step towards empirically estimating the effect of tradable quota systems, the identification strategy leaves doubt about whether the effect can be interpreted as causal or not. In a follow up paper (Costello et al., 2010) the authors address some of the concerns in the original study,

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<sup>3</sup>The concept of EEZs was endorsed by the UN Law of the Sea Conference in the 1970s. An EEZ usually stretches 200 nautical miles from the coast, and defines the area where a state has special rights over the exploration and use of marine resources - as well as other types of resources, like oil and gas. Many coastal countries around the world, although not all, established such zones in the latter part of the 1970s (Hamnesson, 2004).

but the problem of omitted variable bias and reverse causality remains largely unresolved.<sup>4</sup> Furthermore, although PPRs may work on average, their effectiveness will likely vary with country and species characteristics. Understanding the degree and nature of such heterogeneous effects will be important for policymakers in designing and implementing suitable remedies.

In the following years, several papers have appeared to shed light on the question of how catch shares affect the fate of a fishery by using a subset of countries considered by [Costello et al. \(2008\)](#) incorporating not only information on catch, but also on biomass. While this approach may seem more informative at first glance, such subsamples are likely to be biased towards rich and developed countries that can afford collecting biological information. Looking at a combination of case studies, [Chu \(2009\)](#) finds that the implementation of ITQs sometimes have positive, sometimes negative and sometimes insignificant effects on biomass. [Essington \(2010\)](#) analyzes how the introduction of ITQs affect biomass, landings, and other variables of interest for the North American Fisheries and found that the implementation of ITQs did not effect mean values of those variables, but tended to decrease the variance, especially concerning landings and exploitation rate.<sup>5</sup> To investigate the link between PPRs and ecosystem sustainability further, data on biomass, catch, and landings coming from the RAM stock assessment database ([Ricard et al., 2012](#)) has been used. [Melnychuk et al. \(2012\)](#) analyze how catch shares help to achieve specific management targets, such as target biomass levels or TAC, and find fairly weak evidence. In particular, there was no strong correlation between whether catch shares had been employed and how biomass developed, though catch shares seemed to dampen excessive, but infrequent, overexploitation and seemed to reduce variability in catch around the year. These findings have been corroborated by [Essington et al. \(2012\)](#), who found no direct link between how the fishery developed after introducing PPRs compared to how it had been performed before.

It remains puzzling that fisheries using catch shares are less likely to collapse, while effects on biomass are insignificant. Using the RAM database, [Costello and Grainger \(2018\)](#) provide a potential answer, as they find that an ITQ system has a positive effect on overexploited stocks, while it has no effect on healthy stocks. Still, the main problem remains that there may be reverse causality from health of the stock to policy implementation. On the one hand, PPRs may be introduced when the fishery is performing well, as there are substantial rents to be distributed to incumbent fishers, creating broad political support for such policy. On the other hand, PPRs may be introduced as an emergency

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<sup>4</sup>The main specification is a random-effects logit estimator, with no controls except a linear time trend. Other concerns are persistence of the dependent variable, as well as the problem of standard errors being correlated over time and within a geographical area.

<sup>5</sup>While both studies have the strength of using more direct biological information, due to data limitations the analysis is a simple before and after analysis, looking at mean and variance of biomass before and after the policy. [Chu \(2009\)](#) looks at the effect for 20 different stocks and [Essington \(2010\)](#) investigates the effect for 15 stocks in North America, while we consider more than 7000 stocks.



measure rescuing a fishery that is locked in a situation of low biomasses and low catches. A crisis may hence create the necessary support for any regulatory changes that attempt to restore or secure the viability of the system (Libecap, 2009; Hersoug, 2005). Relatedly, the value of the resource may also influence the creation of PPRs. Kaffine (2009) shows that this effect is ambiguous; while higher value may increase the economic incentive for harvesters to strengthen (formal or informal) PPRs, it may also lead to a higher pressure on the resource, requiring higher exclusionary efforts.<sup>6</sup> Overall, the question whether institutional changes, such as a transition from open access to a PPRs, are more likely to happen if the resource is in a good shape or in a crisis is so far unsolved (Young, 2010).

### 3 Data and descriptives

#### 3.1 Data

The data used in the empirical analysis has been compiled from several sources. The **ecological data** comes from the *Sea Around Us* (SAU) global catch database (Pauly and Zeller, 2015), which contains information on global catch in tonnes and US Dollar (USD) from 1950-2006.<sup>7</sup> The geographical breakdown we use is catch per species in each exclusive economic zone (EEZ).<sup>8</sup> Each unique combination of species-EEZ-year then constitutes an observation in the dataset. The main variable of interest for the empirical analysis is not catch itself, but we use catch data to infer the exploitation status of the stock.<sup>9</sup> The **exploitation status** of a specific species in a specific EEZ in a specific year is assigned according to the criteria in Table 1, which builds on the methodology in Froese et al. (2012).<sup>10</sup>

The calculation of exploitation status hinges on a critical assumption of a positive correlation between catch and abundance. The assumption that catch to a large degree reflects biomass, however, might not always be met. First, changes in consumer preferences or management regulation could lead to abrupt changes in catch, implying that low catches may be a result of other factors than stock abundance (Caddy et al., 1998;

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<sup>6</sup>As an example, he looks at surfing spots along the Californian coast and uses waves at the surf break as a plausible exogenous proxy for resource quality. Using cross-sectional data, he finds that an increase in surf quality leads to an increase in the strength of PPRs. In this setting, the impact of PPRs on resource quality would have been overstated if the reverse causality was not accounted for.

<sup>7</sup>The SAU catch database contains catch for several aggregated categories (higher taxonomic rank), like genus, family order and class. We only use catch information that is available at the most detailed level, i.e., the species level. Catch data includes only the part of the catch that is both landed and reported.

<sup>8</sup>Note that we use a different geographical area to define a stock than in Costello et al. (2008). Instead of EEZs, they define a fishery as a stock comprised within a unique Large Marine Ecosystem (LME). The different EEZs are illustrated in Figure A.2 in Appendix A.

<sup>9</sup>In two robustness checks, we also infer exploitation status from (i) a small subsample where biomass data is available, and (ii) a larger sample with simulated biomass data (see Appendix C).

<sup>10</sup>See Appendix A for an illustration of the different exploitation statuses.

**Table 1:** Criteria used to assign exploitation status in a given year

Exploitation status	Criterion 1	Criterion 2
1. Undeveloped	$\text{year} < \text{year}_{\text{MaxCatch}}$	catch < 10% of MaxCatch
2. Developing	$\text{year} < \text{year}_{\text{MaxCatch}}$	catch $\in$ [10%,50%] of MaxCatch
3. Fully exploited		catch > 50% of MaxCatch
4. Overfished	$\text{year}_{\text{Collapsed}} > \text{year} > \text{year}_{\text{MaxCatch}}$	catch $\in$ [10%,50%] of MaxCatch
5. Collapsed	$\text{year} > \text{year}_{\text{MaxCatch}}$	catch < 10% of MaxCatch
6. Rebuilding	$\text{year} > \text{year}_{\text{Collapsed}}$	catch $\in$ [10%,50%] of MaxCatch

Note. The criteria are the same as in [Froese et al. \(2012\)](#). Note that in the analysis we also use a measure of exploitation status that is aggregated into three broader categories in the following way: 1=Undeveloped, Developing or Fully exploited, 2=Overfished or Rebuilding and 3=Collapsed. This means that moving from 1 to 2 and from 2 to 3 implies a deterioration of the status of the stock.

[de Mutsert et al., 2008](#)). Second, many stocks also have naturally large fluctuations, and as the length of the time series increases, the chance of catch falling below a certain threshold relative to a historical peak increases ([Murawski et al., 2007](#)).

Despite potential caveats of using catch as a proxy for abundance, we argue that it might be our best option for a global examination. Compiling data to estimate biomass is expensive, and is therefore usually only done by developed nations for species of commercial importance ([Ricard et al., 2012](#)). This implies that the data on biomass represents a biased subsample of the stocks in a given area. Recent studies have also investigated the relationship between catch and biomass for a subsample of fisheries where both measures are available (see e.g., [Froese et al., 2012](#)). The authors find that there, indeed, is a positive correlation between catch and the underlying biomass, and that trends in catch data are consistent with trends in biomass data for fully assessed stocks.<sup>11</sup> As a complementary analysis, we rerun our regressions with (i) biomass data from the RAM Legacy Stock Assessment Database ([Ricard et al., 2012](#)) for a small subsample of our data and (ii) biomass data for our full sample, which was derived from catch data following [Martell and Froese \(2013\)](#).<sup>12</sup>

From the SAU catch database we also collect **the real price** per tonne in USD, which reflects ex-vessel prices based on the landings value. To mitigate potential problems of endogenous local prices, we average the price for each species across all countries, giving us a single world price. The information on **private property rights** in the form of quota systems in fisheries is collected from the EDF catch share database ([EDF, 2013](#)). The database contains information on different types of quota systems from around the world. Note that we define private property rights (PPR) as any type of quota system with tradable or non-tradable allowances.<sup>13</sup> For the sample used in the main

<sup>11</sup>See also [Pauly et al. \(2013\)](#) for a discussion on the relationship between catch and abundance.

<sup>12</sup>The findings are presented in Appendix C.4 and C.5, and point in the same direction as our main findings in Section 5.

<sup>13</sup>The different quota systems include IQs, ITQs, IVQs, TURFs and cooperatives. We recognize that quota systems can differ substantially across countries and across species. As we are not primarily interested in the specific characteristics of different quota systems, but rather the core characteristics of

analysis only around 4% of all fisheries (i.e., unique species-EEZ combinations) have implemented a quota system in the time span analyzed (see Appendix Table A.1). To capture the regenerative capacity of different species, we collect data on species-specific growth parameters from FishBase (Froese and Pauly, 2015).

Next, we include data on **country characteristics**. In contrast to the ecological data and data on management, these observations are (for natural reasons) only available at the country level. The variables include GDP per capita (in constant 2005 USD), population growth, the degree of openness (measured by export plus import, divided by GDP) and the political system (autocracy/democracy). In an attempt to proxy the “environmental awareness” of a country, we collect data on the number of ratified multilateral environmental agreements.<sup>14</sup> The idea is that a large number of ratified environmental agreements reflects a higher awareness for environmental problems. In order to capture potential impacts of a changing climate and year to year changes in the environment, we use sea surface temperature (SST) from the National Oceanic and Atmospheric Administration (NOAA).<sup>15</sup> Lastly, as we are interested in exploring potential interaction effects between policy implementation and strength of property rights, we collect data on the *Rule of Law* indicator from the World Bank’s Worldwide Governance Indicators.

For one of the main specifications used in the analysis, we have in total 149 914 observations, covering 175 exclusive economic zones (EEZ) and 7234 unique species-EEZ combinations over the time span 1961-2006.<sup>16</sup> Summary statistics for two selected years (1975 and 2005) are shown in Table 2, while summary statistics for the whole period, together with a more detailed description of the different data sources, are found in Appendix A.

### 3.2 Who implements private property rights?

In the time period 1950-2005 around 4 % of all fisheries in the sample have implemented a quota system (PPR).<sup>17</sup> Figure 1 shows the roll-out over time of PPRs, with the cumulative development presented in Figure 1a and the yearly distribution in Figure 1b. From the graphs, we see that most PPRs are implemented from 1980 and onwards, with large roll-outs in the early 1980s and around 1990.<sup>18</sup>

Table 2 shows the summary statistics for different variables, grouped by PPR and

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this type of policy instrument, we do not make any attempt to distinguish these systems from each other. We do, however, examine the difference between tradable and non-tradable systems in Table 6.

<sup>14</sup>We use environmental agreements in other areas than fisheries and water pollution.

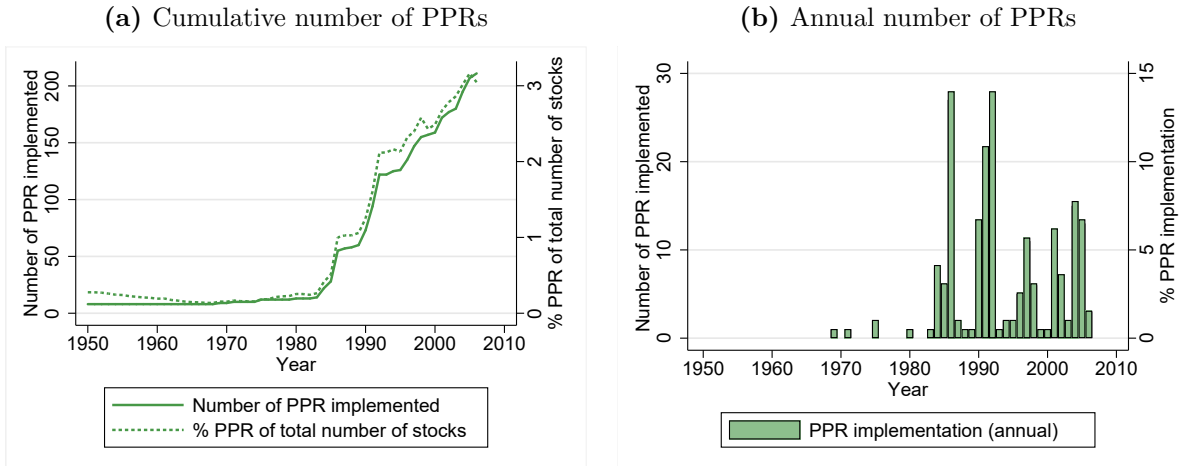
<sup>15</sup>Specifically, we use the Extended Reconstructed Sea Surface Temperature (ERSST) dataset (version v3b), which is a global monthly dataset available on 2°x 2° grids (see Smith et al., 2008). We average the mean temperature over years and EEZs using ArcGIS.

<sup>16</sup>The sample corresponds to the one labeled *Medium* in the Results section.

<sup>17</sup>See Table A.1 in Appendix A for summary statistics on PPRs. Note that we only include PPRs that were implemented before 2006.

<sup>18</sup>The geographical distribution of PPRs is shown in Figure A.2 in Appendix A.

**Figure 1:** The roll-out of PPRs over time. 1950-2005



non-PPR fisheries for the years 1975 and 2005. PPR includes all fisheries that at some point during the time period implement a tradable quota system. As can be seen in Figure 1 only a few fisheries had introduced quota systems before 1975.

Looking at the ecological data, we see that the differences between the two groups are significant for the categories *overfished* and *rebuilding*, where (future) PPR fisheries have a higher share of overfished stocks and a lower share of rebuilding stocks. For the variable we are primarily interested in, *collapsed*, there is no statistically significant difference between the two groups. However, as the (future) PPR-fisheries already have a higher share of overfished stocks, they might be more in danger of experiencing collapses in the future.<sup>19</sup> We can also see that the intrinsic growth rate is lower for PPR-fisheries and the share of highly migratory species is substantially lower. The latter is intuitive as these species move across different jurisdictions, often requiring a bi- or multilateral agreement to be in place to implement a quota system. The fact that the species move over a large geographical area also makes it hard to both monitor and enforce regulations, suggesting that highly migratory species are more prone to overexploitation. Failing to take into account fixed effects at the species level in the estimation strategy could potentially overestimate the effect of PPR, as the share of highly migratory stock are lower for this group.

As for the institutional data, PPR-fisheries tend to be located in countries that are richer, more democratic, have a lower population growth, are slightly less open and have stronger protection of property rights. While it is not straightforward how the different variables affect resource exploitation, the summary statistics highlight the importance of addressing the self-selection problem, as policy implementation does not seem to be

<sup>19</sup>We also calculate the leave-out mean collapse rate within an EEZ. This means that for each fishery we calculate the collapse rate for the EEZ, excluding the specific fishery in question. Each fishery within an EEZ might therefore have a (slightly) different leave-out mean. By including the (leave-out) collapse rate within the EEZ as a covariate in the analysis, we control for common shocks to the EEZ, like e.g., temperature shocks, and for the fact that a high mean collapse rate could impact the fishery-specific collapse rate.

**Table 2:** Summary statistics for 1975 and 2005, by PPR and non-PPR fisheries

	1975			2005		
	PPR	Non-PPR	Diff	PPR	Non-PPR	Diff
Fishery-level data						
Undeveloped (0/1)	0.19	0.21	-0.02	0.00	0.03	-0.02***
Developing (0/1)	0.23	0.29	-0.06	0.01	0.03	-0.03***
Fully exploited (0/1)	0.26	0.24	0.01	0.33	0.32	0.02
Overfished (0/1)	0.23	0.11	0.12***	0.37	0.19	0.18***
Collapsed (0/1)	0.07	0.10	-0.03	0.21	0.31	-0.10***
Rebuilding (0/1)	0.02	0.04	-0.03**	0.07	0.12	-0.05***
Exploitation status (1,3)	1.39	1.36	0.04	1.86	1.94	-0.07
Transition into collapsed (0/1)	0.01	0.02	-0.02*	0.03	0.04	-0.01
PPR (0/1)	1.00	0.00	1.00	1.00	0.00	1.00
PPR in a particular year (0/1)	0.10	0.00	0.10***	0.99	0.00	0.99***
Species-level data						
World price, log	6.96	7.38	-0.43***	7.28	7.64	-0.36***
Growth parameter	0.27	0.38	-0.11***	0.25	0.35	-0.09***
Highly migratory (0/1)	0.05	0.27	-0.21***	0.10	0.26	-0.16***
EEZ-level data						
Sea surface temperature, log (Leave-out) mean collapse within EEZ	1.85	2.89	-1.04***	2.18	2.69	-0.52***
	0.11	0.10	0.01***	0.30	0.31	-0.01**
Country-level data						
GDP per capita, log	9.67	8.43	1.23***	10.19	9.12	1.07***
Population growth (%)	1.43	1.68	-0.24***	1.01	1.34	-0.33***
Openness, %	49.33	53.63	-4.30*	61.85	76.89	-15.04***
Rule of law (-2 to 2)	1.56	0.44	1.12***	1.58	0.53	1.05***
Polity (-10,10)	7.34	2.47	4.87***	9.75	5.95	3.81***
Env.agreements (other), log	2.63	2.50	0.13***	4.04	4.04	-0.00

Note. The table shows the means in 1975 and 2005 for the sample used in the main specification. Exploitation status (1,3) is grouped in the following way: 1=undeveloped, developing or fully exploited, 2=overfished or rebuilding and 3=collapsed. Standard errors are not clustered when testing the difference in means across the two groups.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

random, but rather correlated with several socio-economic dimensions. An important contribution of our study is hence to control for several time-varying country characteristics in the empirical analysis.

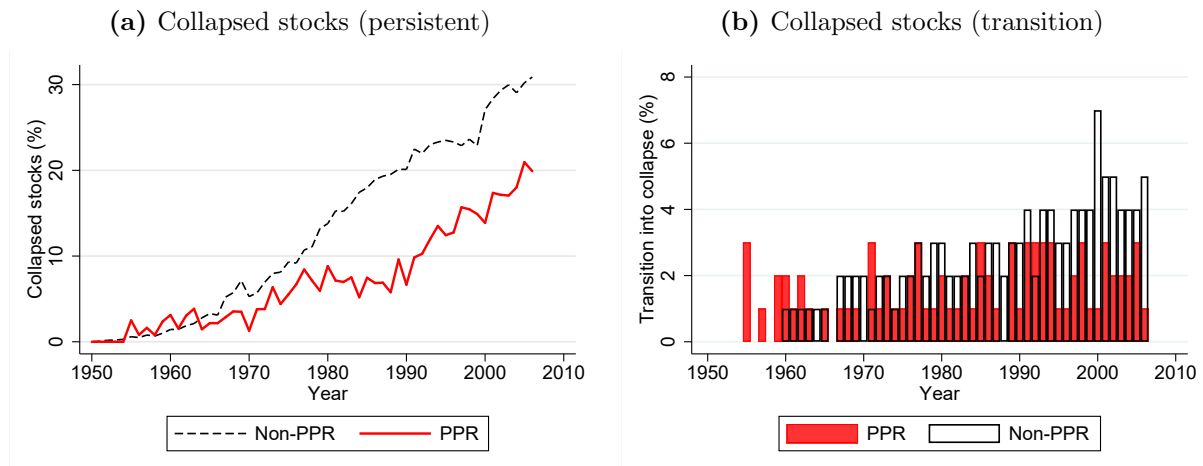
### 3.3 The development of ecological variables for PPR and non-PPR fisheries

Figure 2 depicts the development of ecological outcomes in the years 1950-2006. Figure 2a shows the share of stocks (within the PPR or non-PPR group) that are considered to be collapsed in a given year.<sup>20</sup> From the figure we see that the share of collapsed

<sup>20</sup>Note that a fishery is assigned to the PPR group if a quota system is implemented at some point during the time period 1950-2005. This means that the group composition stays the same throughout the

stocks for the two groups follows a somewhat similar development in the beginning of the period. From the late 1970s, however, the share of collapsed stocks flattens out for the PPR-group, while it continues to increase for the non-PPR group. After about 10-15 years of a relatively constant share of collapsed stocks, it again starts to increase for the PPR-group from around 1990.

**Figure 2:** Collapsed stocks, by PPR and non-PPR fisheries. 1950-2006



Note. PPR includes all fisheries that at some point during the time period have implemented a tradable quota system. Panel (a) shows the share of stocks that are collapsed in a given year. Panel (b) shows the share of stocks that transition from non-collapsed to collapsed in a given year.

The share of collapsed stocks in Figure 2a will typically be a persistent measure as the collapse ratio today will, by construction, depend on the collapse ratio in previous periods. In contrast, Figure 2b presents “new” collapses, i.e., the share of stocks that transition into collapse in a particular year. As seen in the figure, the share of stocks that transition into collapse are fluctuating around 2% for both groups until the late 1980s. In the early 1990s both groups experience an increase in the rate of collapse. After this point, however, the development seems to diverge, where the PPR-fisheries are experiencing a slowdown in new collapses.<sup>21</sup>

Comparing the development in ecological outcomes in Figure 2 with the implementation of PPRs in Figure 1, we see that the divergence in trends happens either around the same time or some years after the roll-out of PPRs. Whether this simply reflects a correlation or in fact a causal relationship is something we investigate in the next sections of the paper.

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period, while the share of fisheries within the PPR-group that have a quota system in place in a specific year will increase over time.

<sup>21</sup>Note that “new” collapses do not necessarily raise the share of collapsed stocks in a given year as this can partly or fully be offset by other stocks moving out of collapse (e.g., to a rebuilding state).

## 4 Empirical Strategy

### 4.1 Difference-in-differences estimation

The main goal of the paper is to estimate a causal effect of introducing private property rights (PPR) on ecological outcomes, and to examine how this effect varies with important institutional and species characteristics. We start by employing a difference-in-differences (DiD) strategy, where the treatment group includes fisheries which at some point during the time span have implemented PPRs, while the control group are all other fisheries.<sup>22</sup> This strategy is similar to that of Costello et al. (2008), except that we define a broader group of quota systems to be in the treatment group, by not only considering individual transferable quotas (ITQs). Also, we use a different geographical breakdown (EEZs). As the quota systems are rolled out at different points in time, the pre and post treatment periods will be different across PPR-fisheries. We control for both fishery-specific time-invariant effects and year specific effects by employing a fixed effects estimation together with year dummies. This means that characteristics that do not vary over time at a fishery level, like geographical conditions and species-specific traits, are controlled for in the estimation. Unlike Costello et al. (2008) we also include several time-variant country and EEZ characteristics to mitigate potential (time-variant) omitted variable bias.

The outcome we focus on is the degree of exploitation of a stock - with a main focus on collapsed stocks. Note that the exploitation status is likely to be highly persistent. In the analysis, we therefore focus on two different measures of collapse: one (persistent) measure that indicates if a fishery has the status of *collapsed* in a given year, and one (transition) measure that indicates a "new" collapse, i.e., whether a fishery has changed status from non-collapsed to collapsed in a given year. The outcome variables correspond to the ones presented in Figure 2.<sup>23</sup>

Using a DiD strategy, the fixed effects (FE) specification takes the following form:

$$Pr(y_{i,j,t} = 1) = \beta_1 PPR_{i,j,t} + X'_{j,t}\gamma + c_{i,j} + \lambda_t + u_{i,j,t}, \quad (1)$$

where subscript  $i$  indicates species,  $j$  indicates EEZ<sup>24</sup> and  $t$  indicates year. Further,  $X'_{j,t}$  includes selected time-variant control variables at the country level,  $c_{i,j}$  indicates fishery-

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<sup>22</sup>Note that introducing PPRs for one species could also have an effect on other species that are not comprised by the quota system. Different species interact in an ecosystem, where some are predators and others are prey, and therefore indirect effects will likely occur. This makes our distinction between the treatment group and the control group less clear. To some extent, we address this by performing a placebo test, where we randomly re-assign treatment to non-PPR stocks within (i) treated EEZs, and (ii) random EEZs; see Appendix C.2.

<sup>23</sup>As we are primarily interested in estimating the effect of PPRs on the probability of *preventing* a stock from collapsing, rather than the probability of a stock being (and having been) collapsed, using "new" collapses seems closer to what we want to measure.

<sup>24</sup>Note that an *EEZ* does not always correspond to a *country*; in some cases a country can have several EEZs. Examples are different islands that constitute separate EEZs, but belong to the same country.

specific fixed effects,  $\lambda_t$  indicates year dummies and  $u_{i,j,t}$  is the time variant idiosyncratic error. The treatment variable,  $PPR_{i,j,t}$  is a dummy variable which takes the value 1 if a quota system has been implemented in a specific year, and 0 otherwise. For the persistent measure of collapse, the variable takes the value of 1 if a stock has the status *collapsed* in a given year, and 0 otherwise. For the transition measure of collapse, the variable takes the value of 1 if a stock moves into collapse in a given year, and 0 otherwise. To account for potential serial-correlation and within country correlation, we cluster standard errors at the country level.

A key assumption underlying the DiD strategy is that in absence of the policy intervention, the treatment group and the control group would have followed a similar development in the outcome variable. While we cannot directly test the common trend assumption, similar pre-treatment trends give an indication that the assumption holds. We can quantitatively verify if the pre-treatment trends are parallel by including leads and lags dummies relative to the time of treatment. In other words: we interact the treatment variable ( $PPR_{i,j,t}$ ) with a dummy variable indicating the time relative to implementation. The lead dummies include the years before implementation, while the lag dummies include the years after implementation. If we denote  $M$  as the number of leads and  $K$  as the number of lags, we can estimate the unfolding of the treatment with the regression:

$$Pr(y_{i,j,t} = 1) = \sum_{m=0}^M \beta_{-m} PPR_{i,j,t-m} + \sum_{k=1}^K \beta_{+k} PPR_{i,j,t+k} + X'_{j,t} \gamma + c_{i,j} + \lambda_t + u_{i,j,t}, \quad (2)$$

where lead  $m$  captures potential deviations in the pre-treatment  $m$  years before treatment and lag  $k$  captures the effect of PPR  $k$  years after the implementation. The estimated coefficients on the lead dummies ( $\beta_{-m}$ ) should show no effect of treatment under the parallel trends assumption, while the coefficients on the lag dummies ( $\beta_{+k}$ ) capture how the treatment effect unfolds over time.

## 4.2 Instrumental variable strategy

While the DiD estimation addresses parts of the omitted variable bias by including both fixed effects and selected time-varying control variables at the country level, it fails to give consistent estimates if the error term is correlated with time-varying omitted variables.<sup>25</sup> It seems likely, however, that there could be some self-selection into the policy driven by omitted time-variant variables. If we have omitted relevant time-varying control variables that follow different trends for the two groups, the exogeneity assumption may be violated. Further, if the status of the ecosystem in previous periods affects the probability of implementing PPRs, then the estimated effect of the policy could be picking up a reverse causality. An instrumental variable (IV) approach will mitigate these problems by ex-

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<sup>25</sup>I.e.,  $E[u_{i,j,t} | c_{i,j}, X'_{i,j,1}, \dots, X'_{i,j,T}] \neq 0$ .



exploiting the correlation between the policy variable (PPR) and an instrumental variable. An IV strategy could also help to mitigate potential problems related to measurement error bias.<sup>26</sup>

Here we propose a novel instrumental variable strategy to deal with the particular endogeneity problem of implementing tradable quota systems in fisheries. The IV strategy we propose is to look at the implementation of tradable quota systems in other environmental domains, like water, forest, land, hunting and pollution. Despite being very different resources, they share some fundamental CPR characteristics, which means that governments face somewhat similar problems in managing the resources. We hypothesize that the implementation of tradable quota systems in different environmental domains reflects a societal preference for market-based policy instruments, and in particular a preference towards assigning private property rights.<sup>27</sup>

In addition to looking at quota systems in other domains within the *same country*, we include a spatial lag indicating if *neighboring countries* have quota systems in other environmental domains, reflecting the idea that preferences for quota systems might be correlated within regions. The spatially lagged variable includes the number of permit systems in other areas than fisheries in the 5 closest countries. To find the 5 closest countries we use a distance measure from the GeoDist database (see [Mayer and Zignago, 2011](#)), which captures the population weighted distance in kilometers between two countries. This second instrument also gives us more data points compared to only looking at quota systems within the same country.<sup>28</sup> The identifying assumptions needed to validate our strategy are then the following:

**Identifying assumption 1:** Having a tradable quota system in other common pool resources (e.g., water, forest, land, pollution) ( $Z'_{j,t}$ ) affects the probability of having a tradable quota system in fisheries ( $PPR_{i,j,t}$ ) (first stage).

**Identifying assumption 2:** Having a quota system in other common pool resources (e.g., water, forest, land, pollution) ( $Z'_{j,t}$ ) does not have a direct effect on the probability of a fish stock collapsing ( $y_{i,j,t}$ ) (exclusion restriction).

**Identifying assumption 3:** The status of the stock ( $y_{i,j,t}$ ) and the tradable quota systems in fisheries ( $PPR_{i,j,t}$ ) do not cause implementation of quota systems in other

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<sup>26</sup>Regression coefficients are biased towards zero when the regressor of interest (here: PPR) is measured with random errors.

<sup>27</sup>This line of reasoning is inspired by [Aichele and Felbermayr \(2012\)](#) who deal with the potential self-selection into environmental agreements by instrumenting ratification of the Kyoto Protocol with membership in the International Criminal Court. The identifying assumption is that both initiatives reflect an underlying preference for multilateral policy initiatives.

<sup>28</sup>A detailed overview of the different data sources used to create the instrumental variable is available in Appendix Table A.5.

common pool resources (e.g., water, forest, land, pollution) ( $Z'_{j,t}$ ) (i.e., the instrument is independent of all potential outcomes).

**Identifying assumption 4:** Having a tradable quota system in other common pool resources (e.g., water, forest, land, pollution) increases the likelihood of having a tradable quota system in fisheries (monotonicity, i.e., those affected by the IV are affected in the same direction).

Note that all identifying assumptions are conditional on the fixed effects ( $c_{i,j}$ ) and time-varying control variables ( $X'_{j,t}, \lambda_t$ ) included in the regressions. Assumption 1 is testable in the form of the first stage in a 2SLS/IV estimation, where the F-statistics indicates the significance of this relationship, see Section 5.2.1. The second assumption is not testable, but relies on plausible reasoning. For instance, having a quota system to manage forest resources should not have a direct effect on the status of a fish stock in the same country. Similarly, if a country has a tradable permit system for forest resources this should not have a direct effect on the status of the fish stock in a neighboring country. This seems not too unlikely. However, one can think of instances where PPRs in other domains could have a direct impact on the probability of a stock collapse. For example, if you implement a private property rights system in forestry, then this could displace harvesters who then have to seek another form of employment. It may be plausible that they could enter a fishery, which could affect collapse. While we cannot rule out the possibility of such an effect, using information on PPRs in forestry in neighboring countries will likely mitigate such a problem. One can also argue that the potential increased pressure from displaced harvesters would not be particular to PPR-fisheries, but could also affect non-PPR fisheries. If this is the case, then this effect would be (partly) differenced out in the DiD set-up. Also, by including the (leave-out) mean collapse rate within the EEZ as a covariate, we control for common trends to the EEZ, such as an increased pressure from displaced harvesters.

Another concern could be that the implementation of different quota systems might reflect a general increase in environmental awareness or improved management capabilities. The IV strategy relies on the assumption that there is no outside factor that both affect the instrument and the fishery. We make several attempts to address potential violations of this assumptions. First, by including GDP per capita as a covariate, we control for the potential that environmental awareness is higher in richer countries. Second, we include ratification of international environmental agreements as a (time-varying) proxy for environmental awareness. Third, if there is a general increase in environmental awareness, then this would presumably affect all stocks within a country. By including the (leave-out) mean collapse rate within the EEZ as a covariate, we control for such common

trends to the EEZ.<sup>29</sup>

The third assumption is also not directly testable. Even though it is not obvious why having a quota system in fisheries should cause implementation of quota systems in other areas, there might be a problem of potential learning effects. If the quota system in fisheries is implemented first, the success or failure of this policy could affect the probability of introducing a quota system in other parts of the economy. As the problem is really an issue of timing, we have lagged both instruments five time periods in order to circumvent potential feedback effects from the endogenous policy variable to the instrument. Also, by using information on quota systems in neighboring countries, we hope to dampen the potential problem of violating assumption 3. Given we believe that assumptions 1-4 hold, we can estimate a causal relationship using the following 2SLS/IV estimation:

$$\text{First stage: } PPR_{i,j,t} = Z'_{j,t-5}\beta_1 + X'_{j,t}\gamma + c_{i,j} + \lambda_t + \epsilon_{i,j,t} \quad (3)$$

$$\text{Second stage: } Pr(y_{i,j,t} = 1) = +\beta_1 \widehat{PPR}_{i,j,t} + X'_{j,t}\gamma + c_{i,j} + \lambda_t + u_{i,j,t}, \quad (4)$$

where the vector  $Z'_{j,t}$  contains the instrument(s) excluded from the second stage. Data on the instruments (PPR in other environmental domains) are collected from a variety of sources, where the main source is the OECD database on tradable permits (OECD, 2013).<sup>30</sup> Summary statistics for the two instruments are reported in Appendix Table A.2. The group means indicate that tradable quota systems in other domains are more common where there are also quota systems in fisheries, supporting our hypothesis of a positive correlation.

Figure 3 depicts the development over time for both the endogenous treatment variable (labeled “PPR-fisheries” in the figure) and the instrumental variable (labeled “IV (Non-PPR)” for the control group and “IV (PPR)” for the treatment group). The plot indicates that for the treatment group the roll-out of PPRs in fisheries to a large degree coincided with the roll-out of PPRs in other environmental domains. Compared to the control group, the treatment group is implementing more tradable quota systems, and at a faster pace. Further, the implementation of PPRs in other areas started somewhat earlier than the implementation of PPRs in fisheries.<sup>31</sup>

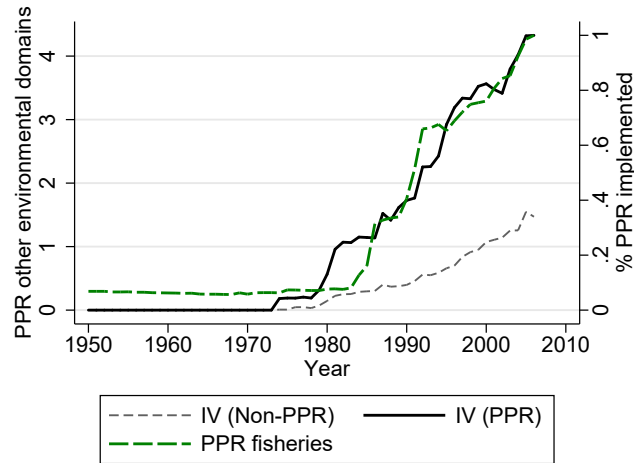
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<sup>29</sup>We also perform a placebo test, where we randomly re-assign treatment to non-PPR stocks within treated EEZs. If countries implementing PPRs were indeed better at managing their resources, we would expect to see a (false) favorable treatment effect in the placebo test - but we don't (see Section 5.4).

<sup>30</sup>The OECD database does not contain information about the year of implementation for the different quota systems. This information has been compiled from multiple sources, see Appendix A.5.

<sup>31</sup>The geographical implementation of PPRs is depicted in Appendix Figure A.2.

**Figure 3:** The roll-out of PPRs over time in fisheries and other environmental domains. 1950-2006



Note. The long-dashed green line shows the implementation of PPRs in fisheries (i.e., the endogenous treatment variable). The solid black line and the short-dashed gray line depict the implementation of PPRs in other environmental domains for the treatment and the control group (i.e., the instrumental variable).

## 5 Empirical results

### 5.1 DiD Results

This section reports on the estimated effects of PPRs on fishery collapse. We start by presenting results from the leads and lags specification of the DiD. Figure 4a plots the DiD coefficients from equation 2 when no time-varying controls are included, and reveals that PPR fisheries were experiencing a downward-sloping trend in the probability of collapse (relative to non-PPR fisheries) prior to policy implementation.<sup>32</sup> In other words: the parallel trend assumption does not seem to hold.

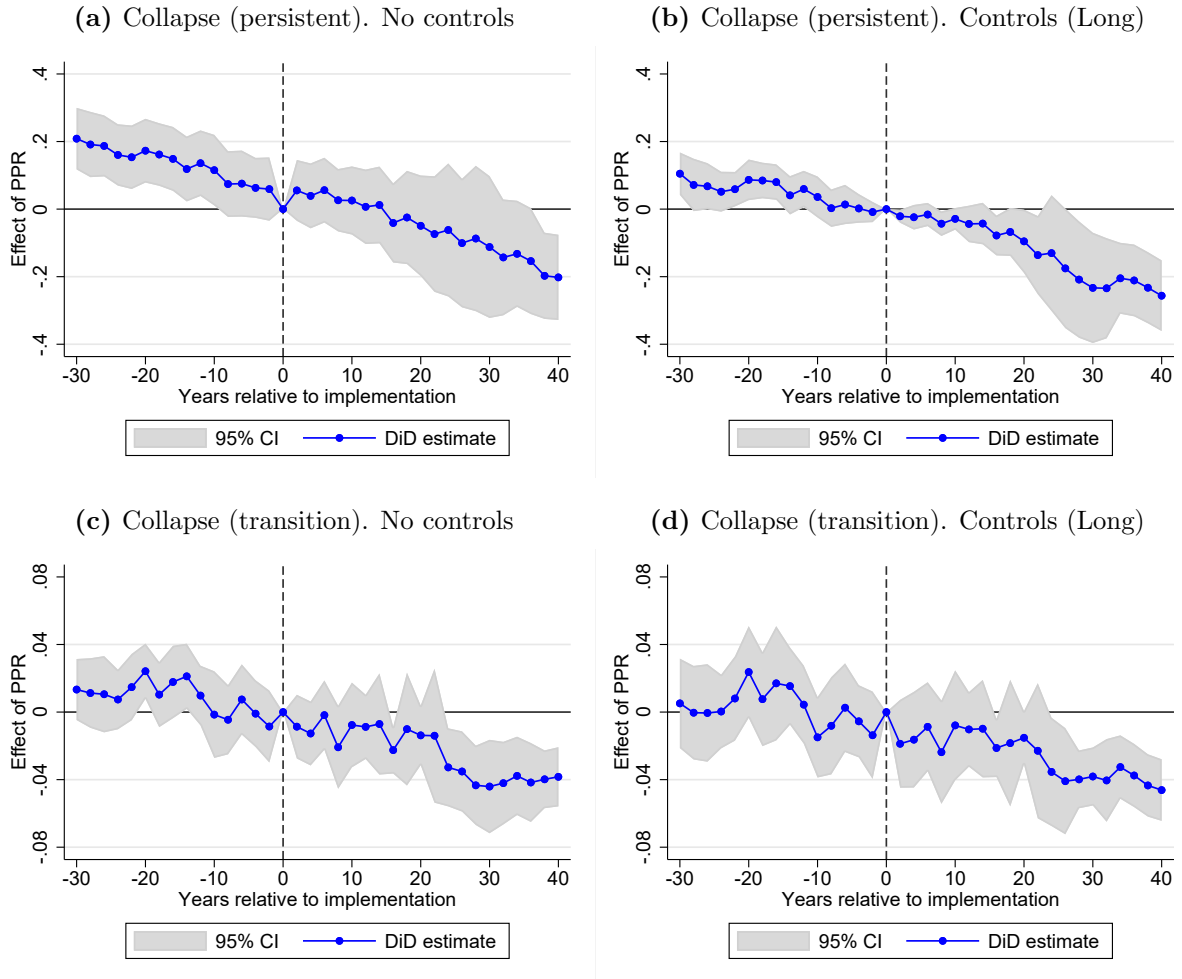
Figure 4b presents estimates when a long list of time-varying covariates are included.<sup>33</sup> By controlling for characteristics like GDP per capita, population growth, and the mean collapse rate within the EEZ, we see that the pre-treatment trend is more similar across the two groups. While the pre-treatment trend is still significantly different in some years, the trend is very similar 10 years prior to implementation. We also see that the confidence intervals are more narrow for all years, meaning that the treatment effect is more precisely estimated.

After policy implementation, we see that PPR-fisheries are less likely to collapse. The treatment effect is relatively small in the 10-15 year period after policy implementation, and only significant for some years. After around 15 years, the effect becomes large and significant. This suggests that it takes time for the policy to work, and for stocks to rebuild. After 15 years, a PPR-fishery is 7% less likely to collapse compared to a non-

<sup>32</sup>This is mainly due to an increase in the collapse rate for non-PPR fisheries, while the collapse rate for PPR-fisheries is stabilizing.

<sup>33</sup>See Appendix Table B.1 for a description of covariates included.

**Figure 4:** The effect of PPRs on probability of collapse



Note. Figures plot the coefficients  $\sum_{m=0}^M \hat{\beta}_{-m}$  and  $\sum_{k=1}^K \hat{\beta}_{+k}$  estimated from equation 2, where  $m$  denotes two year intervals. The outcome variable in panels (a) and (b) is a binary variable equal to 1 if a stock has the status *collapsed*, and 0 otherwise. The outcome variable in panels (c) and (d) is a binary variable equal to 1 in the year of a collapse, and zero otherwise. See Appendix Table B.1 for a description of control sets (*Short*, *Medium*, *Long*). Standard errors are clustered at the country level.

PPR fishery, conditional on covariates. After 20 years, the probability is 11%, and after 26 years the probability is 21%.<sup>34</sup> Panel A in Table 3 reports the average treatment effect for the post treatment period. Going from no time-varying controls (column 1) to the full set of controls (column 4) lowers the average treatment estimate from 10.1% to 7.03%. Taken together, the findings suggest that failing to control for time-varying controls will lead to a violation of the parallel trends assumption, and an overestimation of the average treatment effect.

### 5.1.1 Comparing the magnitude of effects to previous findings

In order to compare the magnitude of effects to previous findings in the literature, we also estimate annual treatment effects using a linear specification (see Appendix Table B.2).

<sup>34</sup>See Appendix Table B.3 for coefficients.

**Table 3:** The effect of PPRs on probability of collapse

	(1)	(2)	(3)	(4)
Panel A. Dep.variable: collapse (persistent)				
PPR	-0.101*** (0.0291)	-0.0868*** (0.0225)	-0.0774*** (0.0211)	-0.0703*** (0.0242)
Panel B. Dep.variable: collapse (transition)				
PPR	-0.0184*** (0.00293)	-0.0142*** (0.00282)	-0.0148*** (0.00289)	-0.0141*** (0.00379)
Controls (time-varying)	No	Short	Medium	Long
Obs	303160	192725	149914	115648
Period	1950-2006	1961-2006	1961-2006	1961-2006
Countries	169	153	153	95
EEZs	193	175	175	112
EEZ-species	11498	9488	7234	5147
Mean collapse (persistent)	0.141	0.180	0.173	0.173
Mean collapse (transition)	0.0252	0.0304	0.0326	0.0326
Mean years of PPR	12.07	12.33	12.54	12.60

Note. Table reports the coefficient  $\hat{\beta}_1$  estimated from equation 1. All regressions include year dummies and EEZ-species fixed effects. See Appendix Table B.1 for a description of control sets (*Short, Medium, Long*). Standard errors clustered at the country level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Using "Years of PPR" as the treatment variable, which assumes a linear treatment effect over time, we find that PPRs reduce the probability of collapse by 0.59% per year.<sup>35</sup> By comparison, Costello et al. (2008) find estimates in the range 0.37-0.54% (see Appendix Figure B.1b). Our estimated treatment effect is hence slightly larger than previously documented.<sup>36</sup> Comparing the unfolding of the treatment effect in Figure 4b to the linear specification, we also see that the treatment effect materializes more slowly in the first 10-15 years than predicted by a linear relationship (see Appendix Figure B.1a). Allowing for non-linearities by estimating DiD with leads and lags hence reveals that it takes longer for the treatment effect to materialize than predicted by a linear model.

<sup>35</sup>The results are for the specification with the full set of controls. A similar sized effect can also be found by taking the average treatment effect in column 4 in Table 3 (-0.0703), and dividing it by the mean years of PPR (12.6), which is reported in the last row in the same table. The linear specification with no controls shows a treatment effect of 0.82% per year, indicating that failing to control for time-varying covariates will overstate the treatment effect.

<sup>36</sup>Note that our study uses a different geographical breakdown (EEZ instead of LME), a longer time period, and a broader category of PPRs, implying that the results are not directly comparable. Note that Costello et al. (2008) also estimates the effect of ITQs using a logit fixed effects estimator, and find that the coefficient is larger than the random effects model. However, it is difficult to interpret the coefficient from such models as the magnitude is reported in odds-ratio effects.

### 5.1.2 Transition into collapse

The results so far show that PPRs lower the probability of collapse. However, Figure 4b shows that there might still be a problem with different pre-treatment trends. Using a less persistent outcome variable may help to mitigate this problem. We therefore estimate the leads and lags specification of the DiD using the transition measure of collapse as dependent variable. In other words, we estimate the probability of a “new” collapse in a given year. Figure 4c shows the results when no time-varying controls are included, and reveal a very similar pre-treatment development for the treatment and control group. Again, we see that the treatment effect is relatively small up until around 15-20 years after the policy implementation. Adding time-varying controls has minor effects on the estimated treatment effect, and seems to widen the confidence intervals somewhat, see Figure 4d. The latter means that the treatment effects are less precisely estimated, which may be caused by the smaller sample. The average treatment effects are shown in Panel B in Table 3. On average, having PPRs reduces the probability of a fishery collapsing in a given year by 1.4-1.5%. This effect is large and relevant given the average number of fisheries that transition into collapse each year is 3.26%.

## 5.2 IV-DID Results

In Section 5.1, we showed that PPRs on average lower the probability of a fishery collapsing. However, for some specifications, we reject the hypothesis of a similar pre-treatment development in the outcome variable. Further, while we are including time-varying covariates, there might be problems of omitted variables affecting the outcome in the post-treatment period. To further investigate the causal effect of PPRs on the probability of a fishery collapsing, we report results using the instrumental variable specification described in Section 4.2.

### 5.2.1 First stage

Table 4 reports the first stage of the IV/2SLS estimation. The first stage estimates the effect of having tradable permit systems in other environmental domains on the probability of having a tradable quota system in fisheries (PPR). Column (1) shows that the coefficient is positive and significant at a 1 % level. The F-statistics is just below the rule of thumb of 10 (F-stat=9.48). Including the spatially lagged IV in column (2) gives both a larger coefficient and a higher F-statistics (F-stat=22.79). The coefficient of 0.0133 tells us that if a neighboring country has one tradable quota system in an environmental domain other than fisheries, it increases the probability of a specific fishery having PPRs in a specific year by 1.33 %, conditional on fishery-specific and year-specific effects. If 5 neighboring countries have 2 tradable quota systems each in an environmental domain other than fisheries (i.e., 10 in total), the probability of a fishery having PPRs increases by 13.3 %.

**Table 4:** First stage. The effect of PPRs in other environmental domains on PPRs in fisheries

	(1)	(2)	(3)	(4)	(5)	(6)
PPR other areas	0.00673*** (0.00219)	0.00377** (0.00150)				
PPR other areas, spatial lag		0.0133*** (0.00256)	0.0146*** (0.00207)	0.0137*** (0.00192)	0.0144*** (0.00201)	0.0150*** (0.00256)
Controls (time-varying)	No	No	No	Short	Medium	Long
Obs	233922	233922	233922	157471	149914	115648
Countries	169	169	169	153	153	95
EEZs	193	193	193	175	175	112
EEZ-species	9376	9376	9376	7621	7234	5147
F-stat excl. instr.	9.481	22.79	49.66	51.26	51.03	34.57

Note. Table reports the coefficient  $\hat{\beta}_1$  estimated from equation 3. Dependent variable, PPR, is a binary variable at the fishery level indicating if a tradable quota system has been implemented in a specific year. Both instruments are lagged five time periods. All regressions include species-EEZ fixed effects, as well as year dummies. See Appendix Table B.1 for an description of control sets (*Short*, *Medium*, *Long*). Standard errors clustered at the country level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Performing a redundancy test shows that the estimation is improved if the first IV (*PPR other areas*) is dropped from the estimation. This leads us to estimate the first stage with the spatially lagged IV only, see column (3). The F-statistics of 49.66 suggests that the spatially lagged IV performs better than the IV reflecting other quota systems within the same country.<sup>37</sup> Adding country-level controls (columns 4-6) has minor effects on the estimated coefficient. Adding more controls reduces the number of observations as some controls are only available for a subset of the sample. Based on the findings in this section, we use the spatially lagged IV to estimate the second stage.<sup>38</sup>

### 5.2.2 Second stage

Table 5 reports the results from the IV-DiD strategy. Panel A shows the effects of PPRs when using the persistent measure of collapse. With the exception of column 1, the IV-DiD coefficients are slightly larger than the DiD coefficients in Table 3. They are also less precisely estimated: the IV strategy gives significant effects for column 2 and 3 only. These are also the specifications with the largest F-statistics (50.99 and 50.83), meaning that the first stage is stronger.<sup>39</sup> Using the short list of controls (column 2), PPR-fisheries

<sup>37</sup>This might be due to the fact that we use more information when constructing the variable, and more countries get non-zero values. A potential advantage with the spatially lagged instrument is that it might be less likely to violate identification assumptions 2-3.

<sup>38</sup>The main results in the paper also carry over to the use of two instruments.

<sup>39</sup>The F-statistic is indicated by *Weak-ID test* in Table 5, and is the F-statistic on the excluded instrument. The F-statistic is the robust Kleibergen-Paap Wald rk F statistic. Using the critical values from [Stock and Yogo \(2005\)](#) to compute the maximum IV bias, we find that the IV bias is maximum 10 %. Using a test statistic robust to weak instruments (Anderson-Rubin), the estimated effect of PPRs ( $\hat{\beta}_1$ ) is still significant at a 5% level in column 2 and 3.



are on average 14.8% less likely to collapse after policy implementation. Compared to the DiD estimate of 8.68%, this effect is about 1.71 times larger. Using the medium list of controls, the probability is 12.9% - about 1.67 times larger than the DiD estimate.

**Table 5:** Second stage. The effect of PPRs on probability of collapse

	(1)	(2)	(3)	(4)
Panel A: Dep.variable: collapse (persistent)				
PPR	-0.0810 (0.149)	-0.148** (0.0697)	-0.129** (0.0616)	0.0760 (0.0694)
Panel B: Dep.variable: collapse (transition)				
PPR	-0.0787*** (0.0301)	-0.0848** (0.0386)	-0.103** (0.0434)	-0.0998* (0.0545)
Controls (time-varying)	No	Short	Medium	Long
Obs	233922	157471	149914	115648
Period	1955-2006	1961-2006	1961-2006	1961-2006
Countries	169	153	153	95
EEZs	193	175	175	112
EEZ-species	9376	7621	7234	5147
Mean collapse (persistent)	0.152	0.184	0.173	0.173
Mean collapse (transition)	0.0284	0.0325	0.0326	0.0326
Weak-ID test	48.55	50.99	50.83	33.57
Anderson-Rubin p-value (persistent)	0.585	0.0237	0.0265	0.254
Anderson-Rubin p-value (transition)	0.00827	0.0132	0.00653	0.0604
Anderson-Rubin CI (persistent)	[-.381, .208]	[-.299,-.024]	[-.263,-.015]	[-.052, .232]
Anderson-Rubin CI (transition)	[-.142,-.023]	[-.175,-.019]	[-.204,-.029]	[-.218, .001]

Note. Table reports the coefficient  $\hat{\beta}_1$  estimated from equation 4. All regressions include species-EEZ fixed effects, as well as year dummies. See Appendix Table B.1 for a description of control sets (*Short*, *Medium*, *Long*). Standard errors clustered at the country level in parentheses. *Weak-ID test* indicates the robust Kleibergen-Paap Wald rk F statistic. The Anderson-Rubin test is a weak-instrument robust test of the significance of  $\hat{\beta}_1$ . Anderson-Rubin CI shows the corresponding 95% confidence intervals.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Panel B in Table 5 reports the IV estimates when using the transition measure of collapse as the outcome. All specifications show a negative and statistically significant effect of PPRs on the probability of collapse.<sup>40</sup> Compared to the DiD estimates in Table 3, the effects are stronger (i.e., more negative coefficients). When no controls are included, the estimated treatment effect is over 4 times larger than the DiD estimates. Adding more controls increases the IV-DID estimate slightly, while leading to less precisely estimated coefficients.

The stronger effects could have several potential explanations. First, one possible explanation is that PPR-fisheries have experienced less favorable *trends* in relevant omitted variables, like a more rapid technological development relative to non-PPR fisheries, which might have put a larger pressure on these fish stocks. If this trend is not captured by the time-varying controls, the DiD estimate might be biased downwards. Second, if

<sup>40</sup>This is also the case when using a weak-instrument robust test (Anderson-Rubin).

the exclusion restriction does not hold (i.e., the instrumental variable has a direct effect on the outcome variable), the IV estimate could be inflated. Third, measurement error in the treatment variable could bias the DiD coefficient downwards. An IV-strategy would mitigate such attenuation bias. Fourth, the IV estimate reflects a *local* average treatment effect; it captures the effect on a subpopulation that is induced by the instrument to change the value of the endogenous regressor *PPR*. The estimate is hence not an average for all treated units, but for an instrument specific subpopulation (the "compliers").<sup>41</sup>

While we cannot pinpoint the reason for the larger IV estimates, we can still conclude that both the DiD and the IV-DID estimate show a favorable effect of PPRs on the probability of a fishery collapsing. Our results also suggest that the treatment effects are at least as large, or larger, than found in the previous literature.<sup>42</sup>

### 5.3 Heterogeneous effects

Having found that PPRs lead to more favorable outcomes on average, we now turn to potential heterogeneous effects. Specifically, we investigate whether the favorable effects of PPRs vary with (i) the transferability of quotas, (ii) the overall level of PPR protection in a country, (iii) trade openness, (iv) species growth rate, and (v) species value. These are characteristics highlighted by Chichilnisky (1994), Brander and Taylor (1997), Bohn and Deacon (2000), Copeland and Taylor (2009), and Kaffine (2009).

When analyzing transferability, we split the sample in two based on whether the quotas are registered as transferable or non-transferable by the EDF catch share database (EDF, 2013). For country and species characteristics, we split the sample in two based on high or low values. To investigate the pre-treatment trend, and to see how the treatment effect unfolds over time, we use the DiD with leads and lags when estimating heterogeneous effects.<sup>43</sup>

#### 5.3.1 Transferable vs. non-transferable

Are transferable quotas more effective in preventing a collapse than non-transferable ones? While allowing quota holders to trade will in theory lead to cost minimization, the effect on ecological outcomes are less clear.<sup>44</sup> Here we run separate regressions for the two

<sup>41</sup>See Section 5.3 for a further discussion on this, as well as how the IV estimate can be interpreted.

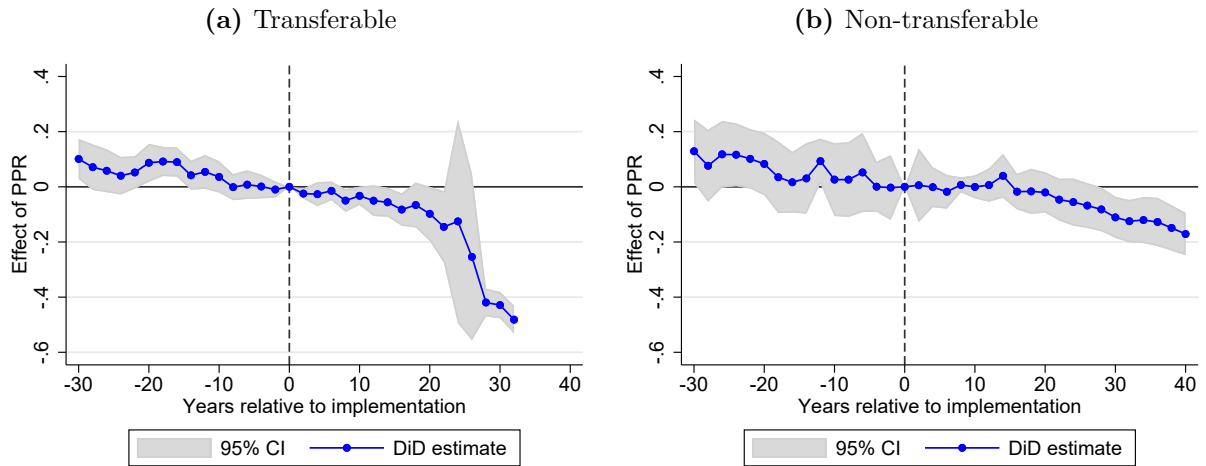
<sup>42</sup>To examine a broader range of outcomes, we also use each of the different exploitation statuses as outcomes. These results are presented in Appendix C.3. In short, we find that PPRs have a significant and favorable effect on two categorical variables of exploitation status (*Exploitation (1-3)* and *Exploitation (1-6)*). Further, we find that PPRs increase the probability of a fishery being *undeveloped*, *developing* and *overexploited*, but none of these effects are statistically significant. The effects of PPRs on *recovering* and *fully exploited* stocks is close to zero and insignificant.

<sup>43</sup>In Appendix B.4, we allow for treatment effects to vary in a linear and non-linear way with the different country and species characteristics. Results are in line with the findings presented in the main text.

<sup>44</sup>As discussed earlier, transferability will typically lead to a concentration of quotas, which may increase the chances to achieve cooperation among users (Ostrom, 2008). At the same time, introducing market

groups of quotas to capture potential heterogeneous performance. Distinguishing between transferable and non-transferable quota systems, we find that transferable quotas show a somewhat stronger treatment effect, see Figure 5 and Table 6. Using the persistent measure of collapse, the effect of PPRs is estimated to be between -7.4% and -9.4% - depending on the specification. By contrast, the estimates for non-transferable quotas are between -4.3% and -6.2% and only significant in two of the specifications.<sup>45</sup>

**Figure 5:** The effect of PPRs on probability of collapse. Transferable vs. non-transferable



Note. Figures plot the coefficients  $\sum_{m=0}^M \hat{\beta}_{-m}$  and  $\sum_{k=1}^K \hat{\beta}_{+k}$  estimated from equation 2, where  $m$  denotes two year intervals. Outcome variable is a binary variable equal to 1 if a stock has the status *collapsed*, and 0 otherwise. Controls: Long. See Appendix Table B.1 for a description of control sets (*Short, Medium, Long*).

While these findings suggest that going from non-transferable to transferable quotas might improve ecological outcomes, it is important to point out that transferability is not necessarily randomly assigned, but might correlate with country characteristics. While our estimation strategy and covariates help to control for some factors, we must be careful in interpreting the difference as causal evidence that tradable quotas are more effective. To get closer to a causal effect, we restrict the treatment group to the only country that has both tradable and non-tradable quotas in place: Chile.<sup>46</sup> This allows us to compare two types of quotas within the same country. A drawback is that we are left with very few treated stocks, which weakens the generalizability of results. Restricting the treatment group to Chilean stocks only, we estimate the effect of tradable quotas to be -11%, while the effect of non-tradable quotas is -6.6% (see Appendix Table B.4).

mechanisms could also crowd out intrinsic motivation for stewardship (Frey and Jegen, 2001).

<sup>45</sup>Using the transition measure of collapse, we only find significant effects for the transferable quotas. While the estimated effects are systematically more favorable for tradable quotas compared to non-tradable quotas, we cannot reject the null hypothesis of similar average treatment effects. The only significant difference is the one between column (1) and (4) in Panel A in Table 6, although only at a 10% level.

<sup>46</sup>Chile has 12 fisheries managed by transferable quotas and 13 fisheries managed by non-transferable quotas, according to the EDF catch share database (EDF, 2013).

**Table 6:** The effect of PPRs on probability of collapse. Transferable vs. non-transferable

	Transferable			Non-transferable		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dep.variable: collapse (persistent)						
PPR	-0.0941*** (0.0257)	-0.0803*** (0.0237)	-0.0741*** (0.0265)	-0.0428** (0.0185)	-0.0619*** (0.0220)	-0.0449 (0.0290)
Panel B. Dep.variable: collapse (transition)						
PPR	-0.0149*** (0.00311)	-0.0168*** (0.00351)	-0.0157*** (0.00447)	-0.0101 (0.0153)	-0.00175 (0.0135)	-0.00299 (0.0171)
Controls (time-varying)	Short	Medium	Long	Short	Medium	Long
Obs	191378	148663	114479	186735	144402	110173
Period	1961-2006	1961-2006	1961-2006	1961-2006	1961-2006	1961-2006
Countries	153	153	95	153	153	95
EEZs	175	175	112	175	175	112
EEZ-species	9450	7198	5113	9308	7056	4970

Note. Table reports the coefficient  $\hat{\beta}_1$  estimated from equation 1. All regressions include year dummies and EEZ-species fixed effects. See Appendix Table B.1 for a description of control sets (*Short*, *Medium*, *Long*). Standard errors clustered at the country level in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

In addition to these findings, one can also argue that the IV strategy employed in Section 5.2.2 captures the effect of tradable quota systems on collapse. The reasoning would be that the "compliers", i.e., those that are "moved" by the instrument, will be countries with a general preference for market-based instruments such as tradable quotas. The smaller (i.e., more favorable) IV-DiD estimate could therefore be interpreted as tradable quotas performing better than non-tradable ones.

Based on the three findings described above, we cautiously suggest that transferability of quotas improve the favorable effect of PPRs on ecological outcomes.

### 5.3.2 PPR strength

The next characteristic we investigate is the strength of PPR protection. If fishermen perceive PPRs as uncertain, they might become more impatient and pay less attention to the long-term sustainability of the fishery. This suggests that the effect of tradable quotas will increase with ownership security. On the contrary, weak PPRs could also deter investment in the sector, dampening the pressure on the stock (Bohn and Deacon, 2000).

To proxy the strength of PPRs, we use the *Rule of Law* indicator from the World Bank's Worldwide Governance Indicators.<sup>47</sup> The first row in Figure 6 shows a histogram

<sup>47</sup>As the data is only available for the last part of the time period analyzed, we use the country average. An alternative approach could be to drop all years lacking observations, or to extrapolate the data back in time. As the indicator varies little over time, we argue that not much is lost by averaging the data;

of the rule of law, as well as results for two subsamples with different levels of ownership protection. From 6a, we see that the rule of law is generally very high for PPR fisheries. This means that we are only able to evaluate how the effects of PPRs vary with high to very high levels of ownership protection. In Figure 6b we have restricted the sample to observations with ownership protection below the 90th percentile, while Figure 6c only uses observations with ownership protection in the top 90th percentile. The DiD estimation suggests that PPRs are more effective for very high levels of PPR protection. Hence, our results do not support the hypothesis put forward by Bohn and Deacon (2000) that ownership security may increase pressure on the stock.

### 5.3.3 Trade openness

Chichilnisky (1994) argues that trade openness decreases overexploitation in countries with strong PPRs by shifting the overuse to regions with weaker regulation. Brander and Taylor (1997), however, demonstrate that openness can also work in the opposite direction: if resource-scarce countries with ill-defined PPRs overexploit their natural resources, opening up for trade will dampen overexploitation. Put differently: countries with well defined PPRs might increase their resource exploitation (vis-a-vis countries with poorly defined PPRs) as a response to openness. Erhardt (2018) tests this question empirically and finds that trade openness reduces pressure on fish stocks in countries with weak governance, in line with what Brander and Taylor (1997) suggested.

To capture the degree of *trade openness*, we use export and imports as a share of GDP. From Figure 6d, we see that trade openness ranges from about 0% to 300%. In Figure 6e we restrict the sample to observations with openness below the mean, while Figure 6f shows the results for values of openness above the mean. The estimation shows that PPRs are more effective in countries with a high degree of trade openness. This suggests that open economies have more to gain from implementing PPRs compared to less open economies.<sup>48</sup> The result lends support to Chichilnisky (1994) who hypothesized that trade may dampen exploitation in countries with strong PPRs, and also consistent with the idea that open economies may suffer particularly from lack of clear property rights (Taylor, 2011).

### 5.3.4 Growth rate of species

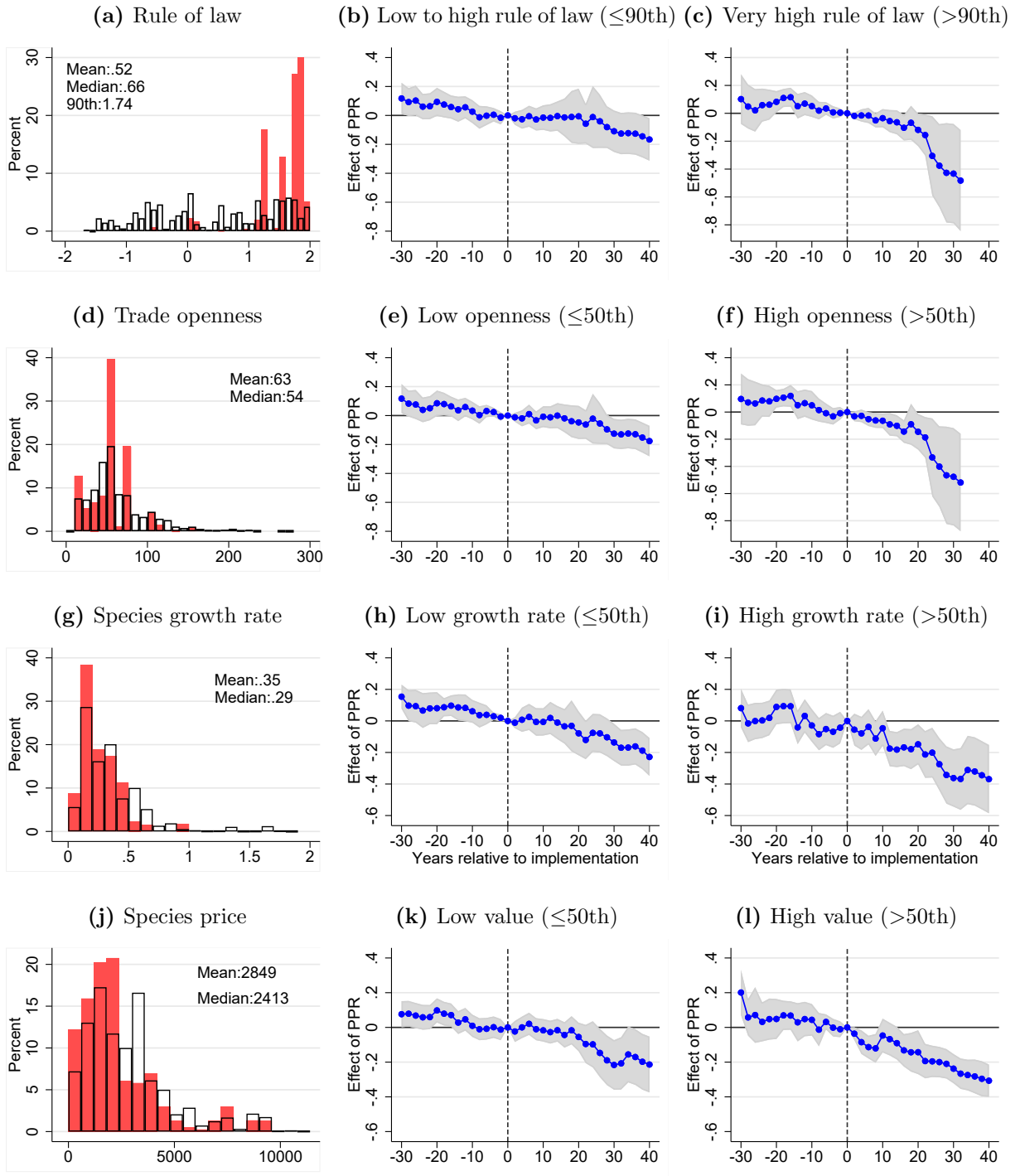
A high regenerative capacity of the resource could potentially strengthen the effectiveness of PPRs (Copeland and Taylor, 2009). In an attempt to capture the *intrinsic growth rate* of different species, we use a growth parameter from Froese and Pauly (2015). From Figure 6g, we see that there is substantial variation in the growth rate across species, and

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most of the variation comes from variation across countries.

<sup>48</sup>Put differently, the finding also suggests that lack of property rights is more detrimental to countries with a high degree of trade exposure compared to less exposed countries.

**Figure 6:** The effect of PPRs on probability of collapse, by country and species characteristics



Note. Figures (a), (d), (g), and (j) show histograms of rule of law, trade openness, species growth rate, and species price by treatment and control group. Hollow bars indicate non-PPR fisheries, while red bars indicate PPR fisheries. The other figures plot the coefficients  $\sum_{m=0}^M \hat{\beta}_{-m}$  and  $\sum_{k=1}^K \hat{\beta}_{+k}$  estimated from equation 2, where  $m$  denotes two year intervals. Outcome variable is a binary variable equal to 1 if a stock has the status *collapsed*, and 0 otherwise. Controls: Long. See Appendix Table B.1 for a description of control sets (*Short, Medium, Long*).

there is also a large degree of overlap between the treatment and control group. Figure 6h and Figure 6i plot the DiD estimate for observations with growth rates below or above the mean. The results suggest that PPRs are more effective when applied to species with

a high intrinsic growth rate. We also see that the treatment effect is materializing earlier on for high growth species compared to low growth species, indicating that the former group "recovers" more quickly. These findings are in line with the theoretical predictions from Copeland and Taylor (2009).

### 5.3.5 Species value

Are PPRs more effective for high value species? On the one hand, higher quality (i.e., price) might require more monitoring and enforcement efforts due to a larger pressure on the resource (Kaffine, 2009), which might in turn lower the effectiveness of PPRs. On the other hand, PPRs might also be more effective for high value species due to harvesters' stronger economic incentive to protect their property rights. Figure 6j shows a histogram of species prices<sup>49</sup> for the treatment and control group. Figures 6k-6l plot the DiD estimate for two subsamples: observations where the species value is either above or below the mean. Results suggest that PPRs are more effective in preventing collapse for stocks with a high value. Or put differently, the absence of PPRs is more detrimental for high-value stocks. As with the results for species growth rate, the effect of PPRs materialize earlier for high-value stocks than low-value stocks, and the magnitude is also larger.

## 5.4 Robustness checks

In this section, we run several robustness checks to test the sensitivity of our main result. A summary of the findings are provided below, while figures, tables and more detailed descriptions are provided in Appendix C.

### 5.4.1 Alternative samples

**Only treated species.** Species managed by quota systems might be systematically different from those that are not.<sup>50</sup> To see to what extent this is influencing results, we restrict the sample to species that are covered by a quota system in at least one country. This reduces the number of EEZ-species combination to about 1/4 of that in the main sample. Figure C.1 and Table C.2 in the Appendix show that results are very similar to the ones reported in Section 5.1.

**Only treated EEZs.** Countries implementing PPRs might be better equipped to manage their stocks. Specifically, if we believe that treated EEZs were experiencing a stronger improvement in management capabilities compared to non-treated EEZ, this might upward bias the effect of PPRs. In the main analysis we have tried to control for different

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<sup>49</sup>Prices are global prices averaged over the time period analyzed.

<sup>50</sup>As an example, migratory species are less likely to be managed by a quota system.

trends in "good management" at the country level by including several time-varying country characteristic, such as GDP per capita, population growth, political orientation, trade openness and the number of ratified international environmental protocols. Here we restrict the sample to treated EEZs only, which means that we compare treated to non-treated fisheries within the same EEZ. Treated and untreated stocks are hence subject to the same country-specific management capabilities. Limiting the sample to the 12 treated EEZs has very little effect on the main result, see Section C.1 in the Appendix.

#### 5.4.2 Placebo treatment

Another way to try to test for "good management" as an omitted variable, is to conduct placebo tests. Specifically, if we think the *trend* in management capabilities were more favorable for treated EEZs, we might expect non-PPR fisheries within these EEZs to perform better as well. To test for this, we re-assign treatment status to a random fishery, which was actually not treated, but within the same EEZ.<sup>51</sup> Next, we drop all fisheries within the EEZ that were actually treated. Lastly, we redo the DiD analysis from Section 5.1. Looping over this procedure 20 times gives us a distribution of placebo treatments. If we think there were certain characteristics of the EEZs or countries that made the PPR-fisheries perform better, we would expect to find a favorable treatment effect in the placebo tests.<sup>52</sup> However, results from the placebo runs show either insignificant effects or a slightly increased probability of collapse (see Figure C.2a). Based on these results, countries implementing PPRs do not seem to be better at managing their stocks in general.<sup>53</sup> If we reassign treatment to a fishery within *any* EEZ, then none of the 20 placebo runs show a negative and significant effect - with the exception of one placebo test, which shows a positive (i.e., unfavorable) and significant effect (see Figure C.2b).

#### 5.4.3 Collapse measure

Lastly, we address the assumption of a positive correlation between catch and biomass in two ways. First, we exploit a different dataset containing information on both catch and biomass - the RAM Legacy Stock Assessment Database (RAM) (Ricard et al., 2012), see Appendix C.4. As the RAM dataset only includes around 2% of the observations from our main dataset, and that this is likely a non-random sample, we cannot directly test our

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<sup>51</sup>See Appendix Table A.3 for an overview of the number of treated and untreated stocks within each EEZ. For New Zealand there are 51 treated and 35 untreated stocks. This means that in the placebo runs we assign treatment to all 35 non-treated stocks.

<sup>52</sup>Note, however, that this strategy will only control for potentially different trends in management capabilities at the EEZ level and not at the fishery (EEZ-species) level.

<sup>53</sup>In fact, the findings suggest that non-PPR fisheries located in EEZs where stocks are being managed by quota systems might actually experience an increased pressure. This could be due to fishing efforts shifting from PPR-stocks to non-PPR stocks. Note, however, that the main results from Section 5.1 are robust to excluding non-treated stocks within treated EEZs, which suggests that such spillover effects are likely small.



main finding of a negative effect of PPRs on the probability of a stock collapsing. Instead, we investigate how sensitive the effect of PPRs is to using catch instead of biomass when constructing the exploitation status. In order to do this we create two different dependent variables using data from the RAM database: one where we assign the exploitation status “collapsed” based on biomass and one where we assign the status based on catch. For the small sample, biomass and catch track each other relatively closely (see Appendix Figure C.4). Using the two measures to estimate the effects of PPRs, we find a favorable effect using both collapse measures. The effect, however, is not significantly different from zero, which may be due to the small sample (see Appendix Figure C.5).

Second, we use catch data to infer parameters of carrying capacity  $K$  and intrinsic growth rate  $r$  of a biological model that can be used to simulate biomass trajectories of fisheries for which assessed biomass data is not available following [Martell and Froese \(2013\)](#); see Appendix C.5 for details.<sup>54</sup> We then use the generated biomass data to assign exploitation status based on biomass, as well as catches. Analyzing the leads and lags specification of the DiD reveals that the results from constructed biomass are very similar to the results obtained using catch data with the full sample (compare Appendix Figure C.6 with Figure 4). Using constructed biomass data shows a slightly larger treatment effect, i.e., 14.2% with no controls and 11.7% with full controls. Overall, these findings suggest that our results in the main text using catch data also hold when inferring stock status from (synthetic) biomass data.

## 6 Discussion

Our main analysis has focused on recovering unbiased parameters of the effect of PPRs on collapse, and exploring how these treatment effects vary with different country and species characteristics. Here, we draw on these estimated relationships to predict and discuss the potential effectiveness of PPRs if implemented in non-PPR fisheries around the world.<sup>55</sup>

Before doing so, it is important to remind the reader that the main goal of this paper has not been to make out-of-sample predictions. We therefore need to exert caution in applying the estimated relationship between PPRs and various country and species characteristics to non-PPR fisheries. This is particularly the case for ownership protection,

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<sup>54</sup>Essentially, the method provides estimates for  $r$  and  $K$  even if the fishery is data-poor and traditional estimation techniques (e.g., linear regression) are not feasible. Obviously, there are important caveats to consider. First, there is substantial room for error, because the estimations often rely on few observations. Second, to simulate biomass trajectories, one needs to assume priors, which are based on little information. Third, catch is still the only explanatory variable, which allows us to use it on the entire sample. Alternatively, one could consider using also broader information (e.g., life-history traits) on a smaller sample to assess the state of the stock ([Costello et al., 2012](#)).

<sup>55</sup>See also [Diekert et al. \(2010\)](#) for a discussion on the potential effectiveness of PPRs for different regions around the world.

where there is little overlap between the two groups, and making predictions will involve large extrapolations. For species characteristics, there is a larger degree of overlap, and predictions might be less problematic. In the following, we explain the steps taken to make predictions for non-PPR stocks, including procedures to alleviate problems of excessive extrapolation.

## 6.1 Predicted effectiveness of PPRs for non-treated stocks

Where would PPRs be most effective if implemented in (currently) non-PPR fisheries? To shed light on this question, we use the estimated heterogeneous effects of PPRs to make out-of-sample predictions. Concretely, we use the estimated relationship between PPRs and country and species characteristics derived from 2-5<sup>th</sup> degree polynomials (see Appendix Figure B.2), and apply these to stocks not covered by PPRs.<sup>56</sup>

We start by making four separate predictions based on (i) species growth rate, (ii) species value, (iii) rule of law, and (iv) trade openness. To mitigate problems of extreme values and excessive extrapolations, we bound the values of the species and country characteristics by the intervals shown in the respective sub-figures in Appendix Figure B.2.<sup>57</sup> As the rule of law variable exhibits little in-sample variation, we introduce an additional variable to proxy for ownership protection: (constant) GDP per capita. While this variable correlates with ownership protection, there is at the same time more overlap between the treatment and control group, which might make it more suitable for making predictions.<sup>58</sup> Averaged predicted effects by EEZs for each of these five characteristics are presented in Appendix Figure D.1. To arrive at a single predicted effect for each EEZ, we calculate an unweighted average of the five characteristics, which is presented in Figure 7c. Lastly, we also show predictions based on species characteristics only (Figure 7a), or country characteristics only (Figure 7b).<sup>59</sup>

Making predictions based on *species characteristics* only, we see that warmer regions close to the equator show a larger potential effect of PPRs relative to colder regions. This is both due to high species growth rates in these regions (see Appendix Figure D.1a) and high species prices (see Appendix Figure D.1b). Regions such as Southeast Asia, Oceania (with the exception of New Zealand), the Caribbean and parts of Africa show a particularly high potential. Colder regions, such as Northern Europe, North America, New Zealand, but also South America and South Africa, show a smaller predicted effect of PPRs. Globally, the EEZs in the top quartile show a predicted effect of 8.9-9.8%, while

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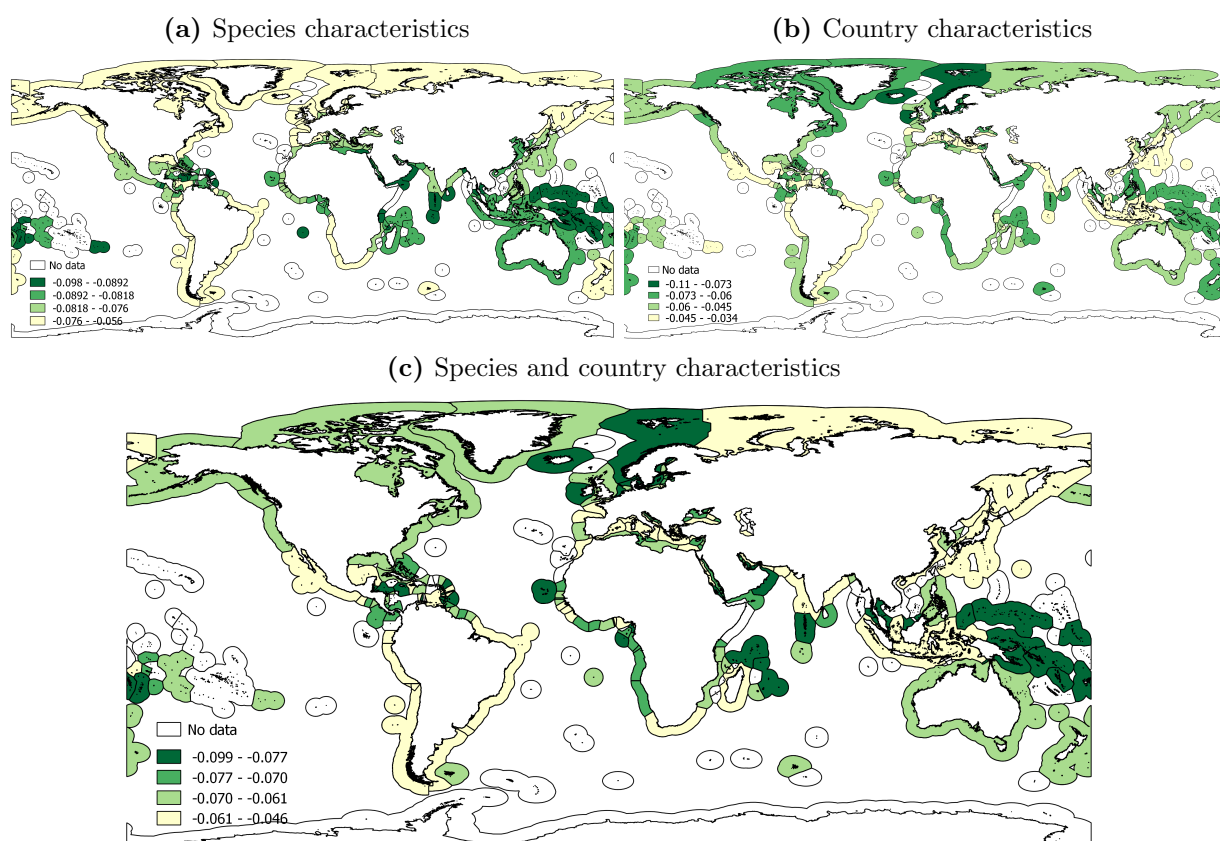
<sup>56</sup>Due to the lack of overlap between the treatment and control group for rule of law, we only use two categories: very high rule of law, and low to high rule of law, which correspond to the groups used in Figure 6b and 6c.

<sup>57</sup>Specifically, we replace the value of the species or country characteristic by the minimum or maximum of the bounded interval. For example, a species value larger than USD 5000/ton is replaced by 5000 before using the values to make predictions.

<sup>58</sup>Estimation results for GDP per capita are presented in Appendix B.5.

<sup>59</sup>These results are also presented as scatter plots in Appendix Figure D.2.

**Figure 7:** Predicted effect of PPRs for non-treated stocks



Note. Maps show the predicted effect of PPRs based on species and/or country characteristics of non-PPR stocks. Map (a) shows the average of predictions based on species growth rate and species price. The species growth rate is restricted to be in the interval  $[0.05, 0.5]$ . Species prices are restricted to be in the interval  $(0, 5000]$ . Values below (above) the bounded interval are assigned the minimum (maximum) value in the given interval. Map (b) shows the average of predictions based on trade openness, rule of law, and GDP per capita. Trade openness is restricted to be in the interval  $[30, 90]$ . GDP per capita is restricted to be in the interval  $[20, 34]$ . Rule of law is divided into two groups based on a value below or above the 90th percentile. Map (c) shows average predictions based on species characteristics (growth rate and price) and country characteristics (trade openness, rule of law, and GDP per capita). See Appendix D for more details.

the bottom quartile show a predicted effect of 5.6-7.6%. Compared to the estimated treatment effect of actually treated stocks (7%), the majority of non-PPR stocks show a more favorable effect of PPRs on collapse, i.e., a higher effectiveness.<sup>60</sup>

Using *country characteristics* to predict the effect of PPRs, the picture is to a large degree reversed (see Figure 7b). Regions with strong ownership protection, high GDP per capita, and high trade openness are predicted to benefit relatively more from PPRs. As a consequence, regions such as Northern Europe show larger predicted effects.<sup>61</sup> The relatively high effectiveness for various small island, e.g., in the Caribbean and Pacific, is to a large degree caused by these EEZs being under British, American or French territory. They are hence assigned country characteristics based on the sovereign country. The relatively large predicted effect for some African EEZs are primarily driven by a high

<sup>60</sup>This may be more clearly illustrated by looking at Appendix Figure D.2, where most non-treated EEZs lie above the horizontal line indicating the estimated average effect for treated stocks.

<sup>61</sup>This is also very clearly seen from Appendix Figure D.2, where most of the EEZs located to the far right on the x-axis are in Western and Northern Europe.

trade openness rather than rule of law and GDP. Regions with relatively low potential based on country characteristics include Latin America, parts of the Mediterranean, India, Pakistan, China, Japan and Indonesia. Globally, the EEZs in the top quartile show a predicted effect of 7.3-11%, while the bottom quartile show a predicted effect of 3.4-4.5%. Compared to the estimated treatment effect of actually treated stocks (7%), the majority of non-PPR stocks hence show a smaller potential effect of PPRs in preventing collapse.

Taking *both species and country characteristics* into account (see Figure 7c), Northern Europe, parts of Central America, as well as islands in Southeast Asia and the Pacific show the largest potential. However, these islands also have a high share of highly migratory species (see Appendix Figure D.2), which are likely harder to manage by PPRs as they require multilateral cooperation. Regions with relatively low potential include South America, Mexico, Russia, Mediterranean, and South Africa. Overall, the EEZs in the top quartile show a predicted effect of 7.7-9.9%, while the bottom quartile show a predicted effect of 4.6-6.1%. Compared to the estimated treatment effect of actually treated stocks (7%), a little over half of non-PPR stocks show a smaller potential for PPRs lowering the collapse probability.

To summarize, species characteristics of non-PPR stocks indicate a potentially large effect of PPRs, while country characteristics pull in the opposite direction. If we give the five characteristics equal weight, about half of the non-treated stocks have predicted effects of PPRs that are at least as favorable as for actually treated stocks.

## 7 Conclusion

The misguided incentive for individuals to overexploit common pool resources (CPR) is a well-known phenomenon, giving rise to the *tragedy of the commons*, where the resource and potential rents are depleted (Hardin, 1968; Stavins, 2011). Manifestations of the tragedy are found in natural resources like forests, rivers, and pasture lands, as well as the global atmosphere in terms of accumulation of greenhouse gases. Of all renewable resources, ocean fisheries are probably the most depressing example of the ill consequences of a common pool resource that is left to open access (Heal, 2007). A potential solution is the establishment of private property rights (PPRs), which has compelling theoretical arguments (Wilens, 2000), but the empirical evidence of whether establishing PPRs does improve sustainability is mixed (van Putten et al., 2014). In this paper, we have tried to quantify a causal relationship from PPRs to fisheries collapse, as well as highlight important institutional and species characteristics that either strengthen or weaken the effect.

Our main finding is that private property rights, in the form of quota systems, lower the probability of a stock collapsing. Using a DiD set-up, we find that the average probability of a PPR-fishery collapsing after policy implementation decreases by 7-10%

- depending on specification and sample size. Further, we find that the probability of a PPR-fishery moving into collapse in a given year decreases by 1.4-1.8%. Applying an IV-DID strategy leads to somewhat larger effects - in particular for the transition measure of collapse. Investigating heterogeneous effects, we find that trade openness, a high species growth rate, and species price tend to strengthen the favorable effect of PPRs. Typically, these factors increase the potential rents that can be extracted from the fish stock, which can be good or bad, depending on the institutional setting. Under open access, higher potential rents may increase the incentive to overexploit the resource, potentially making a stock collapse more likely. At the same time, higher rents may also lead to more effective regulations, in line with our findings.<sup>62</sup> Further, we find that strong ownership protection and transferability of quotas make PPRs more effective. This lends support to the idea that ownership increases the incentives for quota holders to lobby for quotas that are maximizing the net present value of the quota (Arnason, 2012; Costello and Grainger, 2018), though we cannot test this particular mechanism with our data. Carefully designed economic experiments or case studies may shed further light on the importance of different mechanisms explaining why transferable quota may lead to better outcomes.

The effectiveness of PPRs depends on species characteristics, as well as institutional, i.e., country characteristics. As PPRs are mainly implemented in countries with strong formal institutions, we might not see the same favorable effect in countries with weaker institutions. Our exploratory analysis points to an important tension. Many fisheries have either favorable species characteristics or favorable country characteristics, but rarely both. While species characteristics of non-treated stocks point in the direction that PPRs will have relatively large effects, country characteristics pull in the opposite direction. Therefore, the question whether PPRs are a suitable management tool in specific fisheries needs to carefully consider the ecological and institutional conditions at hand.

While we have found that PPRs reduce the chances of a stock collapse, we have only made first steps to disentangle the potential feedbacks between different country characteristics, institutions, and ecosystem state. In order to give credible guidance to policy makers, future research should strive to identify a causal effect of implemented policy, taking into account that policy may interact with institutions already in place, either formal or informal. Further, as natural resources and social-ecological systems are facing increasing pressure, both through high exploitation levels, but also as a result of climate change, a better understanding of the evolution of institutions, and in particular how institutions respond to changes in the environment, will be an important and growing research field in the future.

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<sup>62</sup>See Copeland and Taylor (2009) for a theoretical model.

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