Roads, Rights, and Rewards: Three Program Evaluations in Environmental and Resource Economics

by

David Kaczan

University Program in Environmental Policy
Duke University

Date:_______________________

Approved:

____

Alexander Pfaff, Co-Supervisor

____

Brian Murray, Co-Supervisor

____

Ashwini Chhatre

____

Elizabeth Shapiro-Garza

____

Martin Smith

____

Subhrendu Pattanayak

Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy, in the University Program in Environmental Policy
in the Graduate School of Duke University

2017

ABSTRACT

Roads, Rights, and Rewards: Three Program Evaluations in Environmental and Resource Economics

by

David Kaczan

University Program in Environmental Policy
Duke University

Date:_______________________

Approved:

____

Alexander Pfaff, Co-Supervisor

____

Brian Murray, Co-Supervisor

____

Ashwini Chhatre

____

Elizabeth Shapiro-Garza

____

Martin Smith

____

Subhrendu Pattanayak

An abstract of a dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, in the University Program in Environmental Policy
in the Graduate School of Duke University
2017

Abstract

This dissertation presents three program evaluations in environmental and resource economics. In the first chapter, I ask whether rural roads can contribute to a reversal of tree cover loss. Prior literature shows roads to be strong drivers of deforestation; however, I hypothesize that in some settings the opposite relationship may hold. Roads may (1) increase the relative productivity of labor in non-agricultural sectors, reducing agricultural activity and allowing reforestation; (2) raise profits from forest management or plantations by linking markets, encouraging forest planting; and (3) provide access to imported fuel sources, reducing pressure on forests from firewood collection. I use a large-scale rural road construction program in India to explore these possibilities. I construct a nationwide, village-level panel, and estimate the impacts of roads on tree cover using a differences-in-differences approach. In aggregate I find that road construction contributed to tree cover expansion, in great contrast to the existing empirical road-forest literature. I also find considerable variation in road impacts across settings within India: frontier settings saw reductions in tree cover due to new roads, while less isolated settings with more established agriculture saw increases in tree cover.

In the second chapter, I apply similar quasi-experimental methods to a very different question: does rights-based fisheries management increase fish prices? Rights-based management, specifically "catch shares," is known to extend fishing seasons by slowing the destructive "race to fish." This reduces fishing costs. It may also increase fishing revenues, because longer fishing seasons reduce product gluts that depress prices. I test this hypothesis for the majority of U.S. catch share fisheries (all those with data available) using an individually matched control fishery for each treated, catch share fishery and a difference-in-differences approach. I find evidence for increased ex-vessel prices among fisheries that undergo season decompression; however, highly variable results suggest that there is a need for a richer theoretical understanding of transitions to rights-

based management. I discuss effort substitution in multispecies fisheries systems as a possible explanation for this heterogeneity.

In the third chapter, I consider how environmentally beneficial actions can be incentivized by conditional payments (i.e. payments made in return for specific actions or outcomes) in collective land management settings. I use a framed field-lab experiment with participants from collective lands enrolled in a new payments for ecosystem services (PES) program in Mexico. I test the impact of increasing collective conditionality. Because social interactions are integral in collective decision-making, I also test the impact of PES design features that aim to improve group cooperation. Greater collective conditionality raised contributions, with higher impact on lower baseline contributors. Giving groups a way of participating in program rule-setting further improved their cooperation with those rules.

Table of Contents

List of Tables

List of Figures

List of Abbreviations

2SLS	Two Stage Least Squares
ACL	Annual Catch Limit
AFA	American Fisheries Act
ATE	Average Treatment Effect
BSAI	Bering Sea and Aleutian Islands
CONAFOR	Comisión Nacional Forestal (National Forest Commission)
CPR	Common Pool Resource
DAS	Days at Sea
DFO	Department of Fisheries and Oceans (Canada)
DID	Differences in Differences
FC	Fondos concurrentes (Matching Funds)
FE	Fixed Effect
FPI	Fish Price Index
FT	Forest Transitions
GFC	Global Forest Change
GLS	Generalized Least Squares
GMFMC	Gulf of Mexico Fisheries Management Council
GOM	Gulf of Mexico
IFQ	Individual Fishing Quota
IM	Internal Mechanism
IPHC	International Pacific Halibut Commission
IV	Instrumental Variable
JFM	Joint Forest Management

MAFMC	Mid-Atlantic Fisheries Management Council
MODIS	Moderate Resolution Imaging Spectroradiometer
MXN	Mexican Peso
NASA	National Aeronautics and Space Administration
NE	Northeast
NEFMC	North East Fisheries Management Council
NGO	Non Governmental Organization
NMFS	National Marine Fisheries Service
NOAA	National Oceanic and Atmospheric Administration
NWFSC	North West Fisheries Science Center
OLS	Ordinary Least Squares
OMMS	Online Management, Monitoring and Accounting System
OY	Optimum Yield
PES	Payments for Ecosystem Services
PFMC	Pacific Fisheries Management Council
PFMC	Pacific Fisheries Management Council
PG	Public Good
PMGSY	Pradhan Mantri Gram Sadak Yojana (Prime Minister's Rural Roads Scheme)
PSAH	Pagos por Servicios Ambientales Hidrológicos (Payments for Hydrological Services)
SA	South Atlantic
SAFMC	South Atlantic Fisheries Management Council
SE	Standard Error
TAC	Total Allowable Catch
TE	Treatment Effect

USD	United States Dollar
VC	Voluntary Contributions
VCF	Vegetation Continuous Fields
WATE	Weighted Average Treatment Effect
WSS	Within-class Sum of Squares
WV	Weighted Variance

Acknowledgements

Generous support from many individuals and organizations made this research possible.

Shilpa Aggarwal and Ashwini Chhatre provided data, advice, and hosting at the Indian School of Business. Nikita Lakhotia, Varun Goel, Arnab Dutta, Harry Fisher, and Ridhima Gupta provided assistance and ideas. Sanjog Sahu and Satya Prasanna facilitated field trips in Odisha and Himachal Pradesh, where numerous individuals shared their wisdom and made me feel welcome. Chapter one funding was provided by the Nicholas Institute for Environmental Policy Solutions and the Duke University Department of Economics.

Co-authors Anna Birkenbach and Martin Smith have been a privilege to work with. Together we thank colleagues at NOAA and DFO for their assistance with data - who knew fisheries could be so complicated. Chapter two funding was provided by NOAA, ECS Federal, and NC Sea Grant.

In Mexico, I was fortunate to have the assistance of fieldwork extraordinaires Maria Fernanda Pereira, Fernando Gumeta Gomez, and Brenda Lara. In addition, Paola Bauche Petersen, Matt Hyde, Rubén Pérez Peña Sánchez, Joaquin David Saldaña Herrera, Maria Brenda Aly Zavala López and colleagues at CONAFOR went well out of their way to help us. Individuals at INDAYU, FARCO, MABIO, and Ejido San Agustín did likewise. My time in Mexico was a highlight of my PhD experience, and it was a pleasure to work with co-authors Elizabeth Shapiro-Garza and Luz Rodriguez in the field. Back at Duke, Morgan Capilla assisted with data analysis. Chapter three funding was provided by the Tinker foundation, the Duke Center for Latin American and Caribbean Studies, and the Duke University Graduate School.

Jennifer Orgill, Ben McCartney, Faraz Usmani, Jess Lewis, Katy Hansen, Jen Shen, Anna Birkenbach, and Ana Maria Montoya were (and will remain) my go-to people for sanity, laughter, and brilliance. I appreciate Stephanie Simpson’s encouragement and intellectual input.

Faculty, staff, and the rest of the UPEP student group, made Duke home. Margaret McKean, Billy Pizer, Randy Kramer, Marc Jeuland, Indermit Gill, and many others, kindly provided advice and assistance. Danielle Wiggins and Meg Stephens kept UPEP functioning. From further afield, I am grateful for the wisdom provided by longtime mentors: Vic Adamowicz, Brent Swallow, Jeff Connor, and Darla Hatton MacDonald.

Throughout, I have had the benefit of an ever-supportive committee, academically and personally: Elizabeth Shapiro-Garza, Ashwini Chhatre, Subhrendu Pattanayak, and Marty Smith. Thank you!

I could not image better advisors than Alex Pfaff and Brian Murray. They provided the highest quality academic guidance and personal mentorship at every step of the way. If I ever have a graduate student, I will try to pass on this experience.

Finally, I deeply appreciate the support and encouragement of my family in Australia: Anna, Michael, Mum and Dad. It was in no way diminished by the distance. My late grandparents, Margaret and James Tedder, inspire what I do.

Introduction

Between 2000 and 2012, the planet lost approximately 2.3 million square kilometers of forest, an area larger than Mexico (Hansen and et al., 2013). This conversion is a major driver of biodiversity loss, decreased water quality, local air pollution, and is the second largest source of greenhouse gas emissions globally (IPCC, 2014). Although the rate of global forest loss has slowed slightly from past decades, much greater reductions in deforestation and forest degradation are required to meet climate change mitigation targets and local environmental goals.

Ocean resources similarly show considerable degradation. The proportion of fish stocks harvested above sustainable levels increased from around 10 percent in 1974 to over 30 percent in 2013. Overall, around 90 percent of fisheries are considered either fully exploited or overfished (FAO, 2016a). Mismanagement of fisheries is estimated to cause global losses of economic value on the order of 80 billion dollars annually (Arnason et al., 2016).

In both domains, resource degradation and consequent economic costs are not inevitable. A range of policy approaches have shown success in mitigating tradeoffs between forests and agriculture, the primary cause of deforestation. Forest cover is increasing in many regions (with the important exception of the lower-income tropics), partially due to human migration and agricultural technology change, but helped by improved forest policies (Barbier et al., 2010; FAO, 2016b; Mather, 2007). These include community based forest management, certification programs, and conditional payments programs (Agrawal et al., 2008; Nepstad et al., 2008). Fisheries management has also improved with more sophisticated use of harvest limits, ecosystem-based management, and catch shares (Costello et al., 2008; Worm et al., 2009). Some specific fish stocks have been rebuilt although globally fisheries remain in poor health (Arnason et al., 2016; Worm et al., 2009). Further improvement of both forest and fisheries management could deliver immense economic benefits (Costello et al., 2016; FAO, 2016c).

Knowing what policies are most likely to achieve benefits in a particular setting requires knowledge of the incentives that motivate resource users. In land use policy, for example, incentives are primarily the economic benefits derived from the available land use options, usually some form of agriculture or forestry. At the individual and even national level, benefits are misaligned with the associated larger social costs of many land use choices. The possibility for successful policy intervention depends on the extent to which private benefits can be modified. In the land use domain, two key policies are foci of this thesis: road development, and conditional payments programs.

Generally speaking, road development has been shown to be strongly associated with deforestation (Angelsen and Kaimowitz, 1999; Ferretti-gallon and Busch, 2014). However, much of the evidence is based on a particular setting, namely frontier areas where agriculture and forestland are most directly in conflict. While the evidence regarding roads' negative impacts is consistent for these settings, and a great cause for concern (Caro et al., 2014; Laurance et al., 2014), there are other settings where roads do not have strong detrimental impacts, and can even cause increased reforestation. This is the key finding of my first chapter. I do not claim that roads are less destructive in frontier forests than the existing literature demonstrates, but I do suggest that roads may facilitate positive landscape changes when placed in areas with the potential for economic transitions away from land-extensive agriculture.

I arrive at this conclusion through an evaluation of the tree cover impact of a nationwide rural roads program in India. I use satellite-based land cover data, combined with village-level census and roads data. I use a quasi-experimental econometric approach to establish cause and effect regarding the reduced-form relationship: Roads increase tree cover, particularly in more agriculturally-developed regions closer to urban areas. I believe that a number of mechanisms could underpin this finding. Tree cover increases are most likely in places where roads raise relative returns to non-agricultural activity, and/or where they raise profitability of producing forest products. If roads cause labor and capital to shift away from agriculture and into non-agricultural activity, they could reduce agricultural expansion. If they raise returns to forest products, they incentivize more plantations or better management of existing forests (land tenure arrangements and geography permitting). It is conceivable that roads could also facilitate a substitution away from local firewood towards imported energy sources. More detailed investigation of mechanisms will be the focus of future research.

In arriving at this finding, I providing evidence that rural roads can contribute to forest transitions (FTs). FT theory states that a country or region's forest cover first declines in line with social and economic development, but eventually stabilizes and begins to expand along with rising incomes (Mather, 1992). This is based on the observation, in many countries, of an approximately 'U-shaped' forest cover function over time (Mather, 2007; Rudel, 1998). The two most commonly cited mechanisms of FT are human migration, which reduces population density and allows forests to return, and increased demand for forest products, which encourages reforestation for productive purposes. Roads could plausibly play a role in both mechanisms, and could contribute both to the initial decline in forest and the eventual upswing. This has been the subject of speculation, but has not been previously demonstrated.

My second chapter applies similar program evaluation methods to an important question in fisheries management. Much of the economic inefficiency that occurs in fisheries is due to competitive externalities (Grafton et al., 2006; Kelleher et al., 2009). Under certain management regimes, fishermen compete to secure their proportion of the catch. Known as the "race to fish," this competition increases the costs of fishing (Grafton, 1996; Huang and Smith, 2014), contributes to bycatch, discarding and habitat disruption (Costello et al., 2008; Griffith, 2008) and heightens safety risks (Pfeiffer and Gratz, 2016). While the race to fish may occur in open access fisheries (i.e. in largely unmanaged situations), many tightly managed fisheries also suffer from this problem. The archetypal example of this is a fishery managed with season closures: when the fleet has caught the biologically-determined total allowable catch (TAC), managers close the fishery for the season, therefore incentivizing fishermen to catch as much as possible before closure. A desirable alternative is "rights based management." When fishermen have guaranteed access to a proportion of the season's TAC, that is to say, a quasi-property right over a share of the total catch, there is no (or greatly reduced) need to compete.

In an earlier paper with co-authors Anna Birkenbach and Martin Smith, I established that catch shares slow the competitive race to fish, as predicted (Birkenbach et al., 2017). Given this, we now evaluate a specific, hypothesized economic benefit to ending the competitive race: higher fish prices. Competitive fishing and short seasons means that fish are landed in a concentrated period of time, potentially causing market gluts and encouraging development of low value frozen product markets, rather than higher value fresh product markets (Homans and Wilen, 2005). This is an important theoretical prediction of rights based fisheries management that to date has not been directly tested to our knowledge.

We test this hypothesis for almost all U.S. catch share fisheries (those with adequate data), by comparing each catch share fishery to a similar fishery that did not undergo catch share management reform at the same time. Results are highly variable, but fisheries that expanded in season length also tended to see price increases, as predicted. The variability in our results may be explained by substitution of effort between fish stocks within multispecies fisheries. Because fishermen often target multiple species within a season, expanding the season for a high value fish might mean compressing the season for another, lower value fish. In addition to helping motivate new theoretical work on fishing behavior in multispecies complexes (Smith et al., 2016), this paper provides cautious support for greater catch share use on account of their positive price impacts.

Returning to forests, my third and final chapter focuses on incentives within collective institutions. The context for this chapter is Mexico, a country with both high rates of deforestation and rural economic disadvantage (Brandon et al., 2005; Muñoz-Piña et al., 2008). For these reasons, Mexico developed nation-wide "payments for hydrological services" (PSAH) programming from 2003 under the direction of the National Forestry Commission (CONAFOR). Programs ostensibly aim to improve catchment management through the conservation of forests while supporting rural livelihoods (Shapiro-Garza, 2013). This includes an attempt to develop "local" PSAH programs, where contracts are struck directly between downstream water users and upstream forest managers, with government support. Because the large majority of Mexican forests lie on communally-titled land, these programs face collective action challenges.

With my co-authors Alexander Pfaff, Luz Rodriguez, and Elizabeth Shapiro-Garza, I designed and implemented[1] a framed field-laboratory experiment played with members of PSAH-participating communities in Mexico. We explore mechanisms that may improve the performance of the collective PSAH program currently in operation. We first test whether more demanding contracts are likely to increase cooperative behavior within the collective. We secondly examine how social interactions within the collective modify the community's response to those stricter PES contracts, and thus predict situations in which collective contracts are most likely to be effective. The goal throughout is to determine ways that contracts can elicit "additional" ecosystem services, those beyond what would have been provided in the absence of the contract. Insights from this analysis aims to inform PSAH program refinements as it expands beyond the current set of participating communities.

[1] The assistance of field team members Maria Fernanda Pereira, Fernando Gumeta Gomez, and Brenda Lara is gratefully acknowledged.

The use of a field lab experiment makes this paper particularly distinct from the first two. While I describe the study as a program evaluation, given that the experiment is modeled on – and intends to inform – a particular PES policy, it does not study real program outcomes per se. The many dimensions on which both contracts and communities vary, and relatively small sample sizes, would make a control-treatment analysis of ex-post data more difficult than for my other studies. The big advantage of using an experiment is that it allows us to test outcomes of policy variations that do not currently exist, and to do so in a controlled setting. Although it does this in a stylized "field-lab", it uses real PES participants and frames the problem in a way analogous to their situation. We complement the experimental data with insights from focus groups and interviews.

These papers address distinct problems but share some important themes. Each reflects the importance of property rights on natural resource management outcomes. This is most obvious in the catch shares study, where the implementation of property rights is the actual policy intervention being evaluated. In the PES study, a particular form of property rights – collective forest management – is the context and motivation for the experiment. Here, property rights are not something that policy can explicitly change (at least, not without a referendum), but PES administrators can adapt their contracts to take advantage of the collective context. Group contracts may be cheaper to administer and verify. However, they face barriers to collective action: individuals must work together, at some cost to themselves, for the success of the group. The same collective action barriers, in principle, lead to poor outcomes for many non-catch share fisheries (although they manifest in quite different ways). In the roads paper, the role of property rights is not explicit, yet it is equally as consequential. I argue that one way in which roads may increase tree cover is by increasing the profitability of plantations and forest management. They could do this by linking rural areas to urban areas with higher forest product demand. However, profit opportunities can only be realized, and thus investments in plantations will only be made, if property rights are secure. At the extreme, a complete lack of property rights would lead to more deforestation, rather than replanting, in the face of increased forest product demand. This is because there would be no incentive for conservation or investment, much like the situation faced by an open access fishery. This is not the only pathway by which roads may affect tree cover, but it is a pathway that is notably mediated by property rights, at least in principle.

A second common theme is that heterogeneity offers important insights into mechanisms. In each paper, I start by estimating average treatment effects. I arrive at "overall" answers by blending disparate individual units of analysis together, be they villages, fisheries, or Mexican farmers. For example, my simplest analysis of the impact of roads on tree cover treats villages in very different geographic and economic contexts as equivalent. In isolation, this would be an unsatisfactory analysis as it obscures useful lessons contained within heterogeneity: some areas of India show negative road impacts on tree cover (more remote, 'frontier-like' areas), even while larger areas shows positive impacts. These differences are broadly consistent with theory and help reconcile my findings with existing literature. In the collective PES experiments, we find that less cooperative individuals are more strongly motivated by stricter PES contracts than their more cooperative peers. Although this appears puzzling, it is consistent with insights from social psychology, namely those on "motivational crowding" (Bowles, 2008; Frey and Jegen, 2001). In the fisheries study, we find decreases in prices for fish products that are already sold at a low price, possibly because these products do not have fresh-market potential (and thus are unlikely to benefit in price terms from an extended season). In all cases, these findings from subsample analyses provide useful insights into potential mechanisms. They also provide opportunities for policy targeting, by suggesting which type of fishery, village, or PES contractee will respond most positively to a given intervention.

In summary, my thesis has both theoretical and practical goals. Our understanding of the micro-causal drivers of forest transitions is limited; I show that transport links help explain this phenomenon. On a practical level, an improved understanding of how roads' impacts are modified by setting may help policy makers plan rural development that optimizes over both social and environmental goals. I show that property rights in fisheries can increase revenues received by fishermen, even if the total quantity of fish harvested remains the same. This is valuable information given that the use of catch shares remains highly contested (to give just one example of this debate, bills considered by the U.S. congress in recent sessions are at odds on whether future catch shares should be limited). Finally, I present evidence suggesting that collective PES can deliver additional environmental outcomes relative to baseline outcomes. I further indicate ways in which collective PES's success may be enhanced by harnessing social interactions. It is my hope that these insights can improve livelihoods and environmental quality in each of these study settings.

1. Can Roads Contribute to Forest Transitions?

1.1. Introduction

Transport costs affect socio-economic development. Expansion of road networks in rural areas lowers transport costs, facilitating economic benefits through specialization, trade, and improved public service delivery. These benefits have been observed to contribute to agricultural productivity improvements, economic growth, and poverty alleviation in a range of countries (Aggarwal, 2014; Asher and Novosad, 2016; Bell and Van Dillen, 2012; Gollin and Rogerson, 2014; Khandker et al., 2009; van de Walle, 2002; Warr, 2010).

Expansion of road networks also affects forest cover. The empirical literature describes roads as one of the strongest and most consistent determinants of deforestation, particularly in tropical frontier forests (Angelsen and Kaimowitz, 1999; Ferretti-gallon and Busch, 2014; Geist and Lambin, 2002; Pfaff et al., 2013; Rudel et al., 2009). Roads decrease input costs, and in some cases, increase prices received for agricultural products. The area in which agriculture is profitable consequently expands, causing deforestation (in the absence of countervailing institutional constraints), a process described by the enduring von Thünen (1826) model. While not the sole cause of deforestation, roads and subsequent agricultural expansion have contributed to the high rates of forest loss observed globally over the past several decades. Between 2000 and 2012, 2.3 million km^2 of forest land was converted to other uses (Hansen and et al., 2013) (an area larger than Mexico). This implies the loss of a variety of ecosystem services, including carbon sequestration, water provision, and biodiversity (Foley et al., 2005). Concerns have been raised that investments in roads globally – up to 25 million new kilometers of road by 2050 – will exacerbate these trends (Caro et al., 2014; Laurance et al., 2015, 2014).

Yet there are reasons to believe that roads may not always cause deforestation and could, in particular settings, reduce deforestation or even encourage reforestation. First, roads could change the relative productivity of labor in non-agricultural and agricultural sectors in ways that reduce agricultural activity at low-productivity forest margins, and thereby reduce deforestation or increase reforestation at those margins. Second, roads could facilitate price convergence between rural and urban forest products markets, increasing the profitability of improved forest management or forest plantations. Third, roads could encourage substitution from local fuelwood to imported energy sources such as kerosene or compressed natural gas, reducing the pressure on forests. Whether one or more of these pathways is of sufficient magnitude to deliver net forest increase – given countervailing impacts of roads and other drivers of land cover change – is likely to be a function of local economic and geographic settings. Testing for the existence of such settings, and exploring their characteristics, is this paper's objective.

A majority of previous quantitative studies of deforestation have focused on frontier forests (i.e. forests with limited prior human settlement, limited or no property rights, and situated at or close to the interface between forest and cleared land), particularly in Central and South America (Ferretti-gallon and Busch, 2014; Rudel et al., 2009). This geographic focus is understandable: Forest frontiers feature the most dramatic land cover shifts, and South America has experienced a large absolute loss of forest cover (FAO, 2010) and a very fast rate of tropical rainforest loss (Hansen and et al., 2013). Other contexts may offer substantially different insights. India, the focus of this study, offers settings characterized by extensive histories of settlement and agricultural development, high rural population densities and (relatively) well-defined property rights. This implies a predominance of mixed-use landscapes, 'mosaics' rather than tracts of wilderness (although it should be noted that these forests may still provide a variety of important ecosystem services, see Agrawal et al., 2014). Further, forest cover in India is growing overall (World Bank, 2015). Importantly, India also offers the opportunity to causally test this paper's central hypothesis – that roads may increase tree cover above underlying trends in particular settings – via a recent large-scale rural roads building program. The program has natural experiment characteristics, allowing for the use of evaluation techniques that avoid many of the endogeneity problems that otherwise challenge causal identification of road impacts (van de Walle, 2009).

The program, *Pradhan Mantri Gram Sadak Yojana* (PMGSY) ('Prime Minister's Rural Roads Program') constructs one-lane all-weather sealed roads that provide access to previously unconnected villages across India. Its first stage, the focus of this study, ran from 2001 to 2013 and provided road access to over 110 million people. Construction was prioritized by village population ranges, directing road construction first toward larger villages in each district – thus allowing for evaluation of road impacts using a generalized difference-in-differences strategy.

I apply this strategy to a unique, village-level panel dataset comprised of roads data, remotely-sensed forest change data (Vegetation Continuous Fields (VCF), Townshend et al., 2011; Global Forest Change (GFC), Hansen et al., 2013), and village and district level socio-economic data taken from Indian censuses and National Sample Surveys. My panel includes every Indian village for which matches can be made (over 435,000). The value of this panel is that it documents road construction and forest change within villages across time, allowing for the control of all sources of time-invariant selection bias at a highly localized (village) scale. This is relatively unusual in the roads evaluation literature, particularly in the context of a natural experiment that simultaneously provides plausible control against time-varying selection bias.

The program rules imply that the year in which a village received a road was a function of its own population and of the population distribution in the surrounding district. Populations were determined by a census that took place just before the program commenced. The impact of the village's population (and associated characteristics) at the time of program commencement is time-invariant and thus controlled for by village fixed effects. Meanwhile, the population distribution (i.e. how many villages were higher in priority for roads in that district) is plausibly exogenous to time-varying characteristics of a particular village after the program commenced. Studies that lack these characteristics (observations of roads over time, and rule-based placement), risk bias due to the strong selection pressures that are typically present in rural road placement decisions (van de Walle, 2009; Warr, 2010). I test and support the assumptions underlying this strategy with an event-study analysis and checks on pre-treatment trends. As a further robustness check, I use the thresholds between population categories as instruments for new road construction in a cross section regression.

My empirical analysis shows that the causal impact of new roads on tree cover is positive for India overall, on the order of 0.6 to 1.2 percent relative to baseline tree cover, above underlying trends across the study period (2000-14). While previous studies provided indirect support for the possibility of positive road impacts (Deng et al., 2011; Foster and Rosenzweig, 2003), this is the first direct empirical evidence of such from any country, to my knowledge. The magnitude of this impact is small in absolute terms, yet is substantial given the short 15-year study period: land use decisions are not instantaneous, and trees grow slowly. Moreover, any such positive finding is noteworthy in the context of the deforestation literature that until now overwhelming concludes that new roads are a negative influence of forest cover (reviewed in section 1.2).

This positive average treatment effect suggests that new rural roads are contributing to India's forest transition. A forest transition is a reversal of forest loss on a national scale (Mather, 1992)[1].

[1] A forest transition leads to an approximate 'U-shaped' (or more accurately, reverse 'J-shaped') national forest cover function over time. Early studies documented this relationship for European countries (France,

This phenomenon is often explained in the literature with reference to human migration (urbanization), forest product price changes, agricultural intensification, technological change (such as the transition from wood to coal), and/or institutional change[2] (Barbier et al., 2010; Perz, 2007). Until now, roads have been largely neglected as possible contributors to forest transitions. My positive road impact finding is consistent with forest transitions theory given that linkages in labor and commodities markets (linkages that may be provided by roads) are implicit in the mechanisms proposed for forest transitions. I return to this point in the discussion (section 1.6).

A second key finding is considerable spatial heterogeneity in road impacts. Just as India differs from Central and South America, settings within India differ from each other. Within my study area, I hypothesize and then show that road impacts vary in predictable ways. Settings more characteristic of frontiers – those with more initial tree cover, and located at greater distances from urban centers – show negative road impacts (relative tree cover loss) from new road access, while more agriculturally developed and proximate settings drive the finding of positive impact from new roads (relative tree cover gain) observed for India as a whole.

This finding of significantly heterogeneous impacts, indeed differences in sign, is based on tree cover levels (VCF data) which reflect both deforestation and reforestation. The conclusion also

---

Denmark, Switzerland, and others) which passed through transition points in the 19th century (Mather et al., 2000, 1999). More recent forest transitions have been documented in Asian countries, notably China, India, and Vietnam (FAO, 2010; Foster and Rosenzweig, 2003; Mather, 2007). In India, the proportion of forested land increased from around 10 percent in 1971 to over 23 percent in 2012 (World Bank, 2015).

[2] Policy changes may complement, precipitate or diminish the economic forces underlying forest transitions. In France, for example, management and land tenure changes encouraged private forest investment (Mather, 1992). In India, joint forest management (JFM) was introduced in 1991 to devolve forest management to more local levels and thus improve conservation; and a widespread tree planting program of 'social forestry' increased tree cover in the 1970s and 80s (before the period considered in this study) (Mather, 2007).

finds support, however, when I consider deforestation rates alone (as captured by GFC, a separate data source). Deforestation rates slowed very slightly due to new road connections within the more agriculturally established settings, and increased slightly due to road connections in frontier type settings. Given that existing deforestation studies have focused on forest frontiers, the results from the more ‘frontier-like’ context are concordant with that literature’s negative road-impacts finding. I reconcile these opposing road impacts across settings using a modified von Thünen model, proposing that inter-sectoral transfers in labor may be responsible.

In reporting these findings for India, I do not suggest that the concerns raised about roads’ forest impacts globally (Caro et al., 2014; Laurance et al., 2015, 2014) are erroneous or unjustified. As I show, and as is clear from the existing literature, roads are often destructive. My findings simply suggest that in some settings (those characterized by mosaic landscapes, high rural population densities, and extensive agricultural development), roads give rise to benefits that may previously have been overlooked. On a practical level, my results offer the possibility of targeting new roads to avoid environmental costs and/or to generate environmental benefits. In combination with existing studies (e.g. Aggarwal, 2014; Asher and Novosad, 2016) and future research on socio-economic outcomes, these results can help determine road placement criteria likely to lead to joint social-environmental benefits from new roads.

In the next section I briefly review the road-deforestation literature and describe possible mechanisms for positive road impacts. I also present a modification of the von Thünen model that can account for opposing road impacts across settings. I then describe the multiple datasets used to construct this study’s panel in section 1.3 before expanding on the empirical strategy in section 1.4. Section 1.5 describes the results, and section 6 discusses these in the context of the forest transitions literature.

1.2. The Relationship Between Roads and Forest Cover

1.2.1. Relevant Literature

Early empirical literature on drivers of deforestation used national level indicators, and highlighted population density, agricultural expansion and wood production as key causal factors (e.g. Allen and Barnes, 1985; Rudel, 1989). Subsequent availability of satellite-based land cover data allowed for detailed econometric analysis of deforestation and its spatial correlates, including infrastructure. Early examples using these methods were the first to highlight transport costs, usually in the form of road proximity, as important deforestation factors (Chomitz and Gray, 1996; Cropper et al., 2001; Deininger and Minten, 1998; Nelson and Hellerstein, 1997; Pfaff, 1999). These studies explained their findings with reference to the von Thünen (1826) model, in which land rents are determined by production costs and commodity prices, which are themselves functions of transport costs. Four meta-analyses summarized these and numerous subsequent empirical studies, concluding that roads are among the strongest and most consistent determinants of deforestation (Angelsen and Kaimowitz, 1999; Ferretti-gallon and Busch, 2014; Geist and Lambin, 2002; Rudel et al., 2009).

However, debate exists over the magnitude and direction of road impacts in settings where considerable prior clearing has occurred. Far fewer studies have considered such settings. The deforestation literature gravitates towards places experiencing extensive deforestation, typically frontier forests with large tracts of undisturbed forest and minimal property rights. There is also disproportionate geographic representation from South and Central America (for example, more than 60 percent of studies considered by Ferretti-gallon and Busch, 2014, and more than 50 percent of studies considered by Rudel et al., 2009). A number of authors have highlighted the need to consider road impacts in other settings, including on relatively local scales i.e. within countries (e.g. Chomitz, 2007; Pfaff et al., 2009).

Yet previous studies that compare settings were inconclusive on the question of whether positive road impacts occur. Andersen et al. (2002) analyzed road impacts in the Brazilian Amazon with county-level data, and concluded that sufficiently high prior clearing would cause a reversal of road-induced deforestation. However, Pfaff et al. (2007) who undertook pixel-level analysis of the same region, did not reach this conclusion although acknowledged its possibility in other settings. They reported a reduction in roads' impact due to prior development but never a negative impact. In a later study, Pfaff et al. (2016) refined this finding by demonstrating non-monotonic road impacts for the Brazilian Amazon: small and negative in undeveloped and highly developed settings, and large and negative in moderately developed settings. Deng et al. (2011) analyzed prior road impacts on forest cover in China's Jiangxi Province, but found no robust road impacts either positive or negative. Indirect support for positive road impacts was found by Foster and Rosenzweig (2003) who, in a household sample within 250 Indian villages, showed that rising incomes drive forest product demand and consequent expansion in forest cover[3]. They did not explicitly test the impact of roads, yet roads as drivers of economic development may contribute to the increased demand for forest products they observed.

1.2.2. Potential Pathways

Foster and Rosenzweig (2003) thus highlighted increased demand for forest products (which incentivizes plantations and improved forest management) as a potential road impact mechanism[4].

[3] They rule out agricultural productivity increases as drivers of forest expansion (the 'Borlaug hypothesis').

[4]This hypothesis (not necessarily considering roads) is known as the 'forest scarcity path' in discussions of forest transitions (Barbier et al., 2010; Rudel et al., 2005).

In addition, I propose three other mechanisms: (1) agricultural intensification, (2) inter-sectoral labor shifts, and (3) energy substitution (Figure 1-1). All rest on the assumption that roads change prices due to price convergence between newly linked markets. I describe these mechanisms conceptually, before presenting an extension of the von Thünen model to explain how opposite impacts could manifest across space based on labor shifts between economic sectors. I also explore labor allocation in a simple model of household decision-making in the Supplementary Section (section 1.10).

The agricultural intensification pathway entails a relative shift in agricultural production from within forested areas or near forest margins, to non-forest areas. Such a shift could manifest in a number of ways: vegetable growing, cropping, or stall-fed animal production (e.g. small dairies), for example, could increase in prevalence relative to more land-expansive (but less labor productive) grazing on forestlands or along forest margins. These activity shifts could promote natural regrowth, and/or lower the opportunity cost of replanting[5]. Conditions promoting the shift towards intensification include improved agricultural product prices or reduced agricultural input prices, both of which could result from lower transport costs. Of course, an increase in land-intensive agriculture in itself does not preclude simultaneous growth in land-expansive agriculture or grazing (Angelsen and Kaimowitz, 2001). However, limited local supply of labor or capital, and limitations in the mobility of these factors, may cause a trade-off between agricultural types (explored in more detail in section 1.2.3 and formally in the Supplementary Section). Such a tradeoff, particularly in labor (i.e. localized rural labor markets), is likely in the context of this study

[5] Support for this pathway comes from Burton (2011), who quantified agricultural production and land use in 17 villages in Himachal Pradesh. In response to increased vegetable prices, labor for grazing on forested or marginal land shifted toward more lucrative vegetable cultivation, with a concordant decrease in total land use. Across villages, reduced grazing was correlated with increased forest density.

given the relatively low levels of migration between rural areas in India (Foster and Rosenzweig, 2003, 2004)[6].

The second hypothesized pathway simply extends this idea of sectoral shifts in labor to non-agricultural activities. Reduced input prices or increased output prices of non-agricultural goods and services (due to lower transport costs) could lead to greater employment in small commerce or manufacturing enterprises within newly connected villages. Similarly, roads could increase opportunities for labor to move to urban work opportunities nearby but outside the newly connected village (i.e. temporary migration or commuting). In both, roads could facilitate a labor shift from low-productivity agricultural or grazing sectors to the non-agricultural sector, reducing pressure on forests[7].

Third, increased trade due to roads increases the diversity of the consumption bundle available to households (Aggarwal, 2014). In the case of energy consumption, households may substitute firewood for non-forest fuels, such as kerosene and liquid natural gas (Baland et al., 2006; Veld et al., 2006). Commercial non-forest fuels are widely used in urban India, but less so in rural areas, where firewood remains the primary fuel for approximately 70 percent of households[8]. Firewood is generally collected at zero monetary cost, but at considerable time cost, from village forests,

---

[6] Foster and Rosenzweig (2004) reported that less than 11 percent of their representative rural sample (male, aged 20-37) had moved from their village of birth in 1999.

[7] Support for this pathway comes from Asher and Novosad (2016) in their assessment of the employment impacts of the PMGSY program. They report a ten percentage point reduction in the proportion of workers engaged in agriculture, and an equivalent increase in non-agricultural wage employment, following a new road connection. This reallocation is strongest in villages close to large cities, suggesting that access to urban opportunities is primarily responsible.

[8] Liquid natural gas accounts for approximately 15 percent of rural India's energy consumption (and is growing).

common lands, roadsides, and private fields (Heltberg et al., 2000). A shift from firewood collection to other labor uses could occur if substitute energy sources become available and affordable, reducing pressure on forests (Burton, 2011).

While I describe these mechanisms in terms of a potential positive impact on tree cover, each could have negative effects under different conditions. Commodity price or wage changes due to increased market connectivity could be negative or positive depending on initial prices and local scarcity or abundance. Property rights and their enforcement may further mediate road impacts. The extent to which agricultural expansion is possible depends on whether boundaries between agricultural land (owned by individuals or communities) and forest land (owned by communities or the state) are delineated and respected. Similarly, it is conceivable that higher demand for forest products could increase rather than decrease deforestation (or forest degradation) particularly in cases where property rights are weak (Agrawal and Yadama, 1997)[9]. And as discussed in section 1.2, road impacts will also depend on prior development (including existing land use) (Pfaff et al., 2016)[10]. This large number of possible outcomes motivates firstly my use of reduced form analysis for estimating forest area response to roads, and secondly, my focus on settings. Causal analysis of the impact of mechanisms in isolation is desirable but beyond the scope of this study.

---

[9] Open-access forests, and poorly managed commonly held forests will face increased pressure from higher forest products prices. In contrast, well-defined and enforced property rights will allow landowners to capitalize on increasing prices, encouraging improved management of existing forests or new plantations (Robinson et al., 2013).

[10] How exactly prior land use would impact marginal clearing is not clear from theory alone. One possibility is that localities that have undergone significant deforestation already are unlikely to offer high returns to further clearing (as clearing would likely occur on the most productive land first). Another possibility is that initial deforestation encourages further deforestation due to economies of scale, conglomeration effects, or by simply providing access to more remote locations.

1.2.3. A Model of Heterogeneous Road Impacts

The von Thünen model (Figure 1-2A) provides a useful theoretical starting point given its prevalence in the deforestation literature. Land is allocated to its use of highest rent. Formally, rent, *r*, is a function of output levels, *y*, output prices, *p*, transport costs, *v*, distance to market, *d*, wage costs, *w*, capital costs, *k*, and land rights enforcement costs, *c*. In a model of two land uses, agriculture (A), and forest (unused land):

$$rent_A = p_A y_A - w_A y_A - k - c - v_A d$$

The agricultural frontier is defined as the distance from the market at which agricultural cultivation is no longer profitable, i.e. $rent_A(d) = 0$. Hence:

$$d^{frontier} = \frac{p_A y_A - w_A y_A - k - c}{v}$$

Among other predictions, a decrease in transport costs, *v*, extends the distance to the frontier, implying deforestation. Modification of this model is necessary to predict the reforestation possibilities I raise in section 1.2.2. Imagine the addition of a plantation rent curve. One possibility is that increased rents to plantations could drive reforestation either on the frontier (if forestry and agriculture curves are closely aligned), or closer to markets if plantation rent starts higher but falls more rapidly than agricultural rents as a function of distance.[11]

An alternative explanation arises from consideration of other factors of production (Figure 1-2B). Assuming that profits accrue partially to land and partially to labor (rather than only to land),

---

[11] It should be noted that the original von Thünen model does not predict mixed land use mosaics. For a given distance, the land use with the highest rent dominates. In reality, mixed uses are likely due to local heterogeneity.

I allow wages per unit of agricultural output w_A, to be a function of per unit profits, $p_A - v_A d$. Hence:

$$rent_A + y_A w_A = p_A y_A - k - c - v_A d y_A$$

And thus $w_A = w_A(p_A - v_A d)$, where $w_A'(.) > 0$. This function gives the wage per unit of output. The wage per unit of labor, $\widetilde{w_A}$ (the basis for an individual labor-provider's decision) includes the agent's productivity (output, y, per time unit), γ. Hence: $\widetilde{w_A} = \gamma * w_A(p_A - v_A d)$. This is the 'realized' value marginal product of labor (i.e. that accruing to the laborer). As in the traditional von Thünen model (although omitted above for brevity), I include an additional economic sector that produces non-agricultural agricultural output, y_N.[12] The same assumptions apply with two exceptions: (1) demand for land (per unit of labor utilized) is less than that of the agricultural sector, and transport costs, v_N are greater. The latter simply represents the notion that proximity to urban areas is more important for profitability for the non-agricultural sector than for the agricultural sector due to the need for market access. Labor productivity in this sector is normalized relative to that in the first sector. Of course, no wages will be paid in either sector if the return to labor is zero:

$$\widetilde{w_A} = \begin{cases} \gamma * w_A(p_A - v_A d), & p_A - v_A d > 0 \\ 0, & p_A - v_A d < 0 \end{cases}$$

$$\widetilde{w_N} = \begin{cases} w_N(p_N - v_N d), & p_N - v_N d > 0 \\ 0, & p_N - v_N d < 0 \end{cases}$$

Graphically this is represented by the wage curves' intercept with the x-axis, and is analogous to the von Thünen zero rent condition. Given a uniform distribution of productivity across agents in

[12] In this generalized framework, I call this second sector the 'non-agricultural' sector. In reality, it may be any sector, agricultural or otherwise, which is distinct from the first simply based on land use and the productivity impact of transport costs.

the local labor market, agents choose to work in one sector or the other depending on their realized value marginal product of labor. The pool of available labor in each location is assumed to be fixed (as discussed in section 1.2.2 and footnote 6). Labor may select into either sector, or remain in surplus: $l_A + l_N \leq L$. Labor remaining in surplus is that of low productivity and/or high reservation wages. This setup is consistent with the ratios of agricultural to non-agricultural labor seen across India. There is a weak but statistically significant positive correlation at the district level ($r = 0.20, p < 0.01$) between the proportion of agricultural to total labor, and the average distance from villages in that district to the closest urban area. Districts with a majority of non-agricultural employment have low average distance (i.e. greatest access) to urban areas, almost all within 50 kilometers.[13]

At the frontier, deforestation occurs if the agricultural labor force (and thus agricultural area) expands in response to reduced transport costs. Given the differential transport costs between sectors, there is no viable non-agricultural sector at the frontier (i.e. the local labor force is employed in agriculture or not employed at all) and hence marginal changes in v_N do not lead to positive wages in the non-agricultural sector, $\widetilde{w_N}$. There is no possibility of inter-sectoral labor substitution, and growth in profits (implying both increased land rent and increased value marginal product of labor) can only lead to clearing on the frontier.

In intermediate positions between the frontier and the market, where there is a viable non-agricultural sector, sectors compete for labor. Marginal reductions in v_N and v_A increase demand for labor in both sectors, although disproportionately so in the non-agricultural sector because v_N > v_A. If the net transfer of labor from agriculture to non-agriculture is greater than the uptake of

[13] Data for this statistic and other analyses are described in section 1.3.

underutilized labor by the agricultural sector, agriculture will shrink, reducing pressure on forests. The reduced pressure on forests is because agriculture (and thus agricultural labor) has greater forest impacts than non-agricultural activity.

While reliant on a number of assumptions (importantly, differential labor productivity changes in response to transport cost changes between sectors, and labor market constraints), this analysis shows how opposing road impacts may be reconciled with von Thünen's monotonic predictions, and is useful given the observed heterogeneous impacts presented subsequently in this paper. It should also be noted that this mechanism is complementary with the previous mechanism described – a rise in plantation forestry due to increased plantation land rent – if plantations have lower labor demand than agriculture (as is likely).

1.3. Data

I construct a unique, village-level, countrywide panel of satellite-derived land cover data, socio-economic and demographic survey and census data, and PMGSY roads data, for 2000-14.

1.3.1. Land Cover and Slope

I use two publically available satellite-based products, Vegetation Continuous Fields (VCF) MOD44B (v. 51) (Townshend et al., 2011) and Global Forest Change (GFC) (Hansen and et al., 2013). VCF provides annual 2000-14 tree cover at a resolution of 250 meters, specifically, the percentage of each pixel covered by woody vegetation. The advantage of this product over other similar products is that it provides a continuous measure of tree cover without arbitrary thresholds between cover classes. GFC provides annual 2000-13 forest loss with a resolution of 30 meters. Unlike VCF, data are binary, i.e., a change in pixel value indicates the near-complete removal of tree canopy from that pixel. This gives the deforestation rate, but not cumulative forest cover or forest condition. Like other satellite-based data, these products do not distinguish between plantations and natural forests. I source slope data from the Esri online global terrain layer (Esri, 2013). This product uses NASA Shuttle Radar Topography Mission data with resolution of approximately 30 meters. I create village-level variables by averaging pixel values from the remotely sensed data products in each year over a uniform circular land area (radii of 5 and 10 kilometers) around each Indian village.[14] The location of villages is determined by plotting geographic coordinates extracted from the online portal *India Place Finder* (Sagara, 2011).

1.3.2. Road Data

Data on PMGSY road construction at habitation level, covering the period 2000-12, were sourced from the Indian Government's Online Management and Monitoring System (OMMS)[15]. Habitations are sub-units of villages (which are standardized administrative units for census purposes). PMGSY data includes habitation name, year of connection approval, habitation population, baseline connectivity, and whether a new road or an upgrade was constructed. I create road connectivity variables by aggregating habitation data to the village level. These include a binary variable indicating village connectivity if at least one habitation within the village is connected; a continuous variable indicating the proportion of a village's population connected (based on habitation-level population data), and similarly, a continuous variable indicating the sum of the village's population connected.

---

[14] This leads to some overlap of areas when villages are close together. As a robustness check, I remove up to 90 percent of the sample (randomly), which greatly reduces overlap. Results remain consistent.

[15] Processed records from OMMS were kindly provided by Shilpa Aggarwal (Indian School of Business) for this project.

1.3.3. Socio-Economic and Price Data

The 2001 Census (Ministry of Home Affairs, 2016) gives village-level demographic, socio-economic, land use, and infrastructure information, for the baseline year. The census does not use the village coding system used by the roads data, necessitating fuzzy merging based on district, sub-district, and village names (which contain considerable spelling differences). I successfully match 83 percent of villages. Summary statistics of census 2001 data shows the relative socio-economic disadvantage faced by villages without a road connection (Table 1-1).

1.4. Empirical Approach

1.4.1. The PMGSY Roads Program

The first phase of PMGSY operated between 2001 and 2013 and aimed to increase agricultural incomes via farm-to-market access, provide employment opportunities, and reduce rural poverty (Ministry of Rural Development, 2012a)[16]. Construction took place in 32 out of 36 states/territories, in 663 out of 676 districts. Approximately 45 percent of habitations in the participating districts were unconnected to (i.e. beyond 500 meters of) the national road network in 2000. At the end of the first phase, 175,674 habitations (11.5 percent of the total eligible) had been connected via

[16] Two recent papers suggest these goals were at least partially met. Aggarwal (2014) found that PMGSY roads led to increased use of chemical fertilizer and hybrid seeds, and changes in school attendance (increases for children 5-14 years of age, and decreases for older teenagers (14-20 years age) likely due to both increased access to schools and increased access to work opportunities). Aggarwal (2014) also showed a change in the mix of labor market activity with increased animal raising, textiles and retail, and an increase in consumption variety – specifically a switch from non-perishable foods (cereals and pules, for example) to increased variety in perishable dairy and vegetables, as well as greater non-food consumption. These changes are consistent with predictions based on reduced transportation costs. Asher and Novosad (2016) presented evidence of socio-economic transitions using a regression discontinuity approach and a highly detailed (individual level) panel. Labor switched toward non-agricultural activities, facilitated by local rural-urban migration rather than within-village sectoral shifts.

approximately 360,000 kilometers of new road, giving approximately 110 million people all-weather road access for the first time. Total expenditure was around USD 26 billion (including road upgrades and maintenance). Funding was provided by the federal government, sourced from a fuel tax. A second phase of the program (not part of the present study) is ongoing.

Importantly for causal analysis, road construction was prioritized by habitation population (determined by the 2001 census). National guidelines stipulated that within each district, habitations with a population of 1000 or above were connected first, those with a population of 500-999 second, and those smaller only thereafter (some states included a category of 250-499 in addition) (Ministry of Rural Development, 2012a). The population distribution within each district determined the order of construction within that district, and thus the likelihood that a particular habitation received a new road connection in a particular year. Resulting probabilities of road construction as a function of population categories provides evidence that the rule was in general adhered to (Figure 1-3 and Figure 1-4). In addition, a map of eligible road connections was made in advance of the program (the "core network") based on the minimum number of connections necessary to connect eligible habitations. Only connections specified in the core network could be subsequently constructed under the program. Only rural roads were eligible, and roads constructed had to provide a link to a formerly unconnected habitation. Both phases of the program upgraded existing roads as a second priority.

1.4.2. Identification Strategy

1.4.2.1. Exploiting Differences in Timing

I use a generalized difference-in-differences strategy with village fixed effects to identify the reduced-form impact of new road connections on tree cover. Treatment is receiving a road. Villages that do not receive roads act as controls; tree cover change in these villages is captured by the estimated underlying time trend. Villages that receive roads at some point in the study period are control villages prior to receiving the road and treatment villages after receiving the road. This approach compares the change in tree cover within villages that received roads with the change in tree cover within villages that did not receive a road (or have not yet).

The primary specification is:

$$F_{v,t} = \beta_1 R_{vt} + \boldsymbol{\beta} R_{vt} * \boldsymbol{X}_v + \theta_{st} + \theta_v + \varepsilon_{vt}$$

Where $F_{v,t}$ is average tree cover in time t around village *v*; R_{tv} is an indicator of road connectivity in time *t* in village *v*, and $\boldsymbol{X}_v$ are key time-invariant covariates[17]. I use three specifications of the road variable: a binary variable indicating village connectivity if at least one habitation within the village is connected, $I(R_{th} = 1)$; a continuous variable indicating the proportion of a village's population, N_v connected (where habitation population is given by N_h), $\sum_{h\in v} I(R_{th} = 1) * \frac{N_h}{N_v}$; and similarly, a continuous variable indicating the sum of the village's population connected, $\sum_{h\in v} I(R_{th} = 1) * N_h$.

Considerable changes in Indian tree cover can be expected irrespective of road impacts. To control for underlying trends I use state-year or district-year fixed effects, θ_{st}. Village fixed effects, θ_v, control for all time-invariant factors (i.e. remove time-invariant sources of selection bias). The state-year or district-year fixed effects flexibly and locally (i.e. at the state or district level) control for the underlying trend, allowing for estimation of the treatment effect above the underlying trend (the difference-in-differences estimator), represented by β_1. The treatment effect is an estimate of

[17] All panel regressions are linear OLS, estimated on Stata with user-created program '*reghdfe'* (Correia, 2014).

the average difference between the pre-treatment and post-treatment levels of tree cover, above the underlying trend. Identification is possible using a variety of subsamples: (1) all villages (i.e. those with and those without a road at baseline), (2) only initially unconnected villages, or (3) only initially unconnected but treated villages (i.e. villages in their pre-treatment state serve as control villages). Standard errors are clustered at the district level.

This strategy avoids some of the endogeneity problems often faced by studies of road impacts, specifically selection bias. Selection bias occurs because road placement is not random. Roads may be built in places already experiencing economic growth (correlated with many outcomes of interest, including land use change) or they may be built to places of low growth in an attempt to boost development (van de Walle, 2009). In my approach, selection bias driven by time-invariant factors, such as unobserved baseline village characteristics, is controlled by the village fixed effects. Selection bias driven by time-varying factors is more problematic: roads may be built to service forest plantations installed during the program period, which are unobserved across time and thus cannot be captured by controls or fixed-effects. I rely on the natural experiment characteristics of the PMGSY program to reduce the possibility of this bias, and then test for its presence. The prioritization rule means that the time in which a road is built to a village is a function of (1) the 2001 population of the village (a time-invariant property), and (2) the number of villages with higher priority for a road connection in the district (a characteristic external to the village in question). This reduces the likelihood that the timing of road connections are able to respond to time-varying factors within villages, although does not completely mitigate the possibility. Given this, I test for selection bias in an examination of pre-treatment trends, as well as present estimates disaggregated by time period.

1.4.2.2. Testing for Selection Bias

I regress treatment (whether village v received a road) on the change in forest cover over the first few years of the program:

$$R_v = \beta_1 \Delta F_v^{2000-0x} + X_v + \theta_s + \varepsilon_v$$

Where R_v indicates road treatment (at any point in time during the program), $\Delta F_v^{2000-0x}$ indicates the change in tree cover around the village between years 2000 and either 2003, 2004 or 2005. This variable is the coefficient of a simple linear regression of tree cover on year and thus incorporates information from intermediate as well as start and end-points in each village's time series. In some specifications a suite of controls or state fixed effects are included. Few roads were constructed in these early years of the program: 2.6, 5.0 and 7.5 percent of villages unconnected at baseline had been connected by 2003, 2004, and 2005 respectively (Table 1-3). While the small number of connections that are constructed within these periods could affect the observed trends, this possibility would bias results towards a relationship between tree change and treatment and thus make my pre-trends test more conservative.

1.4.2.3. Exploring Heterogeneity

A holistic description of baseline settings (and thus heterogeneity of settings) can be achieved by grouping like villages according to multiple baseline characteristics. I use k-means clustering, a procedure for partitioning observations into k classes in which each observation is attributed to a class based on its baseline characteristics. Each characteristic is standardized so receives equal weight. The characteristics used are (1) baseline agricultural development indicators (irrigated area, cropped area); (2) baseline infrastructure and services indicators (presence of a grid electricity connection, credit society, primary school, and primary health center); (3) baseline geographic descriptors (forest cover, distance to nearest urban area, and average slope); and (4) baseline scheduled tribe population (see Table 1-1 for summary statistics).[18] Attribution of villages to classes aims to minimize the within-class sum of squares (WSS), i.e. to minimize the squared Euclidian distance between the values of all characteristics $\boldsymbol{x_v}$ for a particular village *v*, and the means (across villages) of those characteristics, $\overline{\boldsymbol{x}}_k$, within a particular class, *k*:

$$\arg\min \sum_k \sum_v ||\boldsymbol{x}_v^{(k)} - \overline{\boldsymbol{x}}_k||^2$$

Attribution proceeds iteratively using a two-step procedure (the k-means algorithm). K initial means are randomly generated, and classes are created by associating each observation with the nearest randomly generated mean. Class means are then updated before observations are reattributed based on the updated means. This process iterates until convergence is reached (i.e. a stable WSS) and no observations change class between iterations. Given the potential for the initial random means to determine outcomes I repeat the above procedure 200 times with different starting points and select the partition most frequently observed.

1.4.2.4. Robustness: Exploiting Threshold Discontinuities

I provide supporting evidence for the observed treatment effect using instrumental variables regression. My instruments are the population thresholds, specified at habitation level, which guide construction prioritization (Figure 1-3 and Figure 1-4). The identifying assumption is that movement across the population threshold (a discrete change) increases the probability of receiving treatment, yet does not affect unobserved covariates that may affect tree cover, once population itself is controlled. I use flexible population covariates to achieve the latter. Thresholds apply to

---

[18] Inclusion of different or additional attributes will change class outcomes; however, given that most variables available in my dataset are correlated to some extent with those in this set, the change is relatively minor.

habitation populations in 2001 (at the start of the program). Given that habitations are sub-units of villages, and that tree cover data and census data is at the village level, I restrict the sample to villages with only one habitation (approximately 79 percent of the matched sample). Thresholds, T, are time-invariant instruments. I convert the panel to a cross section of villages by taking the difference in tree cover between endline and baseline observations, $F_v^{2000-2014}$. I specify road treatment, R_v, as construction of a new road in any year during the program. The IV predicts road treatment, rendering its predicted values exogenous to the extent that the identifying assumption holds. I specify the first stage as:

$$R_v = \beta_0 + \beta_1 \boldsymbol{I}(pop_v > T) + \boldsymbol{\beta} \sum_{q=1}^{4} pop_v^q + \boldsymbol{\beta} X_v + \theta_d + \varepsilon_v$$

I then regress the change in tree cover against predicted treatment in the second stage:

$$\Delta F_v^{2000-2014} = \beta_0 + \beta_1 \widehat{R_v} + \boldsymbol{\beta} \sum_{q=1}^{4} pop_v^q + \boldsymbol{\beta} X_v + \theta_d + \varepsilon_v$$

Controls, X_v, population controls, $\sum_{q=1}^{4} pop_v^q$, and district level fixed effects, θ_d, are included in both stages. I use a window of data (T ± 250) around thresholds. This strategy has one advantage relative to the difference-in-differences strategy, the potential to avoid bias from time varying sources of endogeneity. However, the use of a time invariant-instrument prevents use of the rich panel data, and furthermore estimates treatment effects only for the window of villages around the threshold (thus giving local average treatment effects). Unbiased estimates also rely on accurate population data reporting at the start of the program. Consequently, I use this strategy as a qualitative check of the difference-in-differences estimates rather than the basis for my key reported results.

1.5. Results

1.5.1. Main treatment effects

I first consider the observed change between baseline and endline tree cover around villages, distinguished by road treatment. While both groups indicate an increase in tree cover (as expected given underlying trends in India, see World Bank, 2015) (Figure 1-6), there is a slightly greater gain (0.08 percentage points) among villages that received a road ($p < 0.001$, under both parametric and non-parametric distribution assumptions). Such a raw test of means does not control for differences between treatment and control groups, or for annual anomalies that could disproportionately affect one group. Regression results that do, however, support this finding. My primary specification suggests that roads increased tree cover by 0.05 to 0.122 percentage points ($p < 0.05$) (Table 1-4). These coefficients represent the change in level of tree cover in the zone around a village (defined at radii of both 5 and 10 km from the village center point), relative to the underlying, flexibly specified trend in tree cover. Coefficient values are at the low end of this range under more localized time-trends, i.e. with district-year rather than state-year fixed effects[19]. While this increase in tree cover is small in absolute terms, so is the baseline: average tree cover in zones around unconnected villages is 11.45 percent (and 9.94 percent around all villages, connected and unconnected). In relative terms, these coefficients represent a 0.6 to 1.2 percent increase above underlying trends. Findings are relatively consistent in magnitude across different versions of the treatment variable once average village size is accounted for (807 persons in initially unconnected

[19] District-year fixed-effects provide a more localized (and thus more accurate) estimation of the underlying trend relative to state-year fixed effects, however, may represent excessive control. This is because the decision-making unit for treatment is a district-year, i.e. in every year, a district decides on (or at least submits for approval to higher levels of government) the road projects to undertake (Ministry of Rural Development, 2012b). Consequently, district-year fixed effects control for factors that determine the treatment decision as well as the underlying tree cover trend.

villages). Magnitudes and significance patterns are similar under 5 km radius zones. This finding is a considerable departure from the strong negative road impacts reported elsewhere in the literature (reviewed in section 1.2 and discussed further in the following section).

I apply this specification to three samples: initially unconnected villages (Table 1-4), all villages (Table 1-5), and only treated villages (Table 1-6). My specification, based on village fixed effects, identifies a treatment effect based on change over time within villages. In this context, the role of the control villages is to estimate the underlying trend, represented by the district-year or state-year fixed effects, which is then subtracted from the change observed among treated villages. I find positive, significant average treatment effects under all samples, although coefficients based on the all-villages sample are smaller (0.04-0.07 percentage points). My preferred sample is all unconnected villages at baseline, which provides for strong comparability between treatment and control groups.

1.5.2. Testing for Selection Bias

Threats to causal identification from time-varying sources of selection bias are not addressed by my village fixed effects strategy. To explore the potential for this bias I look for statistical evidence of differences in pre-treatment trends between treatment and control groups. A significant relationship between the change in tree cover prior to treatment and the treatment itself would suggest that roads are constructed in response to economic activities or local policy decisions that are themselves the drivers of land use change. Significance in this relationship can be seen in the simplest regression specification across all three pre-treatment periods (first column of each period in Table 1-7). However, the non-significance that results from when state fixed effects are included, and/or when a small suite of (time-invariant) controls are included, indicates that pre-trends may be explained simply by time-invariant rather than time-varying factors. Village-level fixed effects control for these factors – and do so much more comprehensively than state fixed effects or controls – giving grounds for confidence in the treatment effect estimates presented above.

I further explore endogeneity possibilities by estimating the causal impact within periods relative to treatment for each village: 1-3 years before and after, and greater than 4 years before and after (excluding the construction year). These estimations serve as a placebo test by estimating the relationship between treatment and tree cover prior to treatment taking place (i.e. shifting the construction indicator back in time). Results show no significance in pre-treatment years and significance following treatment (Table 1-8 (which also contains the same check but on the subsamples described in next section) and Figure 1-7). Evidence of reverse causality would show up as significance in pre-treatment years.

1.5.3. Exploring Heterogeneous Settings

I consider treatment effects specific to classes of villages identified using k-means cluster analysis. The optimal number of classes, *k*, may be determined through inspection of the change in the within-class sum of squares (WSS) statistic as *k* increases. I consider $k \in \{1, \dots 12\}$. WSS decreases approximately linearly until a change in slope around k = 6 (Figure 1-8), suggesting an optimum at this point (Makles, 2012). However, an arguably overriding criterion for choosing *k* is the ability to make useful interpretations of the resulting partition. More than three classes tends to result in small classes splitting off due to extremes in a single characteristic (e.g. presence of a medical center). I present two and three class solutions, which have relatively large classes and differ on a number of baseline characteristics.

In the two-partition analysis (Table 1-10, columns 2-3), approximately three quarters of the sample (class 1) is characterized by low initial tree cover, relatively flat topography, and greater areas of irrigation and crop cover. This class has above average infrastructure and services (health, education, credit society and electricity) and is located relatively close to urban centers. It has a below average proportion of scheduled tribe population. The remaining one quarter of the sample (class 2) has opposite characteristics. Geographically, class 1 is centered in the densely populated Gangetic plain of Uttar Pradesh, Bihar and West Bengal, as well as parts of the central highlands of Rajasthan and Madhya Pradesh. Class 2 is found in the Himalayas (north and northeast), and scattered throughout the eastern peninsula plateau in Chhattisgarh and Odisha (Figure 1-9).[20] The three-partition analysis (Table 1-10, columns 4-6) returns one class of 60 percent of villages that looks very similar to class 1 in the two-partition analysis (hence is also called class 1). The remaining 40 percent are relatively distant from urban areas and have less irrigation and crop cover. One class within this group of villages has low initial tree cover and relatively high scheduled tribe population (class 2A, approximately 25 percent of villages) while the other has high initial tree cover and a smaller proportion of scheduled tribe population (class 2B, approximately 12 percent of villages). This class is also found on much hillier land concentrated in the Himalayas, while class 2A is more common on rolling hills in the eastern peninsula plateau areas and western plains of Rajasthan (Figure 1-10).

I estimate the impact of roads on tree cover within classes by interacting class indicators with the treatment variable (Table 1-11). Immediately apparent is a strong and statistically significant bifurcation in road impacts between classes. The more agriculturally developed, closer, better serviced villages (classes 1) show positive road impacts, specifically a 0.10 to 0.25 percentage point increase ($p < 0.01$) between the before and after periods relative to underlying trends. The hillier,

[20] The density of villages included in this analysis is greater in the northern half of India. This is due to my selection of only unconnected villages at baseline (for comparability). Infrastructure and service provision is generally much better in the south of India, and so there were far fewer southern Indian villages eligible for participation in the PMGSY program.

more distant, more treed areas (class 2B) show negative road impacts, specifically a 0.24 to 0.48 percentage point decrease ($p < 0.01$) relative to underlying trends in the 3 class partition. The raw data depicts this result visually, with opposite skews in tree cover change for treated and untreated villages (Figure 1-11), as do event study plots of coefficients for disaggregated time periods (Figure 1-12). In relative terms (relative to both underlying trends and initial tree cover), these coefficients represent a 1.2 to 3.2 percent increase in class 1 villages' tree cover, and a 0.7 to 1.1 percent decrease in class 2B villages' tree cover. I discuss the magnitude of these results in section 1.6 below. It should be noted that the underlying trend in tree cover in all classes is positive (with and without roads), with a faster rate of increase among class 1, intermediate in class 2B, and the slowest in class 2A.

Results for class 2 in the two class partition are negative also but not significant under district-level differential time trends. The weaker robustness of this result is not surprising given that class 2 is more broadly defined than class 2B. The villages that differ between these classes primarily form class 2A, which shows countervailing positive road impacts (although with mixed significance).

1.5.4. Corroborating Evidence

I first repeat the difference-in-differences analysis using my alternative outcome measure, the Landsat-derived annual deforestation rate data (GFC) (previous estimates use the MODIS-derived VCF data). Deforestation is not, in aggregate, a serious problem in India.[21] The high-resolution satellite data I use shows a small amount of deforestation (which may include the cutting of managed or planted forests), amounting to 0.024 percent of India's total land area per year (Figure

[21] The FAO Forest Resources Assessment, for instance, reports zero change in 'primary or old growth' forest cover (a measure of deforestation) over the last 15 years (FAO, 2015).

1-5), concentrated mainly in the northeast and east of the country. A number of villages (34.3 percent) report no deforestation over the 13-year time series. I find no statistically significant change in deforestation rate due to roads overall (Table 1-12). However, I find some evidence of road-induced changes after disaggregating by subsample. There is a 0.002 percentage point decrease ($p < 0.01$) in the annual deforestation rate in class 1, and a 0.006 percentage point increase ($p < 0.1$) in class 2B under state-level time trends, qualitatively consistent with the previously presented findings. Estimates are not significant with district-level time trends. These estimates are small in absolute terms, unsurprising given that underlying deforestation rates are low. In relative terms, these coefficients represent a 10 percent increase in the deforestation rate in Class 2B, and a 47 percent decrease in Class 1, calculated at mean values of baseline deforestation in each class.

I use the IV strategy as a robustness check on the main difference-in-differences results. Estimates are consistent in sign when significant, although as expected are weaker (due to the more limited use of data, see section 1.4.2.4). A positive average treatment effect is observed overall from use of the T=1000 threshold. The magnitude of this effect is larger than that observed under difference-in-differences. This is also to be expected given that in the cross section setup, this estimate describes the total change in tree cover between 2000 and 2014, rather than the more limited before-treatment after-treatment change in the case of difference-in-differences (which takes an average over these periods, reducing the possible observable effect size). Breakdown by class shows that the positive effect is driven by class 1, consistent with the previous heterogeneity analysis. No significant overall effect is observed when using the T=500 threshold. However, a negative and significant effect is seen in class 2B, as expected. Non-significant coefficient estimates are also negative, suggesting that the local average treatment effect around the 500-population threshold is lower than that around the 1000 population threshold. This could imply differential application of thresholds across regions, with more frontier-like regions (within districts and within classes) that see negative road-forest impacts having a higher proportion of smaller villages.

1.6. Discussion

The average difference-in-differences treatment effect, which is small in absolute terms, should be interpreted with a number of considerations in mind. First, the period of observation is relatively short, 2000-14. The median year of construction for treated villages is 2007 and thus the post-construction period for the typical village is 5-6 years once construction time is taking into account (results are robust to assumptions of 0-3 years of construction time)[22]. Given that tree regrowth is slow, and that households and firms take some time to respond to price and wage changes, estimates of tree cover change that were much greater than those observed would be very surprising. Treatment effects should also be considered in the context of low baseline tree cover in India: estimates represent a relative change of 0.6 to 1.2 percent of initial tree cover, above underlying trends. It should also be noted that VCF is a relatively conservative measure. For comparison, the VCF data has baseline average tree cover of 11.4 percent nationwide compared to 23.8 percent reported by the Forest Resources Assessment (FAO, 2015). The latter is based on a combination of field survey data from the Indian government and Landsat satellite imagery. This does not pose a threat to the validity of findings given that the satellite imagery and processing is consistent across space and time, but it does imply that these estimates of tree cover change are also conservative.

[22] Program rules specified that construction was to be completed within one year or within two years if difficult engineering works were required, following a road's approval date. The rules also specified financial penalties for contractors who did not meet these construction time requirements (Ministry of Rural Development, 2012a).

Given the economic and geographic diversity of India, the results in different settings are arguably at least as important as the national average treatment effect. The key difference observed – simultaneous positive and negative road impacts in different settings – is consistent with the conceptual model I propose based on differential changes in the value product of labor across economic sectors (agricultural versus non-agricultural, for instance). While Indian forests are not, in general, frontier forests in any way like those of South America (where much of the prior research on the drivers of deforestation has focused) some Indian settings arguably have more frontier-like characteristics than others. Class 2B, in particular, is more distant from urban centers, is less agriculturally developed, and has more initial forest cover. Negative road impacts are evident here. Opportunities for forest conversion are likely relatively high, and opportunities for local economic transitions away from forest-impacting activities potentially less. By contrast, class 1 is closer to urban centers, has better infrastructure and services (although it should be noted that these villages are not necessarily wealthier), and has less initial tree cover. Economic opportunities from further clearing are likely limited, and opportunities from forest gain or from activities associated with reduced forest pressure are likely greater.

The positive average treatment effect I find suggests that road construction is contributing to India's forest transition – the expansion of a country's forest cover following an extended period of deforestation (Mather, 1992). Forest transitions literature generally describes the forest transition phenomenon as proceeding in stages: (1) a period of initial high forest cover and low deforestation (i.e. a period in which there are large tracts of undeveloped land); (2) a period of high deforestation as agriculture expands in response to increased demand and population growth; (3) a period of forest cover stabilization as agricultural development reaches the limits of land suitability and intensification reduces land demand via productivity increases; and finally, (4) a reforestation period as demand increases for forest products and urbanization further shrinks the agricultural labor force (Angelsen, 2007; Barbier et al., 2010; Rudel et al., 2005) (Figure 1-13). My average treatment effect suggests roads contribute to the processes driving the upswing in the fourth stage.

Additionally, my setting-specific finding, of simultaneous forest loss and forest gain in different locations, is consistent with a spatially differentiated conception of forest transitions in which economic and agricultural development proceed at different paces in different settings. I suggest that roads may contribute to both parts of the forest transitions curve: deforestation (the initial downward trajectory in countries' forest cover) and subsequent reforestation (the post-transition upswing in forest cover). While the existing literature describes countries' forest cover change as a temporal rather than a spatial phenomenon (as noted by Angelsen, 2007), differences in development pace implies that the stages of the forest transition are likely occurring simultaneously in different settings. Hence, while forest transitions unfold over decades or centuries, i.e., over periods longer than the 15 years of data available for this study, differences in settings allow me to see road impacts at what are arguably distinct stages of the forest transition. Arguably, more frontier-like conditions, such as those seen in class 2B, approximate stages 1-3 above, while the more agriculturally developed conditions approximate stage (4)[23]. To summarize, my results suggest that roads, as both a cause and a consequence of the economic development underlying forest transitions, contribute to forest transitions in the upswing (stage 4). To the extent the spatial variation proxies for temporal change, these results also demonstrate roads' contribution to the loss of tree cover, i.e. stages 1-2 of the forest transitions model. Where they do the latter,

[23] In the case of India these distinctions are not enormous; I observe negative trends in tree cover in only the top decile of baseline tree cover, indicating that a large majority of India is either not changing or in the upswing of the forest transitions curve. Average tree cover change in class 2B is positive although lower than that in class 1.

their impact is consistent with conclusions in the existing deforestation literature, which documents strong negative road impacts in frontier settings (Pfaff, 1999; Rudel et al., 2009).

Consideration of institutional heterogeneity is not currently part of this analysis, although this omission should not threaten identification. Over the last 30 years, India's forest policies have featured devolution of state power through joint forest management, state-driven reforestation programs, and restitution of forest rights, among other large-scale initiatives (Agrawal and Ostrom, 2016; Kumar and Kerr, 2012; Mather, 2007). Road impacts are almost certainly modified by the legacy of differential implementation of these programs across regions. In addition, new roads likely facilitate policy implementation. Roads may allow for better enforcement of state property rights, or improved state support for local enforcement of community rights, through improved access for forest officers (Ghate et al., 2009). Alternatively, increased access could lead to conflict over previously isolated resources. The institutional setting, such as the tenure regime under which different forests are managed, is also likely to modify the road-forest relationship. Open-access forests face increasing pressure under higher forest products prices. Commonly-held forests face similar pressures but not necessarily the same outcome: improved collective action that conserves forest may be incentivized under these conditions (Agrawal and Yadama, 1997). Private property rights, meanwhile, should allow landowners to capitalize on increasing forest products prices, encouraging the protection of existing forests or the establishment of new plantations (Robinson et al., 2013). Some of these institutional considerations will be the focus of future analysis.

1.7. Conclusion

These empirics first suggest that at least part of the rise in India's tree cover, and thus India's forest transition, can be attributed to the construction of new roads in rural settings. I estimate a positive causal impact for India overall, on the order of 0.6 to 1.2 percent relative to baseline tree cover, above underlying trends across the study period (2000-14). While previous studies provided indirect or theoretical support for the possibility of positive road impacts (Deng et al., 2011; Foster and Rosenzweig, 2003), this is the first direct evidence of such to my knowledge. While small in absolute terms, these estimates of positive tree cover outcomes from new roads apply to a relatively short period. Should trends continue the impact of the program would compound over time. I note, however, that countervailing general equilibrium effects could mitigate the long-term impact[24].

Regardless of the specific magnitude, this positive average treatment effect runs contrary to the conventional wisdom that new roads drive deforestation. This is not to say that the previous understanding, based on substantial empirical evidence and well-developed theory, is erroneous, or that concern regarding the environmental impact of road construction (e.g. Caro et al., 2014; Laurance et al., 2014b) is unfounded. Nor does it mean that the net provision of environmental services must be positive from expanded tree cover, even in this study location[25]. Instead, the results

[24] For example, there may be a localized price rise for plantation timber following connection of a village to the road network. If many villages experience the same rise, and respond in concert by establishing new plantations, eventually the price of plantation goods will fall again, mitigating further incentives for new plantations. However, the equilibrium local price, and thus the equilibrium level of plantations, should still be higher than the initial price (how long that takes and whether such an equilibrium is meaningful, given many other contemporaneous economic changes, is another matter).

[25] Environmental benefits of tree cover increase are not quantified in this study. Tree cover increases may occur due to new plantations, reforestation on previously cleared land, or densification of existing forest. While these secondary forests many contribute to hydrological services and carbon sequestration, their biodiversity benefits are typically much lower than those of primary forests (Barlow et al., 2007). In the context of my finding of opposing impacts in different settings, it is possible that higher value forests are being lost while (more expansive) low value forests are being gained. Thus despite the net positive impact in total tree cover, environmental services could be diminished due to differences in forest quality. Even within a particular positively-impacted locality, secondary forest gains could come partially at the expense of higher value primary forests. Similarly, secondary forests may have adverse impacts on water tables (Jackson et al., 2005). Data limitations mean that ecological and hydrological impacts are beyond the scope of this study.

simply demonstrate that particular settings, which have not been the subject of much prior scholarship, can give rise to positive road impacts, raising the possibility that improved spatial targeting of roads can not only avoid environmental costs but also deliver environmental benefits. I present evidence regarding the nature of settings in which this occurs in my heterogeneity analysis. The fact that I observe negative road impacts in frontier-like settings helps reconcile my results with the broader deforestation literature, which has focused on frontier-like settings (primarily in central and South America). However, in India, where forest cover is rising overall, negative road impacts on frontier settings are outweighed by positive road impacts in other settings.

I present models of potential mechanisms, and reduced-form results consistent with these models' predictions, but do not present evidence for the mechanisms themselves. It is possible that the relative impact of the potential mechanisms (described in section 0) can be causally explored with additional data, but doing so is beyond the scope of this paper. Additionally, neither deforestation data nor forest cover data distinguishes between natural forests and plantations, and past studies suggest that plantations are particularly important in India's land cover change (Foster and Rosenzweig, 2003; Mather, 2007; Rudel et al., 2005). Insights on mechanisms will be the focus of future extensions to this work. A second line of extension will be to quantify two further types of heterogeneity with important policy implications: ecological quality, and socio-economic impacts. The environmental benefits and costs of new road would be better revealed by forest quality metrics such as biodiversity, forest structure, and carbon sequestration, in addition to simple tree cover (see footnote 25). Secondly, socio-economic impacts of roads vary as a function of setting also. Quantifying over space both types of variation – environmental quality and socio-economic impacts – reveals settings that lead to environmental-social trade-offs, and synergies, from rural road construction.

1.8. Figures

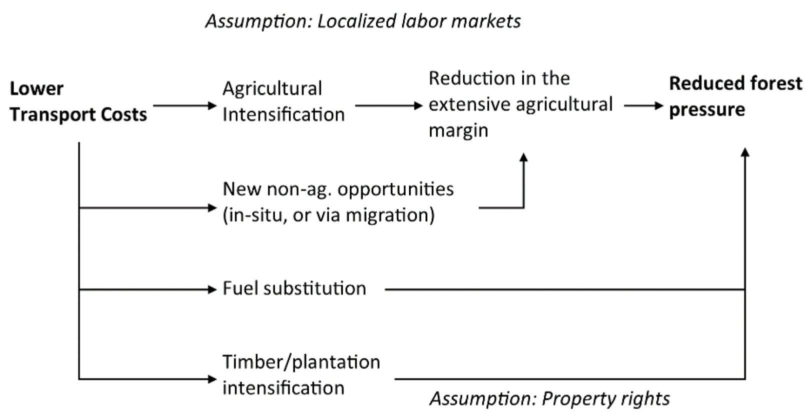


Figure 1-1: Hypothesized relationships between roads (lower transport costs) and land cover change.

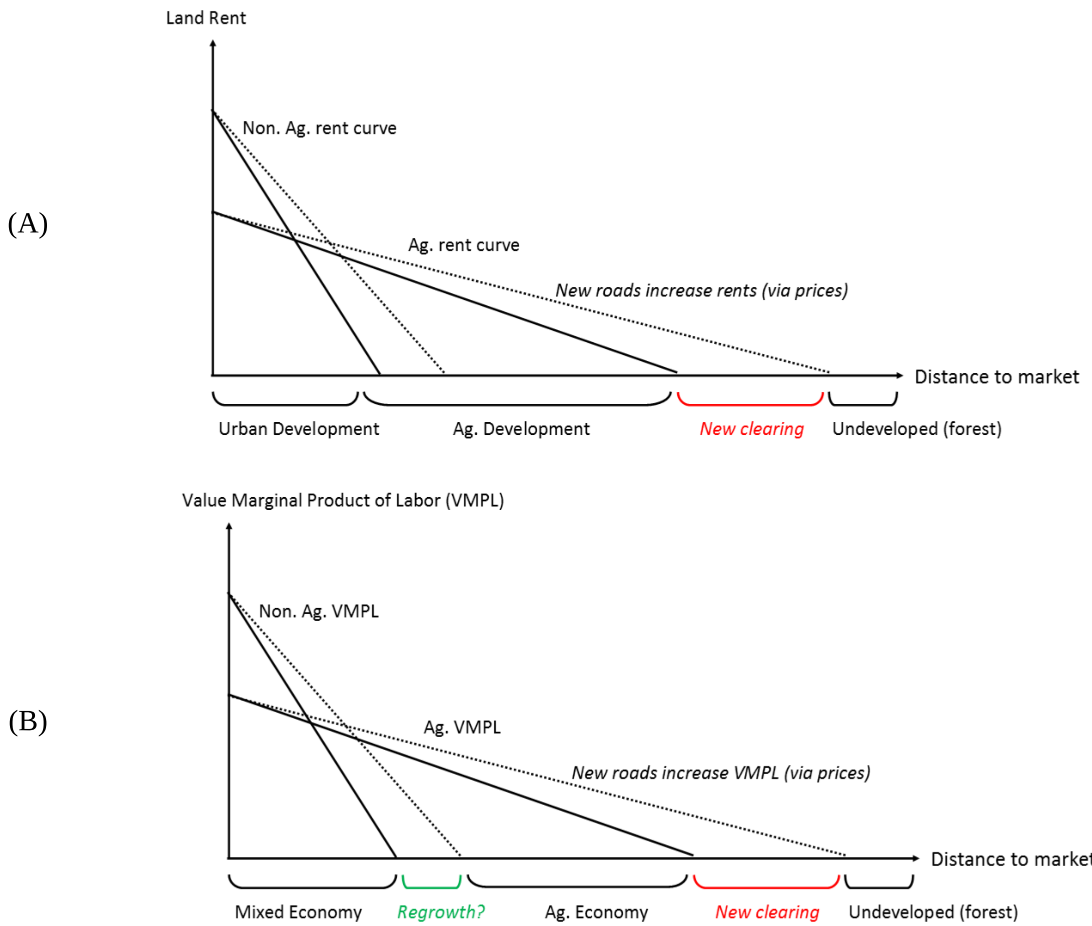


Figure 1-2: Models of land use as a function of distance to market. (A) Traditional von Thünen model. New roads increase land rents, increasing clearing on the frontier where agriculture becomes profitable. (B) Altered von Thünen model: new roads increase the value marginal product of labor, encouraging a move of labor towards the agricultural sector in frontier areas, and a move towards the non-agricultural sector in non-frontier areas (due to differential change in profitability across sectors).

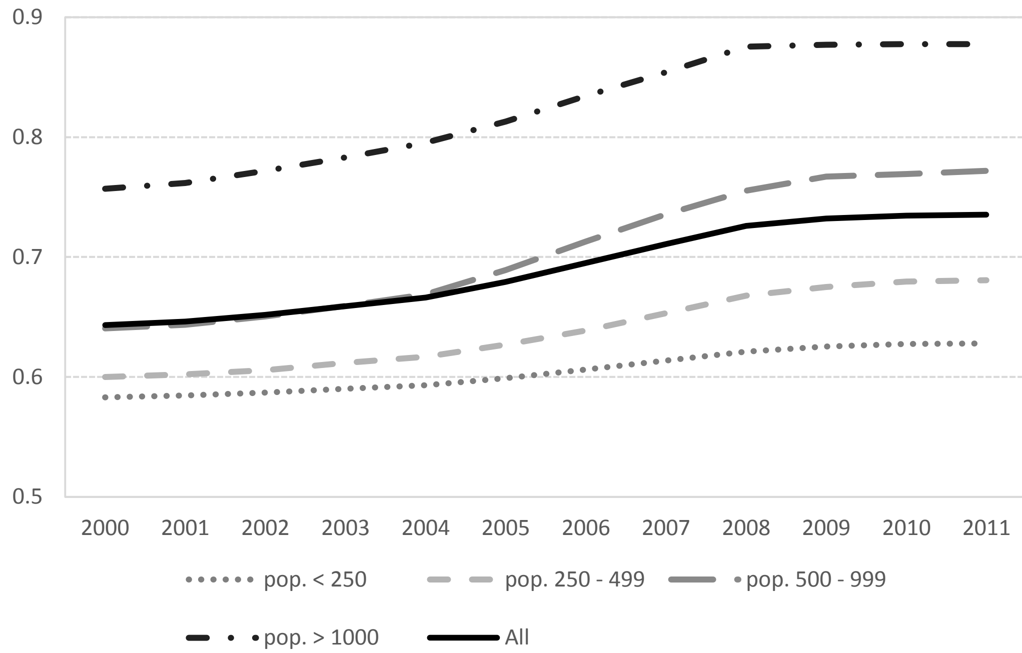


Figure 1-3: Cumulative probability of a habitation receiving a new road by population category over time (all habitations included, those connected and unconnected in baseline).

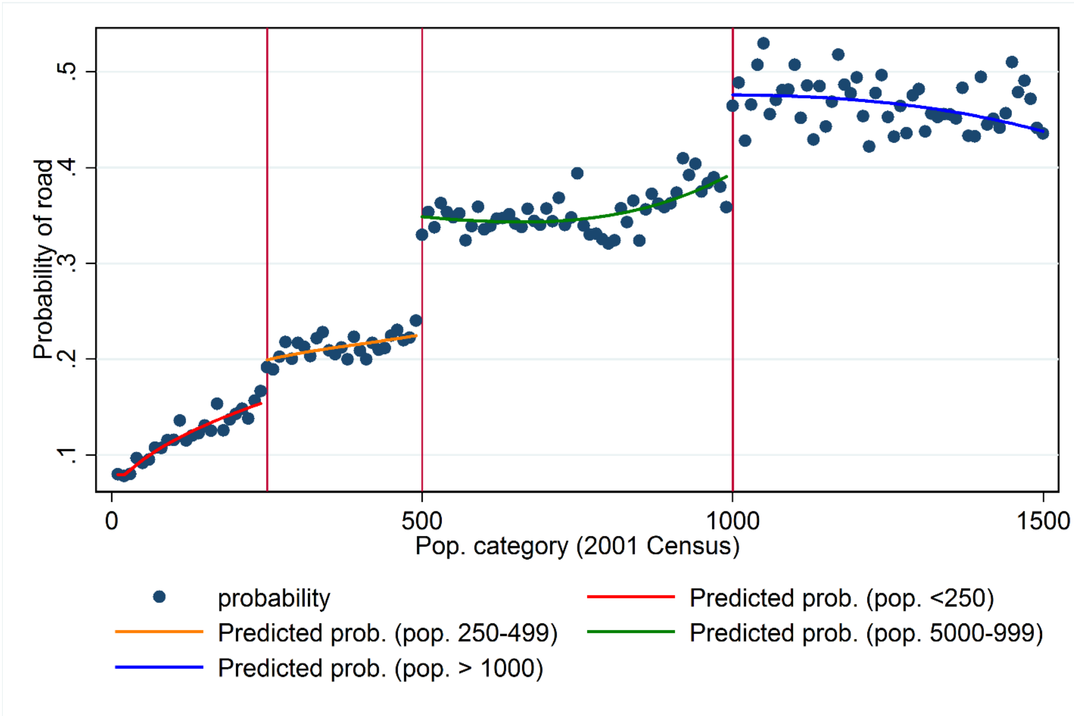


Figure 1-4: Probability of receiving a PMGSY road project by habitation populations using Census 2001 population data (note: zero population habitations removed, 99th percentile and above removed, only villages with one habitation can be considered, villages unconnected in baseline only).

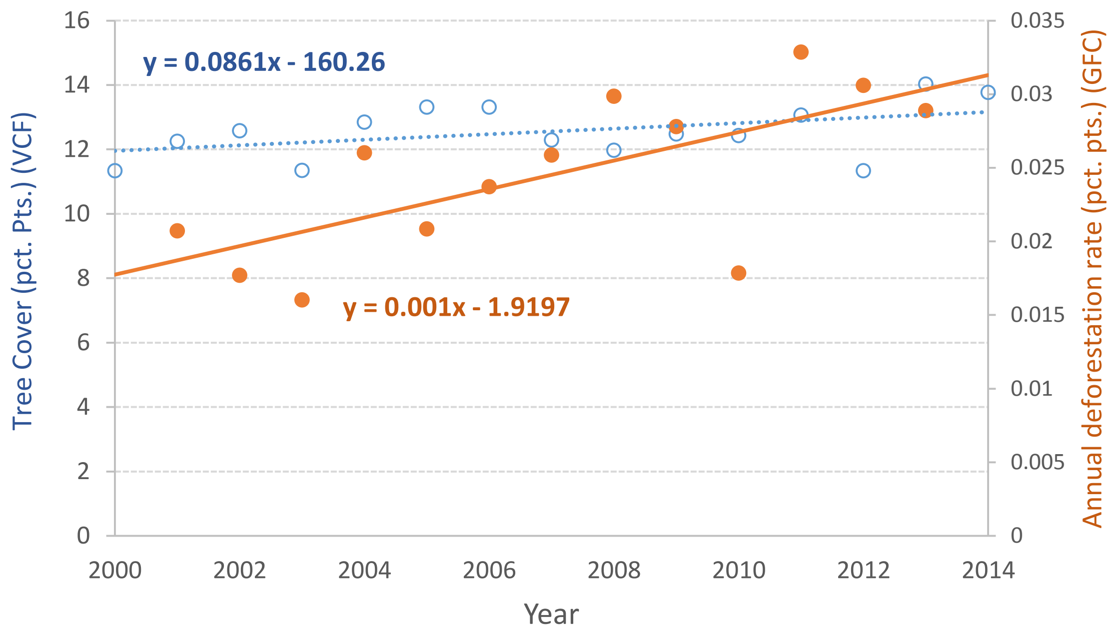


Figure 1-5: Forest cover level (MOD44B v.51, Vegetation Continuous Fields, Townshend, et al. 2011) (dash line, hollow data points) and annual deforestation rate (Global Forest Change, Hansen, et al. 2013) (solid line, colored data points) for India, 2000-14. Linear trends with equations included on graph. Data points calculated using pixel level data, giving percentages as a function of total India land area.

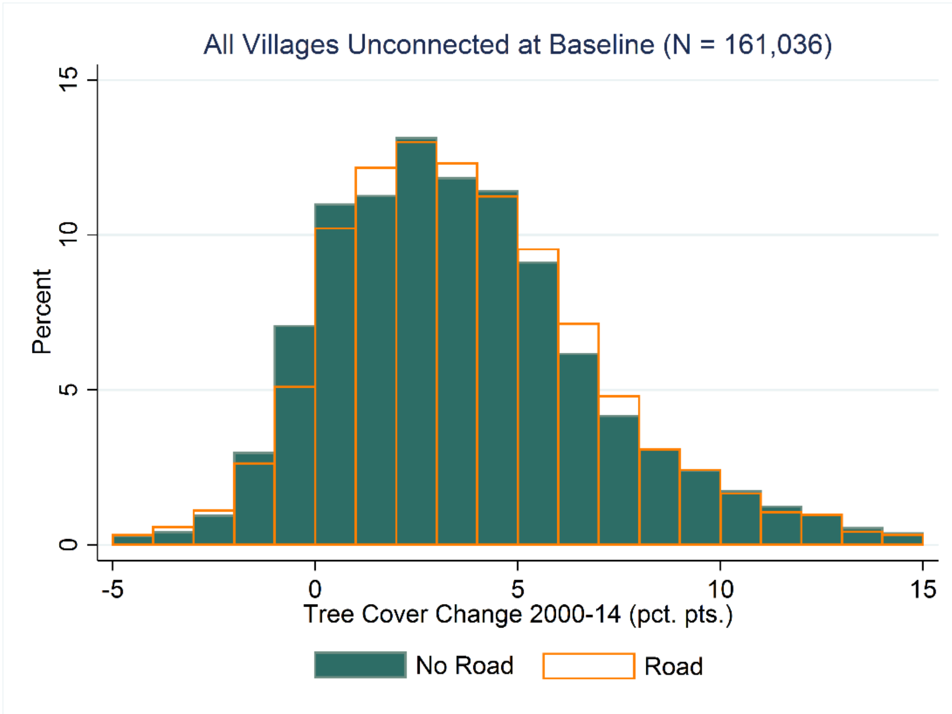


Figure 1-6: Overlaid histograms of tree cover change between baseline and endline. It should be noted that the MOD44B Vegetation Continuous Fields data contains annual fluctuations, and so the absolute changes represented here are a partially a function of the anomalies in the two years used to calculate the change (2000 and 2014).

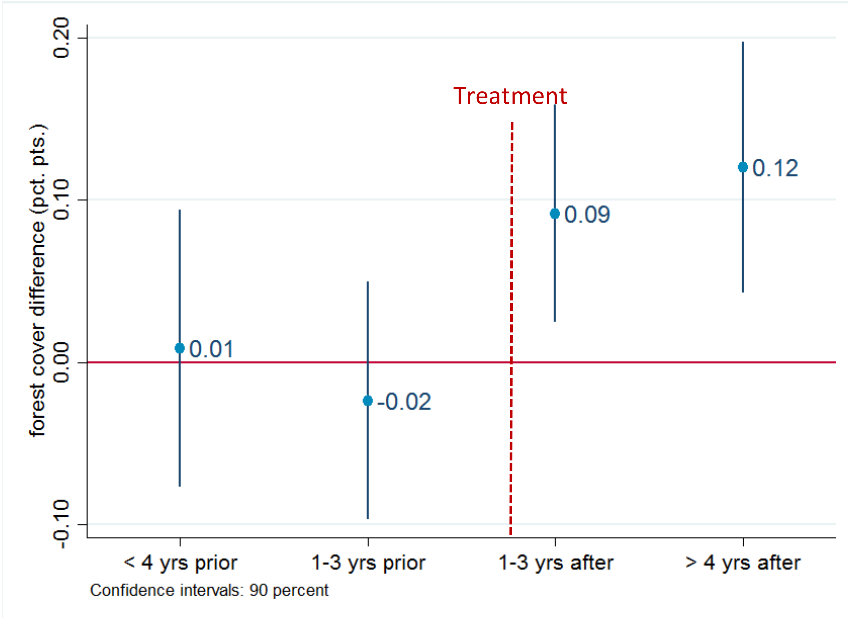


Figure 1-7: Event study plot, average treatment effects for periods before and after treatment for all unconnected villages at baseline (N = 161,036).

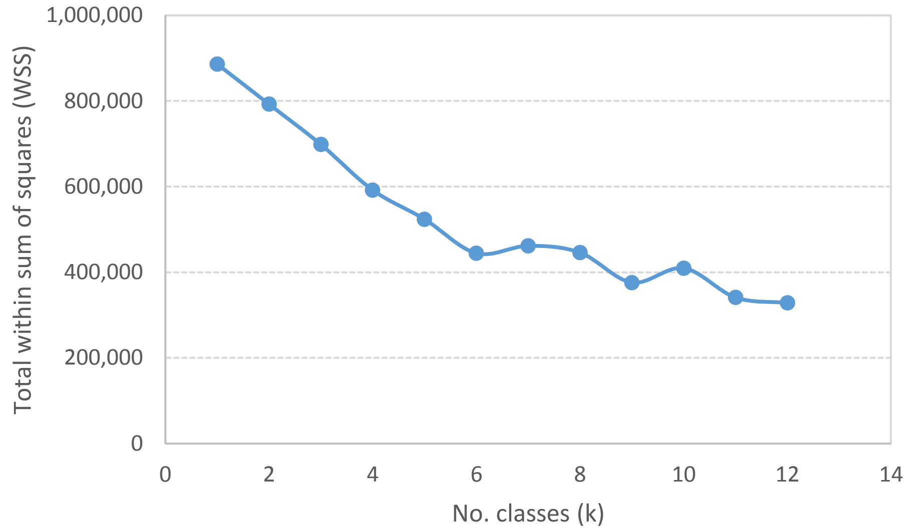


Figure 1-8: Total within sum of squares as a function of the number of classes.

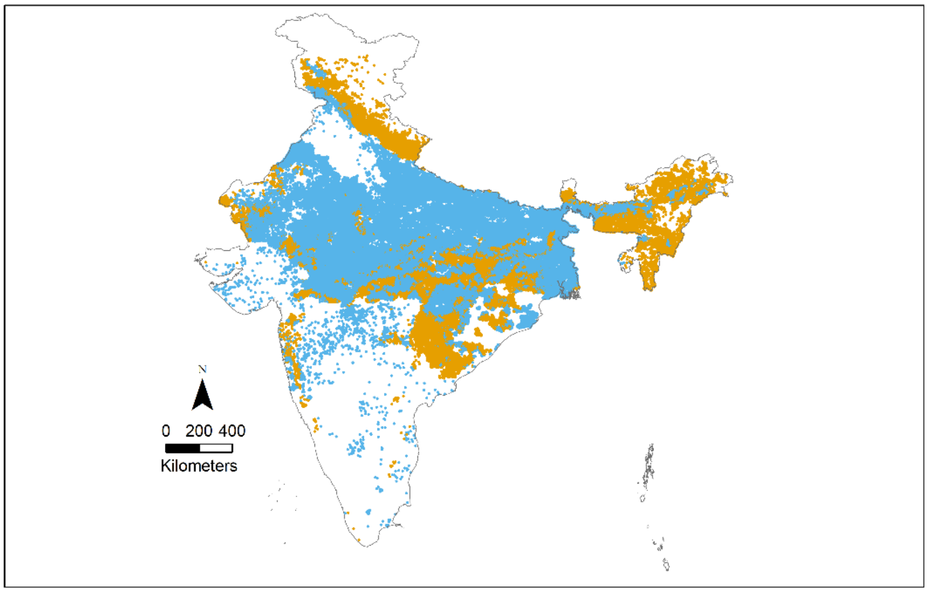


Figure 1-9: Location of villages in each class (k=2 partitions): Class 1 = blue, Class 2 = orange.

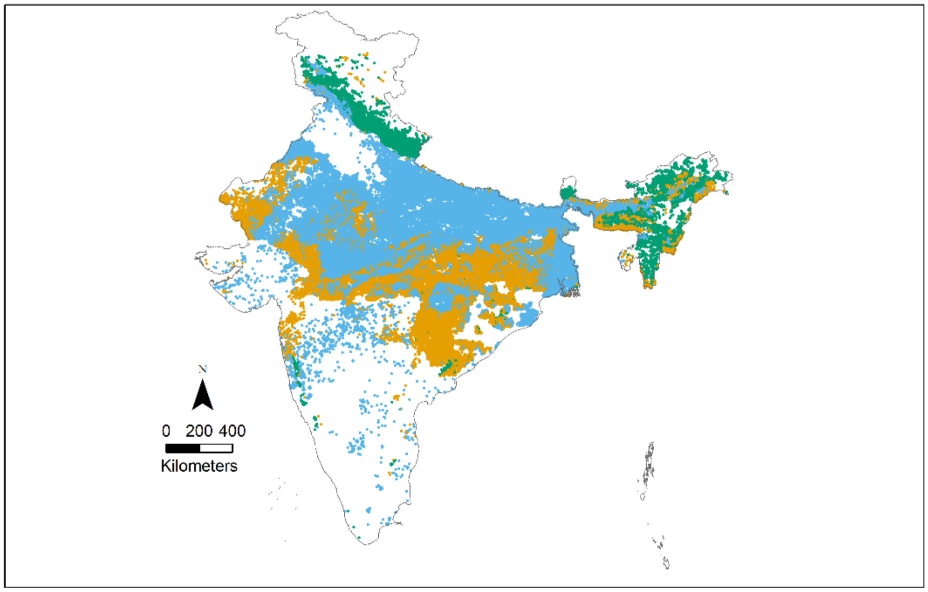


Figure 1-10: Location of villages in each class (k=3 partitions): Class 1 = blue, Class 2A = orange, Class 2B = green.

Class 1

Class 2B

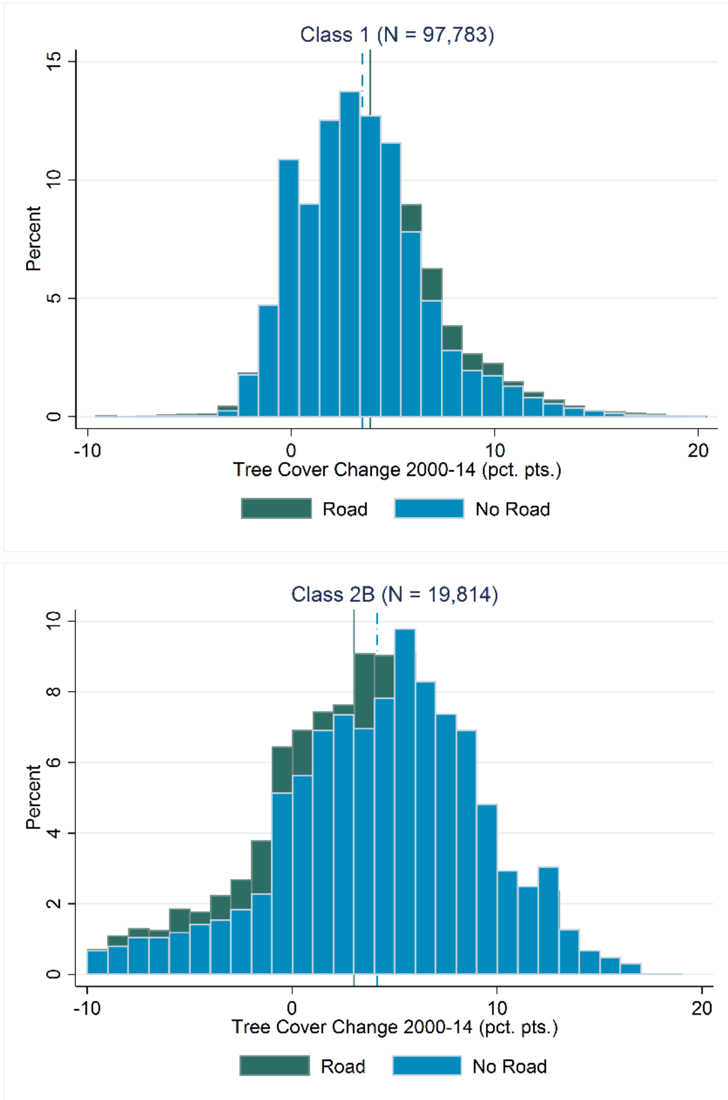


Figure 1-11: Overlaid histograms of tree cover change between baseline and endline for class 1 and class 2B villages of the 3 class partition (the two most opposite classes in terms of road impacts). It should be noted that the MOD44B Vegetation Continuous Fields data contains annual fluctuations, and so the absolute changes represented here are a partially a function of the anomalies in the two years used to calculate the change (2000 and 2014).

Class 1

Class 2A

Class 2B

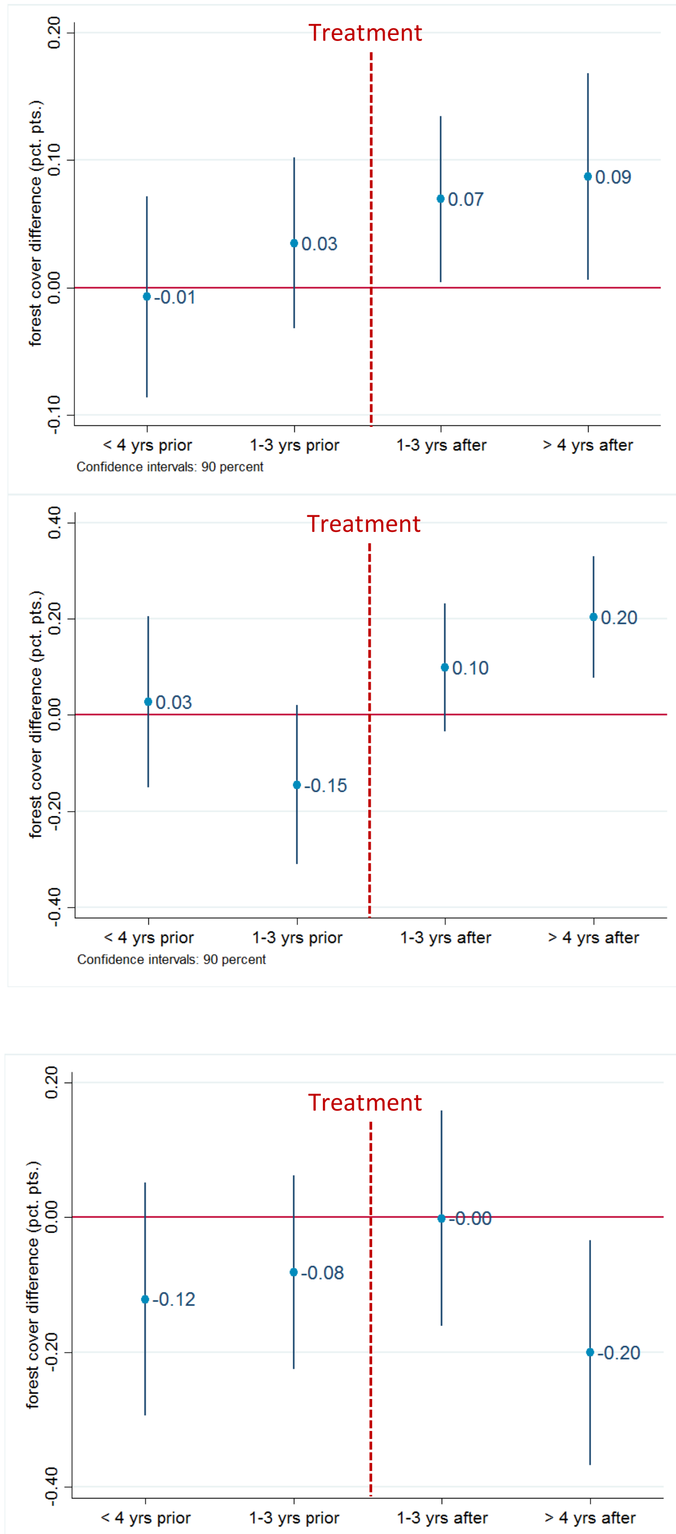


Figure 1-12: Event study plot. Average treatment effects for periods before and after treatment for classes in the 3-class partition. All villages unconnected at baseline.

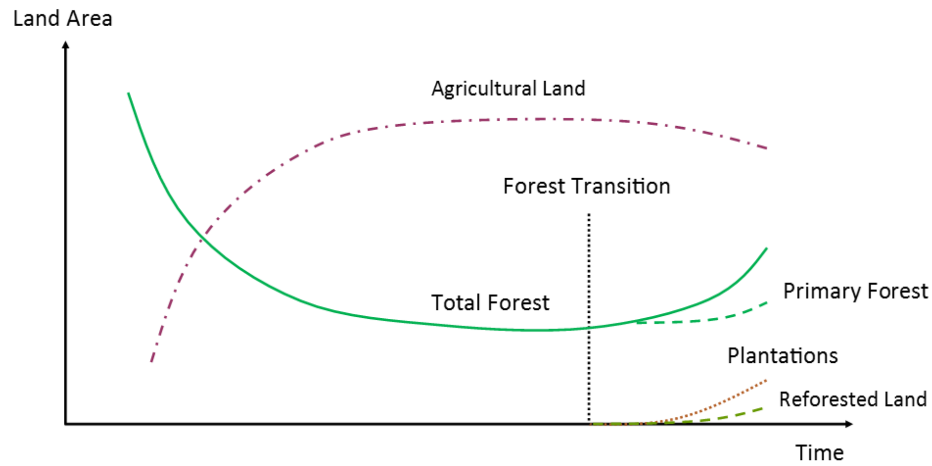


Figure 1-13: The forest transitions hypothesis. Forest cover falls and then rises over time as economic and agricultural development proceeds (adapted from Barbier, *et al.* 2010).

1.9. Tables

Table 1-1: Summary statistics (mean, standard deviation in parentheses): All villages (2001 census) by baseline road access status (connected or unconnected with a paved road).

	Unconnected (2001)	**Connected (2001)**	**All**
Proportion of villages	0.34	0.66	1.00
Population	817	1762	1439
	(1013)	*(2298)*	*(2006)*
Population growth (2001-11) (percent)	0.25	0.19	0.21
	(2.77)	*(1.39)*	*(1.99)*
Scheduled tribe population (avg. proportion)	0.19	0.13	0.15
	(0.35)	*(0.27)*	*(0.30)*
Electricity connection (proportion of villages)	0.57	0.90	0.79
	(0.49)	*(0.30)*	*(0.41)*
Credit Society (proportion of villages)	0.07	0.27	0.20
	(0.25)	*(0.45)*	*(0.40)*
Primary School (proportion of villages)	0.69	0.89	0.82
	(0.46)	*(0.32)*	*(0.39)*
Primary health center (proportion of villages)	0.06	0.23	0.17
	(0.24)	*(0.42)*	*(0.38)*
Distance to nearest town (kilometers)	26.75	20.38	22.56
	(26.18)	*(20.19)*	*(22.63)*

Table 1-2: Road connections at habitation and village level.

Statistic	Unit	
Number:	Habitations	754 813
	Villages	458 560
	Sub-districts	5 310
	Districts`	564
Average population:	Habitation[2]	824
	Village[2]	2 992
Median population:	Habitation[2]	474
	Village[2]	1 335
Average Number:	Habitations per village[2]	1.65
	Villages per sub-district	86.36
	Sub-districts per district	9.41
Median Number:	Habitations per village	2
	Villages per sub-district	56
	Sub-districts per district	7
Connected at baseline[1] (proportion):	Habitations	0.64
	Villages – partial or complete [3]	0.66
Connected at baseline[1] (proportion):	Villages - complete (all habitations within village) [2]	0.57
	Villages - partial (some habitations within village) [2]	0.06
	Villages - none (no habitations within village) [2]	0.37

[1]: Baseline = 2000

[2]: PMGSY data (habitation level)

[3]: Census 2001 data (village level)

Table 1-3: Road connections over time under the PMGSY program.

Year	Proportion of villages with road connection	
Sample:	All	Unconnected at baseline
2000	63.2%	0.0%
2001	63.5%	0.8%
2002	64.2%	2.6%
2003	64.2%	2.6%
2004	65.1%	5.0%
2005	66.0%	7.5%
2006	67.6%	11.8%
2007	69.6%	17.3%
2008	71.4%	22.4%
2009	73.4%	27.6%
2010	74.2%	29.8%
2011	74.4%	30.4%
2012	74.5%	30.6%
Mean (across years)	69.3%	16.7%
No. villages in sample	437,820	161,036

Table 1-4: OLS panel regression with village-level fixed effects. Sample comprises all villages unconnected at baseline.

Dep. Var: tree cover (MOD44B) (10km) (pct. pts)			**All villages unconnected at baseline**			
Road connection (binary)	0.122***	0.060***				
	(0.034)	*(0.019)*				
Road connection (vil. pop. proportion)			0.128***	0.061***		
			(0.036)	*(0.020)*		
Road connection (vil. pop. sum) (‘000)					0.067***	0.016*
					(0.021)	*(0.009)*
R-square	0.97	0.98	0.97	0.98	0.97	0.98
Dep. Var: tree cover (MOD44B) (5km) (pct. pts)			**All villages unconnected at baseline**			
Road connection (binary)	0.113***	0.053**				
	(0.035)	*(0.021)*				
Road connection (vil. pop. proportion)			0.117***	0.051**		
			(0.036)	*(0.021)*		
Road connection (vil. pop. sum) (‘000)					0.062***	0.014
					(0.022)	*(0.010)*
R-square	0.97	0.98	0.97	0.98	0.97	0.98
State*Year FEs	Yes	No	Yes	No	Yes	No
District*Year FEs	No	Yes	No	Yes	No	Yes
No. obs.	2,093,468	2,093,234	2,093,468	2,093,234	2,093,468	2,093,234
No. Villages	161,036	161,018	161,036	161,018	161,036	161,018

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedasticity-robust standard errors clustered on district

Two year lead time is allowed for road construction

Standard errors in parentheses

Table 1-5: OLS panel regression with village-level fixed effects. Sample comprises all villages.

Dep. Var: tree cover (MOD44B) (10km) (pct. pts)	**All villages (connected and unconnected at baseline)**					
Road connection (binary)	0.087***	0.039**				
	(0.032)	*(0.016)*				
Road connection (vil. pop. proportion)			0.094***	0.041**		
			(0.033)	*(0.017)*		
Road connection (vil. pop. sum) ('000)					0.048***	0.005
					(0.018)	*(0.007)*
R-square	0.96	0.98	0.96	0.98	0.96	0.98
Dep. Var: tree cover (MOD44B) (5km) (pct. pts)	**All villages (connected and unconnected at baseline)**					
Road connection (binary)	0.079**	0.031*				
	(0.031)	*(0.018)*				
Road connection (vil. pop. proportion)			0.085***	0.031*		
			(0.033)	*(0.019)*		
Road connection (vil. pop. sum) ('000)					0.044**	0.004
					(0.018)	*(0.008)*
R-square	0.95	0.97	0.95	0.97	0.95	0.97
State*Year FEs	Yes	No	Yes	No	Yes	No
District*Year FEs	No	Yes	No	Yes	No	Yes
No. obs.	6,567,300	6,567,285	6,567,300	6,567,285	6,567,300	6,567,285
No. Villages	437,820	437,819	437,820	437,819	437,820	437,819

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedasticity-robust standard errors clustered on district

Two year lead time is allowed for road construction

Standard errors in parentheses

Table 1-6: OLS panel regression with village-level fixed effects. Sample comprises all villages unconnected at baseline and treated during program.

Dep. Var: tree cover (MOD44B) (10km) (pct. pts)			**Treated villages only**			
Road connection (binary)	0.118***	0.054**				
	(0.041)	*(0.024)*				
Road connection (vil. pop. proportion)			0.127***	0.060**		
			(0.041)	*(0.025)*		
Road connection (vil. pop. sum) ('000)					0.034*	-0.003
					(0.018)	*(0.009)*
R-square	0.97	0.98	0.97	0.98	0.97	0.98
Dep. Var: tree cover (MOD44B) (5km) (pct. pts)			**Treated villages only**			
Road connection (binary)	0.103**	0.044*				
	(0.040)	*(0.026)*				
Road connection (vil. pop. proportion)			0.108***	0.045*		
			(0.040)	*(0.026)*		
Road connection (vil. pop. sum) ('000)					0.03	-0.006
					(0.018)	*(0.010)*
R-square	0.96	0.98	0.96	0.98	0.96	0.98
State*Year FEs	Yes	No	Yes	No	Yes	No
District*Year FEs	No	Yes	No	Yes	No	Yes
No. obs.	740,025	739,815	740,025	739,815	740,025	739,815
No. Villages	49,335	49,321	49,335	49,321	49,335	49,321

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedasticity-robust standard errors clustered on district

Two year lead time is allowed for road construction

Standard errors in parentheses

Table 1-7: OLS regression of tree cover trend during early years of program on treatment (receiving a road at any time during the program).

Period:	**2000-05**				**2000-04**				**2000-03**			
Tree Cover Trend	0.058***	0.035**	0.000	0.001	0.043***	0.016	-0.010	-0.007	0.036***	0.015	-0.005	-0.007
	(0.013)	*(0.014)*	*(0.012)*	*(0.012)*	*(0.012)*	*(0.012)*	*(0.010)*	*(0.01)*	*(0.009)*	*(0.009)*	*(0.007)*	*(0.007)*
Constant	0.279***	0.146***	0.224***	0.159**	0.289***	0.151***	0.229***	0.160***	0.300***	0.154***	0.225***	0.162**
	(0.015)	*(0.023)*	*(0.066)*	*(0.063)*	*(0.014)*	*(0.023)*	*(0.064)*	*(0.062)*	*(0.013)*	*(0.022)*	*(0.066)*	*(0.062)*
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
State FEs	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
District FEs	No	No	No	No	No	No	No	No	No	No	No	No
Adjusted R-square	0.01	0.06	0.08	0.11	0	0.06	0.08	0.11	0.01	0.06	0.08	0.11
No. Villages	161,036	137,818	161,036	137,818	161,036	137,818	161,036	137,818	161,036	137,818	161,036	137,818

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedasticity-robust standard errors clustered on district

Dep. Var: Change in MOD44B v.51 over period specified above

Controls: village population, scheduled tribe population proportion, primary school, primary health center, area of irrigated land, distance to nearest urban center, power supply, average slope, initial forest cover.

Table 1-8: Event study analysis. OLS panel regression with village-level fixed effects. Independent variables are periods before and after treatment. Sample comprises all villages unconnected at baseline.

Dep. Var: tree cover (MOD44B) (10km) (pct. pts)	**All villages**	**Class 1**	**Class 2A**	**Class 2B**
Road connection (binary): ≥ 5 years prior	0.008	-0.007	0.028	-0.121
	(0.052)	*(0.051)*	*(0.108)*	*(0.104)*
Road connection (binary): 1-4 years prior	-0.024	0.035	-0.145	-0.082
	(0.044)	*(0.043)*	*(0.101)*	*(0.087)*
Road connection (binary): 1-4 years after	0.092**	0.069*	0.099	-0.001
	(0.041)	*(0.042)*	*(0.081)*	*(0.096)*
Road connection (binary): ≥ 5 years after	0.120**	0.087*	0.203***	-0.201**
	(0.047)	*(0.052)*	*(0.077)*	*(0.101)*
Adjusted R-square	0.97	0.91	0.93	0.97
State*Year FEs	Yes	Yes	Yes	Yes
No. obs.	2,415,540	1,466,730	605,190	297,195
No. Villages	161,036	97,782	40,346	19,813

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedacity-robust standard errors clustered on district

Standard errors in parentheses

Table 1-9: Instrumental variable regressions (2SLS) on village cross-section. First stage dependent variable is binary road connection (whether village received a road at any point during study period). Sample comprises all villages unconnected at baseline.

Threshold (IV):	**T = 1000**		**T = 500**	
Pred. road connection	1.209*		-0.441	
	(0.701)		*(0.395)*	
Pred. road connection * Class 1		1.249*		-0.191
		(0.646)		*(0.545)*
Pred. road connection * Class 2A		0.599		-0.493
		(1.356)		*(0.346)*
Pred. road connection * Class 2B		3.223		-1.999**
		(2.270)		*(0.800)*
Controls	Yes	Yes	Yes	Yes
Population controls	Yes	Yes	Yes	Yes
Class FEs	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Adjusted R-square	0.6	0.59	0.6	0.59
No. villages	21,581	21,581	52,042	52,042

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedacity-robust standard errors clustered on district

Dep. Var: Change in MOD44B.v51 (2000-14)

Bandwidth: T +/- 250

Table 1-10: Mean standardized values of baseline descriptive variables for village classes (2 and 3 class partitions). Colors highlight signs and magnitudes (blue are above average, orange are below average).

	K = 2 class partition		**K = 3 class partition**		
	Class 1	Class 2	Class 1	Class 2A	Class 2B
Total Irrigated Area	0.10	-0.25	0.14	-0.19	-0.26
Forest Cover (2000)	-0.36	0.94	-0.33	-0.16	1.91
Crop Cover (2001)	0.44	-1.20	0.56	-0.75	-1.24
Electricity connection	0.06	-0.15	0.10	-0.30	0.13
Credit Society	0.06	-0.17	0.11	-0.20	-0.13
Primary School	0.08	-0.14	0.07	0.06	-0.29
Primary health center	0.03	-0.07	0.05	-0.08	-0.04
Distance to nearest town	-0.26	0.76	-0.31	0.57	0.50
Slope	-0.40	1.11	-0.42	-0.04	2.21
Scheduled tribe population	-0.26	0.72	-0.52	1.30	-0.07
Villages in class (total)	115,398	42,545	97,783	40,346	19,814
Villages in class (percent)	**73.06**	**26.94**	**61.91**	**25.54**	**12.55**

Table 1-11: OLS panel regression with village-level fixed effects and interaction terms on village class. Coefficients have been adjusted to give treatment effects relative to zero (i.e. no reference group required).

Dep. Var: tree cover (MOD44B) (10km) (pct. pts)	**All villages unconnected at baseline**		**Treated villages only**	
3 Class Partition				
Road connection * Class 1	0.054	0.109**	0.066	0.074
	(0.062)	*(0.048)*	*(0.066)*	*(0.047)*
Road connection * Class 2A	-0.370***	-0.242***	-0.484***	-0.292***
	(0.121)	*(0.063)*	*(0.163)*	*(0.098)*
Road connection * Class 2B	0.247***	0.095***	0.286***	0.125***
	(0.046)	*(0.021)*	*(0.053)*	*(0.031)*
R-square	0.97	0.98	0.97	0.98
2 Class Partition				
Road connection * Class 1	0.229***	0.104***	0.259***	0.124***
	(0.044)	*(0.021)*	*(0.052)*	*(0.031)*
Road connection * Class 2	-0.157**	-0.062	-0.172**	-0.093
	(0.074)	*(0.050)*	*(0.085)*	*(0.057)*
R-square	0.97	0.98	0.97	0.98
State*Year FEs	Yes	No	Yes	No
District*Year FEs	No	Yes	No	Yes
No. obs.	2,053,259	2,053,038	630,396	630,227
No. Villages	157,943	157,926	48,492	48,479

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedacity-robust standard errors clustered on district

Two year lead time is assumed for road construction

Standard errors in parentheses

Table 1-12: OLS panel regression with village-level fixed effects and interaction terms on village class. Coefficients have been adjusted to give treatment effects relative to zero (i.e. no reference group required).

DV: annual deforestation (Hansen, et al. 2013) (pct. pts)	**All villages unconnected at baseline**				**All villages unconnected at baseline (with some deforestation)**			
Road connection	-0.0004		-0.00047		-0.00053		-0.00066	
	(0.00068)		*(0.00055)*		*(0.00098)*		*(0.00077)*	
Road connection * Class 1		-0.00178***		-0.0005		-0.00273***		-0.00073
		(0.00067)		*(0.00033)*		*(0.00104)*		*(0.00052)*
Road connection * Cluster 2A		-0.00018		-0.00157		-0.00043		-0.00216
		(0.00157)		*(0.00171)*		*(0.00212)*		*(0.00220)*
Road connection * Class 2B		0.00594*		0.00222		0.00586*		0.00221
		(0.00326)		*(0.00186)*		*(0.00326)*		*(0.00187)*
Adjusted R-square	0.67	0.67	0.73	0.73	0.66	0.66	0.72	0.72
State*Year FEs	Yes	Yes	No	No	Yes	Yes	No	No
District*Year FEs	No	No	Yes	Yes	No	No	Yes	Yes
No. obs.	1,932,180	1,895,316	1,931,964	1,895,112	1,279,308	1,254,024	1,279,020	1,253,748
No. Villages	161,015	157,943	160,997	157,926	106,609	104,502	106,585	104,479

* p<0.10, ** p<0.05, *** p<0.01

Heteroskedacitity-robust standard errors clustered on district

Two year lead time is assumed for road construction

Standard errors in parentheses

1.10. Supplementary Section: A Static Model of Household Labor Allocation

I consider decisions at the household level. Households have a fixed time budget,[27] L, which they may allocate to intensive agricultural activities (those not at the forest margin) l_{A1}, extensive agricultural activities (those at the forest margin or within forest areas) l_{A2}, non-agricultural production l_N, and firewood collection, l_F,. Growth in plantations is not considered in this model. To simplify the analysis I treat leisure time as fixed. Hence:

$$L = l_{A1} + l_{A2} + l_N + l_F \quad (1)$$

Firewood is not typically purchased in rural India (Veld, *et al.* 2006), and is consumed within the home. Firewood production, q_F, is a function of forest labor time, and is strictly increasing and concave: $q_F = l_F^{\beta}$, where $\beta < 1$. Non-forest energy (liquefied natural gas or kerosene), q_E, must be purchased at the fixed market price, p_E. Total energy consumption is represented by:

$$E = {l_F}^{\beta} + q_E \quad (2)$$

Agricultural labor produces agricultural goods via increasing and concave production functions: $q_{A1}(l_{A1}) = {l_{A1}}^{\alpha}$, and $q_{A2}(l_{A2}) = {l_{A2}}^{\beta}$, where $\alpha < 1$, $\beta < 1$, and $\alpha > \beta$ represent the higher labor productivity of intensive agricultural production. Equivalent assumptions apply to non-agricultural labor, $q_N(l_N) = {l_N}^{\alpha}$, which captures any other wage-earning activity performed by the household either in the village or outside (based on commuting or temporary migration). All outputs except firewood are sold at market prices p_{A1}, p_{A2}, and p_N. Prices are a function of transport cost only and are thus exogenous at the household level. The modelled effect of road construction is to increase or decrease prices, depending on whether the resulting

[27] This assumes labor markets are incomplete and households do not hire labor.

market linkages provide primarily export opportunities or import opportunities relative to the surrounding region. A household's cash income may thus be represented as:

$$Y = {l_{A1}}^{\alpha} p_{A1} + {l_{A2}}^{\beta} p_{A2} + {l_N}^{\alpha} p_N \tag{3}$$

Household income is spent on the consumption of market goods, q_X, and non-forest energy, q_E:

$$Y = q_E p_E + q_X p_X \tag{4}$$

Households' utility is a Cobb-Douglas function of market goods consumption, q_X, and energy consumption (of either market or forest origin), such that $U(q_X, E) = {q_X}^{\delta} E^{\gamma}$. The household chooses the utility-maximizing combination of variables $q_S, q_X, q_E, l_U, l_F, l_{A1}, l_{A2}$ and l_N, subject to constraints (1) through (3).

$$Max_{q_X, q_E, l_F, l_{A1}\, l_{A2}, l_N} : {q_X}^{\delta} E^{\gamma}$$

Subject to:

$$L = l_{A1} + l_{A2} + l_F + l_N$$

$$Y = {l_{A1}}^{\alpha} p_{A1} + {l_{A2}}^{\beta} p_{A2} + {l_N}^{\alpha} p_N$$

$$Y = q_E p_E + q_X p_X$$

$$E = {l_F}^{\beta} + q_E \tag{5}$$

Substituting constraint (2) into (3), and attaching Lagrangian multipliers gives:

$$\mathcal{L} = {q_X}^{\delta} E^{\gamma} + \lambda_1\left[\left({l_{A1}}^{\alpha} + {l_{A2}}^{\beta}\right)p_A + {l_N}^{\alpha} p_N - q_E p_E - q_X p_X\right] + \lambda_2\left[{l_F}^{\beta} + q_E - \mathrm{E}\right] + \lambda_3\left[l_{A1} + l_{A2} + l_F + l_N - L\right] \tag{6}$$

Which gives first order conditions at the optimum:

$$\frac{\partial \mathcal{L}}{\partial l_{A1}} = \lambda_3 + \lambda_2 \alpha {l_{A1}}^{\alpha-1} p_A = 0 \text{ (if } l_{A1} > 0) \tag{7}$$

$$\frac{\partial \mathcal{L}}{\partial l_{A2}} = \lambda_3 + \lambda_2 \beta {l_{A2}}^{\beta-1} p_A = 0 \text{ (if } l_{A2} > 0) \tag{8}$$

$$\frac{\partial \mathcal{L}}{\partial l_N} = \lambda_3 + \lambda_1 \beta \alpha {l_N}^{\alpha-1} p_N = 0 \text{ (if } l_N > 0) \tag{9}$$

$$\frac{\partial \mathcal{L}}{\partial l_F} = \lambda_3 + \lambda_2 \beta {l_F}^{\beta-1} = 0 \text{ (if } l_F > 0) \tag{10}$$

$$\frac{\partial \mathcal{L}}{\partial E} = \gamma {q_X}^{\delta} E^{\gamma-1} - \lambda_2 = 0 \text{ (if } E > 0) \tag{11}$$

$$\frac{\partial \mathcal{L}}{\partial q_X} = \delta {q_X}^{\delta-1} E^{\gamma} - \lambda_1 p_X = 0 \text{ (if } q_X > 0) \tag{12}$$

$$\frac{\partial \mathcal{L}}{\partial q_E} = -\lambda_1 p_E + \lambda_2 = 0 \text{ (if } q_E > 0) \tag{13}$$

Combining equations (7), (8) and (9) gives the marginal rates of transformation between agricultural and non-agricultural activities:

$$\frac{{l_{A2}}^{\beta-1}}{{l_{A1}}^{\alpha-1}} = \frac{\alpha p_{A1}}{\beta p_{A2}} \tag{14}$$

$$\frac{{l_{A1}}^{\alpha-1}}{{l_N}^{\alpha-1}} = \frac{p_N}{p_{A1}} \tag{15}$$

$$\frac{{l_N}^{\alpha-1}}{{l_{A2}}^{\beta-1}} = \frac{\beta p_{A2}}{\alpha p_N} \tag{16}$$

Combining these expressions with (10) gives expressions relating forest labor to other forms of labor:

$$\frac{\lambda_1}{\lambda_2} = \frac{1}{p_N} \frac{\beta}{\alpha} \frac{{l_F}^{\beta-1}}{{l_N}^{\alpha-1}} = \frac{1}{p_{A2}} \frac{{l_F}^{\beta-1}}{{l_{A2}}^{\beta-1}} = \frac{1}{p_{A1}} \frac{\beta}{\alpha} \frac{{l_F}^{\beta-1}}{{l_{A1}}^{\alpha-1}} \tag{17}$$

Rearranging (11) and (12) shows that these expressions, at the optimum, are equivalent to the marginal rate of substitution between consumed goods, and also to the inverse of the price of energy (from 13):

$$\frac{\lambda_1}{\lambda_2} = \frac{1}{p_X} \frac{\delta}{\gamma} \frac{E}{q_X} = \frac{1}{p_E} \tag{18}$$

These expressions, (17) and (18), allow me to construct simplified relationships between forms of labor:

$$l_F = l_{A2}\left(\frac{p_{A2}}{p_E}\right)^{\frac{1}{\beta-1}} \tag{19}$$

$$l_N = \left(\frac{\beta}{\alpha}\frac{p_{A2}}{p_N}\right)^{\frac{1}{\alpha-1}} l_{A2}{}^{\frac{\beta-1}{\alpha-1}} \tag{20}$$

$$l_{A1} = \left(\frac{\beta}{\alpha}\frac{p_{A2}}{p_{A1}}\right)^{\frac{1}{\alpha-1}} l_{A2}{}^{\frac{\beta-1}{\alpha-1}} \tag{21}$$

Substituted into the household time constraint, $L = l_{A1} + l_{A2} + l_N + l_F$ (1), these give an implicit function relating extensive agricultural labor and prices:

$$l_{A2} + l_{A2}\left(\frac{p_{A2}}{p_E}\right)^{\frac{1}{\beta-1}} + \left(\frac{\beta}{\alpha}\frac{p_{A2}}{p_N}\right)^{\frac{1}{\alpha-1}} * l_{A2}{}^{\frac{\beta-1}{\alpha-1}} + \left(\frac{\beta}{\alpha}\frac{p_{A2}}{p_{A1}}\right)^{\frac{1}{\alpha-1}} * l_{A2}{}^{\frac{\beta-1}{\alpha-1}} = L \tag{22}$$

And after rearranging (A19), an implicit function relating forest labor and prices:

$$l_F + l_F * \left(\frac{p_E}{p_{A2}}\right)^{\frac{1}{\beta-1}} + l_F{}^{\frac{\beta-1}{\alpha-1}} * \left(\frac{\beta}{\alpha}\frac{p_E}{p_N}\right)^{\frac{1}{\alpha-1}} + l_F{}^{\frac{\beta-1}{\alpha-1}} * \left(\frac{\beta}{\alpha}\frac{p_E}{p_{A1}}\right)^{\frac{1}{\alpha-1}} = L \tag{23}$$

I assume that new roads decrease the market price of firewood substitutes, $p_{E,}$ and increase the sales-price for the products of non-agricultural labor, p_N. I consider both increases and decreases in the sales-prices of agricultural products (p_{A1} and p_{A2}) to be possible due to new roads. Note that change in prices p_N or p_{A1} could equally represent a reduction in input prices or an increase in the sales-price received.

The key outcome of interest is the impact of household decisions on forest cover. I assume that damage to forests occurs both due to the harvesting of firewood, l_F, and labor spent in agriculture (most likely grazing) near or within forests, l_{A2}. Price changes that encourage both activities are more likely to lead to observed forest degradation and forest degradation. Price changes that encourage one forest damaging activity at the expense of the other will have an ambiguous impact on forests, while those that discourage both activities are more likely to lead to observed forest regrowth, or a slowing of degradation and deforestation trends.

From (22), partial derivatives of extensive agricultural labor have signs: $\frac{\partial l_{A2}}{\partial p_N} < 0, \frac{\partial l_{A2}}{\partial p_{A1}} < 0,$ $\frac{\partial l_{A2}}{\partial p_{A2}} > 0$, and $\frac{\partial l_{A2}}{\partial p_E} < 0$. As expected, higher returns to extensive agriculture cause households to devote more time to extensive agricultural labor. Higher returns in non-agricultural labor, l_N, causes a monotonic shift away from both types of agriculture and away from forest use, again as expected. This represents an inter-sectoral movement from agriculture to small village enterprises such as shops, mills, and other local or commuter-based urban employment. A similar effect is seen from an increase in returns to intensive agriculture. An increase in the market price of energy reduces extensive agriculture as labor is diverted to relatively lower-cost forest firewood collection. From (23), partial derivatives of firewood collection have signs: $\frac{\partial l_F}{\partial p_N} < 0, \frac{\partial l_F}{\partial p_{A1}} < 0,$ $\frac{\partial l_F}{\partial p_{A2}} < 0,$ and $\frac{\partial l_F}{\partial p_E} > 0$. Higher returns to intensive agriculture reduce firewood collection by reducing time available and providing income for the purchased substitute. Higher energy prices have the opposite effect, as expected due to the relatively lower implicit cost of collecting firewood. In aggregate, these results show:

- Increased returns to intensive agriculture, and increased returns to non-agricultural activities, all else equal, have an unambiguously positive predicted effect on forest cover (i.e. both l_F and l_{A2} are reduced). This is despite an income effect that encourages greater energy consumption overall.

- Increased returns to extensive agriculture (grazing) has an ambiguous predicted effect on forest cover: firewood collection decreases, but extensive agriculture itself increases. The precise net effect is a function of the relative impact (and starting magnitude) of both activities, variables that are highly dependent on local settings.

- Equal, simultaneous, increases in returns in both agricultural sector (p_{A1} and p_{A2}) do not lead to qualitative differences in these results. The labor allocation to both agricultural sectors increase, with disproportionate growth in the more productive sector. The labor allocation to firewood collection decreases. Similarly, the absence of a non-agricultural sector entirely (perhaps representing particularly remote regions) does not change signs. All comparative statics are robust to changes in the utility parameters δ and γ, and robust to changes in the productivity parameters β and α (for β, $\alpha < 1$).

2. Do Catch Shares Increase Ex-vessel Prices?[1]

2.1. Introduction

Property rights have long been considered important for economically efficient outcomes in the use of natural resources (Grafton et al., 2000). In open access fisheries, a lack of property rights and resource use restrictions is responsible for both the dissipation of economic rents and the degradation of fish stocks (Gordon, 1954). Property rights are also absent in many tightly managed, non-open access fisheries, where biological management in the form of effort restrictions or output controls protects stock levels but fails to prevent rent dissipation from excess competition between fishing vessels (Homans and Wilen, 1997; Wilen, 2006). Fisheries with open access or biological management account for the vast majority of the approximately 11,000 fisheries globally (Costello et al., 2010), causing substantial estimated losses in economic value (Kelleher et al., 2009).

By contrast, rights-based fishery management address the problem of excess competition among fishing vessels through the use of individual fishing quotas (IFQs) or similar pseudo-property right systems ("catch shares"). These provide participants – individuals, firms, cooperatives, or communities – with a secure right to a specified share of a total allowable catch (TAC) in a given season. Catch share systems have grown in prominence since they were first proposed in the 1970s (Christy, 1973) in part because the establishment of exclusive economic zones made it possible for regulators to limit access to common-pool fishery resources. These systems currently represent about 2 percent of fisheries worldwide and roughly a third of fish volumes landed, and they have been applied to over 200 species in at least 18 countries (Costello et al., 2010; Tveterås et al., 2011). While catch shares do not represent complete

[1] This chapter was coauthored with Anna Birkenbach (Sanford School of Public Policy and Nicholas School of Environment, Duke University), and Martin Smith (Nicholas School of Environment and Department of Economics, Duke University).

privatization of the resource, they do provide strengthened property rights relative to open access and biologically managed fisheries, which may help eliminate rent dissipation by aligning individual and collective incentives, reducing competition between fishermen, and between fishermen and the regulator. This reduction in competition often results in longer fishing seasons (Birkenbach et al., 2017; Grafton, 1996; Wilen, 2006).

Implementing catch shares could theoretically increase resource rents by lowering costs, increasing revenues, or both (Boyce, 1992; Grafton, 1996; Homans and Wilen, 2005). Empirical studies of the economic benefits of rights-based policies have focused on costs and show that, mainly by eliminating redundant fishing capacity, catch shares lower costs (Grafton et al., 2000; Lian et al., 2009; Weninger, 1998). On the revenue side, increases could occur through improved market timing, changes in the product mix across fresh and frozen, or changes in product quality facilitated by a longer fishing season. This hypothesis of improved revenues was developed in a formal model by Homans and Wilen (2005). They show that in the absence of catch shares, short seasons occur due to the competition between fishing vessels, concentrating landings in a short pulse. This could constrain development of a higher-value fresh fish market, and encourage development of a lower-value frozen market supplied with fish inventoried during the constrained season. By removing incentives to race, Homans and Wilen (2005) theorize that catch shares alleviate landings gluts and thus increase the quantity of product directed towards higher value fresh markets[2]. Reduced racing may also lead to better quality fish, and thus higher price, through more careful handling. These hypothesized revenue benefits are additional to the reduced costs predicted by Gordon (1954).

[2] Higher prices may be realized in both short and long term. In principle, the latent demand for fresh products during what was previously the off-season can be tapped immediately. Longer term gains in revenue are possible also as processor infrastructure and supply chains develop to take advantage of the longer season (Wilen, 2006).

Implicitly, there are two hypotheses embedded in this discussion of revenue benefits of catch shares: 1) catch shares decompress the season, i.e., end the race to fish, and 2) this decompression will lead to higher per unit prices received by fishermen for unprocessed fish upon landing their catch ("ex-vessel prices"). The first hypothesis received strong empirical support in an analysis by Birkenbach et al. (2017), who found considerable season decompression on average in U.S. catch share fisheries. We build on this result to test the second hypothesis.

Price impacts of catch shares have not previously been the focus of systematic study, although there is anecdotal evidence for their existence. Grafton et al. (2000) reported that the implementation of the British Columbia halibut individual vessel quota (IVQ) program in 1991 increased ex-vessel prices by 22 to 34 percent. (Casey et al., 1995) found that the same program stretched the price premium of British Columbia halibut over Alaskan halibut from 15 to 70 percent. Wholesalers were able to sell 94 percent of their catch to the fresh marker relative to 42 percent before catch shares. Alaskan halibut, which did not come under catch share management until later, continued to be sold frozen. Price increases were similarly seen after the introduction of the Northeast Scallop IFQ program, specifically, a 31 percent increase in one year relative to the 3 years prior to implementation. Increases occurred in the Mid-Atlantic Golden Tilefish IFQ program (8 percent), Northeast Multispecies Sector Program (7 percent on average for groundfish), and the Pacific Coast Sablefish Permit Stacking Program (55 percent) (Brinson and Thunberg, 2013a). Another data point comes from the 2009 introduction of catch shares in the Peruvian anchoveta fishery, the largest fishery in the world by volume. Average landing prices increased by 37 percent, the season length increased from approximately 50 days to over 100 days, and the average quality of the anchovy meal improved, all within 1 to 2 years of IFQ introduction (Tveterås et al., 2011). It is possible that these increases were driven by changes in season length, as many fisheries operated under derby conditions prior to program implementation.

These descriptive examples are consistent with the mechanism proposed by Homans and Wilen (2005). While suggestive, much of this evidence risks confounding correlation and causation. Studies that seek to quantify the impact of a management change by examining only the affected fishery (e.g., using a before-after comparison) are problematic because factors besides the management change can influence outcomes concurrently. Such factors include changes in the supply of substitutes, economic conditions, seasonal variation, and technological change. In contrast, we compare each catch share ("treatment") fishery to a matched ("control") fishery, a domestic or imported fish source that serves the same market as the catch share-managed fishery but did not undergo catch share management reform at the same time. Using ex-vessel price as the outcome variable, we estimate difference-in-differences models, controlling for year-specific price shocks across both fisheries, fisheries specific time-invariant determinants of prices, and concurrent changes in the TAC. Our analysis includes almost every U.S. fishery that has adopted catch shares. We compute weighted average treatment effects across fisheries, to provide the first rigorous and comprehensive test of the hypothesis that catch shares cause ex-vessel price increases.

Our empirical strategy is motivated by the nascent economics literature that has adopted quasi-experimental methods for the evaluation of fisheries policies. This literature derives treatment effects by comparing outcomes to a real or simulated counterfactual. Studies of this type include Smith et al. (2006), who used difference-in-differences and detailed panel data to show that marine reserves caused harvest declines in a large multi-species fishery in the Gulf of Mexico. Costello et al. (2008) and Costello et al. (2010) used propensity score matching to show that catch shares reduce the likelihood of fishery collapse. Other studies using quasi-experimental approaches have examined the behavioral effects of information sharing on bycatch (Abbott and Wilen, 2010), the revenue effects of a pilot catch share program in Rhode Island (Scheld et al., 2012), effort spillover effects across regional fisheries management boundaries (Cunningham et al., 2016), and the effects of catch shares on days at sea (Hsueh, 2017). While catch shares have received attention from this quasi-experimental program evaluation literature, systematic investigation of price effects has not.

We find a number of fisheries with considerable positive price effects relative to their control fisheries. Fisheries with statistically significant results across multiple model specifications include Atlantic sea scallop (a 51 percent price increase over 3 years post-implementation), Gulf of Mexico red snapper (40 percent), Atlantic cod (74 percent), and South Atlantic wreckfish (76 percent). However, we also see fisheries with large negative price impacts: Prices for Atlantic plaice flounder, white hake, and winter flounder decreased by 35 to 56 percent. This heterogeneity makes drawing a simple conclusion from individual cases difficult. We apply a meta-analysis to derive average treatment effects across treated fisheries. We find an average treatment effect of 1.1 to 8.3 percent when weighting by the precision of individual fisheries estimates. This aggregate evidence provides cautious support for the tested hypothesis.

The high degree of heterogeneity in individual fishery results indicates that the Homans and Wilen (2005) mechanism is considerably modified by local conditions. To explore, we undertake sub-sample analysis and find outcomes consistent with theory. Price treatment effects are correlated with season decompression treatment effects, supporting the proposed mechanism. As shown in Birkenbach et al. (2017) a minority of fisheries see shorter seasons as a result of catch shares (discussed further in section 2.4). On average, we find significant price increases among decompressing fisheries, and significant price decreases among compressing fisheries. We also see a more pronounced positive average treatment effect among higher priced fisheries. Higher priced fish are disproportionately sold in fresh product markets, and are thus most likely to be harvested in ways that take advantage of the longer seasons made possible by catch share management.

We use meta-regression in an attempt to more precisely determine modifying factors, but results are inconclusive. We conclude with a discussion of factors that may either attenuate price effects or drive the heterogeneity we see. We believe that a particularly important factor is substitution of effort between species within multispecies fisheries. The theory of catch share revenues we test is based on a single-species catch share system, where fishermen optimize catch timing for a single species. Incentives within this model cannot account for our mixed results. By contrast, in many U.S. catch share fisheries, fishermen can catch multiple species and thus respond to multiple profit margins. Incentives to temporally extend harvesting of one species may cause a substitution of effort away from other species, compressing its season (Smith et al., 2016). We cannot explicitly test for this possibility with our data, but our results suggest the need for a richer theoretical understanding of fishing behavior in multispecies contexts.

We proceed in Section 2.2 with a description of our data sources, hand-matching procedure, and empirical strategy. Section 2.3 presents results, starting with some notable individual fishery price effects discussed in conjunction with their observed season length effects. We then present in turn our analysis of TAC changes, the meta-analysis that synthesizes results across fisheries, and our heterogeneity analysis. Section 2.4 discusses the extent to which the combined results support theoretical predictions, along with limitations of this study.

2.2. Methods

2.2.1. Data

We compiled a panel of all federally managed fisheries in the United States that have switched to catch share management (Supplementary Section 2.8) with the exception of the Western Alaska Community Development Quota Program[3]. The 15 included programs manage harvests

[3] The Western Alaska Community Development Quota Program is an unusual case in that it serves community goals of sustenance and remote community development rather than economic efficiency

of 54 species or species groups ("individual fisheries")[4]. Data is sourced from the National Oceanic and Atmospheric Administration (NOAA) Fisheries Statistics Division (NOAA, 2014) and Fisheries and Oceans Canada (DFO, 2016) websites and supplemented with data sourced directly from the Northwest Fisheries Science Center (NWFSC), Fisheries and Oceans Canada,[5] and NOAA Chesapeake Bay. Monthly U.S. landings data are available for the years 1990 to 2014 for most fisheries and include weight in pounds and total dollar value of landings by species, state, gear type, and management region. Four Alaskan fisheries are exceptions: the American Fisheries Act Pollock Cooperative, the Bering Sea and Aleutian Islands Crab Rationalization Program, the Non-Pollock Trawl Catcher/Processor Groundfish Cooperatives (Amendment 80), and the Central Gulf of Alaska Rockfish Cooperatives Programs only have annual data available in the relevant time windows. In addition, due to confidentiality requirements a small number of observations in the monthly data are unavailable in cases where the number of participating vessels in a given month was fewer than three.

We compile TAC data for all fisheries from individual Fisheries Management Plans, Environmental Impact Statements, the Federal Register, and directly from DFO and NOAA[6]. TAC data is available for 46 out of 54 fisheries[7]. We drew on a range of TAC types. Annual

goals. Behavior may not respond to the same market incentives that underpin our hypothesized mechanism.

[4] A program may regulate one or multiple fisheries. A fishery generally represents one species, but in some cases (e.g. shallow water groupers in the Gulf of Mexico), it refers to a group of species (which we thus analyses as a single unit). A number of minor species were excluded due to data unavailability

[5] We thank Gisele Magnusson and Barbara Best (Fisheries and Oceans Canada) for data provision.

[6] TAC data is from a large number of sources. Sources vary across fisheries and years and are omitted from the reference list for space reasons. A full list is available from the authors on request.

[7] During the analysis period, a TAC was not specified for ocean quahog and Atlantic surfclam, to our knowledge. A further six species had TACs, but their control fisheries did not, preventing use of TAC in

catch limits (ACLs) are binding seasonal limits which if exceeded leads to fishery closure. ACLs are used in our study when available; however, ACLs were not required by federal law until 2011 and thus are missing for many fisheries during earlier years. Where necessary, target TACs or harvest guidelines were used in their absence. Target TACs may not trigger season closure if exceeded, but are used to set regulations (such as gear restrictions or days-at-sea limitations) intended to prevent exceedance of the TAC. In some cases we use optimum yield (OY) values, which are assessments of a fishery's maximum sustainable yield adjusted by standardized risk factors (CFMC, 2013). OYs are used to determine TACs. OYs were scaled for equivalency to ACLs and other TAC forms using overlaps of at least two years in individual fisheries' time series.

The treated fisheries represent a wide range of species and fishery types. The largest fisheries by volume are Alaskan pollock, Pacific whiting, Alaskan Pacific cod, Alaskan sole, and Atlantic surfclam. Pollock is an order of magnitude larger than any of the other treated fisheries, which themselves span at least five orders of magnitude. The largest fisheries by total value are Alaskan pollock, Alaskan king crab, Alaskan Pacific halibut, Alaskan snow crab, and Alaskan sablefish. Although the top volume and value lists do not entirely coincide, Alaskan fisheries dominate both. On a price-per-pound basis, Atlantic scallop, Alaskan king crab, Gulf of Mexico gag, and Gulf of Mexico shallow water groupers command the highest prices. Heterogeneity in price spans nearly two orders of magnitude with a pre-treatment price per pound of $7.43 for scallop and $0.10 for Pacific whiting (2015 dollars). A challenge in this analysis is development of an empirical strategy that can a) identify treatment effects at varied scales and b) appropriately combine treatment effects across fisheries.

comparative analysis. These are darkblotched and splitnose rockfishes, red grouper, other shallow water groupers, starry flounder and Atka mackerel.

2.2.2. Empirical Strategy

Each treated fishery is individually-matched to a control fishery. We selected matches that serve similar markets (and are thus affected by the same product demand shocks), are caught using similar gear (and thus are affected by the same input demand shocks), yet have distinct management regimes. Descriptions of management systems for each program and its control fisheries are presented in Supplementary Section 2.8. Species were grouped together in some limited cases where they serve the same market, may not be properly differentiated when landed, or when a match of the same species does not exist. We used a hierarchical approach to selecting matches: A) a fishery of the same species managed by a U.S. management authority that did not implement catch shares at the same time; B) the same species in Canada for which Canadian monthly landings are available and catch share management reform did not occur at the same time; and C) a similar species/market that did not experience catch share management reform at the same time. Two exceptions are a composite of exported products for one particularly distinct species, Atka mackerel, and Russian imports for red and golden king crabs (which are not harvested in Canada). Our controls include both fisheries that have never received catch shares, and fisheries that previously received catch shares but did not undergo catch share implementation within the 6-year window of analysis ("reverse controls"). Some controls were partially covered by catch shares (i.e. for a subset of vessel classes), but again, did not receive or lose catch shares during the window of analysis. Our difference-in-differences (DID) approach (explained below) is equally appropriate for identifying treatment effects in these three cases (Shadish et al., 2002).

We calculated average per-pound ex-vessel prices from total landed quantity and value, for a given region, state (and in some Alaskan cases, a given port), month/year, and species[8]. In cases where multiple species were grouped together, we generated an average per-pound ex-

[8] Prices are deflated and exchange rate-adjusted for international comparisons.

vessel price across species, weighted by pounds landed. DID estimation compares the change in outcome levels over time across treatment and control groups, relying on the assumption that, absent the treatment, the difference between treatment and control outcomes would have remained constant. This controls for time-invariant differences between fisheries, but not for factors that affect the treatment or control fishery in isolation, concurrently with treatment. As previously discussed, a particular threat to validity is changes in the biologically-determined TAC. Fisheries undergoing management reform may be more likely to see adjustments in the TAC, particularly if adverse stock conditions partially motivate the switch to catch shares. We test for a correlation between TAC change and treatment and include TAC in some regression specifications as a control.

As the data permit, we run each of a number of specifications with three-year intervals before and after the policy change. Although price changes may occur quickly, we wish to capture ex-vessel price impacts from new markets that may take a few years to materialize. On the pre-implementation side, a longer time period reduces the chance of bias due to "announcement effects" (i.e., when fishermen and others along the seafood supply chain become aware that the policy change would occur sometime before the implementation date and change their behavior in response) and possible pilot programs or other partial measures enacted prior to full implementation. Longer time periods are increasingly infeasible given data constraints. We estimated individual fishery treatment effects using the specification:

$$P_{tk} = \alpha + \beta_1 POST_t + \beta_2 TREAT_k + \beta_3 POST_t * TREAT_k + \theta_t + \varepsilon_{tk}$$

Where P_{tk} is average ex-vessel price per pound, for year *t* and treatment status *k*. *POST* and *TREAT* are binary variables indicating that an observation occurs after catch share implementation and that it belongs to the treatment fishery, respectively. Regression variants include year fixed effects (θ_y) or a linear time trend variable (controlling for time varying factors that affect treatment and control equally), and state fixed effects (θ_y) to account for time-invariant price differences across regions within treatment and/or control. ε_t represents the idiosyncratic error term, and the treatment effect is the DID estimator, β_3. We estimate monthly and annual models. Monthly models provide more observations, and allow us to more precisely specify the timing of the policy change (mid-calendar year, for instance). Seasonality presents additional challenges for monthly models. We include monthly fixed effects as a seasonality control, and also weight observations by the quantity landed when estimating regression coefficients[9]. This places relatively greater importance on prices determined during months where landings are greater and price variance for individual catches (not observed in the data) is presumed to be lower. Standard errors for monthly models are alternatively estimated with the Huber-White variance estimator, robust to heteroscedasticity, and the Newey-West variance estimator, consistent in the presence of autocorrelation (with a lag time of 12 months). Robust standard errors were not used for individual fisheries' annual models due to small sample sizes (Imbens and Kolesár, 2016).

To determine the aggregate impact of catch shares across U.S. fisheries, we use a meta-analysis in which individual fishery treatment effects are combined in a weighted average. We weight by inverse variance (giving more weight to fisheries with more precisely estimated treatment effects), volume (pounds landed) and dollar value (ex-vessel revenues), to weight more economically important fisheries, and combinations of these weights. Weights, w, for each fishery pair i, for each of the resulting six weighting schemes (including unweighted), s, were applied to DID coefficients (β_3) to give the WATE:

$$WATE = \frac{\sum_i \beta_{3i} * w_{si}}{\sum_i w_{si}}$$

And weighted variance (WV):

[9] We use analytical rather than frequency weights to ensure that the effective sample size does not change from weighting, which would otherwise shrink standard errors.

$$WV = \frac{\sum_i var(\beta_{3i}) * w_{si}^2}{(\sum_i w_{si})^2}$$

From which a t-statistic and associated two-sided p-values were calculated.

2.3. Results

2.3.1. Selected Individual Fishery Results

We first consider a selection of notable individual fishery results, highlighting three potential outcomes: price increase with season decompression (Atlantic cod, red snapper), no price increase with season decompression (deep water groupers), and no price increase and no season decompression (yellowtail flounder) (Figure 2-1). Treatment effects for all individual fisheries are presented in Table 2-6 of Supplementary Section 2.10. Results presented in the meta-analysis (following subsection) quantitatively synthesize all individual results.

Atlantic cod is managed under the Northeast Multispecies Program, a joint management program of nine species divided into 19 stocks. The program commenced in 2010,[10] replacing days-at-sea management (a form of effort control). The distribution of catch throughout the season extended slightly following implementation, relative to the harvest distribution of the control cod fishery in Atlantic Canada[11]. The extent of potential behavior change was attenuated by the prior use of days at sea management, which itself helps to mitigate racing. Nevertheless, we find a 50-89 percent increase in price relative to the control following implementation, with the lower estimate when the TAC control is included. The TAC fell considerably over the period of analysis, slightly faster than the decline in the control TAC, which itself could have

[10] One sector of the Atlantic cod fishery voluntarily adopted catch share type management in 2004 (the Georges Bank stock). Any bias that this introduces makes our results more conservative. We return to this point in the discussion.

[11] See Birkenbach et al. (2017) for detailed season length results for the majority of fisheries analyzed in this study.

put supply-induced pressure on prices. Atlantic cod is a relatively high-value and high-volume fishery (averaging $1.66/pound and 18.5 million pounds landed over the pre-treatment period). Fishermen are likely to optimize timing decisions with respect to this fishery first, and to lower value species within the complex second.

Red snapper shows a similar pattern to Atlantic cod, consistent with the theory. The fishery has been managed under the single-species Gulf of Mexico Red Snapper IFQ Program since 2007. Modest season decompression occurred after catch share implementation along with price increases robustly estimated between 40 and 70 percent relative to the control species, Gulf of Mexico vermillion snapper. The TAC decreased significantly (relative to the control) during the analysis period, which explains part but not all of the total price effect. A sharp increase in price is evident at the time of program implementation.

Yellowtail flounder, like Atlantic cod, is managed under the Northeast Multispecies Program. It has a similar price to Atlantic cod but is much smaller in volume (3.6 million pounds annually, pre-treatment). It represents an unusual example in that it shows season compression following catch shares: the time taken to catch 80 percent of the catch in a given season decreased by 1.3 months on average in the three years following implementation. Yellowtail flounder is managed in a complex of fish caught together, and this squeeze on season length could reflect optimization of season lengths over multiple species, where other species took priority. We return to this possibility in the discussion. Negative (although non-significant) price impacts relative to its Canadian control fishery are consistent with this increased season compression.

The deep water grouper fishery includes four species caught together and managed with a common TAC. It underwent substantial season decompression following implementation of the Grouper-Tilefish IFQ Program in 2010. Time taken to catch 80 percent of the catch increased by 3.2 months relative to its control fishery, deep water groupers (same four species) caught in the South Atlantic. Prices increased in absolute terms across the relevant time period, but prices relative to the control show no significant difference in any model specifications. In combination with the other examples presented above, the case of deep water groupers suggests that the price effects of season decompression are considerably modified or attenuated by other factors.

These examples highlight the considerable heterogeneity in price effects across fisheries, even in cases like Pacific halibut where season decompression effects are clear. Looking at all individual results (Table 2-6 in Supplementary Section 2.10) we see similar numbers of statistically significant ($p < 0.10$) positive and negative coefficients. In our most conservative monthly model (controlling for TAC and state, year, and month fixed effects, Table 2-6, column 9), there are seven fisheries with positive and seven with negative ($p < 0.10$) treatment effects. Many results are statistically non-significant. We return to these price effects in our meta-analysis (section 2.3.3).

2.3.2. Changes in TAC

TACs are determined from biological stock assessments that are adjusted for uncertainty and other sources of fish mortality (e.g., bycatch). Regulations are chosen – or under catch shares, quota is allocated – with the goal of avoiding TAC exceedance. TACs change from year to year. In sufficiently localized markets, these changes may affect prices through their impact on seafood supply. This is a source of concern for our analysis because fisheries reform often involves simultaneous changes to the TAC and management system. If TAC reduction occurs during the reform process, it may cause supply-side induced price impacts that give the impression of stronger catch share price impacts than exist in reality. Alternatively, if TACs increase, the greater supply may mask positive price effects of catch shares.

We thus control for TAC in our regression analysis, and further test for remnant, systematic influence of TAC on our price treatment effect. On the individual fisheries level, unaccountedfor TAC changes, positive or negative, could contribute to the variability in individual fishery price effects. Across fisheries, if TAC changes are systematically correlated with catch share implementation, and supply-side pressures result, average treatment effects may be biased.

To test for systematic correlation, we apply our DID approach (Section 2.2.2) with annual TAC as the dependent variable. Treatment effects of catch share implementation on TAC are statistically significant for 18 of 46 fisheries but show no clear trend, with 23 negative (8 statistically significant, $p < 0.10$), and 23 positive (10 statistically significant) (column 1, Table 2-6)[12]. However, meta-analysis results, which synthesize TAC treatment effects (in percent terms) across fisheries, show a significant negative relationship when weighted by precision of estimation. Catch shares are correlated with an average reduction in the TAC of 5.7 percent. The other weighting schemes do not show significance or sign agreement, indicating that larger or more economically important fisheries did not see disproportionately large cuts.

This suggests that TAC could systematically confound treatment effects, motivating our use of TAC controls in a subset of model specifications[13]. Even in the absence of this evidence, specific TAC changes, positive and negative, could impact individual estimations. We look for evidence that TAC treatment effects are correlated with price treatment effects, which would support the hypothesis that relative price changes following catch share implementation are driven at least partially by simultaneous TAC changes. Figure 2-6 in Supplementary Section 2.10 presents scaled treatment effects for TAC alongside scaled price treatment effects for all

[12] These difference-in-differences results take into account control fishery movements in TAC to give a relative change. The absolute change, considering only treatment fisheries, is similarly mixed with 25 fisheries increasing TAC and 29 decreasing TAC (or equivalent biological limit). The average absolute change in percent of pre-treatment TAC is an increase of 12.4 percent (scaled by fishery size).

[13] It should be noted that the use of the TAC is not equivalent to the use of quantity landed as a covariate. TAC is determined exogenously by biological factors, while quantity landed is endogenous to price and is thus an unsuitable control for supply-determined price changes.

fisheries with monthly data. We calculate correlation using a Monte Carlo analysis to account for variation around the point estimates. We draw 1,000 sets of draws from the sampling distributions of each of the price and TAC treatment effects. We calculate and plot the correlation coefficient for each set of draws. Correlations clustered around zero indicate a lack of evidence for a negative correlation between the TAC and price treatment effects across fisheries. While we would expect no relationship for price treatment effects estimated in models with TAC controls, the absence of a correlation even when TAC controls are omitted suggests that TAC is not systematically driving price effects.

2.3.3. Price

Having ruled out TAC changes as a substantial driver of price changes, we return to the price treatment effects in isolation, and aggregate across fisheries using the same meta-analysis procedure (Table 2-1). There is some evidence of a positive price effect overall when weighted by precision of estimation. Magnitudes range from 1.1 percent to 6.5 percent, $p < 0.1$ for seven of eight model specifications. WATEs under alternative weighting schemes are positive when statistically significant. The greater aggregate magnitudes when weighting by fishery sizes or values indicates that price increases are disproportionately greater among economically more important fisheries.

One potential explanation for relatively weak ATE evidence is that matched control fisheries' markets may be integrated with the treated fisheries' markets. If control and treatment markets are tightly integrated, revenue benefits in the treatment fisheries may contaminate those of the control fisheries. This echoes spatial-dynamic general equilibrium concerns about the use of treatment effects models to evaluate outcomes in marine resources outcomes (Smith et al., 2017, 2014). Treatment effects may also be attenuated by fishing effort spillovers between treated and control fisheries. If catch shares reduce effort and capital deployment in one fishery, it may be redeployed in the control fishery in some circumstances (Cunningham et al., 2016; PFMC, 2000). Although our time series are insufficient to test formally for market integration, we test whether ex-vessel prices increased relative to a more broadly-defined counterfactual, one effectively exogenous to changes in the treatment fishery. We compare each treated fishery to the capture fishery Fish Price Index[14] (FPI) (Tveterås et al., 2012) and calculate the difference-in-differences. Results remain inconclusive: 29 out of 54 fisheries/fishery groups have prices that grow faster than the FPI over the 3-year post-treatment window, and 25 grow slower. The average unweighted treatment effect is 2.9 percent. However, when weighted by volume or value, treatment effects are negative: -4.0 percent by volume and -0.02 percent by value. This suggests that particularly important fisheries declined in relative value, but does not provide much further insight.

2.3.4. Heterogeneity Analysis

The Homans and Wilen (2005) prediction of ex-vessel price increases relies on season decompression. Published analysis (Birkenbach et al., 2017) provides strong empirical support for such season decompression: U.S. fisheries converting to catch shares have, on average, increased the time taken to catch 80 percent of the season's total harvest by 0.8-0.9 months following catch shares. However, as demonstrated in the fishery case studies presented in Section 2.3.1, this effect varies greatly. We expect to find price increases when season decompression occurs (reducing market gluts), and price decreases when season compression occurs (contributing to market gluts). We do not expect to see price increases and season compression, or vice versa. This expectation broadly holds with two exceptions, yellowtail flounder and Atlantic plaice. Other fisheries with both treatment effects significant ($p < 0.10$) are found in quadrants 1 and 3 of the outcomes scatterplot (Figure 2-3). The pattern provides modest support for – or at least does not run counter to –theory. Fisheries that did not experience season decompression were not likely to experience price increases, and fisheries that

[14] The FPI is an index of global seafood prices for capture fisheries (as opposed to aquaculture) (Tveterås et al., 2012).

experienced price decreases mostly did not experience season decompression.

Monte Carlo analysis, incorporating uncertainly around the point estimates, supports this relationship (Figure 2-4). Fisheries that undergo season decompression are those most likely to show price increases. We repeat the meta-analysis, breaking the sample of 54 fisheries into those with season treatment effects indicating decompression (change in Gini < 0) and compression (change in Gini > 0). Weighting by inverse variance to account for point estimate precision, we see ex-vessel price increases in cases of season decompression, and vice versa, with significant ($p < 0.05$) for four out of six models. Incidentally, TAC change is negative for decompressing fisheries ($p < 0.01$) and positive for compressing fisheries (relative to control fisheries).

A related heterogeneity analysis is a breakdown of price treatment effects by pre-treatment price. We expect those fisheries with a viable fresh market to be those that experience the greatest season decompression. Assuming that fisheries with fresh markets tend to have higher per-unit prices, we expect season decompression to be correlated with the pre-treatment price for individual fisheries. This may particularly be the case for fisheries within multispecies complexes, where fishermen face tradeoffs between spreading out the harvests of different species. Monte Carlo analysis indicates that higher price fish are those more likely to undergo season decompression (not shown for space reasons), and more importantly for the present hypothesis, are those that tend to see positive price effects (Figure 2-5).

Finally, we test potential correlates of positive price effects using a meta-regression. Theory suggests that supply chain integration and the abundance of market substitutes could influence the likelihood of positive price outcomes. Fisheries whose products have few substitutes are more likely to be able to tap latent demand (demand that goes unfulfilled outside of the short season opening), and thus should receive higher prices if catch shares permit a more even distribution of the catch throughout the season. We proxy for substitutes using the coefficient of variation of monthly prices in the three years prior to catch shares. Markets that are relatively poorly integrated, or land fish with few substitutes, will face a relatively steep demand curve. Supply fluctuations may cause larger price variations (high coefficient of variation) for these products than for products with many substitutes. By contrast, well-integrated product markets with near-perfect substitutes face a horizontal demand curve and thus no price fluctuation due to supply shocks. Demand fluctuation in such broad markets is also likely to be more modest. Fisheries that have a high degree of supply chain integration, or monopsony situations, are less likely to have price increases due to greater non-competitive market behavior. For example, very large fisheries such as pollock are exploited with catcher-processor vessels. In such situations, the ex-vessel price is a within-company transfer and may not respond to market incentives. We proxy for supply chain integration using a binary variable indicating catcher/processor type harvesting, and by using log size (volume landed) of the fishery.

Variables are entered linearly and are also interacted with the season decompression treatment effect (Table 2-4). The interaction terms attempt to capture the modifying influence that market and supply chain integration should have on season length's impact on price. If the season remains compressed due to unforeseen constraints, we do not expect price effects regardless of other factors. However, we do not find significance on these three proxy variables. We do see significance ($p < 0.10$) consistent with the previously explored fishery descriptors: the extent of pre-treatment compression, and naturally, the extent of decompression. Results are not qualitatively different for other price treatment effect models (monthly versus annual, with and without TAC controls).

2.4. Discussion

We present evidence that in aggregate supports the Homans and Wilen (2005) hypothesis that catch shares increase ex-vessel prices. This takes the form of three inter-related findings. First, we find modest evidence of net positive price effects across U.S. catch shares. Obscured by this result, however, is considerable heterogeneity among individual fisheries, some of which increase in price and some of which decrease. Our second result is an important explanation for this heterogeneity. Fisheries that undergo season decompression tend to experience price increases; fisheries that undergo season compression tend to experience price decreases. This correlation between season length change and price change is consistent with the Homans and Wilen (2005) hypothesis: season length is the key predicted mechanism underlying the price change.

What is not widely acknowledged or featured in existing theory, is that catch shares do, in some cases, compress seasons. This occurs even though on net they much more often decompress seasons (Birkenbach et al., 2017). The reason for their occasional compression impact is likely due to substitution of effort and capital within fishing complexes (Poos et al., 2010; Smith et al., 2016). Fishermen in many U.S. fisheries management regions must make decisions that optimize over multiple species. The Northeast Multispecies Sector Program, for example, manages 13 species, 9 of which are under catch shares management. Fishermen will catch multiple species over the season, but will target different species within the season, according to stock and market conditions. Choices of gear and fishing area, for instance, will increase harvest of one species at the expense of another. Fishermen might reasonably be expected to extend the season of the fish product that best capitalizes on the latent demand that catch shares provide access to, i.e., the species that receives the greatest price increases from an extended season of landings. Or, there may be cost-side reasons for expanding or contracting seasons within a multispecies complex. Either way, a side effect of this behavior may be to further compress the season for fish that do not receive higher prices from a longer season. This compression frees up effort and capital at other times for pursuing the more profitable species. This may occur even if both low and high value fish can gain from a longer season. Provided there is some differential in the revenue gain, there is the potential for complementary season compression and decompression within fishing complexes. Smith et al. (2016) predicted this with numerical simulation, and finds behavior consistent with predictions in the Norwegian groundfish trawl fishery. More valuable fish are caught over a longer season.

While our data do not allow explicit testing of intra-seasonal substitution of effort within multispecies complexes, our third finding is consistent with this prediction. We see that higher priced fish are more likely to undergo season decompression and more likely to increase in price following catch share implementation. These higher priced fish are those most likely to have a viable fresh market. Fish that are exclusively sold frozen, canned, or in other low-value uses, either due to consumer preferences or supply chain configuration, face little gain from a longer season since they are supplying a market that is supplied year-round regardless (assuming sufficient inventories). Prices fell for these species following catch share implementation. While we do not have data on each species' relative suitability for fresh and frozen markets, we consider price to be a reasonable proxy. Our differential price treatment result thus partially supports the Homans and Wilen (2005) model, in which altered product flows into fresh and frozen markets is a key mechanism, while also suggesting a need to consider substitution effects in multispecies fisheries.

While substitution effects within multispecies complexes could explain heterogeneity in season decompression, other structural characteristics of markets could lead to heterogeneity in price effects even *given* season decompression. We speculate that fish products sold in well-integrated markets – i.e., species for which consumers have ready substitutes – will see smaller or no price effects. If true, this would represent general equilibrium effects overwhelming our hypothesized partial equilibrium prediction. We secondly speculate that uncompetitive processor markets may limit revenue benefits from being passed on to fishermen, and thus limit changes in ex-vessel price data. At its extreme, this could take the form of vertically-integrated companies for which ex-vessel price are simply intra-company transfers rather than real market signals. We were not able to substantiate these two hypotheses, possibly due to unsatisfactory proxy variables for these market characteristics.

In addition, there may be features of the pre-existing management regime which have already achieved partial or full season decompression, diminishing the potential for further change from catch shares. The Pacific hake (whiting) fishery, for example, comprised cooperatives among catcher/processor vessels, which allocated quota to members internally on a voluntary basis prior to catch share implementation (PFMC, 2010). While racing remained a feature among other segments of the fleet, these pre-existing cooperatives would diminish the potential impact of catch shares within the catcher/processor section of the fleet. A different prior institution, but one that may have also achieved partial season decompression, are "days-at-sea" restrictions used in the northeast. Vessels were allocated a set number of days at sea which had the effect of partially limiting their catch (Holland et al., 2014a). Vessels could use their set allowance of days at any time while the season remained open. Some racing incentives remained, given that the allocation had to be used before the fishery closed, but it likely allowed for more even distribution of effort across the fishing season than a derby-style fishery.

A number of more minor factors could also contribute to variation across fisheries. One is changes in composition of grouped species, which were aggregated for analysis on account of data limitations and/or because they serve the same market. It is possible that the mix of species within a group changed over time, and did so differentially across treatment and control. We checked this possibility for the deep water groupers aggregation, and found minimal change in composition in both the treatment and control regions at the time of the policy change. Shallow water groupers saw a relative decrease in higher-value scamp after the policy change, which could attenuate positive price impacts. More generally, our price data obviously incorporates quality and size of product, which could change as a result of catch shares (or associated, simultaneous regulation changes). We expect catch shares to improve the ability of fishermen to target quality attributes like fish size which would reinforce, not diminish, positive price treatment effects. However, it is possible that targeting quality in a multispecies fishery could reduce the ability to target quality attributes for another species, accentuating the species-interaction effects previously described.

Finally, we note that idiosyncratic threats to identification in particular fisheries are an inevitable challenge in meta-analysis type studies, which must make tradeoffs between internal and external validity. The geographic and temporal range of this study – with 54 varied fisheries and implementation dates over 20 years – represents prioritization of external validity. While we are unable to control for all idiosyncrasies empirically, we provide qualitative summaries of locally specific characteristics for each catch share program and control fishery (Supplementary Section 2.9). To this we add a mention of a particularly notable event, the Deepwater Horizon oil spill. This occurred in April 2010, just a few months after the switch to catch share management of all the relevant Gulf of Mexico species except red snapper. We see significant season decompression among these GOM species, but statistically significant price increases in only tilefish. This could reflect a countervailing force of consumers avoiding Gulf of Mexico seafood products following the spill, reducing demand and thus price. There is evidence for a shift in consumer preferences for a number of Gulf seafood products (McKendree et al., 2013; Michael Carroll et al., 2016; Morgan et al., 2016).

2.5. Conclusion

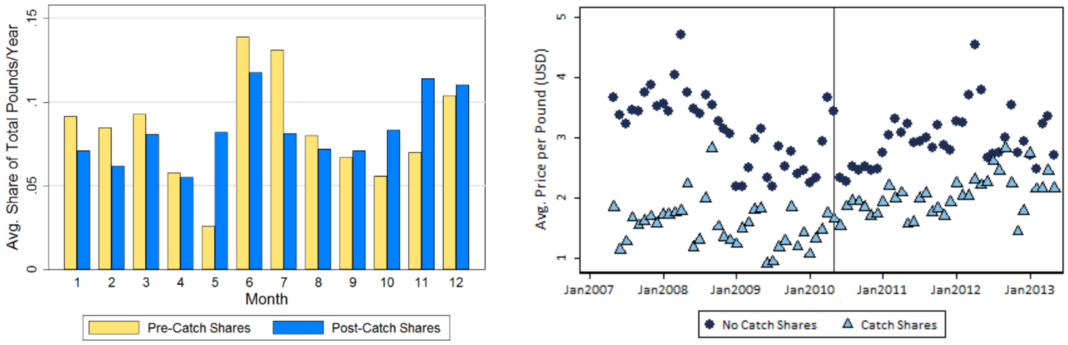
Our analysis is the first to systematically and causally link catch shares to ex vessel price changes. We present evidence of price rises following catch share implementation in cases where catch shares lead to season decompression (i.e., end the “race to fish”). We see a stronger price impact of catch shares among higher value fish, possibly because higher value fish are those with greater fresh product market potential and can thus benefit from more even temporal distribution of landings. We also find that catch share implementation tends to be accompanied by a decrease in TAC relative to control fisheries. We do not find evidence that the TAC decrease contributes to price increases, yet we control for this possibility where possible. We caution against overly broad interpretation of the main result; heterogeneity between individual fisheries is very high, and further exploration of the factors that lead to either positive or negative price effects would be valuable. Longer time series and/or vessel-level micro-level data may either strengthen or qualify our finding, particularly with regard to substitution of effort in multispecies fisheries.

Broadly speaking, the relationship exhibited between catch shares and ex-vessel prices is an example of resource rents increasing as a result of institutional change. For fisheries, property rights not only prevent rent dissipation due to competition externalities; they generate new rent. The value of fish is not only a function of market demand for the delivered good, but is also a function of the institution that manages delivery.

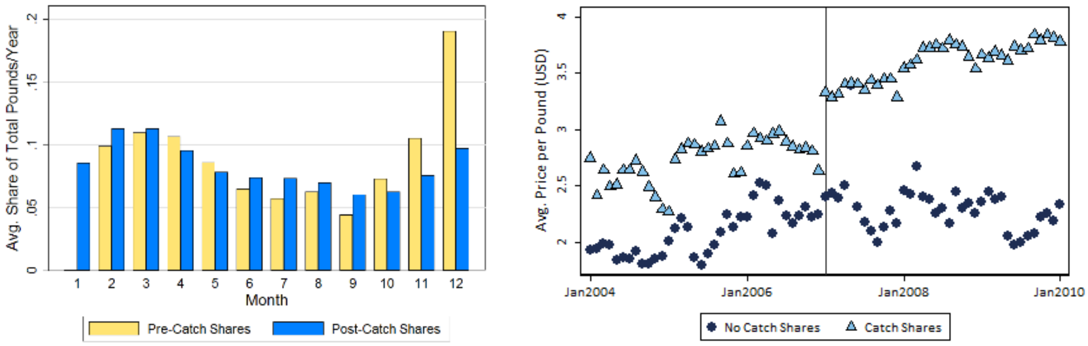
Our results are particularly valuable in light of current policy debate. The Magnuson-Stevens Fishery Conservation and Management Act is overdue for reauthorization. This act governs management of all fisheries in U.S. federal waters, including the type and extent of fisheries reform that regional managers and local fishing communities can take. Proposed amendments introduced to Congress in the last two sessions highlight disagreement over catch shares. Specifically, an amendment bill under consideration in the House of Representatives places restrictions on the future implementation of catch shares (HR. 200, 115th Congress). A different amendment recently proposed (but not passed) by the Senate does not (S. 2991, 113th Congress). A better understanding of catch share revenue impacts can help inform this debate.

2.6. Figures

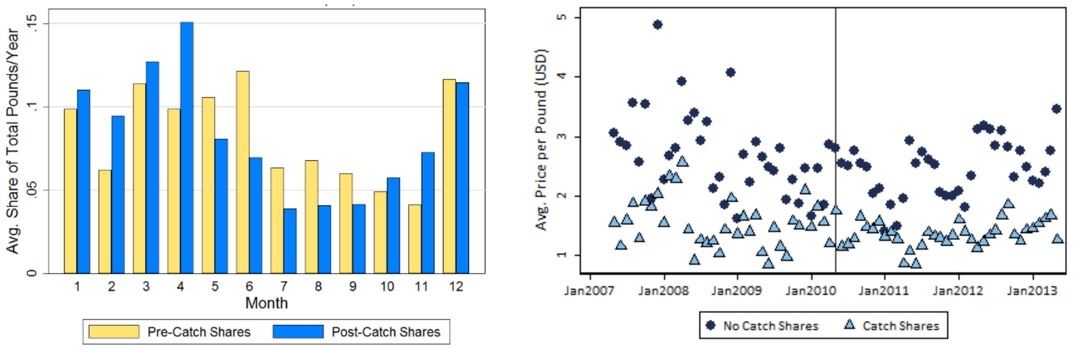
Atlantic cod



Red snapper



Yellowtail flounder



Deep Water Groupers

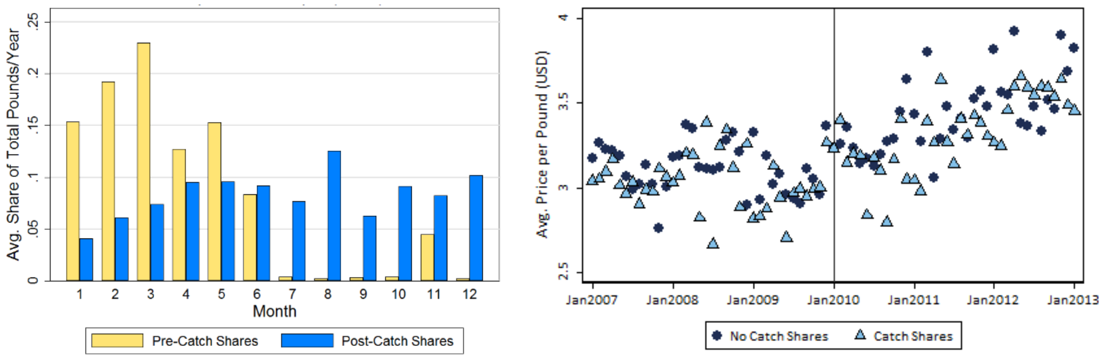


Figure 2-1: Landing distribution and ex-vessel price for four example fisheries. Left: landings by month pre- and post-catch share. Right: average price across states in catch share and control regions, weighted by volume. Vertical line represents date of program implementation.

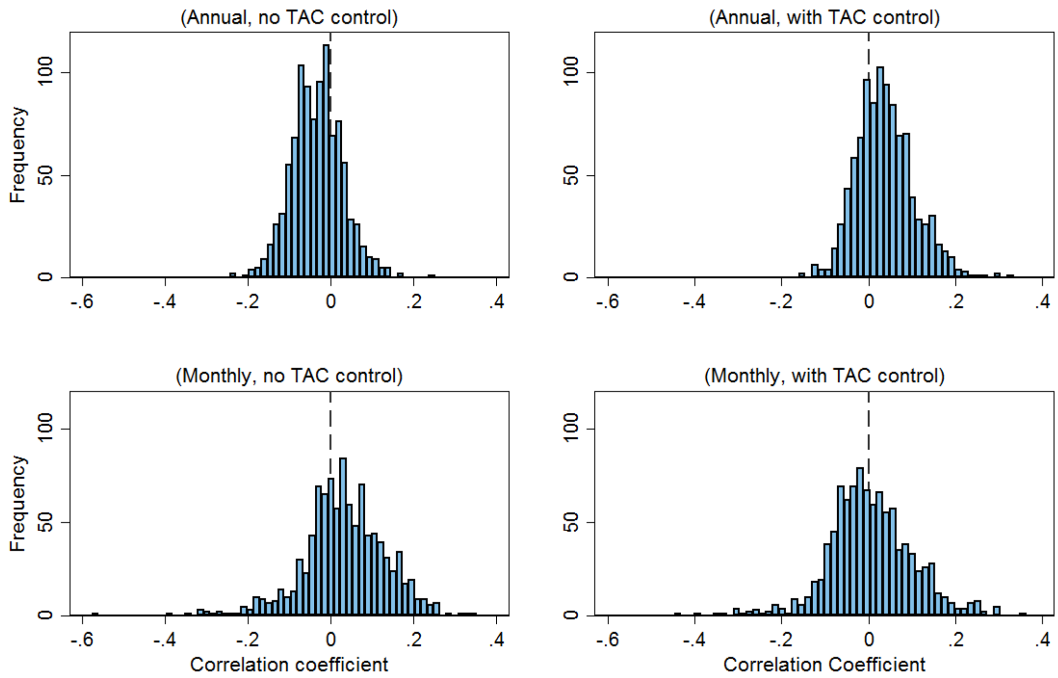


Figure 2-2: Monte Carlo analysis of correlation of price treatment effects and TAC treatment effect for four models: annual and monthly, with and without TAC as a control (column 1 correlation with columns 4, 6, 7, and 9 respectively in Table 2-6).

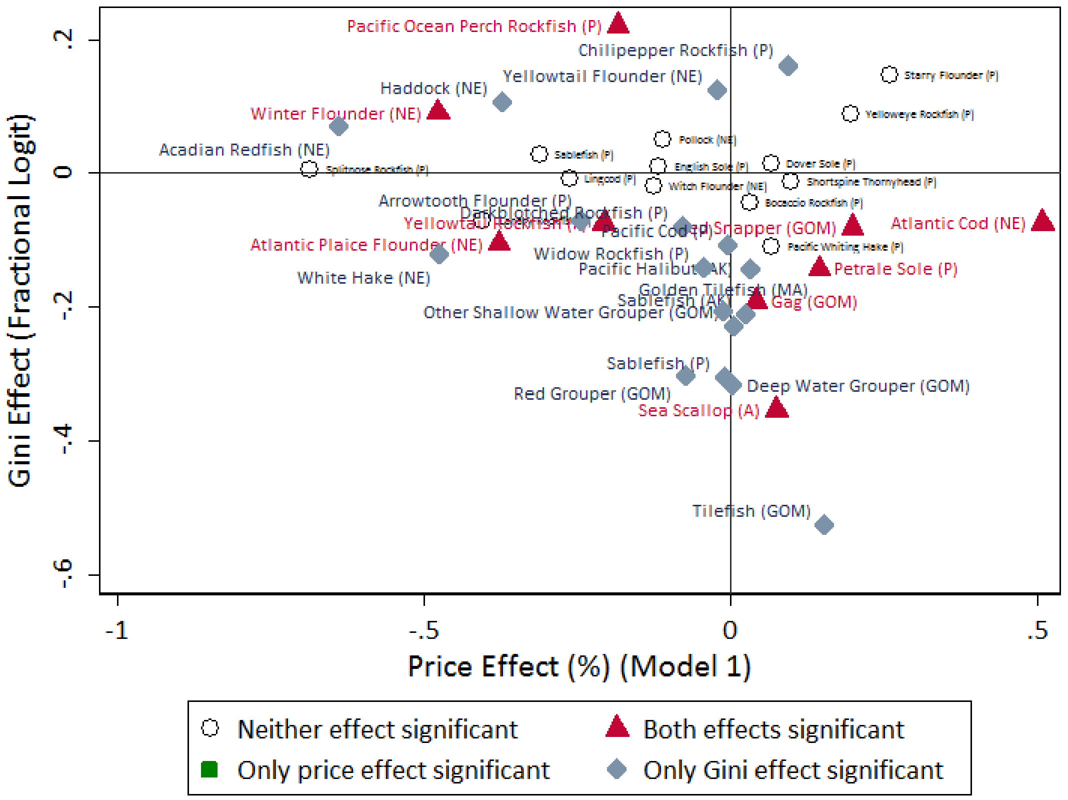


Figure 2-3: Scatter plot of season decompression treatment effects (Birkenbach et al., 2017) and price treatment effects estimated using an annual model (column 2, Table 2-6). A negative Gini coefficient treatment effect signifies season decompression, as the Gini coefficient is a measure of equality of landings across months of the year.

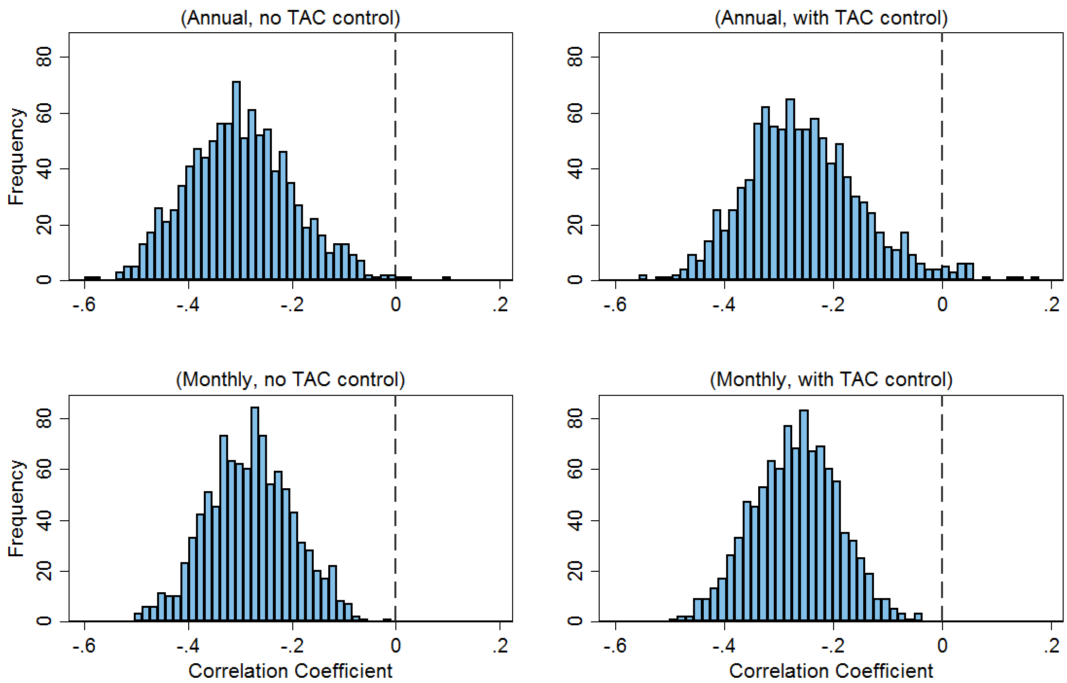


Figure 2-4: Monte Carlo analysis of correlation of price treatment effects and season length (Gini coefficient) treatment effects for four models: annual and monthly, with and without TAC as a control (price treatment effects from columns 4, 6, 7, and 9 respectively in Table 2-6; Gini treatment effects from Birkenbach et al. (2017).

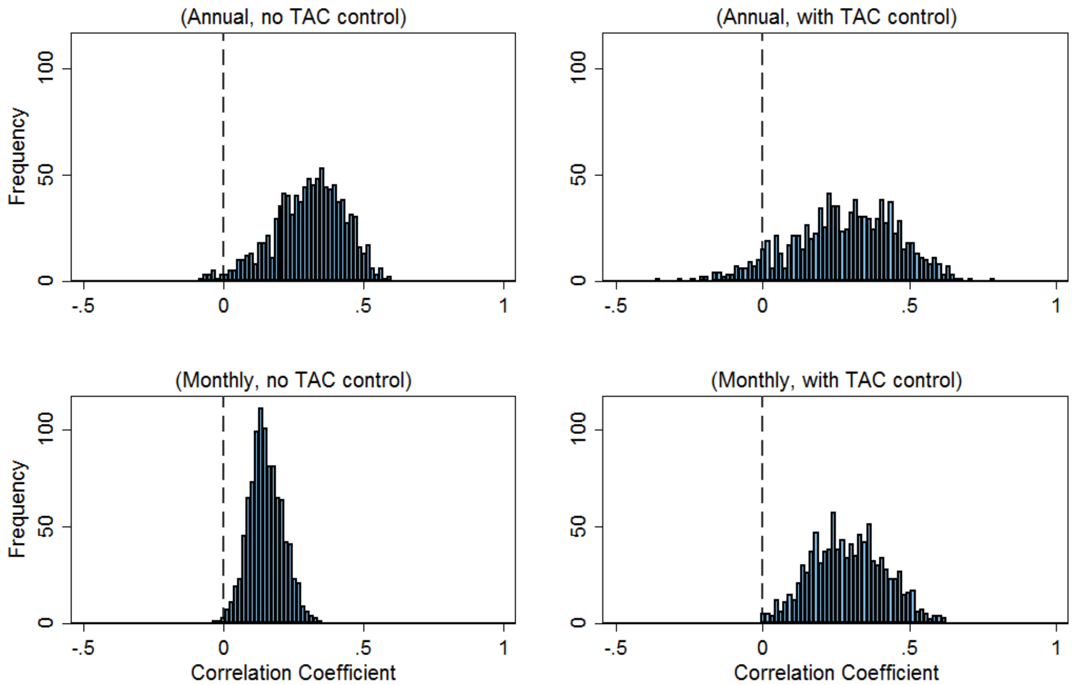


Figure 2-5: Monte Carlo analysis of correlation of price treatment effects and pre-treatment ex-vessel price for four models: annual and monthly, with and without TAC as a control (price treatment effects from columns 4, 6, 7, and 9 respectively in Table 2-6).

2.7. Tables

Table 2-1: Meta-analysis of ex-vessel price treatment effects.

Model (column in Table 2-6)	Weighting Scheme	Weighted Average Treatment Effect (%)	Weighted Variance	No. Fisheries
(4) State FEs Year FEs	Unweighted	-0.008	0.003	54
	1/Variance	**0.083*****	**0.000**	**54**
	Fishery Size (Pounds)	-0.079	0.010	54
	Fishery Size (Dollars)	0.025	0.002	54
	Pounds/Variance	-0.008	0.003	54
	Dollars/Variance	**0.095*****	**0.000**	**54**
(5) State FEs Linear time trend	Unweighted	-0.005	0.003	54
	1/Variance	**0.030+**	**0.000**	**54**
	Fishery Size (Pounds)	-0.078	0.011	54
	Fishery Size (Dollars)	0.030	0.003	54
	Pounds/Variance	-0.098	0.011	54
	Dollars/Variance	0.037	0.001	54
(6) State FEs TAC control	Unweighted	0.109	0.060	46
	1/Variance	**0.046+**	**0.001**	**46**
	Fishery Size (Pounds)	-0.078	0.017	46
	Fishery Size (Dollars)	0.023	0.004	46
	Pounds/Variance	-0.070	0.018	46
	Dollars/Variance	0.054	0.003	46
(7) State FEs Month FEs Year FEs	Unweighted	0.035	0.001	41
	1/Variance	**0.019****	**0.000**	**41**
	Fishery Size (Pounds)	**0.302*****	**0.008**	**41**
	Fishery Size (Dollars)	**0.198*****	**0.003**	**41**
	Pounds/Variance	0.046	0.001	41
	Dollars/Variance	**0.066*****	**0.000**	**41**
(8) State FEs Month FEs Linear time trend	Unweighted	0.035	0.001	41
	1/Variance	**0.019****	**0.000**	**41**
	Fishery Size (Pounds)	**0.302*****	**0.008**	**41**
	Fishery Size (Dollars)	**0.198*****	**0.003**	**41**
	Pounds/Variance	0.046	0.001	41
	Dollars/Variance	**0.066*****	**0.000**	**41**
(9) State FEs Month FEs TAC control	Unweighted	0.117	0.006	34
	1/Variance	0.011	0.000	34
	Fishery Size (Pounds)	0.013	0.007	34
	Fishery Size (Dollars)	0.020	0.003	34
	Pounds/Variance	0.015	0.003	34
	Dollars/Variance	**0.074****	**0.001**	**34**

Table 2-2: Meta-analysis of ex-vessel price treatment effects, differentiated by season decompression treatment effect.

	Season decompression (negative Gini treatment effect)			Season compression (positive Gini treatment effect)		
Model (column in Table 2-6)	**Weighted Average Treatment Effect (%)**	**Weighted Variance**	**No. Fisheries**	**Weighted Average Treatment Effect (%)**	**Weighted Variance**	**No. Fisheries**
Dep. Var: TAC						
(1)	**-0.1253*** **	0.0005	23	**0.3022*** **	0.0008	11
Dep. Var: Price:						
(4) State FEs Year FEs	**0.093*** **	0.0001	26	**-0.148*** **	0.0011	13
(5) State FEs Linear time trend	**0.0384** **	0.0004	26	**-0.1637*** **	0.0016	13
(6) State FEs TAC control	0.0125	0.0008	23	0.0362	0.0033	11
(7) State FEs Month FEs Year FEs	**0.0361*** **	0.0001	25	**-0.1152*** **	0.0005	13
(8) State FEs Month FEs Linear time trend	**0.0361*** **	0.0001	25	**-0.1152*** **	0.0005	13
(9) State FEs Month FEs TAC control	-0.0118	0.0003	22	0.0281	0.002	11

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

All ATEs weighted by inverse variance

Table 2-3: Meta-analysis of ex-vessel price treatment effects, differentiated by pre-treatment price.

	Above median pre-treatment price			Below median pre-treatment price		
Model (column in Table 2-6)	**Weighted Average Treatment Effect (%)**	**Weighted Variance**	**No. Fisheries**	**Weighted Average Treatment Effect (%)**	**Weighted Variance**	**No. Fisheries**
Dep. Var: TAC						
(1)	**-0.1125****	0.001	24	**-0.0423****	0.0003	22
Dep. Var: Price:						
(4) State FEs Year FEs	**0.0924*****	0	27	-0.0388	0.0007	27
(5) State FEs Linear time trend	**0.0608****	0.0004	27	-0.0427	0.0009	27
(6) State FEs TAC control	0.0364	0.0008	24	0.0651	0.0015	22
(7) State FEs Month FEs Year FEs	**0.0646*****	0.0001	22	**-0.1119*****	0.0003	19
(8) State FEs Month FEs Linear time trend	**0.0646*****	0.0001	22	**-0.1119*****	0.0003	19
(9) State FEs Month FEs TAC control	**0.0377+**	0.0003	19	**-0.0536+**	0.0008	15

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

All ATEs weighted by inverse variance

Table 2-4: Meta-regression of ex-vessel price treatment effects.

	Dep. Var: price treatment effects										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Price coef. of variation (pre-treatment)	-0.31				-0.32						
	(0.350)				*(0.393)*						
Gini coef. (pre-treatment)		0.32+			0.29+						
		(0.159)			*(0.173)*						
Primarily caught by catcher/processor vessels?			-0.03								
			(0.065)								
log fishery volume				0.01	0.02						
				(0.014)	*(0.016)*						
Gini TE						-0.45**	2.03	-0.25	-0.36	-0.44**	-1.05
						(0.151)	*(1.489)*	*(0.231)*	*(0.386)*	*(0.160)*	*(1.626)*
Gini TE * log fishery volume							-0.17				
							(0.104)				
Gini TE * Price coef. of variation (pre-treatment)								-2.26			
								(2.246)			
Gini TE * Gini coef. (pre-treatment)									-0.24		
									(0.846)		
Gini TE * Primarily caught by catcher/processor vessels?										-0.03	
										(0.431)	
Constant	0.07	-0.15*	0.03	-0.19	-0.34	-0.07+	-0.10*	-0.07	-0.08+	-0.07+	-0.08+
	(0.058)	*(0.068)*	*(0.032)*	*(0.204)*	*(0.231)*	*(0.039)*	*(0.040)*	*(0.042)*	*(0.041)*	*(0.040)*	*(0.040)*
R-squared	0.02	0.1	0	0.02	0.13	0.19	0.25	0.2	0.12	0.19	0.19
Number of obs. (fisheries)	39	38	54	54	35	39	39	36	38	39	39

Observations weighted by price treatment effect std. error. Estimates obtained from annual model (column 4, Table 2-6); + p<0.10, * p<0.05

2.8. Supplementary Section: List of Catch Share Fisheries Included in Study

Table 2-5: Summary of treated fisheries and matched controls.

Region	Program Name	Imp. Date	Species	Grouping	Comparison Region	Comparison Species/Product
Northeast	Mid-Atlantic Golden Tilefish IFQ Program	November, 2009	Golden tilefish *(Lopholatilus chamaeleonticeps)*	Golden tilefish	South Atlantic	Golden tilefish *(Lopholatilus chamaeleonticeps)*
	Northeast General Category Atlantic Sea Scallop IFQ Program	March, 2010	Sea scallop (IFQ portion only) *(Placopecten magellanicus)*	Sea scallop	Atlantic	Sea scallop (non-IFQ portion of fleet) *(Placopecten magellanicus)*
	Northeast Multispecies Sectors Program	May, 2010	Atlantic cod *(Gadus morhua)*	Atlantic cod	Canada (Atlantic)	Atlantic cod *(Gadus morhua)*
			Pollock *(Pollachius virens)*	Pollock	Canada (Atlantic)	Pollock *(Pollachius virens)*
			Haddock *(Melanogrammus aeglefinus)*	Haddock	Canada (Atlantic)	Haddock *(Melanogrammus aeglefinus)*
			Acadian Redfish *(Sebastes fasciatus)*	Acadian Redfish	Canada (Atlantic)	Acadian redfish *(Sebastes fasciatus)*
			White hake *(Urophycis tenuis)*	White hake	Canada (Atlantic)	White hake *(Urophycis tenuis)*
			Witch flounder *(Glyptocephalus cynoglossus)*	Witch flounder	Canada (Atlantic)	Witch flounder *(Glyptocephalus cynoglossus)*
			Winter flounder *(Pseudopleuronectes americanus)*	Winter flounder	Canada (Atlantic)	Winter flounder *(Pseudopleuronectes americanus)*
			Yellowtail flounder *(Limanda ferruginea)*	Summer flounder	Mid-Atlantic	Summer flounder *(Paralichthys dentatus)*
			American plaice *(Hippoglossoides platessoides)*	American plaice	Canada (Atlantic)	American plaice *(Hippoglossoides platessoides)*
Southeast	Gulf of Mexico Red Snapper IFQ Program	January, 2007	Red snapper *(Lutjanus campechanus)*	Red snapper	Gulf of Mexico	Vermilion snapper *(Rhomboplites aurorubens)*

	Gulf of Mexico Grouper-Tilefish IFQ Program[1]	January, 2010	Snowy grouper *(Epinephelus niveatus)*	Deep-water grouper	South Atlantic	Deep-water grouper (same species)
			Yellowedge grouper *(Epinephelus flavolimbatus)*			
			Gag *(Mycteroperca microlepis)*	Gag	South Atlantic	Gag *(Mycteroperca microlepis)*
			Black grouper *(Mycteroperca bonaci)*	Other shallow-water grouper	South Atlantic	Other shallow-water grouper (same species)
			Scamp *(Mycteroperca phenax)*			
			Red grouper *(Epinephelus morio)*	Red grouper	South Atlantic	Red grouper *(Epinephelus morio)*
			Blueline (grey) tilefish *(Caulolatilus microps)*	Tilefish	South Atlantic	Tilefish (same species)
			Golden Tilefish *(Lopholatilus chamaeleonticeps)*			
Northwest	Pacific Coast Sablefish Permit Stacking Program	August, 2001	Sablefish *(Anoplopoma fimbria)*	Sablefish	Canada (British Columbia)	Sablefish *(Anoplopoma fimbria)*
	Pacific Groundfish Trawl Rationalization Program	January, 2011	Pacific cod *(Gadus macrocephalus)*	Pacific cod	Canada (British Columbia)	Pacific cod *(Gadus macrocephalus)*
			Lingcod *(Ophiodon elongatus)*	Lingcod	Canada (British Columbia)	Lingcod *(Ophiodon elongatus)*
			Pacific hake (whiting) *(Merluccius productus)*	Pacific hake (whiting)	Canada (British Columbia)	Pacific hake (whiting) *(Merluccius productus)*
			Sablefish *(Anoplopoma fimbria)*	Sablefish	Canada (British Columbia)	Sablefish *(Anoplopoma fimbria)*
			Pacific Ocean perch *(Sebastes alutus)*	Pacific Ocean perch	Canada (British Columbia)	Pacific Ocean perch *(Sebastes alutus)*
			Widow rockfish *(Sebastes entomelas)*	Widow rockfish	Canada (British Columbia)	Widow rockfish *(Sebastes entomelas)*
			Bocaccio rockfish *(Sebastes paucispinis)*	Bocaccio rockfish	Canada (British Columbia)	Bocaccio rockfish *(Sebastes paucispinis)*
			Canary rockfish *(Sebastes pinniger)*	Canary rockfish	Canada (British Columbia)	Canary rockfish *(Sebastes pinniger)*

			Chilipepper rockfish *(Sebastes goodei)*	Chilipepper rockfish	Canada (British Columbia)	Yelloweye rockfish *(Sebastes ruberrimus)*
			Splitnose rockfish *(Sebastes diploproa)*	Splitnose rockfish	Canada (British Columbia)	Splitnose rockfish *(Sebastes diploproa)*
			Yellowtail rockfish *(Sebastes flavidus)*	Yellowtail rockfish	Canada (British Columbia)	Yellowtail rockfish *(Sebastes flavidus)*
			Shortspine thornyhead *(Sebastolobus alascanus)*	Shortspine thornyhead	Canada (British Columbia)	Shortspine thornyhead *(Sebastolobus alascanus)*
			Darkblotched rockfish *(Sebastes crameri)*	Darkblotched rockfish	Canada (British Columbia)	Darkblotched rockfish *(Sebastes crameri)*
			Yelloweye rockfish *(Sebastes ruberrimus)*	Yelloweye rockfish	Canada (British Columbia)	Yelloweye rockfish *(Sebastes ruberrimus)*
			Dover sole *(Solea solea)*	Dover sole	British Columbia, Canada	Dover sole *(Solea solea)*
			English sole *(Parophrys vetulus)*	English sole	Canada (British Columbia)	Dover sole *(Solea solea)*
			Petrale sole *(Eopsetta jordani)*	Petrale sole	Canada (British Columbia)	Petrale sole *(Eopsetta jordani)*
			Arrowtooth flounder *(Atheresthes stomias)*	Arrowtooth flounder	Canada (British Columbia)	Arrowtooth flounder *(Atheresthes stomias)*
			Starry flounder *(Platichthys stellatus)*	Starry flounder	Canada (British Columbia)	Starry flounder *(Platichthys stellatus)*
Alaska	Alaska Halibut IFQ Program	March, 1995	Pacific Halibut *(Hippoglossus stenolepis)*	Pacific halibut	Pacific	Pacific halibut *(Hippoglossus stenolepis)*
	Alaska Sablefish IFQ Program	March, 1995	Sablefish *(Anoplopoma fimbria)*	Sablefish	Pacific	Sablefish *(Anoplopoma fimbria)*
	American Fisheries Act (AFA) Pollock Cooperatives	January, 1999	Pollock *(Gadus chalcogrammus)*	Pollock	Pacific	Pacific hake (whiting) *(Merluccius productus)*

Bering Sea and Aleutian Islands Crab Rationalization Program	August, 2005	Red King crab *(Paralithodes camtschaticus)* Golden King crab *(Lithodes aequispinus)*	King crab	Russia	King crab imports (frozen)
		Snow crab *(Chionoecetes opilio)*	Snow crab	Canada	Snow crab imports (frozen)
Non-Pollock Trawl Catcher/Processor Groundfish Cooperatives (Amendment 80)	January, 2008	Atka mackerel *(Pleurogrammus monopterygius)*	Atka mackerel	Japanese export index created from top 5 fish exports (minimal processing) from Alaska to Japan (annual)	
		Pacific Ocean perch *(Sebastes alutus)*	Pacific Ocean perch	Pacific	Pacific Ocean perch
		Pacific cod *(Gadus microcephalus)*	Pacific cod	Pacific	Pacific cod
		Flathead sole *(Hippoglossoides elassodon)* Rock sole *(Lepidopsetta bilineata)* Yellowfin sole *(Yellowfin sole)*	Sole	Pacific	Sole (weighted average of dover sole and petrale sole)
Central Gulf of Alaska Rockfish Cooperatives Program	May, 2007	Pacific Ocean perch *(Sebastes alutus)*	Pacific Ocean perch	Pacific	Pacific Ocean perch
		Pacific cod *(Gadus microcephalus)*	Pacific cod	Pacific	Pacific cod
		Sablefish *(Anoplopoma fimbria)*	Sablefish	Pacific	Sablefish
		Shortspine thornyhead *(Sebastolobus alascanus)*	Shortspine thornyhead	Pacific	Shortspine thornyhead
		Northern rockfish *(Sebastes polyspinis),* Dusky rockfish *(Sebastes variabilis),* Shortraker rockfish *(Sebastes borealis),* Rougheye rockfish *(Sebastes aleutianus)*	Rockfishes	Pacific	Rockfishes (Boccacio, Widow, Canary, Splitnose, Yellowtail, Shortspine thornyhead, Darkblotched, Yelloweye)

Note: Fisheries with insufficient data for differences-in-differences analysis not shown. In cases where a pilot program or partial implementation took place before the full catch share program went into effect, the implementation date used in our analysis (that of full implementation) is shown. Some very minor species (mainly bycatch) belonging to the Pacific Groundfish Trawl Rationalization Program were excluded from analysis.

2.9. Supplementary Section: Descriptions of Fishery Management Systems[15]

To illustrate the rationale for matches, we provide brief descriptions of the programs managing our treatment and control fisheries (see Supplementary Section 2.8 for the full list of treatment-control pairs with scientific species names).

Northeast and Mid-Atlantic. The Northeast General Category Atlantic Sea Scallop IFQ Program, overseen by the New England Fisheries Management Council (NEFMC), is compared to the larger non-catch share sea scallop fishery managed by the same council. In 1994 a limited entry permit program was introduced that utilized days-at-sea (DAS) limits, harvest limits, and rotational area closures. Open access was maintained for smaller boats, a group described as the "General Category Scallop Fishery." Growth in the share of landings in this category prompted the implementation of the sea scallop IFQ Program in 2010. This IFQ program applied to the General Category (with some minor exceptions), which was also subjected to a landings limit of 5.5 percent of the total scallop catch limit (Brinson and Thunberg, 2013a). Our treatment-control pairing thus compares two different management regimes within the same fishery: scallop vessels that received catch share treatment and scallop vessels that did not.

The Northeast Multispecies Sector Program, also overseen by the NEFMC, was implemented in 2010. It has nine species under catch share management, all of which are included in our analysis (four additional species are managed by this program but without catch shares). Prior to the Sector Program these fisheries were managed with increasingly restrictive DAS restrictions and area closures (Holland et al., 2014a). An allocation of quota (and an

[15] The text of these descriptions, with exceptions, also feature in the supplementary information section of Birkenbach et al. (2017). Descriptions unique to this manuscript are American Fisheries Act (AFA) Pollock Cooperatives, Bering Sea and Aleutian Islands Crab Rationalization Program, Non-Pollock Trawl Catcher/Processor Groundfish Cooperatives (Amendment 80), and the Central Gulf of Alaska Rockfish Cooperatives Program.

associated opt-out privilege from some effort controls) was given in 2004 to a cooperative of voluntarily participating vessels for one stock of cod (Georges Bank). This was the initial version of the Sector Program, which was then extended to other species and stocks in 2010, largely replacing DAS restrictions (Brinson and Thunberg, 2013b; Holland et al., 2014a). By 2011, the Sector Program covered 99 percent of the total allowable catch (TAC) allocated to commercial fishermen for these species in the Council's region, and represented approximately 90 percent of the total harvest. We use fisheries of the same species in Atlantic Canada as reverse controls for Atlantic cod, pollock, haddock, Acadian redfish, white hake, witch flounder, winter flounder, and American plaice. Our comparison reverse controls are managed by Fisheries and Oceans Canada (DFO) under a TAC set separately for each management unit which is then shared across the fleets of the adjacent Maritime Provinces and Newfoundland and Labrador. Management units are defined by fleet, which are functions of boat size, distance from shore, and province, for each species. All management units utilize limited entry permits, size limits, gear restrictions, and season closures. In addition, catch shares were introduced in the 1990s (well before catch share introduction in our treatment fisheries) for species in certain management units, at either the individual or cooperative level (Melnychuk et al., 2012; OECD, 2004). Although not all management units received catch shares, treatment status in the reverse controls did not change during the relevant comparison time period (2007-2012). We use a U.S. Mid-Atlantic control fishery, summer flounder (which serves a similar market), for yellowtail flounder due to a lack of data available from the Canadian yellowtail flounder fishery. The fishery is managed by the Mid-Atlantic Fishery Management Council (MAFMC), using limited entry permits, gear and size restrictions, and season closures. Quota is sub-allocated between states within the Mid-Atlantic region (MAFMC, 2015).

The Mid-Atlantic Golden Tilefish IFQ program is also managed by the MAFMC. Prior to catch share introduction in 2009, the golden tilefish fishery was managed with a limited entry, tiered permitting system that allocated a proportion of the overall quota to each tier. Inclusion of fishermen within a tier was based on prior level of fishery participation. Implementation of catch share management was initially hindered by Congress's moratorium on catch shares, which was in effect from 1996 to 2004. However, fishermen in the full-time tier one category arranged sub-allocations of their tier's quota among themselves voluntarily (i.e., an informal catch share), allowing members to optimize harvest times with market conditions. Fishermen in other tiers were unable to come to a self-organized sub-allocation, leading to early closures of those parts of the fishery in some years. The cooperation of the tier-one fishermen, along with the failures of other tiers to cooperate, prompted the MAFMC to formalize and expand the catch share system in 2009 (Brinson and Thunberg, 2013b). We compare Mid-Atlantic golden tilefish to the same species caught in the South Atlantic where it is managed by the South Atlantic Fishery Management Council (SAFMC) using trip limits and season closures rather than catch shares. There are separate seasons for longline and hook and line categories (SAFMC, 2013).

Gulf of Mexico. The Gulf of Mexico Red Snapper ITQ Program was implemented by the Gulf of Mexico Fishery Management Council (GMFMC) in 2007. Previously the commercial harvest was regulated with limited entry permits, trip limits, and season closures, and faced overfishing, derby-style fishing conditions, and market gluts (GMFMC, 2006). Commercial quota was reduced by one third at the time of implementation. We match this fishery to the Gulf of Mexico vermilion snapper fishery (note that a comparison to the non-catch share red snapper fishery in the South Atlantic is problematic due to a moratorium on red snapper landings in that region during the study period). The vermilion snapper fishery is managed under the same GMFMC reef fish management plan as red snapper, but with size limits and season closures rather than catch shares (GMFMC, 2007, 2006).

The GMFMC's Grouper-Tilefish Program manages 13 species of shallow water groupers, deep water groupers, gag, red grouper, and tilefish species (NMFS, 2013). The program, which commenced in 2010, allocates individual quotas for fish categories rather than species, namely gag, red grouper, other shallow-water groupers, deep-water groupers, and tilefishes. Prior to program implementation, trip limits and limited entry permits failed to prevent quota overages and early season closures (Brinson and Thunberg, 2013b). We match the program's species categories to categories comprising the same species caught in the South Atlantic region, where they are managed by the SAMFC as part of the Snapper Grouper Management Complex. During the relevant time period, management in the control fisheries was based on limited entry permits, trip limits, area and season closures rather than catch shares (SAFMC, 2013).

Pacific Northwest. The Pacific Coast Sablefish Stacking Program, operated by the Pacific Fishery Management Council (PFMC), was implemented sequentially. Individual quota was attached to the pre-existing limited entry permit system in 1994 but did not alleviate early season closures due to quota allocations that were much higher than the TAC (i.e., incentives to race were maintained). Adjustments alleviated season constraints partially in 2001 and fully in 2002 [2]. Derby conditions were severe in the years preceding full implementation (Brinson and Thunberg, 2013b). The program covers only the fixed gear sablefish fishery (approximately one third of the total). Sablefish are also harvested in a large trawl fishery (covered by a different catch share program, described below) as well as smaller open access, trip limited, and tribal fisheries. Permits are "stacked," in the sense that one vessel may hold multiple permits representing a unit of quota. We use sablefish caught under British Columbia's Integrated Groundfish Program as a reverse-comparison, made possible by much earlier (1990) catch-share implementation in this region (note that an Alaskan comparison is infeasible because monthly Alaska data during the comparison period is unavailable) (Strauss, 2013).

The PFMC's Pacific Groundfish Trawl Rationalization Program was introduced in 2011. It consists of an ITQ program for a shore-based fleet and a cooperative program for at-sea mothership and catcher/processor fleets. The at-sea fleets focus on whiting, while the shore-based fleet is split between whiting and other groundfish species (with separate management provisions) (Holland et al., 2014b). Prior to the program, the shore-based non-whiting fleet was managed with two-month cumulative trip limits, season closures and effort restrictions. The trip limits reduced racing for target species but did not provide individual accountability for bycatch species (necessitating season closures and/or other restrictions). The mothership and shore-based whiting fleets were managed with season closures, leading to racing. The catcher/processor whiting fleet had already voluntarily formed cooperatives and was thus largely unaffected by the program's implementation (PFMC, 2010). In total, the program allocates quota for 25 species categories, of which we analyze 19 (those that represent individual species, are not affected by data limitations, and are not managed only as bycatch). The same species are harvested in our reverse-comparison, British Columbia's Integrated Groundfish Program. This program is an amalgamation of earlier individual vessel quota (IVQ) programs and other fisheries, including trawl-caught groundfish (an IVQ program implemented in 1997), halibut (1991) and sablefish (1990). Full implementation of catch shares for all commercial fisheries (including hook-and-line rockfish, lingcod, and dogfish) occurred in British Columbia in 2006, well before implementation of the Groundfish Trawl Rationalization Program (2011) (Strauss, 2013). A number of species in the Groundfish Trawl Rationalization Program (Pacific Ocean perch, canary, widow, darkblotched, cowcod, bocaccio, and yelloweye rockfishes) had relatively low quotas during the analysis period due to overfishing concerns.

Alaska. The Alaska Halibut and Sablefish Fixed Gear IFQ program, implemented in 1995, operates in the Bering Sea Aleutian Islands (BSAI) and the Gulf of Alaska with multiple area categories. Each species/areas category has its own TAC, set by the International Pacific Halibut Commission (IPHC) for halibut and North Pacific Fishery Management Council (NPFMC) for sablefish. In the years preceding catch share implementation, management relied on a combination of gear limits, area closures, and season closures. Season length shrunk to just a few days in the most important categories of the halibut fishery (National Research Council, 1999). We use as controls halibut and sablefish from the Pacific Northwest, neither of which used catch shares during the comparison time period (halibut still does not). PFMC sablefish TACs are allocated across trawl and fixed gear limited entry, tribal, and fixed gear open access fisheries. During the comparison time period, season closures were used to control the larger portion of harvests in the limited entry fisheries, with a trip limit used to manage a designated portion (PFMC, 1998, 1996). Halibut are managed jointly by the PFMC and IPHC. During the comparison time period the Pacific Northwest halibut fisheries comprised commercial long line, tribal, and recreational fisheries. A bycatch quota was provided to the groundfish trawl fishery. Management in all sectors used a combination of limited entry permits, area closures, size limits, and season closures (Williams and Blood, 2003).

The American Fisheries Act (AFA) Pollock Cooperatives program governs the largest fishery in the U.S. It is jointly managed by the NPFMC and the National Marine Fishery Service (NMFS). Implemented in 1998, the Cooperatives Program sets quota for inshore, offshore, and mothership Pollock sectors in the Bering Sea and Aleutian Islands (BSAI). Quota allocation with cooperative sectors is based on voluntary arrangements between members. Prior to the Program, the fishery was not overfished but did suffer from short seasons and competition between sectors. Management relied on season closures (NMFS, 2002). We use Pacific hake (whiting), in the U.S. Pacific Northwest, as a control. Pacific hake (whiting) is similarly a mild-flavored white fish, caught in large volumes, which serves similar fresh, frozen and intermediary product markets. During the relevant period the whiting fishery was managed using limited entry permits (PFMC, 2000).

The BSAI Crab Rationalization Program, implemented in 2005, manages harvests of Alaskan king crab, red crab, snow crab, and tanner crab. Crab fisheries are managed by the State of Alaska with oversight from the NPFMC. Management prior to rationalization relied on limited entry, size limits, and season closures (NMFS, 2004). Overfishing, stock collapses, and intense competition were features of the crab fisheries. Even following rationalization, tanner crab and blue king crab harvests required suspension for rebuilding for serval years (and hence these species are excluded from our analysis). We compare red and king crab to Russian king crab imports. Imports of frozen king crab constitute 45 percent of the total U.S. market. A majority of Russian king crab comes from the North West Pacific. The TAC is distributed to firms and community collectives, with additional season limit measures, but enforcement is poor and overfishing widespread (Ivanov, 2002). Our snow crab comparison is to Canadian imports of snow crab which primary serve the U.S. market. Snow crab are managed by Fisheries and Oceans Canada (DFO) using TACs set for different crab management areas in the Maritime Provinces and Newfoundland and Labrador. Catch shares were implemented in the 1990s (well before catch share introduction in our treatment fishery), and are supplemented by size limits, gear restrictions, and season closures (Melnychuk et al., 2012; OECD, 2004).

The Non-Pollock Trawl Catcher/Processor Groundfish Cooperatives (Amendment 80) Program is operated by the NPFMC and NMFS. The Program was implemented in 2008 and covers Atka mackerel, Pacific ocean perch, Flathead sole, Pacific cod, Rock sole, and Yellowfin sole harvested in the BSAI. The program facilitates the formation of voluntary cooperatives of inshore and catcher/processor trawl vessels. Prior to implementation, the fisheries were managed using limited access permits. Early season closures and high levels of bycatch were common (McIlwain and Hill, 2013). The program covers the majority but not the entire fleet, with non-cooperative vessels remaining under the limited access permit system. We compare Pacific ocean perch and Pacific cod to their equivalents caught without catch shares in the U.S. Pacific Northwest, where catch shares were not utilized before 2011 (see the section on the Pacific Groundfish Trawl Rationalization Program above). We compare the three sole species to a weighted composite of Dover and Petrale sole also caught in the U.S. Pacific west coast (no direct equivalent available). Atka Mackerel is almost all exported to Japan, so we compare to an export index of the top 5 minimally-processed, non-catch share exports from Alaska to Japan.

The Central Gulf of Alaska Rockfish Cooperatives Program was implemented in 2007 with a goal of reducing fluctuations in local employment due to short seasons. The program covers Pacific Ocean perch, Pacific cod, Sablefish and a variety of rockfishes, and like a number of Alaskan programs, relies on voluntary cooperative formation. Prior to the Program, the fisheries were managed using limited entry permits and season closures (Mcilwain and Hill, 2013). We use port of landing (Kodiak) to separate data from the same species caught under the Amendment 80 program. For controls, we use the same species in the U.S. Pacific Northwest (non-catch share during the relevant period), except for a number of rockfishes that lack a direct comparison. These rockfish are combined and compared to a composite of rockfishes from the U.S. Pacific Northwest.

2.10. Supplementary Section: Individual Fishery Results

Table 2-6: DID treatment effects (OLS regression) by individual fishery/fishery group: change in TAC and price per pound.

Program	**Species/Group**	Annual Model: TAC ('00,000 pounds)	Annual Models: Price per pound				Monthly Models: Price per pound			
Model No.		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Alaska Crab	King Crab	-163	0.91+	0.91	0.79	0.85	0.21			
		(-120)	*(0.45)*	*(0.97)*	*(0.92)*	*(0.99)*	*(1.11)*			
Alaska Crab	Snow Crab	429**	-0.06	-0.06	-0.03	-0.09	-0.42			
		(-98)	*(0.29)*	*(0.47)*	*(0.17)*	*(0.31)*	*(0.46)*			
Alaska Halibut	Pacific Halibut	-19	0.08	0.08	0.49***	0.51+	0.4	-0.17	-0.17	-0.19
		(-68)	*(0.13)*	*(0.74)*	*(0.10)*	*(0.31)*	*(0.27)*	*(0.13)*	*(0.13)*	*(0.36)*
Alaska Non-Pollock	Atka Mackerel		-0.13		-0.13	-0.15				
			(0.20)		*(0.19)*	*(0.15)*				
Alaska Non-Pollock	Pacific Cod	2213**	-0.02	-0.04	0.01	0.01	0.03			
		(-544)	*(0.06)*	*(0.14)*	*(0.04)*	*(0.08)*	*(0.15)*			
Alaska Non-Pollock	Pacific Ocean Perch Rockfish	2217**	-0.31	-0.11	-0.31	-0.17	0.63			
		(-544)	*(0.16)*	*(0.29)*	*(0.16)*	*(0.28)*	*(0.41)*			
Alaska Non-Pollock	Sole	2095**	0.08*	0.1	0.08*	0.12*	0.30***			
		(-547)	*(0.02)*	*(0.06)*	*(0.04)*	*(0.05)*	*(0.07)*			
Alaska Pollock	Pollock	1186	-0.03	-0.03	-0.03	-0.03	-0.02			
		(-2779)	*(0.03)*	*(0.03)*	*(0.02)*	*(0.03)*	*(0.03)*			
Alaska Rockfish	Pacific Cod	69	0.04	0.04	0.11	0.11	0.12			
		(-74)	*(0.04)*	*(0.12)*	*(0.11)*	*(0.10)*	*(0.13)*			

Alaska Rockfish	Pacific Ocean Perch Rockfish	27	0.04	0.04	0.04	0.04	0.04			
		(-18)	*(0.04)*	*(0.04)*	*(0.04)*	*(0.04)*	*(0.05)*			
Alaska Rockfish	Rockfishes	-62***	0.07	0.07+	0.13+	0.13*	0.13			
		(-5)	*(0.04)*	*(0.04)*	*(0.06)*	*(0.06)*	*(0.22)*			
Alaska Rockfish	Sablefish	-2	0.06	0.06	0.01	0.01	0.04			
		(-4)	*(0.17)*	*(0.33)*	*(0.29)*	*(0.27)*	*(0.33)*			
Alaska Rockfish	Shortspine Thornyhead	-26***	0.44*	0.44***	0.17	0.17	0.38			
		(-2)	*(0.11)*	*(0.09)*	*(0.72)*	*(0.66)*	*(3.71)*			
Alaska Sablefish	Sablefish	-120+	0.07	0.07	0.1	0.09	0.23	0.84**	0.84**	0
		(-54)	*(0.15)*	*(0.40)*	*(0.08)*	*(0.20)*	*(0.23)*	*(0.26)*	*(0.26)*	*(0.36)*
Atlantic Sea Scallop	Sea Scallop	-135*	0.71***	0.75	0.71***	1.06	2.87+	0.51***	0.51***	1.92***
		(-50)	*(0.06)*	*(1.14)*	*(0.06)*	*(0.83)*	*(1.49)*	*(0.12)*	*(0.12)*	*(0.46)*
GOM Grouper-Tilefish	Deep Water Grouper	1	0.01	0.01	-0.06	-0.06	-0.15	0.05	0.05	-0.05
		(0)	*(0.07)*	*(0.20)*	*(0.21)*	*(0.22)*	*(0.28)*	*(0.13)*	*(0.13)*	*(0.15)*
GOM Grouper-Tilefish	Gag	-25**	0.16*	0.16	0.39	0.39	0.15	0.15	0.15	-0.17
		(-6)	*(0.06)*	*(0.13)*	*(0.25)*	*(0.24)*	*(0.69)*	*(0.17)*	*(0.17)*	*(0.24)*
GOM Grouper-Tilefish	Other Shallow Water Grouper		0.02		0.19	0.2		-0.02	-0.02	
			(0.09)		*(0.26)*	*(0.25)*		*(0.22)*	*(0.22)*	
GOM Grouper-Tilefish	Red Grouper		-0.21+		-0.15	-0.16		-0.18	-0.18	
			(0.10)		*(0.24)*	*(0.24)*		*(0.17)*	*(0.17)*	
GOM Grouper-Tilefish	Tilefish	-1	0.29*	0.29	-0.03	-0.04	0.02	0.40*	0.40*	0.40*
		(-1)	*(0.10)*	*(0.20)*	*(0.30)*	*(0.29)*	*(0.29)*	*(0.17)*	*(0.17)*	*(0.19)*
GOM Red Snapper	Red Snapper	-17***	0.72**	0.72***	0.79***	0.80***	0.67	0.70***	0.70***	0.40**
		(-3)	*(0.12)*	*(0.14)*	*(0.18)*	*(0.18)*	*(0.53)*	*(0.09)*	*(0.09)*	*(0.14)*

MA Golden Tilefish	Golden Tilefish	-1	-0.04	-0.04	-0.05	-0.05	0.01	-0.08	-0.08	-0.1
		(-1)	*(0.17)*	*(0.23)*	*(0.26)*	*(0.28)*	*(0.32)*	*(0.16)*	*(0.16)*	*(0.16)*
MA Quahog	Ocean Quahog Clam		0.89		-0.01	-0.01		1.00*	1.00*	
			(0.68)		*(0.83)*	*(0.78)*		*(0.41)*	*(0.41)*	
MA Surfclam	Atlantic Surf Clam		0.76		0.74	0.74		0.89*	0.89*	
			(0.65)		*(0.44)*	*(0.44)*		*(0.42)*	*(0.42)*	
NE Groundfish	Acadian Redfish	53	-0.39	-0.39	-0.60*	-0.58*	-0.41+	-0.27	-0.27	-0.22
		(-61)	*(0.28)*	*(0.26)*	*(0.28)*	*(0.28)*	*(0.24)*	*(0.17)*	*(0.17)*	*(0.15)*
NE Groundfish	Atlantic Cod	72	0.91*	0.91*	0.50*	0.50+	0.38	0.89***	0.89***	0.74**
		(-109)	*(0.29)*	*(0.40)*	*(0.22)*	*(0.25)*	*(0.39)*	*(0.17)*	*(0.17)*	*(0.28)*
NE Groundfish	Atlantic Plaice Flounder	-16	-0.61*	-0.61+	-0.32	-0.33	-0.37	-0.35**	-0.35**	-0.35*
		(-14)	*(0.22)*	*(0.28)*	*(0.23)*	*(0.24)*	*(0.35)*	*(0.12)*	*(0.12)*	*(0.17)*
NE Groundfish	Haddock	-858	-0.49	-0.49	-0.44	-0.41	-0.55	-0.35+	-0.35+	-0.36
		(-699)	*(0.38)*	*(0.56)*	*(0.34)*	*(0.40)*	*(0.42)*	*(0.20)*	*(0.20)*	*(0.38)*
NE Groundfish	Pollock	144*	-0.08	-0.08	-0.08	-0.08	0.12	-0.08	-0.08	0.09
		(-47)	*(0.10)*	*(0.10)*	*(0.10)*	*(0.11)*	*(0.13)*	*(0.07)*	*(0.07)*	*(0.10)*
NE Groundfish	White Hake	47**	-0.55+	-0.55*	-0.53**	-0.53*	-0.68*	-0.56**	-0.56**	-0.56*
		(-14)	*(0.26)*	*(0.21)*	*(0.19)*	*(0.20)*	*(0.28)*	*(0.18)*	*(0.18)*	*(0.23)*
NE Groundfish	Winter Flounder	6	-0.95*	-0.95*	-0.68**	-0.68**	-0.62	-0.46**	-0.46**	-0.47*
		(-29)	*(0.28)*	*(0.34)*	*(0.23)*	*(0.23)*	*(0.51)*	*(0.15)*	*(0.15)*	*(0.23)*
NE Groundfish	Witch Flounder	-53+	-0.29	-0.29	-0.22	-0.2	0.14	-0.32	-0.32	-0.21
		(-25)	*(0.32)*	*(0.30)*	*(0.21)*	*(0.26)*	*(0.28)*	*(0.23)*	*(0.23)*	*(0.18)*
NE Groundfish	Yellowtail Flounder	-13	-0.04	-0.04	-0.47	-0.49	-0.32	-0.05	-0.05	-0.1
		(-14)	*(0.14)*	*(0.28)*	*(0.28)*	*(0.30)*	*(0.36)*	*(0.12)*	*(0.12)*	*(0.21)*
Pacific Groundfish	Arrowtooth Flounder	36	-0.03	-0.03	-0.03*	-0.03*	-0.03+	-0.03	-0.03	-0.02
		(-77)	*(0.02)*	*(0.02)*	*(0.01)*	*(0.01)*	*(0.01)*	*(0.02)*	*(0.02)*	*(0.02)*
Pacific Groundfish	Bocaccio Rockfish	-1	0.02	0.02	0.02	0.02	0.03	0	0	0.01

		(-1)	*(0.07)*	*(0.05)*	*(0.07)*	*(0.05)*	*(0.06)*	*(0.07)*	*(0.07)*	*(0.06)*
Pacific Groundfish	Canary Rockfish	-1	-0.22+	-0.22*	-0.23*	-0.23*	-0.23*	-0.21***	-0.21***	-0.22**
		(-2)	*(0.09)*	*(0.10)*	*(0.09)*	*(0.09)*	*(0.10)*	*(0.06)*	*(0.06)*	*(0.07)*
Pacific Groundfish	Chilipepper Rockfish	-15*	0.06	0.06	0.06	0.06	-0.04	0.11	0.11	0.04
		(-6)	*(0.21)*	*(0.18)*	*(0.21)*	*(0.19)*	*(0.24)*	*(0.16)*	*(0.16)*	*(0.17)*
Pacific Groundfish	Darkblotched Rockfish		-0.04		-0.01	-0.01		-0.05	-0.05	
			(0.07)		*(0.06)*	*(0.06)*		*(0.04)*	*(0.04)*	
Pacific Groundfish	Dover Sole	187	0.03	0.03	0.01	0.01	-0.02	0.03	0.03	0
		(.)	*(0.02)*	*(0.05)*	*(0.02)*	*(0.04)*	*(0.05)*	*(0.02)*	*(0.02)*	*(0.06)*
Pacific Groundfish	English Sole	107	-0.04	-0.04	-0.04	-0.04	-0.02	-0.03	-0.03	-0.01
		(-91)	*(0.04)*	*(0.04)*	*(0.04)*	*(0.04)*	*(0.04)*	*(0.02)*	*(0.02)*	*(0.04)*
Pacific Groundfish	Lingcod	-4	-0.21	-0.21	-0.22	-0.22	-0.22	-0.24**	-0.24**	-0.22*
		(-24)	*(0.13)*	*(0.13)*	*(0.16)*	*(0.15)*	*(0.15)*	*(0.07)*	*(0.07)*	*(0.11)*
Pacific Groundfish	Pacific Cod	-13***	0	0	0.02	0.02	1.38	0.02	0.02	2.39+
		(0)	*(0.06)*	*(0.11)*	*(0.06)*	*(0.07)*	*(3.06)*	*(0.04)*	*(0.04)*	*(1.33)*
Pacific Groundfish	Pacific Ocean Perch Rockfish	23***	-0.10*	-0.10*	-0.10*	-0.10+	0.33**	-0.10***	-0.10***	0.36***
		(-2)	*(0.03)*	*(0.04)*	*(0.03)*	*(0.04)*	*(0.07)*	*(0.02)*	*(0.02)*	*(0.06)*
Pacific Groundfish	Pacific Whiting Hake	1041	0.01	0.01	0	0	-0.01	0	0	0
		(-1229)	*(0.01)*	*(0.03)*	*(0.02)*	*(0.03)*	*(0.03)*	*(0.01)*	*(0.01)*	*(0.02)*
Pacific Groundfish	Petrale Sole	-11	0.18*	0.18	0.16**	0.16	0.09	0.18***	0.18***	0.07
		(-15)	*(0.05)*	*(0.14)*	*(0.05)*	*(0.10)*	*(0.07)*	*(0.05)*	*(0.05)*	*(0.08)*
Pacific Groundfish	Sablefish	-23	-0.64+	-0.64	-0.60*	-0.6	-0.41	-0.67***	-0.67***	-0.52+
		(-19)	*(0.27)*	*(0.48)*	*(0.20)*	*(0.35)*	*(0.37)*	*(0.17)*	*(0.17)*	*(0.31)*
Pacific Groundfish	Shortspine Thornyhead	-2*	0.08	0.08	0.01	0.01	0.02	0.13	0.13	0.16
		(-1)	*(0.33)*	*(0.29)*	*(0.28)*	*(0.26)*	*(0.34)*	*(0.20)*	*(0.20)*	*(0.26)*

Pacific Groundfish	Splitnose Rockfish		-0.27+		-0.27+	-0.27*		-0.26***	-0.26***	
			(0.10)		*(0.10)*	*(0.10)*		*(0.07)*	*(0.07)*	
Pacific Groundfish	Starry Flounder		0.14		0.08	0.06		0.29*	0.29*	
			(0.14)		*(0.24)*	*(0.24)*		*(0.14)*	*(0.14)*	
Pacific Groundfish	Widow Rockfish	11	-0.02	-0.02	0.04	0.04	0.07	-0.03	-0.03	-0.02
		(-8)	*(0.04)*	*(0.05)*	*(0.08)*	*(0.08)*	*(0.08)*	*(0.03)*	*(0.03)*	*(0.05)*
Pacific Groundfish	Yelloweye Rockfish	0	0.11	0.11	0.14	0.14	0.17	0.05	0.05	0
		(0)	*(0.17)*	*(0.17)*	*(0.17)*	*(0.17)*	*(0.17)*	*(0.22)*	*(0.22)*	*(0.20)*
Pacific Groundfish	Yellowtail Rockfish	-9+	-0.11*	-0.11**	-0.11	-0.11	-0.13	-0.12***	-0.12***	-0.09***
		(-5)	*(0.03)*	*(0.03)*	*(0.13)*	*(0.12)*	*(0.14)*	*(0.02)*	*(0.02)*	*(0.02)*
Pacific Sablefish	Sablefish	38	-0.02	-0.02	-0.01	-0.01	0.06			
		(-25)	*(0.16)*	*(0.32)*	*(0.20)*	*(0.27)*	*(0.31)*			
SA Wreckfish	Wreckfish	24	0.58***	0.58***	0.50***	0.48***	0.41+	0.76***	0.76***	0.78***
		(-46)	*(0.12)*	*(0.12)*	*(0.11)*	*(0.11)*	*(0.20)*	*(0.14)*	*(0.14)*	*(0.16)*
TAC Control		N/A	No	Yes	No	No	Yes	No	No	Yes
Year FEs		No	Yes	No	Yes	No	No	Yes	No	No
Month FEs		No	No	No	No	No	No	Yes	Yes	Yes
Linear Time Trend		No	No	No	No	Yes	No	No	Yes	No
State Fixed Effects		No	N/A	N/A	Yes	Yes	Yes	Yes	Yes	Yes

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Note: Three-year time windows before and after implementation used for all models. Where policies changed in January, only 6 calendar years of data were needed to have 3 full years before and after policy implementation for each treatment or control group (resulting in 12 observations total). Where policies changed in the middle of the year, 3 full years before and after policy implementation were included, plus the year of treatment, resulting in 7 observations per treatment or control group (14 observations total). Atlantic sea scallop has 12 observations because only 1 full year of data was available for the treatment group prior to the policy change. GOM = Gulf of Mexico, NE = North East, SA = South Atlantic. All models estimated using OLS; monthly models standard errors are calculated with Newey-West estimator.

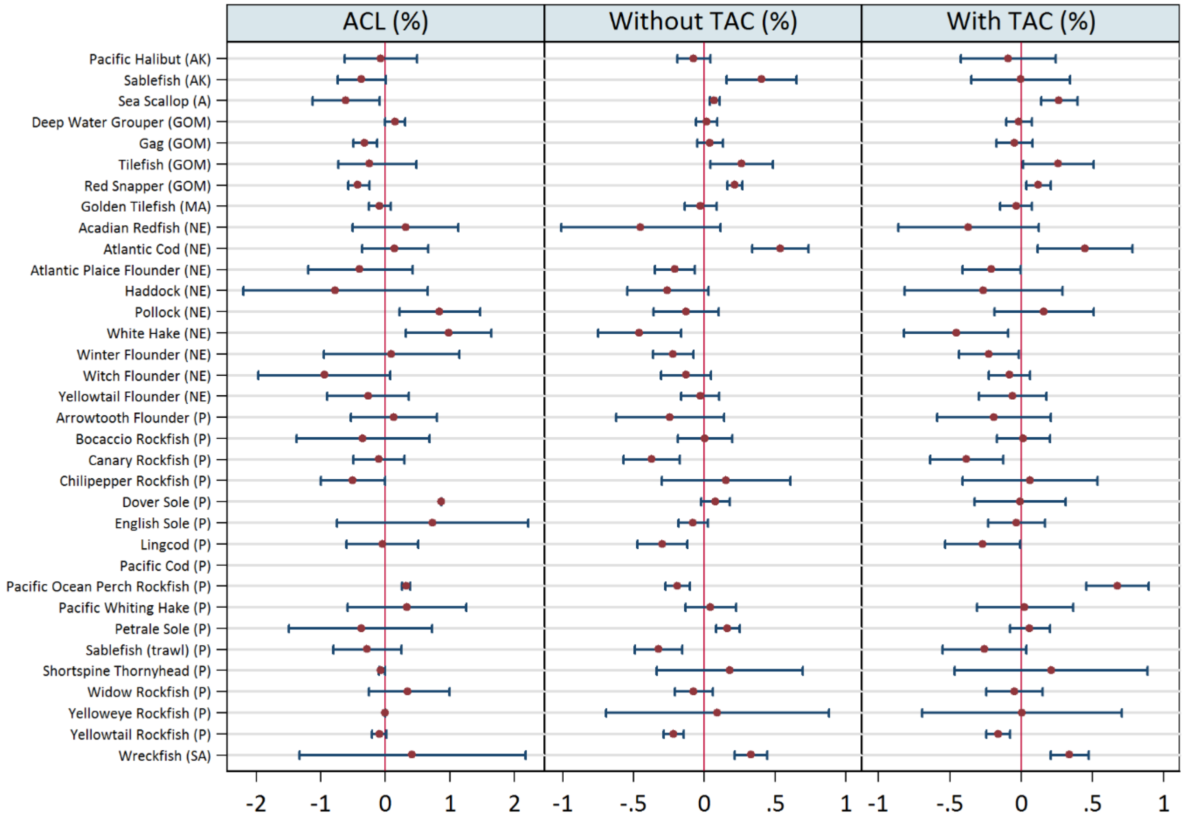


Figure 2-6: Average treatment effect for individual fisheries: TAC as the outcome variable (first column), and price with and without TAC control (second and third columns respectively). Figure includes U.S. catch share fisheries with sufficient monthly data. Markers show the average change in outcome (as a percent of the pre-treatment baseline) with 95 percent confidence intervals around point estimates. Model specifications correspond with columns 1, 7 and 9 respectively, in Table 2-6. Pacific cod is excluded due to suspected multicollinearity issues in the TAC model.

2.11. Supplementary Section: Pooled DID regressions

An alternative to the meta-analysis method of aggregating individual treatment effects is a pooled model, in which a treatment effect is estimated across fisheries, *i*:

$$P_{itk} = \alpha_i + \beta_1 POST_{it} + \beta_2 TREAT_{ik} + \beta_3 POST_{it} * TREAT_{ik} + \theta_t + \theta_{it} + \varepsilon_{itk}$$

Fishery pair fixed effects (α_i) control for time-invariant factors common to the treatment and control. Year fixed effects (θ_t) and year-fishery pair interactions (θ_{it}) control for time-varying factors that influence both treatment and control fisheries together within the larger set of fisheries included in the model. Given the large range of prices across fisheries, we use log price to derive an estimate of the overall treatment effect in percentage terms. Standard errors for monthly models are estimated either with the Huber-White variance estimator, robust to heteroscedasticity, or with Newey-West variance estimator, consistent in the presence of autocorrelation (with a lag time of 12 months).

We combine fixed effects and standard error types to give a range of specifications (Table 2-7). We also test model specifications with an interaction term between the pre-treatment average Gini coefficient, and the DID indicator (columns 5-9). This is motivated by our expectation that fisheries with the greatest potential for season decompression – those with a high Gini coefficient in the pre-treatment period – are those that should be most likely to show price increases

Results are non-significant in eight out of ten specifications, the remainder have signs counter to expectations. We do not draw conclusions from this model.

Table 2-7: DID treatment effects across individual fishery/fishery group (pooled model): change in price per pound.

	Annual models: Price per pound		Monthly models: Price per pound			Annual models: Price per pound		Monthly models: Price per pound		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Catch Share Region-Post Interaction	-0.0694+	-0.0694	-0.0366*	-0.0366	-0.0366	-0.0361	-0.0361	-0.0136	-0.0136	-0.0136
	(0.0404)	*(0.0445)*	*(0.0147)*	*(0.0330)*	*(0.0281)*	*(-0.0794)*	*(-0.1305)*	*(-0.0197)*	*(-0.0676)*	*(-0.0362)*
Pre-Period Gini-Catch Share Region-Post Interaction						-0.1699	-0.1699	-0.1071*	-0.1071	-0.1071
						(-0.1742)	*(-0.343)*	*(-0.0435)*	*(-0.1564)*	*(-0.0769)*
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Fishery Pair FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year*Fishery Pair FEs	Yes	Yes	No	No	No	Yes	Yes	No	No	No
SE Type	Robust	Robust	Robust	Robust	Newey-West	Robust	Robust	Robust	Robust	Newey-West
Cluster Variable	None	Fishery	None	Fishery	None	None	Fishery	None	Fishery	None
N	1559	1559	9462	9462	9462	1234	1234	9280	9280	9280

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

3. Increasing the Impact of Collective Incentives in Payments for Ecosystem Services[1]

3.1. Introduction

Payments for ecosystem services (PES) programs are voluntary contracts that offer transfers, usually cash, to landholders, conditional on agreed environmental actions or outcomes. Recent decades have seen rapid growth in PES programs (Ferraro, 2011; Porras et al., 2008), and in many lower income settings, PES have become a preferred policy approach due to their potential to better balance conservation and livelihood outcomes relative to other types of conservation policies (Ferraro and Kiss, 2002; Leimona, 2009; Pagiola et al., 2005; Sims and Alix-Garcia, 2017; Wunder, 2007).

Early PES typically made payments to individual landholders, for actions undertaken on land managed at an individual or household level (Kerr et al., 2014; Porras et al., 2008). Individuals can weigh the private costs of the actions required relative to the payment offered, to make a rational participation decision[2] (Ferraro and Kiss, 2002). This approach is well suited to contexts with individually-tenured land rights, however, the preponderance of communally titled land, particularly forestlands, means that PES are increasingly being implemented at a collective level[3]

[1] This chapter was coauthored with Alexander Pfaff (Sanford School of Public Policy, Duke University), Luz Rodriguez (School of Management, Universidad de Los Andes), and Elizabeth Shapiro-Garza (Nicholas School of Environment, Duke University).

[2] This is not the only model of PES, but it is a common aspiration (Wunder, 2007). Other models use rewards and social recognition to motivate a change in environmental attitudes, rather than market-based compensation to outweigh land use opportunity cost (Muradian et al., 2010).

[3] Globally, 27 percent of forest area in developing countries is under some form of community title. This proportion is likely to increase given ongoing efforts to devolve forest management to local community levels in many countries (Agrawal et al., 2008; Molnar et al., 2011).

(Hayes et al., 2017; Kerr et al., 2014). Collective PES involves contracts negotiated with groups of neighboring landholders, or with communities that hold land and resources under common title. Responsibility for contract fulfillment is collectivized, as is the distribution of rewards. This approach has hypothesized benefits in addition to more appropriately matching common land tenure arrangements. Group contracts can reduce transaction costs by replacing small individual contracts with fewer larger contracts (Kerr et al., 2014). They can better account for spatial interaction in the generation of ecoservices, for example, by preserving contiguous areas of habitat to match the needs of wildlife (Swallow and Meinzen-Dick, 2009). Group contracts may also help solve information problems: at larger scales it is easier to determine both baseline outcomes and program outcomes, making it possible to write contracts that are "additional" (i.e. achieve outcomes clearly improved relative to baseline conditions).[4]

Research on PES has largely focused on individually-targeted programs. The recent implementation of – and the large potential for – collectively-targeted PES highlight a need for PES research with a collective focus (Kerr et al., 2014). Of particular note is the need to determine how a community's social interactions – particularly the way a community self-organizes to provide public goods – influence responses to conditional PES incentives. Collective contract fulfillment requires that individuals accept private costs in order to achieve payments for the group. This requires collective action, which faces well-known barriers (Feeney et al., 1990; Ostrom, 1990). Furthermore, efforts to overcome these barriers can be hindered by externally imposed incentives (Bowles, 2008; Cardenas et al., 2000). As discussed below, these barriers potentially include the conditional incentives provided by PES.

[4] Programs which generate additional (above baseline) outcomes are described in this paper and throughout the PES literature as delivering "additionality" (Pattanayak et al., 2010; Wunder, 2007).

The clear potential benefits of conditional collective contracts, alongside the uncertainty of collective action, motivate our investigation into collective PES. We evaluate the impact of collective conditionality on ecoservices provision, as well as the modifying effect of social influences. We evaluate stylized PES policy variants using framed field-laboratory experiments in Mexico, at sites where collective land tenure is prevalent. Experiments are played by members of communities that participate in a new collective PES program run by Mexico's National Forestry Commission (CONAFOR). We do not evaluate ex-post outcomes of this program, instead we simulate outcomes with a voluntary contributions (VC) game. We use more- and less-demanding collective targets to alter conditionality. This provides experimental control and allows assessment of policy variants not present in the real program.

We first test whether increased conditionality increases collective contributions towards PES contracts. Theory generates ambiguous predictions. It is possible that conditionality will raise contributions via stronger monetary incentives. Yet in any public goods provision context, individual contributions are monetarily irrational. The lack of a monetary incentive is not fundamentally altered by increased conditionality. Motivation to contribute to collectives thus requires non-monetary "other-regarding" or "prosocial" preferences. These sources of motivation are known to be subject to "motivational crowding", a detrimental interaction of non-monetary and monetary incentives that leads to lower overall contributions (Bowles and Polanía-Reyes, 2012; Fehr and Falk, 2002; Frey and Jegen, 2001; Rode et al., 2015). As we discuss below (section 3.2.2), this mechanism could perversely lead to increased conditionality causing lower contributions in a collective setting.

Well-known influences on collective action motivate our additional focus on three elements of social interactions. First, groups can vary in their members' prosocial tendencies (i.e. their underlying cooperativeness). Second, groups can vary in the mechanisms they use to encourage cooperation. Third, groups can vary in their response to opportunities to participate in institutional design. Variation in these factors is likely to modify groups' responses to externally-imposed, collective incentives.

We hypothesize that collective conditionality's impact will be lower for those groups whose members initially show greater prosocial tendencies. This is expected if such groups' participants have greater non-monetary or intrinsic sources of motivation that can be crowded out by the newly imposed monetary motive. This is relevant for PES programs that target communities based on prior-observed collective behaviors, such as previous environmental actions. Targeting on observed behaviors also motivates our empirical test of the second element described above, namely, whether an internal coordination mechanism influences external incentives' impacts. This is inspired by our policy context: CONAFOR prioritizes communities with good governance for inclusion in PES programs. We hypothesize that an internal mechanism will allow a group to coordinate and thus better respond to an external incentive. Thirdly, we consider *choice* of contract conditionality. Political science literature suggests that group members' participation in rule-setting affects their response to such rules (Del Corso et al., 2017; Kroll et al., 2007; Wahl et al., 2010; Walker et al., 2000). We hypothesize that giving groups the option to veto an increase in conditionality will lead them to respond more positively to that increased conditionality. To provide context and improve external validity for our results, we complement our experiment with participant surveys, focus groups and interviews.

We find that increasing collective conditionality increases contributions, during the treatment and afterward. This is a novel contribution in the context of PES. While the public goods literature has proposed combinations of taxes and subsidies to similarly increase public goods provision (Bagnoli and Mckee, 2016; Cadsby and Maynes, 1999; Segerson, 1988; Segerson and Wu, 2006; Xepapadeas, 1991), these mechanisms do not translate well to voluntary PES contexts due to their reliance on random fines or budget imbalances. We return to this point in section 3.2.1 below.

Second, we find that participants who were initially less cooperative (i.e. those with lower prosocial tendencies) responded more to increased conditionality. We show that this response is not simply because those who initially give less have more room to improve. While we cannot test the precise mechanism, the response is consistent with a model in which intrinsically motivated individuals face partial motivational crowding out from new external incentives. This has implications for collective PES targeting, as well as for the likely success of programs that are expanding beyond highly-motivated, first-mover communities. We are not aware of previous studies that have demonstrated this possibility or explored its implications.

Similarly relevant for community targeting, we find that an internal mechanism (specifically the ability to monitor peers' contributions and issue sanctions) raises both baseline contributions and the impact of increased collective conditionality. We are not aware of any previous study that has explored how better internal group function interacts with collective PES conditionality. Finally, we find that giving participants a veto of the increase in conditionality raises the impact of that increased conditionality. It appears that just having a "voice" in rule-setting is capable of raising the social acceptability of the resulting rule, regardless of whether that voice is used to change the rule. This finding is of relevance for community consultation over contracts.

We proceed in section 3.2 with a review of relevant literature. Section 3.3 provides environmental and institutional context for our field laboratory experiment. Section 3.4 lays out our model and hypotheses in light of that context, and provides details of our experimental design. Sections 3.5 and 3.6 present our empirical analysis and experimental results, supported by insights from survey, focus group, and interview data. Section 3.7 discusses implications.

3.2. Relevant Literature

3.2.1. Collective Action and PES

Research specifically focused on collective PES is limited (Kerr et al., 2014).[5] Relevant conceptual insights, however, can be drawn from a substantial literature on common pool resource (CPR) management.[6] Ostrom (1999, 1990), Feeney et al. (1990), and Baland and Platteau (1996) described conditions in which groups successfully self-organize to manage collective resources. These conditions include: (1) a well-defined resource; (2) a small stakeholder group with shared norms and interdependencies; (3) governance seen as appropriate by those affected; and (4) matched scales between the resource and governing institutions (Agrawal, 2002). These suggest guidelines for effective collective PES design. First, it suggests targeting cohesive, internally cooperative communities with shared norms and interdependencies. Second, it suggests that if collective contracts are negotiated in a way that utilizes and reinforces existing community governance mechanisms, it might strengthen those institutions and promote successful outcomes. Third, it suggests that involving participants in contract design may add legitimacy relative to a top-down

[5] Empirical evidence includes a small number of program evaluations and case studies. Community payments reduced individuals' overuse of shared grazing lands in Ecuador, and increased community rule-setting around grazing lands (Hayes et al., 2017, 2015). Small community payments for reduced forest use in Madagascar improved attitudes towards monitoring and regulation-based conservation actions, but did not change observed behavior (Sommerville et al., 2010). Community payments led to greater support for a bird conservation program in Cambodia (Clements et al. 2010). Variation in policy design suitable for making causal claims is limited.

[6] We consider collective PES to represent a public good (PG) problem. All community members benefit from the collective payments (if they are shared evenly or spent on public goods), and from the ecoservices generated, yet securing those benefits requires costly individual effort. However, there are obvious parallels with CPR situations. Like collective PES, CPR problems require individuals to face private costs (restraint in resource use) for the sake of a social good (a more productive resource).

approach (Del Corso et al., 2017; Marshall, 2005; Reed, 2008). We test these suggestions explicitly in our experiment.

A closely-related problem is addressed by the non-point source pollution literature.[7] "Non-point source" describes pollution that cannot be traced to individual emitters (due to monitoring costs or environmental stochasticity), such that only ambient levels are measurable. Consequently, rules to reduce pollution must target groups of emitters. As in the collective PES situation, an efficient outcome requires mechanisms to align individual incentives with the social benefits of cooperation. Segerson (1988) and Xepapadeas (1991) proposed tax and subsidy mechanisms with this aim. A number of subsequent laboratory experiments (Alpízar et al., 2004; Camacho-Cuena and Requate, 2012; Spraggon, 2004, 2002) demonstrated the potential for these and related approaches to achieve social optima. However, they are not balanced budget mechanisms,[8] creating a potentially high financial burden on the regulator (in cases of subsidies) or on individuals (in cases of taxes). Balanced budget versions (see Xepapadeas, 1991) require random fines that pose fairness concerns in the PES context.[9] Later studies have tested variants of these mechanisms in concert with other ways of inducing cooperation. Poe et al. (2004) and Vossler et al. (2006) combined Segerson's (1988) model with group discussions. Cochard et al. (2005) incorporated a

[7] A helpful reviewer pointed out a further literature that could inform collective PES programs, that on that threshold solutions to public goods (PGs) problems. Threshold solutions elicit efficient PG provision by paying only when a group meets a contributions target. This is combined with a money-back guarantee that eliminates the risk of free riding (Bagnoli and Mckee, 1991; Cadsby and Maynes, 1999). This mechanism depart significantly from collective PES as implemented in Mexico, or elsewhere to our knowledge, and thus does not form the basis for our study.

[8] A balanced budget measure is one where the total tax or subsidy is equal to the social value of the change in behavior.

[9] See, for example, Alpízar et al. (2015) for evidence of fairness-based reactions in PES.

within-group cost of pollution, an approach mirrored in Reichhuber's et al. (2009) framed CPR game. Cason and Gangadharan (2013) combined the tax with peer-to-peer sanctions.

Unlike our experiment, most studies in this literature test policy instruments designed to motivate socially optimal outcomes under assumptions of self-interested behavior. As a result, in cooperative environments these studies tend to find over-compliance. By contrast, we deliberately use a monetary incentive insufficient to motivate monetarily rational contributions. This is a common feature of many PES programs, particularly in developing countries where programs have particularly limited budgets (Porras et al., 2008).[10] The resulting reliance on prosocial tendencies leads to the possibility of motivational crowding (and thus an ambiguous prediction for the sign of the average impact of greater conditionality). Similarly, this reliance on prosocial tendencies increases the likelihood that other elements of social interactions will matter, motivating our focus on these.

3.2.2. Interactions between Monetary and Non-Monetary Incentives

Many activities may be motivated by either intrinsic incentives (e.g. interest, sense of duty) or extrinsic incentives (monetary reward, punishment). For intrinsically-motivated activities, the addition of new extrinsic incentives can cause “motivational crowding,” the displacement of intrinsic motivation and possibly even a net loss of total motivation (Bowles, 2008; Bowles and Polanía-Reyes, 2012; Cameron et al., 2001; Deci et al., 1999; Festré and Garrouste, 2015; Frey and Jegen, 2001). The social psychology literature shows that the extrinsic incentives that have this effect are those that question autonomy or undermine opportunities for positive peer-recognition (see Ryan and Deci, 2000).

[10] Rodriguez et al. (2015) suggested that this is the case for many, if not all, the communities enrolled in the Mexican national PES program that is the focus of this study.

A number of studies have tested for motivational crowding in natural resource management contexts. Using field lab experiments, Cardenas et al. (2000) found that even a weakly enforced regulation crowded out social cooperation for firewood conservation. Velez et al. (2010) found regulation crowded out pre-existing social cooperation in fishing communities. Experiments by (Gelcich et al. (2013) found external sanctions to complement norms of group cooperativeness among fishers, but only for groups with high social capital. Most relevant to our study, Yañez-Pagans (2015) examined collective behavior under Mexico's PSA program and did not find net motivational crowding out from the existing payment. These studies suggest it is possible, but by no means certain, that the average impact of increased collective conditionality could be negative or diminished from what might be expected in the absence of motivational crowding. Sanctions, in the form of withdrawn payments, may lower contributions if motivation crowding outweighs or diminishes the motivation provided by the increased extrinsic incentive. In addition, we might expect a greater chance of such motivational crowding among higher baseline contributors (those with higher intrinsic motivation, and thus more to lose in the presence of increased external incentives). This further motivates our focus on baseline contributions as a modifier of conditionality's impact.

3.3. Setting

Mexico offers an ideal location to study collective natural resource management and social interactions. An estimated 60 percent of forest cover is found in *núcleos agrarios* (Madrid et al., 2009), a form of collective co-governance where land belongs to the state but is managed at a local level. Usufruct rights are granted by the community to individuals for housing plots and agricultural purposes, while forests and large scale pasture lands remain under collective use. Management activities of these areas is conducted through communal labor, coordinated by community governance institutions (Schroeder and Castillo, 2013). Central to the functioning of these systems is community government, whose state-mandated form (although not quality) is consistent across communities. A general assembly of all land rights holders *(Asamblea)*, an executive committee *(Comisariado)* and a supervisory council *(Consejo de Vigilancia)* oversee allotments of individual parcels, land management decisions, and the management of community-allocated subsidy programs including PES programs (Barnes, 2009).

Mexico faces ongoing deforestation, including in ecologically important primary forests, predominantly due to conversion to cropping and pasture[11] (Muñoz-Piña et al., 2008). Mexico also has a high level of biodiversity, ranking among the top five countries globally for endemism of vascular plants and vertebrate species (Mittermeier et al., 1998; Myers et al., 2000). A large rural population facing relative socio-economic disadvantage (Brandon et al., 2005), implies tensions between agricultural land uses and conservation. PES arguably represents a sensible policy response given then need to balance social and environmental goals.

In 2003, CONAFOR introduced a national PES program, *Pagos por Servicios Ambientales* (PSA). This aims to improve water quality via forest conservation, while also reducing rural poverty (McAfee and Shapiro, 2010). Participants receive contracts for 5-year, per-hectare conditional subsidies in locations deemed important for ecoservices provision.[12] PSA is one of the largest PES programs in the world, with about 3.4 million hectares enrolled from 2003-2011 (Shapiro-Garza, 2013). In 2008, 45 percent of recipients were *núcleos agrarios* (Yañez-Pagans,

[11] From 1990 to 2015, Mexico's net change in total forest cover was -5.6 percent. It lost 19.3 percent of its primary forest, an annual rate of ~1 percent. In 2015, 34 percent of Mexico (66,040,000 ha) was covered by some form of forest, of which about half was primary forest. Deforestation rates have decreased in each five year period since 1990 (FAO, 2015).

[12] Recipients are expected to fence forests to exclude stock, prevent illegal logging, collect garbage, create fire breaks, dig water infiltration ditches, and build retention walls to prevent soil erosion, among other activities.

2015). A sub-program of the PSA, *Fondos Concurrentes* (FC), was implemented in 2008 to facilitate locally-designed PES agreements between downstream ecoservices beneficiaries (e.g., utilities and businesses) and upstream land owners. Federal funding is provided to cover half of the payments (FONAFILIO et al., 2012). Although the FC program supports the development of PES initiatives for multiple ecoservices, most of the agreements are targeted towards water provision. While contracts vary across locations, given that they are negotiated between upstream and downstream parties, minimum standards and the overarching framework is imposed by CONAFOR.

Our model and experiment reflect key elements of this PES approach from the upstream (provider communities') perspective. We use a graduated sanctioning mechanism, reflecting the fact that if deforestation is detected (by satellite monitoring and annual field visits), payments are reduced proportional to the percentage of the total area lost. The finite 5-year contract period motivates our test of both treatment and post-treatment impacts. Many FC contracts are negotiated by civil society associations *(socios locales)* and are ratified by community *asambleas*, prompting our focus on participation in rule-setting. Further real-world motivation is explained in the model and experiment design sections below.

3.4. Hypotheses and Empirical Approach

3.4.1. Model

Our model of collective PES follows the structure of a voluntary contributions (VC) game. Contributions g, by individuals i, with endowments y, help a community (of size n) fulfill a PES contract. In return, the contract pays the community an amount a, proportional to the sum of contributions made.[13] Individual payoffs, π_i, are given by:

$$\pi_i = y - g_i + \frac{a}{n}\sum_{i=1}^{n} g_i$$

As in standard VC games, this setup implies optimal individual contributions of zero (assuming $\frac{a}{n} < 1$), regardless of the contributions made by other group members. Increased conditionality is achieved by levying a sanction, *f*, which scales with the discrepancy between total group contributions and a target, *T*. The marginal, rather than lump-sum nature of this sanction is consistent with both the FC program (described above) and many PES programs elsewhere. (Individuals or communities that fail to uphold a contract on a proportion of enrolled land typically lose payments for that proportion.) Given that an individual's marginal payoff is negative in the baseline setup, the effect of the sanction is to lower a contribution's marginal loss, up to the level of the target.[14] We use two levels of target, *T*. Our higher *T* requires *additional* contributions, relative to baseline, from all groups (i.e. motivates additionality).[15] Given the sanction, the individual payoff function becomes:

---

[13] The public good we focus on is the collective contract payment, although we note that collective direct benefits from the forest itself could play the same role. Focus group discussions in our study sites indicated that communities perceived direct benefits from forest conservation including cleaner spring water and ecotourism benefits. These do not involve rival extraction choices, and thus we do not use a CPR game.

[14] A helpful reviewer pointed out that a payment increase could achieve the same change in marginal incentives as this sanction. Whether that would have the same impact is an interesting empirical question (as sanctions and rewards differ in framing for a given incentive) but one that is beyond the scope of this paper. We consider a sanction to be consistent with the real-world policy approach typically taken for contract non-compliance.

[15] Since as in any experiment − academic or policy − we do not observe counterfactual behavior in the absence of treatment, we cannot say with certainty that a given treatment target, *T*, motivates additional contributions.

$$\pi_i = y - g_i + \frac{a}{n}\sum_{i=1}^{n} g_i - \frac{f}{n} * \max\left(0, T - \sum_{i=1}^{n} g_i\right)$$

Which still implies that zero individual contributions are the individual optimum (assuming $\frac{a+f}{n} < 1$). Within the model, of course, such a result is a function of our parameter choices. Yet it is conceptually consistent with collective PES as experienced in our study sites.

Consequently, positive contributions must depend at least partially on non-monetary returns to giving, such as altruism, reputation, or other prosocial concerns (Andreoni, 1993; Chan et al., 2002; Masclet et al., 2003).[16] Salience, *s*, may also matter, by providing higher reputational benefits if an individual's contributions are known to others. Following Levitt and List (2007), we assume an additive utility function with monetary and non-monetary components. Omitting the sanctions, we have:

$$U(\pi_i, m) = s * m(g_i) + y - g_i + \frac{a}{n}\sum_{i=1}^{n} g_i$$

With the first order condition giving an optimum contribution of:

$$g_i = m_{g_i}^{\prime -1}\left(\frac{1 - \frac{a}{n}}{s}\right)$$

In practice, it is clear that our higher T does require additionality, as revealed by comparison to control groups' contributions and to treated groups' contributions immediately prior to treatment. Our low *T* does not. *T* levels were selected based on piloting. This approach to choosing contract targets is feasible for real PES design.

[16] This is consistent with real experience under the FC program. Surveyed participants make considerable voluntary contributions of time, effort and resources to their communities. For example, 31 percent have held leadership positions, 61 percent attend at least one collective meeting per year, and 56 percent households have a member who made a voluntary contribution of time to forest-related work activities.

Where $m_{g_i}'^{-1}(.)$ is the inverse of the non-monetary component of utility. If $m'(g_i) > 0$ and $m''(g_i) < 0$, an internal local maximum ($g_i > 0$) can exist. Greater salience can increase the utility maximizing quantity of g_i. The imposition of a sanction will also increase g_i under these assumptions, as seen in the resulting first order condition:

$$g_i = m_{g_i}'^{-1}\left(\frac{1-\frac{a+f}{n}}{s}\right).$$

This utility function features linear monetary but non-linear non-monetary returns to contributions. The former is a reasonable assumption for transfers at the scale of a laboratory experiment, though it might not be strictly true for transfers at the scale of significant PES for poor households. The relevant assumption, however, is simply greater relative curvature of non-monetary returns. This is supported by behavioral game theory and results from standard experiments.[17]

To summarize, a collective sanction conditional on aggregate contributions of a group can increase the equilibrium amount of individual contributions g_i, provided that there are non-monetary returns. We expect this outcome even though monetary marginal returns remain negative for the individual. We can infer that non-monetary returns are present from contributions made in the baseline round of our experiment. This is the basis for hypothesis 1: increasing conditionality encourages greater contributions. However, as discussed in section 3.2.2, it is possible that the introduction of externally imposed incentives motivationally crowds out non-monetary returns. If

[17] The ultimatum game, for example, shows that perceived fairness can shift with relatively small changes in contributions, triggering dramatic changes in prosocial preferences. In the provision of public goods, group members often make peer comparisons to determine what is fair. Thus, even relatively small changes in contribution levels, such as moving from below what others contribute to moving above what others contribute, can greatly shift non-monetary returns to g_i.

$m = m(g_i, f)$ where $m_f'(g_i, f) < 0,$ the presence of external sanctions reduces $m(g_i)$ for $\forall g_i > 0$. This makes the sign of net impacts from increased conditionality uncertain.

We next consider baseline heterogeneity in non-monetary returns. Communities participating in FC differ considerably in the extent of their apparent cooperation for provision of local public goods (as we expect is the case for communities in any diverse PES setting). Consider two types of participant, differentiated by high (A) and low (B) non-monetary-returns to cooperation such that $m_A(g_i) < m_B(g_i)\ for\ \forall g_i$. The functional form of $m(.)$ determines an individual's response to the sanction. We do not speculate on the relative differences of the functional form for high and low types, except to note that if $m_A''\left(g_i^{A*}\right) > m_B{}''\left(g_i^{B*}\right)$, where g_i^* is an optimum private contribution, the increase in g_i^{A*} due to the sanction will be greater than the increase in g_i^{B*}. Alternatively, or in addition, it is possible that $m(.)$ decreases in the presence of external sanctions due to motivational crowding out, as discussed above. Given that high types by definition have greater intrinsic (non-monetary) motivation, it is likely they would see a greater decrease in their non-monetary-returns from a sanction's imposition than would low types, i.e. $m_{A}{}'_{f}(g_i, f) > m_{B}{}'_{f}(g_i, f)$. This implies that the increase in g_i^{B*} due to the sanction will be larger than the increase in g_i^{A*}. This is the basis for <u>hypothesis 2</u>: groups with lower initial prosocial tendency will respond to increased conditionality with a relatively larger increase in contributions, all else equal.

We can also use the non-monetary component of utility $m(g_i)$ to propose a hypothesis regarding the impact of participation in rule-setting. Increased participation in an institution's design and implementation could engender greater buy-in and thus support for the institution's goal (Del Corso et al., 2017; Frey and Stutzer, 2006; Kroll et al., 2007; van Noordwijk and Leimona, 2010; Wahl et al., 2010; Walker et al., 2000). Extending once again our utility function and indicating choice in rule-setting by c, we have $m = m(g_i, f, c)$ where $m_c'(g_i, f, c) > 0$. In this case, non-monetary returns would be greater (for $\forall g_i > 0$), such that we would expect individuals' responses to an incentive to be more positive when they had the chance to participate in rule-setting. This is the basis for hypothesis 3: individuals who are given the opportunity to veto a conditionality rule will respond more positively to that rule.

Finally, we consider the impact of an internal coordination mechanism. Some group situations permit individuals to see their peer's actions and issue individually-targeted punishments. If we assume that greater penalties (*p*) are levied on those who make "insufficient" contributions in the eyes of their peers, providing those contributions are salient (s), i.e. $p_g'(g_i, s) < 0$, then the utility function becomes:

$$U(\pi_i, m) = s * m(g_i, f, c) + y - g_i + \frac{a}{n}\sum_{i=1}^{n} g_i - \frac{f}{n} * \max(0, T - \sum_{i=1}^{n} g_i) - p(g_i, s).$$

This implies:

$$\frac{dU}{dg} = s * m_g'(g_i, f, c) - p'(g_i, s) - 1 + \frac{a-f}{n}$$

This gives a larger optimum, g_i^*, under both the collective sanction condition and baseline condition, than under the equivalent situations without the internal mechanism. The role of salience, *s*, is also apparent: publically revealing contributions encourages greater cooperation both by increasing the non-monetary component of utility and via the penalty mechanism. This forms the basis for hypothesis 4: groups in which individual members can monitor and respond to others' contributions will contribute more, both in the baseline and in response to increased collective conditionality.

3.4.2. Experiment

Framed field-lab experiments allow the study of potential institutions with relevant populations and with experimental control. Settings and policies are stylized, yet are incentivized and framed in ways analogous to reality (Harrison and List, 2015). They provide a means to simulate the impacts of policy changes that cannot otherwise be easily observed (or do not yet exist). The use of real payments increases incentive compatibility relative to hypothetical questionnaires.

3.4.2.1. The Voluntary Contributions (VC) Game

We used a VC game to operationalize conflict between self and collective interests, framed as a forest-conservation PES. Key features are motivated by characteristics of the FC program. Each group had five participants, each of whom were endowed with 10 cards said to represent time that could be spent on community activities (reforestation, forest management) or on oneself and one's family. Participants were told that community activities improve the quality of community forests and thus generate a PES payment to the community, which is shared equally among the group members.[18] We employed visual aids and detailed examples. A verbal quiz was administered before the game commenced to ensure all participants intuitively understood the payoff function. In the absence of sanctions or internal mechanism treatments, this function is:

$$\pi_i = 10 - g_i + \frac{2}{5}\sum_{i=1}^{5} g_i$$

The dominant individual strategy is always to contribute zero to the group. However, the social optimum was achieved if all participants in the group contributed their entire endowments. Group composition was quasi-anonymous. Three groups played in the same room, so that participants could not know which of the 15 people present were part of their group of 5.

3.4.2.2. Treatments

The game lasted for 12 rounds. Increased conditionality for treatment groups was implemented in rounds 5-8, to test hypothesis 1: that greater conditionality increases collective contributions. As

[18] Focus group evidence suggests that this stylized scenario is realistic. Participants identified "the entire community", or community segments based on tenure status or particular communal working groups, as responsible for forest conservation. Similarly, benefits from PES payments and from forest products were widely perceived as accruing to the community collectively.

described in section 3.4.1, conditionality was increased by imposing a sanction equally on all group members if a target T was not reached. The sanction amount was twice the collective shortfall.[19] This modifies the payoff function to:

$$\pi_i = 10 - g_i + \frac{2}{n}\sum_{i=1}^{5} g_i - \frac{2}{5} * \min\left(0, T - \sum_{i=1}^{5} g_i\right)$$

Pre-treatment rounds 1-4 provide a within-group baseline. Post-treatment rounds 9-12 test for persistent post-treatment impacts. Treatments featured two different target levels, $T \in (20, 50)$, chosen to imply conditionality on *additional* and *non-additional* levels of collective contributions, respectively (see footnote 15). Participants learned of their group's collective contributions, any sanctions, and their resulting payoffs, after each round, via their confidential payoff sheet. Experimenters explained these results verbally to each participant (but without revealing individual payoffs to other players) when updating payoff sheets.

A sorting procedure was used to test hypothesis 2, that increased conditionality has a bigger impact on lower initial contributors. The 15 participants per session were sorted into three groups of 5 participants each, based on their initial ('round zero', or R_0) contributions. Group type A comprised the 5 participants with the lowest R_0 contributions, B comprised the 5 middle-ranking participants, and C the highest. Participants were not told the basis for this sorting, did not know who their initial or resorted group members were, and did not know that the game would repeat after the first round.

Varying levels of group participation in rule-setting were used to test hypothesis 3, that participation in rule-setting increases the response to a rule. Treatments featured three levels of participation in the choice of T: (1) experimenters chose one of the two T options without input

[19] As previously discussed, scaling rather than threshold sanction is a stylized version of the approach used by CONAFOR in cases of partial contract non-fulfillment (see section 3.3).

from participants; (2) a randomly selected participant chose one of the two *T* options; or (3) a randomly selected participant chose one of the two *T* options, which was then put to a (confidential) vote immediately before implementation. In the vote case, *T* was implemented only if it was endorsed by a majority of the group.[20]

Hypothesis 4 (that a means for internal coordination could increase the impact of increased conditionality) was tested using a subset of sessions that featured an exogenously specified "internal mechanism." This provided all players anonymized information about group members' contributions, plus the ability to show disapproval by issuing anonymous penalties to other players. Penalties cost 1 unit to send and 2 units to receive. Table 3-2 shows all treatment combinations.

3.4.2.3. Participants

Four hundred and thirty five participants (and approximately 60 more in pilot sessions), from 15 FC-participating communities too part. Communities were clustered around 4 locations, one each in the states of Jalisco and Colima, and two in Oaxaca (Figure 3-1). We partnered with 3 civil society associations (INDAYU and FARCO in Oaxaca, MABIO in Colima) and, directly, with one *núcleo agrario* (*Ejido* San Agustín in Jalisco). Sites were selected based on discussions with CONAFOR staff and exploratory field visits in 2013, with primary criteria being collective properties enrolled in FC between 2008-2013, and a focus on hydrological services.

[20] To credibly contextualize these participation mechanisms, framing differed slightly between treatments. A lack of choice in (1) was described as a CONAFOR decision and justified on a basis of forest conservation. Randomly selected participant choice in (2) was described as a choice made by a local NGO, interested either in increasing rural incomes or forest conservation. The vote in (3) was described as part of negotiation between a local NGO and the community. Impacts of framing differences, independent of the impact of the participatory mechanism, are discussed in section 3.6.2.1.

Treatments were applied approximately evenly in each region, and randomized within region.[21] While early participants could conceivably communicate with later participants, this possibility was minimized in three out of four regions by only undertaking one or two sessions in each community (each region except San Agustin has multiple communities) and by requesting participants not speak about game details to others. We followed up our experiment with a survey and focus group discussion with participants, as well as interviews with community leaders to gather contextual data. Individual participants' earned between MXN 180-300 (USD 12-20), depending upon performance.

Participant characteristics varied across sites (Table 3-1). Almost two-thirds of participants had lived in their current community all their lives. About half are women and about half farm as their primary source of income. The average time spent in formal education is just under eight years. In terms of forest work, participants' households average five days per month of paid work, and over seven days a month of unpaid work. Characteristics are balanced between the control and treatment group (joint orthogonality test: $F_{(9, 383)}$: 1.10, $p = 0.360$).[22] We note the potential for selection bias. Like all field-lab experiments, participation is voluntary, with invitations to take part extended by our organizational partners to all eligible community members. Results could reflect the tendencies of more-community orientated participants, or those more interested in forest conservation. However, inferences are drawn from relative behaviors across randomized treatments (using

[21] One exception is the internal mechanism treatments, which for logistical reasons were implemented later in the field season. Footnote 22 discusses balance implications and mitigating steps.

[22] As described in footnote 21, later implementation of IM treatments leads to unbalanced characteristics between IM and non-IM groups (joint orthogonality test: $F_{(9, 383)} = 4.45$, $p < 0.01$). If not addressed this could bias our test of hypothesis 4. To avoid such bias we use a difference-in-difference estimation strategy with individual fixed effects (see section 3.5). Characteristics between vote and non-vote groups are balanced ($F_{(9, 383)} = 1.56$, $p = 0.125$).

difference-in-differences with individual fixed effects to control for time-invariant participant characteristics, see section 3.5). Selection bias could alter the magnitude of responses, but is less likely to qualitatively change study conclusions.

3.5. Analysis

To test hypothesis 1 we estimate the average treatment effect (ATE) of increased conditionality using a difference-in-differences (DiD) regression:

$$g_{ti} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \alpha_3 T + \beta_1 P_1 T + \beta_2 P_2 T + v_i + \varepsilon_{ti}$$

Where g_{ti} is the contribution of individual i in round *t,* treatment (increased conditionality) is indicated by dummy variable *T,* and P_1 and P_2 are dummy variables for the policy and post-policy periods respectively. Sorted groups (A, B, and C) are pooled to give ATEs estimated by β_1 (during policy) and β_2 (post-policy). In this and subsequent individual-level panel regressions we use individual fixed effects, v_i, to control for any time-invariant differences between participants.[23] The idiosyncratic error term is ε_{ti}. Standard errors are heteroskedasticity robust and clustered on group. Given the bounded range of the dependent variable (0, 10), we present random-effects Tobit models in addition.

Hypothesis 2, regarding differential impacts of conditionality across sorted groups, is tested with the same regression but with dummy variable interaction terms, $\boldsymbol{k}$, for different group types (A, B, or C):

$$g_{it}^{k} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \boldsymbol{\alpha k} + \boldsymbol{\alpha k} P_1 + \boldsymbol{\alpha k} P_2 + \boldsymbol{\beta k} P_1 T + \boldsymbol{\beta k} P_2 T + v_i^{k} + \varepsilon_{ti}^{k}$$

[23] The fixed effects model is advantageous in cases where the random-effects assumption may be violated. This could occur when individual specific covariates are included, or by chance in the absence of perfect covariant balance across sub-samples. In our results, coefficients and significance levels are highly consistent across models.

Since group types are defined within sessions, the numerical definitions of A, B, and C may differ across sessions. We repeat this analysis using a continuous interaction term, the average of the underlying measure used for group sorting. This is participants' initial contributions, g_0:

$$g_{it} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \alpha_3 P_1 g_0 + \alpha_4 P_2 g_0 + \alpha_5 T + \alpha_6 g_0 + \beta_1 P_1 T + \beta_2 P_2 T + \beta_3 P_1 T g_0 + \beta_4 P_2 T g_0 + v_i + \varepsilon_{ti}$$

To test <u>hypothesis 3</u> (concerning group participation in rule-setting), we add to the first model dummy variables, $\boldsymbol{j}$, representing the three rule-setting sub-treatments: experimenter choice, participant choice, and participant choice + vote.

$$g_{it}^{j} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \boldsymbol{\alpha j} T + \boldsymbol{\beta j} P_1 T + \boldsymbol{\beta j} P_2 T + v_i + \varepsilon_{ti}$$

We test <u>hypothesis 4</u> (concerning the internal coordination mechanism), with a triple-differences framework. The mechanism takes the form of anonymous revelation of contributions and the ability to send penalties, and applies for all rounds for certain sessions. Dummy variable M indicates the coordination mechanism:

$$g_{ti} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \alpha_3 T + \alpha_4 M + \beta_1 P_1 T + \beta_2 P_2 T + \beta_3 P_1 M + \beta_4 P_2 M + \beta_5 P_1 T M + \beta_6 P_2 T M + v_i + \varepsilon_{ti}$$

Experimental results are supported by focus group data. This was transcribed from audio recordings from ten communities, and then coded and analyzed using NVivo qualitative data management software. Interviewer notes were consulted for sessions in two additional communities where audio recordings were not possible.

3.6. Results

3.6.1. Conditionality on Additionality

3.6.1.1. Average Treatment Effects

In support of hypothesis 1, an increase in collective conditionality has a positive and significant impact on contributions during the treatment rounds, $R_{5\text{-}8}$ (Table 3-3). The ATE from imposition of the high collective target (T = 50, which requires additional contributions), is relatively large: 3.5-4.6 units out of an endowment of 10 units. We do not see net motivational crowding out: any decreases in intrinsic motivation were more than offset by the increased external incentive. The post-policy ($R_{9\text{-}12}$) ATE is positive and significant but, not surprisingly, is considerably smaller (0.706 units). This persistent impact from temporary incentives suggests that a durable shift in preferences for cooperative behavior was induced by the external incentive – a process known as "internalization" in the social psychology literature (Ryan and Deci, 2000).

Data from focus groups provides supportive evidence of the positive effect of increased conditionality. Acceptance was widespread among participants for levying sanctions on communities that do not comply with forest-conservation rules. Participants from a majority of communities stated that such community-level sanctions are fair and necessary − or, at the least, understandable given the collective administrative arrangements. Some focus group participants described conditionality and sanctions as reinforcing existing social attitudes and messages from government about the importance of forest conservation. These data also support the experiment's underlying setup: participants at multiple sites described conservation as a community responsibility, one that individuals should feel obliged to support.

We next test for a pure "signaling" impact from the low collective target, T=20. This target is non-binding on almost all groups, meaning that it is usually met or surpassed even in the absence of the treatment. Hence this target does not condition on *additionality*. For groups with total contributions above 20 during pre-treatment rounds (R_{1-4}) any impact from imposing *T*=20 during the treatment rounds (R_{5-8}) can only be due to non-monetary signals sent by the target about the social desirability of contributions (Bowles, 1998). Over all groups, we find an ATE of 0.84-0.90 units ($p < 0.01$) for *T*=20. However, this does not distinguish between those groups for whom *T*=20 is and is not binding. We determine ATEs for subsamples defined by this distinction in Table 3-4. Groups with contributions below 20 in R_{1-4} increase their contributions significantly ($p < 0.01$) in response to the treatment with *T*=20 (columns 1 and 4). Groups with contributions in R_{1-4} that are clearly above *T*=20 (columns 3 and 6) do not change their behavior in response to the *T=20* treatment. Groups with contributions just slightly above *T*=20 (columns 3 and 5) finds a small, significant increase in contributions due to the *T=20* treatment in one model. This suggests a slight "signaling" impact on those groups who (due to their baseline contributions) are close to but are not likely to be materially affected by increased conditionality.[24] Broadly speaking, conditioning on non-additional collective contributions has no or little impact on contributions, as expected.

3.6.1.2. Heterogeneity: ATEs by Initial Contribution

We next consider hypothesis 2, which states that collective conditionality will have a greater impact on initially low contributing groups. The initial contribution, g_0, made during the sorting round (and thus before individuals can be influenced by the behavior of others) proxies for underlying pro-social tendency. A group whose members contribute more in R_0 (i.e. a type C group member) may contribute more over the course of the experiment. Indeed, contributions in R_0 are correlated

[24] This could be explained by risk aversion combined with a psychological cost associated with not making the target. Note that because the target defines graduated, rather than a threshold of increased conditionality, it cannot be that monetary incentives are combining with risk aversion to produce this effect. We repeat this analysis using more finely delineated subsamples and find significance for the 21-50 range, but not 22-50 range or higher (tables not shown for space reasons). This emphasizes our finding that conditionality on non-additionality has essentially no substantial "signaling" impact.

with contributions in subsequent non-treatment rounds R_{1-4} ($r = 0.387$, $p < 0.05$) (see also Figure 3-2). However, that does not in itself suggest which group type, A, B, or C, will respond most strongly to new conditionality.

Difference-in-differences regression, with indicators for the three sorted group types, finds that collective conditionality *(T*=50) has a significantly larger positive effect on groups with low initial contributors (type A). The treatment effect increases by 1.30 units ($p < 0.05$) over groups with high initial contributors (type C) (regression table omitted for space reasons). This result is evident in Figure 3-2, which plots average contributions of individuals belonging to the three group types, both treated and control, over time. The plotted pattern suggests that initially low contributors respond more to increased collective conditionality than do initially high contributors. Although we cannot test the specific causal mechanism, this result is consistent with the model discussed in sections 3.2.2 and 3.4.1. Initially high contributors have greater intrinsic motivation (which explains their high contributions in R_0), which is partially crowded out by the imposition of new external incentives. In contrast, those who contribute less initially (type A) can be expected to have less intrinsic motivation. They thus have less intrinsic motivation to lose, and consequently, less scope for motivational crowding out. As a result they show a greater increase in contributions due to increased collective conditionality.

Further analysis is required for us to be certain of this experimental result. First, a given group type (A, B, or C) is not strictly consistent across sessions in absolute terms, because groups were defined relative to each other in each session. Our improved analysis (Table 3-5) uses a continuous measure, the average group contribution in $R_{0,}$ denoted *avg.* g_0. This is the measure on which groups were sorted. Again we see that higher initial contributions imply a significantly lower treatment impact (a 0.26-0.36 unit reduction in the ATE per additional unit of R_0 contribution, $p < 0.05$).

We further investigate this hypothesis with an eye towards possible ceiling effects (Table 3-6). The heterogeneity result presented so far could reflect the fact that the 10 endowed units are a "ceiling" on contributions, one that is more likely to constrain those individuals who contributed more initially. We calculate a standard difference-in-differences variable, *Change*, from the difference between average contributions over the policy and the pre-policy periods for treated individuals, minus the same difference for the n control individuals (on average):

$$Change_i = \frac{1}{4}\left(\sum_{t=5}^{8} g_{it}^{j=treat} - \sum_{t=1}^{4} g_{it}^{j=treat}\right) - \frac{1}{4} * \frac{1}{n}\left(\sum_{t=5}^{8}\sum_{i=1}^{5} g_{it}^{j=control} - \sum_{t=1}^{4}\sum_{i=1}^{5} g_{it}^{j=control}\right)$$

We regress this measure against individuals' initial contribution, g_0:

$$Change_i = \alpha_0 + \beta_1 g_0 + \varepsilon$$

We remove from the sample all individuals who at any point in the game (including during the treatment round) contributed all ten units of their endowment. The remaining individuals were thus by definition not constrained from contributing more should they have wished to. Consistent with the previous result, we see a negative and significant result (Table 3-6, columns 1 and 2).[25] Initially low contributors are responding more positively to conditionality than their high-contributing counterparts.

We apply a different approach for a final test of this result. We generate a new variable, *Shortfall*, the difference in contributions between the target, *T*, and an individual's average baseline contributions. High contributors have less far to rise to reach the target. We adjust this distance measure for baseline trends by subtracting the average change in contributions over time in the control groups:

[25] As expected, we also see the same result when we run the previous regression specification (that in Table 3-5) with these potentially constrained individuals removed.

$$Shortfall_i = T - \frac{1}{4}\sum_{t=1}^{4}\sum_{i=1}^{5} g_{it}^{j=treat} - \frac{1}{4} * \frac{1}{n}\left(\sum_{t=5}^{8}\sum_{i=1}^{5} g_{it}^{j=control} - \sum_{t=1}^{4}\sum_{i=1}^{5} g_{it}^{j=control}\right)$$

Dividing *Change* by *Shortfall* yields the fraction of the distance to the target (*T*=50) that a group achieved. For a given rise in contributions, in absolute terms, this metric assigns larger fractions achieved to initially higher contributors (i.e. a positive relationship). But if initial low contributors increase by relatively more due to the treatment as hypothesized, we would expect to see a non-monotonic or overall negative relationship. Consistent with this expectation, Table 3-6 column 4 shows a significant negative impact of g_0 when using a non-monotonic specification. Again this suggests that initially low contributors are responding relatively more strongly to conditionality than their initially high-contributing counterparts. We return to this finding in the discussion (section 3.7).

3.6.2. Other Social Interactions

The extent to which a group will successfully manage to fulfill a collective PES agreement depends on group members' attitudes toward the PES institution, and on the relationships among group members. Attitudes toward the PES institution may be influenced by participating in the contract's rule-setting process. Group relationships are partially a function of the extent to which members can see and influence each other's decisions. We test whether holding a veto on increased conditionality increases collective contributions (hypothesis 3), possibly via a sense of ownership over the increase in conditionality. We then test if a mechanism by which members can influence other members allows a group to better respond to an external incentive (hypothesis 4).

To provide baseline context for examination of these social interactions, we first show that peers influence each other's behavior (Table 3-7).[26] Having peers who give more in the initial round encourages a participant to contribute more in subsequent rounds, controlling for one's own initial contribution ($p < 0.05$). Thus peers' propensities appear to create a social expectation of how much one should contribute, even though marginal monetary payoffs are negative and constant (i.e. the private monetary return on an individual's contribution does not change due to others' decisions). Thus social interactions affect behavior in this context, providing a basis on which to test hypothesis 3 and hypothesis 4.

3.6.2.1. *Group Veto of Increased Conditionality*

Hypothesis 3 states our expectation that having a veto over increased conditionality raises the impact of that increased conditionality, should it be implemented. Our hypothesis captures a real life element of institutional variety in this setting. Communities participating in PES develop a forest management plan, from minimum standards set by CONAFOR combined with their own selection of voluntary standards from a menu of options. In some cases, choices are made by community leaders alone *(the comisariado)*, and in others, a vote is held by all the eligible members of the community *(the asamblea)*. Likewise, there is similar variation in the processes that communities use to determine the allocation of PES payments.

---

[26] We regress an individual's contributions against the difference between his/her initial contribution and that of his/her group members $\left(g_{0i} - g_0^{group}\right)$, controlling for his/her own initial contribution (g_{0i}):

$$g_{ti} = \alpha_0 + \alpha_1 g_{0i} + \beta_1\left(g_{0i} - g_0^{group}\right) + \varepsilon_{ti}$$

As previously discussed, participants in different sessions differ in their initial propensity to contribute. Consequently, sorted groups (A, B and C) are not necessarily equivalent across sessions, and a given initial contribution, g_0, could be the lowest in a C group or the highest in a B group, for example. This variation helps to cleanly identify peer effects. Controlling for g_0, β_1 is the average effect of the initial difference between a participant's and his/her peers' contributions on that participant's subsequent contributions.

In line with our hypothesis, the ATE of increased conditionality *(T*=50) is highest under conditions of maximum participation, i.e., when a randomly selected participant is given the power to choose *T* and his/her choice is put to a vote (Table 3-8). This *participant choice + vote* treatment ATE is significantly above the participant choice treatment alone (Wald test: $F_{(1,71)} = 7.17$, $p < 0.01$).

Focus-group data support this finding. Participants in Colima suggested that internal decisions regarding contracts could and should be made through the *asamblea*, implying both local choice and a veto on any contract dimensions. Qualitative data also seem to help us interpret the lack of significant difference between the ATEs for the experimenter-chosen *T* and the random participant-chosen *T* (Table 3-8, columns 1 and 2, Wald test: $F_{(1,71)} = 0.68$, $p > 0.1$). Recall that our framing of experimenter choice differed by necessity across treatments (see footnote 20). In the experimenter-choice case, we described the choice as one simply imposed by CONAFOR. That implies less ownership of the choice of *T*, leading us to expect a lower ATE. However, in this setting, the lack of a lower ATE may be partly a function of CONAFOR's apparent positive reputation. Participants in focus groups reported that majorities in their communities supported CONAFOR's FC program. Rodriguez et al. (2015) also report high levels of community confidence in CONAFOR at other upstream FC sites. This could be confounding the impact of less rule-setting participation in this subsample. Overall, the most striking result is that from the clean comparison between the remaining two treatments, *participant choice*, and *participant choice + vote*. Participation in the form of a simple veto on conditionality significantly increases the impact of that conditionality.

3.6.2.2.Internal Mechanisms for Cooperation

<u>Hypothesis 4</u> states our expectation that a mechanism which facilitates internal cooperation will increase both baseline contribution levels and the response to increased conditionality. The mechanism we test is anonymized information about group members' contributions, coupled with a means to issue anonymous sanctions to other group members. Evidence of this mechanism's

effectiveness can be seen in Figure 3-3. Contributions increase across rounds in groups with the internal mechanism (IM) but decrease in those without. We confirm statistical significance using a triple differences regression, which shows significantly higher contributions in IM treated groups in later rounds (Table 3-9) ($p < 0.01$). We also find a positive and significant increase in the ATE of between 1.74-2.98 units out of 10 ($p < 0.1$), indicating that the increased collective conditionality is more effective in IM conditions. This matches our expectations that communities that are able to coordinate through peer to peer interactions can better solve collection action problems and can therefore better respond to external policy demands.

Supporting the relevance of internal mechanisms in our field setting, focus-group data indicate the presence of intra-community monitoring and sanctioning for environmental behaviors in many communities. Focus group participants consider it their responsibility not only to abstain from acts that damage forests (e.g., lighting fires, cutting protected tree species, littering) but also to report violations by others. Local sanctions varied widely between communities but included fines, additional work and even imprisonment. The rules that were referred to in focus groups include both formal state regulations and a large number of rules that are determined at a community level. While by definition, internal mechanisms are not under the control of policy makers, resources supporting collective PES can be guided toward communities with demonstrated ability to coordinate on key decisions.

3.7. Discussion and Conclusion

Most PES programs to date are based on individual contracts, for environmental actions on land titled to individual households or firms (Kerr et al., 2014; Porras et al., 2008). The prominence of commonly titled land, however, particularly in developing countries, is increasingly motivating PES contracts with collectives – groups of farmers, or communities. In addition to better matching tenure arrangements, collective PES may lower transaction costs and improve spatial congruence with biophysical systems or areas of habitat (Kerr et al., 2014; Swallow and Meinzen-Dick, 2009).

Collective PES may also help solve information problems. At an individual level, PES administrators do not know what actions or ecoservices outcomes individuals would have provided without the program. Even when such baseline information is in hand, monitoring individuals' actions and outcomes within the program is costly. At a group or aggregate level, both baselines and actions under programs may be approximated at a reasonable cost. For example, a program can target areas with higher baseline deforestation risk determined by satellites (Wünscher et al., 2008), or pay for actions not otherwise common across a landscape (Pagiola et al., 2007). At larger scales, it is easier to determine both baseline outcomes and program outcomes, and thus is easier to write contracts that are additional to baseline outcomes. Means for increasing additionality are valuable given that analyses of PES in lower income settings to date have found, at best, modest evidence of programs' environmental impact (Alix-Garcia et al., 2012; Muñoz-Piña et al., 2008; Pattanayak et al., 2010; Porras et al., 2008; Wunder et al., 2008).

Along with others in this small but growing literature (Hayes et al., 2017; Kerr et al., 2014; Swallow and Meinzen-Dick, 2009), we argue that there is great value in understanding what institutional features can contribute to successfully contracted collective action. We used field-laboratory experiments with real PES participants to examine the impacts of collective conditionality in PES contracts. In collective settings where free-riding is possible, positive contributions from individuals require intrinsic motivations such as other-regarding preferences or social norms. External monetary incentives, even if strictly conditional, are in themselves insufficient because individual contributions are monetarily irrational. In addition, external intervention could "crowd out" the critical intrinsic or non-monetary motivations, leading to negative or diminished outcomes (Bowles, 2008; Frey and Jegen, 2001; Rode et al., 2015). Because such motivational shifts could last beyond the life of incentives (Bowles, 2008; Fehr and Falk, 2002), we also test for the persistence of conditionality's impact beyond the policy treatment.

We found that increasing collective conditionality raised collective contributions, despite the fact that for an individual, contributions were monetarily irrational even with increased conditionality. In this regard our model simulates real contexts: many PES, including those at our study sites, provide payments that are in themselves too low to motivate monetarily rational positive contributions (Porras et al., 2008). To the extent possible in a stylized lab setting, we also see evidence for a durable shift in behavior, i.e. post-treatment impacts. This is relevant for PES programs where time-limited, non-renewable contracts are common.

We also found that the increase in contributions was greater for those who initially contributed less. This result is robust to ceiling effects: it is not simply because those who give less initially have more room to rise. A smaller increase in contributions for the initially high contributors is consistent with those participants having more intrinsic motivation and thus more scope to be affected by motivational crowding out. While this does not rule out other explanations *per se,* our results suggest less impact among higher contributors from increasing the conditionality of collective PES. As programs expand, and thus move beyond the most enthusiastic self-selecting communities, they will increasingly need to contract with communities with lower intrinsic environmental motivation and lower baseline contributions. For policy purposes, our finding supports prior suggestions that such targeting may increase program impact (see, for example, Pfaff and Sanchez-Azofeifa, (2004) and Robalino and Pfaff (2013) concerning Costa Rica). This possibility has been raised for the context of Mexico (Muñoz-Piña et al., 2008) and is an ongoing design issue for PES broadly.

We investigated two further social interactions that are relevant for collective PES. One important question is who gets to participate in decisions about the design and implementation of contract rules. We found that giving the community a veto over the imposition of greater conditionality raised the impact of that conditionality. This occurs even if the veto is not used and has no tangible impact on the rule that results. Further, we found that if a community can coordinate responses to external intervention, both baseline contributions and the impact of greater external incentives upon contributions increased. In our experiment, this mechanism took the form of information about others' contributions and an ability to fine peers for unsatisfactory contributions. This mechanism mirrors features of the internal governance structures we see in our Mexican study sites, and in many other social situations.

In sum, our results inform the design and targeting of collective PES. They suggest that policy makers should set contracts that are conditional on additional actions, prioritize contracts with communities with strong internal governance, and offering communities a role in making the rules that affect them. These insights likely apply to other settings with collective characteristics.

3.8. Tables

Table 3-1: Summary statistics and balance check.

		Characteristics by location				Balance across sample		
	All	Tuxtepec, Oaxaca	Evangelista Analco, Oaxaca	Colima, Colima	Ejido San Agustin, Jalisco	Control	Treatment (increased conditionality)	p-value
Age (years)	43.2	51.0	38.1	43.1	39.3	43.7	42.8	0.686
Gender (proportion female)	0.47	0.27	0.33	0.50	0.75	0.42	0.48	0.262
Time lived in community (years)	36.6	46.1	33.0	33.1	32.8	35.9	36.7	0.726
Education (years)	7.7	6.2	9.2	7.8	8.0	7.6	7.8	0.915
Household income (MXN/week)	1075	658	777	963	1836	1239	1026	0.107
Leadership position (present or past) (proportion)	0.28	0.43	0.46	0.19	0.05	0.45	0.41	0.534
Primary income: farming (proportion)	0.45	0.82	0.48	0.43	0.07	0.41	0.36	0.407
Household forest-related work: paid (days/month)	5.05	6.09	6.22	8.53	0.23	4.48	5.23	0.340
Household forest-related work: unpaid (days/month)	7.64	8.79	13.27	7.33	2.61	7.95	7.53	0.623
Sample Size	426	119	89	100	118	103	323	n/a

Note: All locations except San Agustin involved multiple proximate communities.

Table 3-2: Experiment treatments.

	Treatment (R_{5-8})	**Target (T) choice by: (R_{5-8})**	**Internal Mechanism (R_{1-12})**	**Vote (before R_5)**	**Sample Size**
Baseline	No	N/A	No	No	60
Baseline + IM	No	N/A	Yes	No	45
Treatment	Yes	Experimenters	No	No	90
Treatment + IM	Yes	Experimenters	Yes	No	45
Treatment + Participant choice	Yes	Randomly selected participant	No	No	135
Treatment + Participant choice + Vote	Yes	Randomly selected participant	No	Yes	60

Notes: The treatment is increased conditionality, implemented in rounds (R_{5-8}) only by imposing a target, *T*. There are two target levels (*T*=20, *T*=50), which approximate conditionality on non-additional and additional levels of collective contributions relative to baseline, respectively.

Table 3-3: Difference-in-differences: average treatment effect of increased conditionality.

	OLS Fixed Effects		Tobit Random Effects	
	Target = 50	Target = 20	Target = 50	Target = 20
Treat			-0.216	-0.334
			(0.246)	*(0.293)*
$R_{5\text{-}8}$	-0.124	-0.124	-0.131	-0.13
	(0.204)	*(0.206)*	*(0.160)*	*(0.147)*
$R_{9\text{-}12}$	-0.073	-0.073	-0.055	-0.057
	(0.218)	*(0.220)*	*(0.160)*	*(0.147)*
Treat* $R_{5\text{-}8}$	3.539***	0.844**	4.649***	0.898***
	(0.283)	*(0.332)*	*(0.194)*	*(0.222)*
Treat* $R_{9\text{-}12}$	0.419*	0.208	0.487**	0.187
	(0.249)	*(0.299)*	*(0.190)*	*(0.222)*
Constant	5.196***	5.194***	5.388***	5.382***
	(0.069)	*(0.097)*	*(0.207)*	*(0.194)*
Overall R-squared	0.24	< 0.01	-	-
No. Obs.	4242	2195	4242	2195
No. Individuals	354	183	354	183

Notes: Dependent variable is individuals' contributions. Sorted groups (A, B, and C) are pooled. Treat* $R_{5\text{-}8}$ may be interpreted as the impact of the treatment during the treatment rounds; Treat* $R_{5\text{-}8}$ as the impact of the treatment after the treatment rounds. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level for OLS regression. Fixed effects are at the individual level for OLS regression. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-4: Difference-in-differences: average treatment effect by pre-policy (rounds 1-4) contribution subsamples. Target = 20 only.

	OLS Fixed Effects			Tobit Random Effects		
Subsample:	Range 0-20	Range 20-50	Range 25-50	Range 0-20	Range 20-50	Range 25-50
Treat				-0.55	-0.63	-0.276
				(0.761)	*(0.443)*	*(0.579)*
R_{5-8}	0.071	-0.239	-0.375*	-0.45	-0.771***	-0.521*
	(0.299)	*(0.164)*	*(0.197)*	*(0.484)*	*(0.231)*	*(0.271)*
R_{9-12}	0.125	-0.087	-0.068	-0.55	-0.685***	-0.423
	(0.299)	*(0.164)*	*(0.197)*	*(0.484)*	*(0.231)*	*(0.271)*
Treat* R_{5-8}	2.729***	0.089	-0.263	3.250***	0.780**	-0.145
	(0.583)	*(0.297)*	*(0.353)*	*(0.684)*	*(0.358)*	*(0.427)*
Treat* R_{9-12}	1.525***	-0.246	-0.644*	2.200***	0.417	-0.348
	(0.583)	*(0.297)*	*(0.353)*	*(0.684)*	*(0.357)*	*(0.427)*
Constant	3.526***	6.008***	6.465***	3.850***	6.253***	6.804***
	(0.182)	*(0.097)*	*(0.116)*	*(0.538)*	*(0.286)*	*(0.367)*
Overall R-squared	0.15	0	0.01	-	-	-
No. Obs.	228	1188	768	228	1188	768
No. Individuals	19	99	64	19	99	64

Notes: Dependent variable is individuals' contributions. Subsamples are defined by each group's contribution in R_{1-4}. Groups with pre-policy contributions above 20 are those for whom a target of T=20 is not likely to represent additionality. Sorted groups (A, B, and C) are pooled. Treat* R_{5-8} may be interpreted as the impact of the treatment (Target = 20) during the treatment rounds; Treat* R_{5-8} as the impact of the treatment after the treatment rounds. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level for OLS regression. Fixed effects are at the individual level for OLS regression. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-5: Difference-in-differences: average treatment effect of increased conditionality accounting for group heterogeneity in initial (R_0) contributions. Target=50 only.

	OLS Fixed Effects	Tobit Random Effects
g_0 avg.		0.634***
		(0.105)
Treat		0.064
		(0.623)
Treat*g_0 avg.		-0.055
		(0.128)
$R_{5\text{-}8}$	0.58	0.649
	(0.376)	*(0.435)*
$R_{9\text{-}12}$	0.363	0.44
	(0.469)	*(0.436)*
$R_{5\text{-}8}$*g_0 avg.	-0.154**	-0.172*
	(0.068)	*(0.089)*
$R_{9\text{-}12}$*g_0 avg.	-0.095	-0.109
	(0.091)	*(0.089)*
Treat*$R_{5\text{-}8}$	5.173***	5.763***
	(0.582)	*(0.543)*
Treat*$R_{8\text{-}12}$	-0.355	-0.625
	(0.583)	*(0.531)*
Treat*$R_{5\text{-}8}$*g_0 avg.	-0.363***	-0.262**
	(0.108)	*(0.112)*
Treat*$R_{9\text{-}12}$* g_0 avg.	0.17	0.247**
	(0.117)	*(0.110)*
Constant	5.196***	2.484***
	(0.064)	*(0.513)*
Overall R-squared	0.22	-
No. Obs.	4242	4242
No. Individuals	354	354

Notes: Dependent variable is individuals' contributions. g_0 avg. is the average contribution given by group members in R_0 (sorting round). Treat* $R_{5\text{-}8}$*g_0 avg. may be interpreted as the change in the impact of the treatment (Target = 50) during the treatment rounds, due to a one unit change in the average contribution given by group members in R_0. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level for OLS regression. Fixed effects are at the individual level for OLS regression. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-6: Impact of initial (R_0) contributions on treatment effect. Target=50 only.

Dependent Variable:	Change		Change/Shortfall	
Sub-sample:	All	Max. 9	All	All
g_0	-0.364***	-0.321**	-0.007	-0.111***
	(0.061)	*(0.135)*	*(0.012)*	*(0.041)*
g_0^2				0.010***
				(0.004)
Constant	5.194***	3.425***	0.729***	0.957***
	(0.298)	*(0.611)*	*(0.057)*	*(0.103)*
Adjusted R-squared	0.12	0.09	< 0.01	0.02
No. Obs.	251	48	251	251

Notes: *Change* is the difference between an individual's average contribution in pre-policy ($R_{1\text{-}4}$) and policy periods ($R_{5\text{-}8}$). *Shortfall* is the difference between the required contribution (10 units for an individual, given that the group target is 50) and the individual's average contribution in pre-policy ($R_{1\text{-}4}$) periods. g_0 is the contribution given in R_0 (sorting round). The subsample "Max. 9" includes only individuals who never give the maximum of ten units and thus are assumed unconstrained by ceiling effects. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-7: Impact of peer effects on individual's contributions over pre-policy period.

	OLS (R_{1-4} avg.)	Tobit Random effects (R_{1-4})
g_0	0.485***	0.511***
	(0.064)	*(0.044)*
g_0 difference	-0.204**	-0.201***
	(0.082)	*(0.071)*
Round		0.029
		(0.042)
Constant	2.951***	2.795***
	(0.293)	*(0.242)*
Adjusted R-squared	0.25	-
No. Obs.	426	1704
No. Individuals	426	426

Dependent variable is individuals' contributions. This is averaged across pre-policy period rounds (R_{1-4}) in the case of the OLS model (i.e. cross section analysis). g_0 is the contribution given in R_0 (sorting round). *g_0 difference* is the difference between g_0 and g_0 of other group members in R_0. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level for OLS regression. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-8: Average treatment effects on subsamples defined by chooser of target. Target=50 only.

	Chooser of target, T		
	Experimenter	Participant choice	Participant choice + vote
ATE (R_{5-8})	3.944***	2.854***	4.168***
	(0.386)	*(0.290)*	*(0.490)*
ATE (R_{9-12})	0.558*	0.228	0.523
	(0.320)	*(0.244)*	*(0.349)*

Notes: Dependent variable is individuals' contributions. Coefficients presented in this table are ATEs calculated in an OLS fixed effects difference-in-differences model. Interaction terms on the DID coefficient distinguishes ATEs between the three subsamples and two periods. Full model results are omitted for space (available on request). Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level. Fixed effects are at the individual level. * p<0.10, ** p<0.05, *** p<0.01.

Table 3-9: Triple differences: average treatment effect of increased conditionality with internal mechanism. Target = 50 only.

.	OLS Fixed Effects	Tobit Random Effects
Treatment		-0.1
		(0.648)
IM		-0.513
		(0.697)
R_{5-8}	0.086	0.201
	(.384)	*(0.467)*
R_{9-12}	-0.334	-0.257
	(.449)	*(0.467)*
IM*****Treat*****R_{5-8}	1.714*	2.975***
	(.923)	*(1.139)*
IM*****Treat*****R_{9-12}	-0.679	-1.417
	(1.311)	*(0.939)*
Treat*****R_{5-8}	5.366***	5.838***
	(.596)	*(0.574)*
Treat*****R_{9-12}	0.27	0.078
	(.562)	*(0.563)*
IM*****R_{5-8}	0.828**	0.763**
	(.388)	*(0.302)*
IM***** R_{9-12}	1.167***	1.181***
	(.398)	*(0.302)*
g_0 avg.		0.598***
		(0.116)
Treat*g_0 avg.		-0.028
		(0.131)
IM* g_0 avg.		0.087
		(0.143)
g_0 avg.*R_{5-8}	-0.121	-0.143
	(.074)	*(0.089)*
g_0 avg.* R_{9-12}	-0.049	-0.065
	(.088)	*(0.089)*
Treat*g_0 avg.*R_{5-8}	-0.369***	-0.296***
	(.116)	*(0.114)*
Treat*g_0 avg.*R_{9-12}	0.102	0.154
	(.115)	*(0.111)*

IM*Treat*g_0 avg.*$R_{5\text{-}8}$	-0.317*	-0.216
	(.173)	*(0.231)*
IM*Treat*g_0 avg.*$R_{9\text{-}12}$	0.106	0.316*
	(.219)	*(0.187)*
Constant	5.196***	2.708***
	(.055)	*(0.583)*
Overall R-squared	0.23	-
No. Obs.	4242	4242

Notes: Dependent variable is individuals' contributions. IM is a dummy variable indicating internal mechanism treatment (applied in all rounds).). IM*****Treat*****$R_{5\text{-}8}$ may be interpreted as the change in the impact of the treatment (Target = 50) due to presence of the internal mechanism. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the group level for OLS regression. Fixed effects are at the individual level for OLS regression. * p<0.10, ** p<0.05, *** p<0.01.

3.9. Figures

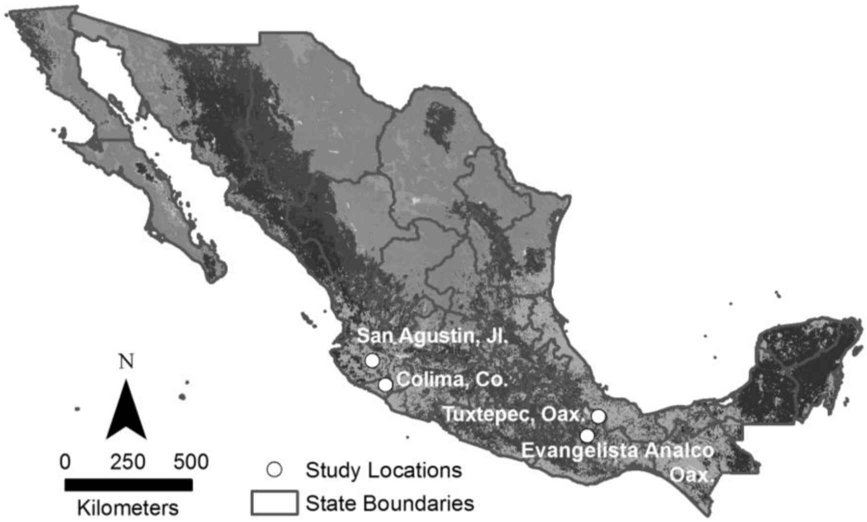


Figure 3-1: Location of study sites in Mexico (with multiple communities involved at each Colima and Oaxaca location). Shading indicates areas of higher forest cover.

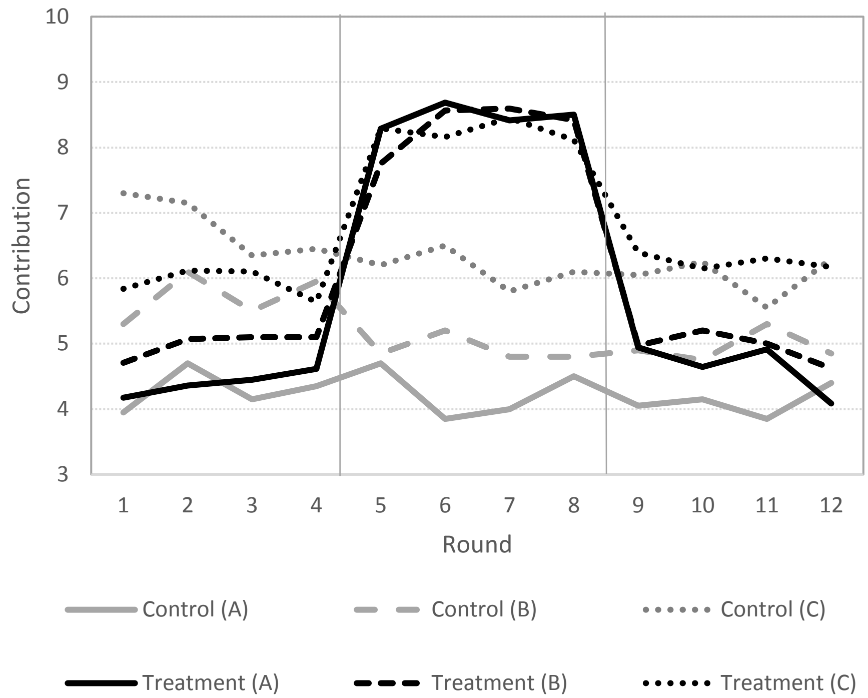


Figure 3-2: Average contributions across rounds, control (grey) versus treatment (black). Group types are plotted separately: A = smallest initial contribution, B = midrange initial contribution, and C = largest initial contribution. Target = 50. No internal mechanism.

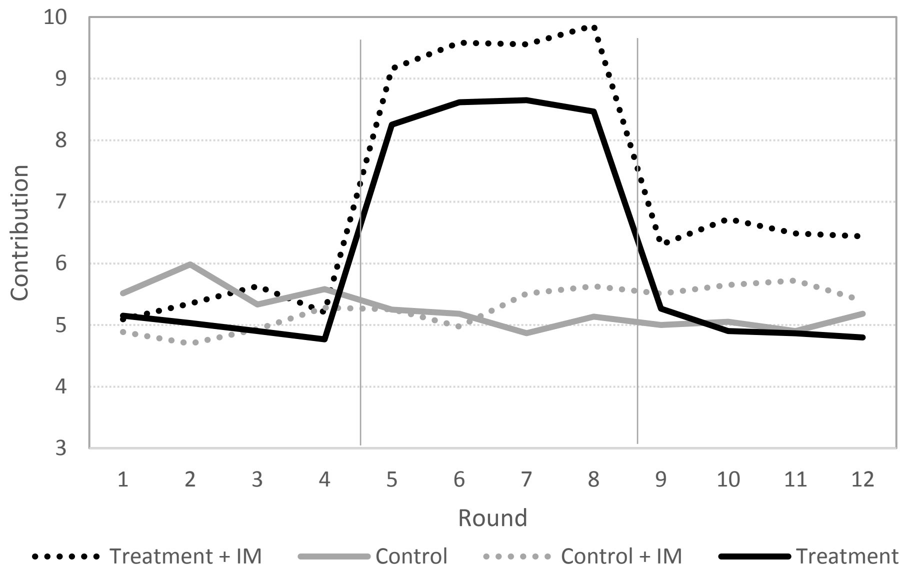


Figure 3-3: Average contributions across rounds, control (grey) versus treatment (black). Group types are pooled to show average treatment effects. Target = 50. IM = Internal mechanism.

References

Abbott, J., Wilen, J., 2010. Voluntary cooperation in the commons? Evaluating the Sea State Program with reduced form and structural models. Land Econ. 86, 131–154.

Aggarwal, S., 2014. Do rural roads create pathways out of poverty? Evidence from India, Job Market Paper.

Agrawal, A., 2002. Common resources and institutional sustainability, in: Ostrom, E. (Ed.), The Drama of the Commons. National Academies Press, pp. 41–85. doi:10.1017/CBO9781107415324.004

Agrawal, A., Chhatre, A., Hardin, R., 2008. Changing governance of the world's forests. Science (80-.). 320, 1460–2. doi:10.1126/science.1155369

Agrawal, A., Ostrom, E., 2016. Collective action, property rights, and decentralization in resource use in India and Nepal. Polit. Soc. 29, 485–514.

Agrawal, A., Wollenberg, E., Persha, L., 2014. Governing agriculture-forest landscapes to achieve climate change mitigation. Glob. Environ. Chang. 29, 270–280. doi:10.1016/j.gloenvcha.2014.10.001

Agrawal, A., Yadama, G., 1997. How do Local Institutions Mediate Market and Population Pressures on Resources? Forest Panchayats in Kumaon, India. Dev. Change 28, 435–465. doi:10.1111/1467-7660.00050

Alix-Garcia, J., Shapiro, E., Sims, K., 2012. Forest conservation and slippage: Evidence from Mexico's national payments for ecosystem services program. Land Econ. 88, 613–638. doi:10.1353/lde.2012.0052

Allen, J.C., Barnes, D.F., 1985. The causes of deforestation in developing countries. Ann. Assoc. Am. Geogr. 75, 163–184.

Alpízar, F., Nordén, A., Pfaff, A., Robalino, J., 2015. Unintended effects of targeting an environmental rebate. Environ. Resour. Econ. 1–22.

Alpízar, F., Requate, T., Schram, A., 2004. Collective versus random fining: An experimental study on controlling ambient pollution. Environ. Resour. Econ. 29, 231–252. doi:10.1023/B:EARE.0000044608.66145.0c

Andersen, L.., Granger, C.W.J., Reis, E.J., Weinhold;, D., Wunder, S., 2002. The Dynamics of Deforestation and Economic Growth in the Brazilian Amazon. Cambridge University Press, Cambridge, UK.

Andreoni, J., 1993. An experimental test of the public-goods crowding-out hypothesis. Am. Econ. Rev. 83, 1317–1327. doi:10.1628/001522104774102815

Angelsen, A., 2007. Forest Cover Change in Space and Time: Combining the von

Thünen and Forest Transition Theories. World Bank Policy Res. Work. Pap. 1–43. doi:doi:10.1596/1813-9450-4117

Angelsen, A., Kaimowitz, D., 2001. Introduction: the Role of Agricultural Technologies in Tropical Deforestation. Agric. Technol. Deforestation 1–17.

Angelsen, A., Kaimowitz, D., 1999. Rethinking the causes of deforestation: Lessons from economic models. World Bank Res. Obs. 14, 73–98.

Arnason, R., Kobayashi, M., de Fontaubert, C., 2016. The Sunken Billions Revisited: Progress and Challenges in Global Marine Fisheries. Washington, D.C.

Asher, S., Novosad, P., 2016. Market Access and Structural Transformation: Evidence from Rural Roads in India, Job Market Paper.

Bagnoli, M., Mckee, M., 1991. Voluntary contribution games: Efficient private provision of public goods. Econ. Inq. 29, 351–361.

Baland, J.-M., Bardhan, P., Das, S., Mookherjee, D., Sarkar, R., 2006. Managing the environmental consequence of growth: Forest Degradation in the Indian Mid-Himalayas. India Policy Forum.

Baland, J.-M., Platteau, J.-P., 1996. Halting degradation of natural resources: Is there a role for rural communities?, International Affairs Royal Institute of International Affairs 1944. doi:10.2307/2624181

Barbier, E.B., Burgess, J.C., Grainger, A., 2010. The forest transition: Towards a more comprehensive theoretical framework. Land use policy 27, 98–107. doi:10.1016/j.landusepol.2009.02.001

Barlow, J., Gardner, T.A., Araujo, I.S., Ávila-Pires, T.C., Bonaldo, A.B., Costa, J.E., Esposito, M.C., Ferreira, L. V., Hawes, J., Hernandez, M.I.M., Hoogmoed, M.S., Leite, R.N., Lo-Man-Hung, N.F., Malcolm, J.R., Martins, M.B., Mestre, L. a. M., Miranda-Santos, R., Nunes-Gutjahr, A.L., Overal, W.L., Parry, L., Peters, S.L., Ribeiro-Junior, M.A., Silva, M.N.F. da, Motta, C. da S., Peres, C.A., 2007. Quantifying the biodiversity value of tropical primary, secondary, and plantation forests. Proc. Natl. Acad. Sci. 104, 18555–18560. doi:10.1073/pnas.0703333104

Barnes, G., 2009. The evolution and resilience of community-based land tenure in rural Mexico. Land use policy 26, 393–400. doi:10.1016/j.landusepol.2008.05.007

Bell, C., Van Dillen, S., 2012. How Does India’s Rural Roads Program Affect the Grassroots? Findings from a Survey in Orissa. Land Econ. 90, 372–394.

Birkenbach, A., Kaczan, D., Smith, M., 2017. Catch shares slow the race to fish. Nature forthcomin.

Bowles, S., 2008. Policies designed for self-interested citizens may undermine “the moral sentiments." Science (80-.). 1605, 1605–1609. doi:10.1126/science.1152110

Bowles, S., 1998. Endogenous preferences: The cultural consequences of markets other economic institutions. J. Econ. Lit. 36, 75–111. doi:10.2307/2564952

Bowles, S., Polanía-Reyes, S., 2012. Economic incentives and social preferences: Substitutes or complements? J. Econ. Lit. 50, 368–425. doi:10.1257/jel.50.2.368

Boyce, J., 1992. Individual transferable quotas and production externalities in a fishery. Nat. Resour. Model. 6, 385–408.

Brandon, K., Gorenflo, L.J., Rodrigues, A.S.L., Waller, R.W., 2005. Reconciling biodiversity conservation, people, protected areas, and agricultural suitability in Mexico. World Dev. 33, 1403–1418. doi:10.1016/j.worlddev.2004.10.005

Brinson, A.A., Thunberg, E.M., 2013a. The Economic Performance of U.S. Catch Share Programs, NMFSF/SPO. National Oceanic and Atmospheric Administration, Washington, D.C.

Brinson, A.A., Thunberg, E.M., 2013b. The Economic Performance of U.S. Catch Share Programs, NMFSF/SPO. National Oceanic and Atmospheric Administration, Washington, D.C.

Burton, G.P., 2011. Increasing forest cover amidst expanding commercial agriculture: An agent-based simulation of markets and rural livelihoods in the Indian Himalayas. University of Illinois at Urbana-Champaign.

Cadsby, C.B., Maynes, E., 1999. Voluntary provision of threshold public goods with continuous contributions: Experimental evidence. J. Public Econ. 71, 53–73. doi:10.1016/S0047-2727(98)00049-8

Camacho-Cuena, E., Requate, T., 2012. The regulation of non-point source pollution and risk preferences: An experimental approach. Ecol. Econ. 73, 179–187. doi:10.1016/j.ecolecon.2011.10.019

Cameron, J., Banko, K.M., Pierce, W.D., 2001. Pervasive negative effects of rewards on intrinsic motivation: The myth continues. Behav. Anal. 24, 1–44. doi:10.1016/j.neuron.2009.04.009

Cardenas, J.C., Stranlund, J., Willis, C., 2000. Local environmental control and institutional crowding-out. World Dev. 28, 1719–1733. doi:10.1016/S0305-750X(00)00055-3

Caro, T., Dobson, A., Marshall, A.J., Peres, C.A., 2014. Compromise solutions between conservation and road building in the tropics. Curr. Biol. 24, R722–R725. doi:10.1016/j.cub.2014.07.007

Casey, K.E., Dewees, C.M., Turris, B.R., Wilen, J.E., 1995. The effects of individual vessel quotas in the British Columbia Halibut fishery. Mar. Resour. Econ. 10, 211–230.

Cason, T.N., Gangadharan, L., 2013. Empowering neighbors versus imposing regulations: An experimental analysis of pollution reduction schemes. J. Environ. Econ. Manage. 65, 469–484. doi:10.1016/j.jeem.2012.09.001

CFMC, 2013. Fact Sheet: Annual Catch Limit.

Chan, K.S., Godby, R., Mestelman, S., Andrew Muller, R., 2002. Crowding-out voluntary contributions to public goods. J. Econ. Behav. Organ. 48, 305–317. doi:10.1016/S0167-2681(01)00232-3

Chomitz, K.M., 2007. At Loggerheads ? Agricultural expansion, poverty reduction, and environment in the tropical forests, The World Bank.

Chomitz, K.M., Gray, D. a, 1996. Roads, Land Use, and Deforestation: A Spatial Model Applied to Belize. World Bank Econ. Rev. 10, 487–512. doi:10.1093/wber/10.3.487

Christy, F.T., 1973. Fisherman quotas: a tentative suggestion for domestic management. Law of the Sea Institute, University of Rhode Island, Kingston, RI.

Clements, T., John, A., Nielsen, K., An, D., Tan, S., Milner-Gulland, E.J., 2010. Payments for biodiversity conservation in the context of weak institutions: Comparison of three programs from Cambodia. Ecol. Econ. 69, 1283–1291. doi:10.1016/j.ecolecon.2009.11.010

Cochard, F., Willinger, M., Xepapadeas, A., 2005. Efficiency of nonpoint source pollution instruments: An experimental study. Environ. Resour. Econ. 30, 393–422.

Correia, S., 2014. reghdfe: Stata module to perform linear or instrumental-variable regression absorbing any number of high-dimensional fixed effects.

Costello, C., Gaines, S., Lynham, J., 2008. Can catch shares prevent fisheries collapse? Science (80-.). 321, 1678–1681.

Costello, C., Ovando, D., Clavelle, T., Strauss, C.K., Hilborn, R., Melnychuk, M.C., 2016. Global fishery prospects under contrasting management regimes. PNAS 113, 1–5. doi:10.1073/pnas.1520420113

Costello, C.J., Lynham, J., Lester, S.E., Gaines, S.D., 2010a. Economic Incentives and Global Fisheries Sustainability. Annu. Rev. Resour. Econ. 2, 299–318. doi:10.1146/annurev.resource.012809.103923

Costello, C.J., Lynham, J., Lester, S.E., Gaines, S.D., 2010b. Economic Incentives and Global Fisheries Sustainability. Annu. Rev. Resour. Econ. 2, 299–318. doi:10.1146/annurev.resource.012809.103923

Cropper, M., Puri, J., Griffiths, C., 2001. Predicting the Location of Deforestation : The Role of Roads and Protected Areas in North Thailand. Land Econ. 77, 172–186. doi:10.3368/le.77.2.172

Cunningham, S., Bennear, L.S., Smith, M.D., 2016. Spillovers in Regional Fisheries Management: Do Catch Shares Cause Leakage? Land Econ.

Deci, E.L., Koestner, R., Ryan, R.M., 1999. A meta-analytic review of experiments examining the effects of extrinsic rewards on intrinsic motivation. Psychol. Bull. 125, 627-668-700. doi:10.1037/0033-2909.125.6.627

Deininger, K., Minten, B., 1998. Determinants of Deforestation and the Economics of Protection: An Application to Mexico. Am. J. Agric. Econ. 84, 943–960.

Del Corso, J.-P., Nguyen, T.D.P.G., Kephaliacos, C., 2017. Acceptance of a payment for ecosystem services scheme: The decisive influence of collective action. Environ. Values 26, 177–202. doi:10.3197/096327117X14847335385517

Deng, X., Huang, J., Uchida, E., Rozelle, S., Gibson, J., 2011. Pressure cookers or pressure valves: Do roads lead to deforestation in China? J. Environ. Econ. Manage. 61, 79–94. doi:10.1016/j.jeem.2010.04.005

DFO, 2016. Commercial Fishing [WWW Document]. Commer. Fish. URL http://www.dfo-mpo.gc.ca/fisheries-peches/commercial-commerciale-eng.html (accessed 3.3.17).

Esri, 2013. Layer Description: Terrain, ArcGIS online [WWW Document]. URL www.arcgis.com/home/item.html?id=58a541efc59545e6b7137f961d7de883

FAO, 2016a. The State of World Fisheries and Aquaculture 2016. Contributing to food security and nutrition for all. Food and Agriculture Organization of the United Nations, Rome.

FAO, 2016b. State of the World's Forests 2016. Forests and agriculture: land-use challenges and opportunities. Rome. doi:10.1146/annurev-environ-020411-130608

FAO, 2016c. State of the World's Forests 2016. Forests and agriculture: land-use challenges and opportunities. Rome. doi:10.1146/annurev-environ-020411-130608

FAO, 2015. Global Forest Resources Assessment 2015. Desk reference. Food and Agriculture Organization of the United Nations, Rome. doi:10.1002/2014GB005021

FAO, 2010. Global Forest Resources Assessment 2010.

Feeney, D., Berkes, F., McCay, B., Acheson, J., 1990. The tragedy of the commons: Twenty-two years later. Hum. Ecol. 18, 1–19. doi:Article

Fehr, E., Falk, A., 2002. Psychological foundations of incentives. Eur. Econ. Rev. 46, 687–724. doi:10.1016/S0014-2921(01)00208-2

Ferraro, P.J., 2011. The future of payments for environmental services. Conserv. Biol. 25, 1134–8. doi:10.1111/j.1523-1739.2011.01791.x

Ferraro, P.J., Kiss, A., 2002. Direct payments to conserve biodiversity. Science (80-.). 298, 1718–1719.

Ferretti-gallon, K., Busch, J., 2014. What Drives Deforestation and What Stops It ? A Meta-Analysis of Spatially Explicit Econometric Studies (No. 361), Center for Global Development Working Paper.

Festré, A., Garrouste, P., 2015. Theory and evidence in psychology and economics about motivation crowding out: A possible convergence? J. Econ. Surv. 29, 339–356. doi:10.1111/joes.12059

Foley, J. a, Defries, R., Asner, G.P., Barford, C., Bonan, G., Carpenter, S.R., Chapin, F.S., Coe, M.T., Daily, G.C., Gibbs, H.K., Helkowski, J.H., Holloway, T., Howard, E. a, Kucharik, C.J., Monfreda, C., Patz, J. a, Prentice, I.C., Ramankutty, N., Snyder, P.K., 2005. Global consequences of land use. Science 309, 570–4. doi:10.1126/science.1111772

FONAFILIO, CONAFOR, Ministry of Environment, 2012. Lessons learned for REDD+ from PES and conservation incentive programs: Examples from Costa Rica, Mexio and Ecuador. Washington D.C.

Foster, A., Rosenzweig, M., 2003. Economic growth and the rise of forests. Q. J. Econ. 118, 601–637.

Foster, A.D., Rosenzweig, M.R., 2004. Agricultural Productivity Growth, Rural Economic Diversity, and Economic Reforms: India, 1970–2000. Econ. Dev 52, 509–542.

Frey, B., Stutzer, A., 2006. Environmental morale and motivation 1–27. doi:http://dx.doi.org/10.1017/CBO9780511490118.017

Frey, B.S., Jegen, R., 2001. Motivation crowding theory. J. Econ. Surv. 15, 589–611. doi:10.1111/1467-6419.00150

Geist, H.J., Lambin, E.F., 2002. Proximate Causes and Underlying Driving Forces of Tropical Deforestation. Bioscience 52, 143–150.

Gelcich, S., Guzman, R., Rodríguez-Sickert, C., Castilla, J.C., Cárdenas, J.C., 2013. Exploring external validity of common pool resource experiments: Insights from artisanal benthic fisheries in Chile. Ecol. Soc. 18.

Ghate, R., Mehra, D., Nagendra, H., 2009. Local institutions as mediators of the impact of markets on non-timber forest product extraction in central India. Environ.

Conserv. 36, 51. doi:10.1017/S0376892909005311

GMFMC, 2007. Regulatory Amendment to Address Vermilion Snapper Regulations under the Fishery Management Plan For the Reef Fish Fishery of the Gulf of Mexico Including Environmental Assessment, Regulatory Impact Review, and Regulatory Flexibility Act Analysis. Gulf of Mexico Fisheries Management Council, Tampa, FL.

GMFMC, 2006. Final Amendment 26 to the Gulf of Mexico Reef Fishery Management Plan to Establish a Red Snapper Individual Fishing Quota Program. Gulf of Mexico Fishery Management Council, Tampa, FL.

Gollin, D., Rogerson, R., 2014. Productivity, transport costs and subsistence agriculture. J. Dev. Econ. 107, 38–48. doi:10.1016/j.jdeveco.2013.10.007

Gordon, H., 1954. The economic theory of a common-property resource: The fishery. J. Polit. Econ. 62, 124–142.

Grafton, R.Q., 1996. Individual transferable quotas: theory and practice. Rev. Fish Biol. Fish. 6, 5–20. doi:10.1007/BF00058517

Grafton, R.Q., Arnason, R., Bjorndal, T., Campbell, D., Campbell, H.F., Clark, C.W., Connor, R., Dupont, D.P., Hannesson, R., Hilborn, R., Kirkley, J.E., Kompas, T., Lane, D.E., Munro, G.R., Pascoe, S., Squires, D., Steinshamn, S.I., Turris, B.R., Weninger, Q., 2006. Incentive-based approaches to sustainable fisheries. Can. J. Fish. Aquat. Sci. 63, 699–710. doi:10.1139/f05-247

Grafton, R.Q., Squires, D., Fox, K.J., 2000. Private Property and Economic Efficiency: A Study of a Common-Pool Resource. J. Law Econ. 43, 679–714. doi:10.1086/467469

Griffith, D.R., 2008. The ecological implications of individual fishing quotas and harvest cooperatives. Front. Ecol. Environ. 6, 191–198. doi:10.1890/050060

Hansen, M.C., et al., 2013. High-resolution global maps of 21st-century forest cover change. Science 342, 850–3. doi:10.1126/science.1244693

Harrison, G.W., List, J.A., 2015. Field experiments. J. Econ. Lit. 42, 1009–1055. doi:10.1257/0022051043004577

Hayes, T., Murtinho, F., Wolff, H., 2017. The impact of payments for environmental services on communal lands: An analysis of the factors driving household land-use behavior in Ecuador. World Dev. 93, 427–446. doi:http://dx.doi.org/10.1016/j.worlddev.2017.01.003

Hayes, T., Murtinho, F., Wolff, H., 2015. An institutional analysis of payment for environmental services on collectively managed lands in Ecuador. Ecol. Econ. 118, 81–89. doi:10.1016/j.ecolecon.2015.07.017

Heltberg, R., Arndt, T.C., Sekhar, N.U., 2000. Fuelwood consumption and Forest Degradation : Fuelwood Consumption A Household Model for Domestic Energy in Rural India Substitution. Land Econ. 76, 213–232. doi:10.2307/3147225

Holland, D., Thunberg, E., Agar, J., Crosson, S., Demarest, C., Kasperski, S., Perruso, L., Steiner, E., Stephen, J., Strelcheck, A., Travis, M., 2014a. U.S. Catch Share Markets: A Review of Characteristics and Data Availability. National Oceanic and Atmospheric Administration, U.S. Department of Commerce.

Holland, D., Thunberg, E., Agar, J., Crosson, S., Demarest, C., Kasperski, S., Perruso, L., Steiner, E., Stephen, J., Strelcheck, A., Travis, M., 2014b. U.S. Catch Share Markets: A Review of Characteristics and Data Availability. National Oceanic and Atmospheric Administration, U.S. Department of Commerce.

Homans, F.R., Wilen, J.E., 2005. Markets and rent dissipation in regulated open access fisheries. J. Environ. Econ. Manage. 49, 381–404. doi:10.1016/j.jeem.2003.12.008

Homans, F.R., Wilen, J.E., 1997. A Model of Regulated Open Access Resource Use. J. Environ. Econ. Manage. 32, 1–21. doi:10.1006/jeem.1996.0947

Hsueh, L., 2017. Quasi-Experimental Evidence on the “Rights to Fish”: The Effects of Catch Shares on Fishermen’s Days at Sea. J. Assoc. Environ. Resour. Econ. 4, 407–445. doi:10.1086/691555

Huang, L., Smith, M.D., 2014. The Dynamic Efficiency Costs of Common-Pool Resource Exploitation. Am. Econ. Rev. 104, 4071–4103. doi:10.1257/aer.104.12.4071

Imbens, G.W., Kolesár, M., 2016. Robust Standard Errors in Small Samples: Some Practical Advice. Rev. Econ. Stat. 98, 701–712. doi:10.1162/REST

IPCC, 2014. Summary for Policymakers, in: Edenhofer, O., Pichs-Madruga, R., Y. Sokona, E.F., Kadner, S., Seyboth, K., Adler, A., Baum, I., Brunner, S., Eickemeier, P., Kriemann, B., Savolainen, J., Schlömer, S., Stechow, C. von, Zwickel, T., Minx, J.C. (Eds.), Climate Change 2014: Mitigation of Climate Change. Contribution of Working Group III to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge, UK, pp. 1–33. doi:10.1017/CBO9781107415324

Ivanov, B.G., 2002. Red king crab (Paralithodes camtschaticus) in the eastern Okhotsk Sea: Problems of stock management and research, in: Paul, A.J., Dawe, E.G., Elner, R., G.S.Jamieson, Kruse, G.., Otto, R.S., Sainte-Marie, B., Shirley, T.C., Woodby, D. (Eds.), Crabs in Cold Water Regions: Biology, Management, and Economics. Alaska Sea Grant, University of Alaska Fairbanks, Fairbanks, AK, pp. 651–680. doi:10.4027/ccwrbme.2002.47

Jackson, R.B., Jobbágy, E.G., Avissar, R., Roy, S.B., Barrett, D.J., Cook, C.W., Farley, K.A., le Maitre, D.C., McCarl, B.A., Murray, B.C., 2005. Trading Water for Carbon with Biological Carbon Sequestration. Science (80-.). 310, 1944–1947. doi:10.1126/science.1119282

Kelleher, K., Willmann, R., Arnason, R., 2009. The Sunken Billions: The Economic Justification for Fisheries Reform. The World Bank, Washington, D.C. doi:10.1596/978-0-8213-7790-1

Kerr, J.M., Vardhan, M., Jindal, R., 2014. Incentives, conditionality and collective action in payment for environmental services. Int. J. Commons 8, 595–616.

Khandker, S., Bakht, Z., Koolwal, G., 2009. The Poverty Impact of Rural Roads: Evidence from Bangladesh. Econ. Dev. Cult. Change 57, 685–722. doi:10.1086/598765

Kroll, S., Cherry, T.L., Shogren, J.F., 2007. Voting, punishment, and public goods. Econ. Inq. 45, 557–570. doi:10.1111/j.1465-7295.2007.00028.x

Kumar, K., Kerr, J.M., 2012. Democratic Assertions: The Making of India's Recognition of Forest Rights Act. Dev. Change 43, 751–771. doi:10.1111/j.1467-7660.2012.01777.x

Laurance, W.F., Clements, G.R., Sloan, S., O/'Connell, C.S., Mueller, N.D., Goosem, M., Venter, O., Edwards, D.P., Phalan, B., Balmford, A., Van Der Ree, R., Arrea, I.B., 2014. A global strategy for road building. Nature 513, 229–232. doi:10.1038/nature13717

Laurance, W.F., Peletier-Jellema, A., Geenen, B., Koster, H., Verweij, P., Van Dijck, P., Lovejoy, T.E., Schleicher, J., Van Kuijk, M., 2015. Reducing the global environmental impacts of rapid infrastructure expansion. Curr. Biol. 25, R259–R262. doi:10.1016/j.cub.2015.02.050

Leimona, B., 2009. Can rewards for environmental services benefit the poor? Lessons from Asia. Int. J. Commons 3, 82–107. doi:10.18352/ijc.121

Levitt, S.D., List, J.A., 2007. What do laboratory experiments measuring social preferences reveal about the real world? J. Econ. Perspect. 21, 153–174.

Lian, C., Singh, R., Weninger, Q., 2009. Fleet restructuring, rent generation, and the design of individual fishing quota programs: Empirical evidence from the Pacific Coast groundfish fishery. Mar. Resour. Econ. 24, 329–359.

Madrid, L., Núñez, J.M., Quiroz, G., Rodríguez, Y., 2009. La propiedad social forestal en México. Invetigaciones Ambient. 1, 179–196.

MAFMC, 2015. Standardized Bycatch Reporting Methodology: An Omnibus Amendment to the Fishery Management Plans of the Mid-Atlantic and New England Regional Fishery Management Councils. Mid-Atlantic Fishery

Management Council, Dover, DE.

Makles, A., 2012. Stata tip 110: How to get the optimal k-means cluster solution. Stata J. 12, 347–351.

Marshall, G., 2005. Economics for collaborative environmental management: Renegotiating the commons. Eathscan, Routledge, New York.

Masclet, D., Noussair, C.N., Tucker, S., Villeval, M.-C., 2003. Monetary and nonmonetary punishment in the voluntary contributions mechanism. Am. Econ. Rev. 93, 366–380. doi:10.1257/000282803321455359

Mather, A., 2007. Recent Asian forest transitions in relation to forest transition theory. Int. For. Rev. 9, 491–502. doi:10.1505/ifor.9.1.491

Mather, A.S., 1992. The Forest Transition. Area 24, 367–379.

Mather, A.S., Fairbairn, J., Needle, C.L., 1999. TIhe Course and Drivers of the Forest Transition: the case of France. J. Rural Stud. 15, 65–90.

Mather, A.S., Fairbairn, J., Road, E., 2000. From Floods to Reforestation: The Forest Transition in Switzerland. Environ. Hist. Camb. 6, 399–421.

McAfee, K., Shapiro, E.N., 2010. Payments for ecosystem services in Mexico: Nature, neoliberalism, social movements, and the state. Ann. Assoc. Am. Geogr. 3, 579–599. doi:10.1080/00045601003794833

Mcilwain, K., Hill, J., 2013. United States Central Gulf of Alaska Rockfish Cooperative Program. Washington, D.C.

McIlwain, K., Hill, J., 2013. United States Bering Sea and Aleutian Islands Non-Pollock (Amendment 80) Cooperative Program. Washington, D.C.

McKendree, M.G.S., Ortega, D.L., Olynk Widmar, N., Wang, H.H., 2013. Consumer Perceptions of Seafood Industries in the Wake of the Deepwater Horizon Oil Spill and Fukushima Daiichi Nuclear Disaster, Staff Paper 2013-03. East Lansing, MI.

Melnychuk, M.C., Essington, T.E., Branch, T.A., Heppell, S.S., Jensen, O.P., Link, J.S., Martell, S.J.D., Parma, A.M., Pope, J.G., Smith, A.D.M., 2012. Can catch share fisheries better track management targets? Fish Fish. 13, 267–290. doi:10.1111/j.1467-2979.2011.00429.x

Michael Carroll, Gentner, B., Larkin, S., Quigley, K., Perlot, N., Dehner, L., Kroetz, A., Prepared, 2016. An Analysis of the Impacts of the Deepwater Horizon Oil Spill on the Gulf of Mexico Seafood Industry. Washington, D.C.

Ministry of Home Affairs, 2016. Indian Census. Office of the Registrar General and Census Commissioner [WWW Document]. URL http://censusindia.gov.in/

(accessed 7.8.16).

Ministry of Rural Development, 2012a. Pradhan Mantri Gram Sadak Yojana – Programme Guidelines. New Delhi.

Ministry of Rural Development, 2012b. Pradhan Mantri Gram Sadak Yojana - Program Guidlines. New Delhi.

Mittermeier, R.A., Myers, N., Thomsen, J.B., da Fonseca, G.A.B., Olivieri, S., 1998. Biodiversity hotspots and major tropical wilderness areas: Approaches to setting conservation priorities. Conserv. Biol. 12, 516–520. doi:10.1046/j.1523-1739.1998.012003516.x

Molnar, A., France, M., Purdy, L., Karver, J., 2011. Community-based forest management: The extent and potential scope of community and smallholder forest management and enterprises. Washington, D.C.

Morgan, O.A., Whitehead, J.C., Huth, W.L., Martin, G.S., Sjolander, R., 2016. Measuring the Impact of the BP Deepwater Horizon Oil Spill on Consumer Behavior. Land Econ. 92, 82–95. doi:10.3368/le.92.1.82

Muñoz-Piña, C., Guevara, A., Torres, J.M., Braña, J., 2008. Paying for the hydrological services of Mexico's forests: Analysis, negotiations and results. Ecol. Econ. 65, 725–736. doi:10.1016/j.ecolecon.2007.07.031

Muradian, R., Corbera, E., Pascual, U., Kosoy, N., May, P.H., 2010. Reconciling theory and practice: An alternative conceptual framework for understanding payments for environmental services. Ecol. Econ. 69, 1202–1208. doi:10.1016/j.ecolecon.2009.11.006

Myers, N., Mittermeier, R.A., Fonseca, G.A.B., Fonseca, G.A.B., Kent, J., 2000. Biodiversity hotspots for conservation priorities. Nature 403, 853–8. doi:10.1038/35002501

National Oceanic and Atmospheric Administration (NOAA), 2014. Commercial Fisheries Statistics [WWW Document]. URL http://www.st.nmfs.noaa.gov/commercial-fisheries/

National Research Council, 1999. Sharing the fish: Toward a national policy on individual fishing quotas. National Academy Press, Washington, D.C.

Nelson, G.C., Hellerstein, D., 1997. Do Roads Cause Deforestation ? Using Satellite Images in Econometric Analysis of Land Use. Am. J. Agric. Econ. 79, 80–88.

Nepstad, D., Soares-filho, B.S., Merry, F., Lima, A., Moutinho, P., Carter, J., Bowman, M., Cattaneo, A., Rodrigues, H., Schwartzman, S., Mcgrath, D.G., Stickler, C.M., Lubowski, R., Piris-cabezas, P., Rivero, S., Alencar, A., Almeida, O., Stella, O., 2008. The End of Deforestation in the Brazilian Amazon. Science (80-.). 326, 1350–1351.

NMFS, 2013. Gulf of Mexico 2012 Grouper-Tilefish Individual Fishing Quota Annual Report. National Marine Fisheries Service, St Petersburg, FL.

NMFS, 2004. Bering Sea Aleutian Islands Crab Fisheries Final Environmental Impact Statement. Juneau, AK.

NMFS, 2002. Final Environmental Impact Statement for American Fisheries Act Amendments 61/61/13/8. Juneau, AK.

OECD, 2004. Country Note on Fishery Management Systems: Canada. Organization for Economic Cooperation and Development, Paris.

Ostrom, E., 1999. Revisiting the commons: Local lessons, global challenges. Science (80-.). 284, 278–282. doi:10.1126/science.284.5412.278

Ostrom, E., 1990. Governing the Commons: The Evolutions of Institutions for Collective Action, Political economy of institutions and decisions. Cambridge University Press, Cambridge, UK.

Pagiola, S., Arcenas, A., Platais, G., 2005. Can payments for environmental services help reduce poverty? An exploration of the issues and the evidence to date from Latin America. World Dev. 33, 237–253. doi:10.1016/j.worlddev.2004.07.011

Pagiola, S., Ramírez, E., Gobbi, J., de Haan, C., Ibrahim, M., Murgueitio, E., Ruíz, J.P., 2007. Paying for the environmental services of silvopastoral practices in Nicaragua. Ecol. Econ. 64, 374–385. doi:10.1016/j.ecolecon.2007.04.014

Pattanayak, S.K., Wunder, S., Ferraro, P.J., 2010. Show me the money: Do payments supply environmental services in developing countries? Rev. Environ. Econ. Policy 4, 254–274. doi:10.1093/reep/req006

Perz, S.G., 2007. Grand Theory and Context-Specificity in the Study of Forest Dynamics: Forest Transition Theory and Other Directions. Prof. Geogr. 59, 105–114. doi:10.1111/j.1467-9272.2007.00594.x

Pfaff, A., 1999. What drives deforestation in the Brazilian Amazon?: evidence from satellite and socioeconomic data. J. Environ. Econ. Manage. 37, 26–43.

Pfaff, A., Amacher, G.S., Sills, E.O., Coren, M.J., Streck, C., Lawlor, K., 2013. Deforestation and Forest Degradation: Concerns, Causes, Policies, and Their Impacts, Encyclopedia of Energy, Natural Resource, and Environmental Economics. doi:10.1016/B978-0-12-375067-9.00052-8

Pfaff, A., Barbieri, A., Ludewigs, T., Merry, F., Perz, S., Reis, E., 2009. Road Impacts in Brazilian Amazonia. Geophys. Monogr. Ser.

Pfaff, A., Robalino, J., Reis, E.J., Walker, R., Perz, S., Laurance, W.F., Bohrer, C., Aldrich, S., Arima, E., Caldas, M., Kirby, K., 2016. Can strategically locating roads "clean" development? Roads' heterogeneous impacts on the Brazilian Amazon forest frontier.

Pfaff, A., Robalino, J., Walker, R., 2007. Road Investments, Spatial Spillovers, and Deforestation in the Brazilian Amazon. J. Reg. Sci. 47, 109–123.

Pfaff, A.S.P., Sanchez-Azofeifa, G.A., 2004. Deforestation pressure and biological reserve planning: A conceptual approach and an illustrative application for Costa Rica. Resour. Energy Econ. 26, 237–254. doi:10.1016/j.reseneeco.2003.11.009

Pfeiffer, L., Gratz, T., 2016. The effect of rights-based fisheries management on risk taking and fishing safety. Proc. Natl. Acad. Sci. 113, 2615. doi:10.1073/pnas.1509456113

PFMC, 2010. Rationalization of the Pacific Coast Groundfish Limited Entry Trawl Fishery: Final Environmental Impact Statement. Pacific Fishery Management Council, Portland, OR.

PFMC, 2000. Amendment 12 to the Pacific Coast Groundfish Fishery Management Plan. Portland, OR.

PFMC, 1998. Amendment 11 to the Pacific Coast Groundfish Fishery Management Plan (Appendix B). Pacific Fisheries Management Council, Portland, OR.

PFMC, 1996. Amendment 9 to the Fishery Management Plan for Pacific Coast Groundfish (Limited Entry Fixed Gear Sablefish Endorsement). Pacific Fisheries Management Council, Portland, OR.

Poe, G.L., Schulze, W.D., Segerson, K., Suter, J.F., Vossler, C.A., 2004. Exploring the performance of ambient-based policy instruments when nonpoint source polluters can cooperate. Am. J. Agric. Econ. 86, 1203–1210.

Poos, J.J., Bogaards, J.A., Quirijns, F.J., Gillis, D.M., Rijnsdorp, A.D., 2010. Individual quotas, fishing effort allocation, and over-quota discarding in mixed fisheries. ICES J. Mar. Sci. 67, 323–333. doi:10.1093/icesjms/fsp241

Porras, I., Grieg-gran, M., Neves, N., 2008. All that glitters: A review of payments for watershed services in developing countries. London, UK.

Reed, M.S., 2008. Stakeholder participation for environmental management: A literature review. Biol. Conserv.

Reichhuber, A., Camacho, E., Requate, T., 2009. A framed field experiment on collective enforcement mechanisms with Ethiopian farmers. Environ. Dev. Econ. 14, 641. doi:10.1017/S1355770X09005178

Robalino, J., Pfaff, A., 2013. Ecopayments and deforestation in Costa Rica: A nationwide analysis of PSA's initial years. Land Econ. 89, 432–448. doi:10.1353/lde.2013.0027

Robinson, B.E., Holland, M.B., Naughton-Treves, L., 2013. Does secure land tenure save forests? A meta-analysis of the relationship between land tenure and tropical deforestation. Glob. Environ. Chang. doi:10.1016/j.gloenvcha.2013.05.012

Rode, J., Gómez-Baggethun, E., Krause, T., 2015. Motivation crowding by economic incentives in conservation policy: A review of the empirical evidence. Ecol. Econ. 117, 270–282. doi:10.1016/j.ecolecon.2014.11.019

Rodriguez, L.A., Shapiro-Garza, E., Pfaff, A., 2015. Los de arriba y los de abajo, los de la selva y los de la ciudad: Perspectivas de los proveedores y los usuarios de servicios ambientales en el programa de fondos concurrentes de CONAFOR.

Rudel, T.K., 1998. Is There a Forest Transition? Deforestation, Reforestation, and Development. Rural Sociol. 63, 533–552. doi:10.1111/j.1549-0831.1998.tb00691.x

Rudel, T.K., 1989. Population, development, and tropical deforestation: A cross-national study. Rural Sociol. 54, 327–338.

Rudel, T.K., Coomes, O.T., Moran, E., Achard, F., Angelsen, A., Xu, J., Lambin, E., 2005. Forest transitions: towards a global understanding of land use change. Glob. Environ. Chang. 15, 23–31. doi:10.1016/j.gloenvcha.2004.11.001

Rudel, T.K., Defries, R., Asner, G.P., Laurance, W.F., 2009. Changing drivers of deforestation and new opportunities for conservation. Conserv. Biol. 23, 1396–1405. doi:10.1111/j.1523-1739.2009.01332.x

Ryan, R., Deci, E., 2000. Self-determination theory and the facilitation of intrinsic motivation, social development, and well-being. Am. Psychol. 55, 68–78. doi:10.1037/0003-066X.55.1.68

SAFMC, 2013. Snapper Grouper Fishery Management Plan Amendment 18B (Apendix F: History of Management of the South Atlantic Snapper Grouper Fishery). South Atlantic Fishery Management Council, Tampa, FL.

Sagara, T., 2011. India Place Finder [WWW Document]. URL http://india.csis.u-tokyo.ac.jp/

Scheld, A.M., Anderson, C.M., Uchida, H., 2012. The Economic Effects of Catch Share Management: The Rhode Island Fluke Sector Pilot Program. Mar. Resour. Econ. 27, 203–228. doi:10.5950/0738-1360-27.3.203

Schroeder, N.M., Castillo, A., 2013. Collective action in the management of a tropical dry forest ecosystem: Effects of Mexico's property rights regime. Environ. Manage. 51, 850–861. doi:10.1007/s00267-012-9980-9

Segerson, K., 1988. Uncertainty and incentives for nonpoint pollution control. J. Environ. Econ. Manage. 15, 87–98. doi:10.1016/0095-0696(88)90030-7

Segerson, K., Wu, J., 2006. Nonpoint pollution control: Inducing first-best outcomes through the use of threats. J. Environ. Econ. Manage. 51, 165–184. doi:10.1016/j.jeem.2005.04.007

Shadish, W., Cook, T., Campbell, D., 2002. Experimental and Quasi-experimental Designs for Generalized Causal Inference. Houghton Mifflin Co., Boston.

Shapiro-Garza, E., 2013. Contesting the market-based nature of Mexico's national payments for ecosystem services programs: Four sites of articulation and hybridization. Geoforum.

Sims, K.R.E., Alix-Garcia, J.M., 2017. Parks versus PES: evaluating direct and incentive-based land conservation in Mexico. J. Environ. Econ. Manage. doi:http://dx.doi.org/10.1016/j.jeem.2016.11.010

Smith, M., Zhang, J., Coleman, F., 2006. Effectiveness of marine reserves for large-scale fisheries management. Can. J. Fish. Aquat. Sci. 63, 153–164. doi:10.1139/F05-205

Smith, M.D., Asche, F., Bennear, L.S., Oglend, A., 2014. Spatial-Dynamics of Hypoxia and Fisheries : The Case of Gulf of Mexico Brown Shrimp Spatial-dynamics of Hypoxia and Fisheries : The Case of Gulf of Mexico Brown Shrimp. Mar. Resour. Econ. 29, 111–131.

Smith, M.D., Asche, F., Birkenbach, A., Cojocaru, A.L., Guttormsen, A.G., 2016. Intra-seasonal Behavior in Multispecies Catch Share Fisheries (No. EE 16-01), Duke Environmental and Energy Economics Working Paper Series. Durham, NC.

Smith, M.D., Oglend, A., Kirkpatrick, A.J., Asche, F., Bennear, L.S., Craig, J.K., Nance, J.M., 2017. Seafood prices reveal impacts of a major ecological disturbance. Proc. Natl. Acad. Sci. 114, 1512–1517. doi:10.1073/pnas.1617948114

Sommerville, M., Milner-Gulland, E.J., Rahajaharison, M., Jones, J.P.G., 2010. Impact of a community-based payment for environmental services intervention on forest use in Menabe, Madagascar. Conserv. Biol. 24, 1488–98. doi:10.1111/j.1523-1739.2010.01526.x

Spraggon, J., 2004. Testing ambient pollution instruments with heterogeneous agents. J. Environ. Econ. Manage. 48, 837–856. doi:10.1016/j.jeem.2003.11.006

Spraggon, J., 2002. Exogenous targeting instruments as a solution to group moral hazards. J. Public Econ. 84, 427–456. doi:10.1016/S0047-2727(01)00088-3

Strauss, K., 2013. British Columbia Integrated Groundfish Program AUTHOR.

Swallow, B., Meinzen-Dick, R., 2009. Payment for environmental services: Interactions with property rights and collective action, in: Beckmann, V., Padmanabhan, M. (Eds.), Institutions and Sustainability: Political Economy of Agriculture and the Environment-Essays in Honour of Konrad Hagedorn. Springer Netherlands, pp. 243–265. doi:10.1007/978-1-4020-9690-7_12

Townshend, J., Hansen, M., Carroll, M., DiMiceli, C., Sohlberg, R., Huang, C., 2011. User Guide for the MODIS Vegetation Continuous Fields product Collection 5 version 1.

Tveterås, S., Asche, F., Bellemare, M.F., Smith, M.D., Guttormsen, A.G., Lem, A., Lien, K., Vannuccini, S., 2012. Fish is food--the FAO's fish price index. PLoS One 7, e36731.

Tveterås, S., Paredes, C.E., Pena-Torres, J., 2011. Individual vessel quotas in Peru: stopping the race for anchovies. Mar. Resour. Econ. 26, 225–232.

van de Walle, D., 2009. Impact evaluation of rural road projects. J. Dev. Eff. 1, 15–36. doi:10.1080/19439340902727701

van de Walle, D., 2002. Choosing Rural Road Investments to Help Reduce Poverty. World Dev. 30, 575–589.

van Noordwijk, M., Leimona, B., 2010. Principles for fairness and efficiency in enhancing environmental services in Asia: Payments, compensation, or co-investment? Ecol. Soc. 15.

Veld, K. van't, Narain, U., Gupta, S., Chopra, N., Singh, S., 2006. India's Firewood Crisis Re-examined, Resources for the Future Working Paper.

Velez, M.A., Murphy, J.J., Stranlund, J.K., 2010. Centralized and decentralized management of local common pool resources in the developing world: Experimental evidence from fishing communities in Colombia. Econ. Inq. 48, 254–265. doi:10.1111/j.1465-7295.2008.00125.x

von Thunen, J.H., 1826. The Isolated State (1996 trans., eds. Hall, P. and Wartenberg, C. M.). Pergamon Press, Oxford, UK.

Vossler, C.A., Poe, G.L., Schulze, W.D., Segerson, K., 2006. Communication and incentive mechanisms based on group performance: An experimental study of nonpoint pollution control. Econ. Inq.

Wahl, I., Muehlbacher, S., Kirchler, E., 2010. The impact of voting on tax payments. Kyklos 63, 144–158. doi:10.1111/j.1365-2893.2011.01128.x.THE

Walker, J.M., Gardner, R., Herr, A., Ostrom, E., 2000. Collective choice in the commons: Experimental results on proposed allocation rules and votes. Econ. J. 110, 212–234. doi:10.1111/1468-0297.00497

Warr, P., 2010. Roads and poverty in rural laos: An econometric analysis. Pacific Econ. Rev. 15, 152–169. doi:10.1111/j.1468-0106.2009.00494.x

Weninger, Q., 1998. Assessing efficiency gains from individual transferable quotas: an application to the mid-Atlantic surf clam and ocean quahog fishery. Am. J. Agric. Econ. 80, 750–764.

Wilen, J., 2006. Why fisheries management fails: treating symptoms rather than the cause. Bull. Mar. Sci. 78, 529–546.

Williams, G.H., Blood, C.L., 2003. Active and Passive Management of the Recreational Fishery for Pacific Halibut off the U.S. West Coast. North Am. J. Fish. Manag. 23, 1359–1368. doi:10.1577/M01-233AM

World Bank, 2015. Forest area (percent of land area) [WWW Document]. URL http://data.worldbank.org/indicator/AG.LND.FRST.ZS (accessed 10.5.16).

Worm, B., Hilborn, R., Baum, J.K., Branch, T. a, Collie, J.S., Costello, C., Fogarty, M.J., Fulton, E. a, Hutchings, J. a, Jennings, S., Jensen, O.P., Lotze, H.K., Mace, P.M., McClanahan, T.R., Minto, C., Palumbi, S.R., Parma, A.M., Ricard, D., Rosenberg, A. a, Watson, R., Zeller, D., 2009. Rebuilding global fisheries. Science (80-.). 325, 578–85. doi:10.1126/science.1173146

Wunder, S., 2007. The efficiency of payments for environmental services in tropical conservation. Conserv. Biol. 21, 48–58. doi:10.1111/j.1523-1739.2006.00559.x

Wunder, S., Engel, S., Pagiola, S., 2008. Taking stock: A comparative analysis of payments for environmental services programs in developed and developing countries. Ecol. Econ. 65, 834–852. doi:10.1016/j.ecolecon.2008.03.010

Wünscher, T., Engel, S., Wunder, S., 2008. Spatial targeting of payments for environmental services: A tool for boosting conservation benefits. Ecol. Econ. 65, 822–833.

Xepapadeas, A.P., 1991. Environmental policy under imperfect information: Incentives and moral hazard. J. Environ. Econ. Manage. 20, 113–126.

Yañez-Pagans, P., 2015. Cash for cooperation? Environmental conditional cash transfers and common property management in Mexico, Working Paper.

Biography

David Kaczan is an economist with a focus on environment, agriculture, natural resources, and development topics. His research to date uses microeconomic theory, econometrics, GIS, field-laboratory experiments, and interviews to test hypotheses and develop policy recommendations. He has a strong interest in interdisciplinary approaches towards environmental problems.

Prior to commencing the work in this dissertation, Kaczan studied the use of environmental payments for forest conservation in Tanzania, the prevalence of water-related poverty in West Africa, and the impacts of climate change on Australian agriculture, among other topics. He spent a number of years working at the Australian National Research Agency CSIRO. He has an M.A (Economics) from Duke University, an M.Sc (Agricultural and Resource Economics) from the University of Alberta, and a B.A (Economics) and B.Sc (Ecology, Environmental Geosciences) from the University of Adelaide.