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Three Essays in Environmental Economics

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by

Ty Colin Robbins

Committee in charge:

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I dedicate this work to my family.
Your absurdity and love keep me sane.

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Abstract

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My dissertation is comprised of three separate essays in the field of environmental economics. The first chapter experimentally models the climate change social dilemma and evaluates how heterogeneous environmental impacts and unequal endowments affect the propensity to avoid catastrophic climate change. Introducing a punishment mechanism to alleviate the collective bargaining problem, I identify the external factors and intrinsic preferences that impede cooperation. Inequality and delayed contributions negatively affect successful provision, while higher levels of collective-risk increase the probability of threshold attainment. A consensual punishment mechanism incentivizes cooperation in low-risk and heterogeneous groups, overcoming the collective action problem.

The second chapter investigates the efficacy of military and legal efforts to thwart environmental domestic terrorism. While passive legislative interventions increase the cost of illegal action and proactive policies thwart terrorism with preemptive strikes, the efficacy of counterterrorism efforts has been questioned. Using quarterly data from 1980 to 2014, I analyze the effect of counterterrorism policy on radical environmental direct action (REDA) modes of attack and the severity of illegal actions. Combining vector autoregression and intervention analysis under a rational choice framework, I find that while legislative policies have decreased the economic severity of attacks, incidents have more than doubled. Proactive interventions reduce domestic terrorism, but by a smaller magnitude than the increase from passive legislation. Substituting between modes of

attack and ideological targets, policies have tripled the use of explosives while REDA attacks against people have increased more than sixfold in the long run.

In the final chapter, I explore the role of payments for ecosystem services (PES) and their impact on conservation efforts to avoid deforestation in developing nations. Targeting counterfactual-based studies to identify additionality gains and minimize leakage impacts, I perform a meta-analysis to evaluate how PES program design and market factors impact avoided deforestation. Program design variables include contract length, payment differentiation, and participation targeting. Environmental variables proxy for opportunity costs by controlling for alternative land use prices and socioeconomic conditions. As each dimension has a varying impact on avoided deforestation, these results aim to influence future market-based interventions.

Contents

Chapter 1

Punishment, Inequality, and Heterogeneous Risk in Threshold Public Goods Games: Tackling the Climate Change Social Dilemma

Abstract: International efforts to stave off adverse climate change have been lengthy in deliberation and largely unsuccessful. While emission reductions are costly for the mitigating individual but benefit the global population, additional factors (varying risk susceptibility to climate change, heterogeneous mitigation efforts, and inequitable pollution shares) have inhibited the ability to reach a binding environmental agreement. I experimentally characterize the climate change social dilemma and evaluate how heterogeneous environmental impacts and unequal endowments affect the propensity to avoid catastrophic climate change. Introducing a punishment mechanism to alleviate the collective bargaining problem, I identify the external factors and intrinsic preferences that impede cooperation. Inequality and delayed contributions negatively affect successful

provision, while higher levels of collective-risk increase the probability of threshold attainment. A consensual punishment mechanism incentivizes cooperation in low-risk and heterogeneous groups, overcoming the collective action problem. Social preferences yield guilt that increases contributions while risk aversion negatively impacts threshold attainment in a game with strategic uncertainty.

1.1 Introduction

A future devoid of dangerous climate change constitutes a global public good with costs incurred by emission-reducing nations and a universal benefit. Disagreements exist as to distributional responsibility in tackling this global dilemma, resulting in an under provision of the public good (Barrett, 2007). Though international meetings extend discussion and boost cooperation, the question persists on whether heterogenous agents can collectively reduce global emissions to avoid the ubiquitous risk of dangerous climate change.

Framed as a "collective-risk social dilemma" (Milinski et al., 2008), a group of agents must often cooperate to provide a public good where failure to do so may harm all individuals. Many examples can be found in public policy and even in cinematic plots, from building levees for flood prevention to vaccinating all citizens to prevent the zombie apocalypse. All it takes is a weak link in the chain of social cooperation to undermine the vested efforts of the willing and leave the global population susceptible to risk. In the context of climate change, this social dilemma is prominent in the effort to reduce emissions to maintain a habitable climate. Rational agents may desire a future without catastrophic changes, but the magnitude and timing of emission reductions continue to be conditionally voluntary. Reasons for reneging on prior commitments include free rid-

ing off the investments of other nations, scientific uncertainty in climate thresholds, and the inequity of developing countries reducing emissions while developed countries benefit from significant historical emissions.

The theoretical and experimental literatures have identified the impact of group size and repeated play in providing public goods, but the climate change dilemma poses a richer environment. In this chapter I investigate the influence of varying risk susceptibility to climate change, heterogeneous mitigation efforts, and inequitable pollution shares on contributions to a threshold public good. High risks of financial loss can incentivize prosocial contributions, but when the risk is only as high as the necessary average investment (or lower), groups fail to reach a common target (Milinski et al., 2008). Assuming homogeneous risk susceptibility to economic loss, however, negates the reality that nations will experience heterogeneous impacts from dangerous climate change. Endowment inequality creates an additional barrier to threshold attainment. Highly-endowed agents often fail to signal cooperation early enough to achieve social efficiency (Tavoni et al., 2011). My experiment models the interaction of heterogeneous risk and endowment inequality on group contributions in the climate game. In groups of four, participants contribute to a climate fund over tens rounds to reach a target threshold. Assigned risk factors denote the individual probability of losing net endowments if a group threshold is not met. Each group has two "rich" and two "poor" players controlled by random predetermined play to induce endowment inequality. Without a salient threat of economic loss, heterogeneity in risks and endowments across group members will deter cooperation relative to the homogeneous baseline.

Complementing the external factors that influence behavior in social dilemmas, intrinsic preferences have only recently been modeled in public-choice research (Ostrom,

1998; Teyssier, 2012). Agents endowed with different levels of risk and inequality have varying attitudes about their economic standing relative to their peers. While risk and inequity aversion have been incorporated in sequential games, I examine the impact of social preferences on cooperative behavior in a multi-period public goods setting. Contributions in the threshold public goods game minimize the probability of economic loss and signal prosocial behavior, suggesting the existence of altruism. Individuals who encourage redistributive policies to reach the social optimum also benefit from their own actions, either directly satisfying their desire for fairness or indirectly influencing cooperation among the group. The reciprocation or deflection of social responsibility to contribute toward the public good is thus contingent on both heterogeneous endowments and preferences. A primary focus of this chapter is to isolate the external and internal factors that simultaneously impact social cooperation in a collective-risk dilemma. A follow-up questionnaire helps identify the motives of individuals who disproportionately contribute to avoid dangerous climate change.

While a joint high probability of losing net earnings frequently induces collective action, the added dimensions of heterogeneous risk, endowments, and intrinsic preferences complicate public good provision. Communicating a non-binding intended contribution increases the probability of threshold attainment (Tavoni et al., 2011), but cheap talk with layers of heterogeneity may necessitate a credible threat. I introduce a consensual punishment mechanism to incentivize cooperation. At the conclusion of the multi-period threshold game, regardless of threshold attainment, players may choose to collectively punish an individual group member. This instrument inhibits unilateral penalties and emulates regional or global economic sanctions levied against free riders.

In this chapter I find that a consensual punishment mechanism is only effective when

perceived risk of economic loss is low among homogeneous or heterogeneous agents. Complementing Milinski et al. (2008), coordination issues are overcome if perceived risk is high enough in homogeneous treatments. Nations in reality develop their own climate change susceptibility beliefs based on political agendas and scientific reports. With a wide spectrum of risk beliefs among countries, I find that punishment may be the "great equalizer" that eliminates total contribution differences between homogeneous and heterogenous groups. Rich players within successful groups significantly reduce, but do not eliminate, the endowment inequality gap. Large early contributions enable rich players to signal cooperation and increase the likelihood of threshold attainment by overcoming trust issues commonly held by poor players. Punishment reduces a coordination problem in heterogeneous groups by holding low-risk players accountable and incentivizing early cooperation.

Incorporating social preferences alongside the previous external factors into a random-effects model, while a player's relative wealth fluctuates every round, both guilt and envy influence individual contributions. Supported by the follow-up questionnaire, guilt is ten times more influential than envy as players avoid material advantages even when faced with heterogeneous risks. Risk aversion is a proxy for distrust in a game with strategic uncertainty and negatively impacts poor player contributions. Aggregating intrinsic preferences, group composition can further influence cooperation. Comparable to the individual analysis, higher levels of collective guilt boost cooperation while increasing levels of mean risk aversion continue to decrease levels of trust and negatively impact contributions. Although intrinsic preferences influence individual contributions, high risk in homogeneous treatments negates the impact of social preferences as a substantial risk of economic loss is the ultimate free riding deterrent. In heterogeneous groups with a complex coordination problem, envy plays a significant role in decreasing contributions

while greater variation in aggregated risk aversion further stunts cooperation.

The remainder of this chapter is organized as follows: Section 1.2 presents a thorough literature review on the varying components of the experiment and my contribution to the literature. After reporting the results of traditional public goods games, I exhibit how threshold mechanisms and collective risk have altered the direction of public goods research. Section 1.3 introduces the experimental design, a procedure for eliciting and quantifying aversion preferences, and the threshold public goods game that simulates dangerous climate change. I also delineate the intricacies of a punishment mechanism implemented in half of the treatments to incentivize cooperation. Section 1.4 presents my hypotheses. External factors (inherited risk susceptibility & endowment inequality) and intrinsic preferences (risk and inequality aversion) impact both individual and group behavior. My hypotheses account for how these factors, in conjunction with punishment possibilities, influence cooperation between the different treatments. Section 1.5 reports trends among the estimation of aversion instruments. Experimental findings are presented in Section 1.6, both summary statistics in the threshold public goods game and a random-effects regression analysis testing for the significance of external and internal factors across treatments. Section 1.7 concludes.

1.2 Literature Review

1.2.1 Public Goods Games

In a traditional public goods game (PGG), homogenous agents have the option to voluntarily contribute personal endowments to attain a public good. Public good costs and benefits are typically linear in the contribution amount and there is no inherent risk

when failing to supply the good, other than potentially losing prior investments and a lack of the public amenity. The threshold PGG varies slightly, incorporating a contribution target that needs to be met or exceeded to supply the public good. Some models incorporate refunds of private investment in the event that the threshold target is not met (making it less risky to contribute), while others utilize a rebate system that returns funds contributed over the necessary threshold (Ledyard, 1995). Within the global game of climate change and emission reductions, no such refund or rebate system exists that can dually compensate over/under efforts to curb green house gases (GHGs), so this chapter will also preclude from such possibilities. In lieu of receiving a traditional public good payoff for successful cooperation, when agents fail to achieve the targeted number of avoided emissions (or contributions toward the public good), all agents face a probabilistic risk of losing remaining endowments not invested in the group account. In reality, contributing to the climate public good by reducing emissions may yield continuous benefits before and up to the threshold, including health gains and a reduced risk of dangerous climate change. In an effort to maintain transparency and simplicity for experimental participants, however, I focus on discrete benefits (i.e., avoiding the risk of losing remaining private endowments) that only apply with threshold attainment.[1]

Theoretical and empirical research for the public goods problem have produced varying equilibria in games with and without a threshold. In a classic PGG without a target threshold, the dominant strategy to maximize individual payoffs is to free ride off the contributions of others, resulting in a socially inefficient outcome. In a survey of experimental research, Ledyard (1995) finds that in repeated linear game settings with small groups,

[1] Asch et al. (1993) examined contribution levels for discrete public goods distributed with a provision point mechanism relative to a continuous public good that returned a constant fraction of group contributions to all subjects for all contribution levels. Though free riding is a dominant strategy in the continuous case and not in the provision of discrete public goods, the authors found that contribution levels were not significantly different.

results converge to the zero contribution equilibrium theoretically predicted. Though the noncooperative equilibrium is rational, recent empirical evidence finds positive initial contributions ranging from 40 to 60 percent of period endowments in a rich variety of models. Utility maximization theory is unable to explain the existence of social preferences, which include inequality aversion, selfishness, and other preferences that differ from material interests (Fehr and Schmidt, 1999; Charness and Rabin, 2002). Analyzing strategic decisions and learning in repeated two-stage contribution games, Muller et al. (2008) show that though contributions are initially positive and decrease over time, experience generates smaller declines in contribution levels between stages in repeated games.

Croson and Marks (2000) explore devices to correct the noncooperative equilibrium by introducing a provision point (threshold) mechanism, intended to increase the costs of free riding and induce cooperation. In traditional public goods games individual free riding marginally impacts the level of public good provision. Using a provision point mechanism, given that a threshold contribution target must be met, individual deviations toward the free rider equilibrium could result in a total lack of public good provision. Multiple theoretical equilibria exist in these games: a set of efficient equilibria where the public good is provided when the threshold is exactly met and a set of inefficient equilibria where provision fails to meet the threshold and the public good is not provided. Complementing Isaac et al. (1989), Croson and Marks find that the inclusion of the threshold mechanism significantly induces higher contributions to the public good. Successful provision, however, varies according to the size of the threshold relative to group wealth, as relatively smaller targets are easier to attain and result in greater provision rates. Bagnoli and McKee (1991) obtain a high rate of threshold attainment (nearly 90%), but had low threshold targets relative to group wealth (about 23%). As the International

Panel on Climate Change (Metz et al., 2007) has called for a 50% reduction in current GHG levels to reduce the risk of dangerous climate change, I adopt this provision point as the target threshold in my experiment.

1.2.2 Collective Risk

Failure to reach a target threshold in the climate game results in a probabilistic risk among all agents of losing remaining net endowments. Embedding this risk factor differs from the traditional setting where failure to reach a threshold merely leads to non-provision of the public good. Incorporating a homogeneous risk factor into their experimental setup of avoiding the public bad of dangerous climate change, Milinski et al. (2008) conclude that a strategy to solve the collective-risk social dilemma is to convince agents that failure to reach the target contribution threshold will result in significant individual financial loss. Modeling 10 groups for each treatment of 10, 50, and 90 percent risk probabilities of losing net endowments when a target threshold is not met, the study found 5 of the 10 groups in the 90% treatment successfully collected the target sum, while the other 5 groups marginally failed. Of the 50 and 10 percent treatments, one and zero groups respectively achieved the target, suggesting the severity of potential risks and economic ruin may induce cooperation. Santos and Pacheco (2011) also found that decisions within small groups under high risk scenarios increase the coordination rate. Large-scale cooperation, they conclude, is difficult to achieve and collective action problems may be better solved with a combination of decentralized local agreements focusing on region-specific issues[2]. If coordination cannot be achieved with high risk factors and small groups, experimental results maintain external validity in the context of the world's nations tackling dangerous climate change.

[2]International environmental agreements (IEA) are typically addressed in a single group setting (Barrett (1997); Asheim et al. (2006))

While incorporating risk is critical to model the provision of climate public goods, nations are unequally susceptible to catastrophic impacts (Metz et al., 2007).[3] Commonly cited consequences of dangerous climate change include the West Antarctic ice sheet collapse and a resulting sea level rise. All nations whose economies are connected through international trade will feel an adverse effect from this event, but citizens in Kentucky do not face equal risks of losing everything compared to those who live in small island states, like Fiji. These latter nations are highly susceptible to extreme loss in the event of catastrophic environmental change. Fisher et al. (1995) do not vary individual risk factors, but instead vary valuation of a public good among subjects finding that group contributions increase relative to the homogeneous valuation baseline. These results may be driven by the common finding that contributions rise as valuations rise, not proving that valuation heterogeneity exclusively overcomes collective action problems. Fischbacher et al. (2012) find that heterogeneity in the return to public goods negatively affects unconditional contributions. Instead of heterogeneous valuations in the traditional public goods setting, games involving collective risk require modeling heterogeneous risk probabilities on threshold attainment.[4] Dividing group members according to high and low risk susceptibility, this chapter models heterogeneity in the global climate game and explores solutions to incentivize social cooperation.

1.2.3 Endowment Inequality in PGGs

Emission reduction agreements are hindered by economic inequality between developed and developing countries. Technology used to decrease emission production is costly and developing nations find it difficult to finance such investments. Developed nations

[3]Unequal risks among affected regions depend on geographic location, ecological conditions, prior preparation for extreme events, and past investments (Ostrom, 2010).

[4]Heterogeneous risk probabilities can also be interpreted as asymmetric adaptation capabilities.

introduced a Green Climate Fund to redistribute aid to poor countries for green investment but significant delays inhibit the program. Distributional responsibility for cutting emissions and sharing costs continues to be a key tipping point in the development of an international climate change treaty.

Relaxing assumptions from earlier work by Warr (1983) that found group contributions to a public good should be invariant after income redistribution, Bergstrom et al. (1986) show that income redistribution away from noncooperators may actually increase group contributions to the public good. An experiment by Chan et al. (1996) reveals that while mean group contributions do increase with redistribution, individual contributions significantly vary between rich and poor players. Inconsistencies between the theoretical model and experiment are possibly driven by notions of fairness. Incorporating social preferences, behavioral models predict that higher income individuals contribute a larger share of their endowments than do low income individuals, though empirical support for this claim is mixed (Fehr and Schmidt, 1999; Charness and Rabin, 2002). Anderson et al. (2008) find that common knowledge of each individual's relative endowment reduces contributions for all participants in public goods games. Reuben and Riedl (2011) also find that free riding is frequent and steadily increases over time comparably in heterogeneous and homogeneously endowed groups. Integrating variation in both income and preferences, Chan et al. (1999) conclude that inequality in one dimension has a strong positive impact on public good provision while heterogeneity in both dimensions simultaneously yields a smaller but still significant effect. Conflicting evidence of contribution levels in the presence of inequality necessitates further empirical investigation.

Modeling endowment inequality in the climate game with homogeneous risk among group members, Tavoni et al. (2011) augmented Milinski et al. (2008) to evaluate how

equity concerns between rich and poor individuals affect group contributions in the attainment of a climate threshold. As the 2009 Copenhagen Climate Change Conference introduced a pledge system for emission reductions, Tavoni et al. induced coordination by including an option to communicate non-binding intended contributions. They found that early signaling by rich agents increased the probability of meeting the target threshold. While communication may alleviate collective action problems, cheap talk might ensue, requiring a stronger mechanism to provide credible threats. Ostrom (2010) notes that persistent communication and updated monitoring, without relying on preexisting levels of trust, are important devices needed to solve the collective action problem. This chapter's experiment takes a similar approach to Chan et al. (1999) by incorporating two types of inequality, endowment and risk heterogeneity, to model the climate game.

1.2.4 Risk Aversion versus Inequality Aversion

Climate change modeling has sparked debate regarding a simplifying assumption that exploits a single parameter (the elasticity of marginal utility of consumption) to capture (i) risk aversion, (ii) intertemporal substitution preferences, and (iii) spatial inequality aversion (Dasgupta, 2007; Dietz et al., 2007; Nordhaus, 2007). My experiment distinguishes risk and inequality aversion to model behavior in cooperative games. Traditional efforts to measure risk aversion present experimental subjects with a set of pairwise choices between lottery distributions containing the same mean but different variances, associating preferences based on the individual's decisions. Kroll and Davidovitz (2003) argue that when the less unequal state is preferred, the subject could be considered inequality and risk averse, rather than exclusively one or the other. Carlsson et al. (2005) investigate the determinants of individual risk (holding inequality constant) and inequality aversion (holding risk constant). They find that inequality averse subjects

are also more risk averse (and vice versa), and that both factors vary significantly with sex, field/major, and political preference. Building off of Kroll and Davidovitz (2003), Magdalou et al. (2009) do not find a significant correlation between the two aversion parameters. Teyssier (2012) finds that risk aversion in a sequential public goods game is negatively correlated with contribution levels of leading movers, while advantageous inequity averse second movers tend to free ride less and cooperate more than others. While contributions may be simultaneously influenced by risk and inequality aversion profiles, a collective-risk threshold PGG introduces a separate psychological risk factor of non-attainment. This chapter elicits risk and inequality aversion, distinguishing the influence of intrinsic preferences on social cooperation.

1.2.5 Punishment Mechanism

Experimental studies have found that costly options to punish free riders greatly incentivize sustained contributions toward public good provision (Ostrom et al., 1992; Fehr and Gächter, 2000). Agents who contradict socially acceptable behavior are retaliated against even if tangible benefits from costly punishment is negligible. While the existence of peer-to-peer sanctions can induce near efficient cooperation, varying the effectiveness of punishment (the factor by which punishment reduces a punished player's income) below relative income thresholds may not be able to prevent the cooperation failure (Nikiforakis and Normann, 2008). Complicating matters is the disparate use of linear versus non-linear punishment that can incorporate variable fine-to-fee sanction schedules (Casari, 2005). The fine-to-fee ratio reflects the punished player's reduced income relative to the punisher's fee to punish. When punishment reduces the punished player's income by a certain percentage in non-linear studies, punishment effectiveness becomes convex in the target's income, making comparisons between treatments difficult.

Higher levels of punishment effectiveness have also been found to increase the propensity to punish (Anderson and Putterman, 2006; Carpenter, 2007). Subjects in these studies who wish to impose punishment on a pair of individuals may opt to only punish the player whose fine-to-fee ratio is greatest, all else equal, maximizing sanctions given costly punishment. Examining this player's decision strategy would fail to reveal the demand for punishing both agents, permitting only partial preference identification. Studies have also identified a propensity for contributors to punish defectors even when the fine-to-fee ratio is one (Falk et al., 2005; Sefton et al., 2005; Nikiforakis and Normann, 2008). In this scenario punishment does not reduce income differences between individuals and may instead indicate the desire to sanction particular actions. Casari (2005) exhibited that the fine-to-fee punishment ratio must be constant for all agents to credibly identify the factors that induce punishment decisions. Successful collective action also depends partly on the types of individuals that comprise the group, shown in Ones and Putterman (2007), where homogeneous and heterogeneous group formation by punishment proclivity helped predict the differences in contributions to a public good.

Bochet et al. (2006) paired punishment with communication, concluding that the paired mechanisms do not significantly increase homogeneous group contribution levels relative to communication alone. They also detected the existence of perverse punishment, that is, punishment being directed at individuals whose contributions were higher than average. Punishment modeled with endowment heterogeneity generally induces stable cooperation in both homogeneous and heterogeneous groups (Visser and Burns, 2006; Reuben and Riedl, 2011). Prediger (2011) finds that heterogeneous groups punish less often and at smaller magnitudes, yielding higher group contributions. Casari and Luini (2009) used a consensual rule mechanism to influence cooperation where punishment

was only carried out if a coalition of two or more agents chose to sanction an individual. Consensual punishment induced higher cooperation and lower rates of punishment than autonomous punishment. The authors contend that the lower threat of punishment under the consensual treatment provides stronger incentives for cooperation as this mechanism censors out perverse punishment, effectively blocking more than 70 percent of attempts to punish strong cooperators while only 10 percent of requests to punish free riders was blocked. Gächter and Herrmann (2006) find that autonomous punishment without constraint may cripple cooperation and circumvent the gains from punishment that intend to credibly threaten free riders. Improved institutions beyond autonomous peer-to-peer punishment seem to exist and warrant closer examination.

In the realm of threshold PGGs, particularly with collective risk, there appears to be a dearth of research that incorporates a punishment mechanism to generate collective action. This chapter contributes to the literature by enriching the experimental environment and identifying the elicited individual preferences and external factors that dually inhibit and induce cooperation.

1.3 Experimental Design

All sessions of the study were carried out in the Experimental and Behavioral Economics Laboratory at UC Santa Barbara with 216 subjects recruited from an ORSEE research pool, programmed and conducted with the experiment software z-Tree (Fischbacher, 2007). At this undergraduate level, subjects have little training in expected utility and public goods games. Fifteen experimental sessions were run, involving between 8 and 16 participants per session (depending on show-ups) who earned an average of $12.20 for about 50 minutes of their time.

In each treatment subjects first played three independent games (described below) followed by a threshold public goods game for 10 rounds. In this last game participants were randomly assigned into four-person groups that were held constant for the duration of play.[5] Instructions appeared on a subject's computer monitor before each independent game, followed by a short set of control questions to check understanding. Any questions of misunderstanding were answered privately. The experimental design avoided giving subjects feedback related to their own and group earnings in the sub-games before the threshold PGG, restricting any incentive to alter behavior in future games based on prior performance. Subjects had common information of the repeated play format with constant group members in the threshold PGG. At the conclusion of this fourth game, a questionnaire was given to elicit beliefs and socioeconomic data. Experimental Tokens (ETs) were used in all games, with a conversion rate boldly stated in each game's instructions. Show up fees were $5 with the chance to increase payoffs depending on individual and group play in the subsequent games. To incentivize maximum effort, subjects were informed at the beginning of the session that final payoffs would be contingent upon the outcome from one randomly selected game of the games played.[6] Final individual payoffs were distributed privately at the end of the session.

1.3.1 Risk Elicitation

In the three independent games played before the threshold PGG, I quantitatively measure risk and inequality aversion.[7] Assuming that a subject's preferences can be ap-

[5]Constant groups in the threshold PGG allows for reputation effects. International environmental agreements typically hold constant the group of nations making collective decisions as there is rarely a change in the number of countries taking part in negotiations.

[6]Participants were advised that one of their peers would roll a die at the conclusion of the experiment to determine the game used for final payoffs.

[7]Complete experimental instructions are included in the Appendix.

proximated by a particular expected utility model, risk aversion is traditionally measured with a lottery game (Holt and Laury, 2002). Regression analysis and qualitative interpretation are improved with point-estimates of aversion parameters whereas Holt's method only supplies an interval estimate of risk aversion. To avoid the analytical problems of interval estimates, I instead employ the method developed in Charness and Genicot (2009) to measure risk aversion. Individuals are endowed with 100 tokens and can invest any portion of this amount in a risky asset that yields a payoff of 2.5 times the amount invested if successful (50% probability)[8], always retaining net endowments not invested. The more an individual invests in a risky asset the less risk-averse she is relative to her peers. Assuming constant relative risk aversion and eliciting the investment decision, I then calculate each individual's point-estimate risk aversion parameter.[9] Robustly measuring risk aversion, Holt's method was utilized during the followup questionnaire to compare against the point-estimates elicited. Subjects sequentially chose between a set of pairwise lotteries, where a given lottery differs from its counterpart in the spread of potential payments and the lottery mean (Beckman et al., 2004).[10]

1.3.2 Inequality Aversion

I adopt the approach from Blanco et al. (2011) and Dannenberg et al. (2007) to measure inequality aversion, using the strategy method to record preferences in a modified dictator game (MDG) and an ultimatum game (UG) described below. Utilizing both games allows for the identification of advantageous and disadvantageous inequality aver-

[8] As noted in Charness and Genicot (2009), a 50% success probability "avoids the problem of subjective over-weighting of low-probability events."

[9] A drawback of this method is that no concrete point-estimate can be derived if the participant chooses to invest 0 or 100. In such cases of relative "extreme" risk aversion or risk-loving, respectively, point-estimates were coded appropriately. Qualitative results proved robust to these coded characterizations of risk aversion.

[10] To avoid hypothetical risks and payoffs, I informed participants that three people would be randomly paid based on the result of this lottery. These payments would supplement their payoffs from the rest of the experiment. This procedure is similar to the payment method used in Charness and Genicot (2009).

sion. Differentiating the magnitude of relative inequality and risk aversion will be useful in order to identify the main drivers that both inhibit and support collaborative efforts in the supply of public goods (or avoidance of public bads). Three independent games (Risk, MDG, UG) are presented to subjects prior to introducing the more complicated threshold PGG.[11] In this fashion there is a natural progression of complexity in the sub-games which may help filter out errors in understanding and decision making. I employ the model developed in Fehr and Schmidt (1999) to parameterize aversion preferences in the two-player UG and MDG games. An advantage of this model is that aversion preferences elicited can be applied to the threshold PGG, assuming that aversion preferences in one game are a simple monotonic transformation of the thresholds in a separate game (Teyssier, 2012). Fehr and Schmidt's utility function is given by:

$$U_i(x_i, x_j) = x_i - \alpha_i \; max[x_j - x_i, 0] - \beta_i \; max[x_i - x_j, 0] \;\; for \; i \neq j, \tag{1.1}$$

where α measures disadvantageous inequality aversion and β measures advantageous inequality aversion. Assumptions in this model include $\alpha_i \geq \beta_i$ and $0 \leq \beta_i < 1$. The first of these conditions imparts the assumption that an individual suffers more disutility from being at a material disadvantage than at an advantage relative to her counterpart. The latter condition rules out the existence of individuals who take pleasure in being better off than others ($\beta_i < 0$), while $\beta_i < 1$ departs from the implausible event that an individual would give up a dollar or more to reduce their advantage relative to player j.[12]

[11] Controlling for possible order effects, I varied the sequence that these games were introduced. Impractical to include all six permutations of possible three-game configurations, I ran three of the possibilities (Risk-MDG-UG, MDG-UG-Risk, UG-Risk-MDG). Finding no detectable differences between these three sequences, all treatments were pooled for data analysis.

[12] Section 1.5 reports estimates for aversion parameters and tests the relevancy of Fehr and Schmidt's assumptions. Empirical evidence in the literature suggests a positive correlation between advantageous and disadvantageous inequity aversion, which will also be explored with the data set.

Deriving disadvantageous inequality aversion, α, participants are introduced to a two-stage ultimatum game whose focal point is the division of a pie worth 20 tokens between two individuals, a proposer and a responder. In the first stage the proposer offers an integer share of the pie, s, of which the responder accepts or rejects in the second stage. The outcome of this offer is $20 - s$ for the proposer and s for the responder, if the proposal is accepted, and zero for each participant otherwise. Unaware of their possible role assignment later in the experiment, all participants made choices in each of the two roles: (i) proposers chose an integer share $s \in \{0, 20\}$ to offer the responder and (ii) responders chose to accept or reject each of the twenty-one possible "Proposer-Responder" distributions of tokens (20-0, 19-1, ..., 0-20).[13] If this game is chosen for final payoffs at the end of the experiment, participants are randomly paired and randomly assigned one of the two roles, at which point the actual proposed offer and responder's decision to this offer are compared to determine payoffs. Applying the strategy method to capture the responder's contingent decision set, the minimum accepted offer yields information to calculate near point-estimates of α. Following Blanco et al. (2011), suppose that $s_i^{'}$ is the lowest offer that individual i hypothetically chooses to accept, thus $(s_i^{'} - 1)$ represents the highest offer they would reject. Assuming well-behaved preferences such that there exists a single point where the individual switches from rejecting a set of offers to accepting the rest, a responder will be indifferent between accepting an offer $s_i \in [s_i^{'} - 1, s_i^{'}]$ and receiving zero payoff from a rejection. Further assuming the proposer offers no more than half the pie, Fehr and Schmidt's utility function (1.1) yields: $U_i(s_i, 20 - s_i) = s_i - \alpha_i(20 - s_i - s_i) = 0 = U_i(0, 0)$.[14] Solving for α_i, the estimate for

[13] See Game 3 in the Appendix for a screen shot.

[14] Of the 216 participants, only 8 offered more than half of the pie. The estimation of α is not affected by these offers but instead impacts the point at which a switch from rejection to acceptance occurs.

disadvantageous inequality aversion is

$$\alpha_i = \frac{s_i}{20 - 2s_i}. \tag{1.2}$$

For the purposes of estimation, I set $s_i = s_i^{'} - \gamma$, where $\gamma = 0.5$.[15] Individuals who repeatedly rejected low proposed offers were characterized as increasingly disadvantageous inequality adverse and consequently had higher values of α. Exploring the implications of equation (1.2), the offer must be $s \in [0, 10]$ as I assume the proposer offers no more than half of the pie. Since rational responders accept an equal share offer then $s_i^{'} \leq 10$ and division by zero does not occur. Extreme values of α_i materialize if the responder never switches from their initial decision in the first set of pairwise choices. Individuals who reject every feasible offer less than half of the pie accept only if $s_i \geq 10$, allowing us to infer at most that $\alpha_i \geq 4.5$. I cautiously assign these individuals $\alpha_i = 4.5$ with no further information to make a better estimation of their preferences. On the other side of the spectrum, I never observe a switching point for individuals who accept every offer ($s_i^{'} = 0$) and assign them $\alpha_i = 0$. This characterization assumes the nonexistence of subjects who derive utility from being at a disadvantage relative to others.

Eliciting advantageous inequality aversion, β, the modified dictator game posits an initial endowment of 20 tokens for the dictator who decides how much of this total she is at most willing to sacrifice for an equitable distribution of payoffs between herself and the recipient. In this setting, participants make choices in the lone role of the dictator. A list of twenty-one pairwise payoff decisions are listed and the participant chose their preferred

[15] To estimate α_i, the choice of a parameter adjustment factor (γ) equal to 0.5 is indeed arbitrary to identify an offer, s_i, that is between the individual's maximum offer rejected and minimum offer accepted. If I assume that γ is normally distributed between the continuum of values in [0,1], then I can justify the choice for γ. A sensitivity analysis inserting $\gamma = 0.1, 0.3, 0.7, 0.9$ for robustness yielded no substantial qualitative differences in the regression analysis.

payoff distribution in each case. The left choice is always a distribution of (20,0) for the Dictator-Recipient payoff and the right choice contained equal payoffs ranging from (0,0) to (20,20).[16] If this game is chosen for final payoffs at the end of the experiment then: (i) participants are randomly paired and randomly assigned one of the two roles, (ii) one of the twenty-one pairwise payoff vectors is randomly chosen, and (iii) the decision of the dictator determines payoffs. Again employing the strategy method to measure aversion preferences, I identify the point at which the dictator switches from the (20,0) unequal distribution to the equitable payoff distribution. Per Blanco et al. (2011), if an individual switches from the unequal payoff vector of (20,0) to the egalitarian outcome at $(x_i^{'}, x_i^{'})$, then I can infer that they prefer the payoff (20,0) over $(x_i^{'} - 1, x_i^{'} - 1)$.[17] Together these two reference points relate the individual's threshold for sacrificing a (20,0) outcome in favor of an equitable one. Since payoffs are based on integer values, there must exist an egalitarian payoff vector, (x_i, x_i), that renders the individual indifferent between this outcome and (20,0). Using Fehr and Schmidt's utility function (1.1) again, it must be that $U_i(x_i, x_i) = x_i = 20 - 20\beta_i = U_i(20, 0)$ for some $x_i \in [x_i^{'} - 1, x_i^{'}]$ and $x_i^{'} \in \{1, 20\}$. Solving for β_i, the estimate for advantageous inequality aversion is

$$\beta_i = 1 - \frac{x_i}{20}. \tag{1.3}$$

Similar to the justification for α, I set $x_i = x_i^{'} - \gamma$, where $\gamma = 0.5$. In accordance with Fehr and Schmidt I assume $\beta \in [0, 1]$, however, the two endpoints warrant discussion.

[16] See Game 2 in the Appendix for a screen shot.

[17] Similar to the UG game, this characterization assumes individuals have well-behaved preferences such that they have a unique switching point from the payoff (20,0) to the egalitarian payoff. Experimentally I found that a number of participants routinely switched between these payoffs more than once. These participants may not have well-behaved preferences, possibly suffered from fatigue, or did not completely understand the logic of the game. For these participants, I calculated three values for their switching point (a minimum, average, and maximum) based on their decisions. The estimation of these individuals' aversion parameters is imperfect and I take care to explore possible implications later in the chapter by analyzing subject behavior with (i) the full pool of participants and (ii) restricting analysis to those individuals with well-behaved preferences.

Individuals who choose the equitable option for each of the 21 decisions (i.e. forgo a payoff of (20,0) for all choices) have a strict aversion to advantageous inequality and $U_i(0,0) > U_i(20,0)$, which implies that $\beta_i > 1$. No switching point is ever observed for these participants and it is possible that they are willing to sacrifice in excess of \$1 to reduce inequality by \$1. As in Blanco et al. (2011), I cautiously assign these participants $\beta_i = 1$. Other subjects for whom a switching point is unobserved include those that never deviate from the (20,0) choice. At the extreme this suggests $U_i(20,0) > U_i(20,20)$ and $\beta_i < 0$, thus they may be willing to sacrifice funds to increase inequality. These individuals are assigned $\beta_i = 0$ since I do not observe a switching point and cannot further divulge their unique preferences.

1.3.3 Threshold Public Goods Game

The threshold public goods game (TPGG) modifies the experimental setup developed by Milinski et al. (2008) and amended by Tavoni et al. (2011), randomly dividing participants into groups of four (constant for the game) whose aim is threshold attainment after 10 rounds of play. Conducted in an environment of complete information, subjects within each group were attributed a commonly known unique endowment and risk factor bundle that defined their initial standing in the TPGG. Though a player's endowment and risk profile were known to all group members, players did not know each other's identities and instead were assigned a Player ID Number (P1-P4) maintained for the duration of the game. Player ID Numbers and associated endowment/risk profiles were displayed constantly on the computer terminal throughout the game. Constant IDs allowed group members to identify each other exclusively by endowment, risk, and contribution profile, enabling the formation of reputations. Each player was endowed with 40 experimental tokens (ETs) at the start of the game.

Modeling endowment inequality within all treatments, I adopt the approach of Tavoni et al. (2011) and subject all group members to three inactive contribution rounds that force half of the subjects (2) to contribute 4 ETs per round to the collective fund while the other half (2) are forced to contribute nothing. "Rich" players are characterized by starting round 4 with 40 ETs in their individual account whereas "poor" players begin with 28 ETs. The target contribution threshold was set at 80 ETs and for the sake of comparability all treatments started the active phase (rounds 4-10) with 24 ETs in the collective fund. A threshold of 80 ETs constitutes 50 percent of the aggregate group endowment of 160 ETs, reflecting the Intergovernmental Panel on Climate Change's suggestion of achieving a 50% reduction in emissions relative to prior levels.[18] There are no framing effects in this experiment as I distinctly avoid using any verbiage related to the climate or climate change. Previous studies relate the group fund as a "climate account," potentially biasing individual behavior.

In four of the six treatments, homogeneous risk factors were distributed to all group members that imposed a collective $\frac{1}{3}$ or $\frac{2}{3}$ risk of losing private net endowments when failing to reach the target threshold by the game's conclusion. All other treatments were assigned heterogeneous risk factors amongst group members, whereby two players inherited a $\frac{1}{3}$ (and the other two a $\frac{2}{3}$) risk susceptibility of losing private net endowments when the threshold was not met. Varying risk factors characterize the different levels of dangerous climate change that individuals and nations may be vulnerable to, depending on geographical location, adaptive capacity, etc. Homogeneous risk groups maintain a single dimension of inequality in endowments, whereas heterogeneous risk groups have an equal

[18] Contributions to the climate fund and reductions in emissions are synonymous in this experiment.

Table 1.1: Threshold PGG Treatment Breakdown

	Treatments					
	T1	T2	T3	T4	T5	T6
Endowment Inequality*	X	X	X	X	X	X
Punishment		X		X		X
Homogeneous low ($\frac{1}{3}$) risk	X	X				
Homogeneous high ($\frac{2}{3}$) risk			X	X		
Heterogeneous risk**					X	X

*Two players from each group are "poorly" endowed; two players are "richly" endowed.
**Two players from each group have high-risk factors ($\frac{2}{3}$); two players have low-risk factors ($\frac{1}{3}$)
See Section 1.3.3 for more details.

distribution of rich/poor and low/high risk types.[19] In each active period of the TPGG (rounds 4-10), subjects were simultaneously asked for a 0, 2, or 4 ET investment from their private account to the group fund.[20][21] At the conclusion of each round, individual contribution levels to the group account were revealed by Player ID with risk/endowment type, as were total past contributions by individual, aggregate group contributions in the current round, and aggregate group contributions for all rounds up to the present.[22]

End game payoffs were calculated according to whether the collective 80 ET threshold

[19]The chosen distribution of risk/endowment types being equal within heterogenous groups (one individual of each risk/endowment profile per group) may have an impact on the results of the TPGG. This experiment attempts to model inherent heterogeneity in the extreme case of one profile type per group, but it could be argued that experimental results may be conditional on group type distributions. A simple uniform distribution of types within each group was chosen to model the heterogeneity in wealth and risk susceptibility, exhibited by the multitude of member types present during international climate agreements.

[20]Isaac et al. (1989) and Croson and Marks (2000) suggest avoiding the word "contribution" (framing effect) and instead phrase as "allotments" or "allocations" to the public fund.

[21]This subset of possible contributions from total endowments reflects the gradual process in cutting back emissions as opposed to a discontinuous bevy of emission reductions in a single round which is difficult to accomplish with known technology constraints. Further, integer contributions are imposed to help identify altruists, fair sharers, and free riders (Milinski et al., 2008).

[22]Tavoni et al. (2011) abstract away from revealing the aggregate group contribution for all rounds up to the present and instead allow players to calculate the total amount on their own. In a game with strategic uncertainty and complicated behavioral interactions, I remove the possibility of individual calculation errors to ensure that decision strategies are based on complete and accurate information, rather than potential mistakes in arithmetic.

was met or exceeded after 10 rounds, including punishment results if relevant. In the no-punishment treatment when the threshold was met after 10 rounds of contributions, a subject retained any private endowments not invested in the public good. If the threshold was not met, remaining private endowments were at risk of being lost with respect to the relative risk factor assigned in their treatment.

Incorporating a mechanism to induce cooperation, most experiments introduce an autonomous form of punishment. Within this experiment, autonomous punishment might be integrated where participants at the conclusion of the 10 round game simultaneously have a decision whether to buy punishment points to decrease the payoffs of fellow group members who deviate from their personal norm of expected contributions.[23] Following recent studies (Fehr and Gächter, 2000; Casari and Luini, 2009), at a private cost of one ET per punishment point purchased, an individual can decrease the earnings of another group member by a constant three ETs[24]. If an individual received multiple punishment points across group members, their reduction in earnings would reflect the cumulative amount of punishment points received. In the event that the target threshold is at a minimum achieved, each player's individual payoff would be:[25]

[23]Contrary to other punishment studies that allow sanctions after each round, I implement end-game punishment. In the complex climate change game, retributive actions will typically be taken in the event that a collective threshold is not met. It would not be practical to punish noncooperators after each round since in the real world the level of emission reductions undertaken is an imperfect measure that only can be quantified after a certain number of lagged periods, justifying end-of-game punishment.

[24]See discussion in Section 1.2 for the impact of different fine-to-fee effective punishment ratios.

[25]This setup assumes that potential punishment will happen before each individual's risk die is rolled, in the event that the target threshold is not met. This is the preferred approach (as opposed to imposing punishment after an individual's risk die is rolled), given the possibility that an agent wishing to punish another may find themselves unable to do so if the targeted agent has lost the game of chance (unfavorable risk outcome) and all remaining private endowments. In this scenario, because the punisher cannot punish the targeted individual, the outcome of the game would not capture one's desire and the magnitude by which they punish another, excluding this important behavioral choice from the results. Additionally, if one agent wanted to punish another agent and can only do so after a die has been rolled (with a favorable result for the risk taker), punishment levels may be more vindictive than the initial desired punishment allocation for game behavior, given that the proposed punishable agent not only escaped positive contributions during the TPGG but also avoided their inherent risk of losing remaining

$$\pi_i^A = max\{e_i - \sum_{t=4}^{10} c_{it} - \sum_{j \neq i} p_i^j - \sum_{j \neq i} p_j^i, 0\} \tag{1.4}$$

where:

$e_i \equiv$ net endowment after 3rd inactive round for person i for $i = 1, \ldots, 4$

$c_{it} \equiv$ contribution by person i in round t

$p_i^j \equiv$ amount of punishment points i buys to hurt j $p_i^j \in 0, 1, \ldots, 7$.[26]

If the threshold target is not met, then subject payoffs (remaining private endowment net punishment) are in jeopardy of being depleted according to the risk factors assigned in the treatment.

Instead of autonomous punishment, I introduce a consensual punishment mechanism (Casari and Luini, 2009), where simultaneous punishment requests are only carried out when two or more members from a group assign punishment points to a particular individual.[27] There only needs to be an agreement in the decision to punish, not the magnitude of punishment. When the reductions are carried out and the threshold has

private funds. One can easily imagine that the punisher may be left fuming at the result of the game and the punishee's favorable die roll, willing to make even more costly punishment decisions that is not exclusively based on participation in the TPGG.

[26] I cap punishment possibilities to detract away from highly endowed individuals having the capacity to punish more than poorly endowed individuals (Fehr and Gächter, 2002; Reuben and Riedl, 2011).

[27] A consensual punishment mechanism parallels governing bodies responsible for imposing sanctions. When punishments are enforced after agreed upon by a majority of members, these sanctions would be carried out by those who vote for them and thus incur the cost of sanctioning themselves. Outside of the environmental arena, examples abound where economic sanctions are imposed upon nations that deviate from the realm of acceptable behavior. A group of nations would vote on whether to impose sanctions and if successful the member nations voting in favor would be responsible for the sanctions imposed. Of course, unilateral sanctions are possible (along the lines of autonomous punishment), but I abstract away from this possibility in the experiment and focus on developing a mechanism that could be implemented along the global scale. (See Section 1.2.5 for more information.

been met, payoffs are:

$$\pi_i^c = max\{e_i - \sum_{t=4}^{10} c_{it} - \sum_{j \neq i} K(j) p_i^j - 3K(i) \sum_{j \neq i} p_j^i, 0\} \tag{1.5}$$

where:

$$K(i) = \begin{cases} 1 & \text{if } (\sum_j I_{i,j}) \geq 2 \\ 0 & \text{otherwise} \end{cases}$$

and $I_{i,j} = 1$ if agent j wants to punish agent i (ie, $p_j^i > 0$).

If a consensus to punish cannot be reached, punishment requests will bear no costs on the punisher and the non-punished individual will maintain their net private endowment that remains after any successful punishment requests of their own. In the event that total punishment reductions exceed the punished player's remaining funds, the punished player is left with a zero payoff and no rebate will be given to punishers for points allocated beyond the punished player's remaining funds.

If the threshold is not met, each agent additionally faces a treatment assigned probabilistic risk (r_i) of losing remaining net private payoffs, resulting in expected earnings of

$$E(\Pi_i^C) = r_i(0) + (1 - r_i)\pi_i^c = (1 - r_i)\pi_i^c.^{28} \tag{1.6}$$

[28]Contrasting Casari and Luini (2009), this setup does not prevent the formation of reputation during the game and allows delayed punishment.

1.3.4 Questionnaire

The questionnaire distributed at the conclusion of the four games collected socioeconomic information (gender, age, number of siblings, college major, political party affiliation, tuition source, etc.) and elicited beliefs regarding the responsibilities of the rich and poor to contribute toward a public good. In addition to fairness beliefs and trust, I also gauged the influence of risk and predetermined play on an individual's contribution choices. Subjects were asked to identify the main driver for their cooperative behavior in the threshold PGG (predetermined endowment inequality, cumulative group investments starting in round 4, monetary self-interest, fairness considerations, or achievement of the targeted threshold). Select summary statistics are presented in Figure 1.10.

1.4 Hypotheses

Collective action is difficult to sustain in multi-period public goods games. Though empirical evidence reveals non-zero contributions in contrast to the dominant strategy of free riding, cooperation in repeated play games tapers off rendering the socially efficient outcome unachievable. Acknowledging general findings from the literature regarding decreasing (stable) contributions in threshold PGGs without (with) punishment, heterogeneous asymmetries along the dimensions of endowment and risk further complicate cooperation.

Group Behavior, External Factors

Hypothesis 1a: *Across non- or pro-punishment treatments, increased homogeneous risk will increase group contributions while heterogenous risk will impede cooperation.*

Hypothesis 1b: *Punishment will lead to higher group contributions and greater threshold attainment, relative to their non-punishment counterparts.*

While high levels of homogeneous collective risk have induced cooperative behavior in experimental games (Milinski et al., 2008), heterogeneous risk factors coupled with endowment inequality is conjectured to hinder cooperative inclinations when punishment is not available. Along the single dimension of endowment inequality with homogeneous risks, Tavoni et al. (2011) showed that collective efforts to reach a target threshold are impaired if no coordination-inducing mechanism is introduced. As punishment has been shown to have a positive impact on group behavior, I predict it will increase cooperation among low-risk treatments and align interests in heterogeneous risk groups.

Individual Behavior, External Factors

Hypothesis 2a: *Poorly endowed individuals will give relatively less than richer group members, across like risk treatments. Conditional cooperation by "poor" players is anticipated if "rich" group members signal willingness to contribute in early rounds.*

Hypothesis 2b: *Identically endowed players with high-risk factors will systematically contribute more than group members with low-risk factors.*

Hypothesis 2c: *Punishment possibilities will serve as a coordination-inducing mechanism to further close the contribution gap between differently endowed participants.*

Although both endowment and risk inequality figure to impact individual contribu-

tions, I hypothesize that the former asymmetry will be the more salient factor influencing choices. Given the immediate specification delegating two individuals poorer in endowment than the remaining group members, this attribute figures to be the defining influence in early periods for individuals with high discount rates and who lack forward-looking tendencies. Large initial endowments and high risk factors will induce risk-averse behavior among "rich" participants who have a significant incentive to protect their private account, leading these individuals to contribute a relatively greater share of their endowment to threshold attainment. Poor subjects with lower initial endowments will conditionally cooperate if significant early contributions are made by those with higher endowments, signaling an intent to reduce distributional endowment inequality. In punishment treatments, available sanctions may further increase the gap in contributions among poor and richly endowed individuals given the mechanism's credible threat.

I anticipate that equally endowed individuals with high-risk factors will contribute relatively more than identically endowed low-risk individuals, given their greater susceptibility to economic ruin. It remains an empirical question whether lowly-endowed high-risk subjects contribute a greater relative share of their initial endowment compared to highly-endowed low-risk subjects.

A punishment mechanism will induce greater responsibility for rich subjects to close the inequality gap, as fairness concerns can only be solved with mirrored inequality in distributional contributions. Incorporating only punishment may improve upon simultaneous punishment and communication mechanisms by making threats credible and diluting the existence of cheap talk (Bochet et al., 2006). In heterogenous risk groups, a consensual punishment mechanism should increasingly help eliminate anti-group behavior relative to homogeneous treatments.

Individual Behavior, Internal Factors

Hypothesis 3: *High risk aversion will induce larger contributions to the group account across equally endowed individuals. High levels of inequality aversion motivate cooperation, but combined with endowment inequality will impact "rich" and "poor" players differently.*

Though I randomly assign endowment and risk factor inequality among participants, inequality and risk aversion will also impact decision-making. Risk aversion should induce individuals with high risk factors to contribute more to the public good. Disadvantageous inequality aversion should negatively impact contribution levels of poorly endowed subjects and to a greater degree among individuals with low risk factors. Examining implications from Fehr and Schmidt (1999), this experiment is able to test the validity that subjects with high levels of advantageous inequality aversion are more likely to cooperate in the public goods game.

Group Behavior, Internal Factors

Hypothesis 4: *Larger mean levels of risk or inequality aversion among group members will positively impact total contributions, but a greater variance between group members will inhibit threshold attainment.*

While individuals marginally impact the final outcome, group composition and collective intrinsic preferences can greatly influence cooperation. Assigned risk and endowment profiles are explicit barriers to cooperation, however, groups have a better chance of

overcoming these obstacles if they share similar risk and inequality aversion preferences. Larger group means for these intrinsic parameters should impact group contributions and threshold attainment similar to individual measures. Greater variation in these measures should negatively impact group cooperation.

1.5 Instrument Check

This section reports trends in the estimation of aversion instruments (see Sections 1.3.1 and 1.3.2 for experimental design), identifies suboptimal decisions among participants, and analyzes how these results compare to those of prior research.

1.5.1 Risk Aversion Estimation

Endowed with 100 tokens and permitted to invest any portion of this amount in a risky asset that returned 2.5 times the amount invested if successful, the average investment amount was about 46 tokens. The estimated risk aversion parameter ranged from 0 to 32.48, representing risk-loving preferences and relative "extreme" risk aversion.[29] The average 46 token investment is associated with a risk aversion parameter equal to 0.755.

[29]This estimation procedure produces a range of risk aversion estimates that is strictly contingent on the specified utility function. Qualitatively, a one-unit increase in the estimated risk aversion parameter does not yield interpretive value beyond being more risk averse relative to another individual. Similar risk elicitation experiments (see Holt and Laury (2002)) produce their own arbitrary range of estimated risk preferences that do not always correlate with the slew of alternate methods. Specifically, my chosen procedure to derive point-estimates for risk aversion does not correlate well with Holt's interval elicitation method, whose method I also used in the end-of-experiment questionnaire. A main reason for this deviation is that Holt's method restricts possible risk aversion into 11 distinct values while the Charness and Genicot (2009) point-estimate method produced 28 separate risk aversion values determined by the amount individuals invested (i.e., there were 28 distinct investment choices among the participants). Comparing the two methods, it is possible that no systematic relationship exists as an increase in the point-estimate risk aversion for Person i may not find itself in a higher Holt risk aversion category, but instead assigned to the same category given the non-comparable cutoff points for this method. When testing for correlation between the two methods, any possible association (positive, negative, none) may be anticipated for the above reasons. With this insight, I restrict my analysis to point-estimates for risk aversion and avoid interval estimates.

As noted earlier, the endpoints of potential investment do not allow for explicit point-estimation of the risk aversion parameter. Of the 216 participants, 26 chose to invest the full 100 token endowment and 1 chose to invest nothing. These extreme choices were coded in accordance with suggestions from Charness and Genicot (2009). Further, 71% of participants chose to invest 50 or fewer tokens.

1.5.2 Inequality Aversion Estimation

In the ultimatum game, used to estimate disadvantageous inequality aversion (α), proposers offered a share of the 20 token pie which the responder could accept or decline. The proposers' mean offer was 44% of the pie, in line with an average offer of 40% in Blanco et al. (2011). About 57% of the proposers offered the even (10,10) split, while eight individuals proposed offers greater than half of the pie.[30] A mere 3% of offers were consistent with the profit-maximizing subgame perfect equilibrium of offering nothing or 1 token. Employing the strategy method by presenting each of the 21 possible Proposer-Responder allocations, I sought to find each individual's switching point from rejection to accepting the proposed offer. The average switching point occurred in between allocations (14,6) and (13,7), with participants being amenable to offers that exceeded about 33% of the pie.[31] Roughly 76% of participants would accept an offer less than the egalitarian (10,10) payoff distribution.

In the modified dictator game to measure advantageous inequality aversion (β), players decided between a selfish (20,0) allocation that benefited the dictator and an increas-

[30] Offers from the ultimatum game have been used to derive advantageous inequality aversion (see Fehr and Schmidt (1999)), however, Blanco et al. (2011) points out certain restrictions and thus I defer to the MDG game to estimate this preference more aptly.

[31] Among the ultimatum and modified dictator games, non-unique switching points occurred in 12% of cases, less than the 15.3% observed in Blanco et al. (2011). Holt and Laury (2002) present a similar format to the UG and find that 18.9% of participants do not have well-behaved preferences. See Footnote 17 for handling these participants.

ing equitable payoff vector (x_i, x_i). The mean switching point occurred just after (9,9) and 31% of participants switched to the equal payoff at or before (9,9). The modal switching point was at (10,10), a level 31% of participants chose as their personal threshold between choosing the selfish payoff and the egalitarian outcome. Eleven (5%) individuals never deviate from the (20,0) option and thus do not exhibit any advantageous inequality aversion ($\beta_i = 0$). On the opposite side of the spectrum 7 (3%) participants chose the equitable outcome for each of the 21 decisions and are characterized as extremely averse to advantageous inequality ($\beta_i = 1$). A total of 7 (3%) participants switch to the equitable choice only when it is costless (i.e., at the (20,20) payoff). These results are comparable to the findings in similar dictator games. Blanco et al. (2011) find a mean switching point around (11,11). They report that 8% of players only switch to the egalitarian outcome when it is costless and 10% never switch from the (20,0) option, preferring this outcome over (20,20).

Across aversion estimates, there is wide berth of heterogeneity among subjects. Over 87% of subjects display aversion to both advantageous and disadvantageous inequality ($\beta > 0$ and $\alpha > 0$), while only 2% can be characterized as purely stoic ($\beta = 0$ and $\alpha = 0$). Exploring the independence between the parameters, advantageous and disadvantageous inequality aversion are not correlated (Spearman correlation test, p=0.533).[32] Figure 1.1 corroborates this test and exhibits a wide distribution among the subject population. Further, I find that 65 of 216 participants (about 30%) violate Fehr and Schmidt's assumption that $\alpha_i \geq \beta_i$. These individuals are represented by points to the left of the line "$\alpha = \beta$" in Figure 1.1. In addition to a lack of correlation between the two inequality aversion parameters, I also find no significant correlation between risk aversion and ad-

[32]This result violates the assumptions by Fehr and Schmidt (1999) that the two factors are positively correlated, but Teyssier (2012) and Blanco et al. (2011) have since exhibited the lack of correlation in empirical studies.

vantageous inequality aversion (Spearman test, p=0.224) or disadvantageous inequality aversion (Spearman test, p=0.182), consistent with prior empirical findings.

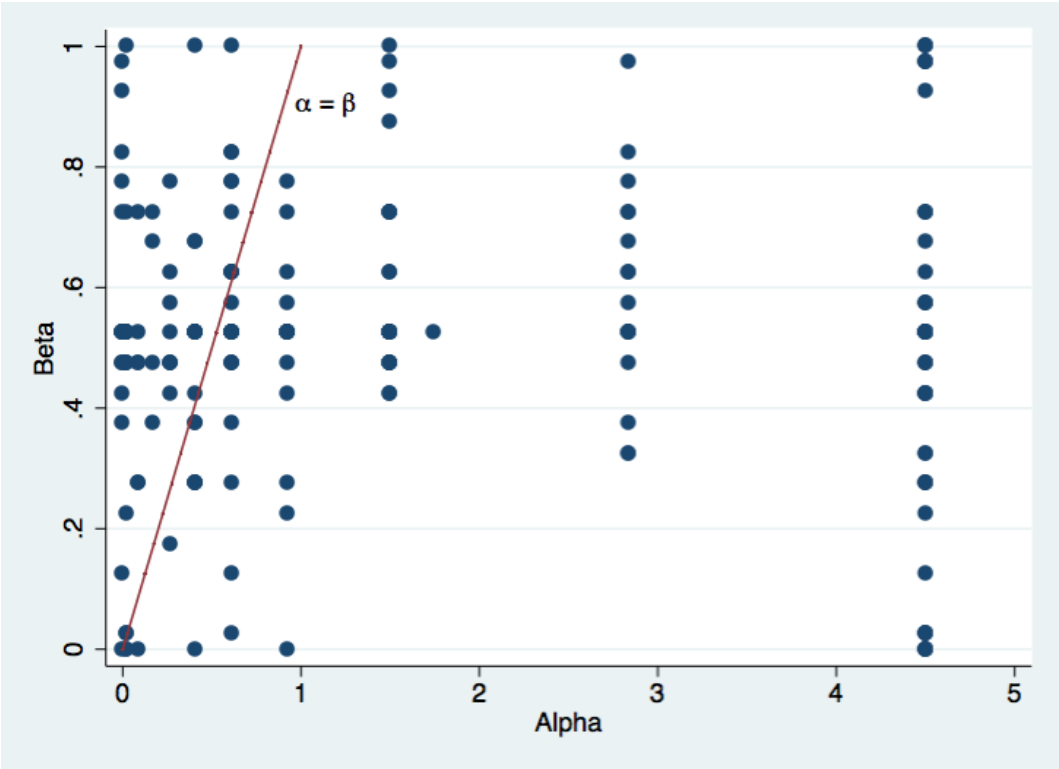

Figure 1.1: Joint (α, β) distribution. Each dot in the figure represents an individual's inequality aversion parameters.

1.6 Results

In this section, I first present aggregate summary statistics and trends from the threshold public goods game, then report observable differences in contribution levels by treatment, period, and player type. The following sub-section is devoted to statistical tests that establish the impact of external factors (risk, inequality, and punishment) on dynamic behavior. Lastly, I delve into the impact of intrinsic preferences on group cooperation and investigate my hypotheses.

1.6.1 Summary Statistics from TPGG

Individual Mean Contributions

Individual mean contribution trends in the aggregate setting of all treatments (Table 1.2) center around the symmetric equilibrium of 2 token contributions in rounds 4-8. Inducing inequality in rounds 1-3, recall that participants were subjected to three inactive contribution periods, forcing half of group members to give 4 tokens while the other half were forced to contribute nothing. By construction individual mean contributions in the three inactive periods was exactly 2 tokens and I refrain from posting summary statistics from predetermined play. Rounds 9 and 10 see a drop in contributions as group totals approach the established threshold. Refining analysis to treatments grouped by the existence of punishment (last two columns), larger individual mean contributions occur in punishment treatments up to and including round 7. In the later rounds (8-10) the trend between these two comparison groups is reversed as participants in the punishment treatment scale back contributions more quickly as they approach the threshold.

Investigating mean individual contributions by treatment reveals similar results. Treatments without punishment options (T1, T3, & T5) generally under contribute relative to their punishment counterparts in the initial active contribution rounds (4-7). Homogeneous high-risk groups (T3 & T4), however, contribute similar amounts irrespective of the existence of punishment. I return to this point following proper testing for statistical differences. Across non-punishment treatments, homogeneous low-risk individuals contribute relatively less than those in high-risk or heterogeneous groups. Between homogeneous high-risk (T3) and heterogeneous risk (T5) individuals, those in the former treatment gave larger amounts on average in the initial contribution rounds, potentially highlighting the absence of accountability among group members in the heterogeneous

Table 1.2: Mean **Individual** Contributions (Threshold PGG)

	Aggregate	by Treatment						by Punishment	
		T1	T2	T3	T4	T5	T6	No	Yes
Round_4	2.08	1.72	2.22	1.83	2.44	1.83	2.44	1.80	2.37
Round_5	2.11	1.67	2.11	2.39	2.28	1.89	2.33	1.98	2.24
Round_6	2.09	1.78	2.22	2.33	2.17	1.72	2.33	1.94	2.24
Round_7	2.10	1.56	2.11	2.28	2.28	2.17	2.22	2.00	2.20
Round_8	2.01	1.94	2.00	2.11	1.83	2.22	1.94	2.09	1.93
Round_9	1.73	1.56	2.06	1.83	1.94	1.89	1.11	1.76	1.70
Round_10	1.64	1.61	1.67	1.78	2.11	1.56	1.11	1.65	1.63
Rds_4_to_10	1.97	1.69	2.06	2.08	2.15	1.90	1.93	1.89	2.04
N	216	36	36	36	36	36	36	108	108

T1: Homo (1/3) Risk, Non-Punish T2: Homo (1/3) Risk, Punish
T3: Homo (2/3) Risk, Non-Punish T4: Homo (2/3) Risk, Punish
T5: Hetero Risk, Non-Punish T6: Hetero Risk, Punish

treatment. Across punishing treatments, contribution levels are consistently higher than the symmetric 2 token contribution equilibrium in early active rounds and slightly larger in groups containing high-risk individuals.

Examining coordination during early and late rounds, Figure 1.2 helps visualize the dynamics of individual behavior across treatments. Bar columns in this figure represent the average number of times group members contributed 0, 2, or 4 experimental tokens (ETs) during early and late rounds, by treatment.[33] For example, in Panel A the initial value of 4.3 represents the mean number of times individuals in Treatment 1 contributed 0 ETs in early rounds (5-7).[34] Among all treatments, free riding increases as the game

[33]Round 4 is excluded from this analysis for two reasons: (i) to compare "early" versus "late" round dynamics, each bin needed to contain an equal number of rounds, and (ii) decisions made in Round 4 more closely reflect inequality or risk aversion preferences as active group interaction has not yet occurred.

[34]Note that all bars in a panel sum to 24 which represents the total number of decisions made by four group members in rounds 5-10.

progresses which can be for one of two reasons: either (i) individuals give up their pursuit of an unattainable threshold and hold on to remaining net private endowments or (ii) contributions are scaled back as groups approach the collective threshold. In homogeneous low-risk groups without punishment (Panel A), relatively high levels of early free riding increase, overwhelming a modest increase in 4 token contributions in late periods. Over half of contributions in low-risk punishment groups (Panel B) were for the 2 token level, while low levels of 0 and 4 token contributions balanced each other out. Homogeneous high-risk treatments (Panels C & D) exhibit nearly identical early and late round contribution trends, enjoying relatively high levels of 2 and 4 token contributions. Coupled with low levels of free riding, these two treatments combined to reach the threshold 83% of the time. Lastly, the punishment mechanism had a visible impact on contributions over time in heterogeneous groups. Without the overt punishment threat (Panel E), a coordination failure occurs with both an increase in free riding and 4 token contributions in late rounds. Punishment possibilities induce cooperation and Treatment 6 (Panel F) enjoys the highest level of early round 4 token contributions. Large early contributions are complemented by increased free riding and decreased 4 token contributions in late periods as players scale back investments with a quickly approaching threshold.

Among all punishment treatments, free riding is mild in early rounds compared to non-punishment groups (p=0.078) as players coordinate investments toward the collective fund. Large contributions regularly decrease in late periods, but only after threshold attainment is nearly guaranteed. Relatively high levels of early free riding is observable across non-punishment treatments, and without the threat of retribution continues into late rounds as large contributions cannot compensate to avoid threshold non-attainment.

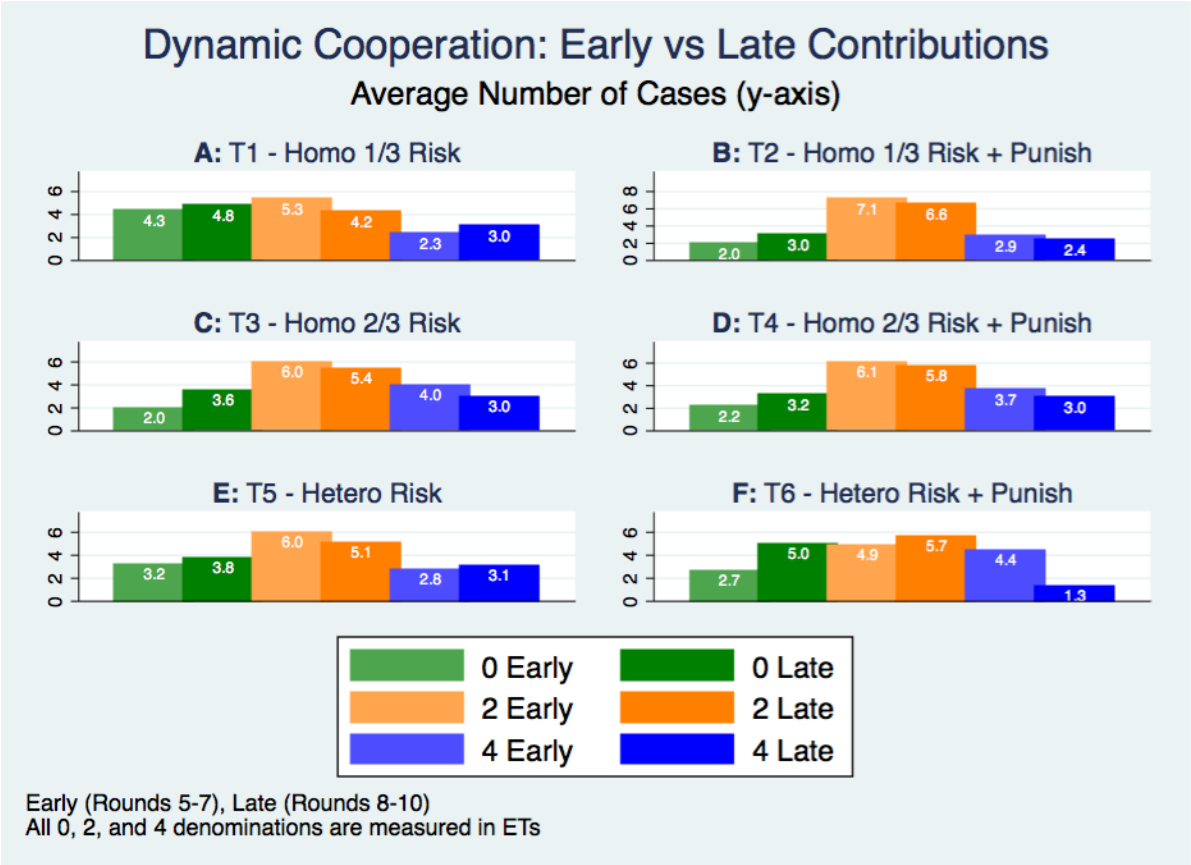

Figure 1.2: Round Dynamics

Table 1.3: Mean **Group** End-of-Round Contribution Totals (Threshold PGG)

	Aggregate	by Treatment						by Punishment	
		T1	T2	T3	T4	T5	T6	No	Yes
Round_4	32.33	30.89	32.89	31.33	33.78	31.33	33.78	31.19	33.48
Round_5	40.78	37.56	41.33	40.89	42.89	38.89	43.11	39.11	42.44
Round_6	49.15	44.67	50.22	50.22	51.56	45.78	52.44	46.89	51.41
Round_7	57.56	50.89	58.67	59.33	60.67	54.44	61.33	54.89	60.22
Round_8	65.59	58.67	66.67	67.78	68.00	63.33	69.11	63.26	67.93
Round_9	72.52	64.89	74.89	75.11	75.78	70.89	73.56	70.30	74.74
Round_10	79.07	71.33	81.56	82.22	84.22	77.11	78.00	76.89	81.26
N	54	9	9	9	9	9	9	27	27

T1: Homo (1/3) Risk, Non-Punish T2: Homo (1/3) Risk, Punish
T3: Homo (2/3) Risk, Non-Punish T4: Homo (2/3) Risk, Punish
T5: Hetero Risk, Non-Punish T6: Hetero Risk, Punish

End-of-Round Aggregates

Exploring end-of-round total contributions (Table 1.3), I evaluate how groups across treatments differ in their pursuit of the collective threshold and Section 1.6.2 presents statistical testing. Among all treatment groups, the mean total contribution after ten rounds of play was about 79 tokens, marginally missing the threshold on average. Distinguishing by threshold attainment, 43 of the 54 groups (80%) were successful in meeting the target (80 tokens) with an average group contribution of 83.2 tokens, while the 11 groups that failed collected an average of 62.9 tokens after ten rounds. Of the non-attainment groups, 7 of the 11 groups were from non-punishment treatments with an average contribution of 59.4 tokens, while the remaining four from punishment treatments averaged 69 tokens.

Breaking down the temporal momentum of total group contributions aiming for threshold attainment (Table 1.3), I find similar patterns to those observed among individual contributions. Non-punishment treatments systematically produce lower con-

tribution totals after every round relative to their punishment counterparts, both in the aggregate and in pairwise comparisons. At the end of ten rounds, punishment treatments on average attain the threshold (81.3 tokens) while non-punishment treatments fail (76.9 tokens). Although there is a sizable increase in group contributions after 10 rounds in punishment treatments relative to their non-punishment baselines, in reality, the former achieves the threshold 85% of the time with the latter not far off at 74% achievement. Whether or not a complex punishment mechanism is necessary to induce cooperation to reliable levels will be explored in the coming analysis.

In determining the impact of the punishment mechanism, I examine final contribution totals among the treatments with their punishment counterparts. In homogeneous low-risk treatments, punishment induces a sizable increase in total contributions to the group fund, pushing the average total (81.6 tokens) beyond the targeted threshold. Further, the number of groups failing to reach the threshold decreased by 67% with the inclusion of the punishment mechanism. When groups were characterized by only high-risk members (T3 & T4), both treatments on average achieve the threshold and punishment is potentially not necessary. Heterogeneous groups, with and without punishment, appear to be plagued by a coordination issue as these treatments on average do not attain the threshold. Delving deeper, however, both treatments bear one group with a very low contribution total, dragging down their respective mean contribution totals. Excluding these possible outliers, the mean contribution totals were 79.3 and 82.0 for the non- and pro-punishment treatments, exhibiting the positive impact of a coordination-inducing mechanism when agents are heterogeneous along two dimensions (risk & endowment). Similar to the homogeneous low-risk treatment, the inclusion of a punishment option decreased the number of groups failing to reach the threshold by 67 percent.

Player-Type Dynamics

Lastly, I examine individual contribution trends by player type (Figure 1.3). Amidst the mix of homogeneous, heterogeneous, and punishment groupings, there were four types of players that varied according to endowment and assigned risk factor: rich/low-risk (P1), rich/high-risk (P2), poor/low-risk (P3), and poor/high-risk (P4). Although I avoid the wording in the actual experiment, "rich" players are those who started round 4 with a 40 token net endowment (forced to contribute nothing during the three inactive rounds) and "poor" players are those who were forced to contribute 4 tokens per inactive round, starting round 4 with a 28 token net endowment. Observing active period (round>3) mean contributions in Figure 1.3, there is a propensity for rich individuals to not only contribute more than the 2 token symmetric equilibrium (p=0.000)[35], but also to contribute more on average than their poor-player counterparts regardless of punishment possibilities (p=0.000 for both low-risk and high-risk types). Such a result suggests that rich players sought to correct the endowment inequality induced at the start of the game, irrespective of the punishment threat. Further, high-risk players unequivocally give more than low-risk players on average (p=0.000). Disaggregating by punishment possibilities, the mechanism imparts greater contributions for most player types (P1-P3) while only player type P4 realizes a decrease in contributions when punishment is present. Testing for significance, low-risk player (P1 & P3) contributions increase (p=0.013 and p=0.078, respectively) with the punishment mechanism. Threatened by punishment, rich high-risk players (P2) have plenty to lose and contribute more (p=0.112) relative to non-punishment treatments, while contributions decrease for poor high-risk players (P4) (p=0.717). These results suggest that at the individual level punishment is effective as a coordination-inducing mechanism when risk is low, but when risk factors are high

[35] All p-values are two-tailed unless noted otherwise.

punishment does not significantly increase contributions to the target fund.

	All Treatments				No Punishment				Punishment			
	P1	P2	P3	P4	P1	P2	P3	P4	P1	P2	P3	P4
Contribution	2.30	2.50	1.35	1.72	2.13	2.42	1.26	1.75	2.47	2.58	1.44	1.69
N	378	378	378	378	189	189	189	189	189	189	189	189

P1: Rich/Low-Risk P2: Rich/High-Risk
P3: Poor/Low-Risk P4: Poor/High-Risk
Note: Summary statistics for active contribution periods only (Round > 3)

Figure 1.3: Mean **Individual** Contributions, by Player Type (Threshold PGG)

Though rich players contribute more on average across all treatments during active periods (Figure 1.3), the question persists as to whether threshold attainment is characterized by equalizing the contribution burden between rich and poor players within groups. Modeling endowment inequality by inducing predetermined play for the first three rounds, it is constructive to examine the share of total contributions from rich and poor players during the *entire* game. In addition to varying degrees of risk susceptibility, unequal endowments serve as a barrier to cooperation that can only be overcome with sufficient redistribution of contribution responsibilities to offset the induced inequality. Scaling up mean contributions for active periods in Figure 1.3 and including predetermined play contributions, I find that although rich players gave more in active periods, they did not fully close the inequality gap for equal burden sharing.[36] Figure 1.4 highlights this point for all treatments, where poor-player contributions make up 58% of the collective fund while rich players contributed 42% of the group total. Distinguishing by

[36]Relative to non-attainment groups, the questionnaire revealed individuals from successful groups increasingly preferred higher contributions from rich players in active rounds (p=0.085). Both attainment types agreed that poor players were not exclusively responsible for increasing their contributions relative to rich players (p=0.668). Further breaking down beliefs, within non-attainment groups rich players significantly disagree with poor players that it is their social responsibility to contribute more in active rounds (questionnaire response, p=0.051).

threshold attainment in Panel B, successful groups decrease the inequality gap by about 4 percentage points relative to failing groups (p=0.069). Panels C and D in Figure 1.4 break down rich and poor contribution shares with the introduction of the punishment mechanism. In both non- and pro-punishment treatments, successful threshold attainment is characterized by reducing the inequality gap beyond the all treatments case (panel A). Without a punishment mechanism (Panel C), successful groups close the inequality gap by over 6 percentage points (p=0.052) relative to non-successful groups. Both failing and successful groups in punishment treatments (Panel D) outperform all other specifications in the quest to equalize contribution shares, however, successful groups do not close the inequality gap any better than failing groups (p=0.592). Never quite achieving full burden sharing, the punishment mechanism successfully reduces the inequality gap which increases the likelihood of threshold attainment.

Signaling cooperation and a willingness to equalize burden sharing, immediate contributions in Round 4 play an important role in successful threshold attainment. Across all treatments (Figure 1.5) rich players, regardless of risk type, contribute over a full token more than poor players in Round 4 (p=0.000). Assessing how larger initial contributions by rich players translate into successful cooperation, I break down the data by non-attainment and attainment. Among rich players (P1 & P2), those in successful groups gave significantly more on average in Round 4 than those in failing groups (p=0.043). Among poor players (P3 & P4), there was no significant difference in contributions between those in successful or failing groups (p=0.313). Applying a simple probit model to ascertain the significance of Round 4 contributions among rich and poor players in reaching the threshold, I further substantiate the importance of rich player contributions (p=0.050) and the non-significance of poor player contributions (p=0.308). These results stress the importance of immediate signaling by rich players in Round 4 to

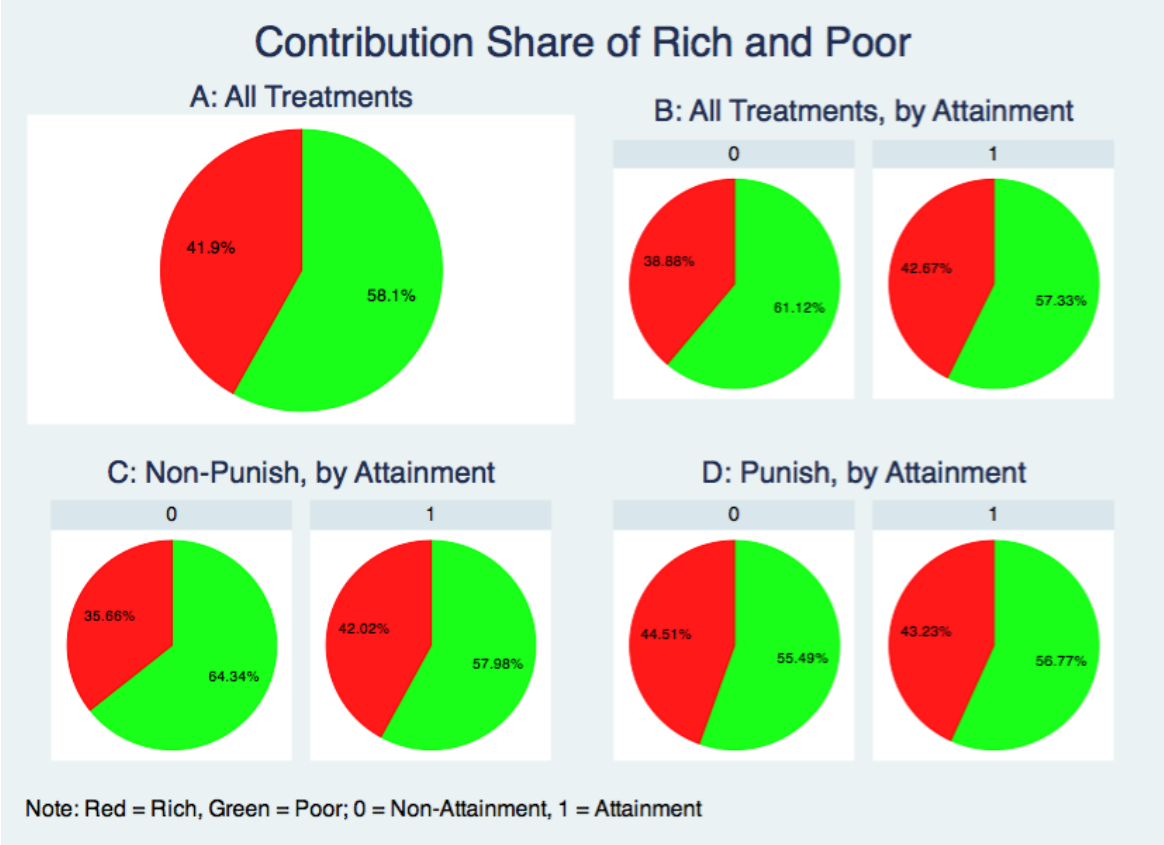

Figure 1.4: Contribution Shares

influence threshold attainment.

	All Treatments				Non-Attainment				Attainment			
	P1	P2	P3	P4	P1	P2	P3	P4	P1	P2	P3	P4
Contribution	2.74	2.70	1.30	1.59	1.50	3.00	0.67	1.80	3.10	2.64	1.48	1.55
N	54	54	54	54	12	10	12	10	42	44	42	44

P1: Rich/Low-Risk P2: Rich/High-Risk
P3: Poor/Low-Risk P4: Poor/High-Risk
Note: (Non)Attainment pertains to reaching threshold by Round 10

Figure 1.5: Round 4 - Mean **Individual** Contributions, by Player Type (Threshold PGG)

1.6.2 Statistical Tests of Group Contributions

On the surface there appears to be a detectable difference between mean contributions with and without a punishment mechanism. With only nine groups per treatment there is insufficient power when testing for differences between final end-game contribution totals. Since participants contribute personal endowments to a group fund for ten rounds and scale back investments as the total gradually reaches the predetermined threshold, end-game totals among all treatments hover above and below the threshold without significant variation in end-game totals.

Acknowledging the above issues, I test for statistical differences in contributions by round among the treatments. It is constructive to compare mean group-period contributions, by treatment, to establish a set of stylized facts consistent among the data. Mean group-period contributions relate average contributions by group, period, and treatment for all active group play (rounds 4-10). The values of this metric can be derived from Table 1.2, multiplying each number by four to account for the complete group. Figure 1.7 tests for differences among all combinations of treatments. The first reported statistic in each column relates two-sided p-values (unless noted otherwise) for statistical differences

when comparing low/high-risk homogeneous and heterogeneous groups to their punishment counterparts. Of these comparisons, only the low-risk homogeneous treatment is statistically different than its pro-punishment alternative (p=0.009) which is consistent with the findings reported in the prior section, comparing mean end-of-round contribution totals among treatments. Varying risk and group composition, I assess differences among treatments in the same punishment category. For non-punishment treatments, homogeneous low-risk groups statistically under contribute relative to high-risk and heterogenous groups.[37] High-risk homogeneous groups are not plagued by coordination issues and significantly contribute more on average per period relative to heterogeneous groups (one-sided, p=0.092). With regard to punishment treatments, varying risk and group composition, no statistical differences exist. This finding suggests that a consensual punishment mechanism may be the "great equalizer" that eliminates the differences in mean contributions among treatments with different group compositions. While perceived risk levels (be they uniformly high, low, or heterogeneous) to dangerous climate change are privately developed in the real world, experimentally I find that a punishment mechanism ensures almost universal threshold attainment regardless of the validity of personal risk beliefs.

Analyzing all active rounds (4-10) abstracts away from particular trademarks of threshold public goods games. Threshold attainment during group play is usually achieved in one of two general ways: (i) group contributions start strong and taper off as the threshold is approached or (ii) group contributions are humble in early periods and ramp up with the looming threat of non-attainment. While it is uncertain if a punishment threat

[37]Comparing non-punishment homogeneous low-risk groups (T1) with heterogeneous groups (T5), I employ a one-sided t-test. The null hypothesis is that though heterogeneity impedes cooperation, the existence of high-risk individuals in the heterogeneous group (T5) relative to uniformly low-risk individual groups (T1) increases the mean contribution level in the heterogeneous group. Similar logic is used with statistics reported alongside an asterisk (*).

among individuals is relevant during the entire game or instead gains traction in later periods, there exists a consistent downshift in mean group contributions (Figure 1.6) between early active rounds (4-8) and late active rounds (9-10).[38] Failure to partition contributions in this fashion conceals the impact of a punishment mechanism on early cooperation. Including all active periods in Figure 1.7, there was no statistical improvement in mean contributions among heterogeneous groups (T5 & T6) when including punishment. In Figure 1.8 I restrict analysis to "early" active round contributions to determine the early impact of punishment across treatments. All statistical differences, both in direction and significance, found in the prior section (see Figure 1.7) are identical, bar two. I find a significant difference in mean contributions between heterogeneous groups with (T6) and without (T5) punishment options. Looking at all active rounds (4-10) in Figure 1.6, Treatment 6 experiences a large decrease in mean contributions between these early and late rounds while Treatment 5 did not experience a dramatic downshift. The result of this sizable difference is that averaged over all active rounds (4-10), as reflected in Figure 1.7, there is no significant difference between T5 and T6 contributions, thus no detectable impact of punishment on heterogeneous group behavior. In Figure 1.8, however, I find that punishment indeed had the intended effect (p=0.057) in overcoming coordination issues among heterogeneous agents, although did not push average group contributions over the target threshold (Table 1.3). Lastly, when parsing out the strong early round mean contributions (9.02) in Treatment 6 from the significant decrease in the later rounds (4.44), I find that there is no statistical difference in mean contributions between punishment treatments with homogeneous high-risk (T4) and heterogeneous (T6) agents (p=0.726).

[38] "Early" and "late" active round designations are arbitrary. Results are qualitatively robust for other specifications: (i) early rounds (4-7), late rounds (8-10); (ii) early rounds (5-7), late rounds (8-10) as in Figure 1.2.

	Rounds 4-8						Rounds 9-10					
	T1	T2	T3	T4	T5	T6	T1	T2	T3	T4	T5	T6
Contribution	6.93	8.53	8.76	8.80	7.87	9.02	6.33	7.44	7.22	8.11	6.89	4.44
N	180	180	180	180	180	180	72	72	72	72	72	72

T1: Homo (1/3) Risk, Non-Punish T2: Homo (1/3) Risk, Punish
T3: Homo (2/3) Risk, Non-Punish T4: Homo (2/3) Risk, Punish
T5: Hetero Risk, Non-Punish T6: Hetero Risk, Punish

Figure 1.6: Mean **Group** Period Contributions, by Treatment (Threshold PGG)

Summarizing Section 1.6.1 and 1.6.2 results, I arrive at a handful of key points:

- Observation #1: Punishment is only effective when perceived risk is low enough among homogeneous or heterogeneous agents
- Observation #2: Coordination issues (without punishment) are overcome if perceived risk is high enough
- Observation #3: Punishment may be the "great equalizer" by eliminating total contribution differences between homogenous and heterogenous groups
- Observation #4: Individual accountability in heterogeneous groups is heightened with a punishment mechanism
- Observation #5: Rich players significantly reduce, but do not eliminate, the endowment inequality gap in successful groups
- Observation #6: Rich players who signal early cooperation significantly increase threshold attainment

Group-Period Mean Comparisons, Round>3 (p-values)

	T1
T2	0.009
T3	0.013
T5	0.083*

	T3
T4	0.634
T5	0.092*

	T5
T6	0.836

	T2
T4	0.473
T6	0.377

	T4
T6	0.072*

T1	Homo (1/3) Risk, Non-Punish	T2	Homo (1/3) Risk, Punish
T3	Homo (2/3) Risk, Non-Punish	T4	Homo (2/3) Risk, Punish
T5	Hetero Risk, Non-Punish	T6	Hetero Risk, Punish

Note: p-values are two-tailed t-tests measuring difference in Group-Period mean contributions, by Treatment
Test results denoted by (*) indicate one-tailed t-tests, with the following hypotheses:
(i) mean(T5)>mean(T1), (ii) mean(T3)>mean(T5), (iii) mean(T4)>mean(T6)

Figure 1.7: Round > 3

Group-Period Mean Comparisons, Rounds 4-8 (p-values)

	T1
T2	0.008
T3	0.008
T5	0.065*

	T3
T4	0.945
T5	0.075*

	T5
T6	0.057

	T2
T4	0.638
T6	0.401

	T4
T6	0.726

T1	Homo (1/3) Risk, Non-Punish	T2	Homo (1/3) Risk, Punish
T3	Homo (2/3) Risk, Non-Punish	T4	Homo (2/3) Risk, Punish
T5	Hetero Risk, Non-Punish	T6	Hetero Risk, Punish

Note: p-values are two-tailed t-tests measuring difference in Group-Period mean contributions, by Treatment
Test results denoted by (*) indicate one-tailed t-tests, with the following hypotheses:
(i) mean(T5)>mean(T1), (ii) mean(T3)>mean(T5)

Figure 1.8: Rounds 4-8

1.6.3 Regression Analysis

External factors including endowment inequality, risk susceptibility, and punishment have been shown to impact contributions and threshold attainment across treatments, however, I have neglected the role of intrinsic preferences that also influence behavior. Incorporating inequality and risk aversion measures from the three independent games prior to the threshold game, I investigate the behavioral preferences that both encourage and inhibit cooperation in the collective-risk public goods setting. With the same set of incentives and constraints, assessing the impact of intrinsic preferences helps identify why similar treatment groups fail to cooperate while others effectively reach the collective threshold.

Collecting repeated decisions from a fixed number of individuals and groups, the experimental data is best analyzed in a panel setting. Before incorporating intrinsic preferences, I first consider the impact of external factors (inequality, risk, and punishment) to corroborate findings from the previous sections. Table 1.4 presents a random-effects model for individual contributions to the group account during active periods (Round $>$ 3). Across all models, lagged group contributions significantly increase an individual's choice to contribute, signaling the typical preference for cooperation as the social norm materializes. Isolating the impact of lagged contributions from *other* group members (i.e., without player i), however, an individual's contribution significantly decreases showcasing a propensity to free-ride that may negate cooperative behavior even when considering the deterrents of punishment and collective risk. Increased risk susceptibility and lower relative endowments each have the predicted impact on behavior, significantly increasing and decreasing, respectively, an individual's contribution during active periods. While the magnitude of these opposing forces indicate that relatively low endowments have

a greater impact on contributions than do high risk factors, for individuals with both high risk and low endowments an interaction term reveals a non-significant combined effect. Lastly after controlling for risk and endowment factors, the punishment mechanism had the intended effect as it significantly increased contributions toward the public good, consistent with prior quantitative findings. Increasing risk susceptibility and relative wealth can significantly impact cooperative behavior, but on average these factors alone do not incentivize ubiquitous threshold attainment (see Section 1.6.1). With respect to the social planner, this is an important result as it highlights the necessity of a cooperation-inducing mechanism to efficiently attain the targeted threshold.

Modeling intrinsic preferences alongside external pressures in Table 1.5, I increase the dimensions by which the rationale of individual behavior can be identified. Excluding from the output external factors modeled in Table 1.4 (all of which maintain their signs and noted significances), varying levels of risk and inequality aversion have a marked impact on individual contributions. In a game shaped by contrived collective risk, risk averse tendencies might conceivably bring about increased contributions. In the Model 4 random-effects regression, however, elicited risk aversion preferences are found to negatively impact contributions, albeit at a small magnitude. Though risk averse agents may contribute more in the threshold game to avoid non-attainment, risk aversion has been found to reduce individual contributions in group games with strategic uncertainty (Teyssier, 2012). Not only are agents internalizing their inherited risk susceptibility, they face the risk of heterogeneous group members failing to cooperate. This two-pronged risk dilemma (with opposite effects) may explain the negative significance of elicited risk aversion.[39] Carrying greater weight in magnitude, both advantageous and disadvanta-

[39]These results are further explored when evaluating questionnaire responses later in the analysis.

Table 1.4: Individual Contributions to Group Account (External Factors)

	Model 1	Model 2	Model 3
Group Contrib, (t-1)	0.067***	0.154***	0.153***
	(0.02)	(0.04)	(0.04)
Dist2Thresh	0.009***	-0.000	0.002
	(0.00)	(0.01)	(0.01)
High Risk	0.381***	0.346***	0.269*
	(0.11)	(0.10)	(0.15)
Poor	-0.865***	-0.845***	-1.077***
	(0.11)	(0.10)	(0.34)
Group Contrib (others), (t-1)		-0.099***	-0.099***
		(0.03)	(0.03)
High Risk*Poor			0.464
			(0.60)
Punish			0.266*
			(0.15)
constant	1.487***	1.809**	1.573**
	(0.26)	(0.78)	(0.76)
Period Dummies	no	yes	yes
Treatment Dummies	yes	yes	yes
R2	0.324	0.476	0.474
N	1512	1512	1512
Wald	125.868	266.135	302.473

Note: Analysis for active contribution rounds (4-10) only

Robust standard errors in parentheses (clustered at group level)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

geous inequality aversion (representing guilt and envy) significantly increase individual contributions, with the former parameter nearly ten times as influential. This result stands in stark contrast to those found in Blanco et al. (2011), where Fehr and Schmidt's inequality-aversion model had little explanatory power in a one-shot simultaneous public goods game at the individual level.[40] In the threshold PGG with constant groups and multiple rounds, players dynamically incur bouts of envy and guilt during the course of the game, thus the saliency of their significant impacts. Punishment continues to increase contributions even when controlling for the wide range of internal and external factors influencing behavior.

Although aversion preferences are shown to influence behavior, anecdotal evidence suggests that these internal factors may impact unequally endowed individuals differently. Under the stress of strategic uncertainty during the multi-round game, risk aversion negatively impacts contributions. After interacting risk aversion with inherited low endowments, however, I find that this interaction is significantly negative while the conditional effect of risk aversion on rich agents is positive without significance (Model 5). Whereas external high-risk susceptibility coupled with poor endowments had no marked impact on contributions (Table 1.4), higher levels of risk aversion coupled with poor endowments increase distrust among participants leading to decreased cooperation. A possible explanation gleaned from the questionnaire is that the risk aversion measure, as priorly maintained, more closely models distrust between participants. Poorly endowed players exhibited lower levels of trust[41], anticipating richer group members to not de-

[40]Similarly in a one-shot sequential public goods game, Teyssier (2012) finds that disadvantageous inequality aversion does not impact the first mover, but advantageous inequality aversion does significantly increase the second mover's contributions.

[41]The final question on Game 4 in the questionnaire (Figure 1.10) highlights accountability beliefs and trust. This question asks players if they believe "richly" endowed players would contribute a higher share than poor players during active rounds. While rich types (P1 & P2) agreed with this sentiment, poor types (P3 & P4) on average "neither agree or disagree," suggesting lower levels of trust in rich

crease endowment inequality by giving a larger proportional share during active rounds. Highlighted in Figure 1.5, however, signaling from rich players in the first active round (Round 4) reverses this trend as poor players on average increased their contributions in response to the positive signal of cooperation. Although I hypothesized that higher levels of (dis)advantageous inequality aversion would significantly (decrease) increase individual contributions among the (poor) rich, there were no detectable effects. Game dynamics increasing or decreasing the inequality gap, rich and poor designations do not explicitly vary the overall impact of inequality aversion. Accounting for period and treatment dummies (Model 6) marginally increases model fit and maintains sign and significance for all regressors.

Outside of individual preferences marginally impacting game dynamics, group composition and collective intrinsic preferences can significantly influence cooperation. Across all pooled treatments (first column of Figure 1.9), prior round cooperation and the existence of punishment both significantly increase group contributions. Mean and standard deviation measures for aversion preferences reveal random group composition. Comparable to the individual analysis, higher levels of mean advantageous inequality aversion (eAIA) significantly increase group cooperation, while increasing levels of mean risk aversion continue to decrease levels of trust and negatively impact contributions. Increasing variation among the envy parameter (eDIA) and risk aversion, however, negate high levels of distrust and instead positively contribute to the collective fund.

Examining each risk treatment in turn (inclusive of punishment possibilities), a clearer story unfolds. Punishment in homogeneous low-risk groups continues to incentivize co-

agents to reduce the endowment inequality gap.

Table 1.5: Individual Contributions to Group Account (Internal Factors)

	Model 4	Model 5	Model 6
eRisk	-0.038***	0.106	0.103
	(0.01)	(0.09)	(0.10)
eAIA	0.594***	0.435*	0.428*
	(0.21)	(0.24)	(0.23)
eDIA	0.064**	0.024	0.022
	(0.03)	(0.04)	(0.05)
Punish	0.145*	0.125*	0.272*
	(0.08)	(0.07)	(0.16)
eRisk*Poor		-0.155*	-0.149*
		(0.09)	(0.08)
eAIA*Rich		0.419	0.433
		(0.38)	(0.48)
eDIA*Poor		0.068	0.072
		(0.05)	(0.05)
constant	0.877***	0.730**	0.802
	(0.29)	(0.33)	(0.72)
Period Dummies	no	no	yes
Treatment Dummies	no	no	yes
External Factors	yes	yes	yes
R2	0.487	0.496	0.501
N	1512	1512	1512
Wald	280.830	782.542	842.551

Note: Analysis for active contribution rounds (4-10) only
Robust standard errors in parentheses (clustered at group level)
eRisk = elicited risk aversion
eAIA = elicited advantageous inequality aversion
eDIA = elicited disadvantageous inequality aversion
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

operation (collecting 8 tokens more than the baseline), whereas the mechanism is not effective (or needed) in high-risk groups, consistent with individual level analyses. Increasing levels of risk aversion and its dispersion among group members have significantly opposite effects for low-risk treatments, while these factors maintain the sign of impact but lack significance among high-risk groups. High risk in homogeneous treatments appears to be the preeminent factor that influences cooperation both at the individual and group level. Although intrinsic preferences impact individual contributions, group variation among these parameters does not influence collective behavior as the fixation with high-risk susceptibility remains the ultimate free riding deterrent. Among group inequality aversion (IA) measures, mean advantageous IA significantly increases contributions in high-risk groups while mean disadvantageous IA imparts the same impact on low-risk groups. The questionnaire (Figure 1.10) reveals high-risk groups agreed that rich players should be responsible for contributing a greater share of their endowment, thus the significant positive influence of the mean guilt parameter. A similar effect is seen among the low-risk treatment, however, incremental guilt does not significantly impact group contributions when coupled with low stakes of economic loss.

	All Treatments	Homo 1/3 Risk	Homo 2/3 Risk	Hetero Risk	Pooled Homo
Group Contrib, (t-1)	0.321***	-0.048	0.322**	0.611***	0.169
	(0.11)	(0.20)	(0.13)	(0.12)	(0.11)
Punish	0.582***	1.196**	0.583	1.093***	0.776***
	(0.20)	(0.50)	(0.53)	(0.39)	(0.23)
mean eAIA	1.867**	0.033	3.303***	5.253**	0.904***
	(0.90)	(0.50)	(1.09)	(2.16)	(0.29)
mean eDIA	0.087	0.453***	-0.346	-1.011***	0.402**
	(0.27)	(0.15)	(1.67)	(0.14)	(0.18)
mean eRisk	-0.976*	-6.906***	-1.480	5.033***	-2.453***
	(0.52)	(2.14)	(1.16)	(1.52)	(0.93)
sd eAIA	1.207	-4.929*	-3.448	0.804	-2.499
	(1.54)	(2.82)	(6.39)	(2.78)	(2.58)
sd eDIA	0.276**	-0.030	0.733	1.966***	-0.225
	(0.14)	(0.23)	(2.15)	(0.56)	(0.23)
sd eRisk	0.547*	4.966**	0.767	-3.117***	1.396***
	(0.29)	(2.49)	(0.78)	(0.96)	(0.52)
High Risk					1.058***
					(0.17)
Period Dummies	yes	yes	yes	yes	yes
Treatment Dummies	yes	no	no	no	yes

Note: Analysis for active contribution rounds (4-10) only
Robust standard errors in parentheses
eRisk = elicited risk aversion
eAIA = elicited advantageous inequality aversion
eDIA = elicited disadvantageous inequality aversion
sd = standard deviation
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.9: Group Contributions per Period

		Player Type				
Select Questions	Response	P1	P2	P3	P4	All
"Those who began round 4 having invested 0 points (predetermined play forced this decision) should contribute more to the group account in the following 7 rounds than the other players."	Likert Scale*	2.9	2.6	2.3	2.1	2.5
What would you consider a fair average investment for the seven **active** rounds for players whose predetermined investment was 0?	0-4 tokens	2.8	2.7	2.7	2.8	2.8
What would you consider a fair average investment for the seven **active** rounds for players whose predetermined investment was 12?	0-4 tokens	1.7	1.8	1.5	1.8	1.7
"I was influenced by my own risk when deciding how much to contribute to the group account in Game 4."	Likert Scale*	3.6	2.6	3.7	2.8	3.2
"I was influenced by other players risk when deciding how much to contribute to the group account in Game 4."	Likert Scale*	4.3	4.2	3.8	4.0	4.1
"I was influenced by my own forced predetermined play when deciding how much to contribute to the group account in Game 4."	Likert Scale*	2.8	2.2	2.1	2.5	2.4
"I was influenced by other players forced predetermined play when deciding how much to contribute to the group account in Game 4."	Likert Scale*	3.4	2.9	3.0	2.8	3.0
"In Game 4 with new group members, players with a predetermined investment of 0 would contribute more to the group account than other players."	Likert Scale*	2.4	2.5	4.1	3.6	3.2
Age		20.4	20.3	20.7	20.7	20.5
Sex	Male=1, Female=2	1.5	1.6	1.6	1.6	1.6
Economics courses taken?		1.6	1.1	1.6	1.2	1.4
Frequency attending religious services?	Time Scale**	4.0	3.9	4.3	4.3	4.1
How do you identify yourself?	Party Scale***	3.7	3.4	3.8	3.8	3.7

***Likert Scale (1-7):** Strongly Agree, Agree, Somewhat Agree, Neither Agree nor Disagree, Somewhat Disagree, Disagree, Strongly Disagree

****Time Scale (1-5):** More than once a week, Once a week, Once or twice a month, A few times a year, Never

*****Party Scale (1-8):** Strong Democrat, Not so strong Democrat, Independent leaning Democrat, Independent Independent leaning Republican, Not so strong Republican, Strong Republican, None of the above

Player Types

P1: Rich/Low-Risk P2: Rich/High-Risk P3: Poor/Low-Risk P4: Poor/High-Risk

Figure 1.10: Questionnaire Summary (Mean Responses)

Among heterogeneous risk groups, nearly all group measures impact contributions. To better interpret the significance of these group factors, it is constructive to consider their effects relative to the *pooled* homogeneous treatment reported in the last column of Figure 1.9. Punishment incentivizes group contributions and adds almost 8 extra tokens in the final collective fund. Increasing levels of mean guilt spur heterogenous group members to contribute more when they are relatively better off than others, however, higher levels of mean envy significantly decrease their contributions in response to many dimensions of inequality. Noted in their relative magnitudes, heterogeneous group contributions are almost five times more influenced by collective guilt (eAIA) than envy (eDIA), qualitatively similar to the individual analyses. With two levels of variety (risk and endowment) in the heterogeneous risk groups, risk aversion no longer proxies as a measure of distrust and higher levels of the group parameter increase contributions. While there were two identically endowed pairs who all shared a common risk factor in homogeneous groups, players in heterogeneous groups were unique in their initial risk/endowment profile and so were not able to form a faux alliance with similarly assigned players, removing trust issues reported in the questionnaire. With four unique players in heterogeneous groups, mean risk aversion impacts contributions in the positive manner anticipated by theory. Among the dispersion parameters, each has an oppositely signed impact on group contributions compared to their influence among pooled homogeneous groups. Increasing disparity in the group envy (eDIA) parameter is beneficial to contributions as group members do not coordinate in pursuing the free riding option. Higher variation among risk aversion decreases group contributions as preferences to avoid risk are no longer aligned.

1.7 Conclusion

Integrating intrinsic preferences and external pressures in a public goods game, this chapter aims to understand the individual and group motivation that stimulate cooperative behavior in a collective-risk social dilemma. Politicians and global think tanks incorporate the influence of risk, uncertainty, and inequality when devising rational expectations and policy suggestions, however, social preferences have improved models of individual and group behavior. While the environmental literature has identified the external factors that impact public good provision, it has failed to control for and test the significance of behavioral preferences that both inhibit and induce cooperative norms. My experiment bridges this gap by constructing a multi-period threshold public goods game that assigns risk and endowment profiles, addresses aversion preferences, and introduces a punishment mechanism to disentangle the internal and external factors that influence cooperation.

Given the multiplicity of equilibria where group members collectively aim for threshold attainment, theoretical predictions of cooperation and behavior are difficult, thereby paving the way for an experiment. Consistent with prior empirical results, contributions in early rounds were optimistically high for all treatments, but cooperation significantly decreased over time in non-punishment groups. The evolution of cooperation was in line with expectations, as homogeneous groups coordinate more than their heterogeneous counterparts. Whereas high-risk groups stabilize near an efficient two token contribution per player-round, low-risk groups systematically coordinate toward a lower contribution average that induces higher rates of threshold non-attainment. This result suggests that without punishment, coordination issues may be overcome if perceived risk is high enough. The consensual punishment mechanism increased the likelihood of threshold at-

tainment for homogeneous low-risk and heterogeneous groups, but did not have a marked impact on high-risk groups that already had an incentive to cooperate. While perceived risk levels of economic loss are privately derived in the real world, punishment may serve as the "great equalizer" by eliminating total contribution differences between homogenous and heterogenous risk groups. Furthermore, punishment attenuated free riding in early rounds which increased threshold attainment.

Within groups inequality significantly impacted group behavior and final outcomes. Regardless of punishment possibilities, rich players contributed more than the two token symmetric equilibrium and more on average than their poor-player counterparts during active rounds. This result is consistent with participant expectations on social responsibility. Seeking to correct the endowment inequality induced by predetermined play, large early contributions by rich players signal cooperation and propel these groups to successfully reach the target threshold. Rich players contribute more than poor players during active rounds, but the endowment inequality gap is never eliminated even among successful groups. As a critical barrier to cooperation, the inequality gap for equal burden sharing was minimized after introducing the punishment mechanism, increasing the likelihood of threshold attainment.

Beyond the external pressures of risk and inequality, the theoretical impacts of intrinsic preferences are mostly supported by the data. As a player's relative wealth fluctuates with every round, both advantageous and disadvantageous inequality aversion increase individual contributions in a public goods game where players dynamically incur bouts of guilt and envy. Supported by the follow-up questionnaire, the impact of guilt was 10-20 times more influential than envy as players were more concerned about being at a material advantage than hurting the group account when relatively poor. Risk averse preferences

negatively impacted players in a game with both strategic uncertainty and the threat of economic ruin. Controlling for disparate responses between those unequally endowed, however, risk aversion coupled with poor endowments negatively impacts contributions while the conditional effect of risk aversion on rich agents is predictably positive without significance. Risk aversion proxies for distrust among the poorly endowed, while it maintains its traditional role among the richly endowed who fear economic ruin with higher risk factors. Group composition among collectively held intrinsic preferences further impacted cooperation. Higher mean values of advantageous inequality aversion significantly propelled group contributions among all treatments as higher levels of collective guilt aversion induced cooperation. Mean risk aversion continued to have a negative impact on contributions where strategic uncertainty and trust issues may have outweighed the collective aversion to possible economic ruin.

Experimental insights may be extended beyond the environmental realm to general areas of policy interest. Expected utility-maximizers often invest resources to protect themselves from the threat of economic loss, reducing exposure to risk with insurance policies and a diversification of loss-prevention tactics. National security and education are prime examples of collective-risk dilemmas where cooperation is costly to the individual and benefits may only be realized when a common target is reached. Whether the nation seeks a sustainable flow of military recruits or parents sacrifice time and resources to educate their children, a target level of costly cooperation must be achieved to supply a public good or avoid a common bad. Incorporating external pressures and intrinsic preferences in policy analysis may reveal behavioral motivations that can be exploited to achieve efficient public good provision.

While this chapter contributes to the environmental and behavioral literature, a bet-

ter characterization of the climate change game would allow for further insight and policy recommendations. Gradual impacts from a lack of investment to curb a public bad, in addition to discontinuous catastrophic events, should be modeled when investigating free riding tendencies and cooperation-inducing institutions. Environmental thresholds are also not known with certainty, which could further weaken collective action. Assigning individuals to groups in the experiment ignores the potential barriers to institution formation that could improve the understanding of international environmental agreements. Improvements aside, this study has identified the external factors, internal aversion profiles, and efficiency gains from a consensual punishment mechanism that influence behavior and induce threshold attainment in a collective-risk social dilemma.

1.8 Appendix

(Game instruction screenshots continued on next page)

Appendix

Welcome to the UCSB Experimental & Behavioral Economics Lab!

In today's experiment, participants will try to **earn as many tokens as possible** in **4 separate games.** Tokens earned will be converted to dollars at the end of the experiment **based on the conversion rate listed in each game.** Of the four games played today, one will be randomly chosen for payment by rolling a die in front of the group at the end of the experiment.

In addition to money earned from playing the games, you will be paid an additional $5 for participating today, regardless of any game results. Earnings will be kept confidential from other participants and will be paid in cash in a separate room at the end of the experiment. **Please try your best in all games!**

Game 1
Please Note: All games are independent and do not affect each other.
Tokens earned in this game are converted at the end of the experiment.

Please read the rules carefully. If you have a question, please raise your hand.

In this game you are given 100 tokens and asked to choose a portion of this amount (between 0 and 100 tokens, inclusive) that you wish to invest in a risky asset. **Those tokens not invested are yours to keep.**

The risky investment: There is a 50% chance that the investment in the risky asset will be successful. If it is successful, you receive 2.5 times the amount you chose to invest; if the investment is unsuccessful, you lose the amount invested.

How do we determine if the investment is successful? The roll of a 6-sided die determines the value of the risky asset. You will be asked to choose 3 "success" numbers.

If this game is chosen at the end of the experiment to determine your earnings, **you will roll a die to determine your payoff depending on the "success" numbers you chose.**

Please choose the number of tokens you wish to invest: _____

My 3 success numbers are: __ 1 __ 2 __ 3 __ 4 __ 5 __ 6

Game 2 - Instructions
Please Note: All games are independent and do not affect each other.
Tokens earned in this game are converted at the end of the experiment. (2 tokens = $1)

Please read the rules carefully. If you have a question, please raise your hand.

In this game Person A is asked to choose between two possible distributions of tokens for themselves and Person B in 21 different decisions. **Person B can only accept Person A's decisions.**

The roles of Person A and Person B will be randomly determined by computer software at the end of the experiment if this game is chosen for final payoffs. Roles will remain anonymous.

Decisions are presented in a chart and will look like the following:

Person A's Payoff	Person B's Payoff	Decision 1	Person A's Payoff	Person B's Payoff
20	0	Left Right	5	5

You make your decision in the role of Person A:
If in this decision you choose Left, you decide to keep 20 tokens for yourself and Person B's payoff will be 0 tokens. If you choose Right, you and Person B will each earn 5 tokens.

You will choose one distribution (Left or Right) for each of the 21 decisions when the game begins.

If this game is chosen to determine your earnings, the computer will randomly pair you with another participant, assign the roles, and choose one of the 21 decisions. The matching and role assignment remain anonymous. The outcome in the chosen decision determines your earnings.

Note that you will make all decisions as Person A, but the computer might assign you Person B's role if this game is chosen for payment.

If assigned the role of Person A, you will earn the amount that you have chosen for Person A in the decision selected by the computer and the person paired with you will earn the amount that you have chosen for Person B.

If you are assigned the role of Person B, you will earn the amount that Person A with whom you are paired has chosen for Person B in the decision selected by the computer.

(Game 2 Screen Shot)

Person A's Payoff (you)	Person B's Payoff	Decision	Person A's Payoff (you)	Person B's Payoff
20	0	Left ○ ○ Right	0	0
20	0	Left ○ ○ Right	1	1
20	0	Left ○ ○ Right	2	2
20	0	Left ○ ○ Right	3	3
20	0	Left ○ ○ Right	4	4
20	0	Left ○ ○ Right	5	5
20	0	Left ○ ○ Right	6	6
20	0	Left ○ ○ Right	7	7
20	0	Left ○ ○ Right	8	8
20	0	Left ○ ○ Right	9	9
20	0	Left ○ ○ Right	10	10
20	0	Left ○ ○ Right	11	11
20	0	Left ○ ○ Right	12	12
20	0	Left ○ ○ Right	13	13
20	0	Left ○ ○ Right	14	14
20	0	Left ○ ○ Right	15	15
20	0	Left ○ ○ Right	16	16
20	0	Left ○ ○ Right	17	17
20	0	Left ○ ○ Right	18	18
20	0	Left ○ ○ Right	19	19
20	0	Left ○ ○ Right	20	20

You are Person A.

Please choose Left or Right for each distribution option.

Continue

Game 3 - Instructions
Please Note: All games are independent and <u>do not</u> affect each other.
Tokens earned in this game are converted at the end of the experiment. (2 tokens = $1)

Please read the rules carefully. If you have a question, please raise your hand.

In this game Person A is asked to **choose 1 out of 21 possible distributions** of tokens for themselves and Person B. Person B knows that A has been asked to make this decision, and **may either accept or reject the distribution chosen by A.**

If Person B accepts A's proposal, this payoff choice is implemented. If B rejects the proposal, both receive nothing.

If this game is chosen to determine earnings, the computer will randomly pair you with another participant after your choices have been made and will assign the roles. Matching and role assignment remain anonymous.

You will make decisions as if you were Person A and also as if you were Person B.

If you are assigned the role of Person A, you will earn the payoff you chose for yourself if the Person B you are paired with accepts your offer. Otherwise, both will earn nothing.

If you are assigned the role of Person B, you will earn the payoff that Person A with whom you are paired has chosen for B **only if** you had accepted that particular offer. Otherwise, both will earn nothing.

(Game 3 Screen Shot – Person A's Choice)

	Person A's Payoff (you)	Person B's Payoff
Choice 1	20	0
Choice 2	19	1
Choice 3	18	2
Choice 4	17	3
Choice 5	16	4
Choice 6	15	5
Choice 7	14	6
Choice 8	13	7
Choice 9	12	8
Choice 10	11	9
Choice 11	10	10
Choice 12	9	11
Choice 13	8	12
Choice 14	7	13
Choice 15	6	14
Choice 16	5	15
Choice 17	4	16
Choice 18	3	17
Choice 19	2	18
Choice 20	1	19
Choice 21	0	20

Choice

Choice 1
Choice 2
Choice 3
Choice 4
Choice 5
Choice 6
Choice 7
Choice 8
Choice 9
Choice 10
Choice 11
Choice 12
Choice 13
Choice 14
Choice 15
Choice 16
Choice 17
Choice 18
Choice 19
Choice 20
Choice 21

Choosing as **Person A**, which option do you propose to Person B?

Please choose 1 of the 21 options.

Continue

(Game 3 Screen Shot – Person B's Choice)

Person A's Payoff	Person B's Payoff (you)	Decision
20	0	Accept Reject
19	1	Accept Reject
18	2	Accept Reject
17	3	Accept Reject
16	4	Accept Reject
15	5	Accept Reject
14	6	Accept Reject
13	7	Accept Reject
12	8	Accept Reject
11	9	Accept Reject
10	10	Accept Reject
9	11	Accept Reject
8	12	Accept Reject
7	13	Accept Reject
6	14	Accept Reject
5	15	Accept Reject
4	16	Accept Reject
3	17	Accept Reject
2	18	Accept Reject
1	19	Accept Reject
0	20	Accept Reject

Choosing as **Person B**, do you Accept or Reject Person A's proposed distribution?

Please Accept or Reject each distribution option.

Continue

Game 4 - Instructions
Please Note: All games are independent and <u>do not</u> affect each other.
Tokens earned in this game are converted at the end of the experiment. (2 tokens = $1)

Please read the rules of the game carefully. If you have a question, please raise your hand.

In this game participants are randomly split up into groups of 4. You will not know the identities of your group members other than their assigned Player ID Number on your screen. **Your group members and their Player ID Numbers <u>remain constant</u> for the duration of this game.**

This game takes exactly **10 rounds**. At the beginning of each round, each player receives an income of **4 tokens**.

In each round you can invest tokens (like all players) in the effort to **avoid damage** (described on next page). If damage is not avoided, it could lead to participants losing **all** their tokens at the end of this game.

In each round, all players are simultaneously asked: **"How many tokens would you like to invest in the prevention of damage?"** The possible choices are 0, 2, or 4 tokens.

After deciding how many tokens to invest, the investment of each player is displayed as well as the group total. All tokens invested in the round are credited to the group account for damage avoidance.

At the end of the game (ie after 10 rounds), the computer compares the total invested tokens of all group members with a predetermined amount of **80 tokens**.

At the end of 10 rounds this amount must be jointly reached to avoid possible damage to each player's individual account.

This total is achieved when each player in each round invests an average of 2 tokens, for a group total of **8 tokens** per round.

If the necessary total of **80** tokens is invested into the group account, the group avoided possible damage and each player will retain the points remaining in their individual account at the end of 10 rounds. That is, 40 tokens (you get 4 tokens in each of 10 total rounds) minus what the player has invested in damage avoidance.

If the necessary total of **80** tokens **is not** invested into the group account by the end of 10 rounds, the group **did not avoid damage** and each player will face their assigned **risk probability** of losing all remaining funds in their individual account for this game.

Players are assigned different risks of losing their remaining individual funds if the necessary token total **is not** met after 10 rounds. Some players face a **1/3 risk** (about 33% chance) of losing remaining individual funds, other players face a **2/3 risk** (about 66% chance) of losing remaining individual funds.

In this game you are Player # and your assigned risk is (varies by treatment).

Example:

The game in this example includes **4 players.** To avoid damage there must be a total of **80 tokens** invested in the group account by the end of 10 rounds.

Round 1: Alice receives an income of 4 tokens and invests 2 of them. All other players make the same decision. After the first round, the account to avoid damages has collected 8 tokens.

Round 2: Alice receives an income of 4 tokens and invests all 4 tokens. All other players invest 2 tokens each. After the second round, the account to avoid damages contains 18 tokens.

Rounds 3-10: Alice receives an income of 4 tokens and invests all 4 tokens. All other players invest 2 tokens each. After the tenth round, the account to avoid damages contains 98 tokens.

With a total of 98 tokens in the group account to avoid damage, more than the required 80 tokens, **damage is avoided.** Each player retains the remaining points in their individual account. Alice retains 2 tokens because she chose to keep only 2 tokens in the first round. Every other round she gave the full amount of tokens possible.

Suppose instead that Alice's group **did not** collect the necessary **80 tokens** after 10 rounds and that her individual risk is **1/3.** Alice still contributed the same amount as previously stated.

If this game is chosen for payment, she faces a **1/3** chance of losing her remaining 2 tokens and a **2/3** chance of keeping them. Other players face their own risk to determine their own payment.

A die will be rolled to determine each individual's outcome.
(End of Example)

Please note the following two important features in this game:

First, the computer predetermines the decisions of each player in the first 3 rounds (predetermined play). This means that you and your fellow players **cannot decide freely** how much you want to invest in the first three rounds. Instead, the computer forces you to choose a particular option in the first three rounds. Starting in round 4, you decide freely the amounts you want to invest.

(The following is ONLY for Punishment Treatments)
Second, after all 10 rounds are complete, we proceed to Stage 2 where you may choose to reduce the earnings of other group members who had the same opportunity to invest in avoiding damage.

You may reduce or leave equal the earnings of each of the other group members. Conversely, other group members can lower your earnings as well. Your decision is about distributing "**points**" to the other three players if you want to reduce their earnings. **Though identities are not revealed, you do know each player's history of investment to the group task of avoiding damage, individual risks, and predetermined investments the computer chose at the beginning.** This information will be presented on your screen.

If you want to reduce a player's earnings (for example Player X), you can request to distribute a number of **points** from 0 to 7 to Player X. If you do not want to change their earnings, you will choose 0 points for them. Each point Player X receives

reduces their earnings by **3 (THREE)** tokens. For the player who gave the point, each point given costs 1 token out of their own account.

Your overall cost is equal to the sum of the points that you have distributed to each one of the other three group members. Your maximum cost possible for distributed points is then 21 tokens (7 tokens times 3 persons). Your cost is zero if you do not distribute points to anybody.

As will be explained, a request to distribute points is not always carried out. For each player, there are two cases:

1. When **only you** have requested to distribute points to Player X, your decision has no effect. In particular, there is no reduction in their earnings and you are not assessed any cost for your request since it was not carried out.
2. When both you and at least one other group member have requested to distribute points to Player X, then your decision to distribute points is carried out. Requests made by others to distribute points to Player X will also be carried out. Thus, there **must be at least two requests** to distribute points to Player X in order for the request to be carried out. <u>It does not matter if requests made by multiple players are for different amounts.</u>

At the end of Stage 2 your total earnings will be:

= Round 1-10 remaining earnings – Stage 2 earnings reduction – cost to distribute points
= Round 1-10 remaining earnings – (total points received)x3 – (total points successfully distributed)

After all decisions are complete, we reveal points successfully distributed and cumulative earnings after the reduction.

Negative earnings are not possible. If a player has their earnings collectively reduced by more than they have remaining in their account, they simply earn zero tokens for this game.

After Stage 2 reductions, if **80 tokens** were not collected in Rounds 1-10, then we proceed to let you roll the dice to determine if damage hurts you based on your assigned risk. Otherwise, the game is over.

Control Questions

1. How many total tokens would **each player** have to invest into the group account, on average, to avoid possible damage after 10 rounds?
2. How many rounds can players freely decide their investments into the group account?
3. If after the first 6 rounds your group collected 28 tokens, how many additional tokens would your group need to collect to avoid possible damage after 10 rounds?
4. Assume your assigned risk is 1/3 (about 33% chance) and your group does not collect 80 tokens after 10 rounds. This means that you _____ (1=keep, 2=lose) your remaining funds with a 1/3 chance.
5. (Punishment treatment) What is the maximum number of points you can assign to any ONE player in Stage 2?
6. (Punishment treatment) If you give 2 points to Player X and Player Y gives 1 point to Player X, how many tokens does it cost you?
7. (Punishment treatment) If you give 2 points to Player X and no one else gives points to Player X, how many tokens does it cost you?
8. (Punishment treatment) In Stage 2, if Player X gives you 2 points and Player Y gives you 1 point, how many tokens are taken from your account?

Chapter 2

An Economic Analysis of Radical Environmentalism and Counterterrorism Policy

Abstract: The War on Terror has motivated military and legal efforts to prevent not only transnational terrorist incidents, but domestic terrorism as well. Operating on the fringes of the mainstream movement, the FBI identifies radical environmental direct action (REDA) groups as the number one domestic terrorist threat with over $110 million in damages between 1995 and 2005. While passive legislative interventions increase the cost of illegal action and proactive policies thwart terrorism with preemptive strikes, the efficacy of counterterrorism efforts has been questioned. Using quarterly data from 1980 to 2014, I analyze the effect of counterterrorism policy on REDA modes of attack and the severity of illegal actions. Combining vector autoregression and intervention analysis under a rational choice framework, I find that while legislative policies have decreased the economic severity of attacks, incidents have more than doubled. Proactive interventions reduce domestic terrorism, but by a smaller magnitude than the increase from passive

legislation. Substituting between modes of attack and ideological targets, policies have tripled the use of explosives while REDA attacks against people have increased more than sixfold in the long run.

2.1 Introduction

Since the early 1970s, the modern environmental movement has fought to conserve natural resources, protect wildlife, and mitigate the ill-effects of pollution on human health. Whereas mainstream groups target public opinion and introduce legislation to enact change, radical factions utilize illegal direct action to obstruct environmentally destructive practices. Activists tree-spike old growth forests in the Pacific Northwest to deter loggers from harvesting trees and physically engage whaling ships in the Antarctic to prevent the culling of whales for scientific research. Accruing over $110 million in damages between 1995 and 2005, the FBI has labeled radical environmental direct action (REDA) groups as the number one domestic terrorist threat (Inhofe, 2005).[1]

While passive legislative interventions increase the cost of illegal action and proactive policies thwart terrorism with preemptive strikes, the efficacy of counterterrorism efforts has been questioned. Studying U.S. aircraft hijackings from 1960-1976, Landes (1978) found that regulations to improve pre-boarding screening increased the probability of apprehension and decreased the number of skyjackings. Cauley and Im (1988) report that embassy fortification does not significantly prevent diplomatic attacks and that only the addition of metal detectors have had an impact in the long run. Enders and Sandler (1993) found that the installation of metal detectors reduced skyjackings and diplomatic

[1]REDA incidents are comprised of radical actions from both environmental and animal rights ideologies. Environmental advocates combat urban sprawl and point source pollution, while animal rights groups tackle animal exploitation, experimentation, and use in the entertainment industry.

incidents, but terrorists substituted toward hostage attacks and assassinations.

Relative to the extensive literature on transnational terrorism, few studies have probed domestic terrorism. Beck (2007) studied the diffusion of radicalism in social movements, sources of militancy, and the terrorist lifecycle. Webb (2010) explored geospatial trends and new methods of attack among radical groups in the United States. Assessing the effective elements of counterterrorism policy, Deshpande and Ernst (2012) qualitatively evaluated a federal operation to apprehend a radical environmental group responsible for over $40 million in damages from 1996-2001. Carson (2010) found that legal sanctions deter radical environmental incidents, but the direction of impact was contradictory depending on the model employed. Prior studies have not investigated the direct and indirect impact from multiple interventions on substitutable modes of terrorism and the increasing severity of illegal direct action.

Using quarterly data from 1980 to 2014, I analyze the effect of counterterrorism policy on who attacks (ideology), where they attack (target type), how they attack (weapon type), and the severity of illegal actions as measured by economic damage and targeting people versus property.[2] A second objective is to distinguish immediate impacts from long-run intervention effects after controlling for interrelationships between terrorism tactics. Given that governments allocate resources to thwart terrorism and activists optimally choose modes of attack, a rational choice framework is employed to hypothesize behavioral responses to counterterrorism policy and supply a set of testable predictions. As substitution between targets, weapons, and ideologies of attack may bias a univariate approach to modeling terrorism, I combine vector autoregression and intervention

[2]Prior studies have also included lawful protests in their analyses, but given that interventions are meant to reduce illegal actions, I exclusively focus on these incident types.

analysis to efficiently model interdependencies between multiple time series (Enders and Sandler, 1993). Both passive and proactive interventions are included to identify their disparate impact on the severity and magnitude of REDA attacks. Each of the four passive legislative policies (ADA, AEPA1, AEPA2, AETA; see Section 2.3) enacted since the 1980s have increased restitution and imprisonment penalties, while Operation Backfire in 2004 infiltrated an activist network to prevent illegal direct action.

Evaluating the impact of counterterrorism interventions on REDA attacks, I find that passive policies enacted to deter illegal activity have instead increased direct action in the name of animal rights. As interventions increased the cost of animal rights attacks, activists substituted resources toward relatively cheaper environmental direct action not formally covered by animal enterprise legislation. Operation Backfire's proactive approach significantly reduced illegal action across both ideological movements, albeit to a lesser degree than the relative increase from passive policy interventions. AETA legislation induced a significant long run increase in actions against private business and the education sector, targeting university labs and researchers engaged in animal experimentation and genetically engineered crop research. While minor sabotage remained the attack mode of choice, passive policies almost tripled the use of explosive and incendiary devices. Speaking to the severity of incidents, legislation that intensified penalties and broadened the scope for conviction incentivized both animal and environmental rights movements to substitute to low-damage attacks to minimize detection. Passive interventions increased attacks against people over sixfold in the long run as REDA activists engaged in tertiary targeting to attack individuals affiliated with companies and universities that opposed their ideological agenda.

2.2 Literature Review

Economic analysis on domestic and international terrorism has gained prominence since the events of September 11, 2001 (hereafter 9/11). The FBI and US Department of Defense define terrorism as "the premeditated use or threat of use of unlawful violence to obtain a political, religious, or ideological objective through intimidation and coercion of the larger population." Schneider et al. (2009) contend that this definition has its shortcomings, suggesting that an overreaching view of terrorism obscures from the heterogeneity of terrorism, behavioral tendencies, and its impact on intended targets.[3][4] General goals of terrorist activities include (i) gaining publicity and media attention, (ii) damaging industry and national economies, (iii) governmental destabilization, and (iv) the redistribution of power, influence, and wealth (Schneider et al., 2009). Tackling these goals, terrorists are assumed to be rational actors who maximize expected utility while considering incentives, costs, and the uncertainty of mission success.

Characterizing individual and national factors that induce terrorist activities, the economic, political, and sociology literatures have developed several theories as to the root causes of terrorism.[5] Media outlets typically frame political instability and destitute economic conditions as determinant factors that influence individuals to engage in illegal activities. Controlling for the level of political freedom, however, Abadie (2005) reveals an empirical regularity that risks of terrorism are not significantly higher in poorer countries (also see Kurrild-Klitgaard et al. (2006)). Surveying the Middle East over three

[3]While terrorism has traditionally been reserved for incidents involving the use or threat of use of violence against a given population, the term's applicability has broadened its scope in the past two decades to include acts of violence toward inanimate objects with the intention of altering the owner's actions and practices.

[4]See Vanderheiden (2005) for a theoretical debate, framed around just war theory, on the conventional limits and merits of adhering the stamp of terrorism to particular transgressions.

[5]This discussion draws partly on Schneider et al. (2009).

decades, Piazza (2007) further contends that it is not poor economic conditions that induce terrorism, but heightened levels of democracy that attract terrorism. Anecdotally thought to combat the spread of terrorism, advanced education and higher incomes have instead been correlated with its rapid proliferation. Empirical evidence suggests that awareness in the deficiencies of politics and social equity incite the willingness of individuals to participate in illegal activity (Krueger and Maleckova, 2003; Krueger, 2008).

Analogous to the hypothesized root causes that apply to the broad spectrum of terrorism, "ecoterrorism" is not driven by governmental instability or dire economic circumstances, but rather from the reverse. REDA actions frequently occur in developed countries with strong institutions that protect corporate enterprise and punish transgressors. Although this chapter does not identify the root causes for REDA incidents, counterterrorism efforts may have unintended consequences on activists who fight for animal and environmental rights.

2.2.1 Economic Impacts of Terrorism

Identifying the economic consequences of terrorism, empirical efforts have endeavored to measure its direct and indirect impact on economic agents. While terrorism directly impacts the intended target (consumers, businesses, or the government) by destroying human and physical capital, an interdependent economy causes social and economic disruptions for non-targeted entities (Schneider et al., 2009). Literature on the direct impact of terrorism has considered its effect on GDP, money markets, foreign investment, tourism, and loss of life (Enders and Sandler, 1991; Abadie and Gardeazabal, 2003; Eckstein and Tsiddon, 2004; Crain and Crain, 2006; Frey et al., 2007; Enders and Olson, 2012). A stable supply of terrorist activity may also negatively impact industry

operations by increasing credit and insurance risks (Schneider et al., 2009). Stock market performance can further approximate economic damages from terrorism, as its variability includes direct losses and expected future damages to company profits.[6] While transnational terrorism impacts have been widely approximated, the events of 9/11 generated a substantial number of reports on its economic impact (Navarro and Spencer, 2001). Accounting for over 3,000 lives lost from the attacks on the World Trade Centers and Pentagon, estimates of the destruction range between US$ 25 and 60 billion. An outlier in the terrorism impact literature, the economic costs of the 9/11 terrorist attacks are orders of magnitude greater than other terrorist events.

Investigating the economic impact from radical environmental direct action, congressional testimony revealed an excess of US$ 110 million in damages between 1995 and 2005 (Inhofe, 2005).[7] A report by the Foundation for Biomedical Research (2006) contends that attacks are increasing in both magnitude and severity, as violence has moved beyond quantitative damages to the qualitative impact of driving researchers from their professions and the societal costs of not finding cures to studied diseases. Terrorist attacks in general have been found to become more violent over time (Enders and Sandler, 2000; Schneider et al., 2009). Evaluating the radicalization of domestic terrorism, Beck (2007) finds that this phenomenon is not characteristic for ecoterrorist incidents in the United States. Beck's analysis, however, is constrained by its definition of radicalization (whether or not a "spree" of attacks from imputed ecoterrorist cells escalated in tactics, targets, and frequency) as well as the modeled time limit for groups to be identified as

[6]Insurance premiums may reflect costs from terrorist activity and the insurer's assessment of repeated attack probability. Assessing economic damages in private companies, however, is difficult with an incentive to guard profit forecasts and insurance risk premiums. Disclosing attacks often incentivizes additional terrorist activity and increases long-run aggregate damage.

[7]While a significant total, data limitations suggest an underestimation of damages. See Data for further insight.

radicalized. Further, these results may fail to detect long term changes in attack severity by confining the analysis to a subset of historical REDA incidents (1998 to 2005). If the economic impact of terrorism generally increases in severity overtime, or if there exists substitution between targets and attack methods, counterterrorism policy may effectively curtail illegal actions.

2.2.2 Impacts of Counterterrorism Interventions

Tipping the cost-benefit calculus of terrorist activity toward deterrence, the efficacy of counterterrorism policies has been questioned. Counterterrorism efforts can be separated into passive and proactive policies. Passive or defensive methods increase the cost of an attack by making it more difficult to access targets with technology-based barriers (e.g. metal detectors, reinforced infrastructure) or by instituting stricter laws and penalties. Proactive policies utilize intelligence-led investigations to prevent terrorist acts with group infiltration and preemptive strikes. Investigating the impact of improved pre-boarding screening at airports, Landes (1978) found that metal detectors and increased surveillance decreased the number of hijackings from 1960-1976. Cauley and Im (1988) report that embassy fortification does not significantly prevent diplomatic attacks and that only the addition of metal detectors have had an impact in the long run. Enders and Sandler (1993) found that the installation of metal detectors reduced skyjackings and diplomatic incidents, but terrorists substituted toward relatively cheaper hostage attacks and assassinations. Studying the Spanish ETA separatist movement, Barros (2003) found that military and police deterrence were generally ineffective in curbing terrorism, while political influence from the regional Socialist party increased the number of assassinations. Drakos and Giannakopoulos (2009) investigated transnational counterterrorism methods, finding that the probability of thwarting a terrorist incident has increased over

time. The authors note, however, that the increased propensity in preventing terrorist actions has had a greater impact on preventing property loss than reducing casualties.

Few quantitative studies have assessed the effectiveness of counterterrorism measures on environmental terrorism (Liddick, 2006). Under a rational choice framework, Carson (2010) analyzes the impact of legal sanctions and morality on the illegal actions of radical environmental and animal rights groups. Employing an interrupted time-series model and a series hazard analysis, Carson found that passive legislation was influential, but the direction of impact varied depending on the model used. Proactive methods have also been analyzed in an effort to curb radical environmentalism. Deshpande and Ernst (2012) qualitatively depict the efforts of an American inter-agency investigation, Operation Backfire, that sought to cripple the actions of a radical group who were responsible for over US$ 40 million in damages in the Pacific Northwest (US) from 1996-2001 (see Counterterrorism Interventions). Operation Backfire successfully infiltrated, apprehended, and sentenced individuals in the network, preventing future illegal actions. Preemptive tactics not only impact a targeted group, but may indirectly influence the behavior of other movement factions.

2.3 Counterterrorism Interventions

Both passive and proactive policies are evaluated in this analysis. Passive policies include state and federal legislation enacted to increase the cost of illegal action. Proactive policies thwart terrorism with preemptive strikes and group infiltration. The following interventions were implemented at the federal level during the target period of study, 1980-2014.

Anti-Drug Abuse Act of 1988

The Anti-Drug Abuse (ADA) Act of 1988 sought to prevent the manufacturing, distribution, and use of illegal drugs by strengthening federal drug control oversight and increasing minimum penalties for drug traffickers and users.[8] While the legislation intended to fight the war on drugs and not curtail illegal environmental actions, congressional testimony by Senator James McClure addressed a provision criminalizing dangerous booby traps used on public lands that could harm humans (Smith, 2008). Citing a tree-spiking incident that injured a logger, Senator McClure proposed broadening the Act's oversight to include hazardous devices that could harm law enforcement and loggers alike. The amended Act penalized transgressions meant to obstruct or harass the harvesting of timber, imprisoning offenders between one and 20 years depending on the frequency and magnitude of the incident. In an effort to minimize escalating direct action, the ADA Act set a precedent to criminalize illegal activism in the name of animals and the environment.

Animal Enterprise Protection Act of 1992

In August 1992 Congress passed the Animal Enterprise Protection Act (hereafter, AEPA1) that coined the crime of "animal enterprise terrorism" and criminalized the unlawful sabotage of commerce or academic research involving animals.[9] The Act targeted anyone who "intentionally causes physical disruption to the functioning of an animal enterprise" and thereby causes economic damage to that enterprise. Sentences under AEPA1 included fines, restitution, and possible imprisonment of not more than one year for infractions not involving serious bodily injury. Explicitly defining targets and po-

[8] Anti-Drug Abuse Act of 1988, Public Law No. 100-690 (November 18, 1988).

[9] Animal Enterprise Protection Act of 1992, 18 U.S.C. § 43, Public Law No. 102-346 (August 26, 1992).

tential punishment, this legislation served to protect animal enterprises and increase the cost of illegal direct action.

In June 2002 Congress amended AEPA1 to increase maximum penalties (hereafter, AEPA2).[10] Whereas convictions under AEPA1 required economic damage and property loss in excess of $10,000, the modified act allowed for a broader spectrum of offenses. Section 43(b) of title 18, United States Code, amended the penalties to include:

- Any person who causes economic damage not exceeding $10,000 shall be fined or imprisoned not more than 6 months, or both.
- Any person who causes economic damage exceeding $10,000 shall be fined or imprisoned not more than 3 years, or both.

Relative to AEPA's original wording, the newly amended act now outlined penalties for small-scale illegal actions ($< \$10{,}000$) and increased penalties threefold for large-scale actions ($> \$10{,}000$). Increasing the scope of offenses and the magnitude of punishment, this amendment further increased the cost of illegal direct action.

Operation Backfire (2004-2006)

Confounded with a wave of illegal direct action in the Pacific Northwest, the FBI's Portland office consolidated seven independent investigations in 2004 and named it "Operation Backfire." The primary target of the proactive investigation was a group known as "The Family," who were responsible for more than US$ 40 million in property damage between 1996-2001. On December 7, 2005, aided by other federal agencies and group infiltration, the FBI arrested seven people in four different states in connection with a

[10] 18 U.S.C. § 43 (a), (b), (c) (amended June 12, 2002).

variety of environmental sabotage actions. Following a year of informant testimony and plea deals, a total of seventeen people were sentenced to between 3 and 16 years in federal prison. Engendering fear and distrust in fellow activists, Operation Backfire capitalized on confidential informants and cooperation following arrest to disincentivize illegal direct action amidst the threat of betrayal and imprisonment (Deshpande and Ernst, 2012).

Animal Enterprise Terrorism Act of 2006

As the animal rights movement escalated and evolved, animal industry groups clamored for broader legislation citing a lack of convictions under AEPA. In November 2006 Congress passed the Animal Enterprise Terrorism Act (AETA) that increased penalties and further protected animal enterprises.[11] Continuing to criminalize illegal action directly against animal enterprises, AETA could now convict anyone who targeted "any real or personal property of a person or entity having a connection to, relationship with, or transactions with an animal enterprise." AETA proponents included this clause to protect against "tertiary targeting," a new strategy that attacked not only a specific business or organization, but anyone affiliated with animal enterprises. Expanding AEPA's scope further, AETA protected against "intentionally placing a person in reasonable fear of death" by way of "threats, . . . , harassment, or intimidation." Adding this language intended to deter protests outside an individual's home or place of business that increasingly caused a climate of fear for those targeted.

In addition to broadening potential offenses, AETA also served to increase penalties for those convicted under its guidelines. Section 43(b) of title 18, United States Code, amended the penalties to include:

[11] Animal Enterprise Terrorism Act, 18 U.S.C. § 43, Public Law No. 109-374 (November 27, 2006).

- Any person who causes economic damage not exceeding $10,000 and does not instill reasonable fear of injury or death, shall be fined or imprisoned not more than 1 year, or both.
- Any person who causes economic damage between $10,000 and $100,000 or instills reasonable fear of injury or death, shall be fined or imprisoned not more than 5 years, or both.
- Any person who causes economic damage between $100,000 and $1,000,000, shall be fined or imprisoned not more than 10 years, or both.
- Any person who causes economic damage exceeding $1,000,000, shall be fined or imprisoned not more than 20 years, or both.

Relative to AEPA2, AETA's penalties significantly increase the potential punishment for incidents that cause economic damage and those that instill reasonable fear in targeted individuals. The maximum imprisonment for small-scale actions ($< \$10,000$) doubled and penalties for large-scale actions ($> \$10,000$) increased between 67 and 567 percent depending on the magnitude of economic damage.

While AEPA acts and AETA have focused on animal rights criminal activity, illegal direct action against environmental enterprises has also been tried under AETA. Furthermore, given that the animal and environmental movements fight for similar ideologies and its activists have worked together on covert operations, legislation targeting one group can have a tangible impact on the broader movement by increasing the costs of illegal direct action.

2.4 Theoretical Framework

Examining terrorism in a rational choice framework, a resource allocation problem can be modeled to identify behavioral responses to counterterrorism interventions (Enders and Sandler, 1993; Barros, 2003). The standard approach allows the terrorist choice set to span alternative modes of attack (e.g. bombs or threats) or targets of attack (e.g. businesses or universities) in an effort to maximize their intended goal, such as political instability or forcing an enterprise out of business (Enders and Sandler, 2004; Sandler, 2013). Having allocated time between terrorist and non-terrorist activities, assume an agent optimizes between two attack modes to maximize utility subject to a budget constraint. If the two attack modes available to the agent are bombs (B) and threats (T), then the budget constraint can be written as $p_B B + p_T T = M$, where p_B and p_T are the per-unit prices of their respective attack mode and M is the level of income allocated to terrorist activities. Per-unit prices comprise both the value of time and the pecuniary cost of resources utilized for an action. Assuming an interior solution and well-behaved preferences, Point A in Figure 2.5 illustrates the utility maximization problem when selecting the optimal consumption bundle of terrorist tactics. Reflected in the BT-plane, a steep indifference curvature implies a low degree of substitutability between bombs and threats which ensues if an agent believes one mode of attack is more effective than another. Perfectly elastic or inelastic indifference curves are possible if agents strictly prefer one tactic (e.g. suicide attack), resulting in a corner solution.

Increasing the cost of illegal activity, the impact of counterterrorism policy can be identified in this model as rational agents respond to relative price changes and behavioral preferences shift in an evolving terrorism campaign. Assume new legislation imposes significant imprisonment penalties on actions that create a climate of fear from threats

or intimidation, increasing p_T, in an effort to deter tertiary targeting (see Section 2.3: Counterterrorism Interventions). Increasing the costs of threat tactics, this intervention would rotate the budget constraint inwards along the vertical axis in Figure 2.5. The new optimal bundle will depend on how the consumption of each tactic responds to an income change and whether they are substitutes or complements. For simplicity assume bombing activities are normal goods and substitutes for threat tactics. Assessing the legislation's aggregate impact on T, it is important to isolate the income and substitution effects. An increase in p_T causes terrorists to substitute away from threats and towards relatively cheaper bombing tactics. A Hicksian compensated demand curve reveals the pure substitution effect (Figure 2.5, Point B).[12] If threats are a normal good, the decrease in purchasing power further decreases the consumption of threat tactics. With negative income and substitution effects, T decreases relative to its pre-intervention level (Figure 2.5, Point C). It is equally likely, with a finite set of attack modes, that threats are an inferior good as they may constitute the cheapest terrorist tactic to achieve the agent's objective. The decreased purchasing power from an increase in p_T would thus impart a positive income effect. If the magnitude of the negative substitution effect outweighs the income effect, then there is still an aggregate decrease in T (see Figure 2.6). In the case of major inferiority, however, the positive income effect could outweigh the substitution effect and increase the consumption of threat tactics above its pre-intervention level.

Although governmental intervention may increase the costs of terrorism, behavioral responses may result in unintended outcomes which can be tested in the rational choice model. If there existed substitutable non-terrorist tactics that increased the success rate of a terrorist objective, governmental policy could reduce the cost or remove constraints on these legal forms of activism, thereby disincentivizing illegal methods. Coined benev-

[12]See Varian (2014) for the complete spectrum of price decomposition scenarios.

olence policy in the literature, this approach aims to increase the opportunity cost of terrorist methods by reducing the price of substitutable non-terrorist activities (Frey and Luechinger, 2003; Anderton and Carter, 2005). Corresponding measures may include improving access to legitimate means of protest or arbitrated negotiations between activists and target companies. Intended to appease the mainstream movement, benevolence policy might instead incense radical fringes who discredit cooperation and denigrate compromise. Lastly, laws criminalizing an attack mode or protecting a target may substantiate the efficacy of illegal forms of activism that prompt special interests groups to lobby for increased constitutional protection. Such a realization may theoretically alter terrorist preferences toward an escalated campaign against its enemies.

2.5 Methodology

A variety of statistical methods (time series, survival analysis, multinomial logistic models) have been used to measure the effectiveness of counterterrorism interventions. In addition to testing for direct policy impacts, a robust identification strategy should control for (i) dynamic links between separate time series and (ii) substitution possibilities (see Section 2.4). Measuring the impact of policy interventions with a classic autoregressive method, consider the following model:

$$y_t = \alpha + \beta y_{t-1} + \gamma p_t + \epsilon_t, \tag{2.1}$$

where y_t is the number of bombing attacks in quarter t, p_t is a dummy variable indicating whether a policy intervention was in effect at time t, and ϵ_t is a white noise variable. Assuming $|\beta| < 1$ precludes system shocks from having a persistent, increasingly large influence through time. The short-run impact of policy p_t can be evaluated

by the magnitude and significance of γ, while the long-run impact is calculated as the difference between the post-intervention mean, $(\alpha + \gamma)/(1 - \beta)$, and its pre-intervention level, $(\alpha)/(1 - \beta)$.

Traditional univariate models may confirm a policy's ability to decrease bombing incidents, but terrorists may substitute between other modes of attack. Sims (1980) advocated a vector autoregressive (VAR) approach to estimate economic relationships and capture interdependencies between multiple time series. Modeling endogenous variable interactions, the VAR framework can identify indirect policy impacts through relationships with variables that may or may not be directly influenced by counterterrorism interventions. Returning to the example in Section 2.4, let $y_{1,t}$ and $y_{2,t}$ be the number of bombing attacks and threat incidents, respectively, in quarter t. Analogous to the framework in Enders and Sandler (1993), consider the vector autoregressive system:

$$y_{1,t} = \alpha_1 + \beta_{11} y_{1,t-1} + \beta_{12} y_{2,t-1} + \gamma_1 p_t + \epsilon_{1,t} \tag{2.2}$$

$$y_{2,t} = \alpha_2 + \beta_{21} y_{1,t-1} + \beta_{22} y_{2,t-1} + \gamma_2 p_t + \epsilon_{2,t} \tag{2.3}$$

where β_{ij} $(i, j \in \{1, 2\})$ represents the impact of variable j's first-order lag on the i-th attack mode. Illustrating the direct and indirect impact of counterterrorism tactics, assume policy p increases imprisonment penalties on threat and intimidation campaigns to reduce threat incidents in Equation (2.3). Coefficients γ_1 and γ_2 capture the policy's direct impact on attack modes 1 and 2, respectively. While a policy targeting threat incidents may not have a direct impact on a substitute attack mode (i.e., $\gamma_1 = 0$), if β_{12} is non-zero the policy will have an indirect impact on attack mode 1 through its impact on

attack mode 2 (Enders and Sandler, 1993).[13] Interactions between attack modes imply that a policy may reduce one terrorist tactic directly, but positive gains can be nullified (increased) through indirect effects on substitute (complementary) incident types. Intertemporal dynamics and substitution between modes, targets, and ideologies of attack may bias an inefficient univariate approach to modeling terrorism. A simultaneous vector analysis aims to model the interdependence between multiple time series.

Generalizing the analysis to include additional attack modes or targets, policy interventions, lags, and exogenous variables, a vector autoregressive framework is represented by the equation:

$$y_t = \alpha + \sum_{l=1}^{L} B_l y_{t-l} + \Gamma P_t + \Lambda x_t + \epsilon_t \tag{2.4}$$

where y_t is a vector of n incident types and their respective count in quarter t, α is an n x 4 matrix containing a constant and three seasonal dummy variables, L indicates the number of lags specified by the model, P_t is a vector of k dummy variables $p_{k,t}$ that define if policy k was in effect at time t, x_t is a vector of exogenous political and economic variables, and ϵ_t is a vector of white noise innovations that may be correlated across equations. Controlling for exogenous impacts in matrix Λ, each B_l coefficient matrix (n x n) represents the within and between impact of incident type i's l-th order lag on the VAR system. Direct policy impacts are contained in matrix Γ. Illustrated in the two incident type VAR system above, even if $\gamma_{ik} = 0$, non-zero off-diagonal elements in matrix B_l mean that policy p_k will have an indirect effect on incident type i through its dynamic relationship with other incident types (Enders and Sandler, 1993).

[13] Without loss of generality, assume there are two attack modes ($y_{i,t}$ and $y_{j,t}$) and interventions ($p_{i,t}$ and $p_{j,t}$) targeting specific attack modes. In the VAR system, if β_{ij} and/or β_{ji} ($i \neq$j) are non-zero coefficients, then policy j will have an indirect impact on attack mode i through its lagged relationship with attack mode j, and vice versa.

Utilizing the VAR framework to study terrorism-thwarting interventions, the impact coefficient matrices in Equation (2.4) are measured similar to the method outlined in Enders and Sandler (1995). An identical step-by-step approach is employed to identify the impact of counterterrorism policy on who attacks (ideology), where they attack (target type), how they attack (weapon type), and the severity of illegal actions as measured by economic damage and targeting people versus property.

Step 1: Stationarity in levels for each time series is evaluated by testing for a unit root with an augmented Dickey-Fuller (ADF) test.[14] ADF regressions include a requisite number of lags to avoid serial correlation in the residuals, determined by a Breusch-Godfrey test. If a series contains a unit root, successive tests are employed to determine the order of integration (d) and assign d differences to induce stationarity.

Step 2: Lag length in Equation 2.4 is determined by pre-estimation information criteria, namely AIC and SBIC measures. Containing identical regressors for each endogenous equation in vector y_t, Equation 2.4 is efficiently estimated with ordinary least squares (Enders, 2015).[15] A Lagrange-multiplier test is used to check for autocorrelation in the residuals of the VAR.

Step 3: To avoid over-parametrization in the VAR model, variance decomposition and Granger-causality tests help identify relationships between incident types to restrict insignificant parameters from the analysis. Using a vector moving-average (VMA) repre-

[14]Each endogenous and exogenous time series is visually inspected to determine if a drift and/or trend component should be included in the augmented Dickey-Fuller test.

[15]To ensure reliable estimates and interpretable findings, the stability of the VAR system is analyzed post-estimation to confirm that its characteristic roots (eigenvalues) lie outside (inside) the unit circle (Lütkepohl, 2005). VAR stability is a necessary condition to calculate the moving-average representation of the VAR system, to be used in Step 3.

sentation of Equation (2.4) that relates each incident type in y_t as a function of current and past values of all error terms, the variance decomposition breaks down the proportion of forecasted movement of a sequence due to its own shocks or the shocks of other variables.[16] When two series are closely related, at least one will explain a significant proportion of the forecast error variance in the other series.[17]

Further exploring relationships between time series, Granger-causality tests whether lags of one variable are useful in forecasting another variable. Formally, a times series y_j does not Granger-cause series y_i if and only if all lagged variables of y_j are jointly insignificant. Adopting exclusion criteria from prior studies, if i) incident type y_j does not explain at least 10 percent of the forecast error variance of incident type y_i and ii) y_j does not Granger-cause y_i at the 0.10 level of significance, then zero-restrictions ($\beta_{1ij} = \cdots = \beta_{Lij} = 0, i \neq j$) are imposed in Equation 2.4.

Step 4: With nonidentical regressors in Equation 2.4 after applying Step 3's parameter restrictions, the parsimonious system is efficiently estimated using a constrained vector autoregression (Enders, 2015). Estimated coefficient matrices yield intervention direct impacts (Γ), within and between lagged relationships (B), and control for exogenous effects (Λ). Indirect and long-run impacts are identified with impulse response and cumulative dynamic-multiplier functions (Lütkepohl, 2005).[18]

[16] With a possibility of short or long-term relationships, 8, 12, and 20-quarter forecasting horizons are explored.

[17] To control for residual correlation across time series, a Choleski decomposition is used to orthogonalize innovations in the moving-average process. As the Choleski decomposition postulates a causal relationship between variables, results may be sensitive to the strict ordering of variables in y_t. The standard procedure is to i) list the variables in increasing order of contemporaneous dependence on the prior series and ii) reverse the variable ordering to test the sensitivity of the variance decomposition. See Enders (2015) for details.

[18] Impulse response functions (IRFs) detect dynamic relationships by subjecting the VAR system to a one-time exogenous shock to one variable. The endogenous adjustment across equations reveals how innovation shocks impact future variable values and measures the persistent effect of these shocks

2.6 Data

The REDA (radical environmental direct action) database includes illegal incidents perpetrated by the animal and environmental rights movements in the United States from 1980 to 2014. Quarterly counts were compiled from many sources, including mainstays from the terrorism literature: the Global Terrorism Database (GTD) maintained by the University of Maryland (START, 2015), RAND's incident collection (RAND, 2015), and the FBI's Terrorism in the United States reports. These datasets are developed via publicly available media reports, electronic news archives, and existing databases. While these sources encompass a wide array of domestic terrorist events, they systematically underreport criminal REDA incidents which are not always included in terrorist chronologies. Maintaining consistency with published datasets, additional data was collected and coded in accordance with inclusion criteria outlined in the GTD codebook (START, 2015). Supplemental sources are reported in Figure 2.2. Collecting a total of 3,444 incidents from all sources, 1,511 unique incidents were included in the REDA database after purging data that (i) did not meet minimum criteria (time frame, definition of REDA action), (ii) lacked credible primary sources, or (iii) duplicated previously collected events.[19] The below figure depicts aggregate REDA incidents from 1980 until 2014.

(Lütkepohl, 2005). Determining long-run policy impacts, the dynamic-multiplier function captures the effect of a one-unit change in an exogenous variable on endogenous variables over time.

[19]Limitations in terrorism datasets are important to address as incidents often entail covert operations by clandestine groups. In addition to the upward trend in terrorism reporting after the events of 9/11, supplemental databases are often compiled by interest groups that represent industries directly impacted by terrorist groups; such data may be challenged as non-objective. Other databases include self-reported incidents by activists and their own networks. Actions may be over-reported to instill fear, coerce current and future targets, and induce targeted industries to finance interest group research. Actions may be underreported by activists to prevent targets from investing in costly security deterrents and by victims to minimize copy-cat crimes, increased insurance premiums, and reduced business from industry partners and customers. Surveying multiple sources allows for cross validation to obtain the richest data possible. Each incident has been verified by an alternate news source or an online database (LexisNexis, Internet Archive) to substantiate information provided.

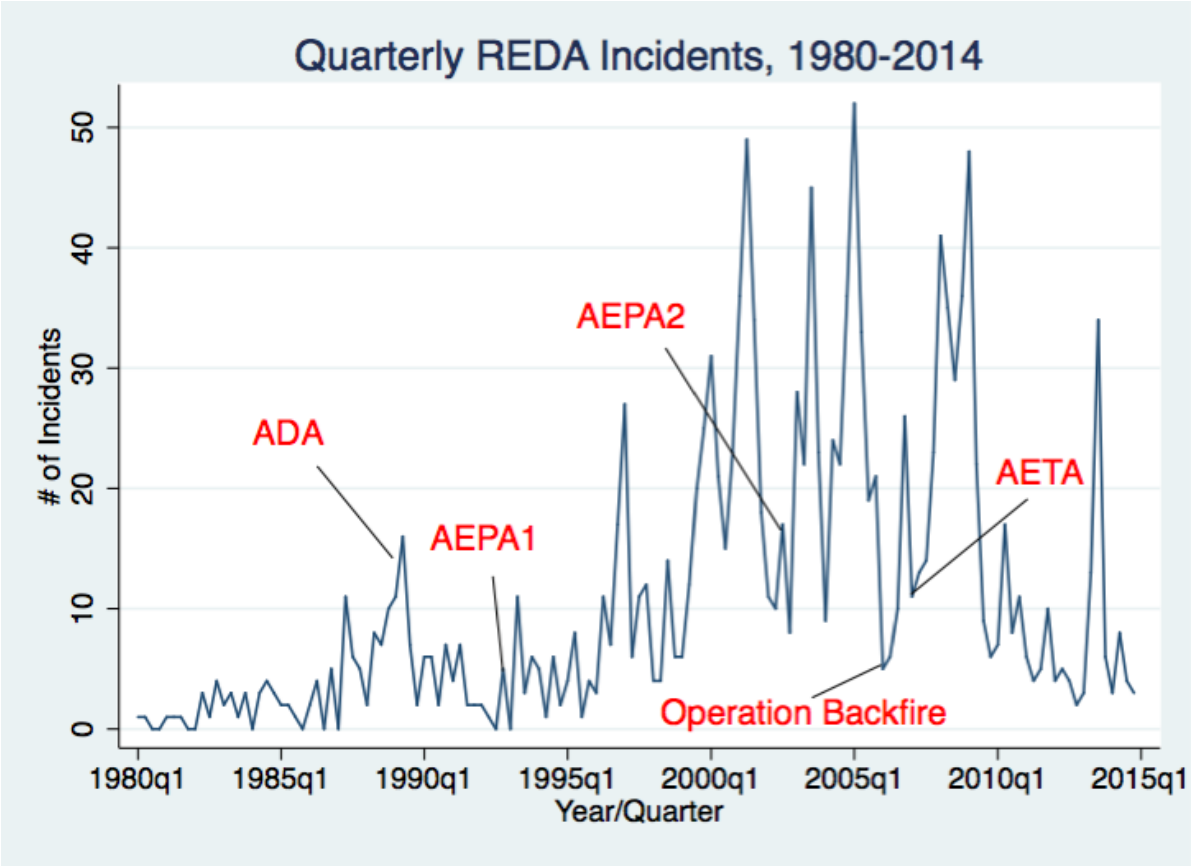

Figure 2.1: Quarterly REDA Incidents

Each REDA incident contains information on both the target and perpetrator to analyze domestic terrorism. In addition to cataloging the date and location of each incident, the REDA database includes the perpetrator group name, ideology, and weapon type.[20] Victim characteristics identify target type and the amount of economic damage sustained.[21] Other time series were manufactured from these primary variables (see Table 2.5). Descriptive statistics are reported in Table 2.1.

[20]Ideology identifies each incident as motivated by either the animal or environmental rights movement. Weapon type categories include biological/chemical, explosives/incendiary devices, firearms/melee, minor sabotage, and other.

[21]Target type is divided into four categories: private business, private citizens, public sector, and education. The economic damage variable is divided into six non-overlapping bins: \$0, <\$1,000, <\$10,000, <\$100,000, <\$1,000,000, and <\$10,000,000.

Source	Years	Description
Diary of Actions/Bite Back magazine	2002-present	activist and media reports
Foundation for Biomedical Research (FBR)	1981-2006	activist reports, news archives
North American Animal Liberation Press Office (NAALPO)	2004-present	activist reports
North American Earth Liberation Front Press Office (NAELFPO)	1996-2008	activist and media reports
National Animal Interest Alliance (NAIA)	1983-present	media and industry reports
Southern Poverty Law Center (SPLC)	1984-2002	activist and crime reports

Figure 2.2: Supplemental Sources for REDA Database

Controlling for exogenous factors and opportunity costs, the REDA database also includes period-specific economic and political factors. These variables include the unemployment rate and the presidential political party. Dummy variables have also been incorporated to control for seasonal effects. Lastly, I include a dummy variable to control for post-9/11 terrorism reporting. The events of 9/11 increased media coverage of all incidents marginally tied to terrorism, potentially biasing data collection relative to pre-9/11 efforts.[22] Variable descriptions for passive counterterrorism legislation (ADA, AEPA, AETA) and proactive interventions (Operation Backfire) can be found in Section 2.3.

[22]It is plausible that the events of 9/11 did in fact breed more terrorism, responsible for the frequency increase in many data sets. Empirical reports have also identified that incidents may have been underreported due to a lack of media coverage prior to 9/11 events.

Table 2.1: Quarterly Descriptive Statistics for REDA Incident Variables (1980-2014)

Category	Variable	μ	σ	Min	Max
Ideology	Animal Rights	7.84	9.66	0	48
	Environmental Rights	2.95	4.93	0	29
Target Type	Private Business	8.10	9.51	0	46
	Private Citizens	0.49	0.97	0	5
	Public Sector	0.76	1.15	0	6
	Education	1.44	3.47	0	21
Weapon Type	Biological/Chemical	0.14	0.41	0	2
	Explosive/Incendiary	2.08	2.67	0	13
	Firearm/Melee	0.46	2.08	0	21
	Minor Sabotage	7.11	8.18	0	37
	Other	1.01	2.30	0	15
Economic Damage[a]	\$0	0.45	1.45	0	15
	<\$1,000	3.70	6.42	0	39
	<\$10,000	2.86	3.99	0	23
	<\$100,000	2.00	2.37	0	17
	<\$1,000,000	0.86	1.29	0	7
	<\$10,000,000	0.20	0.57	0	3
Property vs People	Property	8.28	8.88	0	48
	People	2.51	4.95	0	32
Exogenous Controls	Unemployment Rate	6.45	1.63	3.9	10.67
	Republican	0.57	0.50	0	1

[a] Economic Damage bins are non-overlapping.

2.7 Empirical Results

Estimation results present the effects of passive and proactive interventions (see Section 2.3) on REDA incidents and possible substitution between modes of attack. These models identify the impact of counterterrorism policy on who attacks (ideology), where they attack (target type), how they attack (weapon type), and the severity of illegal actions as measured by economic damage and targeting people versus property. Each model controls for unemployment, presidential political party, the 9/11 effect, and seasonal dummy variables that will be excluded from the output.

2.7.1 Counterterrorism Impact on Ideology of Attack

Federal investigations have revealed overlapping membership in the animal and environmental rights movements. If legislation increases penalties for illegal action in one movement, activists may reallocate their resources toward a relatively cheaper substitute cause. Given this dynamic relationship, evaluating policy effects on aggregate REDA data fails to control for substitution possibilities and the disparate effect on each movement.

Measuring the impact of counterterrorism policy on animal and environmental illegal actions, vector autoregression (VAR) captures the interrelationships between the ideologies. Following steps outlined in Section 2.5, each series was found to be stationary and two lags best characterized the autoregressive system. Although the variance decomposition did not identify any links between the series, the animal rights ideology was found to Granger-cause the environmental rights ideology. This finding implies that an intervention directly impacting the animal rights series would indirectly impact the level of environmental incidents.

Imposing restrictions from the variance decomposition and Granger-causality tests, Table 2.2 presents the coefficient estimates from the constrained VAR system. Although the animal enterprise acts (AEPA1, AEPA2, AETA) aimed to deter domestic terrorism, illegal activity in the name of animal rights significantly increased following legislative interventions. While each successive policy increased restitution and imprisonment penalties, animal activists increased illegal actions between 6 and 12 incidents per quarter. Consistent with a rational choice framework, increasing the cost of illegal animal rights action caused some activists to substitute toward relatively cheaper environmental direct action. Following each animal enterprise act, illegal environmental direct action increased between 3 and 10 incidents per quarter. Counterbalancing passive legislation with proactive network infiltration, Operation Backfire targeted environmental activists in the Pacific Northwest and significantly reduced illegal environmental activity by over 4 incidents per quarter. This proactive intervention also decreased illegal animal rights actions by almost 6 incidents per quarter. In aggregate, passive counterterrorism policies have significantly incited a greater magnitude of illegal incidents relative to proactive efforts to quell direct action.

Beyond immediate impacts, a cumulative dynamic-multiplier function identifies a policy's impact on endogenous series over time to determine long-run effectiveness in the autoregressive system. The bottom of Table 2.2 reports the long-run impact of each intervention after three years. Among the three animal policies, only the 1992 act had a significant long-run impact on ALF activity. Instead of reducing illegal actions, however, it significantly increased animal rights attacks by double digits per quarter. A notable result from the VAR method is that each legislative animal rights policy had a permanent effect on environmental actions. Stemming from the relationship between animal

and environmental incidents, animal policies permanently caused activists to substitute to environmental direct action and indirectly increased long-run illegal action between 7 and 23 incidents per quarter. The proactive Operation Backfire intervention only significantly reduced environmental actions in the long run, consistent with the rational choice hypothesis.

Table 2.2: Quarterly REDA Incidents by **IDEOLOGY** (Model 1)

	Endogenous Series	
Counterterrorism Interventions	Animal Rights (ALF)	Enviro Rights (ELF)
	Immediate Impacts	
ADA	-1.224	-0.280
	(2.02)	(1.04)
AEPA1	6.135**	3.413**
	(2.76)	(1.39)
AEPA2	10.660**	10.611***
	(4.96)	(2.48)
AETA	12.662**	9.106***
	(5.39)	(2.66)
Backfire	-5.740*	-4.186**
	(3.24)	(1.68)
	Long-Run Impacts	
ADA	-1.901	-0.642
AEPA1	11.288‡	7.698‡
AEPA2	15.829	23.871‡
AETA	19.638	20.740‡
Backfire	-10.484	-9.471‡

Note: Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

‡ Denotes a significant long-run impact after 12 quarters.

ADA: Anti-Drug Abuse Act (1988)

AEPA1: Animal Enterprise Protection Act (1992)

AEPA2: Animal Enterprise Protection Act (2002)

AETA: Animal Enterprise Terrorism Act (2006)

Backfire: Operation Backfire (2004-2006)

2.7.2 Counterterrorism Impact on Attack Mode

Among the various modes of attack including who, where, when, and how illegal actions materialize, this section focuses on the targets of attack and weapon of choice during REDA incidents.

Table 2.3 identifies intervention effects on targets of REDA actions, broken down into private business, private citizens, the public sector, and education. The education series includes incidents against teachers, researchers, and publicly funded scientific labs. From vector decomposition and Granger-causality tests, there emerged a rich network of interrelationships between the targets of attack. Important distinctions include all targets Granger-causing private business attacks and private citizen incidents being tied to education attacks. One lag best characterized the vector autoregressive system and all series were stationary in levels.

The descriptive statistics in Table 2.1 reveal that activists have overwhelmingly allocated resources in attacking the private sector, averaging 8 incidents per quarter from 1980 to 2014. These actions include attacks on car dealerships, fur shops, and the entertainment industry among others. Mirroring intervention impacts on ideological attacks, passive animal legislation significantly increased actions against private business between 6 and 15 incidents per quarter. Easily accessible relative to other targets, striking private business may constitute a low-risk, high-reward allocation of resources compared to attacking governmental buildings and individuals at home. With a peak of 21 quarterly attacks, education was the other major target as activists attempt to disrupt animal experimentation and genetically engineered crop research. The widely publicized AETA legislation induced a strategic response to target highly visible labs and researchers,

boosting incidents almost 400 percent above average levels. Each successive passive animal policy also significantly increased actions against private citizens, targeting sports utility vehicles and executives at their homes. Operation Backfire continued to deter illegal REDA incidents, significantly decreasing actions against private business and the public sector.

All highlighted immediate impacts maintained their significance in the long run. Stemming from its interdependencies with other targets of attack, each intervention's effect on private business attacks more than doubled after three years. Two more changes are worth noting. While not immediately significant at conventional levels, AEPA1 induced a marginal increase in public sector attacks in the long run. In the education sector, not only was there a marked rise in illegal actions after AETA, AEPA1 significantly increased incidents against educators in the long run. Two instrumental counterterrorism acts have not only failed to deter illegal REDA actions, but have substantially increased the targeting of researchers and university laboratories.

Focusing on how incidents transpired, Table 2.4 reports the effects of counterterrorism policy on weapon type in REDA actions. All series were found to be stationary in levels and one lag best characterized the system. Variance decomposition and Granger-causality tests directly link explosives and minor sabotage, while minor sabotage also impacted each series. Explosives and minor sabotage against property constituted over 85 percent of actions per average quarter, while deadlier weapons of attack against people (biological, firearms) were far less common. Although animal and environmental rights activists frequently utilize terror tactics, each's movement's press office publicly heralds the fact that neither group has ever killed anyone.

Table 2.3: Quarterly REDA Incidents by **TARGET TYPE** (Model 2)

	Endogenous Series			
Counterterrorism Interventions	Private Business	Private Citizens	Public Sector	Education
	Immediate Impacts			
ADA	-0.485	-0.021	-0.145	-0.514
	(1.94)	(0.26)	(0.30)	(0.84)
AEPA1	6.228**	0.999***	0.540	1.771
	(2.67)	(0.35)	(0.42)	(1.14)
AEPA2	15.508***	2.035***	1.389*	1.773
	(4.78)	(0.64)	(0.75)	(2.05)
AETA	11.441**	2.348***	0.342	5.561**
	(5.11)	(0.67)	(0.79)	(2.21)
Backfire	-6.913**	0.135	-1.477***	-1.633
	(3.19)	(0.42)	(0.49)	(1.33)
	Long-Run Impacts			
ADA	-1.750	-0.054	-0.178	-0.886
AEPA1	13.980‡	1.254‡	0.731‡	4.394‡
AEPA2	30.451‡	2.459‡	1.750‡	5.747
AETA	28.173‡	2.672‡	0.850	12.249‡
Backfire	-15.253‡	-0.176	-1.634‡	-2.712

Note: Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

‡ Denotes a significant long-run impact after 12 quarters.

ADA: Anti-Drug Abuse Act (1988)

AEPA1: Animal Enterprise Protection Act (1992)

AEPA2: Animal Enterprise Protection Act (2002)

AETA: Animal Enterprise Terrorism Act (2006)

Backfire: Operation Backfire (2004-2006)

Increasing the cost of illegal action, animal legislation intensified activists' weapon use against property. While passive interventions significantly increased minor sabotage, these policies almost tripled the use of explosive and incendiary devices. Given the potential to harm human life, this effect was an unintended consequence of legislative policy. Disincentivizing illegal modes of activism, costly penalties create a positive income ef-

fect on inferior goods that could outweigh negative substitution effects, increasing overall use of tactically efficient weapon types including small bombs and minor sabotage. Intervention impacts were magnified in the long run given the compounding ties between series. Operation Backfire decreased overall weapon use, but its magnitude paled in comparison to the aggregate increase from passive animal legislation. Interventions did not significantly impact the use of deadlier weapons that target people.

Table 2.4: Quarterly REDA Incidents by **WEAPON TYPE** (Model 3)

	Endogenous Series				
Counterterrorism Interventions	Biological/ Chemical	Explosives/ Incendiary	Firearms/ Melee	Minor Sabotage	Other
	Immediate Impacts				
ADA	-0.037	0.400	0.018	-1.886	0.008
	(0.12)	(0.62)	(0.64)	(1.57)	(0.62)
AEPA1	0.179	3.167***	0.453	5.338**	0.347
	(0.17)	(0.87)	(0.88)	(2.20)	(0.88)
AEPA2	0.363	6.392***	0.494	12.014***	2.192
	(0.30)	(1.52)	(1.57)	(3.86)	(1.53)
AETA	0.513	4.415***	0.808	15.881***	0.250
	(0.32)	(1.62)	(1.70)	(4.11)	(1.63)
Backfire	0.006	-3.011***	-0.034	-5.005*	-1.921*
	(0.20)	(1.02)	(1.04)	(2.59)	(1.03)
	Long-Run Impacts				
ADA	-0.047	0.284	-0.118	-2.852	-0.138
AEPA1	0.189	4.168‡	0.986	12.491‡	1.821‡
AEPA2	0.467	8.753‡	1.646	28.104‡	5.072‡
AETA	0.605‡	6.719‡	2.151	31.727‡	3.387‡
Backfire	-0.17	-4.033‡	-0.558	-12.602‡	-3.094‡

Note: Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

‡ Denotes a significant long-run impact after 12 quarters.

ADA: Anti-Drug Abuse Act (1988)

AEPA1: Animal Enterprise Protection Act (1992)

AEPA2: Animal Enterprise Protection Act (2002)

AETA: Animal Enterprise Terrorism Act (2006)

Backfire: Operation Backfire (2004-2006)

2.7.3 Counterterrorism Impact on Severity of Attack

Intervention effectiveness can also be gauged by its impact on the severity of REDA actions. A two-pronged approach is used to measure severity impacts, differentiating between the level of economic damage and the propensity to target people versus property.

Figure 2.3 identifies counterterrorism impacts on REDA incidents by damage amount, broken down into six non-overlapping bins. Variance decomposition and Granger-causality tests reveal a complex set of interrelationships between series that reduce as the bins increase in magnitude. Information criteria identify one lag as best characterizing the stationary system.

Descriptive statistics in Table 2.1 reveal a positively skewed distribution that favors low-damage attacks. Incidents with less than $1,000 in damage are confined to minor sabotage including spray painted property, broken windows, glued locks, and destroyed fence enclosures. The animal rights movement made up most of the low-damage attacks (92%), which constitute 44 percent of their total actions.[23] Legislation targeting the animal rights movement (AEPA1, AEPA2, AETA) failed to decrease low-damage actions which instead increased between 113 and 300 percent relative to average levels. Surveying higher damage bins, interventions continued to increase illegal activity but by a decreasing magnitude. Targeting the environmental rights movement and infiltrating activist networks, Operation Backfire significantly reduced all incidents with under $1 million in damage. Constituting 68 percent of all environmental actions, Operation Backfire particularly reduced incidents between $1,000 and $1 million in damage by incentivizing

[23]The remaining 8 percent of low-damage incidents were from the environmental rights movement, which constitute 9 percent of their total actions.

Counterterrorism Interventions	Endogenous Series					
	$0	<$1,000	<$10,000	<$100,000	<$1 million	<$10 million
	Immediate Impacts					
ADA	0.032	-0.825	-0.186	-0.187	-0.076	-0.124
	(0.40)	(1.30)	(0.78)	(0.57)	(0.33)	(0.16)
AEPA1	0.721	4.180**	1.767*	2.097***	1.209***	0.570***
	(0.55)	(1.81)	(1.07)	(0.79)	(0.45)	(0.22)
AEPA2	1.451	9.069***	7.709***	2.430*	1.563*	1.026***
	(1.00)	(3.22)	(1.95)	(1.40)	(0.81)	(0.38)
AETA	1.181	11.002***	3.755*	2.380*	1.684**	1.121***
	(1.09)	(3.50)	(2.02)	(1.46)	(0.84)	(0.41)
Backfire	-1.421**	-3.848*	-5.752***	-1.654*	-1.067*	0.096
	(0.68)	(2.25)	(1.32)	(0.98)	(0.56)	(0.27)
	Long-Run Impacts					
ADA	-0.100	-1.596	-0.344	-0.270	-0.107	-0.138
AEPA1	1.107	7.810‡	3.802‡	3.361‡	1.627‡	0.821‡
AEPA2	1.851	13.937‡	12.557‡	5.260‡	2.613‡	1.459‡
AETA	2.393‡	20.801‡	7.609‡	4.495‡	2.515‡	1.493‡
Backfire	-1.054	-2.899	-9.035‡	-3.685‡	-1.715‡	-0.194

Note: Damage amount bins are non-overlapping; Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

‡ Denotes a significant long-run impact after 12 quarters.

ADA: Anti-Drug Abuse Act (1988)

AEPA1: Animal Enterprise Protection Act (1992)

AEPA2: Animal Enterprise Protection Act (2002)

AETA: Animal Enterprise Terrorism Act (2006)

Backfire: Operation Backfire (2004-2006)

Figure 2.3: Quarterly REDA Incidents by **DAMAGE AMOUNT** (Model 4) Immediate and Long-Run Counterterrorism Impacts

smaller attacks to minimize detection.

In addition to impacting incident levels, REDA interventions also influenced each ideology's composition of damage attacks. Figure 2.4 breaks down the percentage of incidents by damage amount before and after interventions. Before each intervention, an average of 35 percent of animal actions were low-damage ($< \$1,000$) compared to only 7 percent for the environmental movement. The bulk of pre-intervention environmental actions occurred in the \$1,000-\$1 million range, averaging 71 percent compared to 51 percent for the animal rights movement. Highlighted table cells identify post-intervention differences of at least 5 percentage points. Consistent with the rational choice framework and prior results, Figure 2.4 shows animal legislation caused activists to not only substitute toward low-damage animal incidents, but increasingly allocate resources to mid-level damage actions in the environmental movement. Severe punishment penalties also decreased the composition of large-scale actions ($>$ \$1 million) across interventions and movements.

All immediate impacts in Figure 2.3 remain significant in the long run except Operation Backfire's effect on low-damage attacks ($< \$1,000$). While this proactive intervention targeted environmental groups, a large compositional shift in animal rights incidents may have negated the overall reduction in low-damage illegal action. AETA caused the largest absolute increase in long-run aggregate incidents alongside the highest restitution and imprisonment penalties to date.

An alternate method to measure policy's impact on the severity of REDA incidents is to model its effect on the propensity of actions to target people versus property. Table 2.5 presents intervention impacts on these two endogenous series. Each series was

found to be stationary and one lag best characterized the vector autoregressive system. Although the variance decomposition did not identify any links between the series, the "target property" series was found to Granger-cause the "target people" series.

Descriptive statistics in Table 2.1 reveal an overall tendency to target property over people, averaging 8.3 and 2.5 incidents per quarter, respectively. Targeting people includes terrorizing researchers, home visits to executives, and endangering loggers. Passive animal legislation (AEPA1, AEPA2, and AETA) significantly increased actions against property between 8 and 17 incidents per quarter, almost tripling incidents relative to average quarterly counts. Only AEPA2 significantly increased actions against people. Enacting strict legislation to protect individuals from "threats, . . . , harassment, or intimidation," AETA prevented immediate increases in the targeting of people. Operation Backfire ubiquitously decreased incidents in both series.

Beyond immediate effects, the bottom of Table 2.5 exhibits long-run impacts from passive and proactive interventions. All animal legislation continues to have a significant impact on property attacks in the long run, albeit increasing illegal actions as opposed to deterring illegal actions. Negative substitution effects that decrease actions against property may be eclipsed by positive income effects from the tactic's inferior status relative to targeting people, leading to an overall increase in incidents. The model's striking result is the significant increase in actions against people following all passive animal legislation. At the height of their influence, these interventions increased people incidents over sixfold in the long run. Protecting against "tertiary targeting" that attacks those affiliated with particular companies, AETA incentivized activists to bundle property damage with attacking individuals. Operation Backfire significantly reduced long-run incidents in both series, but by a smaller magnitude than passive legislation counterparts.

		AEPA1		AEPA2		AETA	
	Damage Bin	Animal*	Enviro*	Animal	Enviro	Animal	Enviro
	$0	7	2	3	3	2	5
Before	< $1,000	41	5	31	7	34	8
Intervention	$1,000 – $1 million	45	71	53	70	55	72
	> $1 million	7	22	13	20	9	15
	$0	4	5	4	7	5	4
After	< $1,000	44	9	51	10	59	4
Intervention	$1,000 – $1 million	46	72	42	76	33	77
	> $1 million	6	14	3	7	3	15

Note: Highlighted values exhibit a net change of at least 5 percentage points.
Before/after statistics are from non-independent samples; cannot test for significance between proportions.
*Animal=Animal Rights Movement, Enviro=Environmental Rights Movement
AEPA1: Animal Enterprise Protection Act (1992)
AEPA2: Animal Enterprise Protection Act (2002)
AETA: Animal Enterprise Terrorism Act (2006)

Figure 2.4: **Ideology's Percentage of Incidents by DAMAGE AMOUNT**

2.8 Conclusion

As governments allocate resources to thwart terrorism and terrorists weigh costs and benefits in their optimal choice of attack, this study modeled the quarterly impact of passive and proactive interventions on deterring radical environmental direct action. Passive legislative policies increase restitution and imprisonment penalties, while proactive operations infiltrate terrorist networks to prevent terrorist activity. Combining vector autoregression with intervention analysis, this methodology identifies the substitutable nature of terrorism tactics and models the interrelationships between series to measure

Table 2.5: Quarterly REDA Incidents: **TARGETING PROPERTY vs PEOPLE** (Model 5)

	Endogenous Series	
Counterterrorism Interventions	Target Property	Target People
	Immediate Impacts	
ADA	-1.116	-0.178
	(1.95)	(1.18)
AEPA1	8.710***	1.040
	(2.73)	(1.65)
AEPA2	17.393***	5.084*
	(4.73)	(2.91)
AETA	17.455***	4.849
	(5.09)	(3.11)
Backfire	-6.613**	-4.364**
	(3.14)	(1.93)
	Long-Run Impacts	
ADA	-2.130	-0.632
AEPA1	15.160‡	4.179‡
AEPA2	29.715‡	12.724‡
AETA	30.370‡	12.479‡
Backfire	-11.740‡	-8.519‡

Note: Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

‡ Denotes a significant long-run impact after 12 quarters.

ADA: Anti-Drug Abuse Act (1988)

AEPA1: Animal Enterprise Protection Act (1992)

AEPA2: Animal Enterprise Protection Act (2002)

AETA: Animal Enterprise Terrorism Act (2006)

Backfire: Operation Backfire (2004-2006)

both direct and indirect effects of counterterrorism policy. Previous studies have qualitatively evaluated the effect of government intervention on domestic terrorism or calculated policy impacts without modeling interaction among terrorism series.

I find that passive policies enacted to prevent illegal activity have instead increased incidents across animal rights and environmental movements. Activists respond to incentives and increasingly substitute resources toward relatively cheaper environmental direct action that is not formally covered by animal enterprise legislation. Proactive federal operations reduce illegal actions, but these gains pale in comparison to unintended spikes in attack after passive interventions. Criminalizing a broader spectrum of attack types, passive policies have also incentivized activists to substitute toward low-scale actions to avoid detection and elevated prison sentences. With increased lobbying to protect industry and research interests, each successive legislative act may impart the belief that illegal actions have been successful at intimidating and causing economic harm to targeted entities, thereby escalating the number of incidents over time.

While the economic severity of each attack has steadily decreased, activists tripled the use of deadly explosives and incendiary devices after passive legislation. Incidents against people have increased over sixfold in the long run after primarily attacking property in the past. These actions target not only researchers engaged in animal experimentation and GMO testing, but individuals affiliated with companies and universities that oppose their ideological agenda.

Although single-issue terrorism has declined in the last decade, violent actions are cyclical and the government continues to bolster counterterrorism efforts. Unintended consequences of government intervention should give policymakers pause before future

legislative changes. Supported by the rational choice framework, passive policies have incentivized activists to increase illegal actions and substitute toward violent forms of attack. After forty years of mainstream campaigns and marginal direct action impacts, activists have intensified attacks against individuals and tertiary businesses to dissuade animal use and environmental exploitation. Complementing policies that increase costs for illegal actions, interventions should account for the substitutable nature of terrorism tactics and incentivize lawful forms of protest to prevent illegal activism.

2.9 Appendix

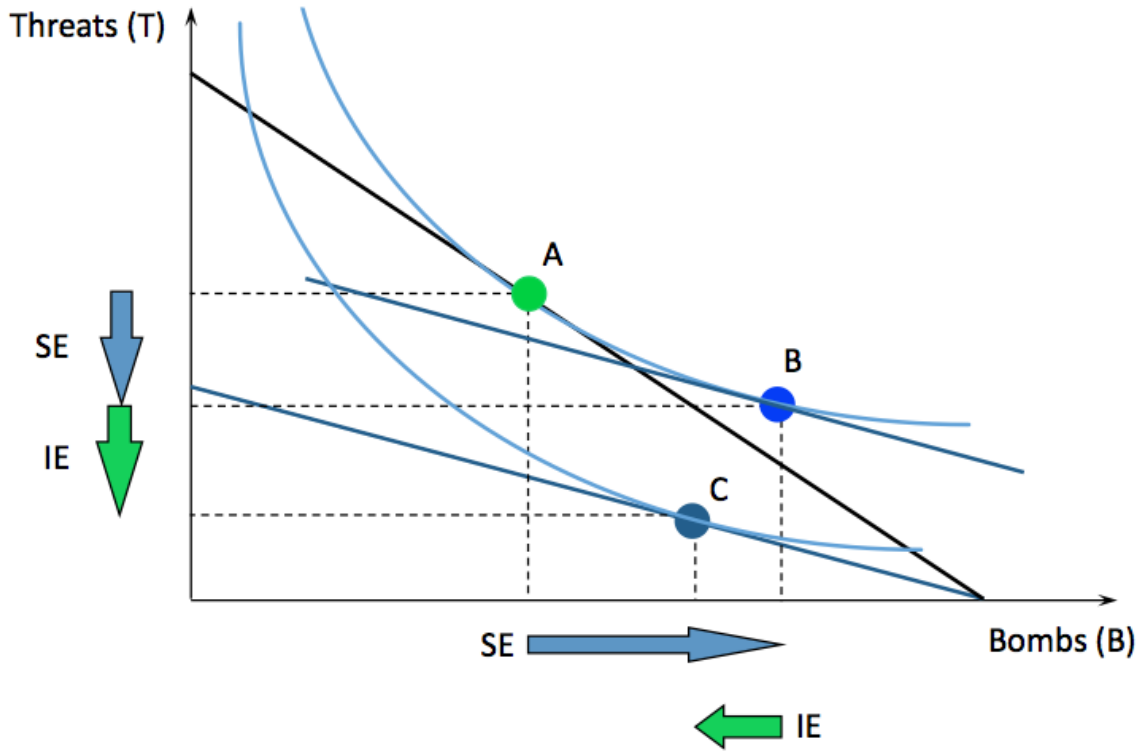

Figure 2.5: **Attack mode substitution (Threats: normal goods)**

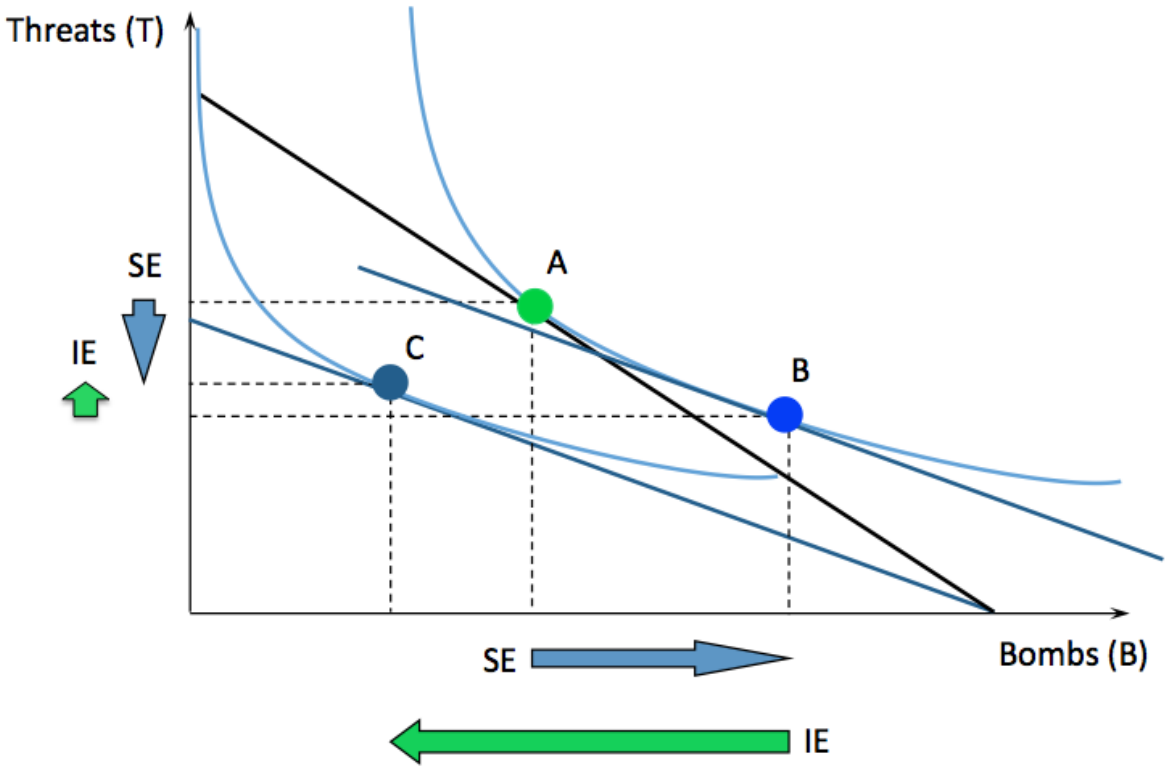

Figure 2.6: **Attack mode substitution (Threats: inferior goods)**

Chapter 3

Payments for Ecosystem Services and the Impact on Avoided Deforestation: A Meta-Analysis

Abstract: Payments for ecosystem services (PES) have complemented conservation efforts to avoid deforestation in developing nations. Alongside protected areas and sustainable forest management regulations, PES programs provide a market-based mechanism to compensate landowners and farmers for the environmental benefits they provide. While deforestation rates worldwide have stabilized, the impact and cost-effectiveness of PES programs have been questioned. Targeting counterfactual-based studies to identify additionality gains and minimize leakage impacts, I perform a meta-analysis to evaluate how PES program design and market factors impact avoided deforestation. Program design variables include contract length, payment differentiation, and participation targeting. Environmental variables proxy for opportunity costs by controlling for alternative land use prices and socioeconomic conditions. As each dimension has a varying impact on avoided deforestation, these results aim to influence future market-based interventions.

3.1 Introduction

Deforestation in developing nations contributes to soil erosion, flooding, and biodiversity loss, accounting for almost a fifth of net anthropogenic greenhouse gas emissions. Landowners increasingly clear private and public forests for soy and palm oil production, cattle ranching, logging, and road construction to spur economic development (IPCC, 2015). In an effort to conserve natural resources, payments for ecosystem services (PES) create a market-based mechanism to align private and social interests, compensating landowners and farmers for the environmental services they provide. As opposed to costly regulatory enforcement, PES is a voluntary agreement where payments are made from beneficiaries to individuals conditional on adequate ecosystem service provision (Engel et al., 2008). PES projects have expanded significantly under the United Nation's initiative for Reducing Emissions from Deforestation and Forest Degradation (REDD+), targeting climate change mitigation, improved watershed services, and biodiversity conservation.

Complementing protected areas, forest management, and law enforcement measures, the effectiveness of PES programs to avoid deforestation has been questioned in an effort to identify optimal approaches to forest conservation (Pattanayak et al., 2010; Samii et al., 2014; Börner et al., 2017). Beyond controlling for administrative difficulties, institutional barriers, and concurrent alternative forest conservation policies, voluntary PES programs suffer from potential adverse selection and leakage. Participants who would meet program requirements in the absence of payments often self select into PES programs, reducing program gains (i.e., additionality) and cost effectiveness. Negative spillovers can further diminish program gains. Leakage (or "slippage") occurs if deforestation is substituted away from enrolled parcels toward alternative land plots that would have otherwise re-

mained forested. Further leakage occurs when market prices increase for limited timber products, inducing other landowners to deforest marginal land.

While the PES literature has tackled mechanism design, theoretical benefits, and qualitative case studies, a growing number of impact evaluations employ counterfactual-based designs to identify causal PES gains in avoided deforestation. Börner et al. (2017) compare theoretical PES effects to empirical evidence and find that actual gains may lag behind forecasted impacts because of contextual factors (political and socio-economic conditions) and PES design (payment type, length, enforcement, etc). Samii et al. (2014) systematically review the empirical literature, concluding that PES programs reduce deforestation rates, however gains may be modest and program designs inefficient while providing little evidence of poverty alleviation for poor landowners.

Synthesizing the empirical findings in counterfactual-based PES studies, I perform a meta-analysis to identify how PES program design and market factors promote avoided deforestation in developing nations.[1] This report contributes to the literature by including a database of research through 2016, narrowing study inclusion to quantitative counterfactual-based assessments, and collecting new socioeconomic and market variables that may help explain the variation in PES program effectiveness.

3.2 Data

Surveying the literature for empirical studies on forest conservation and PES programs, articles in this meta-analysis are peer-reviewed articles and working papers sourced from Google Scholar, Webs of Science, and the NBER catalogue following the guidelines

[1] I follow the guidelines of Stanley et al. (2013) to systematically collect, code, and analyze results for this meta-analysis.

of Stanley et al. (2013). Although theoretical and qualitative sources were consulted for background research, this study specifically focused on the growing literature on counterfactual-based impact evaluation. Table 3.1 reports the twelve articles, published between 2007 and 2017, included in this study. While there is documented evidence that exclusively including published literature may bias meta-analysis results, peer-reviewed work may indicate a measure of quality necessary for the counterfactual-based studies and techniques this chapter analyzes.

Studies in this meta-analysis vary in many ways including intervention design, targeted country, scale, and importantly the econometric method used and outcome variable measured. The bulk of intervention studies have been in Mexico and Costa Rica given their historical forest conservation efforts. Only Jayachandran et al. (2016) look at a program not located in North or Central America, evaluating the impact of cash payments to forest-owning households in western Uganda. Jayachandran et al. (2016) is also the first study to employ a randomized control design to measure PES impacts while previous studies have used matching estimators and difference-in-difference methods. The database of twelve studies includes a total of 35 unique impact estimates, varying by matching method, filtering, and caliper choice.

Figure 3.1 reports both program design and market factors that may impact the effectiveness of PES programs. Although each study attempts to measure the same general phenomenon, the outcome variables collected include change in deforestation rates or forest cover over different periods of time. In an attempt to standardize results across all studies, the dependent variable Effect in Figure 3.1 measures the annualized rate of avoided deforestation relative to a counterfactual estimate (see Section 3.3 for details). The estimated effect size varies between 0 and 3 percentage points, with an average annu-

alized avoided deforestation rate of 0.95 percent. While average impact magnitudes are relatively low, PES programs have difficulty controlling for participation selection bias, leakage issues, and isolating effects alongside simultaneous forest conservation initiatives.

The many program design variables controlled for include: outcome variable measured (Method), a regional variable detecting scale of project, and country location. Specific to contracts signed between PES organizers and landowners, I also control for program and contract length, payment differentiation relative to the ecosystem benefits of enrolled forested land, and relative payments. Individual contracts for landowners last about four years before renewal, while actual PES programs can last up to twelve years in an area. Although absolute payments levels are important to incentivize participation, the relative payment variable accounts for a payment's magnitude relative to the per capita income in the region. Representing between 0.4 and 4.7 percent of the average per capita income, relative payments are generally low which may account for modest PES impacts. Additional variables control for PES intervention alongside concurrent conservation policies and whether studies target participation based on individual or land-based ecosystem characteristics.

In an effort to capture opportunity costs and market characteristics, this meta-analysis controls for average soy, timber, and cattle prices. Obtained from the IMF's commodity report, monthly nominal data was aggregated into yearly averages and adjusted for inflation.[2] Yearly inflation-adjusted averages were then averaged over program period length for each study. Increasing market prices for non-forest land use alternatives may disincentivize PES participation and contract compliance. Market leakage is also a concern if participants enroll agriculturally poor land areas while substitut-

[2]http://www.imf.org/external/np/res/commod/index.aspx

ing non-conservation activities to alternate parcels. Controlling for agriculture's average share of GDP and annual deforestation present additional proxies for forest conservation pressures.[3] Ranging from a modest 3 percent to over a quarter of a nation's annual GDP, agricultural economies present a challenge to successful forest conservation efforts. Plagued by high levels of historical land clearing and timber production, average deforestation rates are relatively low at about one percent per year. Lastly, unemployment rates further control for opportunity costs for landowners and illegal deforesters.

3.3 Methodology

Synthesizing empirical findings in the area of forest conservation, I employ the tools of meta-analysis to statistically evaluate the effectiveness of PES programs and its determinants. As these studies explore the impact of a range of economic and political variables, as well as distinct PES experimental designs, meta-analysis permits the identification of a standardized common effect using regression techniques (Nelson and Kennedy, 2009; Stanley et al., 2013; Stanley, 2013). Targeted forest conservation studies commonly identify effects in terms of either a proportion of fully forested parcels that have been deforested or by reporting deforested area sizes. To compare empirical results across the twelve studies in this meta-analysis, I employ a metric developed by Puyravaud (2003) and used by Samii et al. (2014) to standardize the annual rate of deforestation across studies. Intervention effect sizes on average annual avoided deforestation and the associated standards errors are calculated as:

$$\alpha = [\ln(\frac{FC_T}{FC_T - \Delta})/(t_2 - t_1)] * 100 \tag{3.1}$$

[3] www.data.worldbank.org

and

$$\gamma = se(\alpha) = [(FC_T - \Delta) * (t_2 - t_1)] * 100 \tag{3.2}$$

where FC_T is average forest coverage for treated parcels, Δ is the estimated treatment effect, and $(t_2 - t_1)$ represents the program period length.[4]

Relative to counterfactual deforestation rates, most studies report an effect size between 0 and 3 percentage points which complements findings in the literature (Samii et al. (2014) and Börner et al. (2017)). Breaking down the studies by targeted dependent variable, those that measure deforestation rates produce standardized effect sizes that range between 0 and 0.8 percentage points ($\mu = 0.20, \sigma = 0.22$) while studies that focus on forest cover changes yield effect sizes that range between 0.7 and 3 percentage points ($\mu = 1.81, \sigma = 0.90$). The small effect of forest conservation programs may be due to a number of factors including simultaneous intervention programs, selection bias in regions not immediately in threat of deforestation, and market leakage. Although standardized annual effect sizes are low, ongoing research has investigated the cost-effectiveness of PES programs and seeks to identify the biodiversity benefits, increased ecosystem services, and heterogeneous impacts relative to the size of enrolled areas.

[4]When estimated treatment effects are reported as changes in deforestation rates as opposed to changes in forest cover area, FC_T in equations 3.1 and 3.2 is replaced with $P_a = \frac{C_a}{A}$, where C_a is actual post-treatment forest cover, A is the sum and size of treated parcel areas, and $\frac{C_a}{A}$ represents the average actual post-treatment forest cover proportion in treated areas. If a study fails to report forest cover data, values are imputed from other descriptive statistics and treatment parcel deforestation rates or obtained from the authors.

Table 3.1: Summary of PES studies in meta-analysis

Study (Year)	NObs	Outcome Variable	Country	Scale	Method
Alix-Garcia et al. (2012)	3	Defor Rate	Mexico	National	Matching
Arriagada et al. (2012)	4	Forest Cover	Costa Rica	Regional	Matching & DiD
Baylis et al. (2012)	8	Defor Rate	Mexico	Regional	Matching & DiD
Costedoat et al. (2015)	3	Forest Cover	Mexico	Regional	Matching
Honey-Rosés et al. (2011)	2	Forest Cover	Mexico	Regional	Matching & DiD
Jayachandran et al. (2016)	2	Forest Cover	Uganda	Regional	RCTs
Robalino et al. (2008)	2	Defor Rate	Costa Rica	National	Matching
Robalino and Pfaff (2013)	4	Defor Rate	Costa Rica	National	Matching
Robalino et al. (2015)	2	Defor Rate	Costa Rica	National	Matching
Sánchez-Azofefia et al. (2007)	1	Defor Rate	Costa Rica	National	Regression
Scullion et al. (2011)	2	Forest Cover	Mexico	Regional	DiD
Sims and Alix-Garcia (2017)	2	Defor Rate	Mexico	National	Matching

Category	Variable	Description	Units	μ	σ	Min	Max
Dependent Variable	Effect	Annual rate of avoided deforestation (AD)	%/year	0.95	1.29	0	3.1
Project Design & Location	Method	Forest Cover==1, Defor Rate==0	Dummy	0.37	0.49	0	1
	Regional	If not regional, then national study	Dummy	0.60	0.50	0	1
	Mexico	Country location	Dummy	0.57	0.50	0	1
	CostaRica	Country location	Dummy	0.37	0.49	0	1
	DiffPay	Payment differentiation	Dummy	0.49	0.51	0	1
	RelPay	(Avg payment /ha/year)/(Per capita GDP)	%	0.98	0.99	0.38	4.69
	Program	Program length	years	8.06	1.89	2	12
	Contract	Contract length	years	3.69	1.86	1	5
	PolicyExisting	Concurrent conservation programs	Dummy	0.86	0.36	0	1
	Targeting	Targeting based on individual or parcel characteristics	Dummy	0.71	0.46	0	1
Socioeconomic/Market	Soy	Avg market soybean price	US$/MT	309.17	64.38	252.26	488.09
	Timber	Avg market timber price	US$/$m^3$	550.58	19.18	513.65	577.62
	Beef	Avg market beef price	US$/lb	1.25	0.16	1.10	1.76
	Cattle	Avg market cattle price	US$/cattle	96.38	6.44	87.27	114.43
	AgGDPShare	Avg percent of GDP in agriculture	%	7.72	6.12	3.39	26.98
	AnnualDefor	Annual deforestation rate	%/year	1.23	1.64	0.03	5.83
	Unemployment	Average yearly level	%	4.57	0.99	2.65	6.13

Figure 3.1: Meta-analysis Variables

3.4 Empirical Results

Estimation results in Table 3.2 present the impact of both PES program design and market variables on the effectiveness of payments for ecosystem services to avoid deforestation. Model 1 exclusively looks at the impact of PES program design on avoided deforestation. Increasing contract lengths and relative payments improve the ability of PES programs to decrease deforestation. With a steep learning curve from contract agreement to implementation for landowners, increasing both contract duration and payments may allow participants time to learn best practices and develop relationships with the surveyors to boost forest conservation. Increasingly lucrative payments relative to the average annual per capita income unsurprisingly induces higher forest protection. Containing multiple phases and participation cohorts, extending PES program length may unfortunately attract non-optimal landowners who self-select into the program and fail to generate additional avoided deforestation. Targeting participation based on individual parcel characteristics may combat this negative self-selection, increasing ecosystem benefits alongside payment differentiation among landowners with environmentally valuable land holdings. Although simultaneous forest conservation measures may negate one another, I find that existing policies and programs in and around the study area have a positive impact on avoided deforestation. These positive spillovers may result from successful landowner experiences and informational transfers. Contrary to previous evidence, I find that a regional program focus reduces the measured impact as these programs may not control for leakage effects.

Focusing on market forces and alternative land uses, Model 2 explores the role of opportunity costs on successful avoided deforestation projects. As close substitutes to payments for forest conservation, increases in the price of soy, timber, and cattle products

decrease the amount of forest preserved. Primarily subsistence farmers, an increase in potential income increases the likelihood of deforestation in the studied areas. Looking at the magnitudes of these three drivers for deforestation reveals the relatively sizable impact soy and cattle price fluctuations have on forest loss. Beyond commodity prices, high unemployment rates also reduce avoided deforestation in PES programs. Anecdotal evidence suggests that reduced labor force opportunities may spur illegal deforestation and increased market leakage as citizens exploit local natural resources to supplement their income during times of economic hardship. A higher level of average annual deforestation is found to increase the gains of avoided deforestation, possibly a result of increased efforts for conservation during times of environmental hazard.

Model 3 incorporates both program design characteristics and market forces to evaluate their impact on avoided deforestation. Most explanatory variables were found to be robust in their sign and significance of impact across model specifications. I find that relative pay and differential pay that targets higher payments for increased levels of ecosystem services (carbon storage, biodiversity, etc) exhibit a marked increase in magnitude relative to Model 1. These findings are consistent with the literature and reflect the growing propensity to increase payments in PES programs by making higher payments contingent on land characteristics. Among the proxy variables for opportunity costs, rising beef and timber costs continue to deflate avoided deforestation efforts, while increasing soy prices non-significantly impact the estimated effect.

Table 3.2: Meta-Regression Results, Depen Var: Effect

	Model 1	Model 2	Model 3
	b/se	b/se	b/se
Method	0.926***		0.926***
	(0.00)		(0.00)
Regional	-0.133***		-0.107
	(0.04)		(0.11)
Mexico	1.444***		1.562***
	(0.01)		(0.03)
DifferentialPay	1.081***		3.090**
	(0.01)		(1.23)
RelativePay	0.462***		1.337*
	(0.01)		(0.69)
ProgramLength	-0.547***		-0.458*
	(0.01)		(0.25)
Contract	0.456***		0.433***
	(0.00)		(0.04)
ExistingPolicy	1.595***		1.341***
	(0.02)		(0.13)
Targeting	0.306***		0.624
	(0.05)		(2.90)
Soy		-0.105***	-0.004
		(0.00)	(0.02)
Timber		-0.019***	-0.027***
		(0.01)	(0.00)
Cattle		-0.188***	-0.148***
		(0.06)	(0.02)
AgGDPShare		0.114*	0.106
		(0.06)	(0.20)
AnnualDefor		0.589***	0.623
		(0.10)	(0.79)
Unemployment		-1.433***	-2.391**
		(0.31)	(1.17)
Year Dummy	yes	yes	yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors, in parentheses, are clustered by study.

3.5 Conclusion

As governments and international programs allocate funds to target poverty alleviation and forest conservation, this study has aimed to calculate the net impact of payments for ecosystem service projects and its determinants. In an attempt to incentivize ecosystem service production, field researchers must design payment programs to be both cost-effective and impactful. Over the past decade, a growing body of literature has incorporated counterfactual-based measures to evaluate PES programs beyond qualitative assessments and anecdotal evidence. Surveying these rigorous studies, this study's meta-analysis serves to test both program design features and economic variables that characterize successful avoided deforestation projects.

I find that PES program design and exogenous market factors can significantly impact the effectiveness of payment schemes to conserve forests. Targeting payments toward landowners who provide higher ecosystem services improves the additionality of a project. Payment differentiation enables the project to capture both quality from highly valuable land reserves and quantity from cost-effective low-yield participants. Longer contracts with higher relative payments further improve the ability of PES programs to decrease deforestation by supplying both time to effectively integrate best practices and funds to substitute for alternative land uses. PES project administrators may also be able to magnify the positive impact of payment programs if offered alongside other forest conservation efforts that produce information spillovers and trust in the targeted community. Along the market factors dimension, commodity prices from timber, soy, and beef markets proxy for opportunity costs in PES programs. Robust across specification strategies, increasing timber and cattle prices induce participants to renege on contractual agreements and market leakage to deflate any avoided deforestation gains. In lieu

of controlling commodity market prices, field researchers may use results of this analysis to construct state-contingent payment plans that increase payments when market forces reduce PES participation and compliance. Increased unemployment may further strain forest cover protection as natural resources are commonly overexploited to substitute for lost wages.

Although estimated avoided deforestation impacts are small, in comparable contexts these programs may be as or more cost-effective compared to alternative forest conservation efforts. Addressing controllable design features and exogenous market variables that impact the effectiveness of PES programs can inform future projects and help identify optimal strategies for forest conservation.

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