Winners, Losers, and Losers Who Might Be Winners

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Degree

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I dedicate this dissertation to my wife, Katie.

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And everything was... pretty okay

— *Jake Ewald*

Abstract

Chapter 2 evaluates whether winning a subsidy auction improves local economic outcomes. I construct a county-level dataset that links subsidy winners from Good Jobs First with runner-up locations identified through Site Selection magazine. I estimate difference-in-differences models that compare winners, runners-up, and nearby counties using pre-treatment matching and fixed effects. Governments offer large incentives to attract firms, yet the effects remain unclear. I find little evidence that subsidies affect employment or establishments at the county level. Effects on business activity differ across winners, runners-up, and nearby counties, which suggests spatial reallocation rather than net growth.

Chapter 3 develops a model of a generalized divide-and-choose mechanism in which the youngest agent divides an endowment into n bundles and others choose sequentially. We characterize equilibrium behavior under alternative informational environments. With private preferences, maxmin reasoning leads the divider to choose an equal split, which is envy-free but not generally efficient unless preferences are homogeneous. With perfect information and heterogeneous preferences, backward induction produces allocations that satisfy a weaker, directional notion of fairness and may be inefficient. The sequential structure also creates scope for collusion between early movers.

Chapter 4 provides a laboratory test of this mechanism in three-person groups using controlled variations in communication and transfers. In treatments without transfers, outcomes concentrate at the efficient, envy-free benchmark. Allowing

communication and voluntary transfers enables coalition formation and generates some collusion between early movers. These outcomes shift payoffs toward the divider and first chooser, reduce efficiency, and disproportionately harm the second chooser. Collusion remains incomplete, which indicates limits to coordination and enforcement.

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Chapter 1

Introduction

This dissertation spans two distinct fields of economics: public economics and fair division. Although the chapters draw on different literatures and methodologies, they are unified by a common theme—the equitable allocation of resources. Each chapter examines this idea from a different perspective, ranging from empirical analysis of government policy to theoretical and experimental studies of fairness in strategic environments.

The first chapter examines questions in public economics by studying the local effects of firm-specific subsidies. Governments frequently offer large incentives to attract firms, expecting that these investments will generate employment growth and broader economic spillovers. However, the true consequences of these policies remain uncertain. When firms announce relocation searches, local governments often engage in competitive bidding wars, creating both winners and runners-up. This chapter analyzes the outcomes for both groups of counties, with particular attention to the runners-up, whose costly participation raises questions about the efficiency of subsidy competitions. Using data from Site Selection magazine and Good Jobs First, I construct a county-level dataset and estimate difference-in-differences models to evaluate changes in business activity and employment. The results suggest that winning a subsidy does not generate the anticipated positive spillovers and may

even reduce business activity, while runner-up counties experience gains among small businesses. Overall employment effects appear negligible. These findings contribute to the growing literature on place-based policies and highlight potential unintended consequences of targeted economic development incentives.

The second chapter turns to fair division theory through a historical lens, presenting a theoretical analysis of a Welsh inheritance practice known as gavelkind. Under this mechanism, an estate is divided into equal portions by the youngest heir, while the eldest chooses first, followed sequentially by the remaining heirs until the divider receives the final parcel. This procedure generalizes the classic divide-and-choose mechanism and raises important questions about fairness, strategic behavior, and efficiency. I model the game induced by this inheritance rule and examine its properties under maxmin, strategic, and collusive behavior. Although the mechanism does not always produce efficient allocations, it generates outcomes that satisfy a directional form of envy-freeness. At the same time, the procedure is vulnerable to collusion, allowing certain players to benefit at the expense of others. By formally characterizing a historical institution that predates modern mathematical treatments of fair division by nearly a millennium, this chapter contributes to both the theoretical and historical literature on equitable allocation.

The third chapter builds on the theoretical framework by testing the Welsh inheritance mechanism in a controlled laboratory experiment. While the model predicts strategic behavior, real-world decision-making may deviate from theoretical equilibrium. The experiment introduces treatments that allow for communication (cheap talk) and ex-post transfers to examine whether participants engage in collusion, strategic play, or non-standard behavior. The results provide insight into how individuals exploit positional advantages and whether increased transparency mitigates or amplifies fairness concerns. By bridging theory and practice, this chapter highlights the practical challenges of implementing fair division mechanisms and explores their potential applications in settings such as business dissolution or multi-party financial disputes.

Although each chapter stands independently within its respective field, together they offer a broader perspective on fairness in economic decision-making. The first chapter contributes empirical evidence on public policy and resource allocation through government intervention, while the second and third chapters advance theoretical and experimental research on fair division. Collectively, these studies deepen our understanding of how resources are distributed across strategic, institutional, and policy contexts. Further details on each study are provided in the chapters that follow.

Chapter 2

Playing Favorites: The Impact of Subsidy Auctions on Business Activity

2.1 Introduction

Place-based policies are widely used tools by state and local governments to promote regional economic development.[1] These policies are designed to generate job growth in targeted areas either to stabilize struggling economies or to stimulate further expansion. Common examples include tax credits and reductions in corporate tax rates, which extend benefits to a broad set of firms meeting specified eligibility requirements. In contrast, governments also use firm-specific subsidies. These subsidies are incentive packages offered to a single company through an informal auction process. These packages may include cash grants, tax abatements, or land transfers in exchange for the firm's commitment to establish a large-scale facility, often referred to as a Million Dollar Plant (MDP). Policymakers justify these subsidies on

[1] See Bartik (1991), Bartik (2018), Bartik (2020), Black and Hoyt (1989), and Glaeser and Gottlieb (2009) for broader discussions of place-based policies.

the grounds that the incoming firm will generate local spillovers sufficient to offset the public cost.

Firm-specific subsidies are particularly vulnerable to criticism compared to other place-based policies due to the lack of transparency in government decisions and firm behavior. It is often unclear why governments choose to bid for certain firms but not others. These choices require policymakers to anticipate a firm's long-run performance and its capacity to generate substantial local spillovers. While some factors that make a location attractive are well understood, firms' preferences are largely undisclosed, as revealing this information could disadvantage them in competition with rivals also searching for a new site. This opacity heightens concerns about whether scarce public funds are allocated efficiently. A central question, therefore, is whether the economic gains from securing a subsidy are sufficiently large to offset these concerns; or whether those resources might have generated greater returns if invested elsewhere.

This study is motivated by two key papers on Million Dollar Plants (MDPs). Greenstone et al. (2010) provide the first systematic analysis of spillovers from MDPs. They compare counties that won discretionary subsidies with runners-up, using a difference-in-differences framework. The empirical strategy is based on the idea that endogeneity of firm preferences can be resolved within an auction. Their findings suggest that new manufacturing plants generate substantial productivity spillovers, providing empirical support for the rationale behind these subsidies. In contrast, Patrick (2016) develops an alternative identification strategy based on causal inference techniques. This study uses propensity score and covariate matching to construct weighted difference-in-differences estimates. Without relying on runner-up counties, this study finds that MDPs are associated with a decrease in firm establishments and an increase in employment and earnings per worker. These studies provide the foundation for empirical analysis of subsidy auctions and the relocation of a MDP. Yet important gaps remain. First, little is known about heterogeneous effects from MDPs, specifically in regards to effects on small or large firms. Heterogeneous effects could alter the government's "portfolio" of development strategies and generate

unintended consequences. Second, no study has examined the outcomes for runner-up counties, which continue to pursue economic development despite losing the auction. Third, no paper has studied the spillover effects on nearby counties. These counties are exogenous to the subsidy and may have unintended consequences. Understanding these effects is important given that governments typically finance subsidies through special economic development funds,[2] direction appropriations, or foregone tax revenue, all of which carry opportunity costs.

I investigate the consequences of subsidy auction outcomes on local business activity. Key outcomes include establishments, firms, and employment in three categories: all firms, small firms, and large firms. Other outcomes include personal income and county-level GDP. Small firms are important for stability and growth of an economy. If the effects are disproportionate between small and large firms, policymakers should be informed when making a decision. On one hand if the subsidies benefit larger firms, there may be efficiency gains through economies of scale but limit future opportunities for smaller firms. On the other hand if the subsidies benefit smaller firms, this may enhance competition, entrepreneurial dynamism, and local employment. Recognizing these trade-offs ensures that policy design balances growth objectives and aligns subsidy programs with broader goals of inclusive and sustainable economic development. They require substantial resources from both firms and governments. Governments in particular curate incentive packages to attract the firm. If the economic returns to winning are limited, these auctions can be a costly misallocation. Further, I consider the impacts on nearby counties. It is unlikely these spillover effects are concentrated to one county. There may be migratory employment effects in which nearby counties lose employment. And lastly, the runner-up counties still search for economic growth or stability. Analysis of runner-up counties highlights the unintended consequences of participating in the auction.

[2]For example, Georgia uses the state-funded Regional Economic Business Assistant (REBA) program.

Small and large firms likely experience these auction outcomes differently, with small businesses bearing a greater influence on local innovation and job creation (Acs and Audretsch (1990), Acs (1992), Brock and Evans (1989) , Van Praag (2003)). There is substantial literature highlighting the role of credit and capital constraints in the entry and survival of small firms, as well as their critical contributions to economic growth and employment (Dunne et al. (1988), Dunne et al. (1989), Evans and Leighton (1989), and Holtz-Eakin et al. (1994)). Subsidy auctions may create disproportionate impacts; large subsidies to multinational corporations may crowd out smaller firms or drive up their costs. These effects also may spillover into nearby counties through various channels.

Counties that lose subsidy competitions may also be affected by the auction process. Runner-up counties could face short-run disruptions if they temporarily commit fiscal resources and administrative capacity in anticipation of securing an MDP, only to redirect those efforts after losing the bid. This holdup mechanism may lead to temporary declines in business activity that later dissipate once plans adjust. Conversely, if megadeals impose negative externalities or crowd out local investment in winning counties, runner-up counties could benefit from avoiding these costs. A third possibility is that losing counties experience no measurable change at all. If both winners and runners-up exhibit null effects across key business outcomes, the rationale for engaging in costly subsidy competitions would become increasingly difficult to justify and raises broader questions about the effectiveness of the subsidy process.

The analysis draws on a hand-collected dataset that combines two primary sources: *Good Jobs First* and *Site Selection* magazine. Good Jobs First provides detailed information on subsidy recipients but is limited to auction winners. To capture runner-up counties, I systematically collect unstructured data from *Site Selection*, drawing on its monthly updates and search features to identify finalists corresponding to the winners listed in Good Jobs First. I compare each to geographically matched surrounding counties in order to assess the broader effects of subsidy allocation. The

analysis focuses on key business dynamics before focusing on the dynamic effects of the same outcomes.

I make three main contributions to the literature with this paper. First, I construct an updated dataset that enables an analysis using modern causal inference techniques. Prior studies largely rely on cases from the 1980s and 1990s, whereas my dataset extends the evidence to more recent decades. Second, this is the first study to examine runner-up county outcomes after failing to secure a subsidy. In nearly all cases, runner-up counties do not reappear in subsequent auctions because the process spans several years. This highlights the need for governments to prepare contingency plans in the event of a loss. If losing counties are better off than winning counties, the policy implications are substantial by calling into question the wisdom of bidding aggressively for MDPs. Third, I analyze the spillover effects of subsidy awards on neighboring counties, providing the first systematic evidence of these unexpected consequences. If neighboring counties are significantly worse off when a Million Dollar Plant is awarded nearby, the results offer a strong caution to policymakers considering whether to participate in future subsidy auctions.

I provide institutional details in section 2. In section 3, I discuss the empirical strategy and the data collection process in section 4. I present the main results in section 5 and follow with heterogeneous results and event studies in sections 6 and 7. I test the models for robustness in section 8 and conclude with a discussion in section 9.

2.2 Institutional Background

The site selection process begins when a firm announces its intent to establish a new manufacturing facility or corporate headquarters. To manage the process, the firm often contracts with a site selection agency, which issues a public announcement soliciting bids from state and local governments. Consultants then engage with

state and local officials, including representatives from state development agencies, to evaluate potential locations.

The process unfolds in stages. Initially, governments submit proposals outlining the incentives they are willing to offer. The packages frequently include property and income tax abatements, targeted tax credits, reduced training costs for employees, and more. Governments also specify expectations regarding job creation and capital investment requirements. As the process advances, the site selection agency and firm narrow the list of candidates. Governments may revise or increase their bids in an effort to remain competitive. Once a small group of finalists is identified, firms conduct site visits and detailed evaluations before making a final decision.

The objective of state and local governments can be viewed as maximizing the welfare of workers, capital owners, and the government itself. Subsidized firms create new jobs by hiring local workers, but the net benefits depend on the region's initial employment conditions. An increase in local labor demand can raise wages, but workers may also bear costs through higher taxes or reductions in public services required to finance the subsidy. Governments, in turn, benefit from economic growth and stabilization, yet must balance these gains against the fiscal strain of foregone revenues or increased expenditures. While the subsidized firm is the direct beneficiary of the incentive package, other firms may be indirectly affected through productivity and demand spillovers. These effects are uneven. Larger firms may be able to adapt more easily than smaller firms due to less rigid capital constraints in larger firms. All firms may face higher input costs, declining demand, or heavier tax burdens. In some cases, firms may contract or exit the market.

Firms pursue their own strategic objectives when deciding where to locate. Subsidy auctions are structured as scoring contests in which the size of the bid is only one component of the decision. In practice, firms prioritize efficiency and practicality, giving weight to factors such as the composition of the labor market, demographic characteristics, and the presence of existing firms in the region. The subsidized firm seeks a location with both high-quality workers and limited competition for labor. At

the same time, firms may also consider broader business-climate factors. One major factor is if the firm can monopolize or have access to nearby suppliers. As a result, I include employment percentage based on NAICS codes as matching and variables.

Recent evidence highlights additional nuances in this process. Patrick (2016) draws on primary documents to find other relevant factors for a firm's selection process. They find that firms tend to favor specific geographic locations, creating systematic biases in site selection outcomes. The documents imply that losing counties are not appropriate counterfactuals for winners, as the two groups may be systematically different. The documents also reveal the runners-up were never in true contention for the MDP. Despite ultimately losing, the runners-up may behave like a second treatment group rather than a true never treated group. The runners-up invest heavily in the bidding process through financial commitments, administrative capacity, and community planning to signal a strong intent to attract the firm. As a result, the auction process may shape local development trajectories, generating economic consequences for both winners and runners-up.

2.3 Empirical Strategy

The empirical strategy considers three main treatment groups: winners, runners-up, and nearby. Winning counties are counties that win the subsidy auction and receive an MDP. Runners-up are counties that participate in the auction but lose in the final stage. Nearby counties are counties within 50 miles of the winning county. Separate regressions are estimated for each treatment. In an ideal setting, counties would be randomly assigned to treatment or control groups; however, endogeneity arises because firms' preferences influence which counties become winners, runners-up, or remain untreated. The primary sources from Patrick (2016) reveal that firms exhibit strong regional preferences when relocating. For example, a winning county

may be in the Southeast while the public runner-up is in the Midwest.[3] To produce consistent estimates, I begin by restricting my counterfactual counties to be located within 50-150 miles of the treatment group. All distances are measured from the centroid of each county. This captures the regional preference of the firm and some unobservable labor market characteristics. I use nearest neighbor covariate matching on time-variant characteristics that influence the selection of the treatment group. The nearest neighbor matching is implemented using the Mahalanobis distance metric over the selected covariates. After matching and differencing out the unobservables, the estimator's performance increases as shown in previous literature. The matching process occurs three years prior to treatment. Three years reflects the length of these subsidy auctions. I generate weights from the nearest neighbor covariate match. I then use these weights as frequency weights in a difference-in-difference model. I apply the same approach for all treatments. This matching technique comes from the treatment effects literature (see Rubin and Thomas (1992), Heckman and Todd (2009), List et al. (2003), Drake (1993), and Dehejia and Wahba (2002)). The matching, even if imprecise, reduces bias and more accurate results.[4]

I estimate the effects of subsidy auctions using a difference-in-differences framework:

$$\begin{aligned} Y_{kjt} = \delta \mathbf{1}(\text{Treatment})_{kj} + \kappa \mathbf{1}(\text{Post})_{tj} + \theta\big[\mathbf{1}(\text{Treatment})_{kj} \times \mathbf{1}(\text{Post})_{tj}\big] \\ + c_k + \mu_t + \lambda_j + \varepsilon_{kjt}. \end{aligned} \tag{2.1}$$

This specification represents a difference-in-differences model with staggered treatment adoption, designed to estimate the impact of a treatment that may represent either a "Winner," a "Runner-up," or a "Nearby County" depending on the specification. The outcome variable Y_{kjt} is observed for each county k in each year t and case j. Each outcome variable is measured in terms of 1,000 per capita,

[3]This discrepancy is likely from consultants leveraging the firm's power to receive a higher bid from the winning county.

[4]For a further detailed explanation, see Patrick (2016).

e.g., 1,000 establishments per capita.[5] $1(Treatment)_{ckj}$ is an indicator for being located in a treated county, and $Post_{jt}$ is an indicator for being the year of the announcement.[6] The model includes county-level fixed effects α_k to control for time-invariant county-specific characteristics, and year fixed effects μ_t to account for common time shocks. The model also includes case fixed effects λ_j which are for each auction. Covariates at the county and state levels $\mathbf{X}_{kjt}$ are included to control for other influencing factors, while the error term ϵ_{kjt} captures unexplained variation. The nearest-neighbor covariate matching uses the same covariates as the regression. The coefficient of interest in Equation 1 is θ. The standard errors are clustered at the state level due to statewide budget shocks that may affect counties within the state in a similar way.

I include additional restrictions on my data that differ from previous papers in this literature. I restrict the data to prevent runners-up from acting as a counterfactual for the winners and vice-versa. I also remove the following states from my dataset: California, Florida, Michigan, New York, and Texas. There are several subsidies from these states which are statewide or do not properly identify the treated county.[7] I remove these states to prevent confounding results. If included, these states would have treated counties potentially acting as counterfactuals which could bias the coefficient of interest. In this scenario, it would likely bias the estimates towards zero.

[5]For log specification, see the appendix. The per capita specification is preferred to the log specification to capture real effects on the economy rather than gross gains or losses. An additional regression is included in the appendix to estimate the effect of these subsidies on population and a null effect is found.

[6]I employ a binary indicator for treatment rather than a continuous measure of subsidy intensity because detailed information on bid amounts is not systematically observed for all participants, particularly runner-up locations. This limits the ability to construct a consistent measure of treatment "dosage" across treated and comparison groups. Moreover, even when subsidy values are reported, they are often incomplete, non-comparable across deals, or reflect negotiated packages with heterogeneous components (e.g., tax abatements, infrastructure investments, and discretionary grants) that are difficult to aggregate into a single continuous metric. A binary indicator therefore provides a more reliable and consistently defined measure of exposure to a subsidy win, aligning with the available data and reducing measurement error that would arise from attempting to impose a noisy continuous treatment variable.

[7]For example, Ford Motor received a statewide subsidy in 2010 to expand in various counties.

2.3.1 Runners-up

Runner-up counties are considered as a treatment group in this paper. Runners-up, by participating in the auction, are willing to allocate funds towards place-based policy. These counties, under a null effect, would simply revert to a baseline. However, it is a possibility that they re-optimize their local development strategies along several margins.

Runner-up counties may reallocate fiscal resources that were initially earmarked for the focal project. This can include redirecting subsidy dollars toward smaller firms, expanding tax abatements across a broader set of businesses or increasing spending on business development programs. In this sense, the "treatment" shifts from a large single subsidy to a portfolio of smaller interventions.

Counties may adjust their targeting and recruitment strategies. Rather than competing for large, nationally mobile firms, they may shift toward regionally anchored businesses, suppliers, or mid-sized establishments. This may diffuse growth, particularly among smaller firms, rather than concentrated employment effects with a single large investment.

Further, the bidding process itself generates information and signaling effects. Participation reveals information about local capacity such as workforce characteristics, land, and willingness to subsidize. This signal may improve future matching with firms.

Taken together, these mechanisms imply that runner-up counties experience a form of treatment that is different from both winners and never-treated counties. Their outcomes reflect the combined efforts of foregone large-scale investment and the reallocation of resources across alternative development strategies. As such, treating runners-up as their own group allows for a more accurate characterization of how local economies respond to participation in subsidy competitions.[8]

[8]In future work, I will add government spending variables as metrics to determine whether or not governments realign their fiscal policy. This is likely a lagged effect if any is found because the runners-up truly believe they are in contention to win the subsidy and cannot make adjustments quickly.

2.3.2 Information Asymmetries

Information asymmetries are central to the site selection process and introduce an important source of endogeneity. Firms and their site selection consultants possess private information about true location preferences, production needs, and the relative value of different site characteristics, while bidding counties must formulate offers under incomplete information. As a result, the observed set of finalists does not necessarily reflect the firm's true margin of indifference. Instead, consultants may strategically include locations that are not close substitutes in order to intensify competition and extract higher subsidy offers from preferred jurisdictions. This implies that publicly identified runners-up may be geographically or economically distinct from the winning county, even if they appear comparable in the bidding process. Consequently, treatment assignment reflects both underlying firm preferences and strategic behavior within the auction, complicating the interpretation of observed outcomes.

2.4 Data

The primary data source for discretionary subsidies is *Good Jobs First* (2023), a nonprofit organization that compiles information on economic development programs and firm-specific incentives. The database classifies subsidies as "megadeals" if they are discretionary firm-specific subsidies. This category is the focus of the study. These data are based on official disclosures from state and local governments and provide detailed breakdowns of subsidy components, including allocations to job development programs, infrastructure development, county and property tax abatements, cash, and land transfers. The dataset also reports the number of jobs and capital investment promised by the firm.[9] While *Good Jobs First* offers comprehensive coverage of

[9]Most of this information is currently available as unstructured text; I plan to extract it and process it for future research.

subsidy recipients and winning counties, it does not contain information on the counties that are the runners-up.

To capture data on runner-up counties, I construct a novel dataset from *Site Selection*, a corporate real estate magazine that documents major site selection processes. I search all issues from 2000 to 2023 and match the magazine's coverage of site selection finalists to the auction winners listed in *Good Jobs First*. This process ensures that I identify cases where runner-up counties are reported. Some counties appear as both winners and runners-up across different deals (e.g., Huntsville, Alabama won a bid for Airbus but lost a bid for Volkswagen). To maintain clean treatment comparisons, I exclude such cases and further restrict the analysis to counties that are either one-time winners or one-time losers. It remains unclear how repeat winners or winners who turn into runners-up at a later date would create inconsistencies in the analysis. One possibility is a multiplier effect, where benefits compound. A competing hypothesis is diminishing marginal returns, such that the second auction yields a smaller effect. Either way, this would violate model assumptions of multiple treatments.

For outcome variables, I use the Business Dynamic Statistics (BDS) from the U.S. Census Bureau, which provides county-level measures of establishments, firms, and employment. These data are also available based on the firm size which is categorized as follows: Small Firms (1-24 employees), Medium Firms (25-499 employees), and Large Firms (500+ employees). I group small and medium firms to match the Small Business Association definition of small businesses which are firms with less than 500 employees. Large firms remain as any with 500 or more employees. Additional outcome variables include personal income and GDP measured as sums per county from the Bureau of Economic Analysis. The values of these variables are reported in terms of $2017 USD.

I also incorporate a range of covariates. Demographic characteristics include education, age, gender and minority composition, are obtained from the American Community Survey (ACS) via IPUMS (Ruggles et al., 2020) and aggregated to the

county level. I include the employment percentage in each one-digit NAICS code calculated from the U.S. Bureau of Labor Statistics' QCEW.[10]

Descriptive Statistics

The matching process is essential to the validity of my results. To ensure the covariate match is valid, I present balance tables for each treatment group.

I present descriptive statistics in Tables 2.1, 2.2, and 2.3. Table 2.1 compares the covariates of the winners to a matched sample of never-treated counties using nearest neighbor matching on covariates. The means of the covariates are quite similar and bias-reducing from an unweighted and unmatched never treated group. Table 2.2 provides the same procedure but compares runners-up to their own never-treated group. While there are some small issues with the variance, this approach ensures a more credible control group for both winners and runners-up. This method follows Patrick (2016)'s approach and is the best available method for determining the impacts of Million Dollar Plants and through the subsidy auction results.[11][12]

The balance tables indicate that some differences remain across groups despite the matching procedure. For winning counties, the observed differences run in opposing directions. Winners tend to have a lower average age and lower shares of employment in agriculture, both of which would tend to bias estimated treatment effects upward. In contrast, winners also exhibit lower average education, which would tend to bias estimates downward. Although the magnitudes of these differences are small, several are statistically significant, suggesting that any remaining bias for winners likely nets to a modest upward bias in estimated effects.

For runner-up counties, the direction of bias is somewhat different. The most notable imbalance is in the manufacturing share of employment, which is substantially

[10] See appendix for Industry Codes.

[11] Note that there are less observations for the covariates than the treatment group in Tables 2.1 and 2.2. This is because the matching process uses a matching with replacement so the same county may be matched with more than one treated county. This method is accounted for by using frequency weights in the regressions.

[12] NAICS codes are explained in the Appendix.

higher among runner-up counties and would tend to bias estimates upward. At the same time, other covariates—such as slightly lower education—would generate downward bias. On net, the remaining imbalance suggests that negative estimated effects for runner-up counties should be interpreted cautiously, as they may partially reflect underlying differences rather than true treatment effects.

Nearby counties also exhibit residual imbalance. In particular, nearby counties tend to have lower average age, higher levels of education, larger populations, and greater employment shares in business, finance, and logistics services. These characteristics are typically associated with stronger economic outcomes, implying that estimated effects for nearby counties are likely biased upward. As a result, positive coefficients for nearby counties should be interpreted with caution.

It is important to note, however, that the comparisons between winners and runner-up counties, as well as between nearby winners and nearby runner-ups, do not rely on the matching procedure. Instead, these comparisons are based on the assumption that, conditional on reaching the final stage of the auction, the identity of the winning location is as good as randomly assigned. This framework mitigates concerns about systematic differences in observable characteristics across these groups.

Finally, the matching procedure itself plays a critical role in improving comparability. Without matching, the set of control counties would include a large number of very small and economically distinct counties, leading to substantially worse balance across covariates. These differences would introduce considerably more bias than what remains after matching. While matching does not fully eliminate imbalance, it meaningfully reduces it, and in the context of available data and institutional constraints, represents the most credible approach for constructing an appropriate comparison group. I provide balance tables for the covariates without matching in the appendix.

Table 2.1: Summary Statistics for Covariates of Winners and Never Treated

	Winners		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.16	0.14	0.17	0.14	0.62
Female (%)	0.51	0.01	0.51	0.01	0.88
Average Age (Years)	37.85	2.75	38.50	2.06	0.07*
Average Educ. (Years)	5.81	0.42	6.07	0.71	0.003***
Log Population	12.83	0.76	12.92	1.03	0.49
Agriculture Emp. (%)	0.14	0.25	0.27	0.39	0.01**
Utilities Emp. (%)	6.20	2.75	5.95	3.00	0.53
Manufacturing Emp. (%)	12.80	7.41	12.90	7.91	0.93
Trade Emp. (%)	20.64	4.31	19.95	3.76	0.24
Business, Finance, and Logistics Services Emp. (%)	22.74	8.15	22.76	7.71	0.99
Education & Health Services Emp. (%)	19.02	8.05	18.41	6.65	0.57
Leisure & Hospitality Emp. (%)	10.93	3.50	10.28	2.50	0.14
Other Emp. (%)	3.13	0.54	3.74	0.97	0.00001***
Observations	97		93		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.2: Summary Statistics for Covariates of Runners-up and Never Treated

	Runners-up		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.18	0.17	0.17	0.12	0.64
Female (%)	0.51	0.01	0.51	0.01	0.92
Average Age (Years)	38.15	1.71	38.40	2.18	0.36
Average Educ. (Years)	5.85	0.32	5.97	0.41	0.02**
Log Population	12.64	0.59	12.70	0.74	0.53
Agriculture Emp. (%)	0.34	0.44	0.06	0.10	0.000***
Utilities Emp. (%)	6.04	1.32	5.75	2.69	0.34
Manufacturing Emp. (%)	13.62	6.36	9.23	5.81	0.000***
Trade Emp. (%)	21.99	2.76	21.13	4.32	0.10*
Business, Finance, and Logistics Services Emp. (%)	20.65	4.77	26.18	9.58	0.000***
Education & Health Services Emp. (%)	18.96	6.38	19.04	5.26	0.93
Leisure & Hospitality Emp. (%)	10.88	1.91	9.89	2.71	0.003***
Other (%)	3.29	0.48	3.32	0.57	0.71
Observations	105		98		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: Summary Statistics for Covariates of Nearby and Never Treated

	Megadeal Nearby		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.16	0.16	0.12	0.13	0.0002***
Female (%)	0.51	0.01	0.51	0.01	0.78
Average Age (Years)	38.55	2.68	39.88	2.58	0.0000***
Average Educ. (Years)	5.87	0.39	5.78	0.37	0.0005***
Log Population	12.73	0.85	12.34	0.62	0.0000***
Agriculture Emp. (%)	0.24	0.39	0.35	0.73	0.004***
Utilities Emp. (%)	5.84	2.18	6.58	1.96	0.0000***
Manufacturing Emp. (%)	12.77	6.90	13.62	6.65	0.05*
Trade Emp. (%)	21.29	5.08	21.31	3.17	0.94
Business, Finance, and Logistics Services Emp. (%)	20.81	7.28	16.91	5.91	0.0000***
Education & Health Services Emp. (%)	20.55	6.83	20.59	5.06	0.91
Leisure & Hospitality Emp. (%)	10.70	2.72	11.48	5.33	0.01**
Other Emp. (%)	3.27	0.60	3.41	0.52	0.0003***
Observations	484		474		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.5 Results

I begin by examining the effects of winning the subsidy and receiving the MDP on key business outcomes. I follow the same exercise the runners-up and nearby counties of the auction.

Winners

Table 2.4 reports the per-capita effects for counties that won a discretionary subsidy and attracted a MDP. The most salient finding is the absence of statistically significant impacts on local business activity. This pattern stands in contrast to earlier work: Greenstone et al. (2010) report positive effects for winning counties relative to runners-up, while Patrick (2016) finds negative employment effects. Using updated data and the empirical framework from Patrick, I find no measurable impact on establishments, firms, or employment for winning counties. Given an average winning-county population of roughly 375,000 residents, these estimates rule out economically meaningful gains of more than roughly 185 establishments or 1,500 jobs, and similarly rule out losses larger than about 110 establishments or 1,600 jobs. In short, the

confidence intervals are narrow enough to exclude the type of large effects that are often claimed by policymakers and subsidy proponents.

Table 2.4: Winning County Main Results

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
MDP	0.148	0.134	-0.225	-1.205	-1.020
	(0.156)	(0.123)	(4.642)	(0.802)	(2.23)
Observations	3,574	3,574	3,574	3,574	3,574
R2	0.9946	0.9955	0.9890	0.9779	0.9738
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

A frequent argument in favor of large subsidies is that, even if the policy does not increase net employment, it may still raise job quality by upgrading the local wage structure or generating productivity spillovers. The evidence does not support this mechanism either. I find no significant effects on personal income or county-level GDP, suggesting that the arrival of an MDP does not translate into measurable gains in county-wide economic well-being. Together, these results point to a null effect of winning discretionary subsidy competitions on key measures of local economic activity and income, challenging the presumption that such programs generate broad-based development benefits for the communities that secure them.

2.5.1 Runners-up

The results for runner-up counties mirror those of the winning counties. Table 2.5 shows that counties that aggressively pursued but ultimately did not win an MDP also experienced no statistically significant changes in establishments, firms, or employment. The confidence intervals similarly rule out economically meaningful

gains or losses, indicating that these places did not materially diverge from the winners following the subsidy competition. These findings are consistent with two interpretive possibilities. First, losing counties may have redirected fiscal and policy efforts toward other development strategies, allowing them to remain on a similar economic trajectory as winners. Alternatively, these counties may not have required an intervention to achieve comparable economic performance, implying that the MDP itself did not spur sufficient business activity to differentiate winners from strong runner-up contenders.

A key rationale for MDP subsidies is that they may improve the quality of economic activity even if they do not expand its scale. However, as with the winning counties, runner-up counties exhibit no detectable changes in personal income or county-level GDP following the subsidy competition. These null results further reinforce the conclusion that the subsidy itself does not generate broad-based income or productivity gains. Rather than seeing a divergence in economic fortunes between winners and near-winners, both groups appear to follow similar paths, suggesting limited local economic returns associated with discretionary subsidy competitions.

Table 2.5: Runner-up County Main Results

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
Runner-up	0.006	0.006	0.781	-0.206	0.731
	(0.134)	(0.120)	(3.502)	(0.437)	(0.917)
Observations	2,900	2,900	2,900	2,900	2,900
R2	0.9973	0.9949	0.9946	0.9898	0.9904
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

2.5.2 Nearby Counties

A common policy rationale for discretionary subsidies is that, even if the direct host county experiences limited gains, MDPs may generate positive spillovers across the surrounding region. To examine this hypothesis, I define counties within 50 miles of a winning county as "treated," while counties 50–150 miles away serve as a comparison group.[13] Using the same nearest-neighbor covariate matching approach, implemented three years prior to treatment, I construct weighted comparisons to estimate spillover effects. This design tests whether MDPs generate regional business dynamism—where positive effects would signal broader benefits, and negative effects would indicate unintended harms to neighboring areas (e.g., displacement or crowd-out). Table 2.6 shows that there are no statistically significant effects on establishments, firms, or employment in nearby counties, suggesting that MDPs do not meaningfully alter business activity outside the host county.

Table 2.6: Nearby County Main Results

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
Megadeal Nearby	0.344	0.400	-5.644	0.479	-0.339
	(0.200)	(0.205)	(3.439)	(0.343)	(0.528)
Observations	15,053	15,053	15,053	15,053	15,053
R^2	0.9926	0.9898	0.9908	0.9898	0.9807
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

In addition to business outcomes, I investigate whether MDPs improve the broader region's economic well-being through higher incomes or productivity growth. Here too

[13] I include a robustness check in the appendix where nearby counties are within 25 miles of the winning county.

the evidence suggests no meaningful spillovers: neither personal income nor county-level GDP increases for nearby counties following a MDP. These results reinforce the broader pattern observed in winning and runner-up counties. Across all geographic groups, there is no evidence of significant crowd-out or economic expansion. MDPs do not appear to harm neighboring counties, but they also fail to generate the regional gains that are often cited to justify large subsidy packages.

2.6 Heterogeneity by Firm Size

I next examine whether the effects of subsidy auctions vary by firm size. Specifically, I distinguish between small firms—those with fewer than 500 employees—and large firms, defined as having 500 or more employees. This classification follows the standard threshold used by the Small Business Administration. I estimate heterogeneous treatment effects for winning, runner-up, and nearby counties to assess whether MDPs influence business dynamics differently across the firm size distribution. This analysis helps determine whether megadeals disproportionately benefit large firms, stimulate small-business growth, or have uniformly null effects across the local firm structure.

2.6.1 Winners

Table 2.7 reports the heterogeneous effects for winning counties by firm size. Consistent with the main findings, there are no statistically significant effects for either small or large firms. The confidence intervals rule out economically meaningful gains or losses (e.g., improvements in small-firm formation or large-firm growth on the order typically expected from MDP announcements). While these are null results, they provide meaningful insight into the mechanisms commonly cited to justify megadeals. In theory, MDPs could expand the small-firm sector through increased local demand, entrepreneurial activity, and supply-chain expansion; they could

also stimulate growth in large firms through productivity spillovers and knowledge transfer, as discussed in Greenstone et al. (2010). Yet neither channel materializes. Instead, the evolution of the local firm size distribution in winning counties is statistically indistinguishable from that of comparable counties without an MDP. In other words, despite substantial taxpayer investment, winning counties experience no measurable shift in either small-business dynamism or large-firm activity.

Table 2.7: Winning County Heterogeneous Results

	(1) Small Estabs.	(2) Large Estabs.	(3) Small Firms	(4) Large Firms	(5) Small Emp.	(6) Large Emp.
MDP	0.091	0.057	0.118	0.016	0.485	-0.709
	(0.123)	(0.053)	(0.113)	(0.018)	(2.631)	(4.562)
Observations	3,574	3,574	3,574	3,574	3,574	3,574
R2	0.9943	0.9853	0.9952	0.9954	0.9783	0.9825
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

2.6.2 Runners-up

Table 2.8 presents the heterogeneous results for runner-up counties. As with the main estimates, there are no statistically significant effects across firm sizes. The confidence intervals rule out economically meaningful changes in small- or large-firm activity (e.g., more than 40 small establishments or 10 large establishments). The absence of heterogeneous effects in runner-up counties serves as a useful placebo check. These counties do not experience meaningful shifts in business activity among either small or large firms, suggesting that potential targeted policy responses or compensatory development strategies did not follow their unsuccessful bids.

This pattern is informative for understanding policy behavior in subsidy competitions. If losing counties engaged in alternative development efforts aimed at bolstering small businesses—for example, through targeted small-business credits or

entrepreneurship programs—we would expect detectable growth in the small-firm sector. Likewise, if they attempted to compensate by attracting or supporting large employers through other means, we would expect movement in the large-firm margin. Instead, the lack of heterogeneous effects suggests that the absence of a megadeal does not trigger a systematic policy response. This finding aligns with the interpretation in Slattery (2022), who argues that participation in megadeal competitions is often driven more by political optics and public positioning than by substantive economic development strategy.

Table 2.8: Runner-up County Heterogeneous Results

	(1) Small Estabs.	(2) Large Estabs.	(3) Small Firms	(4) Large Firms	(5) Small Emp.	(6) Large Emp.
Runner-up	0.090	-0.035	0.060	-0.004	-0.389	1.170
	(0.118)	(0.037)	(0.112)	(0.031)	(1.756)	(2.595)
Observations	2,900	2,900	2,900	2,900	2,900	2,900
R2	0.9967	0.9939	0.9966	0.9939	0.9916	0.9892
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

2.6.3 Event Studies

I present event studies for the impact of winning, losing, and being nearby the megadeal subsidies on the level of establishments, firms, employment, personal income, and county GDP. The figures display the dynamic treatment effects estimated using the Callaway and Sant'Anna (2021) framework. The point estimates exhibit no pre-trends prior to the treatment. The estimates are not statistically signifcant and fluctuate around zero. These studies provide little evidence of any statistically significant effects. Across all periods, the magnitude of the estimates remain relatively small.

I normalize the event time to $t = -1$, corresponding to the period immediately prior to the treatment. This period aligns with the onset of formal subsidy negotiations, when counties and firms may begin adjusting expectations or behavior. To the extend that anticipatory effects exist, they would likely emerge during this period; however the absence of pre-trends suggest limited evidence of such behavior. In certain cases such as employment and GDP, there is cyclical behavior where coefficients switch from positive to negative or vice-versa.

Formally, I estimate group-time average treatment effects defined as

$$ATT(g,t) = \mathbb{E}\big[Y_{i,t}(1) - Y_{i,t}(0) \mid G_i = g\big], \quad t \geq g \tag{2.2}$$

where G_i denotes the first treatment period for county i. These effects are aggregated into event time $\ell = t - g$ according to

$$ATT(\ell) = \sum_{g \in \mathcal{G}} w_{g,\ell}\, ATT(g, g+\ell) \tag{2.3}$$

where $w_{g,\ell}$ are weights that average cohort-specific treatment effects across groups. The resulting event-study coefficients $ATT(\ell)$ represent average treatment effects at each relative time period, normalized to $\ell = -1$.

Estimation is conducted using the doubly robust inverse probability weighting (DR-IPW) estimator, which compares treated counties to not-yet-treated counties in each period and allows for heterogeneous treatment effects across cohorts and time.

Results for alternative outcome variables and treatment definitions are qualitatively similar and are reported in the appendix.

2.6.4 Nearby Counties

While nearby counties experience no overall changes in business activity in the main results, the heterogeneous analysis reveals meaningful differences by firm size (Table 2.9). Smaller establishments appear to benefit modestly, with an increase of

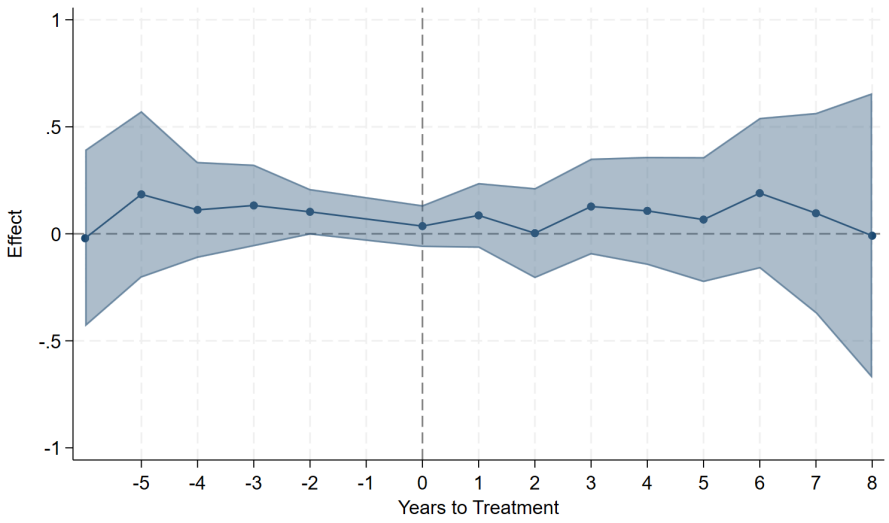


Figure 2.1: Event Study: Establishment Per Capita (Winners)

approximately 68 small establishments and 782 small-firm jobs. In contrast, large-firm activity declines, with roughly 17 fewer large establishments and a reduction of 1,734 large-firm jobs. These results indicate that MDPs can meaningfully alter the composition of the local industrial base in surrounding counties, generating small-firm expansion while displacing or crowding out large-firm activity.

These findings have direct implications for local economic development policy. First, they suggest that regional spillovers may take the form of redistribution across firm sizes rather than net growth, shifting the business environment toward smaller firms in neighboring counties. Such changes in the firm-size portfolio have been largely overlooked in prior evaluations of subsidy auction literature and highlight a nuanced dimension of economic adjustment. Second, this pattern may reflect a form of winner's curse: winning counties secure the MDP, but neighboring counties may absorb the costs of reallocation across firm sizes. Alternatively, policymakers in winning counties may willingly accept these trade offs, foregoing regional large-firm growth in exchange for the political and symbolic benefits associated with landing a marquee investment.

Table 2.9: Nearby County Heterogeneous Results

	(1) Small Estabs.	(2) Large Estabs.	(3) Small Firms	(4) Large Firms	(5) Small Emp.	(6) Large Emp.
Megadeal Nearby	0.450*	-0.106*	0.445*	-0.045**	4.592**	-10.237*
	(0.199)	(0.041)	(0.203)	(0.013)	(1.608)	(3.746)
Observations	15,053	15,053	15,053	15,053	15,053	15,053
R^2	0.9908	0.9837	0.9909	0.9919	0.9896	0.9819
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

2.7 Robustness

Greenstone et al. (2010) argue the primary source of endogeneity in firm site selection arises from the auction process itself. To address this concern, I conduct a series of robustness checks to ensure that the results are not driven by selection into the bidding process or unobserved factors that influence the bidding process and local economic trajectories.

2.7.1 Direct Comparison

The main results rely on the assumption that firm location preferences are driven primarily by underlying geographic and economic fundamentals, rather than by strategic features of the auction process. However, if the auction mechanism itself introduces additional endogeneity, then comparing winning counties to a broader set of nonparticipants may not fully isolate causal effects. In this alternative scenario, the more appropriate comparison group is the set of runner-up counties. This approach rests on the idea that winners narrowly outbid runners-up, and that conditional on reaching the final stage, firm selection among top locations is effectively random from the perspective of local economic fundamentals. Under this assumption, differences between winners and runners-up reflect the effect of securing the megadeal rather than pre-existing differences in regional attractiveness.

Importantly, this comparison between winners and runner-up counties does not rely on balance tests in the same way as comparisons with never-treated counties. Instead, identification comes from the institutional structure of the auction, where counties that reach the final stage are competing for the same project and are plausibly similar along both observable and unobservable dimensions. As a result, this approach is less susceptible to bias arising from residual differences in covariates. Despite this stronger identification strategy, the estimated effects remain statistically insignificant across outcomes. This reinforces the main finding that the absence of detectable impacts is not driven by imbalance or imperfect matching, but rather reflects a genuine lack of measurable effects from securing the megadeal.

Winners vs. Runners-up

I estimate the effects of winning a subsidy competition by restricting the comparison group to runner-up counties. In this specification, I estimate Equation 1 using winners as the treated units and runners-up as the control group. Because every runner-up represents the closest possible counterfactual location, no separate matching procedure is required. The results for winning counties are presented in Table 2.10. As expected, effects for runner-up counties are not estimated, as they serve as the comparison group in this setting. Consistent with the main findings, the estimates for winning counties remain null and closely align with the baseline results.

Nearby vs. Nearby Runners-up

I repeat this exercise for nearby counties by comparing counties located within 50 miles of winning counties to counties located within 50 miles of runner-up counties. This comparison isolates whether any regional spillovers differ between areas adjacent to winners and those adjacent to near-miss finalists. As with the previous specification, no matching procedure is required because runner-up regions provide the closest feasible counterfactual geography. I estimate Equation 1 on this

Table 2.10: Winners vs. Runners-up

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
MDP	0.015	0.056	1.935	0.359	0.336
	(0.115)	(0.106)	(2.928)	(0.449)	(0.971)
Obs	3531	3531	3531	3531	3531
R2	0.9871	0.9855	0.9869	0.9759	0.9767
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

sample and report the results in Table 2.11. Consistent with the main findings, there is no evidence of differential economic effects for nearby counties when using this more restrictive comparison group.

The main assumption behind this model is that nearby counties and nearby runners-up are a "coin flip" and randomly determined. Therefore, there is no balancing required for this section. This model addresses the concerns of potential bias from the matching procedure in the earlier section. Similar to previous section about nearby counties, once again, there is no statistically significant effect on establishments, firms, employment, personal income, and GDP.

2.7.2 Aggregation of Nearby Counties

For additional robustness, I aggregate nearby counties into a single treated region and, importantly, I apply the same aggregation to their matched comparison counties. This ensures that the comparison remains consistent with the original matching strategy rather than introducing imbalance through aggregation. Because these aggregated regions can span state lines, I replace county and state fixed effects with group fixed effects and cluster standard errors at the group level.

Table 2.11: Nearby Winners vs. Nearby Runners-up

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
Megadeal Nearby	-0.017	0.000	-3.218	0.288	0.046
	(0.078)	(0.075)	(1.896)	(0.251)	(0.576)
Observations	9,538	9,538	9,538	9,538	9,538
R^2	0.9856	0.9821	0.9728	0.9649	0.9173
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

I then re-estimate Equation (1) using this aggregated sample. The results are presented in Table 2.12.[14] The aggregated estimates show no statistically significant effects across any outcome. The point estimates are small in magnitude. Establishments and firms are essentially unchanged. Employment shows a modest negative coefficient. Personal income and GDP have small positive coefficients, but neither is statistically significant.

There are trade-offs to this approach. By aggregating counties into regions, I lose county-level fixed effects, which reduces the ability to control for time-invariant heterogeneity at the county level and may mask important within-region differences. At the same time, aggregation increases the R^2 by reducing noise across units. Additionally, the constructed regions can span state lines, which may introduce heterogeneity from differing state policies, tax systems, and economic conditions that are not fully captured by the fixed effects. More generally, aggregation smooths over local variation, so while it is useful for identifying broader regional patterns, it may obscure more localized effects present in the county-level analysis.

[14]Further analysis will examine heterogeneous effects across firm size and industry composition. In particular, I will study whether the arrival of a new firm affects existing firms differently based on size and whether impacts are concentrated within the same NAICS sector as the incoming industry.

Overall, this specification reinforces the main conclusion that firm specific subsidies do not generate broad regional economic benefits, even when accounting for spillovers across nearby counties. Any localized changes do not appear to scale into measurable gains at the regional level.

Table 2.12: Aggregation of Nearby Counties

	(1) Establishments	(2) Firms	(3) Employment	(4) Personal Inc.	(5) GDP
Megadeal	0.287	0.277	-3.214	0.112	-0.787
	(0.167)	(0.159)	(3.112)	(0.312)	(0.684)
Obs	2,935	2,935	2,935	2,935	2,935
R2	0.683	0.688	0.610	0.788	0.581
Controls	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SE	Group	Group	Group	Group	Group

Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

2.8 Discussion

This paper evaluates the effects of large discretionary subsidies, commonly referred to as Million Dollar Plants (MDPs), on local business dynamics. Building on Greenstone et al. (2010) and Patrick (2016), I extend the empirical setting by incorporating three distinct comparison groups: winning counties that secure an MDP, runner-up counties that narrowly miss selection, and nearby counties that may experience spillover effects. This expanded approach allows for a more complete assessment of the consequences of subsidy competitions, both for locations that win the bidding process and for surrounding areas that are indirectly exposed to these large investments. Prior research has not fully examined whether runner-up counties adjust their economic

strategies after losing a bid or whether surrounding counties benefit or suffer from the presence of a new large facility.

The results carry meaningful implications for economic development policy. Across a series of core specifications, there are no statistically significant effects on establishments, firms, employment, personal income, or GDP for winning counties. Runner-up counties likewise show no measurable differences in business activity or income trajectories, suggesting that losing the bidding competition does not redirect their economic path or prompt alternative development strategies. In both cases, the absence of effects persists in heterogeneous and dynamic analyses. These findings suggest that discretionary subsidies do not generate broad-based economic gains at the county level, even when focusing on periods after construction is completed and the facility is operating at scale.

Nearby counties present a more nuanced story. In the main heterogeneous analysis, small firms experience modest gains while large firms experience losses, indicating a shift in the composition of business activity in surrounding areas. However, when aggregating nearby counties into a single regional treatment group, both small and large firms experience declines in establishments and employment. This pattern suggests that MDPs can burden surrounding regions and may reduce business activity when viewed in the aggregate. Crucially, neither personal income nor county-level GDP increase for nearby counties in any specification, demonstrating that these compositional shifts and declines do not translate into improvements in economic well-being or productivity. Together, the results show that MDPs do not meaningfully improve regional economies and may impose costs beyond the host jurisdiction.

These findings challenge the idea that winning subsidy competitions yields meaningful economic advantage or that losing counties fall behind without intervention. They also complicate the narrative of regional spillovers. While some displacement effects are present in certain specifications, the overall pattern is one of neutral or negative outcomes across affected geographies. The results therefore suggest that MDPs and subsidy auctions provide limited value as a strategy for broad regional

development. Instead, benefits appear to be absent at best, while costs may diffuse across local markets in ways that are rarely acknowledged in policy discussions. These results provide evidence of a new winner's curse unstudied in the previous literature.

Future research should further explore why local governments remain willing to offer substantial subsidies in light of weak economic returns. Potential explanations include political incentives, information asymmetries, pressure to appear proactive in economic development, or beliefs that large facilities produce intangible benefits not captured in standard economic measures. Additional work should also examine alternative policy tools that support inclusive growth without shifting costs onto taxpayers or neighboring regions. By analyzing winners, runners-up, and nearby counties, this study underscores the importance of evaluating firm-specific subsidies within a regional equilibrium framework and invites policymakers to reconsider the reliance on discretionary incentives as a primary strategy for economic development.

Chapter 3

Fair Division in the Welsh Inheritance Scheme: A Study of Gavelkind[1]

3.1 Introduction

Nations within the United Kingdom share similarities due to their related ties in culture and geography despite key differences. While the Romans settled in both Wales and England around 50 CE, the Germanic tribes known as the Anglo-Saxons moved into power in England, but failed to make such a conquest in Wales. Consequently, the Welsh legal traditions evolved distinctly from their English counterparts and were based primarily on ancient Celtic law. An important example of this divergence was how the two nations handled inheritance. The Welsh practiced the custom of *gavelkind* - the custom where inheritance was shared equally between all male sons. This contrasted sharply with the medieval English custom of *primogeniture*, where the eldest son inherited everything.

[1]This paper is joint work with Matt Van Essen at the University of Tennessee.

Gavelkind induces a fair division problem among the set of inheritors. This type of allocation problem generates natural questions such as: What constitutes an equal division? How do we make such a division? What if there is disagreement? Trying to answer these questions opens the possibility of conflict. It is, therefore, not surprising that rules for estate division found their way into the Welsh legal tradition in the Code of Venedotion circa 1050 CE. In this book of law, a procedure or mechanism is described for creating an equitable estate division as follows:[2]

...the youngest son is to divide the patrimony and the eldest is to choose, and each seniority choose, unto the youngest

This passage describes a divide-and-choose mechanism for any finite number n of people. The youngest splis the assets in n piles and then the brothers take turns choosing a pile oldest to youngest. When $n = 2$, this is the classic divide-and-choose mechanism. The purpose of this paper is to provide a formal game theoretic analysis of the Welsh procedure.

The Welsh inheritance procedure, hereafter Welsh Divide-and-Choose, is important to study for several reasons: (i) It contributes to the history of thought concerning fair division and fair division mechanisms; (ii) It has potential for modern adaption; and (iii) It provides lessons about the use of fair division mechanisms in practice.

First and foremost, the Welsh's application of divide-and-choose to resolve an inheritance problem is an early advance in the theory of fair division. Classic divide-and-choose is the oldest fair division mechanism. Written records concerning divide-and-choose for two people go back to at least Hesiod's *Theogony* circa 700 BCE. By 1050 CE, the idea was well known and widely applied.[3] The Welsh in particular seemed to have found the idea appealing. The following is also from the Code of Venedotion:

[2] Compton (1878) provides an overview of the Code of Venedotion. He organizes its contents and provides translations of passages containing the rules of division we focus on in this paper.

[3] Christ (1990) describes an interesting application of divide-and-choose used by the ancient Greeks to deal with liturgy avoidance.

"Whoever shall have anything to share with the king, whether he be the chief huntsman or another; that person is to divide and the king to choose."

While examples concerning the application of two person divide-and-choose abound in the ancient world, methods for finding equitable solutions for more than two people are far less common.[4] Welsh Divide-and-Choose is a rare example of a method for arriving at an equitable allocation of contested goods when there are *many people*. As such, it extends the applicability of divide-and-choose. This is a leap in abstraction. Mathematicians did not seriously think about the problem of extending divide-and-choose to n people until Steinhaus' paper in the 1940's and Kuhn's in the 1960's. The Welsh procedure pre-dates this literature by about 900 years. Its invention is thus a significant contribution that should be recognized and scrutinized.

In addition to its historical importance, Welsh Divide-and-Choose is also important to study because it was used in practice. It provides us with a case study in institutional design and conflict resolution. The mechanism could be useful for resolving conflicts. Classic divide-and-choose, for example, has found modern application in the dissolution of business partnerships. The "Texas Shoot-Out" is a *buy-sell clause* used in two person partnership contracts. A partner wishing to dissolve their partnership names a price, the other partner is compelled to either buy their partner's shares at the named price of sell their own shares at the named price. This disguised version of divide-and-choose is favored by many legal professionals due to its simplicity and transparent fairness. The problem of dissolving n person partnerships has developed into a serious literature in auction theory and mechanism design.[5] However, there is not yet a commonly used dissolution procedure for larger partnerships. Thus, Welsh Divide-and-Choose may provide us with some practical insights into the resolution of modern problems. Of course, there could be a reason

[4]Some of these examples have been discussed elsewhere in the literature. O'Neill (1982) and Aumann and Maschler (1985), for example, study the bankruptcy problem from the Talmud with an interesting connection with a solution from cooperative game theory.

[5]See the literature review for examples.

the Welsh mechanism is no longer used. This cannot be determined without studying the procedure in detail.

We begin our analysis by modeling the allocation problem generated by gavelkind inheritance and defining our main normative benchmarks for allocations: efficiency and fairness. Efficiency is the standard notion of Pareto efficiency used in economics. However, for fairness, we use to two notions: envy-free and lexicographic envy-free allocations. An allocation is envy-free if no person wants to swap their bundle of goods with someone else's bundle.[6] This is a strong form of equity. We also have occasion to use a weaker form of equity which we call lexicographic envy-free allocations. In this notion of fairness, we must first specify an ordering or ranking of the people. An outcome will be lexicographic envy-free with respect to this ordering if no person wants to swap allocations with a lower ranked person. This notion of equity is natural in the context of the Welsh inheritance problem where an ordering based on age is already specified by the Code of Venedotion. In this case, for example, an outcome is lexicographically envy-free if the division of the resources is such that no brother envies the bundle of goods received by one of his younger brothers. We then define Welsh Divide-and-Choose as a mechanism for making an allocation decision in this environment. The inheritance procedure thus induces a game between the set of heirs, and we then study the properties of this game from three perspectives: maxmin behavior, equilibrium behavior, and collusive behavior.

A maxmin strategy is one that maximizes a player's minimum payoff in the game. It represents prudent behavior of a player. It is especially relevant in games of fair division. In the two player version of divide-and-choose, for example, maxmin strategies are well known. The divider splits the resources into two piles so that he is indifferent between the two. The chooser chooses his preferred pile. By playing such a strategy a player guarantees himself roughly half of the resources that were to be shared - an equitable outcome - independent of what the other person does. In

[6]This concept of equity is developed in an economic context by Varian (1973). Envy-free allocations also play an important role in cake cutting algorithms. see, for example, Brams and Taylor (1995) or Su (2000).

Proposition 1, we characterize the set of maxmin strategies for each player position in Welsh Divide-and-Choose. If the divider plays his maxmin strategy, then the resulting outcome is envy-free but not efficient (unless all players have the same preferences).

While maxmin behavior is logical in some circumstances, it is not typically equilibrium behavior. We therefore look at the outcomes the procedure produces when participants behave strategically. The game induced by Welsh Divide-and-Choose is dynamic and we look for a subgame perfect Nash equilibrium of the game. The divider can expect each chooser to select the best available bundle (from the chooser's perspective) when it is their turn to select. Knowledge of the choosers' preferences allows the divider to improve on the maxmin outcome. Proposition 2 characterizes the set of subgame perfect Nash equilibrium outcomes. The equilibrium outcome is not generally efficient nor envy-free unless all individuals have the same preferences. This is analogous to results known in the literature about classic divide-and-choose. However, the equilibrium is lexicographically envy-free with respect to the player ordering used in the game. Thus, the equilibrium does satisfy a weak form of equity

We conclude our analysis with a discussion about collusive possibilities in Welsh Divide-and-Choose. A surprising feature of Proposition 1 is that only the divider and the (sometimes, depending on preferences) the first chooser have a positive maxmin value - i.e., they are the only ones that can guarantee a payoff larger than zero in the procedure. All the remaining players have a maxmin value of zero. This result is important. A player's maxmin value can be thought of as a restraining force on the choices of the group. If a player can only guarantee zero, then they cannot stop a group of the other players from giving them zero. The divider and the first chooser are the only players that can benefit from collusion. This may not always be possible. In Proposition 3, we show, under some conditions, these two players can collude and both achieve a payoff in excess of their equilibrium payoff. Although collusion is a potential concern, it remains unclear whether it was a serious problem. On the contrary, the division of family plots of land witnessed in Wales is more supportive

of the predictions made by our model under maxmin or equilibrium behavior. We discuss this point in the discussion section to conclude.

3.2 Literature Review

This paper is about Welsh Divide-and-Choose. In this section, we briefly describe the literature around the classic divide-and-choose procedure for two and fair division more broadly. This serves two purposes. First, it gives the reader a sense for the Welsh's contribution to the early theory of fair division. Second, the literature review will allow us to compare the properties of Welsh Divide-and-Choose with the other mechanisms.

3.2.1 Maxmin in Fair Division Procedures

The academic fair division literature has its roots with Steinhaus (1948). This is the first paper to model a fair division problem and provides a number of interesting schemes to arrive at an equitable solution. The paper begins with classic divide-and-choose and motivates the desire to find fair division procedures to n players. The focus of the paper was on finding procedures where each player could guarantee himself at least $1/n$-th of his value for the whole - a proportional allocation. Classic divide-and-choose, for example, endows each player with a strategy that guarantees himself $1/2$ of his value for the whole. The schemes Steinhaus presented all had this proportionality property. They include a three player extension of divide-and-choose due to Steinhaus, an n person cake cutting algorithm due to Banach, and an n person fair division auction due to Knaster.[7] Kuhn (1967) is an important paper in this literature. He is the first to formally model the fair division problem using game

[7] Dubins and Spanier (1961) provide a dynamic implementation of Banach's procedure. A recent paper of Ericson and Zeager (2022) uses Banach's procedure in to help solve the problem of refugee sharing between host countries.

theoretic ideas and, among other things, extends Steinhaus' three person divide-and-choose to n players using a clever application of the Frobenious-König Theorem. Kuhn Divide-and-Choose has the proportionality property. Kuhn provides an algorithm for each so they can guarantee themselves at least $1/n$-th of their value for the whole.[8] In this paper, we show Welsh Divide-and-Choose has some properties in common with the classical version of divide-and-choose. For example, both mechanisms lead to envy-free allocations when the divider plays maxmin. However, Welsh Divide-and-Choose does not have the proportionality property. There are $n-2$ players in the mechanism who cannot *guarantee* a proportional payoff. Furthermore, this result implies the remaining 2 players can benefit from collusive agreements - assuming transfers are not possible at the conclusion of the procedure.

3.2.2 Strategic Behavior in Divide-and-Choose

The initial fair division literature focused on what players could guarantee. However, this is not always optimal. Rather, players should use the information they have or can guess about the behavior of the others when making their decisions. In other words, optimal behavior is strategic. Crawford (1977) studies classic divide-and-choose from a strategic perspective. Welsh Divide-and-Choose includes the classic version as a special case. Hence, many of Crawford's results apply to the Welsh procedure. For example, the classic procedure's equilibrium allocation is generally inefficient. The Welsh procedure necessarily inherits this property as well. Interestingly, in the final section of his paper, Crawford speculates about how divide-and-choose may be generalized to n players - one of the two methods he posits is precisely Welsh Divide-and-Choose! Crawford was certainly unaware of the Welsh's application of this procedure more than 1000 years prior and spent little time analyzing the mechanism. We, however, find it remarkable to see another example in scientific thinking of

[8] Van Essen and Wooders (2023) study Kuhn's Divide-and-Choose in detail. They characterize maxmin behavior in the procedure and demosntrate a close relationship between maxmin outcomes and the Shapley value outcomes from a family of cooperative sharing games.

how the same ideas can be re-discovered millennia apart. Crawford (1979, 1980) used divide-and-choose as a vehicle for creating novel procedures for implementing equitable allocations - see also Demange (1984).

3.2.3 Divide-and-Choose and Dissolving Partnerships

Dividing a cake is analogous to the surplus sharing problem seen in dissolving a partnership. As a consequence, divide-and-choose has seen a modern application in the guise of a "Texas Shoot-Out" - a method of dissolving two person partnerships described in the introduction. McAfee (1992) examines the Texas Shoot-Out under incomplete information.[9] He finds the mechanism is inefficient and gives the chooser a payoff advantage. In general, a dissolution procedure like a Texas shoot-out needs to be triggered by one of the partners. Thus, there is a larger game played by partners involved than simply interacting in the mechanism. Under some circumstances, the Texas shoot-out can lead to wars of attrition in this game. This creates a potential second source of inefficiency - see de Frutos and Kittsteiner (2008) or Brown and Van Essen (2022). While not as popular as the Texas Shoot-Out in practice, there are other dissolution mechanisms with better theoretical properties. Cramton et al. (1987) show that, unlike trading mechanisms, partnership dissolution problems can be resolved efficiently with an auction. Van Essen and Wooders (2016) study a dynamic compensation auction inspired by Dubins and Spanier's cake cutting algorithm for dissolving n-person partnerships. This auction is ex-post efficient and has the proportionality property - each bidder has a maxmin strategy which guarantees they can leave the auction with at least $1/n$-th of their value for the whole company.[10] Welsh Divide-and-Choose could also be converted into a method for dissolving n person partnerships. For example, the junior partner could announce

[9] This game has been studied under several different preference models. McAfee examines the usual Bayesian case. In a recent paper, Bauch and Riedel (2024) examine behavior in the Texas Shoot-Out when players have Knightian uncertainty. This change is perspective gives several dfferent results from McAfee's model.

[10] Van Essen and Wooders (2020) look at maxmin behavior in the Texas shoot-out, K+1 auction, and the compensation auction.

$n-1$ prices $p = (p_1, ..., p_{n-1})$. This implicitly defines n bundles: One bundle for each price $p_1, ..., p_{n-1}$ and one bundle that includes the company along with the liability of paying the compensation to the other partners i.e., (company, $-\bar{p}$), where $\bar{p} = \Sigma_{j=1}^{n-1} p_j$. Partners, ordered by seniority, could then take turns selecting which of the available bundles they prefer. However, this mechanism doesn't appear to have any advantage over mechanisms already introduced in the literature when values for the company are private information.

3.3 The Welsh Inheritance Problem

The *Welsh Inheritance Problem*, hereafter WIP, is defined by a set $N = \{1, 2, ..., n\}$ of people and an endowed collection L divisible goods which is to be divided up and assigned to people in N. The total endowment of each good l to be divided is given by $\mathring{x}_l > 0, l = 1, ..., L$, where the vector $\mathring{\mathbf{x}} \equiv (\mathring{x}_1, ..., \mathring{x}_L)$. An *allocation* to a person i is a bundle $\mathbf{x}_i = (x_{i1}, ..., x_{iL}) \in \mathbb{R}_+^L$. We let $\mathbf{x} = (\mathbf{x}_1, ..., \mathbf{x}_n)$ denote a list of allocations - one for each person. Preferences for each person are given by a strictly increasing quasi-concave utility function $u_i : \mathbb{R}_+^L \to \mathbb{R}_+$ which maps each allocation $\mathbf{x}_i \in \mathbb{R}_+^L$ into a payoff number $u_i(\mathbf{x}_i)$.

A solution to the WIP is given by a division of $\mathring{\mathbf{x}}$. A *division* is function $D : \mathbb{R}_+^L \to \mathbb{R}_+^{L \times n}$ mapping the goods available into an allocation for each person. We say a division $\mathbf{x}$ is *admissible* if the vector of goods assigned in the division is feasible = i.e., $\Sigma_i \mathbf{x}_i \leqq \mathring{\mathbf{x}}$. An admissible division $\mathbf{x}$ is *non-wasteful* if it fully exhausts the endowment $\Sigma_i \mathbf{x}_i \leqq \mathring{\mathbf{x}}$.

We are interested in fair solutions to the WIP. An admissible division is *envy-free* (i.e., fair) if no i prefers his allocation $\mathbf{x}_i$ to the allocation $\mathbf{x}_j$ receiving by j - i.e., for every $i, j \in N$ we have $u_i(\mathbf{x}_i) \geq u_i(\mathbf{x}_j)$. This is also a strong form of equity which may not always be satisfied. Thus, we also introduce a weaker form of equity called lexicographic envy-freeness. This definition requires an ordering or ranking of the players and requires that no person envy any person who is lower ranked than

themselves. Formally, let π be a permutation of N. This defines an ordering of people where $\pi(1)$ is the index of the person who is first in the ordering (i.e., the highest ranked), $\pi(2)$ is the index of the person who is second in the ordering (i.e., 2nd highest ranked), etc. An admissible division $\mathbf{x}$ is *lexicographically envy-free* with respect to the ordering π if for any $i, j \in N$ such that $\pi(j) > \pi(i)$ we have

$$u_i(\mathbf{x}_i) \geq u_i(\mathbf{x}_j).$$

Hence, in a lexicographic envy-free allocation, players may feel envy toward another, but it is restricted to those of higher rank. In the WIP there is a natural ordering - the seniority of the children, oldest to youngest. Here, a lexicographic envy-free allocation requires that no person envy the allocation of a younger sibling.

3.3.1 Overview of the Game

The fair division scheme codified in the Welsh Code of Venedotion describes an algorithm for making a division in the WIP. There are n players, with two distinct roles: one "divider" and $n-1$ "choosers". Players are ordered from eldest to youngest. This ordering determines each player's role. Player n is the youngest and divider. All other players are choosers ranked by seniority, i.e., the oldest is rank 1.

The divider makes an admissible division of the resource by partitioning the endowment into n bundles. This division is observed. Players then take turns selecting a single bundle according to their seniority. Each choice is observed and once a bundle has been selected it cannot be selected by another. After $n-1$ choosers make a selection, the remaining bundle is assigned to the divider and the division is constructed.

3.4 Analysis

Welsh Divide-and-Choose induces a game between the n players. The analysis that follows seeks to understand the properties of this mechanism from three perspectives. First, we examine the induced game when the players act to maximize their minimum possible payoff. This is known as maxmin behavior and serves a useful starting place in games of fair division. In addition, in this case, it highlights some unexpected features of the mechanism. Maxmin behavior is typically not equilibrium behavior. Second, under the assumption of complete information, we provide conditions for strategies to be part of a subgame perfect Nash equilibrium. Full information doesn't change the behavior of the choosers, but allows the divider to tailor individual bundles. Hence, the divider is better off. Third, we ask what type of allocations are produced in equilibrium. Are the equilibrium allocations efficient? Are they fair? Not surprisingly, the answers to these questions depend on the preferences and the definition of fairness. Finally, we examine the mechanism's vulnerability to collusive behavior. While it is clear that the mechanism was intended to generate equitable divisions of inheritance, it is possible that the rules of the mechanism were insufficient to prevent bad actors from colluding at the expense of their brothers. Our maxmin results signal that collusion could be a serious problem in the Welsh procedure and we confirm this suspicion in the last part of this section.

3.4.1 Maxmin Behavior

The *maxmin value* of a game for a player is the largest payoff that a player can guarantee himself independent of the choices of other players. This value is an important benchmark since it identifies a lowerbound for any equilibrium payoff. A strategy that satisfies this condition is a *maxmin strategy.* This is considered prudent if a player has a high degree of uncertainty about the behavior of other players. We begin our analysis by characterizing maxmin play for the divider (i.e., Player n).

Lemma 3.1. *In any maxmin strategy, the divider's division* $\mathbf{x}$ *is non-wasteful.*

While the divider should not waste in any maxmin strategy, allowing a wasteful choice seems contrary to the idea of inheritance division. Thus, from here on in the paper we strict all divisions of the divider to be non-wasteful.

As in the classic divide-and-choose, the divider's maxmin strategy should be one where he is indifferent between the bundles. This is a necessary condition for maxmin.

Lemma 3.2. *In any maxmin strategy, the divider's division* $\mathbf{x}$ *satisfies*

$$u_n(\mathbf{x}_1) = u_n(\mathbf{x}_2) = ... = u_n(\mathbf{x}_n).$$

While there are many partitions of the endowment that satisfy the above, the "equal split" bundle is of particular importance. This division assigns each person an even share of each good - i.e., $(\mathring{x}_j/n)_{j=1}^n$.

Proposition 1 characterizes the set of maxmin strategies for each player.

Proposition 3.1. *In the game induced by Welsh Divide-and-Choose, maxmin behavior is characterized as follows:*

1. *The divider's unique maxmin strategy is the equal split division. His maxmin value is* $u_N(\mathring{\mathbf{x}}/n)$.

2. *The chooser with the highest priority (i.e., eldest heir) selects their most prerferred bundle in the division - if indifferent, then a randomized choice among the indifferent bundles is optimal. His maxmin value is bounded from above by* $u_1(\mathring{\mathbf{x}}/n)$.

3. *Any strategy of a chooser with lower-than-highest priority is maxmin. The maxmin value of these players is zero.*

A surprising result of Proposition 1 is that only the divider and the first chooser can guarantee themselves a positive payoff. This contrasts with the majority of mechanisms studied in the fair division literature where, typically, each player

can guarantees themselves a payoff proportional to their value.[11] This result has consequences demonstrated in the collusion section.

A result of the divider following his maxmin strategy is that every player is indifferent between all bundles in the division. This is an envy-free or fair division and achieves the definition of gavelkind. However, this result is generally not efficient since it does not adjust to differences in preferences. For example, suppose we have two goods, x_1 and $x2$, with $\mathring{x}_1 = \mathring{x}_2 = 12$. In addition, suppose we have two players where their preferences are

$$u_1(\mathbf{x}) = x_{11}^{20} x_{12}$$
$$u_2(\mathbf{x}) = x_{21} x_{22}^{20}.$$

The unique maxmin outcome is the equal split bundle where each player receives six of each good. This is envy-free, but clearly not efficient since the players can trade x_2 for x_1 and both be better off.

Corollary 3.1. *Any outcome of the game induced by Welsh Divide-and-Choose where the divider plays maxmin is envy free. However, the maxmin outcome is not always efficient.*

Maxmin behavior is prudent for players under severe uncertainty. However, it often differs from equilibrium behavior.

3.4.2 Perfect Information (Subgame Perfect Strategy)

In this section, we characterize the subgame perfect equilibrium outcomes of the game induced by the WIP. Equilibrium is characterized via backward induction. A chooser, in equilibrium, always selects their most preferred available bundle.[12] The divider takes this behavior into consideration and partitions the resources to maximize

[11] See for example Steinhaus (1948), Kuhn (1967) or Van Essen and Wooders (2020).
[12] This is also consistent with maxmin behavior.

his payoff under the constraints that designated bundles will be selected by each chooser in their turn. Hence, the divider's subgame perfect equilibrium strategy is characterized by the solution to the following maximization problem - which we call the *D-Max* problem.

$$\max_{(\mathbf{x}_1,\ldots,\mathbf{x}_n)\in\mathbb{R}^{nL}_{+}} u_n(x_n) \text{ subject to } :$$

$$\sum_i \mathbf{x}_i = \mathring{\mathbf{x}}$$

$$u_i(\mathbf{x}_i) \geq u_i(\mathbf{x}_j) \quad \text{for } i = 1,\ldots,n-1 \text{ and } j > i.$$

The constraints in the D-Max problem restrict the divider to (a) select a feasible, non-wasteful division; and (b) to select each i's bundle $\mathbf{x}_i$ so i doesn't prefer the bundle dedicated to a higher labeled chooser $j > i$ (i.e., any younger sibling).

Proposition 3.2. *A subgame perfect equilibrium in the game induced by the Welsh Inheritance Mechanism is one where each chooser always selects their most preferred available bundle, and the divider's partition of the resources is a solution to the D-Max problem.*

The equilibrium outcome is preferable to the divider relative to playing maxmin. Since the divider can guarantee the equal split bundle moving away from this bundle must make him better off.

Crawford (1977) showed that the classical two person version of divide-and-choose did not always lead to efficient outcomes. Since Welsh Divide-and-Choose contains the classic procedure as a special case it inherits this same inefficiency. In addition, the equilibrium outcome is not always envy-free. Simple examples can be constructed where players follow equilibrium, but where one of the choosers will envy the outcome an elder brother. One exception to both of these negative results occurs when preferences are identical. In this case, the unique equilibrium outcome is the equal split bundle which, given identical preferences, is efficient and envy-free.

Corollary 3.2. *The subgame perfect equilibrium outcome of the game induced by Welsh Divide-and-Choose is not always efficient. If, however, players have identical preferences, then the unique subgame perfect equilibrium outcome is the equal split outcome which (since preferences are identical) is both efficient and envy-free*

The equilibrium has the property that each chooser selects their designated bundle. Hence, no chooser envies a younger sibling. In other words, the equilibrium outcome is therefore lexicographically envy-free.

Corollary 3.3. *Any subgame perfect equilibrium outcome is lexicographically envy-free.*

Thus, equilibrium outcomes aren't always efficient or envy-free, but they are at least envy-free in a particular direction.

3.4.3 Collusion

Last, we go beyond individual behavior and ask whether groups of players in the WIP can benefit by forming a syndicate. We have already seen evidence from maxmin behavior that suggests this possibility.

Maxmin strategies are security strategies. They maximize the minimum payoff a player could receive in the game. The payoff that the strategy guarantees is the maxmin value. In classic divide-and-choose each player has a maxmin strategy which guarantees a payoff of the equal split bundle (i.e., a positive payoff) independent of the choice of the other person in the game. The Welsh procedure does not have this feature. Choosers 2,..., $n-1$ all have a maxmin value of zero. They can guarantee nothing.

Why is this result important? It reveals these players have no ability to influence the outcome of the procedure. For example, they cannot stop the divider and chooser 1 from forming a group with the following scheme: First, the divider puts all resources in bundle $\mathbf{x}_1 = \mathring{\mathbf{x}}$ and sets $\mathbf{x}_2 = ... = \mathring{\mathbf{x}}_\mathbf{n} = \mathbf{0}$. Second, chooser 1 selects $\mathbf{x}_1$ - leaving

nothing for all remaining players. Finally, after the procedure is over, chooser 1 can make a transfer of goods to the divider in excess of the divider's equilibrium outcome. This scheme is predicated on such a transfer being possible.

Proposition 3.3. *Welsh Divide-and-Choose is vulnerable to collusive agreements between the divider and chooser 1. If ex-post transfers are possible, then these two players can obtain a payoff beyond their equilibrium payoff while leaving the remaining choosers with a payoff of zero.*

3.5 Concluding Remarks

The generalized divide-and-choose mechanism introduced by the Welsh is an interesting contribution to the theory of fair division. We have modeled the inheritance problem and studied the game the mechanism induces under three different behavioral assumptions. First, we characterized maxmin play. The surprising result here is that only the divider and the first chooser are able to guarantee a positive payoff. We argued this feature opens the mechanism up to collusive arrangements if ex-post transfers are feasible. Maxmin outcomes are attractive. If the divider plays maxmin, then the resulting allocation is envy-free. This is analogous to the classic two person version of divide-and-choose under maxmin behavior and likely the reason the mechanisms was adopted in the first place. Second, we characterized the subgame perfect equilibria of the game. Strategic behavior improves the divider's welfare over maxmin, and the equilibrium outcome is lexicographic envy-free in the sense that no brother prefers the equilibrium allocation of one of his younger siblings. However, the equilibrium outcome need not be envy-free. Finally, we illustrated how Welsh Divide-and-Choose is vulnerable to collusive arrangements by the divider and the oldest brother.

The vulnerability of Welsh Divide-and-Choose to collusion is unusual. For example, the n player version of divide-and-choose proposed by Kuhn (1967) does not have this feature. In Kuhn's scheme, players can guarantee themselves a proportional

share of the total resources by following a maxmin strategy.[13] Thus, unlike the Welsh procedure, a player in Kuhn's mechanism following a maxmin strategy cannot be left with a zero payoff even if players make collusive arrangements. The degree to which collusive opportunities were exploited in ancient Wales is difficult to judge. However, there is anecdotal evidence that supports our equilibrium/ non-collusive predictions. In Wales, we observe widespread intergenerational division of single plots of land. In contrast, we do not observe this outcome in England with its primogeniture inheritance custom. This is consistent with our equilibrium/maxmin prediction of the game. That said, the vulnerability of the mechanism certainly detracts.

Finally, independent of the procedure, there were unintended costs associated with gavelkind. Yorke (1887) suggests gavelkind contributed to the downfall of the Welsh Empire. He argues the continuous division of land that resulted from the practice led to the absence of large plots of land. Further, small landowners often engaged in fierce competition, squeezing profits, and this made it difficult to raise tax revenue. Pryor (2011) observes that small farms in Wales tended towards bankruptcy in the later stages of the Welsh empire.

[13] See Van Essen and Wooders (2023) for a formal proof.

Chapter 4

Divide and Collude: A Fair Division Experiment

4.1 Introduction

Financial disputes such as inheritances and business divorces require allocating resources fairly among multiple claimants. One simple rule is classic divide-and-choose: one party cuts the assets into two shares and the other chooses. This method has been extended historically in medieval Wales. Welsh law let the youngest heir partition an estate into n bundles, with senior heirs picking in order by seniority and the divider receiving the remainder. Such sequential rules are transparent and easy to explain, making them attractive in practice. However, theory also highlights serious drawbacks. The sequential nature gives the divider a first-mover advantage under complete information. Most importantly, if the divider and first chooser collude by splitting a bundle ex-post, they can exclude other players and violate fairness. In short, the rule's simplicity masks a structural vulnerability to collusion.

In this paper, I report the first laboratory experiment on the generalized divide-and-choose procedure used in the Welsh Inheritance Scheme (WIS). I ask natural questions arising from the theory. Do realized allocations satisfy standard fairness

and efficiency criteria? Do subjects exploit collusion opportunities when allowed? To investigate, I vary two key features of the experiment: communication between the players and the ability to make ex-post side-payments. Under the experiment with no transfers, theory predicts envy-free, equal split outcomes that are efficient. But with communication and binding transfers, the divider and first chooser can coordinate a highly unequal split. I test these predictions under controlled conditions.

The experiment yields four main findings. First, in the Simple Inheritance Game, dividers cut the tokens equally and envy is low. Payoffs are similar across roles as predicted by the theory. Second, efficiency is higher under the Simple Inheritance Game and a treatment where communication is allowed. By contrast, the Communication + Transfers treatment produces significantly lower efficiency. Group payoffs are stochastically dominated by other treatments. Third, consistent with collusion, the divider and first chooser earn higher payoffs on average in the Communication + Transfers treatment, while the second chooser suffers. Almost all transfers flow between the divider and first chooser. Fourth, fairness deteriorates with transfers: post transfer-allocations show higher envy and greater payoff dispersion. In sum, the mechanism functions as theory expects when side contracting is infeasible, but breaks down once tacit agreements and payoffs are possible.

These results have practical implications. The WIS is attractive because it is historically grounded and easy to implement, similar to a simple buy-sell clause. Yet, the data show that with communication and transfers, it can generate unfair outcomes. Real-world application would thus require institutional safeguards.

4.1.1 Related Literature

A central applied motivation for studying sequential fair-division mechanisms is the dissolution of common-ownership relationships such as partnerships, closely held firms, and other "business divorce" settings. The economics of partnership dissolution formalizes the problem of reallocating jointly owned indivisible assets

(often with monetary compensation) under informational frictions. Cramton et al. (1987) characterizes mechanisms that can implement efficient dissolution and provides the benchmark against which simpler contractual clauses are evaluated. A major practical contrast is that legal contracting often relies on buy-sell (Texas shootout) clauses procedurally simple rules that are transparent but may be inefficient or role-biased under private information as shown by McAfee (1992).

Laboratory evidence shows that these theoretical properties are not always transported into application. In independent private values, experimental comparisons between a widely used buy-sell clause and an efficient auction benchmark find that many subjects behave in ways not captured by standard equilibrium predictions (notably, bidding close to valuations). These actions result that buy-sell clauses can weakly outperform the auction on efficiency in the laboratory under particular environments as shown by Kittsteiner et al. (2012). In complete-information environments like Brown and Velez (2016) intended to represent long-standing relationships where parties "know each other well", divide-and-choose style mechanisms can dominate winner's bid type auctions on multiple performance measures, while the primary sources of inefficiency differ across mechanisms (bounded rationality versus reciprocity-driven behavior). More recent experiments by Hyndman (2021) incorporate additional realistic frictions such as risk or ambiguity in the post-dissolution value of the asset. These new findings highlight that mechanism performance is sensitive to environment and behavioral response, and that laboratory tests are informative when field data are scarce.

Within the broader fair division literature, moving from bilateral to three-party division raises both feasibility and strategic complexity. Experimental evaluations by Kyropoulou et al. (2022) of three-player cake-cutting procedures find that envy-free mechanisms can generate allocations that are both fairer and perceived as fairer than proportional alternatives. Subjects frequently attempt to manipulate procedures through strategic behavior that evolves with repetition and learning. This evidence motivates using envy-freeness and measuring efficiency directly rather than presuming

theoretical guarantees will be realized behaviorally. It also motivates explicitly testing vulnerability to strategic exploitation. Ortega and Segal-Halevi (2022) formalizes that celebrated envy-free procedures can be manipulable in ways that concentrate value in a single piece under strategic cutting.

A key reason sequential procedures are attractive in applied disputes is their salience and transparency. These same features make them vulnerable once communication and side payment are possible. In mechanism design, Laffont and Martimort (1997) study collusion-proofness to address environments where agents can use side contracts. Experimental evidence from Agranov and Yariv (2018) on auctions and oligopoly markets shows that communication can facilitate coordination on collusive strategies, and that the ability to make transfers can substantially amplify collusive outcomes even when commitments are non-binding. Dechenaux and Mago (2019) highlight that transfer options by themselves do not guarantee collusion or efficient coordination; in market experiments with private information, side payments tend to matter when they are used and communication supports the complexity of the coordinated plan. This motivates experimental tests that explicitly vary (i) communication and (ii) the feasibility of ex-post transfers, to determine whether collusion is a purely theoretical concern or a practical threat in the intended application.

In the applied setting, inheritance disputes, divorce-like division, and dissolution of business partnerships, has direct support in recent applied scholarship connecting fair division to legal and market-value constraints. For example, decision-support work by Dall'Aglio (2023) explicitly frames these core domains where fair division principles and practical elicitation constraints must be combined. While such procedures differ from divide-and-choose, they reinforce the overarching point: implementability and transparency are not secondary. They are binding constraints in the mechanisms that real disputants and legal professionals can actually use.

4.2 Theory Overview

This experiment is motivated by the game-theoretic analysis of the Welsh Inheritance Scheme (WIS), a sequential n-player generalization of divide-and-choose. In the WIS, one player (the "Divider") partitions the endowment into n bundles, then choosers select bundles sequentially by rank, and the Divider receives the remaining bundle. The theory delivers sharp predictions about fairness, efficiency, and the scope for strategic exploitation under different informational and contracting environments.

4.2.1 Core theoretical results

Homogeneous preferences: equal split as the equilibrium outcome. When all players share identical preferences over bundles, the informational environment no longer affects the prediction. Whether preferences remain private or become common knowledge, the strategic incentives of the Divider coincide with the maxmin logic. Because every chooser ranks bundles in the same way, any unequal partition would induce earlier choosers to select strictly better bundles, leaving the Divider with a strictly worse residual bundle. Anticipating this, the Divider maximizes his payoff by creating an *equal split division* in which all n bundles are identical and exhaust the endowment.

Under homogeneous preferences, backward induction and maxmin reasoning yield the same outcome. Sequential choice does not generate a first-mover advantage, and the Divider cannot profitably extract surplus from other bundles without harming his own residual payoff. The realized allocation therefore consists of identical bundles for all players, which implies both envy-freeness and Pareto efficiency.

Collusion: a coalition can overturn fairness. A distinctive theoretical vulnerability arises from the sequential structure: the Divider and the First Chooser are the only players who can guarantee themselves positive payoffs in the secured-payoff logic, which makes them natural coalition partners. The theory provides a simple

collusive construction: the Divider concentrates the entire endowment in one bundle; the First Chooser selects it; and the pair share via an ex-post side agreement. This drives other players' payoffs toward zero and violates both strong and lexicographic fairness.

4.2.2 Implications for the experimental treatments

The focus of the experiment is to determine whether participants collude and if so, to what extent. The experiment varies (i) whether subjects can communicate and (ii) whether they can make ex-post transfers. These design features map directly onto the theory's key comparative statics: without transfers, collusion is difficult to implement; with transfers (and especially with targeted bilateral communication), collusion becomes feasible.

The theory therefore motivates hypotheses in three domains: (a) *fairness* (envy and equality), (b) *efficiency* (distance from efficient benchmarks), and (c) *coalition behavior* (role-based advantages and transfer patterns).

4.3 The Experiment

This paper features an experiment environment designed to capture the key features of the theoretical model. Treatments vary by both communication and transfer availability to test the model's major implications.

4.3.1 Experimental Design

The experiment consisted of three treatments using the same Divide-and-Choose mechanism. In the Simple Inheritance Game (SIG) treatment, subjects play the game without being able to communicate or send transfers. In the Communication Only (C) treatment, subjects had the opportunity to discuss with players within their group prior to making decisions. The third treatment, Communication + Transfers

(C+T), allowed subjects to communicate and make ex-post transfers to players within their group at the end of the round. Table 4.1 summarizes these treatments.

Table 4.1: Summary of Treatments

Treatment	Communication Allowed	Transfers Allowed
SIG	No	No
C	Yes	No
C+T	Yes	Yes

Across all treatments, participants were randomly placed into groups of three and assigned one of three roles: Divider, First Chooser, and Second Chooser. Groups and roles were re-assigned each period to eliminate repeated-game considerations.

The objective was to share 12 green and 12 red tokens among the group. The Divider allocated these tokens into three bundles, labeled A, B, and C, with no constraints except tokens may not be subdivided and all tokens must be allocated (see Figure C.1). Next, the First Chooser selects one of the three available bundles (see Figure C.2). The Second Chooser selects from the remaining two bundles (see Figure C.3). The Divider receives the remaining bundle. Each player receives a score for the round based on their selection.

Each participant faces the same payoff function:

$$2.1 * ln[1 + GT + RT + (GT \times RT)].$$

The efficient and envy-free allocation is for each participant to receive 4 green and 4 red tokens. This equal split bundle yields the highest group payoff. I include two types of tokens to capture complementarities across attributes of the good. While green and red tokens are symmetric in the payoff function, they can be interpreted as representing different dimensions of value such as quantity (e.g., land size) and quality (e.g., land characteristics). The multiplicative term in the payoff function implies that bundles combining both types of tokens generate higher utility, reflecting complementarities across attributes.

At the end of each round, a feedback screen describes the tokens in each player's bundle and their earnings. When the round ends, subjects are assigned to a new group and a new role. This process continues for approximately 70 minutes.

Communication Stage

In treatments where communication was permitted, participants could send free-form text messages for 90 seconds using private chat channels (see Figure C.4). Each bilateral chat was visible to the two participants involved. For example, the Divide and First Chooser could communicate without the Second Chooser observing.

Participants could not communicate once the communication stage ended. Subjects then proceeded to the allocation stage to make decisions.

Transfer Stage

In the treatment with transfers allowed, an additional stage followed the allocation. Each participant could simultaneously transfer any number of green or red tokens from their own bundle to other members of their group (see Figure C.5). At the end of the transfer stage, a screen would inform the group of any transfers. Final scores for the round were recalculated to account for the transfers.

4.3.2 Experimental Procedures

The experiments were conducted at the Experimental Economic Laboratory at the University of Tennessee in 2025. Subjects were recruited using the Online Recruiting System for Experimental Economics (ORSEE) software developed by Greiner (2015). Participants made their decisions on computers using programs written in the z-Tree language (Fischbacher, 2007).

A session operates as follows. Subjects enter the laboratory and sit at computer terminals separated by privacy dividers to maintain anonymity. Subjects participate in a risk elicitation task modeled after Holt and Laury (2002). The risk elicitation

task is used for analysis to determine whether collusion is determined by a divider's risk tolerance. Since the transfers are non-binding, a divider may require a higher risk tolerance to engage in collusion. The moderator then reads the instructions for the relevant treatment. Subjects take a short quiz about the rules and payoffs. An unpaid practice round takes place to allow participants to see where information is located on the screen and to further their understanding of the decision tasks. The moderator announces that all rounds going forward may be selected for payment. At the end of each session, subjects complete a demographic questionnaire. Each session lasted approximately 90 minutes. There is no set number of rounds due to the varying time durations for each treatment.

The experiment ran for a total of ten sessions with 174 participants. Two groups of eighteen subjects participated in the Simple Inheritance Game treatment. Four groups, one of twenty-four, one of twenty-one, and two of twelve, participated in the Communication Only treatment. Four additional groups, one of twenty-four, one of twenty-one, and two of twelve, were in the treatment with both communication and transfers. At the end of each session, three periods were randomly selected for payment: one in which the subject served as the Divider, one as the First Chooser, and one as the Second Chooser. Earnings ranged from \$7.50 to \$29.75 with averages of \$21.99, \$23.11, \$22.06 for the SIG, C, and C+T sessions, respectively.

4.3.3 Hypotheses

The theoretical model highlights a vulnerability to collusion when transfers are feasible. However, whether individuals actually implement such coordination in practice remains unclear. The mechanism is simple, transparent, and symmetric in payoffs. Absent enforceable contracts, collusion requires trust and credible side payments. My prior is therefore that collusion will be limited in practice and that outcomes will approximate the equal-split benchmark even when transfers are available.

Hypothesis 4.1. Group efficiency will not differ across treatments.

Under symmetric payoff functions, the equal-split allocation is both envy-free and Pareto efficient. In the absence of enforceable contracts, collusive agreements may break down due to mistrust or the risk of reneging. Additionally, individuals may simply prefer the equal split outcome. Therefore, even when transfers are allowed, total group payoffs should remain close to the efficient benchmark.

This prediction implies the following empirical outcomes: (i) Similar fractions of efficient outcomes across C and C+T treatments, (ii) No stochastic dominance in the distribution of group payoffs, and (iii) No systematic difference in mean total group profits.

Hypothesis 4.2. Role payoffs will remain symmetric across treatments.

Because all players share identical payoff functions, equilibrium predicts similar expected payoffs across roles. If collusion fails to materialize in practice, post-transfer payoffs for the Divider, First Chooser, and Second Chooser should not systematically diverge across treatments.

Hypothesis 4.3. Fairness will not deteriorate under transfer-enabled communication.

Equal-split allocations imply low envy and limited dispersion of payoffs within groups. If collusion does not arise in practice, allowing transfers should not meaningfully increase within-group payoff inequality or observed envy.

4.3.4 Power Analysis

To inform the experimental design, I conduct a power analysis using pilot data from the baseline treatment. In the pilot, average group-level profit is 19.5, which I treat as the benchmark corresponding to efficient, equal-split outcomes.

The power analysis focuses on scenarios in which a small fraction of groups experience extreme outcomes. For example, suppose 5 percent of groups result

in a single player capturing the entire resource, reflecting breakdowns in collusive agreements. Even if only 10 percent of groups engage in collusion overall, with the remaining groups continuing to play the equal split, these extreme outcomes generate a sizable reduction in average efficiency.

Under this scenario, the implied treatment effect is large enough that it can be detected with fewer than 200 group-period observations per treatment while maintaining a statistical power of 0.8 at the 0.05 significance level.

These results suggest that a target of at least 200 group-period observations per treatment provides more than sufficient power to detect economically meaningful efficiency losses in the presence of occasional extreme outcomes. Accordingly, the experimental design is calibrated to exceed this threshold in each treatment arm.

4.4 Results

I begin presenting the results with some summary statistics of the experiment. I then present the results for the above hypotheses in this section. I begin with the group results then the individual results. I finish with a brief comment regarding some anecdotal evidence highlighting the risks of these collusive deals.

4.4.1 Summary Statistics

I present the summary statistics for the experiment.

Table 4.2 reports summary statistics for the 174 participants in the experiment. Subjects are predominantly traditional college-aged students, with an average age of 20 years, and the sample is evenly split by gender (49 percent female). The average academic year is 2.59, indicating that most participants are sophomores or juniors. Risk preferences are heterogeneous: 58 percent are classified as risk averse, 6 percent as very risk averse, 21 percent as risk seeking, and 21 percent as risk neutral.

Table 4.2: Summary Statistics

	mean	sd	min	max
Female	0.49	0.50	0	1
Year	2.59	1.14	1	5
Risk Averse	0.52	0.50	0	1
Very Risk Averse	0.06	0.23	0	1
Risk Seeking	0.21	0.41	0	1
Risk Neutral	0.21	0.41	0	1
Age	19.97	1.41	18	23
Understood the Experiment	4.78	0.53	2	5
Happy with Results	4.49	0.82	1	5
Happy with Compensation	4.66	0.64	2	5
Thought About Others	4.11	1.18	1	5
Insufficient Information	1.81	1.21	1	5
Thought the Game Was Fair	3.63	1.30	1	5
More Information Helps	4.16	1.13	1	5
I Changed My Strategy	3.01	1.58	1	5
I Would Change Strategy If I Knew the Players in My Group	3.05	1.54	1	5
I Am Charitable	3.80	0.77	1	5
I Am Self-Interested	3.41	0.89	1	5
I Am Envious	2.63	0.97	1	5
I Am Rational	4.37	0.71	2	5
I Am Sympathetic	3.98	0.85	1	5
I Am Informed	4.03	0.76	1	5
I Am Reckless	2.26	1.01	1	5
I Would Share	0.57	0.50	0	1
More Likely to Lie for Gain	0.74	0.44	0	1
I Would Cheat a Friend to Gain	0.97	0.17	0	1
I Was Honest During the Experiment	0.16	0.37	0	1
I Acted to Gain the Most Money in the Experiment	0.35	0.48	0	1
I Acted to Help Others Earn in the Experiment	0.48	0.50	0	1
I Think Others Acted to Help Me Earn More in the Experiment	0.41	0.49	0	1
Observations	174			

Survey responses suggest that participants understood the experiment well (mean = 4.78 out of 5) and report high satisfaction with both results and compensation. Perceptions of fairness are more moderate (mean = 3.63), and respondents indicate some strategic adjustment over time. Self-reported traits show relatively high levels of rationality, sympathy, and informed decision-making, alongside moderate self-interest and low recklessness. Overall, the sample reflects a young, balanced, and cognitively engaged participant pool with meaningful variation in risk attitudes and social preferences.

4.4.2 Group Payoffs

I begin the analysis by calculating the Euclidean distance from the equal split bundle for each treatment. The results by treatment are presented in Figures 4.1, 4.2, and 4.3. The SIG and C treatments are quite similar. The C+T treatment shows more variation from the equal split division. This result provides indication that the C+T differs from the other two. For a simpler comparison, I compare the C and C+T treatment going forward since they only differ in the ability to transfer at the end of the round.

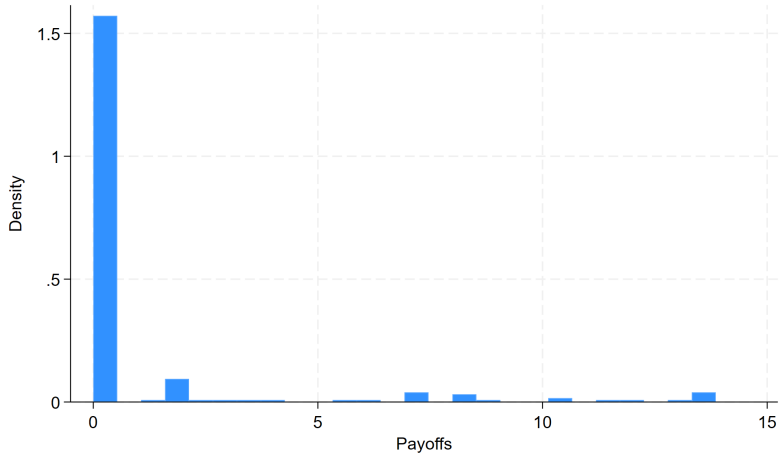


Figure 4.1: Euclidean Distance from Equal Split (SIG)

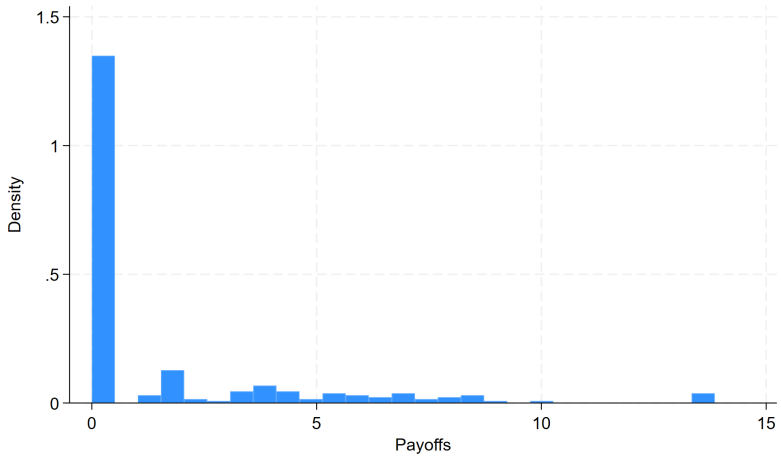


Figure 4.2: Euclidean Distance from Equal Split (C)

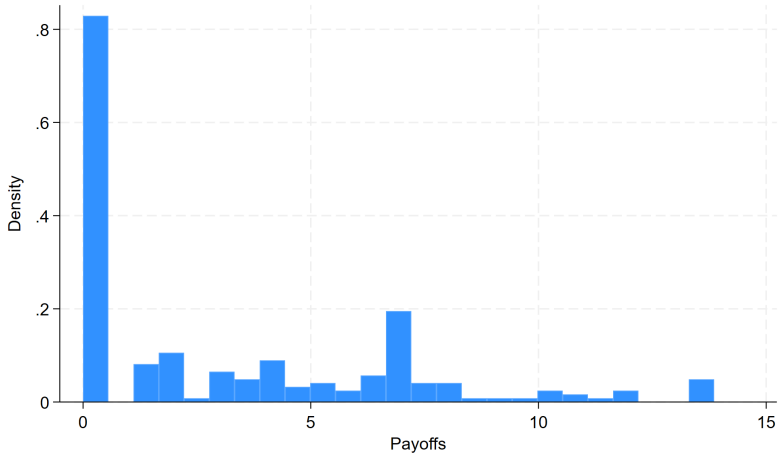


Figure 4.3: Euclidean Distance from Equal Split (C+T)

To test the first prediction of whether groups receive similar payoffs based on treatment, I use a binary indicator where cases are equal to 1 if the group is efficient and 0 if else. Table 4.3 reveals that the treatment with transfers is inefficient more frequently than efficient.

Table 4.3: Efficiency by Treatment

	C	C+T	Total
Not Efficient (0)	80	120	200
Efficient (1)	180	102	282
Total	260	222	482

Notes: Pearson $\chi^2(1) = 26.7448$, $p < 0.001$.

Efficiency rates differ significantly across treatments, and groups in the Communication + Transfers treatment are substantially less likely to achieve efficient allocations. The introduction of transfers appears to reduce efficiency, consistent with strategic distortions that arise when collusive arrangements become feasible.

Efficiency is not always binary. For example, it may be the case that the C+T treatment yields close to efficiency. This outcome would mean that we should not write off the mechanism. I also treat efficiency as a continuous concept, measured by group profits, so that allocations can be ranked by their degree of efficiency even when they are not Pareto optimal. I begin by plotting the CDF of the C and C+T treatments as shown in Figure 4.4. It is clear to see that the C+T is less efficient because it is stochastically dominated by the C treatment. For a mathematical test, I use a Kolmogorov-Smirnov test to compare the C and C+T profit distributions. The null hypothesis for each of these tests is equal distribution. I reject the null hypothesis in Table 4.4. These results further confirm the findings of the CDF; the C treatment is more efficient than when allowing for ex-post transfers. These results require further exploring of individual payoffs to understand why there is a loss in efficiency.

Table 4.4: Kolmogorov-Smirnov Test Results

Smaller Group	D	p-value
Communication	0.1126	0.000
Comm + Transfers	-0.1013	0.001
Combined K-S	0.1126	0.000

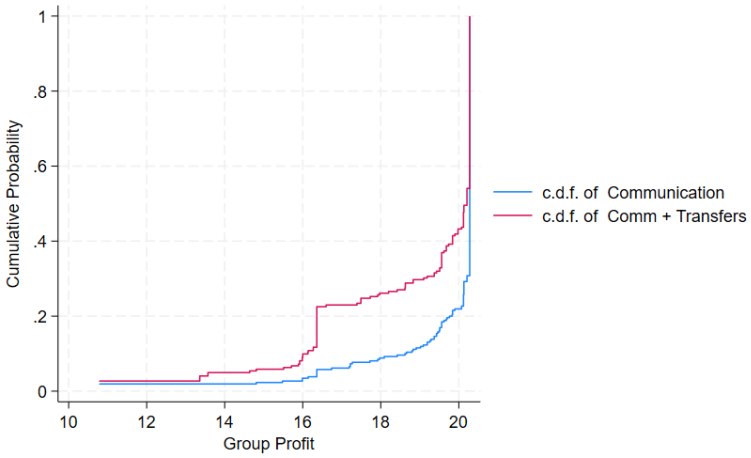


Figure 4.4: CDF of Group Payoffs by Treatment

4.4.3 Individual Payoffs

I next explore the outcomes for individual players by role in the experiment. Under the null hypothesis in 4.2, the player's role should not impact the player's payoff regardless of treatment.

Beginning with the cumulative distribution function (CDF) for each role, this reinforces evidence provided by the group payoff analysis. The CDF under the transfer treatment for the first chooser and divider stochastically dominate those that do not include transfers, at least where the majority of observations occur. This of course inverts for the second chooser because the observations are not independent - i.e., when the first chooser and divider benefit, someone must lose. This result is further reinforced when looking at the bar charts showing the mean of profit by role. The second chooser experiences a large drop off of nearly $1.00 per round when transfers are included. In contrast, there are marginal gains for the divider and first chooser due to the diminishing marginal returns of the payoff function. Even the small gains are concentrated on the first chooser suggesting he may not always follow through on the transfer.

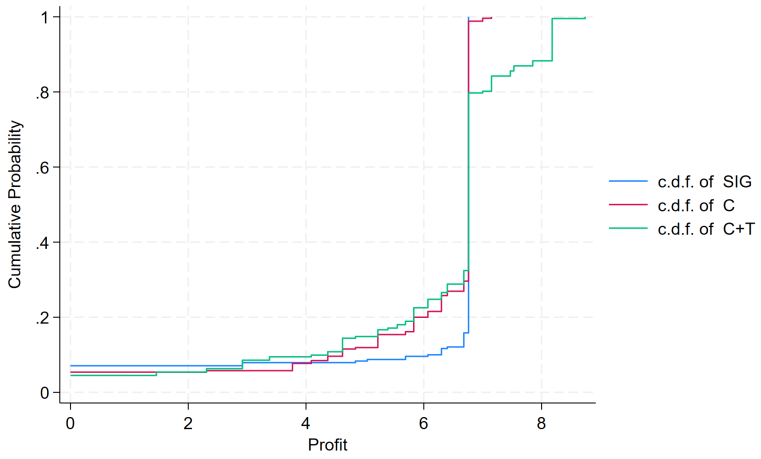


Figure 4.5: The CDFs by Treatment for the Divider.

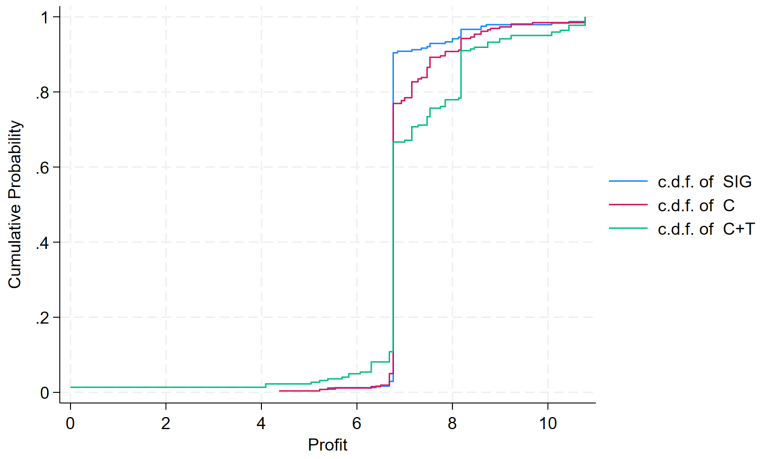


Figure 4.6: The CDFs by Treatment for the First Chooser.

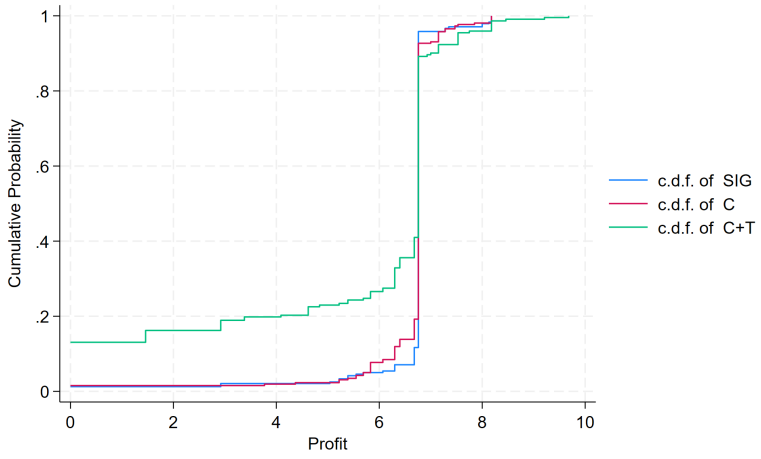


Figure 4.7: The CDFs by Treatment for the Second Chooser.

This evidence suggests there is something at play under the transfer treatment. While this suggests some potential for collusion, it fails to capture how the gains are split.

A natural question is who gains from the transfer stage. I provide two histograms by frequency to determine who gains more. Based on a simple visual test, the frequency for gains by transfer from the first chooser suggests the divider is much better off than the second chooser. I also perform a Mann Whitney test on these distributions which confirms the suspicion. The divider is better of compared to the second chooser. There are 23 cases of equal split collusion. The collusion may not be rampant, but the mechanism is vulnerable to collusion in application.

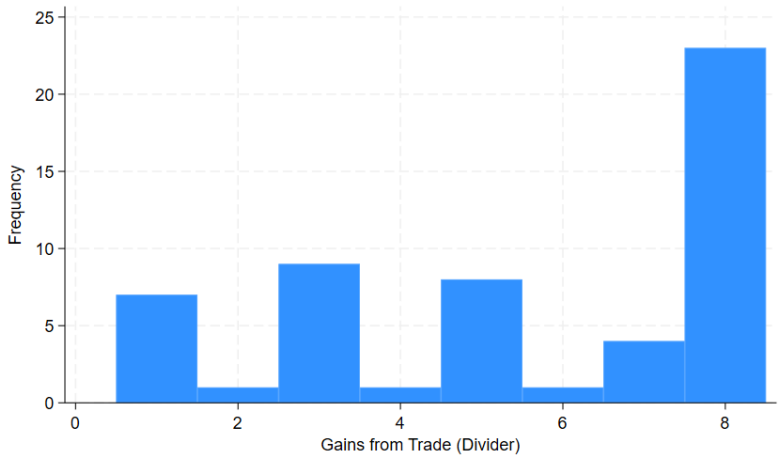


Figure 4.8: Gains from Trade for the Divider.

Further results of the experiment highlight that roles matter in their outcomes. These results indicate that the first chooser is best off regardless of role. This result actually contradicts the equilibrium theory under symmetric payoff functions. Under symmetric payoff functions, as in the experiment, all players should receive equal payoffs. However, some divisions yield an opportunity for the first chooser to take advantage of unequal divisions.

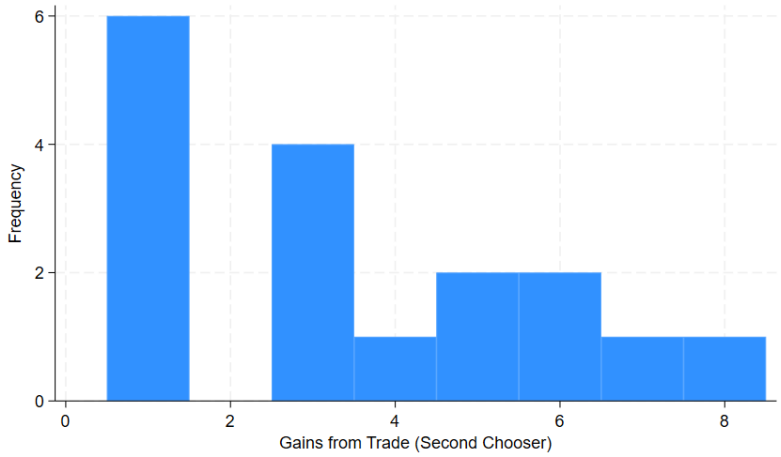


Figure 4.9: Gains from Trade for the Second Chooser.

Because all players face the same payoff function, any scenario in which a player receives a higher payout than another is evidence of collusion. The treatment with the most cases of envy is therefore the C+T treatment.

4.4.4 Potential Mechanisms

In this section, I investigate potential behavioral mechanisms that may contribute to inefficient divisions arising from collusive behavior. In particular, I examine whether individual characteristics of the divider, specifically gender and risk preference, help explain variation in the efficiency of outcomes. The divider is the first-mover and determines the efficiency of the division. Efficiency is measured using the sum of bundle payoffs within a group-period, where an equal split represents the benchmark of maximal fairness and efficiency, while collusive allocations concentrate tokens into a single bundle and generate the lowest efficiency levels.

To test these mechanisms, I estimate random effects panel regressions at the subject-period level. Random effects models are appropriate in this setting because individual characteristics such as gender and risk preferences are time-invariant, and the specification allows for unobserved heterogeneity across participants while

preserving between-subject variation. The analysis focuses on dividers, since they are the agents who determine how bundles are initially allocated.

The first specification examines whether gender predicts inefficient allocations:

$$\text{Efficiency}_{it} = \beta_0 + \beta_1 \,\text{Female}_i + \beta_2 \,\text{Treatment}_{it} + \beta_3 \big(\text{Female}_i \times \text{Treatment}_{it}\big) + \varepsilon_{it}. \quad (4.1)$$

The second specification evaluates whether heterogeneity in risk preferences explains differences in efficiency. Risk attitudes are measured using the Holt-Laury lottery task and categorized into mutually exclusive indicator variables for risk-seeking, risk-averse, and very risk-averse individuals:

$$\begin{aligned} \text{Efficiency}_{it} = \beta_0 &+ \beta_1 \,\text{RiskSeeking}_i + \beta_2 \,\text{RiskAverse}_i + \beta_3 \,\text{VeryRiskAverse}_i \\ &+ \beta_4 \,\text{Treatment}_{it} \\ &+ \beta_5 \,(\text{RiskSeeking}_i \times \text{Treatment}_{it}) \\ &+ \beta_6 \,(\text{RiskAverse}_i \times \text{Treatment}_{it}) \\ &+ \beta_7 \,(\text{VeryRiskAverse}_i \times \text{Treatment}_{it}) \\ &+ \varepsilon_{it}. \end{aligned} \quad (4.2)$$

In both equations, Efficiency_{it} denotes the total bundle payoff for divider i in period t, ε_{it} is an idiosyncratic error term. Treatment indicators capture institutional differences across experimental conditions, while interaction terms allow participant characteristics to influence behavior differently depending on the strategic environment.

The results in Tables 4.5 and 4.6 provide little evidence that observable divider characteristics explain inefficient allocations. Neither gender nor risk preference categories predict deviations from equal splits or greater susceptibility to collusive outcomes. These findings suggest that inefficiencies observed in the experiment are

unlikely to be driven by stable individual traits, but instead may arise from strategic considerations embedded in the sequential structure of the division mechanism. It should be noted that risk aversion does predict efficiency but does not determine whether or not collusion is more likely to occur.

Table 4.5: Group payoff by Gender

	(1) Group Efficiency
Female	-0.07
	(1.67)
C+T	-0.82
	(1.66)
Female x C+T	1.31
	(2.39)
_cons	14.87***
	(1.17)
N	482

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.4.5 Anecdotal Evidence

One interesting situation arose during the experimental sessions regarding collusion. A few key participants acting as First Chooser offered to cut a deal for collusion with the Divider. The Divider put all tokens into one bundle per agreement. The First Chooser reneged on the deal and kept all the tokens for themselves. This led to those Choosers opting to not participate in future collusive agreements. While these examples are not the norm, they offer insight into the risks from these types of agreements. There are no guarantees with this type of deal.

Table 4.6: Group payoff by Risk

	(1) Group Efficiency
Risk Averse	4.64*
	(2.14)
C	-3.44
	(2.52)
RA x C+T	-3.52
	(3.03)
Risk Seeking	-0.05
	(2.47)
Risk Seeking x C+T	-0.35
	(3.47)
Very Risk Averse	3.11
	(3.60)
Very Risk Averse x CT	-2.33
	(6.26)
_cons	14.98***
	(1.78)
N	482

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.5 Discussion

The experimental results indicate that allowing ex-post transfers materially changes behavior in the generalized divide-and-choose procedure. Relative to the C treatment, the C+T treatment exhibits significantly lower rates of efficiency and lower average group payoffs. This pattern is consistent with the central theoretical vulnerability identified in the Welsh Inheritance Scheme: the divider and first chooser can coordinate on division that facilitation coalition behavior. And once transfers are feasible, payoffs are redistributed in ways that disadvantage the later choosers.

A key distributional result is that the payoff distributions for the divider and first chooser shift upwards when transfers are available, while the second chooser's payoff distribution shift downward. This role-asymmetric treatment matches the coalition structure highlighted in the theory. In the WIS analysis, only the divider and first chooser have strong strategic protections under secured-payoff reasoning, and the sequential choice structure makes the second chooser vulnerable to coordinated deviations by earlier movers. The evidence that the second chooser bears the primary losses under transfers therefore supports the interpretation that the mechanism's collusion vulnerability is behaviorally relevant in the laboratory.

At the same time, the data suggest that strong-form collusion is not pervasive. While some rounds display clear "collusion signatures" (highly unequal partitions paired with transfers), many outcomes reflect either partial collusion or failed coordination. This is consistent with two frictions that limit collusion in one-shot environments with voluntary transfers. First, transfers are not enforceable: even when the divider and first chooser agree ex-ante on a side payment, the first chooser can renege after securing the most favorable bundle. Second, the transfer stage increases strategic and cognitive complexity, which can lead to miscoordination and surplus destruction even in the absence of deliberate collusion. Both frictions align with broader evidence that communication alone often has limited effects, but that

communication paired with transfers can enable collusion without commitment in some interactions while still leaving substantial scope for defection and instability.

From an applied perspective, these findings sharpen the tradeoff between transparency and robustness. The Welsh mechanism is attractive because it is simple and easily explained to disputants, similar in spirit to widely used buy-sell ("shotgun") clauses in business dissolution. However, the experiment shows that when side contracting is feasible, especially with private bilateral communication and ex-post transfers—the mechanism can generate outcomes that are less efficient and substantially less equitable for the most vulnerable participant. This suggests that real-world implementation, if considered, would require institutional restrictions (e.g., limiting side payments, structuring communication to be public within the group, or incorporating collusion-robust design features) to preserve fairness. These conclusions align with the mechanism-design view that collusion-proofness is a first-order concern whenever parties can coordinate and contract on the side.

Finally, the results contribute to the experimental fair-division literature by showing that fairness and efficiency guarantees may be sensitive not only to the underlying allocation rule, but also to the surrounding institutional features that shape coordination and redistribution. Even when envy-free mechanisms can perform well in controlled settings, strategic manipulation and coalition behavior can emerge when the environment allows it.

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Appendices

Appendix A

Chapter 2

NAICS Codes

Table A.1: NAICS Sectors by First Digit

First Digit	NAICS Sector(s)	Description
1	11	Agriculture, Forestry, Fishing, Hunting
2	21, 22, 23	Mining; Utilities; Construction
3	31–33	Manufacturing
4	42, 44–45	Wholesale Trade; Retail Trade
5	48–56	Transportation; Information; Finance; Real Estate; Prof. Services; Management; Admin. Support
6	61–62	Educational Services; Health Care & Social Assistance
7	71–72	Arts, Entertainment, Recreation; Accommodation & Food Services
8	81	Other Services (except Public Administration)
9	92	Public Administration

A.1 Event Studies

In this section, I present the remaining event studies.

A.1.1 Winners:

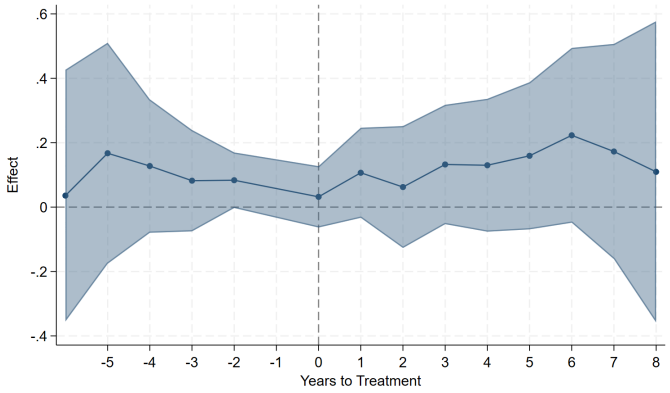


Figure A.1: Event Study: Firms Per Capita (Winners)

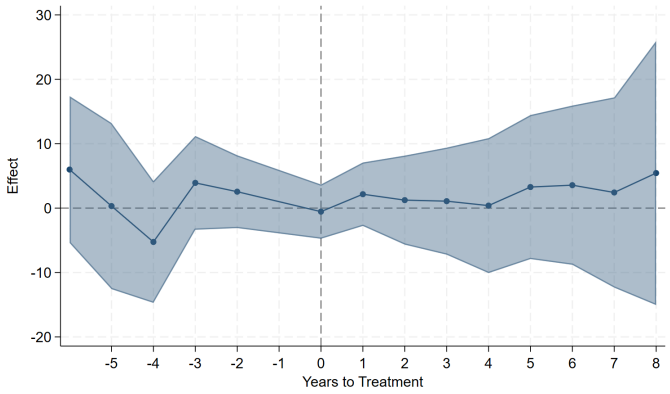


Figure A.2: Event Study: Employment Per Capita (Winners)

There may be an issue with parallel trends for GDP per capita in period $t = -5$. It is unlikely the firm considered this an issue when selecting the county. This may downward bias some estimates but due to the upward bias of the balancing, this unlikely causes a major issue. The log results match those of Patrick (2016). So I am confident in the overall results.

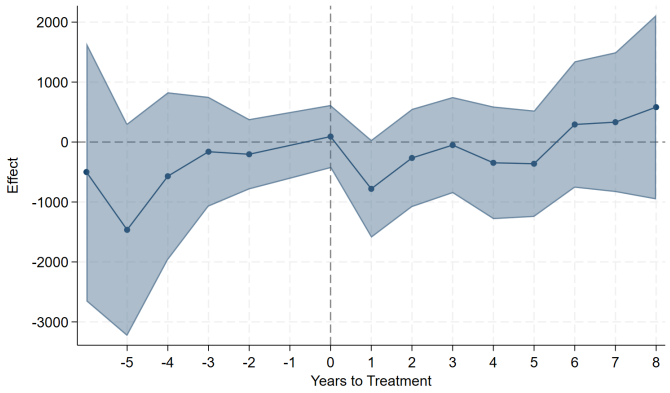


Figure A.3: Event Study: Personal Income Per Capita (Winners)

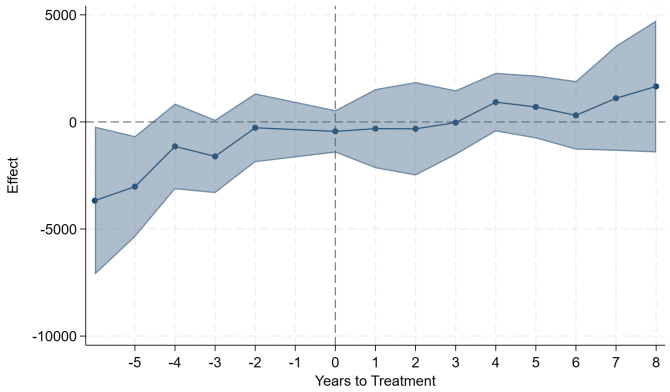


Figure A.4: Event Study: GDP Per Capita (Winners)

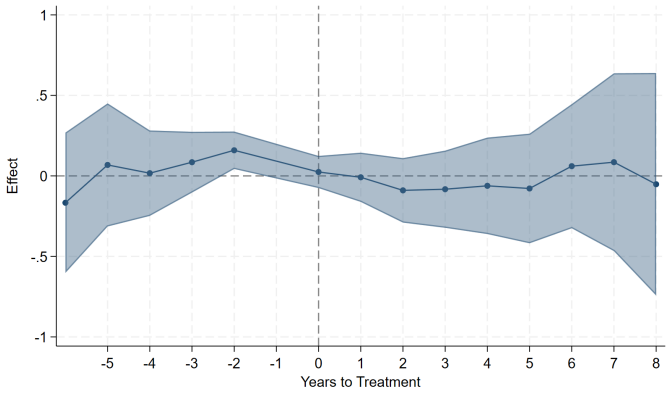


Figure A.5: Event Study: Establishments Per Capita (Runners-up)

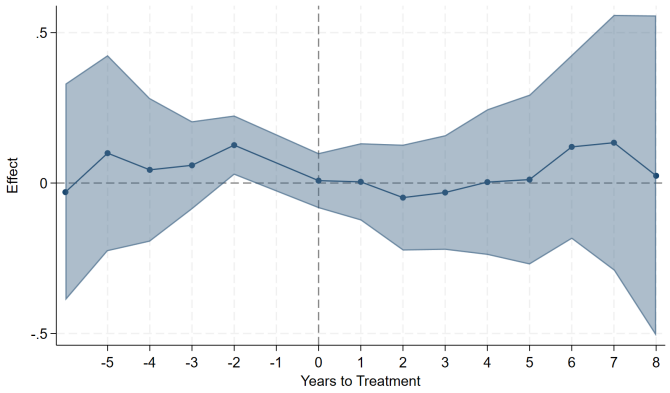


Figure A.6: Event Study: Firms Per Capita (Runners-up)

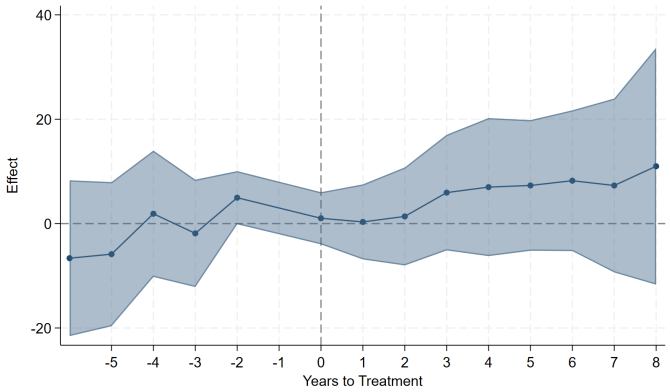


Figure A.7: Event Study: Employment Per Capita (Runners-up)

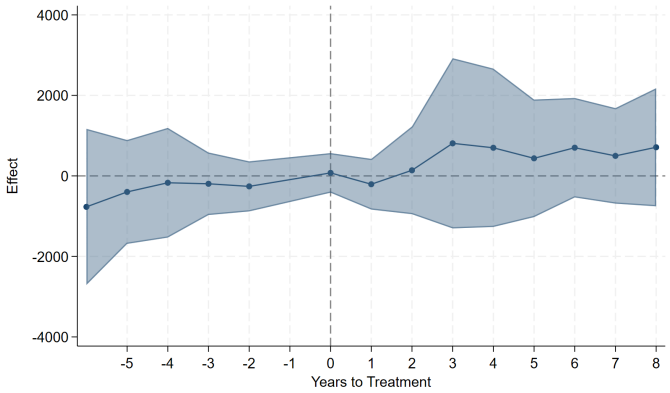


Figure A.8: Event Study: Personal Income Per Capita (Runners-up)

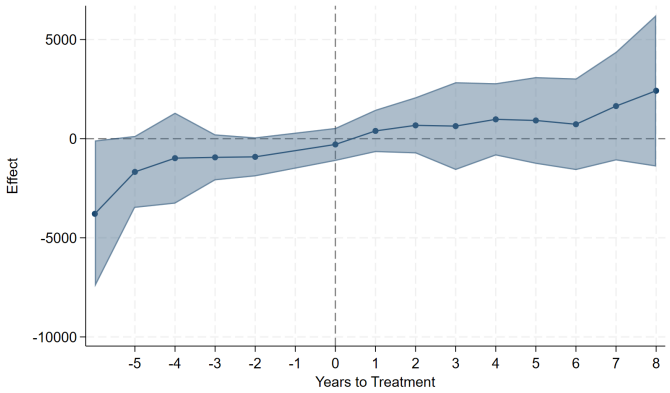


Figure A.9: Event Study: GDP Per Capita (Runners-up)

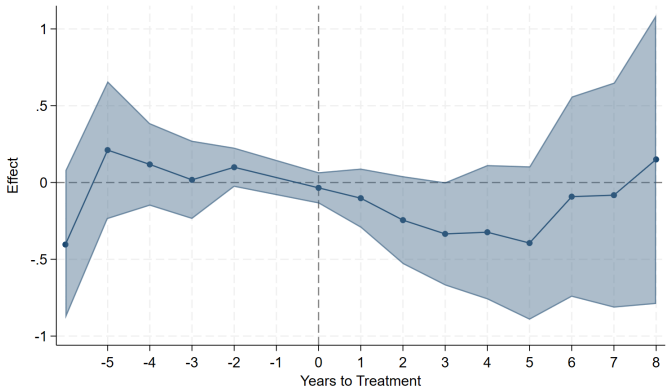


Figure A.10: Event Study: Establishments Per Capita (Nearby)

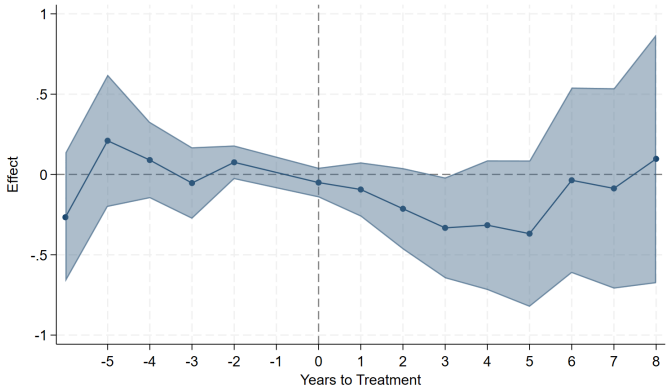


Figure A.11: Event Study: Firms Per Capita (Nearby)

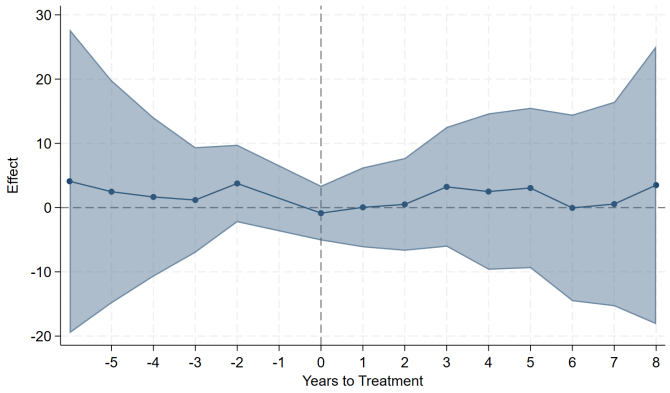


Figure A.12: Event Study: Employment Per Capita (Nearby)

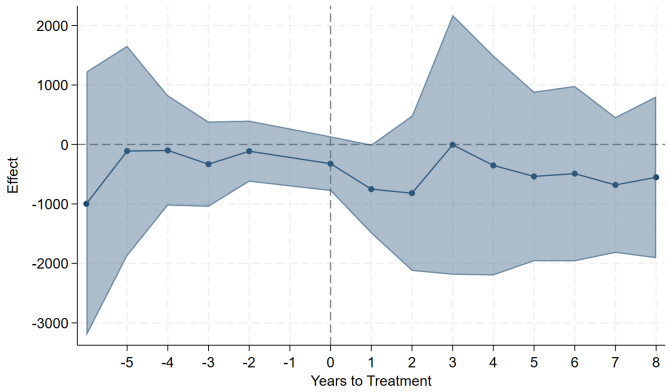


Figure A.13: Event Study: Personal Income Per Capita (Nearby)

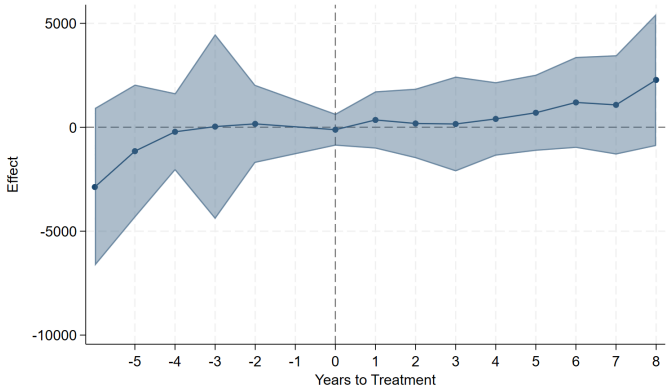


Figure A.14: Event Study: GDP Per Capita (Nearby)

A.1.2 Runners-up

A.1.3 Nearby

A.2 Additional Results

A.2.1 Summary Statistics

In this section, I present three tables. The tables are summary statistics are unmatched include a much larger sample of counties. These tables show significantly more bias than the matched sample. The main biases stem from including more smaller and older counties with a completely different business structure. While the main estimates still biased, the matching improves the estimates significantly.

Table A.2: Unmatched Summary Statistics (Winners)

	Winners		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.16	0.14	0.14	0.18	0.32
Female (%)	0.51	0.01	0.51	0.02	0.08*
Average Age (Years)	37.85	2.75	39.24	2.52	0.000***
Average Educ. (Years)	5.81	0.42	5.91	0.46	0.04**
Log Population	12.83	0.76	10.53	1.21	0.000***
Agriculture Emp. (%)	0.14	0.25	1.20	3.37	0.001***
Utilities Emp. (%)	6.20	2.75	7.67	6.31	0.07*
Manufacturing Emp. (%)	12.80	7.41	21.93	14.65	0.000***
Trade Emp. (%)	20.64	4.31	23.93	7.63	0.001***
Business, Finance, and Logistics Services Emp. (%)	22.74	8.15	12.63	7.12	0.000***
Education & Health Services Emp. (%)	19.02	8.05	13.17	10.52	0.000***
Leisure & Hospitality Emp. (%)	10.93	3.50	9.05	6.21	0.01**
Other Emp. (%)	3.13	0.54	3.30	2.10	0.28
Observations	97		1481		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Unmatched Summary Statistics (Runners-up)

	Runners-up		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.18	0.17	0.12	0.17	0.01**
Female (%)	0.51	0.01	0.51	0.02	0.06*
Average Age (Years)	38.15	1.71	38.76	2.31	0.01**
Average Educ. (Years)	5.85	0.32	5.83	0.44	0.72
Log Population	12.64	0.59	10.45	1.20	0.000***
Agriculture Emp. (%)	0.34	0.44	1.47	4.81	0.002***
Utilities Emp. (%)	6.04	1.32	7.49	6.02	0.01**
Manufacturing Emp. (%)	13.62	6.36	22.11	15.16	0.000***
Trade Emp. (%)	21.99	2.76	23.69	7.97	0.02**
Business, Finance, and Logistics Services Emp. (%)	20.65	4.77	12.05	6.78	0.000***
Education & Health Services Emp. (%)	18.96	6.38	13.47	10.92	0.000***
Leisure & Hospitality Emp. (%)	10.88	1.91	9.03	6.84	0.003***
Other (%)	3.29	0.48	3.20	2.03	0.58
Observations	105		1696		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Unmatched Summary Statistics (Nearby)

	Megadeal Nearby		Never Treated		
	mean	sd	mean	sd	p-value
Non-white Proportion (%)	0.16	0.16	0.14	0.18	0.04**
Female (%)	0.51	0.01	0.51	0.02	0.01**
Average Age (Years)	38.55	2.68	39.12	2.71	0.002***
Average Educ. (Years)	5.87	0.39	5.83	0.39	0.29
Log Population	12.73	0.85	10.55	1.22	0.000***
Agriculture Emp. (%)	0.24	0.39	1.16	3.17	0.000***
Utilities Emp. (%)	5.84	2.18	7.72	6.29	0.000***
Manufacturing Emp. (%)	12.77	6.90	21.88	14.50	0.000***
Trade Emp. (%)	21.29	5.08	23.84	7.62	0.000***
Business, Finance, and Logistics Services Emp. (%)	20.81	7.28	12.61	7.04	0.000***
Education & Health Services Emp. (%)	20.55	6.83	13.37	10.59	0.000***
Leisure & Hospitality Emp. (%)	10.70	2.72	9.09	6.19	0.000***
Other Emp. (%)	3.27	0.60	3.27	2.16	0.98
Observations	484		1540		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.2 Population

The main results for the paper include a specification with outcome variables in terms of per capita. One concern is that population is also an outcome variable. There are no significant effects found on population as an outcome variable.

Table A.5: Effects on Population

	(1) Population	(2) Population	(3) Population
MDP	-0.0077		
	(0.0083)		
Runner-up		0.0096	
		(0.0105)	
MDP Nearby			-0.0093
			(0.0085)
Obs	3,251	2,435	15,109
R2	0.9997	1.000	0.999
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Case FE	Yes	Yes	Yes
Clustered standard errors	State	State	State

Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.

Controls: average age, education, female and minority proportions, industry composition.

A.2.3 Nearby Runners-up

For additional robustness, I show there are null effects for counties within 50 miles of the runners-up. I estimate these according to the same process as the nearby counties for the winners of the MDP.

A.2.4 Log Results

I also estimate the main specifications using log-transformed outcome variables rather than per capita measures. In contrast to the baseline results, which are generally null, the log specifications tend to produce negative coefficients across several outcomes.

Table A.6: Counties Nearby to Runners-up

	(1) Establishments	(2) Firms	(3) Employment
Runner-up Nearby	-0.0000	-0.0000	-0.0010
	(0.0001)	(0.0001)	(0.0017)
Observations	6,660	6,660	6,660
R^2	0.9852	0.9817	0.9798
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Case FE	Yes	Yes	Yes
Clustered SEs	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001. Controls include average age, education, female and minority proportions, and industry composition.

While this difference in sign may appear notable, it does not alter the core conclusion of the paper. The negative estimates are not consistently statistically significant and do not indicate meaningful economic declines; rather, they highlight the sensitivity of results to functional form when underlying effects are small.

Given this sensitivity, I focus on per capita specifications as the primary results. Per capita measures more directly capture changes in local economic activity relative to the size of the resident population, which is the relevant margin for welfare and regional development. In contrast, log specifications place greater weight on proportional changes and can be influenced by baseline levels, particularly in a setting with substantial heterogeneity in county size. Because counties differ markedly in population and economic scale, log transformations may obscure economically meaningful differences when effects are concentrated in levels or vary with size. Per capita estimates therefore provide a more transparent and policy-relevant benchmark for evaluating how the benefits of a megadeal translate to local residents. Taken together, both approaches lead to the same substantive conclusion: these subsidies do not generate the large, positive local economic impacts often anticipated.

Table A.7: Winner Results with Logs

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
MDP	-0.0030	0.0014	0.0150	-0.0169	-0.0118
	(0.008)	(0.009)	(0.018)	(-1.13)	(-0.54)
Obs	3685	3685	3685	3685	3685
R2	0.9994	0.9994	0.9984	0.999	0.999
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

Table A.8: Runner-up Results with Logs

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
Runner-up	-0.0212*	-0.0165	-0.0374**	0.0101	0.0024
	(0.0086)	(0.0083)	(0.0130)	(0.0220)	(0.0214)
Observations	2,776	2,776	2,776	2,776	2,776
R^2	0.9998	0.9998	0.9994	0.9990	0.9990
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

Table A.9: Nearby Results with Logs

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
Megadeal Nearby	0.0060	0.0116	-0.0210*	-0.0030	-0.0231
	(0.0078)	(0.0092)	(0.0090)	(0.0076)	(0.0178)
Observations	15,053	15,053	15,053	15,053	15,053
R^2	0.9998	0.9997	0.9989	0.9990	0.9980
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

A.2.5 Growth Rates

In this section, I investigate the impact of these subsidies on growth rates rather than per capita or log outcomes. The growth rate allows for differences in an attempt to reduce the R^2 by differencing. The R^2 does reduce but the coefficients remain null.

The only positive estimate appears in GDP growth for winning counties. This result should be interpreted with caution. The matching procedure introduces upward bias, particularly due to residual differences in average education between treated and control units. In addition, given the large number of statistical tests conducted, it is likely that at least one coefficient attains statistical significance purely by chance. Even taking the estimate at face value, an isolated increase in GDP growth is not sufficient to justify the use of these subsidies.

A.2.6 Distance

For additional robustness, I restrict the nearby sample to counties within 25 miles of the treated county. Employment in these counties declines significantly, supporting the crowding-out mechanism discussed earlier. The estimates imply a reduction of

Table A.10: Growth Rates for Winning Counties

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
MDP	0.0017	0.0018	0.0035	0.0130	0.0354*
	(0.0026)	(0.0028)	(0.0051)	(0.0081)	(0.0153)
Obs	3,574	3,574	3,574	3,574	3,574
R2	0.808	0.791	0.697	0.632	0.507
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

Table A.11: Growth Rates for Runner-up Counties

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
Runner-up	0.0026	0.0010	0.0006	0.0039	-0.0129
	(0.0025)	(0.0028)	(0.0044)	(0.0068)	(-1.17)
Obs	2,900	2,900	2,900	2,900	2,838
R2	0.924	0.915	0.876	0.815	0.677
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

Table A.12: Growth Rates for Nearby Counties

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
MDP Nearby	0.0034	0.0049	-0.0044	0.0018	-0.0052
	(0.0022)	(0.0026)	(0.0050)	(0.0026)	(0.0053)
Obs	15,109	15,109	15,109	15,109	15,109
R2	0.709	0.679	0.449	0.521	0.346
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEss	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, log(population), female and minority proportions, and industry composition.

approximately 12 employees per 1,000 residents. This decline appears to be driven primarily by large firms. This provides further evidence against these subsidies.

Table A.13: Per Capita Effects on Counties within 25 Miles

	(1) Estabs.	(2) Firms	(3) Emp.	(4) Personal Inc.	(5) GDP
MDP Nearby	-0.109	-0.078	-12.125**	-1.659	-2.152
	(0.198)	(0.233)	(4.093)	(0.964)	(1.310)
Obs	6,535	6,535	6,535	6,535	6,535
R2	0.995	0.993	0.995	0.986	0.993
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

Table A.14: Per Capita Effects on Counties within 25 Miles

	(1) Small Estabs.	(2) Large Estabs.	(3) Small Firms	(4) Large Firms	(5) Small Emp.	(6) Large Emp.
MDP Nearby	-0.138	0.029	-0.092	0.014	1.198	-13.323**
	(0.263)	(0.129)	(0.257)	(0.060)	(1.829)	(3.781)
Obs	6,535	6,535	6,535	6,535	6,535	6,535
R2	0.995	0.989	0.994	0.989	0.992	0.991
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	State	State	State	State	State	State

Note: Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001.
Controls include average age, education, female and minority proportions, and industry composition.

Appendix B

Chapter 3

B.1 Proofs for Maxmin Strategy

Proof of Lemma 1. Suppose $\mathbf{x}$ is maxmin for the divider that is wasteful. Then, there is a good j whose endowment is not fully exhausted. In particular, $\Sigma_{i=1}^{n} x_{ij} < \mathring{x}_j$. Let s_j be the unallocated amount of good j. Consider the alternative division $\tilde{\mathbf{x}}$, where, for each i, $\tilde{\mathbf{x}}_\mathbf{i} = \mathbf{x}_\mathbf{i} + (0, ..., \frac{s_j}{n}, ..., 0)$. Since preferences are strictly increasing in each good for each $i = 1, ..., n$, we have $u_n(\tilde{\mathbf{x}}_\mathbf{i}) > u_n(\mathbf{x}_i)$. This implies the worst allocation in $\tilde{\mathbf{x}}$ is better than the worst allocation in $\mathbf{x}$ - i.e., $\min_i\{u_n(\tilde{\mathbf{x}}_i)\} > \min_i\{u_n(\mathbf{x}_i)\}$. This is a contradiction. □

Proof of Lemma 2. Define $W(x)$ to be the set of indices of the worst bundles for n in the division $\mathbf{x}$, where $u_n(\mathbf{x}_i) = u_n(\mathbf{x}_j)$ for all $i, j \in W$. If the proposition is not true, then there exists a second worst allocation x_B in the divider's division such that $u_n(\mathbf{x}_B) > u_n(\mathbf{x}_j)$ for each $j \in W$. Let good l be any good where $x_{Bl} > 0$. There must be at least one such good otherwise $u_n(\mathbf{x}_B) = 0$ and it is certainly a worst bundle. Consider the alternative division $\tilde{\mathbf{x}}_\lambda$, where, for each $i \notin \{W, B\}$, $\tilde{\mathbf{x}}_{i\lambda} = \mathbf{x}_i$, $\tilde{\mathbf{x}}_B = \mathbf{x}_B - \lambda(0, ..., x_{Bl}, ..., 0)$ and $\tilde{\mathbf{x}}_j = \mathbf{x}_j + \lambda(0, ..., \frac{x_{Bl}}{|W|}, ..., 0)$. For λ close to zero $\tilde{\mathbf{x}}_\lambda$ is an admissible division. Since preferences are strictly increasing in each good for each $i = 1, ..., n$, we have $u_n(\tilde{\mathbf{x}}_j > u_n(\mathbf{x}_j)$ for $j \in W(\mathbf{x})$. We have increased the

value of all of the worst bundles and decreased the value of the 2nd worst bundle. Moreover, we can choose λ to be sufficiently small so that $\tilde{\mathbf{x}}_B$ is not the worst bundle in the new division and $W(\tilde{\mathbf{x}}) \subseteq W(x)$. Such a λ exists by continuity of preferences. However, this new division is admissible and $min_i\{u_n(\tilde{\mathbf{x}}_i)\} > min_i\{u_n(\mathbf{x}_i)\}$. This is a contradiction. □

Proof of Proposition 1. (Part 1) By Lemma 1 and 2, any division $\mathbf{x}$ is non-wasteful and satisfies

$$u_n(\mathbf{x}_1) = u_n(\mathbf{x}_2) = ...u_n(\mathbf{x}_n)$$

The divider's problem is therefore to

$$\max_{x \in \mathbb{R}_+^{Ln}} \quad u_n(\mathbf{x}_n) \quad \text{subject to} \quad \sum_i \mathbf{x}_i \;=\; \mathring{\mathbf{x}},$$
$$u_n(\mathbf{x}_n) \;=\; u_n(\mathbf{x}_i) \quad \text{for all } i = 1, \ldots, n-1.$$

The Lagrangian for the problem is

$$\begin{aligned} L(\mathbf{x}, \sigma, \lambda) \;&= u_n(x_{n1}, \ldots, x_{nL}) \\ &- \sum_{l=1}^{L} \sigma_l \left[\sum_{i=1}^{n} x_{il} - \mathring{x}_l \right] \\ &- \sum_{i=1}^{n-1} \lambda_i \big[\, u_n(x_{n1}, \ldots, x_{nL}) - u_n(x_{i1}, \ldots, x_{iL}) \,\big], \end{aligned}$$

where (σ, λ) are the Lagrange multipliers.

The first order conditions of the Lagrangian with respect to the instruments x are

$$1 - \Sigma_{i=1}^{n-1} \lambda_i \frac{\partial u_n(x_n)}{\partial x_l} = \sigma_l,$$

for $l = 1, ..., L$; and,

$$\lambda_i \frac{\partial u_n(\mathbf{x}_i)}{\partial x_l} = \sigma_l,$$

for $i = 1, ..., n-1$ and $l = 1, ..., L$.

We can write these conditions in terms of the divider's marginal rate of substitution. Let $MRS^n_{l,L} = \frac{\partial u_n / \partial x_l}{\partial u_n / \partial x_L}$. Then re-arranging the above, we have

$$MRS^n_{l,L}(\mathbf{x}_n) = \frac{\sigma_l}{\sigma_L}$$

for $l = 1, ..., L = 1$. In addition, for each $i = 1, ..., n-1$,

$$MRS^n_{l,L}(\mathbf{x}_i) = \frac{\sigma_l}{\sigma_L}$$

for l=1,...,L-1. Hence, bundles $(\mathbf{x}_1, ..., \mathbf{x}_n)$ satisfy

$$MRS^n_{l,L}(\mathbf{x}_n) = MRS^n_{l,L}(\mathbf{x}_i)$$

for each $i \neq n$. In addition to the above marginal conditions, the optimal constraints. If $\mathbf{x}_1, \mathbf{x}_2, ..., \mathbf{x}_n$ are not all the same, then strict convexity of the preferences allows us to improve the outcome for the divider. The equal split bundle satisfies all the above conditions and is therefore optimal. The divider's maxmin value is $u_n(\mathring{\mathbf{x}}/n)$.

The first chooser, Player 1, chooses the best possible bundle in any division - randomizing if he is indifferent. His maxmin value is bounded by his payoff at the equal split bundle, but depending on preferences can be lower. Player n can hold him to equal split value by proposing the equal split bundle. Thus, he cannot guarantee more. In some examples, the divider can choose a division of the goods where all choosers get a zero payoff.

Choosers 2 through $n-1$ cannot guarantee a payoff larger than 0. To see this, suppose Player n divides the endowment so that everything is in a single pile. Player 1 chooses this bundle leaving zero for everyone else. Since there is nothing these players

can do to prevent such an outcome, their maxmin payoff is zero and any strategy they choose is maxmin. □

Appendix C

Chapter 4

C.0.1 Instructions

Each participant is provided with a packet of instructions. All players receive Experiment 1, a copy of their assigned experiment, a set of scoring instructions, and scoring table.

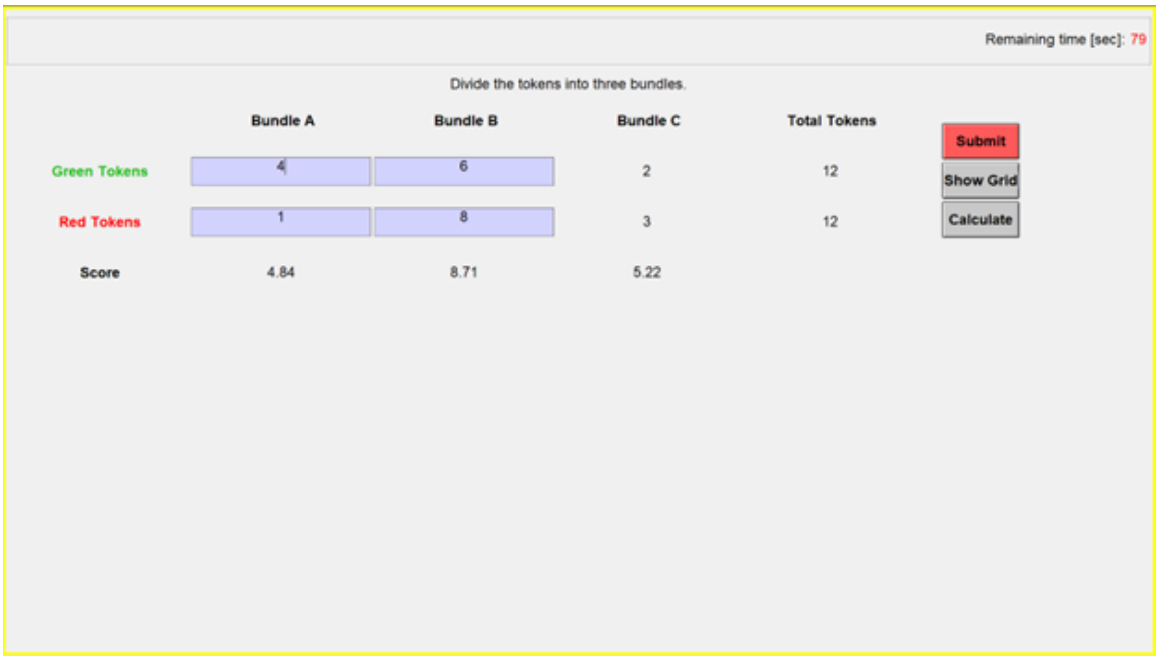
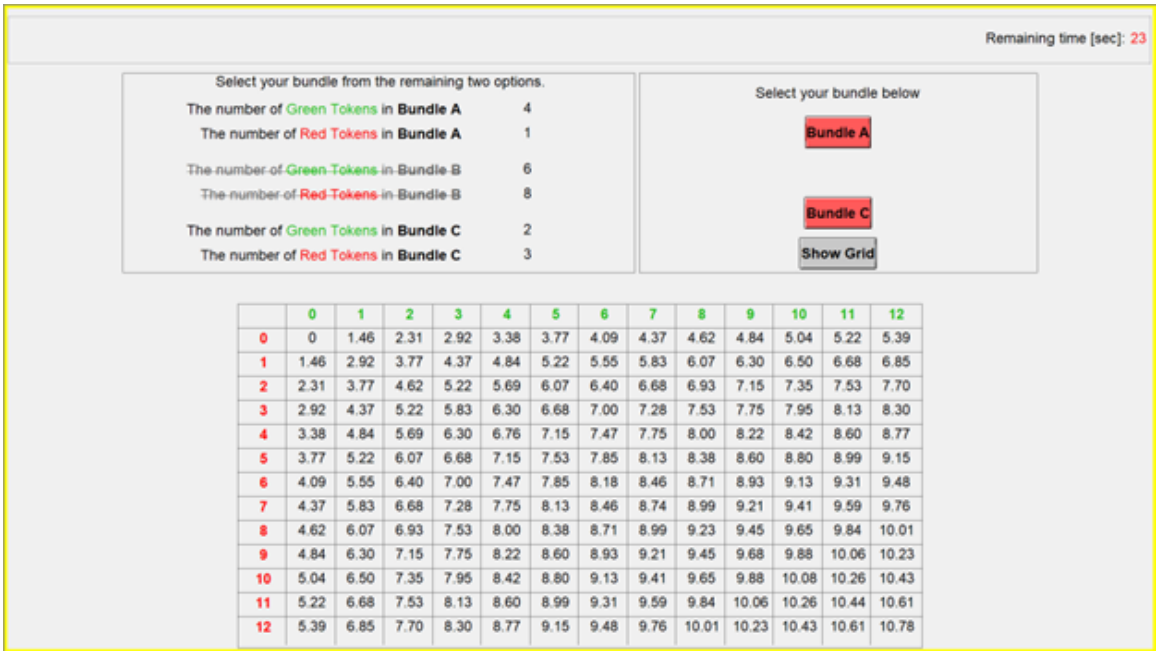


Figure C.1: Divider decision screen.

I provide bar graphs for payoff by treatment for each type below.



Figure C.2: First chooser screen.



	0	1	2	3	4	5	6	7	8	9	10	11	12
0	0	1.46	2.31	2.92	3.38	3.77	4.09	4.37	4.62	4.84	5.04	5.22	5.39
1	1.46	2.92	3.77	4.37	4.84	5.22	5.55	5.83	6.07	6.30	6.50	6.68	6.85
2	2.31	3.77	4.62	5.22	5.69	6.07	6.40	6.68	6.93	7.15	7.35	7.53	7.70
3	2.92	4.37	5.22	5.83	6.30	6.68	7.00	7.28	7.53	7.75	7.95	8.13	8.30
4	3.38	4.84	5.69	6.30	6.76	7.15	7.47	7.75	8.00	8.22	8.42	8.60	8.77
5	3.77	5.22	6.07	6.68	7.15	7.53	7.85	8.13	8.38	8.60	8.80	8.99	9.15
6	4.09	5.55	6.40	7.00	7.47	7.85	8.18	8.46	8.71	8.93	9.13	9.31	9.48
7	4.37	5.83	6.68	7.28	7.75	8.13	8.46	8.74	8.99	9.21	9.41	9.59	9.76
8	4.62	6.07	6.93	7.53	8.00	8.38	8.71	8.99	9.23	9.45	9.65	9.84	10.01
9	4.84	6.30	7.15	7.75	8.22	8.60	8.93	9.21	9.45	9.68	9.88	10.06	10.23
10	5.04	6.50	7.35	7.95	8.42	8.80	9.13	9.41	9.65	9.88	10.08	10.26	10.43
11	5.22	6.68	7.53	8.13	8.60	8.99	9.31	9.59	9.84	10.06	10.26	10.44	10.61
12	5.39	6.85	7.70	8.30	8.77	9.15	9.48	9.76	10.01	10.23	10.43	10.61	10.78

Figure C.3: Second chooser screen.

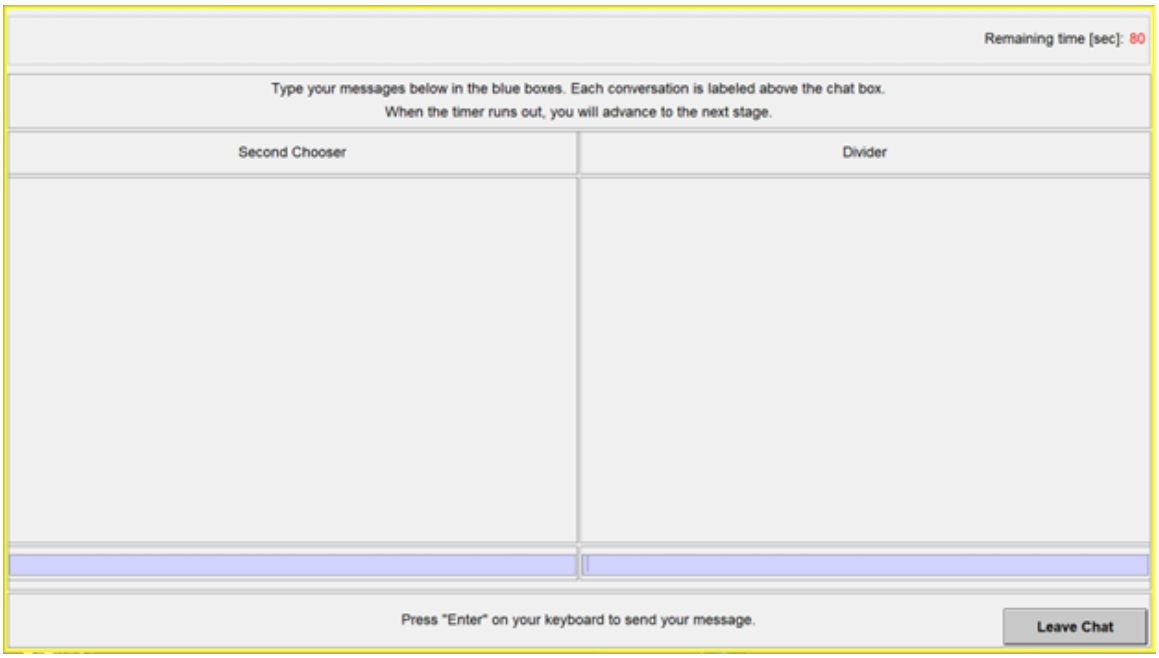


Figure C.4: Communication screen

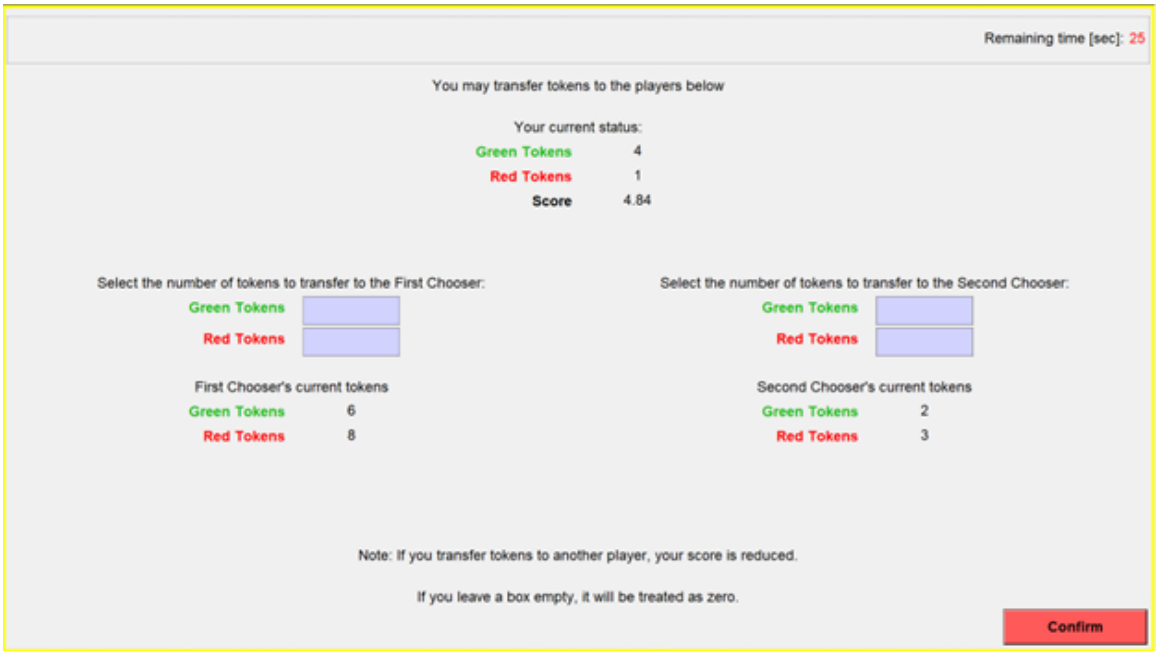


Figure C.5: Transfers screen

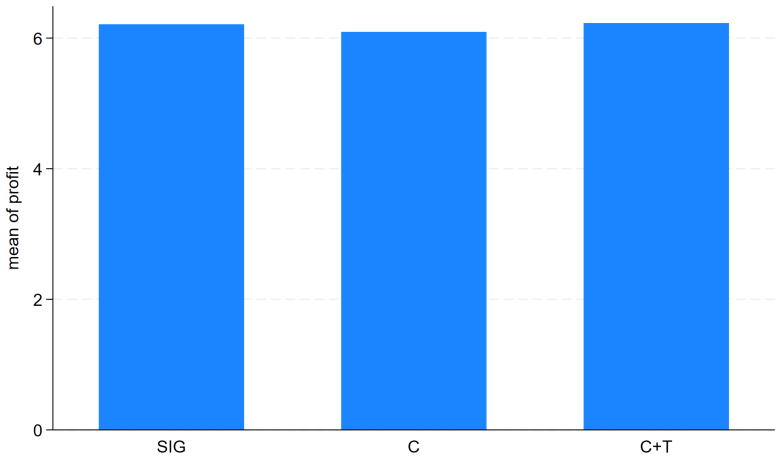


Figure C.6: Payoffs for the Divider by Treatment

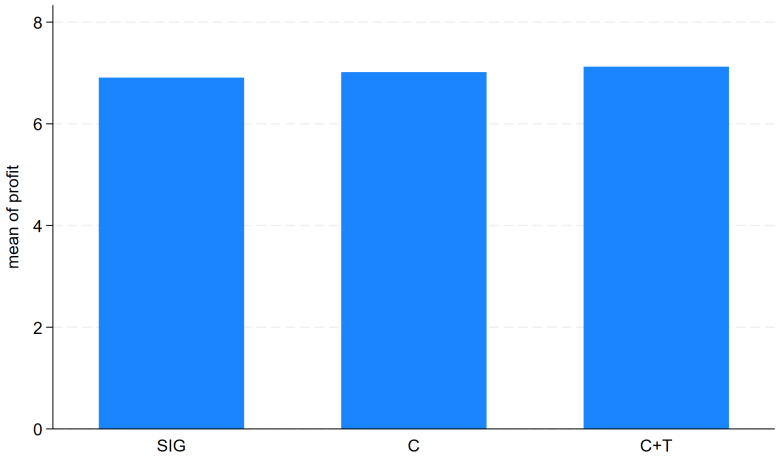


Figure C.7: Payoffs for the First Chooser by Treatment

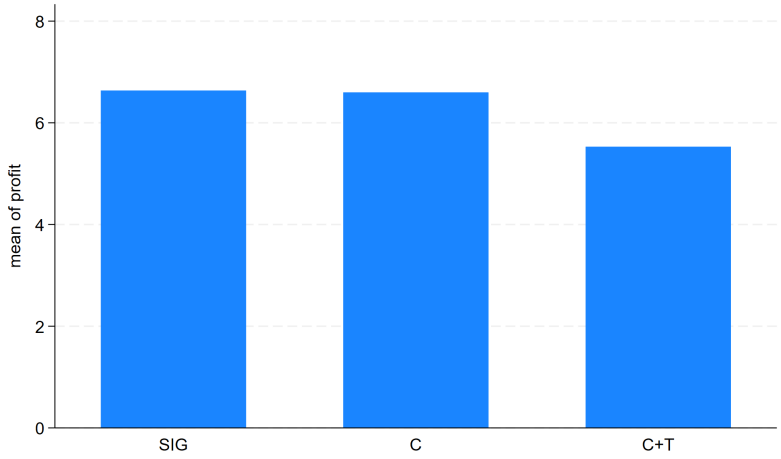


Figure C.8: Payoffs for the Second Chooser by Treatment

Instructions for each experiment are found below:

C.0.2 Repeated Games

The experiment is designed to approximate a one-shot environment. If participants instead treated the interaction as a repeated game, they would face an incentive to sustain the equal-split allocation, earning approximately $6 per round. However, this cooperative outcome would not be subgame perfect, as participants would have an incentive to deviate in the final period by engaging in collusion, increasing their payoff to approximately $8 in that round.

A simple repeated-game framework suggests that the gains from cooperation may be insufficient to fully offset the temptation to deviate in the final period. Moreover, if participants anticipate collusion throughout the game and adjust their behavior accordingly, this can undermine cooperative outcomes. In particular, widespread collusion would disadvantage the second chooser, who could receive a payoff of $0. Under this scenario, total earnings across the three paid rounds would fall from approximately $18 under sustained equal-split behavior to roughly $16.

Introduction

Thank you for participating in this study. Please follow the instructions carefully. At any time, please feel free to raise your hand if you have a question. At the end of the session, you will be paid your earnings privately and in cash.

You have been randomly assigned an ID number for this session. You will make decisions using a computer. You will never be asked to reveal your identity to anyone. Your name will never be associated with any of your decisions. To keep your decisions private, please do not reveal your choices or otherwise communicate with any other participant. Importantly, please refrain from verbally reacting to events that occur.

Today's study consists of the following parts:

- Experiment 1
- Followed by one of the following: Experiment 2, 3, 4, or 5
- Concluding with a brief questionnaire.

We will proceed through the written materials together. Please do not enter any decisions on the computer until instructed to do so.

Instructions for Experiment 1

Please refer to your computer screen while we read the instructions.

We would like you to make a decision for each of 10 scenarios. Each scenario involves a choice between playing a lottery that pays either $4 or $0 according to specified chances (Option A) or receiving $2 for sure (Option B).

You will notice that the only differences across scenarios are the chances of receiving the high or low prize for the lottery. Later in the session, ONE of the 10 scenarios will be selected at random and you will be paid according to your decision for this selected scenario ONLY. Each scenario has an equal chance of being selected.

Please consider your choice for each scenario carefully. Since you do not know which scenario will be played out, it is in your best interest to treat each scenario as if it will be the one used to determine your earnings.

Before making decisions, are there any questions?

Please proceed to entering decisions on your computer. Once you are ready to submit your decisions, please click the "Submit" button.

IRB NUMBER: UTK IRB-23-07884-XM
IRB APPROVAL DATE: 08/27/2024

Figure C.9: Instructions for Experiment 1

Experimental Instructions (Treatment 2)

This experiment focuses on players' decisions. You will be playing a game called "divide-and-choose" with other players. The decisions you make in the experiment will determine your earnings.

The experiment will be conducted using computers. Please avoid talking or communicating with the other participants during the experiment.

The experiment consists of one practice period before the paid periods begin.

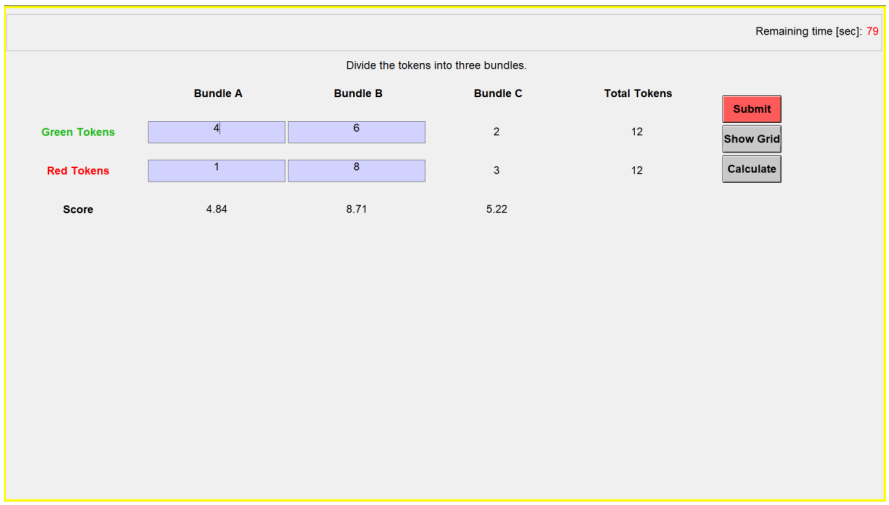
One player divides the tokens into three bundles. Then one at a time, the other players choose their bundles.

Here are the rules of the game:

1. At the beginning of each round, each player is randomly matched with two other players to form a group of three.
2. Each player in a group is assigned to one of three roles: Divider, First Chooser, and Second Chooser.
3. The Divider splits 12 Green Tokens and 12 Red Tokens into three bundles: Bundle A, Bundle B, and Bundle C.
4. The First Chooser selects one of the three bundles. That bundle is no longer available to the other players.
5. The Second Chooser selects one of the two remaining bundles.
6. The Divider receives the last remaining bundle.
7. Each player receives a score based on the tokens they have received. See the Scoring Instructions and Scoring Table for more information.
8. See the Scoring Instructions to see how your overall earnings are calculated.

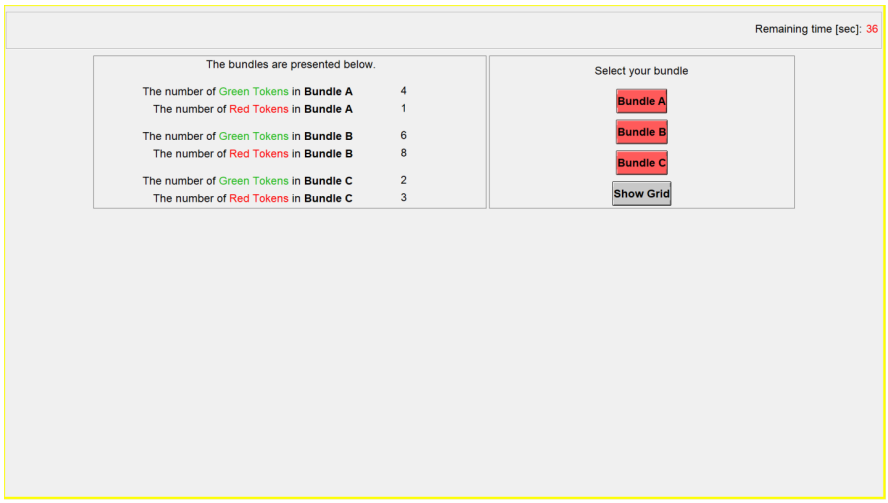
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Figure C.10: Instructions for Experiment 2



Above is an example of the **Divider's** screen. They allocate tokens to Bundles A and B – the remaining tokens are assigned to Bundle C. The "calculate" button shows the number of tokens assigned to Bundle C and the score for all three bundles.

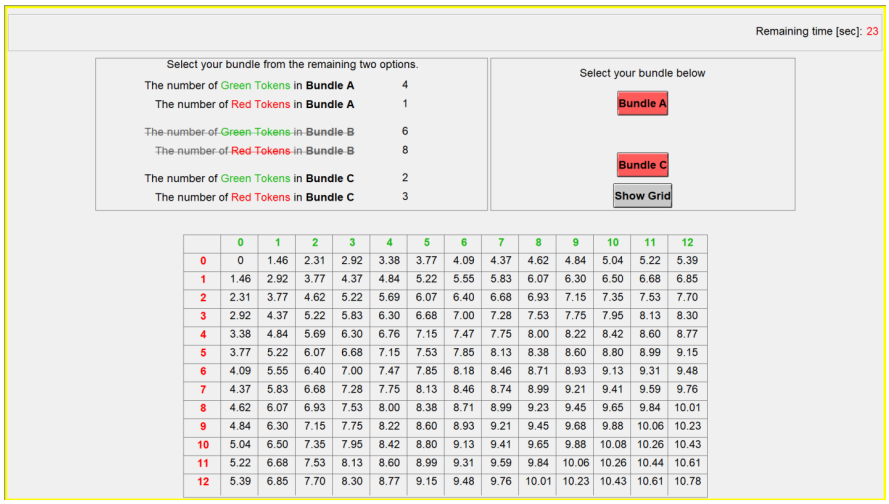
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In the image, you see the **First Chooser's** screen. This player selects one of the three bundles.

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Below is an example of the **Second Chooser's** screen. In this example, the **First Chooser** selected Bundle B and it is removed from the options. The **Second Chooser** may select between Bundle A and Bundle C. The **Divider** will receive the remaining bundle. Note on this screen, the player clicked the "Show Grid" button to display the scoring table.



	0	1	2	3	4	5	6	7	8	9	10	11	12
0	0	1.46	2.31	2.92	3.38	3.77	4.09	4.37	4.62	4.84	5.04	5.22	5.39
1	1.46	2.92	3.77	4.37	4.84	5.22	5.55	5.83	6.07	6.30	6.50	6.68	6.85
2	2.31	3.77	4.62	5.22	5.69	6.07	6.40	6.68	6.93	7.15	7.35	7.53	7.70
3	2.92	4.37	5.22	5.83	6.30	6.68	7.00	7.28	7.53	7.75	7.95	8.13	8.30
4	3.38	4.84	5.69	6.30	6.76	7.15	7.47	7.75	8.00	8.22	8.42	8.60	8.77
5	3.77	5.22	6.07	6.68	7.15	7.53	7.85	8.13	8.38	8.60	8.80	8.99	9.15
6	4.09	5.55	6.40	7.00	7.47	7.85	8.18	8.46	8.71	8.93	9.13	9.31	9.48
7	4.37	5.83	6.68	7.28	7.75	8.13	8.46	8.74	8.99	9.21	9.41	9.59	9.76
8	4.62	6.07	6.93	7.53	8.00	8.38	8.71	8.99	9.23	9.45	9.65	9.84	10.01
9	4.84	6.30	7.15	7.75	8.22	8.60	8.93	9.21	9.45	9.68	9.88	10.06	10.23
10	5.04	6.50	7.35	7.95	8.42	8.80	9.13	9.41	9.65	9.88	10.08	10.26	10.43
11	5.22	6.68	7.53	8.13	8.60	8.99	9.31	9.59	9.84	10.06	10.26	10.44	10.61
12	5.39	6.85	7.70	8.30	8.77	9.15	9.48	9.76	10.01	10.23	10.43	10.61	10.78

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Experimental Instructions (Treatment 3)

This experiment focuses on players' decisions. You will be playing a game called "divide-and-choose" with other players. The decisions you make in the experiment will determine your earnings.

The experiment will be conducted using computers. Please avoid talking or communicating with the other participants during the experiment.

The experiment consists of one practice period before the paid periods begin.

One player divides the tokens into three bundles. Then one at a time, the other players choose their bundles.

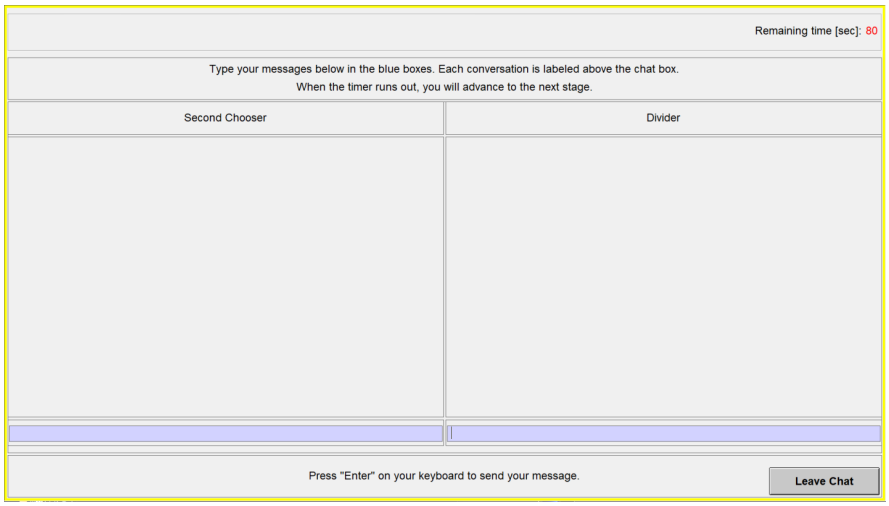
Here are the rules of the game:

1. At the beginning of each round, players are randomly matched with two other players.
2. Each player in a group is assigned to one of three roles: Divider, First Chooser, and Second Chooser.
3. Players communicate with other players in their group using the in-game chat feature. These are private communication channels. For example, the First Chooser cannot see the chats between the Divider and Second Chooser. This stage lasts for 90 seconds.
4. The Divider splits 12 Green Tokens and 12 Red Tokens into three bundles: Bundle A, Bundle B, and Bundle C.
5. The First Chooser selects one of the three bundles. That bundle is no longer available to the other players.
6. The Second Chooser selects one of the two remaining bundles.
7. The Divider receives the last remaining bundle.
8. Each player receives a score based on the tokens they have received. See the Scoring Instructions and Scoring Table for more information.
9. See the Scoring Instructions to see how your overall earnings are calculated.

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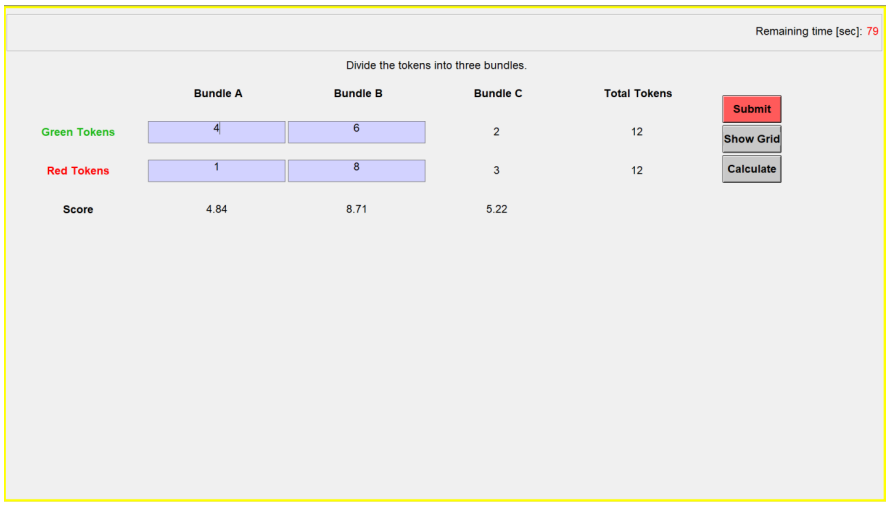
Figure C.11: Instructions for Experiment 3

Please note that this study requires professionalism and appropriate language usage. We kindly request all participants to communicate using respectful and suitable language throughout the duration of the study. Thank you for your cooperation.



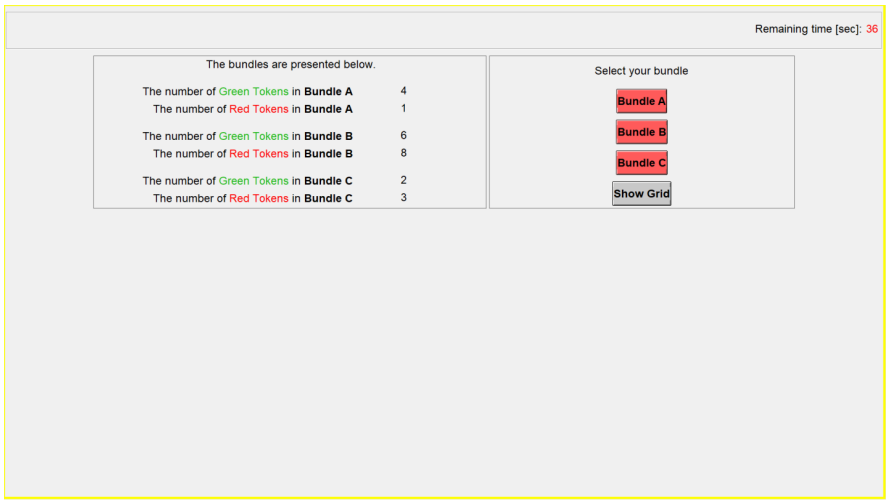
Here is an example of the **First Chooser's** chat screen. In the left box, they chat with the **Second Chooser**. In the right box, they chat with the **Divider**. Press "Enter" on your keyboard to send messages.

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Above is an example of the **Divider's** screen. They allocate tokens to Bundles A and B – the remaining tokens are assigned to Bundle C. The "calculate" button shows the number of tokens assigned to Bundle C see the score for all three bundles.

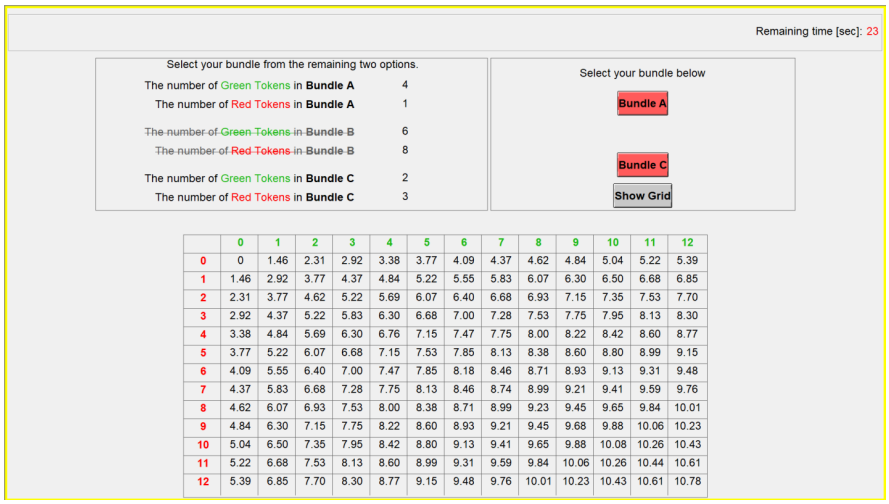
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In the image, you see the **First Chooser's** screen. This player selects one of the three bundles.

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Below is an example of the **Second Chooser's** screen. In this example, **the First Chooser** selected Bundle B and it is removed from the options. The **Second Chooser** may select between Bundle A and Bundle C. The **Divider** will receive the remaining bundle. Note on this screen, the player clicked the "Show Grid" button to display the scoring table.



	0	1	2	3	4	5	6	7	8	9	10	11	12
0	0	1.46	2.31	2.92	3.38	3.77	4.09	4.37	4.62	4.84	5.04	5.22	5.39
1	1.46	2.92	3.77	4.37	4.84	5.22	5.55	5.83	6.07	6.30	6.50	6.68	6.85
2	2.31	3.77	4.62	5.22	5.69	6.07	6.40	6.68	6.93	7.15	7.35	7.53	7.70
3	2.92	4.37	5.22	5.83	6.30	6.68	7.00	7.28	7.53	7.75	7.95	8.13	8.30
4	3.38	4.84	5.69	6.30	6.76	7.15	7.47	7.75	8.00	8.22	8.42	8.60	8.77
5	3.77	5.22	6.07	6.68	7.15	7.53	7.85	8.13	8.38	8.60	8.80	8.99	9.15
6	4.09	5.55	6.40	7.00	7.47	7.85	8.18	8.46	8.71	8.93	9.13	9.31	9.48
7	4.37	5.83	6.68	7.28	7.75	8.13	8.46	8.74	8.99	9.21	9.41	9.59	9.76
8	4.62	6.07	6.93	7.53	8.00	8.38	8.71	8.99	9.23	9.45	9.65	9.84	10.01
9	4.84	6.30	7.15	7.75	8.22	8.60	8.93	9.21	9.45	9.68	9.88	10.06	10.23
10	5.04	6.50	7.35	7.95	8.42	8.80	9.13	9.41	9.65	9.88	10.08	10.26	10.43
11	5.22	6.68	7.53	8.13	8.60	8.99	9.31	9.59	9.84	10.06	10.26	10.44	10.61
12	5.39	6.85	7.70	8.30	8.77	9.15	9.48	9.76	10.01	10.23	10.43	10.61	10.78

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Experimental Instructions (Treatment 5)

This experiment focuses on players' decisions. You will be playing a game called "divide-and-choose" with other players. The decisions you make in the experiment will determine your earnings.

The experiment will be conducted using computers. Please avoid talking or communicating with the other participants during the experiment unless instructed to do so.

The experiment consists of one practice period before the paid periods begin.

One player divides the tokens into three bundles. Then one at a time, the other players choose their bundles.

Here are the rules of the game:

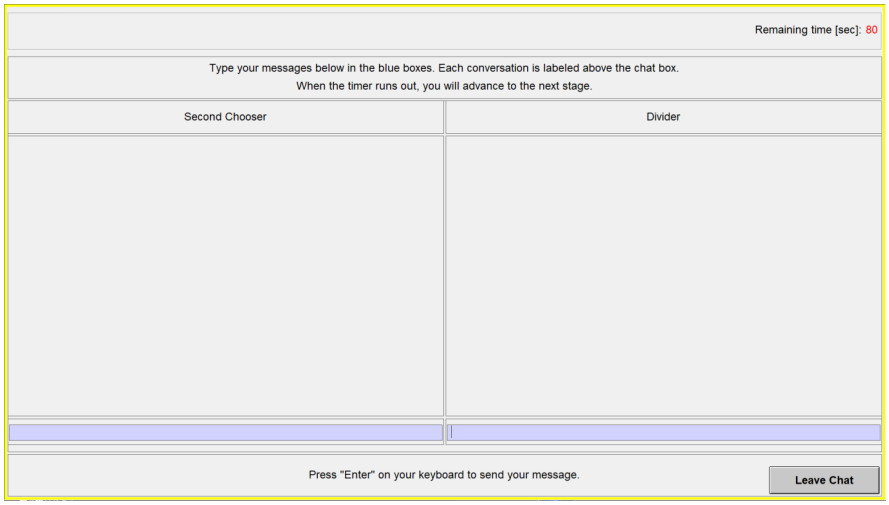
1. At the beginning of each round, players are randomly matched with two other players.
2. Each player in a group is assigned to one of three roles: Divider, First Chooser, and Second Chooser.
3. Players communicate with other players in their group using the in-game chat feature. These are private communication channels. For example, the First Chooser cannot see the chats between the Divider and Second Chooser. This stage lasts for 90 seconds.
4. The Divider splits 12 Green Tokens and 12 Red Tokens into three bundles: Bundle A, Bundle B, and Bundle C.
5. The First Chooser selects one of the three bundles. That bundle is no longer available to the other players.
6. The Second Chooser selects one of the two remaining bundles.
7. The Divider receives the last remaining bundle.
8. Each player receives a score based on the tokens they have received. See the Scoring Instructions and Scoring Table for more information.
9. Players may send any amount of their earned tokens to their groupmates.

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Figure C.12: Instructions for Experiment 5

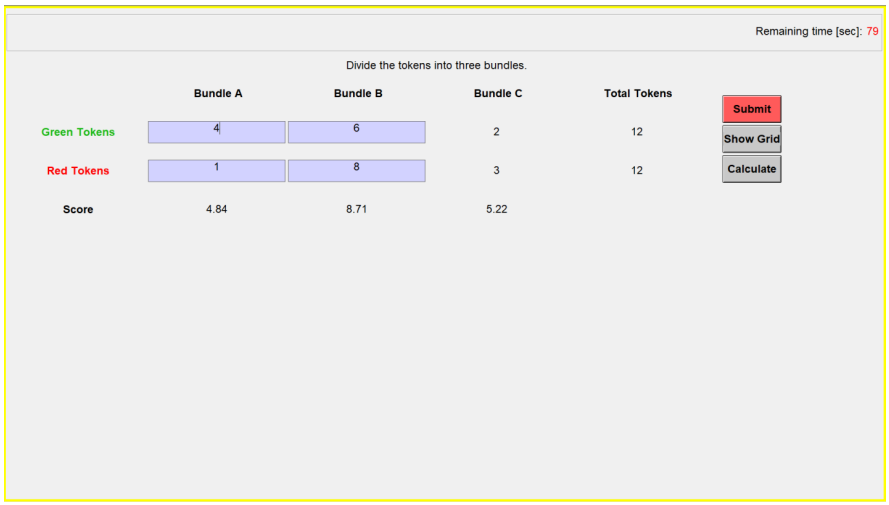
10. Payoff scores are recalculated for the round. This new score is your earnings for the round.
11. See the Scoring Instructions to see how your overall earnings are calculated.

Please note that this study requires professionalism and appropriate language usage. We kindly request all participants to communicate using respectful and suitable language throughout the duration of the study. Thank you for your cooperation.



Here is an example of the **First Chooser's** chat screen. In the left box, they chat with the Second Chooser. In the right box, they chat with the Divider. Press "Enter" on your keyboard to send messages.

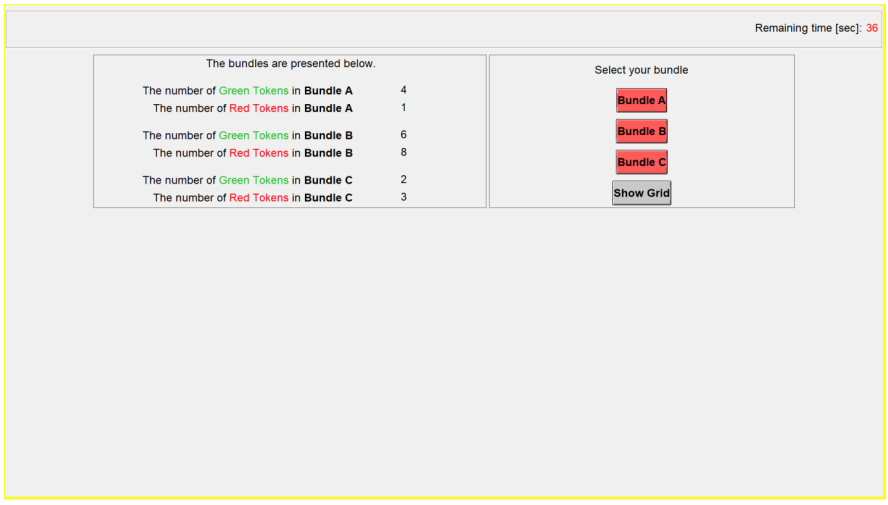
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Above is an example of the **Divider's** screen. They allocate tokens to Bundles A and B – the remaining tokens are assigned to Bundle C. The "calculate" button shows the number of tokens assigned to Bundle C and the score for all three bundles.

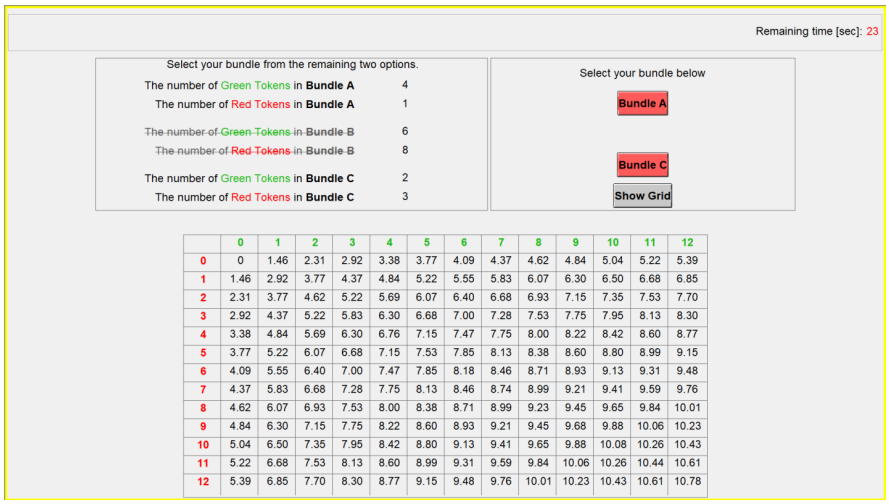
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In the image, you see the **First Chooser's** screen. This player selects one of the three bundles.



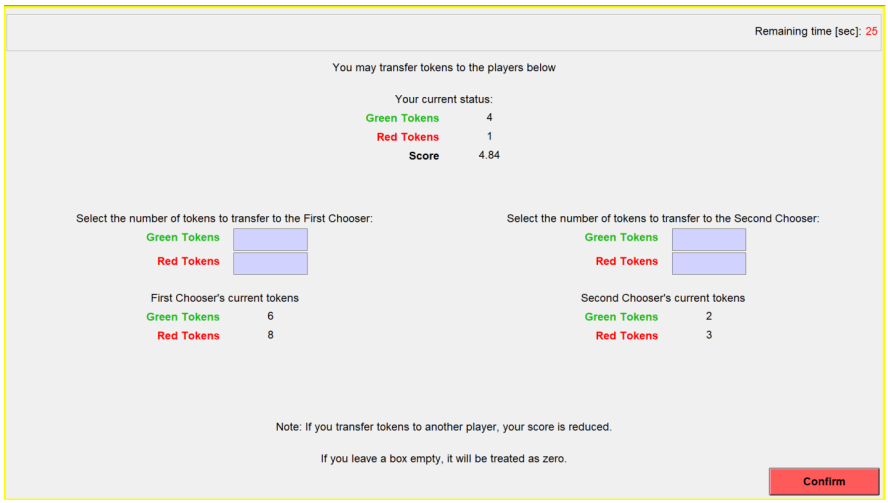
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Below is an example of the **Second Chooser's** screen. In this example, the **First Chooser** selected Bundle B and it is removed from the options. The **Second Chooser** may select between Bundle A and Bundle C. The **Divider** will receive the remaining bundle. Note on this screen, the player clicked the "Show Grid" button to display the scoring table.



	0	1	2	3	4	5	6	7	8	9	10	11	12
0	0	1.46	2.31	2.92	3.38	3.77	4.09	4.37	4.62	4.84	5.04	5.22	5.39
1	1.46	2.92	3.77	4.37	4.84	5.22	5.55	5.83	6.07	6.30	6.50	6.68	6.85
2	2.31	3.77	4.62	5.22	5.69	6.07	6.40	6.68	6.93	7.15	7.35	7.53	7.70
3	2.92	4.37	5.22	5.83	6.30	6.68	7.00	7.28	7.53	7.75	7.95	8.13	8.30
4	3.38	4.84	5.69	6.30	6.76	7.15	7.47	7.75	8.00	8.22	8.42	8.60	8.77
5	3.77	5.22	6.07	6.68	7.15	7.53	7.85	8.13	8.38	8.60	8.80	8.99	9.15
6	4.09	5.55	6.40	7.00	7.47	7.85	8.18	8.46	8.71	8.93	9.13	9.31	9.48
7	4.37	5.83	6.68	7.28	7.75	8.13	8.46	8.74	8.99	9.21	9.41	9.59	9.76
8	4.62	6.07	6.93	7.53	8.00	8.38	8.71	8.99	9.23	9.45	9.65	9.84	10.01
9	4.84	6.30	7.15	7.75	8.22	8.60	8.93	9.21	9.45	9.68	9.88	10.06	10.23
10	5.04	6.50	7.35	7.95	8.42	8.80	9.13	9.41	9.65	9.88	10.08	10.26	10.43
11	5.22	6.68	7.53	8.13	8.60	8.99	9.31	9.59	9.84	10.06	10.26	10.44	10.61
12	5.39	6.85	7.70	8.30	8.77	9.15	9.48	9.76	10.01	10.23	10.43	10.61	10.78

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This image is an example of the transfer screen. You may transfer tokens to other players in your group. **Your score is recalculated after this screen.**

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Scoring Instructions (Treatments 2, 3, 4, and 5)

Let's discuss how your score is calculated in each round. Your score depends on the number of Green Tokens, GT, and Red Tokens, RT, in your bundle. The formula to calculate your score is:

$$Score = 2.1 * \ln [1 + GT + RT + (GT \; x \; RT)]$$

You don't need to worry about doing the math yourself! A **Scoring Table** is provided with all possible scores for different combinations of tokens.

This is a complicated math function, so let's focus on the part of the equation that you can control:

$$GT + RT + (GT \; x \; RT)$$

The other parts of the equation are "modifiers" for your score.

Here are some key points about this equation to help you make decisions:

1. Any time your bundle includes any tokens, green or red, your score will be greater than zero.
2. Receiving an additional token is always better. For example:
    - 6 GT and 0 RT gives a score of 4.09.
    - 7 GT and 0 RT gives a score of 4.37.
3. GT and RT work together to increase your score. For example:
    - A bundle with 0 GT and 3 RT gives a score of 2.92.
    - A bundle with 1 GT and 2 RT gives a score of 3.77.

Don't think too much about the function. This is all handled for you in the **Scoring Table**. Focus on understanding how your choices affect your score.

At the experiment's conclusion, your earnings will be determined by your performance. Three rounds will be randomly selected for payment – one for each role (**First Chooser**, **Second Chooser**, and **Divider**). The total payout will be the sum of your earnings from these randomly selected rounds. Since you do not know which rounds will be selected, it is in your best interest to make decisions to maximize your earnings in every round.

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Figure C.13: Scoring Instructions

Scoring Table

Use the scoring table below to calculate your score.

	0	1	2	3	4	5	6	7	8	9	10	11	12
0	0.00	1.46	2.31	2.92	3.38	3.77	4.09	4.37	4.62	4.84	5.04	5.22	5.39
1	1.46	2.92	3.77	4.37	4.84	5.22	5.55	5.83	6.07	6.30	6.50	6.68	6.85
2	2.31	3.77	4.62	5.22	5.69	6.07	6.40	6.68	6.93	7.15	7.35	7.53	7.70
3	2.92	4.37	5.22	5.83	6.30	6.68	7.00	7.28	7.53	7.75	7.95	8.13	8.30
4	3.38	4.84	5.69	6.30	6.76	7.15	7.47	7.75	8.00	8.22	8.42	8.60	8.77
5	3.77	5.22	6.07	6.68	7.15	7.53	7.85	8.13	8.38	8.60	8.80	8.99	9.15
6	4.09	5.55	6.40	7.00	7.47	7.85	8.18	8.46	8.71	8.93	9.13	9.31	9.48
7	4.37	5.83	6.68	7.28	7.75	8.13	8.46	8.74	8.99	9.21	9.41	9.59	9.76
8	4.62	6.07	6.93	7.53	8.00	8.38	8.71	8.99	9.23	9.45	9.65	9.84	10.01
9	4.84	6.30	7.15	7.75	8.22	8.60	8.93	9.21	9.45	9.68	9.88	10.06	10.23
10	5.04	6.50	7.35	7.95	8.42	8.80	9.13	9.41	9.65	9.88	10.08	10.26	10.43
11	5.22	6.68	7.53	8.13	8.60	8.99	9.31	9.59	9.84	10.06	10.26	10.44	10.61
12	5.39	6.85	7.70	8.30	8.77	9.15	9.48	9.76	10.01	10.23	10.43	10.61	10.78

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Figure C.14: Payoff Table

Vita

Thomas Verville grew up in Portage, Michigan, where he attended Portage Public Schools. He developed an early interest in mathematics, which led him to pursue a Bachelor's degree in Economics at Kalamazoo College, a private liberal arts institution. Following graduation, he worked at Hormel Foods in Austin, Minnesota, before returning to Michigan to join a small investment firm affiliated with United Bank of Michigan. Seeking to deepen his training in economics, he enrolled in the Applied Economics Master's program at Western Michigan University. During this time, mentorship from faculty including Mark Wheeler, Michael Ryan, Donald Meyer, and Matthew Higgins shaped his academic trajectory and strengthened his interest in economic research. With their encouragement, he pursued doctoral study and was admitted to the University of Tennessee, Knoxville. At UTK, his academic interests expanded to include game theory and public economics. Thomas Verville is currently on the academic job market and pursuing opportunities for the next stage of his career.