The Interaction of Political Persuasion, Taxation, and Inequality: A Laboratory Study

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1 Problem Statement

We study the interaction of economic inequality and political persuasion. In particular, we ask to what extent access to channels of political persuasion (e.g., media) can reinforce inequality and generate a suboptimal size of the public sector. We design a laboratory experiment where each person in a nine-person society performs a real effort task and gets assigned gross (pretax) income from a fixed, highly skewed distribution. Then, depending on the treatment, citizens have the opportunity to communicate with other citizens in an open forum. After reading the messages, all citizens vote, declaring their preferences over tax policy alternatives, and the median desired tax is implemented.

We have three treatment arms: (i) whether income inequality is generated by chance or primarily by performance; (ii) whether citizens have access to channels of persuasion (no media, free access media, costly access to media); and (iii) whether the message includes the information about the sender. This experiment design allows us to test whether unequal opportunity to message can reinforce income inequality by changing the political preferences of lower-income citizens. It also allows us to assess whether this impact exists when the income level of the messenger is disclosed.

2 Contribution

The proposed study contributes to the broad literature on political persuasion. In particular, the effect of communication on taxation choices within groups of people with full information but unequal access to messaging has not received much attention. The existing literature emphasizes the role of communication as a means to transmit objective information (that may be true or false) in an attempt to influence others' choices. In this vein, Petrova (2008) proposes a model in which the rich members of a society can bribe the media to underreport the efficiency of public spending to convince the rest of the voters to favor lower taxes. In accordance with the model's predictions, she presents country-level evidence showing that higher inequality is associated with a higher degree of media capture in strong democracies but has no such relationship in autocracies and that a higher degree of media capture is associated with lower levels of public spending. Similarly, Roemer (1998) suggests that the rich can use the media – which they control – to argue that high levels of redistribution would lead to inefficiency in allocating their scarce and valuable resources (such as their high-productivity labor). We differentiate our study by providing agents with full, reliable information. In this setup, any effect of communication between agents cannot be explained by the manipulation of some private knowledge.

One potential explanation for why, even with full information, messages can be persuasive is social preferences. Therefore, another relevant area of research is literature on social norms and, specifically, people's preferences regarding income distributions. The potential purpose of messaging might be to influence these preferences. In fact, subjects in laboratory settings that rule out such considerations do not behave in a purely payoff-maximizing manner; rather, they at least tend to resist both complete equality and extreme inequality. For example, Durante, Putterman and Van der Weele (2014) find that subjects are willing to pay to achieve greater income equality among their fellow players, even when redistribution personally gives them no monetary benefit. Moreover, a large body of literature (Bicchieri and Xiao, 2009; Xiao and Houser, 2007; Ratchford, Victor and Pew, 2019) finds that decision-makers in dictator games prefer giving at least some money to their partners, although payoffs rarely turn out completely equal.

Of course, the benchmark for fairness in laboratory experiments need not be the egalitarian or efficient outcome. Communication might be used to influence others' beliefs on what constitutes a "fair" benchmark. Since our setting is somewhat cooperative, the literature suggests that communication could be used to induce mutually beneficial strategies ((Bochet, Page and Putterman, 2005; Cardenas, 2003; Gantner, Horn and Kerschbamer, 2019). However, the literature has not explored this possibility extensively in the context of unequal access to messaging. Our setting allows this effect to manifest in, for instance, situations when, under the guise of advocating for a better social norm, the rich try to induce a better outcome for themselves.[1]

3 Model

At this stage, we are still developing a model that can be analytically solved. However, the experiment is underpinned by some theoretical justifications, which we describe below. The taxation in our experiment closely resembles the first-order public goods problem. It reflects features of a public sector financed by taxes that can be (1) re-distributive and (2) create additional surplus. Therefore, the voting decision encapsulates the allocation of resources between some private income-generating activity and an activity that creates public goods. We modify the model outlined in (Kamei, Putterman and Tyran, 2019), among other things, by including messaging in the decision-making process.

Consider a game with two sub-periods: messaging and voting. In the first sub-period, each individual i can send a signal m_i to others for some payment C, advocating for a particular tax level t. In the second period, every individual observes the signals and then votes v_i on which tax should be implemented. Formally, the final payoff is given by

[1]The cost of communication is a barrier to itself in our setup. In most of the mentioned studies, communication was not costly, presumably facilitating its positive effects. Kriss, Blume and Weber (2016) explore how communication costs affect its use and effectiveness by implementing a public goods game with three types of communication: free, costly, and subsidized. They find that requiring subjects to pay even a small amount for communication results in substantially fewer messages than when communication was free. As a result, there was less coordination, thus leading to lower contributions and payoffs. However, if communication cost was small, as when communication was partially subsidized, the minimum effort level was significantly higher than without communication; in other words, coordination was higher. Thus, these results indicate that communication significantly decreases when it is costly, even if its benefits outweigh the costs. The primary implication for our study is that rich people, for whom sending a message is relatively cheap, are not expected to decrease their messaging activity. In contrast, a poor portion of the population is expected to find communication costly and avoid sending messages altogether.

$$Y_i(\hat{t}, \omega_i, m_i) = (1-\hat{t})\omega_i + D(\hat{t}\sum_i^N \omega_i) + \mathbb{1}_{m_i \neq \emptyset} C \tag{1}$$

where ω_i is the wealth of individual i, $\hat{t}$ is the median tax chosen after voting, and m_i is the message of individual i. $D(.)$ is a function that aggregates taxes, creates some public good, and redistributes it. We set it up so that each individual has their own payoff-optimal t_i^* and social surplus is maximized at some $\tilde{t}$.

Messaging. One important consideration is the message space $\mathcal{M}$. In our experiment, we make almost no restrictions in this regard, only requiring each message to be of reasonable length and adhere to the following structure: "*Vote for ... tax because ...*". This allows each participant to come up with any justification related to social efficiency, norms, fairness, etc.[2] Our primary focus, however, is not on the content of the messages but on the decision to message itself.

$$m_i^* = \underset{m_i \in \mathcal{M} \cup \{\emptyset\}}{\arg\max} \{Y_i(.)\} \tag{2}$$

The only reason why a non-empty message can be chosen is that (i), the perceived probability of not being a median voter is strictly positive and (ii) the optimal message m_i^* is expected to be persuasive enough to justify the cost C. The latter fact highlights a *crucial* assumption for our experiment: messages can alter voting behavior of others to benefit the sender.

Voting. The following mechanism helps us justify this *crucial* assumption. Each individual has both preferences for the payoff dictated by t_i^*, personal optimal tax, and social outcome (Gantner, Horn and Kerschbamer, 2019). The social outcome here is the final income distribution, which is judged based on fairness concerns, equity, and norms. Therefore, each voting decision v_i can be described as

$$v_i = \underset{t_i \in T}{\arg\max} \{U(Y_i(t_i), g(Y_{j\neq i}|m_{j\neq i}))\} \tag{3}$$

where $U(.)$ represents the preferences for both individual payoff and the payoffs of others. The exact manner in which the latter is incorporated can be further described by $g(.)$, which depends on the messages received.

Hypotheses

Each element of the theoretical structure underlined above can be directly tested in our experimental setup. We focus our attention on the following primary hypothesis. Hypotheses 2 and 3 are necessary conditions to prove it. Hypothesis 3 speculates on potential remedy if the primary hypothesis does indeed manifest.

HYPOTHESIS [Primary]. *Unequal access to messaging can be leveraged to ensure better voting outcomes.*

The difference between unequal and no communication is exactly the influence the rich have secured through messaging. In turn, the median vote is expected to be consistently more favorable toward the group with an outsized presence in messaging.

[2]There might be some concerns related to rational inattention when there are too many messages (Maćkowiak, Matějka and Wiederholt, 2023). We do not expect it to be a problem even if $C = 0$ since there are only 9 participants in each iteration of the experiment. Thus, the amount of messages is naturally limited.

HYPOTHESIS 1 [Secondary]. *Voting decisions can be influenced by the income distribution of others.*

We expect Treatment Branch (i) to induce some additional demand for fairness when the initial income is assigned randomly. When this is not the case, and there is no messaging, any deviations from t^* could only be explained by some preference for equity.

HYPOTHESIS 2 [Secondary]. *Voters can be persuaded by the messages received.*

Treatment Branch (ii) is constructed to isolate the effect of communication on outcomes. If results for free communication or unequal communication are consistently different from no communication, it implies that agents do indeed incorporate signals into their $g(.)$.

HYPOTHESIS 3 [Secondary]. *Disclosure can mitigate the effects of unequal access to messaging.*

A logical follow-up to the primary hypothesis is considering how the problem can be remedied.[3] Since even in an unequal environment, messaging is accessible to a portion of the distribution, revealing the sender's identity exactly identifies their payoff preferences. This information can then either be used in a simple manner, e.g., for out-group discrimination, or in a more sophisticated analysis of the message content. Both could influence how persuasive each message is.

4 Experimental design

Experimental sessions will be conducted in the LEEPS lab at UCSC. Each session will involve 9 participants and last 60 minutes. Sessions will begin with instructions that appear on participants' computer screens and are simultaneously read aloud by the experimenter. Subjects will be informed they will receive a show-up fee plus an additional payoff that will depend on the experiment's outcome. The core of the experiment will consist of three parts, which we refer to as "Part 1", "Part 2", and "Part 3". Near the end of the experiment, one of the rounds will be randomly selected for payment. The timing of each session is illustrated in Figure 1.

We start Part 1 by telling subjects that each will be assigned one of 9 possible wealth levels, ranging from 9 to 125 experimental currency units (ECUs). Figure 2, shown on subjects' computer screens, illustrates the distribution of the initial wealth. We then will explain that provisional earnings are assigned to subjects in two possible ways: (1) randomly or (2) according to performance in a computer-based real-effort task (e.g., Tetris). Which method would actually be used to assign payoffs to subjects would be determined by a random draw at the end of Part 1. We tell subjects they will be able to alter the initial distribution by taxing earnings and redistributing the modified proceeds equally among all; in particular, they will be asked to choose a proportional tax rate ranging from 0% to 100% in increments of 10%. We will also explain to the subjects how the decision rule, median voter, is implemented in Figure 3. We will illustrate the effect of taxation on earnings graphically and through a table. The table is produced in Figure 4.

We continue the experiment in Part 2, where the income distribution rule is finalized, and each individual observes both their wealth ω_i and ranking in real effort task. Then, we will reveal the results of a random draw for the cost of the message (free, costly, or unaffordable) and if each message will include information about the sender. Then, participants will be allowed to send one message to others. The communication occurs in an open forum/broadcast manner, and everyone can see all sent messages. When everyone finishes writing and sending their messages, they see their received messages.

Part 3 includes participants reading the messages and then moving on to a screen where they can

[3] One apparent way to do it is to lower messaging costs for everyone, which is explored in Treatment Branch (ii). However, this is a somewhat radical solution that does not appear realistic. A less demanding alternative is to require disclosure of the income level of each sender, Treatment (iii).

vote for the tax rate that should be imposed on the group. The median of all chosen tax rates will be the one implemented for the whole group. The interface for making this choice can be seen in Figure 5.

5 Data

The data collected at the end of each round will include the initial and final wealth, real task effort ranking, messaging decision, and final vote. In addition, at the end of each session, the exit survey will collect demographic and socioeconomic information of participants. The scope of uses other than the direct purpose of the study for this data is limited. However, the variety of treatments and covariates collected can be used to establish additional patterns in behavior and later used as a justification for additional experiments.

Treatment variables. Table 1 provides a detailed description of the treatment branches used in the experimental study. We outline three main variables: the *Income determination method* (i), *Message cost* (ii), and *Sender identity* (iii). All three variables vary between subjects. The *Income determination method* refers to the method used to assign individuals to pretax payoffs, which can be either random or based on effort. This variable is hypothesized to influence the demand for fairness and equity, as outlined in Hypothesis 1. The *Message cost* variable regulates who can send messages, with the values being 0, C, or ∞. The *Sender identity* variable concerns the wealth level of the message sender, which can be revealed or hidden.

Power. For power analysis, we follow guidelines outlined by Vasilaky and Brock (2020). The statistical power of our experiment primarily depends on the anticipated features of the voting behavior. We go into detail on how the analysis is conducted in the Appendix. Notably, the experiment includes several treatments. Therefore, we adjust for multiple hypothesis testing when conducting power analysis. Rather than using p values generated from single t-tests, rejection rates are based on p values that are Bonferroni corrected; that is, the significance level is divided by the number of total comparisons made with the same data. Figure 7 reports the results. The power of 0.8 is achievable by 9 players in 11 sessions consisting of 3 game rounds.

IRB. The project has received the Human Research Subjects approval. Office of Research Compliance Administration, UC Santa Cruz, determined the exempt status of the project from the IRB review. Full approval details can be found in the IRB# HS-FY2021-16 protocol.

6 Timeline and deliverables

The timeline for the project is outlined in Figure 8. From June to July 2024, we plan to finalize the experimental design and run several pilots to prepare for the full-scale implementation. The primary stage of the project is planned to take place from July to September 2024, when we anticipate collecting most of the data. From September to November 2024, we plan to analyze data and prepare some preliminary results that could be presented at UCSC's Brown Bag seminar and the Experimental Economics workshop. This would give us necessary feedback that can later be incorporated into the final draft, which we plan to complete by March 2025.

There are two sets of deliverables for this project. Primary deliverables are a pilot testing report, collected data, an initial draft of findings, and the final paper. Secondary deliverables are exploratory data analysis unrelated to the research question, intermediary drafts, and workshop presentations. Once the paper is finalized by soliciting feedback from colleagues, circulating the preprint through SSRN/arXiv, and presenting it, we plan to submit the paper to a peer-reviewed journal.

7 Budget

We plan to conduct this experiment in the LEEPS Laboratory, online or in person, depending on the available funding. The programmers associated with the laboratory have developed most of the experimental interface, but some elements are still in progress. We seek funding for subject payments for 300 participants, with an average payment of $15. This amounts to $4,500. We also seek funding for finalizing the software interface, which we estimate will require an additional $500. That is, in total, we seek $5,000. We plan to make the first expenses during June 2024 and primary expenses during July/August 2024.

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8 Appendix

8.1 Session Structure

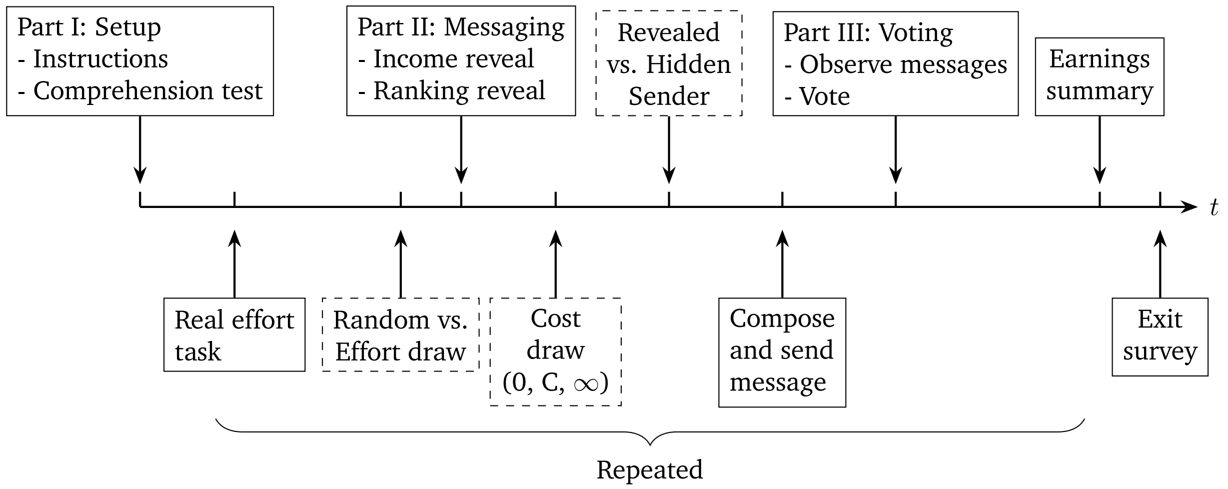
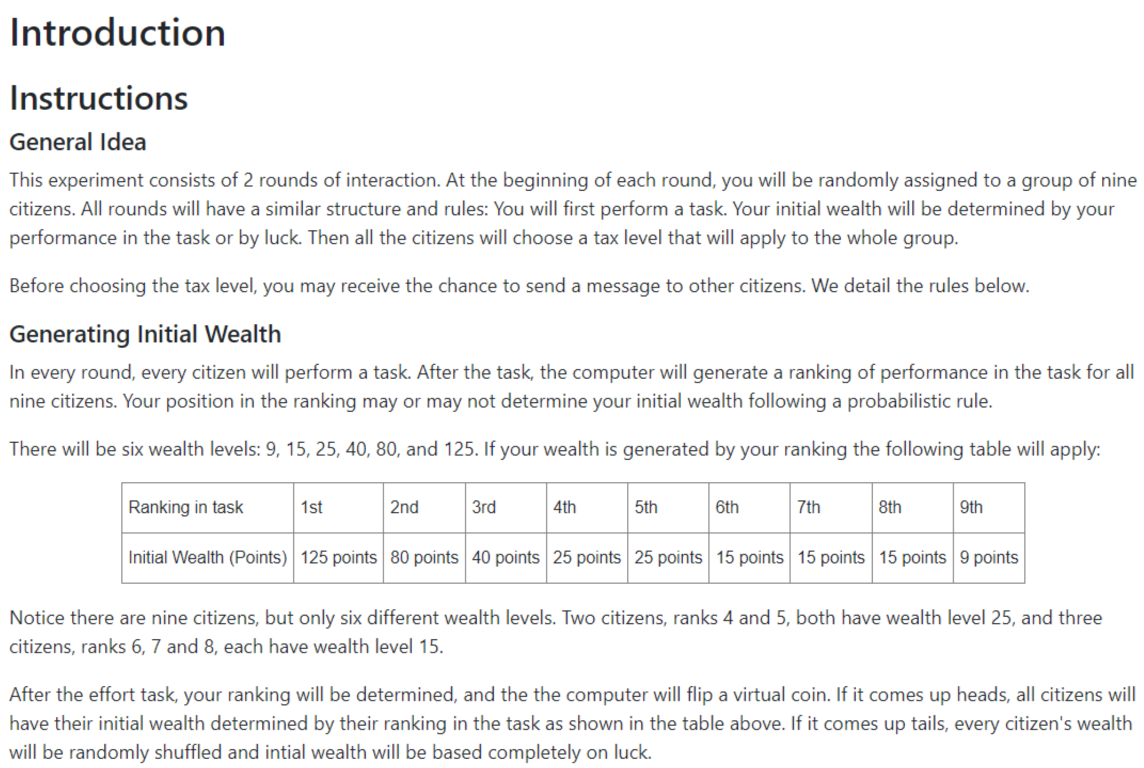


Figure 1: Session Timing

8.2 Interface



Introduction

Instructions

General Idea

This experiment consists of 2 rounds of interaction. At the beginning of each round, you will be randomly assigned to a group of nine citizens. All rounds will have a similar structure and rules: You will first perform a task. Your initial wealth will be determined by your performance in the task or by luck. Then all the citizens will choose a tax level that will apply to the whole group.

Before choosing the tax level, you may receive the chance to send a message to other citizens. We detail the rules below.

Generating Initial Wealth

In every round, every citizen will perform a task. After the task, the computer will generate a ranking of performance in the task for all nine citizens. Your position in the ranking may or may not determine your initial wealth following a probabilistic rule.

There will be six wealth levels: 9, 15, 25, 40, 80, and 125. If your wealth is generated by your ranking the following table will apply:

Ranking in task	1st	2nd	3rd	4th	5th	6th	7th	8th	9th
Initial Wealth (Points)	125 points	80 points	40 points	25 points	25 points	15 points	15 points	15 points	9 points

Notice there are nine citizens, but only six different wealth levels. Two citizens, ranks 4 and 5, both have wealth level 25, and three citizens, ranks 6, 7 and 8, each have wealth level 15.

After the effort task, your ranking will be determined, and the the computer will flip a virtual coin. If it comes up heads, all citizens will have their initial wealth determined by their ranking in the task as shown in the table above. If it comes up tails, every citizen's wealth will be randomly shuffled and intial wealth will be based completely on luck.

Figure 2: Initial Wealth

Selecting a Tax Rate for the Group

citizens' initial wealth levels help to determine their earnings in the experiment by serving as a potential resource that they may invest in an economic activity. Exactly how much each citizen earns is also determined by (1) the amounts of public goods (analogous to roads, bridges, public transit, and other government services in the real world) that are funded by taxes paid by the citizens, and (2) the amount of tax that each individual pays. In general, you'll earn more if there are more public goods being funded by the tax payments of the group as a whole, and you'll earn more if the amount of tax that you personally pay is lower, since paying more tax leaves you less to put into your own economic activity.

Once all citizens have been informed of their initial wealth levels, the group will collectively decide the tax level (from 0-100%) that will be binding for all citizens. Each person will vote for a tax rate that they would like to be applied to all citizens, themselves included. Once the votes are submitted, the median of all submitted preferred tax rates will be determined and selected as the tax rate applicable to the group.

How is the median determined?

In the context of nine values, the median M is defined as the value such that there are five numbers that are smaller than or equal to M; and there are five numbers that are greater than or equal to M. For example, suppose you and your friends must all choose an amount to contribute to pay for the food at a party. You select $5, and the other eight citizens of your group have chosen: $1, $4, $8, $9, $10, $7, $8, $2. To find the median number, you can sort from low to high as in the table below, then find the number in the middle position (position 5). That is the median.

Number Order	1	2	3	4	**5**	6	7	8	9
Contribution	$2	$3	$3	$6	**$6**	$9	$10	$12	$16

The median in the example would be $6, which is the middle amount in a sorted list. When you and other citizens select a tax rate, the median amount will be computed in this fashion.

Figure 3: Voting procedure explanation

Payoffs

The payoff takes into consideration the resources that have been paid to the government. In particular, we consider the direct benefit from public goods (ex.: the benefit of social security, or unemployment insurance) and the indirect benefit via positive effects of public goods on our own private productivity (ex.: the benefit that companies receive from having roads and private property rights).

To better understand how these payoffs work, please see the table below that shows a citizen's potential payoff based on their initial wealth level and tax rate.

Initial Wealth (Points)	Tax Rates (%)										
	0	10	20	30	40	50	60	70	80	90	100
9	45	60	72	84	98	115	116	100	85	70	54
15	75	98	117	132	147	163	156	131	105	80	54
25	125	163	192	213	230	242	224	182	139	97	54
40	200	260	304	334	353	362	326	258	190	122	54
80	400	518	604	658	683	680	598	462	326	190	54
125	625	809	941	1021	1053	1038	904	692	479	267	54

Similarly, the following graphs show the payoffs for every tax rate. Each wealth level has its own graph where you can see the relationship between the round payoff and the group-chosen tax rate.

Figure 4: Page with payoff table

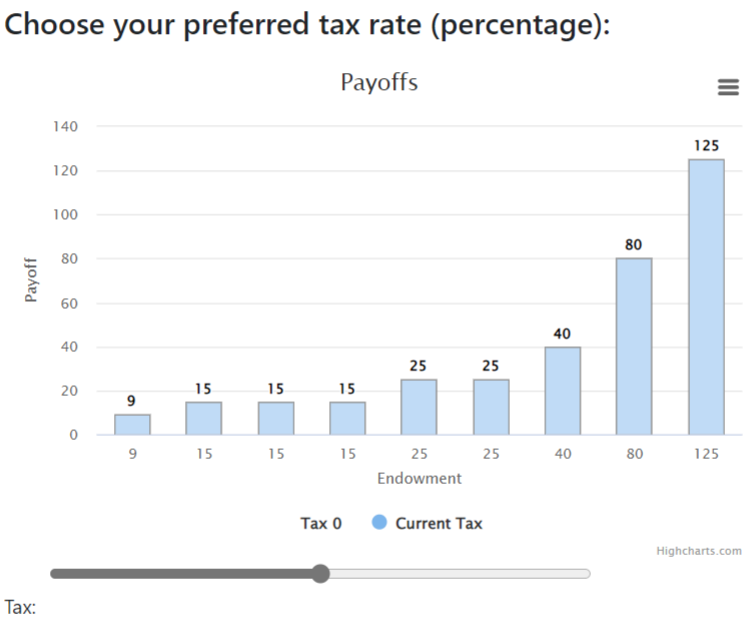


Figure 5: Tax rate system

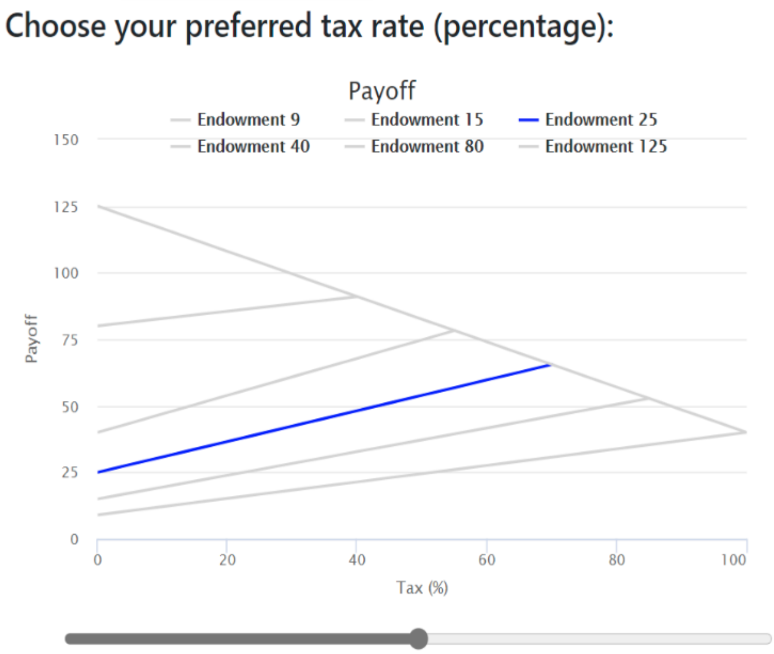


Figure 6: Full distribution of wealth for each tax level

8.3 Power

For power analysis, we follow guidelines outlined by Vasilaky and Brock (2020). The statistical power of our experiment primarily depends on the anticipated features of the voting behavior. The payoff function is constructed so that each wealth level has its own unique tax t_i^* that maximizes their after-tax income. As outlined above, $v^* = t_i^*$ is a weakly dominant voting strategy. Thus, we assume that in equilibrium under the null, the final votes are uniformly distributed over $T^* = \{t_i^*\}_1^9$. This gives us the following population parameters: $sd = 0.26$, $\sigma^2 = 0.067$, $mean = 0.5$. Another important consideration is the expected Average Treatment Effect (ATE) for each branch. We implement a conservative approach that sets the expected ATE at the level of the lowest possible deviation from optimal tax t_i^* that is observable for each individual. In our case, this value is -0.1 given the logic of political persuasion toward lower tax when the access to messages is unequal. The approach is similar for other branches.

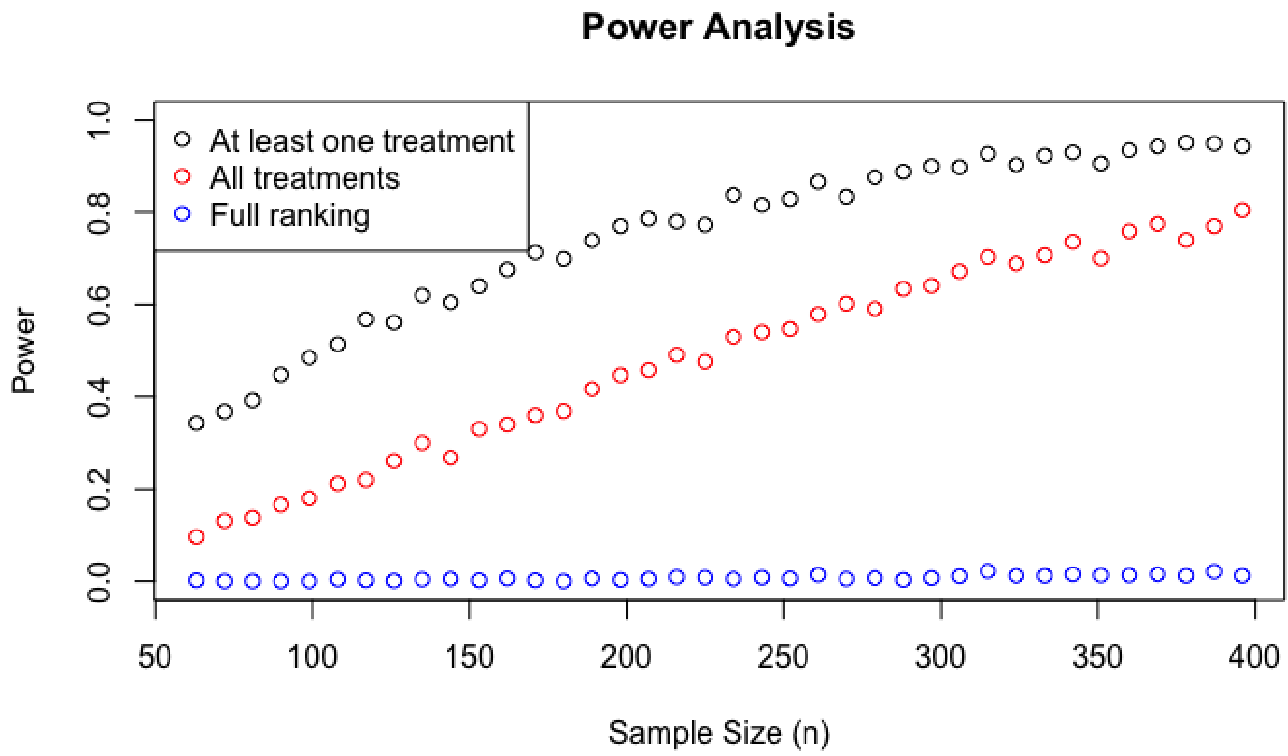


Figure 7: Power Analysis for Different Sample Sizes. Assumes that the true effect is -0.1, i.e., one jump from the preferred tax rate toward a lower one given there is unequal messaging. Population parameters are as follows: $sd = 0.26$, $\sigma^2 = 0.067$, $mean = 0.5$.

8.4 Treatments

Treatment variable	**Description**	**Values**	**Source of variation**	**Hypotheses**
Income determination method	Method used to assign individuals to pretax payoffs	Random, Effort	Between-subject	Influences demand for fairness&equity
Message cost	Regulates who has the opportunity to send messages	0, C, ∞	Between-subject	Unequal access to messages ensures a better outcome for the wealthier group
Sender identity	Wealth level of the message sender	Revealed, Hidden	Between-subject	Inclusion of identity attenuates persuasion

Table 1: Description of treatment variables.

8.5 Timeline

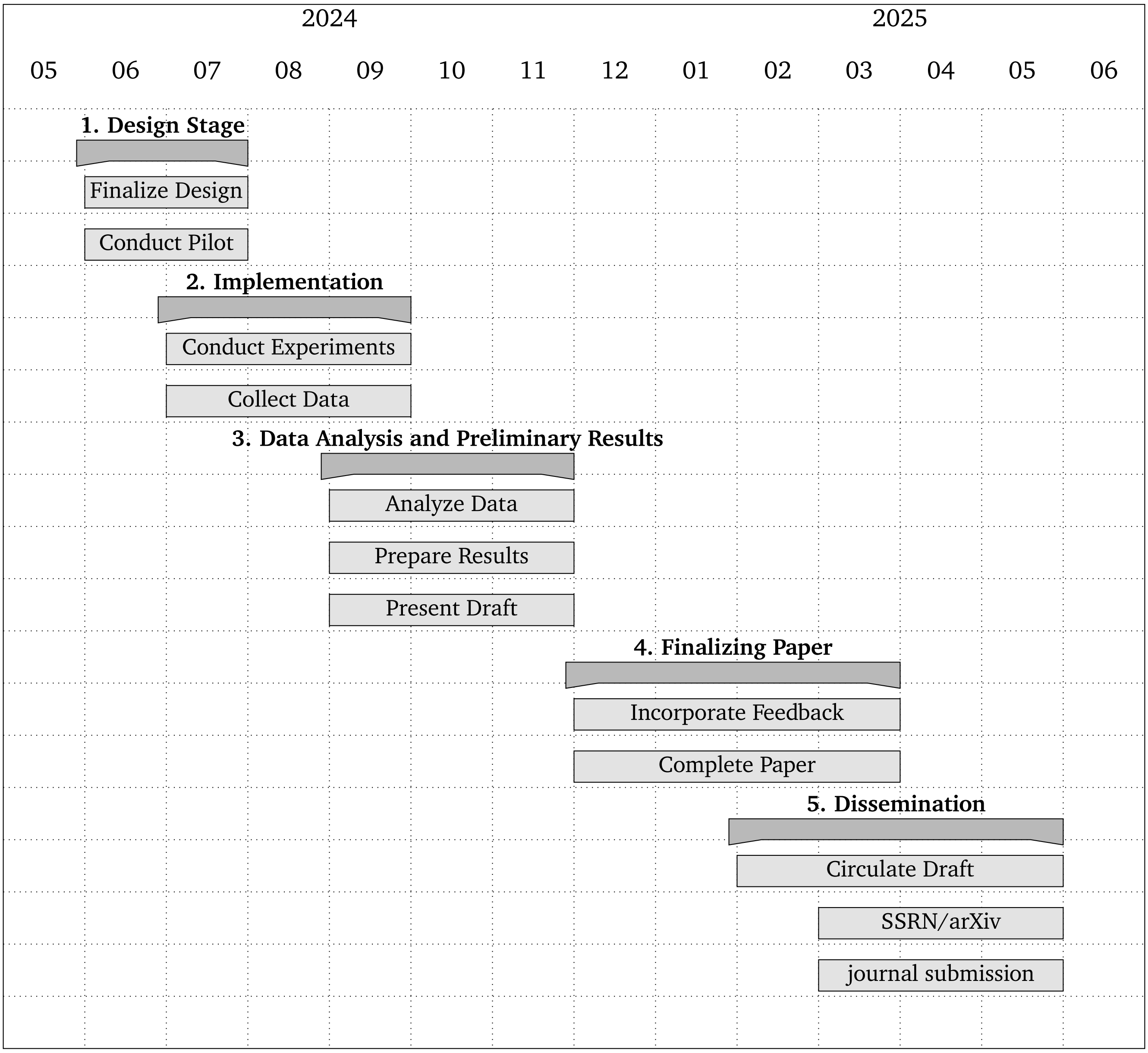


Figure 8: Project timeline