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Contents

List of Figures

List of Tables

Introduction

Economic agents form long-term relationships with each other. For instance, customers develop relationships with firms and may become upset if they feel betrayed by the firm's behavior, while managers within the same sector can form friendships.

This dissertation analyzes the interaction between economic activity and social relationships in three settings: friendships in markets, customer relationships in auctions, and village communities in the 1930s Weimar Republic.

All these settings share a common element: Economic actions have long-term consequences for relationships. These consequences shape both economic and social activity. A seller may charge high prices to aid their friend who sells a substitute. Firms may be cautious in exploiting customers' biases to avoid damaging customer relationships. Villagers may comply with restrictive social norms within their community to continue enjoying the economic benefits of community membership.

Chapter 1 examines the influence of friendships on imperfectly competitive markets with substitutes and complements. I test a model of friendships in these markets, in which people are altruistic towards their friends. The model predicts that friendships among sellers of substitutes increase prices and decrease efficiency, whereas friendships between sellers of complements decrease prices and increase efficiency.

I invite pairs of real-world friends to participate in a laboratory market experiment, assigning them different roles. Depending on their role, these friends sell complements, substitutes, or are strangers to each other. This procedure generates exogenous social network wile keeping the market and the individual constant. Each individual chooses prices for different social networks within the same market. I compare these prices to estimate the causal effects of friendship networks on prices.

Chapter 2, we investigate customer relationships in a teleshopping multi-unit auction. The firm operates through two retail channels: a televised multi-unit descending auction with uniform pricing and an online shop that offers goods at fixed prices. In this auction, every participant pays the lowest successful bid, and we refer to bids that exceed the fixed price as "overbids" and to bids where the bidder has to pay above the fixed price as "overpaid." An overbid is considered overpaid if the lowest successful bid is an overbid.

Customers who overpay could have obtained the goods at a lower cost. Consequently, some discontinue overbidding (intensive margin learning) or cease bidding entirely, leading to a severed customer relationship. This severed relationship encourages the firm to take measures against overpayment. Empirically, we witness responses at both margins.

Additionally, we note that the firm started to prevent customers from overpaying. Initially, this was likely done by increasing supply whenever there was a risk of overpayment. Eventually, the firm began initiating auctions below the fixed price.

In Chapter 3, we compile large-scale survey data from German-speaking villages in the 1930s to investigate drivers of norms concerning cooperation, gender, and religion. Through geographic cluster analysis, we show that inter-regional variation explains only a small amount of heterogeneity in norms. Villages in the same geographical and institutional environment still maintain different norms. Local differences in the structure of social relationships can explain intra-regional heterogeneity in norms. We focus on a community's ability to transmit and enforce norms to derive theoretical links between correlates of community social relationships and the number of norms it maintains (norm prevalence). Empirically, we find that

(1) norm prevalence is higher in villages that: are religiously homogeneous, border other villages with a different majority religion, and have many within-village social gatherings;
(2) villages with stronger community-level social relationships are also less likely to segment their reference group for the cooperation norm to smaller social units;
(3) communities that have neighborhood help norms also have more restrictive social norms.

These findings indicate that cohesive communities have more social norms in a variety of domains. Further, there is an institutional complementarity between economic cooperation norms (neighborhood help) and restrictive social norms.

Chapter 1

Substi-dudes and Comple-mates: The Effect of Friendship on Market Prices and Efficiency*

1.1 Introduction

Markets are intertwined with social relationships (Granovetter, 1985). People selling houses in Amsterdam go to church together (Lindenthal, Eichholtz, and Geltner, 2017), friends of rival CEOs serve on a company's board (Westphal and Zhu, 2019), and hotel managers in Sydney befriend the managers of their competition (Ingram and Roberts, 2000). How do these friendships interact with the market? Do friends conspire and raise prices (A. Smith, 1776, p. 130), or can their cooperation benefit consumers?

Little is known about the causal effects of network structure on market efficiency. Three problems explain lack of knowledge. First, we need exogenous variation in social networks to estimate their causal effects. Without exogenous variation, treatment effect estimates may be confounded by common causes. For example, physical distance facilitates both trade and social network connections. Second, market efficiency is unobservable because we need to know individuals' private costs and values. These data are necessary to compute the gains from trade and, thus, market efficiency. Third, we need a theoretical model of social relationships and market efficiency. Social networks are high dimensional: There are many possible ways to link

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market participants. Each social relationship can have many aspects: Friendships can affect markets because friends are more altruistic towards each other (altruism or social sanctions) or because they know more about each other. We need a model to learn which social relationships and which aspects of them are essential.[1]

My solution to these problems is a controlled laboratory experiment. First, I make the social network exogenous by assigning real-world friends to different roles in a market experiment. Second, the experiment solves the problem of private values and costs, because it induces them (V. L. Smith, 1976): The experimenter knows and controls private values and costs because they can set participants' monetary rewards for the experiment.

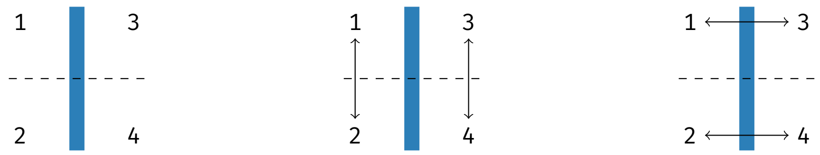
I assume that friends are more altruistic towards each other than towards strangers (directed altruism, Leider et al., 2009). In this model, friendships between two people affect efficiency in the same way as mergers: Friendships between sellers of complements increase efficiency, and friendships between sellers of substitutes decrease it.

I confirm this prediction in an experiment and estimate the altruism parameter with a structural model. The model fits the data well. The familiarity between friends does not affect market outcomes in my experiment. Consequently, directed altruism between friends is a helpful model for analyzing social networks' effect on market efficiency.

I conduct my experiment in a simple market that includes substitutes and complements (from the buyers perspective). Four participants are assigned the role of sellers that each own one plot of land. Sellers 1 and 2 own land to the left side of a river, and sellers 3 and 4 own land to the right side of a river (see Panel a of Figure 1.1). A computerized buyer wants to buy precisely two plots on the same side of the river. Thus, plots on the same side of the rivers are complements, and plots on different sides are substitutes. Each seller makes a (simultaneous) take-it-or-leave-it offer to the buyer. The buyer aggregates the prices and buys the bundle of land that gives them the highest surplus (or abstains from buying).

I test if friendships between owners of substitutes (substitute friendships) and owner of complements (complement friendships) have different effects. I compare substitute and complement friendships by comparing three symmetric social networks. These networks are depicted in Panels a–c of Figure 1.1, where arrows indicate friendships. I name social network treatments after the properties of their friendships. In the *Complements Symmetric* treatment, friendships are between people that sell complements. In the *Substitutes Symmetric* treatment, friendships are between people that sell substitutes. In the *Baseline* treatment, all players are strangers.

1. While we have theoretical models of contract enforcement through social networks (Karlan et al., 2009) and enabling exchange (Kranton, 1996), we lack a model of how social networks affect efficiency inside formal market institutions.



a. Baseline

b. Complements (Symmetric)

c. Substitutes (Symmetric)

Figure 1.1. The experimental market with different social networks.

I create these friendships exogenously in the lab by inviting pairs of friends and assigning them to different roles.[2]

I exploit an analogy between friendships and partial mergers to derive my predictions. Friends might want their friends to get higher payoffs, and partially merged firms would like each other to make higher profits and set prices correspondingly. I formalize this argument by applying the common ownership model (Rubinstein and Yaari (1983), Rotemberg (1984), and Azar, Schmalz, and Tecu (2018)) to friendships within a market.[3] This model is observationally equivalent to linear altruism among firms with common owners. Applied to friendships, this model is a linear version of directed altruism among friends (e.g. Leider et al. (2009)). I test if the linear, directed altruism model predicts the empirical effects of friendships.

The merger analogy suggests that friendships between sellers of complements and sellers of substitutes have different effects on prices. Mergers between sellers of complements decrease prices, whereas mergers between sellers of substitutes increase prices (See in particular chapter IX of Cournot (1897), which has been reproduced and extended in Economides and Salop (1992)). Friendships might behave similarly.

Compared to the benchmark without friendships, friendships between sellers of complements (same side friendships) should decrease prices, and friendships between sellers of substitutes (cross-river friendships) should increase prices. The reason is that directed altruism partially internalizes an externality between friends: Lower prices increase the demand for complements (plots on the same side of the river) and decrease the demand for substitutes (plots on different sides of the river). Sellers want to increase the demand for their friend's product. Thus, compared to the benchmark without friendships, sellers with friends that sell complements (same side friendships) decrease their prices, and sellers with friends that sell substitutes (cross-river friendships) increase their prices.

2. Chandrasekhar, Kinnan, and Larreguy (2018) inspired this design.
3. For a survey of the more recent literature see Schmalz (2021).

The effect of friendships on prices translates into an effect on market efficiency.[4] Since we are in an imperfectly competitive market, prices start above the competitive level. Therefore, increasing them lowers efficiency, and lowering them increases it as long as prices remain above the competitive benchmark.

The experiment's results are consistent with the qualitative predictions of directed altruism theory. The markets with the *Complements Symmetric* network are the most efficient and have the lowest prices, followed by the *Baseline* network and the *Substitutes Symmetric* network as the least efficient network with the highest prices:

This finding suggests a policy implication: Social networks that connect sellers of Complements boost efficiency and social networks that connect sellers of substitutes decrease it. Consequently, we should boost the former's effects and dampen the latter's. In my experiment, I increase price transparency to facilitate social sanctions.

Leider et al. (2009) suggests that altruistic behavior among friends increases when friends can be socially sanctioned. I test this by adding a price transparency treatment in which the chosen prices are revealed, which allows participants to sanction their friends for their choices. The theory predicts that transparency leads to lower prices in the *Complements Symmetric* networks and to higher prices in the *Substitutes Symmetric* network. In the experiment, however, transparency lowers prices in both cases. The answers to open questions after the experiment suggest a possible reason: Participants lower their prices because they do not want to appear greedy. That is they have social image concerns (e.g. Andreoni and Bernheim (2009)). This unexpected effect of price transparency indicates that findings from two person experiments, such as Leider et al. (2009), do not necessarily generalize to larger markets. Further, in my setting, price transparency does not increase the effects of social networks.

Do friendships behave like partial mergers qualitatively as well as quantitatively? To answer this question, I need to calculate the linear directed altruism model predictions. These predictions depend on directed altruism's strength. I estimate this parameter with a structural model. The estimated model makes in-sample and out-of-sample predictions which I can compare to the data. Additionally, it allows me to disentangle the effect of price transparency on social image concerns from its effect on altruistic behavior.

In addition to linear directed altruism the structural model includes decision error, joy of winning and social image concerns. I model decision errors with a quantal response equilibrium (QRE) (McKelvey and Palfrey, 1995). I use a homogeneous parameter for altruism among friends and add two other parameters to the utility function: First, a constant to rationalize a downward shift of all prices compared to the Nash Equilibrium, capturing for example joy of winning and risk aversion, and

4. I define *efficiency* as the expected realized material gains from trade. If there is a trade, the gains from trade are the difference between the seller's costs and the buyer's values.

second a penalty for high prices in the transparency treatment, capturing the social norm for low prices.

The good fit of the structural model suggests that one parameter, directed altruism, rationalizes the effects of different social networks. Besides the altruism parameter, all parameters mainly affect the average magnitude and variance of prices and not the treatment effects of different social networks. The model fits the data well. Therefore a single altruism parameter can rationalize the effects of complement and substitute friendships. The representative participant is willing to pay 20 and 36 cents for their friend to receive one dollar.

To know if directed altruism should be the workhorse theory for the effects of friendships on market efficiency, we need to compare it to other theories. Independently from altruism, a friendship might have strategic effects if participants have more accurate beliefs about their friend's actions than about strangers' (familiarity). Although 60% of the participants state that they have more accurate beliefs about their friend's actions, it does not seem to be true: I elicit beliefs about other players' actions with the binarized scoring rule (Hossain and Okui, 2013) and measure belief accuracy by the quadratic distance of beliefs from the corresponding actions. Conditional on the treatment, beliefs about a friend's actions are roughly as accurate as beliefs about a stranger's actions.

My paper contributes to the experimental literature on the effects of social networks on economic decision-making. The existing literature shows that tighter social network links facilitate informal contract enforcement and increase cooperative behaviors and equitable sharing among friends (Leider et al., 2009; Goeree et al., 2010; Leider et al., 2010; Ligon and Schechter, 2012; Chandrasekhar, Kinnan, and Larreguy, 2018). I complement this literature and investigate the effect of social networks in small markets with more than two people. Consistent with the literature, friends are more altruistic towards each other than strangers. However, my richer setting puts some of the results that were obtained in simple games into a new perspective: in my setting price transparency fails to increase altruistic behavior among friends.

Friends are not better at predicting friends' actions than strangers' actions. While this finding is unexpected for the participants it is mostly in line with the literature. Leider et al. (2010) finds that friends are not better at predicting friends' allocations than strangers' allocations, in a modified dictator game. However, Gächter et al. (2022) and Chierchia, Tufano, and Coricelli (2020) find that friends coordinate better than strangers in some coordination games.

My paper contributes to the literature on market design for the assembly of complements, for example: plots of land into a building site, patents into an invention, components into a car (Kominers and Weyl, 2012; Sarkar, 2017; Bryan et al., 2019; Grossman et al., 2019). This paper suggests that social network data can help market-designers to decide when it might be worthwhile to harness social relationships to increase market efficiency.

Furthermore, the paper connects IO and social network research. I connect to an older qualitative literature about the role of informal social contacts for oligopolistic coordination (Scherer and Ross, 1990, p.311-315) and the literature on common ownership.

My empirical results establish a link between research on firms with common owners and friendships in markets. The same utility function that rationalizes the behavior of friends in this study is used to model firms with common owners (linear-directed altruism).

We can use this bridge to import methods from common ownership research to analyze friendships. Backus, Conlon, and Sinkinson (2021a) test the common ownership model with field data. Ederer and Pellegrino (2022) use a structural common ownership model to quantify the effect of common ownership on (real-world) market outcomes. We can repurpose these existing methods for common ownership networks to quantify the effect of friendship networks on market outcomes and test the linear, directed altruism model with field data. To do this, we could replace the common ownership network with a friendship network.

The connection also suggests individual-level friendships as an additional mechanism behind firm-level common ownership preferences. The literature on common ownership also looks for mechanisms by which a firm's owners can induce common ownership preferences in their managers. This paper suggests a complementary approach to the one already discussed in the literature (less sensitive incentives for top managers as in Anton, Gine, and Schmalz (2022) and others discussed in Schmalz (2021)). Firms' owners could staff management positions with friends and pay these friends directly for their firm's performance. The altruism between managers then induces common ownership preferences. Westphal and Zhu (2019) document that there are consultancies that could provide owners with the necessary data on social networks.

1.2 Theoretical Framework

I model a symmetric market to test for different effects of friendships between sellers of substitutes (*substitute friendships*) and sellers of complements (*complement friendships*). In this section, I outline this experimental market, apply the linear directed altruism model to this market, and derive predictions for the effect of different social networks on prices.

1.2.1 Model

Participants play one of four human sellers that sell land to a computerized buyer. Sellers 1 and 2 own land to the left side of a river, and sellers 3 and 4 own land to the right side of a river. Sellers make a simultaneous take-it-or-leave-it price offers. Seller i's offer is denoted p_i, $i \in \{1, 2, 3, 4\}$. I develop the theory for the continuous case where $p_i \in [0,50]\ \forall i$, but run the experiment with discrete prices ($p_i \in \{0,1,2,...,50\}\ \forall i$).

The buyer wants to build a single building that spans two plots on the same side of the river. He has i.i.d. uniform private values θ_ℓ and θ_r for two plots on the left or right sides, respectively. The value distribution's support reaches from 0 to 100. Sellers' take-it-or-leave-it offers are aggregated ($p_\ell = p_1 + p_2$ and $p_r = p_3 + p_4$) and transmitted to the buyer. The buyer buys the bundle of land that gives him the highest surplus ($\theta_\ell - p_\ell$ or $\theta_r - p_r$) if this surplus is positive. In some rounds of the experiment, I pay a subsidy of s for successful sales.

I distinguish between a participant's material utility (m_i) and their utility (U_i). In this section I assume that the material utility is equal to the expected monetary pay-off from the experiment. The utility (U_i) incorporates altruism between friends.

If a participant sells, their material utility (m_i) is their price plus the subsidy; in all other cases, it is zero.

I use the simplest possible model of friendships and cooperation: linear directed altruism with a homogeneous altruism parameter $\mu \in [0,1]$. The model allows us to define a player's utility in terms of all players' material utility. Define the adjacency matrix $\boldsymbol{M}$. This matrix has dimensions 4 × 4, and its typical element m_{kl} is equal to 1 if players k and l are friends and equal to 0 otherwise. The main diagonal is zero. Then the utilities of all players are given by

$$\underbrace{\begin{bmatrix} U_1(p_1,p_2,p_3,p_4) \\ U_2(p_1,p_2,p_3,p_4) \\ U_3(p_1,p_2,p_3,p_4) \\ U_4(p_1,p_2,p_3,p_4) \end{bmatrix}}_{\text{expected utilities}} = \underbrace{\begin{bmatrix} m_1(p_1,p_2,p_3,p_4) \\ m_2(p_1,p_2,p_3,p_4) \\ m_3(p_1,p_2,p_3,p_4) \\ m_4(p_1,p_2,p_3,p_4) \end{bmatrix}}_{\text{material utility}} \underbrace{+\mu \times \boldsymbol{M} \times \begin{bmatrix} m_1(p_1,p_2,p_3,p_4) \\ m_2(p_1,p_2,p_3,p_4) \\ m_3(p_1,p_2,p_3,p_4) \\ m_4(p_1,p_2,p_3,p_4) \end{bmatrix}}_{\text{altruism term}}$$

In a literal interpretation, the parameter μ captures altruism between friends. I also interpret it as a reduced form summary of all cooperation effects of friendships, such as social sanctions.

Social sanctions work better between friends than strangers because friends value their friendship and can use it as *social collateral* (e.g., Leider et al. (2009)). In theory, friends derive utility from their friendships. If someone observes that their friend does not cooperate, they can stop being friends and withdraw that utility. This threat can enforce cooperation.

I conceptualize changes in social sanctions as shocks to the directed altruism parameter (μ). In the experiment, I run a price transparency condition. This condition facilitates social sanctions. Consequently, I assume price transparency increases μ.

1.2.2 Social Network Treatments and Theoretical Predictions

My main analysis compares symmetric social networks (Substitutes Symmetric and Complement Symmetric) to a Baseline social network without social relationships.[5] These networks are depicted in Figure 1.1. Market institutions and social networks jointly induce a game.

I analyze the symmetric equilibria of these games. Participants did not receive any feedback before making their last decision and were not able to communicate. With feedback or communication, participants could coordinate on an asymmetric equilibrium; coordination is very hard without these elements. Therefore the symmetric equilibrium is a better prediction for participant's behavior.

I can analyze each round of the experiment as a separate game, because participants do not get feedback in between decisions. Therefore they cannot condition their action on other participants' actions in previous rounds. This prevents repeated game effects.

I focus on pure strategy equilibria for reasons of tractability. However, the structural model in Section 1.4.6 allows for mixed strategies.

Lemma 1 shows that symmetric equilibria exist in all games with symmetric networks. Further, the symmetric equilibrium strategies solve the player's first order conditions. This Lemma's proof is in Appendix 1.A.

This Lemma uses the additional assumption that $50 > (1 + \mu) \times s$. This assumption guarantees that the player's maximization problems have an interior solution. In the experiment $s \leq 20$, thus the assumption holds for all $\mu \in [0, 1]$.

Lemma 1. If $50 > (1 + \mu) \times s$, the games generated by the Substitutes Symmetric, Baseline and Complement Symmetric networks have a unique symmetric equilibrium. The symmetric equilibrium price solves the players first order conditions and is always on the interior of the price interval.

Prices in the Substitutes Symmetric network are higher than in the Baseline Network, and prices in the Complements Symmetric network are lower than in the Baseline network. This effect occurs because friends internalize externalities between them more, and externalities between sellers of substitutes and sellers of complements go in opposite directions. If a plot's price rises, the demand for its complement falls, and the demand for its substitute rises. High prices have negative externalities on sellers of complements and positive externalities on sellers of substitutes. If sellers of complements are friends, they internalize the negative externality of high prices and lower their prices. If sellers of substitutes are friends, they internalize the positive externality between them and increase their prices. I formalize this argument in Proposition 1.1. This proposition's proof is in Appendix 1.A.

5. I also run treatments with asymmetric social networks. I discuss these treatments in Subsection 1.4.7.

Proposition 1.1. *The symmetric equilibrium price in the Substitutes Symmetric network (p_s) exceeds the price in the Baseline network (p_b), which exceed the price in the Complement Symmetric network (p_c): $p_s > p_b > p_c$.*

We can get an economic intuition for this result by looking at price and quantity effects. Classically, IO decomposes the revenue effect of an increase into a *price effect* and a *quantity effect*. The price effect is the rise in revenue through higher prices, keeping quantities constant. The quantity effect is the fall in revenue through lower quantities, keeping prices constant. A revenue maximizing firm (marginal costs are zero) balances price and quantity effect.

Introducing friendships adds an additional element to this decomposition. We can decompose the quantity effect into an *own quantity effect* and a *friend quantity effect*. The own quantity effect is the traditional quantity effect, whereas the friend quantity effect is the effect of a price increase on a friend's quantity. We can see this decomposition in the first-order conditions (FOC) (example for player 1) from the Complements Symmetric network,

$$\underbrace{\frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}(p_1+S)}_{\text{own quantity effect (-)}} + \underbrace{\mu\frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}(p_2+S)}_{\text{friend quantity effect (-)}} + \underbrace{\Pr_\ell(p_1,p_2,p_3,p_4)}_{\text{own price effect (+)}} = 0,$$

and the Substitute Symmetric network

$$\underbrace{\frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}(p_1+S)}_{\text{own quantity effect (-)}} + \underbrace{\mu\frac{\partial \Pr_r(p_1,p_2,p_3,p_4)}{\partial p_1}(p_3+S)}_{\text{friend quantity effect (+)}} + \underbrace{\Pr_\ell(p_1,p_2,p_3,p_4)}_{\text{own price effect (+)}} = 0.$$

For $\mu = 0$ these FOC coincide with the FOC of the baseline case.

The friend quantity effect in the complement symmetric network leads to lower prices in the Symmetric Complements than in the Baseline network. A higher p_1 makes it less likely that the buyer buys on the left side (the side of players 1 and 2). Consequently, the cross price elasticity of demand ($\frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}$) is negative. The friend price effect decreases the marginal utility from higher prices.

The effect is reversed in the Substitutes Symmetric network. Hence $p_s > p_b > p_c$.

In the symmetric equilibrium efficiency (total expected material surplus) is highest for the Complements Symmetric network, second highest for the Baseline network, and third highest for the Substitutes Symmetric network. If all prices are the same, the buyer either buys on the side where he has the highest value or does not buy. Prices are a transfer and do not change overall welfare. When the buyer buys, the social surplus is the utility of the buyer ($\max\{\theta_\ell, \theta_r\}$) and the subsidy for the sellers (s); if he does not buy, there is no social surplus. Define the symmetric equilibrium price $p_{\ell r} = p_\ell = p_r$. The overall expected welfare is

$$\int \underbrace{\mathbb{1}[\max\{\theta_\ell, \theta_r\} > p_{lr}]}_{\text{successful trade}}(\max\{\theta_\ell, \theta_r\} + S) f(\theta_r) f(\theta_\ell) \mathrm{d}\theta_\ell \mathrm{d}\theta_r.$$

This expression falls in $p_{\ell r}$. Consequently, social networks with lower prices have a higher expected surplus.

1.3 Experimental Design

The experiment investigated the effect of social networks on market efficiency. I varied participants' social networks in an experimental market.

The experiment proceeded in five steps: (1) I recruited pairs of friends to participate in the experiment; (2) participants filled out a survey about their friendship; (3) they read an explanation of the experiment's rules and answered control questions. Then, in the central part of the experiment, (4) participants made decisions in the experimental market for different social networks. Throughout this process, participants did not receive any feedback and were not able to communicate. Finally, after making all of their decisions, (5) participants received feedback and answered some open-ended questions.

The experiment was conducted in German. The following explanation translates all terms into English. The experiment was implemented in oTree (Chen, Schonger, and Wickens, 2016).

1.3.1 Recruitment

I recruited participants from the database of the BonnEconLab (via hroot (Bock, Baetge, and Nicklisch, 2014)). Each participant, from the database, acted as an anchor and had to bring one friend to the experiment. The anchor participants got an e-mail with an invitation and a link. Participants were told to forward this link to their respective friend who used it to register for the experiment.

The experiment requires precisely four pairs of participants to generate the Baseline network, consisting of four strangers.

As a precaution, for the case of no-shows, I recruited an extra pair of participants. Redundant participants either got to participate in an unrelated individual choice experiment, or were paid a show-up fee and left. [6]

To incentivize bringing a friend, I announced that, as in Leider et al. (2009), all participants could earn 5 Euro for correctly answering a trivia question about their friend. I verify in Section 1.4.1 with some additional survey questions that the participants' friendships are strong and meaningful social relationships.

1.3.2 Payoffs

Participants in the experiment were compensated through a combination of show-up fees, trivia question rewards, and decision payoffs. To begin with, each participant

6. I preregistered the design, the analysis, most hypotheses and the sample size (240) at https://osf.io/5ytnz. With a minor deviation, which I discuss later, I stuck to the preregistered design.

received a show-up fee of 7.50 Euro. Additionally, they had the opportunity to earn 5 Euro by successfully answering a trivia question about their friend.

Throughout the experiment, participants made 40 decisions in the market and participated in 8 belief elicitations. These decisions were all payoff-relevant, each with an equal probability of 1/48. Payoffs in the experiment are the same as in Section 1.2 (the theory section). The theory section omits units; the experiment uses the experimental currency unit, Thaler. Two Thaler correspond to one Euro.

Participants, on average, earned an additional 10.44 Euros based on their choices and their answer to the trivia question.

1.3.3 Survey

The experiment started with a survey. I reproduce this survey in Appendix 1.B.

I used this survey to ask the announced trivia question. At the beginning of the experiment, participants were asked when they usually get up and when their friends usually get up. Then, participants could enter their and their friend's wake-up times in brackets of one hour that reach from 5 to 11 a.m. They won 5 Euros if they guessed the correct bracket for their friend's wake-up time. To avoid participants preparing for this question, I later switched it to another question: "Is your friend a vegetarian?"

I measured friendship closeness with the inclusion of the other in the self (IOS) scale (Aron, Aron, and Smollan, 1992). This scale asks participants to pick one of seven pictures with overlapping rings that best describe their friendship. These pictures range from (1) no overlap to (7) almost complete overlap. Gächter, Starmer, and Tufano (2015) find that the IOS measure correlates strongly with six other measures of relationship closeness.

I asked four survey questions as an alternative measure of friendship closeness. First, I asked if participants brought their best friend to the experiment. Then, I separately inquired about the hours spent with the friend they brought and the hours spent with other friends each week. Lastly, I asked if their relationship with their friend was romantic or sexual, allowing participants to decline answering due to privacy concerns.

I elicited risk aversion with a question from Falk et al. (forthcoming).

1.3.4 Implementation of the Experimental Land Market

The experiment started with an explanation of the market's general rules (see Section 1.2.1). Then participants were asked several control questions, followed by an explanation of some features of the market related to the treatments.

Control questions tested participant's knowledge about the cross-price derivatives of the seller's probability to buy a specific plot of land (demand) (for more details see Appendix 1.C). For example (fill in the blanks): "The probability that you sell your plot of land [rises/falls] if player LL increases their price." I asked 5

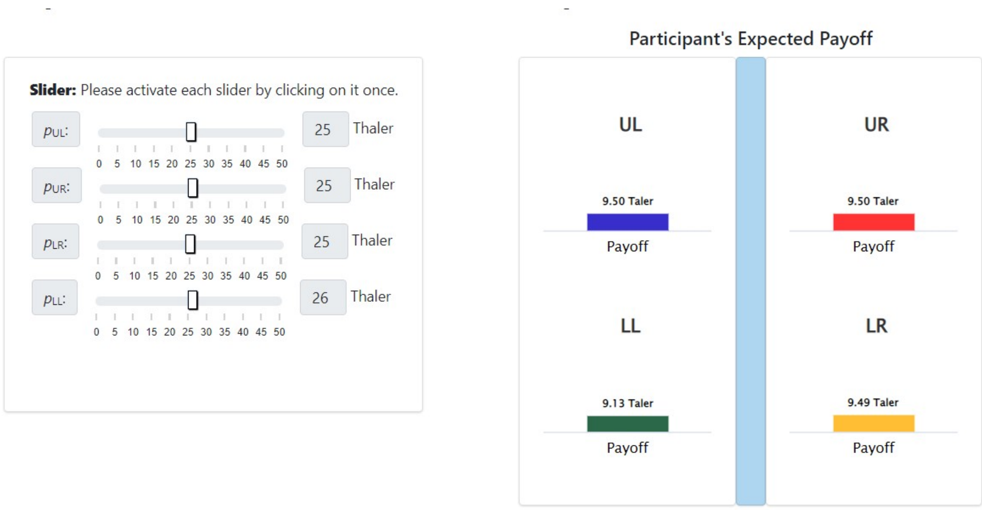


Figure 1.2. A decision aid that helps participants' decision making. I depict the version for the Baseline network.

questions of this type. I did not exclude any participants from the experiment. On average participants answered 4.8 questions correctly and approximately 88% of participants got every question right.

I visualized the market with a map of the four plots. In the experiment, I indicated positions by UL (upper left), UR (upper right), LL (lower left), and LR (lower right). Each participant saw an individual map from their perspective, as UL. I showed this map when explaining the game and when asking the control questions. I also used it to explain each social network treatment and incorporated it into a decision aid.

I gave participants this decision aid to reduce decision error (Figure 1.2) . It calculates each player's expected pay-off from all player's prices. Participants received one slider for each participant's price, including their own. A map of all plots, the river, and friendships between participants is displayed next to the sliders. Bar charts and numbers on each plot indicated the respective participants' expected payoffs. Participants could simulate how changes in their and others' prices affected everyone's expected payoffs by moving the sliders.

To avoid anchoring, I started the decision aid without the bars and the sliders without the slider thumbs. Slider thumbs appeared at the spot where the participants initially clicked the sliders. After the participants clicked on each slider, the bars appeared.

I asked participants to make choices in slightly varying market environments to further increase statistical power. While keeping all other variables, including the treatments, constant, participants were asked to decide on prices for several possible subsidies, ranging from 0 to 20 Thaler. When a sale occurred, the subsidy was added

Table 1.1. All combinations of treatments and belief elicitation.

Treatment	Public/Private	Beliefs
Baseline	Public	Yes
Baseline	Private	No
Complements Symmetric	Public	Yes
Complements Symmetric	Private	No
Substitutes Symmetric	Public	Yes
Substitutes Symmetric	Private	No
Substitutes Asymmetric Couple	Public	Yes
Substitutes Asymmetric Separate	Public	Yes

to the price. This method increases the precision of my estimates if decisions for different subsidies are not perfectly correlated.

1.3.5 Treatment Conditions

I varied two elements of the market: the social network, and price transparency. Sometimes, I also elicited beliefs about players' prices.

Table 1.1 shows the combinations of social network and transparency treatments used in the experiment. It also indicates for which treatments I elicit beliefs. Each participant makes 5 decisions for each row in this table (within subject design). That is one decision for each possible value of the subsidy.

Figure 1.3 depicts all social network treatments. I used these conditions to identify the effect of network links on an individual's prices and equilibrium spillovers of social network links. Each sub-figure represents one treatment from the perspective of a specific participant. This participant is in position UL. Arrows depict friendships. I generated these treatments by assigning participants to different positions in the experimental market.

Before making any decisions, participants saw a diagram of the current social network treatment (see Figure 1.D.1 in Appendix 1.D). This diagram was based on the map of the four plots. I indicated friendships between other players without revealing their names. For example if the player 3 was friends with player 4, I indicted this by writing "LL (Friend of LR) "and "LR (Friend of LL)" in the positions of player 3 and 4, respectively. I remind participants of the current social network by adding the same labels to the diagram on the right side of the decision aid.

I vary price transparency with two treatments: In the *public*, treatment prices could be revealed at the end of the experiment, and in the *private* treatment, they always stayed private. Recall that in both treatments, there were no feedback in-between decisions. At the end of the experiment, participants learned their total payoff. They also received feedback if the computer selected a decision from the pub-

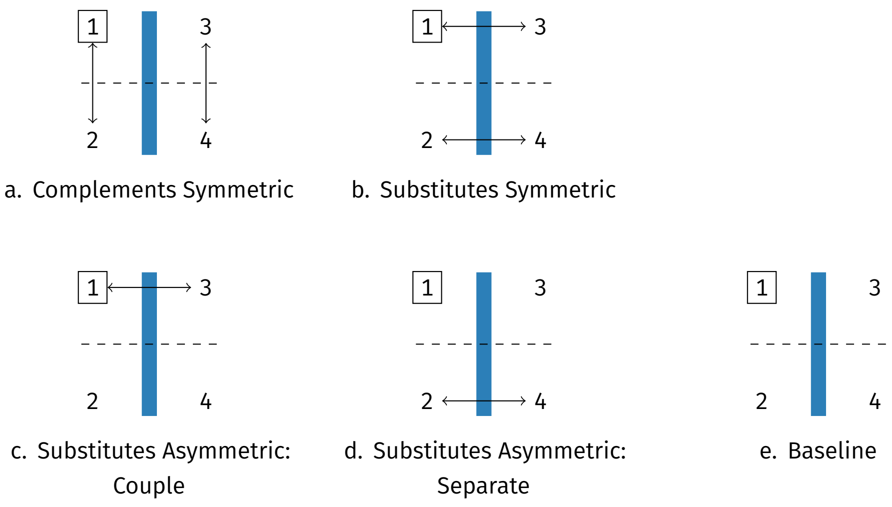


Figure 1.3. The experimental market with different social networks.

lic treatment for payout. In this case, participants learned all prices, their monetary payoff, and which plots were sold. I omit the private treatment for the asymmetric networks (see Table 1.1).

Participants made their decisions, on screens that showed the current transparency treatment and subsidy as well as the decision aid. I reproduce such a screen in Figure 1.D.2 in Appendix 1.D.

1.3.6 Belief Elicitation

I elicited each player's beliefs regarding the expected value of other players' prices. To save time, I concentrated on markets without subsidies and selected treatments (refer to Table 1.1). Participants had to express distinct beliefs about each other player's price.

The belief elicitation process was incentivized with the binarized scoring rule (Hossain and Okui, 2013). Players could win a prize based on a specific probability. This probability increased with the squared distance between the belief and the actual price. This scoring rule is incentive compatible for expected utility maximizers.

I took additional steps to ensure participants stated their expected value of other players' prices. I informed participants that more accurate beliefs would result in higher payoffs, and they could open a collapsed text box to view the exact scoring rule. This approach aligns with best-practice methods (Danz, Vesterlund, and Wilson, 2022), wherein participants can request the scoring rule at the end of the experiment. Participants could not hedge because either a belief task or one of the rounds was randomly chosen for payout (Blanco et al., 2010).

1.3.7 Avoiding Possible Confounds

I took steps to address two potential confounds: minimal group effects and order effects.

Minimal group effects can lead participants to feel a sense of connection with others who are arbitrarily grouped with them, even if the group has no actual significance (Charness and Chen, 2020). In this experiment the river could lead to such groups. To prevent this effect from influencing my results, I used an alternative frame to balance the minimal group effect across sellers of substitutes and complements.

Specifically, I framed the experiment in two ways: a building condition (the one that I used to describe the experiment in the preceding sections) and a bridge condition. In the bridge condition, the buyer wants to build a bridge across the river instead of building on one side. To do so, the buyer wants to buy two adjacent plots on different sides of the river. Both the building and the bridge treatment are strategically equivalent and differ only in framing. These frames are meant to adjust for minimal group effects.

In the building condition, people on the same riverside sell complements, while in the bridge condition, people on the same riverside sell substitutes. I balanced the potential minimal group effect across substitutes and complements by running half of the sessions with the building condition and half with the bridge condition.

Order effects can occur when the order in which participants make decisions affects their subsequent decisions. To minimize this effect, I used two social network treatment orders.[7] I randomized the transparency treatment order and the order of subsidies within each social network treatment.[8] I tried to balance the bridge and building conditions across treatment orders.[9]

1.4 Empirical Results

In this section, I discuss the effect of social networks on prices and efficiency an how it varies with price transparency. I investigate an alternative theory of friendships: higher belief accuracy among friends. After ruling out this theory, I compare

7. Treatment order A is: Substitute Asymmetric, Substitutes Symmetric, Baseline, Complements Symmetric, Substitutes Asymmetric 2; and treatment order B is: Substitute Asymmetric, Complements Symmetric, Baseline, Substitutes Symmetric, Substitutes Asymmetric.

8. For example participants could make decisions in the following order: (Substitute Asymmetric Transparent: 10, 0, 20, 5, 15), (Substitute Asymmetric Private: 10, 0, 20, 5, 15), (Baseline Transparent: 10, 0, 20, 5, 15), (Baseline Private: 10, 0, 20, 5, 15), and so on.

9. I ran 15 session in the bridge and 15 in the building condition. In the building condition I ran 8 sessions with treatment order A and 6 sessions with treatment order B. In the bridge condition I ran 7 sessions with treatment order A and 8 sessions with treatment order B. This differs slightly from the pre-registration (by accident).

an estimated structural directed altruism model to the data to test its quantitative implications and gain further insights.

I always indicate which analyses I preregistered and which are exploratory. I preregistered the analysis, most hypotheses, and the sample size (240) at https://osf.io/5ytnz I preregistered the direction of all effects and one-sided t-tests. My analysis deviates by presenting coefficient plots with 95% confidence intervals instead of these tests.

1.4.1 Friendship Strength

The introductory survey's results suggest that participants have strong and meaningful social connections with their friends (Table 1.2). Participants have an average value of 5 on the IOS scale. This value compares to 3.7 for friends and 5.7 for close friends in Gächter, Starmer, and Tufano (2015). Participants spend 33 hours per week with their friends compared to slightly below twenty hours found by Goeree et al. (2010), who find strong effects of friendship on dictator game contributions. The majority answered the trivia question correctly, two-thirds are best friends, and one-third are romantic or sexual partners.[10]

Table 1.2. Summary of answers to the introductory survey.

Statistic	Obs	Mean	Std. Dev.	Min	Max
Romantic Relationship	233	0.33	0.47	0	1
Time with Friend (h/week)	240	33.80	39.49	0	168
Time with Others (h/week)	240	14.61	13.54	0	100
Best Friend	240	0.60	0.49	0	1
IOS	240	4.96	1.50	1	7
Correct Trivia	240	0.87	0.34	0	1

1.4.2 Estimation Framework

The following sections describe various treatment effect estimates, all of which employ the same regression equation, unless specified otherwise. I regress the price ($p_{i,D,O,S}$) on a treatment indicator (T) and a constant:

$$p_{i,D,O,S} = \alpha + \beta \times T + \epsilon_{i,D,O,S}. \tag{1.1}$$

The treatment indicator (T) and the sample vary across analyses. I index individuals by i, social network treatments by D (Baseline, Substitutes Symmetric, Comple-

10. Answering this question was voluntary, since romantic or sexual relationships are a sensitive topic. Seven people declined to answer.

ments Symmetric, Substitutes Asym. Separate, Substitutes Asym. Couple), the transparency condition by $O = \{public, private\}$, and subsidies by $S \in \{0, 5, 10, 15, 20\}$. Unless specified otherwise, I pool data from both the "public" and "private" treatments and always pool data from different subsidy levels. I cluster standard errors at the friendship pair level.

1.4.3 The Effect of Social Networks on Prices and Efficiency

I examine the impact of social networks on prices by comparing prices in symmetric network treatments to those in the Baseline network. For example, to estimate the treatment effect of substitute friendships, I subtract average prices in the Baseline network from average prices in the Substitutes Symmetric network.

I implement the estimation with the regression from the preceding Subsection. In the example, the sample comprises data from the Substitutes Symmetric and Baseline networks. The treatment indicator (T) is set to 1 for observations from the Substitutes Symmetric network and 0 for those from the Baseline network. I estimate the treatment effect of complement friendships through a parallel comparison for the Complements Symmetric network. Both analyses encompass 4800 observations.[11]

I preregistered these analyses and the following hypotheses: *complement friendships decrease prices, and substitute friendships increase prices.*

Empirically, complement friendships lower prices, and substitute friendships increase prices. Figure 1.4 depicts the estimated causal effect of friendships on prices. The horizontal axis shows the social network treatment, and the vertical axis shows the effect on Thaler prices. Prices are approximately 2 Thalers lower in the complement network and approximately 2.5 Thalers higher in the substitute network.[12] At the end of this Section I interpret these magnitudes in terms of the directed altruism parameter (μ). Participant's beliefs about other's prices move in the same direction as the corresponding prices (See Figure 1.E.1 in Appendix 1.E).

I calculate the expected total surplus to investigate the effects of social networks on efficiency. Since the buyer is computerized, I know his behavior. Consequently, I can take the expected value over the buyers' actions. I do this for each iteration of the market. Then I average over all markets I observed. These markets differ in subsidies, transparency conditions, and the players involved. Figure 1.5 reports average expected total payoffs by network (social surplus). Table 1.J.1 in Appendix 1.J decomposes this surplus into buyer and seller payoffs. I report the average maximum surplus ($p_\ell = p_r = 0$) for reference.

11. These observations are from 240 participants × 2 Networks × 2 Transparency Treatments × 5 subsidies. Since standard errors are clustered by friendship pairs, the sample includes 120 clusters.

12. Prices range from 0 to 50, and one Thaler equals 0.5 Euro, paid out with a probability of 1/48.

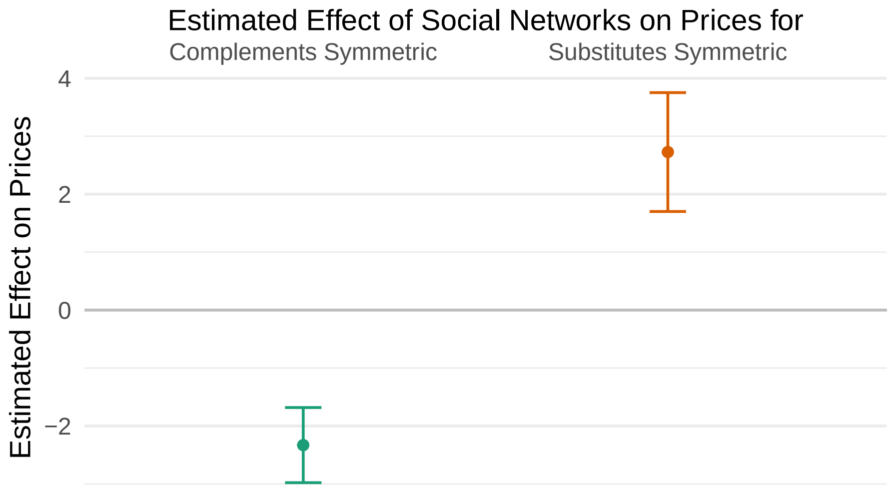


Figure 1.4. Estimated effects of Complement Symmetric and Substitutes Symmetric networks relative to the Baseline network. Standard errors are clustered on the friendship pair level. Error bars indicate 95% confidence intervals. Each analysis includes 4800 observations from 120 friendship pairs.

The causal effects of social networks on prices imply a corresponding change in total surplus. Since the market is imperfectly competitive (prices are too high) lower prices increase efficiency. As shown in Figure 1.J.1, markets with the Complements Symmetric Network have the highest total surplus, followed by markets with the Baseline network and then the Substitutes Symmetric network.

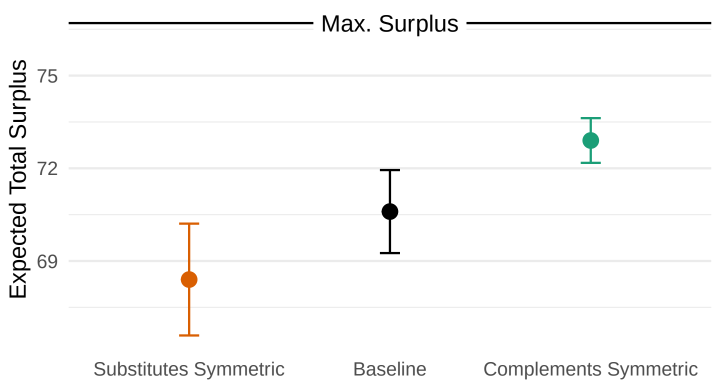


Figure 1.5. Average expected total surplus for all symmetric social networks. Confidence intervals are 95%. Standard errors are taken from a network-wise linear regression of average prices on a constant, with standard errors clustered by session. Each regression uses 600 observations for 29 sessions.This includes 18 sessions with 8 people each and one session with 16 people.

Efficiency in the Substitutes Symmetric network is significantly lower and efficiency in the Complements Symmetric network is significantly higher than in the Baseline network (at the 5% level). To test this I regress total surplus on a Dummy for the each of the two networks with the Baseline network as the reference category. I cluster standard errors at the session level. Both dummies significantly differ from zero at the 5% level in the expect direction.

While the changes in buyers' payoff and total surplus likely hold more generally, the change in sellers' payoff might change with the experiment's parameters. The seller's payoff is inversely related to the buyer's payoff and total surplus. This relationship occurs because prices in the Baseline condition are below the monopoly price. Thus the profit, locally, rises in price. If the holdout problem were severe enough, prices could (in theory) be above the monopoly price. In this case, the profit would fall in price.

1.4.4 The Effects of Transparency on Prices

Social collateral theory predicts that price transparency lowers prices in the substitutes' symmetric network and increases prices in the complements symmetric network.

To sanction your friends, you must know what they did to you. Consequently, social sanctioning is easier in the public than in the private condition. If social sanctioning facilitates cooperation, it should increase the effects of social networks, *raising prices for the Substitutes Symmetric network and lowering them for the Complements Symmetric network.* I preregistered this hypothesis.

I test this hypothesis by comparing prices with and without transparency in the Substitutes Symmetric and the Complements Symmetric treatment. The left part of Figure 1.6 shows the difference in prices between decisions in the complements symmetric network with and without price transparency. The right part shows the corresponding difference for the Substitutes Symmetric network. Error bars indicate 95% confidence intervals with standard errors clustered at the friendship pair level. Figure 1.E.2 in Appendix 1.E shows that price transparency affects first order beliefs in the same way as the underlying prices.

Contrary to my hypothesis, price transparency lowers prices in both networks. Since this finding was unexpected, I started to ask participants, after the experiment, how they reacted to price transparency in the Substitute Symmetric treatment. I also asked them to justify their answer (exploratory and not preregistered). The majority (107) said they did not change their price, 25 said they lowered their price, and 12 said they increased their price. I reproduce the question and the (translated) justifications of participants that lowered their prices in Appendix 1.F.1.[13]

13. Some people gave a generic answer that applies to the public and private treatments, some seemed to misunderstand the incentives, and one statement was too incoherent to be translated.

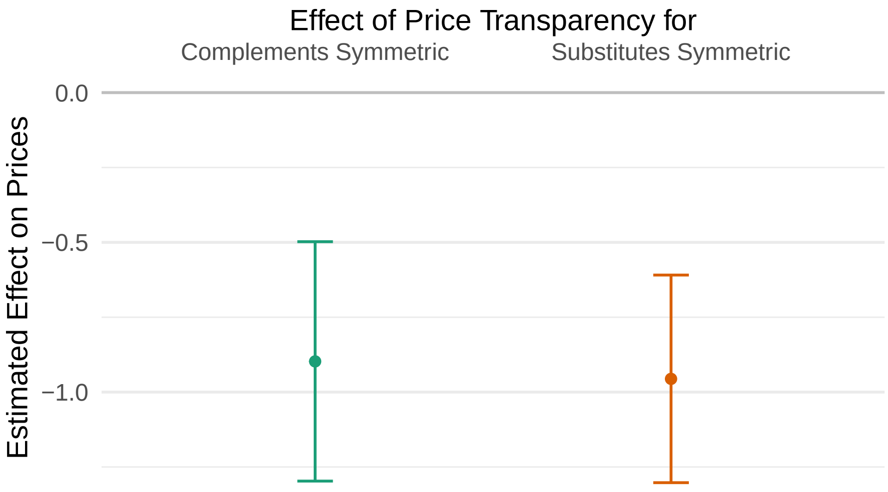


Figure 1.6. Estimated effects of price transparency on prices in the Complements Symmetric and Substitutes Symmetric treatments. Standard errors are clustered by friendship pair. Error bars indicate 95% confidence intervals. Each regression includes 2400 observations in 120 clusters.

Many answers point toward social image concerns (e.g. Andreoni and Bernheim (2009)). In particular, people did not want to appear risk-seeking or greedy. Some of the most explicit statements were:

- "Social desirability. You didn't want to disappoint the others by gambling too high."
- "Because I think that many people are more willing to take risks anonymously (myself included)."
- "I was venturesome about staying secret and didn't want to quote extreme prices that would portray me as greedy."
- "vanity"

1.4.5 Friendship and Belief Accuracy

The familiarity between friends could also affect behavior in the experimental market. I conducted a pilot with strangers instead of friends and asked these strangers to speculate about the effects of friendship. Many of them stated that they know how their friend "ticks", which might affect their behavior. After the experiment, a subset of participants was asked (not preregistered) if they agreed with the following statement "I am a better judge of the price [Name of my Friend] is asking for than what a stranger is asking for." Approximately 63% answered yes (n = 144). Are they right, and does it affect prices?

I address this question by comparing belief accuracy between friends and strangers. I measure belief accuracy by the quadratic deviation of elicited beliefs from realized actions. The expected value of a person's prices maximizes this measure. I divide by the maximum possible deviation (50^2), to normalize the values from 0 (lowest deviation/highest accuracy) to 1 (highest deviation/lowest accuracy).

I test if beliefs are more accurate for friends than strangers by regressing this quadratic deviation on a dummy for friendship, a complement dummy, and dummies for each treatment. This regression includes one observation per belief. The complement dummy is one for beliefs about the prices of other participants that sell complements to the person who believes and zero for beliefs about the prices of participants who sell substitutes. The friendship dummy is one if the person having the belief is friends with the person about whom they have the belief. I cluster standard errors on the friendship pair level for the believers. This analysis was preregistered.

Participants' beliefs are not significantly more accurate for friends than for strangers. Row one of Table 1.3 reports the result of the preregistered specification. The coefficient of the friendships dummy is insignificant and small. Consequently, beliefs are likely not more accurate for friends than for strangers. The other rows report exploratory analyses that I did not pre-register. These analyses indicate that closer friends (as measured by the standardized IOS value) are not better at predicting their friends' actions. People who stated that they had more accurate beliefs about their friends than strangers (Better Beliefs Dummy) do not have significantly more accurate beliefs about their friends than strangers.

We would expect to find a correlation between friends' prices if they had more accurate beliefs about their friend's strategies than strangers'. In the experimental market, prices of substitutes are strategic complements, and prices of complements are strategic substitutes. Thus we would expect a positive correlation between friends' prices if they sell complements and a negative correlation if they sell substitutes. I test this theory in Appendix 1.G and do not find any evidence for it. Consequently, participants' choices are consistent with the finding that beliefs are not more accurate for friends than for strangers.

1.4.6 Structural Model

I test if the data fit the theory quantitatively and qualitatively, by comparing the data to a fitted structural model. I did not pre-register the specification of my structural model. I estimate the model only on the symmetric network treatments (Symmetric Substitutes, Symmetric Complements and Baseline).

To get accurate estimates of the directed altruism parameter (μ), I amend the model from Section 1.2 with joy of winning, decision error, social image concerns and social sanctions. Recall that I denote the subsidy by S, the transparency treatment by O and the social network treatment by D. I write the adjacency matrix as a function of D ($\boldsymbol{M}(D)$) to indicate that the social network treatment determines it.

- My experiment shares a lot of features with a reverse auction. Auction participants often bid above the risk-neutral Nash equilibrium (John H Kagel, 1995;

Table 1.3. Do participants have more accurate beliefs about friends? Regressions of belief accuracy on a friendship dummy an additional controls. All regression controll for treatment dummies and a dummy that indicates if the belief is about a person selling a complement.

	Dependent variable:		
		$\frac{(Belief-Price)^2}{50^2}$	
	(1)	(2)	(3)
Friend	0.005	0.005	0.020*
	(0.007)	(0.007)	(0.012)
IOS Scale (standardized)		0.004	
		(0.003)	
Friend*IOS (standardized)		−0.005	
		(0.005)	
Better Beliefs			−0.003
			(0.007)
Friend*Better Beliefs			−0.021
			(0.013)
Observations	5,757	5,757	3,453
R^2	0.014	0.015	0.013

Notes: *p<0.1; **p<0.05; ***p<0.01; Standard errors are clusterd on the friendship pair level.

Kagel and Roth, 2016). Since my experiment is akin to a reverse auction, on average bids are below the risk-neutral Nash equilibrium. I model this by adding a constant joy of winning (α) to the utility function. This parameter also captures all other forces that may push bids downwards (e.g., risk-aversion, a norm against high prices in the private condition).

- I model the effect of price transparency (social image concerns) with a "tax" (ρ) on high prices in the public treatment.
- Real-world choices are noisy; I model this noise as decision error and estimate a Quantal Response Equilibrium (QRE; McKelvey and Palfrey (1995)).
- I let the directed altruism parameter depend on the transparency condition ($\mu(O)$), to capture that fact that social sanctions may intensify altruism between friends.

Since I focus on symmetric treatments, I focus on player 1's perspective. I collect all parameters in the vector $\gamma = (\mu(public), \mu(private), \alpha, \rho, \lambda)$.

Player 1's material utility is given by,

$$m_1(p_1,p_2,p_3,p_4,S,\gamma) = Pr_\ell(p_1,p_2,p_3,p_4)(\alpha+S+p_1). \tag{1.2}$$

The only difference to the initial theory section is that players get an additional utility of α when they sell their land.

We obtain the vector of utility functions by adding a tax on high prices in the public treatment and replacing material utility with the new specification,

$$\underbrace{\begin{bmatrix} U_1(p_1,p_2,p_3,p_4,S,D,O,\gamma) \\ U_2(p_1,p_2,p_3,p_4,S,D,O,\gamma) \\ U_3(p_1,p_2,p_3,p_4,S,D,O,\gamma) \\ U_4(p_1,p_2,p_3,p_4,S,D,O,\gamma) \end{bmatrix}}_{\text{expected utilities}} =$$

$$= \underbrace{\begin{bmatrix} m_1(.) \\ m_2(.) \\ m_3(.) \\ m_4(.) \end{bmatrix}}_{\text{own payoff}} \underbrace{+\mu(O) \times \boldsymbol{M}(D) \times \begin{bmatrix} m_1(.) \\ m_2(.) \\ m_3(.) \\ m_4(.) \end{bmatrix}}_{\text{altruism term}} \underbrace{-\mathbb{1}(O = public) \times \rho \times \begin{bmatrix} p_1 \\ p_2 \\ p_3 \\ p_4 \end{bmatrix}}_{\text{tax on high prices}}.$$

The parameter ρ captures participants' social image concerns when their prices can get published. This term is motivated by my previous results on price transparency. I include it to separate the effects of friendships from the impact of social image concerns. This method allows me to use data from the public and private treatments without confounding the estimate of the friendship parameter. In particular, I can see if transparency increases cooperation between friends, net of the social image concerns.

QRE generalizes discrete-choice, random-utility models to games.[14] Instead of best-responding players, best-respond noisily. This noise is added to the utility. I use the parametrized version Logit-QRE. The parameter λ captures the relative size of material pay-offs and noise. Higher values of λ, lower the noise. If incentives decrease, decisions become noisier.

I denote player i's probability distribution over prices by σ_i. The probability of player 1, choosing p_1 is given by

$$\sigma_1(p_1,S,D,O,\gamma) = \frac{exp(\lambda \mathbb{E}_{p_2,p_3,p_4}[U_1(p_1,p_2,p_3,p_4,S,D,O,\gamma)])}{\Sigma_{p_1' \in \mathbb{P}} exp(\lambda \mathbb{E}_{p_2,p_3,p_4}[U_1(p_1',p_2,p_3,p_4,S,D,O,\gamma)])} \tag{1.3}$$

, where

$$\begin{aligned} \mathbb{E}_{p_2,p_3,p_4}&[U_1(p_1,p_2,p_3,p_4,S,D,O,\gamma)] = \\ &= \sum_{p_2\in\mathbb{P}} \sum_{p_3\in\mathbb{P}} \sum_{p_4\in\mathbb{P}} U_1(p_1,p_2,p_3,p_4,S,D,O,\gamma) \times \\ &\quad \times \sigma_2(p_2,S,D,O,\gamma)\sigma_3(p_3,S,D,O,\gamma)\sigma_4(p_4,S,D,O,\gamma). \end{aligned} \tag{1.4}$$

14. Recall that I use discrete prices ($\mathbb{P} = \{0, 1, ..., 50\}$).

The probabilities for the other players are analogous.

I estimate the model by maximum likelihood and introduce some additional notation to state the likelihood function. Observations are indexed by $j \in \{1, ..., N\}$. The price of player 1 in observation j is p_{1j}. Treatment D and O differ across observations j, I show this by adding the index j to these variables.

Usually, estimating a QRE model requires solving for the equilibrium for many different parameter values. I use a trick from structural auction models to avoid this. Equation 1.3 depends on the strategies of all other players: $\sigma_2(p_2, S_j, D_j, O_j, \gamma)$, $\sigma_3(p_2, S_j, D_j, O_j, \gamma)$ and $\sigma_4(p_2, D_j, O_j, S_j, \gamma)$. The standard approach would use the analogous equations for the other players and solve for these quantities as equilibrium objects. Following Bajari and Hortaçsu (2005), I plug in these quantities' empirical analogs instead. For example I substitute $\sigma_2(p_2, S_j, D_j, O_j, \gamma)$, with the empirical frequency that a player plays p_2, when the subsidy is S_j, for social network treatment D_j, and transparency condition O_j.

I estimate the model with quasi-maximum likelihood. I maximize the log-likelihood function,

$$LLH(\gamma) \;=\; \Sigma_{j=1}^{N} \log(\sigma_1(p_{1j}, S_j, D_j, O_j, \gamma)), \tag{1.5}$$

with respect to the parameter vector γ. This process generates a covariance matrix under the assumption of independent observations. I adjust these standard errors for clustering with the Huber-White sandwich estimator as implemented in Zeileis (2006).

Table 1.4 lists the estimated parameters with 95% confidence intervals. Directed altruism in the private condition ($\mu(private)$) is between 0.2 and 0.36. This implies that a participant is willing to pay approximately 30 cents for their friend to receive one dollar. Directed altruism does not significantly differ between public and private treatments. The estimated joy of winning parameter (α) is larger than 20. Social image concerns impose a tax of 4% on prices in the public treatment. This value is small but significant, in line with the small treatment effects of price transparency.

Table 1.4. Parameter estimates for the QRE-Directed-Altruism model.

Parameter	Explanation	Estimate	95% CI
Directed Altruism			
$\mu(private)$	private	0.277***	(0.193, 0.361)
$\mu(public) - \mu(private)$	increase public	0.009	(−0.057, 0.074)
ρ	social image concerns	0.037***	(0.013, 0.060)
α	constant	24.600***	(20.60, 28.60)
λ	QRE-parameter	0.250***	(0.189, 0.312)

Notes: *p<0.1; **p<0.05; ***p<0.01; standard errors are clusterd on the friendship pair level.

I plot the fitted model alongside the data to determine if directed altruism can rationalize behavior in the experiment. Figure 1.7 shows the treatment effects of the symmetric networks compared to the Baseline network. I reproduce the empirical treatment effect estimates from Figure 1.4 (Main Effect) with yellow triangles labeled “Data.” I conduct the same analysis used to come up with these estimates on the structural model predictions. These predictions are depicted with purple dots. Model predictions and treatment effect estimates are similar and not significantly different. I do not quantify the uncertainty of the model’s predictions.

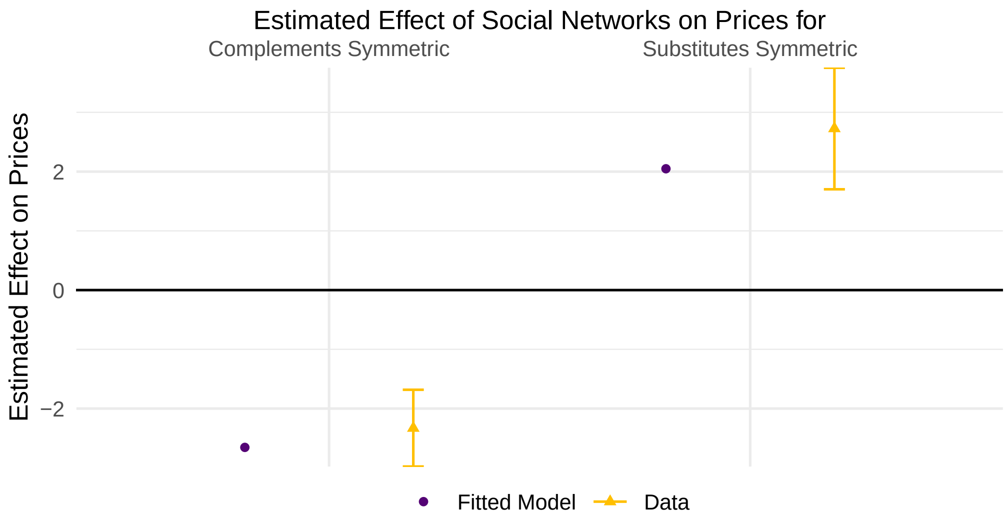


Figure 1.7. This figure shows the estimated treatment effects predicted by the fitted structural model and the reduced form treatment effect estimates, along with 95% confidence intervals calculated using standard errors clustered at the friendship pair level. The estimated treatment effects are drawn from the main analysis, which is reported in Figure 1.4.

Homogeneous linear directed altruism rationalizes the data after accounting for lower bids and decision errors. While the model includes other parameters, these parameters are not concerned with fitting the effects of social networks on prices. Decision error mainly fits the variance of prices. Joy of winning explains the general level of prices without reacting to the social network. The parameter ρ mainly fits the differences between the transparency and private condition. Only the altruism parameter μ directly interacts with the network’s structure. This parameter fits two treatment effects: the effect of symmetric substitute friendships and the effect of symmetric complement friendships.

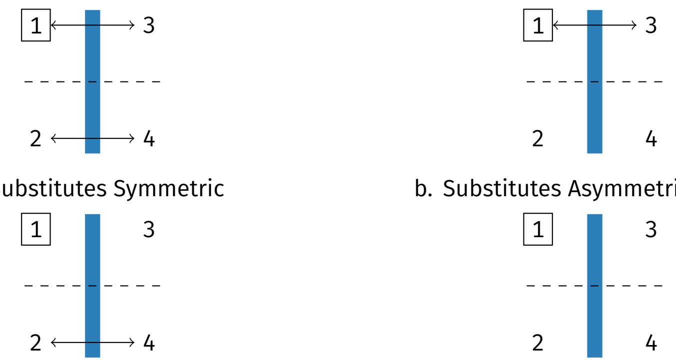
Introducing altruism among strangers has minimal impact on the structural estimates. The experiment is primarily designed to uncover the consequences of altruism among friends rather than strangers. As a result, altruism among strangers is not expected to substantially affect prices, making it challenging to estimate. Appendix 1.I presents a variant of the model incorporating linear altruism among strangers.

The confidence interval for the altruism parameter among strangers is broad, while other parameter estimates remain similar to those in this section.

Closer friends exhibit higher directed altruism parameters. I generate a friendship closeness index using responses from the introductory survey. By fitting a unique directed altruism parameter for each tercile of this index, I find that participants in the lowest tercile have significantly lower directed altruism parameters. For additional details, refer to Appendix 1.H.

1.4.7 Equilibrium Spillover of Friendships

Does the linear, directed altruism model also predict the equilibrium spillovers of friendships? Participants should anticipate that they face different prices dependent on other participants' friendships. In equilibrium, they should react to these changed expectations about other participants' prices. Friendships should have spillovers on people that are not directly affected by them. For friendships among sellers of substitutes, the structural model from the previous section predicts these spillovers to be one-fourth of the size of the direct effect. I use data from the asymmetric substitutes treatment to estimate the spillovers and find that they do not significantly differ from zero.



a. Substitutes Symmetric

b. Substitutes Asymmetric: Couple

c. Substitutes Asymmetric: Separate

d. Baseline

Figure 1.8. All social network treatments used to test for the equilibrium effects of friendships.

To test for the equilibrium effects of friendships, I keep players 1 and 3's friendship constant and vary the friendships of players 2 and 4. Figure 1.8 reports the social network treatments used for this comparison. In the Substitutes Symmetric treatment (row one on the left), players 2 and 4 are friends; in the Substitutes Asymmetric couple (row one on the right) treatment, they are not. The second row shows the same comparison, with a slight difference: players 1 and 3 are friends in both cases.

I estimate the treatment effect of players 2 and 4's friendship as the difference between two means: The treated mean is the average price in the Substitutes Symmetric" and Substitutes Asymmetric: Separate" treatments, where 2 and 4 are friends, and the control mean is the average price in the Substitutes Asymmetric: Couple" and Baseline" treatments, where 2 and 4 are strangers. Both treatment and control groups include an equal number of observations where 1 and 3 are friends and where they are strangers. I run both networks only in the public treatment.

The structural model from the preceding section makes quantitative out-of-sample predictions for the equilibrium effects of friendships. Assuming that participants have consistent beliefs, I can estimate player 1's equilibrium beliefs about other players' prices from realized price frequencies, considering each social network depicted in Figure 1.8. Then, I calculate the noise best response by plugging them into Equation 1.3 (the QRE best response) and use the parameters estimated from the symmetric treatments. I average over all subsidies and calculate the predicted treatment effect of a friendship between players 2 and 4 on player 1's prices. Figure 1.9 shows the QRE prediction as a grey line.

The friendship between players 2 and 4 should lower player 1's prices. Since players 2 and 4 sell substitutes for each other's goods, their friendship raises their prices. Player 1 is now faced with a higher price for their complement (p_2) and slightly higher prices for their substitutes ($p_3 + p_4$). The higher p_2 raises the price for both plots on the left. Player 1 should react by lowering their price. The higher price on the right softens competition and would allow player 1 to lower their price. The structural model predicts that the former effect is much stronger. The model predicts that player 1 will lower their price in response to the friendship between 2 and 4.

The actual equilibrium effects of friendships (between 2 and 4) are estimated with a similar regression as the main effects (Section 1.4.2). The dependent variable is the price of player one in each network from Figure 1.8. Each participant is player 1 in these networks for five different subsidies. Consequently, we observe each player ten times when 2 and 4 are friends and ten times when they are not. Observations from Substitutes Symmetric and Substitutes Asymmetric (separate) are in the treated group, and observations from Substitutes Asymmetric (couple) and Baseline are in the control group. I conduct this regression twice: once with the actual prices as the dependent variable and once with all other players' beliefs about these prices. I cluster standard errors at the friendship pair level for the participants that decided on the price and the participants that stated the belief. I preregistered this analysis with the hypothesis that the friendship between 2 and 4 lowers 1's price and that first-order beliefs behave accordingly. The estimated treatment effect on prices is depicted on the left side and the treatment effect on beliefs on the right side of Figure 1.9.

Compared to the model benchmark, participants under-react to other participants' friendships. As Figure 1.9 shows, the model predicts participants to lower their prices in response to the other participant's friendship. The data do not show any evidence for that.

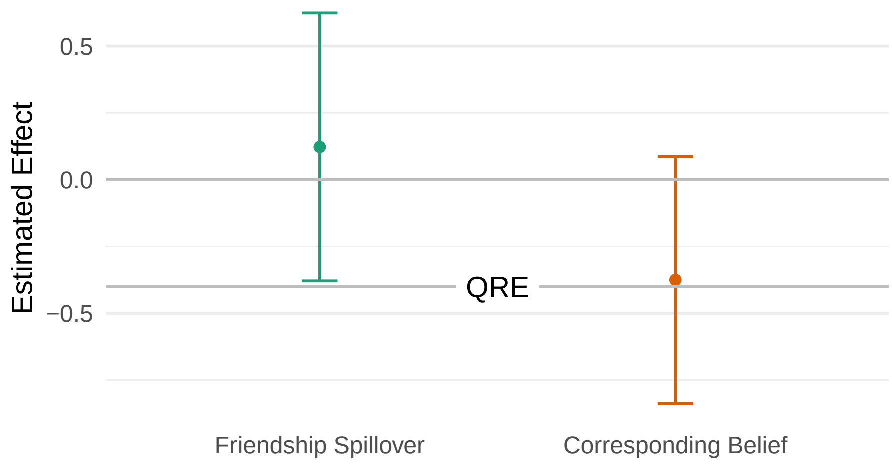


Figure 1.9. Estimated effects of friendships between 2 and 4 on 1's prices and beliefs about 1's prices. Standard errors are clustered on the friendship pair level. Error bars indicate 95% confidence intervals. The analyses uses 4800 observations in 120 clusters.

I do not find evidence for the theory that players under-react because of biased beliefs. Figure 1.E.3 in Appendix 1.E reports the effect of a substitute friendships on beliefs about the friends prices. Participants always (in symmetric and asymmetric networks) belief that substitute friends charge higher prices than strangers. Consequently they should reduce their prices when they face substitute friendships, as predicted by the structural model.

1.5 Conclusion

I conduct an experiment with real world friendships in a laboratory market with substitutes and complements. In this experiment, complement friendships decrease prices and increase efficiency and substitute friendships do the opposite. The linear directed altruism model fits the data well. Price transparency reduces prices for all symmetric social networks. This data and the estimated structural model suggest that price transparency increases social image concerns and does not increase cooperativeness between friends. In this experiment, participants' beliefs about their friend's actions are not more accurate than about strangers' actions.

The unexpected effect of price transparency suggests that more than findings from simple two-person experiments on cooperation in markets is needed to predict behavior in more complex markets with more participants. With more than two persons, a player's action may affect people other than their friend. Adding these people to the situation may alter the effects of friendship. Leider et al. (2009) vary the ability for social sanctions in a modified dictator game by hiding and revealing the dictator's identity. They find that the ability for social sanctions increases altruistic behavior. I vary the ability for social sanctions by hiding and revealing players' actions and find no effect of transparency on altruistic behavior but uniformly lower prices. This price reduction could be due to increased social image concerns. Participants care how they look in front of their friends and strangers. While the discrepancy could also stem from the difference in how this paper facilitates social sanctioning, the finding still suggests that previous results on friendship and social sanctioning might not be applicable to price transparency in larger markets.

My results suggest that markets for the assembly of complements can be particularly efficient when there are complement friendships. This result suggests a lower need for government intervention in markets with complement friendships.

The result also suggests that market designers want to emphasize social networks when there are complement friendships. This can occur through, reducing anonymity and using mechanisms that retain externalities between participants instead of reducing them like Bierbrauer et al. (2017). In this experiment price transparency does not boost the effects of social networks.

One example for markets with complement friendships are land markets with geographic social networks (Ambrus, Mobius, and Szeidl, 2014). In land markets often close plots are complements and distant plots are substitutes. In geographic networks neighbors are more likely to be friends. Consequently, these two properties lead to complement friendships.

This experiment indicates that friendships in markets can be described by the same preferences as firms with common owners. However, We need further research to investigate the connection between common ownership and friendship. In this paper, firms are unitary actors. Each participant owns one piece of land that they can sell. Real-world firms have a more complex corporate governance structure. Directed altruism at the level of individual decision-makers is embedded in this structure. To understand the firm-level impact of linear, directed altruism preferences, we must understand the interplay between these preferences and corporate governance. How can individual-level directed altruism translates to firm-level common ownership preferences?

Friends in my experiment be

Empirically, shareholders likely want firms to higher friends for top-management positions. Backus, Conlon, and Sinkinson (2021b) calculate the profit weights implied by common ownership models. Common ownership theory implies that firms in the S&P 500, on average, weigh the profits of other firms in the S&P 500 at 70% of their profits. In my experiment, people weigh their friends' profits at 30% of their profits. Hiring friends likely moves firms closer to common ownership preferences without the risk of overshooting.

Hiring friends to implement common ownership weights is consistent with the mechanisms described in Anton, Gine, and Schmalz (2022). Friendships are unlikely to induce the total common ownership profit weights (30% vs. 70%). Thus firms would want to use weakened management incentives as an additional mechanism.

Further research should embed linear, directed altruism preferences in the Anton, Gine, and Schmalz (2022) model and investigate the correlation between profit weights and friendships between managers.

Appendix 1.A Proof of Proposition 1

Proof of Lemma 1. I write this proof for a uniform value distribution from 0 to 1 and prices from 0 to 0.5. It also holds for a uniform value distribution from 0 to 100 (which I use in the main text) and prices from 0 to 50.

Recall that $p_\ell = p_1 + p_2$ and $p_r = p_3 + p_4$. The probability that the buyer buys on the left-side is,

$$\begin{aligned}\Pr_\ell(p_1,p_2,p_3,p_4) &= \int_0^1 \int_0^1 \mathbb{1}(\theta_\ell - p_\ell > \theta_r - p_r)\mathbb{1}(\theta_\ell - p_\ell > 0)\times \qquad (1.A.1)\\ &\quad \times f(\theta_r) f(\theta_\ell) d\theta_\ell d\theta_r \\ &= \begin{cases} (1-p_\ell) - 0.5(1-p_r)^2 & \text{if } p_\ell \le p_r \\ (1-p_\ell)\times p_r + 0.5(1-p_\ell)^2 & \textit{if } p_r < p_\ell \end{cases}. \qquad (1.A.2)\end{aligned}$$

I start by characterizing the symmetric equilibrium of the Substitutes Symmetric network. Player 1 solves

$$\max_{p_1 \in [0,0.5]} \Pr_\ell(p_1,p_2,p_3,p_4) \times (p_1 + S) + \mu \times \Pr_r(p_1,p_2,p_3,p_4) \times (p_3 + S)$$

The first order condition is:

$$\frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1} \times (p_1 + S) + \Pr_\ell(p_1,p_2,p_3,p_4) + \\ + \mu \frac{\partial \Pr_r(p_1,p_2,p_3,p_4)}{\partial p_1} \times (p_3 + S) = 0$$

and the second order condition is:

$$\frac{\partial^2 \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1^2} \times (p_1 + S) + 2 \times \frac{\partial \Pr_\ell(p_1,p_2,p_3,p_4)}{\partial p_1} + \\ + \mu \times \frac{\partial^2 \Pr_r(p_1,p_2,p_3,p_4)}{\partial p_1^2} \times (p_3 + S) < 0$$

By plugging in the derivatives of Equation 1.A.2 into the second order condition we get

$$-(2 + \mu(p_3 + S)) < 0, \text{ if } p_\ell \le p_r$$

and

$$-(p_1+S)-2(1+p_r-p_\ell)-\mu(p_3+S) \ < \ -(p_1+S)-\mu(p_3+S) \ < \ 0, \text{ if } p_r \ < \ p_\ell,$$

which is true and implies that player 1's utility function is strictly concave in p_1. Therefore all players ($i \in \{1,2,3,4\}$) utility functions are strictly concave in their own price (p_i).

Any symmetric equilibrium strategy p_s satisfies the first order condition:

$$\begin{aligned} g(p_s,\mu) := \frac{\partial Pr_\ell(p_s,p_s,p_s,p_s)}{\partial p_1}(p_s+S)+\mathrm{Pr}_\ell(p_s,p_s,p_s,p_s)+ \\ +\mu\frac{\partial\,\mathrm{Pr}_r(p_s,p_s,p_s,p_s)}{\partial p_1}(p_s+S)=0 \\ \Leftrightarrow g(p_s,\mu) = -(p_s+S)+(1-2p_s)-0.5(1-2p_s)^2+\mu(1-2p_s)(p_s+S)=0 \end{aligned} \tag{1.A.3}$$

.

I use the intermediate value theorem to show that this equation has a solution. The function g is continuous because it is a composition of continuous functions. I calculate that $g(0,\mu)=(-1+\mu)S+0.5$ and $g(0.5,\mu)=-(1+S)$. The first expression is larger than 0 if $(-1+\mu)S+0.5>0 \Leftrightarrow 0.5>(1-\mu)\times S$. This is true because $0.5>(1+\mu)\times s$. The second ($g(0.5,\mu)$) is always larger than zero. Consequently, the FOC has an interior solution by the intermediate value theorem. Furthermore this solution is the symmetric equilibrium price $0<p_s<0.5$.

Now I characterize the symmetric equilibrium of the Complements Symmetric network. Player 1 solves

$$\max_{p_1} \mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)\times(p_1+S)+\mu\times\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)\times(p_2+S)$$

The first order condition is:

$$\begin{aligned} \frac{\partial\,\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}(p_1+S)+\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)+ \\ +\mu\frac{\partial\,\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}\times(p_2+S)=0 \end{aligned}$$

and the second order condition is:

$$\begin{aligned} \frac{\partial\,\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)}{\partial^2 p_1}(p_1+S)+2\frac{\partial\,\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)}{\partial p_1}+ \\ +\mu\frac{\partial\,\mathrm{Pr}_\ell(p_1,p_2,p_3,p_4)}{\partial^2 p_1}\times(p_2+S)<0 \end{aligned}$$

By plugging in the derivatives of Equation 1.A.2 into the second order condition we get

$$-2 \ < \ 0, \text{ if } p_\ell \ \leq \ p_r$$

and

$$-(p_1+S)-2(1+p_r-p_\ell)-\mu(p_3+S) < -(p_1+S)-\mu(p_3+S) < 0, \text{ if } p_r < p_\ell,$$

which is true and implies that player 1's utility function is strictly concave in p_1. Therefore all players utility functions are strictly concave in their own price p_i.

Any symmetric equilibrium strategy p_s satisfies the first order condition:

$$\begin{aligned} g(p_c,\mu) := \frac{\partial \Pr_\ell(p_c,p_c,p_c,p_c)}{\partial p_1}(p_c+S)+\Pr_\ell(p_c,p_c,p_c,p_c)+ \\ +\mu\frac{\partial \Pr_\ell(p_c,p_c,p_c,p_c)}{\partial p_1}(p_c+S)=0 \\ \Leftrightarrow g(p_c,\mu)=(1-2p_c)-0.5(1-2p_c)^2-(1+\mu)(p_c+S)=0. \end{aligned}$$

I use the intermediate value theorem to show that this equation has a solution. The function g is continuous because it is a composition of continuous functions. I calculate that $g(0,\mu)=0.5-(1+\mu)S$ and $g(0.5,\mu)=-(1+\mu)(1+S)$. The first expression is larger than 0 if $0.5-(1+\mu)S>0 \Leftrightarrow 0.5>(1+\mu)\times S$, which is true by assumption. The second ($g(0.5,\mu)$) is always larger than zero. Consequently, the FOC has an interior solution by the intermediate value theorem. Furthermore this solution is the symmetric equilibrium price $0<p_c<0.5$.

In conclusion the Substitute Symmetric and Complement Symmetric networks have an interior symmetric equilibrium: In each of these networks player's utility functions are strictly concave in their own price. Since both networks nest the Baseline network, for $\mu=0$, this also holds for the Baseline network. □

Proof of Proposition 1.1. In all three symmetric networks the equilibrium is on the interior of the price space and the objective function is concave. Therefore symmetric equilibrium prices solve the first order conditions:

$$\begin{aligned} \frac{\partial \Pr_\ell(p_s,p_s,p_s,p_s)}{\partial p_1}(p_s+S)+\Pr_\ell(p_s,p_s,p_s,p_s)+ \\ +\mu\frac{\partial \Pr_r(p_s,p_s,p_s,p_s)}{\partial p_1}(p_s+S)=0 \end{aligned} \tag{1.A.4}$$

$$\begin{aligned} \frac{\partial \Pr_\ell(p_c,p_c,p_c,p_c)}{\partial p_1}(p_c+S)+\Pr_\ell(p_c,p_c,p_c,p_c)+ \\ +\mu\frac{\partial \Pr_\ell(p_c,p_c,p_c,p_c)}{\partial p_1}(p_c+S)=0 \end{aligned} \tag{1.A.5}$$

$$\begin{aligned} \frac{\partial \Pr_\ell(p_b,p_b,p_b,p_b)}{\partial p_1}(p_b+S)+ \\ +\Pr_\ell(p_b,p_b,p_b,p_b)=0. \end{aligned} \tag{1.A.6}$$

Define the marginal private gain from higher prices in the symmetric equilibrium as:

$$h(p) = \frac{\partial \Pr_\ell(p,p,p,p)}{\partial p_1}(p+S) + \Pr_\ell(p,p,p,p).$$

This expression ($h(p)$) falls in p because $\frac{\partial \Pr_\ell(p,p,p,p)}{\partial p_1} = -1$.

Taking the difference between Equations 1.A.4 and 1.A.6 and rearranging yields:

$$h(p_b) - h(p_s) = \mu \frac{\partial \Pr_r(p_s,p_s,p_s,p_s)}{\partial p_1}(p_s+S) > 0 \quad (1.A.7)$$

$$\leftrightarrow h(p_b) > h(p_s) \Leftrightarrow p_s > p_b. \quad (1.A.8)$$

Taking the difference between Equations 1.A.5 and 1.A.6 and rearranging yields:

$$h(p_b) - h(p_c) = \mu \frac{\partial \Pr_\ell(p_c,p_c,p_c,p_c)}{\partial p_1}(p_c+S) < 0 \quad (1.A.9)$$

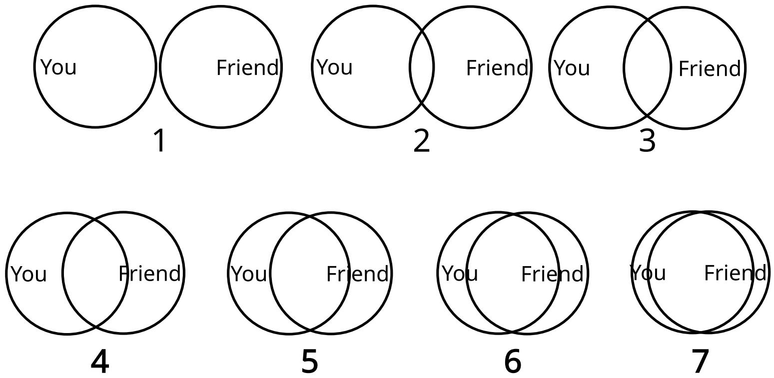
$$\leftrightarrow h(p_b) < h(p_c) \Leftrightarrow p_b > p_c. \quad (1.A.10)$$

□

Appendix 1.B Survey Questions

I asked the following Survey questions. I give possible answers in square brackets.

- Did you bring your best friend with you? [yes, no]
- How many hours do you and the friend you brought with you spend together every week? [number between 0 and 168]
- How many hours do you spend with other friends each week in total? [number between 0 and 168]
- Trivia question (one of the following):
  - Are you vegetarian or vegan? [yes, no]
  - What time do you usually wake up on weekdays? [hourly brackets from before 5 am to after 11 am]
- What do you think your friend answered to the last question? If you are correct, you will receive a prize of 10 Thalers. [same as the trivia question]
- Which of the following pictures best describes your friendship?



- Are you in a romantic or sexual relationship with your friend? [yes, no, do not want to say]
- In general, how willing or unwilling are you to take risks? [integers from “0 - Not at all willing to take risks” to “10 - Very willing to take risks”]

Appendix 1.C Control Questions

I asked the following control questions in two batches (1-3 and 4-5).

(1) The probability that you (Participant UL) will sell your property, [*decreases*, *increases*], when Participant LL raises the price.
(2) The probability that you (Participant UL) will sell your property, [*decreases*, *increases*], when Participant UR raises the price.
(3) The probability that you (Participant UL) will sell your property, [*decreases*, *increases*], when Participant LR raises the price.
(4) When you (Participant UL) raise your price, [*decreases*, *increases*] the probability that the buyer will purchase property LL.
(5) When you (Participant UL) raise your price, [*decreases*, *increases*] the probability that the buyer will purchase properties UR and LR.

After each batch I gave participants feedback that corrected the wrong answers. Together with each batch I showed participants a map of the experimental land market (see Figure 1.C.1).

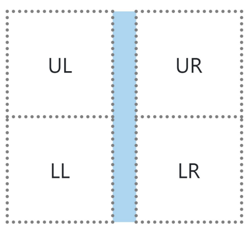


Figure 1.C.1. Map that I showed before each batch of control questions.

Appendix 1.D Screenshots from the Experiment

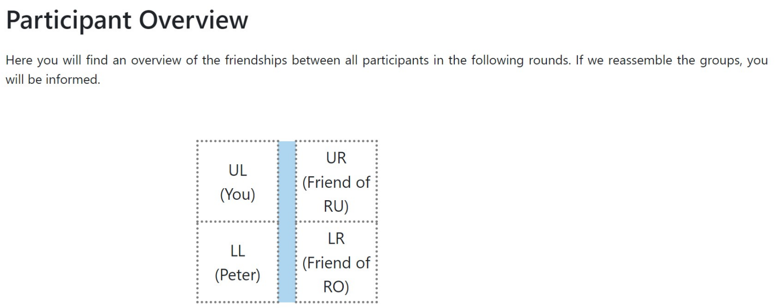


Figure 1.D.1. Overview of the social network treatment: An example of the Complements Symmetric network in the building condition, with the participant's friend's name set to Peter.

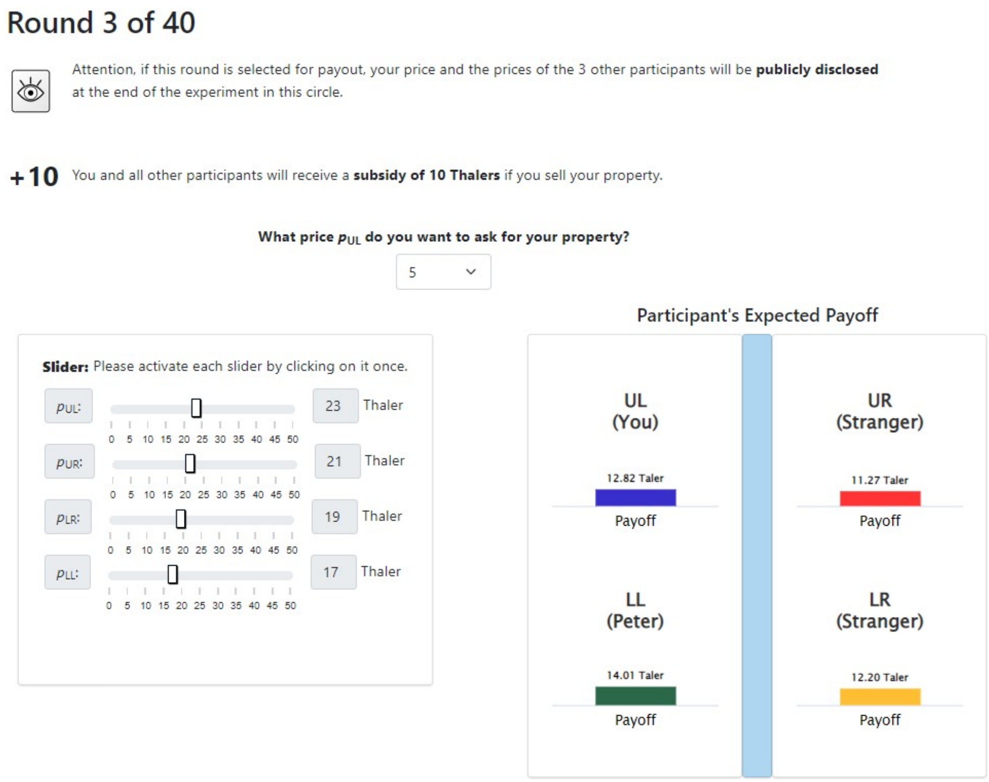


Figure 1.D.2. Screenshot of the decision screen used in the experiment to elicit participant choices for different subsidy levels. The top of the screen displays information about the subsidy and the transparency treatment, which varied between public and private. Participants were asked to enter a price for their property (indicated by UL on the map) and were provided with a decision aid (shown in Figure 1.2) to simulate the consequences of their and others' decisions.

Appendix 1.E Beliefs

Figures 1.E.1 and 1.E.2 revisit analyses from Section 1.4, using beliefs as the dependent variables instead of participants' prices. The belief data contain three observations for each observation in the price data since for each price there are three participants who have a belief about it. This analysis was preregistered with the hypothesis that beliefs would react in the same direction as the actual variables. Standard errors are clustered at the friendship pair level of those who formed the belief. Clustering at the individual level yields identical results since individuals are nested within friendship pairs. Each table caption refers to a figure for the corresponding analysis, where prices serve as the dependent variable instead of beliefs.

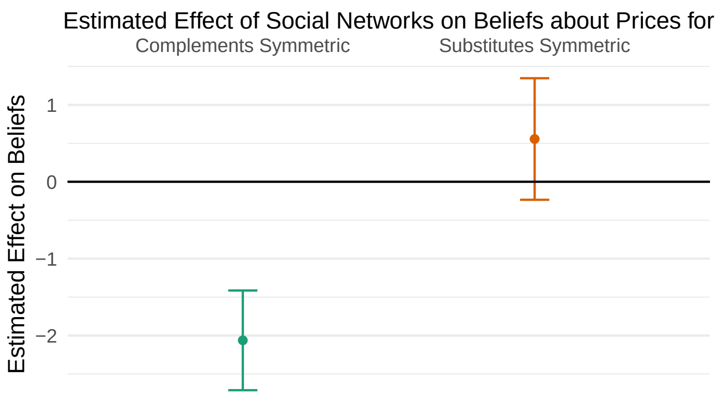


Figure 1.E.1. Estimated effect of complement and substitute friendships on first-order beliefs. Standard errors are clustered at the friendship pair level. This figure is analogous to Figure 1.4.

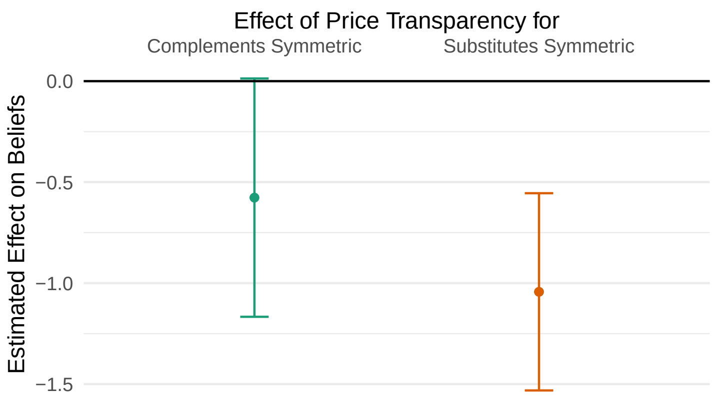


Figure 1.E.2. Estimated effects of price transparency on beliefs in the complement symmetric and substitute symmetric treatments. Standard errors are clustered at the friendship pair level. This figure is analogous to Figure 1.6.

The left side of Figure 1.E.3 examines asymmetric networks, focusing on how a participant's (he) belief about another participant (she) changes when she transitions from being isolated to being friends with a seller of a substitute, while his friendships remain constant. To estimate this effect, I compare beliefs about participants in the Substitutes Symmetric and Substitutes Asymmetric Couple treatments to beliefs about participants in the Baseline and Substitutes Asymmetric: Separate treatments. This analysis corresponds to the left part of Figure 1.9, with prices replaced by first-order beliefs about them.[15]

The right side of Figure 1.E.3 investigates symmetric networks, replicating the right side of Figure 1.E.1.

In both asymmetric and symmetric networks, participants expect prices to be higher when individuals are friends with others selling a substitute, as opposed to when their friends do not participate in the market. The coefficients on both sides of Figure 1.E.3 are very similar, indicating that participants anticipate similar effects of substitute friendships in both asymmetric and symmetric networks. The consistency in belief-changes across different network structures indicates that an underreaction in asymmetric networks compared to symmetric networks is unlikely to be the source of a lack of equilibrium spillovers.

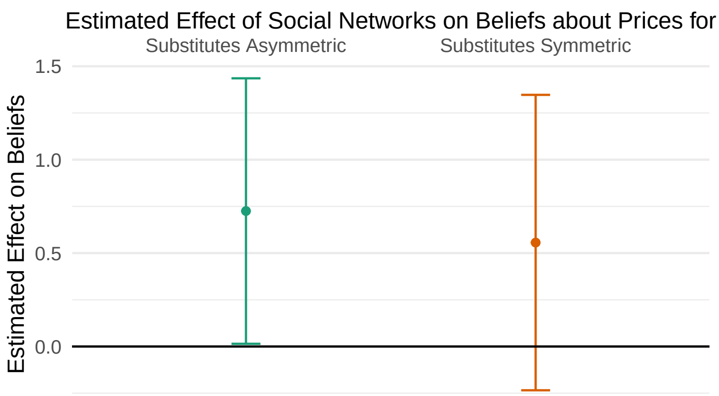


Figure 1.E.3. Effect of substitute friendships on beliefs about substitute prices in the substitute symmetric and substitute asymmetric treatments.

15. The right part of Figure 1.9 also reports an analysis about beliefs. However, this analysis considers the mirror image of the analysis reported in Figure 1.E.3. It looks at the belief of people who change from being isolated to selling substitutes about people whose friendships do not change.

Appendix 1.F Open Question Price Transparency

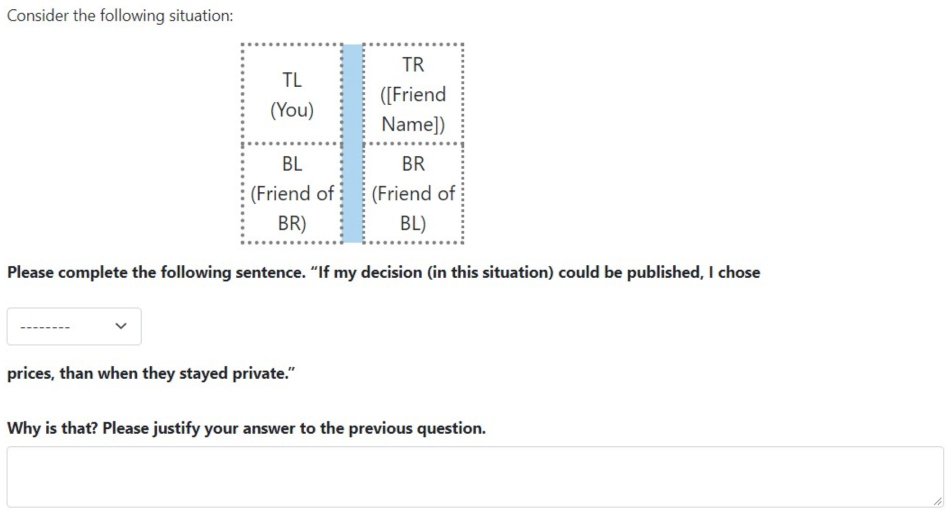


Figure 1.F.1. Open question regarding price transparency in the substitutes treatment (translated from German).

1.F.1 Answers of Participants that Lowered Prices

"I think in this situation I could have brought a win for both sides."
"If there is no payout, the disclosed price is not too risky."
"So that I can sell my property with a higher probability."
"Because I feel safer with a lower price."
"I was venturesome about staying secret and didn't want to quote extreme prices that would portray me as greedy. I also expected that a decision that could be published, would be selected."
"Because I didn't want to be responsible for a failed sale because I set a high price. "
"You don't want to come across in front of others as if you're just out for the money. In addition, people does not want to be publicly responsible if the other does not receive a price either. "
"vanity"
"Better lower payouts than no payouts."
"So my chances of winning are higher."
"I chose low prices because I suspect that the knowledge about my higher pricing could potentially negatively impact trading."
"I wanted to choose a lower price so that the probability of selling the property is higher. If I had chosen the price too high and we had not sold, I would have felt guilty to my counterpart."
"Because I believe that if the decision could be announced, [name] also chose lower

prices.”
“Because I think that many people are more willing to take risks anonymously (myself included).”
“So that I have not chosen too high prices and therefore the upper plots are not sold by me.”
“[name] would see that I chose too high, unpleasant.”
“If it is not anonymous, I do not want to take too high prices myself.”
“Because that decides whether you get the profit.”
“So that I don’t look greedy and I’m not fault that our site is not bought.”
“So that nobody is angry if they don’t earn money because of me.”
“Probably I would have compared my prices with those of [name] and noticed that hers are lower than expected, so I would have started to set lower ones as well.”
“Social desirability. You didn’t want to disappoint the others by gambling too high.”
“Because you may be fault afterwards if a purchase does not take place.”
“I didn’t want to overestimate my prices when other participants see that. ”

Appendix 1.G Correlation Between Prices

I test for the correlation between friend’s prices by regressing a person’s price on their friend’s price. I restrict the sample to the Complements Symmetric and substitutes treatments, as well as the substitutes asymmetric couple treatment. I estimate the following regression

$$p_{i,D,O,S} \;=\; \alpha + \beta * p_{-i,D,O,S} * S_{-i,D,O,S} + \gamma * p_{-i,D,O,S} * (1 - S_{-i,D,O,S}) + \delta * X_i + \epsilon_{i,D,O,S},$$

$p_{i,D,O,S}$ is the price of participant i in network D, transparency treatment (O) and subsidy S, $p_{-i,D,O,S}$ is the corresponding price of i’s friend and $S_{-i,D,O,S}$ is one, if the friend sells a substitute. The variable X_i includes additional controls: player i’s prices in the Baseline and Substitutes Asymmetric: Separate treatments, a social network treatment indicator and fixed effect for a player’s answer on the risk aversion questions. I cluster standard errors at the friendship pair level.

Appendix 1.H Friendship Closeness and the Strength of Directed Altruism

I investigate the relationship between friendship closeness and market cooperation, hypothesizing that closer friends exhibit greater cooperation. Specifically, closer friends should raise prices more when selling complements and less when selling substitutes. In my model, the closer friendships should exhibit a higher directed altruism parameter.

To create a friendship closeness index, I conducted a principal component analysis using responses from the introductory survey’s friendship questions, as outlined

Table 1.G.1. Estimated relationship between friends' prices.

	Dependent variable:	
	Price	
	(1)	(2)
Complements · Price Friend	−0.009	−0.031
	(0.053)	(0.055)
Substitute · Price Friend	−0.024	−0.034
	(0.044)	(0.046)
Controll Variables		
Treatment Dummies	Yes	Yes
Baseline and Sep. Prices	Yes	Yes
Risk Aversion	Yes	Yes
Cost	No	Yes
Secret	No	Yes
Observations	3,000	3,000
R^2	0.361	0.364

Notes: *p<0.1; **p<0.05; ***p<0.01; Standard errors are clustered on the friendship pair level.

in Appendix 1.B. I incorporated a dummy variable for accurate guesses in the trivia question and log-transformed the values for time spent with friends and others. I addressed missing data on romantic or sexual relationships by employing a dummy variable that indicates if this question has a missing value. In this case, the original variable is coded as zero. The resulting index is the first principal component, multiplied by (-1). I conduct this analysis on an individual level; therefore, friends have correlated but different values for this index.

The friendship closeness index is positively related to all variables representing strong and meaningful friendships. Figure 1.H.1 displays the factor loadings for the first principal component multiplied by (-1). Since the friendship closeness index is derived from the first principal component multiplied by (-1), a positive factor loading, after being multiplied by (-1), indicates a positive relationship between that variable and the friendship closeness index. All variables, except for the log of time spent with others and missing values in the romantic relationship question, have a positive association with the friendship index.

A reduced form analysis is not powerful enough to test for the hypothesized effect. I use data for all symmetric social networks and regress prices on social network dummies interacted with my friendship closeness indicator. If closer friends act more altruistically, the coefficient of Complements × Friendship Closeness Index" should

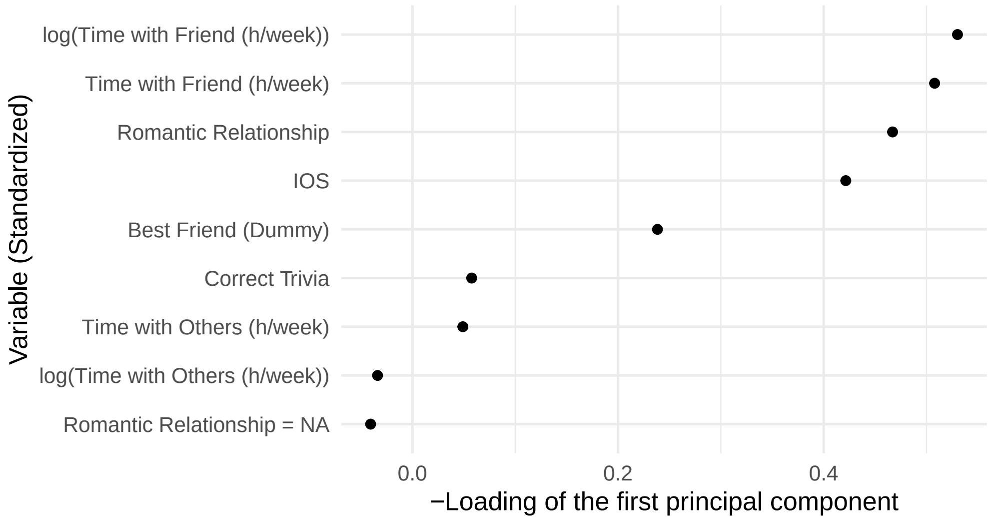


Figure 1.H.1. Factor loadings of the first principal components of friendship measures. All factor loadings are multiplied by -1, because I use -1 times the first principal component to measure friendship strength.

be negative and the coefficient of Substitutes × Friendship Closeness Index" should be positive. These coefficients have the expected sign, but they are not significantly different from zero. This is due to the fact that I am making a between-subject comparison in an experiment that is powered to detect a within-subject treatment effect. I can increase power by enforcing that friendship closeness should act similarly in the Complements and Substitutes networks, but in different directions. I do this with the help of a structural model.

I estimate a version of the structural model where the directed altruism parameter can vary with relationship closeness. I define a participant's directed altruism parameter as a function of transparency treatments and the friendship closeness index (FCI). To facilitate my estimation, I bin the FCI into terciles ($FCI_{1/3}, FCI_{2/3}$). The lowest tercile forms the Baseline, and belonging to the middle tercile can change the Baseline directed altruism parameter by δ_m, while belonging to the highest tercile can change it by δ_h,

$$\begin{aligned} \mu(T,FI) =& \mu(private) + \mathbb{1}(T = public) \times (\mu(public) - \mu(private)) \\ &+ \mathbb{1}(FCI_{1/3} < FCI < FCI_{2/3})\delta_m + \mathbb{1}(FCI_{2/3} < FCI)\delta_h. \end{aligned}$$

Participants who are not very close to their friends exhibit lower directed altruism. Table 1.H.2 reports the parameter estimates from the structural model where the directed altruism parameter can vary with relationship closeness. I find lower directed altruism parameters for participants whose friendship closeness falls in the bottom tercile. The directed altruism parameters for the top two terciles are very similar.

Table 1.H.1. Do closer friends behave more altruistically? Regression of prices on social network treatments interacted with the friendship closeness index.

	Dependent variable:
	Price
Substitutes	-2.15^{***}
	(0.31)
Complements	2.61^{***}
	(0.48)
Friendship Index	-0.25
	(0.26)
Substitutes x Friendship Closeness Index	-0.08
	(0.18)
Complements x Friendship Closeness Index	0.30
	(0.24)
Constant	16.04^{***}
	(0.44)
Observations	9,600
R^2	0.03

Notes: *p<0.1; **p<0.05; ***p<0.01; Standard errors are clustered on the friendship pair level.

Table 1.H.2. Parameter estimates for the QRE-Directed-Altruism model, when the altruism parameter varies with relationship closeness (measured by the friendship index).

Parameter	Explanation	Estimate	95% CI
Directed Altruism			
$\mu(private)$	bottom tercile & private	0.14^{***}	$(0.072, 0.21)$
δ_m	increase medium tercile	0.24^{***}	$(0.060, 0.41)$
δ_h	increase top tercile	0.18^{***}	$(0.062, 0.30)$
$\mu(public) - \mu(private)$	increase public	0.009	$(-0.037, 0.054)$
ρ	social image concerns	0.037^{***}	$(0.016, 0.058)$
α	constant	25^{***}	$(21, 28)$
λ	QRE-parameter	0.25^{***}	$(0.20, 0.30)$

Notes: *p<0.1; **p<0.05; ***p<0.01; tandard errors are clustered on the friendship pair level.

Appendix 1.I Structural Model with Baseline Altruism

I re-estimate the structural model with a Baseline Altruism parameter. In this specification, participant 1's utility is as follows:

$$U_1(p_1, p_2, p_3, p_4, S, D, O, \gamma) \;=\; m_1(.) + \mu_{bl}(O)\sum_{i=2}^{4} m_i(.) + \mu(o) m_{friend},$$

where μ_{bl} is the baseline altruism parameter. This implies that people weigh their friend's payoff with $\mu_{bl}(O) + \mu(O)$.

The baseline altruism parameter is likely difficult to estimate from my experiment. Baseline altruism should push participants' actions closer to the collusive outcome. This shift is very small and unlikely to differ with the social network. The constant (α) in the utility function has similar consequences. Therefore, it is hard to disentangle the two.

I test if changes in baseline altruism can explain the effect of price-transparency. If participants' prices become more observable, they could react by behaving more altruistically towards all other participants. I estimate different baseline altruism parameters for each price-transparency condition (O) and drop the term for social image concerns from the participant's utility. If participants do indeed become more altruistic, their baseline altruism parameter should increase when switching from the private to the public treatment ($\mu(public) - \mu(private) > 0$).

The estimation reflects that the level of baseline altruism is difficult to estimate from the data. Table 1.I.1 reports the parameter estimates for the model with baseline altruism. The confidence interval for $\mu_{bl}(private)$ ranges from −0.99 to 0.19.

Table 1.I.1. Parameter estimates for the QRE-Directed-Altruism model, incorporating baseline altruism.

Parameter	Explanation	Estimate	95% CI
Baseline Altruism			
$\mu_{bl}(private)$	private	-0.40	$(-0.99, 0.19)$
$\mu_{bl}(public) - \mu_{bl}(private)$	increase public	-0.16^{***}	$(-0.26, -0.047)$
Directed Altruism			
$\mu(private)$	private	0.24^{***}	$(0.17, 0.31)$
$\mu(public) - \mu(private)$	increase public	-0.003	$(-0.077, 0.071)$
α	constant	23^{***}	$(19, 27)$
λ	QRE-parameter	0.25^{***}	$(0.18, 0.31)$

Notes: *p<0.1; **p<0.05; ***p<0.01; standard errors are clusterd on the friendship pair level.

The model estimates indicate that an increase in Baseline altruism cannot explain the fall in prices due to increased transparency. Table 1.I.1 reports a significant decrease in baseline altruism in response to increasing price transparency. This suggests that a model that uses baseline altruism to explain the effect of increasing price transparency is misspecified.

The decrease in estimated baseline altruism can be explained by examining the externalities between participants. From the perspective of a specific player, higher prices benefit the two other participants selling substitutes and harm the one participant selling a complement. On average, across all experimental conditions, the first externality outweighs the latter. Therefore, the model estimates a decrease in baseline altruism to rationalize the decrease in prices.

Appendix 1.J Buyer and Seller Payoffs

I calculate buyer and seller payoffs analogously to total welfare. The sellers' payoff is higher for networks with higher prices. The buyer's payoff is lower for networks with higher prices.

Table 1.J.1. Empirical expected profits and expected total surplus.

	Seller	Buyer	Total	Max Total
Complements	17.30	40.00	57.30	76.70
Baseline	19.30	34.30	53.60	76.70
Substitutes	20.50	30.60	51.10	76.70

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Chapter 2

Managing Bidder Learning in Teleshopping Auctions*

Joint with Simon Schulten

Firms have an incentive to offer pricing schemes that exploit consumer biases (DellaVigna and Malmendier, 2006). However, consumers may learn to become less susceptible to exploitative pricing schemes. Consequently, firms have an incentive to shape consumer learning.

While the incentives to shape consumer learning are important we know very little about how firms react to them. The reason is that a profit-maximizing firm may prevent consumers from learning when learning reduces profits. This behavior complicates observing consumer learning in its unaltered state. While we may see a firm's optimal behavior, it is more challenging to know which learning it prevents. Researchers can address this challenge if they find situations where a firm becomes increasingly better at managing consumer learning. In these cases, they can collect data on consumer learning closer to its natural state and on the firm's reaction.

We collected a two-year panel from teleshopping auctions to analyze consumer learning and the firm's actions to shape it. At the beginning of our sample, we observe pervasive bidder mistakes that trigger learning, but only when they hurt the bidder. Next, we analyze a simple dynamic model and derive conditions under which the firm prevents consumer learning. We argue that these conditions are met at the beginning of our sample by causally estimating consumer learning. We then document empirically that the firm becomes better at managing these mistakes.

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The firm operates two retail channels: A televised multi-unit descending auction with uniform pricing and an online shop selling goods at a fixed price. We call a bid that is higher than the fixed price an *overbid*.[1] According to the auction rules, every winning bidder pays the lowest successful bid. When this auction price is higher than the fixed price, we call the auction *overpaid*. Overbidding does not imply overpaying, as overpaying requires that all bids in an auction are overbids (the lowest bid is higher than the fixed price).

Overpaying is a negative experience that may cause bidders to reconsider their actions. We differentiate two ways of learning from overpaying. We refer to the *intensive* margin response if the bidder only bids below the fixed price in the future and to the *extensive* margin response if bidders leave the auction altogether.

Our empirical analysis focuses on quantifying bidder responses. We estimate the causal effect of overpaying on observed future overbids and non-overbids. Since we only observe successful bids, we use a model to connect these treatment effects on observed variables to extensive and intensive margin responses. We find evidence for responses at both margins. We find that overpaying causes 5% of bidders to leave the auction (extensive margin) and 7% stop overpaying (intensive margin).

We use a simplified version of this empirical model to clarify the firm's incentives. The firm trades off present and future profit. If bidders overpay, the firm increases current profits, but the bidders learn. Learning reduces future demand and profits.

Our firm is behavioral and improves at navigating this trade-off.[2] The firm improves at managing bidder learning because it extends the scope of its maximization: Initially, the firm takes fixed prices as given and maximizes only over quantity. Finally, the firm optimizes more broadly, and changes fixed prices and quantities.

If the seller takes fixed prices as given, they can avoid overpaid auctions by increasing the quantity. The model shows that the seller's choice mainly depends on the bidder's extensive margin response: Increasing the quantity is optimal if this response is sufficiently large.

In the second version of the model, the seller simultaneously chooses the quantity and fixed price. In this case, the seller wants to avoid any consumer learning. Therefore, it sets a high fixed price to avoid overpaying, removing the learning stimulus.

We find two policy changes in line with our model of firm incentives. An entirely rational firm exploiting biased bidders would immediately jump to the revenue-maximizing policy (high fixed prices). In our data, we observe an adaptive firm that gradually learns to increase its revenue in the presence of bidder learning. A sudden decrease in the empirical probability of overpaying indicates the firm's response to

1. For a description of overbidding in auctions run by the same firm see Ocker (2018).

2. For surveys of this literature see Armstrong and Huck (2010), Goldfarb et al. (2012), and Heidhues and Kőszegi (2018).

bidder learning. The firm likely increased supply to avoid overpaid auctions.[3] A novel pricing strategy completely circumvents overpaying: auctions always start below the fixed price outside option. This change prevents overpaying by design.

To our knowledge, we are the first to investigate firms shaping consumer learning that alleviates consumers' behavioral biases. Learning needs feedback. Without feedback, biases persist. Our results suggest that firms may strategically withhold this feedback. So, we should expect biases to survive if they benefit the firm and feedback is easy to withhold.

Considering a dynamic setting complements the existing literature: In static settings, sellers maximize their profit by designing auctions that maximally exploit consumers, e.g., by letting them impact the price (Malmendier and Lee, 2011; Malmendier and Szeidl, 2020). Adding future periods and repeat purchases adds downsides to exploitation.[4] If consumers learn from being exploited, they change their future behavior, e.g., by leaving the market. The firm then loses the leaver's customer lifetime value. The firm trades off revenue maximization in an individual auction and customer retention across auctions. The learning opportunities that the firm allows the customers alleviate this trade-off. In our data, the firm can remove the learning stimulus altogether by changing the fixed prices and resolving the trade-off.

Market designers lack a model of customer retention in platform markets. We provide such a model for our context. We demonstrate the need to disentangle platform exit (extensive margin) from strategic learning on the platform (intensive margin) and provide an empirical method for that. The closest paper to ours in that regard is Backus et al. (2021), which investigates platform exit.

Our analysis demonstrates a novel way to combine a traditional economic model, a directed acyclic graph (DAG) (Pearl (2009), Imbens (2020) and Hünermund and Bareinboim (2019)), and the sufficient statistics approach (Chetty, Looney, and Kroft, 2009). We start with an economic model of firm optimization and bidder learning. Then, we expand this model with non-parametric equations that model specific channels of confounding. Finally, we represent this non-parametric model as a DAG to show non-parametric identification of treatment effects. As in the sufficient statistics approach, we estimate the treatment effects by OLS and use structural assumptions in a part of the model to recover two structural parameters: the probabilities of intensive and extensive margin learning. With the DAG, we can use the same model to estimate reduced form effects and to recover structural parameters from these effects. We keep the strength of the sufficient statistics approaches because we only need structural assumptions on parts of our model.

The paper proceeds as follows. In Section 2.1 we discuss the rules of the multi-unit descending auctions and further institutional details. In Section 2.2 we report

3. The firm's current management agrees that this explanation is plausible. However, we cannot strictly rule out other causes since the firm lost knowledge because they changed their management.

4. There is a lack of dynamic models in behavioral IO (Heidhues and Kőszegi, 2018)

on data collection. In section 2.3 we describe our data including the empirical evidence on firm behavior. The model of firm incentives is laid out in Section 2.4. Section 2.5 discusses our empirical strategy. We report estimates of the bidders' learning response in section 2.6. Section 2.7 concludes.

2.1 The Multi-Unit Auctions

The seller uses a multi-unit descending price auction embedded in a German shopping television show. Each show lasts one hour and consists of several auctions for similar products such as home textiles, men's watches, or jewelry. The seller broadcasts auction shows 20 hours a day, by TV (bids submitted by phone) or online (websites, several apps). The seller only runs one auction simultaneously. On average, a single auction on average takes approximately 11 minutes.

Bidders can also purchase every product up for auction through an online shop at a fixed price. The online shop is available on the same website that also hosts the live stream of the auction shows.

The auction rules ensure that only people who bid above the fixed price (overbid) also pay above the fixed price (overpay). At the beginning of each auction, the auctioneer announces the number of items and the auction's starting bid. This starting bid is then gradually lowered over time in discrete increments. Bidders can enter the auction at the current bid and claim one or more units of the good. The auction ends when all units are claimed. All bidders pay the lowest successful bid (uniform pricing rule). Because of this uniform pricing rule, an auction is only overpaid if all bids are overbids.

Bidders that bid by phone have to pay a flat fee of one Euro. Since research on shipping costs suggests that this fee is likely (at least partially) ignored we do not include this fee when we create our overbidding variable (Hossain and Morgan, 2006). Shipping costs apply to the fixed price and the auction in the same way. For that reason, we can also ignore shipping costs in our discussions of overbidding and overpaying.

2.2 Data

We scraped data on bids and products from the seller's website from October 20, 2016, to January 3, 2019. Since after some time, data is removed from the website, we ran the scraping script in hourly intervals.[5]

First, we access the schedule in the TV programming section of the website. This schedule gives us information on the show level, such as time and date, product cat-

5. Due to a small coding error we did not collect auction shows at 6, 10 and 11 pm. Other than that we observe all shows and within shows all auctions and bids that took place.

egory, and the auctioneer running the show. Second, we collect auction-level data by going through the list of all planned auctions. This list contains an auction ID that we use to scrape bids and bidder nicknames from a separate part of the website. Third, we collect product information from the online shopping section of the website. Most importantly, we collect the fixed price of each product at the time of the auction.[6]

This data collection yields a bidder level panel of 8.48 million bids in more than 69000 auctions spanning over 2 years and 2 months. We use this raw data to calculate several variables, including the auction price (the minimum of the bids), bidder history variables that capture typical behavior and past experience on the auction platform, and dummies indicating whether a bid is an overbid or overpaid.

2.3 Descriptives

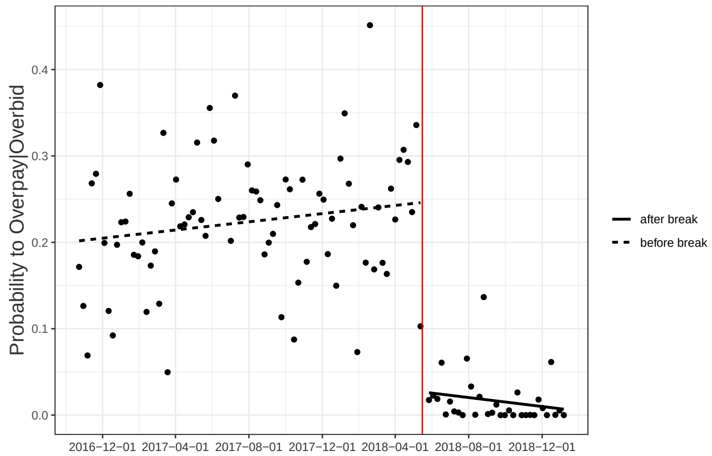
At the beginning of our observation period, a substantial fraction of overbids are likely to be overpaid. That is, initially, overbidders are likely to get a stimulus that (as we hypothesize) triggers bidder learning. Subsequently, we observe a structural break after which the probabilities of overbidding, overpaying and the conditional probability of overpaying given one has overbid decline sharply. This break is associated with a sudden, but small increase in the total number of products sold in each weak. We determine the date of this structural break with a QLR test (Kleiber and Zeileis, 2008).[7]

To illustrate the structural break, we average the probability of overpaying conditional on an overbid for each week in our data and plot it against time (Figure 2.1, panel a)). We fit a linear trend to each side of the break on the aggregated data. The probability of overpaying conditional on an overbid declines from around 20% to practically 0%, decreasing the probability of bidder learning because overpaying becomes less likely. This decrease in overpaid auctions coincides with a discrete increase in the number of products sold in each weak (see panel b). The number of products primarily increased because the seller conducted more auctions (see Figure 2.G.2 in Appendix 2.G).

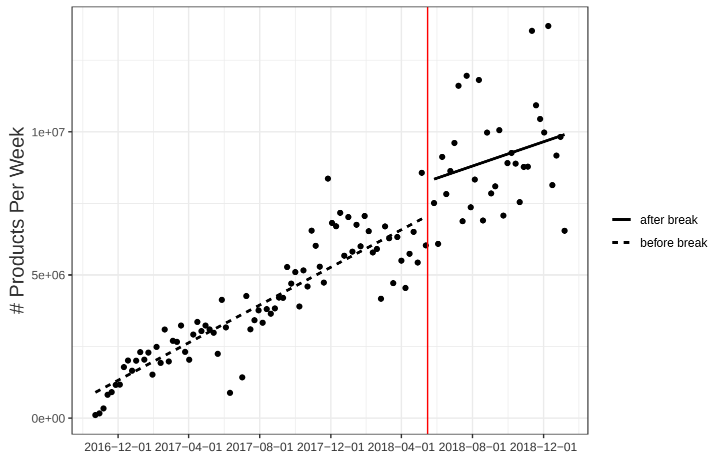
Since the structural break is a defining feature of our data, we report our summary statistics split at that break in Table 2.1. Before the break, there is a substantial amount of overbidding (17%). While the overall probability of overpaying is small at 4%, an overbid is punished by an overpaid with a higher conditional probability of 23%. After the break, the probability of overbidding collapses to just below 10%,

6. We also collected product ratings, but those are quite sparse at this retailer, so we do not use them.

7. The most probable breakpoint is the day with the highest individual test statistic, in our case, the 16th of May 2018. We plot the time series of test statistics in Figure 2.G.1 in Appendix 2.G.



(a) Weekly averages of overpaying probabilities conditional on overbidding.



(b) Number of Products sold each week.

Figure 2.1. Probability being punished for overpaying (P(overpaid|overbid)) and number of products per week to both sides of the structural break. We use weekly aggregates and fit a linear trend at both sides of the structural break. We ommit the week that overlaps with the break.

which lowers the probability of overpaying to essentially 0%. Together, these statistics indicate that before the break consumer learning is a lot more likely than after the break.

Overpaying may come at the cost of losing customers. Hence, a high customer lifetime value increases the seller's costs from an overpaid auction. The seller has an unusually high customer lifetime value compared to other e-commerce companies. According to the 2012-2014 investor presentations[8], approx. 90% of customers

8. https://www.1-2-3.tv/uploads/files/2013_06_123tv%20Company%20Profile.pdf, https://www.1-2-3.tv/uploads/files/2012_10_%20123tv%20Das%20Unternehmen.pdf, https://www.1-2-3.tv/uploads/files/PM_123tv_2014_07_01.pdf accessed 12.01.2022

Table 2.1. Estimated probabilities of overpaying and overbidding.

	before break (N = 4573854)	after break (N = 1960103)
overbid		
probability	0.17	0.093
average amount	3.6	4.4
median ammount	1.1	1.1
overpaid		
probability	0.039	0.0014
average amount	2.5	2.9
median amount	1.1	0.6
overpaid | overbid		
probability	0.23	0.015
auctions		
average duration (minutes)	11.25	11.9

watched the program several times a week, and 45% of them followed the shopping offers daily for more than an hour. Two-thirds of customers are return customers.

We estimate the customer lifetime value from our data by looking at a subset of bidders for which we can be sure that we follow them through a large part of their customer journey. Our sample runs from October 2016 to January 2019. We select bidders for which we observe the first bid between 1st of March 2017 and the 31st of December 2017. Thus their first observed bid likely coincides with their first actual bid, and we follow them at least a year after their first bid. These bidders spend on average 322 euros during our observation period. However, the distribution is positively skewed, with a median of 58 euros.

While the customer lifetime value influences the seller's cost from overpaid auction, the seller's gain from an overpaid auction is the difference between the auction price and the fixed price (the amount overpaid). The average amount overpaid for auctions that end above the fixed price is 2.50 euros. The seller wants to prevent overpaying if their costs from the demand response to overpaying are higher than the amount overpaid. In the following section, we use a model to study the kinds of consumer learning that incentivize the firm to decrease the probability of an overpaid auction.

2.4 Firm Incentives

The seller wants to prevent overpaying if the costs from consumer learning (less overbidding in the future, less bids in the future) exceed the benefits from overpaying (higher auction price today). In this section, we model these considerations and show that the firm's main incentive to prevent overpaid auctions comes from extensive margin learning. Further, we discuss the firm's instruments to prevent overpaid auctions and argue that the firm uses progressively better instruments. Our discussion of the firms strategic considerations is based on our conversations with the firms management and information provided on their homepage.

Customers. Our model assumes two crucial things about bidders: they respond to overpaying, and the firm knows about it. We model two kinds of learning responses: avoiding bidding in the future (extensive margin) and avoiding over-bidding in the future (intensive margin). The literature on consumer learning in the field supports these assumptions. The firm and the authors of this paper know that bidders react to overpaying because the bidders complained about it. [9]

Others that study learning also find an extensive margin response: bidders leave the market instead of adjusting their behavior. A transaction that leaves the bidders worse off than the reference point (overpaying) results in a negative transaction utility (Thaler, 1999) and reduces future market participation through several channels: antagonizing consumes (Anderson and Simester, 2010), updated beliefs about the utility from market participation (Backus et al., 2021) and updated beliefs about their abilities (Seru, Shumway, and Stoffman, 2010). In our context, the latter means that bidders could realize that they are irredeemably bad at bidding, so they should stop bidding altogether.

In our model *intensive margin adaption* means that bidders that experience the cost of an action avoid this action in the future. Laboratory research on auctions and research in field settings (other than auctions) find evidence for this behavior. In laboratory experiments, bidders adjust their bid in the direction that would have been better in the past (Neugebauer and Selten, 2006). People who pay a fee started to avoid the activity that led to the fee (Agarwal et al., 2013; Ater and Landsman, 2013; Haselhuhn et al., 2018). While losing a customer costs the firm a lot, they gain very little from an overpaid auction (compared to auctions that end at the fixed price). As we show in section 2.3 bidders are very likely to be repeat customers and have a very high customer lifetime value: on average, a customer spends more than 322 euros during their lifetime. However, the average overpaid auction only ends 2.50 euros above the fixed price.

Running an Auction. To motivate the seller's instruments in our simplified model, we briefly describe how they run the auctions behind the scenes. The auctions are jointly run by a presenter (on-screen) and a director (off-screen). The presenter is in charge of conducting the auction; the director directs the presenter and can expand

9. We know this because we talked with the firm's marketing manager. This mode of firm decision-making is described in Masterov, Mayer, and Tadelis (2015). Gesche (2019) also reports customers complaining, albeit through the seller ratings on eBay, rather than the hot-line.

the number of products on the fly. They base this decision on the number of people currently watching and revenue per minute.

The firm changed its policy twice: at the structural break in our data and after the management change (not covered in our data). We interpret this as the firm discovering new ways to cope with consumer learning and optimizing more broadly. We analyze the structural break as a jump from a sub-optimal choice to an optimal one with exogenous fixed prices. With exogenous fixed prices, the firm tries to reduce overbidding by targeted increases in quantity. We explain the subsequent change in auction rules by tweaking our model: The firm discovers fixed prices as an additional instrument. Before we turn our attention to these two models, we explain how the firms' instruments (quantities and fixed prices) work in practice.

Instrument 1: Quantity. An increase in quantity as we observe it in Figure 2.1 leads to a downward move along the demand curve in each auction, which lowers prices. This could simply be done by offering extra units in auctions that happen to have high demand. Management tells us that this is routinely done while the auction is already proceeding.[10].

The seller can also use quantity targeting to avoid overpaying without raising the overall quantity. For example, suppose demand varies slightly over time. Then, some auctions might end above the fixed price, and some will not. In this case, the seller can shift products from non-overpaid auctions to overpaid auctions to reduce overpaying. This strategy works very well when supply is limited by the seller's inventory (in the short run). Unfortunately, this strategy is tough to detect.

One might argue that we observe an increase in the number of auctions, not in the number of products per auction. However, increasing the number of auctions can still reduce overpaying. For example, suppose before the structural break, the seller offers one kind of canned stew, and after the break, they offer two kinds of stew and run the auctions right after each other. In the second case, we would expect lower demand, lower prices, and a lower probability of overpaying for each of them since different kinds of stew are close substitutes.

Instrument 2: Changes in Fixed Price. Since the seller makes most revenue in the auctions, the fixed-price outside option primarily acts as a reference price. Consequently, the seller can use adjustments of this reference price as a second instrument to avoid overpaying.

After our sample ended, the seller changed the rules to use that instrument. Initially, the fixed price is high, and the auction starts below. After an auction ends, the fixed price falls to just above the auction price for 24 hours. This strategy gives potential customers who missed the auction the chance to purchase the good at a

10. While we observe the number of units sold in each auction, we do not observe the number of units that were originally planned for the auction. Unfortunately, this means we do not know which auction increased supply dynamically during the auction.

price below the recommended retail price. For example, an item may be offered at its recommended retail price of 30 euros, while the auction starts at 20 euros. The auction price may realize at 12 euros, and the fixed price falls to 15 euros for 24 hours. Since we did not collect data after the policy change, we cannot check if the fixed prices rose on average. However, the new policy, by construction, makes overpaying impossible.

2.4.1 Model of Firm Incentives: Exogenous Fixed Price

To model the relationship between bidder adaption and seller incentives, we model the seller in the simplest way possible. We assume the seller has constant marginal costs of 0. They supply the product perfectly in-elastically at a fixed price of p. We assume that in each period $t \in \{1, ..., \infty\}$ the firm runs an auction for the same product. Finally, we assume that the firm knows the buyers' preferences and chooses the profit-maximizing quantity (q_t) to supply in the auction.

We model bidder adaption with two types of bidders, overbidders, and sophisticates. Denote the number of sophisticates at time t by s_t and the number of overbidders by o_t.

We assume that bidders have a homogeneous latent bid $\beta > p$ and bid for one good in each auction. Overbidders bid their latent bid in every auction, whereas sophisticates only bid their latent bid if the auction price is below the fixed price; otherwise, they buy at the fixed price.

This bidding model rules out strategic reactions to other bidder's behavior. The main consequence of this assumption is that latent bids could be endogenous to the other bidders' number and types. We explore the robustness of our results to weakening this (and other) assumptions after stating our first proposition.

We assume that overbidders never buy at the fixed price even if the auction does not offer a sufficient quantity. While this assumption sounds restrictive, it is not, since offering fewer products than the number of overbidders is sub-optimal.

We use these assumptions to express the auction price (p_a) as a function of the auction quantity (q_t) offered by the seller and the number of overbidders and sophisticates (o_t, s_t).

$$p_a(q_t, o_t, s_t) = \begin{cases} \beta & q_t \leq o_t \\ p & o_t < q_t \leq o_t + s_t \\ 0 & o_t + s_t < q_t \end{cases}$$

overbidders always bid in the auction if the auction price is larger than β. Sophisticates only bid in the auction if it ends below the fixed price and otherwise buy in the store. As a result the auction price is $\beta > p$ as long as there are fewer products than overbidders. As soon as there are more products than overbidders the sophisticates enter the auction and the auction price drops to p. If there are more products than buyers the auction-price drops to zero.

We model extensive margin learning as bidders changing from being an overbidder to leaving the auction and never bidding again. If an auction ends above the fixed price a fraction $\epsilon \in [0,1]$ of overbidders learn at the extensive margin and leave the auction. We model intensive margin learning as changing from being an overbidder to being a sophisticate. If an auction ends above the fixed price a fraction $\iota \in [0,1]$ of overbidders becomes sophisticates. We further assume that $\epsilon + \iota \leq 1$.

Having discussed price formation and the laws of motion, we can write down the seller's maximization problem. We assume the seller discounts their profits at a rate $\delta \in (0,1)$ and the initial stock of overbidders and sophisticates are positive ($o_0 > 0 \wedge s_0 > 0$)

Definition 2.1. Seller's Problem
A profit-maximizing firm solves the following problem:

$$\max_{\{q_t\}_{t=0}^{\infty}} \delta^t \pi(q_t, o_t, s_t),$$

where

$$\pi(q_t, o_t, s_t) = \begin{cases} \beta \cdot q_t + p \cdot s_t & \textit{if } q_t \leq o_t \\ p(o_t + s_t) & \textit{if } o_t < q_t \leq o_t + s_t \\ 0 & \textit{if } o_t + s_t < q_t \end{cases}$$

subject to:

$$o_{t+1} = \begin{cases} o_t - (\epsilon + \iota) q_t & \textit{if } q_t \leq o_t \\ o_t & \textit{if } q_t > o_t \end{cases}$$

$$s_{t+1} = \begin{cases} s_t + \iota q_t & \textit{if } q_t \leq o_t \\ s_t & \textit{if } q_t > o_t \end{cases}.$$

The seller wants to prevent overpaying auctions if the gains from preventing overpaid auctions (less bidder learning) exceed the costs (lower period profits). We solve the seller's problem by guessing and verifying the policy function.

We guess two simple policy functions, (1) the seller always sets a quantity of $q_t \in (o_t, o_t + s_t]$ (2) the seller sets a quantity of $q_t = o_t$. The first case leads to an auction price of p and no overpaying, while the latter leads to an auction price of β and overpaying. In both cases, the seller sells $o_t + s_t$ units because sophisticates that do not buy in the auction buy in the store.

To verify our guesses, we check if the increase in period profits from an overpaid auction is higher or lower than the decrease in future profits from an overpaid auction. Our conjectured policy functions characterize the optimal choice for all parameter combinations. The conditions for the optimal policy do depend on extensive margin learning (ϵ) but not on intensive margin learning (ι). We report these conditions in Proposition 2 whose proof we give in Appendix 2.A.

Proposition 2. If $\frac{\beta-p}{p} \leq \frac{\delta}{1-\delta} \cdot \epsilon$ the profit maximizing quantity is any $q_t \in (o_t, o_t + s_t]$, $\forall t$. If $\frac{\beta-p}{p} \geq \frac{\delta}{1-\delta} \cdot \epsilon$ the profit maximizing quantity is $q_t = o_t$, $\forall t \in \{1, ..., \infty\}$.

According to the condition in Proposition 2, the seller wants all auctions to end in an overpay ($q_t = o_t$) if the percentage-increase in the price paid by an individual bidder exceeds the discount-factor adjusted extensive margin learning parameter e. They want all auctions to be non-overpaid ($q_t \in (o_t, o_t + s_t]$) if this is not the case. That is, overpaid auctions are more likely if the seller is less patient or extensive margin adaption is higher.

The firm's optimal choice does not depend on intensive margin adaption. The only constraint posed by intensive margin adaption is that the seller can't charge sophisticates an auction price above the fixed price. This constraint can't be an effective deterrent against charging someone above the fixed price.

We illustrate this lack of deterrence from intensive margin adaption with an example. Imagine two bidders: One sophisticated bidder and one overbidder who transforms into a sophisticate when overpaying ($i = 1$). The firm can now supply either one or two units in the auction: supplying one unit results in an overpaid auction, and supplying two units results in a non-overpaid auction.

If the firm offers one unit in period zero, they get period profits of $\beta + p$ followed by profits of $2p$ for all subsequent periods. If the firm supplies one unit, the overbidder buys in the auction for β, and the sophisticated bidder buys at the fixed price in the shop. The resulting profits are $p + \beta$. After this period, both bidders will be sophisticated. Sophisticates bid at most p, independent of the number of products in the auction. Thus after this period, the firm wants to sell at the fixed price and earns profits of $2p$ each period.

Offering two units in period zero and one unit in some later period only postpones the period with high revenue. Suppose we offer two units until period k and switch to one unit afterward. Both bidders buy in the auctions until the end of period k. Consequently, all the auctions end at the fixed price and generate a revenue of $2p$, each. Both bidders keep their type. Now the firm offers only one unit. As in the previous paragraph, this results in profits of $p + \beta$, this time in period $k + 1$. Then bidders adapt, and the firm is back to earning $2p$ each auction.

The asymmetry between extensive and intensive margin learning depends on the assumption of exogenous latent bids. Suppose we weaken this assumption. In this case, bidder learning becomes more effective in deterring overpaid auctions, and intensive margin learning becomes a deterrent too.

If bidders were more strategic, they would adjust their latent bid to the number and types of the other bidders. They would bid less when they faced fewer other bidders. This strategic reaction strengthens the effect of extensive margin learning.

Further, bidders would bid less when interacting with a higher fraction of sophisticates (ceteris paribus) because sophisticates bid less aggressively than overbidders. This strategic reaction creates a deterrent effect for intensive margin learning as it decreases the latent bids and, thus, the revenue.

We assume unit demand and do not model product returns. Altering these assumptions is unlikely to change the main results of our model. In our model each bidder only buys one unit of the good. In reality, each bidder is able to buy several units. If we assume that bidders have an elastic demand curve, we need to impose the assumption that the incentive to avoid overbidding is monotonous in the number of overbidders and sophisticates to preserve the cut-off strategies from theorem 2. As obligated by law, bidders are able to return their purchases for a limited time after the auction. We learned in our conversation with the seller that returns are higher in overpaid auctions. This further incentivizes the seller to avoid overpaid auctions.

2.4.2 Model of Firm Incentives: Endogenous Fixed Price

As we have seen in Proposition 2, the fixed price constrains the firm in its ability to extract revenue from overbidders. The firm's new policy removes this constraint by starting auctions below the fixed price. We can model this new policy by allowing the seller to set the fixed price (p).

If the fixed price p is weakly larger than the latent bid, β bidders always buy in the auction or are indifferent between the store and the auction. In this case, the auction price equals β if there are at least as many products as customers. Consequently, there is no customer adaption to overpaying. If the fixed price is smaller than the latent bid, we are back in the previous case.

Since setting the fixed price above the latent bid effectively removes bidder adaption as a constraint, the seller always wants to do it and it increases their revenue. We formalize this intuition in Proposition 3.

Proposition 3. If sellers can choose p, they maximize their profits by setting $p > \beta$ and $q_t \in (o_t, o_t + s_t],\ \forall t$.

Proof. If $p > \beta$ no auction ends in an overpay, the firm always gets the maximum number of customers. If $q_t \in (o_t, o_t + s_t],\ \forall t$ the firm sells each customer as much as it can at the maximum price. Profits are maximal since the firm sells the maximum quantity to the maximum number of customers at the maximum price. □

Theorem 3 shows that instead of addressing the cause of bidder adaption (high auction prices) the firm can also remove the stimulus (overpaid auctions) by adjusting the store price. This policy increases prices without changing quantity (in the case of constant marginal costs below the previously exogenous fixed price and unit demand). Consequently, surplus is redistributed to the firm.

In our model the firm can costlessly increase the fixed price. In reality, there are two countervailing forces: the credibility of the fixed price and the existence of buyers who prefer to buy at that fixed price instead of in the auction. One channel for bidder adaptions is that bidders see the fixed price as a reference price. Buying at a higher price than the reference price results in negative transaction utility (a negative value of the deal) (Thaler, 1999) which triggers bidder adaption. While implausibly high reference prices still have some affect, increasing them above a certain level becomes counterproductive (Compeau, Grewal, and Chandrashekaran, 2002).

2.5 Empirical Strategy

We use a model to clarify the interpretation of our treatment effects and represent this model as a directed acyclic graph (DAG) to show that they are identified. With this approach, we can automatically derive the conditional independence assumption and the required controls from our model primitives (using the back-door criterion) instead of relying only on verbal arguments. Furthermore, we estimate treatment effects instead of the deeper underlying structural parameters (ϵ and ι). This approach is advantageous because our treatment effect estimates are valid under weaker assumptions than a fully structural estimate. The drawback is that we cannot directly obtain parameter estimates for ϵ and ι. Nevertheless, we can identify these parameters from our treatment effect estimates with structural assumptions on parts of our model.

2.5.1 Empirical Model

We use a general version of our model from Section 2.4. This model includes bid heterogeneity and firm behavior as a function of exogenous shocks. We model bidder learning parametrically and avoid parametric assumptions on firm behavior and the latent-bid distribution.

We introduce new notation to describe bidder behavior. We focus on a specific bidder i that has their first overbid at time $t \in \{1, ..., \infty\}$. We observe this bidder from starting at their first overbid until the end of our sample. Since this time differs between bidders we aggregate bidder's outcomes over a standardized time-period. We exclude bidders that we do not observe long enough from our analysis. We assume that the number of participants in the market is sufficiently large that bidder i faces new bidders in each of their auctions.[11] Consequently the behavior of all other bidders is uncorrelated across auctions. We collect all other bidders in the set $J_t = \{1, ..., N_t\}$.

11. This assumption ensures that we can treat different auctions as independent observations. According to this assumption, the treatment assignment of another bidder cannot influence a specific bidder's future outcomes because they never meet these other bidders again. Thus this assumption implies the stable unit treatment value assumption.

Our model includes exogenous shocks to model empirically relevant sources of heterogeneity. We assume that each period auction-specific characteristics A_t, the fixed price shock $\tilde{p}_t$, the auction quantity shock $\tilde{q}_t$ and an individual specific time-varying shock $v_{i,t}$ realize as independent draws from a continuous distribution. All shocks are independent from each other and across time. We model individual specific unobserved heterogeneity with the time-constant variable u_i, which in our model realizes before any choices are made and is independent across individuals.

We again separate bidding into latent (unmodeled) bids and a bidder type (naive, sophisticate, leave) that determines the submission of these bids. We let the individual latent bid depend on auction-specific characteristics A_t, the individual specific time-varying shock $v_{i,t}$ and the time-constant unobserved heterogeneity u_i. These shocks are i.i.d. from a continuous distribution. We denote the latent bid of the individual in question by $\beta_{it} = \beta(A_t, u_i, v_{i,t})$ and the set of latent bids by all other bidders by $\beta_{-i,t} = \{\beta(A_t, u_j, v_{j,t})\ \forall j \in J_t\}$.

The dependence of β_{it} on auction characteristics models that different individuals might be interested in different auctions. A special case of this is that most individuals do not participate in an auction. In this case their latent bid is 0. The dependence on u_i models that different bidders might differ in the amount they usually bid. The time-varying individual specific shock v_{it} models the main source of heterogeneity for a specific bidder across auctions.

According to the model in Section 2.4 the firm targets its quantities and fixed prices to latent bidder demand. While our empirical analysis focuses on the period before the structural break when the firm likely does not behave optimally, we still allow for the fixed price p_t and the auction quantity q_t to depend on latent bids β_{it} and β_{-it}. Since we do not specify the parametric form of this dependence we allow for optimal as well as non-optimal firm behavior in our empirical analysis. Further the firm might tailor fixed-prices to auction characteristics, e.g. the type of products on sale. We model this by letting the fixed price and the auction quantity depend on auction-characteristics A_t. Thus, auction quantity (q_t) and fixed-price (p_t) may depend on these quantities as well as their specific exogenous shock ($\tilde{q}_t$ and $\tilde{p}_{st}$).

$$
\begin{aligned}
p_t &= c(\tilde{p}_t, A_t, \beta_{it}, \beta_{-it}) \\
q_t &= d(\tilde{q}_t, A_t, \beta_{it}, \beta_{-it})
\end{aligned}
$$

As argued in Section 2.4 a bidders bid depends on their type $\theta_{i,t}$ and their latent bid β_{it}. The bidder's type at time t is $\theta_{i,t} \in \{o, s, l\}$, where we denote overbidders by o, sophisticate by s, and someone who left the platform by l. Since we look at bidders after their first overbid, we only select overbidders. Overbidders always bid their latent bid β_{it}, while sophisticates wait until the price drops below the fixed price, that is they bid $min(\beta_{it}, p_t)$. Bidders who left always bid zero. We summarize this behavior in the following bid function,

$$b_{i,t} = f(p_t, \beta_{i,t}, \theta_{i,t}) = \begin{cases} \beta_{i,t} & \text{if } \theta_{i,t} = o \\ min(\beta_{i,t}, p_t) & \text{if } \theta_{i,t} = s \\ 0 & \text{if } \theta_{i,t} = l \end{cases} .$$

In our empirical model we allow for heterogeneity in learning responses. Overpaying turns overbidders into sophisticates with probability ι_i (intensive margin learning), and makes them leave the platform with probability ϵ_i (extensive margin learning). We allow for dependence between these treatment effect parameters and bidder specific shocks u_i.

We express the auction's outcome from the perspective of bidder i in terms of two order statics of all other bids (the set $\beta_{-i,t}$): the q_t-highest and the $q_t - 1$ highest bid, which we denote by $b(q_t)$ and $b(q_t - 1)$ respectively. The q_t highest bid determines if bidder i wins, and the $q_t - 1$ highest bid influences the auction price. The auction ends when all products are sold, and the lowest successful bid determines the price. Bidder i looses the auction if all q_t units are sold to bidders in J_t. That is if $b_{i,t} < b(q_t)$. Conversely, bidder i's bid is successful if $b_{i,t} > b(q_t)$[12]. In this case, there are $q_t - 1$ units that remain for the other bidder included in J_t. The lowest successful bid is then either by the bidder in question or the lowest successful bid by the other bidders ($b(q_t - 1)$). If bidder i places a winning bid, the auction price is $min(b_{i,t}, b(q_t - 1))$.

While we use our parametric assumptions on bidding behavior to interpret our treatment-effects, we do not need parametric assumptions to estimate these effects. For this purpose, we summarise our model as a system of non-parametric structural equations. Each structural equation expresses a left-hand side variable in terms of other variables and exogenous shocks. This model is non-parametric because we do not use any functional form assumptions on the right-hand side.

$$\begin{aligned}
p_t &= c(\tilde{p}_t, A_t, \beta_{it}, \beta_{-it}) && (2.1)\\
q_t &= d(\tilde{q}_t, A_t, \beta_{it}, \beta_{-it}) && (2.2)\\
\beta_{i,t} &= \beta(A_t, u_i, v_{i,t}) && (2.3)\\
b(q_t) &= f(q_t, p_t, A_t, \beta_{-i,t}, \theta_{-i,t}) && (2.4)\\
overbid_{i,t} &= g(\beta_{i,t}, b(q_t), b(q_t - 1), p_t) && (2.5)\\
non - overbid_{i,t} &= v(\beta_{i,t}, b(q_t), b(q_t - 1), p_t, \theta_{i,t}) && (2.6)\\
overpaid_{i,t} &= v(overbid_{i,t}, b(q_t), p_t) && (2.7)
\end{aligned}$$

12. Since we use continuous distributions ties happen with probability zero.

We briefly go through each equation and relate it to the previous discussion. We explicitly introduced equation 2.1 to 2.3 in the preceding section. Equation 2.1 and 2.2 describe the information set of the firm when setting auction parameters. One model that fits these equations is the simplified model from section 2.4. However, since they do not assume any structure, these equations nest all firm policies (optimal or not) that condition fixed prices and quantities on a signal of latent demand and auction-specific characteristics. Equation 2.4 summarizes the order statistics of the other bidder's bids and expresses that these statistics may depend on all determinants of these bids. Equations 2.5 to 2.7 apply the auction's rule to this section's expression of bidder's successful bids ($b_{i,t}$). We will introduce parametric versions of equations 5 to 7 in the next section.

2.5.2 Interpretation of Treatment Effects

A first starting point to test for extensive margin adaption is to estimate the treatment effect of overpaying on observed bids that are lower and not equal to the fixed price (strictly lower), the treatment effect on strict non-overbids. Bidders that leave the platform altogether (extensive margin) do not bid anymore, decreasing the number of overbids and non-overbids. Whereas bidders that become sophisticates only reduce their overbids. These bidders bunch at the fixed price, and we eliminate them by focusing on strict non-overbids. Consequently, a negative treatment effect indicates an extensive margin response. Intensive margin adaption leads to sophisticates pooling at the fixed price.

Figure 2.2 depicts the bid function as well as the distributions of submitted and latent bids. For the line chart in the center, the horizontal axis denotes latent bids, and the vertical axis indicates submitted bids. The whole figure uses blue for overbidders and red for sophisticates.

Below each axis, we depict the corresponding marginal distributions.[13] You can think of these marginal distributions as two density plots glued to the side of the plot in the center. The x-axis of the density plot for the submitted bids is the y-axis of the central line chart, which also displays the submitted bids. The y-axis of the density plot for the latent bids is the x-axis of the central line chart, which indicates the latent bids. To read Figure 2.2, start with the distribution of latent bids below the horizontal axis, then imagine this distribution is projected through the bid function (of each type) onto the vertical axis. For example, for sophisticates bids above the fixed price, indicated by the kink in the red bid functions are projected onto the point mass at the fixed price, marked by the red dot.

Figure 2.2 illustrates what we can learn about latent changes in type from changes in the observed bid distribution. Bids strictly below the fixed price (strict

13. In this example, latent bids are uniformly distributed, but the argument does not depend on this

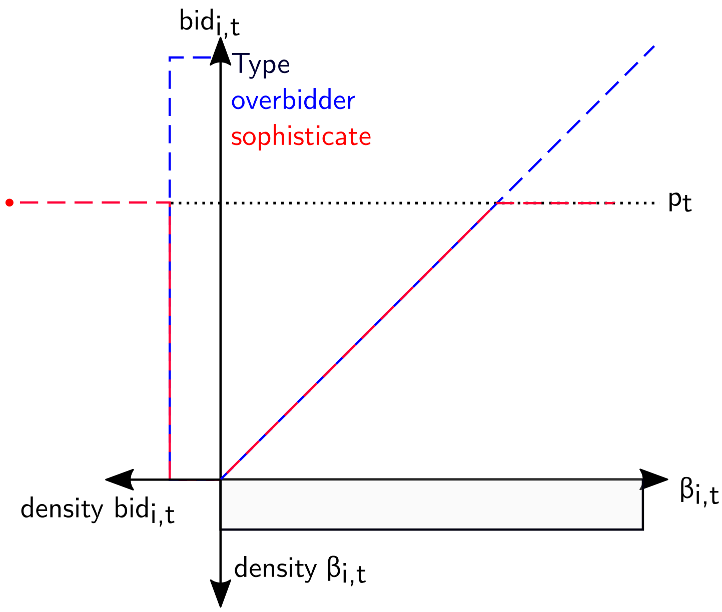


Figure 2.2. Bids as a function of latent bids. Marginal distribution of bids and latent bids for uniformly distributed latent bids.

non-overbids) were submitted by overbidders and sophisticates and bids strictly above the fixed price (strict overbids) were only submitted by overbidders. Latent bids below the fixed price (p_t) directly translate into observed bids. Latent bids above the fixed price bunch at the fixed price for sophisticates and directly translate into observed bids for overbidders. Bids at the fixed-price are composed of bunched overbids by sophisticates and latent bids at the fixed price by both sophisticates and overbidders.

Since strict overbids are only submitted by overbidders a decrease in these bids indicates a reduction in overbidders. Intensive as well as extensive margin learning can cause such a decrease. Since strict non-overbids are submitted by overbidders as well as sophisticates a decrease in these bids indicates extensive margin learning (overbidders leaving the auction). Bids directly at the fixed price increase when there are more sophisticates and decrease when there are more overbidders. Consequently these bids increase with intensive margin learning and decrease with extensive margin learning. We focus on strict overbids and strict non-overbids to avoid this issue.

The only way to observe a strict non-overbid is a latent strict non-overbid ($\beta_{i,t} < p_t$), which is successfully ($\beta_{i,t} > b(q_t)$) submitted by an overbidder or a sophisticate. Thus the treatment effect of overpaying on strict non-overbids in the next period is the expected extensive margin learning parameter scaled by the probability of a successful strict non-overbid. We calculate this effect conditional on u_i. This conditioning renders ϵ_i and the latent bid independent.

$$E[TE^{t+1}_{non-overbid}|u_i] = -E[\epsilon_i|u_i]P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1}))|u_i) \tag{2.8}$$

We can also investigate the interpretation of this treatment effect if we weaken our model assumption. Suppose sophisticates do not only learn to avoid overbids but shift their whole latent bid distribution. In this case there is an additional change in non-overbids stemming from this change in the latent bid distribution.

Proposition 4. If sophisticates shift their distribution of latent bids compared to overbidders, the treatment effect of overpaying on non-overbids in the next period is given by,

$$E[TE^{t+1}_{non\text{-}overbid}] = -E[\epsilon_i|u_i]P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i)$$
$$+E[\iota_i|u_i](P'(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i) - P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i)),$$

where P' is a probability calculated from the latent bid distribution of sophisticates and P is a probability calculated from the latent bid distribution of overpayers.

The proof of this result is in appendix 2.B. Assuming an additional shift in the latent bid distribution due to overpaying has an ambiguous influence on the treatment effect on non-overbids. On one hand, there could be a strong shift, where a lower number of latent non-overbids are successful and hence $(P'(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i) - P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i)) < 0$. This would make sophisticates look like an intermediate step to leaving the market. Thus we would overestimate extensive margin learning using our approach. However, the subtle distinction of losing bids due to the shift in the latent bid distribution instead of losing bids because of a slightly higher extensive margin response is likely irrelevant for the seller's incentive.

On the other hand, the shift in the latent bid distribution could be such that more bids are below the fixed price, but still winning bids. This would mean $(P'(p_{t+1} > \beta_{i,t+h} > b(q_{t+h})|u_i) - P(p_{t+1} > \beta_{i,t+h} > b(q_{t+h})|u_i)) > 0$ and thus we would underestimate extensive margin learning using our approach.

Since we want to examine learning as a response to overpaying we also look at the most narrow way to avoid overpaying: reducing overbidding. In our model only overbidders overbid. Thus a type transition from overbidder to sophisticate as well as leaving the market reduces overbidding. Consequently, the treatment effect of overpaying on overbidding in the next period is given by the sum of learning at both margins multiplied by the probability of a latent successful overbid.

$$E[TE^{t+1}_{overbid}] = -E[\epsilon_i + \iota_i|u_i] \cdot P(\beta_{i,t+1} > p_t \wedge \beta_{i,t+1} > b(q_{t+1}))|u_i) \tag{2.9}$$

Until now, we focused on treatment effects for behavior directly after the first overbid. However, we need to pool observations over a time period to estimate these effects. This method introduces the issue of subsequent treatments within that time period. Note that only untreated and those who were treated but did not change type due to treatment are subject to a possible second treatment. Hence, the control group is more likely to be treated over a longer time period, which attenuates treatment effects. The treatment effects of overpaying in t on strict non-overbids and overbids k periods after are given in Proposition 5.

Proposition 5. Define $p_{l,h}$ ($p_{l,s}$) as the probability of someone that did not change their type in t to leave (become a sophisticate) until period h. If bidders are treated in period t the effects on behavior in period $t+k$ are given by

$$E[TE^{t+k}_{non\text{-}overbid}] = E[-\epsilon_i(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i])|u_i]P(p_{t+k} > \beta_{i,t+k} > b(q_{t+k})|u_i)$$
$$E[TE^{t+k}_{overbid}] =$$
$$E[-(\epsilon_i+\iota_i)(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i]-E[p_{s,k}|\epsilon_i,\iota_i,u_i])|u_i]\cdot P(\beta_{i,t+k} > p_{t+k} \wedge \beta_{i,t+k} > b(q_{t+k})|u_i).$$

The proof of this result is in appendix 2.C. Proposition 5 shows that treatment effect estimates are attenuated by a factor that is smaller than one. This factor falls in the probability of a change in type between t and $t+k$.

Under some further assumptions, we can use our expressions for the treatment effects to calculate the raw extensive and intensive margin learning parameters. To do this, we have to tackle two issues. First, we only observe latent changes in type with a small probability (equations 2.8 and 2.9. Second, treatment effects may be attenuated due to subsequent treatments (proposition 5).

We can get at the learning parameters if we divide the corresponding treatment effects by the potential outcome for the untreated. For now, we focus on extensive margin learning and non-overbids in the period directly after the treatment. The treatment effect of overpaying on non-overbids is the probability of a latent overbid multiplied by the expected extensive margin parameter $E[\epsilon_i|u_i]$. Untreated individuals are still in the auction and submit all their latent non-overbids. Thus the potential outcome for an untreated individual is the probability of a latent non-overbid. We get the expected margin learning parameter by dividing the treatment effect by this potential outcome,

$$-E[\epsilon_i|u_i] = \frac{-E[\epsilon_i|u_i]P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1}))|u_i)}{P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1}))|u_i)}.$$

We estimate our treatment effects from data that aggregates over several auctions. Consequently, we also want to identify the learning parameters from these aggregate effects. This issue complicates the identification argument from the previous paragraph. We sum treatment effects and potential outcomes over all auctions from t to k. Since aggregation opens up the possibility of subsequent treatments between t and k, we use the expression for the treatment effects from Proposition 5. We divide the aggregated treatment effects by the corresponding potential outcomes to get the expressions in Proposition 6. We prove this proposition in Appendix 2.D.

Proposition 6. Suppose individuals are treated at time $t \in \{1, ..., \infty\}$ and we aggregate our treatment effects over the following $k \in \{1, ..., \infty\}$ periods. Then the treatment effects divided by the potential outcomes are given by the following expressions:

$$\frac{\Sigma_{m=0}^{k} E[TE_{non\text{-}overbid}^{t+m}]}{\Sigma_{m=0}^{k} E[non\text{-}overbid_t^{t+m}(0)|u_i]} = \frac{\Sigma_{m=0}^{k} E[\epsilon_i(1-E[p_{l,m}|\epsilon_i, \iota_i, u_i])|u_i]}{\Sigma_{m=0}^{k} E[(1-E[p_{l,m}|\epsilon_i, \iota_i, u_i])|u_i]}$$
$$\frac{\Sigma_{m=0}^{k} E[TE_{overbid}^{t+m}]}{\Sigma_{m=0}^{k} E[overbid_t^{t+m}(0)|u_i]} = \frac{\Sigma_{m=0}^{k} E[-(\epsilon_i+\iota_i)(1-E[p_{l,m}|\epsilon_i, \iota_i, u_i]-E[p_{s,m}|\epsilon_i, \iota_i, u_i])|u_i]}{\Sigma_{m=0}^{k} E[(1-E[p_{l,m}|\epsilon_i, \iota_i, u_i]-E[p_{s,m}|\epsilon_i, \iota_i, u_i])|u_i]}.$$

The expressions in 6 do not immediately simplify because the probabilities of subsequent treatment ($p_{l,k}$ and $p_{s,k}$) are functions of the corresponding learning parameters (ϵ_i and ι_i). Take for example the expression $E[\epsilon_i(1-E[p_{l,m}|\epsilon_i, \iota_i, u_i])|u_i]$, we cannot separate $E[\epsilon_i|u_i]$ from this expression since ϵ_i and $E[p_{l,m}|\epsilon_i, \iota_i, u_i]$ are dependent.

Assuming that ϵ_i is homogeneous for every value of u_i solves this issue and allows us to calculate ϵ_i. If we commit to this assumption, the expectation of ϵ_i conditional on u_i is a number and non-stochastic ($E[\epsilon_i|u_i] = \epsilon_u$). Thus we can separate the expectation $E[\epsilon_u(1-E[p_{l,m}|, \iota_i, u_i])|u_i] = \epsilon_u E[E[p_{l,m}|\epsilon_i, \iota_i, u_i])|u_i]$. Then first expression in Proposition 6 then simplifies to ϵ_u, because ϵ_u does not depend on m and we can pull it in front of the sum. We can apply similar arguments to the second expression to identify $\epsilon_u + \iota_u$.

Corollary 1. If there are real numbers ϵ_u and ι_u, such that $E[\epsilon_i|u_i] = \epsilon_u$ and $E[\iota_i|u_i] = \iota_u$,

$$\frac{\Sigma_{m=0}^{k} E[TE_{non\text{-}overbid}^{t+m}]}{\Sigma_{m=0}^{k} E[non\text{-}overbid_t^{t+m}(0)|u_i]} = \epsilon_u$$
$$\frac{\Sigma_{m=0}^{k} E[TE_{overbid}^{t+m}]}{\Sigma_{m=0}^{k} E[overbid_t^{t+m}(0)|u_i]} = \epsilon_u + \iota_u$$

Since the probabilities of subsequent treatment depend on the time, we can check violations of the assumptions behind Corollary 1 by aggregating over different time-horizons. For example, recall that $p_{l,k}$ is the probability that a bidder that was treated in t but did not leave then leaves until $t+k$. This probability increases in k. Same holds for $p_{l,s}$. If $p_{l,s} = p_{l,k} = 0$ the expressions in Proposition 6 simplify to the expected learning parameters. The same holds under our homogeneity assumption. If $p_{l,s}$ and $p_{l,k}$ are large and out homogeneity assumption does not hold, this is not necessarily the case. Thus a change in the expressions from Proposition 6 when aggregating over a time-period farther into the future indicates a violation of our homogeneity assumption.

2.5.3 Identification of Treatment Effects

In the preceding section, we clarified the connection between treatment effects on observed variables and the learning parameters of our underlying model. Now we turn to the identification of those treatment effects. For this purpose, we represent the model from the preceding section as a causal Directed Acyclic Graph (DAG). This Representation illustrates the causal relationships implied by that model and also allows us to compute a set of control variables that guarantee the conditional independence assumption that we need to estimate our treatment effects. Since in our case identification depends on unobserved bidder characteristics, we conclude by a discussion of how we can implement our identification strategy using past bidder behavior as a proxy for this unobserved variable. This discussion motivates the estimation of our treatment effects by OLS in the subsequent section.

If we did not use any control variables, we would underestimate the magnitude of our treatment effects. Bidders have unobserved characteristics, such as their bidder characteristics (u_i) or their type (overbidder, sophisticate, or leave). Suppose these characteristics increase a bidder's probability to overbid; in this case, they also increase the probability of overpaying today since overbidding is a prerequisite for overpaying. We are interested in tomorrow's overbids as an outcome, which also increases in unobserved characteristics. Consequently, unobserved bidder characteristics lead to a positive association between our treatment (overpaying today) and future overbids. This positive correlation leads to an upwards bias and thus an underestimation of the magnitude of our (negative) treatment effects.

We try to address this issue in two ways. First, we adjust for a bidder's past actions as a proxy for unobserved bidder characteristics, and second, we adjust for the treatment assignment process by controlling for overbidding.

We clarify the assumptions of this strategy with a DAG. In a DAG, a directed edge (an arrow) indicates a causal relationship. For example, if we draw an arrow from $overpaid_{i,t}$ to $overbid_{i,t+1}$, we show that our model allows for a causal effect of overpaying on overbidding in a subsequent auction. The direction of the arrows indicates the direction of causality. In our context, the fact that DAGs do not contain any cycles has an economic interpretation: bidders are myopic. Otherwise, future auctions would influence bidding behavior in today's auction, and we would get a cycle in our graph. This assumption is in line with other behavioral economics auction papers such as Malmendier and Lee (2011). We try to explain the theory on DAGs as far as we need it. For a gentle introduction, see chapter 3 of Cunningham (2021).

We can generate our DAG from the non-parametric structural equation model at the end of section 2.5.1. Peters, Janzing, and Schölkopf (2017) explains how we can represent a non-parametric structural equation model as a DAG. We go through each equation and draw an arrow from each right-hand side variable to each left-hand-side variable. We leave out exogenous shocks for ease of exposition and draw

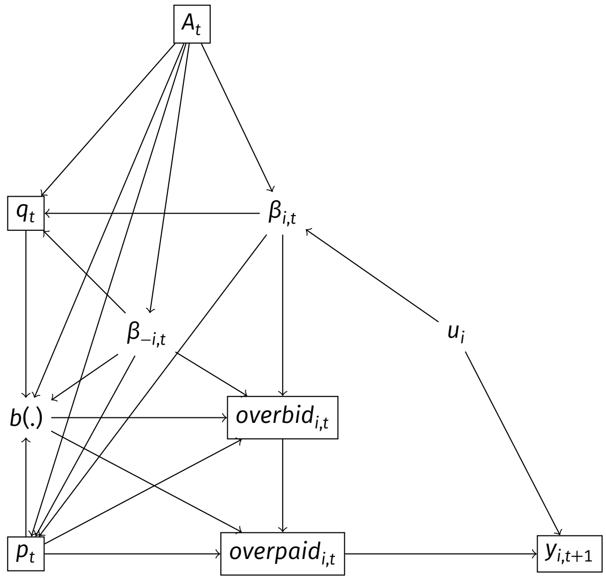


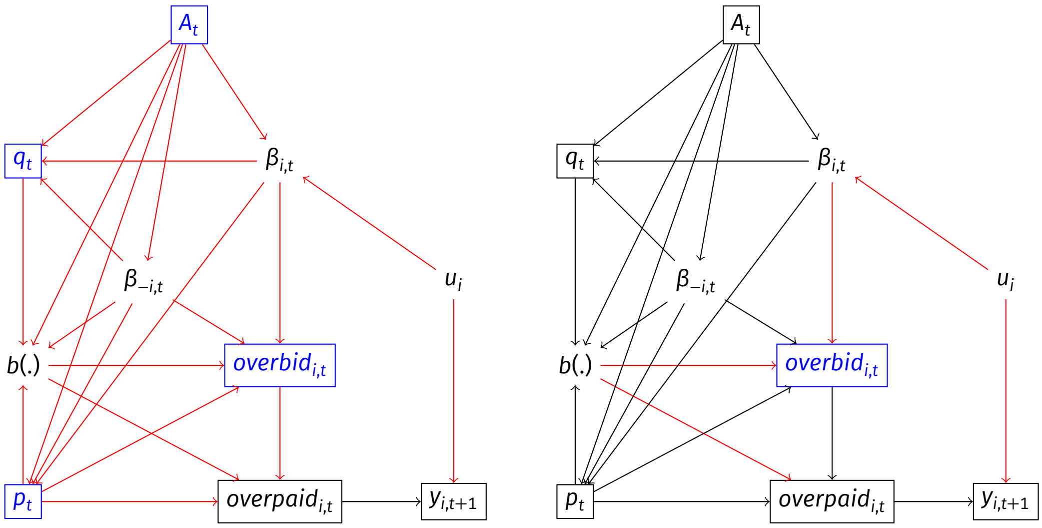
Figure 2.3. Our empirical model represented as a a directed acyclic graph (DAG).

rectangular boxes around all observed variables. This procedure results in the DAG that we depict in figure 2.3.

To simplify the DAG to make it clearer. Since the structural equations for overbid and non-overbid depend on the same variables, we use $y_{i,t+1}$ as a stand-in for both types of outcomes. We focus on time-period t and display arrows pointing from t to $t+1$ only in a stylized way. In particular, the path $u_i \rightarrow y_{i,t+1}$ abstracts from the fact that this effect is again channeled through the bidding process. This simplification is without loss of generality since u_i is the only connection between behavior in t and $t+1$. We also abstract from bidding behavior before $t+1$. We restrict our data set to behavior after the first overbid in t. This restriction selects only bidders that were overbidders in t. Consequently, there is no remaining variance in $\theta_{i,t}$ and can omit it from our DAG. However, we have to verify that $overbid_{i,t}$ is not a bad control (in DAG parlance: it can be part of the admissible adjustment set).

The main idea behind identification proofs with DAGS is that we want to select control variables to block all non-causal paths (back-door paths). Panel a) of figure 2.4 depicts all back-door paths in red. The causal path of interest in our DAG is $overpaid_{i,t} \rightarrow y_{i,t+1}$: overpaying leads to a change in type (which is omitted for simplicity), and that leads to a change in future behavior. The back-door paths consist of two patterns: confounders (e.g. $\leftarrow u_i \rightarrow$) and colliders (e.g. $\rightarrow overbid_{i,t} \leftarrow$). A back-door path through a confounder is blocked if we control for that confounder. A back-door path through a collider is blocked if we do not control for that collider.

Our first strategy uses proxies of unobserved bidder heterogeneity (u_i) to identify the treatment effect. Since all confounding paths go through u_i we could identify the treatment effect by controlling for it, which blocks all back-door paths. Unfortunately, u_i is unobserved. Thus we have to rely on proxies that are, by definition, imperfect. The variable u_i mainly determines the height of a bidder's latent bid. Thus variables such as the average amount of a bidder's past bids are very informative about that variable.



(a) All backdoor paths **(b)** Collider Path

Figure 2.4. Panel (a) shows that all backdoor paths go through u_i. All backdoor paths also go through the (blue) adjustment set $A_t, q_t, p_t, overbid_{i,t}, overpaid_{i,t}$. Panel (b) shows that the collider path opened by conditioning on $overbid_{i,t}$ (in blue) also goes through u_i.

Since we have to rely on proxies, we use the DAG to guide us in choosing additional control variables. We rely on the DAG in two ways: we verify that we do not include any bad controls (the expanded adjustment set still blocks all back-door paths), and we use it to identify further confounding mechanisms.

Our section strategy relies on restricting our sample to first overbids to remove the link between a bidder's behavior and their treatment status. We also want to block the part of the path between u_i and overpaying that goes through current bidder behavior. The most important part of this is conditioning on $overbid_{i,t}$ since this variable is the most direct link between bidder *i*'s characteristics and their treatment status. By controlling for overbid, we are controlling for bidder *i*'s behavior and render other bidder's behavior the main determinant of treatment assignment.

This approach has two shortcomings: variables that connect the characteristics of all bidders in the auction and the fact that $overbid_{i,t}$ is a collider. As we can see auction characteristics (A_t) influence $\beta_{-i,t}$ as well as $\beta_{i,t}$. While controlling for $overbid_{i,t}$ disconnects the direct link between bidder *i*'s treatment status and their characteristics, these two variables remain indirectly connected. Similar bidders can sort into similar auctions. Thus if I am an overbidder, I am likely to be in an auction together with other bidders that turn my overbid into an overpaid. There are similar issues with the fixed price p_t and the auction quantity q_t. We address these issues by controlling for these variables as well. This strategy is also illustrated in Panel a) of figure 2.4, where we mark these variables in blue. As you can see all back-door paths in the left side of the graph go through these variables.

Finally, since $overbid_{i,t}$ is a collider adjusting for it opens up a new back-door path. Panel a) of figure 2.4 shows this path marked in red. Adjusting for $overbid_{i,t}$ renders other bidder's action and my latent bid dependent. This occurs because the fact that my overbid is observed indicates that I submitted a high bid and thus have a high latent bid. It also indicates that the other bidders submitted a low bid. Thus if we observe an overbid by a bidder, we ought to think that their latent bids are likely high and the other bidder's bids are likely low. That is conditional on the observed overbid they are dependent.

We can fix this issue by adjusting for u_i, which was already part of our initial strategy. All back-door paths, including our new one, go through u_i. Thus conditioning on u_i also closes the back-door path, opened by conditioning on $overbid_{i,t}$. Hence $overbid_{i,t}$ is not a bad control, and our sample restriction is justified.

We formalize our empirical strategy with the back-door criterion (Theorem 3.3.2 in Pearl (2009)). As we have shown $\{u_i, A_t, p_t, overbid_{i,t}\}$ or $\{u_i\}$ block all back-door paths. Thus the causal effects of overpaying on future observed overbids and future observed bids are identified and can be computed by controlling for these variables. This statement is equivalent to the statement that our potential outcomes are independent conditional on $\{u_i, A_t, p_t, overbid_{i,t}\}$ or $\{u_i\}$.

2.6 Estimated Treatment Effects

We adjust for overbidding by restricting the sample to the first overbid for any customer. These initial overbids can be in an auction that ends below or above the fixed price. Bidders whose initial overbid was in an overpaid auction overpay and are in our treatment group. Bidders whose initial overbid was not in an overpaid auction do not overpay and form the control group. We follow these bidders for 60 days after their first overbid and count all overbids and strict non-overbids during that period. These two variables are our outcomes. This process results in a data-set with bidders that overbid at least once. The data includes one row per bidder and columns with a dummy indicating if the first bid is overpaid, our outcome variables, and several controls. We exclude data after the structural break (see figure 2.1).

Finally, we adjust for the remaining control variables from section 2.5 by fitting a linear regression on this data-set. We control for product price (p_t) and the number of products for sale (q_t). We operationalize the auction characteristics (A_t) with the following variables: weekday, week, hour, product category, and auctioneer fixed effects. In addition, we use bidder history variables such as the average of bids before a bidder's first overbid (in Euro) as a proxy for (u_i). We also include the average of these variables for all other bidders in the auction of a bidder's first overbid in

Table 2.2. Coefficients from a regression of the number of overbids and non-overbids (between 0 and 60 days after the first overbid) on an overpaid dummy and controls. Standard errors are clustered on auction level.

	# Overbids	# Overbids	# Non-Overbids	# Non-Overbids
Overpaid	−0.144***	−0.154***	−0.165	−0.343**
	(0.027)	(0.027)	(0.114)	(0.109)
Num.Obs.	121836	117973	121836	117973
R2	0.055	0.092	0.090	0.168
Counterfactual Mean	1.206	1.213	6.191	6.387
Bidder History	No	Yes	No	Yes
Window	0-60	0-60	0-60	0-60

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the regression. We report a list of control variables, tabulate summary statistics and explain the construction of our control variables in appendix 2.F. Since our treatment assignment occurs at the auction level we follow the recommendation in Abadie et al. (2017) and cluster standard errors at the auction level.

Table 2.2 reports the results of these regressions. The first row reports the causal effect estimates for overpaying on future overbids and non-overbids. The row labelled counterfactual mean reports the fitted value for the regression with all variables set at their means and overpaid set to zero. With the full set of controls we find that overpaying decreases overbids in the following 60 days by −0.15 (compared to a counterfactual mean of 1.2) and non-overbids by −0.34 (compared to a counterfactual mean of 6.4).

We assess our strategies of using bidder histories to proxy for u_i by looking at coefficient movements when adding these variables. Since our proxies have a good theoretical justification (high bids in the past are likely a good indicator of a tendency for high bids), our estimates should move closer to the truth when controlling for these proxies. Thus if the magnitude of our estimates increases when we add the proxies, it should increase even more if we were to add the real thing.[14] According to table 2.2, adding history controls (our proxies) increases the magnitude of our coefficient estimates. We take this as evidence that our identification strategy works well.

We apply corollary 1 to calculate estimates of extensive and intensive margin learning from our estimated treatment effects. We use 60 days after the first overbid and 60-120 days after the first overbid (see Table 2.E.1 in Appendix 2.E). We calculate the treatment effect for the average individual from our OLS estimates

14. Oster (2019) formally makes this argument for a specific parametric relationship between proxies and the underlying variable.

with history controls and estimate the potential outcome for the untreated with the counterfactual mean. Thus we estimate ϵ_u and ι_u for the value of u_i implied by the average value of our control variables.

Table 2.3. Estimates for ε_u and ι_u using the treatment effect estimates and counterfactual means with history controls from Table 2.2 and Table 2.E.1.

Time Period	$\hat{\varepsilon_u}$	$\hat{\iota_u}$
0-60	0.054	0.073
60-120	0.049	0.068

We find evidence for learning at both margins. As table 2.3 shows, the estimates vary little with the aggregation window. Consequently our homogeneity assumption seems to perform well. For the mean individual we estimate an extensive margin learning parameter of approximately 5% and an intensive margin learning parameter of approximately 7%.

2.7 Conclusion

We find evidence for extensive as well as intensive margin learning. Overpaying lowers future overbids and non-overbids. We use our model to calculate extensive and intensive margin learning from these causal effects. The causal effects imply that 5% of bidders that overpay learn not to overbid (intensive margin), and 7% of bidders that overpay leave the market (extensive margin).

A simple economic model teaches us how the firm should react to learning at these margins. We model an increase in firm sophistication by a broader scope of optimization: initially, the firm behaves sub-optimally, then optimally with exogenous fixed prices; and finally, the firm chooses the fixed prices optimally. If fixed prices are exogenous and extensive margin learning is high, the firm offers quantities that prevent overpaying. On the other hand, if the firm endogenously chooses fixed prices, it sets them high enough to prevent overbidding entirely.

According to this theory, we should observe a period of overpaying, followed by a sudden reduction in overpaying and increased quantities. Finally, we should observe overpaying prevention through high fixed prices. Halfway through our observation period, the probability that auctions end in an overpay suddenly drops. As in our model with exogenous fixed prices, this drop is likely due to increased product quantity. After our observation period, the firm implemented a new strategy, increasing fixed prices and undercutting those higher prices with the auction's starting bid, which mechanically rules out overbidding. This policy is in line with our model with endogenous fixed prices.

We find that strategic learning and leaving the market are roughly equally likely. This finding unites the literatures on learning (Agarwal et al., 2013; Ater and Landsman, 2013; Haselhuhn et al., 2018, e.g.) and customer retention (Anderson and Simester, 2010; Seru, Shumway, and Stoffman, 2010; Backus et al., 2021, e.g.). From the perspective of the learning literature, consumers try to avoid the action that had negative consequences: they avoid overbidding because they overpaid. According to the literature on customer retention, they might also leave the market. Bidders might leave the market because they learn about their abilities as bidders or the value of participating in auctions. They can also become angry and leave the market.

The firm's shaping of this learning process is a new reason for the persistence of consumers' biases. The previous literature finds that firms can exploit consumer biases because consumers forget what they have learned (Agarwal et al., 2013), or new naive consumers replace experienced ones (Wang and Hu, 2009; Augenblick and Rabin, 2016). We document that biases may also persist because firms make learning harder. Our results on firms shaping consumer learning can explain market design choices and suggests possible avenues for regulating this behavior.

Complementing previous results, we find that when biased consumers learn, market-like institutions might be preferable to multiple single-unit auctions. Malmendier and Szeidl (2020) argue that firms want to sell several goods in individual auctions to fish for fools. In single-unit auctions, the highest bidder (likely upward biased) sets the price, whereas, in markets (and in the market-like auction we study), a larger share of biased buyers is needed to influence the price. According to Malmendier and Szeidl (2020) choosing individual auctions maximizes period profits. However, we show that it might lose the firm customers because more individual auctions end overpaid. Consequently, sellers should be more likely to choose markets when bidders learn.

Firms can shape consumer learning in two ways: ways that benefit and ways that harm consumers.[15] According to our model, consumers are worse off when firms can change reference prices. In this case, the firm can remove the learning stimulus without benefiting the consumer. If the reference prices are exogenous, the firm prevents consumer learning through lower prices, which helps consumers.

This mechanism opens an avenue for consumer protection regulation. Suppose the regulator forbids instruments that allow firms to deceive consumers and exploit their biases. In that case, firms are left with actions that shape consumer learning to benefit consumers. The restricted action set incentivizes firms to protect biased consumers. This type of regulation incentivizes private paternalism in the sense of Laibson (2018).

In our setting, reference price regulation can constrain a firm's harmful ways of shaping consumer learning. For example, a regulator can mandate a minimum revenue share through sales at fixed prices. This policy diminishes a firm's ability

15. We do not model consumer preferences. Consequently, our only criterion for welfare analysis is that lower prices for the same quantity are good for consumers.

to raise fixed prices. Consequently, firms have to shape consumer learning through higher quantities, which benefits the consumer. There are already other types of reference price regulation. In Germany, for example, firms that advertise undercutting a reference price need to offer that reference price previously.[16]

We build the basis for further research on customer retention and learning in platform markets. While we study policies specific to our context (higher quantities and higher fixed prices), these policies suggest a general pattern. Consumers learn from negative experiences. Consequently, the firm can reduce the number of negative experiences (higher quantities) or make existing negative experiences less salient (higher fixed prices). Further, more general research can build on our work and map features of existing markets into these two categories.

Appendix 2.A Proof of Proposition 2

Proof. We guess two policy functions (always choose $q_t = o_t + s_t$) and always choose $q_t = s_t$). Since the union of these conditions covers the parameter space the desired result follows.

Because o_0, s_0 are both larger than one and ϵ, ι are both smaller than one we guarantee that $o_t > 0 \wedge s_t > 0 \ \forall t$.

In the remainder of this proof we drop the time index to simplify our notation.

We can simplify the strategy space because some actions are dominated and some are outcome-equivalent. All actions with $q > o + s$ are dominated because profits are zero and we can get positive profits with $q = o + s$. Profits are constant over $o < q \leq o + s$. Thus we can eliminate this interval from the action space if we include its upper boundary $o + s$.

Having simplified the strategy space in this way we state the Bellman equation.

$$V(o,s) \;=\; max_{q \in Q} \begin{cases} q \cdot \beta + sp + \delta V(o - (\epsilon + \iota) \cdot q, s + \iota \cdot q) & \text{if } q \;\leq\; o \\ (s+o)p + \delta V(o,s) & \text{if } q \;=\; o + s \end{cases}, \tag{2.A.1}$$

$$\text{where } Q \;=\; [0,o] \cup \{o + s\}. \tag{2.A.2}$$

We guess and verify the policy $q = o + s$. The result of this policy is is that the firm sells $o + s$ unity each period at a price of p. This leads to the following value function

$$V(o,s) \;=\; \Sigma_{k=0}^{\infty} \delta^k (o_t + s_t) p \;=\; \frac{(o_t + s_t)p}{1 - \delta}. \tag{2.A.3}$$

16. https://www.frankfurt-main.ihk.de/recht/uebersicht-alle-rechtsthemen/wettbewerbsrecht/unlauterer-wettbewerb/irrefuehrende-werbung/mondpreise-5196206 accessed: 2.02.2022

We derive conditions under which this value function solves the Bellman equation

$$\frac{(o+s)p}{1-\delta} = max_{q\in Q}\begin{cases} q\cdot\beta + sp + \delta\frac{(o+s-\epsilon q)p}{1-\delta} & \text{if } q \le o \\ (s+o)p + \delta\frac{(o+s)p}{1-\delta} & \text{if } q = o+s \end{cases}, \tag{2.A.4}$$

$$\text{where } Q = [0,o] \cup \{o+s\}. \tag{2.A.5}$$

We need to check two cases, either the left arm ($q \le o$) of the right-hand side of the Bellman equation rises or falls in q. It (weakly) rises if

$$\beta \ge \frac{\delta}{1-\delta}\epsilon p. \tag{2.A.6}$$

In this case profits are either maximized at $q = o+s$ or at $q = o$. They are maximized at $q = o+s$ and our guess is true if

$$o\cdot\beta + sp + \delta\frac{(s+(1-\epsilon)o)p}{1-\delta} \le (o+s)p + \delta\frac{(s+o)p}{1-\delta} \tag{2.A.7}$$

$$\leftrightarrow \frac{\beta - p}{p} \le \frac{\delta}{1-\delta}\epsilon. \tag{2.A.8}$$

If

$$\frac{\beta}{p} < \frac{\delta}{1-\delta}\epsilon \tag{2.A.9}$$

the left arm ($q < o$) of the right-hand side of the Bellman falls in q.

In this case profits are either maximized at $q = 0$ or at $q = o+s$. They are maximized at $q = o+s$ if

$$sp + \delta\frac{(s+o)p}{1-\delta} < (o+s)p + \delta\frac{(s+o)p}{1-\delta} \tag{2.A.10}$$

$$\leftrightarrow 0 < op, \tag{2.A.11}$$

which is true. Since condition 2.A.8 is strictly stronger than 2.A.9, we can verify our guess of no overbidding if condition 2.A.8 holds.

We guess that the seller wants all auctions to end in an overpay. Then the seller derives a profit of $p \cdot s$ from the initial sophisticates in perpetuity. They derive a profit of β per overbidder in each period from a steadily declining stock of overbidders. This results in $o_t(1-\epsilon-\iota)^k\beta$ in each future period k. In each future period a fraction i of the current overbidders is transformed into sophisticates $o_t(1-\epsilon-\iota)^{p-1}\iota p$. Consequently, in period k there are $\Sigma_{p=1}^{k} o_t(1-\epsilon-\iota)^{p-1}\iota p$ that were generated through intensive margin learning. The discounted sum of these period profits yields the value function under the conjecture that the seller ends all auctions in an overpay

$$V(o,s) \;=\; \Sigma_{k=0}^{\infty}\delta^k(o(1-\epsilon-\iota)^k\beta+sp)+\Sigma_{k=1}^{\infty}\delta^k\Sigma_{p=1}^{k}o(1-\epsilon-\iota)^{p-1}\iota p \quad (2.A.12)$$

$$= o\beta\Sigma_{k=0}^{\infty}\delta^k(1-\epsilon-\iota)^k+sp\Sigma_{k=0}^{\infty}\delta^k+\frac{o\iota p}{1-\epsilon-\iota}\Sigma_{k=1}^{\infty}\delta^k\Sigma_{p=0}^{k}(1-\epsilon-\iota)^p-1 \quad (2.A.13)$$

$$= \frac{o\beta}{1-\delta(1-\epsilon-\iota)}+\frac{sp}{1-\delta}+\frac{o\iota p}{1-\epsilon-\iota}\Sigma_{k=1}^{\infty}\delta^k\left(\frac{1-(1-\epsilon-\iota)^{k+1}}{\epsilon+\iota}-1\right) \quad (2.A.14)$$

$$= \frac{o\beta}{1-\delta(1-\epsilon-\iota)}+\frac{sp}{1-\delta} \quad (2.A.15)$$

$$+\frac{o_t\iota p}{(1-\epsilon-\iota)(\epsilon+\iota)}\Sigma_{k=1}^{\infty}\delta^k(1-\epsilon-\iota)-(1-\epsilon-\iota)\delta^k(1-\epsilon-\iota)^k \quad (2.A.16)$$

$$= \frac{o\beta}{1-\delta(1-\epsilon-\iota)}+\frac{sp}{1-\delta}+\frac{o_t\iota p}{\epsilon+\iota}\Sigma_{k=1}^{\infty}\delta^k-\delta^k(1-\epsilon-\iota)^k \quad (2.A.17)$$

$$= \frac{o\beta}{1-\delta(1-\epsilon-\iota)}+\frac{sp\delta}{1-\delta}+\frac{o\iota p}{\epsilon+\iota}\left[\frac{\delta}{1-\delta}-\frac{\delta(1-\epsilon-\iota)}{1-\delta(1-\epsilon-\iota)}\right]. \quad (2.A.18)$$

We look for conditions under which this conjecture for the value function solves the seller's Bellman equation (equation 2.A.2). If

$$\beta \;<\; \delta(\epsilon+\iota)\left(\frac{\beta}{1-\delta(1-\epsilon-\iota)}+\frac{\iota p}{\epsilon+\iota}\left[\frac{\delta}{1-\delta}-\frac{\delta(1-\epsilon-\iota)}{1-\delta(1-\epsilon-\iota)}\right]\right)-\frac{\delta p}{1-\delta} \quad (2.A.19)$$

there is no overpaying because the left arm of profits fall in q. Then we have to compare $q=0$ with $q=o+s$. Since the latter leads to higher period profits and both lead to the same future profits the firm prefers $q=o+s$, which refutes our conjecture.

If condition 2.A.19 does not hold the left-arm of the values function rises in q and the seller ends every auction in overpaying if he prefers that to $q=o$. This is the case if

$$\beta - p \geq \delta(\epsilon + \iota)\Bigg(\beta(1 - \delta(1 - \epsilon - \iota))^{-1} \tag{2.A.20}$$

$$+ \iota p(\epsilon + \iota)^{-1}\left[\frac{\delta}{1-\delta} - \frac{\delta(1-\epsilon-\iota)}{1-\delta(1-\epsilon-\iota)}\right]\Bigg) - \iota\delta p(1-\delta)^{-1} \tag{2.A.21}$$

$$\leftrightarrow \tag{2.A.22}$$

$$\beta - p \geq \delta(\iota + \epsilon)\beta(1 - \delta(1 - \epsilon - \iota))^{-1} + \iota p\frac{\delta^2}{1-\delta} - \iota p\frac{\delta}{1-\delta} \tag{2.A.23}$$

$$- \iota p\frac{\delta^2(1-\epsilon-\iota)}{1-\delta(1-\epsilon-\iota)} \tag{2.A.24}$$

$$\leftrightarrow \tag{2.A.25}$$

$$\beta - p \geq \delta(\iota + \epsilon)\beta(1 - \delta(1 - \epsilon - \iota))^{-1} + \iota p\frac{\delta^2 - \delta}{1-\delta} - \iota p\frac{\delta^2(1-\epsilon-\iota)}{1-\delta(1-\epsilon-\iota)} \tag{2.A.26}$$

$$\leftrightarrow \tag{2.A.27}$$

$$\beta - p \geq \delta(\iota + \epsilon)\beta(1 - \delta(1 - \epsilon - \iota))^{-1} + \iota p\frac{\delta^2 - \delta}{(1-\delta)(1-\delta(1-\epsilon-\iota))}, \tag{2.A.28}$$

where the last step follows if since $\frac{1}{d} > 1 - \epsilon - \iota$, which is always true since ϵ, ι and d are all between zero and one. Having simplified the condition so far we can collect terms and solve for a condition on ϵ

$$(1 - \delta(1 - \epsilon - \iota))(\beta - p) \geq \delta(\iota + \epsilon)\beta + \iota p\frac{\delta^2 - \delta}{1-\delta} \tag{2.A.29}$$

$$\leftrightarrow (1-d)\beta - \frac{1 - 2\delta + \delta^2 + \delta e - \delta^2\epsilon}{1-d}p \geq 0 \tag{2.A.30}$$

$$\leftrightarrow (1-\delta)\beta - (1-\delta)p - \delta\epsilon p \geq 0 \tag{2.A.31}$$

$$\leftrightarrow \frac{\beta - p}{p}\frac{1-\delta}{\delta} \geq \epsilon. \tag{2.A.32}$$

This condition covers all cases in which the other strategy is not optimal. Consequently, the seller either sets $q = o$ or $o < q \leq o + s$.

□

Appendix 2.B Proof of Proposition 4

Proof. The potential outcome for the the untreated (people that did not overpay) is the probability that an overbidder submits a strict non-overbid,

$$E[\textit{non-overbid}_t^{t+1}(0)|u_i] = P(p_{t+1} > \beta_{i,t+h} > b(q_{t+h})|u_i).$$

If we exogenously assign a bidder to the treated status they either stay an overbidder, become a sophisticate or leave. In the cases in which they become a sophisticate they also change their latent bid distribution. This leads to a change in probabilities which we denote by switching from P to P'. We calculate the potential outcome of a bidder treated in t and observed in $t+1$ as

$$\begin{aligned} E[\textit{non-overbid}_t^{t+1}(1)|u_i] =& E[(1-\epsilon_i-\iota_i)|u_i]P(\beta_{i,t+1} < p_{t+1} \wedge \beta_{i,t+1} > b(q_{t+1})|u_i) \\ &+E[\iota_i|u_i]P'(\beta_{i,t+1} < p_{t+1} \wedge \beta_{i,t+1} > b(q_{t+1})|u_i). \end{aligned}$$

Adding an intelligent zero and taking the difference of potential outcomes yields the following expression for the treatment effects

$$\begin{aligned} &E[TE_{\textit{non-overbid}}^{t+1}] = E[-\epsilon_i|u_i]P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i) \\ &+\iota_i(P'(p_{t+1} > \beta_{i,t+h} > b(q_{t+1})|u_i) - P(p_{t+1} > \beta_{i,t+1} > b(q_{t+1})|u_i)) \end{aligned}$$

□

Appendix 2.C Proof of Proposition 5

Proof. Only overbidders can potentially change their type. Thus the probability of changing your type in period $t+k$ is the probability to stay an overbidder until that period multiplied with either ϵ_i or ι_i. To calculate this probability we condition on individual level characteristics $(\epsilon_i, u_i, \iota_i)$. We calculate the probability of an Untreated individual to be a sophisticate in $t+h$ $(p_{s,h}(0))$, and to leave the auction until $t+k$ $(p_{l,k})$.

$$\begin{aligned} E[p_{s,k}|\epsilon_i,\iota_i,u_i] &= E\left[\Sigma_{m=0}^{k-1} overpaid_{i,t+m}\iota_i(1-(\epsilon_i+\iota_i)overpaid_{i,t+m-1})^m|\epsilon_i,\iota_i,u_i\right] \\ E[p_{l,k}|\epsilon_i,\iota_i,u_i] &= E\left[\Sigma_{m=0}^{k-1} overpaid_{i,t+m}\epsilon_i(1-(\epsilon_i+\iota_i)overpaid_{i,t+m-1})^m|\epsilon_i,\iota_i,u_i\right]. \end{aligned}$$

We can then use these probabilities to characterize the potential outcomes for strict non-overbids in period $t+k$ as a function of overpaying in t.

$$\begin{aligned} &E[non\text{-}overpaid_t^{t+k}(0)|u_i] = E[(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i])\mathbb{1}(\beta_{i,t+k} < p_{t+1}\wedge\beta_{i,t+k} > b(q_{t+k})|u_i] \\ &\quad = E[1-E[p_{l,k}|\epsilon_i,\iota_i,u_i|u_i]P(\beta_{i,t+k} < p_{t+1}\wedge\beta_{i,t+k} > b(q_{t+k})|u_i) \\ &E[non\text{-}overpaid_t^{t+k}(1)|u_i] = E[(1-\epsilon_i)(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i])\mathbb{1}(\beta_{i,t+k} < p_{t+1}\wedge\beta_{i,t+k} > b(q_{t+k})|u_i] \\ &\quad = E[(1-\epsilon_i)(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i])|u_i]P(\beta_{i,t+k} < p_{t+1}\wedge\beta_{i,t+k} > b(q_{t+k})|u_i). \end{aligned}$$

The last step in each follows because conditional on u_i, ϵ_i and $\beta_{i,t+k}$ are independent. If we take the difference of potential outcomes we get the treatment effect

$$E[TE\text{-}overbid_t^{t+k}] = E[-\epsilon_i(1-E[p_{l,k}|\epsilon_i,\iota_i,u_i])|u_i]E[\mathbb{1}(\beta_{i,t+k} < p_{t+1}\wedge\beta_{i,t+k} > b(q_{t+k})|u_i].$$

The calculation for the treatment effect on observed overbids is analogous. □

Appendix 2.D Proof of Proposition 6

Proof. We take the expression for the potential outcome of the untreated and the treatment effect from the proof of 5 and divide one by the other.

$$\begin{aligned} &\frac{-\Sigma_{m=0}^{k} E[TE_{non\text{-}overbid}^{t+m}]}{\Sigma_{m=0}^{k} E[non\text{-}overbid_t^{t+m}(0)|u_i]} \\ =&\frac{\Sigma_{m=0}^{k} E[\epsilon_i(1-E[p_{l,m}|\epsilon_i,\iota_i,u_i])|u_i]P(p_{t+m} > \beta_{i,t+m} > b(q_{t+m})|u_i)}{\Sigma_{m=0}^{k} E[(1-E[p_{l,m}|\epsilon_i,\iota_i,u_i])|u_i]P(p_{t+m} > \beta_{i,t+m} > b(q_{t+m})|u_i)} \\ =&\frac{\Sigma_{m=0}^{k} E[\epsilon_i(1-E[p_{l,m}|\epsilon_i,\iota_i,u_i])|u_i]}{\Sigma_{m=0}^{k} E[(1-E[p_{l,m}|\epsilon_i,\iota_i,u_i])|u_i]}. \end{aligned}$$

The proof for the treatment effect on strict overbids is analogous. □

Table 2.E.1. Coefficients from a regression of the number of overbids and non-overbids (between 60 and 120 days after the first overbid) on an overpaid dummy and controls. Standard errors are clustered on auction level.

	# Overbids	# Overbids	# Non-Overbids	# Non-Overbids
Overpaid	−0.103***	−0.115***	−0.108	−0.217*
	(0.023)	(0.023)	(0.089)	(0.086)
Num.Obs.	115 295	111 812	115 295	111 812
R2	0.049	0.079	0.068	0.126
Counterfactual Mean	0.963	0.977	4.263	4.385
Bidder History	No	Yes	No	Yes
Window	60-120	60-120	60-120	60-120

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix 2.E Additional Regression Results

Table 2.E.1 reports additional regression results pertaining the time window of 60 to 120 days after the first overbid.

Appendix 2.F Control Variables

We calculate two sets of bidder history variables. First, we calculate histories for the bidders in our control and treatment group. Given that we restrict attention to the first overbid of each bidder our bidder history variables only capture behavior and experience in the auctions of this seller before that first overbid. Hence our variables do not control for a previous overbid as there was none by construction. When a bidder overbids on the first bid we do not observe a history before that, because there was none. In this case we substitute the average from treatment and control bidders in the same auction. This substitute may not be available if all control and treatment bidders were new bidders. In this case we keep the NA and exclude these observations in regression that include bidders histories.

The bidder history variables roughly fall into two categories. First, there are variables that measure the average previous behavior. For example, the average difference to the high bid measures whether a bidder usually bids early in the auction and the share of bids by phone measures whether a bidder usually bids by telephone or online. Second, some variables refer more to the experience that the bidder had in the previous auctions. For example, the time in the market measures how many hours have past since the first observed bid for that bidder in our sample and total savings measures how much money the bidder has saved compared to the fixed price.

We calculate the same set of bidder history variables also for the other bidders in the auction, even if they are not in the treatment or control group. Referring back to Section 2.5.3, this controls for the other bidders individual characteristics u_{-i}, that were left out of the DAG for simplicity.

Table 2.F.1 gives summary statistics for all history variables that we calculate. It is evident that there are differences between the treatment and control groups, which reassures us that it is helpful to control for this set of proxy variables.

Table 2.F.1. Average value of bidder history variables, and fixed price (p_t) and number of products (q_t) at the first overbid, split by overpaid.

	0		1	
	Mean	Std. Dev.	Mean	Std. Dev.
fixed price	29.63	67.20	30.78	85.86
quantity	282.88	275.17	272.61	257.81
new bidder	0.39	0.49	0.35	0.48
own number of bids	6.74	6.95	7.25	7.63
own average savings, logged	3.50	1.07	3.63	1.09
own average bid, logged	3.56	0.62	3.60	0.66
own time in market (hours)	1479.14	2491.12	1756.69	2690.80
own share of bids by phone	0.82	0.32	0.79	0.34
own average difference to the high bid	11.44	18.44	12.19	24.16
others average number of bids	44.56	39.27	47.42	39.18
others logged total savings	4.99	1.33	5.13	1.30
others logged average bid	3.35	0.47	3.43	0.49
others time in market (hours)	2502.92	2471.15	2701.00	2404.57
others fraction of new bidders	0.13	0.14	0.10	0.11
others share of bids by phone	0.58	0.15	0.60	0.15
others average difference to the high bid	7.73	3.75	8.21	4.63

Table 2.F.2. Average probability of a bidder to be treated (overpay) at their first overbid by show category.

Category	Share Overpaid	n
Heimwerken & Garten	0.32	6246
Mode & Accessoires	0.28	13269
Beauty & Wellness	0.26	15257
Uhren	0.25	8059
Schmuck	0.22	7950
Haushalt	0.20	16786
Möbel & Heimtextilien	0.16	6981
Freizeit & Sammeln	0.08	157

Table 2.F.3. Average probability of a bidder to be treated (overpay) at their first overbid by day of the week.

Weekday	Share Overpaid	n
Sunday	0.21	13909
Monday	0.17	9706
Tuesday	0.27	10209
Wednesday	0.28	9329
Thursday	0.24	9856
Friday	0.25	9581
Saturday	0.26	12115

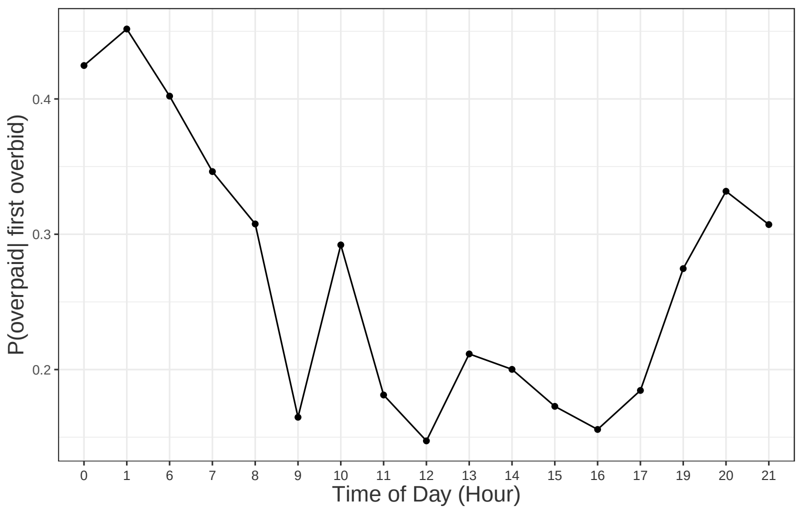


Figure 2.F.1. Average probability of a bidder to be treated (overpay) at their first overbid by time of day. Averages are by hour. The time between 18:00 and 19:00 is missing from hour data because of a coding error.

Appendix 2.G Structural Break

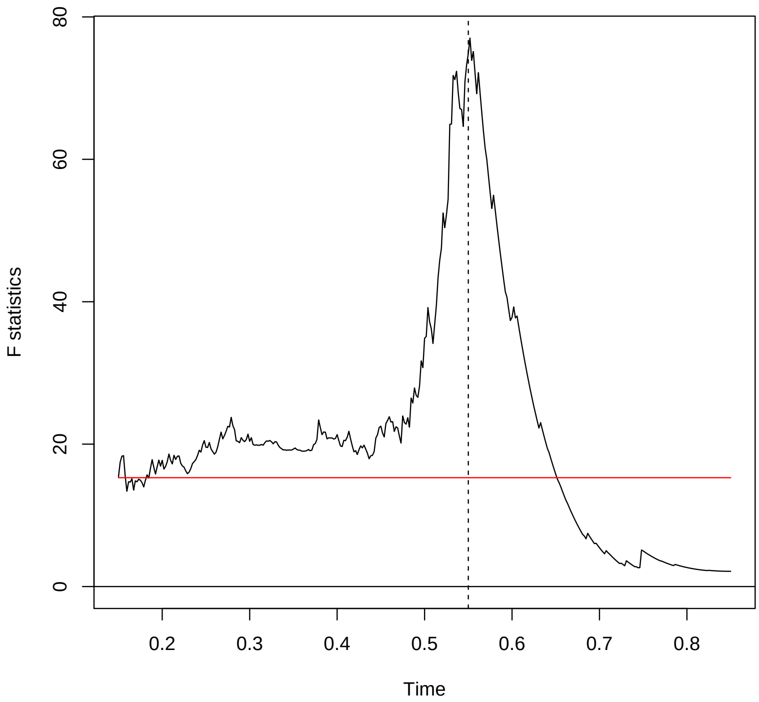


Figure 2.G.1. Time series of F statistics for a single shift hypothesis, fitted at every day in our sample. The red line gives the critical value at the 1 percent significance level. We accept the most probable break-point at the dashed line, 16th of May 2018.

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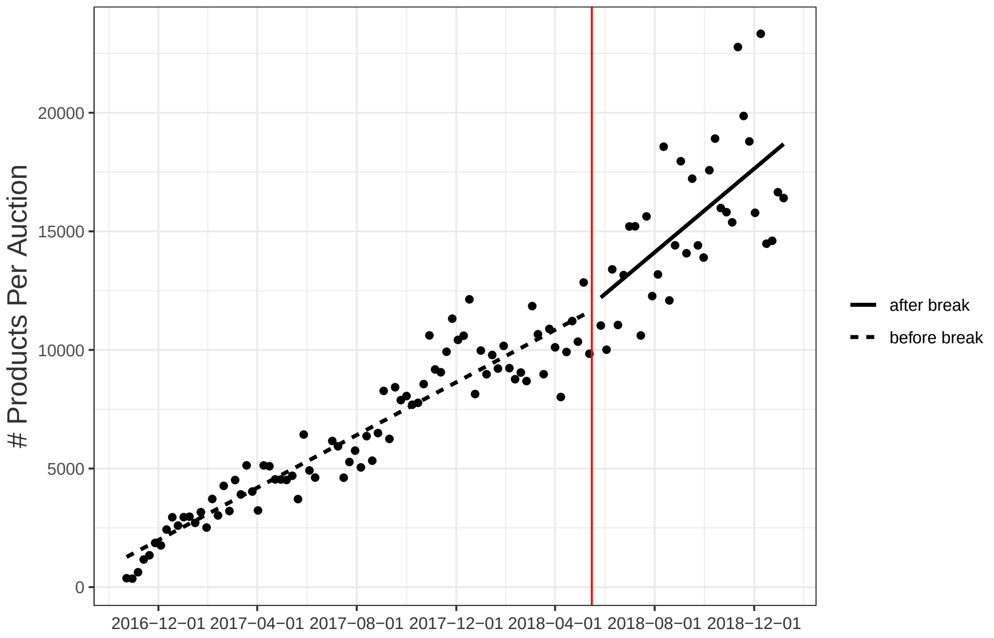
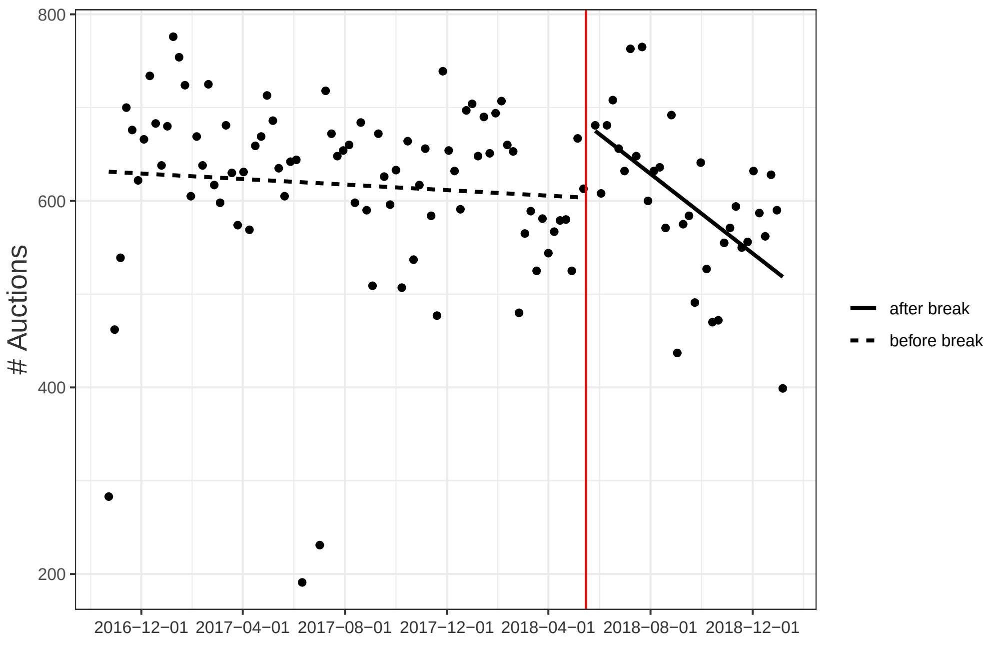


(a) Number of products per auction.



(b) Number of auctions each week.

Figure 2.G.2. Changes in number of auctions and number of products per auction to both sides of the structural break. We use weekly averages and fit a linear trend at both sides of the structural break.

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Chapter 3

Norm Prevalence and Interdependence : Evidence from a Large-Scale Historical Survey of German speaking Villages ⋆

Joint with Radost Holler

3.1 Introduction

Economic and social science research has shown that social norms [1] are an important factor in explaining cross-cultural differences in economic and political

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1. There are multiple ways to define social norms (see Legros and Cislaghi, 2020, for a cross-disciplinary review). We view social norms as informal standards of behavior within a community to which individuals in a community conform even in the presence of deviating incentives on the individual level. This definition is similar to Burke and Young (2011) and Bicchieri, Muldoon, and Sontuoso (2018). As opposed to suggestions of Bicchieri et al. (2006) and Bicchieri, Muldoon, and Sontuoso (2018), we will not distinguish between expected and actual conformity because we cannot disentangle them empirically. However, we will implicitly assume that a norm's existence also implies that it is adhered to at least to some degree.

outcomes.[2]Heterogeneity in the existence and evolution of norms is frequently attributed to large and medium-scale environmental and institutional variations. Variation in norms within cultural groups has rarely been explored because researchers are usually constraint by the available data. The available data sets have limited within cultural coverage. To overcome this issue, we present a newly digitized data set. This data set contains information on particular norms concerning religion, gender, and cooperation for up to 23,000 Central European, German speaking villages.

We first demonstrate that norms are local. That is, norms vary largely within regions even when regions are explicitly chosen to be variance minimizing. In a second step, we explore a potential mechanism underlying this local variation: within-community social relationships. We build on theories in which within-community social relationships foster norms through transmission and social sanctioning: a community that frequently interacts can transmit information, monitor norm adherence, and enact sharper social sanctions. Thus, the existence of a specific norm does not only depend on institutional and environmental variation changing the value or the need of a norm but also on community-level characteristics that determine the ability of a community to maintain a norm.

We further explore two important implications of this mechanism. First, when communities lack sufficiently dense social-relationships to implement a norm on the community-level, norms may still be fostered within better-connected subgroups, thus changing the reference group of a norm to smaller, more segmented units of the community. Second, norms that foster within-community social interactions or make community membership more beneficial, such as norms of mutually beneficial cooperation, make other norms more common on the community-level by improving norm transmission and social sanctioning. This should induce a positive interdependence between some norms, but not all.

Our analysis relies on newly digitized data from the German Ethnographic Atlas (GEA) on norms, customs, and religious denomination on the village level. The GEA was collected in the early 1930s and sampled up to 23,000 German speaking villages in Central Europe.[3] Thus, the data were collected when contacts and mobility between rural villages were low. In this setting, the naturally largest reference group for a norm and the level of observation in the data is the village community. This congruence, in addition to the dense distribution of data points, makes the data well suited for studying local variation in norms and the role of community-level social relationships. In contemporary Western societies, communities are less isolated than in the society described by our data set. Because our data contains many

2. See, for instance, McCloskey (1991), Gelfand et al. (2011), Alesina, Giuliano, and Nathan (2013), Gelfand, Harrington, and Jackson (2017), Buggle (2020), Jackson, Gelfand, and Ember (2020), and Buggle and Durante (2021).

3. Sample size varies by questionnaire and variable.

non-overlapping village communities, we can exploit local variation to a degree that would be impossible with contemporary data.

The data set contains three types of norms: cooperation norms as measured by neighborhood-help obligations, gender norms as measured by restrictions on women after giving birth, and religious norms as measured by the presence of individuals that are religiously unaffiliated. The data also contain information on the reference group for neighborhood help obligations. Obligations may, for instance, apply to the whole village, or more segmented groups, such as next-door neighbors. In addition, the data set contains community-level characteristics from which we construct three correlates of community-wide social interactions: religious heterogeneity within villages, religious heterogeneity across villages, and communal labor activities.

We conduct our analysis in several stages. We start by investigating the geographic distribution of norms by conducting a geographic cluster analysis for each domain (gender, cooperation, and religion) of norms. We choose the resulting geographic regions to minimize the within region variance in the respective variables. The cluster analysis reveals that the observed norms are widespread over the sampling area, and intra-regional variation explains a large fraction of overall variability in the existence of these norms on a community-level.

In the second step, we explore the local determinants of *norm prevalence* (the number of different norms in a village). We focus on the above mentioned mechanism, namely the role of community-level social relationships in maintaining norms. We use three indicators of community-wide social relationships: religious heterogeneity within a village, religious heterogeneity across villages, and communal labor activities. We argue that within-village religious heterogeneity is associated with lower community-wide interactions. On the contrary, heterogeneity across villages shifts social interactions towards the religiously more similar local community. Communal labor activities are voluntary production activities and primarily provide occasions for socializing and regular community gatherings. Thus, they foster interaction among community members.

Our results suggest that indeed the structure of social relationships is a driver of local norm prevalence. First, our correlates of community-wide social interactions are associated with norms in the predicted direction. Opportunities for regular community interactions increase norm prevalence, social heterogeneity within communities is associated with lower norm prevalence, and heterogeneity between groups is associated with higher norm prevalence across domains. Second, communities adapt to obstacles for community-wide social interaction by changing the reference group of norms. Third, cooperation norms that increase the intensity of social interactions within a community are associated with a higher prevalence of norms unrelated to cooperation, while these other norms are insignificant or negatively related to each other.

While our results are only correlational, they form a coherent picture in line with theories on norm transmission and social sanctioning. There are two major challenges to our interpretation: (1) unobserved variables might affect the prevalence of norms and at the same time determine community characteristics and social relationships; (2) reverse causality. Our results continue to hold when accounting for narrow geographical fixed effects (20 km × 20 km) and political boundaries. Consequently, remaining confounders vary within these grid cells and affect norm measures as well as determinants of social relationships. We try to control for ruralness as one likely candidate. We further discuss the issue of unobserved heterogeneity, internal validity and reverse causality in greater detail when interpreting our results.

Our work is related to the literature on the role of social sanctioning in establishing cooperative behavior and norms. The theoretical strand of this literature defines norms as equilibria in (repeated) social dilemma games. In these theories, cooperative equilibria are maintained by (off equilibrium path) sanctioning of deviant behavior and social monitoring (see, e.g. Schelling, 1958; Ullmann-Margalit, 1977; Kreps et al., 1982; Axelrod, 1986; Kandori, 1992; Coleman, 1994; Aoki, 2001; Genicot and Ray, 2003). This literature's empirical strands focus on lab and lab-in-the-field experiments to analyze conditions amenable to cooperation, despite the threat of free-riding. It shows that altruistic punishment of uncooperative behavior is frequent when available to the individual and that the availability helps to sustain cooperation (see Fehr and Fischbacher, 2004, and references therein). It further highlights that repeated interaction, monitoring, and stable social network ties are key in maintaining cooperation at high levels (see, e.g. Fehr and Gächter, 2000; Duffy and Ochs, 2009; Rand, Arbesman, and Christakis, 2011; Chandrasekhar, Kinnan, and Larreguy, 2018). We extend this literature by showing that these mechanisms are not only key for maintaining cooperation, but also for maintaining other norms prevailing in communities. Further, we argue that these mechanisms imply a direct interdependence between cooperation norms and other norms. Both of these implications are important for policy design. Strengthening cooperation norms can strengthen other norms within the same communities. Thus, when evaluating the welfare improvements of these type of interventions, this potential side-effect should be considered.

In addition, our research contributes to the literature on cultural tightness, which studies the prevalence of norms across domains (Gelfand et al., 2011; Gelfand, Harrington, and Jackson, 2017; Jackson, Gelfand, and Ember, 2020). Tight cultures have a higher prevalence of norms and higher levels of conformity. According to this literature, variations in cultural tightness are related to differences in social or ecological threat. Jackson, Gelfand, and Ember (2020) finds that cultural tightness is not domain-specific. That is, the prevalence of norms across domains is correlated. Jackson, Gelfand, and Ember (2020) attribute this to spillovers across domains. We contribute to this literature by examining social relationships and social sanctioning as another possible mechanism. Our results differ from Jackson, Gelfand, and Ember (2020), because we do not find a general positive complementarity in norms across domains. In our context the positive complementarity seems to be limited to cooperation norms only.

Besides this broader contribution, we contribute to the literature about social heterogeneity and cooperation (Varughese and Ostrom, 2001; Miguel and Gugerty, 2005; Alexander and Christia, 2011; Hoang, Pasquier-Doumer, and Saint-Macary, 2021). In particular, we analyze the dimension of religious heterogeneity, which has been under-represented in this literature. Further, we expand on Posner (2004) and investigate the effects of heterogeneity at different levels. We find that heterogeneity between communities increases norm prevalence while heterogeneity within communities decreases norm prevalence. These results are consistent with the theory that differences between groups foster in-group cohesion.

Finally, our data addresses a lack of data lamented in the literature on collective action and ethnographic data in historical economics (Poteete and Ostrom, 2004, 2008; Lowes, 2020). Lowes (2020) notes that while ethnographic datasets can be very useful for economic historians, currently available datasets have several shortcomings. Existing datasets are compiled from many ethnographies that might use differing definitions. These data sets' patchwork nature makes the resulting data less systematic and hides variation within pre-defined cultural boundaries. Such data sets also include very few European data-points. The data, we digitized, contributes towards filling those gaps. In particular, the village-level data allows for geographically narrow fixed effects to partially account for institutional and geographical variation.

We introduce our data in section 3.2. Section 3.3 uses cluster analysis to describe large-scale spatial patterns in norm prevalence. Starting with the theory section 3.4 we shift perspective and focus on social relationships as a local determinant of norm prevalence. In this section we describe our conceptual framework and empirical predictions. In section 3.5 we test these prediction. Section 3.6 concludes.

3.2 Data

We use a newly digitized data set containing the results of the German Ethnographic Atlas (GEA) collected by anthropologists between 1930 and 1935. The data consists of five questionnaires, each containing different questions. In total, the GEA contains 243 questions. With a sample of more than 20,000 German Speaking communities, the project pioneered the concept of an Ethnographical atlas (Schmoll, 2009). The aim of this data collection was to capture rural culture before its transformation caused by industrialization (Schmoll, 2009, p. 236-238).

The target population of the GEA consists of German speaking villages that have at least one school.[4] We compare the coverage of the GEA with the official number of

4. Note that even though the aim was to capture rural life, the final sample also contains cities such as Hamburg, and part of cities such as Berlin-Charlottenburg or Berlin-Spandau.

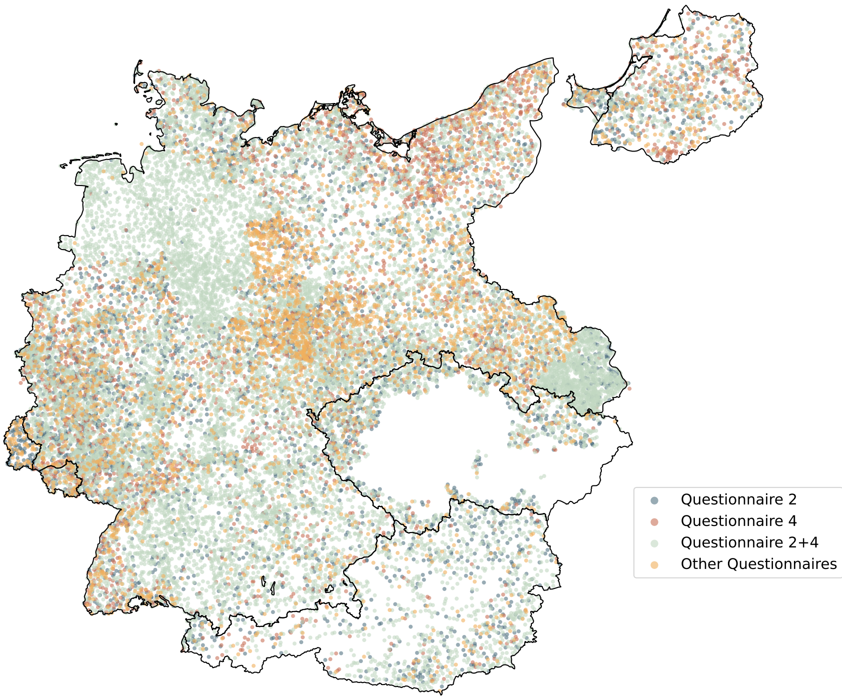
municipalities in a region in Appendix 3.A. For each sample village, researchers recruited volunteers to fill out the questionnaires for one or multiple villages (Kehren, 1994). We display characteristics of respondents in the Rhine Province digitized by Kehren (1994) in Appendix 3.B. The questions asked about customs in a village, and not about the individual behavior of the respondent. The topics of the survey range from agricultural production, food, festivities, folklore, religious and profane rituals, to marriage customs, and norms concerning varying areas of life. We focus on the questions about norms, communal labor, and religion. Cooperation norms as well as particular norms affecting women were asked in questionnaire 4 collected in 1933[5]; communal labor was surveyed in questionnaire 2 collected in 1931, and religious composition of the villages was asked in every questionnaire (1930-1933).

The researchers behind the GEA collected their data mainly in the German Reich in its inter-war borders (1919-1939) including the Saar Region, Gdansk, the Czech part of Czechoslovakia, Luxembourg, Liechtenstein and the First Austrian Republic.[6] Some questionnaires were only asked in some regions, and some data is only fragmentally included in the published materials.[7] As we attempt to analyze spatial variation, we exclude data points that are geographically comparably isolated because of the fragmented sampling within the region. In consequence, we only include answers from the German Reich, the first Austrian Republic, the Czech part of Czechoslovakia, Luxembourg, Liechtenstein, and Gdansk in this study.

5. We discuss the relationship between the GEA researchers and the national socialist government in Appendix 3.A.

6. More fragmented attempts at data collection were conducted in Slovakia, Transylvania, Bessarabia, Banat, Lorraine, Klaipėda Region, Switzerland, Poland, German speaking parts of Belgium, regions ate the German-Dutch border, and Denmark. Sampled villages and regions are taken from the official list of villages provided in the GEA. This contains only the list of villages included in questionnaires one through four. Not all regions participated in all questionnaires.

7. Switzerland and Lorraine were only included in the questionnaire 1, Luxembourg only in the questionnaire 1 and 2. Border region with Denmark is only available in questionnaire four. Data collection in the Polish corridor was conducted covertly (Schmoll, 2009, p. 88) and is only partially documented in the official list of villages.



Notes: Sample restricted to villages included in questionnaires two or four in the German Reich, Austria, Czech part of Czechoslovakia, Liechtenstein, Gdansk, and Luxembourg.

Figure 3.1. Data points by questionnaire.

Figure 3.1 displays the geographic distribution of data points of the questionnaires. We indicate each surveyed village with a point on the map. We display Villages surveyed in both, questionnaire 2 and 4, in green, villages only surveyed in questionnaire 2 in blue; villages only surveyed in questionnaire 4 in red and those from other questionnaires, namely 1 and 3, and that are not included in questionnaire 2 and 4 in orange. [8] Most observations lie in the German Reich (15,096 of questionnaire 2, 14,540 of questionnaire 4, 15,799 of the other questionnaires). The data set covers the whole area of the German Reich. The majority of observations outside the German Reich are in Austria and Czechoslovakia. Both contain more than 1,100 observations from questionnaire 2 and the other questionnaires and between 868 and 901 from questionnaire 4. While Austria is fully covered, the observations

8. These additional observations come from the religion questions that were asked in all questionnaires. We use them for descriptive analyses concerning religion.

in Czechoslovakia are clustered around the border to the German Reich, reflecting German settlement patterns. For an overview of sample sizes by region see Table 3.A.1 in Appendix 3.A.

The samples of the questionnaires mostly overlap. As Figure 3.1 shows, questionnaire 2 has more observations in Austria, and Czechia while questionnaire 4 contains more observations in the Northeast of the German Reich. The majority of observations (13,818) are contained in both questionnaires. Luxembourg was not covered in questionnaire 4 with the exception of one village. The number of observations decreases from questionnaire two to questionnaire four by roughly 1,200 observations. This decrease is concentrated outside of the German Reich.

As the questions were open and not ratings or multiple-choice, the answers ten to be texts with varying details of background explanations.[9] The answers were transcribed onto answer cards by the researchers. After WWII, a group of anthropologists additionally categorized the answers to a subset of questions, among which are the questions about local cooperation and norms (Schmoll, 2009). We rely on their very broad categorizations of the raw data. We provide additional information on our digitization procedure in Appendix 3.H.

3.2.1 Measuring Norms

Cooperation Norms. Our measure for the prevalence of cooperation norms is the number of activities with which community members are obligated to help their neighbors. Neighborhood help as a common cooperative activity of historic village communities is also documented in Weber (1922), Kramer (1954), and Wurzbacher (1961). We use answers to the following (translated) survey question in order to quantify the extent of neighborhood help and the structure of obligations.[10]

a) In your village, are neighbors traditionally obligated to mutual assistance?

b) At which occasions of family life, like childbirth (e.g. care for women in childbed), weddings (e.g. help in the kitchen), illness (e.g. night watch), death (carrying the coffin, digging the grave)

c) for which economic tasks, like harvests, building a house (transporting wood) etc.?

d) to whom do these obligations apply?

e) to whom, who isn't a neighbor do these obligations apply?

9. Some were even essay-like answers (Zender and Wiegelmann, 1959).
10. In the German original the survey question is given by:

a) Sind in ihrem Ort die Nachbarn noch von alters her zu gegenseitiger Hilfeleistung verpflichtet? b) Bei welchen Anlässen des Familienlebens , wie Geburt (z.B. Pflege der Wöchnerin), Hochzeit (z.B. Hilfe in der Küche), Krankheit (z.B. Nachtwache), Tod (Tragen des Sargs, Graben des Grabs)? c) bei welchen wirtschaftlichen Arbeiten, wie Ernte, Hausbau (Anfahren des Bauholz) usw.? d) Für wen gelten diese Nachbarschaftspflichten? e) wer sonst ist dazu verpflichtet ohne Nachbar zu sein? (Zender and Wiegelmann, 1959, p. 30)

To quantify the degree of neighborhood obligations, we rely on answers to part (a)-(c). Barruzi-Leicher and Frauenknecht (1966) pre-categorized the answers to the question. Their categories are displayed in Figure 3.C.1 in Appendix 3.C.1. We summarize the specific obligations under three coarser categories: help at death, help with weddings, help with (re-)building a house, and help at birth or sickness. While we use an aggregated measure for neighborhood help in our final analysis, we use this broader categorization to describe the spatial patterns of neighborhood help and the heterogeneity hidden behind this aggregated measure.

While most communities only prescribe neighborhood obligations in one specific activity, more than a third require neighborhood help in more than one area and multiple activities. The relative frequencies of each aggregated sub-type of neighborhood help are shown in Table 3.1. For a bar-chart of the disagregated data, see Figure 3.C.1 in Appendix 3.C.1. More than 50% of villages prescribe some neighborhood obligation. With 45% of communities prescribing it, neighborhood help at death is the most common neighborhood help obligation. In 39% of communities, neighbors are obligated to help with (re-)building a house; in 24% neighbors help each other at weddings. Help at birth or sickness is with 3% of villages less frequent. On average, villages have 1.4 neighborhood obligations.

We use the answers to parts (d)-(e) of the neighborhood help survey question to measure neighborhood segmentation. These sub-questions explicitly ask about the reference group for neighborhood-help. These answers were grouped into 42 categories by Barruzi-Leicher and Frauenknecht (1966). All 42 categories with their corresponding sample frequency are displayed in Figure 3.C.2 of the Appendix. We focus our analysis on answers that indicate that the *whole* village is obligated to mutual help. These answers indicate to which degree a village is segmented into particular subgroups. Measuring segmentation into sub-groups allows us to analyze endogenous adaptations of the community to external factors such as religious heterogeneity. In our analysis we use an indicator whether neighborhood help is conducted at the village level (*Unsegmented Neighborhood*). Out of the villages that conduct neighborhood help, 17% do so as an unsegmented village (see Table 3.1).

Gender Norms. The GEA asks about norms that apply to women in the weeks after birth, also called '*Wöchnerin*' which can be translated as woman in childbed. The birth of a child used to be surrounded with behavioral rules for the new mother in Europe (see e.g. Nowottnick, 1935; Labouvie, 1992).[11] The GEA contains the first quantitative assessment of the prevalence of these norms. The question reads as follows:

a) Where is the woman in childbed not allowed to go before her first churchgoing? (e.g. basement, attic, barn, well, neighbor)

11. The time period that new mother was considered to be a 'Wöchnerin' was usually between 2-4 weeks (max. 40 days) after birth and was oftentimes connected to the time a new mother was not allowed to go to church (Nowottnick, 1935).

Table 3.1. Summary statistics: norms

	N	Share	Mean	Std	Min	p25	p50	p75	Max
N. Nbh. Help	16,467		1.37	1.44	0	0	1	2	8
Type of Help									
Death		0.45							
Building house		0.39							
Wedding		0.24							
Birth/Sickness		0.03							
Unsegmented Neighborhood	10,088	0.17							
N. Childbed Norms	14,927		0.74	0.95	0	0	0	1	9
Type of Norm									
Both		0.10							
Protective		0.23							
Impurity		0.15							
None		0.51							
Dissidents	22,967	0.08							

Note: For categorical and dichotomous variables, table displays shares. For continuous variables mean, standard deviation, minimum, 25th, 50th, 75th percentile and maximum are reported. Gender norms have a lower number of observation because part of the data was destroyed in the war, for more details see Appendix 3.H. N. Nbh. Help = Number of neighborhood help obligations; N. Childbed Norms = Number of childbed norms.

b) Which boundary is she not allowed to pass? (e.g. gutter, street, crossroad, village border)

c) Which other traditional precautions does the women in childbed follow?[12]

There are two different hypothesized origins of these behavioral rules. On the one hand, general behavioral rules for the women in childbed can be found in the old testament and are related to beliefs about womens' impurity after birth.[13] On the other hand, the rules usually prescribed are not related to the rules prescribed in the old testament. Additionally, both the Protestant church as well as the Catholic church have not stipulated rules related to the women in childbed at least since the 17th century (Grober-Glück, 1977). A different approach explains the existence of some of these rules by their protective function for women in the vulnerable weeks after birth. They are hypothesized to function as an early maternity leave and protect women from hard work, they would have to do otherwise (Grober-Glück, 1977; Arx, 1978). Grober-Glück (1966) analyzed the original answers of the question regarding norms that apply to the women in childbed. Her categorization yields 93 different subcategories. The translation of each subcategory including the answer frequency can be found in Tables 3.C.1-3.C.2. She additionally divides the rules according to their likely function or origin. She argues that rules that are connected to the belief that women in childbed bring harm, such as "Do not attend a public event [...] because it will cause a dispute or fight" are likely connected to the impurity notion, while rules such as "The woman in childbed should not mend, knit, spin [...]" likely function as protection of the young mother's health. We categorize her subcategories into *Impurity* and *Protective* norms accordingly.

In appendix 3.C.2, we regress *Impurity* and *Protective* norms child mortality, ratio of female to male labor force participation, and the ratio of female to male mortality on the county level. We, indeed, find different associations for each category. Protective norms are more prevalent in regions where there is relatively higher female mortality and relatively lower child mortality, while impurity norms are not significantly associated with relative female mortality, but positively associated with child mortality (see Table 3.C.3). This seems to support the categorization of Grober-Glück (1966). Since both the regression results as well as the pre-existing categorization

12. Original German: a) Wohin darf die Wöchnerin vor dem ersten Kirchgang (Aussegnung) nicht gehen? (z.B. Keller, Boden, Stall, Brunnen, Nachbar) b) Welche Grenze darf sie nicht überschreiten? (z.B. Dachtraufe, Gosse, Straße, Kreuzweg, Dorfgrenze) c) Welche besonderen altherkömmlichen Vorsichtsmaßnahmen beachtet die Wöchnerin sonst vor dem ersten Kirchgang?

13. "A woman who [..] gives birth to a son will be ceremonially unclean for seven days, just as she is unclean during her monthly period. [...] 4 Then the woman must wait thirty-three days to be purified from her bleeding. She must not touch anything sacred or go to the sanctuary until the days of her purification are over. If she gives birth to a daughter, for two weeks the woman will be unclean, as during her period. Then she must wait sixty-six days to be purified from her bleeding." (Leviticus 12)

of norms indicate that impurity and protective norms are two distinct groups we also distinguish between these groups in our analysis.

Table 3.1 displays the relative frequency of each norm type. Note that the number of observations of childbed norms is lower than those of neighborhood help obligations because some part of the data was destroyed in World War II or deemed unreadable. 49% of villages have at least one norm or custom regarding the woman in childbed.[14] 10% of villages have both a protective norm and an impurity norm. 23% of villages only display protective norms, 15% of villages have only an impurity norm. On average, a village displays 0.74 restrictions for women in childbed. The distribution is highly right skewed with the 75th percentile being one norm and the maximum being nine.

Religious Norms. All questionnaires asked about the religious composition of the village. Accordingly, respondents also indicated whether there were dissidents, i.e., they did not belong to any major religious denomination.[15] We interpret this as a measure of deviations from religious norms since Protestants and Catholics require adherence to their dogma, and dissidents by definition reject that norm. Nine percent of villages are home to dissidents. However their number as a fraction of the total population mostly remains below 5 percent (see Figure 3.C.3 in the Appendix) (Grober-Glück, 1966). Because the information on the population share of dissidents is imprecise, we use an indicator variable which is one if dissidents are absent.

3.3 Spatial Dependence of Norms

To analyze the geographic distribution of the norm measures (neighborhood obligations, norms regarding women in childbed, dissidents), we conduct a regionalization analysis (geographic clustering) separately by each norm domain (gender, religion and cooperation).[16] If the prevalence of norms is mostly driven by medium to large scale environmental, political or economic factors that shift the need or value of norms, we would expect that clear cut regions emerge that explain a large part of

14. The frequency of a particular subcategory mostly ranges from below 0.5% to 3%. A notable exception builds the rule: 'do not visit the neighbor' which is prevalent in 15% of all settlements.

15. Starting with the reformation, the term dissident changed its meaning from protestant, to being a member of a catholic or protestant sect, to being an atheist (Dehli, 2001). In 1910 the Prussian statistical office defined a dissident as a person that does not belong to any official denomination (Dehli, 2001) (In contemporary parlance Konfessionslose / religious "nones"). The GEA's definition includes Atheists as well as members of smaller religious sects. All definitions point to the dissident as a person who rejected mainline religious dogma and religious norms as the protestant or catholic church represented them.

16. An alternative way to look at spatial dependence is to investigate the spatial autocorrelation in our norm measures. Spatial autocorrelation is also a necessary condition for regionalization to work well. If variables are not at all spatially autocorrelated, will not be able to explain any variation in the variables because values are randomly distributed across space. Table 3.D.1 shows that all of our norm measures display significant spatial autocorrelation.

the overall variability in norms. If, however, norms are also strongly influenced by locally varying factors, such as community-level characteristics, we would expect that clusters can only explain little variation in norms. That the intra-regional variance should remain high.

Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Given the number of clusters, the algorithm chooses clusters such that the sum of squared differences in the input variables within all clusters is minimized given the connectivity constraint. The connectivity constraint is given by the geographic distribution of observations. We define each point to be connected to its four nearest neighbors in our data set (see Figure 3.D.1). The connectivity constraint ensures that a point can only be added to a cluster if it is directly connected to another point in that cluster. As a result, points within clusters are all geographically connected.

The German Reich was geographically separated by Poland in the East, and some points lie isolated around Prague. The four nearest neighbors connectivity matrix yields three disconnected components in all cases.[17] We perform the cluster analysis only on the largest component which comprises the main land of the German Reich, Austria, Luxembourg, Liechtenstein and the border region of Czechoslovakia. We treat East Prussia as an exogenously given geographic cluster.

We do not have an ex-ante prior about the number of clusters. Instead, we determine the number of clusters by eyeballing the Calinski-Harabasz metric for three to 50 clusters. The Calinski-Harabasz metric is the ratio of within to between cluster dispersion. In neither of our cluster analyses, a clear elbow emerges. However, the gain from including an additional cluster above ten clusters tends to be very low. Increasing the number of clusters chosen by one or two only yields very small subregions of existing regions – the tendency of results thus tends to stay rather stable. A robustness check in which we double the number of chosen clusters is contained in Appendix 3.D.3.

Norms in different norm domains (gender, cooperation, religion) are likely affected by different environmental factors. We analyze each norm domain separately to account for these differences. Joint clustering, which forces the same regions on all norm domains may smooth clusters across those environmental factors and thus pushing down the overall explanatory of the clusters. As a result, clusters for different domains are incongruent. In an additional robustness check in Appendix 3.D.4, we further segregate our norm measures and conduct a separate cluster analysis for each variable. The overall picture and the explanatory power of the resulting clusters remain similar.

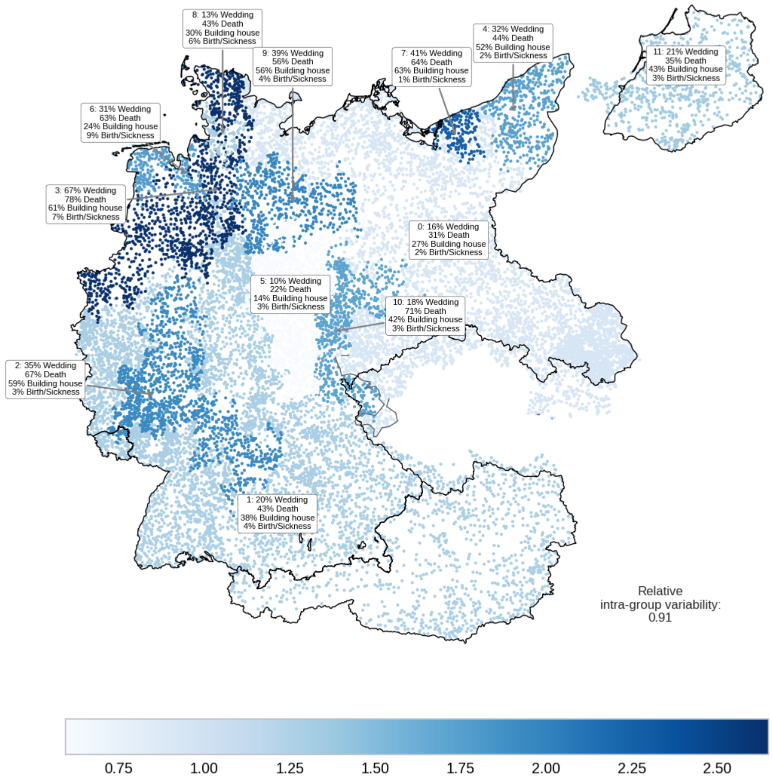
17. We exclude isolated points lying in and around the center of the Czech part of Czechoslovakia, the island Helgoland. When using only variables available in questionnaire 4, we additionally exclude the area around Berlin because it is not connected to the remaining points according to the connectivity matrix.

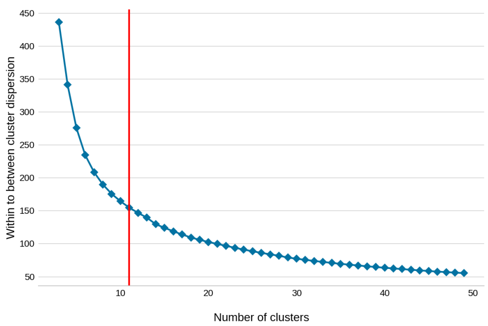
We start by analyzing the geographic distribution of our cooperation norm measures. We use the four underlying indicator variables: help at wedding, death, house building and birth/sickness, as our input variables to the clustering algorithm. The results are displayed in Figure 3.2. We choose eleven clusters in addition to the exogenous cluster of East Prussia according to the Elbow Graph depicted in Figure 3.2a. We calculate the average number of the sum of neighborhood help obligations per village for each cluster. The points which belong to a cluster with a high average number of neighborhood help activities are colored in a darker shade of blue.

Each cluster contains a large share of villages that have at least one neighborhood obligation (see Figure 3.2b). Neighborhood help obligations are strongest in the Northwest and weakest in the eastern and center of the main land of the German Reich. Clusters in the Northwest tend to display a higher prevalence of neighborhood obligations across all activities. Only the small cluster 7 in the Northeast matches the level of neighborhood activities of this region. Neighborhood help obligations are also relatively less pronounced in the South. In Cluster 1, which covers Baden-Wurttemberg, Bavaria, Austria as well as the Saar Region, south-western part of the Rhine province and part of the center, all types of neighborhood obligations are prevalent but on average each village performs slightly more than one activity. Thus, there is some regional variation in the prevalence of neighborhood help. However, it is neither universal nor non-existent in either region. Instead, all type of neighborhood obligations are widespread across the sampling region. Accordingly, the explanatory power of the cluster regions is low. The intra-cluster variability is still approximately 91% of the overall variability in the underlying variables. Further, cluster regions do not display clear-cut boundaries and do not strongly coincide with political borders. Most cluster regions have fuzzy edges in which villages of neighboring clusters are intermingled with each other.

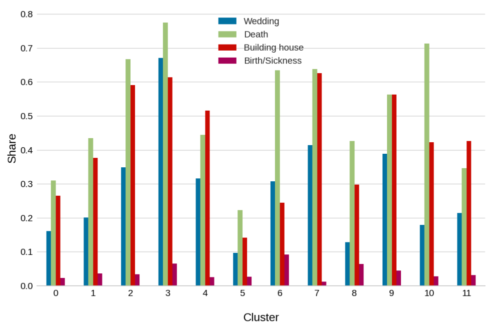
Next, we turn to the cluster analysis of our gender norms. As input, we use the two different indicator variables of gender norms: impurity norm, and protective norm, explained in previous section. The results are displayed in Figure 3.3. The resulting regions are colored by the average number of childbed norms each cluster has. Note that in the southern center there is a connected region for which the original answer cards of the data regarding these norms were destroyed or partly unreadable. We hence have to exclude these villages. As can bee seen in Figure 3.3a no clear ellbow emerges, however, above ten clusters an additional cluster does not capture a lot of additional variation. Thus, we choose ten clusters in addition to the disconnected component of East Prussia.

Every resulting region contains a large share of villages that have some gender norm regarding the woman in childbed. However, some geographical patterns emerge. Impurity norms are relatively most prevalent in the North, and center, while protective norms are most prevalent in the center and the South. As both impurity as well as protective norms are wide-spread in the center, the center region (clusters 2, 3, 5 and 8) displays the highest average number of childbed norms per village.





(a) Elbow graph



(b) Shares by cluster

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Variables that are clustered: Help at wedding, death, building a house, birth/sickness. Help at birth and sickness is not available separately. Number of clusters=11 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables. Elbow graph is based on the Calinski-Harabasz metric, which gives the ratio of within to between cluster dispersion. Red line indicates the number of clusters used.

Figure 3.2. Clustered cooperation norms

Despite this rather consistent picture, the clusters explain little variation (90% of overall variability) and are incongruent with political and religious boundaries as well as the clusters from our analysis of cooperative norms.

Last but not least, we investigate the spatial dependence in our religious norm measure, namely the existence of dissidents in a village. With only 8% of villages, villages with dissidents are rare. We depict the distribution of villages with dissidents on a map in Figure 3.4a. This map shows that these villages occur in all regions. However, they are relatively more concentrated in the center-east of the German Reich (Thuringia, Saxony, Anhalt) and Bohemia. Because villages that have dissidents are rare, and are rather evenly distributed even within regions in which they are less rare, the explanatory power of clusters obtained by means of cluster analysis remains low. When choosing eight clusters – which is the closest we can get to an elbow (see Figure 3.4b) –, the intra-cluster variability is 91% of the overall variability.[18] As the resulting clusters hide the underlying spatial distribution in the variable, we display the resulting clusters only in the Appendix (see Figure 3.D.2).

We conclude that across domains and norms, intra-regional variation does not explain a large chunk of the variation in norms even if regions are explicitly chosen to be variance minimizing. In addition, emerging regions rarely coincide with institutional boundaries. Thus, the existence of particular norms seems to depend to a large degree on local factors. In the next section, we will present one of such local factors that can help explain the local variation in norms across communities, namely the structure of community-level social relationships.

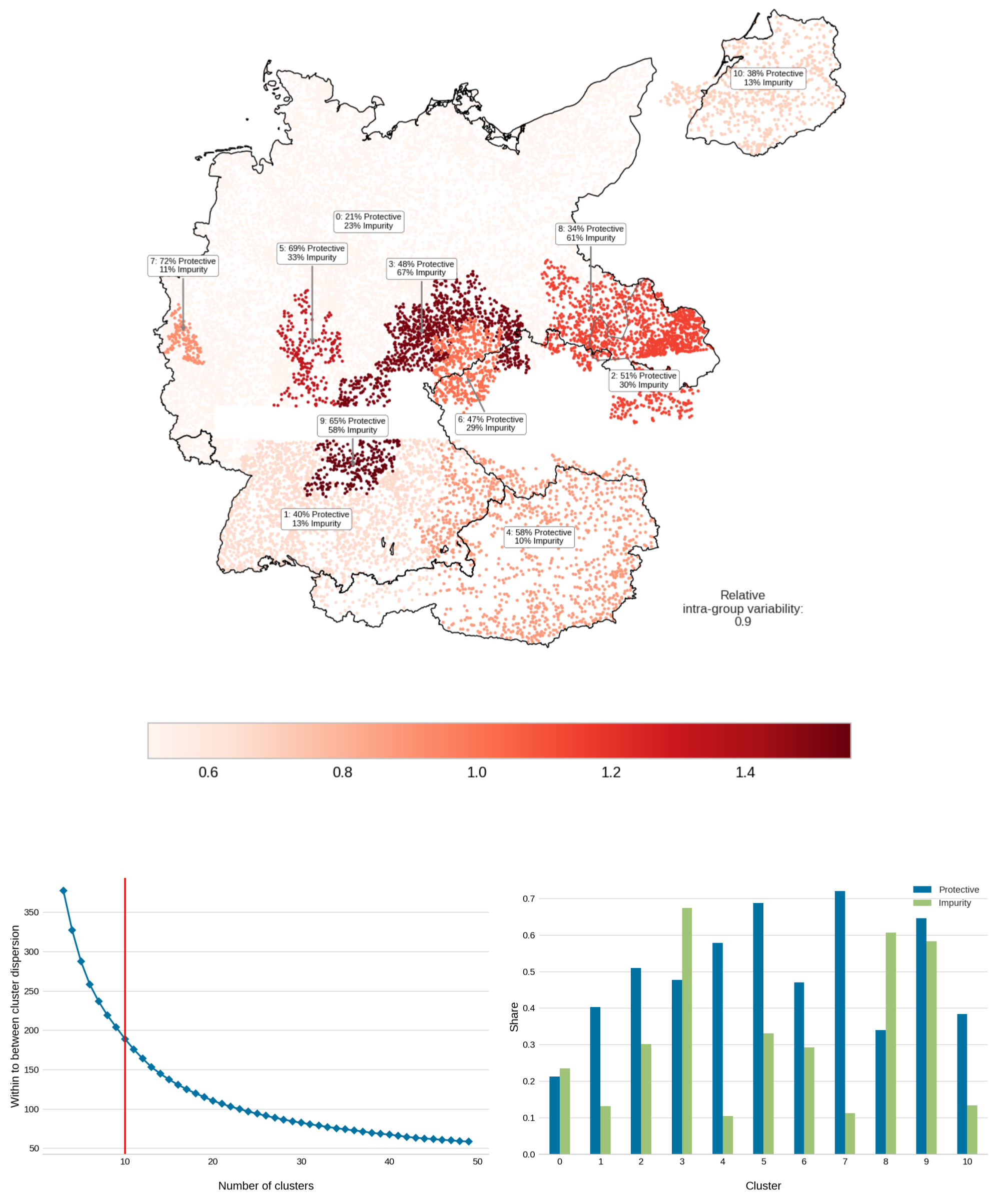
3.4 Conceptual Framework

As we observe in the previous section, macro-level institutions are unlikely to account for a large share of heterogeneity in norm prevalence. Thus we shift our focus towards micro-institutions enacted through social relationships. Social relationships provide a community with two ways of maintaining norms: social sanctioning and norm-transmission. In the first part of this section, we will explain these two ways in more detail. In the second part, we map the theoretical arguments and concepts to empirical predictions for our data. In this discussion, we argue that:

(1) norm following is costly on the individual level,
(2) community members punish deviations from gender and religious norms by excluding them from neighborhood help, and
(3) social norms are transmitted through regular social interactions.

Social sanctioning of deviations enables a community to enforce its social norms. Wurzbacher (1961) reports multiple forms of social sanctions, such as talking badly

18. For reference, when using the 44 states and Prussian provinces as exogenous clusters, the intra-region variability is 93% of the overall variability.

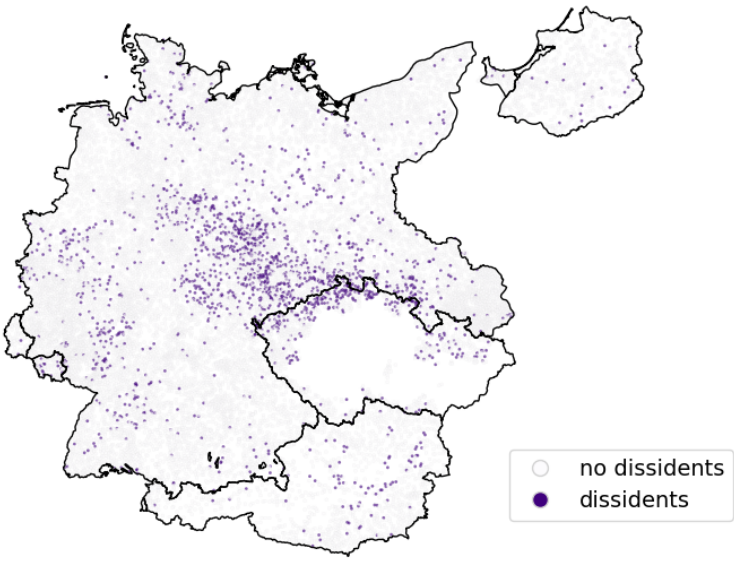


(a) Elbow graph

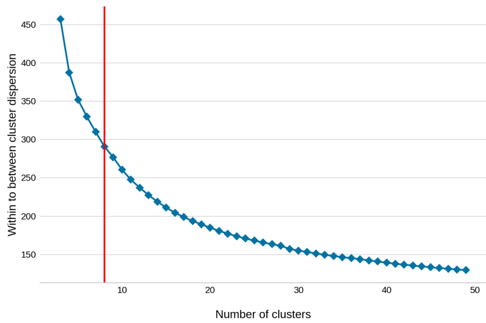
(b) Shares by cluster

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Variables that are clustered: *Impurity norm*, *Protective norms*. Number of clusters=10 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables. Elbow graph is based on the Calinski-Harabasz metric, which gives the ratio of within to between cluster dispersion. Red line indicates the number of clusters used. White space in the center are regions in which the data on women in childbed are partly destroyed.

Figure 3.3. Clustered gender norms



(a) Geographic Distribution of Dissidents



(b) Elbow graph

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors).

Figure 3.4. Geography of religious norms

about someone, not greeting them, confronting them, shunning them, excluding them from neighborhood help, boycotting them economically, or ostracizing them . These sanctions deny a community member access to at least some of the benefits flowing from within-community social relationships.

According to social collateral theory increasing an individual community member's benefits from their relationships to other community members increases norm prevalence by increasing social sanctions' efficacy. A community member decides to follow a norm if the present and future norm adherence costs are lower than the costs of being sanctioned. This trade-off shifts towards norm-adherence if the value of social relationships rises. Social collateral theory is a common element of repeated game and social network models of norm enforcement, informal insurance, or public goods provision (e.g. Akerlof, 1976; Basu, 1986; Kandori, 1992; Besley and Coate, 1995; Aoki, 2001; Bowles and Gintis, 2002; Genicot and Ray, 2003; Bloch, Genicot, and Ray, 2008; Ambrus, Mobius, and Szeidl, 2014; Ali and Miller, 2016; Chandrasekhar, Kinnan, and Larreguy, 2018).

Social sanctioning may be more effective within subgroups than on the community-level if social relationships within subgroups are more developed than those across subgroups. A person that deviates from a norm suffers more from punishment by people with whom they interact regularly. If a community contains a religious minority that interacts less with the broader community, the community is worse at enforcing norm adherence from members of that sub-group.

If norms have to apply uniformly to the whole community, the existence of that sub-group lowers community-wide norm prevalence. For norms such as neighborhood help, the community can counteract this tendency and adjust the reference group to which the norm applies. For example, a heterogeneous neighborhood might split into homogeneous sub-groups for neighborhood-help (segmentation). This segmentation of the reference group mitigates the problems stemming from the limited possibility for community-level social sanctioning.

Norm Transmission links social ties to norm prevalence. This mechanism can be distinguished into three parts: transmitting information about deviations, coordinating on the norm, and transmitting the norm through socialization. Transmitting information about deviations from the norm directly enables social sanctioning. For deviations to be sanctioned, they have to be known (Genicot and Ray, 2003; Carpenter, Kariv, and Schotter, 2012; Ambrus, Mobius, and Szeidl, 2014). Additionally, information transmission facilitates belief coordination among community members. Thus, even if norm adherence were not costly to the members, it influences norm prevalence by helping to coordinate on the equilibrium where the norm is adhered to.[19]

19. see Bicchieri et al. (2006) and Bicchieri, Muldoon, and Sontuoso (2018) for the role of beliefs and norm adherence in coordination games.

If within-community social relationships are robust, norm transmission through socialization favors having many community norms. A community that has more inward-facing social relationships compared to outward facing social relationships can maintain cultural practices for a longer time because it is less receptive to outside influences. This orientation towards the local community facilitates community norm transmission through role models from a different generation and peers from the same generation. First, community relationships increase local families' interaction with other local families, leading to a higher scope of oblique transmission of local norms and customs to the next generation.[20] More within-community relationships increase the chance of being exposed to a role model from the local community instead of an outsider (see e.g. Brueckner and Smirnov, 2007; Panebianco, 2014; Patacchini and Zenou, 2016; Panebianco and Verdier, 2017, for the role of social networks in the transmission process). Secondly, within-community relationships also facilitate norm transmission among peers (see Burke and Young, 2011; Jackson, 2011, and references therein).

The theories laid out in the preceding paragraphs provide a common framework for explaining norms in all areas of life. We can use the GEA to analyze three major predictions of these theories.

Prediction 1. Intensive and valuable social interactions facilitate social sanctioning and norm transmission and thus increase the prevalence of all norms. The GEA contains three types of norms in different domains of life: cooperation norms measured by neighborhood help obligations, gender norms measured by restrictions for women in childbed, and norms regarding religious dogma as measured by the existence of religious dissidents in a village. When we speak of increasing norm prevalence, we mean that all of these norms are more likely to exist.

Prediction 1: Factors that influence the frequency and value of social interactions within a community influence the prevalence of all norms within that community: factors that make interaction more frequent or increase their value make the prevalence of all norms more likely; Factors that make interaction less frequent or decrease their value make the prevalence of all norms less likely.

We can construct three factors that influence community wide social relationships from the GEA: community gatherings in the form of communal labor, within village social heterogeneity in the form of religious heterogeneity within a village, and across village heterogeneity in the form of religious heterogeneity across villages. Column 3 of Table 3.2 and the following paragraphs illustrate of how these measures should map into the prevalence of all norms according to *Prediction 1*.

20. Empirical evidence on the role of the social environment on the inter-generational transmission is provided by Dohmen et al. (2012). Henrich and Broesch (2011) measure transmission networks on a Fijian Island. In addition to an individual's prestige, its proximity to the child in question makes her more likely to be selected as a role model in the cultural transmission process.

Table 3.2. Theoretical predictions for the association between correlates of social relationships and norm prevalence as well as segmentation.

Concept	Operationalization	*Prediction 1* Norm Prevalence	*Prediction 2* Village Nbh.
Community Gatherings	Communal Labor	Positive	Positive
Within Village Heterogeneity	Religion	Negative	Negative
Heterogeneity Between Villages	Rel. diff. 4NN.	Positive	Positive

Notes: Norm prevalence stands for the existence of cooperation norms, gender norms, and norms regarding religious dogma. Village Nbh. is an indicator variable that is 1 if neighborhood obligations apply to the village level and 0 if they apply to only a subgroup of the community.

Community gatherings. With communal labor, the community meets at a specific place and works. Common communal labor activities are processing poultry or produce, or spinning in a shared room together. In these activities, villagers mainly worked on their own projects but next to each other. The supplementary material to the GEA characterizes these activities mainly as an opportunity for socializing (Baruzzi-Leicher, 1959). Regular community gatherings affect the information flow inside communities in two ways: they act as a direct conduit for information transmission and lead to long-term social relationships. The social ties that have been formed in these activities can then facilitate norm enforcement and transmission. [21] For that reason, we should expect a positive correlation between the number of communal labor activities and norm prevalence across domains (see first row of Table 3.2). [22]

Social Heterogeneity within Villages. The second row of Table 3.2 contains the relationship of norm prevalence and within-community social heterogeneity. [23] Heterogeneity within a village is likely to reduce norm prevalence on the village level because it is associated with looser within village social relationships. We use religious heterogeneity as a measure of heterogeneity in group membership. Members of each group interact more with each other than with members of the other group: Catholics interact with Catholics and Protestants with Protestants. [24] This lack of interactions across groups is reflected in less relationships across-groups. Since the

21. For evidence that repeated interactions can lead to the formation of social tie see for example Feinberg, Willer, and Schultz (2014) and Fafchamps and Quinn (2018). For evidence that these ties can lead to higher adherence to (pro-social) norms see Chandrasekhar, Kinnan, and Larreguy (2018)

22. For a detailed discussion of this measure that draws on the historical literature about communal spinning see appendix 3.F

23. By social heterogeneity, we mean heterogeneity in the ways in which an individual relates to their community. We do not mean heterogeneity in exogenous preferences.

24. Contact and therefore social relationships between Catholics and Protestants have been sparse because a large part of social life was happening in church or clubs aligned with the corresponding religious denominations (Tillmann Bendikowski, 2016, p.208).

members of heterogeneous communities belong to different groups, a lack of across-group relationships leads to looser within-community relationships. This lack of connection across-group lines can lead to lower enforce-ability and, thus, prevalence of norms [25].

The GEA includes a binned measure of village-level religious denomination. We report the distribution of this original categorization in table 3.C.5 in appendix 3.C.4.1. As can be seen from this table there are very few heterogenous villages and the overwhelming majority of them is included in one bin that specifies a minority share between five and thirty per-cent. Consequently, we classify a village as *religiously-heterogeneous* if the share of inhabitants that does not belong to that religious majority is above 5%. According to this definition, 19% of villages are religiously heterogeneous. For more information on the distribution of religious denomination in our sample and a validation against official statistics, consult Appendix 3.C.4.1.

Social Heterogeneity across Villages. While heterogeneity within a community likely decreases norm prevalence, heterogeneity between communities likely increases norm prevalence. We measure heterogeneity between village communities by the fraction of the four closest neighboring villages with a different majority denomination than the village itself. Heterogeneity between villages decreases interactions with inhabitants from neighboring villages and increases interactions between inhabitants of the same village. Further, heterogeneity between communi-

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Further impediments to inter group contact were religious stigma or church prohibitions and animus between the denominations. Religions can use behavioral restrictions and stigma to tax activities outside of the religion and induce higher contributions to club goods within the religion (Iannaccone, 1992; Berman, 2000). Consistent with these considerations the Protestant as well as the Catholic church did their best to reduce contact between Catholics and Protestants.

The churches' main target were mixed-marriages between the denominations. These marriages were only permissible under strict constraints (Tillmann Bendikowski, 2016). These attempts to separate Protestants and Catholics were at least partly successful. For example, in the 18th century, Protestants and Catholics tried to avoid each other and not to depend on each other (Dietrich, 2004, p.183).

Besides official prohibitions, the church's discouragement of interdenominational contact was also reflected in people superstitions. People believed that if Catholics and Protestants met after church service a person in the village was going to die the next day (Hoffmann-Krayer and Bächtold-Stäubli, 1974, p.181). The discouragement of mixed marriages was reflected in the belief that the remains of people that were part of a mixed marriage were cursed (Hoffmann-Krayer and Bächtold-Stäubli, 1974, p. 179). Protestants and Catholics disliked each other and used slurs for each other even centuries after the reformation (See Hoffmann-Krayer and Bächtold-Stäubli (1974, p.177-178) as well as https://www.welt.de/print-wams/article106154/Die-geteilte-Kleinstadt.html, accessed 15.03.2021.) While a lot of the descriptions in this paragraph concern earlier periods than the 20th century the conflicts and separation between Catholics and Protestants persisted up to the 20th and 21st century (Tillmann Bendikowski, 2016, p.267, p.334).

25. The consequences of this mechanism for the enforcement of cooperation through social sanctions are explored in Fearon and Laitin (1996), Miguel and Gugerty (2005), and Alexander and Christia (2011).

ties provides a markedly different out-group, which can increase in-group cohesion (Koyama and Johnson, 2019). These two effects shift an individual's social relationships further towards their village community, making social sanctions (from their community) more painful and fostering transmission of their village's social norms.[26].

We measure *religious heterogeneity between villages* by the difference in majority religion in a village and its 4 nearest neighbors (*Villages Diff. Rel. 4 NN*). We count the number of neighboring villages that have a different majority religion than the village in question and divide this number by four. As a result we get a variable that ranges from zero to 1, where zero means all neighboring villages have a different majority religion and one means no neighboring village has a different majority religion.

We predict that between village heterogeneity likely raises norm prevalence and within village heterogeneity lowers it. As we show in Appendix 3.C.4.1 between and within village heterogeneity are more common in more heterogenous districts. Consequently, we predict effects of opposite signs for two aspects of the same macro-level concept (district level religious heterogeneity).

Prediction 2 is a consequence of *Prediction 1*. A lack of community-level interactions makes norm transmission and enforcement on the community-level more difficult. This impediment results in fewer community-level norms (*Prediction 1*). Communities, however, can segment into subgroups for which norms are more sustainable and, thus, shift the reference group of a particular norm. The GEA data contains the reference group for neighborhood help obligations. Neighborhood help obligations can apply to smaller groups such as the two next-door neighbors or the whole village. The absence of regular village-level social interactions make it more difficult to implement neighborhood help obligations on the village level because it impedes social sanctioning of the marginal villager making this reference group for neighborhood obligations less likely. Consequently, we expect that among the communities that conduct some neighborhood help, factors that decrease the frequency and value of social interactions are associated with more segmented neighborhood help.

Prediction 2: Communities react to obstacles to community-level social interactions by adapting the reference group of norms.

The last column of Table 3.2 displays the predicted relationships of our measures and the likelihood of village-level neighborhood obligations. Community-level social gatherings and across villages heterogeneity are predicted to be positively as-

26. The conflict between Catholics and Protestants can also lead to a positive effect of heterogeneity between villages on norm prevalence. For evidence that inter-group conflict facilitates social sanctioning see Bornstein and Ben-Yossef (1994), Abbink et al. (2010), and Gneezy and Fessler (2012). While violent open conflicts between Protestants and Catholics mostly ended with the Westfalian peace, the overall conflict lasted until the 1970s, when both churches became less important.

sociated with village-level neighborhood obligations because they increase the value and frequency of village-level social interactions. Within village social heterogeneity, on the other hand, impede these, and should thus be negatively associated with village-level neighborhood obligations.

Prediction 3 is again a consequence of prediction 1. If community-level-social interactions and the value of social relationships increase norm prevalence, norms that increase these community features increase the prevalence of other norms. Adherence to cooperation norms (such as neighborhood obligations) benefits community members and makes them interact. Community members interact while performing neighborhood help and benefit from being helped, thus increasing the efficacy of social sanctioning and norm transmission. This pattern does not apply to norms unrelated to cooperation that do not directly increase the value of belonging to a community or increase the value by very little.

Prediction 3: Cooperation norms display a positive complementarity with other norms such as gender or religious norms.

3.5 Results

To test *prediction 1*, we first investigate the relationship of our covariates of village-level social relationships and an aggregate norm measure *Norm Index*. We construct this measure by standardizing our measures of norm adherence in each domain (religion, gender, and cooperation) and taking the average. We standardize within each norm domain to weight each domain equally.

Column (1) of Table 3.3 shows the relationship between our aggregate norm measure and our covariates of social relationships: village-level religious heterogeneity; religious heterogeneity across villages; and the number of communal labor activities conducted in a village. In all specifications (including this one) we include latitude, longitude and their interaction as a rough way to model some of the spatial auto-correlation within the data. All of the coefficients are statistically significant at 1% and go in the predicted direction. Religiously heterogeneous villages display less norms, villages that deviate from their surrounding religious denomination as well as villages that conduct more communal labor display more norms.

In column (2) of Table 3.3, we add variables indicating distance to the nearest city (in km) , an indicator variable whether a village is close to a border, and whether a village's majority religious denomination is Protestant (as opposed to Catholic) to the regression (see appendix 3.C.5 for more information on these controls).

Being closer to a city allows the inhabitants of the village to migrate to said city, access the market of that city, and expose them to new ideas from the city. The opportunity to migrate to a city or access the market in the city undermines community sanctions (Aoki (2001, p. 51) and Kranton (1996)), provides a substitute for neighborhood help (Wurzbacher, 1961, p. 114), and may make villagers less reliant on the local community.[27] At the same time, cities are more lenient with respect to religious dogma and enforcing boundaries between denominations (Ti!lmann Bendikowski, 2001), which can increase religious diversity, making it a potential confound of our heterogeneity measure.

Further, as religious denomination in the German speaking area is mainly determined by macro-level political institutions, such as the Westfalian peace and religious conversions of state rulers (Tillmann Bendikowski, 2016), religious denomination varies more strongly in a country's border region than in its interior (see 3.C.4.1). This affects both, the geographic distribution of within village heterogeneity as well as the likelihood to deviate in the religious denomination from the surrounding villages. For instance, looking at the geographic distribution of across village heterogeneity (see Figure 3.C.5) reveals that this variable traces out the border between Prussia (predominantly Protestant) and Czechoslovakia (predominantly Catholic). Villages that are closer to the border may however be different in several ways from other villages. For instance, they may be more exposed to conflict than other villages which may also affect the ability to maintain norms.

Including these control variables decreases the size of each relationship slightly. However, all coefficients remain statistically significant at 1% and point in the predicted direction. As expected, being closer to a city increases norm prevalence. The relationship between Norms and majority denomination is insignificant. Being close to the border does not have a statistically significant relationship with our norm index.

Our results may be driven by large or medium scale environmental or political factors that both affect the value of a given norm as well as religious denomination and/or suitability for certain agricultural practices that foster communal labor. In order to reduce our identifying variation to variation within fine-grained regions and to account for these influences, we introduce grid fixed effects and province/state [28] fixed effects in column (3) of Table 3.3. For the grid fixed effects, we divide our data into equally sized grid cells of 400 square kilometers (20 times 20 km). This results in 3,534 grid cells (see Figure 3.E.1). Grid cells contain between 1 and 39 data points. Between 86 and 189 grid cells – depending on the specification – only contain 1 point. When we include grid cell fixed effects, we do not use variation from these points. The geographic distribution of these grid cells is depicted in Figure 3.E.1 in the Appendix.

27. High distance to markets is associated with less public good contribution (Gebremedhin, Pender, and Tesfay, 2004). Informal credit, which is also enforced through social sanctions declines with distance to cities (Moahid and Maharjan, 2020) and households with more external network connections participate less in reciprocal exchange (Jaimovich, 2015).

28. To get areas of roughly equal size we use provinces within Prussia and states outside of Prussia. This results in 44 different spatial units.

Column (3) of table 3.3 shows that after including grid and province/state fixed effects all associations between proxies for village-level social relationships and our norm index stay the same. All coefficients point into the predicted direction and remain significant at 1%. The coefficient for across village religious heterogeneity halves in size. This may be partly driven by low intra-regional variation in this variable (see Figure 3.C.5). [29]

To check if our determinants of social relationships affect norms in all domains similarly, we regress norm prevalence in each domain separately on our correlates of social relationships, controls and fixed effects. We report OLS estimates of these relationships in table 3.4. To compare results across specifications, we standardize all outcomes by subtracting the mean and diving by the standard deviation. The results reveal that within-village heterogeneity is associated with reduced norm prevalence across all domains, however, at different magnitudes. It is associated with a reduction of more than 0.1 standard deviations in the number of neighborhood help obligations as well as the absences of dissidents. The relationship with childbed norms is up to 0.05 standard deviations weaker and only statistically significant for impurity norms.

Across village heterogeneity is positively associated with norms across domains. The coefficient on childbed norms, however, turns weakly negative in column (6) of table 3.4. A possible explanation is that it is poorly identified in this specification, because as mentioned above the intra-regional variation in across village heterogeneity is bunched in the southwest corner of the German Reich, where childbed norms vary little. The number of communal labor activities is positively associated with the number of neighborhood obligations and the number of childbed norms within a village across specifications. It is not associated with the absence of dissidents in a village.

Next, we turn to *Prediction 2*, namely, the effect of village-level social relationships on segmentation. According to *prediction 2*, variables that facilitate norm maintenance should lead to less segmentation and variables that impede norm maintenance should lead to more. We measure an unsegmented neighborhood by the standardized dummy indicating village-level neighborhood help (Unsegmented Neighborhood). Our results are displayed in column (4) - (6) of Table 3.3. Similarly to before, we start by investigating the raw (partial) correlation. As predicted, we find that within village heterogeneity increases the likelihood of village-level segmentation. The number of communal labor activities as well as across village religious

29. Within Prussia (with the exception of Schlesia), Austria, and Bavaria this variable almost does not vary because of the strong enforcement of religious denomination of the state's rulers (Tillmann Bendikowski, 2016). Thus, including grid fixed effects as well as the indicator variable of being close to the border strongly reduces our identifying variation to the regions of to the southwest corner of the German Reich – mostly Palatine and Baden-Wurttemberg – as well as Schlesia which switched from Habsburg Rule to Prussia.

heterogeneity reduce the likelihood of village-level segmentation. When including our fixed effects in column (6), the relationship between across village heterogeneity and neighborhood segmentation reduces in magnitude and turns insignificant. The coefficients of within village heterogeneity as well as the number of communal labor activities stay qualitatively as well as quantitatively the same.

We report two *robustness checks* of our regressions concerning prediction 1 and 2 in appendix 3.G. Similar to the childbed norms, we can also divide neighborhood help obligations into its different subcategories (see 3.G.1). The results stay very similar. We also check if our results are robust to using a different measure of between village heterogeneity. Instead of using the share of villages with a different majority religion, we use the continuously measured absolute difference in the share of protestants between a village and its four nearest neighbors. We take the midpoint of the bins to approximate the share of protestants in a village. We document the results of these regressions in table 3.G.2. The results stay qualitatively similar.

According to *prediction 3* cooperation norms should be positively associated with religious and gender norms because of positive complementarities. The left panel of Figure 3.5 displays the OLS estimate of the standardized number of neighborhood help obligations on the standardized variables of religious norms and childbed norms for different specifications, accordingly. The first line for each outcome variable depicts the coefficient from a regression that controls for latitude, longitude and their interaction. The second line adds community specific covariates of village-level social relationships as well as the control variables majority religious denomination, distance to the nearest city, and closeness to the closest border. The third line adds province and grid cell fixed effects to account for potentially joint environmental causes of these norms.

The results show that all coefficients are positive and significant at 5% across specifications. The coefficient of the number of neighborhood help does not vary a lot across the type of norm and specification and lies around 0.05 standard deviations. The raw relationship between religious norms and cooperation norms tends to be with 0.1 standard deviations larger, however less precisely estimated. This coefficient shrinks towards 0.03 standard deviations when including geographic and political fixed effects, suggesting that part of the relationship is driven by joint environmental factors.

We cannot exclude that there are omitted variables that drive all of our norms simultaneously. In particular, there is no reason to believe that religious heterogeneity and communal labor activities cover all potential drivers of differences in social relationships between villages and as we put forth, above this positively influences the prevalence of all norms, so one may suspect that estimate is upward biased. We try to address this issue partially by investigating the relationship between religious norms and gender norms for which we do not predict a positive direct interdependence. If there were joint factors the positively influence all norms simultaneously, we should also see a positive empirical relationship between these types of norms,

Table 3.3. Regressions of the norm index and the unsegmented neighborhood dummy on determinants of social relationships.

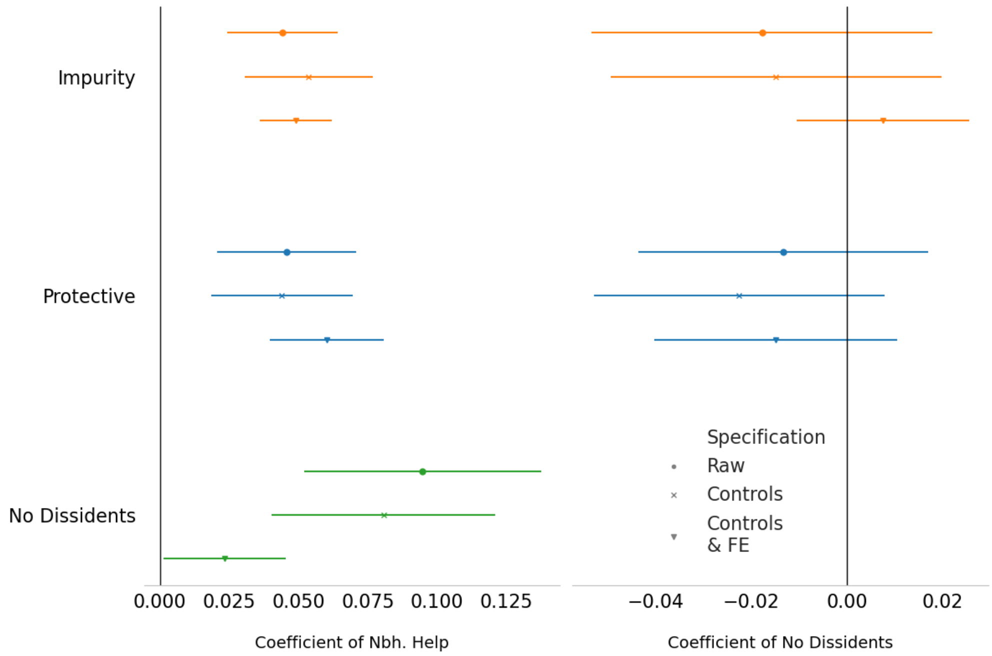
	Dependent variable:					
	Norm Index			Unsegmented Neighborhood		
	(1)	(2)	(3)	(4)	(5)	(6)
Heterogeneous	-0.120^{***}	-0.101^{***}	-0.107^{***}	-0.043^{***}	-0.042^{***}	-0.029^{**}
	(0.028)	(0.030)	(0.019)	(0.011)	(0.010)	(0.014)
Villages Diff. Rel. 4NN	0.270^{***}	0.269^{***}	0.091^{***}	0.070^{**}	0.080^{***}	0.041
	(0.047)	(0.040)	(0.030)	(0.027)	(0.027)	(0.034)
N. Communal Labor	0.041^{***}	0.050^{***}	0.055^{***}	0.009^{*}	0.009^{*}	0.014^{***}
	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Majority Protestant		−0.076	−0.041		0.025	0.008
		(0.053)	(0.032)		(0.018)	(0.015)
Distance to Next City		0.083^{***}	0.065^{***}		0.032^{***}	0.023^{*}
		(0.015)	(0.016)		(0.008)	(0.012)
Close to Border		−0.0005	0.001		−0.0002	0.0001
		(0.0004)	(0.001)		(0.0001)	(0.001)
Sample	Full	Full	Full	Nbh. Help > 0	Nbh. Help > 0	Nbh. Help > 0
Grid FE			✓			✓
Province FE			✓			✓
Observations	11,900	11,900	11,900	8,159	8,159	8,159
Adjusted R^2	0.016	0.032	0.158	0.002	0.006	0.036

Notes: $^{*}p < 0.1$;** $p < 0.05$,*** $p < 0.01$. Table displays OLS Estimates. We exclude some regions in column (1)-(3), because the childbed norms are incomplete in this region as some of the data has been destroyed in the war (for more details see Appendix 3.H). *Norm Index* is the average over number of neighborhood help obligations, number of childbed norms, and an indicator variable that is one if a village has no dissidents, standardized respectively. *Unsegmented neighborhood* is an indicator variable that is one if a the whole village is obliged to help a neighbor in at least one task. Grid cells have an approx. area of 400km^2; province fixed effects account for the states and provinces (in case of Prussia) of the German Reich, Austria, Gdansk, Liechtenstein, and Czechoslovakia. Close to Border is an indicator variable that is one for the 5% of observation closest to the border of Austria and the German Reich. All specifications adjust for latitude and longitude and their interaction. Column (1) and (4) include an intercept. Standard errors are clustered by the 400 km^2 grid and Prussian provinces/states.

Table 3.4. Regressions of norm prevalence in different domains on determinants of social relationships.

	Dependent variable:							
	Nbh. Help		No Dissidents		Impurity		Protective	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Heterogeneous	−0.146*** (0.031)	−0.149*** (0.030)	−0.139*** (0.041)	−0.170*** (0.028)	−0.030 (0.054)	−0.058* (0.032)	−0.051 (0.035)	−0.021 (0.027)
Villages Diff. Rel. 4NN	0.202*** (0.059)	0.121 (0.080)	0.366*** (0.074)	0.156*** (0.037)	0.107 (0.090)	−0.053 (0.054)	0.087 (0.084)	0.012 (0.063)
N. Communal Labor	0.106*** (0.013)	0.100*** (0.011)	−0.002 (0.015)	0.006 (0.009)	0.021 (0.013)	0.023** (0.010)	0.042*** (0.013)	0.056*** (0.008)
Majority Protestant	−0.049 (0.072)	0.065 (0.056)	−0.170 (0.126)	−0.043* (0.023)	0.123 (0.086)	−0.012 (0.050)	−0.105* (0.060)	−0.174** (0.065)
Distance to Next City	0.146*** (0.016)	0.089*** (0.022)	0.165*** (0.047)	0.145*** (0.024)	−0.097*** (0.030)	0.013 (0.023)	−0.042* (0.023)	−0.051*** (0.019)
Close to Border	−0.001** (0.001)	0.00004 (0.001)	−0.0004 (0.001)	0.002 (0.001)	−0.0004 (0.001)	−0.0005 (0.002)	0.0001 (0.0004)	0.001 (0.001)
Grid FE		✓		✓		✓		✓
Province FE		✓		✓		✓		✓
Observations	13,117	13,117	16,563	16,563	11,900	11,900	11,900	11,900
Adjusted R^2	0.069	0.195	0.031	0.155	0.022	0.117	0.077	0.125

Notes: $^{*}p < 0.1$;** $p < 0.05$,*** $p < 0.01$. Table displays OLS Estimates. Outcome variables have been standardized. We additionally have to exclude some regions in column (5)-(8), because the childbed norms are incomplete in this region as some of the data has been destroyed in the war (for more details see Appendix 3.H). Grid cells have an area of 400km^2; province fixed effects account for the states and provinces (in case of Prussia) of the German Reich, Austria, Gdansk, Liechtenstein, and Czechoslovakia. Close to Border is an indicator variable that is one for the 5% of observation closest to the border. All specifications adjust for latitude and longitude and their interaction. Standard errors are clustered by a 400 km^2 grid and provinces/states.



Notes: All regressions include latitude, longitude and their interaction. Lines are 95% confidence intervals. Standard errors are clustered by a 400 km^2 grid and provinces/states. Controls include the community specific covariates of village-level social relationships as well as the control variables majority religious denomination, distance to next city, and closeness to border. The right panel additional includes number of neighborhood help obligations. Fixed effects adjust for 400km^2 grid fixed effects and for states and Prussian Provinces. All outcome variables are standardized by subtracting the mean and dividing by the standard deviation.

Figure 3.5. Fixed effect regressions to examine complementarity between norms.

conditional on neighborhood help norms. The results are displayed in the right panel of Figure 3.5. We use the same specifications as for the neighborhood help norms, with the exception that we additionally control for neighborhood help (in all except for the raw specification). They show that the relationship between religious norms and gender norms are not statistically significant and largely negative. According to this result our previous results on norm prevalence are unlikely to be driven by confounders that affect all norms equally.

3.6 Discussion

We exploit geographically fine-grained data of German speaking villages from the 1930s to investigate drivers of the prevalence of norms. Through geographic cluster analysis, we show that geographic variation in the institutional or physical environment explains little heterogeneity in norms. Consequently, villages in the same physical environments, e.g., mountains, and in the same state, can still exhibit marked differences in the norms they enforce. We argue that locally different community structures inducing tighter or looser social relationships can explain these differences. That is, while environmental factors may shift the value or need for a norm, in order for norms to stick, communities need mechanisms through which they can transmit and enforce these norms.

Accordingly, we find that overall norm prevalence, and the reference group for cooperation norms (segmented versus unsegmented neighborhood) depend on correlates of social relationships in a consistent pattern. Higher religious heterogeneity within villages is associated with fewer norms and a higher likelihood of segmented neighborhood help. Higher heterogeneity across villages and regular community gatherings are associated with more norms and a lower likelihood of segmented neighborhood help. That is, norm prevalence as well as segmentation into smaller subgroups are associated with these correlates as predicted by the theories we review in section 3.4.

According to our conceptual framework, our results may reflect simultaneity. Tight social relationships facilitate cooperation norms, and cooperation norms tighten social relationships. That is, a village might fail to enforce a cooperation norm (e.g., neighborhood help) because it lacks tight within-community social relationships. However, this village might lack social relationships because the village's inhabitants have fewer reasons to engage with each other because of low cooperation among villagers. After all, there is no cooperation norm. Another village might have both of these things, supporting each other's existence. One important implication of this argument is that there should be a direct empirical relationship between cooperation norms and norms unrelated to cooperation. Consequently, we find that cooperation norms correlate with other norms conditional on the external environment: a village that conducts more neighborhood help is also likely to have a higher prevalence of childbed norms and a smaller likelihood of having dissidents. However, childbed norms and religious norms are empirically unrelated. These correlations suggest that cooperation norms facilitate maintaining other norms and that this relationship is one-sided.

Our estimates are strongly suggestive, however, not necessarily causal. As we use variation within small geographic units (20km × 20km) potential confounding is limited to factors that vary within these units. Our results are robust to including three obviously locally varying factors: ruralness, religious denomination, and political threat proxied by distance to the nearest border. Neither of these factors play a consistent role in explaining norm prevalence across domains. Our empirical identification still relies on the assumption that environmental factors influencing norm prevalence directly do not vary strongly within our 20km × 20km grid. While we cannot generally exclude the violation of this assumption, it seems rather plausible when looking at we currently know about environmental causes of norms and cooperation. For instance, Alesina, Giuliano, and Nathan (2013) explains differences in the evolution of gender norms by plough agricultural practices; Buggle and Durante (2021) connects the evolution of social cooperation with varying climatic risk across regions; Buggle (2020) explains the existence of collectivist norms by the geographic suitability for irrigation agriculture; Gelfand et al. (2011) argues that norms are caused by social and ecological threat. These types of environmental causes usually vary at a lager scale than our fixed effects.

Other potential confounding may be due to unobserved community-level characteristics. Our correlates of social relationships do not cover the universe of potential factors influencing community-level social relationships. In particular, if we think of the strength of community-level social relationships as latent factor, our correlates may be both causes and consequences of this latent variable: social heterogeneity may yield weaker social ties, but weaker social ties may also allow for more social heterogeneity. Either way, however, social heterogeneity is a proxy of weaker social ties. Thus, this type of simultaneity does not generally invalidate the core interpretation of our results, namely that the prevalence of norms is related to community-level social relationships. Whether this is driven, by ex-ante weaker social ties or directly by heterogeneity cannot be fully answered in our setting. It seems likely that both is true. Religious heterogeneity in the German speaking area is strongly determined by macro political factors such as the Westphalian Peace. However, which villages within a macro political area are heterogeneous is likely determined by the ex-ante community structure. Further research may help to shed more light on this.

Our results show that norms vary locally. Consequently, researchers need to consider locally varying factors, such as community-level social relationships, when explaining the spatial distribution of norms.

Most economic theories of norms and social relationships are concerned with cooperation norms in particular. However, our findings suggest that these theories extend beyond the domain of cooperation. In particular, tight social relationships might provide a common cause behind norm prevalence in all domains and could provide a microfoundation for the concept of tight cultures.

We argue that norms in different domains do not only have a common cause but are also interdependent: more cooperation norms are associated with more norms in other domains. Consequently, strengthening cooperation norms likely also strengthens other norms. Researchers should consider this side-effect when investigating policies to foster cooperation.

Last but not least, we find that heterogeneity at different levels can have very different effects. Religious heterogeneity within villages impedes norm maintenance within villages, while religious heterogeneity between villages fosters it. This also implies that when investigating the effects of heterogeneity, the level of aggregation matters. Investigating the impact of more aggregated statistics may yield misleading conclusions depending on the underlying distribution and on which effect dominates in the aggregate.

Our study leaves many questions unanswered that could be potentially studied with the GEA. First and foremost, the GEA can aid the study of cultural persistence with respect to norms, but also other phenomena. Research on economic history has shown that cultural phenomena and shocks persist across centuries and influence present-day institutions and behaviors. Often, the original causes are in the past and poorly measured (Voth, 2021). Because of the GEA's timing and the rise of social surveys in the 1980s, the GEA can help us to measure these original causes and understand micro-mechanisms of cultural persistence.

Our results suggest that close community relations stabilize norms. Conversely, norms should be less persistent in villages with looser or loosening social relationships. This mechanism may interact with the depreciation of the value of a particular norm. Putting these two mechanisms together may help understand why and where norms persist over time and what is likely to change them: Norms persist because they form institutional complementarities (Belloc and Bowles, 2013). An important norm loosing its value can undermine tight community social relationships and lead to the disappearance of a whole bundle of norms.

Secondly, the GEA's timing right before the rise of the Nazis in Germany may help to understand this rise better. Satyanath, Voigtländer, and Voth (2017) show that membership in the NSDAP is connected to social capital as measured by membership in other voluntary forms of association. Social capital, however, has multiple facets. One of these is local cooperation which can be measured with our norms related to cooperation and regular community gatherings. Thus, researchers can use the GEA to study whether this association is limited to social capital measured by formal voluntary associations or also holds for other aspects of social capital. Further, it may help us to understand the persistence of Nazi voting and anti-Semitism in Germany (Voigtländer and Voth, 2012; Cantoni, Hagemeister, and Westcott, 2017) by separating cultural from persisting economic, institutional, and geographic differences.

Appendix 3.A Sample and Data Gathering of the GEA

Table 3.A.1. Number of observations

	Quest. 2	Quest. 4	Other Quest.	Quest. 2 & 4
German Reich	15,124	14,625	15,799	12,164
Austria	1,141	901	1,143	826
Czechoslovakia	1,119	868	1,134	771
Gdansk	90	65	62	49
Liechtenstein	6	7	8	5
Luxembourg	77	1	68	1
Sum	17,557	16,467	18,214	13,816

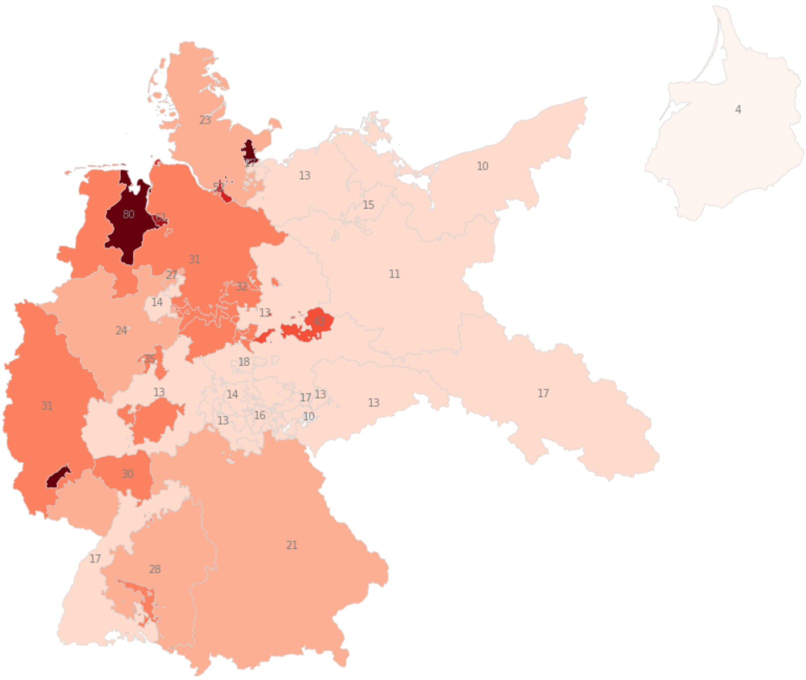
Notes: Sample restricted to villages in the German Reich, Austria, the Czech part of Czechoslovakia, Liechtenstein, Gdansk, and Luxembourg. Other Quest. = Villages included in questionnaires 1 or/and 3.

For villages in the German Reich, we can compare the number of villages in the sample to the number of municipalities in 1910 in each state or province.[30] Figure 3.A.1 displays the number of villages contained in, both, questionnaire 2 and 4 as share of the total number of municipalities in a German States and Prussian Provinces in 1910. It shows large variation in the share of municipalities captured in the GEA. The share is with 80% highest in Oldenburg and with 4% lowest in East Prussia. In general, there is a visible East West divide. The share of municipalities captured in the GEA is lower in the Eastern part of Prussia than in the remainder of the German Reich. However, overall coverage seems to be high and for most regions between 10 and 30%. Naturally, the shares tend to be higher when examining questionnaires separately (see Figure 3.A.2).

While the GEA was conducted between 1930 and 1935, the researcher remained largely independent of the national-socialist government until the last wave in 1935, which we do not use in our analysis. The first four questionnaires were conducted by a team of independent researchers financed by the German Science Foundation's predecessor called "Notgemeinschaft der Deutschen Wissenschaften". In contrast, the fifth questionnaire was conducted under a leadership connected to the national-socialist government (Schmoll, 2009). The GEA contains 243 open questions, with most of the questions consisting of three or more subquestions. We focus on ques-

30. We use the digitized registry of municipalities in the German Reich, provided to us by Ulli Schubert

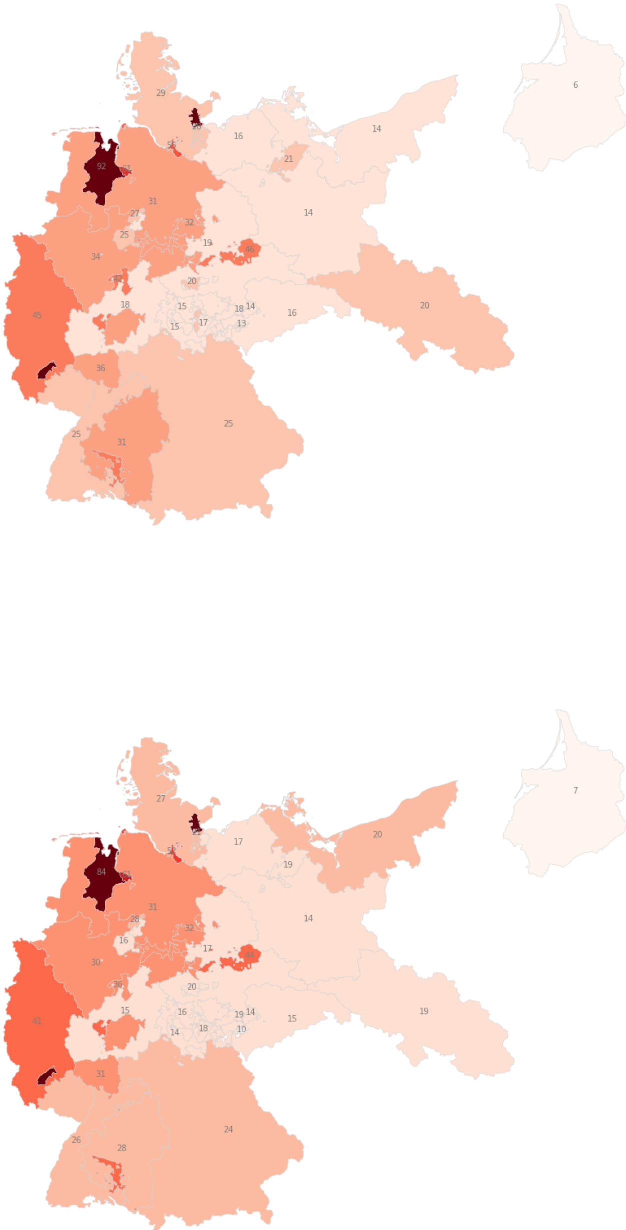
tionnaires two and four because these contain the questions on norms and cooperation.[31]



Notes: Sample restricted to villages included in both questionnaire 2 and questionnaire 4. German Reich in its borders from 1914 excluding the Provinces West Prussia, Posen an Alsace-Lorraine.

Figure 3.A.1. Number of villages in, both, questionnaire 2 and 4 as share of the total number of municipalities in a State/Province in 1910.

31. Information on the content and sample of all questionnaires is available upon request.



Notes: Sample restricted to villages included in both questionnaire 2 or questionnaire 4. German Reich in its borders from 1914 excluding the Provinces West Prussia, Posen an Alsace-Lorraine.

Figure 3.A.2. Number of villages in questionnaire 2 and 4, respectively, as share of the total number of municipalities in a State/Province in 1910.

Appendix 3.B Volunteer Characteristics

To get a more detailed view of the volunteer's background characteristics, we use data covering the Rhine-Province digitized by Kehren (1994). Around 90% of respondents in this region were teachers. The occupations of the remaining respondents are heterogeneous. The most common additional groups are farmers, students, craftsmen, and individuals occupied in some administrative positions (mostly local government). Each of those groups covers between 1 and 3% of respondents. Below 1% of respondents in the Rhine Province were women, and most of the respondents were between 30 and 50 years old when they answered the survey (see Figure 3.B.1a). The low share of women might lead to higher measurement error in the variables concerning restrictions on women.

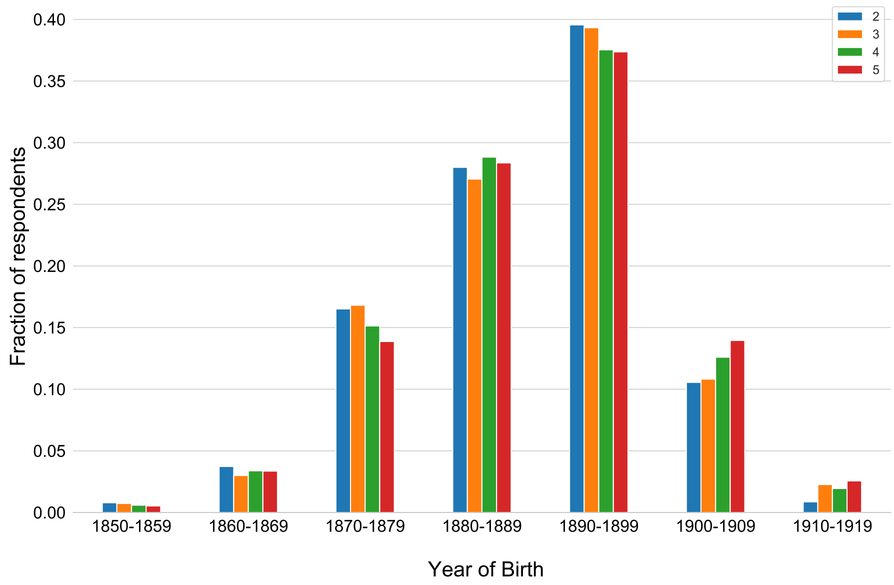
We can also use the data of Kehren (1994) to learn about the volunteer's familiarity with the village they were covering. The share of respondents born in the village they answer for varies between 11.8% and 17.4% and increases over time. In the samples of questionnaires two and four, it is 11.8% and 14.4%, respectively. The majority of the volunteers who were not born in the village moved there before or in 1920, so they spent at least 10 years in the village they answered for.[32] However, a large part also moved there only in the 1920s or even in the 1930s. While volunteers who did not come from the village had a disadvantage in accurately answering the questions, they did so by relying on a village's inhabitants' help.

Table 3.B.1. Occupation of volunteers by questionnaire

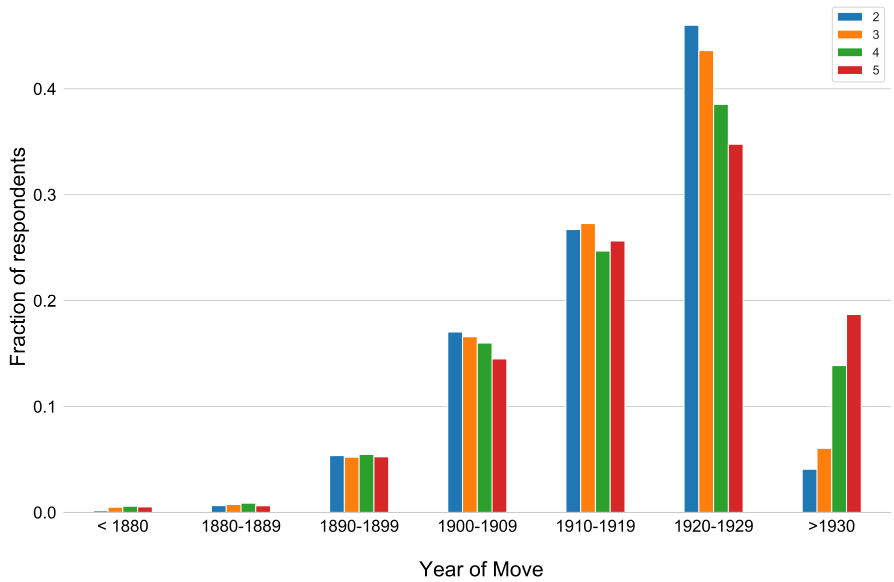
Quest. Occupation	2	3	4	5
Teacher/Principal	92.2	90.8	89.0	87.1
Other	2.1	2.5	3.5	4.3
Farmer/Winemaker	1.7	1.5	2.3	3.4
Student	1.5	2.3	1.2	1.5
Administration	0.9	1.5	1.7	1.7
Craftsman	0.9	0.8	1.3	1.0
Pastor/Chaplain	0.4	0.5	0.3	0.4
No occupation	0.2	0.1	0.5	0.3
Innkeeper	0.1	0.1	0.3	0.3

Notes: Sample restricted to the Rhine Province. Data obtained from Kehren (1994). Own calculations.

32. We know neither the locality of origin nor the year since when the respondent moved to the village for 1% of the sample in questionnaires two and three, 6.7% in questionnaire four, and 1.7% in questionnaire five. The numbers refer to the remaining sample.



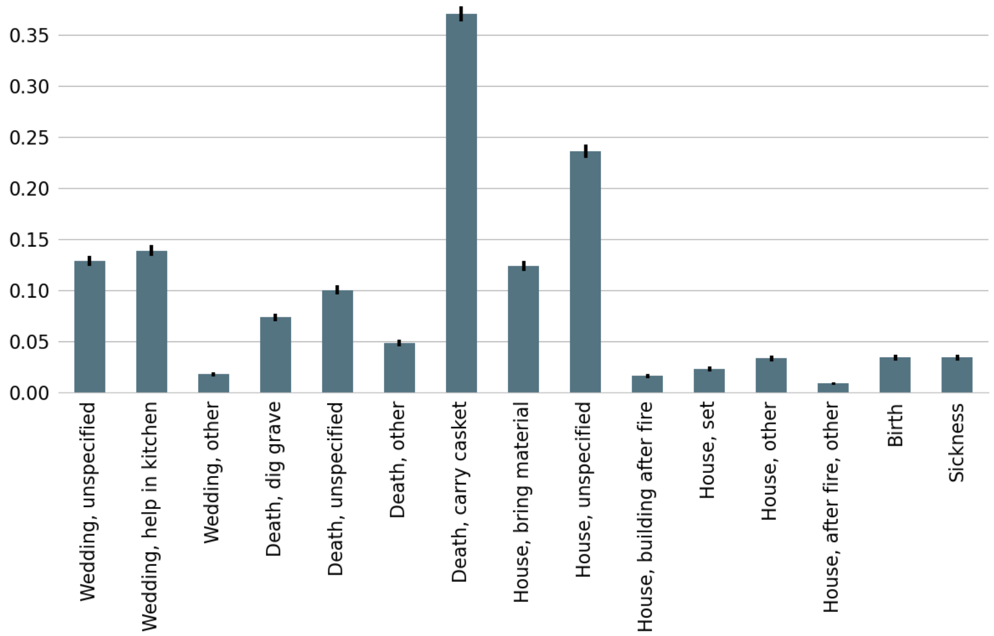
(a) Volunteers year of birth.



(b) Year when volunteer moved to village.

Notes: Sample restricted to the Rhine Province. Data obtained from Kehren (1994). Own calculations. Part 3.B.1b is restricted to volunteers who are not born in the village they answer for.

Figure 3.B.1. Characteristics of volunteers in the Rhine Province.



Notes: Categorization of neighborhood help answers as categorized by Barruzi-Leicher and Frauenknecht (1966). Multiple answers possible.

Figure 3.C.1. Fraction of villages in each subcategory of neighborhood obligations

Appendix 3.C Measures

3.C.1 Neighborhood OFbligations

3.C.2 Women in Childbed

Table 3.C.1. Protective norms: subcategories

Category English	Category German	Fraction
The woman in childbed should avoid or not surpass...	Die Wöchnerin soll meiden bzw. nicht überschreiten...	
(House)threshold, front door, yard gate	(Haus)türschwelle, Haustür, Hoftor	0.052
Attic, stable	Boden, Stall	0.001
Ground	Boden	0.005
Eaves, roof	Dachtraufe, Dach	0.090

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Protective norms: subcategories

Foreign eaves, foreign roof	fremde Dachtraufe, fremdes Dach	0.002
Foreign doorstep	fremde Türschwelle	0.002
Foreign stable	fremden Stall	0.001
Cellar, attic, stable	Keller, Boden, Stall	0.017
Basement, attic	Keller, Boden	0.019
Cellar, stable	Keller, Stall	0.006
Basement	Keller	0.036
Crossroad	Kreuzweg	0.023
Gutter	Rinnstein	0.026
Other	Sonstiges	0.002
Stable	Stall	0.014
Lane, driveway, road embankment	Wagenspur, Fahrweg, Fahrdamm	0.005
Water, jetty	Wasser, Steg	0.007
Burial, open grave, sight of a corpse	Begräbnis, offenes Grab, den Anblick einer Leiche	0.003
Other rules …	Andere Regeln …	
Do not look after a funeral procession	keinem Leichenzug nachblicken	0.002
Don't go to the cemetery (alone)	nicht (allein) auf den Friedhof gehen	0.007
Avoid other encounters with death and grief	sonstige Begegnungen mit Tod und Trauer meiden	0.000
Throw stones across the street before crossing	Steine über die Straße werfen vorm Überschreiten	0.001
The woman in childbed is blessed or crossed herself or sprinkled with holy water, among other things	die Wöchnerin wird gesegnet bzw. bekreuzigt oder mit Weihwasser besprengt, u.A.	0.005
Have a straw (in the shoe)	einen Strohhalm (im Schuh) bei sich haben	0.001
Have pious books (hymn book, bible) with her (in clothes, in bed)	fromme Bücher (Gesangbuch, Bibel) bei sich haben (in der Kleidung, im Bett)	0.002

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Protective norms: subcategories

Have metal objects (scissors, ax, knife, needle, wedding ring, etc.) with her (in clothing, in bed)	Gegenstände aus Metall (Schere, Axt, Messer, Nadel, Trauring u.A.) bei sich haben (in der Kleidung, im Bett)	0.003
Have consecrated objects (e.g. rosary, holy water, etc.) with her (in clothing, in bed)	geweihte Gegenstände (z.B. Rosenkranz, Weihwasser u.A.) bei sich haben (in der Kleidung, im Bett)	0.004
Have herbs and spices with her (in clothes, in bed)	Kräuter und Gewürze bei sich haben (in der Kleidung, im Bett)	0.001
Cross oneself	sich bekreuzigen	0.001
Have other items (e.g. children’s stuff, comb, etc.) with her (in clothing, in bed)	sonstige Gegenstände (z.B. Kinderzeug, Kamm u.A.) bei sich haben (in der Kleidung, im Bett)	0.001
Praying the Lord’s Prayer (and circling the table three times (weekly prayers), saying the blessing formula)	Vaterunser beten (und dabei dreimal den Tisch umkreisen (Wochengebete) beten, Segensformel sprechen)	0.003
Take holy water among others	Weihwasser nehmen u.A.	0.001
The woman in childbed should not wash any clothes	Die Wöchnerin soll keine Wäsche waschen	0.008
The woman in childbed should not mend, knit, spin, etc.	Die Wöchnerin soll nicht flicken, stricken, spinnen usw.	0.002
The woman in childbed takes measures to change clothes and shoes (e.g. turn around, stuffing, swapping)	Die Wöchnerin trifft verändernde Maßnahmen an Kleidung und Shuhwerk (z.B. umkehren, ausstopfen, vertauschen)	0.000
The woman in childbed has to go outdoors under a roof (umbrella or similar)	Die Wöchnerin muss im Freien unter einem Dach gehen (Schirm o.Ä.)	0.003
The woman in childbed should not have anything to do with the clothesline (e.g. do not go under the clothesline, do not hang up laundry, do not pull the clothesline)	Die Wöchnerin soll nichts mit der Wäscheleine zu tun haben (z.B. nicht unter die Wäscheleine gehen, keine Wäsche aufhängen, keine Wäscheleine ziehen)	0.003
The woman in childbed wears a headscarf, also known as a mulch, outdoors (rarely also indoors)	Die Wöhnerin trägt im Freien (selten auch im Haus) ein Kopftuch, auch Maultuch genannt	0.008

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	Protective norms: subcategories	
The women in childbed wears her husband's things on or with her or has them in bed	Die Wöchnerin trägt Sachen ihres Mannes an oder bei sich oder hat sie im Bett	0.004
Avoid encounters with foreign animals (e.g. dogs, cats)	Begegnung mit fremden Tieren (z.B. Hund, Katze) meiden	0.000
Don't go out alone, and variations	nicht allein ausgehen und Variationen	0.001
Do not stay alone or sleep (especially in childbed)	nicht allein bleiben bzw. schlafen (besonders im Wochenbett)	0.004
Don't borrow anything	nichts entleihen	0.003
Don't lend anything	nichts verleihen	0.011
Avoid other contacts such as answering to a knock, shaking hands and others	sonstige Kontakte meiden wie z.B. Antwort auf Anklopfen, die Hand geben u.A.	0.002
Avoid encounters with strangers, and variations	Zusammentreffen mit Fremden meiden, und Spezifikationen	0.001
Avoid meeting other people (e.g. old women, Sinti and Roma, wrong people, etc.)	Zusammentreffen mit sonstigen Personen meiden (z.B. alten Frauen, Sinti und Roma, falschen Leuten u.A.)	0.002
Do not make the bed, do not ventilate and observe the prohibitions on care	Bett nicht machen, nicht lüften u.Ä. Pflegeverbote beachten	0.001
Do not cover the bed with a blanket	Bett nicht mit einer Decke zudecken	0.001
Do not move the bed, etc.	Bett nicht verrücken u.Ä.	0.001
Do not climb stairs or ladders	keine Treppen oder Leitern steigen	0.004
Do not change body wash, etc.	Körperwäsche nicht wechseln u.Ä.	0.001
Don't look out the window, don't go to the window	nicht aus dem Fenster sehen, nicht zum Fenster gehen	0.003
Do not look in the mirror or cover the mirror	nicht in den Spiegel sehen bzw Spiegel verhängen	0.002
Do not sweep or wipe	nicht kehren oder wischen	0.001
Do not go over the threshold, etc.	nicht über die Stubenschwelle gehen u.Ä.	0.001
Do not comb; other prohibitions for activities on the body	sich nicht kämmen; sonstige Verbote für Verrichtungen am Körper beachten	0.001

continued on next page

Protective norms: subcategories

Do not leave the house after sunset or go to the house before sunset	nach Sonnenuntergang das Haus nicht mehr verlassen bzw vor Sonnenuntergang das Haus aufsuchen	0.008
Do not be in the dark (alone) in the room	nicht im Dunkeln (allein) im Zimmer sein	0.000
Do not leave the house in the dark	nicht im Dunkeln das Haus verlassen	0.002
Other prohibitions against being out of the house at night, e.g. not letting the moon shine on you	sonstige Verbote, nachts außer Haus zu sein, beachten, z.B. sich nicht vom Mond bescheinen lassen	0.001
Stay home during lunchtime or take other precautions	unter Mittag zu Hause bleiben oder andere Vorsichtsmaßnahmen treffen	0.002
Do not leave the house before the prayer rings	vor dem Gebetsläuten früh das Haus nicht verlassen	0.000
Do not leave the house before sunrise	vor Sonnenaufgang nicht aus dem Haus gehen	0.000
Go into the house for the prayer (evening, Ave, after work) rings	zum Gebets- (Abend-, Ave-, Feierabend-) läuten ins Haus gehen	0.007
Do not speak or speak softly	nicht oder nur leise sprechen	0.000
Don’t argue, don’t scold, and similar	nicht streiten, nicht schelten u.Ä.	0.001
Keep yourself separate, for example also at the table	sich abgesondert halten, z.B. auch bei Tisch	0.005
Do not let the rain drip on you, and similar	sich nicht vom Regen übertropfen lassen, u.Ä.	0.001
Other instructions for behavior indoors and outdoors	sonstige Gebote für das Verhalten im Haus und im Freien beachten	0.002
Be careful with thunderstorms	vorsichtig sein bei Gewitter	0.001
Any of above		0.338

Notes: Categorization according to Grober-Glück (1966).

Table 3.C.2. Impurity norms: subcategories

Category English	Category German	Fraction

continued on next page

[ ]Impurity norms: subcategories

Hail and thunderstorms are the result of premature exit or other misconduct	Hagel, Gewitter sind Folgen vorzeitigen Ausgangs oder anderen Fehlverhaltens	0.001
Don’t make a fire, Don’t go to the fire, Don’t look into the heated oven	kein Feuer machen, nicht an Feuer gehen, nicht in den angeheizten Backofen schauen	0.002
Don’t touch or salt meat or go to the salting tub	kein Fleisch anfassen oder einsalzen bzw nicht ans Pökelfaß gehen	0.001
Don’t go to a wedding, don’t look over to a bride and groom	keine Hochzeit besuchen, keinem Brautpaar nachsehen	0.002
Don’t attend a public event (e.g. festivity, dance) because there will be dispute or a fight	keine öffentliche Veranstaltung (z.B. Festlichkeit, Tanz) besuchen, weil Streit, Schlägerei entstehen	0.032
Don’t visit the neighbor	keinen Besuch beim Nachbarn machen	0.153
Don’t step on a green lawn	keinen grünen Rasen betreten	0.002
Don’t fill cider or other drinks or vinegar	keinen Most oder andere Getränke bzw Essig abfüllen	0.001
Don’t take part in slaughter	nicht am Schlachten teilnehmen	0.001
Don’t eat canned fruits or vegetables	nicht an konservierte Früchte oder Gemüse gehen	0.002
Don’t go to open containers for laundry and clothing	nicht an offene Behälter für Wäsche und Kleidung gehen	0.002
Don’t bake (e.g. bread) or handle baked goods	nicht backen (zB Brot) bzw mit Backsachen umgehen	0.002
Don’t boil down or pickle	nicht einkochen oder einlegen	0.001
Don’t work in the soil	nicht in der Erde arbeiten	0.001
Don’t go to grocery stores	nicht in Lebensmittelläden gehen	0.001
Don’t touch the salt	nicht ins Salz fassen	0.001
Don’t meet other women who have recently given birth	nicht mit anderen Wöchnerinnen zusammenkommen	0.000
Don’t socialize with women or girls who can conceive	nicht mit empfängnisfähigen Frauen oder Mädchen zusammenkommen	0.001
Other prohibitions	sonstige Verbote beachten	0.000

continued on next page

[ ]Impurity norms: subcategories

Other prohibitions regarding food	sonstige Verbote für das Umgehen mit Lebensmitteln beachten	0.001
Other prohibitions regarding garden and field	sonstige Verbote für Garten und Feld beachten	0.001
Don’t visit the well	Nicht zum Brunnen gehen	0.089
Any of above		0.257

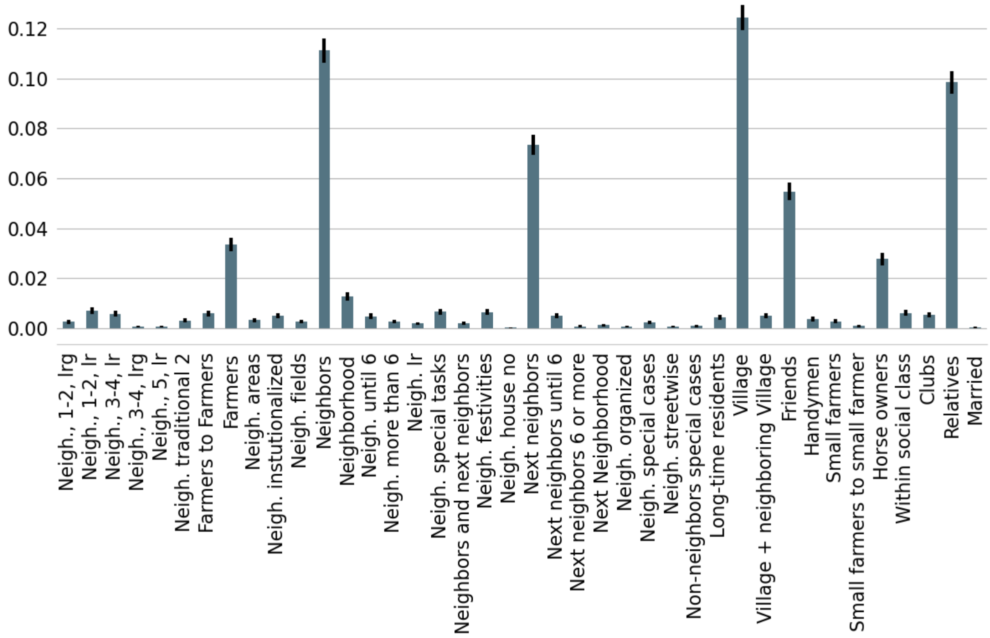
Notes: Categorization according to Grober-Glück (1966).

Notes: Categorization according to Grober-Glück (1966).

Table 3.C.3. Determinants of Protective vs. Impurity Norms

	Dependent variable:			
	Protective		Impurity	
	(1)	(2)	(3)	(4)
log(Female-to-male mortality 1914)	0.164***	0.187**	0.031	−0.116
	(0.052)	(0.085)	(0.058)	(0.084)
log(Mortality: Child below 1 1914)	−0.196***	−0.307***	0.126***	0.335***
	(0.046)	(0.069)	(0.047)	(0.065)
log(Ratio of female-to-male lfp 1933)	−0.061	−0.104	0.060	0.131
	(0.065)	(0.106)	(0.076)	(0.106)
Share of aggricultural pop 1933	−0.115	−0.110	−0.074	−0.107
	(0.121)	(0.192)	(0.140)	(0.188)
Distance to city	−0.002*	−0.002	0.001	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Majority protestant	−0.109***	−0.120***	0.025	0.126***
	(0.019)	(0.025)	(0.018)	(0.026)
Close to border	0.002	0.050	−0.024	−0.045
	(0.028)	(0.046)	(0.029)	(0.048)
Constant	3.066***	11.878***	−5.537***	−11.487***
	(0.761)	(1.060)	(0.676)	(1.043)
Sample	Full	Childbed Norms > 0	Full	Childbed Norms > 0
Observations	7,934	3,604	7,934	3,604
Adjusted R^2	0.063	0.121	0.026	0.102

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Sample is restricted to the German Reich. All specifications additionally control for latitude, longitude and their interaction. Standard errors are clustered on the county level. Ratio of female-to-male labor force participation 1933 as well as the share of agricultural population 1933 are constructed from the social data of the Reichsstatistik 1933 obtained from Hänisch (1989). Female-to-male mortality 1914 as well as the mortality of children below one year of age 1914 is constructed from Galloway (2007). Column (2) and column (4) only include villages that display at least one norm regarding women in childbed.



Notes: Categorization of neighborhood scope answers as categorized by Barruzi-Leicher and Frauenknecht (1966). Answers to part (d)-(e). Multiple answers possible. Neigh. = Neighbors, l=left, r=right, g=across the street. Multiple answers possible.

Figure 3.C.2. Fraction of villages in each subcategory of neighborhood scope

3.C.3 Dissidents

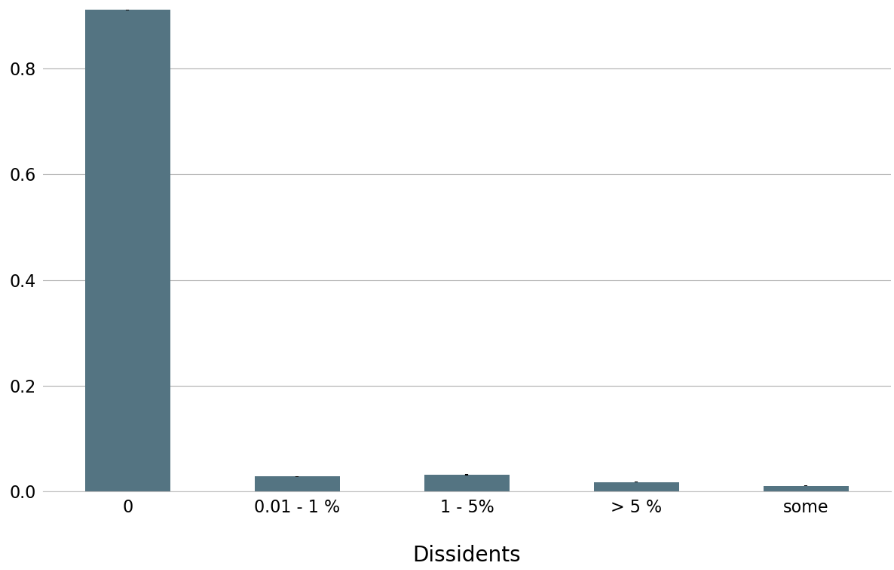


Figure 3.C.3. Frequency of the original coding of the existence of dissidents by Grober-Glück 1966.

3.C.4 Determinants of Social Relationships

Table 3.C.4. Summary Statistics: Other characteristics

	N	Share	Mean	Std	Min	p25	p50	p75	Max
Majority protestant	20,067	0.59							
Heterogeneous	20,067	0.19							
Villages Diff. Rel. 4nn	20,063		0.07	0.19	0.00	0.00	0.00	0.00	1.00
N. Communal Labor	16,908		0.93	1.03	0.00	0.00	1.00	1.00	5.00
Type of Communal Labor									
Poultry		0.24							
Vegetables		0.04							
Fruit		0.08							
Flailing		0.03							
Spinning		0.03							
Flax		0.04							
Distance to city	23,617		10.16	7.17	0.01	5.43	8.76	13.18	86.90
Distance to border	23,617		65.20	54.61	0.01	19.93	48.24	101.04	222.24

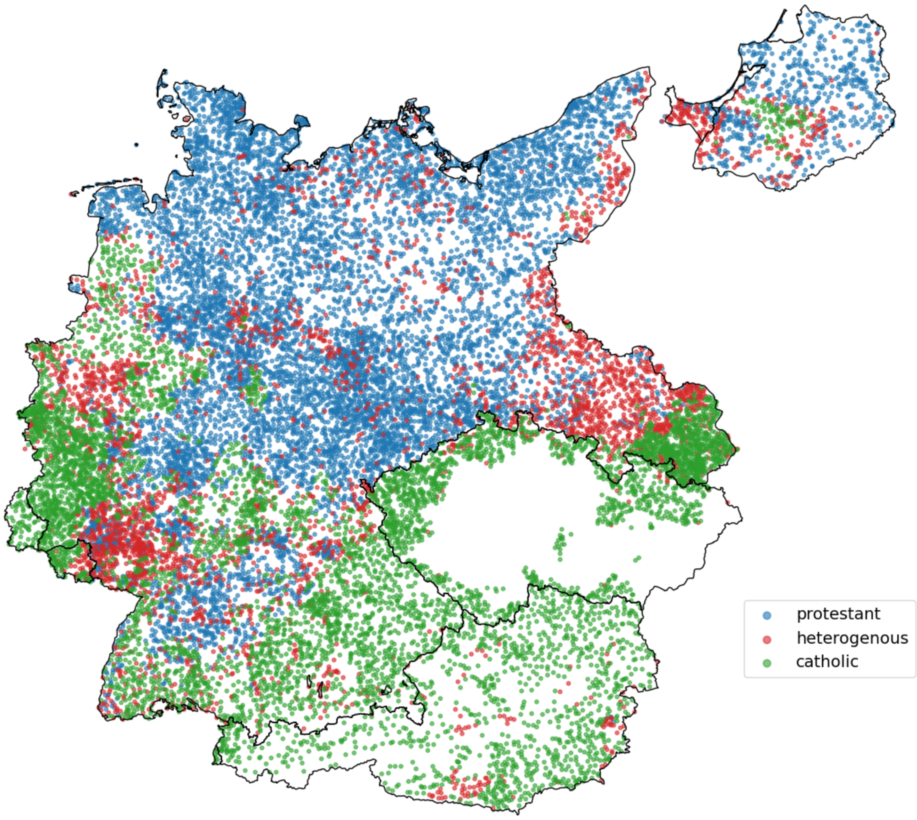
Note: For categorical and dichotomous variables, table displays shares. For continuous variables, table displays mean (sd). Religious composition was asked in all questionnaires.

3.C.4.1 Religion

We want to gauge the accuracy of our religion data by comparing it to previously digitized administrative data. Since administrative data covers the whole of Germany we would expect discrepancies for three reasons: measurement error, differences between rural and urban populations and sampling variation.

The 1925 Reichstatistik reports district level Protestant shares for the whole German Reich. We use the digitized data provided by Falter and Hänisch (1990). To compare these data, we aggregate the village-level GEA data to the district level by taking the means of the interval middles for the Protestant share in each village. Figure 3.C.7 reports a bin-scatter of these comparison. If both datasets were drawn from the same population and without measurement error we would expect all of the black dots to line up on the 45 degree line. This is the case for low Protestant shares. For high Protestant share the GEA data overestimates the district-level Protestant share. That is in close to completely Protestant districts the GEA misses the small Catholic population. One likely reason for this is that this Catholic population lived in cities because these were more hospitable to religious minorities.

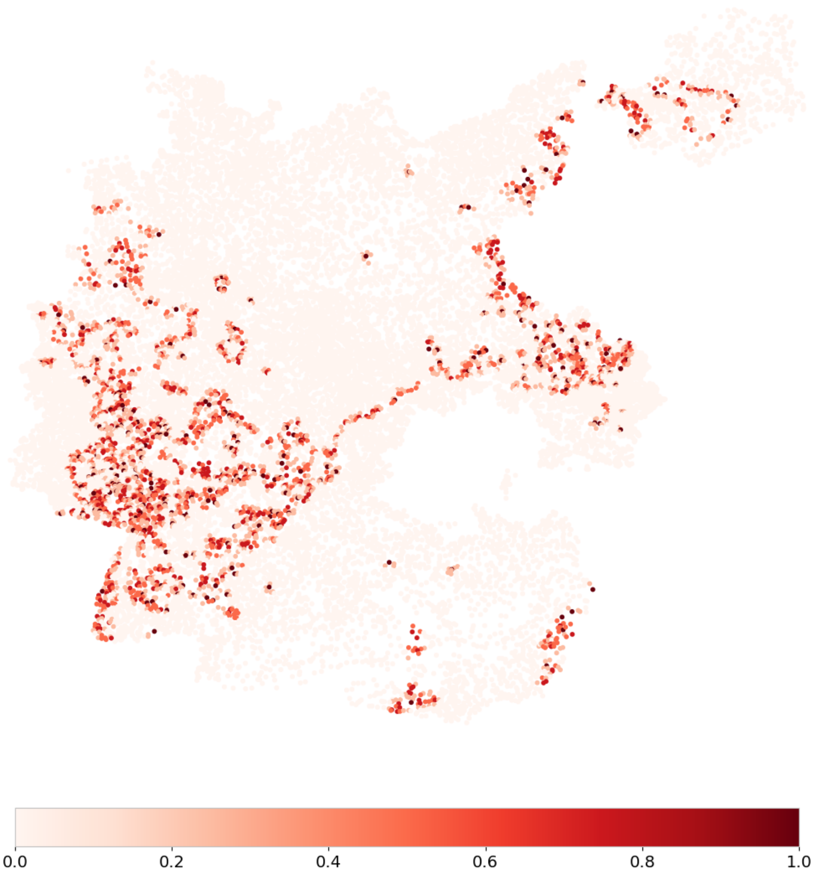
The only other source of religious heterogeneity data is Becker and Cinnirella (2020), who digitize (Prussian) locality level data on religious composition. Becker et. al. calculate a district level dissimilarity index from these data. We calculate the same segmentation index for our data, using a 600km^2 grid. In figure 3.C.8, we reproduce Becker et. al.'s map next to a map of dissimilarity indices calculated from the GEA. The comparison reveals that we come to opposite conclusions for exclusively protestant areas. In our data we do not observe any Catholics in these areas which leads to a dissimilarity index of 0 (unsegregated). In contrast to that Becker and Cinnirella (2020) report a dissimilarity index close to 1 (segregated) because they observe a small share of Catholics clustered in a few places. These places are likely the more heterogeneous cities.



Notes: Heterogeneous villages are drawn in red, homogeneous Protestant villages in blue and homogeneous Catholic villages in green.

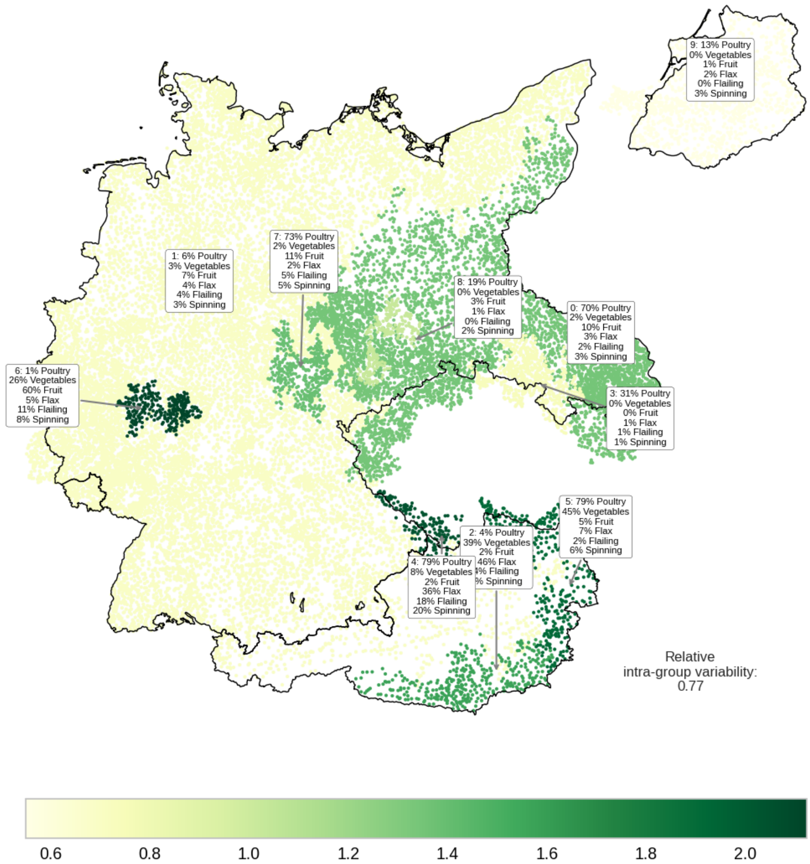
Figure 3.C.4. Geographic distribution within-village religious heterogeneity

The major component of district level dissimilarity is segregation across villages. We measure this (at the village level) by the fraction of a village's four closest neighbors with a different religion. We measure segmentation within villages by the village-level religious minority share. As figure 3.C.9 shows these two dimensions of heterogeneity are both positively correlated to the district level minority share. That is more heterogeneous districts on average have higher within as well as between village religious heterogeneity.



Notes: Across village heterogeneity is defined as the deviation of a village's majority religious denomination from the religious denomination of surrounding villages. The religious denomination of surrounding villages is calculated by using the four nearest villages in our sample.

Figure 3.C.5. Geographic distribution of across village religious heterogeneity



Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=9 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.C.6. Geographic Distribution of Communal Labor Activities

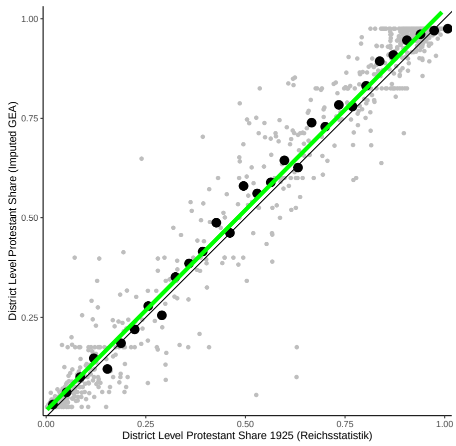
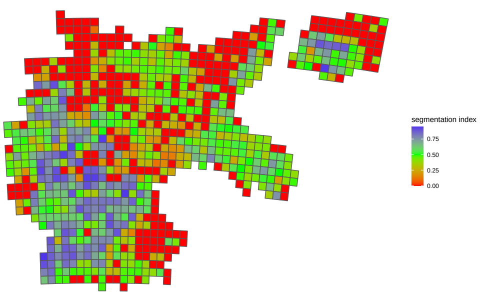


Figure 3.C.7. Bench-marking the (imputed) district level Protestant shares from the GEA against the district-level Protestant shares from the 1925 Reichstatistik (Falter and Haenisch 1990).

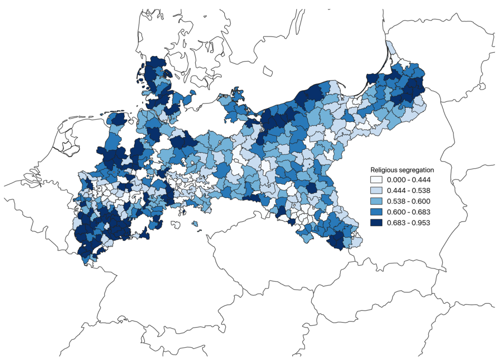
Table 3.C.5. Share of Protestants: original categorization

	Share of Protestants
0 - 4.9%	0.34
5 - 29.9%	0.05
30 - 49.9%	0.01
50%	0.00
50 - 69.9%	0.03
70 - 94.9%	0.09
95 - 100%	0.48

Notes: Shares of observations in original bins as constructed by Grober-Glück (1966)

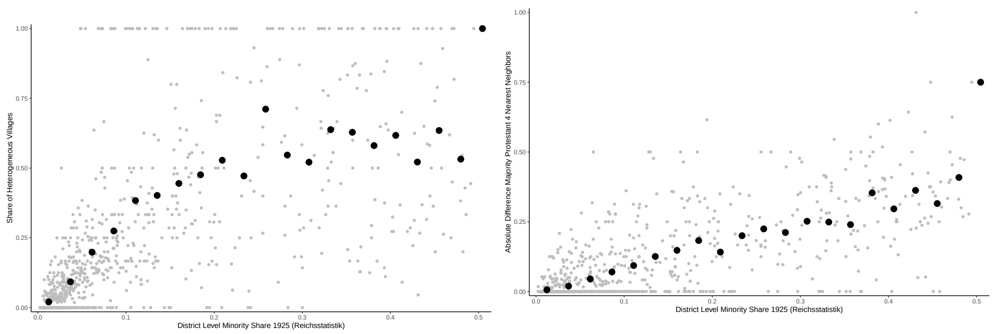


(a) Segmentation index for a 600km^2 grid calculated from GEA data.



(b) Dissimilarity index for Prussian counties in 1871 taken from Becker and Cinnirella (2020), figure 5.

Figure 3.C.8. Religious segregation in the German Reich - comparison GEA data to census data



(a) Villages with a minority share < 5% as a fraction of total villages in the 600km^2 grid cells.

(b) Average (600km^2 Grid) difference in Protestant share between each village and its neighbors in a 20km Radius.

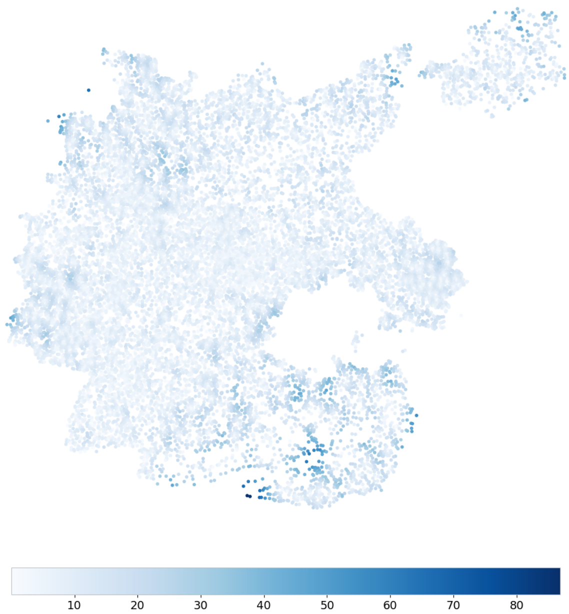
Figure 3.C.9. Different measures of religious segregation/heterogeneity as a function of the population share of the minority in 600km^2 grids

3.C.5 Additional Controls

We measure urbanization by the distance of each village to the nearest city. We extract a list of cities in the German Reich from the 1910 directory of municipalities in the German Reich. Ulli Schubert from https://www.gemeindeverzeichnis.de/ kindly provided us with a digitized version of that directory. For Austria, we use towns as classified by Census, 2001 of Austria Statistics, for the Czech Republic we use municipalities with a population of more than 10,000 inhabitants as of January 2021. For Luxembourg and Liechtenstein, we similarly use current (2016, 2019) census results.

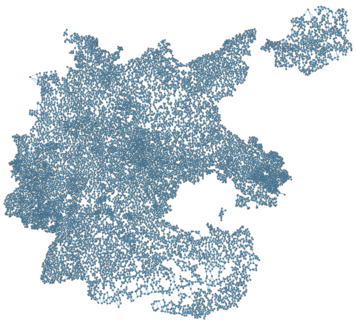
We geocode the location of these cities with the contemporary nominatim geocoder https://nominatim.openstreetmap.org/. For implausible results and for east Prussia (because of the change in language) we manually checked the results of this automated geocoding by hand using Wikipedia and Google Maps. Using the geocoded list of cities, we calculated the distance of each village to the closest city from that list.

(a) Distance to the closest border (in km).

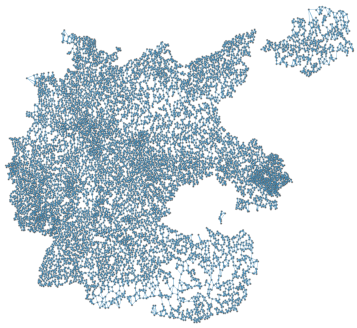


(b) Distance to the nearest city (in km).

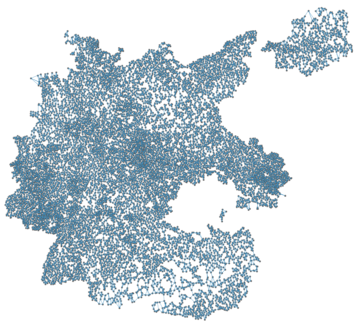
Figure 3.C.10. Geographic distribution of additional control variables .



(a) Connectivity graph questionnaire 2



(b) Connectivity graph questionnaire 4



(c) Connectivity graph dissidents.

Notes: Connectivity graph based on connection between the four nearest neighbors.

Figure 3.D.1. Connectivity graphs

Appendix 3.D Spatial Dependence

3.D.1 Spatial Autocorrelation

Table 3.D.1. Global Spatial Autocorrelation (4NN)

	Moran's I	Chi2	*p*-value
N. Nbh. Help	0.18		$p < 0.01$
Type of Help			
Building house		489.34	$p < 0.01$
Wedding		335.68	$p < 0.01$
Death		409.91	$p < 0.01$
Birth/Sickness		22.72	$p < 0.01$
N. Childbed Norms	0.11		$p < 0.01$
Type of Norm			
Protective		303.10	$p < 0.01$
Impurity		651.40	$p < 0.01$
Dissidents		1173.88	$p < 0.01$
Covariates			
Heterogeneous		6252.09	$p < 0.01$
N. Communal Labor	0.29		$p < 0.01$
Villages Diff. Rel. 4nn	0.61		$p < 0.01$

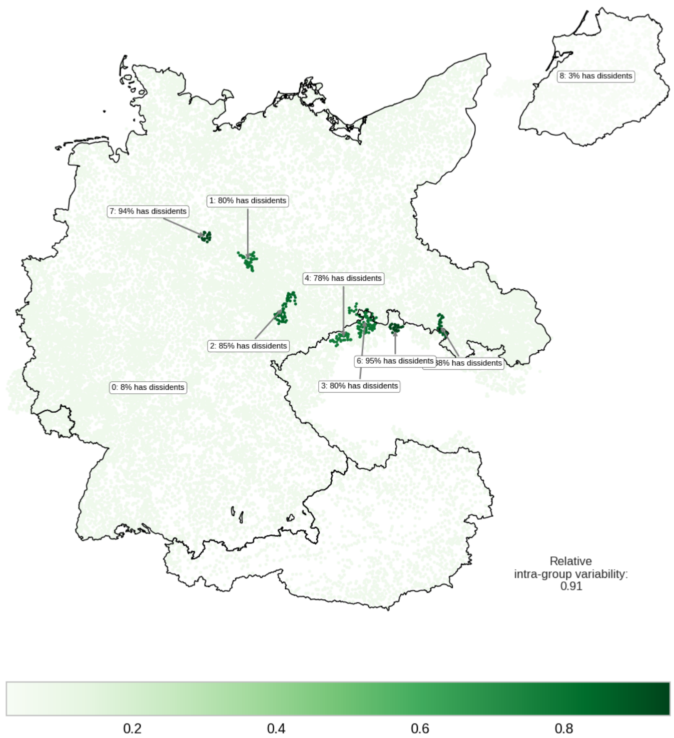
Notes: Reports Moran's *I* for continuous variables and Chi2 of the contingency table of of joint counts for binary variables based on the four nearest neighbors weighting matrix. *p*-values are based on random permutations, respectively.

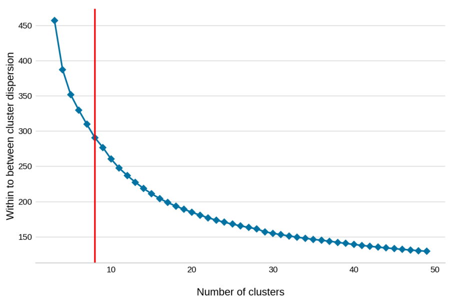
Table 3.D.2. Global Spatial Autocorrelation (Distance 20km)

	Moran's I	Chi2	*p*-value
N. Nbh. Help	0.15		$p < 0.01$
Type of Help			
Building house		3017.59	$p < 0.01$
Wedding		2992.50	$p < 0.01$
Death		3272.50	$p < 0.01$
Birth/Sickness		96.62	$p < 0.01$
N. Childbed Norms	0.10		$p < 0.01$
Type of Norm			
Protective		2729.36	$p < 0.01$
Impurity		4937.24	$p < 0.01$
Dissidents		13019.94	$p < 0.01$
Covariates			
Heterogeneous		45366.86	$p < 0.01$
N. Communal Labor	0.25		$p < 0.01$
Villages Diff. Rel. 4nn	0.31		$p < 0.01$

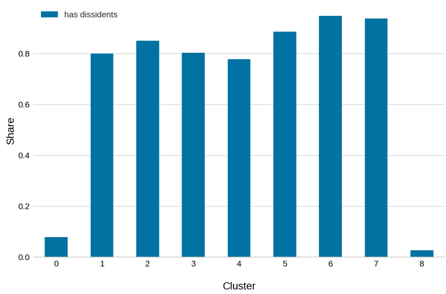
Notes: Reports Moran's *I* for continuous variables and Chi2 of the contingency table of joint counts for binary variables based on the 20km distance-band weighting matrix. *p*-values are based on random permutations.

3.D.2 General Information on Clusters





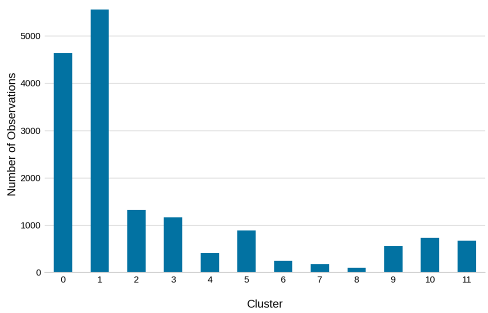
(a) Elbow graph



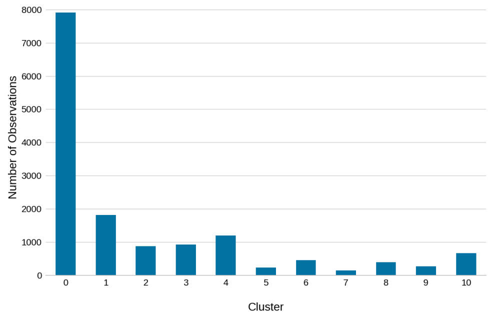
(b) Shares by cluster

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=8 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables. Elbow graph is based on the Calinski-Harabasz metric, which gives the ratio of within to between cluster dispersion. Red line indicates the number of clusters used.

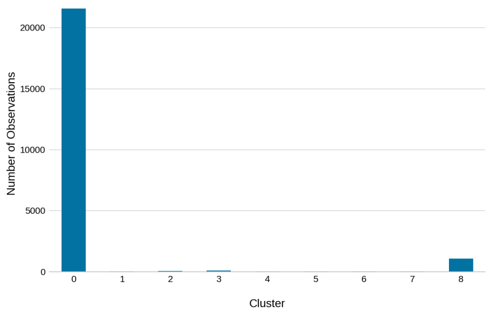
Figure 3.D.2. Clustered Religious Norm



(a) Clustered by Cooperative Norms



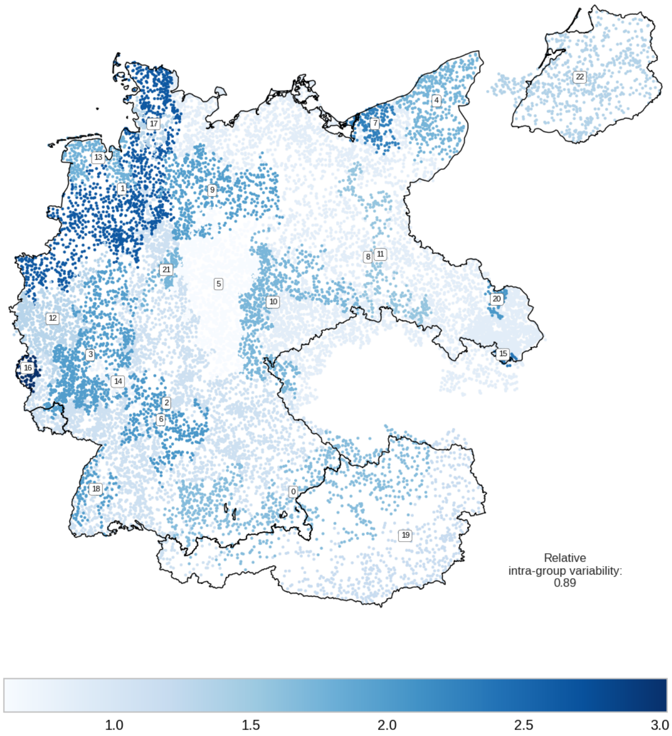
(b) Clustered by Gender Norms

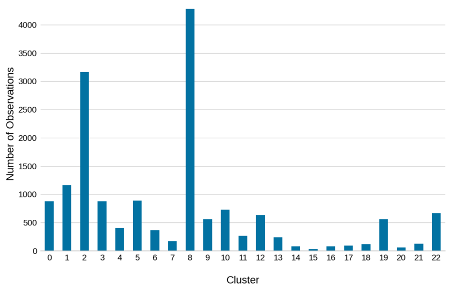


(c) Clustered by Religious Norms

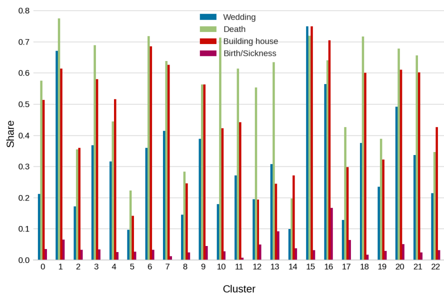
Figure 3.D.3. Number of Observations by Cluster

3.D.3 Doubling the Number of Clusters





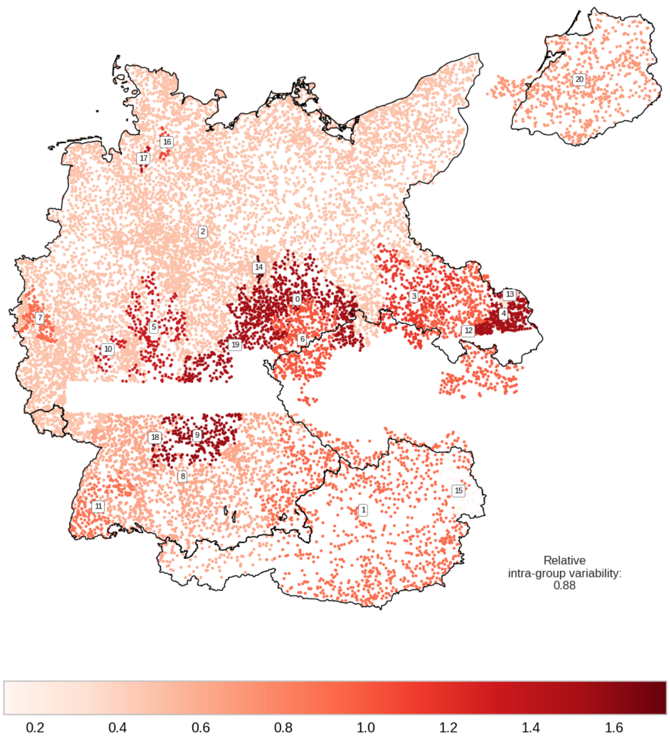
(a) Number of Observations by Cluster

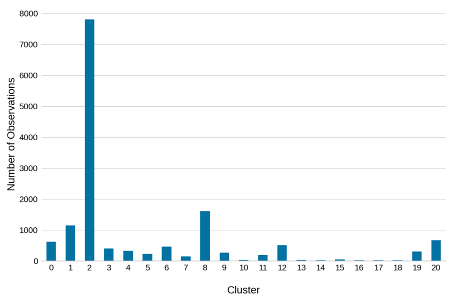


(b) Shares by cluster

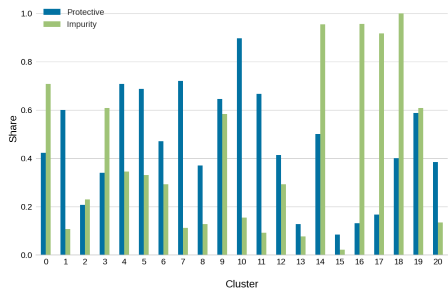
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Variables that are clustered: Help at wedding, death, building a house, birth/sickness. Help at birth and sickness is not available separately. Number of clusters=22 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.4. Clustered cooperation norms





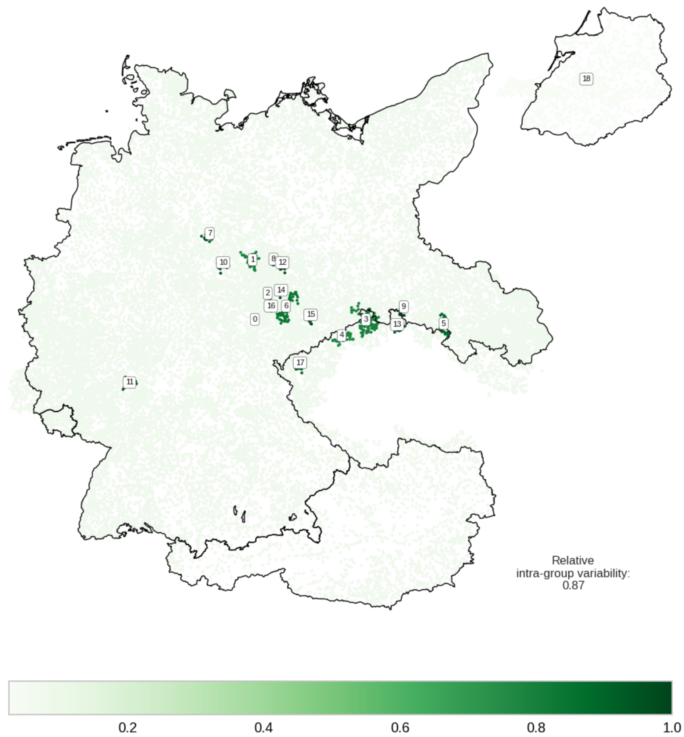
(a) Number of observations by cluster

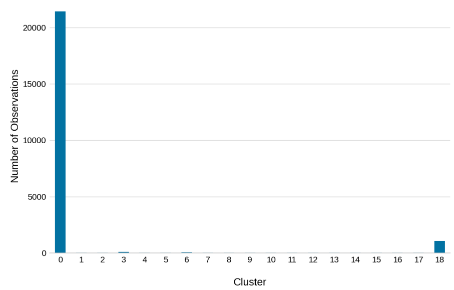


(b) Shares by cluster

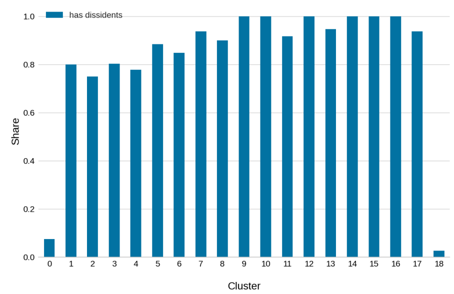
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Variables that are clustered: *Impurity norm*, *Protective norms*. Number of clusters=20 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.5. Clustered gender norms





(a) Number of observations by cluster

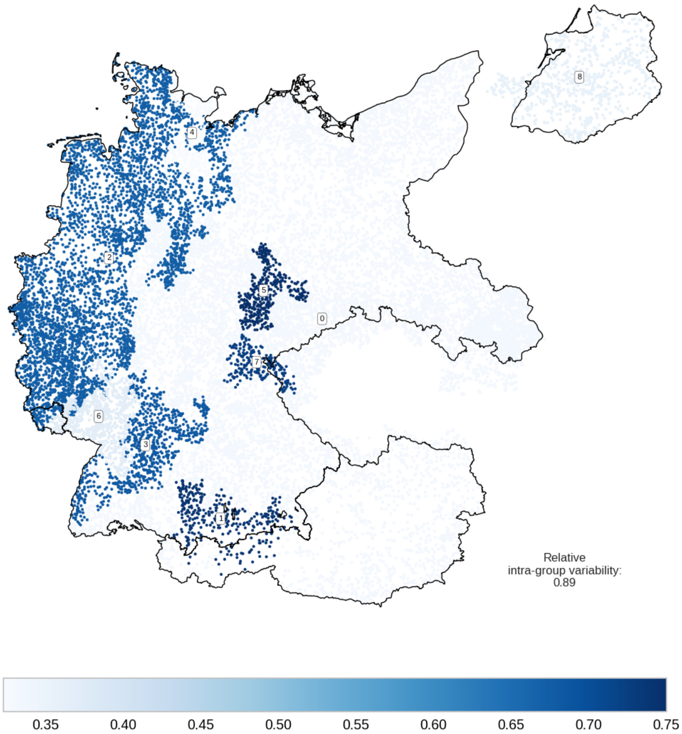


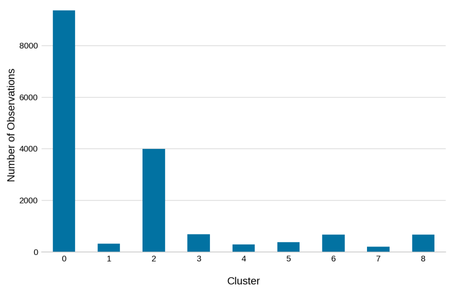
(b) Shares by cluster

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=16 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

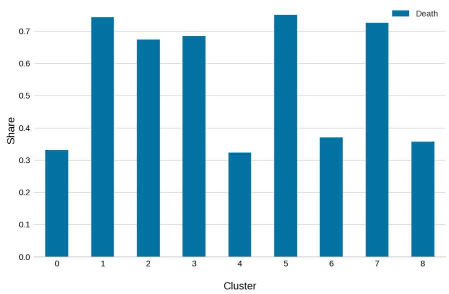
Figure 3.D.6. Clustered gender norms

3.D.4 Each Variable Clustered Separately





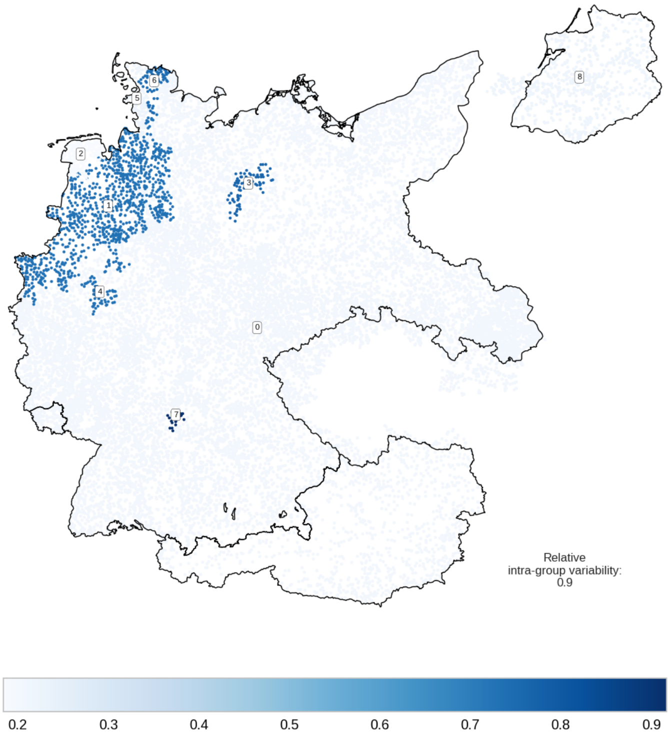
(a) Number of observations by cluster

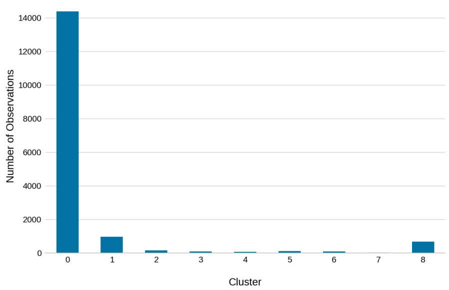


(b) Shares by cluster

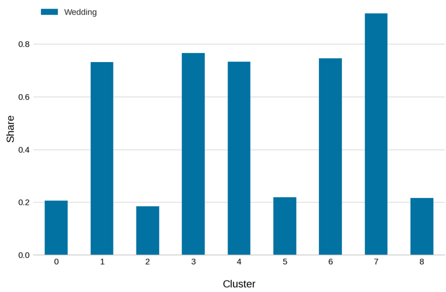
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=8 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.7. Clustered help at death





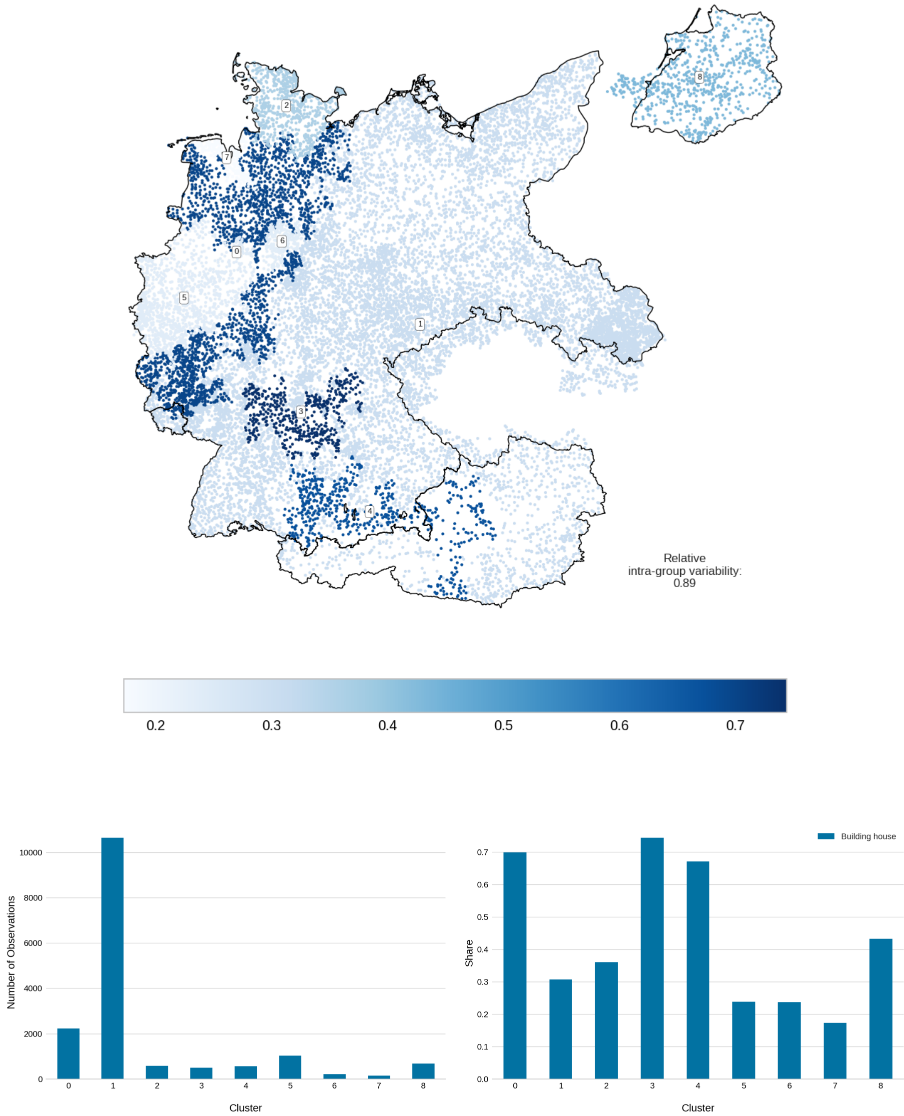
(a) Number of observations by cluster



(b) Shares by cluster

Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=8 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.8. Clustered help at weddings

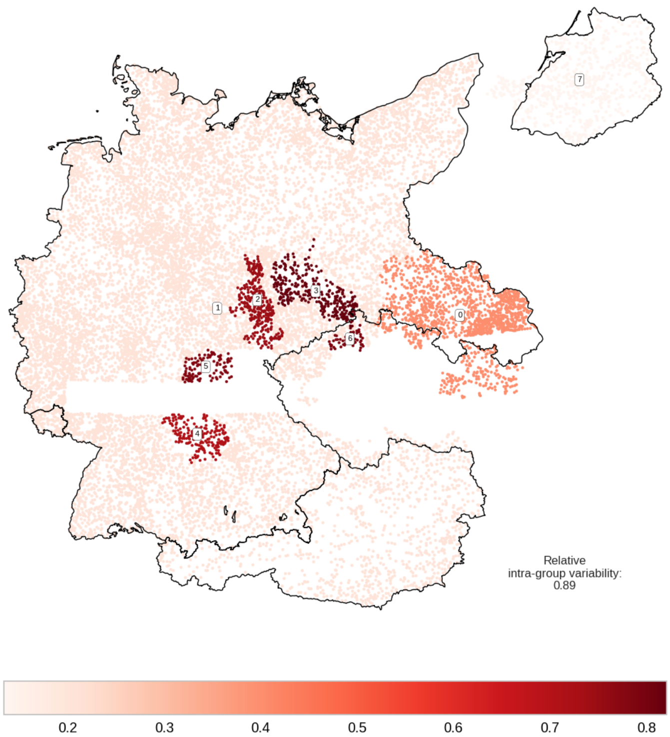


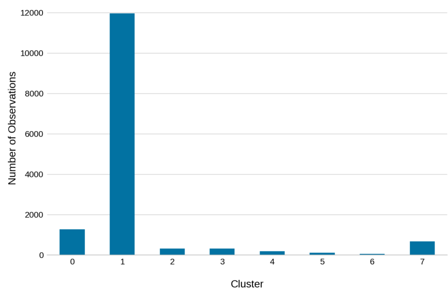
(a) Number of observations by cluster

(b) Shares by cluster

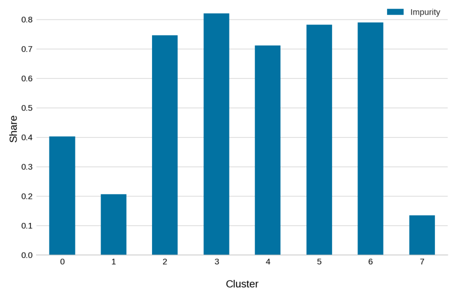
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=8 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.9. Clustered help with house building





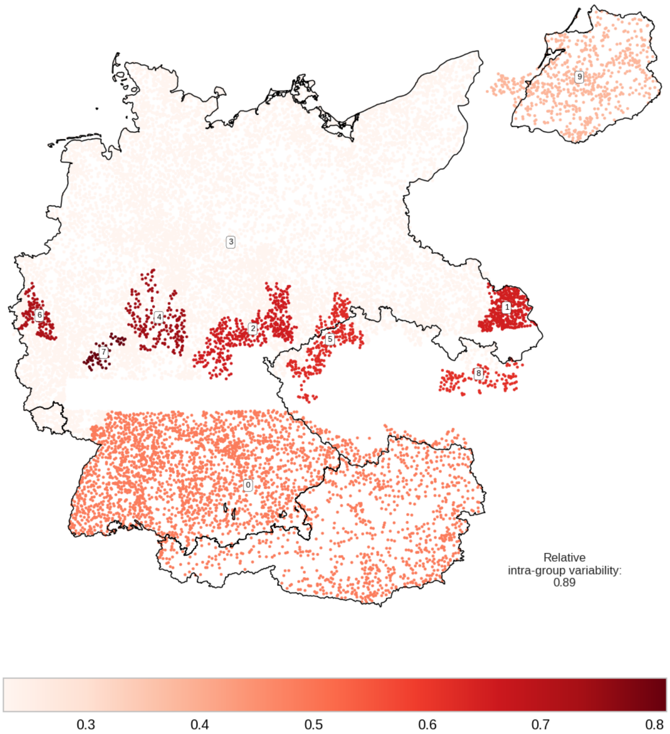
(a) Number of observations by cluster

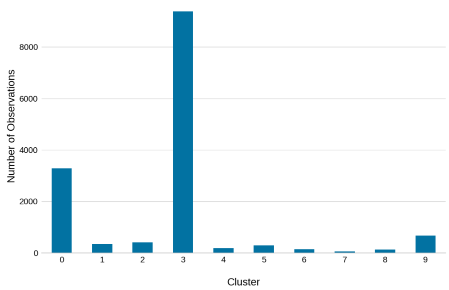


(b) Shares by cluster

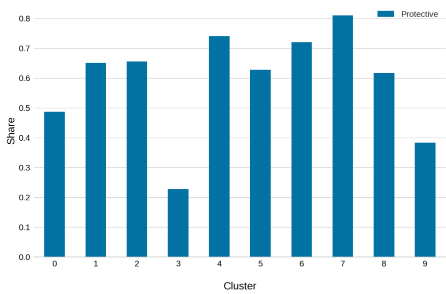
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=7 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.10. Clustered impurity norms





(a) Number of observations by cluster

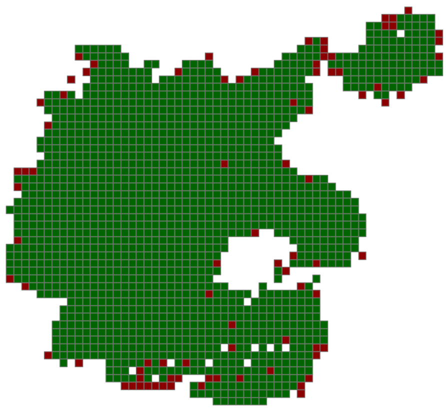


(b) Shares by cluster

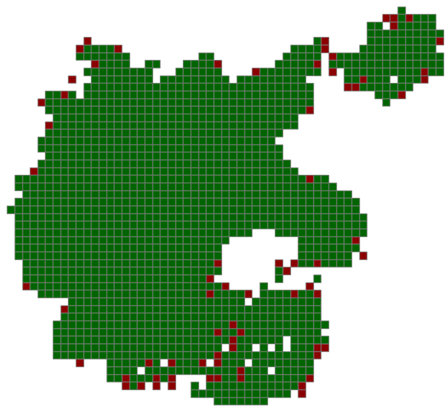
Notes: Clusters are obtained by agglomerative hierarchical clustering under connectivity constraints (four nearest neighbors). Number of clusters=9 + one disconnected component. Relative intra-group variability is defined as the summed intra-group variances scaled by the number of observations in each cluster and meaned across variables divided by the overall variances meaned across variables.

Figure 3.D.11. Clustered protective norms

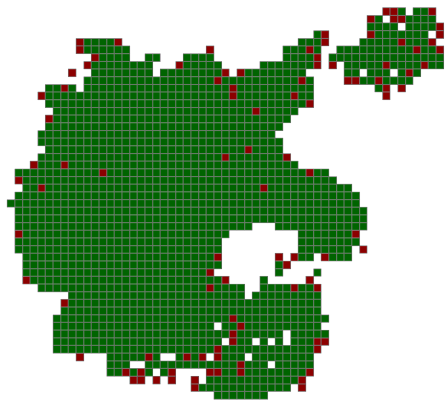
Appendix 3.E Grid Cell Fixed Effects



(a) Questionnaire 2



(b) Questionnaire 4



(c) Questionnaire 2+4

Notes: Red grid cells contain only one data point; green grid cells contain more than one data point. Sample restricted to villages included in questionnaires two or four in the German Reich, Austria, Saarland, Czechoslovakia, Liechtenstein, Gdansk, and Luxemburg. Luxemburg was no longer part of the sample in questionnaire 4. For display purposes we only display the border of the Czech part of Czechoslovakia.

Figure 3.E.1. Points per grid cell by questionnaire 2

Appendix 3.F Communal Labor

Our communal labor variable aggregates different communal labor activities such as poultry, fruit and vegetable processing, flailing, and communal spinning. (Baruzzi-Leicher, 1959) points out that communal labor activities emphasize community building over the specific activity. We follow this claim and use communal labor activities as a proxy of within-community social ties instead of a more general measure of cooperativeness or another measure of cooperation norms. While we cannot check the validity of this approach for every communal labor activity, there is a large historical literature, in particular with respect to communal spinning, that supports it.

Communal spinning describes a regular gathering mostly in a common room provided by the community at which mostly women spun (Baruzzi-Leicher, 1959; Medick, 1980; Göstrich, 1986). It combined work with sociality. Communal spinning enabled the village youth to socialize without control by the married adults of the village. While socializing, such as communal spinning, that is not directly related to collective action has mainly been studied in the context of inter-group conflict, there is evidence from field experiments that working together does lead to the formation of social ties (e.g.: Feinberg, Willer, and Schultz (2014), Fafchamps and Quinn (2018)). Compared to most field experiments and to other collective activities in our data set communal spinning allows us to isolate the relationship forming and information transmitting aspects of social interactions as opposed to the linking of several similar collective activities.

Further historical research reveals that especially the women taking part in communal spinning used it as an opportunity to form life-long friendships. During the winter primarily young women and girls of the village met after lunch at the home of a neighbor for communal spinning. The purpose of communal spinning was either to collect an endowment of laundry to be brought into a marriage or the industrial production of textiles in the workshop system (Medick, 1980; Shnyder, 1996). While spinning the women ate, drank coffee or less commonly brandy and sang, told folktales or talked about village affairs and formed relationships with the other participants (Medick, 1980; Göstrich, 1986; Frey, n.d.). One visible sign of these relationships was the custom that at a woman's marriage, the women she spun with contributed to her endowment. The villagers saw these contributions as a sign of the social exchange relationships she formed with the other women (Medick, 1980). Because community building was central to communal spinning the German words for communal spinning, began to stand for communal village-level gatherings more generally, when the custom of communal spinning began to fizzle out (Medick, 1980). Communal spinning is also often described as one of the precursors of modern rural associations (e.g. Medick (1980) pp.21 and Baruzzi-Leicher (1959)).

Appendix 3.G Robustness Checks

Table 3.G.1. Regressions of norm prevalence in different domain neighborhood help categories on determinants of social relationships.

	Dependent variable:							
	Death		Wedding		House Building		Birth/Sickness	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Heterogeneous	−0.066**	−0.091**	−0.046*	−0.062*	−0.165***	−0.186***	−0.039*	−0.034
	(0.033)	(0.037)	(0.026)	(0.031)	(0.034)	(0.026)	(0.020)	(0.022)
Villages Diff. Rel. 4NN	0.162***	0.087	0.165***	0.041	0.216***	0.074	0.024	0.072
	(0.046)	(0.059)	(0.054)	(0.065)	(0.073)	(0.078)	(0.051)	(0.066)
N. Communal Labor	0.068***	0.063***	0.078***	0.074***	0.084***	0.079***	0.023**	0.030***
	(0.014)	(0.013)	(0.011)	(0.010)	(0.013)	(0.008)	(0.010)	(0.010)
Majority Protestant	−0.003	−0.024	−0.085	0.062	0.020	0.099**	−0.028	0.057*
	(0.067)	(0.060)	(0.080)	(0.056)	(0.058)	(0.045)	(0.025)	(0.028)
Distance to Next City	0.086***	0.066**	0.089***	0.044**	0.108***	0.066**	0.032***	0.008
	(0.014)	(0.027)	(0.017)	(0.021)	(0.018)	(0.025)	(0.012)	(0.016)
Close to Border	−0.001***	−0.001	−0.001***	−0.002	0.0001	0.001	−0.001**	0.002
	(0.0004)	(0.002)	(0.0004)	(0.001)	(0.001)	(0.002)	(0.0002)	(0.001)
Grid FE		✓		✓		✓		✓
Province FE		✓		✓		✓		✓
Observations	13,213	13,213	13,177	13,177	13,237	13,237	13,123	13,123
Adjusted R^2	0.049	0.123	0.041	0.121	0.027	0.118	0.006	0.029

Notes: $^{*}p < 0.1$;** $p < 0.05$,*** $p < 0.01$. Table displays OLS Estimates. Outcome variables have been standardized. We additionally have to exclude some regions in column (5)-(8), because the childbed norms are incomplete in this region as some of the data has been destroyed in the war (for more details see Appendix 3.H). Grid cells have an area of 400km^2; province fixed effects account for the states and provinces (in case of Prussia) of the German Reich, Austria, Gdansk, Liechtenstein, and Czechoslovakia. Close to Border is an indicator variable that is one for the 5% of observation closest to the border. All specifications adjust for latitude and longitude and their interaction. Standard errors are clustered by a 400 km^2 grid and provinces/states.

Table 3.G.2. Regressions of norm prevalence in on determinants of social relationships using alternative measure of between village religious heterogeneity.

	Dependent variable:					
	Norm Index			Unsegmented Neighborhood		
	(1)	(2)	(3)	(4)	(5)	(6)
Heterogeneous	-0.107^{***}	-0.091^{***}	-0.106^{***}	-0.035^{***}	-0.035^{***}	-0.026^{*}
	(0.029)	(0.031)	(0.019)	(0.012)	(0.011)	(0.015)
Deviation from Share of Protestants 4NN	0.269^{***}	0.292^{***}	0.107^{***}	0.038	0.054	-0.001
	(0.059)	(0.053)	(0.035)	(0.035)	(0.035)	(0.033)
N. Communal Labor	0.042^{***}	0.051^{***}	0.055^{***}	0.009^{*}	0.009^{*}	0.014^{***}
	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Sample	Full	Full	Full	Nbh. Help > 0	Nbh. Help > 0	Nbh. Help > 0
Grid FE			✓			✓
Province FE			✓			✓
Observations	11,900	11,900	11,900	8,159	8,159	8,159
Adjusted R^2	0.013	0.030	0.158	0.002	0.005	0.035

Notes: $^{*}p < 0.1$;$^{**}p < 0.05$,$^{***}p < 0.01$. Table displays OLS Estimates. Outcome variables have been standardized. We additionally have to exclude some regions in column (5)-(8), because the childbed norms are incomplete in this region as some of the data has been destroyed in the war (for more details see Appendix 3.H). Grid cells have an area of 400km^2; province fixed effects account for the states and provinces (in case of Prussia) of the German Reich, Austria, Gdansk, Liechtenstein, and Czechoslovakia. Close to Border is an indicator variable that is one for the 5% of observation closest to the border. All specifications adjust for latitude and longitude and their interaction. Standard errors are clustered by a 400 km^2 grid and provinces/states.

Appendix 3.H Digitization

The anthropologists' drew their coding of the original material was drawn on maps (see Zehnder, 1958). The data used in this paper is created through digitizing these maps as well as the list of villages included in questionnaire 1 to 4 and matching these two. There is no official list of villages that are included in questionnaire 5.

Our digitization procedure went as follows. We first georeference the scanned maps and then vectorize the points on the maps. We then geocode the list of villages according to the customized geographic coordinate system used to draw the original maps. Third, we match the vectorized points to the geocoded list of villages.

The researchers of the GEA created their own coordinate system in which the map of Cental Europe was divided into a rectangular grid and each village was assigned a four to five part coordinate. The first coordinate divides the map into 287 large rectangles. The rectangles are displayed in Figure 3.H.1 The second coordinate divides the large rectangles into 36 smaller rectangles, the third coordinate divides these smaller rectangles again into 25 small rectangles, and the fourth coordinate divides each of these 25 rectangles into another four rectangles indicated by letters a-d. The fifth coordinate is only assigned when two villages are directly next to each other, i.e. to ensure uniqueness. It is indicated by the letters l,r,o,u, where l is left, r is right, o is up, u is down. So an example coordinate is, thus, given by 105 2 25 al. The customized grid does not follow any standard coordinate system and we recreated this digitally using the description contained in official list of villages.

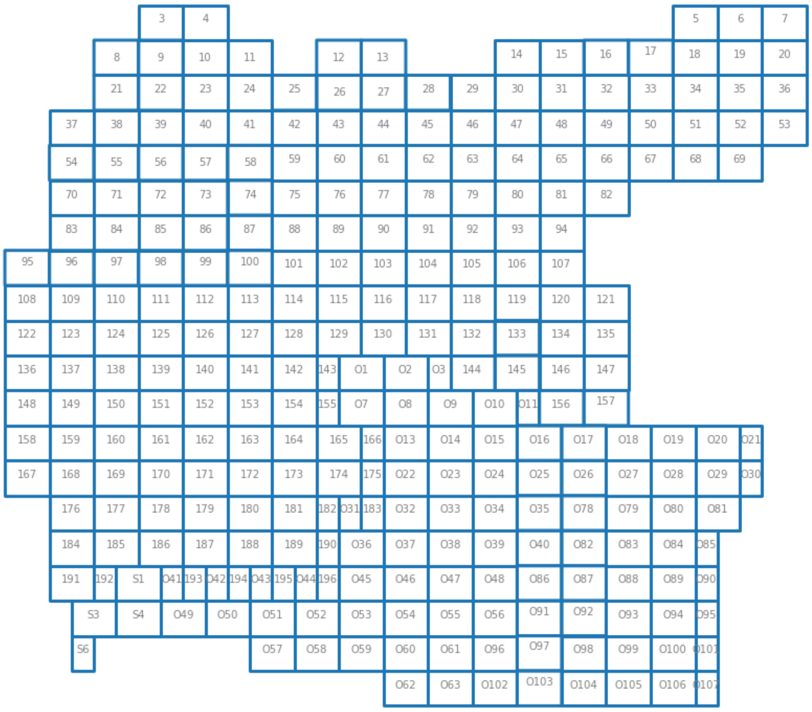


Figure 3.H.1. Large rectangles

We match the vectorized data to the geocoded list of villages by assigning a vectorized point to the geocoded village closest to it. It is important to note that some points on the maps drawn by Zehnder (1958) cannot be assigned to any village contained in the list of villages. This may be due to two reasons. First, it may be that this is a drawing mistake, second it may be that the list of villages is incomplete. Here it is important to note that the list of villages is an old document that contains a lot of handwritten notes on top of the normal typed list. On top of the list of villages, however, there exist maps that contain all villages: basemaps by questionnaire drawn in the interwar period by Röhr and Harmjanz (1936), the map of religious denomination, as well as the map of the number of communal labor activities (for questionnaire 2). We call them *basemaps*. In case where we cannot assign a point to a village within a radius of at most 3km, we investigate whether the point is contained on any of the basemaps. If yes, we add the point to the list of villages, if not we remove the point. The vast majority vectorized points (more than 90%) can be matched to a village less than 1 or 2 km away.

Women in Childbed

The answers to the question about rules for women in childbed are partly unreadable and destroyed by war. This affects answers within the rectangles 142, 144-147, 150-157 (Grober-Glück, 1966). We, thus, drop these from our analysis when using this variable.

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