REPUTATION EFFECTS IN PEER-TO-PEER ONLINE MARKETS:

meta-analyses and laboratory experiments

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Reputation effects in peer-to-peer online markets: meta-analyses and laboratory experiments

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CHAPTER

Synthesis

1.1 Background

1.1.1 Development of reputation systems

From the spread of 'gossip' through spoken language in prehistoric communities (Dunbar, 2003) to the rating scores or the number of 'stars' of a hotel on websites like booking.com, reputation has always played an important role in maintaining social cohesion in human societies (Diekmann & Przepiorka, 2019). Due to its importance in facilitating successful commerce, the reputation information of transaction partners has taken various forms in accompanying our daily life, either carved in stone in ancient Mesopotamia, recorded in logbooks in Renaissance Italy, or rated digitally and anonymously as open information on the Internet nowadays (Rule & Singh, 2012). Unlike the traditional word-of-mouth (WOM) reputation that is very much limited to personal relationships, online environments provide the possibility of electronic word-of-mouth (eWOM) communication that forms a large scale of ratings and reviews open to the public, leading to the rise of online reputation systems (Dellarocas, 2003; Gruen et al., 2006; Jones & Leonard, 2017).

With the rapid development of Internet technology, the rise of online markets has dramatically changed where, how, and with whom we engage in economic exchange. Between 2014 and 2019, there was a notable increase in the proportion of retail goods purchased online globally, with the figure rising from 6% to 13%, according to Euromonitor International.[1] Online shopping not only offers consumers the possibility to freely compare prices of products or retailers, but also the convenience of shopping from home without visiting a store physically (Aryani et al., 2021).[2] However, when faced with seemingly countless choices of products and product providers, how do consumers choose the best products and minimize the potential risk of obtaining inferior products?

The commonly used reputation systems in online markets facilitate establishing trust in a potential transaction partner who is often anonymous and geographically distant. In the widely used transaction process, buyers

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1 https://www.forbes.com/sites/michelleevans1/2020/05/19/7-predictions-for-how-covid-19-will-change-retail-in-the-future/?sh=38e2d5c35be3

2 The latter is especially important after the outbreak of COVID-19 pandemic, and indeed more consumers are shifting to e-commerce since then.

need to pay before receiving the goods or services they ordered. Therefore, buyers will naturally need to take the risk that they might not receive the good or receive something inadequate to the agreed quantity or quality. To avoid disappointment, it is the buyer's own responsibility to make wise choices regarding reliable transaction partners.

1.1.2 Distinguishing important concepts

Before we delve into the mechanisms of the functioning of reputation systems, it is important to make three distinctions: (1) between peer-to-peer (P2P) and business-to-consumer (B2C) online markets; (2) between reputation systems and recommender systems; (3) between 'open' and 'closed' reputation systems.

First, in P2P online markets (also known as consumer-to-consumer or C2C online markets), buyers interact and transact directly with individuals, instead of professional business partners as in B2C online markets. Such business partners can have substantial brand reputation. This reputation influences the prediction for its actions as "customers anticipate a brand will meet their expectations, formed by existing reputation" (Veloutsou & Moutinho, 2009, p. 315) and large user-base, such as Amazon.com. Researchers have argued that trust is essential in both types of markets (Möhlmann, 2015), but trust may come from different sources in the trust model for e-commerce, namely disposition to trust, institution-based trust and trusting beliefs (McKnight et al., 2002). Here we focus on P2P online markets such as eBay and Taobao,[3] where interpersonal trust between sellers and buyers plays a more important role for transaction success.

Second, a reputation system can be defined as a socio-technical structure through which third-party information about an actor's past behavior is recorded, aggregated, and transmitted (e.g., Resnick et al., 2000). A recommender system can be defined as a software tool or technique that suggests items that are of potential interest to a particular actor (see Ricci et al., 2015; Palopoli et al., 2013, 2016). Both reputation systems and recommender systems are employed by online market platforms and fueled by user-generated data but should be

3 Taobao.com, run by Chinese e-commerce giant Alibaba Group Holding Ltd., is one of the world's largest shopping sites. Its gross merchandise value (GMV) ranks third globally in 2022. See https://ecommercedb.com/marketplace/taobao-258 (accessed in June 2023).

kept distinct. The focus of this dissertation is on reputation systems in online markets.

Third, an 'open' reputation system (e.g., TripAdvisor) allows anyone with a registered account to leave feedback. Open reputation systems allow for unsolicited, unverified and potentially fake feedback ratings.[4] Our research focuses on 'closed' reputation systems, where feedback can only be provided by the users who participated in a particular transaction (e.g., eBay, Taobao).

1.1.3 Trust and social embeddedness in reputation systems

According to economic and marketing theory, successful transactions will only take place when sellers and buyers trust each other, and a good reputation from previous experience will induce such mutual trust and lower the risks for both parties (Andrews & Benzing, 2007; Swan & Nolan, 1985). From the perspective of sociology, reputation can be conceived as a form of social embeddedness to promote cooperation by the mechanisms of "learning" information on potential interaction partners past behavior and "controlling" the course of action of these interaction partners in the future (see Buskens & Raub, 2013; Diekmann & Przepiorka, 2019). Thus, the reputation systems widely used nowadays are aimed at solving the trust problem by substituting the lack of social embeddedness in an "extreme form of anonymous, long-distance interactions" (Snijders & Matzat, 2019, p.2) on online platforms such as eBay. This is especially relevant when most transaction partners do not expect to encounter each other again and sellers have incentives to abuse buyers' trust. The reputation system artificially establishes a network that connects buyers and sellers by allowing them to rate and review each other based on their past transaction experiences, so opportunistic behaviors (e.g., a seller abusing a buyer's trust) are less wise or favorable in consideration of future transaction success. In reputation-based P2P markets, new sellers without a record of past transactions will need to provide lower prices to compensate buyers' higher risk from dealing with sellers without a reputation. However, once these sellers obtain a good reputation, this

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4 Critics of online reputation systems often cite a well-known case involving a British journalist who fabricated a fake restaurant called "The Shed" on the website TripAdvisor and used fake reviews to make it a "top-rated" restaurant in London. This example highlights the importance of having a robust reputation system that can effectively filter out fake or malicious feedback ratings to prevent fraudulent activities.

initial investment will be reimbursed (Diekmann et al., 2014; Przepiorka, 2013; Resnick & Zeckhauser, 2002; Shapiro, 1983).

1.1.4 Remaining questions about reputation systems

In spite of their ability to elicit more reliable and valid feedback information than open reputation systems, some problems regarding closed reputation systems have also been identified. For example, reputation information can be biased because insufficient feedback is provided, or feedback follows a u-shaped distribution indicating that feedback is left mainly if the experience was extremely good or bad (Meijerink & Schoenmakers, 2019; Veldhuizen, 2020). Researchers have identified fraudulent behaviors leading to inaccurate or false feedback in online markets that undermine the integrity and reliability of reputation systems such as brushing, i.e. placing fake sales to inflate sales and positive ratings (Liu et al., 2023). Other studies found that user-generated feedback in reputation systems can be systematically biased (Dellarocas & Wood, 2008; Nosko & Tadelis, 2015; Tadelis, 2016). For example, Nosko and Tadelis (2015) show that the percentage of positive ratings that is publicly shown for the sampled eBay sellers has a mean of 99.3% and a median of 100%. Yet, their paper also shows that for the same sample of sellers, there are two times more complaints registered with customer service than negative feedback scores, indicating that the observable seller reputations are positively biased compared to actual seller quality. The reason could be that buyers often avoid leaving negative feedback because of fear of seller retaliation and harassment.

Other questions regarding the effectiveness of reputation systems focus on the size of the reputation effect. Most studies in related fields tend to focus on how a better seller reputation would increase product prices or selling volumes by examining reputation effects with digital trace data from online markets. As stated by Andrews and Benzing (2007), online markets have become a real-world laboratory for researchers to investigate how seller reputation affects selling success using empirical data captured from online market transactions. However, it remains an open question what the sizes of these reputation effects really mean.

Researchers have interpreted the reputation effect with substantial meaning, e.g., Cabral and Hortaçsu (2010) reveal a statistically insignificant finding that a 1% increase in the fraction of negative feedback rating corresponds to a price reduction of 7.5% decrease in price. The interpretation of the coefficients of reputation effect needs to be paid extra attention because these estimated coefficients do not seem to be related to the benefits that sellers achieve due to the good reputation of themselves but are more likely to depend on the distribution of reputations of others (Snijders & Matzat, 2019, p14).

Another problem with the interpretation of the reputation coefficients is the consideration of endogeneity (Tadelis, 2016), as there might be other factors correlated with a good reputation and better selling performance that are not included in the analyses, e.g. seller experience. For instance, experienced sellers may achieve better selling performance not only because they have higher rating scores, but also, because they provide more detailed product descriptions.

A few studies provide a review of the findings of empirical studies investigating the effect of reputation on selling performance (e.g. Andrews & Benzing, 2007; Cabral & Hortaçsu, 2010; Diekmann et al., 2014; Tadelis, 2016), and they show that results are not consistent.

Intuitively, it makes sense to argue that, to prove that reputation systems are working effectively, the effect reputation has on seller success needs to be large. Yet, in many previous studies, the reputation effects are estimated to be small (Livingston, 2005; Snijders & Matzat, 2019; Standifird, 2001). Why are reputation effects detected from previous research sometimes small or insignificant? It might be due to the heterogeneity in (1) the targeted product that is chosen for each research (Bajari & Hortaçsu, 2005; cf. Resnick et al., 2006), (2) the difference between professional and amateur sellers (Snijders & Matzat, 2019); or as Livingston (2005) argues, (3) the positive reputation effects are more substantial when sellers have only a few positive ratings, which implies that the large volume of positive ratings may potentially undermine the effectiveness of the reputation system. Some researchers also argue that sellers with higher past ratings appear to offer goods at lower prices but for the same quality. The mechanism could be that reputable sellers learn that ratings are responsive to buyer surplus (which has both a quality and a price dimension). By keeping offering discounts, sellers could maintain high ratings (Solimine & Isaac, 2023). However, this is unlikely to be a sustainable pricing strategy as it would take a rather long time to compensate the initial investment in building a good reputation.

Even though the observed reputation effects might not always be substantial, researchers generally affirm the effectiveness of the reputation system to some extent. Bolton et al. (2004) argue that what makes the reputation system work is its existence. On the other hand, Bajari and Hortaçsu (2004) believe that the current evidence suggests that "the jury is still out on the effectiveness of the reputation systems" (p.20), and researchers need to further explore how market participants utilize reputation information in their decision making in online markets. Interestingly, some researchers describe it with "Yhprum's Law" (Yhprum is Murphy spelled backward): a reputation system that "merely reduces the lure of cheating and chiseling without eliminating it" may work well enough (Arenas et al., 2010; Jøsang, 2011; Resnick et al., 2006). However, there is broad consensus that reputation systems would be ineffective if too little truthful feedback was left after completed online market transactions.

Participation in the reputation system means voluntarily leaving feedback after transactions. As Diekmann et al. (2014) stated, The Achilles' heel of a reputation system remains market participants' willingness to rate each other. If participants do not provide truthful ratings, the market loses its capacity to identify fraudulent traders and reputation loses its value. Buyers may find the reputation system especially useful while making a decision on whether to make a transaction. At the same time, these buyers may not have enough incentive to leave feedback after finishing a transaction, which will result in a low feedback rate. Given the important role of feedback rates in online reputation systems, how exactly do they shape sellers' and buyers' decision-making? And how do they affect the reputation effects of a certain reputation system?

In line with Przepiorka (2013), we argue that there is a negative relationship between the rate of truthful feedback and the size of reputation effects. When there is a high probability of getting truthful feedback after each transaction, untrustworthy traders will be quickly identified and disincentivized to enter the market. Consequently, with mostly trustworthy traders entering the market, buyers will demand fewer discounts from new traders without a good reputation. Therefore, the reputation mechanism still works as it should even though the detected reputation effects are small and sellers with a better reputation do not necessarily exhibit a better selling performance.

1.1.5 Research aim and framework

In conclusion, while reputation systems are designed to solve trust problems in online market platforms by providing users with a way to gather information about the trustworthiness of potential transaction partners, how seller reputation information affects selling performance remains ambiguous. Therefore, we use meta-analysis to synthesize evidence from over a hundred empirical studies investigating the reputation effect in peer-to-peer online markets (see Chapter 2), and a meta-analytic model selection approach and multi-model inference to identify potential moderators of reputation effects (see Chapter 3). Despite the usefulness of the meta-analytic methods in confirming the general existence and explaining the variance of reputation effects, the conclusion is still limited as the crucial factor – feedback rates of reputation systems are rarely measured and reported. Consequently, we investigate how exactly seller reputation could impact the decision-making of sellers and buyers across diverse online markets with systematically varying feedback rates by conducting a series of controlled laboratory experiments (see Chapter 4).

In the remaining sections of this chapter, we first present the fundamental theoretical propositions of this dissertation, including our game-theoretic conceptualization of the role of the reputation system in online exchanges, the mechanisms bringing about the reputation effect and arguments on its interpretation, as well as the relation between the feedback rates and the size of the reputation effect. Subsequently, we provide an overview of each chapter, followed by the highlights of our methodological contribution in this dissertation. Lastly, we discuss the principal findings and provide suggestions for future research.

1.2 Theoretical arguments

1.2.1 Conceptualizing the reputation effect

The interaction between a buyer and a seller in an online market is usually conceptualized as a one-shot trust game with incomplete information (TGI; Güth & Ockenfels, 2003, see Figure 2.1 in Chapter 2) with the assumption that both parties have never encountered each other before, and they are not expected to encounter each other again. Accordingly, online exchange can be conceived as a sequential game with different stages: the buyer first decides whether to trust the seller and pay for the product; upon receipt of the money, the seller decides whether or not to ship the item with the same quality and/or quantity that was agreed upon with the buyer. Building on the standard trust game, the TGI accounts for the uncertainty of the buyer regarding the seller's trustworthiness. That is, in the TGI, the trust problem is modeled with the buyer being in one of two games, the assurance game (AG) or the standard trust game (TG), with a certain probability (α or 1 – α, respectively).

The two games merely differ in the order of seller payoffs. In the TG, the payoffs are ordered such that the seller would not ship ($T > R$) and, therefore, the buyer does not buy ($P > S$). As a result, the exchange does not take place, and both parties earn payoff P, which is lower than the gains from trade R. In the AG, the seller has an additional benefit b for shipping and/or cost c for not shipping, such that $T - c < R + b$.[5] This order of payoffs implies that, in the AG, the seller has an incentive to ship if the buyer buys. The uncertainty of the buyer is modeled by a so-called move of nature (N) determining which game the buyer is in. While the seller knows whether they are in the AG or the TG, the buyer only knows the probability α of being in the AG. This is denoted by the dashed line connecting the two decision nodes of the buyer.

Knowing α and the TGI payoffs, the buyer calculates the expected payoff from either of their two actions and takes the one that maximizes their expected payoffs. If the buyer buys, their expected payoff is $\alpha R + (1 - \alpha)S$. If the buyer does not buy, their payoff is P in either case. The buyer chooses 'buy' if doing so results in a larger expected payoff than choosing 'not buy', that is, if $\alpha R + (1 - \alpha)S > P$. Since the payoffs of the TGI are fixed, we solve this equation for α and obtain

$$\alpha > \alpha^* = \frac{P - S}{R - S} \tag{1}$$

5 In the AG, the honest sellers might just have social preferences either of the type that increase their utility if the buyer's utility is increased (+b) or of the type that decreases their utility if the buyer's utility is decreased (–c) or both (Becker, 1976; Fehr & Schmidt, 1999).

Hence, if α, the probability of being in the AG, is larger than α^*, the buyer buys and abstains from buying otherwise. Note that α^* is determined entirely by the buyer's payoffs.

Reputation, sometimes referred to as "the shadow of the past" (Diekmann & Przepiorka, 2019, p. 9), carries information about the expected action of sellers. Consequently, sellers with a good reputation are perceived as trustworthy. Otherwise, buyers are less inclined to place their trust in sellers without a good reputation unless they make discount offers that ensure buyers' payoff while diminishing their own. For buyers to be indifferent between a seller with a good and a seller with an unknown reputation, buyers' payoffs from buying from either of them must be equal. Since a seller with a good reputation is in the AG, a buyer's payoff from buying from that seller is R. Recall that a buyer's expected payoff from buying from a seller with an unknown reputation is $\alpha R + (1 - \alpha)S$. Hence, sellers with an unknown reputation must offer a discount d, such that $\alpha R + (1 - \alpha)S + d = R$. When solving this equation for d, we obtain

$$d = (1 - \alpha)(R - S). \tag{2}$$

In game-theoretic terms, seller reputation is conceived as a costly signal of seller trustworthiness: (1) new sellers are required to make investments by offering lower prices to attract buyers' trust for some time; (2) since not all potential sellers are patient enough to incur these costs, potential buyers can infer sellers' trustworthiness from their good reputation (Dellarocas, 2003; Przepiorka, 2013; Przepiorka & Berger, 2017; Shapiro, 1983). Based on this model we can formulate our main hypothesis that sellers' reputations and their selling performance in terms of sales and prices will be positively correlated. That is, sellers with a better reputation realize higher prices and larger selling volumes. Such a correlation is commonly called *reputation effect*.

1.2.2 Feedback rate

As an indicator of trustworthiness, sellers' feedback ratings generated in a reputation system play a significant role in the online market, and obtaining a good reputation is an important stepping stone to sellers' success in a market. However, feedback ratings in reputation systems can be regarded as a public good that is under-provided (Lafky, 2014) as market participants are not always motivated enough to leave a feedback and it can be time consuming. Providing truthful ratings after transactions is largely motivated by reciprocity, altruism, and strategic motives (Dellarocas et al., 2004; Diekmann et al., 2014; Diekmann & Przepiorka, 2019). Some online markets attempt to take measures to provide additional incentives for buyers to enhance the feedback rate, such as through feedback payment schemes (monetary rewards for submitted feedback) (Jurca & Faltings, 2006). For instance, Taobao introduced a feedback reward mechanism called Rebate-for-Feedback (RFF), but such a measure could be problematic as it may cause biases in feedback ratings (Li et al., 2020).

Since sellers are not always rated (truthfully) after a transaction, the less frequently sellers are rated truthfully, the longer it will take to screen untrustworthy sellers and for trustworthy sellers to build their reputation. Consequently, more untrustworthy sellers will have an incentive to enter the market, which in turn will oblige trustworthy sellers to offer larger discounts for buyers to trust them. In other words, the lower the rate of truthful feedback, the higher will be the initial investment trustworthy sellers have to make to build their reputation. Therefore, we assume the feedback rates are negatively correlated with the size of reputation effects. This is the central claim that this dissertation sets out to test by means of meta-analyses and behavioral laboratory experiments.

1.3 Summary of each chapter

1.3.1 Chapter 2: a synthesis of reputation effects

Reputation systems govern online exchanges by providing numeric ratings and text messages that reflect traders' trustworthiness in previous exchanges. There has been a large body of empirical studies investigating the effect of seller reputation on selling performance using digital trace data. These studies provide mixed results on the existence, magnitude, and interpretation of the reputation effect. To help reach a consensus on whether the reputation effect exists and what it means, in Chapter 2, we performed a comprehensive meta-analysis of 378 reported effect sizes from 107 empirical studies that used 181 unique datasets. We classified seller reputation into three categories: number of positive ratings, number of negative ratings, and overall reputation scores. We also categorized the selling performance into four types: the probability of sale, selling price, selling quantity, and the ratio of selling price to reference price. We divided the data into these twelve subsets, one for each combination of three types of seller reputation variables and four types of selling performance variables, to demonstrate the consistency of the reputation effect across various operationalizations of seller reputation and selling performance.

Overall, the findings of the twelve meta-analyses supported the hypothesis that a good seller reputation positively affects selling performance. Specifically, the number of positive ratings had consistently and significantly positive effects on all types of selling performance. In comparison, the overall reputation score had a positive but generally smaller effect.

This study offered valuable insights into the significance of seller reputation in reputation systems within P2P online markets, highlighting the importance of distinguishing different measurements of seller reputation and selling performance. It established the existence of reputation effects from over a hundred previous studies. In addition, the study found that reputation effects exhibited excess variation that could not be attributed to sampling error alone. These findings led to the subsequent study, described in Chapter 3, which delved deeper into the potential factors causing the large variation in reputation effects.

1.3.2 Chapter 3: moderators of reputation effects

In Chapter 3, we performed an exploratory study on how the size of reputation effects should be interpreted in P2P online markets and attempted to identify the potential causes of the variation. The data collected from Chapter 2 not only provided the sizes of reputation effects but also included abundant data at the levels of study, dataset, and regression models. With the benefits of the data collection,[6] this study explored the moderating effects of various factors, categorized as (1) contextual moderators, which referred to cultural, spatiotemporal, and institutional embeddedness of market participants; (2) product-related moderators, such as product price, condition, and popularity; (3) method-related moderators regarding data collection, operationalization, and statistical modeling choices.

6 At the request of editors from the *Journal of Computational Social Science*, an updated version of meta-analytic data collection was used for Chapter 3. Chapter 3 incorporated data from 18 more recent studies and included 28 additional effect sizes compared to Chapter 2.

The results showed that the variation of observed reputation effects could be partly explained by these moderators. As for contextual moderators, reputation effects were substantially larger in the Chinese context than in the European or US contexts. In terms of the product condition, seller reputation exhibited more important effects for new products than used products, which was contrary to our expectations. Regarding the method-related moderators, we did not observe significant moderating effects across the entire dataset consistently. The study also emphasized that we should be cautious while interpreting the identified or insignificant moderating effects since this might be due to the limited statistical power of the given dataset.

Furthermore, within our theoretical framework, we assume that there are other factors like the rate of truthful feedback leaving that would influence the size of reputation effects. However, there are too few observational data that report information on market-level feedback rates to estimate that effect reliably. Therefore, we resort to laboratory experiments to study the effect of the variability of feedback rates within the decision-making environment of online markets on the size of the reputation effect.

1.3.3 Chapter 4: the role of the feedback rate

To further understand how the sizes of reputation effects come about, we conducted two laboratory experiments to test whether the reputational effect sizes are negatively correlated with feedback rates. These experiments are described in Chapter 4. We argue that in a market with a higher feedback rate, dishonest sellers will be more quickly filtered out, so the transaction costs for sellers without a reputation will be lower. Thus, the reputation systems with a higher rate of truthful feedback are more effective than those with lower feedback rates, even if the observed reputation effects will be smaller. However, as an essential attribute of the reputation system, the rate of feedback leaving after each transaction is almost never reported as open information in online markets and, therefore, not possible to be captured or collected in research using observational data. Therefore, we conducted two controlled lab experiments in which we emulated online market transactions with trust at stake and with diverse feedback rates (i.e. 20%, 40%, and 60%).[7] Using the trust game with incomplete information, we mainly measured (1) sellers' behavior, i.e. whether to offer a discount and whether to be trustworthy while being trusted, (2) buyers' behavior, i.e. which seller to transact with (for the second experiment only, where buyers have the opportunity to choose one out of the matched two sellers), as well as whether to trust the seller. The basic setups of the two experiments were similar, but the second experiment also addressed the competition mechanism between sellers. That is, every buyer was matched with two potential sellers in each round. In this way, buyers were provided with the opportunity to choose one of the sellers based on the reputation of the sellers and their willingness to offer discounts.

In this study, we investigated the differences in sellers' reputation building behavior (i.e. whether they offer discounts when they do not have a good reputation) and trustworthiness, as well as buyers' trust across markets with varying feedback rates. Overall, our findings reaffirmed the working of the reputation mechanism. That is, sellers were more inclined to offer discounts when they lacked a good reputation. The reputation effect was especially substantial in the second experiment, where sellers were competing with each other. Also, sellers who had a good reputation or offered discounts were more often trusted or chosen by the buyers. However, we did not observe significant differences across different feedback rates to support our hypothesis that a higher feedback rate significantly decreases sellers' propensity to give discounts. With an exploratory analysis, we argued that this might be due to the existence of unconditional trust in the market. If sellers who do not have a good reputation are aware of or have experienced a high level of unconditional trust from buyers, they would not be sufficiently motivated to offer discounts.

7 Previous research reported that the feedback rate ranges between 33 and 68 percent for buyers, and 55 to 78 percent for sellers (Bolton et al., 2013; Dellarocas et al., 2004; Jian et al., 2010; Resnick & Zeckhauser, 2002), and we design the laboratory experiments based on this available information.

1.4 Methodological contribution

1.4.1 Overview of the mixed method

This dissertation mainly comprises three research designs. Firstly, a series of meta-analyses were performed to synthesize the results of previous studies on the relationship between seller reputation and selling performance in online markets, to confirm the existence of the reputation effects. Secondly, an exploratory analysis using model selection and multi-model inference was conducted to identify potential moderators that could affect the observed sizes of reputation effects. Thirdly, behavioral laboratory experiments were carried out to investigate how feedback rates influence reputation effects and the decision-making of sellers and buyers in anonymous markets.

1.4.2 A series of meta-analyses

A meta-analysis "refers to the analysis of analyses", which means the statistical analysis based on the collection of existing individual studies in order to integrate previous findings (Glass, 1976; Stanley, 2001). There have been various empirical studies attempting to examine how seller reputation brings about selling performance, with datasets of various sample sizes (ranging from 14 to 3,981,429 observations) and product categories (e.g. stamps, mobile phones, see Table 2.3 in Chapter 2 for more details); these studies also differ in how authors define and operationalize dependent and independent variables. Therefore, the best way to synthesize the existing evidence on reputation effects is through a series of meta-analyses. The research is completed with 12 separate meta-analyses due to the different categories of dependent (selling performance) and independent (seller reputation) variables. For the seller reputation, it is straightforward that we cannot compile different measurements like positive and negative ratings in the same analysis, but we should also realize that the mechanism of reputation effect on different types of selling performance should be treated separately.

Another feature of the method of meta-analysis is that many studies included were not focusing on the reputation effects as we do, but more on other relevant topics such as consumers' bidding behaviors or sellers' pricing strategies. Nevertheless, those studies include the necessary information (mostly coefficient estimates and *t*-values from regression models) for evaluating reputation effects. Incorporating studies from various topics allows for a broader examination of the research landscape and enhances the generalizability of the findings.

The main challenge we overcame in conducting this meta-analysis was to make use of regression coefficients as comparable effect size estimates. There were only 40 out of the 378 collected coefficients reported as zero-order correlations that can be directly used as correlational effect sizes for the meta-analysis. The other 338 coefficients came from multiple regression model estimates with various explanatory and control variables. For most meta-analytic studies, these non-standardized coefficient estimates across different studies are challenging to be converted into comparable effect sizes to be included in the meta-analysis. However, since the goal of the study was to establish the general existence and variation of the reputation effect, we attempted to transform them into appropriately comparable effect sizes. As suggested by methodological literature (see Aloe, 2014; Aloe et al., 2017; Rosenthal, 1991; Tong & Guo, 2022), we used partial correlations as they present the relationship of the effect while controlling for the number of predictors in each regression model. Partial correlations can be calculated from the corresponding *t*-values (for linear regression models) or *z*-values (for non-linear regression models), as well as the degrees of freedom (*df*) of the regression models as the effect sizes for the meta-analysis. With this method, we were able to incorporate the largest number of studies among the existing systematic reviews about reputation effects. Also, this study gained much more statistical power than previous similar studies that used only the signs and statistical significance of reputation effects, i.e., the vote-counting method (see Liu et al., 2007; Schlägel, 2011).

1.4.3 Meta-analytic model selection and multi-model inference

During the process of literature search and screening for the meta-analysis in Chapter 2, we realized that a lot of included empirical studies reported the inconsistencies in the observed reputation effects and attempted to provide arguments on these matters (e.g. reputation effects might differ between new and used product, as suggested by Diekmann et al., 2014). Consequently, we formulated general expectations for all potential moderators and performed an exploratory analysis to find moderators of reputation effects.

We applied model selection and multi-model inference analyses within the framework of meta-analysis and meta-regressions. Model selection examines all competing models simultaneously based on a certain information criterion such as the Akaike information criterion (AIC) (Burnham & Anderson, 2002) and model weights (aka Akaike weights) that indicate the probability that a model is a best-fitting model. The result of the model selection approach presented the best-fitting models among the thousands of possible moderator combinations and assessed the most important moderators in general. Rather than selecting the single “best” model, multi-model inference combines the results of multiple models to provide the relative importance of each potential moderator by summing the likelihood of each moderator being included in a well-fitting meta-regression model. Hence, a moderator that is included in more models with larger weights will be considered more important in explaining reputation effect variance. The advantage of multi-model inference is that it reduces the risk of selecting one of the less probable models by chance because the relative importance of all moderators is listed (Cooper et al., 2019). To our knowledge, we are the first to apply model selection and multi-model inference in meta-analysis to address the substantial question of why reputation effects vary in size.

However, the disadvantage of this approach is that it requires large datasets to achieve sufficient statistical power. Built on the dataset with 378 effect sizes reported in 107 studies in Chapter 2, the dataset used in Chapter 3 was updated and expanded, comprising 406 effect sizes reported in 125 studies,[8] which is quite large as a meta-analytic dataset. Nevertheless, due to the existence of missing information, we could only examine moderators for which sufficient data were available in the conducted studies.

1.4.4 Behavioral laboratory experiment

Suggested by Snijders and Matzat (2019), it is important to include all potential choices a buyer would consider when measuring the value of reputation to the buyer. As online markets such as eBay typically do not disclose the number of past transactions or the frequency of buyers’ leaving feedback, obtaining open data from online transactions for feedback rates is not feasible. Consequently,

8 The meta-analytic dataset in Chapter 2 was collected until September 2018, and the dataset in Chapter 3 was updated until October 2021.

the impact of feedback rates on sellers' and buyers' decision-making could only be estimated by manipulating feedback rates as an exogenous variable in laboratory experiments. Laboratory experiments allow for manipulation of specific conditions and control over the decision environments (e.g., how are participants paired, whether the pairs interact repeatedly or one-time only), and to test the precise predictions derived from game-theoretic models (Falk & Heckman 2009). The experimental design in Chapter 4 is based on the TGI, as mentioned above, ideal for disentangling interrelated effects that take place in actual anonymous markets (Keijzer & Corten, 2023) and serves as a solution for addressing the endogeneity problem (Tadelis, 2016).

A prevalent critique of laboratory experiments revolves around the perception that experiments conducted with students as subjects fail to generate representative evidence, but it should be less problematic to test predictions from most economic models as these predictions do not depend on specific subject pools (Falk & Heckman 2009). Another common objection is that lab experiments may lack external validity, as the results obtained may not always accurately reflect real-world behaviors even for the specific groups observed in the lab. Indeed, in the experimental designs used in Chapter 4, for instance, we assumed that subjects behave in the hypothetical situations they received as if those were real-world situations, but clearly this cannot be guaranteed. This limitation should be taken into consideration when interpreting the findings of the experiments.

1.5 General conclusion and discussion

1.5.1 Conclusion

The findings of this dissertation can be summarized into three main points. First, we reaffirmed the general existence of reputation effects between seller reputation and selling performance. Despite the small overall effect sizes, it is argued that they should not be interpreted as the reputation system being ineffective. Second, we explored various potential moderators to explain the variation in reputation effects and identified factors such as contextual region, product condition, and some method-related characteristics as important moderators. Lastly, we argued that reputation effects are negatively related to feedback rates, but no significant differences in reputation effects were observed across varying feedback rates in our experiments, which is likely due to high unconditional trust levels of the participants in our experiments.

1.5.2 Discussion and future research

One of the most essential contributions of this dissertation is the seemingly counterintuitive argument: The more effective a reputation system is in identifying dishonest sellers in an online market, the smaller will be the reputation effect. The effectiveness of a reputation system may not necessarily be that reputable sellers always earn a large price premium, but rather due to the system's ability to attract and maintain a majority of trustworthy and reliable sellers, reputation will be less important.

While the findings in Chapter 3 did not reveal significant moderators that could substantially account for the variability in reputation effects, our efforts to examine the moderating effects of the contextual, product-related, and method-related moderators are noteworthy. Considering the limitations of the available data collection, the innovative approach of model selection and the multi-model inference was considered the most suitable approach. As a recommendation for future research, obtaining a more extensive dataset on a larger scale would enhance the statistical robustness of the exploratory analysis. Alternatively, researchers could also try to retrieve the raw data of the studies included in the meta-analytic dataset from the authors of these studies and conduct meta-analyses with individual subject data (ISD), which allows for a more comprehensive and detailed assessment of certain moderating effects of interest, such as the effect of used vs. new products.

Further research is encouraged to conduct a more comprehensive examination of the influence of truthful feedback provision rates on the effectiveness of a reputation system. Our study presented in Chapter 4 did not identify substantial differences in reputation effects or behaviors of sellers and buyers across experimental conditions with varying feedback rates, and we assume this outcome could be constrained by the choices of parameters in the experimental design. To better delve into this matter, future research could improve the experimental design by incorporating more distinct feedback rates. Also, the dynamics of online markets can be studied using simulations, where programmed agents can act as participants in the market and engage in behaviors like pricing strategies and market competition. With this method, researchers can incorporate various factors and provide more substantial evidence on how different market conditions, e.g., truthful feedback rates or the level of seller trustworthiness, would impact the effectiveness of online markets. The conditions that theoretically suggest to be most distinctive in terms of prediction for the effect of reputation scores could be translated into subsequent experiments.

2
CHAPTER

Reputation effects in peer-to-peer online markets:

A meta-analysis

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This chapter has been published as Jiao, R., Przepiorka, W., & Buskens, V. (2021). Reputation effects in peer-to-peer online markets: A meta-analysis. *Social Science Research*, *95*, 102522. Jiao collected the meta-analytic dataset and did the meta-analysis. Jiao wrote the manuscript in collaboration with Przepiorka. Przepiorka and Buskens contributed to the conceptualization of the model. All authors contributed to (double checking) the coding and manuscript editing. We would also like to thank Andreas Diekmann, Sanne Kellij, Irene Klugkist, Andreas Schneck, Jeroen Weesie and Rafael Wittek for their insightful comments and suggestions.

Abstract:

Most online market exchanges are governed by reputation systems, which allow traders to comment on one another's behavior and attributes with ratings and text messages. These ratings then constitute sellers' reputations that serve as signals of their trustworthiness and competence. The large body of research investigating the effect of reputation on selling performance has produced mixed results, and there is a lack of consensus on whether the reputation effect exists and what it means. After showing how the reputation effect can be derived from a game-theoretic model, we use meta-analysis to synthesize evidence from 107 studies investigating the reputation effect in peer-to-peer online markets. Our results corroborate the existence of the reputation effect across different operationalizations of seller reputation and selling performance. Our results also show the extent to which the reputation effect varies. We discuss potential explanations for the variation in reputation effects that cannot be attributed to sampling error and thereby point out promising avenues for future research.

Keywords: online market; trust; reputation; reputation system; reputation effect; meta-analysis

2.1 Introduction

Reputation as a mechanism to govern market exchanges is undergoing its most successful propagation. Although humans' ability to share information about others' deeds and misdeeds has promoted market exchange throughout history (Diekmann & Przepiorka, 2019; Greif, 1989; Hillmann, 2013) modern information and communication technology (ICT) has reduced the costs of sharing information to a minimum (Rifkin, 2014). In recent years, online markets have increased in popularity and fundamentally transformed the ways in which we engage in economic exchange. There are online markets for consumer goods, loans, plumbing work, academic positions, illegal drugs, etc. If one conceives of markets as institutions that facilitate exchange (Coase, 1988), it becomes apparent how online markets have made generalized social exchange possible at a large scale (Blau, 1964). The number of online platforms for dating, car-sharing, time-sharing, house-swapping, etc. is steadily increasing (Botsman & Rogers, 2010).

Most of these market exchanges are governed by online reputation systems (Dellarocas, 2003), which allow buyers to comment on sellers' behaviors and attributes with ratings and text messages.[1] These ratings then constitute these sellers' reputations, which can be conceived of as signals of their trustworthiness and competences (Li et al., 2019; Przepiorka & Berger, 2017). Indeed, research shows that information about seller reputations can be predictive of online exchange fraud and disputes (Gregg & Scott 2006; MacInnes et al., 2005). However, trustworthy and untrustworthy sellers are indistinguishable when they enter a market because they have no records of past behavior. One way for honest market entrants to build their reputation is to lower prices or offer other types of discounts to attract interaction partners and prove their trustworthiness and competence. Building a good reputation is therefore costly. However, honest agents will be compensated for their investment if they remain in the market

1 Although reputation systems and recommender systems are related in that they are employed by online market platforms and fueled by user generated data, they must be kept distinct. A reputation system can be defined as socio-technical structure through which third-party information about an actor's past behavior is collected, transmitted and aggregated (see also Resnick et al., 2000). A recommender system can be defined as a software tool or technique that suggests items that are of potential interest to a particular actor (see, e.g., Ricci et al., 2015; Palopoli et al., 2013, 2016).

long enough, whereas dishonest agents will not bother to invest in building a good reputation. Hence, traders can infer potential trading partners' honest intentions from their good reputations (Przepiorka, 2013; Shapiro, 1983). A similar argument can be made with regard to the quality of commodities and services offered via online markets.

This argument implies that online sellers' reputations and their business success will be correlated, that is, sellers with a better online reputation will realize more sales at higher prices. We henceforth call this the reputation effect.[2] However, there is a lack of consensus regarding the existence and meaning of the reputation effect (see, e.g., Lindenberg et al., 2020), especially because in many previous studies that estimated it, the reputation effect appeared to be small (Livingston, 2005; Snijders & Matzat, 2019; Standifird, 2001). Given the persistently increasing popularity of online market platforms that are governed by reputation systems, it is important to understand in what ways the reputation effect can be meaningful.

The aim of this paper is first and foremost to establish the general existence and the variation of the reputation effect reported in the literature. We do this by synthesizing evidence from 107 studies that investigated the reputation effect. Our meta-analysis includes 378 coefficients estimated based on 181 different datasets comprising a total of 14.04 million observations of online market transactions. More precisely, we conduct twelve separate meta-analyses, one for each combination of three types of seller reputation variables (number of positive ratings, number of negative ratings, overall reputation scores) and four types of selling performance variables (probability of sale, selling price, selling quantity, ratio of selling price to reference price). By splitting the data into these twelve subsets, we establish the robustness of the reputation effect across different operationalizations of seller reputation and selling performance. We

2 Several studies have corroborated that the relation between seller reputation and business success is causal (Przepiorka, 2013; Resnick et al., 2006; Snijders & Weesie, 2009). Snijders and Weesie have established the causal relation between seller reputation and success by estimating buyers' willingness to pay for seller reputation. Note that we refer to the reputation effect as the premium sellers can expect for their reputation rather than buyers' willingness to pay for reputation. All studies included in our meta-analysis conform with our definition of the term.

leave elaborations and tests of explanations for the variation in the size of the reputation effect within these subsets for subsequent studies.

Before we describe how we conduct our meta-analyses and present our results, we recap the game-theoretic underpinnings of the reputation effect in the next section. In the concluding section, we highlight possible reasons for the variation of the reputation effect that cannot be attributed to sampling error and suggest directions for future research. In particular, we list moderating factors that could be used in subsequent sub-group analyses and meta-regressions to identify potential determinants of the size of the reputation effect.

2.2 Theory

An interaction between a buyer and a seller in an online market is usually conceptualized as a trust game with incomplete information (Güth & Ockenfels, 2003). In the standard trust game (Dasgupta, 1988, see the right sub-tree denoted TG in Figure 2.1), a first moving agent (the buyer) decides whether to trust the second moving agent (the seller) and send money to buy an item. Upon receipt of the money, the seller decides whether to ship the item the buyer paid for. In the TG, the payoffs are ordered such that the seller would not ship ($T > R$) and, therefore, the buyer does not buy ($P > S$). As a result, the exchange does not take place and both earn payoff P, which is lower than the gains from trade R.

Unlike the standard trust game, the trust game with incomplete information (TGI) accounts for the fact that the buyer is uncertain about the seller's incentives and ability to be trustworthy because the seller holds private information about their preferences and constraints (Raub, 2004). In the TGI (Figure 2.1), this important aspect of a trust problem is modelled with the buyer being in one of two games, the assurance game (AG) or the TG. The two games merely differ in the order of seller payoffs. In the AG, the seller has an additional benefit b and/or cost c for shipping or not shipping, respectively, such that $R + b > T - c$. This order of payoffs implies that, in the AG, the seller has an incentive to ship if the buyer buys. The uncertainty of the buyer is modelled by a so-called move of nature (N) determining which game the buyer is in. While the seller knows whether they are in the AG or the TG, the buyer only knows the probability α of being in the AG. This is denoted by the dashed line connecting the two decision nodes of the buyer. Given the limited information the buyer has about the seller, how does the buyer decide whether to choose ‘buy’ or ‘not buy’ in the TGI?

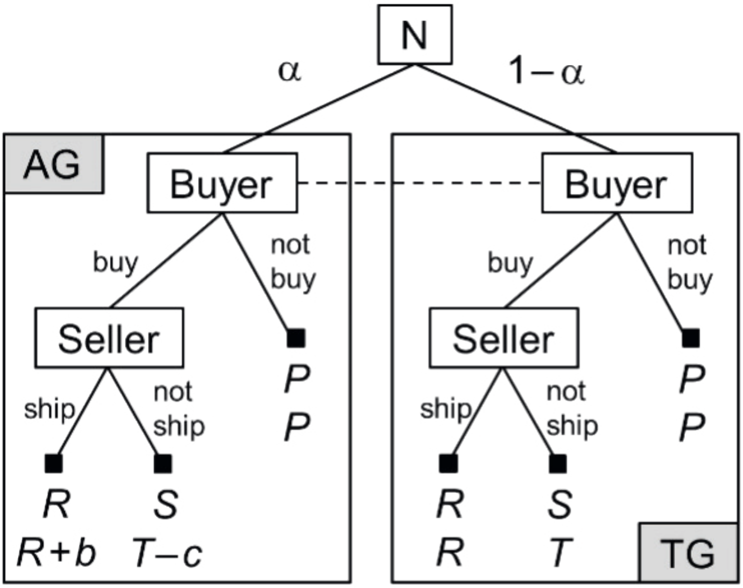

Figure 2.1. Trust game with incomplete information (TGI)

Knowing α and the TGI payoffs, the buyer calculates the expected payoff from either of their two actions and takes the one that maximizes their expected payoffs. If the buyer buys, their expected payoff is $\alpha R + (1 - \alpha)S$. If the buyer does not buy, their payoff is P in either case. The buyer chooses ‘buy’ if doing so gets them a larger expected payoff than choosing ‘not buy’, that is, if $\alpha R + (1 - \alpha)S > P$. Since the payoffs of the TGI are fixed, we solve this equation for α and obtain

$$\alpha = \frac{P - S}{R - S} \tag{1}$$

Hence, if α, the probability of being in the AG, is larger than a certain threshold value, the buyer buys and abstains from buying otherwise. Note that the α-threshold is determined entirely by the buyer’s payoffs.

Theoretically, information about a seller’s reputation tells buyers something about the likelihood of being in the AG or the TG. Without additional information about a particular seller, *a* might correspond to buyers' prior beliefs about the trustworthiness of online sellers in general. How does information about seller reputation affect a buyer's belief about a seller's trustworthiness?

To answer this question, we first have to expand on the reasons for why some sellers are trustworthy and others are less so and thus what *b* and *c* in the AG comprise. Of course, many sellers are just honest; they never think about cheating buyers. These sellers might have social preferences either of the type that increase their utility if the buyer's utility is increased (+*b*) or of the type that decreases their utility if the buyer's utility is decreased (–*c*) or both (Becker, 1976; Fehr & Schmidt, 1999). However, let's not be too quick with invoking social preferences to explain why some sellers are more trustworthy than others.

Most sellers are interested in making money from doing business online. These sellers thus have an interest in staying in the market and expanding their business. Although fraudulent sellers also have an interest in staying in the market, a reputation system would not allow them to stay if they behave untrustworthily. The reason is that once a seller who received a buyer's money and did not send back the item the buyer paid for is rated negatively by the buyer, they will be perceived as dishonest and no other buyer will buy from them in the future. In other words, a onetime failure to reciprocate a buyer's trust will result in the seller having to leave the market and possibly re-enter the market using a new pseudonym (Friedman & Resnick, 2001). However, re-entering the market under a new pseudonym implies that the seller has no previous records of past transactions (neither good or bad). Hence, sellers who value the stream of future gains from trading with buyers higher than making a gain from cheating a buyer once and having to start from scratch will enter the market and build up their good reputation through cooperative and honest business conduct (see also Buskens & Raub, 2013). But how do these sellers build their reputation given that without one they are indistinguishable from dishonest market entrants?

A good reputation is a reliable signal of trustworthiness because it is costly to produce and separates sellers who can incur the costs to produce it from those who cannot (Przepiorka & Berger, 2017). Building a reputation is costly not only because sellers have to behave persistently cooperatively but also because market entrants, i.e. sellers without a reputation, have to accept lower prices for their offers. New sellers have to offer their items at prices that make buyers indifferent between their offer and an offer by a seller with a good reputation.

In terms of our TGI, for buyers to be indifferent between a seller with a good and a seller with an unknown reputation, buyers' payoffs from buying from either of them must be equal. Since a seller with a good reputation is in the AG, a buyer's payoff from buying from that seller is R. Recall that a buyer's expected payoff from buying from a seller with an unknown reputation is $\alpha R + (1 - \alpha)S$. Hence, sellers with an unknown reputation must offer a discount d, such that $\alpha R + (1 - \alpha)S + d = R$. When solving this equation for d, we obtain

$$d = (1 - \alpha)(R - S). \tag{2}$$

In other words, d corresponds to a buyer's expected net loss from buying from a seller with no reputation. However, once honest sellers without a reputation have received positive ratings (because of the great buyer experience), they do not need to offer this discount anymore and can increase their prices to a level that compensates them for their initial investment in building a reputation (Przepiorka, 2013; Shapiro, 1983).

From this argument it follows that sellers' reputations and their selling performance in terms of prices will be positively correlated. Moreover, in case supply exceeds demand and the market does not clear, by the same argument we can expect sellers' reputations and their selling performance in terms of probability of sale to be positively correlated:

H1. The better a seller's reputation, the higher is the price the seller can obtain for their items.

H2. The better a seller's reputation, the higher will be the probability the seller's items will be sold.

Note that H1 and H2 imply that sellers with a better reputation will obtain higher prices compared to a reference price and will sell larger quantities of their items in a given time frame, respectively:

H3. The better a seller's reputation, the higher is the price the seller can obtain for their items compared to a reference price.

H4. The better a seller's reputation, the larger will be the quantities at which the seller's items will be sold.

The previous literature included in our meta-analysis has tested these hypotheses more or less explicitly using different operationalizations of seller reputation. In our meta-analysis, we assess whether these four hypotheses are generally supported, considering each with three different operationalizations of seller reputation: number of positive ratings, number of negative ratings and reputation score (i.e. the number of positive ratings minus the number of negative ratings).

2.3 Methods

In this section we describe in detail how we conducted our meta-analysis. We first describe our literature search and the criteria for the inclusion of previous empirical studies in our analysis. Next, we describe the model selection process that we employed when studies reported more than one model estimating reputation effects based on the same data. Finally, we describe our approach to making effect sizes comparable so they could be included in our meta-analyses (see Tong & Guo, 2022).

2.3.1 Literature search

We conduct a meta-analysis on the relation between selling performance and seller reputation with results from existing empirical studies. The process of collecting all relevant articles on reputation effects in peer-to-peer online markets starts with two previous meta-analytic studies.

In their meta-analysis, Liu et al. (2007) focus on the relationship between seller reputation (number of positive, neutral and negative ratings) and the success of online auctions (number of bids, price premium and selling probability). Their paper integrates findings from 42 articles and uses combined significance tests for the meta-analysis. That is, their analysis only takes into account whether a regression coefficient has the expected sign and is statistically significant, and tests if overall a significant effect exists for a certain relationship (e.g. between the number of negative ratings and the number of bids). Another meta-analytic study conducted by Schlägel (2011) includes 58 articles researching the effects of seller reputation (number and/or ratio of positive ratings, neutral and negative ratings) on online auction outcomes (selling probability, number of bidders, number of bids and the final price). Similarly to Liu et al. (2007), Schlägel (2011) only considers the direction and sign of each type of relationship to obtain an overall assessment.

These two articles provide a first general overview of existing research on reputation effects in online markets. Our study extends these two studies in the following respects: (1) While Liu et al. (2007) and Schlägel (2011) account for the literature published before 2007 and 2011, respectively, our meta-analysis includes studies published until September 2018. (2) Our analysis also considers effect sizes and not only the signs and statistical significance of reputation effects, so called vote-counting. Vote-counting has been criticised for being ineffective in finding small but exiting effects in research fields with mostly underpowered studies (Combs et al., 2011). (3) We make effect sizes as comparable as possible by using appropriate transformations and specifying their relative importance by accounting for the sample size based on which they are estimated. (4) Liu et al. (2007) and Schlägel (2011) limit their analyses to online auctions whereas we also consider studies that analyzed fixed-price transactions.

The studies included in Liu et al. (2007) and Schlägel (2011) form our initial set of studies to be included in our analyses. We conducted our literature search on Web of Science, Google Scholar, RePEc (Research Papers in Economics) and CNKI (China National Knowledge Infrastructure, in Chinese). The following search strings were used: (online auction OR Internet auction OR eBay OR Taobao) AND (reputation OR rating OR feedback). Next we checked the titles, abstracts, and introduction sections of each study for their relevance for our analyses. Moreover, we checked the online platform, the time period of data collection and the type of products for each dataset to make sure there is no overlap between studies in terms of datasets. The search process resulted in 141 relevant research articles written in English, Chinese or German. The reference list of all these articles is provided in Appendix A.1. Figure 2.2 summarizes the study selection process that we employed. For a general approach to study selection for systematic reviews and meta-analyses, see Moher et al. (2009).

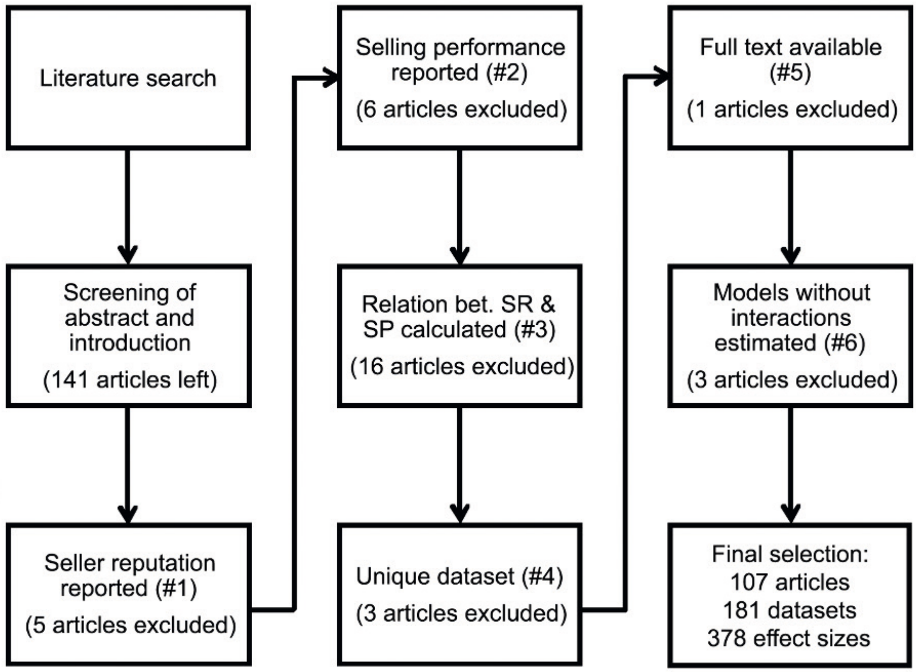

Figure 2.2. Steps of literature search, study and model selection

In the next step, we took a closer look at the full body of each article, in particular at hypotheses, descriptions of datasets, and results. At this stage, 31 of the 141 articles were excluded for the following reasons:

(1) No comparable seller reputation information reported: Seller reputation is mostly measured by means of the reputation score (i. e. the number of positive ratings minus the number of negative ratings), the number or percentage of positive, neutral, and/or negative ratings. Studies using other types of reputation measures are excluded. For example, some studies use the duration of sellers' membership in the online market as a measure of these sellers' reputations, and some operationalize seller reputation as a dummy variable indicating whether a seller is a 'top-seller'. These studies are excluded (indicated with #1 in Table A.1 in Appendix A.2).

(2) No selling performance reported: Selling performance is mostly measured by means of the final price of a fixed-price transaction or auction (i.e. highest bid), the selling probability (i.e. whether or not an item was sold), the selling volume (i.e. amount of items sold within a limited time period), and the price ratio of a sold item indicating the rate of the selling price to the standard or average price of similar items. Studies are excluded if they do not report any of these variables measuring selling performance (indicated with #2 in Table A.1 in Appendix A.2).

(3) No relationship between selling performance and seller reputation is reported: Our meta-analysis requires that the association between seller reputation and selling performance is calculated and reported including information on coefficients and *t*-values (or *p*-values and standard errors). Studies without empirical analysis of the relationship between selling performance and seller reputation, or studies in which these relationships are reported without explicit mention of test-values, are excluded (indicated with #3 in Table A.1 in Appendix A.2).

(4) Same dataset used in another study: Some authors use the same dataset in more than one article. In these cases only the most recent analysis or the one with the best fitting model is included (indicated with #4 in Table A.1 in Appendix A.2).

(5) Full text of the article is not available: One paper included in Schlägel (2011) is not available in online databases, and the author's contact information is not available. It is therefore not included in our analysis (indicated with #5 in Table A.1 in Appendix A.2).

2.3.2 Model selection

After removing studies based on the exclusion criteria listed above, 110 research articles are left. During the screening process, we noticed that many authors ran multiple models on the same kind of reputation effect on the same type of selling performance, and these models often produced similar results. To avoid the inclusion of the same kind of effect from the same study in our analysis more than once, only one model for each type of selling performance is selected as the final model for the calculation of effect sizes. The selection is based on the following criteria:

(1) No interaction effects with seller reputation: Some studies include interaction effects with seller reputation to explore how other factors (i.e. moderators) may affect the reputation effect. For example, in the study by Cai et al. (2013), the second model reported in table 6 includes seller reputation and an interaction of seller reputation and a dummy variable indicating if it is before or after a 'buyer protection system' is implemented in the online market. The result shows that there is a significant interaction effect of seller reputation and the introduction of a 'buyer protection system', which indicates that the reputation effect varies between two subsamples. However, no further information regarding the subsamples is reported in the article. Therefore, we cannot use the coefficients of this model in our meta-analysis. Unless there is enough information provided to disentangle reputation effects for each subsample (such as in Jin & Kato, 2006), models that interact seller reputation with other variables are excluded. There are three studies reporting only regression models with such interaction effects, so these studies are excluded at this stage (indicated with #6 in Table A.1 in Appendix A.2).

(2) Interpretation by authors in the result section: Models analyzed and interpreted by the authors as the main result of the research are prioritized. Since these models were selected by the authors as final research findings, these models are regarded as the most informative and suitable for drawing substantial conclusions.

(3) The best fitting model: Adding to the two conditions above, the selection of the final model is made based on the reported goodness of fit. With all other conditions equal, the model with the best model fit is selected.

2.3.3 Effect sizes

The correlational effect sizes (r), variances of these effect sizes (v_r), and the corresponding sample sizes are necessary to perform a meta-analysis. Depending on the features of statistical modelling used in each study, we use one of two methods to calculate r (and v_r) from reported coefficients.

2.3.4 Pearson correlation coefficients

Some of the studies that we selected for our meta-analyses report effect sizes from bivariate relationships only. If the Pearson correlation coefficient (ρ) is used to quantify the relation between seller reputation and selling performance, ρ is used as the correlational effect size (eq. (3)). The variance of the effect size is calculated based on r and the corresponding sample size n (eq. (4)) (see Borenstein et al., 2009: Ch. 6).

$$r = \rho \tag{3}$$

$$v_r = \frac{(1 - r^2)^2}{n - 1} \tag{4}$$

2.3.5 Multiple linear regression coefficients

Of the 378 coefficients included in our meta-analysis, 40 are zero-order correlations. However, 338 coefficients stem from multiple regression models. In these cases, the methods used for meta-analysis of effect sizes from bivariate relationships cannot be applied (see e.g. Lipsey & Wilson, 2001, p. 67–71). The reporting of non-standardized coefficient estimates and the different sets of explanatory and control variables used across different studies make comparability of coefficients difficult (although see Bowman, 2012; Peterson & Brown, 2005, respectively). However, our primary aim is to establish the general existence and variation of the reputation effect across different operationalizations of seller reputation and selling performance. This means that we do not need to rely on effect sizes from bivariate relationships only. In this case, the literature suggests several ways forward (also see Aloe & Becker, 2012; Bowman, 2012; Tong & Guo, 2022).

First, Borenstein et al. (2009, p. 314) point out that regression coefficients and their standard errors could be used directly in meta-regression if the aim is to examine in how far study-level characteristics affect effect sizes rather than obtaining an overall regression coefficient. This approach could be particularly fruitful if the research question addressed by means of multiple regression is unambiguous and consensus exists on which explanatory and control variables should be used in the models (e.g., as in the estimation of the determinants of housing prices; see Sirmans et al., 2006). This does not apply in our case.

Second, unstandardized regression coefficients can be standardized and used in meta-analysis if information on both the standard deviation (SD) of the predictor variable X and the SD of the target variable Y are reported (Bowman, 2012). However, in our case, the necessary information is mostly lacking and even if studies report the SDs of the predictor and target variables, if log-transformed variables are used for example, it is not possible to calculate their SDs from the SDs of the untransformed variables.

Third, Aloe and Becker (2012) show how the semi-partial correlation between variables X and Y included in a multiple regression model can be computed from the *t*-value of the coefficient of X, the R^2 of the regression model and the model's degrees of freedom (df = n – k – 1, where n is the number of cases and k is the number of model parameters). Aloe and Becker (2012) point out that the use of semi-partial correlations (rather than partial correlations) is preferable as it comes closest to the bivariate correlation coefficients used in standard meta-analysis. However, in our case, this is not the aim and we therefore favor the fourth approach.

Fourth, effect sizes can be calculated as partial correlations (Aloe, 2014; Aloe et al., 2017; Rosenthal, 1991; Tong & Guo, 2022).[3] We use partial correlations in our meta-analyses because they present the relationship of the effect of interest while controlling for the number of predictors included in each regression model. Partial correlations have been used as effect sizes in previous meta-analytic studies because they can be easily calculated from reported significance tests and make effect sizes comparable across different operationalizations of the variables of interest (e.g., Djankov & Murrell, 2002; Doucouliagos & Laroche, 2003). The partial correlational effect sizes (r) can be calculated based on the corresponding *t*-value and degrees of freedom (df) of the regression model (eq. (5)). The variance of the effect size is then calculated accordingly (eq. (6)).

3 A semi-partial correlation establishes the relation between a predictor variable *X* and a target variable *Y* net of the portion of *Y* explained by other predictors used in the model. A partial correlation establishes the relation between a predictor variable *X* net of the portion of *X* explained by other predictors used in the model and a target variable *Y* net of the portion of *Y* explained by other predictors used in the model (Aloe & Becker, 2012).

$$r = \frac{t}{\sqrt{t^2 + df}} \tag{5}$$

$$v_r = \frac{(1 - r^2)^2}{df} \tag{6}$$

Given that our partial correlations are quite heterogeneous in terms of controls used in the different studies, we also include the bivariate correlations as they form just a special case (with no controls), which is not in principle different from partial correlations with different sets of controls.

It is important to note moreover that the degrees of freedom (*df*) must be calculated differently, if studies account for clustered data by estimating cluster-robust standard errors. Studies that estimate reputation effects based on data containing, for example, repeated observations on same sellers must take into account that offers posted by the same seller are not independent. This can be done by applying various multilevel techniques, one of which is the calculation of cluster-robust standard errors of regression coefficients (Cameron & Trivedi, 2005; Snijders & Bosker, 2012). In the large majority of cases, the calculation of cluster-robust standard errors results in these standard errors becoming considerably larger (as compared to calculations that treat every observation as independent) and, consequently, corresponding *t*-values becoming considerably smaller. In these cases, to calculate effect sizes and variances of these effect sizes correctly, the appropriate degrees of freedom are $df = n_c - k - 1$, where n_c is the number of clusters and not the number of cases (see, e.g., StataCorp, 2015, p. 478).[4]

Not all of the studies that we selected for our meta-analyses report the *t*-values of coefficient estimates. For studies that report standard errors (SE) of regression

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4 In case of other multilevel approaches such as random intercept models, the appropriate degrees of freedom can be calculated from a weighted average of the number of cases (*n*) and the number of clusters (n_c) where the weight is the proportion of between-cluster variation (see also Aloe et al., 2017; Hedges, 2007). None of the coefficient estimates included in our meta-analysis stems from such multi-level regressions.

coefficients, the *t*-values are obtained by dividing the regression coefficients by the corresponding SEs. However, in 43 cases, the information on *t*-values and SEs is missing, and *p*-values are only reported in terms of 'stars' (i.e. *p*-value ranges). We apply two strategies to obtain the *t*-values from *p*-value ranges in these cases (see Table 2.1). We either take the median of the *p*-value range (Strategy 1), or we take the upper bound of the *p*-value range if the p-value is reported to be smaller than 0.1 (Strategy 2). For insignificant coefficients (i.e. for $p > 0.1$ or $p > 0.05$, depending on a study's cut-off for statistical significance), we assume $p = 0.5$. Our analysis is based on Strategy 1; our results do not change much when Strategy 2 is used instead (see Appendix A.3).

Table 2.1. Strategies to determine *p*-values from reported *p*-value ranges

Reported *p*-value range ("stars")	Number of cases	Used *p*-value is median (Strategy 1)	Used *p*-value is upper bound (Strategy 2)
$0 < p < 0.001$	2	0.0005	0.001
$0 < p < 0.01$	16	0.005	0.01
$0.01 < p < 0.05$	8	0.03	0.05
$0.05 < p < 0.1$	7	0.075	0.1
$p > 0.05$	3	0.525	0.5
$p > 0.1$	7	0.55	0.5

Notes: Two-tailed tests are assumed if not otherwise specified.

The *t*-values of regression coefficients can be calculated based on the corresponding *p*-values and *df*. These *t*-values are then used to calculate effect sizes and corresponding variances as described above (eqs. (5) and (6)).

This approach for linear models (e.g., OLS) we use as well for non-linear regression models such as logit and probit. In the latter case, however, we use z-values from these models instead of *t*-values to calculate partial correlational effect sizes and the corresponding effect size variances using equations (7) and (8), respectively. Since z-values do not depend on the number of model parameters, we use the number of cases (n) or the number of clusters (n_c) instead of the degrees of freedom (*df*) in these calculations.

$$r = \frac{z}{\sqrt{z^2 + n_c}} \tag{7}$$

$$v_r = \frac{(1 - r^2)^2}{n_c} \tag{8}$$

2.3.6 *Fisher z-transformation*

Correlational effect sizes are bound to be between -1 and 1. Their sampling distribution is therefore not normal, which makes the calculation of confidence intervals and comparisons of correlational effect sizes difficult. Fisher's *r-to-z* transformed correlation conversion is used, so that after the transformation, the sampling distribution of *r* becomes normally distributed (Fisher, 1921). The transformed correlation coefficient (*z*) and the variance of *z* (*vz*) can be calculated by means of equations (9) and (10), respectively. These transformed correlation coefficients are then used as units of analysis in our meta-analyses. The results of the meta-analyses are transformed back to correlation coefficients (*r*) for interpretation and presentation. However, there is an ongoing debate about whether the Fisher *z*-transformation should be applied to partial correlations as well (Aloe & Becker, 2012; Suurmond et al., 2017). Our results hardly change if we perform the analyses described below without first transforming the data.

$$z = \frac{1}{2}\ln\left(\frac{1 + r}{1 - r}\right) \tag{9}$$

$$v_z = \frac{1}{n - 3} \tag{10}$$

2.4 Data

Our dataset consists of 378 effect sizes, estimated with 181 different datasets, reported in 107 studies. The dataset was created by hand-coding the relevant information contained in the 107 studies. In a first step, the hand coding was performed and double-checked by one of the authors. In a second step, the other two authors independently checked a total of 125 (about 33%) of the data rows pertaining to the 378 coefficients. In a third step, the entire dataset was checked once more and updated based on insights gained in the second step. Upon publication of this article, we will make our data available via a public repository for reproduction purposes.

Figure 2.3 and Figure 2.4 provide overviews of the dependent and independent variables in the included studies. Descriptive statistics of our dataset are presented in Table 2.2, and, where available, mean prices of products at dataset level are reported in Table 2.3.

Figure 2.3 shows the number of studies that use one or several of the four dependent variables in their estimation of reputation effects. For example, out of the 107 studies included in our meta-analyses, 42 studies estimate reputation effects using only the final price as the dependent variables, and 15 studies use both the final price as well as the selling probability as dependent variables. Correspondingly, Figure 2.4 shows the number of studies that use one or several of the three operationalizations of seller reputation in their estimation of reputation effects. For example, there are 34 studies, that use only the reputation score and 25 studies that use both the number of positive and the number of negative ratings but not the reputation score.

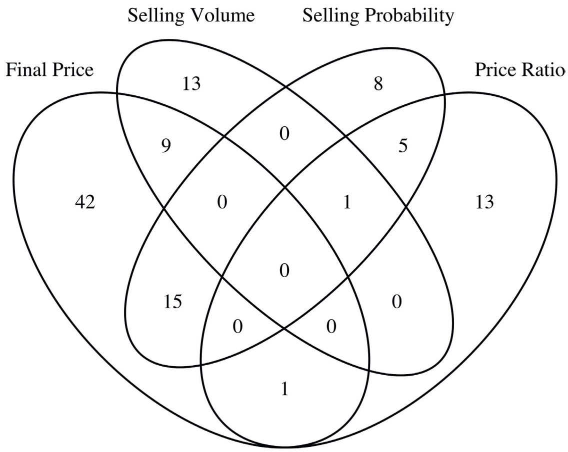


Figure 2.3. Summary of dependent variables in included studies

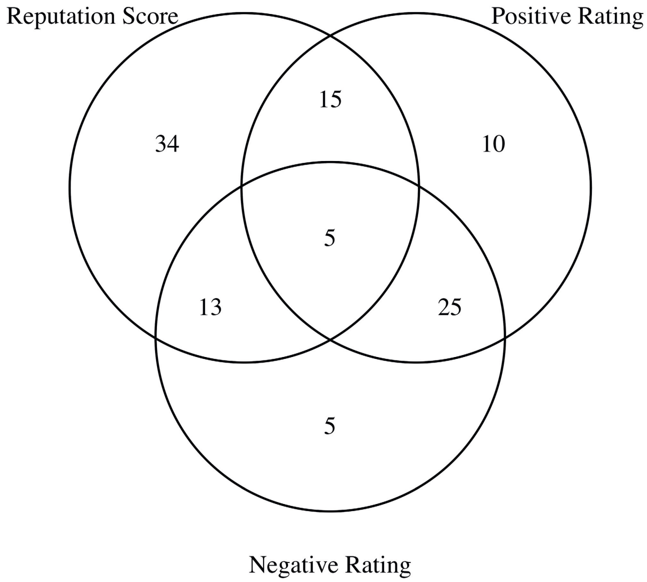


Figure 2.4. Summary of independent variables in included studies

Table 2.2. Descriptive information on included studies and datasets

Study level	*N*	*n* Min	*n* Max
Publication status			
Journal (English)	78	14	3981429
Journal (Chinese)	12	137	182853
Working paper	1	180	473152
Conference/workshop	4	22	1379
Book chapter	4	117	2133
Dissertation/thesis	6	129	16032
Unpublished	2	169	807
Study type			
Observational	105	14	3981429
Experimental	1	137	982
Mixed [a]	1	100	1124

Dataset level	*N*	*n* Min	*n* Max
Country			
US	113	14	1311452
China	48	22	182853
Europe [b]	18	192	3981429
Mixed [c]	2	418	55094
Platform			
eBay	120	14	339517
Taobao	38	22	1311452
Yahoo	7	91	551
Eachnet	4	1053	182853
Priceminister	4	1759572	3981429
Allegro	1	15033	15033
Huuto	1	227	227
Bizerate	1	445	445
Ricardo	1	204	204
Silkroad	3	119	16243
Mixed [d]	1	418	418
Year of data collection [e]			
1998	2	407	460
1999	11	94	1822

Table 2.2. Descriptive information on included studies and datasets (continued)

Study level	***N***	***n Min***	***n Max***
2000	40	14	861
2001	12	100	9981
2002	13	82	182853
2003	10	117	2133
2004	10	126	339517
2005	8	91	1665
2006	10	107	89982
2007	12	38	14689
2008	15	95	3981429
2009	8	22	4226
2010	4	445	1311452
2011	6	205	1251
2012	4	196	16032
2013	2	119	1379
2014	2	3433	16243
2015	10	137	982
2016	2	237	15033
Transaction type			
Auction	132	14	339517
Fixed-price	44	22	3981429
Unknown/Mixed [f]	5	38	14689

[a] *Jin and Kato (2006) reported results from an observational and experimental study.*
[b] *The category "Europe" includes France, Germany, Finland, Poland, Switzerland.*
[c] *Lei (2011) collected a dataset with eBay sellers from 42 countries, and Snijders and Zijdeman (2004) combined data from two Dutch and two US websites.*
[d] *The dataset of Snijders and Zijdeman (2004) was collected from eBay.nl (n = 111), Ricardo.nl (n = 125), eBay.com (n = 103), and ePier.com (n = 79).*
[e] *If year of data collection was not specified, the study's publication year was used.*
[f] *Chen et al. (2018), Przepiorka (2013) and Zhu et al. (2009) reported datasets combining auctions and fixed-price offers.*

2

Table 2.3. Descriptive information on included studies' datasets

Category/Product	Mean price (in USD)	# Datasets
Toy and hobby		
board game	131.9	1
game ticket	n/a	1
computer/video games	29.11	5
coin	62.28	25
stamp	33.07	2
artwork	47.74	1
music instrument	1636.04	2
baseball card	75.45	4
sport equipment	509.96	1
toy	263.21	2
CD/DVD	9.77	13
book	180.48	4
Computer and electronics		
PDA	252.75	4
scanner and printer	260.5	2
memory disk	76.06	7
computer software	434.36	5
(video) camera	681.27	12
DVD player	286.1	2
MP3 player	197.12	13
mobile phone	453.74	16
game station	n/a	1
computer/laptop	n/a	2
other electronic device	167.13	7
Other		
car	7601	3
prepaid/gift card	31.4	6
kitchen supply	56.2	8
illegal drug	99.8	3
clothing	19.35	2
food/drink	33.98	7
watch	791.61	2
cosmetics	18.98	3
Mixed/unavailable	256.96	15

Notes: The mean prices are calculated at the dataset level.

2.5 Results

Meta-analyses are performed separately for each of the twelve types of reputation effects, i.e. each combination of type of seller reputation and selling performance. As mentioned above, for methodological reasons, the effect size *r* is *z*-transformed for meta-analysis; thereafter, the overall effect is converted back to facilitate interpretation. The meta-analyses are performed with the 'metafor' package in R (Viechtbauer, 2010). The overall reputation effects are estimated using random-effects models. We also assess the heterogeneity of the results with Q-statistics and I^2 as well as publication bias by reporting the Egger's regression test in Table 2.4.

2.5.1 Overall effect sizes

Table 2.4 lists the overall effect sizes (*ES*) and corresponding 95% confidence intervals (CI) that resulted from the twelve meta-analyses performed for each combination of outcome and reputation variables. For each overall effect size, the table also shows the results of heterogeneity measures (Q and I^2) and the assessment of publication bias (Egger's test). We discuss overall effect sizes first.

The results reported in Table 2.4 corroborate the general existence of the reputation effect. All overall effect sizes point in the expected direction. However, three of the twelve overall effect sizes are statistically insignificant. With final price as the outcome variable, the reputation score and the number of positive ratings have a significantly positive overall effect (*ES* = 0.05, $p = 0.025$ and *ES* = 0.11, $p < 0.001$, respectively), and the number of negative ratings has a significantly negative overall effect (*ES* = -0.10, $p < 0.001$). Results are similar if the selling price relative to a reference value (i.e. price ratio) is used as the outcome variable. The reputation score and the number of positive ratings have a significantly positive effect (*ES* = 0.08, $p = 0.039$ and *ES* = 0.28, $p < 0.001$, respectively), and the number of negative ratings shows a negative but statistically insignificant overall effect (*ES* = -0.06, $p = 0.160$).

Table 2.4. Overall effect sizes

				Heterogeneity test		Egger's test
Relation	**ES**	**95% CI**	**N**	**Q**	**I^2 (%)**	
Final price						
Reput. score	0.05*	[0.01, 0.09]	66	1118.06***	99.76	-0.91
Pos. ratings	0.11***	[0.06, 0.15]	53	555.07***	99.15	3.12**
Neg. ratings	-0.10***	[-0.13, -0.07]	44	288.01***	92.04	-2.42*
Price ratio						
Reput. score	0.08*	[0.00, 0.15]	16	410.68***	99.17	0.25
Pos. ratings	0.28***	[0.18, 0.37]	35	485.31***	99.62	5.10***
Neg. ratings	-0.06	[-0.15, 0.03]	25	183.76***	87.14	1.94
Selling probability						
Reput. Score	0.04*	[0.01, 0.07]	26	965.71***	96.59	-2.25*
Pos. ratings	0.07***	[0.04, 0.10]	19	1146.62***	97.20	0.05
Neg. ratings	-0.05***	[-0.07, -0.03]	16	39.86***	62.36	-1.73
Selling volume						
Reput. score	0.08	[-0.01, 0.16]	31	5514.45***	99.81	-2.12*
Pos. ratings	0.14***	[0.09, 0.20]	31	892.68***	99.41	4.29***
Neg. ratings	-0.06	[-0.17, 0.06]	16	103.85***	92.58	-3.15**

*Notes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.*

With selling probability as the outcome variable, reputation scores and the number of positive ratings exhibit statistically significant increases in the probability of sale (*ES* = 0.04, $p = 0.016$ and *ES* = 0.07, $p < 0.001$, respectively), whereas the number of negative ratings has a significantly negative effect on selling probability (*ES* = -0.05, $p < 0.001$). Finally, with selling volume as the outcome variable, the reputation score and the number of positive ratings exhibit positive effects on the number of sold items but only the overall effect of the number of positive ratings is statistically significant (*ES* = 0.08, $p = 0.085$ and *ES* = 0.14, $p < 0.001$, respectively). The effect of the number of negative ratings points in the expected direction but is statistically insignificant (*ES* = -0.06, $p = 0.331$).

Overall, these results corroborate that a good seller reputation has a positive effect on selling performance. Especially the number of positive ratings exhibits a consistent, significantly positive effect on all types of outcome variables. The reputation score also has positive but generally smaller effects on selling performance than the number of positive ratings. Results regarding the number of negative ratings are mixed although all overall effects are negative, as expected.

Given that correlational effect sizes can range between -1 and 1, the overall effect sizes reported in Table 2.4 appear relatively small. This by no means should be interpreted as ‘weak’ or small average reputation effects. In order to interpret reputation effects substantially, a single study should be considered. For example, the coefficient of the log-transformed number of positive ratings in a model of log-transformed price (in EUR) reported by Przepiorka (2013) has a partial correlational effect size of 0.08. However, the raw coefficient is 0.078 and can be interpreted as follows: a tenfold increase in the number of positive ratings (e.g., from 40 to 400) corresponds with a final price increase of $100 \times [\exp(0.078 \times \log 10) - 1] = 20\%$. Based on the average selling price of items analyzed in this study (about EUR 15), the increase in a seller’s positive reputation corresponds to a price increase of EUR 2.95. The overall effect sizes reported in Table 2.4 can be interpreted as corroborations of the existence of reputation effects across different studies and operationalizations of seller reputation and seller performance. We will have a closer look at effect size heterogeneity next.

2.5.2 Effect size heterogeneity

Statistical heterogeneity refers to the variability in effect sizes that cannot be attributed to sampling variability only. For each type of reputation effect reported in Table 2.4, we assess the extent of statistical heterogeneity using Cochrane's homogeneity test and the I^2 statistic.

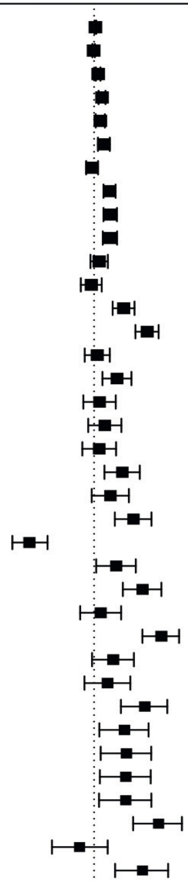
Cochran's Q is a measure of weighted squared deviations around the overall effect size (see Borenstein et al., 2009, p. 109–113). All but one Q-values are statistically significant leading us to reject the null hypothesis of homogeneity in effect sizes in these cases. I^2 describes the percentage of between-study variability to total variability (i.e. within and between study variability in effect sizes). It indicates how (in)consistent findings are across studies; it is not a measure of the variation of the true effect (see Borenstein et al., 2009, p.117–119). Eleven out of the twelve I^2 values presented in Table 2.4 are above 75%, suggesting that a large part of effect size heterogeneity results from differences in true effect sizes rather than sampling variability. These results suggest that statistical heterogeneity is very high. It can be attributed to differences in study designs (e.g., type and number of explanatory and control variables in multiple regression models), market platforms (eBay, Taobao, etc.), samples of product items (price levels, unaccounted heterogeneity) etc. (also see Tables 2.2 and 2.3). To be better able to assess the extent of effect size heterogeneity, we will next have a look at four forest plots. The four forest plots shown in Figure 2.5 through Figure 2.8 list the study abbreviations, years of data collection, sample sizes, meta-analytic weights, and effect sizes together with their 95% confidence intervals. The effect sizes are also plotted with their 95% confidence intervals. The diamond at the bottom of each figure indicates the combined result of all individual effect sizes (i.e. the overall effect size that is also reported in Table 2.4). The four forest plots are for effect sizes of positive ratings on final price, negative ratings on final price, positive ratings on selling probability and negative ratings on selling probability.

Figure 2.5 shows the forest plot of the effect sizes of the number of positive ratings on final price. Positive ratings have a significantly positive overall effect on final prices ($ES = 0.11$, $p < 0.001$); also, it presents a significant heterogeneity across the included studies (Q ($df = 52$) = 555.07, $p < 0.001$). It is noticeable that the study of Song and Baker (2007) reports a negative correlation with a relatively high weight. Tracing back to the original paper, it indicates a negative Pearson correlation ($\rho = -0.32$) between the number of positive feedback ratings and prices of MP3 players on eBay; but with the same dataset, there is a same negative Pearson correlation ($\rho = -0.32$) between the number of negative feedback ratings and prices, suggesting positive and negative feedback ratings have the same effect on selling prices, which is an unexpected result.[5] The forest plot of the association between the number of negative ratings on final price is shown in Figure 2.6. The combined result of all individual effect sizes is negative and statistically significant ($ES = -0.10$, $p < 0.001$); it also presents a significant heterogeneity across the included studies (Q ($df = 43$) = 288.01, $p < 0.001$). However, some studies report non-negative effects. For instance, the dissertation of Sears (2016) reports a null effect of the log-transformed number of negative ratings on selling prices of marijuana sold via the cryptomarket Silk Road. Although small and insignificant, this effect size exhibits one of the highest weights (2.98%) in the meta-analysis.

Figure 2.7 presents the forest plot of reputation effects of positive ratings on selling probability. The combined result of all individual effect sizes is positive and statistically significant ($ES = 0.07$, $p < 0.001$); the heterogeneity test on the included effects sizes is significant (Q ($df = 18$) = 1146.62, $p < 0.001$), indicating that there is a great variation in effect sizes across studies. One of the coefficients pointing in the opposite direction is reported by Xiao and Liu (2009); it suggests that the percentage of positive ratings has a negative but insignificant effect on the selling probability of Nokia mobile phones in the Chinese online market Taobao.

Figure 2.8 is the forest plot of the effect sizes for selling probability and the number of negative ratings. The figure shows that negative ratings have a small, yet significantly negative overall effect on selling probability (ES = -0.05, $p < 0.001$); and the heterogeneity is significant among the included effect sizes (Q ($df = 15$) = 39.86, $p < 0.001$). Of the 16 included effect sizes, two exhibit insignificantly positive values (To et al., 2008) on Yahoo.

5 Since it seemed plausible to assume that this was a typo, we re-estimated the overall effect size excluding the negative coefficient reported in Song and Baker (2007). While the overall effect size remains the same, the 95% CI becomes slightly smaller (ES = 0.11, p < 0.001, 95% CI [0.07, 0.15]).



Study	Year of dataset	N	weight	Correlation (95% CI)
Fan_et_al_2013.2	2010	519131	2.22%	0.01 [0.00, 0.01]
Fan_et_al_2013.1	2010	1189225	2.22%	−0.00 [−0.01, 0.00]
Diekmann_et_al_2014.3	2004	180881	2.22%	0.02 [0.01, 0.03]
Chen_Lai_Yu_2018	2007	13183	2.21%	0.04 [0.02, 0.06]
Diekmann_et_al_2014.2	2004	8727	2.21%	0.03 [0.01, 0.05]
Sears_2016.1	2012	12303	2.20%	0.05 [0.02, 0.08]
Sears_2016.2	2012	12305	2.20%	−0.01 [−0.04, 0.02]
Diekmann_et_al_2014.1	2004	5269	2.20%	0.08 [0.05, 0.11]
Wu_Xu_Fan_2014	2014	3433	2.19%	0.08 [0.05, 0.11]
Przepiorka_2013.1	2006	63815	2.19%	0.08 [0.04, 0.11]
Schamel_2004	2003	2133	2.17%	0.03 [−0.02, 0.07]
Quill_2007	2006	1442	2.15%	−0.01 [−0.07, 0.04]
Zhang_2006	2004	1276	2.14%	0.15 [0.09, 0.20]
Zhou_Dresner_et_al_2009	2004	1025	2.13%	0.26 [0.20, 0.32]
Ye_Xu_et_al_2013.2	2011	1003	2.12%	0.02 [−0.05, 0.08]
Hou_2007.2	2006	742	2.09%	0.11 [0.04, 0.18]
Livingston_2005	2000	615	2.07%	0.03 [−0.05, 0.11]
Melnik_Richardson_et_al_2011	2007	579	2.06%	0.05 [−0.03, 0.13]
Kalyanam_McIntyre_2001	2001	564	2.05%	0.03 [−0.06, 0.11]
Eddhir_2009.1	2008	490	2.03%	0.14 [0.05, 0.23]
Lucking−Reiley_et_al_2007	1999	461	2.02%	0.08 [−0.01, 0.17]
Przepiorka_et_al_2017	2012	3153	2.01%	0.20 [0.10, 0.28]
Song_Baker_2007.2	2005	412	2.00%	−0.32 [−0.40, −0.23]
Baker_Song_2008.1	2005	404	1.99%	0.11 [0.01, 0.21]
Song_Baker_2007.1	2005	378	1.98%	0.24 [0.14, 0.33]
Resnick_Zeckhauser_2002.1	1999	378	1.98%	0.03 [−0.07, 0.13]
Cui_Huang_2010	2008	372	1.98%	0.33 [0.24, 0.42]
Baker_Song_2008.2	2005	366	1.97%	0.09 [−0.01, 0.20]
Grund_Gurtler_2008	2005	313	1.93%	0.07 [−0.05, 0.18]
Zhu_Leboulanger_Li_2009.1	2007	259	1.89%	0.25 [0.13, 0.36]
Berger_Schmitt_2005.2	2003	254	1.88%	0.15 [0.03, 0.27]
Berger_Schmitt_2005.1	2003	240	1.86%	0.16 [0.03, 0.28]
Hou_2007.1	2006	246	1.86%	0.16 [0.03, 0.28]
Sears_2016.3	2012	2090	1.84%	0.16 [0.02, 0.28]
Sears_2016.4	2012	2091	1.84%	0.32 [0.19, 0.43]
Ye_Xu_et_al_2013.1	2011	205	1.82%	−0.07 [−0.21, 0.07]
Przepiorka_2013.2	2006	9767	1.81%	0.24 [0.10, 0.37]

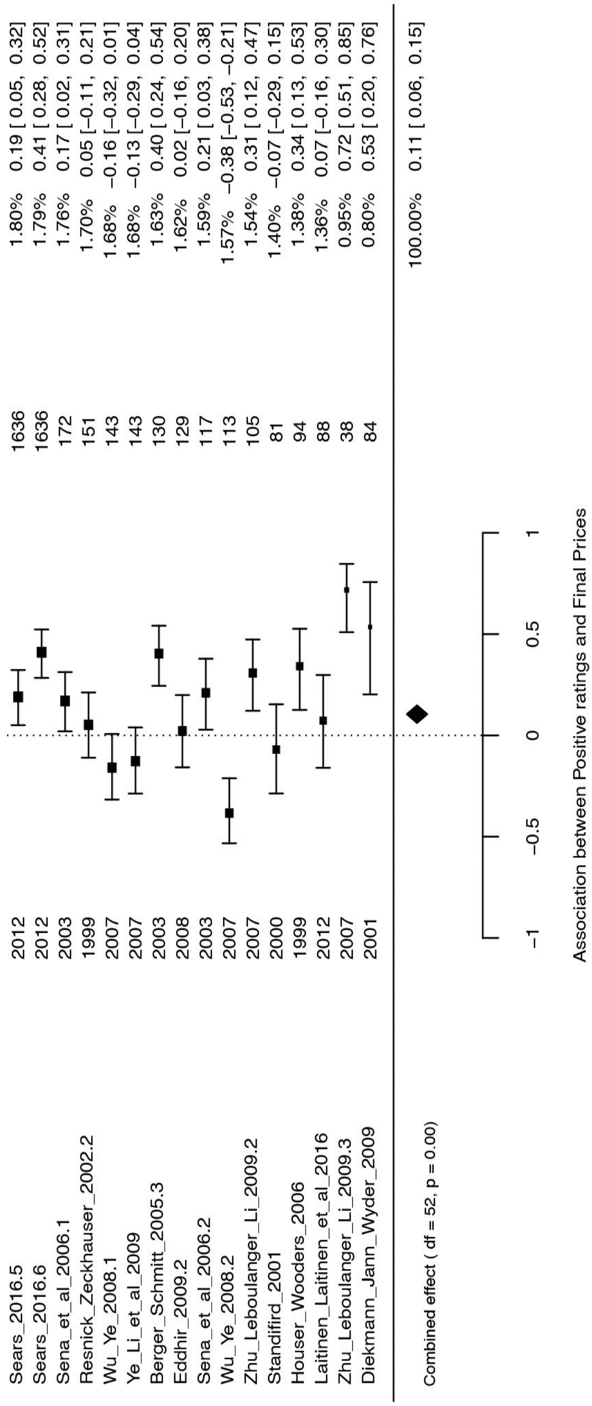
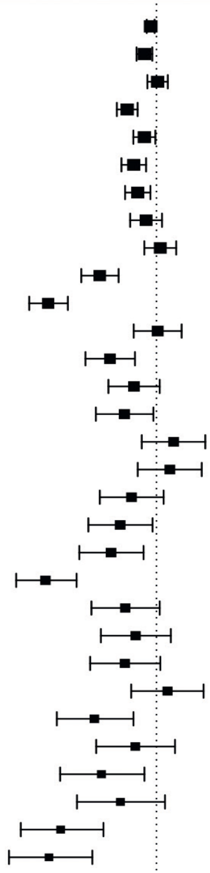


Figure 2.5. Forest plot of effect sizes of positive ratings on final prices

2

Study	Year of dataset		N	Weight	Correlation (95% CI)
Diekmann_et_al_2014.3	2004		180881	3.04%	−0.02 [−0.03, −0.01]
Diekmann_et_al_2014.2	2004		8727	3.01%	−0.03 [−0.06, −0.01]
Sears_2016.1	2012		12303	2.98%	0.00 [−0.03, 0.03]
Diekmann_et_al_2014.1	2004		5269	2.97%	−0.08 [−0.11, −0.05]
Melnik_Alm_2005	2002		3830	2.97%	−0.03 [−0.07, −0.00]
Przepiorka_2013.1	2006		63815	2.94%	−0.07 [−0.10, −0.03]
Dewally_Ederington_2006	2001		2906	2.94%	−0.05 [−0.09, −0.02]
Kauffman_Wood_2000.1	1999		1822	2.87%	−0.03 [−0.08, 0.02]
Kauffman_Wood_2000.2	1999		1822	2.87%	0.01 [−0.03, 0.06]
Zhang_2006	2004		1276	2.80%	−0.16 [−0.22, −0.11]
Zhou_Dresner_et_al_2009	2004		1025	2.74%	−0.31 [−0.37, −0.26]
Cabral_Hortaqcsu_2010	2002		1053	2.67%	0.00 [−0.07, 0.07]
Hou_2007.2	2006		742	2.64%	−0.13 [−0.20, −0.06]
Anderson_et_al_2007	2001		722	2.62%	−0.06 [−0.14, 0.01]
Kalyanam_McIntyre_2001	2001		564	2.52%	−0.09 [−0.17, −0.01]
McDonald_Slawson_2002	1998		460	2.44%	0.05 [−0.04, 0.14]
Sun_Liu_2010	2005		466	2.43%	0.04 [−0.05, 0.13]
Lucking−Reiley_et_al_2007	1999		461	2.43%	−0.07 [−0.16, 0.02]
Melnik_Alm_2002	2000		450	2.42%	−0.10 [−0.20, −0.01]
Przepiorka_et_al_2017	2012		3153	2.41%	−0.13 [−0.22, −0.04]
Song_Baker_2007.2	2005		412	2.38%	−0.32 [−0.40, −0.23]
Baker_Song_2008.1	2005		404	2.36%	−0.09 [−0.19, 0.01]
Song_Baker_2007.1	2005		378	2.33%	−0.06 [−0.16, 0.04]
Cui_Huang_2010	2008		372	2.32%	−0.09 [−0.19, 0.01]
Baker_Song_2008.2	2005		366	2.30%	0.03 [−0.07, 0.14]
Grund_Gurtler_2008.2	2005		313	2.19%	−0.18 [−0.29, −0.07]
Grund_Gurtler_2008.1	2005		313	2.19%	−0.06 [−0.17, 0.05]
Berger_Schmitt_2005.2	2003		254	2.08%	−0.16 [−0.28, −0.03]
Berger_Schmitt_2005.1	2003		240	2.04%	−0.10 [−0.23, 0.02]
Hou_2007.1	2006		246	2.03%	−0.28 [−0.39, −0.15]
Sears_2016.2	2012		2090	1.99%	−0.31 [−0.42, −0.18]

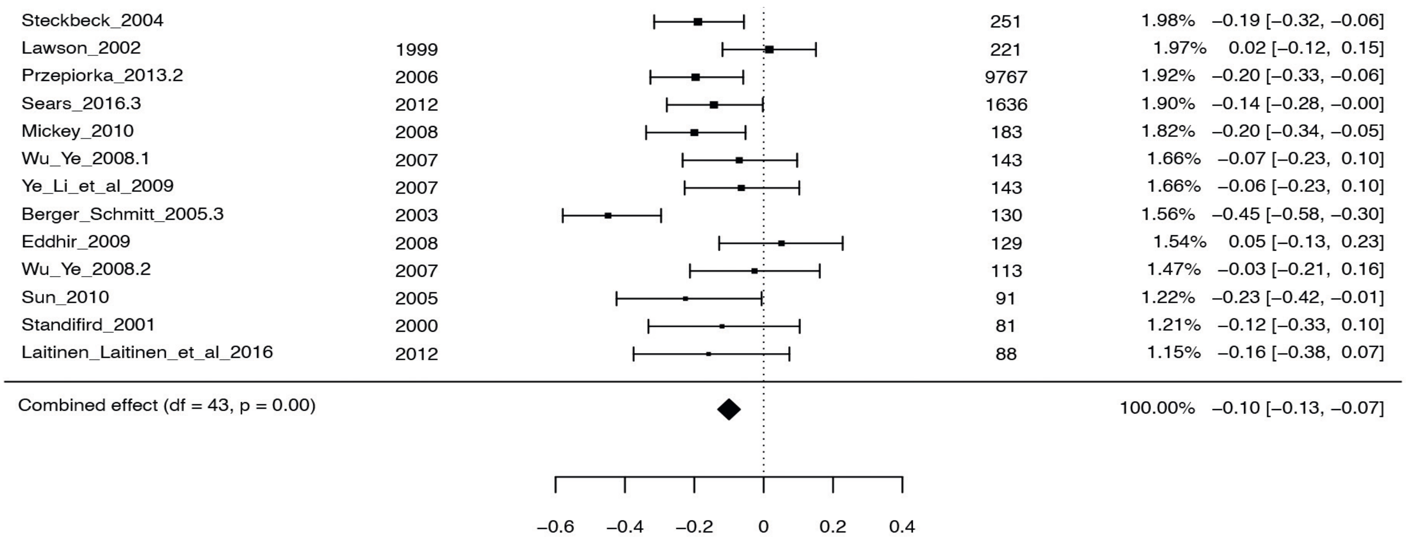
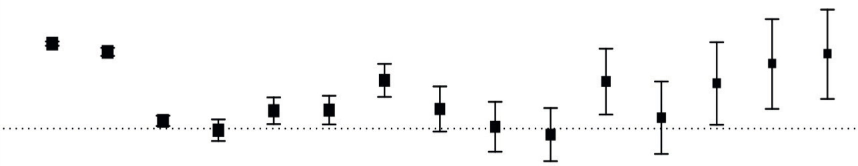


Figure 2.6. Forest plot of effect sizes of negative ratings on final prices

Study	Year of dataset	N	Weight	Correlation (95% CI
Zhou_Zhang_et_al_2006.1	2002	182853	7.04%	0.18 [0.17, 0.18]
Zhou_Zhang_et_al_2006.2	2002	52363	7.01%	0.16 [0.15, 0.17]
Diekmann_et_al_2014.3	2004	339517	6.99%	0.02 [0.01, 0.03]
Diekmann_et_al_2014.2	2004	9128	6.81%	−0.00 [−0.03, 0.02]
Shen_Chiou_Kuo_2011	2004	5013	6.68%	0.04 [0.01, 0.06]
Diekmann_et_al_2014.1	2004	5499	6.63%	0.04 [0.01, 0.07]
Przepiorka_2013.1	2006	89982	6.50%	0.10 [0.07, 0.13]
Zhang_2006	2004	1768	6.10%	0.04 [−0.01, 0.09]
Quill_2007	2006	1442	5.92%	0.00 [−0.05, 0.06]
Xiao_Liu_2009	2008	1283	5.80%	−0.01 [−0.07, 0.04]
Wolf_Muhanna_2005	2003	815	5.27%	0.10 [0.03, 0.17]
Melnik_Richardson_et_al_2011	2007	694	5.03%	0.02 [−0.05, 0.10]
Li_Cui_2013	2012	526	4.60%	0.09 [0.01, 0.18]
Przepiorka_2013.2	2006	38985	4.30%	0.14 [0.04, 0.23]
Andrews_Benzing_2007	2004	437	4.29%	0.16 [0.06, 0.25]



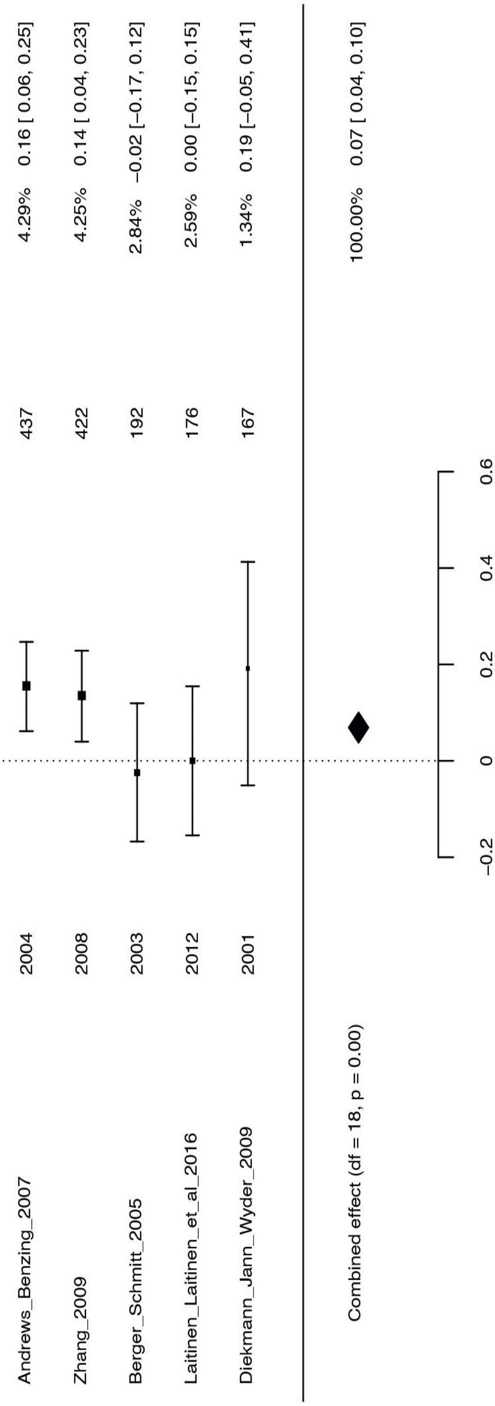
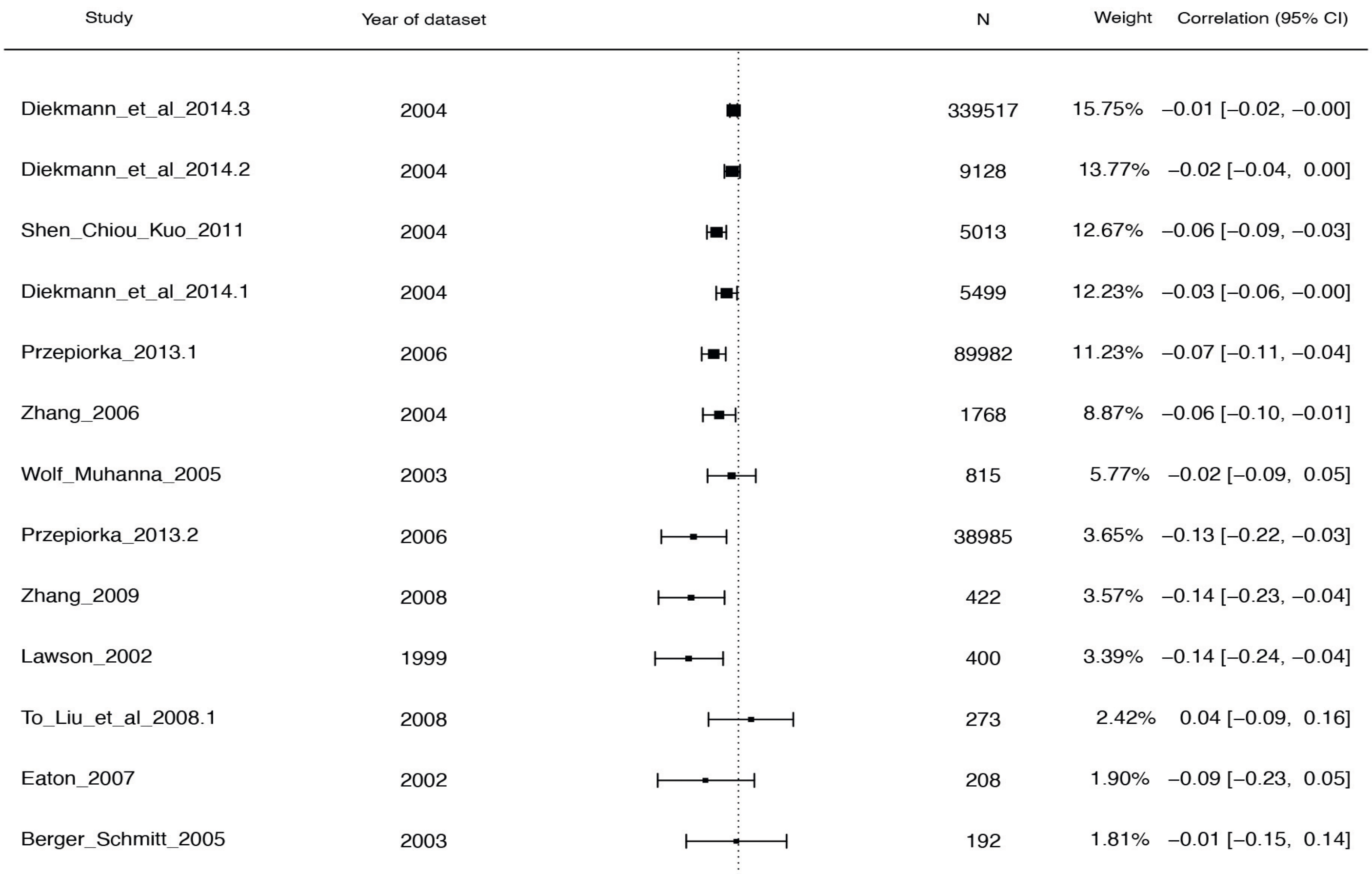


Figure 2.7. Forest plot of effect sizes of positive ratings on selling probabilities

2


Study
Year of dataset
N
Weight
Correlation (95% CI)
Diekmann_et_al_2014.3 2004 339517 15.75% −0.01 [−0.02, −0.00]
Diekmann_et_al_2014.2 2004 9128 13.77% −0.02 [−0.04, 0.00]
Shen_Chiou_Kuo_2011 2004 5013 12.67% −0.06 [−0.09, −0.03]
Diekmann_et_al_2014.1 2004 5499 12.23% −0.03 [−0.06, −0.00]
Przepiorka_2013.1 2006 89982 11.23% −0.07 [−0.11, −0.04]
Zhang_2006 2004 1768 8.87% −0.06 [−0.10, −0.01]
Wolf_Muhanna_2005 2003 815 5.77% −0.02 [−0.09, 0.05]
Przepiorka_2013.2 2006 38985 3.65% −0.13 [−0.22, −0.03]
Zhang_2009 2008 422 3.57% −0.14 [−0.23, −0.04]
Lawson_2002 1999 400 3.39% −0.14 [−0.24, −0.04]
To_Liu_et_al_2008.1 2008 273 2.42% 0.04 [−0.09, 0.16]
Eaton_2007 2002 208 1.90% −0.09 [−0.23, 0.05]
Berger_Schmitt_2005 2003 192 1.81% −0.01 [−0.15, 0.14]

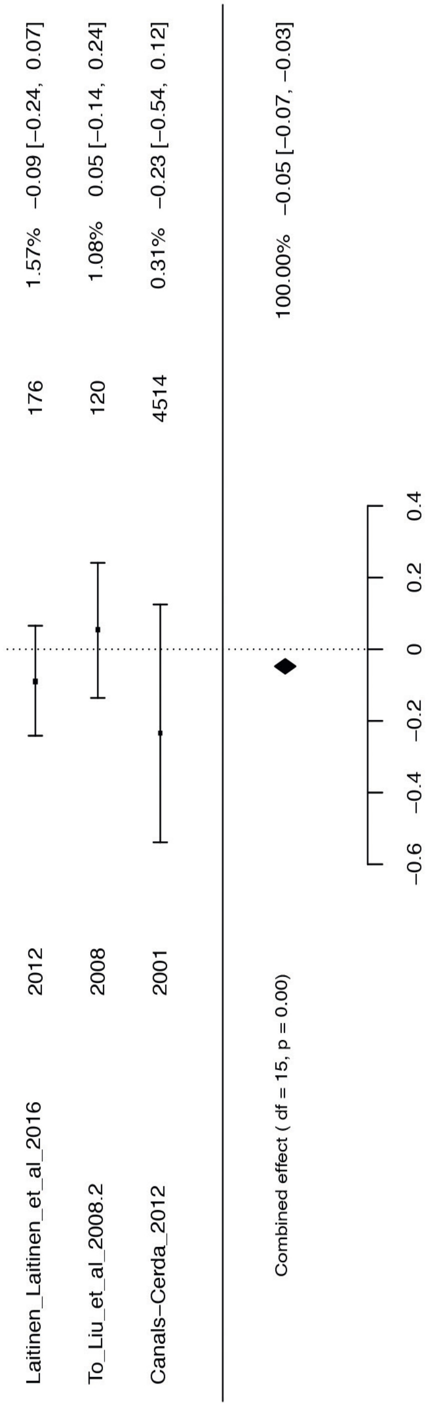


Figure 2.8. Forest plot of effect sizes of negative ratings on selling probabilities

2

2.5.3 Publication bias

Lastly, the Egger's regression test (Egger et al., 1997) for funnel plot asymmetry on each set of meta-analyses is reported in Table 2.4. Seven out of 12 meta-analyses exhibit asymmetric funnel plots (i.e. statistically significant Egger's test statistics). This is a first indication of publication bias. However, only five of these seven tests have a sign in line with the sign of the hypothesized reputation effect. For example, Egger's test in case of the effect of the number of positive ratings on final price has the same sign as the overall effect size estimate. Here, funnel plot asymmetry is likely due to publication bias. In case of the effect of the reputation score on selling probability, the Egger's test has a negative sign whereas the overall effect size estimate has, as expected, a positive sign. This may imply that smaller studies that show a large standard error and are more prone to publication bias exhibit a lower reputation effect.

Simulation studies have shown that the Egger's test is not sensitive to effect size heterogeneity stemming from sampling error. However, if effect size heterogeneity is due to differences in samples and study designs, which is the case in our meta-analyses, Egger's test may be biased (see, e.g., Schneck, 2017; Sterne et al., 2011).

2.6 Discussion and conclusions

The increasing popularity of peer-to-peer online markets brings attention to the role of reputation systems, which collect and present information on the trustworthiness and competence of traders based on their past online market exchanges (Dellarocas, 2003; Diekmann et al., 2014). From a game-theoretic perspective, information about seller reputation helps to promote buyer trust as it decreases untrustworthy behaviors of sellers. Sellers have to behave cooperatively to build and maintain and good reputation, and since they also have to offer discounts when entering the market, their reputations and business success in terms of prices and sales will be correlated (Przepiorka, 2013; Shapiro, 1983).

This relation between seller reputation and success, which also has been shown to be causal (Przepiorka, 2013; Snijders & Weesie, 2009), is called the reputation effect. In the last 20 years, a large body of literature has accumulated that seeks to find evidence for the reputation effect in real-world online markets. However, past studies present inconsistent results and there is a lack of consensus on what the reputation effect means and how substantial it might be (Lindenberg et al., 2020; Snijders & Matzat, 2019).

In this paper we integrated evidence from 107 existing studies, including 378 coefficients estimated based on 181 different datasets comprising a total of 14.04 million observations of online market transactions. We conducted twelve separate meta-analyses, one for each combination of three seller reputation variables and four seller performance variables commonly used in the literature. This approach allowed us to establish the robustness of the reputation effect across different operationalizations of seller reputation and selling performance.

To our knowledge, our study incorporates the largest number of studies among the existing systematic reviews on the subject of reputation effects and is the first to consider effect sizes rather than only signs and statistical significances of reputation effects (also see Liu et al., 2007; Schlägel, 2011). We were also able to interpret papers in languages other than English. There are thirteen papers written in Chinese and one paper in German. Moreover, we exhibited great effort to incorporate any possible study or research outcome. For instance, 11% (43 out of 378) of the coefficients we used were not accompanied with information about standard errors, *t*-scores or *p*-values, which are needed to calculate effect sizes and make them comparable. Instead only *p*-value ranges indicated by stars were reported in these cases. We proposed different strategies (as reported in Table 2.1) to determine estimated *p*-values for subsequent calculations of effect sizes. Finally, we used a relatively new approach that relies on the calculation of partial correlation coefficients to make effect sizes comparable (Aloe, 2014; Aloe et al., 2017; Rosenthal, 1991; Tong & Guo, 2022).

Our results show that seller reputations affect seller performance in the expected directions: overall, positive ratings have positive effects on all types of selling performance and negative ratings have negative effects (although two of the four negative overall effects are statistically insignificant). Although the overall effect sizes (as reported in Table 2.4) appear to be small, they should not be interpreted as ‘weak’ or substantially insignificant. Tracing back effect sizes to the original studies reveals that what sellers in online markets obtain for a good reputation can be substantial. However, the reputation effects included in our meta-analyses exhibit a high degree of heterogeneity that cannot be attributed to sampling error only. This is not entirely surprising given the differences in market platforms, item data, and modelling approaches used across studies (see Tables 2.2 and 2.3). Although we already grouped coefficients that were estimated using the same type of seller reputation and performance variables, we will try to identify the different sources of variation of reputation effects reported in previous literature by means of subgroup analysis and meta-regression (see, e.g., Tong & Guo, 2022) in a subsequent paper.

There are three categories of moderator variables that suggest themselves: (1) contextual factors (e.g., market platform, geo-cultural region of market participants, time), (2) overall characteristics of the traded products (e.g., price category, usage status, complexity), and (3) methodological factors (e.g., number and type of explanatory variables and controls, specification of functional form of seller reputation, statistical model construction). For example, more expensive, more complex and used rather than new products face buyers with higher risks and uncertainty. It can be therefore expected that these product characteristics will have a positive, moderating effect on the reputation effect. However, it remains to be shown in how far meta-analysis that uses coefficient estimates from multiple regression models can shed light on substantial (rather than methodological) reasons for the variation in reputation effects. We conclude this paper with pointing out an often neglected, substantial reason for the excess variation in reputation effects.

Note that in our game theoretic model above, we made the implicit assumption that, after every transaction, a seller is rated truthfully with certainty. Relaxing this assumption does not only pay justice to the fact that a substantial part of transactions are not rated or not rated truthfully (Dellarocas & Wood, 2008; Diekmann et al., 2014), but it also unveils a substantial reason for why the size of the reputation effect may vary across markets and within markets over time. The intuition behind this theoretical argument goes as follows (for a formal derivation see Przepiorka, 2013): The lower the rate of truthful ratings is, the longer it will take to identify untrustworthy sellers. The longer it takes to identify untrustworthy sellers, the higher is the incentive for these sellers to enter the market. The more untrustworthy sellers enter the market, the higher will be the probability of encountering an untrustworthy seller ($1 - \alpha$). By equation (2), the higher is $1 - \alpha$, the higher will be the price discount d honest sellers will have to make when entering the market.

This argument results in a seemingly counterintuitive conjecture: The more effective a reputation system is in identifying dishonest sellers in an online market, the smaller will be the reputation effect. This is an important point to make because a small reputation effect is often used as first evidence for positive evaluation bias and the malfunctioning of a reputation system (e.g., Tadelis, 2016). In other words, a reputation system may be effective not because sellers earn large premiums for their good reputations, but because the mere presence of the reputation system attracts a majority of trustworthy and reliable sellers (Diekmann et al., 2014). Bockstedt and Goh (2011) found evidence that in an online market concentrated with experienced and reputable sellers, the reputation scores indeed are less relevant for seller differentiation. If, as a result, buyers' a priori levels of trust are high, these buyers will be less inclined to pay for reputation and the reputation effect will thus be smaller.

3

CHAPTER

Moderators of reputation effects in peer-to-peer online markets:

a meta-analytic model selection approach

Ruohuang Jiao, Wojtek Przepiorka and Vincent Buskens

This chapter has been published as Jiao, R., Przepiorka, W., & Buskens, V. (2022). Moderators of reputation effects in peer-to-peer online markets: a meta-analytic model selection approach. Journal of Computational Social Science, 5, 1041-1067. Jiao collected and updated the meta-analytic dataset and did the meta-analysis. Jiao wrote the manuscript in collaboration with Przepiorka. All authors contributed to (double checking) the coding, interpretation of the findings and editing the manuscript.

Abstract:

The effect of seller reputation on seller success in peer-to-peer online markets has been investigated in dozens of studies by means of the analysis of digital trace data. A recent meta-analysis synthesizing evidence from over a hundred studies corroborates that sellers with a better reputation sell more products at higher prices. However, the meta-analysis also shows that these reputation effects exhibit excess variation that cannot be attributed to sampling error. Moreover, there is still little consensus on how the size of a reputation effect should be interpreted and what might cause its variation. Here we use a meta-analytic model selection approach and multi-model inference on two subsets of 406 coefficient estimates to identify potential moderators of reputation effects. We identify contextual, product-related, and method-related moderators. Our results show that, among others, geographical region, product condition, sample size, and type of regression model have a bearing on the size of the reputation effect. The moderating effect of the geographical region suggests that reputation effects are substantially larger in the Chinese context than in the European or US contexts. The moderating effect of product condition—estimates based on new products are larger than estimates based on used products—is unexpected and worthwhile investigating further. The moderating effects of sample size and model type could be related to study quality. We do not find evidence for publication bias as a potential explanation for the effects of method-related moderators.

Keywords: online market; reputation system; reputation effect; meta-regression; model selection; multi-model inference

3.1 Introduction

With the increasing popularity of online markets, more research is concerned with how reputation systems promote cooperative market exchanges. Reputation systems that are commonly employed in online market platforms collect, aggregate, and disseminate information about traders' past behaviors and the quality of their goods and services (Kollock, 1999; Resnick et al., 2000; Swamynathan et al., 2010). Trader reputation profiles are created from numeric scores (positive, neutral or negative ratings, or five-star ratings) and feedback texts (i.e. feedback messages describing the experience with certain traders and their goods and services).

Reputation systems are particularly useful for buyers, who decide which sellers to transact with but are uncertain about seller trustworthiness. In offline economic exchange, uncertainty and trust issues are often managed through networked structures through which firms establish reputations (e.g. Podolny, 1994; Uzzi, 1996; and see Buskens & Raub, 2013 for a review). However, long-term business relations in larger business networks can hardly be established and maintained online without additional trust-building mechanisms. Reputation systems replace the role of offline networks in managing trust in online transactions. In reputation-based online markets, sellers have an incentive to be trustworthy and send back the merchandise or provide the service the buyer paid for to maintain a good reputation in favor of future business success. In addition, new sellers, who do not yet have a record of past transactions, must invest in building their reputation by reducing prices or sending other signals of their trustworthiness (Elfenbein et al., 2012; Przepiorka, 2013; Shapiro, 1983). As a consequence, these sellers' reputations and their business success will be positively correlated, a phenomenon that is also known as the reputation effect.

Many studies have estimated the reputation effect based on digital trace data of online market transactions. Jiao et al. (2021) performed a series of meta-analyses synthesizing evidence from over hundred such studies. Their meta-analyses corroborate the existence of reputation effects in peer-to-peer online markets. Their results provide evidence for the general relationship between seller reputation and selling performance in terms of the direction (a positive relation for positive ratings and a negative relation for negative ratings) and statistical significance. However, there is substantial heterogeneity in reputational effect sizes that cannot be explained by sampling error alone (Borenstein et al., 2009). In their study, this is evidenced by high I^2 statistics, which describe the percentage of between-study variability to total variability (i.e. within and between study variability in effect sizes) (see Jiao et al., 2021, Table 4).

The aim of the present study is to explain the excess variation in seller reputation effects by means of the dataset created by Jiao et al. (2021). Inspired by arguments and discussions in previous literature on possible moderators of the reputational effect, we identify contextual moderators, product-related moderators and method-related moderators. For example, the market context in which the online transactions take place should be taken into consideration because traders' behavior will likely be influenced by their cultural, spatiotemporal and institutional embeddedness (Beckert, 2009; Nee, 2005). Moreover, the types of traded products, which range from small stamps to large motor vehicles, will also have a bearing on the size of the reputation effect. In particular, product prices, item conditions and their popularity are likely moderators of reputation effects. Finally, the methods applied across existing studies vary considerably. Even though it is possible to make reputational effect sizes comparable for the purpose of including them in meta-analyses, the variety of statistical modelling approaches will influence the estimation of the reputation effect.

We use a meta-analytic model selection approach and multi-model inference to integrate the findings from previous studies and identify potential moderators of reputation effects empirically. The model selection approach allows us to systematically consider and compare meta-regression models and determine which set of moderators contributes to the best fitting models. The multi-model inference part provides us with the relative importance of each moderator, i.e. the likelihood of each moderator to be included in a well-fitting meta-regression model. To our knowledge, we are the first to apply model selection and multi-model inference in meta-analysis to address the substantial question why reputation effects vary in size (Lindenberg et al., 2020).

Even though we propose a few general expectations regarding potential moderators based on suggestions provided in the literature, our study is largely exploratory. Our main interest lies in determining the most influential moderators of reputational effect size within the dataset we have available. Hence, our paper contributes to the discussion on what moderators influence observable reputation effects and applies computational social science methodology to test the validity of our conjectures. Although many of the moderators that we consider in our analyses are likely correlated with variables we do not observe, this shall not prevent us from learning something from the rich dataset created by Jiao et al. (2021) and discover interesting relations that could be followed up on in future research using methods that are more suitable for detecting causal relations.

3.2 Theoretical considerations

To provide explanations for the excess variation of observed reputation effects reported in Jiao et al. (2021), we draw on theoretical considerations from previous literature. Potential moderators can be categorized as contextual moderators, product-related moderators and method-related moderators. In this section, we outline general expectations as to why and how these three sets of moderators might have a bearing on the size of the reputation effect. Figure 3.1 summarizes our considerations.

3.2.1 Contextual moderators

Context refers to the cultural, spatiotemporal and institutional embeddedness of online market exchanges (Beckert, 2009; Nee, 2005). Contextual differences may lead to different attitudes towards strangers, perceptions of trustworthiness (Lo Iacono & Quaranta, 2019; Lo Iacono & Sonmez, 2021) and propensities of leaving feedback after completed online market transactions. As a result, reputation effects may differ (Schilke et al., 2021). How do context-dependent generalized trust levels and propensities to leave feedback affect the size of the reputation effect?

Generalized trust refers to individuals' trust in strangers, i.e. people that are not part of one's family, friendship and acquaintance network (Nannestad, 2008; Putnam, 1993; Uslaner, 2002). If generalized trust is low, people may resort to other trust-building mechanisms such as reputation systems. However, because of these people's low a priori expectations of online sellers' trustworthiness, sellers with no or short records of successfully completed transactions will have to offer their items at lower prices compared to established sellers with a good reputation (Jiao et al., 2021; Przepiorka, 2013). As a consequence, the reputation effect will be larger in markets embedded in low-trust contexts.

In our analysis we distinguish between three geographical regions: USA, China, and Europe. Theoretical arguments corroborated by empirical evidence suggest that functioning legal systems protecting property rights (Berggren & Jordahl, 2006) and democratic institutions (Ljunge, 2014) promote generalized trust. This, in turn, suggests that the Chinese context will be characterized by lower levels of generalized trust and thus exhibit larger reputation effects than online market exchanges in the USA or Europe. However, this conjecture does not seem to be valid as to the level of generalized trust, which is relatively high in China (Steinhardt, 2012; Tan & Tambyah, 2011). The results of Steinhardt's study (2012) suggest that it is people's confidence in political institutions, irrespective of these institutions' suitability to protect property rights or promote democratic processes, that is positively related with generalized trust. We therefore refrain from stating expectations regarding the moderating effect of geographical region (via generalized trust) on reputation effects.

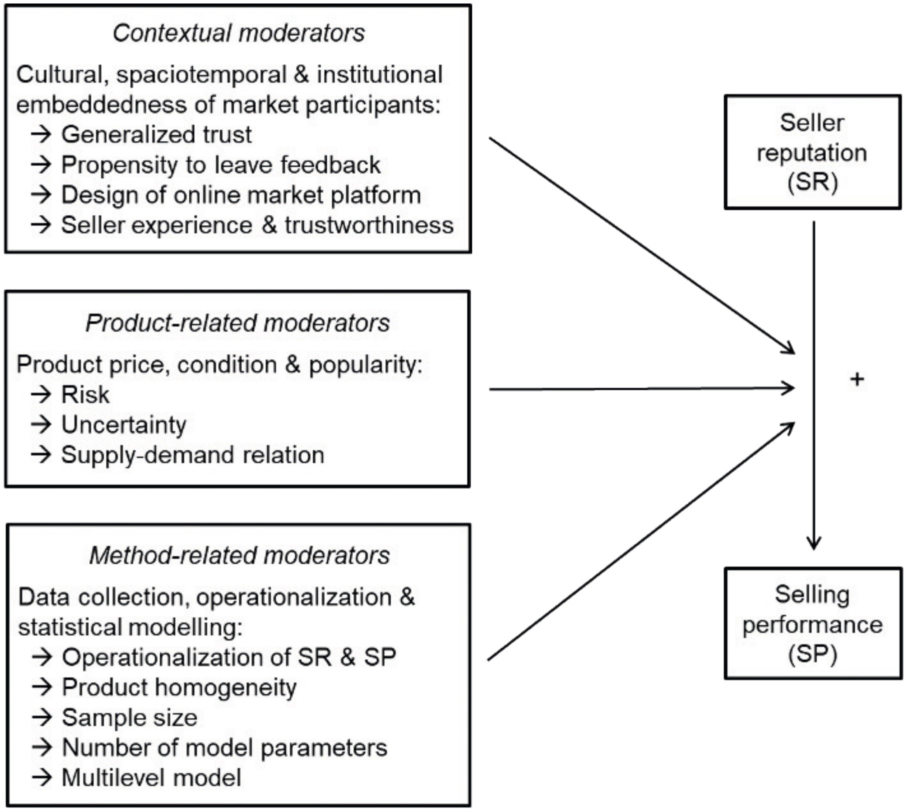


Figure 3.1. Contextual, product-related, and method-related moderators of reputation effects

Not leaving truthful feedback after a bad experience with an online transaction or leaving overly positive feedback after a mediocre experience can lead to a positive bias in seller reputations. As a consequence, untrustworthy sellers selling low quality items will be identified with a delay or not at all, which will make it more attractive for these sellers to enter the market. Because, as a result, the likelihood of encountering a untrustworthy seller will be higher, buyers will demand larger discounts when dealing with sellers without an established reputation record. This, in turn, will lead to a larger reputation effect (Jiao et al., 2021; Przepiorka, 2013).

In as far as people differ in their propensity to leave truthful feedback after completed online market transactions, contextual factors can also affect the magnitude of the reputation effect. For example, in some cultures there are norms proscribing overly critical feedback whereas in other cultures there are norms proscribing overly praising feedback. Zhao and Huang (2008) state that in China buyers rarely give negative ratings, and in case of a negative experience with a seller, buyers tend to resort to neutral ratings. However, there is also evidence that people are reluctant to give negative feedback in the US context and rather refrain from giving feedback after a bad experience (Dellarocas & Wood, 2008; Nosko & Tadelis, 2015). Relatedly, results from the Global Preference Survey (Falk et al., 2018) indicate that people's intentions to positively reciprocate good deeds and to negatively reciprocate misdeeds may not be so different in China and the USA, whereas in Europe people on average exhibit more variation in these intentions across countries. We therefore refrain from stating expectations regarding the moderating effect of geographical region (via feedback behavior) on reputation effects.

Differences in design features of online market platforms and reputation systems may instigate the emergence of different rating conventions through which buyers might perceive seller reputations differently (Ahrne et al., 2015). For example, the possibility to leave feedback may be used to establish seller trustworthiness, but in a two-sided rating system, where buyers can rate sellers and vice versa, ratings can be used as a means to positively or negatively reciprocate one's trading partner's positive or negative rating, respectively (Bolton et al., 2013; Diekmann et al., 2014; Dini & Spagnolo, 2009). As a consequence of the threat to retaliate a negative rating with a negative rating that can be upheld in two-sided rating systems, seller reputations will be positively biased. Hence, buyers may trust seller reputation information in such systems less, so that the reputation effect will be smaller (Tadelis, 2016). However, since information on how truthful ratings in different online market platforms are is not usually available and reported, we account for the market platform as a potential moderator but do not state any expectations as to the direction of its effect on the reputation effect.

Since first implemented on eBay, online reputation systems have gone through changes with the aim to make reputation systems more effective in keeping untrustworthy sellers out of business (e.g., Bolton et al., 2013; Dini & Spagnolo, 2009; Roth, 2015). Therefore, over time, buyers may have become more aware of the effectiveness of reputation systems. If, over time, the average seller quality has indeed become higher as a consequence of the improvements in reputation systems, we can expect the reputation effect to become smaller; if more sellers are trustworthy, buyers need to rely less on information about seller reputations when making their buying decisions. In our analysis, we will account for the time of data collection as a moderator of the reputation effect and expect its effect to be negative. However, the argument that, over time, the reputation effect will become smaller because average seller quality increases, hinges on the assumption that the number of new sellers entering the market is relatively low. A substantial number of sellers that enter the market anew may positively affect the size of the reputation effect because these sellers still need to build their reputation by offering price discounts (Przepiorka, 2013; Shapiro, 1983). However, information on the average experience of sellers in a particular online market platform is not usually reported and therefore not available to be included in our analyses. Figure 3.1 summarizes the contextual moderators of reputation effects in peer-to-peer online markets.

3.2.2 Product-related moderators

The size of the reputation effect may systematically vary with product features because of the information asymmetries buyers face in online transactions and the risks and uncertainties that result from such information asymmetries (Akerlof, 1970; Diekmann & Przepiorka, 2019). The risk that a buyer takes in an online market transaction depends on the probability of the seller being trustworthy and the price of the traded product. Since more expensive products exhibit a higher risk, a buyer might be willing to pay a higher price to a seller with a better reputation to reduce the probability of being cheated (Yin, 2017). Hence, the higher the price of the traded product, the larger will be the reputation effect.

The uncertainty buyers face in online transactions also stems from the uncertainty regarding product quality. In peer-to-peer online markets both new and used products are sold. While uncertainty about product quality is low for new products, it will be high for used products. For used products, buyers' expectations are formed based on how sellers describe and present these products and, therefore, eventually, on how trustworthy buyers expect these sellers to be. We thus expect that for used products or for products of unknown condition, seller reputation information will play a more important role for buyers. As a result, the reputation effect will be larger for used products or products of unknown condition than for new products (Diekmann et al., 2014).

Finally, the extent of market clearing will influence the size of the reputation effect. If the market for a certain product experiences higher demand than supply (e.g., because of the product's newness and popularity), buyers may be willing to take higher risks and pay less attention to seller reputations and other signs of seller trustworthiness (Doleac & Stein, 2013; Przepiorka, 2011). However, if supply is higher than demand, sellers will experience more competition among each other and sellers with a lower reputation may need to accept lower prices for their products to be chosen by buyers (Frey & van de Rijt, 2016). Therefore, we expect that in markets in which demand exceeds supply, the seller reputation effect will be smaller than in markets in which supply exceeds demand.

3.2.3 Method-related moderators

Various types of operationalizations of seller reputation have been used among the studies included in our meta-analyses. For example, the reputation score (the number of positive ratings a seller received minus the number of negative ratings), the number of positive ratings, the percentage of positive ratings (the percentage of positive ratings among all received ratings), etc., have been used as measures of seller reputation. In most online market platforms, the number and percentage of positive ratings, the reputation score and the number of negative ratings are presented on seller profile pages, but it is researchers' decisions which of these measures are used in their analyses. Although the different operationalizations of seller reputation are highly correlated with each other (Zhu et al., 2009), the size of the reputation effect may still depend on which one is used in statistical data analysis. For example, the reputation effect may be lower if it is estimated based on the percentage of positive ratings, because the variance of this variable appears to be small (Andrews & Benzing, 2007; Yin, 2017). In many online market platforms, most sellers have very high percentages of positive ratings (98% and higher). This may result from the default rating set by the platform (Przepiorka et al., 2017), or from sellers trying to prevent any non-positive ratings (even with inappropriate means) (Cui & Huang, 2010). Thus, the small range of the percent-of-positive-ratings variable may provide too little leverage to identify a sizable reputation effect. Another reason for why using the percentage of positive ratings might result in a lower reputation effect is that it is not a valid measure of seller trustworthiness. Sellers with 50 transactions and sellers with 1000 transactions alike can have 99% positive ratings but the latter can be considered as more trustworthy than the former. This is because a good reputation must be costly to acquire and therefore 1000 mostly positive ratings are a stronger sign of trustworthiness than 50 mostly positive ratings (Przepiorka & Berger, 2017). Therefore, we assume that the size of the reputation effect will be smaller if sellers' positive (or negative) ratings are measured in terms of percentages rather than absolute numbers.

Moreover, whether the variables measuring seller reputations and product prices are log-transformed or untransformed may also influence the size of the reputation effect. If the relation between seller reputation and selling performance indeed is non-linear (e.g., increasing at a decreasing rate, see Przepiorka, 2013), a linear model might produce a downward biased reputation effect. We thus expect models with log-transformed seller reputation and selling performance to produce larger reputation effects. To better understand how reputation effects depend on such method-related moderators, the types of operationalizations of seller reputation and selling performance are included in the exploratory model selection process.

Whether the traded products are homogeneous within each included dataset is a second aspect of methodological concern that is likely to affect the size of the reputation effect. Although this might appear to be a product-related moderator (see above), we include product homogeneity as a method-related moderator; it is researchers' decisions how homogeneous the products are they collect data on and analyze in their studies. Even for the same category of products (e.g., mobile phones), some researchers choose a specific category to collect data on, e.g., Eddhir (2009) collected data on unlocked iPhone 3G; whereas other researchers choose a more general product category, e.g., Zhou (2014) collected data on mobile phones, and provide no further information on what the product category comprises. We assume that there is a larger variation in observed reputation effects if a dataset of heterogeneous products is used, and we account for product heterogeneity in our meta-analyses. However, we do not have any expectations as to the direction of its effect on the reputation effect.

Finally, we expect that the way in which the statistical models are constructed with which reputation effects are estimated will have a bearing on the size of the reputation effect. We identify three aspects of statistical model building: (1) whether it is a multi-level model, (2) the number of observations or clusters and (3) the number of parameters. As for the type of model, a sizable proportion of models account for the repeated observations on same sellers by fitting multi-level regression models. In particular, these models estimate cluster-robust standard errors of coefficient estimates to account for the dependence of observations stemming from the same seller. The number of observations, clusters and parameters influence the calculation of reputational effect sizes through the degrees of freedom (df = N – k – 1, where N indicates the number of observations or number of clusters and k indicates the number of parameters). Models with more cases are more likely to provide better estimations of reputation effects. However, random deviation can suggest that an effect is larger as well as smaller. Models with cluster-robust standard errors tend to be conservative in terms of estimating significance, but do not affect the effect size. Thus, we do not state any expectations as to the net direction of the effect of number of observations, clusters and parameters on the size of the reputation effect.

3.2.4 Control variables

Apart from the moderators that we identify above based on theoretical considerations and evidence from empirical research, we include a few control variables to capture attributes that are commonly reported and appear to be relevant to explain the between-study or between-dataset differences in reputation effects. The control variables are the publication status of the study (published in English vs published in other language vs not published) and the type of transaction captured in a dataset (auction vs fixed price offer).

3.3 Data and methods

The meta-analytic dataset was collected until October 2021. It includes 406 estimates of reputation effects (i.e. effects of seller reputation on selling performance), estimated with 202 different datasets, and reported in 125 empirical papers (also see Jiao et al., 2021). The literature search started with two previous meta-analytic studies (Liu et al., 2007; Schlägel, 2011) and was extended with literature searches to include more recent studies and studies written in other languages than English.[1]

This search process resulted in 205 relevant research articles written in English, Chinese or German. The PRISMA flow diagram (Moher et al., 2009) presented in Figure 3.2 outlines the criteria we used for the inclusion of studies for our meta-analyses.[2]

Further details on search criteria, study inclusion criteria and the calculation of effect sizes are described in Jiao et al. (2021).

Table 3.1 provides basic descriptive statistics of reputational effect sizes[3] across potential moderators at the study level and dataset level. Descriptive statistics are provided separately for the three different types of seller reputation information: reputation score, positive ratings and negative ratings. What is apparent from Table 3.1 is the large extent of variance in reputational effect sizes within categories of potential moderator variables. In what follows, we will describe each moderator variable in detail.

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1 Compared to the two previous meta-analyses, the current study not only updates and extends the data (Liu et al., 2007 include 42 articles and Schlägel (2011) includes 58 articles), but also extends our insights by using different methodology. The previous studies used combined significance tests and sign tests as the meta-analytic approaches, which only consider the sign of reputation effects. In our study, we transform the estimates of the reputation effects into comparable effect sizes. This allows us to assess the variation of these effect sizes and identify potential moderators of the reputation effect.

2 The complete list of the 205 articles and the reasons for exclusion (if excluded) is provided in Appendix B.1, and the complete reference list is provided in Appendix B.2

3 The effect sizes were calculated as Pearson correlations from bivariate relationships with $r = \rho$, or partial correlations from multiple regression models with $r = t/\sqrt{t^2+df}$, depending on the information reported in included studies. For more details, see Jiao et al., (2021).

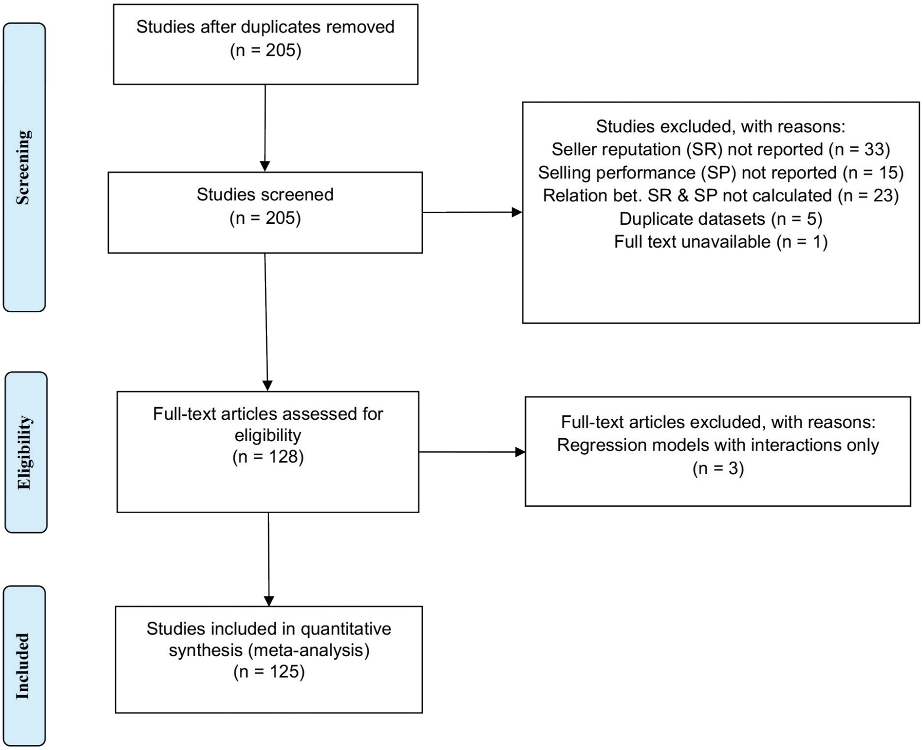

Figure 3.2. PRISMA flow diagram of literature search and inclusion criteria

Table 3.1. Summary of effect sizes (*ES*) for each category of seller reputation

	ES								
	Reputation score			***Positive ratings***			***Negative ratings***		
	N	***Mean***	***S.D.***	***N***	***Mean***	***S.D.***	***N***	***Mean***	***S.D***
Contextual moderators									
Region									
United States	88	0.03	0.18	73	0.19	0.23	57	-0.07	0.17
China	56	0.08	0.23	46	0.15	0.19	29	-0.10	0.24
Europe	13	0.02	0.07	23	0.12	0.14	20	-0.10	0.10
Platform									
eBay	85	0.02	0.17	80	0.18	0.22	69	-0.08	0.16
Taobao	49	0.06	0.24	44	0.14	0.20	22	-0.11	0.26
Other	23	0.09	0.16	18	0.16	0.15	15	-0.07	0.13
Year of data collection									
1998~2002	61	0.03	0.15	32	0.28	0.26	36	-0.04	0.18
2003~2007	27	0.03	0.18	55	0.12	0.18	41	-0.09	0.13
2008~2012	42	0.02	0.20	43	0.15	0.17	27	-0.11	0.22
2013~2019	27	0.14	0.25	12	0.08	0.07	2	-0.28	0.35
Product-related moderators									
Condition									
New	60	0.07	0.24	83	0.20	0.22	69	-0.07	0.20
Used	25	-0.02	0.17	10	0.01	0.12	8	-0.09	0.14

Table 3.1. Summary of effect sizes (*ES*) for each category of seller reputation (continued)

	ES								
	Reputation score			***Positive ratings***			***Negative ratings***		
	N	***Mean***	***S.D.***	***N***	***Mean***	***S.D.***	***N***	***Mean***	***S.D***
Unknown	72	0.05	0.15	49	0.12	0.17	29	-0.10	0.12
Average price (in US dollar, in 25% percentiles)									
2.86~26.63	41	0.06	0.16	25	0.15	0.14	19	-0.02	0.13
26.63~156.12	43	0.01	0.21	24	0.18	0.19	16	-0.07	0.16
156.12~353.6	16	0.03	0.12	36	0.16	0.20	33	-0.07	0.15
353.6~19342.4	24	0.08	0.23	34	0.19	0.28	24	-0.10	0.21
Missing	33	0.06	0.21	23	0.12	0.15	14	-0.15	0.24
Rate of market clearing (in 25% percentiles)									
0~49.34%	11	0.05	0.09	13	0.12	0.15	12	-0.08	0.09
49.34%~64.3%	15	0.04	0.09	11	0.08	0.08	9	-0.10	0.08
64.3%~82.58%	20	0.04	0.09	8	0.09	0.14	6	-0.10	0.18
82.58%~98%	8	-0.02	0.10	16	0.09	0.09	11	-0.10	0.08
missing	103	0.05	0.23	94	0.20	0.23	68	-0.07	0.21
Method-related moderators									
Log-transformed /percentage SR									
Log-trans.	75	0.05	0.22	78	0.22	0.22	73	-0.07	0.19
Percentage	n/a	n/a	n/a	50	0.11	0.15	13	-0.04	0.08
Untrans.	82	0.04	0.16	14	0.03	0.17	20	-0.15	0.16

Table 3.1. Summary of effect sizes (*ES*) for each category of seller reputation (continued)

	ES								
	Reputation score			***Positive ratings***			***Negative ratings***		
	N	***Mean***	***S.D.***	***N***	***Mean***	***S.D.***	***N***	***Mean***	***S.D***
Log-transformed SP									
Log-trans.	52	0.04	0.25	52	0.17	0.17	32	-0.14	0.19
Untrans.	105	0.05	0.16	90	0.16	0.22	74	-0.06	0.17
Product homogeneity									
Homogeneous	136	0.04	0.20	126	0.17	0.21	99	-0.08	0.18
Heterogenous	21	0.06	0.11	16	0.09	0.09	7	-0.10	0.14
Multilevel									
Multilevel	18	0.05	0.17	25	0.12	0.14	19	-0.12	0.13
Other	139	0.04	0.20	117	0.17	0.22	87	-0.08	0.19
Number of observations (in 25% percentiles)									
14~130	30	-0.06	0.30	40	0.34	0.25	33	-0.08	0.25
130~389	40	0.12	0.21	33	0.12	0.14	27	-0.04	0.14
389~2433	43	0.04	0.11	35	0.09	0.14	24	-0.11	0.14
2433~3051759	44	0.05	0.11	34	0.07	0.09	22	-0.09	0.12
Number of parameters (in 25% percentiles)									
1~3	21	0.09	0.30	52	0.23	0.27	41	-0.02	0.20
3~6	47	-0.01	0.23	27	0.15	0.17	14	-0.23	0.22
6~14	47	0.06	0.10	29	0.13	0.15	30	-0.10	0.13
14~348	42	0.07	0.15	34	0.09	0.09	21	-0.08	0.06

Table 3.1. Summary of effect sizes (*ES*) for each category of seller reputation (continued)

	ES								
	Reputation score			***Positive ratings***			***Negative ratings***		
	N	***Mean***	***S.D.***	***N***	***Mean***	***S.D.***	***N***	***Mean***	***S.D***
Controls									
Publication status									
English journal	108	0.04	0.17	83	0.17	0.22	81	-0.06	0.16
Local journal	24	0.14	0.19	30	0.11	0.11	10	-0.09	0.20
Other	25	-0.02	0.25	29	0.19	0.23	15	-0.16	0.25
Transaction type									
Auction	99	0.04	0.14	82	0.17	0.21	77	-0.07	0.16
Fixed price	54	9.97	0.24	51	0.14	0.19	25	-0.13	0.24
Unknown	4	-0.22	0.39	9	0.25	0.21	4	-0.12	0.06

Notes: For each category of seller reputation, the number of effect sizes that are included in the meta-analytic dataset, their mean and standard deviation are reported. Year of data collection, average price, rate of market clearing, number of observations and number of parameters are treated as continuous variables in the data analysis. SR stands for seller reputation and SP for selling performance.

3

3.3.1 Moderator variables

Region indicates the country or region where the dataset was collected. It reflects the cultural difference among datasets. As is shown in Table 3.1, most datasets were collected in the USA and China. Datasets in the category “Europe” were collected in France, Poland, Germany, Finland and Switzerland.

Market platform indicates on which market platform the dataset was collected, taken to explain the institutional differences among datasets. As is shown in Table 3.1, most datasets were collected from eBay (the largest peer-to-peer online market in the world) and Taobao (the largest peer-to-peer online market in China). Category “Other” includes the following platforms: Yahoo! (in USA and China), Eachnet (China), Priceminister (France), Allegro (Poland), Huuto (Finland), Bizerate (USA), Ricardo (Switzerland), Bonanza (USA) and Silkroad (a platform focusing on illegal drugs)[4].

Year of data collection is a continuous variable, indicating the year in which the collection of the dataset started. The variable ranges from 1998 to 2019. It is included in our analysis to capture the influence of the time period on the size of reputation effects.

Condition indicates whether the products comprised in a dataset were marketed new or used. The category “unknown” indicates that this information is not mentioned in the paper or the dataset includes both new and used products without differentiating between them.

Average price is a continuous variable reporting the mean price of products in the collected dataset. Given datasets were collected in various currencies, the mean prices are converted in US Dollars with Purchase Power Parities (PPP)[5] for comparability across countries with different price levels. The mean prices were mostly reported in the data description section of included papers, but for 55 of the 202 datasets the mean price was not reported. To reduce the number of missing values, we estimate the mean price in datasets with missing values

4 Jian et al. (2019) collect data from an unspecified platform, and the dataset from Snijders and Zijdeman (2004) comes from four different platforms.

5 https://data.oecd.org/conversion/purchasing-power-parities-ppp.htm

based on product mean prices from similar datasets[6]. In this way, we obtained for 17 of the 55 datasets an estimated product price. In our analysis, we use the log-transformed average price.

Rate of market clearing indicates how large the proportion of sold products contained in a dataset was. This variable is a proxy for the popularity of the product. However, for 144 out of 202 datasets this information is not reported. To avoid too many missing values, this variable is not included in our analysis.

Log-transformed SR is a dichotomous variable that indicates whether the variable of seller reputation was log-transformed or not.

Percentage SR is a dichotomous variable that indicates whether seller reputation is measured in percentages (e.g., percent of positive ratings) or in absolute terms. This variable only applies to positive and negative ratings as measures of seller reputation but not to reputation scores, which is the number of positive ratings minus the number of negative ratings.

Log-transformed SP is a dichotomous variable that indicates whether the variable of selling performance was log-transformed or not.

Product homogeneity is a dichotomous variable indicating whether the products contained in a dataset are homogeneous or heterogeneous.

Multilevel is a dichotomous variable that indicates whether data analysis includes and accounts for clustered observations by using multilevel techniques.

N is the number of observations in each model that is included. In our analysis, we use the log-transformed number of observations.

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6 The estimation is based on similar datasets with reported product mean prices. For instance, datasets with similar types of products (e.g., SD card and U disk), year of data collection (within 5 years), and same usage condition (i.e. new, used or unknown). If no similar dataset is found, it is estimated with an average value of price within the same product category (e.g., mobile phones).

Parameters is the number of parameters that was included in each model. In our analysis, we use the log-transformed number of parameters.

Publication status is collected at the study level and indicates whether the paper reporting the results has been published. The publication types include International/ English journals, local journals and other, i.e. book chapters, conference/workshop presentations, dissertation/thesis, working paper, and unpublished. To simplify the variable, the publication status is treated as a nominal variable during data analysis. That is, papers are treated as “published in English journals”, “published in local journals” and “other (unpublished)”.

Transaction type reports whether the collected dataset contains auctions or fixed-price transactions. It is categorized as “unknown” if this information is not reported in the paper or the dataset contains mixed types of transactions.

3.3.2 Reproduction of previous findings with a meta-regression model

Jiao et al. (2021) ran 12 separate meta-analyses, one for each combination of seller reputation (reputation score, number of positive ratings, number of negative ratings) and selling performance (final price, price ratio, selling probability, selling volume), using 378 effect sizes reported in 107 empirical studies. Their dataset has been updated in the meantime and now comprises 406 effect sizes reported in 125 studies. Here we reproduce their results by fitting a saturated, random-effects meta-regression model with the updated sample of 406 reputational effect sizes. In this model, the type of seller reputation and the type of selling performance are the only explanatory variables and are fully interacted with each other. The results are presented in Table 3.2 and correspond to the findings reported by Jiao et al. (2021).

In general, seller reputation has a small but significant effect on selling performance. The overall effects of reputation score and positive ratings are positive, and the overall effects of negative ratings are negative. Moreover, in absolute terms, the overall effects of positive ratings are larger than both the overall effects of reputation score and negative ratings. Three out of twelve overall effect sizes are statistically insignificant: reputation score (p = 0.19) and negative ratings (p = 0.10) on price ratio and negative ratings on selling volume (p = 0.36).

The significant Q-statistic for moderators ($QM_{(df = 12)} = 160.75$, $p < 0.001$) suggest that the inclusion of all interactions of type of seller reputation with type of selling performance, which corresponds to the twelve sub-group meta-analyses conducted by Jiao et al. (2021), explains a substantial proportion of the variation in reputational effect sizes. However, the significant Q-statistic for residual heterogeneity ($QE_{(df = 394)} = 17917.34$, $p < 0.001$) suggests that further exploration with potential moderators is likely to be worthwhile because the amount of residual heterogeneity is still considerably high (Borenstein et al., 2009; Hak et al., 2018). Next, we explain in more detail our data analysis strategy.

Table 3.2. Reproduction of Table 4 in Jiao et al. (2021) with an updated dataset (n = 406) and a saturated, random effects meta-regression model

Relation	Coef.	*p*-value	95% CI	*N*	*Z**
Final Price × Reputation Score	0.04*	0.02	[0.01, 0.08]	75	-0.65
Final Price × Positive Ratings	0.11***	< 0.001	[0.07, 0.15]	56	2.58**
Final Price × Negative Ratings	-0.09***	< 0.001	[-0.14, -0.05]	47	-2.55*
Price Ratio × Reputation Score	0.07	0.19	[-0.04, 0.18]	17	0.43
Price Ratio × Positive Ratings	0.27***	< 0.001	[0.18, 0.35]	35	5.10***
Price Ratio × Negative Ratings	-0.09	0.10	[-0.19, 0.02]	26	2.51**
Selling Probability × Reputation Score	0.04**	0.003	[0.01, 0.07]	26	-2.25**
Selling Probability × Positive Ratings	0.07***	< 0.001	[0.04, 0.10]	19	0.05
Selling Probability × Negative Ratings	-0.05**	0.004	[-0.09, -0.02]	16	-1.73
Selling Volume × Reputation Score	0.10**	0.002	[0.04, 0.16]	39	-2.19**
Selling Volume × Positive Ratings	0.14***	< 0.001	[0.08, 0.21]	33	4.66***
Selling Volume × Negative Ratings	-0.05	0.36	[-0.14, 0.05]	17	-3.01**

Notes: Test of moderators (coefficients 1:12): QM(df = 12) = 160.75, p < 0.001. Test for residual heterogeneity: QE(df = 394) = 17917.34, p < 0.001.
** Egger's regression test for funnel plot asymmetry, as the assessment of publication bias. All the funnel plots are reported in Appendix B.3.*

Although the total number of 406 reputational effect sizes used in the meta-regression reported in Table 3.2 appears to be large, the number of cases will be quickly diminished in sub-group analyses or meta-regressions that include additional moderators. In order to test the influence of the potential moderators listed earlier, we will focus on two homogeneous subsets of the dataset. To create these subsets, we first pool cases that use final price or price ratio as outcome variables because they are both measures of product prices. While the final price is the absolute selling price, the price ratio is the relative selling price compared to a reference value such as a book value provided by a third party or the average price of similar products in the dataset. Moreover, we pool cases that use selling probability or selling volume as outcome variable because they both capture the number of sales; a higher selling probability would imply a higher selling volume within a certain time period. We also pool cases that use reputation score (number of positive ratings minus number of negative ratings) or number of positive ratings to operationalize seller reputations because the two variables are highly correlated and both exhibit positive effects on selling performance (Table 3.2). Next, we create two subsets of the original dataset for further analyses. Subset 1 comprises the 183 cases that use the final price or price ratio to operationalize selling performance and reputation score or number of positive ratings to operationalize seller reputation. Subset 2 comprises the 117 cases that use selling probability or selling volume to operationalize selling performance and reputation score or number of positive ratings to operationalize seller reputation. In our analyses, we include dummy variables to control for the type of selling performance and seller reputation operationalizations used within each subset (e.g., DV is final price vs price ratio). Unfortunately, due to the small number of cases, we are unable to use the subset of cases that used negative ratings to operationalize seller reputation in our analyses.

3.3.3 Model selection and multi-model inference

To investigate how potential moderators affect reputation effects, we take an information- theoretic approach and apply model selection analysis and multi-model inference to our rich meta-analytic dataset. Model selection analysis examines several competing models simultaneously to identify the best set of models via information criteria such as the Akaike information criterion (AIC) (Burnham & Anderson, 2002) and model weights (aka Akaike weights) that indicate the probability that a model is a best-fitting model. In this way, it is possible to uncover (in statistical terms) the model that explains the dataset best given different combinations of moderator variables. Furthermore, we perform multi-model inference to better evaluate the importance of each included moderator. We report the relative importance value of each moderator, which is the sum of Akaike weights of all models that include the moderator. Hence, a moderator that is included in more models with larger weights will receive a higher importance value. The advantage of multi-model inference is that it reduces the risk of selecting one of the less probable models by chance, because the relative importance of all moderators is listed (Cooper et al., 2019). With this approach, we can learn which moderators play an important role in explaining the variance in reputational effect sizes.

The combination of model selection and multi-model inference with meta-analyses has been used in linguistics (Matsuki et al., 2016), biology (Samia et al., 2019), biogeoscience (Lavoie et al., 2019), psychiatry (Holper, 2020) and ecology (Cheng et al., 2019). This approach is still rarely used in the social sciences. Young and Holsteen (2017) introduced multi-model analysis as a methodological application in sociology to examine the choice of controls and check the robustness of empirical results with regard to model specification. Here we apply the combination of these methods to address a sociologically relevant question: What affects the size of the seller reputation effect in peer-to-peer online markets? Our analyses are conducted in R using the "MuMIn" (Barton, 2020) and "metafor" (Viechtbauer, 2010) packages.

We start our analyses by applying model selection to Subset 1 and Subset 2. We examine the fit and plausibility of random effects, meta-regression models with all possible combinations of moderators and select three best-fitting models with Subset 1 (Table 3.4) and one best-fitting model with Subset 2 (Table 3.5) for interpretation. To better grasp the importance of the various moderators, we apply multi-model inference and report the relative importance of each moderator in the last column of the two regression tables. Because Egger's test (see Table 3.4 in Jiao et al., 2021) suggests that there might be publication bias in our set of studies, we do a robustness check for publication bias by adding SE to the best-fitting meta-regression models (Auspurg et al., 2019; Peters, 2006). We do not find any significant effects of SE (or SE^2) suggesting limited evidence for publication bias. Detailed results are reported in Appendix B.4.

3.4 Results

Table 3.3 presents descriptive information based on the entire sample of all moderators to be considered in our analyses. *Average price* is the only variable with missing values.

Table 3.3. Descriptive information of moderators (n = 406)

Variable	Type	Mean	S.D.
Contextual moderators			
Region	Nominal		
United States		0.54	0.50
China		0.32	0.46
Other		0.14	0.35
Platform	Nominal		
eBay		0.58	0.49
Taobao		0.28	0.44
Other		0.14	0.29
Year of data	Continuous	2006	5.03
Product-related moderators			
Condition	Nominal		
New		0.52	0.50
Used		0.11	0.31
Unknown		0.37	0.48
Avg. price [a]*	Continuous	589.30	2094.36
Method-related moderators			
Log-transformed SR	Dichotomous	0.56	0.50
Percentage SR	Dichotomous	0.16	0.37
Log-transformed SP	Dichotomous	0.33	0.47
Product homo.	Dichotomous	0.89	0.31
Multilevel	Dichotomous	0.15	0.36
N*	Continuous	47438.74	245755.9
Parameter*	Continuous	23.33	109.87
Controls			
Published	Nominal		
English journal		0.67	0.47

Table 3.3. Descriptive information of moderators (n = 406) (continued)

Variable	Type	Mean	S.D.
Local journal		0.16	0.36
Other		0.17	0.38
Transaction type	Nominal		
Auction		0.64	0.48
Fixed price		0.32	0.46
Unknown		0.04	0.20

[a] 70 missing values on the average item price.
** Variables are log-transformed in the model selection analysis.*

3

3.4.1 Results on Subset 1

The results of the model selection analysis for Subset 1 are presented in Table 3.4. The best-fitting models are ranked by Akaike information criterion corrected for small sample size (AICc) (Akaike, 1973; Berggren & Jordahl, 2006). AICc is calculated by

$$AICc = -2\log(L) + 2k + \frac{2k(k+1)}{n-k-1} \tag{1}$$

with the number of model parameters (k), the maximum likelihood estimate for the model (L) and the sample size (n). The model with the lowest AICc is considered the best fitting model. Table 3.4 presents the best fitting model (M1) as well as the two models (M2 and M3) that are less than 1.5 AICc units away from M1. The weight information listed at the bottom of each column is the Akaike weight of each model. The sum of Akaike weights of all possible models is 1 (Burnham & Anderson, 2002; Symonds & Moussalli, 2011). The Akaike weight of M1 indicates that there is a 3.3% chance that M1 is the best model for explaining the data, and that is the highest chance among all fitted models.

Model M1 includes usage conditions, a product-related moderator. The detected reputation effect is significantly smaller for used products than new products (coef. = −0.17, $p < 0.001$), which is contrary to our expectation. Because of the higher uncertainty regarding the condition of used products, we expected the reputation effect to be larger for used products than for new products.

M1 also includes method-related moderators of the reputation effect. If the model accounts for clustered data, the reputation effects are significantly larger (coef. = 0.14, p = 0.002). The number of observations (coef. = −0.03, p < 0.001) shows a significantly negative effect, which indicates that models with larger samples exhibit smaller reputation effects.

As for control variables, we find a significant distinction between the two types of dependent and independent variables used in models included in our meta-regressions. The effect of seller reputation on final price is smaller than on price ratios (coef. = −0.10, p = 0.002). Besides, reputation effects appear to be smaller when the independent variable is reputation score instead of positive ratings (coef. = −0.05, p = 0.05). These results are in line with the results of the saturate meta-regression model reported in Table 3.2.

Compared to M1, the other two best-fitting models, i.e. M2 and M3, are similar except for the inclusion and/or exclusion of a few variables, namely the market platform (included in M2), seller reputation operationalized in terms of the percentage of positive ratings (included in M3), and IV being reputation scores (excluded in M3). The regression coefficients of the variables included in all three models do not show any substantial differences across the three models.

To better evaluate the importance of each moderator, we examine their relative importance values with the method of multi-model inference. As is shown in Table 3.4, the most important moderators are product usage condition (0.99), the log-transformed number of observations (0.98), DV being final price (0.97) and model being multi-level model (0.95). The relative importance scores of these variables suggest that they are included in almost all models with high weights. Less important moderators are the IV being the reputation score (0.56), whether seller reputation is a percentage (0.41), market platform (0.39), the log-transformed number of parameters (0.39) and whether seller reputation is log-transformed (0.32). This relative importance ranking is consistent with our model selection analysis in that the best-fitting model, M1, includes the five moderators with the highest importance values.

Table 3.4. Best-fitting models and relative importance of moderators for Subset 1 focusing on effects of measures of positive reputation on selling price (n = 183)

	M1		M2		M3		Relative importance
	Coef.	**SE**	**Coef.**	**SE**	**Coef.**	**SE**	
Const.	0.39***	0.05	0.39***	0.05	0.39***	0.05	
Contextual moderators							
Region	excl.		excl.		excl.		0.13
Platform	excl.				excl.		0.39
eBay			(ref.)				
Taobao			-0.04	0.04			
Other			0.05	0.04			
Year of data	excl.		excl.		excl.		0.26
Product-related moderators							
Condition							
new	(ref.)		(ref.)		(ref.)		0.99
used	-0.17***	0.04	-0.17***	0.04	-0.19***	0.04	
unknown	-0.02	0.03	-0.02	0.03	-0.03	0.03	
Avg. price	excl.		excl.		excl.		< 0.01
Method-related moderators							
Log-transformed SR	excl.		excl.		excl.		0.32
Percentage SR	excl.		excl.		0.06	0.04	0.41
Log-transformed SP	excl.		excl.		excl.		0.25
Product homo.	excl.		excl.		excl.		0.27
Multilevel	0.14**	0.04	0.12**	0.04	0.15***	0.04	0.95
Log (*N*)	-0.03***	0.01	-0.03***	0.01	-0.03***	0.01	0.98
Log (Parameter)	excl.		excl.		excl.		0.39
Controls							
Published	excl.		excl.		excl.		0.25
Transaction type	excl.		excl.		excl.		0.12
DV final price	-0.10**	0.03	-0.08**	0.03	-0.10***	0.03	0.97
IV reputation score	-0.05	0.03	-0.06*	0.03	excl.		0.56
n	183		183		183		
logLik	39.72		41.35		39.09		
AICc	-62.6		-61.4		-61.3		
weight	0.033		0.018		0.018		

3.4.2 Results on Subset 2

When combining selling probability and selling volume as the dependent variable in Subset 2, the best-fitting is model M4 in Table 3.5. Model M4 includes region, seller reputation being a percentage rather than an absolute value and the IV being the reputation score rather than the number of positive ratings. As for the region, compared to the USA, seller reputation effects are significantly higher in China (coef. = 0.11, $p < 0.001$). That is, for online transactions in China, the seller reputation scores and positive ratings have larger positive effects on selling probability and selling volume than in the USA. In Europe, the reputation effect is smaller than in the USA but statistically insignificant (coef. = −0.07, $p = 0.15$). Moreover, as expected, the reputation effect is smaller if seller reputation is measured in percentage than in absolute terms (coef. = −0.14, $p < 0.001$). That is, among the studies using positive ratings as the independent variable, the effect of percentage positive ratings (e.g., seller with 98% of positive ratings among all ratings) is smaller than the effect of the absolute number of positive ratings (e.g., seller receiving 50 positive ratings).

With regard to the relative importance (see last column in Table 3.5), the most important moderators are region (0.96), the IV being the reputation score rather than the number of positive ratings (0.94), and seller reputation being a percentage of positive ratings rather than the absolute number of positive ratings (0.92). And these are also the moderators included in the best-fitting model M4.

Table 3.5. Best-fitting model and relative importance of moderators for Subset 2 focusing on the effect of measures of positive reputation on sales (n = 117)

	M4		**Relative importance**
	Coef.	**SE**	
Const.	0.13***	0.04	
Contextual moderators			
Region			0.96
USA	(ref.)		
China	0.11***	0.03	
Europe	-0.07	0.05	
Platform	excl.		0.11
Year of data	excl.		0.25

Table 3.5. Best-fitting model and relative importance of moderators for Subset 2 focusing on the effect of measures of positive reputation on sales (n = 117) (continued)

	M4 Coef.	SE	Relative importance
Product-related moderators			
Condition	excl.		0.09
Avg. price	excl.		<0.01
Method-related moderators			
Log-transformed SR	excl.		0.31
Percentage SR	-0.14***	0.05	0.92
Product homo.	excl.		0.26
Multilevel	excl.		0.25
Log (*N*)	excl.		0.25
Log (Parameter)	excl.		0.29
Controls			
Published	excl.		0.27
Transaction type	excl.		0.12
DV selling probability	excl.		0.25
IV reputation score	-0.11**	0.03	0.94
n	117		
logLik	55.59		
AICc	-98.4		
weight	0.059		

Notes: Model M4 is the best-fitting model. The second-best-fitting model is more than 1.5 AICc points away from M4.

3.5 Discussion

This study builds on and extends a previous meta-analysis that synthesizes evidence from over one hundred empirical studies of the reputation effect in peer-to-peer online markets (Jiao et al., 2021). The aim of this paper is to explain excess variation in seller reputation effects. Combining model selection analyses and multi-model inference in a meta-analytic context, we find that the excess variation of the observed reputation effects can partly be explained by contextual moderators, product-related moderators and method-related moderators.

In terms of contextual moderators, our results show that the region factor is an important moderator of the effect of measures of positive reputation (i.e. the number of positive ratings and the reputation score) on sales (i.e. Subset 2). Measures of positive reputation have a larger positive effect on the probability of sale and selling volume in studies that use data collected in China rather than the USA and Europe. However, there is no sufficient evidence for a moderating effect of region on the effect of measures of positive reputation on selling price (i.e. Subset 1). One plausible explanation for this observation is that reputable sellers in China or Chinese online platforms (e.g., Taobao) achieve better selling performance through more sales instead of higher prices, suggesting different reputation mechanisms for different types of selling performance. Other contextual moderators are not significant.

For product-related moderators, we only find that used products exhibit a significantly negative moderating effect on the effect of measures of positive reputation on selling price as compared to new products. This is contrary to the expectation that a good reputation is more important for transactions with used products because the uncertainty regarding the condition of these products is higher. One explanation for this finding might be that used products are often relatively unique. For example, for collectors' items there are often very few sellers. This might increase competition between buyers as well as make transactions between specific buyers and sellers more recurrent. Both these mechanisms would lead to reduce the importance of seller reputation. Another, partly related explanation could be that buyers of used products are more risk taking and less attentive to information about seller reputation than buyers of new products. More generally, the moderating effect of product condition might indicate the presence of interaction effects between product, seller and buyer characteristics that could be investigated in future research.

Concerning method-related moderators, we do not find consistent moderators across the two subsets. In Subset 1, multilevel models (i.e. models accounting for clustered observations) produce significantly larger reputation effects whereas studies with larger numbers of observations produce significantly smaller reputation effects. However, we have to be careful to interpret the relation between effect sizes and methodological choices as indications for better (or worse) methodological approaches. Rather, the use of multilevel methodology might be an indication of more sophisticated data collections and better controls for correlated groups. This conjecture could be tested through a re-analyses of studies that used multilevel techniques (Auspurg & Brüderl, 2021; Breznau et al., 2021; Silberzahn et al., 2018). The larger N effect is surprising and can certainly not be seen as a reason for smaller N studies, but is also not completely unknown in the literature (Sterne et al., 2000). Publication bias favoring studies with significant effects could be a reason for this finding given that smaller N studies are less likely to detect significant effects if effects are smaller. However, also other reasons such as study quality might be behind this finding given that study quality is also related to the size of a study (Sterne et al., 2000).

The smaller effect found on selling volume if the seller's reputation is measured as a percentage of positive ratings rather than in absolute terms was expected. This is an indication that the number of positive ratings is a better indicator for the actual reputation of the seller than the percentage of positive ratings.

Applying model selection and multi-model inference analyses in the meta-analytic context in the current study allows us to present the best-fitting models among the thousands of possible moderator combinations, and evaluate which are the more important moderators in general. However, there are also limitations to this approach. We can only investigate moderators for which enough information is provided in the studies that have been conducted. Therefore, we should also not too strongly interpret the absence of moderation effects of some moderators.

4

CHAPTER

Building a reputation for trustworthiness:

Experimental evidence on the role of the feedback rate

Ruohuang Jiao, Wojtek Przepiorka and Vincent Buskens

This chapter is based on a paper written by Ruohuang Jiao, Wojtek Przepiorka and Vincent Buskens. The paper has been submitted to an international peer-reviewed journal. Jiao wrote the manuscript in collaboration with Przepiorka and Buskens. All authors contributed to the experiment design. Jiao developed and executed both experiments and did the analyses. The faculty of Social and Behavioural Science of Utrecht University funded both experiments

Abstract:

In 25 years, research on reputation-based online markets has produced robust evidence on the existence of the so-called reputation effect, i.e. the positive relation between online traders' reputations and these traders' market success in terms of sales and prices. However, there is an ongoing debate on what the size of the reputation effect means. Here we argue that the rate of truthful feedback that traders leave after completed transactions is negatively related to the size of the reputation effect. The higher the rate of truthful feedback, the quicker will untrustworthy traders be screened and disincentivized to enter the market. With mostly trustworthy traders entering the market, buyers will demand smaller price discounts from market entrants without a good reputation. To test this mechanism experimentally, we systematically vary the probability with which information about trustees' behavior in a trust game is recorded and shown to future interaction partners of these trustees. We find that trustees give discounts to trusters to build or repair their reputation and that trustees that give discounts or have a good reputation are trusted more. However, we do not find support for our hypothesis that a higher feedback rate significantly decreases trustees' propensity to give discounts. We argue and show in an exploratory analysis that this is likely due to the high level of unconditional trust trusters exhibit towards trustees without a reputation.

Keywords: trust; trustworthiness; reputation; online market; reputation effect; feedback rate

4.1 Introduction

The expansion of peer-to-peer (P2P) online markets has spurred the development of reputation systems. Reputation systems help to reduce trust problems arising from information asymmetries between traders and the sequential nature of market exchanges. In a typical exchange, the buyer advances the money and the seller provides the product or service in return. Next, these traders can share their experiences in the form of numeric and textual feedback via the reputation system of the market platform.[1] In this way, reputation systems collect, aggregate, and distribute feedback information about traders, products, and services (Resnick et al., 2000). Anyone can access the shared reputation information about traders' past exchange experiences in a particular market, even without having participated in an exchange before (Frey, 2017). Reputation systems are a particularly valuable source of information for traders who decide which trading partners to engage with and trust. As a result, a good reputation is essential for traders' success, and there have been over one hundred empirical studies on the relation between online traders' reputations and these traders' market performance – the so-called reputation effect (Jiao et al., 2021). Traders, especially sellers, need to manage their reputation, if they want to improve their selling performance. However, building a good reputation can be challenging even for trustworthy and reliable sellers because not all transaction partners would leave feedback. How the frequency of feedback in a market impacts traders' behavior and the size of the reputation effect is a hitherto unanswered question.

Although the reputation effect has been estimated in over one hundred studies that analyze online market data obtained from different platforms and on different product categories, there is still no consensus as to how its size should be interpreted (Jiao et al., 2022; Snijders & Matzat, 2019). The discussion is fueled by some studies finding a large reputation effect, but many studies

1 We distinguish between what we call 'open' reputation systems and 'closed' reputation systems. In open reputation systems such as Google reviews, TripAdvisor, or Yelp, people can leave unsolicited, anonymous feedback on a business, product, or service at any time. Closed reputation systems are part of a particular market platform (e.g., eBay, Airbnb, Taobao) and feedback can only be left by the people that took part in a particular transaction. The considerations we offer are made with close reputation systems in mind.

finding a seemingly small effect. The common conjecture is that a large reputation effect evidences the monetary value of reputation – the primary incentive for sellers to behave cooperatively. By implication, it is conjectured that a small reputation effect evidences that other than reputational incentives promote seller cooperation. However, observing a small reputation effect does not necessarily mean that a market's reputation system is ineffective or even malfunctioning (Tadelis, 2016). It could also suggest that the reputation system is effective to such an extent that dishonest sellers rarely attempt to enter the market because they would readily obtain negative feedback, a bad reputation, and remain unsuccessful in the market (Diekmann et al., 2014).

The size of the reputation effect can be interpreted as an indicator of the information costs that accrue on the side of the sellers due to buyers' uncertainty about sellers' trustworthiness (and other qualities related to sellers, their products, and services) (Akerlof, 1970; Shapiro, 1983). The frequency of (truthful) feedback affects these information costs as follows: Since it takes longer to screen untrustworthy sellers if the rate of truthful feedback is low, untrustworthy sellers will have a greater incentive to enter the market, and the likelihood that buyers will encounter untrustworthy sellers increases. As a result, buyers will expect higher price reductions from new sellers as interactions with them will bear higher risks. Hence, in reputation-based online markets, the rate of truthful feedback can be expected to have a negative effect on information costs (Przepiorka, 2013). This argument has a counterintuitive implication: The better a reputation system is at identifying untrustworthy sellers, the smaller will be the reputation effect (i.e. the lower the relationship between seller reputation and seller performance in terms of sales and prices).

The effectiveness of reputation systems to screen untrustworthy traders hinges on traders leaving truthful feedback after completed transactions. This makes reputation systems a collective good, the production of which depends on voluntary feedback provision and thus is threatened by free-riding behavior (Bolten et al., 2004; Chen et al., 2021; Lafky, 2014). Yet, in practice, traders leave feedback for a variety of reasons (Chen et al. 2017; Macanovic & Przepiorka, 2023). However, only a few studies that investigate reputation effects with online market data also report market-level feedback rates. The available evidence stems mainly from eBay and ranges between 50 and 70 percent of rated market transactions (Bolton et al., 2013; Diekmann et al., 2014). This makes it difficult to research the relation between market-level feedback rates, trader behavior, and the size of the reputation effect with observational data. Despite the scientific and practical relevance of the role of the feedback rate in reputation building, surprisingly few studies have been conducted on this topic. One experimental study conducted by Du et al. (2013) finds that buyers are more trusting and sellers are more trustworthy in a market where more feedback of any kind (fair, unfair, or a mixture of both) is provided than if no feedback is provided at all.

Here we report the results from two lab experiments designed to further examine the impact of the rate of leaving feedback on trader behavior and the size of the reputation effect. Both our experiments emulate the exchange process between buyers and sellers in an online market by means of the trust game with incomplete information. Participants are either in the role of a buyer or a seller. Over an indeterminate number of rounds, buyers decide in each round whether to trust the sellers they are randomly matched with. In our experiment, feedback is provided automatically and truthfully, and we vary the feedback rate systematically in three experimental conditions. With our design, we investigate (1) whether sellers give discounts to build a good reputation, (2) whether buyers' trust increases when sellers offer discounts or have a good reputation, (3) whether sellers are more frequently trustworthy when they are building their reputation or have a good reputation to lose. Most importantly, (4) we test the prediction that a lower feedback rate causes sellers to invest more in building their reputation by giving price discounts.

The remainder of the paper is structured as follows: In the next section, we argue why and how sellers might develop a strategy to build their reputation by offering discounts under different experimental conditions, as well as how buyers' behavior might change in response to sellers' decisions. We then present the experimental design and data collection for the first experiment and report the outcomes of hypotheses tests and exploratory analyses. The results of the first experiment have informed a revised design implemented in the second experiment. We present the design and data collection of the second experiment next, along with the results of the hypotheses tests and exploratory analyses. Finally, we discuss how our findings correspond to our theoretical considerations, draw conclusions, and outline future research directions.

4.2 Theory and hypotheses

In reputation-based online markets, sellers' reputations play an important role in earning buyers' trust. Sellers' good reputations indicate that these sellers are more likely to be trustworthy, and sellers with a bad reputation would be perceived as dishonest and buyers would be less inclined to take the risk and trust them. Initially, sellers do not have a reputation. Therefore, obtaining a good reputation is an important stepping stone to sellers' success in a market. However, sellers are not always rated (truthfully) after a transaction. The less frequently sellers are rated truthfully, the longer it will take to screen untrustworthy sellers and for trustworthy sellers to build their reputation. Consequently, more untrustworthy sellers will have an incentive to enter the market, which in turn will oblige trustworthy sellers to offer larger discounts for buyers to trust them. In other words, the lower the rate of truthful feedback, the higher will be the initial investment trustworthy sellers have to make to build their reputation. This can be demonstrated with the following model.

We assume a reputation-based market in which buyers and sellers interact in a sequential move game that includes up to three stages (Figure 4.1).

First, the seller decides whether to offer a discount to the potential buyer. By not offering a discount, the seller chooses to interact with the buyer in a trust game (TG), with the payoffs being $T = 80$, $R_B = R_S = 60$, $P = 40$, and $S = 20$ (see right subgame in Figure 4.1). By offering a discount, the seller chooses to interact with the buyer in an assurance game (AG) with payoffs of $T = 40$, $R_B = 80$, $R_S = 40$, $P = 40$, and $S = 40$ (see left subgame in Figure 4.1). Note that in the AG, $T = R_S = 40$. This implies that if buyers decide to buy (trust), sellers who need to build (or maintain) their good reputation (see the third stage) have an incentive to ship (i.e. be trustworthy).

Second, after having learned the seller's decision and reputation for trustworthiness (if already available), the buyer decides whether to trust (i.e. choose whether to buy and send money to the seller). If the seller offered a discount (i.e. in the AG), the buyer cannot lose from buying, and gains 40 ($R_B - P$) if the seller ships. If the seller did not offer a discount (i.e. in the TG), the buyer can gain 20 ($R_B - P$) or lose 20 ($P - S$) from buying if the seller ships or does not ship, respectively; but in this case, the buyer may anticipate that the seller will not ship (since $T > R_S$) and choose not to buy (since $P > S$). Irrespective of whether the seller offered a discount, if the buyer does not choose to buy, the interaction between the buyer and the seller ends, and both receive a payoff of $P = 40$. The interaction continues into the third stage of the game only if the buyer decides to buy.

Third, the seller decides whether to ship the merchandise the buyer paid for. If the seller did not offer a discount, they gain 20 ($R_S - P$) from shipping and 40 ($T - P$) from not shipping. After having offered a discount, the seller does not gain or lose anything from shipping or not shipping ($R_S - P = T - P = 0$), so the seller will be indifferent between shipping and not shipping. However, a seller's propensity to ship will increase once the seller can build a reputation for being trustworthy.

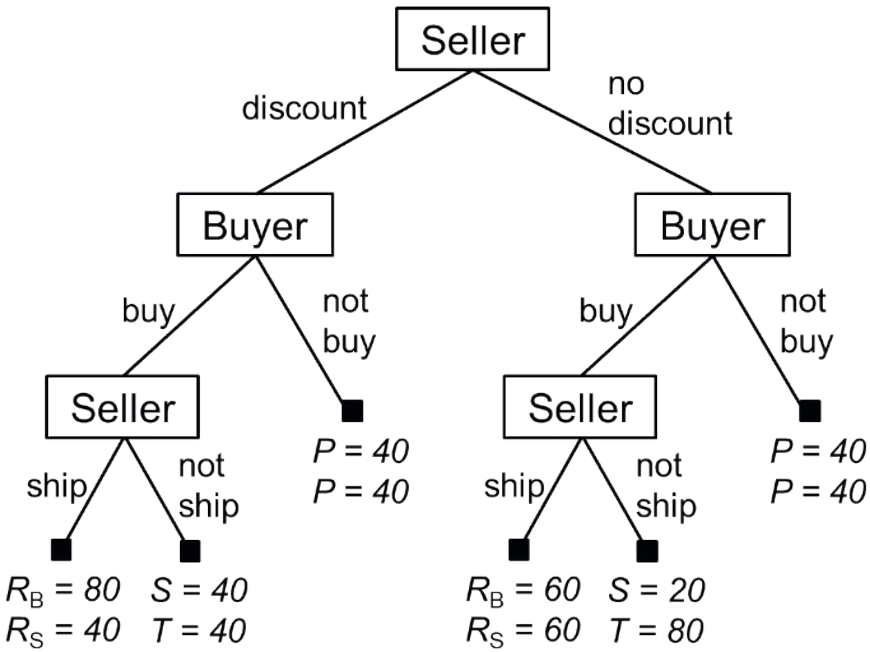

Figure 4.1 The choice set and payoffs for the buyer and the seller.

A seller's reputation is updated with probability π after an interaction with a buyer who decides to buy. With probability $(1 - \pi)$, a seller's reputation is not updated. The seller's reputation is updated to be 'good' if the seller chooses to ship and is updated to be 'bad' if the seller chooses not to ship. In this model, π is an exogenous parameter and does not depend on, for example, the outcome of the interaction between a buyer and a seller.

We further assume that buyers expect the proportion of sellers who would ship in the TG to be below the threshold above which it would be worthwhile for these buyers to buy without additional information about sellers. However, because buyers can only gain from choosing to buy in the AG, buyers can be expected to always buy if the seller offers a discount. Finally, buyers expect the proportion of sellers with a good reputation who would ship to be above the critical threshold and therefore will always buy from these sellers, and they will never buy from sellers with a bad reputation. Table 4.1 shows the conditions under which a buyer is expected to buy or not to buy in an interaction with a seller.

Table 4.1 Assumptions of buyers' decision to buy with sellers under different conditions.

	Seller reputation		
Seller gives discount	**bad**	**none**	**good**
yes	buy	buy	buy
no	not buy	not buy	buy

In anticipation of buyers' behavior, sellers without a reputation will invest in building a good reputation by offering discounts. Sellers will invest in building a reputation by offering discounts because not investing and not being trusted leads to a strictly lower payoff as long as the number of expected interactions is $k > 1$ and the probability of receiving truthful feedback after an interaction is $\pi > 0$. For example, if $k = 2$, it holds that $2P < P + \pi R + (1 - \pi)P$.

Based on these assumptions, we formulate hypotheses on how buyers' preferences to buy vary across sellers' decisions on discounts and sellers' reputations:

H1: Sellers without a reputation are more likely to give discounts.
H2a: Buyers are more likely to buy from sellers who give discounts.
H2b: Without discounts, buyers are more likely to buy from sellers who have a good reputation.
H3: Sellers who do not give a discount and do not have a good reputation are less likely to ship, while sellers who give discounts or have a good reputation are similarly likely to ship.

Finally, because it takes longer for sellers to build their reputation as the probability of receiving truthful feedback (π) decreases, sellers need to offer more discounts if π is lower. This constitutes our fourth hypothesis:

H4: For sellers without a reputation, the lower the probability that a seller's reputation will be updated after interacting with a buyer choosing to buy, the more likely the seller will offer a discount.

Next, we describe the experiments we designed and conducted to test these hypotheses empirically.

4.3 Experiment 1

4.3.1 Experimental design and procedure

The experiment is designed based on the trust game described in the previous section. The feedback rate π is the probability that a seller's reputation is updated at the end of an interaction with a trusting buyer. To test how the feedback rate affects seller behavior, we employed three experimental conditions: $\pi = 0.2$, $\pi = 0.4$, and $\pi = 0.6$. Conditions were systematically varied across experimental sessions. The experiment was conducted in the Experimental Laboratory at University XY, consisting of 12 sessions, and with 152 participants. Participants were mostly female (69.6%) and their mean age was 23.96 years (SD = 6.22). Each participant took part in only one session, sessions lasted for about 75 minutes, and participants earned EUR 12.62 on average.

After reading the instructions (see section C.1 in Appendix C), every participant was randomly assigned to be a seller or buyer until the end of the session. There was an equal number of sellers and buyers in each session. Participants were informed in the instructions that the experiment consisted of 20 to 30 rounds; they were not told the exact number of rounds, which was 24. In every round, a seller was randomly matched with a buyer such that no seller-buyer pair interacted in two consecutive rounds.

In all experimental conditions, the experimental procedure and game payoffs are identical (see Figure 4.1). Each round begins with sellers choosing whether to give a discount to the matched buyer (i.e. choose AG or TG). The buyers then choose to buy or not to buy, possibly based on information about the seller's reputation. If the buyer chooses to buy, the seller decides whether or not to ship. Both participants receive the payoff that corresponds to their combined decisions. If the buyer decided to buy, the seller's shipping decision is recorded with probability π; with probability $1 - \pi$, no information on the seller's decision is recorded.

In our experiment, a seller's reputation is operationalized as the information about the decision the seller made ('ship' or 'not ship') in the last round in which the seller's decision was recorded. A seller's reputation does not contain information on the interaction situation (AG or TG), in which the seller's decision was recorded. Once a seller's decision is recorded in one round, buyers interacting with the seller in subsequent rounds are informed about the seller's reputation on their screens, when they decide whether to buy or not to buy. Before a seller's decision is recorded for the first time, buyers interacting with the seller are informed that no record of the seller's past behavior exists. A seller's reputation is updated after every round in which the seller's decision is recorded. The previous record of a seller's decision is thereby replaced by the new record. Buyers do not know in which round a seller's reputation information was recorded or updated.

In this experiment, we measure sellers' decisions to offer discounts, buyers' decisions to buy, and sellers' decisions to ship under the three experimental conditions. The payoffs of participants in each round are summed up and converted into monetary payments at the end of the session at a rate of EUR 1 per 120 payoff-points.

4.3.2 Variables

Condition is a categorical variable indicating which experimental condition the participants are in. There are 4 sessions (52 participants) under condition $\pi = 0.2$, 4 sessions (50 participants) under condition $\pi = 0.4$, and 4 sessions (50 participants) under condition $\pi = 0.6$. π denotes the probability of a seller's reputation being updated after an interaction with a trusting buyer. π is thus an exogenous variable manipulated in the experiment.

Discount is a binary variable, indicating whether the seller chose to offer a discount i.e. chose to play AG with the matched buyer for the current round. In 9% of all the rounds, sellers gave discounts, and in 91% they did not (Table 4.2).

Buy is a binary variable, indicating the buyer's decision to buy from the matched seller. Buyers chose to buy in 73% of all rounds.

Ship is a binary variable indicating a seller's second decision to ship or not if the buyer decided to buy. Sellers chose to ship in 81% of cases.

Seller's reputation is a categorical variable denoting the seller's last recorded decision as shown to the current matched buyer: bad (i.e. did not ship), none (i.e. not yet recorded), or good (i.e. shipped).

Table 4.2 Descriptive statistics of variables in Experiment 1

	N	Percentage
Dependent variables		
Seller's decision 1 (discount = 1)	1824	.090
Buyer's decision (buy = 1)	1824	.733
Seller's decision 2 (ship = 1)	1337	.806
Independent variables		
Seller's reputation	1824	
bad		.206
none		.233
good		.561
Experimental condition	1824	
π = 0.2		.34
π = 0.4		.33
π = 0.6		.33

4.3.3 Analyses and results

We use STATA (version 17.0) to calculate proportions from saturated logit models and test the statistical significance of the differences between the proportions of sellers' and buyers' decisions under various conditions to test our hypotheses. We also perform exploratory analyses using logistic regression models as all dependent variables (sellers' and buyers' decisions) are binary.

4.3.3.1 Sellers' decisions to give discounts

Each interaction starts with sellers choosing whether to offer discounts to attract buyers to buy. Figure 4.2 shows how sellers' decisions of giving discounts vary across experimental conditions and seller reputations.[2] More detailed analyses on the proportion of sellers giving discounts are provided in section C.2 in Appendix C. The linear comparison between overall proportions shows that sellers without a reputation are more likely to offer discounts than sellers with a good reputation (coef. = 0.039, p = 0.008). Therefore, hypothesis 1 is supported. However, compared to sellers without a reputation, sellers with a bad reputation are even more likely to give discounts (coef. = 0.272, p < 0.001). Sellers with a

2 To control for the effect of the period, we also ran the same analysis only for the first ten rounds. The results are quite similar to Figure C.2. (see section C.2 in Appendix C).

bad reputation do not give discounts more often, the higher the chance π is that their reputation is updated. The increase observed in Figure 4.2 is statistically insignificant ($\chi^2(2) = 1.96$, $p = 0.376$).

As for the experimental conditions, the joint test suggests that for sellers without a reputation, the proportion of giving discounts does not vary significantly across the three conditions ($\chi^2(2) = 2.5$, $p = 0.286$). Therefore, hypothesis 4 is not supported. We investigate additional predictors of sellers' decisions of giving discounts in the exploratory analysis section.

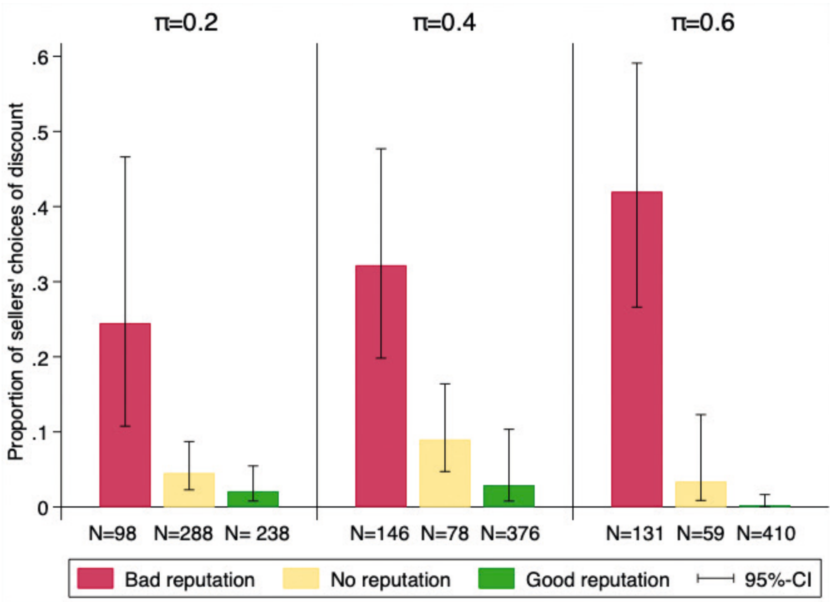

Figure 4.2 Proportion of sellers giving discounts across experimental conditions and seller reputations (Experiment 1)

4.3.3.2 Buyers' decisions to buy

After the sellers choose whether to give a discount, buyers need to choose whether to buy from the matched sellers. Figure 4.3 presents how the proportion of buyers choosing to buy varies across sellers' reputations and decisions to give a discount. More detailed analyses on the proportion of buyers deciding to buy are provided in section C.3 in Appendix C. Overall, buyers are more likely to buy when offered a discount (coef. = 0.140, $p = 0.003$). This finding supports hypothesis 2a. In line with our assumptions in Table 4.1, Figure 4.3 shows that buyers choose to buy at very high rates irrespective of sellers' reputations as long as they are offered a discount. Without a discount, buyers' decisions to buy differ across seller reputations. In this case, buyers are more likely to buy from sellers with a good reputation than from sellers with a bad reputation (coef. = 0.664, $p < 0.001$), and they are more likely to buy from sellers with a good reputation than from sellers without a reputation (coef. = 0.283, $p < 0.001$). Therefore, hypothesis 2b is also supported. A figure showing buyers' decisions to buy across sellers' reputations and experimental conditions is included in section C.3 in the Appendix C. Further analyses of buyers' decisions to buy are presented in Table 4.3 (model 3) and will be discussed below.

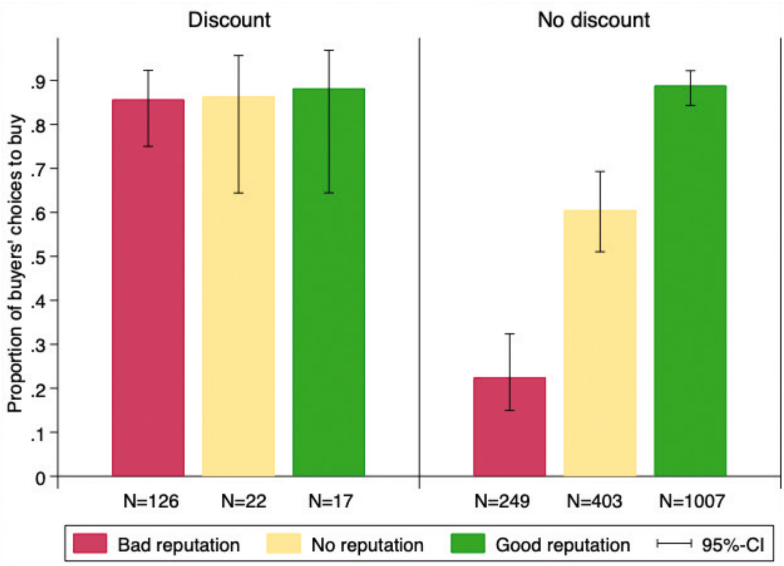

Figure 4.3 Proportion of buyers deciding to buy across seller reputations and sellers' decisions to give a discount (Experiment 1)

4.3.3.3 Sellers' decisions to ship

After a buyer chooses to buy, the matched seller gets the chance to choose whether to ship, knowing that, with a certain probability π, this decision will be recorded and presented to their interaction partner in the next round. The proportions of sellers choosing to ship across the different experimental conditions are provided in section C.4 in Appendix C. In Figure 4.4, we present how the proportion of shipping decisions varies across seller reputations and sellers' decisions to give a discount. In general, the proportion of shipping does not differ significantly depending on whether a discount was given (coef. = 0.043, $p = 0.527$). Furthermore, sellers with a bad reputation are significantly more likely to ship when they gave discounts than when they did not give discounts (coef. = 0.292, $p = 0.003$). However, there is no statistically significant difference in the proportions of shipping decisions between sellers who gave or did not give discounts, if these sellers have no reputation (coef. = -0.123, $p = 0.342$) or have a good reputation (coef. = 0.045, $p = 0.620$). Therefore, hypothesis 3 is only partly supported. An additional figure on sellers' decisions to ship across experimental conditions is provided in section C.4 in Appendix C, showing that the proportions of shipping are very similar across experimental conditions.

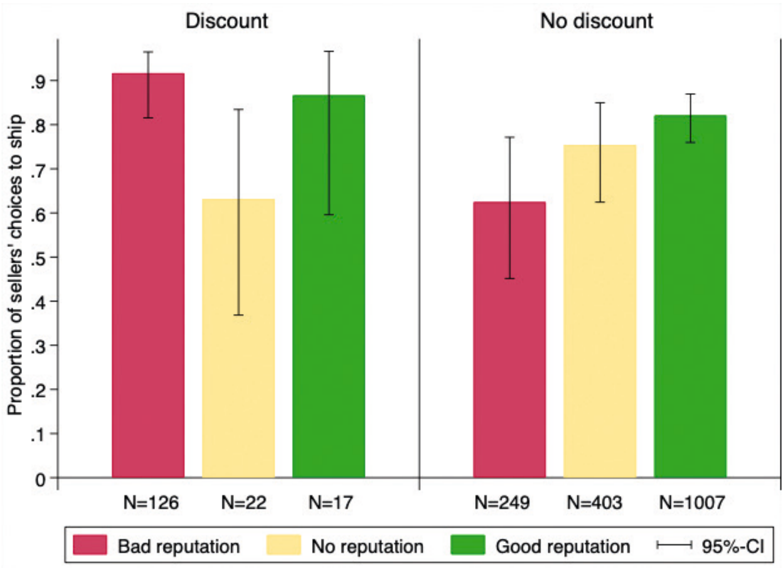

Figure 4.4 Proportion of sellers deciding to ship across seller reputations and sellers' decisions to give a discount (Experiment 1)

4.3.3.4 Exploratory analysis

In this subsection, we seek to investigate the potential predictors of sellers' decisions to give discounts, buyers' decisions to buy, and sellers' decisions to ship beyond what we hypothesized. Sellers with a good reputation rarely offer discounts (see Figure 4.2) and, in line with the expectations, there is no significant difference across conditions (coef. = -0.011, p = 0.630). Surprisingly, however, less than 10% of sellers without a reputation offer discounts. This is not in line with our expectations as these sellers should have a clear incentive to offer discounts to attract buyers and establish a reputation. How can we explain this finding?

We observe that there is a considerable proportion of buyers who choose to buy from sellers, even if these sellers lack a reputation and do not offer discounts (around 60%, see Figure 4.3). We, therefore, conjecture that sellers would not be sufficiently motivated to offer discounts if they are aware of or have already experienced such unconditional trust. And the same should hold for sellers with a bad reputation.

To test this conjecture, we run a set of logistic regression models with sellers' decisions to give a discount as the dependent variable and the following predictors: (1) experimental conditions; (2) seller reputation; (3) a binary variable ('prior trust') indicating whether a buyer chose to buy from the seller when the seller did not have a good reputation and did not offer a discount in the previous round. The latter is to test how sellers' prior experience with being trusted unconditionally influences their decisions to offer discounts. The outcome of our exploratory analysis is shown in models M1a and M1b in Table 4.3. Model M1a shows that, compared to sellers with a good reputation, sellers with a bad reputation (coef. = 3.420, $p < 0.001$) and sellers without a reputation (coef. = 1.358, $p = 0.001$) are more likely to give discounts. Moreover, the proportion of discount-offering sellers is the lowest when $\pi = 0.2$, but the coefficient estimates of experimental conditions are not significantly different from each other ($\chi^2(2) = 1.08$, $p = 0.582$). These results corroborate our findings thus far. In line with our new conjecture, M1b shows that sellers who experienced unconditional trust without having a good reputation and without offering a discount are less inclined to offer a discount later (coef. = -1.463, $p < 0.001$).

Table 4.3 Logistic regression models predicting sellers' and buyers' decisions (Experiment 1)

	Sellers' decisions of giving a discount		***Buyers' decisions to buy***		***Sellers' decisions to ship***	
	M1a	M1b	M2a	M2b	M3a	M3b
Exp. Condition						
$\pi = 0.2$	-0.414	-0.315	-0.041	-0.019	-0.517	-0.489
	(0.457)	(0.440)	(0.315)	(0.320)	(0.378)	(0.384)
$\pi = 0.4$	0.022	0.030	0.024	0.054	0.123	0.158
	(0.403)	(0.393)	(0.310)	(0.317)	(0.398)	(0.402)
$\pi = 0.6$	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)
Seller reputation						
bad	3.420^{***}	3.528^{***}	-3.207^{***}	-3.317^{***}	-0.551	-0.996^{**}
	(0.527)	(0.530)	(0.325)	(0.316)	(0.397)	(0.377)
none	1.358^{***}	1.647^{**}	-1.630^{***}	-1.632^{***}	-0.236	-0.147
	(0.524)	(0.523)	(0.228)	(0.230)	(0.286)	(0.306)
good	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)
Prior trust						
yes		-1.463^{***}				
		(0.365)				
no		(ref.)				

Table 4.3 Logistic regression models predicting sellers' and buyers' decisions (Experiment 1) (continued)

	Sellers' decisions of giving a discount		***Buyers' decisions to buy***		***Sellers' decisions to ship***	
	M1a	M1b	M2a	M2b	M3a	M3b
Seller's decision						
discount			2.703***	-0.078	0.897	0.378
			(0.469)	(0.736)	(0.555)	(0.813)
no discount			(ref.)	(ref.)	(ref.)	(ref.)
Interaction						
bad × discount				3.106***		1.481
				(0.756)		(1.068)
none × discount				1.486†		-0.982
				(0.765)		(1.044)
good × discount				(ref.)		(ref.)
Constant	-4.008***	-3.980***	2.061***	2.064***	1.610***	1.596***
	(0.440)	(0.436)	(0.290)	(0.294)	(0.289)	(0.290)
Observations	1824	1824	1824	1824	1337	1337
Pseudo R^2	0.259	0.280	0.219	0.224	0.023	0.032

Robust standard errors in parentheses (adjusted for clustering in same deciding participant).
*† $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$*

Next, we analyze buyers' decisions to buy using logistic regression models. Model M2a shows that compared to being matched with a seller with a good reputation, buyers are less likely to buy when being matched with a seller who has a bad reputation (coef. = -3.207, $p < 0.001$) or a seller without a reputation (coef. = -1.630, $p < 0.001$), and buyers are more likely to buy when the seller offers a discount (coef. = 2.703, $p < 0.001$). The proportion of buying is lowest when $\pi = 0.2$, but the effect of the experimental condition is not significant (χ^2 (2) = 0.04, $p = 0.978$). With M2b, we add the interaction between seller reputation and sellers' decision of giving a discount. Offering discounts increases buyers' likelihood to buy more when the seller has a bad reputation than when the seller has a good reputation (coef. = 3.106, $p < 0.001$), and the effect of a discount is marginally larger when the seller has no reputation than when the seller has a good reputation (coef. = 1.486, $p = 0.052$). Still, these interaction effects show that the effect of a discount decreases the better the reputation of the seller.

Lastly, we run logistic regression models to examine the effects of the experimental condition, seller reputation, and whether a discount was given on sellers' decisions to ship. M3a detects no significant effects, and we include the interaction between seller reputation and whether a discount was given in model M3b. According to M3b, neither of the effects of offering a discount or the interaction is significant, but we observe a significant difference between sellers with a good reputation and sellers with a bad reputation (coef. = -0.996, $p = 0.008$). This indicates that sellers with a bad reputation who did not give a discount are less likely to ship than sellers with a good reputation who did not give a discount. The non-significant interaction effects show that this difference does not remain if a discount is given.

4.4 Experiment 2

In Experiment 1, we observe that buyers' decisions to buy are not uncommon even when sellers do not have a good reputation. In 61% of cases, buyers choose to buy from sellers without a reputation, and in 31% of cases, these buyers even buy from sellers with a bad reputation (see Figure 4.4). We assume that this is due to a lack of alternative sellers; buyers facing only one seller at a time may want to take the risk by deciding to buy even if the information provided about the seller's reputation indicates that they should not. This behavior could change when buyers have the option to choose from among different sellers. We, therefore, conduct a second experiment introducing competition among sellers by allowing buyers to choose one of two sellers in each round.

4.4.1 Experimental design and procedure

The experiment was conducted in the Experimental Laboratory at University XY in 9 sessions and with 150 participants in total. The experimental conditions varied across sessions and were conducted in the same way as in Experiment 1.

Similar to Experiment 1, each participant is randomly assigned to be a seller or buyer for the duration of the session. However, in Experiment 2, each buyer is matched with two sellers at the start of every round. Hence, each group consists of three participants – one buyer and two sellers. Participants are informed that the experiment consists of 20 to 30 rounds, and they are not told that the actual number of rounds is 20 (24 in Experiment 1).

Unlike in Experiment 1, sellers are informed in Experiment 2 about their own last recorded decision as well as that of the seller they are matched with in the current round (the competitor). Sellers then choose whether to offer discounts, i.e. play AG or TG. Next, the buyer decides which of the two sellers to interact with after being given information about their reputations and whether they offer discounts. Figure 4.5 contains an example screenshot of this decision situation. Following that, the steps are the same as in Experiment 1. The instructions used for Experiment 2 are provided in section C.5 in Appendix C.

Table 4.4 lists the proportion of times seller 1 (the seller shown on the left side of the screen) is chosen by the matched buyer for every possible combination of the two sellers' reputations and decisions to give discounts (absolute frequencies in parentheses). Moreover, in Table 4.4, we highlight the situations in which the reputation of both sellers are identical. This information demonstrates sellers' preferences for situations while knowing whether the competitors' reputation is better than their own, as well as buyers' preferences for sellers based on a comparison of their reputations and decisions to offer discounts and those of their competitors.

In Experiment 2, participants were mostly female (58.1%) and their mean age was 23.9 years (SD = 7.13). The payoff of participants in each round is summed up and converted into monetary payment at the end of the session at a rate of EUR 1 per 70 payoff-points. An experimental session lasted for about 75 minutes and participants earned 15.74 EUR on average.

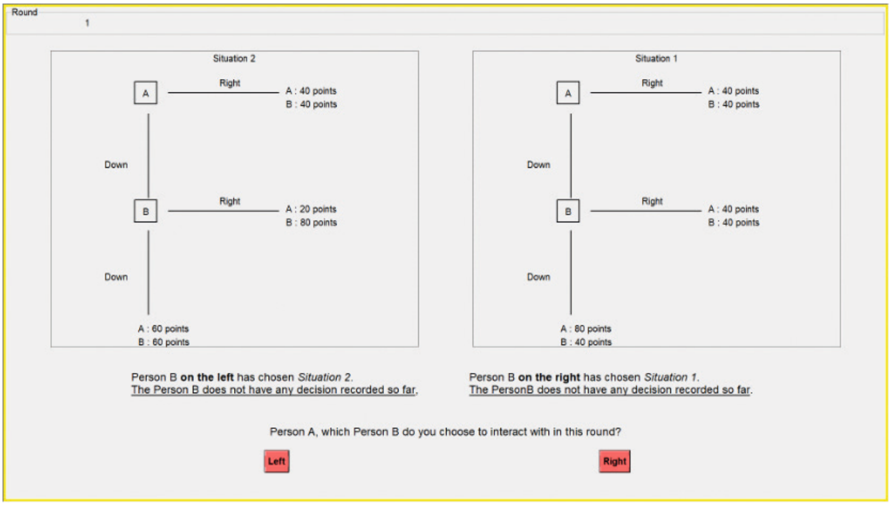

Figure 4.5 Screenshot of a buyer's decision to choose one of two sellers

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Table 4.4 The proportion of Seller 1 being chosen in each combination of sellers a buyer could encounter. The frequency of each combination is also reported

		Seller 2 reputation and decision of giving discounts						
		bad, discount	bad, no discount	none, discount	none, no discount	good, discount	good, no discount	Total
Seller 1 reputation and decision of giving discounts	bad, discount	0.636 (11)	1 (12)	0.4 (5)	0.875 (16)	0.2 (5)	0.586 (58)	(107)
	bad, no discount	0.077 (13)	0.563 (16)	0 (5)	0.143 (14)	0 (3)	0.179 (39)	(90)
	none, discount	1 (2)	0.8 (5)	0.486 (35)	0.961 (51)	0 (3)	0.958 (24)	(120)
	none, no discount	0 (5)	0.833 (12)	0.152 (46)	0.537 (136)	0 (8)	0.175 (40)	(247)
	good, discount	0.6 (5)	1 (5)	0.8 (5)	1 (8)	0.667 (15)	0.969 (32)	(70)
	good, no discount	0.278 (54)	0.744 (39)	0.132 (38)	0.719 (57)	0.096 (52)	0.548 (126)	(366)
	Total	(90)	(89)	(134)	(282)	(86)	(319)	(1000)

4.4.2 Variables

Table 4.5 Descriptive statistics of variables in Experiment 2

	N	**Percentage**
Dependent variables		
Seller's decision 1 (discount = 1)	2000	.304
Buyer's decision (buy = 1)	1000	.816
Seller's decision 2 (ship = 1)	816	.712
Independent variables		
Seller's reputation	2000	
bad		.188
none		.392
good		.421
Experimental condition	2000	
$\pi = 0.2$		.32
$\pi = 0.4$		.34
$\pi = 0.6$		.34

4.4.3 Analyses and results

Similar to Experiment 1, our data analysis strategy is to employ linear comparisons with the proportions from saturated logit models to test hypotheses and perform logistic regressions for exploratory analyses on sellers' decisions to offer discounts and decisions to ship. For analyzing the buyers' decision to buy, conditional logit models based on the choice sets for the buyer are more appropriate assuming that buyers' decisions among available sellers are a function of the characteristics of the sellers rather than the buyers themselves (Hoffman & Duncan, 1988). For the conditional logit, we use a long data format in which each potential seller to be chosen by a buyer is a case in the data. The choice sets are composed of two sellers from which a buyer has to choose one in each round of the experiment. All analyses are run with STATA (version 17.0).

4.4.3.1 Sellers' decisions to give discounts

Figure 4.6 shows the frequency of sellers choosing to offer discounts across seller reputations and experimental conditions.[3] Overall, sellers give discounts more frequently in Experiment 2 (30%) than in Experiment 1 (9%), which is consistent with our expectations. The proportion of sellers giving discounts under each condition is provided in section C.6 in Appendix C. The linear parameter test shows that sellers without a reputation are more likely to provide a discount than sellers with a good reputation (coef. = 0.121, p = 0.004). This supports hypothesis 1. However, again, sellers with a bad reputation offer discounts even more frequently than sellers without a reputation (coef. = 0.199, p = 0.003). Moreover, in line with hypothesis 4, we observe that the proportion of sellers giving discounts while not having a reputation decreases as π increases. However, the joint significance test indicates that this decrease is not statistically significant ($\chi^2(2) = 4.12$, p = 0.128).

In Experiment 2, information about the reputation of the competitor is available while a seller is deciding whether to give a discount. As a result, we assume that to increase their chance of being chosen by the buyer, sellers will make decisions also based on the reputation of their competitors. For example, sellers should be more likely to give discounts if the competitor has a better reputation than they do (see Table 4.4). Yet, similarly to Experiment 1, 30% - 50% of buyers decide to buy even when the chosen seller does not have a good reputation and does not offer a discount (see Figure 4.7). Again, we conjecture that sellers who have experienced such unconditional trust would be less inclined to give discounts. We test this conjecture as well as the role of the reputation of the competing seller in the exploratory analysis section below.

3 The corresponding figure for only the first ten rounds is included in section C.6 in Appendix C.

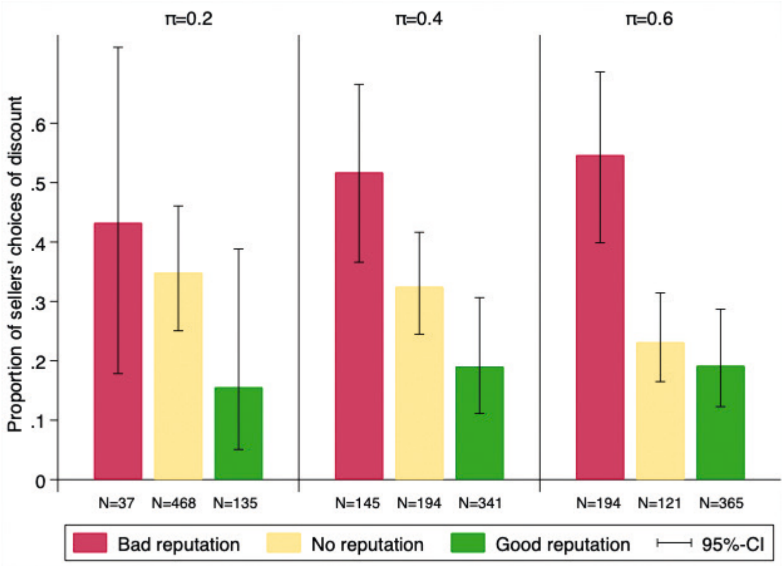

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Figure 4.6 Proportion of sellers giving discounts across experimental conditions and seller reputations (Experiment 2)

4.4.3.2 Buyers' decisions to buy

Table 4.4 above shows the frequency of sellers being chosen given their own as well as their competitors' reputations and choices regarding discounts. Based on the numbers along the diagonal of the matrix, buyers seem to favor seller 1, whose information is shown on the left side of the computer screen. The slight preference for seller 1, however, should not have any impact on the results as the sellers' order of appearance on buyers' decision screens is randomly determined in every round.

The proportions of buyers choosing to buy under different conditions are provided in section C.7 in Appendix C. Figure 4.7 shows how buyers' decisions to buy vary across sellers' reputations and experimental conditions. Along with Figure 4.3, Figure 4.7 demonstrates that the buying decision patterns in the two studies are similar. Without splitting by sellers' decisions on discounts, the general level of buyers' decisions to buy does not differ among the partners' reputations ($\chi^2(2) = 3.49$, $p = 0.175$). When discounts are given, buyers almost always choose to buy, regardless of the seller's reputation ($\chi^2(2) = 2.47$, $p = 0.291$), and the proportion to buy is significantly higher than if no discounts are offered (coef. = 0.303, $p < 0.001$). This is in support of hypothesis 2a. When there is no discount, we observe a difference in buyers' decisions to buy across seller reputations. Buyers are more likely to buy from sellers with a good reputation than those with a bad reputation (coef. = 2.028, $p = 0.001$), and buyers with no reputation (coef. = 1.084, $p = 0.004$). Therefore, hypothesis 2b is supported.

The pattern of buyers' decisions to buy is the same across experimental conditions (see section C.7 in Appendix C). We also run a conditional logit of buyers' decisions to buy from sellers based on sellers' discernible qualities (M5, Table 4.6). These results are discussed in the exploratory analysis section below.

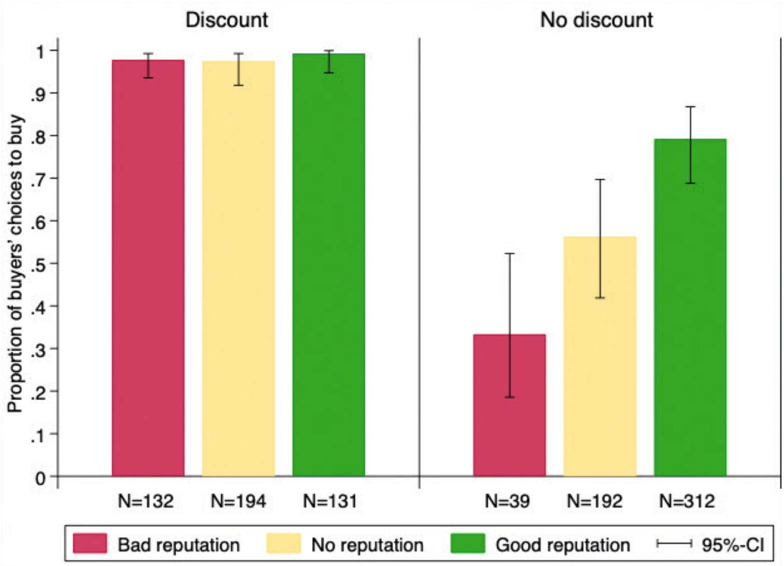

Figure 4.7 Proportion of buyers deciding to buy across seller reputations and sellers' decisions to give a discount (Experiment 2)

4.4.3.3 Sellers' decisions to ship

Of the 816 times in which a seller is chosen and bought from by a buyer, the seller chooses to ship in 581 (71%) cases. The proportions of sellers choosing to ship under different conditions are provided in section C.8 in Appendix C. In Figure 4.8, we show how sellers' decisions to ship vary depending on the situation and the reputation of these sellers. In contrast to our findings in Experiment 1, sellers are overall more likely to ship when they offer discounts (coef. = 0.281, $p < 0.001$). This is true for sellers who have a bad reputation (coef. = 0.366, $p = 0.015$), sellers who have a good reputation (coef. = 0.251, $p < 0.001$), as well as for sellers without a reputation (coef. = 0.279, $p = 0.001$). Additionally, when there is no discount, sellers with a good reputation are more likely to ship than sellers with a bad reputation (coef. = 0.441, $p < 0.001$). The difference with sellers without a reputation is not significant (coef. = 0.126, $p = 0.132$). Therefore, hypothesis 3 is partly supported. In the exploratory section, we examine the relationship between sellers' decisions to offer discounts and decisions to ship using logistic regression models (M6, Table 4.6).

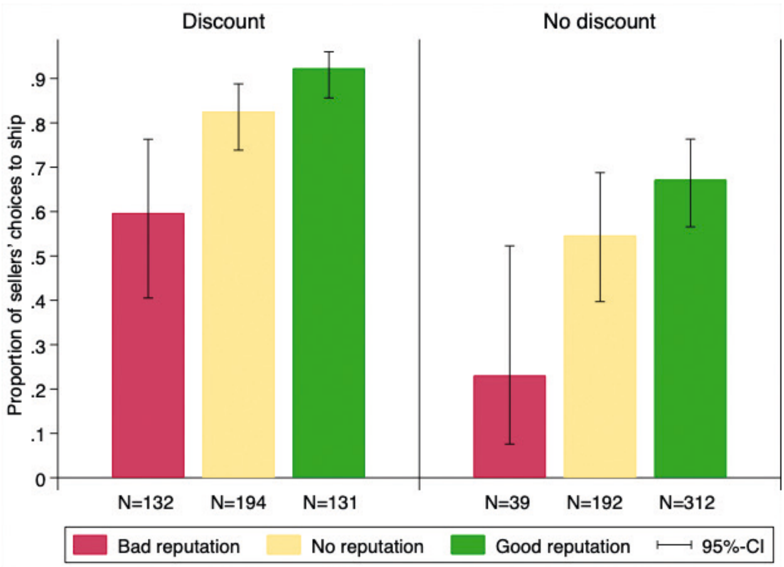

Figure 4.8 Proportion of sellers deciding to ship across seller reputations and sellers' decisions to give a discount (Experiment 2)

4.4.3.4 Exploratory analysis

As discussed above, due to the existence of competition, we expect sellers to take the reputation of the competing seller into account when choosing whether to offer discounts. Figure 4.9 shows how frequently sellers offer discounts based on both their own reputation and the reputation of their competitors. Overall, regardless of their own reputation, sellers are most likely to offer discounts when the competitor has a good reputation. To determine how the experimental conditions, the sellers' own reputation, prior trust experience, and the reputation of the competitors affect sellers' decisions to give discounts, we conduct a series of logistic regression analyses (M4, Table 4.6).

The result of M4b suggests that compared to sellers with a good reputation, sellers who have a bad reputation (coef. = 1.702, $p < 0.001$) and who have no reputation (coef. = 0.903, $p < 0.001$) are more likely to give discounts. We expect that choosing to offer a discount is more common when the competing seller has a good reputation, indicating that sellers may devise such a strategy in response to the reputation of the competitor. Compared with being matched with a competitor with a good reputation, sellers are significantly less likely to give discounts if matched with a competitor with no reputation (coef. = -0.536, $p = 0.003$), and less likely to give discounts if the competitor has a bad reputation (coef. = -0.807, $p < 0.001$). Additionally, sellers who received unconditional trust in the previous round are less likely to offer discounts (coef. = -1.460, $p < 0.001$).[4] Finally, the joint test suggests that the experimental conditions have no significant effects on sellers' decisions to give discounts ($\chi^2(2) = 0.48$, $p = 0.789$).

To investigate how buyers' decisions to buy are influenced by sellers' reputations and these sellers' decisions to offer discounts, we run conditional logit regressions with seller-pair fixed effects and estimate robust standard errors accounting for within-buyer clustering (M5, Table 4.6). Because the experimental condition does not vary within a seller pair, the coefficients for experimental condition cannot be estimated using this method. Moreover, 368 cases are excluded from the analysis because in these cases the buyers abstained from buying from either of the two sellers in those rounds. We also performed

4 We also estimated a model including the interaction effect of seller reputation and situation choice. Because this interaction effect is not significant and does not increase model fit, this model is not reported in the paper.

conditional logit analyses of whether buyers chose sellers, regardless of whether buyers decided to buy from these sellers. However, if a buyer favors one seller over the other but chooses not to buy from them, this may just indicate that the buyer does not want to buy from either of the two sellers (a choice option that was not available in the experiment). The results of this model are reported in section C.9 in Appendix C.

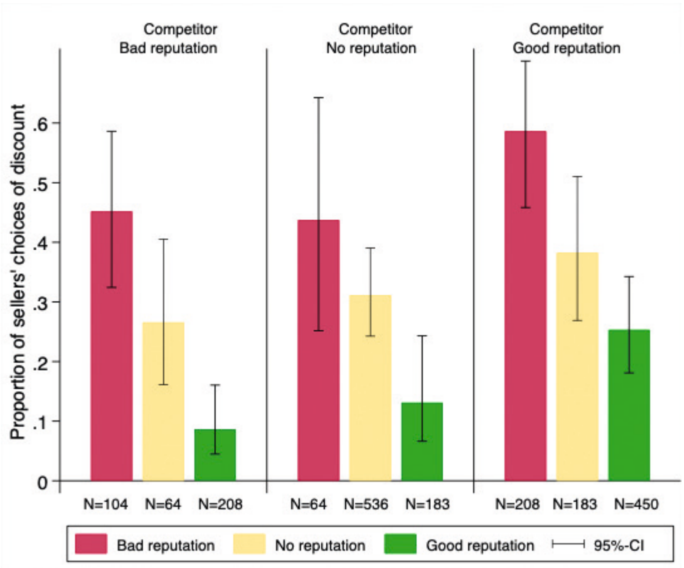

Figure 4.9 Proportion of sellers giving discounts across own and competing sellers' reputations (Experiment 2)

Table 4.6 (Conditional) logit analysis predicting sellers' and buyers' decisions (Experiment 2)

	Sellers' decisions of giving a discount		***Buyers' decisions to buy***		***Sellers' decisions to ship***	
	M4a	M4b	M5a	M5b	M6a	M6b
Experimental condition						
$\pi = 0.2$	0.169	0.189			0.644†	0.663†
	(0.284)	(0.281)			(0.376)	(0.377)
$\pi = 0.4$	0.047	0.054			0.402	0.417
	(0.270)	(0.269)			(0.366)	(0.374)
$\pi = 0.6$	(ref.)	(ref.)			(ref.)	(ref.)
Seller reputation (self)						
bad	1.623***	1.702***	-2.164***	-2.248***	-1.833***	-1.971**
	(0.293)	(0.296)	(0.313)	(0.410)	(0.415)	(0.756)
none	0.839**	0.903***	-1.373***	-1.433***	-0.854**	-0.710*
	(0.274)	(0.274)	(0.170)	(0.226)	(0.280)	(0.348)
good	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)	(ref.)
Seller reputation (competitor)						
bad	-0.800***	-0.807***				
	(0.198)	(0.200)				
none	-0.567***	-0.536**				
	(0.178)	(0.183)				
good	(ref.)	(ref.)				
Prior trust						
yes		-1.460***				
		(0.268)				
no		(ref.)				
Situation						
discount			2.874***	2.639***	1.521***	1.797***
			(0.270)	(0.418)	(0.279)	(0.380)
no discount			(ref.)	(ref.)	(ref.)	(ref.)
Interaction						
bad × discount				0.328		-0.078
				(0.625)		(0.836)
none × discount				0.326		-0.489
				(0.510)		(0.536)
good × discount				(ref.)		(ref.)

Table 4.6 (Conditional) logit analysis predicting sellers' and buyers' decisions (Experiment 2) (continued)

	Sellers' decisions of giving a discount		***Buyers' decisions to buy***		***Sellers' decisions to ship***	
	M4a	M4b	M5a	M5b	M6a	M6b
Constant	-1.243***	-1.237***			0.500†	0.448
	(0.257)	(0.258)			(0.298)	(0.305)
Observations	2000	2000	1632	1632	816	816
Clusters	100	100	50	50	100	100
Pseudo R^2	0.073	0.087	0.328	0.329	0.095	0.096

Robust standard errors in parentheses (adjusted for clustering in same deciding participant)
*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

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The result of M5a suggests that buyers prefer to buy from sellers who offer discounts (coef. = 2.874, p < 0.001); compared to sellers who have a good reputation, buyers are less likely to buy from sellers with a bad reputation (coef. = -2.164, p < 0.001) and sellers without a reputation (coef. = -1.373, p < 0.001). M5b introduced the interaction between seller reputation and sellers' decisions of giving discounts. Although the model fit is slightly increased, the interaction effects are statistically insignificant. This suggests that the effects of seller reputation and discounts are largely additive and that the effect of reputation does not differ depending on whether a discount is given.

Finally, we run logistic regression models of sellers' choices to ship with seller reputation and the chosen situation as the main predictors (M6, Table 4.6). The result of M6a shows that compared with sellers with a good reputation, sellers who have a bad reputation (coef. = -1.833, p < 0.001) and sellers who do not have a reputation (coef. = -0.854, p = 0.002) are less likely to ship. Moreover, the likelihood to ship is higher when sellers offer discounts (coef. = 1.521, p < 0.001). Model M6a also controls for the experimental condition. Sellers appear to be more likely to ship when π is lower, but the effects are not statistically significant. In model M6b, we added the interaction between seller reputation and offering discounts. None of the interaction terms is statistically significant, which suggests that the positive effect of giving discounts on choosing to ship does not differ among sellers with different reputations.

4.5 Conclusion and discussion

Previous research on reputation-based online markets has presented evidence of the existence of reputation effects, with a considerable variation in effect sizes (Jiao et al., 2021). One way to interpret this variation is that the effectiveness of reputation systems may not only impact the selling profit or volume of more reputable sellers but also the incentives for untrustworthy sellers to enter the market. In a perfectly functioning reputation-based online market, sellers would always receive truthful feedback after each transaction. As a result, untrustworthy sellers would immediately be exposed. In such a market, uncertainty about sellers' trustworthiness would be minimal and therefore information about sellers' reputations would not contribute much to buyers deciding on which sellers to buy from. However, because transaction partners do not always leave truthful feedback in real-world markets (Bolton et al., 2004; Chen et al., 2021), the uncertainty about sellers' trustworthiness will be higher, and buyers will demand price discounts to mitigate their risks when dealing with market entrants without a reputation (Przepiorka, 2013).

The relationship between the rate of truthful feedback on completed market transactions and the reputation effect has received surprisingly little attention in previous research (Jiao et al., 2022). Here we conduct two lab experiments that emulate the interactions between buyers and sellers in online markets using trust games – with buyers being the trusters and sellers being the trustees. In our experiments, we systematically vary the feedback rate through the probability of a seller's decision being recorded and shown to future interaction partners. Our study aims to examine how sellers offer discounts as a strategy to build their reputation, as well as how buyers' decisions to buy and sellers' decisions to ship depend on discounts and seller reputations. Our two experiments build on each other with the second introducing seller competition to examine whether such competition would affect sellers' decisions of giving discounts.

We find evidence to support that sellers base their decisions to offer discounts on their own reputation; sellers are more likely to offer discounts if they do not have a good reputation. If sellers give discounts, buyers are more likely to buy from them. However, buyers are more likely to buy from sellers with a good reputation even if no discount is offered. Furthermore, sellers are less likely to ship when they have a bad reputation, and they are more likely to ship when they gave a discount.

We also observe that many buyers buy from sellers who do not offer discounts and do not have a good reputation. In exploratory analyses, we show that such unconditional trust negatively influences sellers' decisions to offer discounts. That is, sellers who do not have a good reputation but experienced unconditional trust in previous interactions are less likely to give discounts. Based on this observation in Experiment 1, we decided to induce seller competition in Experiment 2. When there is competition among sellers, a seller is more willing to offer discounts in general (as compared to Experiment 1) and, in particular, if the seller's competitor has a good reputation. There is no compelling evidence that sellers or buyers behave differently across experimental conditions. Yet, we observe a tendency that as π increases, more discounts are given by sellers with a bad reputation. This suggests that sellers are more motivated to repair their bad reputation when the probability of getting a truthful rating is higher.

Our approach has a few limitations. In both studies, we explicitly inform participants that with a certain probability, the sellers' decision from the previous round is recorded. That is, we treat the feedback rate as public knowledge and vary the feedback rate exogenously. By doing so, we make a few latent assumptions that do not accurately reflect the real feedback mechanism: (1) As the feedback rate of a certain market is practically never (accurately) disclosed publicly, consumers may not pay much attention to it or do not realize how important it may be for their decision-making. And even if consumers consider it, they may only be able to infer the feedback rate based on their own observations and experiences. (2) The three experimental conditions varying the feedback rate (π = 0.2, 0.4, and 0.6) may not be sufficiently different to induce differences in participant behavior. Therefore, future research could build on an improved experimental design with more distinct feedback rates. (3) The controlled experimental design helps to provide an ideal exchange environment that can disentangle interrelated effects that take place in reality (Keijzer & Corten, 2023). For instance, with the two experimental designs described in this paper, we assume all feedback, i.e. the recorded behavior, is given truthfully, which may not always be the case in real-world settings. As a result, the external validity of our findings may be limited.

A

APPENDIX

Supplementary material for Chapter 2

A.1. References of papers considered for meta-analysis

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A.2. Table of papers and in/exclusion criteria for the meta-analyses

Table A.1. Table of papers and in/exclusion criteria for the meta-analyses

Study abbreviation	Exclusion criterion
Ahlee_Malmendier_2005	0
Andrews_Benzing_2007	0
Anderson_Friedman_et_al_2007	0
Ariely_Simonson_2003	0
Ba_Pavlou_2002	0
Bajari_Hortacsu_2003	0
Baker_Song_2008	0
Baranowski_Komor_Wójcik_2017	0
Berger_Schmitt_2005	0
Bland_et_al_2005	1
Bockstedt_Goh_2011	0
Brint_2003	2
Brown_Morgan_2006	3
Bruce_et_al_2004	0
Cabral_Hortaqcsu_2010	0
Cai_et_al_2011	4
Cai_et_al_2013	6
Cai_et_al_2014	1
Canals-Cerda_2012	0
Chan_et_al_2007	2
Chen_Lai_Yu_2018	0
Chiou_Pate_2018	0
Cui_Huang_2010	0
Dellarocas_Wood_2008	3
Depken_Gregorius_2010	0
Dewally_Ederington_2002	4
Dewally_Ederington_2006	0
Dewan_Hsu_2001	0
Dewan_Hsu_2004	0
Diekmann_et_al_2014	0
Diekmann_Jann_Wyder_2009	0

Doong_Hsieh_2005	3
Du_Yu_et_al_2012	0
Duan_et_al_2006	1
Durham_et_al_2004	0
Eaton_2005	0
Eaton_2007	0
Eddhir_2009	0
Elfenbein_McManus_2010	0
Erlenkämper_2005	5
Fan_et_al_2013	0
Flanagin_2007	0
Fuchs_Eybl_et_al_2011	3
Ghose_et_al_2009	3
Gilkeson_Reynolds_2003	0
Grund_Gurtler_2008	0
Haley_Scyoc_2010	0
Hardy_Norgaard_2015	0
Highfill_O'Brien_2007	1
Highfill_O'Brien_2008	0
Hou_2007	0
Houser_Wooders_2006	0
Huang_2011	2
Huang_et_al_2011	0
Huang_Su_2011	0
Jin_Kato_2006	0
Johnston_2003	0
Jolivet_et_al_2016	0
Kalyanam_McIntyre_2001	0
Kauffman_Wood_2000	0
Kauffman_Wood_2005	0
Klein_et_al_2014	2
Kim_2005	3
Laitinen_Laitinen_et_al_2016	0
Lawson_2002	0
Lee_Im_et_al_2000	3
Lei_2011	0
Li_2017	0

Li_Cui_2013	0
Li_Li_2012	0
Li_et_al_2009	2
Li_Wu_et_al_2008	0
Li_Ye_et_al_2010	0
Liu_Hui_et_al_2008	0
Liu_Wei_et_al_2009	0
Livingston_2005	0
Lucking-Reiley_et_al_2007	0
Luo_Chung_2010	0
Malmendier_Lee_2011	3
McDonald_Slawson_2002	0
Melnik_Alm_2002	0
Melnik_Alm_2003	4
Melnik_Alm_2005	0
Melnik_Richardson_et_al_2011	0
Mickey_2010	0
Mink_Seifert_2006	3
Nosko_Tadelis_2015	3
Nurmi_et_al_2017	0
Obloj_Capron_2010	3
Ockenfels_2003	3
Ottaway_Bruneau_et_al_2001	1
Pan_Chen_2012	0
Pan_Liao_2009	0
Park_Bradlow_2005	2
Pavlou_Dimoka_2006	0
Przepiorka_2013	0
Przepiorka_et_al_2017	0
Quill_2007	0
Rabby_Shahriar_2016	0
Resnick_Zeckhauser_2002	0
Resnick_et_al_2006	3
Ruiz_2004	0
Schamel_2005	0
Sears_2016	0
Sena_et_al_2006	0

Sena_Braun_2006	0
Shen_Chiou_Kuo_2011	0
Shi_ Zhu_et_al_2015	0
Simonsohn_Ariely_2008	0
Snijders_Zijdeman_2004	0
Song_Baker_2007	0
Standifird_2001	0
Standifird_Weinstein_2007	0
Steckbeck_2004	0
Sun_2010	0
Sun_Liu_2010	0
To_Liu_et_al_2008	0
Wan_Teo_2001	0
Waterson_Doyle_2012	6
Wolf_Muhanna_2005	0
Wu_Xu_Fan_2014	0
Wu_Ye_2008	0
Xiao_Liu_2009	0
Yao_Xu_Shen_2014	0
Ye_Li_et_al_2009	0
Ye_et_al_2010	3
Ye_Xu_et_al_2013	0
Yin_2006	6
Yin_2017	0
Yoo_Ho_2006	0
You_Liu_et_al_2011	3
Zeithammer_2006	3
Zhang_2006	0
Zhang_2009	0
Zhang_Zhang_2011	0
Zhao_Huang_2008	0
Zhao_Sun_et_al_2013	0
Zhou_2014	0
Zhou_Dresner_et_al_2009	0
Zhou_Zhang_et_al_2006	0
Zhu_Leboulanger_Li_2009	0

A.3. Overall effect sizes estimated based on Strategy 2 for accounting for p-value ranges

Table A.2. Overall effect sizes estimated based on Strategy 2

Relation	*ES*	95% CI	*N*	Heterogeneity test Q	I^2 (%)	Egger's test
Final price						
Reput. score	0.05*	[0.01, 0.09]	66	1114.65***	99.76	-0.91
Pos. ratings	0.10***	[0.06, 0.15]	53	554.03***	99.15	3.12**
Neg. ratings	-0.10***	[-0.13, -0.07]	44	287.57***	92.07	-2.42*
Price ratio						
Reput. score	0.08*	[0.00, 0.15]	16	410.68***	99.17	0.25
Pos. ratings	0.28***	[0.18, 0.37]	35	483.75***	99.62	5.11***
Neg. ratings	-0.06	[-0.15, 0.03]	25	183.76***	87.14	1.94
Selling probability						
Reput. Score	0.04*	[0.01, 0.07]	26	965.00***	96.48	-2.28*
Pos. ratings	0.07***	[0.04, 0.10]	19	1147.40***	97.14	-0.01
Neg. ratings	-0.05***	[-0.07, -0.03]	16	39.19	61.12	-1.63
Selling volume						
Reput. score	0.08	[-0.01, 0.16]	31	5507.72***	99.80	-1.90*
Pos. ratings	0.14***	[0.09, 0.19]	31	889.53***	99.39	4.02***
Neg. ratings	-0.05	[-0.15, 0.06]	16	99.27***	91.52	-2.92**

*Notes: *** p < 0.001, ** p < 0.01, * p < 0.05.*

B

APPENDIX

Supplementary material for Chapter 3

B.1. Complete list of screened articles with exclusion reasons

Table B.1. Complete list of screened articles with exclusion reasons (in number)

Study abbreviation	Exclusion reason*
Ahlee_Malmendier_2005	0
Anderson_Friedman_et_al_2007	0
Andrews_Benzing_2007	0
Ariely_Simonson_2003	0
Ba_Pavlou_2002	0
Bajari_Hortacsu_2003	0
Baker_Song_2008	0
Baranowski_Komor_Wójcik_2017	0
Berger_Schmitt_2005	0
Bland_et_al_2005	1
Bockstedt_Goh_2011	0
Bodoh-Creed_2018	1
Brint_2003	2
Brosig-Koch_et_al_2021	1
Brown_Morgan_2006	3
Bruce_et_al_2004	0
Cabral_Hortaqcsu_2010	0
Cai_et_al_2011	4
Cai_et_al_2013	0
Cai_et_al_2014	1
Canals-Cerda_2012	0
Casalin_Dia_2019	0
Chan_et_al_2007	2
Che_et_al_2019	0
Chen_et_al_2015	0
Chen_Lai_Yu_2018	0
Chen_tu_2019	0
Chen_et_al_2020	3
Cheng_et_al_2020	0
Chintagunta_chu_2021	6
Chiou_Pate_2018	0

Cui_Huang_2010	0
DalVecchio_et_al_2018	1
Dellarocas_Wood_2008	3
Depken_Gregorius_2010	0
Dewally_Ederington_2002	4
Dewally_Ederington_2006	0
Dewan_Hsu_2001	0
Dewan_Hsu_2004	0
Diekmann_Jann_Wyder_2009	0
Doong_Hsieh_2005	3
Drab_2020	1
Du_Yu_et_al_2012	0
Duan_et_al_2006	1
Durham_et_al_2004	0
Eaton_2002	0
Eaton_2007	0
Eddhir_2009	0
Elfenbein_et_al_2019	2
Elfenbein_et_al_2012	6
Elfenbein_et_al_2015	6
Elfenbein_McManus_2010	0
Erlenkämper_2005	5
Esenduran_et_al_2020	2
Fan_et_al_2013	0
Flanagin_2007	0
Fuchs_et_al_2008	3
Fuchs_Eybl_et_al_2011	3
Ghose_et_al_2009	3
Gilkeson_Reynolds_2003	0
Grund_Gurtler_2008	0
Haley_Scyoc_2010	0
Han_et_al_2018	0
Hardy_Norgaard_2015	0
Haruvy_et _al _2018	1
He_2013	3
He_et_al_2020	2
He_li_2018	0

Hendricks_Sorensen_2018	3
Hesse_Teubner_2020	0
Highfill_O'Brien_2007	1
Highfill_O'Brien_2008	0
Hossain_et_al_2018	1
Houser_Wooders_2006	2
Hu_et_al_2021	1
Huang_2011	2
Huang_et_al_2011	0
Huang_et_al_2021	2
Huang_Su_2011	0
Hui_et_al_2016	1
Hui_et_al_2018	0
Jian_et_al_2019	0
Jin_Kato_2006	0
Johnston_2003	0
Jolivet_et_al_2016	0
Kalyanam_McIntyre_2001	0
Kauffman_Wood_2000	0
Kauffman_Wood_2005	0
Kim_2005	3
Klein_et_al_2014	2
Kong_Rao_2021	1
Kuruzovich_Etzion_2018	0
Laestadius_et_al_2019	1
Laitinen_Laitinen_et_al_2016	0
Lawson_2000	0
Lee_Im_et_al_2000	3
Lei_2011	0
Li_2017	0
Li_2020	0
Li_Cui_2013	0
Li_et_al_2009	2
Li_et_al_2019	0
Li_et_al_2020	0
Li_Li_2012	0
Li_Wu_et_al_2008	0

Li_Ye_et_al_2010	0
Lin_et_al_2019	1
Liu_et_al_2020	1
Liu_Hui_et_al_2008	0
Liu_Wei_et_al_2009	0
Livingston_2005	0
Livingston_Scholten_2019	1
Lucking-Reiley_et_al_2007	0
Luo_Chung_2010	0
Majid_Russel_2019	0
Malmendier_Lee_2011	3
Marra_2020	1
McDonald_Slawson_2002	0
Melnik_Alm_2002	0
Melnik_Alm_2003	4
Melnik_Alm_2005	0
Melnik_Richardson_et_al_2011	0
Mickey_2010	0
Mink_Seifert_2006	3
Nosko_Tadelis_2015	3
Nurmi_et_al_2017	0
Obloj_Capron_2010	3
Ockenfels_2003	3
Ocker_2018	1
Onur_2020	1
Ottaway_Bruneau_et_al_2001	1
Pan_Chen_2012	0
Pan_Liao_2009	0
Park_Bradlow_2005	2
Pavlou_Dimoka_2006	0
Perez-Truglia_2018	2
Przepiorka_2013	0
Przepiorka_et_al_2017	0
Przepiorka_et_al_2021	4
Qiu_et_al_2018	0
Quill_2007	0
Rabby_Shahriar_2016	0

Resnick_et_al_2006	3
Resnick_Zeckhauser_2002	0
Ruiz_2004	0
Saeedi_2019	0
Schamel_2004	0
Sears_2016	0
Sena_Braun_2006	0
Sena_et_al_2006	0
Shen_Chiou_Kuo_2011	0
Shi_ Zhu_et_al_2015	0
Silva_et_al_2018	3
Silva_Rita_Topolinski_2018	1
Simonsohn_Ariely_2008	0
Song_Baker_2007	0
Standifird_2001	0
Standifird_Weinstein_2007	0
Steckbeck_2004	0
Steinhart_et_al_2019	1
Su_Xu_2019	3
Sun_2010	0
Sun_et_al_2021	2
Sun_Liu_2010	0
To_Liu_et_al_2008	0
Tu_et_al_2019	1
Vonessen_et_al_2019	1
Waisman_2021	2
Wan_Teo_2001	0
Wang_kim_2018	0
Wang_li_cai_2021	1
Wang_qu_tan_2018	0
Wang_Tariq_Alvi_2021	1
Waterson_Doyle_2012	0
Watts_2019	1
Wei_et_al_2019	1
Wolf_Muhanna_2005	0
Wu_Ren_2013	0
Wu_Xu_Fan_2014	0

Wu_Ye_2008	0
Xiao_Liu_2009	0
Yamamoto_et_al_2019	1
Yang_et_al_2019	4
Yao_Xu_Shen_2014	0
Ye_et_al_2010	3
Ye_Li_et_al_2009	0
Ye_Xu_et_al_2013	0
Yin_2006	1
Yin_2017	0
Yoo_Ho_2006	0
You_Liu_et_al_2011	3
Zeithammer_2006	3
Zhang_2006	0
Zhang_2009	0
Zhang_Zhang_2011	0
Zhao_et_al_2020	3
Zhao_Huang_2008	0
Zhao_Sun_et_al_2013	0
Zhao_zhao_deng_2019	0
Zheng_et_al_2019	2
Zhou_2014	0
Zhou_Dresner_et_al_2009	0
Zhou_et_al_2019	1
Zhou_Gupta_2020	1
Zhou_Zhang_et_al_2006	0

** 0 means the study is included in the meta-analytic collection. For corresponding reasons, see Figure 3.2.*

B.2. Complete reference list of screened articles

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B.3. Funnel plots for each set of meta-analysis

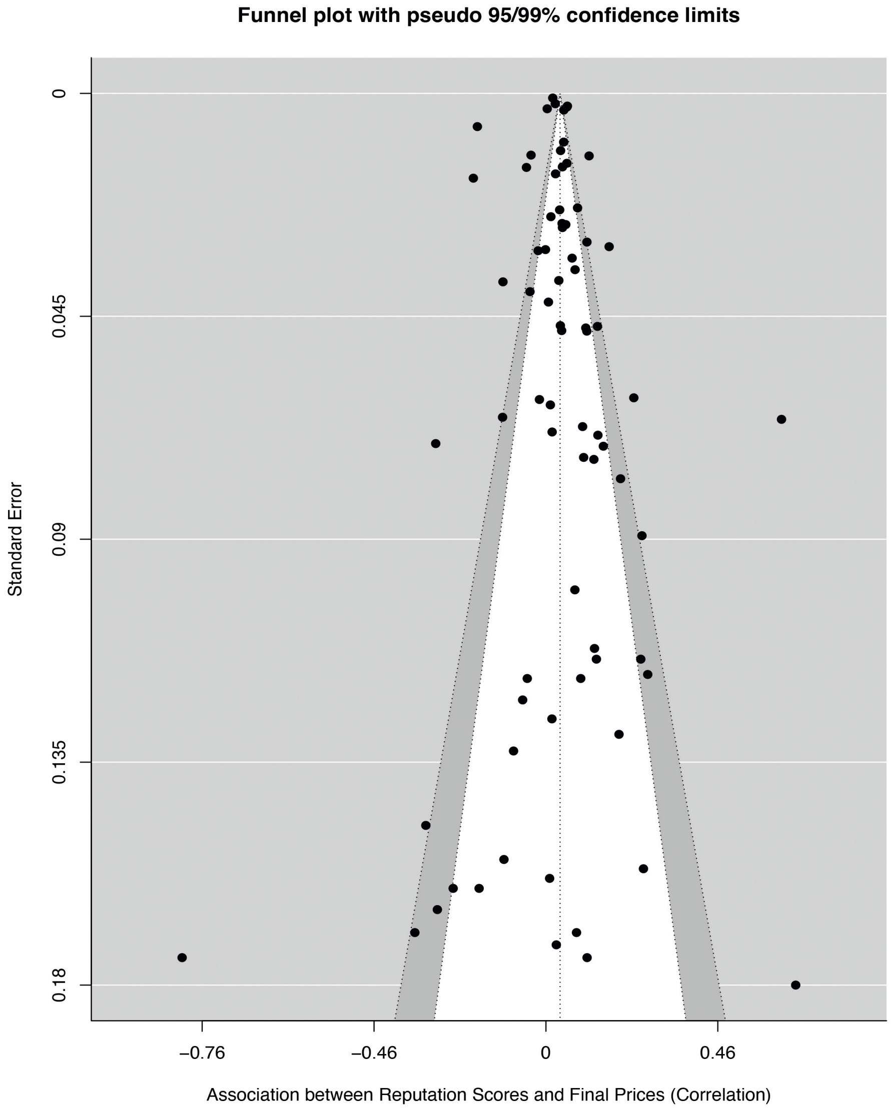

Figure B.1. Association between Reputation scores and Final Prices (Correlation)

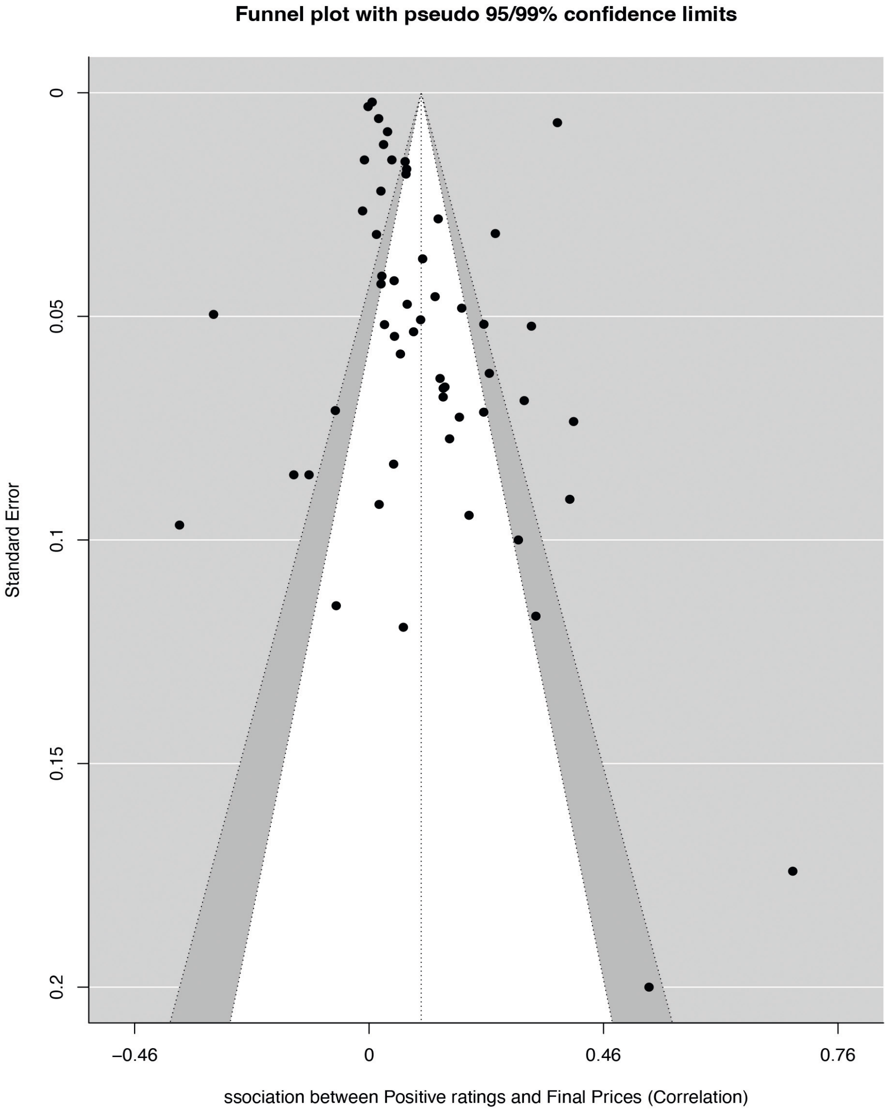

Figure B.2. Association between Positive ratings and Final Prices (Correlation)

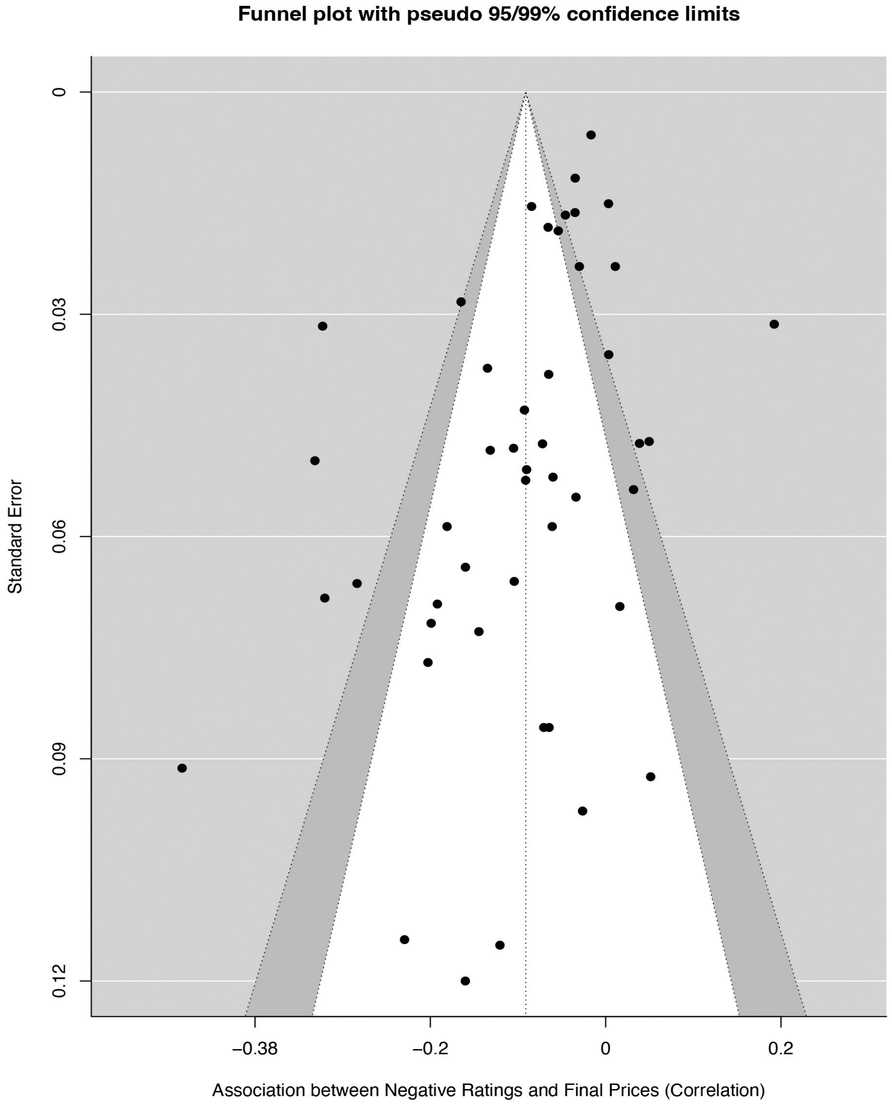

Figure B.3. Association between Negative Ratings and Final Prices (Correlation)

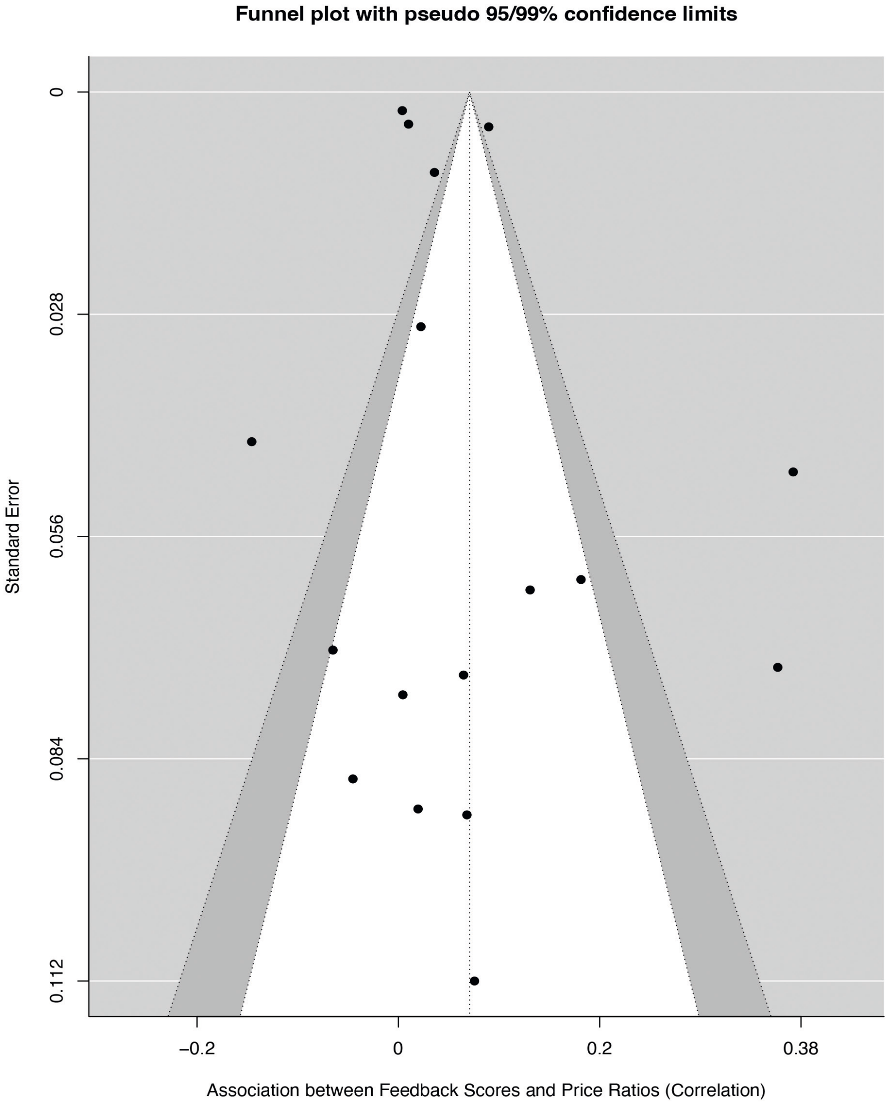

Figure B.4. Association between Reputation Scores and Price Ratios (Correlation)

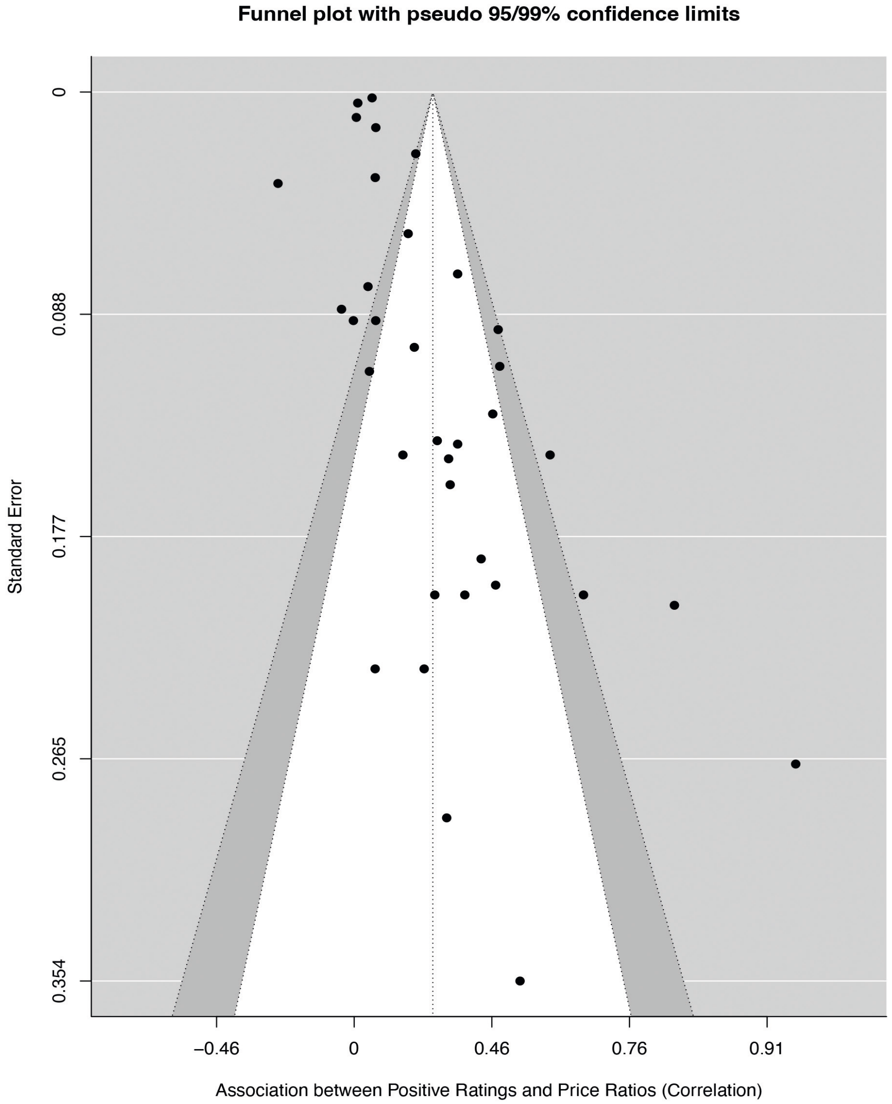

Figure B.5. Association between Positive Ratings and Price Ratios (Correlation)

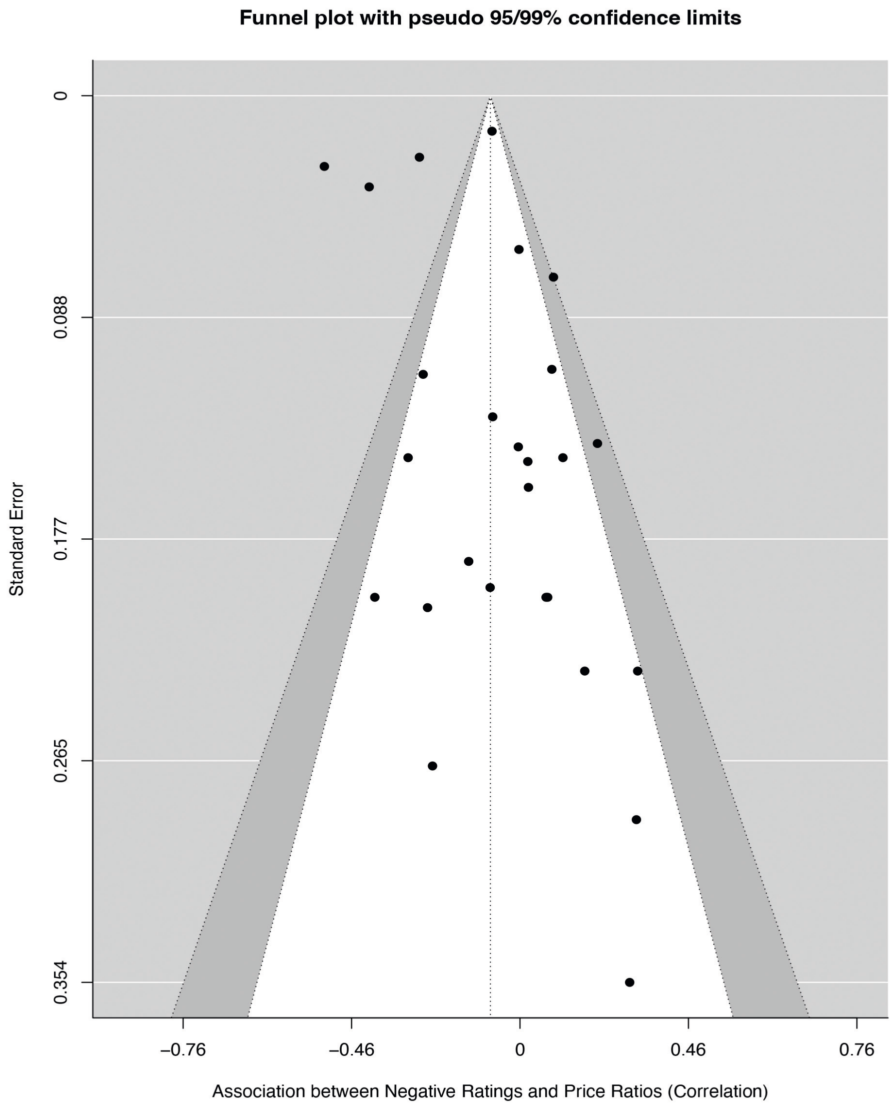

Figure B.6. Association between Negative Ratings and Price Ratios (Correlation)

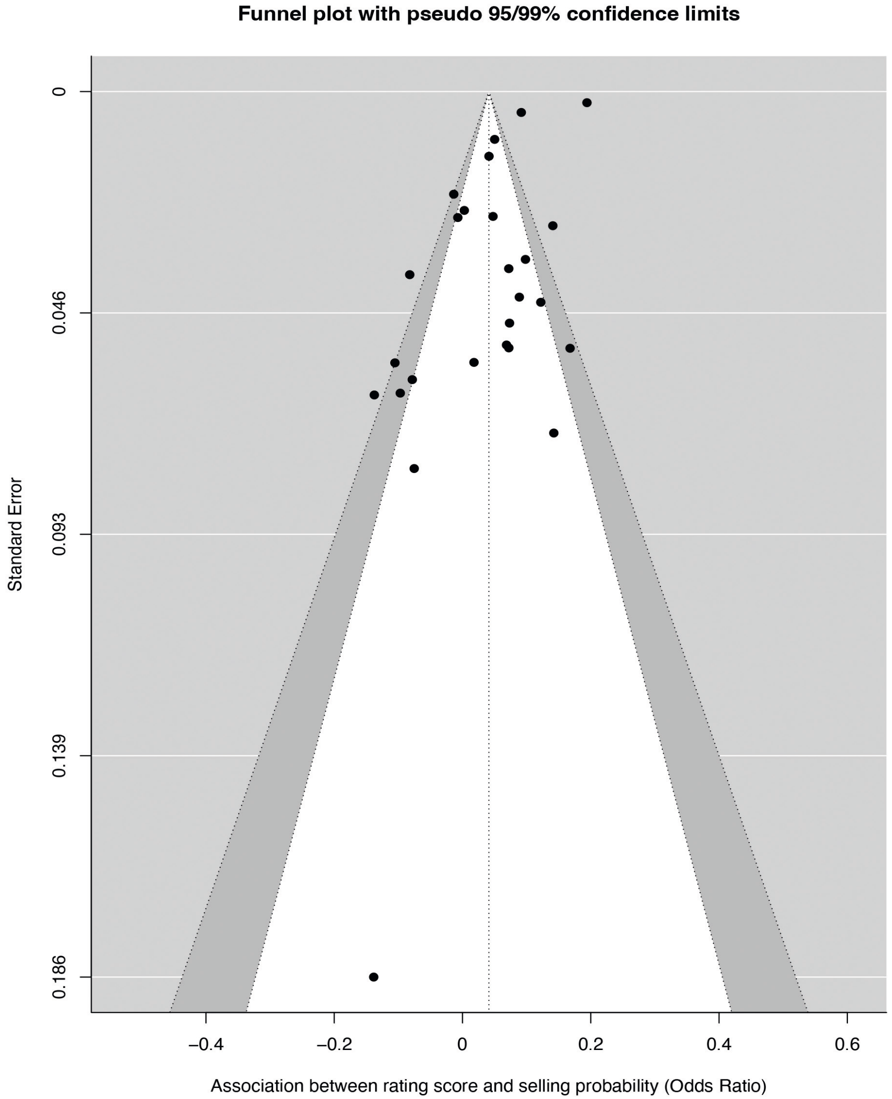

Figure B.7. Association between Reputation Scores and Selling Probability (Odds Ratio)

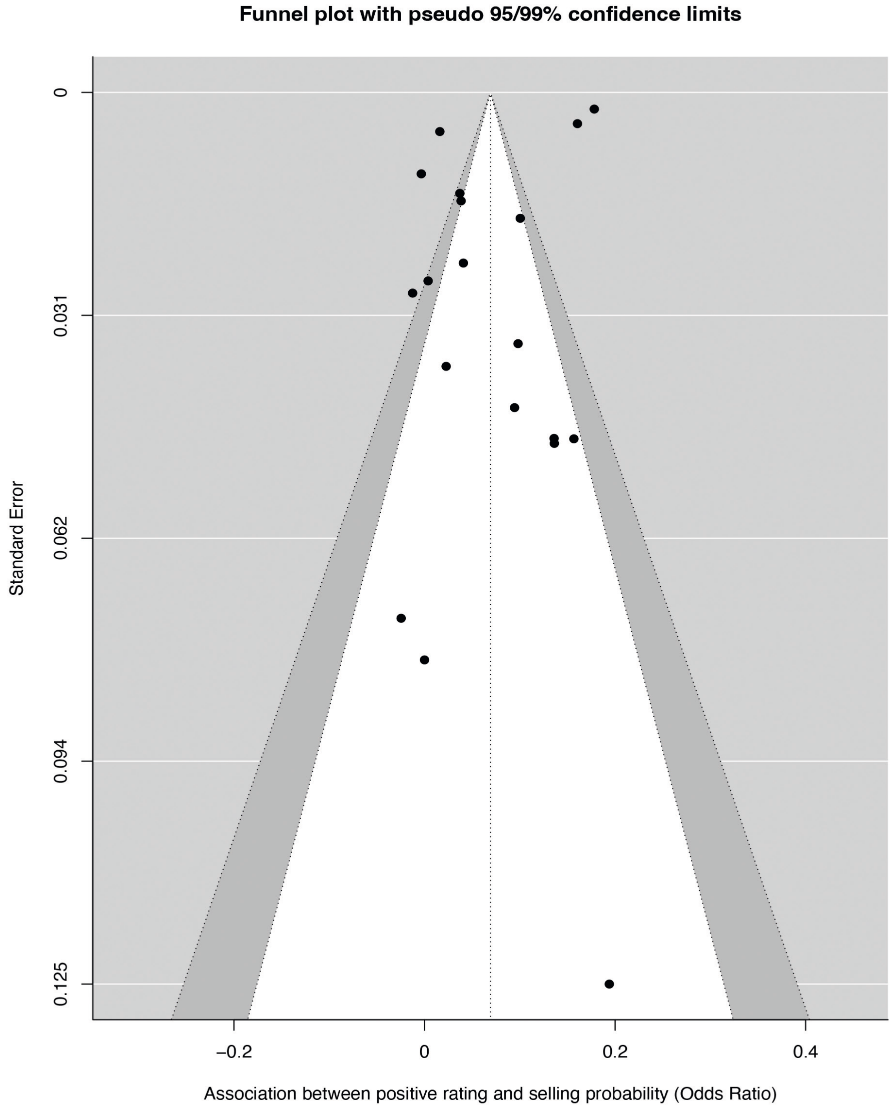

Figure B.8. Association between Positive Ratings and Selling Probability (Odds Ratio)

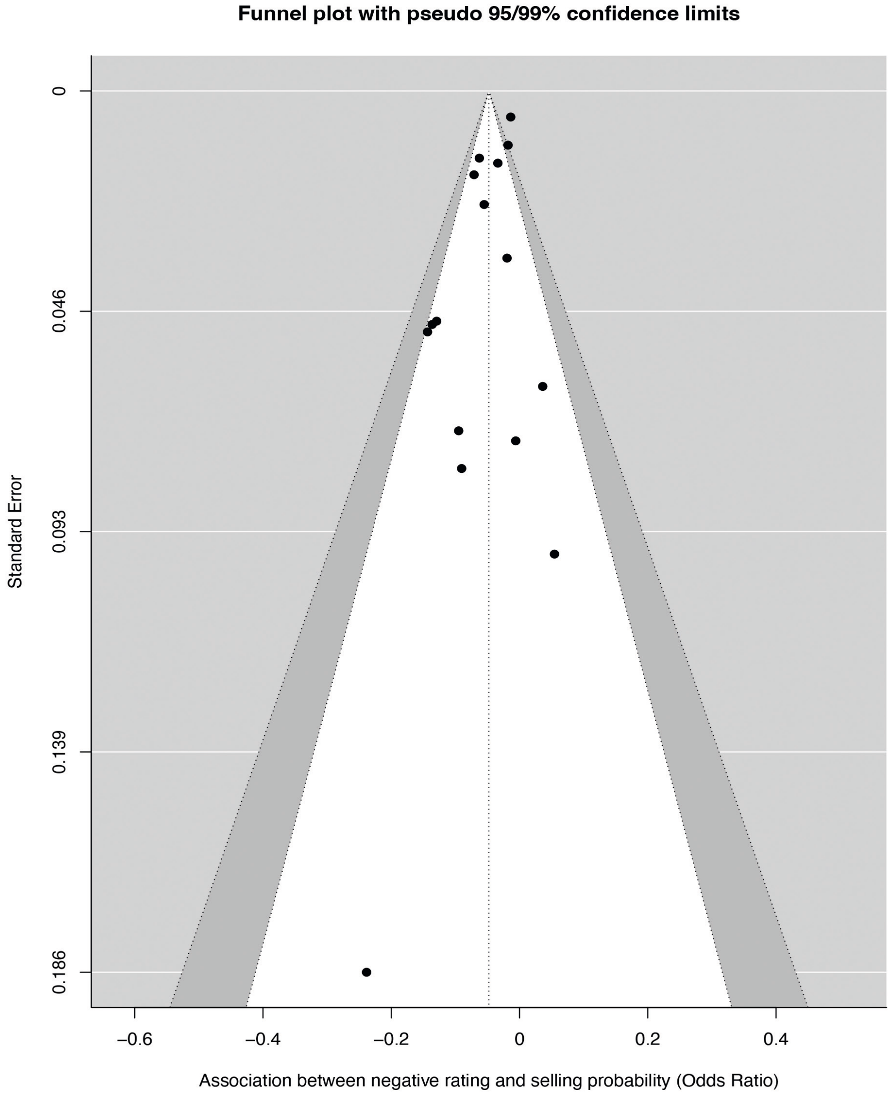

Figure B.9. Association between Negative Ratings and Selling Probability (Odds Ratio)

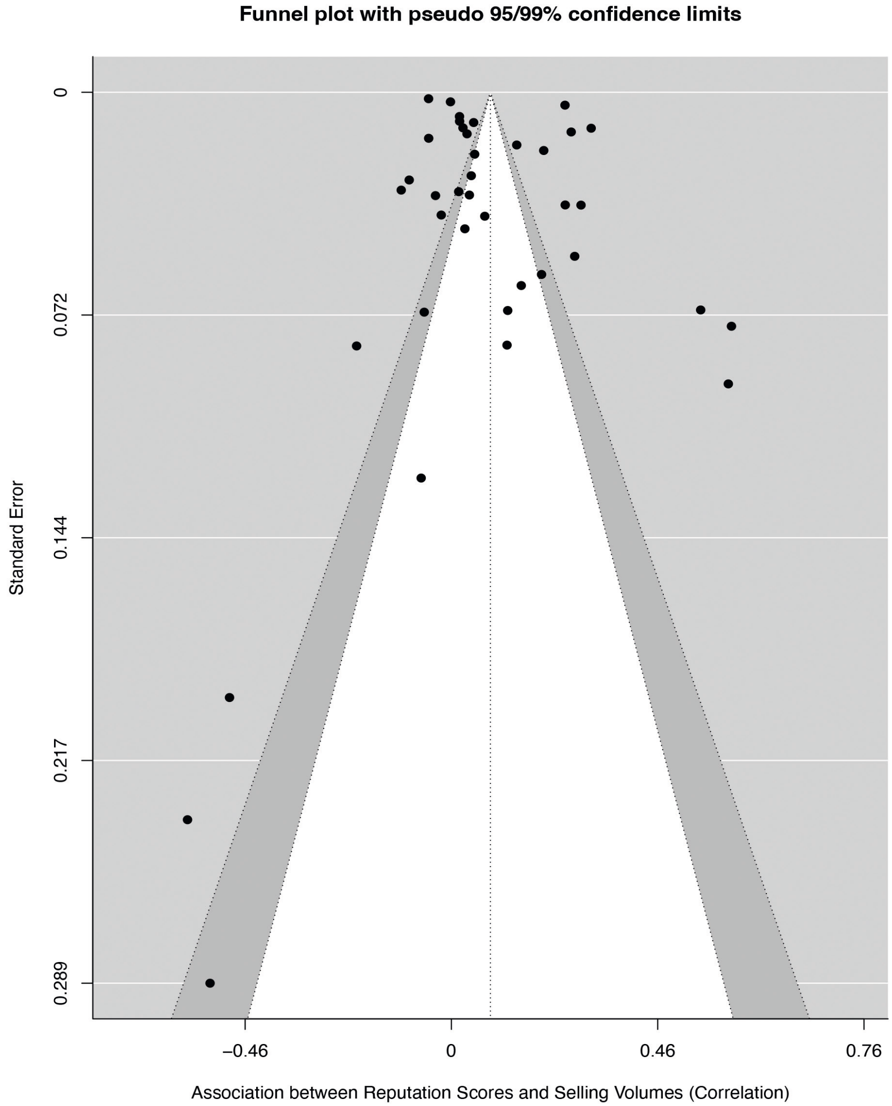

Figure B.10. Association between Reputation Scores and Selling Volumes (Correlation)

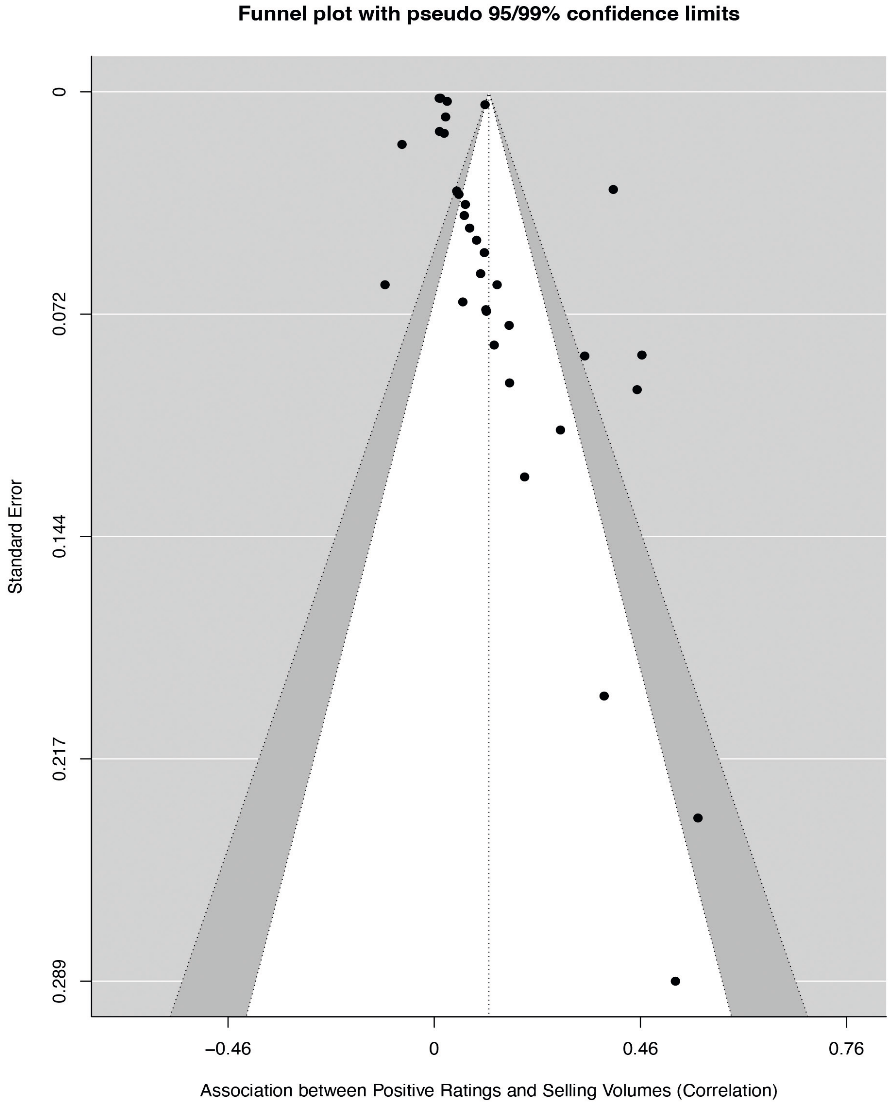

Figure B.11. Association between Positive Ratings and Selling Volumes (Correlation)

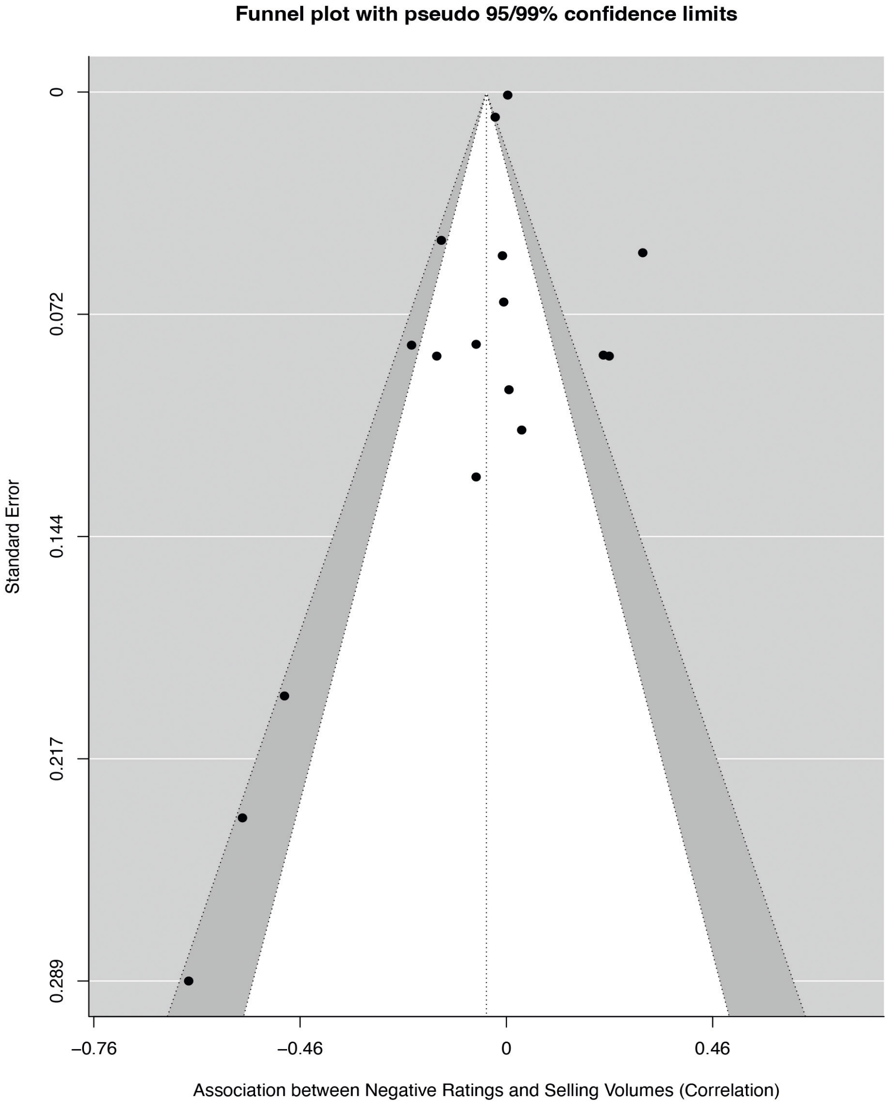

Figure B.12. Association between Negative Ratings and Selling Volumes (Correlation)

B.4. Identify publication bias with SE

Publication bias is assessed by adding SE to the meta-regression models M1~M4, i.e. M1a~M4a. If there is a publication bias, we should observe a significant positive effect of SE as a regressor, indicating that effect sizes are larger for studies with lower measurement precision. However, SE does not have a significant effect in any of these models, and adding this factor does not change the results of other factors. So statistically speaking, publication bias is not a major concern for these meta-regression models. Additionally, with M1b~M4b, we intend to correct for potential publication bias by adding SE^2, but it does not change the model results either.

Table B.2. Best-fitting model M1 (Subset 1) focusing on effects of positive ratings on selling price (N = 183; SE and SE^2 added for publication bias)

	M1		**M1a**		**M1b**	
	Coef.	**SE**	**Coef.**	**SE**	**Coef.**	**SE**
Const.	0.39***	0.05	0.39***	0.05	0.39***	0.05
SE			-0.01	0.01		
SE^2					-0.0004	0.0004
Product-related moderators						
Condition						
new	(ref.)		(ref.)		(ref.)	
used	-0.17***	0.04	-0.17***	0.04	-0.17***	0.04
unknown	-0.02	0.03	-0.01	0.03	-0.01	0.03
Avg. price	excl.		excl.		excl.	
Method-related moderators						
Multilevel	0.14**	0.04	0.13**	0.04	0.13**	0.04
Log (N)	-0.03***	0.01	-0.03***	0.01	-0.03***	0.01
Controls						
DV final price	-0.10**	0.03	-0.08**	0.03	-0.09**	0.03
IV reputation score	-0.05	0.03	-0.06*	0.03	-0.06*	0.03

Table B.3. Best-fitting model M2 (Subset 1) focusing on effects of positive ratings on selling price (N = 183; SE and SE^2 added for publication bias)

	M2		**M2a**		**M2b**	
	Coef.	**SE**	**Coef.**	**SE**	**Coef.**	**SE**
Const.	0.39***	0.05	0.39***	0.04	0.39***	0.05
SE	excl.		-0.01	0.01		
SE^2	excl.				-0.0004	0.0004
Contextual moderators						
Platform						
eBay	(ref.)		(ref.)		(ref.)	
Taobao	-0.04	0.04	-0.03	0.04	-0.03	0.04
Other	0.05	0.04	0.05	0.04	0.05	0.04
Product-related moderators						
Condition						
new	(ref.)		(ref.)		(ref.)	
used	-0.17***	0.04	-0.17***	0.04	-0.17***	0.04
unknown	-0.02	0.03	-0.01	0.03	-0.01	0.03
Method-related moderators						
Multilevel	0.13**	0.04	0.12**	0.04	0.13**	0.04
Log (N)	-0.03***	0.01	-0.03***	0.01	-0.03***	0.01
Controls						
DV final price	-0.10**	0.03	-0.08**	0.03	-0.09**	0.03
IV reputation score	-0.05	0.03	-0.06*	0.03	-0.06*	0.03

Table B.4. Best-fitting model M3 (Subset 1) focusing on effects of positive ratings on selling price (N = 183; SE and SE^2 added for publication bias)

	M3		M3a		M3b	
	Coef.	**SE**	**Coef.**	**SE**	**Coef.**	**SE**
Const.	0.39***	0.05	0.39***	0.05	0.39***	0.05
SE	excl.		-0.01	0.01		
SE^2	excl.				-0.0004	0.0004
Product-related moderators						
Condition						
new	(ref.)		(ref.)		(ref.)	
used	-0.19***	0.04	-0.19***	0.04	-0.19***	0.04
unknown	-0.03	0.03	-0.02	0.03	-0.02	0.03
Method-related moderators						
Percentage SR	0.06	0.04	0.05	0.03	0.06	0.03
Multilevel	0.15***	0.04	0.15**	0.04	0.15***	0.04
Log (N)	-0.03***	0.01	-0.03***	0.01	-0.03***	0.01
Controls						
DV final price	-0.10***	0.03	-0.09**	0.03	-0.09**	0.03

Table B.5. Best-fitting model M4 (Subset 2) focusing on effects of positive ratings on selling volume (N = 117; SE and SE^2 added for publication bias)

	M4		M4a		M4b	
	Coef.	**SE**	**Coef.**	**SE**	**Coef.**	**SE**
Const.	0.13***	0.04	0.13***	0.04	0.13***	0.04
SE	excl.		0.0001	0.0002		
SE^2	excl.				0	0
Contextual moderators						
Region						
USA	(ref.)		(ref.)		(ref.)	
China	0.11***	0.03	0.11***	0.03	0.11***	0.03
Europe	-0.07	0.05	-0.07	0.05	-0.07	0.05
Method-related moderators						
Percentage SR	-0.14***	0.05	-0.14***	0.05	-0.14***	0.04
Controls						
IV reputation score	-0.11**	0.03	-0.11**	0.03	-0.11**	0.03

C

APPENDIX

Supplementary material for Chapter 4

C.1. Experimental instructions (Experiment 1, condition π = 0.4)

Experimental Laboratory for Sociology and Economics

- General instructions -

Welcome and thank you for participating in this experiment. Please read the following instructions carefully. If you have any questions, please do not hesitate to raise your hand.

You are participating in an experiment, in which you can earn some money. How much you earn depends on the decisions you will make and the decisions other participants will make. The experiment lasts for about 75 minutes and consists of two parts. The two parts are not related to each other. You receive instructions on Part 1 here on these pages. You will receive the Part 2 instruction on the screen after Part 1 has ended.

Your total earnings will be the sum of what you earn in the two parts. Your earnings will be paid out to you in cash at the end of the experiment, and the other participants will not be able to see how much you earned. At the end of the experiment you will also be asked to fill in a questionnaire.

Note that in the experiment, there are no correct or incorrect decisions. Your decisions, and the decisions of the other participants remain anonymous and will not be linked to any information that would allow to identify you (for example your name). The results of this experiment serve a purely scientific purpose.

Please note that during the experiment you are not allowed to communicate with the other participants, and we kindly ask you to mute or switch off your phone and store it in your bag or coat.

Step-by-step description of Part 1 of the experiment

At the start of the Part 1, you will be randomly assigned the label "Person A" or "Person B", and you will be informed about your label. Please keep in mind that **your label remains the same until the end of Part 1.**

Part 1 consists of 20 to 40 rounds. You will not be told the exact number of rounds this part will last. In each round you will be randomly paired with another participant in the room. If you are Person A, you will be paired with a Person B, and vice versa. Each round consists of up to three consecutive steps.

Step 1. First, Person B chooses between Situation 1 and Situation 2 shown in Figure C.1. The two decision situations have the same decision options but different outcomes.

Step 2. Person A is presented with the decision situation chosen by Person B in Step 1 and asked to choose between RIGHT and DOWN. If Person A chooses RIGHT, Person A and Person B receive 40 points each in this round (irrespective of the decision situation) and the round ends after Step 2. Only if Person A chooses DOWN, Person B gets to make a choice in Step 3.

Step 3. Person B chooses between RIGHT and DOWN.

o ***If Person B chooses DOWN, the outcome depends on the situation Person B chose in Step 1:***

- ***In Situation 1, Person A receives 80 points and Person B 40 points.***
- ***In Situation 2, Person A and Person B receive 60 points each.***

o ***If Person B chooses RIGHT, the outcome depends on the situation Person B chose in Step 1:***

- ***In Situation 1, Person A and Person B receive 40 points each.***
- ***In Situation 2, Person A receives 20 points and Person B 80 points.***

At the end of every round, both Person A and Person B are informed about each other's choices, and the points they earned. **Important!** In every round in which Person B gets to make a choice in Step 3, Person B's decision is recorded with a certain probability and shown to Persons A in subsequent rounds. This is explained on the next page.

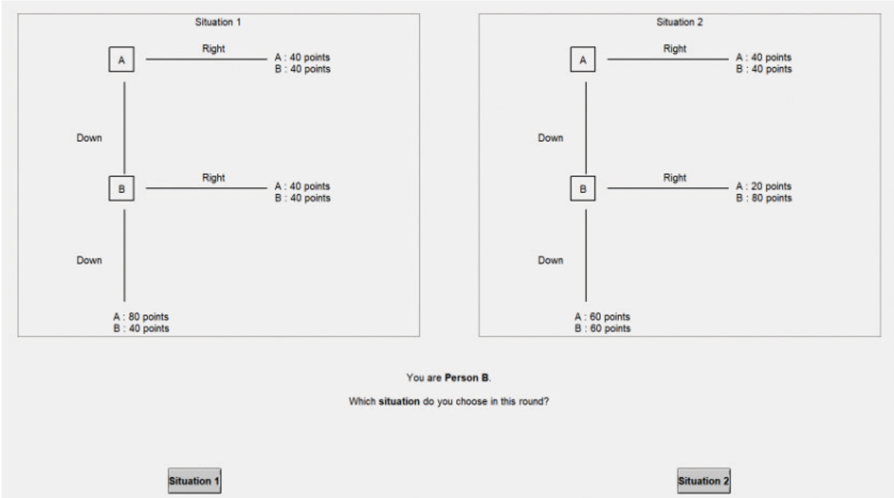

Figure C.1. Person B chooses one of two decision situations in Step 1

Information about Person B's decision in Step 3 in previous rounds
In every round in which a Person B gets to make a choice in Step 3, his or her decision (RIGHT or DOWN) is **recorded with a probability of 40%** (i.e. two out of five times).

- ***As long as Person B's choice in Step 3 is not recorded, Persons A with whom the Person B interacts in subsequent rounds will not be shown any information about the Person B's decisions in previous rounds.***
- ***When recorded, Person B's decision will be shown to the Persons A that this Person B interacts with in subsequent rounds, until it is overwritten by a new recording.***

Starting from round two, at the beginning of every round, both Person A and Person B are shown the same information about Person B's last recorded decision (if any).

Recall that your label as "Person A" or "Person B" stays the same until the end of Part 1, and you are randomly paired with a different participant in each round. This part runs for 20 to 40 rounds, and your payoff in each round will be added up and converted into monetary payment with a conversion rate that is **120 points = 1 euros**.

If you have read the Part 1 instructions carefully at least once, please proceed on the screen. There will be a quiz to test your understanding of the instructions. After the quiz, there will be an opportunity to ask questions. Thereafter, the experiment will start.

C.2. Sellers' decisions to give discounts (additional results, Experiment 1)

Table C.1. Proportion of sellers giving discounts across seller reputations and experimental conditions (Experiment 1)

	Bad reputation		No reputation		Good reputation	
	coef.	SE	coef.	SE	coef.	SE
$\pi = 0.2$	0.245**	0.093	0.045**	0.016	0.021*	0.010
$\pi = 0.4$	0.322***	0.073	0.090**	0.029	0.029	0.019
$\pi = 0.6$	0.420***	0.086	0.034	0.023	0.002	0.002
Observations	375		425		1024	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

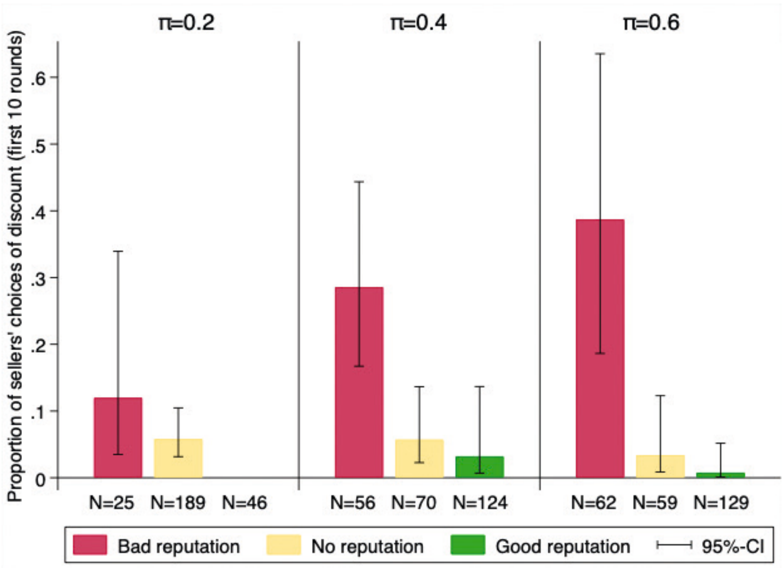

Figure C.2. Proportion of sellers giving discounts across experimental conditions and seller reputations in the first 10 rounds (Experiment 1)

C.3. Buyers' decisions to buy (additional results, Experiment 1)

Table C.2. Proportion of buyers deciding to buy across seller reputations and sellers' decisions to give a discount (Experiment 1)

	Bad reputation		No reputation		Good reputation	
	coef.	SE	coef.	SE	coef.	SE
Discount	0.857***	0.043	0.864***	0.075	0.882***	0.075
No discount	0.225***	0.045	0.605***	0.047	0.889***	0.020
Observations	375		425		1024	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

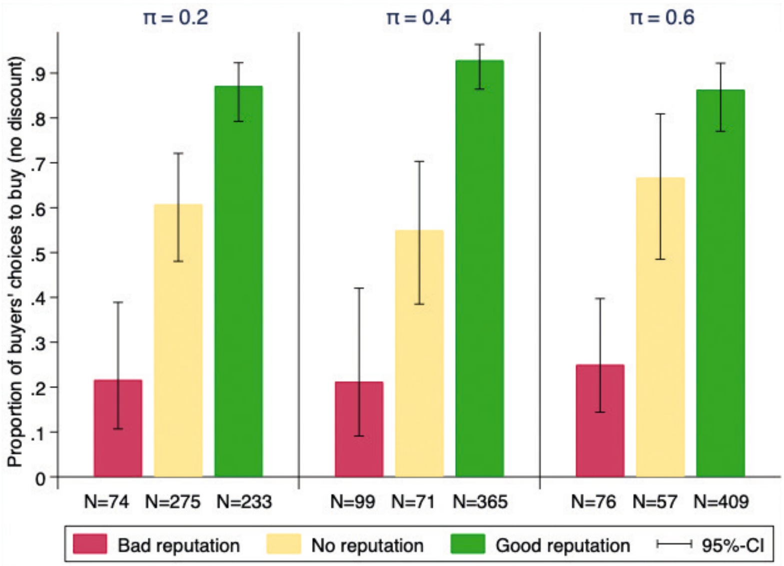

Figure C.3. Proportion of buyers deciding to buy across experimental conditions, seller reputations, and sellers' decisions to give a discount (Experiment 1)

C.4. Sellers' decisions to ship (additional results, Experiment 1)

Table C.3. Proportion of sellers deciding to ship across seller reputations and sellers' decisions to give a discount (Experiment 1)

	Bad reputation coef.	SE	No reputation coef.	SE	Good reputation coef.	SE
Discount	0.917***	0.036	0.632***	0.128	0.867***	0.088
No discount	0.625***	0.084	0.754***	0.058	0.821***	0.028
Observations	164		263		910	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

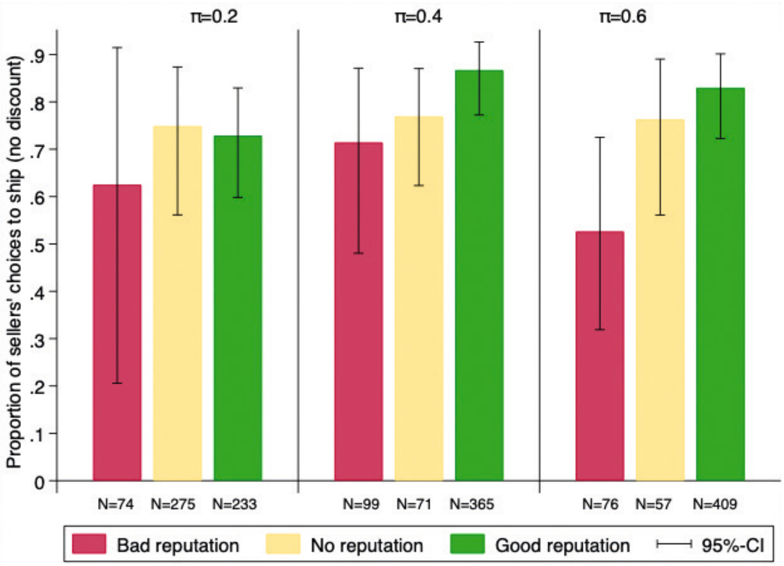

Figure C.4. Proportion of sellers deciding to ship across experimental conditions, seller reputations, and sellers' decisions to give a discount (Experiment 1)

C.5. Experimental instructions (Experiment 2, condition π = 0.4)

Experimental Laboratory for Sociology and Economics

- General instructions -

Welcome and thank you for participating in this experiment. Please read the following instructions carefully. If you have any questions, please do not hesitate to raise your hand.

You are participating in an experiment, in which you can earn some money. How much you earn depends on the decisions you will make and the decisions other participants will make. The experiment lasts for about 90 minutes and consists of two parts. The two parts are not related to each other. You receive instructions on Part 1 here on these pages. You will receive the Part 2 instructions on the screen after Part 1 has ended.

Your total earnings will be the sum of what you earn in the two parts. Your earnings will be paid out to you in cash at the end of the experiment, and the other participants will not be able to see how much you earned. At the end of the experiment you will also be asked to fill in a questionnaire.

Note that in the experiment, there are no correct or incorrect decisions. Your decisions, and the decisions of the other participants remain anonymous and will not be linked to any information that would allow to identify you (for example your name). The results of this experiment serve a purely scientific purpose.

Please note that during the experiment you are not allowed to communicate with the other participants, and we kindly ask you to mute or switch off your phone and store it in your bag or coat.

Step-by-step description of Part 1 of the experiment
At the start, you will be randomly assigned the label “Person A” or “Person B”, and you will be informed about your label. Please keep in mind that **your label remains the same until the end of Part 1.**

Part 1 consists of 20 to 40 rounds. You will not be told the exact number of rounds this part will last. In each round you will be randomly grouped with two other participants in the room. Each group of three consists of one Person A and two Person Bs. Each round consists of up to four consecutive steps.

Step 1. First, both Person Bs choose independently from each other between Situation 1 and Situation 2. The two decision situations have the same decision options but different outcomes (see Figure C.5 on the next page).

Step 2. Person A is presented with the decision situations chosen by both Person Bs in Step 1 and asked to choose whether to interact with Person B on the left or Person B on the right. For the Person B not chosen by Person A the round ends and this Person B receives **35 points**. The interaction between Person A and the chosen Person B continues in Step 3.

Step 3. Person A is asked to choose between RIGHT and DOWN. If Person A chooses RIGHT, Person A and Person B receive 40 points each in this round (irrespective of the decision situation) and the round ends after Step 2. Only if Person A chooses DOWN, can Person B make a choice in Step 4.

Step 4. Person B chooses between RIGHT and DOWN. The outcome depends on the situation Person B chose in Step 1:

o ***In Situation 1***
- ***If Person B chooses DOWN, Person A receives 80 points and Person B 40 points.***
- ***If Person B chooses RIGHT, Person A and Person B receive 40 points each.***

o ***In Situation 2***

- ***If Person B chooses DOWN, Person A and Person B receive 60 points each.***
- ***If Person B chooses RIGHT, Person A receives 20 points and Person B 80 points.***

At the end of every round, both Person A and Person B are informed about each other's choices, and the points they earned. **Important!** In every round in which Person B gets to make a choice in Step 4, Person B's decision is recorded with a certain probability and shown to Person A in subsequent rounds. This is explained on the next page.

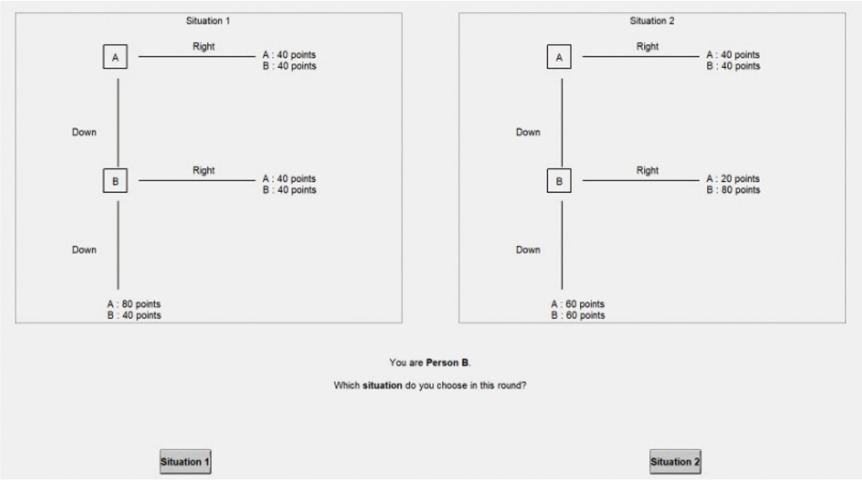

Figure C.5. In Step 1, both Person Bs independently choose one of two decision situations

Information about Person B's Step 4 decisions in previous rounds
In every round in which a Person B gets to make a choice in Step 4, his or her decision (RIGHT or DOWN) is **recorded with a probability of 40%** (i.e. two out of five times).

- ***As long as a Person B is not chosen by Person A or a Person B's choice in Step 4 is not recorded, Person A with whom the Person B interacts in subsequent rounds will not be shown any information about the Person B's decisions in previous rounds.***
- ***When chosen and recorded, Person B's decision will be shown to the Person A that this Person B interacts with in subsequent rounds. This Person B again has to make a choice and that decision is also (with a probability of 40%) recorded and replaces the previous recording.***

Starting from round 2, at the beginning of every round, both Person Bs are told what information is shown to Person A about both of them (see Figure 2 on the next page), and Person A is shown information about both Person B's last recorded decisions (if any, see Figure 3 on the next page).

Recall that your label as "Person A" or "Person B" stays the same until the end of Part 1, and you are randomly grouped with two other participants in each round. This part runs for 20 to 40 rounds, and your payoff in each round will be added up and converted into monetary payment with a conversion rate that is **70 points = 1 euros**.

If you have read the Part 1 instructions carefully at least once, please proceed on the screen. There will be a quiz to test your understanding of the instructions. After the quiz, there will be an opportunity to ask questions. Thereafter, the experiment will start.

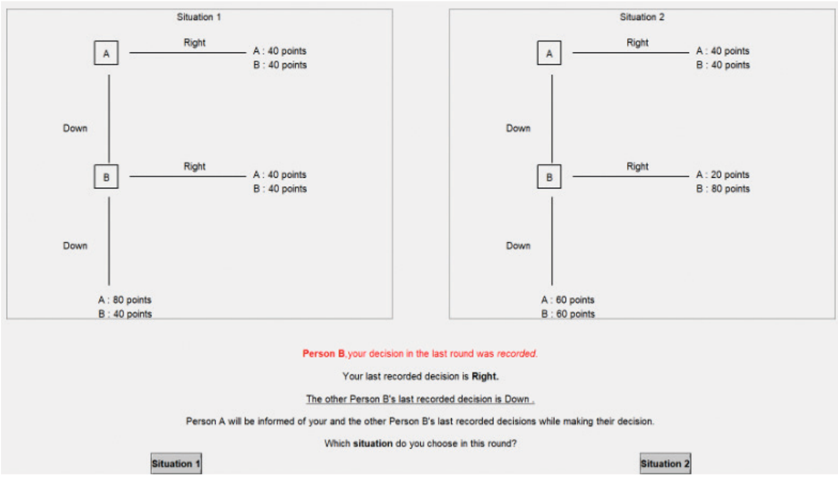

Figure C.6. In Step 1, Person B is shown the other Person B's information

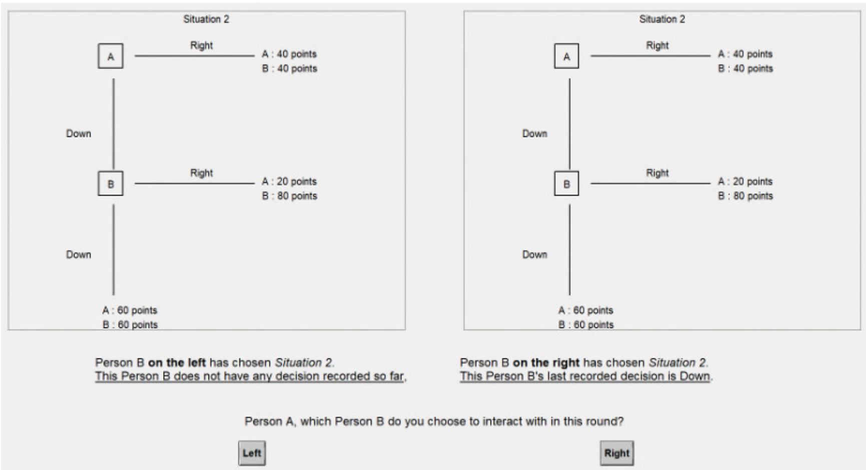

Figure C.7. In Step 2, Person A receives the information of both Person Bs

C.6. Sellers' decisions to give discounts (additional results, Experiment 2)

Table C.4. Proportion of sellers giving discounts across seller reputations and experimental conditions (Experiment 2)

	Bad reputation		**No reputation**		**Good reputation**	
	coef.	SE	coef.	SE	coef.	SE
π = 0.2	0.432**	0.157	0.348***	0.0543	0.156	0.0829
π = 0.4	0.517***	0.0788	0.325***	0.0441	0.191***	0.0495
π = 0.6	0.546***	0.0755	0.231***	0.0382	0.192***	0.0417
Observations	376		783		841	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

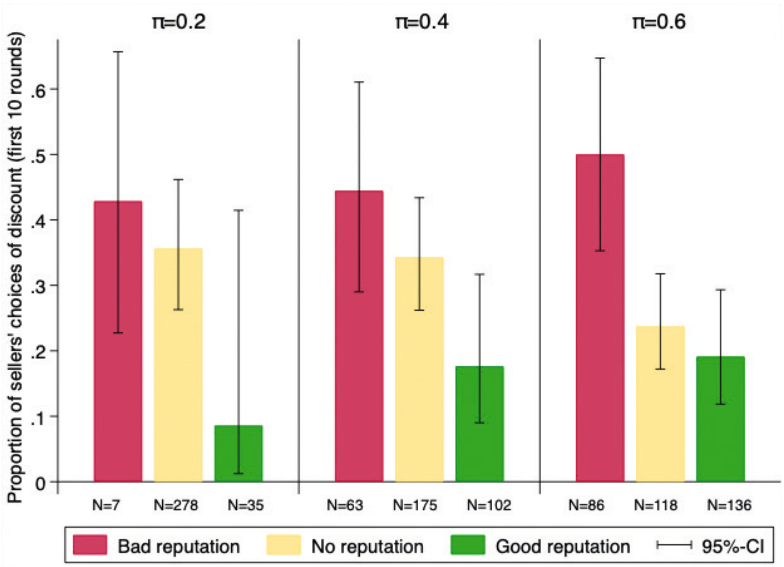

Figure C.8. Proportion of sellers giving discounts across experimental conditions and seller reputations in the first 10 rounds (Experiment 2)

C.7. Buyers' decisions to buy (additional results, Experiment 2)

Table C.5. Proportion of buyers deciding to buy across seller reputations and sellers' decisions to give a discount (Experiment 2)

	Bad reputation		No reputation		Good reputation	
	coef.	SE	coef.	SE	coef.	SE
Discount	0.977***	0.0124	0.974***	0.0156	0.992***	0.0077
No discount	0.333***	0.0891	0.562***	0.0728	0.792***	0.0459
Observations	171		386		443	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

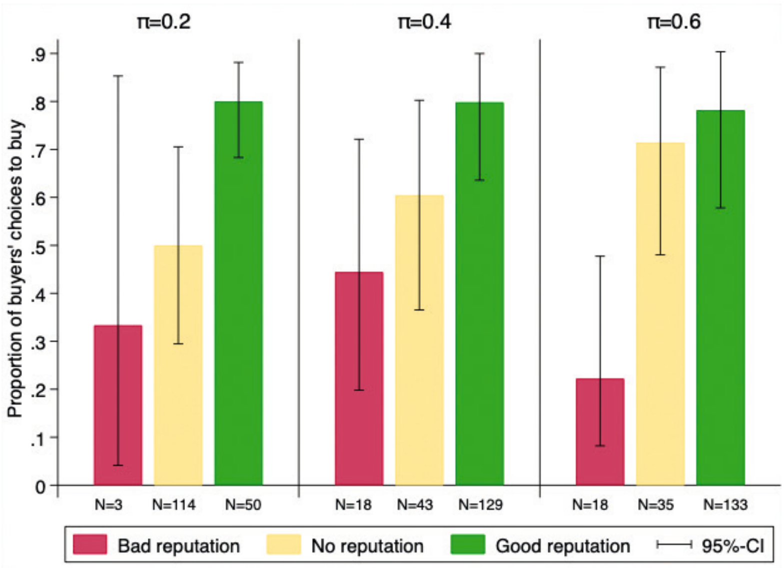

Figure C.9. Proportion of buyers deciding to buy across experimental conditions and seller reputations when sellers do not give discounts (Experiment 2)

C.8. Sellers' decisions to ship (additional results, Experiment 2)

Table C.6. Proportion of sellers deciding to ship across seller reputations and sellers' decisions to give a discount (Experiment 2)

	Bad reputation		No reputation		Good reputation	
	coef.	**SE**	**coef.**	**SE**	**coef.**	**SE**
Discount						
$\pi = 0.2$	0.800***	0.143	0.876***	0.0458	1	N.A.
$\pi = 0.4$	0.596***	0.170	0.800***	0.0627	0.925***	0.041
$\pi = 0.6$	0.569***	0.126	0.609***	0.112	0.897***	0.042
Observations	129		189		111	
No discount						
$\pi = 0.2$	0	N.A.	0.544***	0.124	0.700***	0.113
$\pi = 0.4$	0.125	0.115	0.615***	0.108	0.728***	0.070
$\pi = 0.6$	0.500	0.265	0.480***	0.130	0.606***	0.085
Observations	12		108		247	

*† p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001*

C.9. Exploratory analyses Experiment 2 (additional results)

Table C.7. Conditional logistic regression of buyers' choice of seller

	M7	
	coef.	**SE**
Seller reputation		
bad	-1.733***	0.279
none	-0.916***	0.185
good	(ref.)	
Sellers' decision		
discount	2.539***	0.222
no discount	(ref.)	
Observations	2000	
Pseudo R^2	0.251	

Standard errors adjusted for 50 clusters in buyer id.
*† $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$*
The model fit does not increase by including the interaction effect, so it is not reported.

Nederlandse samenvatting
中文梗概

Hoofdstuk 1. Synthese

Met de snelle ontwikkeling van internettechnologie heeft de opkomst van online markten drastisch veranderd waar, hoe en met wie we economische uitwisselingen doen. De veelgebruikte reputatiesystemen in online markten faciliteren het opbouwen van vertrouwen in een potentiële transactiepartner die vaak anoniem en geografisch ver weg is. Ze creëren kunstmatig een netwerk dat kopers en verkopers met elkaar verbindt door hen in staat te stellen elkaar te beoordelen en te evalueren op basis van hun transactie-ervaringen in het verleden, zodat opportunistisch gedrag (bijvoorbeeld een verkoper die misbruik maakt van het vertrouwen van een koper) minder verstandig of gunstig is met het oog op toekomstig transactiesucces.

Hoewel reputatiesystemen zijn ontworpen om vertrouwensproblemen op online marktplatforms op te lossen door gebruikers een manier te bieden om informatie te verzamelen over de betrouwbaarheid van potentiële transactiepartners, blijft het onduidelijk hoe de informatie over de reputatie van verkopers de verkoopprestaties beïnvloedt. Daarom synthetiseren we met behulp van meta-analyse het bewijs en identificeren we potentiële moderatoren uit meer dan honderd empirische onderzoeken naar het reputatie-effect in peer-to-peer online markten.

Als indicator van betrouwbaarheid spelen de feedbackratings van verkopers die worden gegenereerd in een reputatiesysteem een belangrijke rol in de online markt, en het verkrijgen van een goede reputatie is een belangrijke opstap voor het succes van verkopers in een markt. Feedbackratings in reputatiesystemen kunnen echter worden beschouwd als een publiek goed dat onvoldoende wordt aangeboden, omdat marktdeelnemers niet altijd gemotiveerd genoeg zijn om feedback achter te laten. Verder onderzoeken we hoe de reputatie van verkopers de besluitvorming van verkopers en kopers op diverse online markten met systematisch variërende feedbackpercentages beïnvloedt.

De bevindingen van dit proefschrift kunnen worden samengevat in drie hoofdpunten. Ten eerste bevestigden we opnieuw het algemene bestaan van reputatie-effecten tussen verkopersreputatie en verkoopprestaties. Ten tweede onderzochten we verschillende potentiële moderatoren om de variatie in reputatie-effecten te verklaren en identificeerden we factoren zoals contextuele regio, productconditie en enkele methode-gerelateerde kenmerken als belangrijke moderatoren. Tot slot stelden we dat reputatie-effecten negatief gerelateerd zijn aan feedbackpercentages, maar er werden geen significante verschillen in reputatie-effecten waargenomen bij variërende feedbackpercentages in onze experimenten, wat waarschijnlijk te wijten is aan hoge onvoorwaardelijke vertrouwensniveaus van de deelnemers in onze experimenten.

Hoofdstuk 2. Reputatie-effecten in peer-to-peer online markten: Een meta-analyse

Reputatiesystemen sturen online uitwisselingen door numerieke beoordelingen en tekstberichten te geven die de betrouwbaarheid van handelaren in eerdere uitwisselingen weergeven. Er is een groot aantal empirische onderzoeken gedaan naar het effect van verkopersreputatie op verkoopprestaties met behulp van digitale traceergegevens. Deze studies geven gemengde resultaten over het bestaan, de omvang en de interpretatie van het reputatie-effect. Om consensus te bereiken over de vraag of het reputatie-effect bestaat en wat het betekent, hebben we in hoofdstuk 2 een uitgebreide meta-analyse uitgevoerd van 378 gerapporteerde effectgroottes uit 107 empirische studies die 181 unieke datasets gebruikten. We hebben de reputatie van verkopers ingedeeld in drie categorieën: aantal positieve beoordelingen, aantal negatieve beoordelingen en totale reputatiescores. We categoriseerden ook de verkoopprestaties in vier soorten: de waarschijnlijkheid van verkoop, verkoopprijs, verkoophoeveelheid en de verhouding tussen verkoopprijs en referentieprijs. We verdeelden de gegevens in deze twaalf subsets, één voor elke combinatie van drie soorten variabelen voor de reputatie van de verkoper en vier soorten variabelen voor de verkoopprestaties, om de consistentie van het reputatie-effect aan te tonen bij verschillende operationaliseringen van de reputatie van de verkoper en de verkoopprestaties.

In het algemeen ondersteunden de bevindingen van de twaalf meta-analyses de hypothese dat een goede reputatie van de verkoper een positief effect heeft op de verkoopprestaties. Meer specifiek had het aantal positieve beoordelingen consistent en significant positieve effecten op alle soorten verkoopprestaties. Ter vergelijking, de algemene reputatiescore had een positief maar over het algemeen kleiner effect.

Deze studie bood waardevolle inzichten in het belang van verkopersreputatie in reputatiesystemen binnen P2P online markten en benadrukte het belang van het onderscheiden van verschillende metingen van verkopersreputatie en verkoopprestaties. Het onderzoek bevestigde het bestaan van reputatie-effecten op basis van meer dan honderd eerdere onderzoeken. Bovendien bleek uit het onderzoek dat reputatie-effecten een overmatige variatie vertoonden die niet alleen kon worden toegeschreven aan steekproeffouten. Deze bevindingen leidden tot de volgende studie, beschreven in hoofdstuk 3, waarin dieper werd ingegaan op de mogelijke factoren die de grote variatie in reputatie-effecten veroorzaken.

Hoofdstuk 3. Moderatoren van reputatie-effecten in peer-to-peer online markten: Een meta-analytische modelselectiebenadering

In hoofdstuk 3 voerden we een verkennende studie uit naar hoe de omvang van reputatie-effecten moet worden geïnterpreteerd in P2P online markten en probeerden we de mogelijke oorzaken van de variatie te identificeren. De verzamelde gegevens uit hoofdstuk 2 leverden niet alleen de omvang van reputatie-effecten op, maar bevatten ook een overvloed aan gegevens op het niveau van onderzoek, dataset en regressiemodellen. Met de voordelen van de gegevensverzameling onderzocht deze studie de modererende effecten van verschillende factoren, gecategoriseerd als (1) contextuele moderatoren, die verwezen naar culturele, ruimtelijk-temporele en institutionele inbedding van marktdeelnemers; (2) productgerelateerde moderatoren, zoals productprijs, conditie en populariteit; (3) methodegerelateerde moderatoren met betrekking tot gegevensverzameling, operationalisering en statistische modelleringskeuzes.

De resultaten toonden aan dat de variatie in waargenomen reputatie-effecten gedeeltelijk verklaard kon worden door deze moderatoren. Wat de contextuele moderatoren betreft, waren de reputatie-effecten aanzienlijk groter in de Chinese context dan in de Europese of Amerikaanse context. Wat betreft de productconditie vertoonde de reputatie van de verkoper belangrijkere effecten voor nieuwe producten dan voor gebruikte producten, wat tegen onze verwachtingen inging. Wat betreft de methode-gerelateerde moderatoren, hebben we geen significante modererende effecten waargenomen in de gehele dataset. Het onderzoek benadrukte ook dat we voorzichtig moeten zijn bij het interpreteren van de geïdentificeerde of niet-significante modererende effecten, omdat dit te wijten kan zijn aan de beperkte statistische kracht van de gegeven dataset.

Hoofdstuk 4. Het opbouwen van een reputatie van betrouwbaarheid: Experimenteel bewijs over de rol van het feedbackpercentage

Om beter te begrijpen hoe de grootte van reputatie-effecten tot stand komt, hebben we twee laboratoriumexperimenten uitgevoerd om te testen of de grootte van reputatie-effecten negatief gecorreleerd is met feedbackpercentages. We stellen dat in een markt met een hogere feedbackratio oneerlijke verkopers sneller worden uitgefilterd, zodat de transactiekosten voor verkopers zonder reputatie lager zullen zijn. Reputatiesystemen met een hoger percentage waarheidsgetrouwe feedback zijn dus effectiever dan systemen met een lager percentage feedback, ook al zullen de waargenomen reputatie-effecten kleiner zijn. Echter, als een essentieel kenmerk van het reputatiesysteem, wordt het percentage feedback dat wordt achtergelaten na elke transactie bijna nooit gerapporteerd als open informatie in online markten en kan daarom niet worden vastgelegd of verzameld in onderzoek met behulp van observationele gegevens. Daarom hebben we twee gecontroleerde laboratoriumexperimenten uitgevoerd waarin we online markttransacties emuleerden met vertrouwen op het spel en met verschillende feedbackpercentages (d.w.z. 20%, 40% en 60%). Met behulp van het vertrouwensspel met onvolledige informatie hebben we voornamelijk (1) het gedrag van verkopers gemeten, d.w.z. of ze korting aanbieden en of ze betrouwbaar zijn terwijl ze worden vertrouwd, (2) het gedrag van kopers, d.w.z. met welke verkoper ze een transactie aangaan (alleen voor het tweede experiment, waarbij kopers de mogelijkheid hebben om een van de twee gematchte verkopers te kiezen) en of ze de verkoper vertrouwen. De basisopstellingen van de twee experimenten waren vergelijkbaar, maar in het tweede experiment kwam ook het concurrentiemechanisme tussen verkopers aan bod. Dat wil zeggen dat elke koper in elke ronde aan twee potentiële verkopers werd gekoppeld. Op deze manier kregen kopers de kans om een van de verkopers te kiezen op basis van de reputatie van de verkopers en hun bereidheid om kortingen aan te bieden.

In deze studie onderzochten we de verschillen in het reputatie-opbouwende gedrag van verkopers (d.w.z. of ze korting geven als ze geen goede reputatie hebben) en betrouwbaarheid, evenals het vertrouwen van kopers op markten met verschillende feedbackpercentages. Over het algemeen bevestigden onze bevindingen de werking van het reputatiemechanisme. Dat wil zeggen dat verkopers meer geneigd waren om kortingen aan te bieden als ze geen goede reputatie hadden. Het reputatie-effect was vooral substantieel in het tweede experiment, waar verkopers met elkaar concurreerden. Ook werden verkopers die een goede reputatie hadden of kortingen aanboden vaker vertrouwd of gekozen door de kopers. We zagen echter geen significante verschillen tussen verschillende feedbackpercentages om onze hypothese te ondersteunen dat een hoger feedbackpercentage verkopers significant minder geneigd maakt om kortingen te geven. Met een verkennende analyse stelden we dat dit te wijten zou kunnen zijn aan het bestaan van onvoorwaardelijk vertrouwen in de markt. Als verkopers die geen goede reputatie hebben een hoge mate van onvoorwaardelijk vertrouwen van kopers kennen of hebben ervaren, zouden ze niet voldoende gemotiveerd zijn om kortingen aan te bieden.

第一章 综述

随着互联网技术的飞速发展，线上交易平台的兴起极大地改变了我们进行经济交换的地点、方式和对象。线上交易平台中常用的信用评价体系有助于建立对潜在交易伙伴的信任，而这些潜在交易伙伴往往是匿名的，而且地理位置遥远。信用评价体系人为地建立了一个连接买卖双方的网络，允许买卖双方根据以往的交易经验对彼此进行评分和评价，从而使机会主义行为（如卖方滥用买方的信任）变得不那么明智或不利于未来交易的成功。

虽然信用评价体系旨在解决线上交易平台中的信任问题，为用户提供了收集潜在交易伙伴可信度信息的途径，但卖家的信用信息如何影响销售业绩仍不明确。因此，借助元分析的方法，我们综合了百余项调查以个人用户为主体的在线交易平台声誉效应的实证研究中的证据，并找出了潜在的调节因素。

作为衡量信誉的指标，信誉评价体系中产生的卖家反馈评级在网上交易平台中发挥着重要作用，获得良好的信誉是卖家在市场中取得成功的重要垫脚石。然而，信誉评价体系中的反馈评级可以被视为一种公共资源，但由于市场参与者并不总是有足够的动力留下反馈，因此这种公共资源的供给不足。此外，我们还研究了在反馈率系统性变化的多样化线上交易平台中，卖家信誉究竟会如何影响卖家和买家的决策。

本论文的研究结果可归纳为三点。首先，我们再次证实了卖家信誉与销售业绩之间普遍存在信誉效应。其次，我们探索了各种潜在的调节因素来解释信誉效应的变化，并确定了诸如所在地区、产品状况和研究方法相关特征等因素是重要的调节因素。最后，我们认为信誉效应与反馈率呈负相关，但在我们的实验中，并没有观察到信誉效应在不同的反馈率上的显著差异，这可能是由于我们实验中参与者的无条件信任水平较高。

第二章 个人线上交易平台的信誉效应： 元分析

信用评价体系通过提供量化评级和文本信息来反映交易者在以往交易中的信誉，从而对在线交易进行管理。已有大量实证研究利用数字追踪数据调查卖家信誉对销售业绩的影响。这些研究对信誉效应的存在、程度和解释提供了不同的结果。为了帮助就信誉效应是否存在及其意义达成共识，我们在第二章中对 107 项实证研究中的 378 个报告效应大小进行了全面的元分析，这些研究使用了 181 个独特的数据集。我们将卖家信誉分为三类：正面评价数量、负面评价数量和总体信誉得

分。我们还将销售业绩分为四类：销售概率、销售价格、销售数量以及销售价格与参考价格的比率。我们将数据分为这十二个子集，三种类型的卖家信誉变量和四种类型的销售业绩变量的组合各一个，以证明信誉效应在卖家信誉和销售业绩的不同操作化中的一致性。

总体而言，12 项元分析的结果都支持卖方良好信誉会对销售业绩产生积极影响的假设。具体来说，正面评价的数量对所有类型的销售业绩都有持续且显著的积极影响。相比之下，总体信誉得分具有积极影响，但一般较小。

这项研究为卖家信誉在 P2P 线上交易平台信誉评价体系中的重要性提供了宝贵的见解，强调了区分卖家信誉和销售业绩的不同衡量标准的重要性。它从之前的百余项研究中确立了信誉效应的存在。此外，该研究还发现，信誉效应呈现出的变化不能仅仅归因于抽样误差。这些发现促成了第三章所述的后续研究，该研究深入探讨了造成信誉效应巨大差异的潜在因素。

第三章 个人线上交易平台中信誉效应的调节因素：元分析模型选择方法

在第三章中，我们运用第二章收集的数据对在 P2P 线上交易平台中应如何解释信誉效应的大小进行了探索性研究，并试图找出造成这种差异的潜在原因。这一丰富的数据不仅提供了信誉效应的大小，还包括研究、数据集和回归模型等层面。借助数据收集的优势，本研究探讨了各种因素的调节作用，包括：（1）背景因素，即市场参与者的文化、时空和制度嵌入；（2）与产品相关的因素，如产品价格、状况和受欢迎程度；（3）研究方法相关的因素，即数据收集、操作和统计模型选择。

结果表明，研究者所观察到的信誉效应的变化可以部分地由这些调节因素解释。在背景因素方面，中国的信誉效应远远大于欧美背景下的信誉效应。在产品条件方面，卖方信誉对新产品的影响比对二手产品的影响更大，这与我们的预期相反。至于与研究方法相关的调节因素，我们在整个数据集中没有观察到显著的调节作用。研究还强调，我们在阐释已识别或不显著的调节效应时应谨慎，因为这可能受限于给定数据集的统计能力。

此外，在我们的理论框架内，我们假设还有其他因素，如真实反馈的比例，会影响信誉效应的大小。然而，研究者可获取到市场层面反馈率信息的观察数据太少，无法可靠地估计这种效应。因此，我们在下一章节借助实验的方法来研究在线交易平台决策环境中反馈率的变化对信誉效应大小的影响。

第四章 塑造信誉：信用反馈率作用的实验证据

为了进一步了解信誉效应的大小是如何产生的，我们进行了两个实验，以检验信誉效应大小是否与反馈率呈负相关。我们认为，在反馈率较高的市场中，不诚实的卖家会更快地被过滤掉，因此没有信誉的卖家的交易成本会更低。因此，真实反馈率较高的信誉评价体系比反馈率较低的信誉评价体系更有效，即使研究者所观察到的信誉效应会小一些。然而，作为信誉评价体系的一个基本属性，每次交易后留下反馈的比例在网上交易平台几乎从未作为公开信息被报告过，这一信息无法在使用观察性研究中被捕捉到。因此，我们进行了两次实验室实验，模拟了以信任为筹码、反馈率各不相同（即 20%、40% 和 60%）的线上交易平台交易。 利用不完全信息的信任博弈模型，我们主要测量了（1）卖方的行为，即是否提供折扣以及是否在被信任的同时也值得信任；（2）买方的行为，即与哪个卖方交易（在第二个实验中，买方有机会从匹配的两个卖方中选择一个）以及是否信任卖方。两个实验的基本设置相似，但第二个实验引入了卖家之间的竞争机制。也就是说，每个买方在每一轮都与两个潜在卖方配对，这样买家就有机会根据卖家的信誉和提供折扣的意愿选择其中一个卖家。

在这项研究中，我们调查了在反馈率不同的市场中，卖家塑造良好信誉的行为（即在信誉不佳时是否提供折扣），值得信赖的程度，以及买家的信任度的差异。总体而言，我们的研究结果再次证实了信誉机制的作用。也就是说，当卖家缺乏良好信誉时，他们更倾向于提供折扣。在卖家相互竞争的第二个实验中，信誉效应尤为显著。此外，信誉好或提供折扣的卖家更容易得到买家的信任或选择。然而，我们并没有观察到不同反馈率之间的显著差异来支持我们的假设，即较高的反馈率会显著降低卖家提供折扣的倾向。通过探索性分析，我们认为这可能是由于市场中存在无条件信任。如果信誉不佳的卖家意识到或经历过买家的高度无条件信任，他们就不会有足够的动力提供折扣。

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About the author

Ruohuang Jiao was born in Dalian, China, on May 30, 1992. In 2014, she obtained her bachelor's degree in sociology at Renmin University of China. She completed the research master's study in sociology and social research in 2017 at Utrecht University. In the same year, she started working as a Ph.D Candidate at the Interuniversity Centre for Social Science Theory and Methodology (ICS) at the Department of Sociology at Utrecht University. She wrote her dissertation under the supervision of prof. dr. ir. Vincent Buskens (Department of Sociology, Utrecht University) and dr. Wojtek Przepiorka (Department of Sociology, Utrecht University). Her research interests include trust and cooperative relations, game theory, meta-analysis, and experimental sociology.

Peer-reviewed publications

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Working papers

Jiao, R., Przepiorka, W., & Buskens, V. (2023). Building a reputation for trustworthiness: Experimental evidence on the role of the feedback rate. under review

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