Experimental Evidence on the Role of Outside Obligations in Wage Negotiations[Ψ]

Shakun D. Mago[‡]

University of Richmond

Jennifer Pate[†]

Loyola Marymount University

Laura Razzolini[◊]

University of Alabama

Abstract: We examine how sharing information about outside obligations impacts wage negotiations. Consider an 'employee' with an outside obligation, whose performance determines the surplus and an 'employer' with the power-to-give, who determines the employee's wage. We find that wage offers increase with obligation amounts, when the level of obligation is known. However, the employer simply redistributes surplus from employees with no obligations to those with higher obligations. We find no evidence of gender bias in wage offers similar to other ultimatum games. Our experiment provides a potential explanation for some of the gender wage gap, and shows how seemingly equitable policies may perpetuate inequities among employees.

Key Words: Ultimatum games; workplace negotiations; outside obligations; organizational inequities.

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[‡] Corresponding Author: Robins School of Business, Queally 380, 1 Gateway Road, University of Richmond, Richmond, VA 23173. Phone: 804-287-6631. E-mail: sdatta@richmond.edu.

[†] Department of Economics, 1 LMU Drive, Suite 4200, Loyola Marymount University, Los Angeles, CA 90045. Phone: 310-338-1887. E-mail: Jennifer.Pate@lmu.edu.

[◊] Department of Economics, Finance and Legal Studies, 361 Stadium Drive, Tuscaloosa, AL 35487. Phone: 205-348-8067. Email: lrazzolini@cba.ua.edu.

1. Introduction

In March 2020, in response to a global pandemic, Facebook created a series of initiatives to aid its employees during the crisis. Among these initiatives was an emergency care leave program that offered up to 10 weeks of paid leave to all employees dealing with children at home or taking care of a sick relative. Other tech giants like Google and Microsoft extended similar emergency paid leave to their employees. These types of aid policies stand in stark contrast to the pre-employment wage negotiations, where the employer is prohibited from asking non-job-related questions about outside obligations, such as marital status, number and/or age of children or dependents, etc.[1]

In this experimental study, we examine how an employer's knowledge of an employee's outside obligations affects the wage negotiation process. We model the context of the employer/employee relationship using a modified version of the ultimatum game with a real-effort task and a multi-stage negotiation that mirrors the situational context of two scenarios. The first scenario is one where employers can observe an employee's level of outside obligation and apply differential pay based on this information. This captures the parental leave policy described above. The second scenario is one where the employer has no information about the employee's outside obligations and, thus, has no direct ability to act on this information. Given the wide preponderance of gender bias in wage negotiations, we also incorporate a matching protocol that allows us to test for gender bias within negotiations under any configuration of same and opposite-gender matching in both employer and employee roles.

We present the negotiation/remuneration process as an ultimatum game with a real-effort task. This is based on García-Gallego et al. (2008, 2012) and Ruffle (1998), who argue that the most obvious real-world example of asymmetric negotiations that underlie ultimatum game framework can be found in employer-employee wage negotiations, where the surplus is not a "manna from heaven," but is earned by the employee. The proposer/employer is in a position of authority and can propose the division of surplus by making a take-it-or-leave-it offer, but ultimately it is the responder/employee who provides the labor to generate the surplus. Thus, wages offered by employers and accepted by employees should reflect on each agents' contribution to the common enterprise. Costly, pre-play labor to produce surplus generates a notion of entitlement that can influence decisions over distributions (Cherry et al., 2002, Oxoby and Spraggon, 2008, and Korenok et al., 2017). Supporting this argument, García-Gallego et al. (2008) find that when the

[1] The U.S. Equal Employment Opportunity Commission states that the following pre-employment inquiries may be regarded as evidence of intent to discriminate when asked in the pre-employment context: (a) whether applicant is pregnant (b) marital status of applicant or whether applicant plans to marry (c) number and age of children or future child bearing plans (d) child care arrangements (e) employment status of spouse (f) name of spouse. However, such inquiries may be made after an employment offer has been made and accepted, if needed for insurance or other legitimate business purposes.

task to be performed by the employee involves real effort, employers post higher offers and employees are more likely to reject a given offer.

In this extension, we argue that outside obligations further raise the employee's cost of participation in the real effort task, and therefore, should affect both the aspired and actual distribution of the pie. Our experimental design incorporates an outside obligation, which automatically reduces only the responder's final payoff by a given percentage. In the experiment, we consider three levels of obligations, each equally likely to occur – 0, 25, and 50 percent of earnings, and these percentage amounts are automatically deducted from the employee's earnings.[2] Our experiment proceeds as follows: upon learning the level of outside obligation, and before performing the real effort task, the employee suggests a possible distribution of the surplus (a 'wage ask'). Next, the proposer/employer sends back a 'wage offer' (which may differ from the wage ask). The responder/employee can accept or reject the offer in the usual fashion. Accepting the offer commits the employee to performing the real effort task – moving a number of randomly placed icons along a slider line to the extreme right which generates the group surplus. If the employee rejects the offer, there is no group surplus and both players complete the task themselves in order to receive a minimum level of earnings. The employees must pay their outside obligations irrespective of whether they accept the offer or not.

Note that we assume that the employees' productivity is not affected by the presence or the level of outside obligation to avoid the issue of skill-based wage discrimination.[3] We also do not model the nature or source of outside obligation because subjects may ascribe different subjective valuation to various types of obligations (child care costs vs. college loans vs. credit card loans etc.). Our focus is on how the level of employee's outside obligation affects the employer wage decision. We do not examine why outside obligations occur and how they may be perceived by the employers. While the antecedent of an obligation can certainly affect the employer response in the real world, in the laboratory setting this would mean significant loss of experimenter control. Instead, without postulating the nature of outside obligation, we simply examine whether knowledge (or lack thereof) of the employee's outside obligations affects the employer's wage offer. Players in the experiment know that the outside obligations are exogenously

[2] This is similar to the structure of Kerschbamer and Kirchsteiger (2000) who incorporate a tax that is sometimes on the proposer and other times on the responder to analyze tax equivalence and burden, although framed as an insurance environment and distinctly different from our design in a variety of ways.

[3] Although evidence of (unfounded) bias against working mothers exists in the workplace (Correll et al. 2007), there is little evidence of lower productivity in working parents (Krapf et al. 2017). In fact, a recent study found working parents to be more productive than employees without children during the pandemic (Valoir 2020). We avoid the question of skill-based differential pay by normalizing productivity across all employees, regardless of outside obligation.

determined and cannot be altered at the discretion of the employee. We use proportional obligation because the dollar amount that an individual spends on outside obligations is dependent on one's income.[4]

Our work contributes to two related segments of experimental literature on ultimatum games: (a) variants of the game with asymmetric information and (b) variants of the game where the endowment is earned by the responder. There is a vast literature on ultimatum games where the asymmetric information is about the *size* of the pie (or surplus), and the general finding is that subjects opportunistically take advantage of their extra information, though not to the extent predicted by theory with self-regarding players (Camerer, 2003; Güth et al,1996; Besancenot et al., 2013; Klempt et al., 2019). In contrast, games 'where one player has private information about the proposed *distribution* of a surplus whose *size* is common knowledge have been the subject of much less theoretical and experimental study' (Anbarci et al., 2015, pg. 346). They argue that 'these games model interactions where one side has an informational advantage regarding the ultimate transaction price and deserve attention in their own right.' By incorporating outside obligations, our experiment fills this gap in the literature. Furthermore, while Anbarci et al. (2015) assume that proposers have private information about the proposed distribution of the surplus, we examine the opposite case where the responders possess this information. The central question remains the same across the two studies: how do agents respond to this asymmetric information? ~~To the best of our knowledge, outside obligations have not be explored in the ultimatum game literature.~~

An important feature of our design is that the surplus is generated by only one agent. This is in stark contrast to a majority of ultimatum game experiments, where individuals either bargain over a windfall endowment (the first iteration by Güth et al., 1982, summarized in Roth, 1995, and, more recently, in Larney et al., 2019) or over a surplus that is jointly produced (e.g. Rodriguez-Lara, 2016, Bolton and Karagozoglu, 2016, Fischbacher et al., 2017, and Ridinger, 2018). In case of windfall endowment, the stable norm observed is equality of payoffs (a 50/50 split) or a split of the surplus slightly more favorable to the proposer. But when both players earn the money to be divided, the modal outcome of the game stops being the equal

[4] This is a stylized argument, but one that we believe is appropriate given our research question. We also note that the exogenous nature of obligations can be applied to a wide range of settings. For instance, while the decision to have children and the number of children are choice variables, child care costs become an exogenous outside obligation for all employees who are parents. It is straightforward to envision that the expense to raise a child is an external obligation whose cost relates to one's income, as indicated by wealthier parents' greater willingness to spend larger amounts on education, clothing and apparel, and entertainment. 2019 SIPP data reported by BLS notes that among working families with children under age 5 that pay for child care, average child care spending amounts to nearly 10 percent of the average family income but high income families use licensed child care—which is likely to be safer and of higher quality—much more frequently than lower-income families and therefore, spend more. Kornrich and Furstenber (2013) state that "differences in monetary expenditure that make up a substantial portion of the advantage conferred by parents with higher income: spending buys access to higher-quality child care and education, and places children in environments that are more likely to build human and cultural capital" (p. 2). Similarly, alimony payments are based on a percentage of one's income.

split and the final allocation tends to proportionally reward each players' contribution to the pie, i.e., equity rather than equality. For example, Barber and English (2019) find that 48% of the low earners propose keeping less than 50% of the pie for themselves.[5] To the best of our knowledge, there are only two other studies that examine real-effort endowment generation in ultimatum games. Ruffle (1998) and Carr and Mellizo (2017) examine the sensitivity of rejection rates in the ultimatum game to the source of endowment – whether exogenously bestowed by the experimenter or endogenously generated by the responder. Ruffle (1998) reports low rejection rate overall, and no change in rejections across the two endowment treatments. Carr and Mellizo (2017), on the other hand, find that proposers' offered amounts increase and responders' rejection rates decrease when the responders produce the endowment.

In our framework, the employee is (a) the only player performing the real effort task to generate the group surplus and (b) the only player who faces the possibility of outside obligations that can reduce the final payoff. Thus, both equity and equality norms suggest that employees should get a favorable share of the pie and that this share ought to increase with their outside obligations. Using a simple theoretical framework derived from Fehr and Schmidt (1999), we show that fairness considerations in an ultimatum game with outside obligations can yield a more favorable outcome for the responder.

Finally, note that the power-to-give rests with the employer and all the employee can do is 'ask' for a fair split. That is, wage ask is cheap talk and strategically irrelevant in a theoretic analysis of the ultimatum game. Nevertheless, its ability to influence employers' belief and, thereby, the wage offers is well documented in the literature (e.g. Rigdon, 2012). In our design, when the employer knows the employee's outside obligation, the wage ask is a non-binding suggestion about how to split the surplus; and when the employer does not have that information it may serve as a signaling mechanism of the outside obligation level.

In summary, our ultimatum game design contains a unique combination of the following features– surplus generation by the employees alone using a real effort task, wage asks by employees, and the novel addition of an outside obligation imposed on the employee that generates asymmetric information about the distribution of the surplus. This design provides us with a contextual framework to analyze some of the interesting facets of employer-employee wage negotiations. In our analysis, we focus on how an employee's wage ask may vary depending on the outside obligation, and on whether the employer can

[5] Furthermore, prior bargaining studies have found that subjects show a self-serving bias when fairness norms are in conflict. Bediou et al. (2012) find that proposers offer equal split if they produced less than half the surplus and equitable split if they produced greater than half. Other such studies include Rodriguez-Lara and Moreno-Garrido (2012), Cappelen et al. (2007), Rodriguez-Lara (2016) and Hennig-Schmidt et al. (2013). The possibility of earning the endowment has also been extensively studied in dictator games. Studies such as Cherry et al. (2002), Oxoby and Spraggon (2008) and Korenok et al. (2017) demonstrate that making the participants earn the endowment greatly affects the generosity of dictators.

directly observe this obligation; and similarly, whether the employer's wage offer is influenced by the wage ask and the knowledge of the employee's outside obligation.

One important aspect to consider is the role that gender could play in these kinds of negotiations. It is well known that gendered expectations, bias, and discrimination often result in significant economic and social penalties for women in the workplace. In the U.S., there is a persistent gender wage gap, as women are paid around 80 cents for every dollar paid to men, all else equal. Some of this wage gap results from outright gender discrimination, which we can identify in our experiment by comparing offers of employers to employees of different genders, keeping all other factors constant.[6]

The addition of an outside obligation provides another avenue for gender bias to enter the equation. Waldfogel (1998a, 1998b) found that 40 to 50 percent of the gender wage gap could be explained by the differential impact of parental and marital status on men's and women's earnings. Hodges and Budig (2010) find that, all else equal, parenthood increases per-child earnings of men by over 6% and decreases the per-child earnings of women by 4%. Often described as "fatherhood bonus" or, inversely, as "motherhood penalty" (Budig and England, 2001), these findings suggest that the impact of outside obligations could vary by gender.[7] Budig (2014) argues that framing parenthood as a "choice," and one for which parents alone must accept its consequences, is an inadequate response to the known effects of gendered parenthood on wages. We examine in this context the extent to which others in society (i.e., employers) acknowledge the added burden of these obligations. Our design framework allows for employers to provide their own resources to support a more equitable distribution. The careful construction of employer/employee matching in our experiment and the use of a highly simplistic and gender-neutral task control for concerns about employees' quality to avoid skill-based gender discrimination.[8]

[6] Evidence from the lab generally confirms this. For instance, Rigdon (2012) consider an ultimatum game in which the responder can propose a wage prior receiving an offer from the proposer. In her design, subjects did not know the gender of the subject they were matched with. She found that women make significantly lower asks and, in return, receive lower offers. However, when social information about previous bargaining outcomes is made available, the gap between men and women's asks and offers is reduced.

[7] Analyzing 2013 BLS data, Budig (2014) finds that the smallest gender wage gap is found among unmarried men and women: Unmarried women earn 96 cents to an unmarried man's dollar, and childless women (including married and unmarried) earn 93 cents on a childless man's dollar. In contrast, wives and mothers fare far less well. Even among full-time workers, married mothers with at least one child under age 18 earn 76 cents on a married father's dollar. Single mothers earn 83.1 cents to a single custodial father's dollar (that single moms are much less likely to be employed full-time relative to single dads is masked by this estimate among full-time workers). These figures show that married mothers of minor children experience the largest wage gaps. This data robustly supports the "lifecycle effect" where the gender wage gap widens within the cohorts, as they age and are exposed to life processes that affect earnings. See also, Budig and Hodges (2010).

[8] All subjects participated in a practice round where they could attempt the slider task and observe for themselves that it was easy to complete all of the sliders well within the allotted time, thus negating any possibility for quality or productivity-based bias.

Our results show that when employers are aware of employees' outside obligations (as in our Information treatment), wage asks are lower and increase with outside obligations; whereas when the obligation information remains private (as in the No Information treatment) wage asks are uniformly high and do not change relative to obligations. For the employers, knowledge of employees' outside obligations has a positive and significant impact on wage offers when employees have high obligations, but has a similarly significant, but negative impact on wage offers for employees without such obligations. If employers can observe an employee's outside obligation, wage offers become positively correlated with greater need and the final payoff outcome is more equitable among employees. This suggests that business policies that aim to treat all employees equally, as in the No Information treatment, create inequities once we factor in outside obligations. Furthermore, it is noteworthy that the average wage offers do not differ between the Information and the No Information treatments, which implies that responding to employees' outside obligation does not place an additional cost on the employer. The employer simply redistributes the surplus from employees with no obligations to those with higher outside obligations. Finally, we do not find any evidence of gender bias in wage offers among any of the employer/employee combinations, consistent with recent replication studies (Li et al., 2018). We also do not find gender bias in wage offers as a result of an employee's outside obligations. Overall, employers do not exhibit any evidence of overt gender discrimination, likely because of the simplicity and neutrality of the task.

The rest of the paper is organized as follows: In Section 2, we present how fairness considerations affect predictions in an ultimatum game and state our testable hypotheses. The experimental design and procedures are discussed in Section 3. Section 4 presents the results and Section 5 concludes.

2. Theoretical Model and Testable Hypotheses

In a basic ultimatum game two players, proposer (P) and responder (R), negotiate to divide the surplus denoted by M. The proposer offers the responder an amount x, where $0 < x < M$, and the responder can accept or reject this proposal. In our framework, the responder also has an outside obligation (b) that reduces its final payoff. This implies that if the responder accepts the proposal x, the proposer's payoff is $M - x$, and responder's payoff is $x_R = (1 - b)x$, where $0 \leq b < 1$. If responder rejects the offer x, then both players get their outside option, which is φ for the proposer and $(1 - b)\varphi$ for the responder.

The subgame perfect Nash equilibrium with pure self-interested players predicts that the responders will accept any positive amount, irrespective of the level of outside obligation. This result has been falsified by numerous studies. In a meta-analysis of the standard ultimatum game, Oosterbeek et al. (2004) document that proposers offer in average no less than 40% of the surplus, and at least 16% of the positive offers are

rejected. Since these results have generally been interpreted as evidence in favor of fairness concerns, we extend the canonical model of Fehr and Schmidt (1999) to include outside obligations.

Fehr and Schmidt (1999) describe the utility function for agent *i* as follows

$$U_i = x_i - \alpha_i\, Max(0, x_j - x_i) - \beta_i\, Max(0, x_i - x_j),$$

where x_i is individual *i*'s monetary payoff and x_j is individual *j*'s monetary payoff. This utility function captures the distaste for *i*'s payoff being lower than *j*'s with the "envy" parameter, α, and the distaste for *i*'s payoff being greater than *j*'s with the "guilt" parameter, β. It is assumed that $\alpha \geqslant \beta \geqslant 0$, i.e., both disadvantageous and advantageous inequity are utility diminishing, the former more so than the latter. In the absence of payoff differences between *i* and *j*, or when $\alpha_i = \beta_i = 0$, utility is entirely determined by an individual's own monetary payoff. Also, note that "fair" offer that yields equal payoff to both players is $x = \frac{M}{2-b}$.[9]

If responder accepts the offer *x,* the utility of the proposer is

$$U_P(Accept\ x) = \begin{cases} M - x - \beta_P[(M - x) - (1 - b)x] & if\ x \leq \dfrac{M}{2-b} \\ M - x - \alpha_P[(1 - b)x - (M - x)] & if\ x > \dfrac{M}{2-b} \end{cases}$$

which can be simplified to

$$U_P(Accept\ x) = \begin{cases} (1 - \beta_P)M - (1 - 2\beta_P + \beta_P b)x & if\ x \leq \dfrac{M}{2-b} \\ (1 + \alpha_P)M - (1 + 2\alpha_P - \alpha_P b)x & if\ x > \dfrac{M}{2-b}. \end{cases}$$

Analogously, if the responder accepts the offer *x,* then the utility of the responder is

$$U_R(Accept\ x) = \begin{cases} [(1 - b) + 2\alpha_R - \alpha_R b]x - \alpha_R M & if\ x < \dfrac{M}{2-b} \\ [(1 - b) - 2\beta_R + \beta_R b]x + \beta_R M & if\ x \geq \dfrac{M}{2-b}. \end{cases}$$

If, on the other hand, responder rejects the offer, the utility of the responder is

$$U_R(Reject\ x) = (1 - b)\varphi - \alpha_R[\varphi - (1 - b)\varphi] = \varphi(1 - b - \alpha_R b)$$

Analogously, the utility of the proposer is

[9] We term $x < \frac{M}{2-b}$ as "unfair" offer and $x > \frac{M}{2-b}$ "hyper-fair" offer.

$$U_P(Reject\ x) = \varphi - \beta_P[\varphi - (1-b)\varphi] = \varphi(1-\beta_P b)$$

The responder will accept an offer x if $U_R(Accept\ x) > U_R(Reject\ x)$. Solving for x, we get the minimal acceptable offer for the responder:

$$x^* = \frac{\alpha_R M + \varphi(1-b-b\alpha_R)}{[(1-b)+2\alpha_R - \alpha_R b]}.$$

We note the following: (a) as b increases, the minimal acceptable offer x^* increases.[10] That is, the threshold of acceptance for the responder is positively related to the outside obligation b; (b) As α_R increases, the minimal acceptable offer x^* increases. If $\alpha_R = 0$, the responder does not care about being treated unfairly and will accept any unfair offer as long as it is higher than the outside option. If α_R is sufficiently large, then the offer must be close to the fair offer to be accepted. In any case, the minimally acceptable offer is less than the fair offer ($x^* < \frac{M}{2-b}$). That is, offers do not have to be hyper-fair to be accepted; (c) As the outside option φ increases, the minimal acceptable offer x^* increases.

Examining the proposer, the proposer's utility function $U_P(Accept\ x)$ is strictly decreasing in x when $x > \frac{M}{2-b}$. This implies that a rational proposer will not extend a hyper-fair offer. Let the offers be fair or less than fair: $x \leq \frac{M}{2-b}$. The amounts offered by the proposer depend on the value of β_P and b. If $\beta_P > \frac{1}{2-b}$ (i.e., if the proposer feels sufficiently guilty about the advantageous inequity), then the utility function is strictly increasing in x and the best choice for the proposer is to make the fair offer, $x = \frac{M}{2-b}$. If, on the other hand, $\beta_P < \frac{1}{2-b}$ (i.e., if the proposer does not feel sufficiently guilty about the advantageous inequity), then the utility function is strictly decreasing in x and the best choice for the proposer is to make the minimal acceptable offer, $x^* = \frac{\alpha_R M+\varphi(1-b-b\alpha_R)}{[(1-b)+2\alpha_R-\alpha_R b]}$. If $\beta_P = \frac{1}{2-b}$, then the proposer's offer lies between the minimal acceptable offer $\frac{\alpha_R M+\varphi(1-b-b\alpha_R)}{[(1-b)+2\alpha_R-\alpha_R b]}$ and the fair offer $\frac{M}{2-b}$.

More generally speaking, it is clear that the amounts offered by the rational proposer depends on β_P (own guilt at advantageous inequity), and the belief about the type of responder α_R (other's envy at disadvantageous inequity), as in the original Fehr and Schmidt model. In addition, we find that in all cases, the amounts offered by the proposer are monotonically increasing in the outside obligation b and in the outside option φ. In our experiment, the employer is the proposer and the employee is the responder. While

[10] This holds as long as $M > 2\varphi$, which is a standard assumption implying that gains from agreement are greater than the outside option for both players.

our focus is primarily on *b,* we note that there is strong support in the literature for the other three variables as well.

We begin with α_R and β_P. Creation of surplus by the employee performing a real effort task can create a sense of entitlement for the employee or an argument supporting deservingness for the employer. Prior studies in dictator games such as Cherry et al. (2002), Oxoby and Spraggon (2008) and Korenok et al. (2017) demonstrate that the source of income greatly affects the generosity of dictators. Frohlich et al. (2004) and Gachter and Riedl (2005) have argued that dictators are motivated by entitlements (division based on subject inputs) and not egalitarianism (equal division), although others including Rodriguez-Lara and Moreno-Garrido (2012) and Cappelen et al. (2007) find that subjects 'endorse a fairness ideal that most benefits themselves' (i.e., self-serving bias). In our framework, since the surplus is generated only by the employee, whether it is equity or egalitarianism, employee/responder might feel entitled to a greater share of the pie. An obvious way to model a greater sense of entitlement is to assume that the responder's distaste for inequality increases. As shown earlier, as α_R increases, the minimal acceptable offer x^* increases.

A similar interpretation can be extended to a sense of deservingness. Gee et al. (2017) argue that the cues individuals get about deservingness are different when the income is earned vs. randomly allocated. Agranov and Palfrey (2015) show that increase in inequality due to randomly assigned productivity levels leads to increased redistribution. In our framework, if the employer is motivated by entitlements and feels sufficiently guilty about the advantageous inequality, as denoted by β_P, then the employer is more likely to make a fair offer. Finally, we find that the outside option, φ, increases both asks and offers and this is consistent with numerous studies in the literature (e.g., Knez and Camerer, 1995; Schmitt, 2004; Kohnz, 2004; Hennig-Schmidt and Walkowitz, 2010; Henning-Schmidt et al., 2018).

In our design, the treatment variable is the information about the employee's outside obligation and we state our main testable hypotheses as follows:

Hypothesis 1: Wage asks increase with the level of outside obligation when this is public information.

In the experiment, we allow employees to make a non-binding wage request or a 'wage ask' to the employers. From a game theoretical standpoint, wage asks are irrelevant and amount to cheap talk. Models that incorporate fairness and intentions also do not offer a role for cheap talk (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). However, numerous experimental studies have shown that cheap talk affects behavior in various scenarios, such as bargaining (Farrell and Rabin, 1996), normal form games (Brandts and Charness, 1999; Bottom et al., 2002) and market competition (Lee and Hoffman, 2021). Croson et al. (2003) argue that players may engage in cheap talk for two reasons: to convey their private information or convey information about their preferences. Our information treatment allows us to analyze these motivations. When outside obligations are publicly known, employees can use wage asks to convey their

preference for acceptable offers. In the Fehr and Schmidt model, wage asks can be used as a signal by the responder/employee to convey to the proposer/employer their disadvantageous inequity aversion, α_R. As α_R increases, the minimal acceptable offer increases. Hence, we predict that wage asks increase with the level of publicly known outside obligation.

Hypothesis 2: Wage offers increase with the level of outside obligation when this is public information.

The minimal acceptable offer provides the lower bound for the wage offers, and it is increasing in the outside obligation, b. This implies that fairness predicts that employers respond to the employees' obligation amounts by offering higher wages. Correspondingly, when the obligation amount is not known to the employer, it is reasonable to expect that the wage offers are independent of this private information.

Before concluding this section, we note that we do not make any conjecture regarding wage asks in the treatment where employee's outside obligation is not known to the employer. As argued above, wage asks are strategic attempts to influence the employer's wage offer, in part by conveying private information. In this setup, an employee may inflate her wage asks (beyond the fair split) to appear to have high outside obligations and receive a more favorable offer. If higher asks are effective in generating higher offers, then it is an optimal strategy for a self-interested employee to send an inflated ask - there are no explicit costs to doing so without ex-post feedback to the employer about the employee's true outside obligation and no reputational concerns because of anonymous decision-making and stranger matching. However, it would not remain effective if exaggerated wage asks are widespread over time. Moreover, some people are intrinsically concerned about telling the truth even when misreporting cannot be detected.[12] For our purposes, we can simply state that there is enough theoretical and experimental evidence that people display a spectrum of preferences for truthfulness and misreporting. The usual trade-off between costs and monetary benefits will lead some individuals to inflate their wage asks, but the precise distribution of these asks remains an empirical question.

[12] A robust result in the lying literature is that while many subjects misreport their private information to their own advantage, a substantial share of subjects do not lie or do not lie maximally. Fischbacher and Föllmi-Heusi (2013) report that 20% of people lie to the fullest extent, 39% choose to be honest, and a sizeable proportion cheat a bit. Among the many explanations proposed for this lying aversion are psychological cost of lying and credibility of lies (Fischbacher and Föllmi-Heusi, 2013), perceived cheating aversion (Dufwenberg and Dufwenberg, 2018), self-concept maintenance (Mazar et al., 2008), and reputational costs associated with inference about their honesty (Gneezy et al., 2018). In a recent meta-study, Abeler et al. (2019) examine a wide range of potential explanations for truthfulness and test them empirically. The focus of these studies is to cleanly identify the motivations for lying aversion. To the best of our knowledge, this paradigm has not considered games with strategic interaction between players.

3. Experimental Design and Procedures

The experiment was conducted in the TIDE Lab at the University of Alabama using the program z-Tree (Fischbacher, 2007). We recruited 192 subjects to participate in eight independent sessions, with 24 subjects in each session. All subjects were undergraduate students and inexperienced in this type of decision-making environment. No subject participated in more than one session. The instructions (reported in the Appendix) were read aloud prior to each section of the experiment, and subjects were not allowed to communicate at any time. The instructions utilized neutral terminology for the roles to avoid any potential confounds as a result of framing the experiment in explicit terms.

To be able to properly observe and account for any potential gender bias, the gender pairing in the sessions was carefully controlled. Each session was comprised of exactly 12 men and 12 women. Prior to the start of the experiment, subjects were asked to assemble outside the lab, and they were let into the lab only when the required number of participants of each gender had reported to the lab. This procedure allowed the subjects to see each other and observe the gender composition of the group. Once we reached the required number of participants, subjects were invited to take a seat in such a way that all women sat on one side and all men sat on the other side of the lab. The experimental lab was set up with a see-through divider in the middle identifying two separate sides, where all participants could see the other participants in the lab, both on their side of the room and in the other side of the room. Other than the seat assignment, we made no other explicit mention of gender anywhere in the instructions or during the experiment. This omission was by design. The experimenter read the instructions to all 24 subjects at the same exact time, standing in the middle of the room.

The 12 men and 12 women in each side of the lab were randomly assigned the role of either employer or employee. Thus, in every session there were six male employees and six male employers in one side and similarly, six female employees and six female employers in the other side of the room (see Figure 1). These roles remained fixed for the entire length of the experiment. All experimental sessions consisted of two sequences of six periods each, during which subjects were paired in the negotiation game using a one-time (stranger) matching. One sequence featured same gender composition for both employee and employer (MM or FF) and the other sequence featured mixed genders (MF or FM). To operationalize this, in one sequence of six periods each employee was randomly matched with each employer on the same side of the lab, and in the other sequence of six periods each employee was matched with each employer on the other side of the lab. We used a perfect stranger matching protocol, so that a subject was never matched with the same other subject more than once in the experiment.[13] Recall that participants were able to clearly observe

[13] Although one can argue that in real-life, employers and employees (e.g., employees and employers in Facebook, Google, Microsoft, etc.) are in an ongoing relationship, which entails repeated interaction, we chose to model our

the gender composition of the whole group of participants at all times and knew with certainty the gender of their partner in each round. However, experience in the first six periods could affect subjects' behavior in the following six periods. Therefore, we also switched the sequence order to control for experience effects.

In each period, the employee and the employer negotiated in an ultimatum-game-like scenario to determine how to split the payoff earned from completing a pre-specified task. The task we chose required the employee to slide a randomly placed icon (anywhere from 0-50) along a slider line to the extreme right (at value 100). Employees were given one minute to complete 15 sliders and each slider task was rewarded with 100 Experimental Dollars.

Our choice of task was deliberate. We had two competing objectives. On the one hand, we wanted to clearly establish the structural asymmetry in this employer-employee relationship where employer's endowment depends on the efforts of the employee, and its division takes the form of a take-it-or-leave-it offer to the employee. This power dynamic is already implicit in ultimatum games, but as prior literature suggests, endogenous surplus creation reinforces it. On the other hand, we wanted to make sure that the task in the experiment is straightforward, i.e., it does not require a specific skillset, and there are no gender stereotypes associated with it. We wanted to clearly establish that everyone who 'wants' to do this task will be able to easily complete it within the allotted time regardless of gender (or general intelligence or ability), so that there is no skill-based discrimination.[14] This is relevant because in many prior studies, performance determines the size of the pie, and often performance is a function of luck, ability, and/or effort. In our design, the size of the total pie is not performance-related. This allows the focus to remain solely on the discriminatory explanations for any observed gender differences in wage offers. Random assignment of roles allowed us to avoid selection issues relating to the identity of employee and employer, and we can, therefore, interpret the behavior in this controlled setting as causal.

Our experimental design is summarized in Table 1. It features two treatment variables: the *information* about the outside obligation, which varied across sessions and the *gender* composition, which varied within session. In the No Information treatment sessions, the outside obligation was private information of the employee, although employers knew employees may have an outside obligation. In the Information treatment sessions, the outside obligation was public information available to both employee and employer.

experimental environment as a one-time interaction. This is because such policies are instituted company-wide and not based on past interaction with a particular employee.

[14] See Carpenter and Huet-Vaughn (2019) for a rich discussion on real effort tasks. Prior to the first period, both employees and employers attempted the slider task in a practice round so the difficulty level was common knowledge.

Employees in our experiment have an outside obligation that varies among 0, 25, and 50 percent of their total earning from the task, and each obligation level is equally likely to occur. These percentage amounts are automatically deducted from the employee's earnings. We used abstract, minimal framing cues for these outside obligations because the nature of outside obligation could affect one's sense of equity in ways that cannot be controlled. [15,16] Employers are simply aware of the existence and distribution of outside obligations. Each period proceeds as follows: First, the employee learns his or her outside obligation and makes a wage ask to the employer, $w_a \epsilon [10, 90]$, indicating how the earnings from completing the slider task should be distributed between employee and employer. A w_a of 50, for instance, indicates the employee is asking for 50 percent of the generated earnings. In the No Information treatment, these wage asks can potentially serve as a signaling device of the employee's outside obligation. In the Information treatment, the employer observed the level of outside obligation and the wage asks, and these wage asks can be used by the employees to signal acceptable offers.

Next, the employer makes the wage offer, $w_o \epsilon [10, 90]$, a counteroffer about how to split the earnings from the completed task. The latter may or may not differ from w_a. The employee, then, either accepts or rejects the offered split. If the employee accepts the offer, they have one minute to complete 15 sliders, with each slider task rewarded at the rate of 100 Experimental Dollars per slider. The total earnings are then divided according to the negotiated split. If the employee rejects the offer, then both parties must perform the task themselves and each will earn 10 Experimental Dollars for each completed slider. It is relevant to note that the outside obligations are deducted from the responder's earnings, regardless of whether the offer is accepted or rejected. Also, outside option in our design is less than or equal to the lower bound of the feasible wage offer and wage ask. Thus, players would always get a higher utility from making an offer that is accepted. This is not the case with most studies that analyze asymmetric outside options (e.g., Hennig-Schmidt et al., 2008; Henning-Schmidt et al., 2018; Schmitt, 2004).

At the end of the period, both players learn their individual period earnings, including the pre- and post-obligation earnings for the employees. At the end of experiment, subjects completed a demographic questionnaire. We also collected information on the subjects' altruistic and risk preferences using a one-

[15]We model outside obligations as a percentage of earned income rather than as fixed amounts because expenses most often associated with outside obligations (like mortgages or child care costs) are indeed indexed to income. Furthermore, in our experiment, employees who accept the offered wage generally earn a fixed amount (1500 experimental dollars), so the distinction between percentage or fixed amount for the outside obligation is irrelevant.

[16] For instance, in the real-world, credit card debt, student loans, mortgage payments, childcare payments, alimony payments all count as outside obligations. These obligations may be equivalent theoretically, but not behaviorally. Using a particular instance of outside obligations may result in loss of experimental control, because we cannot discern how the subjects view that obligation. Therefore, in the experiment, we used a sterilized environment where all that the subjects know is that "the Green players may have an obligation to pay a portion of their earnings to a third party."

round dictator game and a risk elicitation task.[17] We randomly selected three out of 12 periods for payment and subjects received their earning privately and in cash. Experimental Dollars were exchanged to US$ at the rate of 1ED=$0.10. The sessions lasted around 60 minutes and the average earnings were approximately US$23.00 (in addition to a $5.00 participation fee).

4. Results

Our dataset is comprised of 1,152 paired observations. In each session, 24 subjects conducted 144 negotiations, half with a counterpart of the same gender and half with a counterpart of the opposite gender. The primary variables of interest are the wage ask proposed by the employee (i.e., the percentage split of total earnings suggested by the employee before completing the task), the wage offer (i.e., the counteroffer by the employer), and the observed likelihood of each wage offer being accepted. In turn, each of these variables can be analyzed by treatment (No Information versus Information), by level of outside obligation (no obligation, small or high level of outside obligation) and by gender (male and female). Table 2 contains the corresponding summary statistics that helps anchor our discussion. Figure 2 presents the wage asks and offers across all 12 periods with trend lines for each treatment.

4.1 Wage Ask

We begin by defining the "equal wage split" as the wage ask that guarantees equal final earnings to both employer and employee, after the latter has paid his/her outside obligation. When the outside obligation is zero, equal wage split remains at 50%. It increases to 57.1% when the outside obligation is 25 and reaches a peak of 66.7% when the outside obligation is 50. While the equal wage split is an abstract construct, it helps explain why wage asks by employees increase with the outside obligation level. Figure 3 presents the data on wage asks across the two treatment variables, aggregating across genders. For all obligation amounts, the observed wage ask is lower in the Information treatment. Furthermore, wage ask increases with the outside obligation level in the Information treatment, whereas in the No Information treatment, wage ask remains high and varies little across the level of outside obligation.

To corroborate this visual presentation and provide a more formal analysis, we employ multivariate panel regression models that include random effects for subject-level choices. The standard errors are clustered at the session level and include controls for time trend, sequence order, risk preference, altruism and various demographic variables. The regression results across a series of specifications are included in Table 3. Looking at the aggregate data, we find that the wage ask is lower in the Information treatment

[17] See the experimental instructions in the Appendix for the lotteries used in the elicitation of risk aversion.

compared to the No Information treatment (p-value < 0.01 in Column (3); also, all pairwise comparisons of obligation levels).

Result 1: Wage asks are significantly lower in the Information treatment.

Hypothesis 1 states that wage asks increase with the level of outside obligations when the latter is public information. Columns (1) and (2) segregate the data by information treatment. When employers possess full information about their employees' outside obligation, wage asks are positively correlated with the level of outside obligations. This increase in wage asks is significant for all step-wise increase in obligation: when the outside obligation increases from 0 to 25, wage asks increase significantly from 56 percent to 61.7 percent of the pie (p-value < 0.01), and when the outside obligation increases from 25 to 50, wage asks are even higher at 67 percent (p-value = 0.04). It is worth noting that this increased differential in wage asks across obligation levels is qualitatively similar to the requested equal wage split proportions. This suggests that in the Information treatment, employees use wage asks to signal their preference for an acceptable offer.

In the No Information treatment, wage asks can be used to signal of the privately known obligation amounts. In the data, we find that employees ask for wages that are consistently high (65 to 70 percent of the pie), irrespective of their true obligations. Figure 3 presents little evidence of a positive relationship between the level of outside obligation and wage asks in the No Information treatment. Regression results in Column (1) of Table 3 indicate that wage asks are not significantly higher when the outside obligation increases from 0 to 25 (p-value = 0.13). However, when the outside obligation increases from 25 to 50, employees respond by asking a greater proportion of the pie for themselves. The average wage ask increases from 66.8 percent to 70.2 percent and, while this difference is small, it is statistically significant (p-value < 0.01). We propose that the observed invariance of these wage asks renders them a non-credible signal of the employee's outside obligations.

Finally, we note that these results are similar to Rankin (2003) and Rigdon (2012). In both studies, responders/employees could make a non-binding request. Although the employees in Rankin's study did not engage in a real effort task to generate the group surplus, the average request was for 65% of the pie. In Rigdon's study, property rights were earned by answering a popular-culture quiz, and the average ask was about 55-60% of the pie.

Result 2: Wage asks increase with the level of outside obligation in the Information treatment, but are uniformly high in the No Information treatment.

Next, we examine whether there are any gender differences in wage asks. Table 2 includes data on how males and females differ in their wage asks across the information treatments and obligation amounts. We find that that the proposed wage ask by female employees are marginally (and yet consistently) higher than those of male employees across all levels of obligation and in both treatments. For instance, in the Information treatment, the average wage ask by female employees across 0, 25, and 50 obligation levels increases from 57 to 62.4 to 67.7 of the pie, respectively. The corresponding data for male employees shows a similar pattern, with wage asks of 55, 60.9, and 66.3, respectively.

Figure 4 further analyzes wage asks for the gender composition, whether an employee makes the wage ask from an employer of the same gender or an employer of the opposite gender. We find that the wage ask by female employees from female employers is slightly higher than the wage ask proposed to employers of the opposite gender (see Figure 4b). Aggregating across both information treatment and obligation amounts, the average wage ask by female employees from female employers is 66 percent and from male employers is 64.5 percent. Random effect regression indicate that this difference is statistically significant (Wald t-test: p-value < 0.01). On the other hand, men do not differentiate between male and female employers in their wage asks (p-value $= 0.24$, average wage ask 63.3 percent from male employers and 64.5 percent from female employers).

Result 3: Wage asks by female employees are (slightly) higher than asks by male employees. Wage asks by women from female employers are (slightly) higher than women's asks by male employers. Additionally, men do not differentiate between employers' gender in their wage asks.

4.2 Wage Offer

Figure 5 presents wage offers across the two information treatments and for varying obligation levels. Table 4 contains random effect regression results for wage offers (with specifications similar to wage asks in Table 3). A quick glance at Figure 5 reveals that in the Information treatment, wage offers increase with the level of outside obligation (support for Hypothesis 2). Regression results indicate that this increase is significant for each stepwise increase in obligation level (all p-values < 0.01). On the other hand, when employers do not possess information about the obligation amount, it seems logical that their wage offers bear no relationship with employees' obligation. As evident from Figure 5, wage offers in the No Information treatment are uniform across obligation levels. Since our main objective is to analyze how employers respond to the information about the employee's outside obligation, it is most instructive to compare the wage offers in the No Information and Information treatment separately for each level of obligation.

When employees have no outside obligation, we find that wage offers are significantly lower in the Information treatment as compared to the No Information treatment. As shown in Table 2, the average wage offer decreases from 52.1 to 47 percent of the pie. This behavior is consistent with the notion that when employers are aware that the employee has no outside obligations, they take advantage of this information by offering the employees a smaller share of the earnings and securing a larger share of the surplus for themselves. Table 4 regressions support the finding that when the outside obligation is zero, wage offers are significantly lower in the Information treatment (p-value = 0.08). Kerschbamer and Kirchsteiger (2000) find in their experiment that the party responsible for paying the tax receives lower earnings, regardless of whether they are the proposer or responder, which consistent with our results.

When the obligation is 25 percent of employee earnings, there is no difference in average wage offers across the two information treatments. The average wage offer in the No Information treatment is 51.5 and in the Information treatment is 52 (p-value = 0.67). This indicates that when employers do not possess precise information about the employee's outside obligation (but know that these amounts are equally likely to be 0, 25 or 50 percent), they simply base their offers on the expected value of the outside obligations. The offers in the No Information treatment are similar to when the employers know that the employee has a small, but positive obligation. In both cases, the employer offers about half of the pie.

Employers are much more responsive to situations where the employee is known to have a large outside obligation. We find that when the outside obligation is 50 percent of the employee's earnings, the employers offer significantly higher wages in the Information treatment than in the No Information treatment. Average wage offers are 51.8 in the No Information treatment and rise to 56.7 in the Information treatment. This increase is statistically significant (p-value = 0.03).

Before proceeding further, it is worthwhile to compare our wage offers to those documented in the literature. We note that the surplus distribution observed in our experiment is more generous to employees compared to most standard ultimatum games, which generally feature a 60/40 split (starting with Güth et al. 1982). We observe the lowest wage offer in the case where the employer is perfectly aware that the employee has no outside obligation, and even then, the average wage offer is 47%. Following García-Gallego et al. (2012), we consider whether to attribute this higher offer to the effect of employees having to perform the real effort task.[18] As argued before, real effort exerted by employees could generate 'entitlement effects' that affect the behavioral norms for both employees and employers. A priori, it is

[18] Specifically, García-Gallego et al. (2012) find that the average offer by the employer when the surplus was exogenously given is 40% but when the employee must engage in real effort task to generate the group surplus (like in our setting), the average offer increased to 45%. Similarly, Hoffman et al. (1994) document lower offers when the right to be a proposer is awarded through a contest. Also, see survey by Güth and Kocher (2014).

unclear whether the more generous split is because the employers thought it was a fair outcome, or because they believed that lower offers would be perceived as unfair by the employee and thus more likely to be rejected. Irrespective of whether it is because of a change in fairness perception, or based on strategic considerations, the empirical consequence is the same: higher wage offers. Since the employee is the only player performing the real effort task and further, faces the possibility of outside obligations that can reduce his/her payoff, both equity and fairness norms suggest that employees should get a more favorable share of the pie and that this share ought to increase with their outside obligations. This positive relation between wage offers and obligation levels is clearly observed in the Information treatments (see Figure 5).[19]

Nevertheless, it is important to note that the split is not entirely equitable. Even when employers possess full and complete information about the obligation level, the wage offers do not approach the equal wage split. Specifically, even when the employer knows that the obligation amounts to 50 percent of the employee's earnings, the average wage offer of 56.7 is far below the equal wage split of 66.7. The employers have an advantage in this ultimatum-style negotiation and they do not fail to yield it. Finally, Table 4 also shows that wage offers are positively related to wage asks. This is corroborated by Figure 6 that displays the combinations of wage asks and wage offers, differentiated by level of obligation.[20]

Comparing the macro landscape, we make an interesting observation. We find that although the wage offers by the employers are positively correlated with the level of outside obligation in the Information treatment, on the whole, average wage offers do not differ across the two information treatments. Pooling across obligation levels, the average wage offer in the No Information treatment is 51.8 compared to 51.9 in the Information treatment, not a significant difference (p-value = 0.46). This effectively demonstrates that employers utilize the information about outside obligations to redistribute wages from employees with no obligations to employees with large obligations. Thus, being responsive to employees' needs (when known) is not a losing proposition for the employer. For the employer, there is no net loss, because the policy simply entails surplus redistribution between employees' types.

[19] One well-known example of information sharing and its impact on negotiated outcome is that of Michelle Obama who included her husband and children in her job negotiations (Shonk, 2019)."This is what I have: two small kids. My husband is running for the U.S. Senate. I will not work part time. I need flexibility. I need a good salary." Michelle Obama. She stated that "[i]t was one of the best experiences that I had because (my employer) put my family first and I felt like I owed that hospital because they were supporting me." (Today.com, June 24, 2014).

[20] Rankin (2003) also finds that non-binding requests for larger fraction of the pie result in larger offers. Croson et al. (2003) note that even disregarded cheap talk can influence one's decisions through anchoring and insufficient adjustment bias. Presentation of a wage ask can lead the employer to anchor on that number. The wage offers are subtly affected by that anchor, even though employers recognize the self-interest inherent in the wage ask and consciously reject it as a reasonable suggestion.

Result 4: In the Information treatment, wage offers increase with the level of outside obligation. On average, wage offers do not differ between the Information and No Information treatments.

Next, we analyze whether there are gender differences in wage offers. We find that both male and female employers respond similarly to a known increase in the outside obligation. That is, wage offers increase with obligation amounts. However, it is important to examine whether there are gender differences in the absolute offer amounts. Do women offer higher wages than men? Data included in Table 2 indicates that, all else equal, the average wage offer by female employers is fairly similar to the wage offered by male employers. In the No Information treatment, the average wage offer by female employers is 53.7 and by male employers is 49.9; the corresponding amounts for the Information treatment are 52.9 and 50.9, respectively.

Another relevant question is whether there is a within or across gender bias in these offer amounts. Do members of the same or opposite gender get preferential treatment in the offer they receive? Figures 7a and 7b display the wage offers across same-gender and mixed-gender pairings for both male and female employers. Regression results in Table 4 provide a formal proof. We find that wage offers made by female employers to male and female employees do not differ (p-value = 0.67) and that male employers do not differentiate between male and female employees in their wage offer (p-value = 0.67). The lack of gender bias in employer/employee combination, with the same or opposite gender, is important because it demonstrates that employers respond to the employees outside obligation in a similar manner, regardless of gender. It can also be stated that even though the female employees ask for higher wages from female employers (see Result 3), they end up with similar wage offers regardless of the gender of the employer.[21]

[21] We chose to vary gender composition within each session in a controlled manner - 6 periods of same (mixed) gender matching followed by 6 periods of mixed (same) matching – to account for any gender-specific learning. By simply stating that a subject is matched with a person in the same (different) side of the lab, we could avoid any mention of gender, per se. However, it is possible that the design of our gender matching (and the lab setup) made it obvious to subjects that the researchers were interested in knowing whether there were gender differences in behavior. Therefore, one can contend that the observed lack of gender difference is due to experimenter demand effect. We provide the following counter-arguments. First, De Quidt et al. (2019) state that there is no evidence that experimenter demand effects have influenced the qualitative inference from a study (though there is potential for a quantitative bias). We do not make any quantitative claims in our study. Second, our experiment features some of the best practices for mitigating experimenter demand effect such as anonymous decision making, incentivized choices, neutral instructions and progressive revelation of instructions. We control for order effects in all the parametric regressions. As a further robustness check, we conducted a between-session comparison for the mixed-gender and same-gender pairings using data from only the first 6 periods. The results do not change. These regressions are available from the authors. Finally, similar to our design, Mago and Razzolini (2019) also assigned subjects to different sides of the lab according to gender (without making it focal) and they find strong gender differences. There are numerous other studies that make gender a focal point of the experiment protocol and still find gender differences in behavior, for example, Chen et al. (2015), He and Noussair (2022). Accordingly, we contend that our lack of gender differences may not be attributed to the experimental design and the corresponding experimenter demand effect. More conclusive evidence can be garnered by considering an alternative design where subjects are randomly matched across all 12 periods using

Result 5: Wage offers made by male and female employers do not differ. There is also no evidence of within or across gender bias in the offer amounts.

The body of literature on gender differences in ultimatum games is inconclusive, where different variants of the game yield different results. In a repeated ultimatum game, Eckel and Grossman (2001) find that women make more generous offers as compared to men, regardless of the gender of the recipient. In contrast, Saad and Gill (2001) find that in a one-shot ultimatum game, male proposers are more altruistic when the recipient is a female, whereas female proposers make equal offers independently of the gender of the recipient. Solnick (2001) employs the strategy method to find that both sexes make lower offers to women, while in a replication study using both U.S. and Chinese students, Li et al. (2018) find little evidence of gender differences. Sutter et al. (2009) conclude that gender pairing (and not gender per se) has a significant effect on the distribution.[22] A meta-study on negotiations conducted by Stuhlmacher and Walters (1999) find that factors like the power/authority of the negotiators, mode of communication, and the task itself moderate this gender imbalance. Subsequent experimental examination has demonstrated that gender effects favoring either male or female negotiators depend on the content of the sex stereotype and how it is activated in the negotiations (Kray et al., 2001, 2002). Focusing on studies that feature a real effort task, we find no gender differences in wage offers, while Rigdon (2012) find a smaller gender gap when subjects have information about prior bargaining outcomes. In García-Gallego et al. (2012), female employers make lower offers than males, but Heinz et al. (2016) find that female dictators show reciprocity while male dictators exhibit a more selfish behavior. This lack of consensus is consistent with Li et al. (2018), who conclude that gender differences in ultimatum games are context based and may not hold stable across different paradigms.

4.3 Likelihood of Accepting the Offer and Earning Outcomes

In order to analyze earnings, it is important to first consider the likelihood of accepting wage offers across the two information treatments. Our ultimatum game framework includes a real effort task, the ability for the employee to make a wage request, and outside obligations. Prior literature presents mixed

gendered avatars, or by using the photo of the opponent (as in Babcock et al., 2017), or by presenting a recorded greeting with the opponent (as in Bordalo et al., 2017). We leave this to future research.

[22] Experimental examination of gender dynamics in other economic frameworks yields similar results. Gneezy et al. (2003) report that women are more competitive in single-gender tournaments compared to mixed-gender tournaments. Analyzing the impact of quota-style affirmative action, Niederle et al. (2013) find that women are more likely to enter the competition when they just have to outperform other women as compared to a mixed-gender setting. In a study of best-of-five contest, Mago and Razzolini (2019) find that women exert significantly higher effort only when competing against other women and there is no difference in competitive behavior between men and women in mixed-gender sessions. For men, the gender of the opponent is of no consequence. This result is also consistent with Price (2012), who notes that gender of the person imposing the competition is consequential to the performance.

hypotheses on the likelihood of accepting wage offers. García-Gallego et al. (2008) find that rejection rates are higher with real effort tasks, but Rankin (2003) finds that the ability to make requests increases the likelihood of accepting the offer. In our framework, it is not obvious whether acceptance rates should increase or decrease with outside obligations. It is plausible that norms of fairness may increase the willingness of the employee to punish the employer for a low offer, especially when the employee has high obligations and the employer is aware of this, but this scenario generally features higher wage offers.

The bottom part of Table 2 reports the likelihood of acceptance across the various treatment variables. The average probability of accepting an offer is 82.1% in the No Information treatment and it is 86.8% when outside obligations are known. Probit regressions included in column (1) of Table 5 indicates that this difference is not statistically significant (p-value = 0.11). A more relevant result is that for both treatments, the likelihood of accepting the offer decreases as the level of outside obligation increases (p-value $\leq$ 0.09 in all cases). In particular, the situation where an employee has a high obligation that is not revealed to the employer, the negotiation is significantly more likely to break down than in the case when the employer knows the employee's obligation. That is, in the No Information treatment with high obligation level, only 76.1% of wage offers are accepted, compared to 83.9% when the high outside obligation is known (p-value = 0.06). Table 6 also indicates that higher wage asks reduce the likelihood of acceptance (p-value < 0.01), but as expected, higher wage offers are more likely to be accepted (p-value < 0.01). Finally, there is no difference in average acceptance rates across genders (p-value = 0.46), although there seems to be greater disparity in response by women when the obligation level is high. Thus, if the percentage of successful negotiations is used as an indicator of efficiency, our results suggest that for employees with outside obligations, sharing that information with the employer would improve market outcomes by increasing the likelihood of a successful match, as compared to the no information policy.

Result 6: The likelihood of accepting an offer is higher in the Information treatment.

When the offer is accepted, employees complete the slider task 98.5 percent of the time. Higher rejection rates in negotiations lead to lower earnings for both employees and employers, all else equal.[23] Keeping this in mind, Figure 8 and Table 5a present the earnings for both employees and employers across the obligation levels and information treatments. The corresponding regressions are included in columns (2) and (3) of Table 5b. The trends in earnings generally follow the pattern of wage acceptance rates, with lowest overall earnings for both employers and employees occurring when the outside obligation is the

[23] If the wage offer is accepted and the employee completes the slider task, the total surplus generated is 1500 experimental dollars. However, if the wage offer is rejected, both employee and employer complete the slider task on their own to earn 150 experimental dollars each (minus the outside obligation for the employee). The completion rate is 98.2 percent for the employees and 72.1 percent for the employers.

highest, at 50 percent of employee earnings (p-value < 0.01). As expected, higher wage offers positively impact employee earnings and negatively impact employer earnings (p-value < 0.01). Higher wage asks are associated with greater rejection rates and, therefore, yield lower earnings for both employees (p-value < 0.01) and employers (p-value =0.02). We find that female employers earn more than male employers (p-value = 0.05), but this gender difference is not significant for employees (p-value = 0.44).

Neither employees nor employers, as a group, fare significantly better or worse in the No Information environment, as compared to the setting when outside obligations are known (p-value = 0.72 for employees and 0.28 for employers). It is quite remarkable that possession of information about the employee's outside obligation has no impact on the overall employer earnings. We contend that this is because the average wage offer is not significantly different in the two information treatments (see Result 4) and neither is the overall likelihood of accepting the offer. However, there is significantly greater variance in the employee earnings across the information treatments, depending on whether they have high or low outside obligations. The employees with no outside obligations still earn significantly more overall, since they do not have to pay any portion of their earnings. However, the no obligation employees fare worse in the Information treatment (p-value = 0.03), because the offered wages are lower. Correspondingly, the employees with the highest outside obligation benefit from this information being available, since higher wage offers translate into higher earnings. Together, our analysis illustrates why it makes sense for employees with no outside obligations (like the childless employees at Facebook) to complain against policies that allow employers to offer differential wages or benefits to employees with high obligations (such as parents). Similarly, the insignificant impact on employer earnings (i.e. profits) explains why employers might be willing to undertake such policies.

5. Conclusion

Given the recent use of parent-targeted policies by employers in response to the pandemic, it is worthwhile to consider how having children or other outside obligations affects an employee's position in negotiations, and the employers' willingness to share surplus. Our experimental framework is designed to capture such a ubiquitous employer-employee scenario, where an employer's surplus depends on the efforts of the employee, and the division of the resulting surplus comes in the form of take-it-or-leave-it offer to the employee. In addition to outside obligations, we also incorporate two important features that are known to affect negotiation outcomes and/or distribution in ultimatum games: endogenous endowment generation and non-binding wage request by the employees.

We find that, as a group, both employees and employers fare about the same when no information is available (No Information treatment), as compared to the setting where the employers know the outside obligations (Information treatment). However, there is significantly greater variance in the employee earnings across the information treatments, depending on the level of outside obligations. Full information improves the wage offers for employees with the highest level of obligation, but this comes at the expense of employees with the least demonstrated need, who realize a significant decrease in their wage offers. Since the average wage offer does not differ across the two treatments, the employers experience no net loss from addressing the needs of the employees with greater outside obligations. They simply redistribute the surplus from employees with no obligations to employees with higher obligations. We postulate that this redistribution captures the tension between parents and their childless colleagues at Facebook and other companies.[24]

Extrapolating from the idea of differential treatment based on outside obligations, it is worthwhile to point out that some of the gender wage gap can be explained by the fact that the ability to share information in the job negotiations has a differential impact across the two genders. If employers are unable to learn an employee's outside obligation, they may implicitly assume that a man either has or will have a wife, partner, or family to support and, thus, a higher outside obligation level. According to a Pew Research Center survey, "Americans still see men as the financial providers of the household, even while women's contributions to household income continues to grow."[25] The perception that women are less likely to need a higher income to support a spouse/family may explain why women are offered lower wages as compared to men, even when we control for skill-based gender discrimination and a number of other gender-related variables. On the other hand, if employers do learn that the female employee is a mother, 'motherhood penalty' occurs at least partially because of the perceived incompatibility between stereotypes about motherhood and job performance. Becker (1985) offered the 'work effort' explanation according to which mothers may be less productive at work because they have dissipated their reserve of energy on child care.

There is a growing body of literature that documents employer discrimination based on outside obligations. Experimentally holding constant the qualifications and background experiences of a pair of

[24] The well-intentioned pandemic policies created tension between parents and their childless co-workers, who deemed such targeted policies as unequal. At a company-wide meeting, Facebook employees repeatedly argued that work policies enacted in response to Covid-19 "have primarily benefited parents" and it was "unfair" that nonparents could not take advantage of the same leave policy afforded to parents. See "Parents Got More Time Off. Then the Backlash Started." (New York Times, September 5, 2020). https://www.nytimes.com/2020/09/05/technology/parents-time-off-backlash.html

[25] See the survey from the Pew Research Center, "Americans say a man should be able to support a family financially but don't say the same about women," September 19, 2017, https://www.pewresearch.org/fact-tank/2017/09/20/americans-see-men-as-the-financial-providers-even-as-womens-contributions-grow/ft_17-09-20_spouses_americanssay/.

fictitious job applicants and varying only their parental status, Correll et al. (2007) find that evaluators rated mothers as less competent and less committed to paid work than nonmothers, and consequently, discriminated against mothers when making hiring, promotion and salary decisions. Fathers, on the other hand, were evaluated as more committed to work compared to childless men and, therefore, offered higher wages. Similar other studies find that visibly pregnant women managers are evaluated as less committed, less dependable and less authoritative compared to otherwise equal women managers who are not visibly pregnant (Halpert et al., 1993; Corse, 1990). Ishizuka (2021) find that employer discrimination against mothers occurs both in professional/managerial occupations and in low-wage jobs. Thus, women may draw the short straw, regardless of whether or not they can share this information.

Our paper does not contest that beliefs about gendered labor markets and a family wage still shape the allocation of organizational rewards. On the contrary, since an important avenue for gender discrimination is skill differential (whether perceived or real - when employee productivity is unknown or uncertain), our study controls for any such quality concerns or bias rationalizations. All individuals are equally and fully capable of completing the task in our experiment. All else equal, we find that when the task is simple enough to remove any possibility for skill-based gender discrimination, no such bias is exhibited. People need to be able to rationalize their bias, and our experiment provides evidence that removing gender stereotypes or potential gender-based perceived quality differences from the equation renders it difficult for employers to invoke gender differences to enact differential treatment. Using a clean, carefully controlled setting, we establish an empirical endpoint where outright gender bias does not appear to exist and, thus, men and women treat others equally, and are treated by others equally, as it realizes in this experiment.

We conclude by observing that employment policies which treat employees equally may create or perpetuate inequities among employees. As a result of the pandemic in 2020, many institutions in higher education allowed all pre-tenure faculty to extend their tenure clock by one year. However, it is well-known that women are more likely to provide a greater share of care-giving and, therefore, the additional year granted to all might further aggravate the gender imbalance in academia.[26] On the other hand, employment policies that are cognizant of employee's outside obligations will be favorable for employees with high obligations, but at the cost of potentially disgruntled employees on the other end, as evident in the recent events at Facebook. This creates an interesting dilemma for the employers: Is the goal of the policy to appear equal to employees or to create equity among employees? Employers cannot have both!

[26] The Chronicle of Higher Education, "The Pandemic is Dragging on. Professors are Burning Out." November 5, 2020, https://www.chronicle.com/article/the-pandemic-is-dragging-on-professors-are-burning-out

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Table 1 - Experimental Design

Treatment	Sequence 1 (Periods 1-6)	Sequence 2 (Periods 7-12)	Number of Sessions
No Info	MM FF	MF FM	2
	MF FM	MM FF	2
Info	MM FF	MF FM	2
	MF FM	MM FF	2

Note: MM refers to Male employee matched with a Male employer.
FF refers to Female employee matched with Female employer.
FM refers to Female employee matched with Male employer.
MF refers to Male employee matched with Female employer.

Table 2 - Summary data on wage asks and wage offers (expressed as percentage of total earnings) and acceptance rates across obligation levels

		No Info Treatment			Info Treatment		
		F	**M**	**Aggregate**	**F**	**M**	**Aggregate**
Wage Ask	No obligation (0)	66.5	65.1	65.8	57	55	56
	Small obligation (25)	67.5	66	66.8	62.4	60.9	61.7
	Large obligation (50)	70.3	70.1	70.2	67.7	66.3	67
Wage Offer	No obligation (0)	53.7	50.5	52.1	48.5	45.4	47
	Small obligation (25)	53.4	49.7	51.5	53.4	50.6	52
	Large obligation (50)	54.1	49.4	51.8	56.8	56.6	56.7
Probability of accepting the wage offer	No obligation (0)	85.4	90.6	88	90.6	90.6	90.6
	Small obligation (25)	84.4	80.2	82.3	88.5	83.3	85.9
	Large obligation (50)	72.9	79.2	76.1	87.5	80.2	83.9

Note: When outside obligation = 0, fair wage split is 50%. When outside obligation = 25, fair wage split is 57.1%. When outside obligation = 50, fair wage split is 66.7%. More generally, fair wage split is $\frac{M}{2-b}$ of the pie.

Table 3 - Random effect regressions for wage ask

	(1)	(2)	(3)	(4)	(5)	(6)
	No Info	**Info**	**All sessions data**			
Dummy = 1 if MF	1.61	0.64	1.12	1.13	1.12	1.12
	(1.19)	(0.81)	(1.00)	(0.96)	(0.96)	(0.96)
Dummy = 1 if FF	2.68***	2.56***	2.62***	2.62***	2.65***	2.42***
	(0.97)	(0.62)	(0.53)	(0.53)	(0.42)	(0.66)
Dummy = 1 if FM	1.05	1.31**	1.18***	1.18***	1.21**	0.98
	(0.83)	(0.51)	(0.34)	(0.36)	(0.56)	(0.78)
Dummy = 1 if Obligation = 25	0.98	5.70***		0.98	0.98	0.98
	(0.65)	(0.53)		(0.60)	(0.60)	(0.60)
Dummy = 1 if Obligation = 50	4.70***	10.92***		4.51***	4.51***	4.51***
	(0.80)	(1.31)		(0.71)	(0.71)	(0.71)
Period	0.33**	-0.16*	0	0.09	0.09	0.09
	(0.16)	(0.09)	(0.13)	-0.13	(0.13)	(0.13)
Order	-0.13	-1.09*	-0.61	-0.61	-0.48	-0.8
	(1.17)	(0.60)	(0.64)	(0.64)	(0.84)	(0.75)
Dummy = 1 if Information Treatment			-6.04***	-9.81***	-9.89***	-9.98***
			(0.64)	(0.80)	(0.77)	(1.05)
Dummy Info * Obligation = 25				4.72***	4.72***	4.72***
				(0.77)	-0.77	-0.77
Dummy Info * Obligation = 50				6.59***	6.59***	6.59***
				(1.40)	(1.40)	(1.40)
Dictator Keep					0.99	0.79
					-0.77	-0.81
Risk Aversion					0.42	0.43
					(0.52)	(0.44)
Constant	62.38***	57.52***	67.25***	64.86***	59.41***	69.75***
	-2.66	-1.79	-1.55	-1.78	-2.65	-12.15
Demographic Controls	No	No	No	No	No	Yes
No. of Observations	576	576	1152	1152	1152	1152

Notes: Random effects (individual subjects) regressions with session clustering (robust standard errors in parentheses). ***, ** and * denote coefficients significantly different from zero at 1%, 5% and 10% levels (all two-tailed tests). MF: Male employee matched with female employer; FF: Female employee matched with female employer; FM: Female employee matched with female employer. Order dummy is 1 if first 6 periods of a session feature mixed gender pairing and the last 6 periods feature same gender pairing. Demographic controls include race, age, major, marital status, number of siblings and religion.

Table 4 - Random effect regressions for wage offer

	(1)	(2)	(3)	(4)	(5)	(6)
	No Info	**Info**	**All sessions data**			
Dummy = 1 if MF	-1.34*	0.6	-0.42	-0.39	-0.39	-0.39
	(0.75)	(0.77)	(0.92)	(0.91)	(0.91)	(0.91)
Dummy = 1 if FF	2.71	2.71	2.59	2.75	1.84	1.92
	(3.24)	(2.37)	(1.86)	(1.86)	(2.12)	(1.84)
Dummy = 1 if FM	3.08	1.92	2.44	2.48	1.57	1.66
	(2.83)	(3.08)	(1.85)	(1.91)	(2.21)	(2.03)
Dummy = 1 if Obligation = 25	0	3.53***		-0.42	-0.42	-0.43
	(0.50)	(0.47)		(0.50)	(0.50)	(0.50)
Dummy = 1 if Obligation = 50	-0.98	7.58***		-1.05**	-1.05**	-1.06**
	(0.63)	(0.93)		(0.51)	(0.51)	(0.51)
Wage Ask	0.1	0.19***	0.19***	0.13**	0.13**	0.13**
	(0.08)	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)
Previous Rejection	3.29*	1.34	1.87	2.24*	2.23*	2.19*
	(1.97)	(1.37)	(1.26)	(1.20)	(1.20)	(1.19)
Period	-0.72***	-0.23***	-0.50***	-0.48***	-0.48***	-0.48***
	(0.18)	(0.09)	(0.14)	(0.13)	(0.13)	(0.14)
Order	-4.03*	5.25***	0.77	0.76	0.07	0.21
	(2.08)	(1.06)	(2.12)	(2.11)	(2.38)	(2.83)
Dummy = 1 if Info Treatment			1.34	-3.63*	-3.46*	-3.94*
			(2.04)	(1.91)	(2.07)	(2.26)
Dummy Info * Obligation = 25				4.69***	4.69***	4.69***
				(0.88)	(0.88)	(0.89)
Dummy Info * Obligation = 50				9.19***	9.19***	9.19***
				(0.98)	(0.98)	(0.99)
Dictator Keep					-1.96	-2.02*
					(1.29)	(1.21)
Risk Aversion					-0.49	-0.42
					(0.36)	(0.40)
Constant	52.91***	27.75***	38.48***	42.69***	53.36***	32.63
	(6.51)	(1.96)	(6.63)	(5.23)	(7.97)	(28.21)
Demographic Controls	No	No	No	No	No	Yes
No. of Observations	528	528	1056	1056	1056	1056

Notes: Random effects (individual subjects) regressions with session clustering (robust standard errors in parentheses). ***, ** and * denote coefficients significantly different from zero at 1%, 5% and 10% levels (all two-tailed tests). MF: Male employee matched with female employer; FF: Female employee matched with female employer; FM: Female employee matched with female employer. Order dummy is 1 if first 6 periods of a session feature mixed gender pairing and the last 6 periods feature same gender pairing. Demographic controls include race, age, major, marital status, number of siblings and religion.

Table 5 - Earnings and likelihood of accepting the offer

Table 5A – Earnings

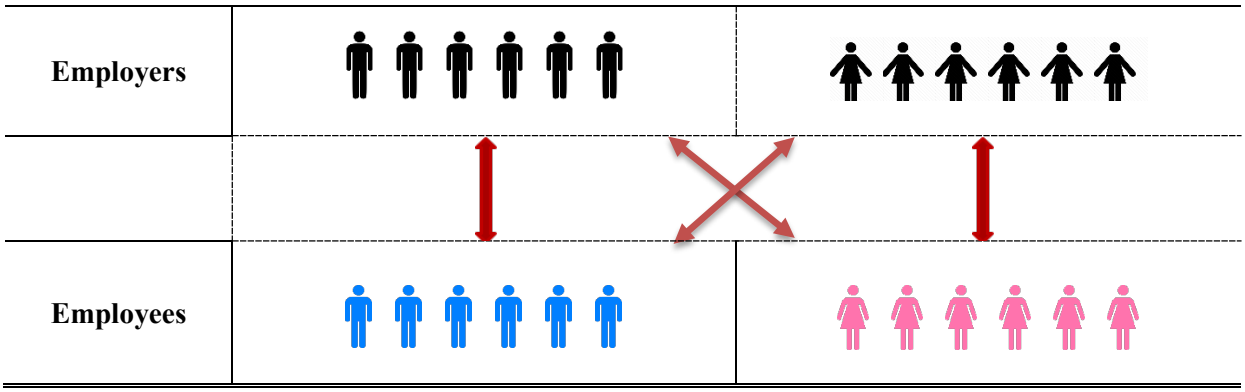
	No Info Treatment		**Info Treatment**	
	Employee Earnings	**Employer Earnings**	**Employee Earnings**	**Employer Earnings**
No obligation (0)	731.8	634.1	672.1	716.1
Small Obligation (25)	509.7	614.5	591.5	635.9
Large Obligation (50)	328.9	576.7	492.8	557.4
Aggregate	523.4	608.2	585.4	636.4

Table 5B – Regression for the likelihood of accepting the offer and earnings

	(1)	(2)	(3)
	Likelihood of Offer Being Accepted	**Employer Earnings**	**Employee Earnings**
Dummy =1 if Female	0.14 (0.19)	28.59* (14.61)	10.42 (13.49)
Dummy = 1 if Information Treatment	0.46 (0.29)	16.2 (15.09)	-7.46 (20.76)
Dummy = 1 if Obligation = 25	-0.36* (0.21)	-22.74 (15.13)	-212.19*** (17.04)
Dummy = 1 if Obligation = 50	-0.61*** (0.21)	-49.50*** (12.53)	-385.19*** (12.61)
Dummy Info * Obligation = 25	-0.34 (0.31)	-0.59 (28.04)	71.98 (58.88)
Dummy Info * Obligation = 50	-0.4 (0.31)	3.29 (25.40)	95.11 (95.89)
Wage Ask	-0.02*** (0.01)	-1.97** (0.85)	-2.08*** (0.46)
Wage Offer	0.09*** (0.01)	-9.03*** (0.89)	14.12*** (0.47)
Period	0.08*** (0.02)	3.84*** (1.35)	4.91*** (1.10)
Order	0.29 (0.19)	24.71 (18.19)	59.51 (46.91)
Constant	-2.62*** (0.68)	1127.99*** (77.53)	-5.05 (80.40)
Demographic Controls	No	No	No
No. of Observations	1152	1152	1152

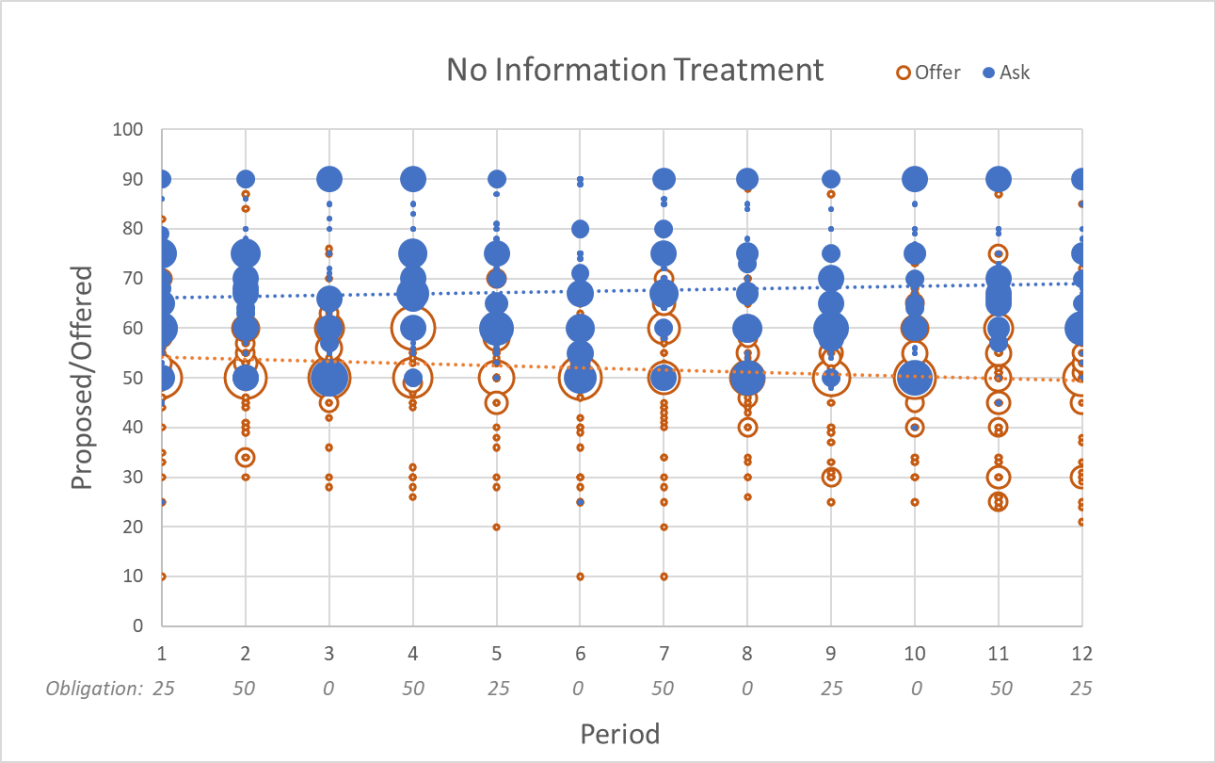
Notes: Column (1) includes results of a Probit Regression and Column (2) and (3) includes a random effects (individual subjects) regressions with session clustering (robust standard errors in parentheses). ***, ** and * denote coefficients significantly different from zero at 1%, 5% and 10% levels (all two-tailed tests). Order dummy is 1 if first 6 periods of a session feature mixed gender pairing and the last 6 periods feature same gender pairing.

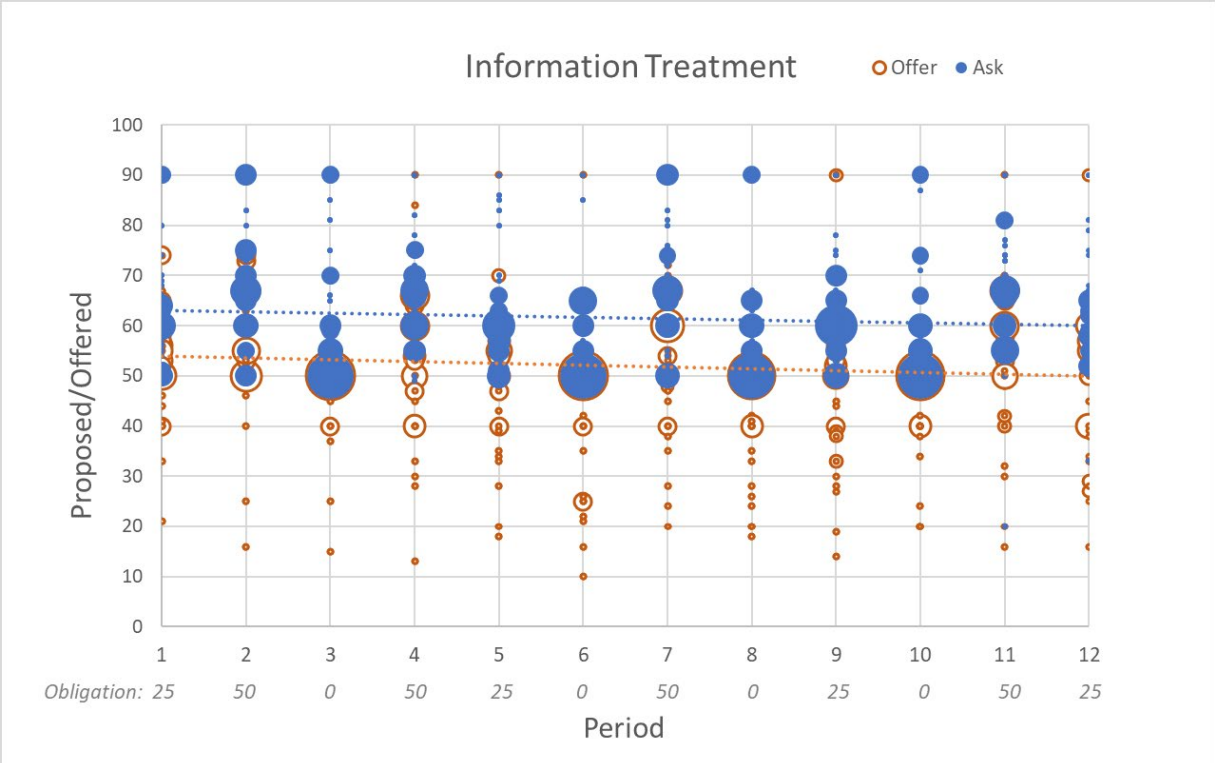
Figure 1 – Experimental Design



Round *n*	Mover	Action
Step 1	Employee	Learns the outside obligation: b = 0, 25, or 50. Makes a wage ask: $\boldsymbol{w_a \epsilon [10, 90]}$.
NO INFORMATION treatment Outside obligation is not revealed to employer.		**INFORMATION treatment** Outside obligation is common knowledge.
Step 2	Employer	Learns outside obligation (*if applicable*). Learns the wage ask $\boldsymbol{w_a}$. Makes a wage offer $\boldsymbol{w_o \epsilon [10, 90]}$. (Same or different from the wage ask.)
Step 3	Employee	Learns the wage offer $\boldsymbol{w_o}$. **Accepts** or **rejects** the offer. - If Accept: split is implemented. - If Reject: both receive their outside options

Figure 2 – Wage asks and offers across all 12 periods with trend lines.



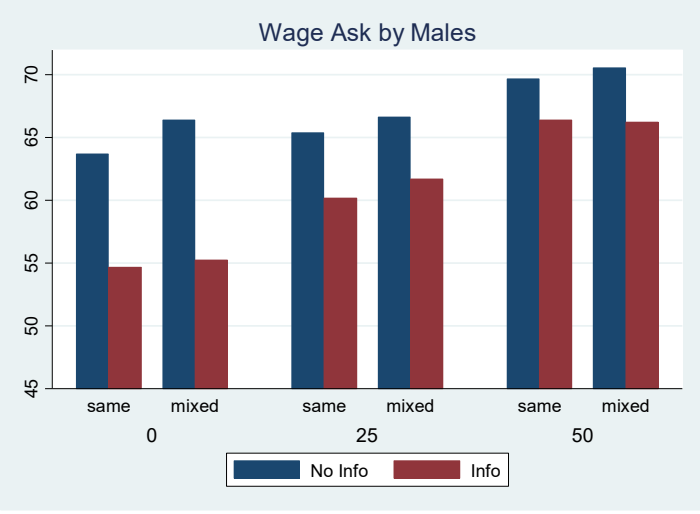


Notes: Wage asks and wage offers are plotted individually with bubbles weighted by number of observations.

Figure 3 – Aggregate wage ask across the two information treatments.

Figure 4 – Wage ask across the two information treatments, by gender pairings.

(4a)



(4b)

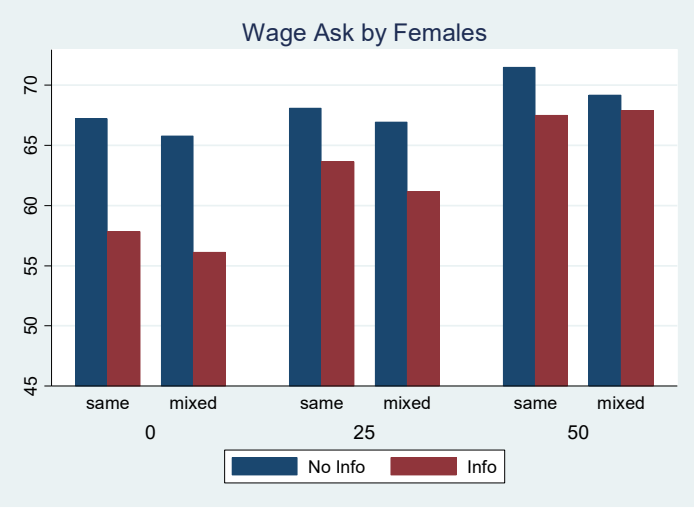
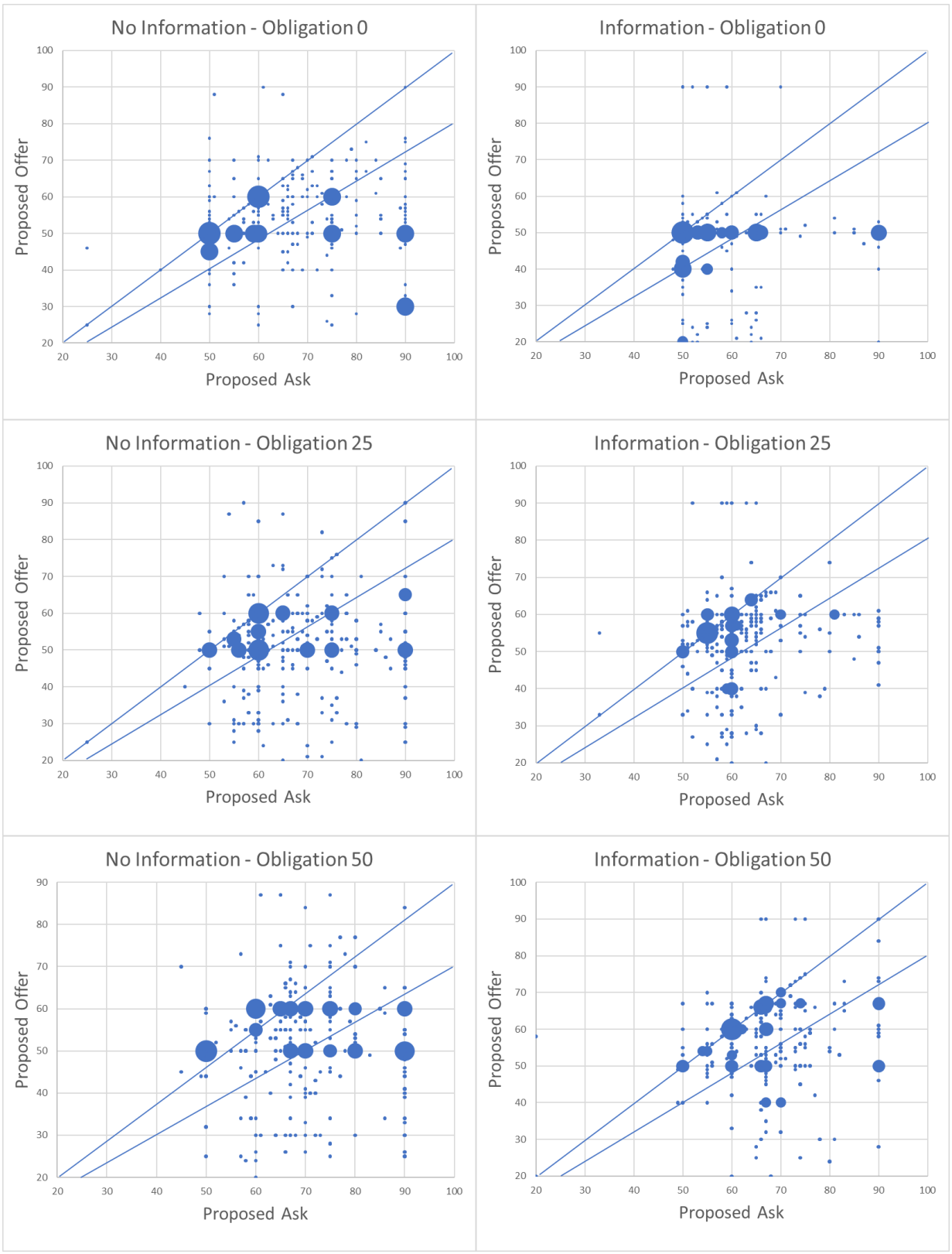


Figure 5 – Aggregate wage offers across the two information treatments.

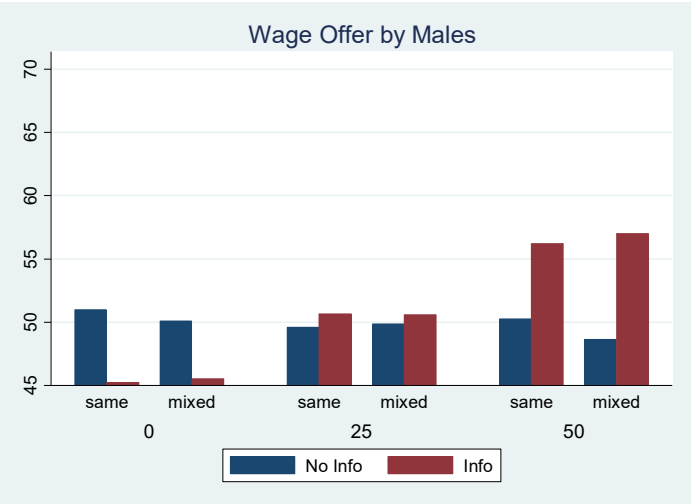
Figure 6 – Combination of wage asks and wage offers, sorted by obligation amount



Notes: Wage asks and wage offers are plotted in pairs, sorted by treatment and by obligation with bubbles weighted by number of observations. The 45° line indicates where asks equal offers and a line below indicating an offer at least 80% of the wage ask.

Figure 7 - Wage offers across the two information treatments, by gender pairings.

(7a)



(7b)

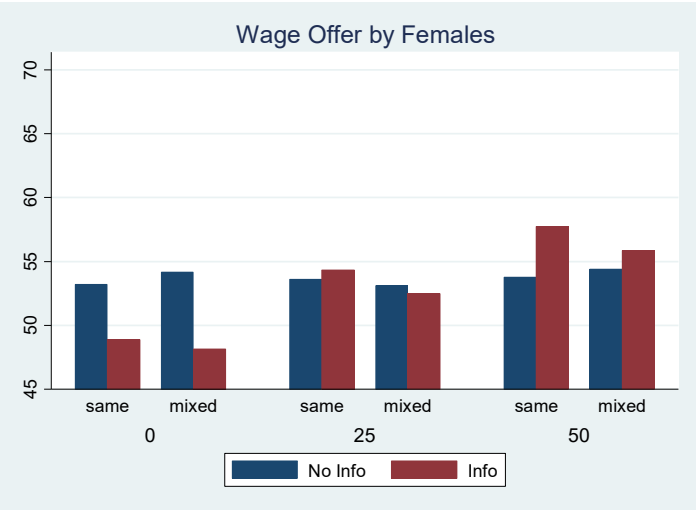
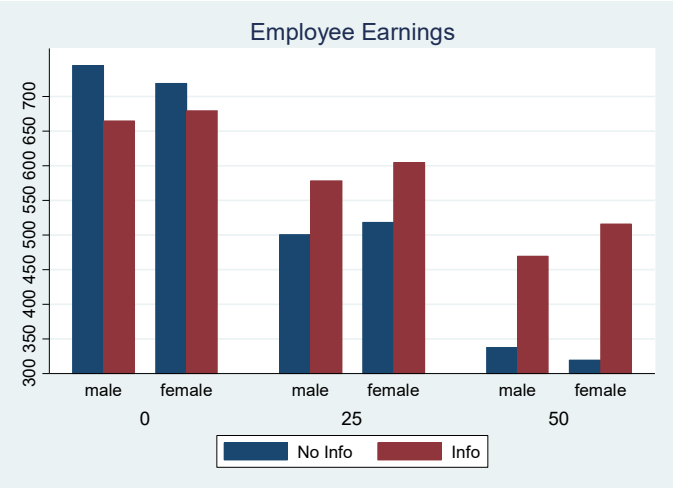


Figure 8 - Earnings for employees and employers across two information treatments, by gender.

8a)

8b)



Appendix: Experimental Instructions

General Information

This is an experiment on decision making. Several research agencies have provided funds for this study.

There are three parts to this experiment. Follow the instructions closely, as we will explain how your earnings will depend on the choices that you make. All of your earnings will be paid to you in cash at the end of the experiment.

You will be paid your earnings privately, meaning that no other participant will find out how much you earn. Also, for simplicity, we will hand out and read the instructions for each part before beginning that part. Each participant will have a printed copy of the instructions. You may refer to your printed instructions at any time during the experiment.

If you have any questions, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Also, please ensure that your cell phones are turned off and put away. Participants intentionally violating the rules will be asked to leave and will not be paid. We will now begin the first part of the experiment.

PART ONE

In this part of the experiment, you will be randomly paired with one other person in the room. You will not know the identity of the other person and they will not know who you are.

All participants in the room will be asked to allocate $5 between you and the other randomly matched participant. There are 6 ways that $5 can be allocated between two people in even dollar amounts (see the table below). You must select one (and only one) of these listed options.

Check one of the following boxes:

- $0 for you, $5 for the other person
- $1 for you, $4 for the other person
- $2 for you, $3 for the other person
- $3 for you, $2 for the other person
- $4 for you, $1 for the other person
- $5 for you, $0 for the other person

After all participants have made their choices, the computer will randomly determine if you are the *proposer* or the *receiver*. If you are the proposer, then your proposed allocation is implemented. For example, suppose you proposed $X for you and $5-X for the other person, then your earnings for this part of the experiment

is $X. However, there is an equal chance that you are a receiver, and in that case your earnings will be determined according to the allocation proposed by the other person matched with you.

The selection of your role (proposer/receiver) and the actual earnings for this part will be determined at the end of the experiment, and will be independent of your earnings from other parts.

Please make your decision now. If you have any questions, raise your hand.

Part Two

In this part, you will see a screen with seven option. Each option offers two possibilities with two US dollar amounts. Each amount has an equal chance of being chosen. You are going to select **one** of the options as your choice and that option will be used to calculate your payment for this part of the experiment. How much you receive will depend partly on **chance** and partly on the **choices** you make. The decision problems are not designed to test you. We simply want to know what choices you would make in them. The only right answer is what you really would choose.

Example: Consider the $8.00 | $2.50 option below. At the end of the experiment, a coin will be flipped to determine the outcome. If the coin lands on "heads" then you would earn the larger amount, which is $8.00. If instead the coin lands on "tails" then you would earn the smaller amount, $2.50.

Since each option is divided exactly in half, there is a 50% chance you will earn the larger amount shown and a 50% chance you will earn the smaller amount shown for whichever option you select.

$6.00	$4.00
$7.00	$3.50
$8.00	$2.50
$9.00	$2.00
$10.00	$1.50
$11.00	$1.00
$12.00	$0

Once you have made your decision, please click the "OK" button. Once everyone clicks "OK" we can move to the next part of the experiment.

The actual earnings for this part will be determined at the end of the experiment, and will be independent of your earnings from other parts.

Please make your decision now. If you have any questions, please raise your hand.

Part Three

Overview

This part of the experiment consists of **12** decision-making **periods**. How much you receive will depend partly on the choices you make and partly on the choices made by the other participants in the room.

There are **24 participants** in today's experiment: **12** participants are seated in **Room A** and **12** participants are seated in **Room B**. At the beginning of the experiment, the computer will randomly determine whether you are a Blue player or a Green player. Your role will remain the same for the entire length of the experiment. There are 6 Blue and 6 Green players in each Room.

In each of the first **6** periods, a Blue player from Room A (B) will be randomly matched with a Green player from the *other room*, Room B (A). In each of the remaining periods **7 - 12**, a Blue player from Room A (B) will be randomly matched with a Green player from the *same room*, Room A (B). Therefore, in every period, there will be **12** groups with **two** participants in each group, one from Room A and one from Room B for the first 6 periods and then both players from Room A or Room B in the last 6 periods. The specific person you are paired with will change randomly after each period and you will not be paired with the same person more than one time. The pair assignment is anonymous, so you will not be told which of the participant from the other room or your room is matched with you in each period.

Task Instructions

In each period, a task must be completed. The task requires a player to slide a randomly placed cursor (anywhere from 0-50) along the slider line to the extreme right (at the value of 100). See Figure 1 below. Players have **one minute** to complete **15 sliders**. Each slider completed is worth **100 Experimental Dollars**. Thus, successful completion of the task and all 15 sliders earns **1,500 Experimental Dollars** to be distributed among the Blue and Green players. The Blue and Green players will jointly determine the distribution, that is, how much each will earn from the successful completion of the task.

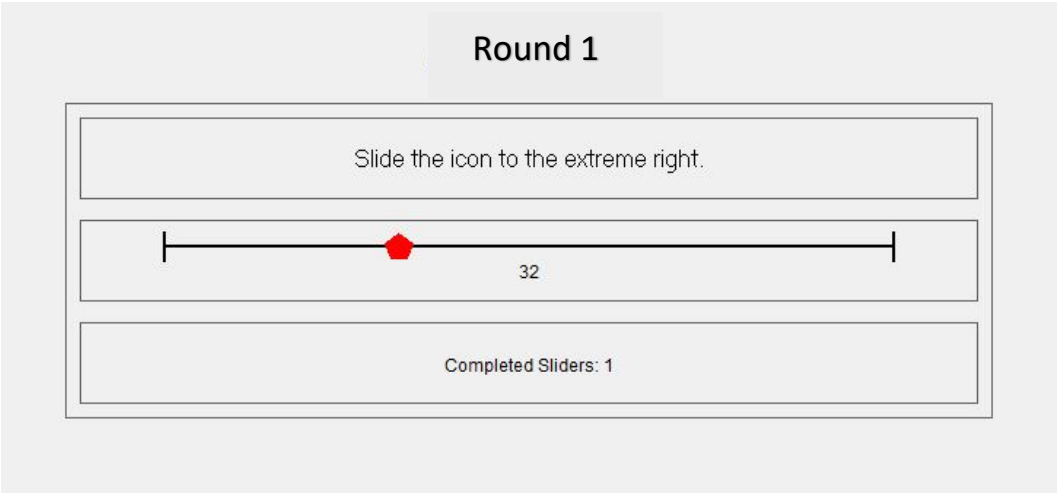


Figure 1

In each period, the Green players may have an obligation to pay a portion of their earnings to a third party. This "outside obligation" will be told to the Green players at the start of each period. The obligations are expressed as a *percentage*. That is, a Green player will be notified that they will be obligated to pay either 0%, 25%, or 50% of his/her earnings in that period to a third party. Each of the three obligation amounts – 0%, 25%, or 50% – are all equally likely to be selected in any given period.

Here is how the experiment will proceed in each period:

1. First, the Green player will discover his/her outside obligation – 0%, 25%, or 50%. Again, these are *percentage* amounts that will be automatically deducted from Green player's total earnings in that period.

2. Next, the Green player will propose how they want the earnings from the task to be distributed. That is, the Green player will use a slider to indicate how what percentage of the total earnings will be kept/retained by them (Green) and how many EDs will go to Blue.

   For example, the Green player could propose a distribution of 10:90 which implies that they keep 10% of the earnings and the Blue player would get 90%. Translating these percentage amounts into ED out of 1500 (which the maximum possible earnings), the Green player would keep 150ED for themselves and 1350ED would go to the Blue player. The Green player can propose any distribution from 10:90 to 90:10. That is, 11:89, 41:59, 63:37 are all acceptable distributions.

3. The Blue player will see the Green player's proposal, as well as Green's outside obligation in that period (0%, 25%, or 50%). Please note that the computer will truthfully disclose the Green player's obligation.

   Alternative instructions in the No Information treatment:

   3. The Blue player will then see the Green player's proposal. The Blue player will not observe the Green player's outside obligation.

4. The Blue player will then have the opportunity to send a counter-offer to the Green player, with the same specifications as described above. This offer could be the same as the proposal from the Green player, or it could differ. For example, the Blue player could propose a distribution of 10:90 (out of the maximum of 1500, 150ED for themselves and 1350ED for Green), or they could propose 90:10 (out of the maximum of 1500, 1350ED for themselves and 150ED for the Green player (or anything in between). Figure 2 presents a decision screen for the Blue Player in period 1.

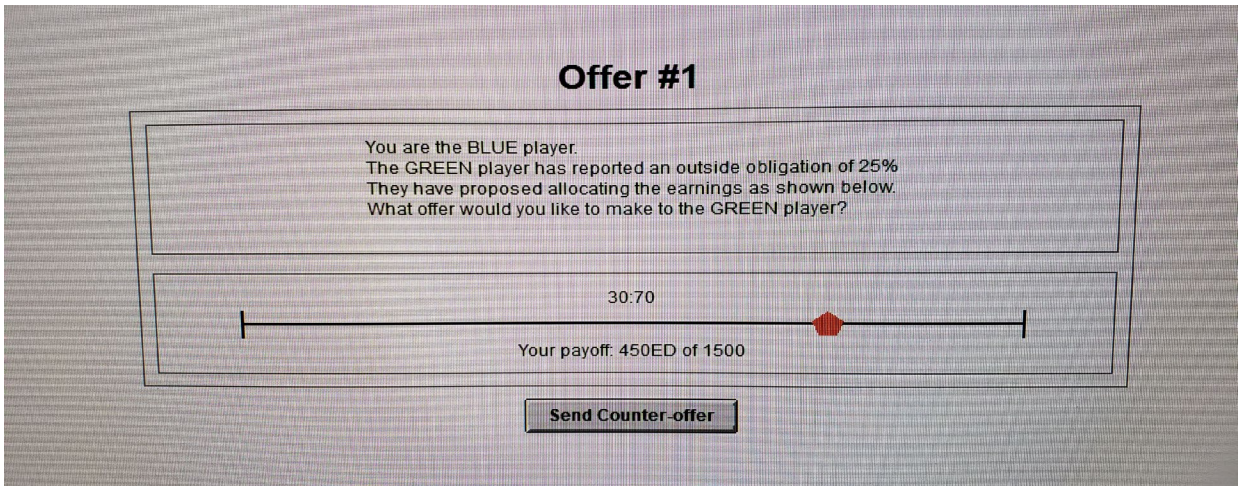


Figure 2

5. The Green player will see the Blue player's counter-offer and then decide whether to accept or reject the offer.

   -*If Green rejects Blue's offer*: Both Green and Blue players attempt the task for one minute and each player will earn **10ED** for each completed slider. Thus, if both players complete all 15 sliders then they each earn 150ED. If Blue completes 14 sliders and Green completes 15 sliders, then Blue gets 140ED and Green gets 150ED. Green will pay the outside obligation out of his/her earnings.

   -*If Green accepts Blue's offer*: ONLY the Green player attempts the slider task for one minute and the total earnings are distributed among Green and Blue players as determined by the proposal.

   However, Green player has to pay his/her outside obligation out of his/her earnings. Consider the following example: If Green earns 600Ed, his earning are determined as follows:

   -If Green's obligation is 0%, then Green would get to keep 600ED.

   -If Green's obligation is 25%, then Green would get to keep 600 – 0.25*600 = 450ED.

   -If Green's obligation is 50%, then Green would get 600 – 0.50*600 = 300ED.

   Consider another example. If Green finishes only 10 slider tasks in one minute, the total amount to be distributed between the two players is 1000ED. If Green player gets 400ED if the outside obligation is 0%, 300ED if the outside obligation is 25% and 200ED if the outside obligation is 50%.

6. This concludes the period. Players will then be randomly matched with a different new person in the next period.

This process described above will stay the same for each of the 12 periods. Again, for the first **6** periods, a Blue player from Room A will be randomly matched with a Green player from Room B (and a Blue

player from Room B will be matched with a Green player from Room A). In each of the periods **7 - 12**, a Blue player from each room will be randomly matched with a Green player from the same room (Room A or Room B).

At the end of the experiment, three (3) periods from this part of the experiment will be randomly selected for payment by rolling a 12-sided die. You will be paid all of your earnings for each of those 3 periods. Each period has an equal chance of being selected, so you should act as if your decisions in each period are going to be paid. The Experimental Dollars will be converted to real US Dollars using the exchange rate **150ED = $1US**.

In just a moment, all participants will be given the opportunity to attempt the slider task prior to the start of the experiment.

Please raise your hand if you have any questions at this time.