**Win–Win Denial:**

**The Psychological Underpinnings of Zero-Sum Thinking**

**Supplementary Materials**

**Part A: Study 1 Stimuli and Results Details**

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| --- | --- | --- | --- | --- | --- | --- |
|  | **Means** | | **Transaction Types** | | | |
| **Monetary Transactions** | **Buyer** | **Seller** | **Win–Win** | **Win–Lose (Seller Wins)** | **Zero–Zero** | | |
| Jenny goes to Anne's grocery store. Jenny pays Anne $8 for a bottle of olive oil. | 0.51 | 1.61 | 46.5% | 34.9% | 4.7% | | |
| Sam goes to Matt's car dealership. Sam pays Matt $33,000 for a Toyota. | –0.02 | 3.09 | 52.3% | 39.5% | 0.0% | | |
| Mary goes to Jack’s supermarket. She pays Jack $2 for a chocolate bar. | 0.65 | 1.19 | 46.5% | 27.9% | 7.0% | | |
| Sally goes to Tony’s clothing store. She pays Tony $30 for a shirt. | 0.42 | 2.16 | 53.5% | 38.4% | 4.7% | | |
| Eric goes to Paul's barber shop. Eric pays Paul $15 for a haircut. | 1.10 | 1.92 | 60.5% | 19.8% | 2.3% | | |
| Jane goes to Anna’s massage parlor. Jane pays Anna $60 for a 1 hour massage. | 1.35 | 2.07 | 67.4% | 17.4% | 1.2% | | |
| Kevin goes to Betty's pet grooming center. Kevin pays Betty $45 for his dog to be groomed. | 0.66 | 2.23 | 57.0% | 29.1% | 2.3% | | |
| Amy goes to David’s plumbing company. Amy pays David $130 for her shower to be fixed. | 0.81 | 2.36 | 59.3% | 31.4% | 2.3% | | |
| **Barter Transactions** | **Traders** | | **Win–Win** | **Win–Lose** | **Zero–Zero** | | |
| Mark goes to his neighbor Fred. Mark trades his bottle of soy sauce for Fred's bottle of vinegar. | 0.55 | | 31.4% | 15.1% | 43.0% | | |
| Angela goes to her friend Clair’s house. Angela trades her pencil for Clair’s eraser. | 0.38 | | 32.6% | 18.6% | 32.6% | | |
| Maria goes to her cousin Ivy’s house. Maria trades her Kate Spate crossbody bag for Ivy’s Michael Kors crossbody bag. | 0.63 | | 27.9% | 29.1% | 34.9% | | |
| Vivian goes to her colleague Tommy’s office. She trades her Burger King hamburger for Tommy’s McDonald’s hamburger. | 0.46 | | 29.1% | 25.6% | 37.2% | | |
| **Table S1**. Study 1 Stimuli and Results Details. | | | | | |
| *Note.*Entries in the “Means” columns are the means for buyers and sellers (for the monetary transactions) and for traders (averaged across the two parties for barters). Entries in the “Transaction Types” columns are the proportion of times that participants categorized each transaction as win–win (positive scores for both parties), win–lose (for monetary transactions, positive scores for the seller and negative scores for the buyer; for barters, positive scores for one party and negative scores for the other), or zero–zero (scores of zero for both parties), calculated as in Table 1 in the main text. No other transaction type had greater than 7% prevalence for any item (see Table 1 in the main text for aggregated data). | | | | | |

**Part B: Supplementary Studies**

**Study S1: Replication of Study 1 with “Benefit” Wording**

One potential concern about our studies is that participants are misinterpreting the terms “better off” and “worse off.” These terms are used by economists to refer to improvements and decrements in an individual’s overall welfare. However, in common parlance these terms might be used at times to refer to specifically *financial* well-being, as opposed to overall well-being. If participants are misinterpreting the terms in this way, it is no surprise that they would look like mercantilists, “overvaluing” money, since they interpret the question as referring *only* to money.

To rule out this concern, we used an alternative phrasing of the key dependent measure, asking if the buyers, sellers, and traders “benefited” from the transactions. This wording in fact stacks the deck in the opposite direction, since one could (in principle) answer this question affirmatively if one thinks a buyer benefitted to some degree but not to an extent worth the cost—such a buyer still “benefited” from the transaction, just not on balance. Thus, although we anticipated that people would be more likely to give affirmative responses to this question overall, we should still see the same asymmetries among buyers, sellers, and traders seen in Study 1.

**Methods.** We recruited 100 participants (*M*age = 37.6, 62% female, 55% college educated) from Mechanical Turk; 15 were excluded from analysis based on the criterion used in the other studies.

The method was identical to Study 1, except that participants were asked “To what extent do you think [*buyer* / *seller* / *trader*] benefited from this transaction?” for each transaction party, instead of the Study 1 phrasing of “How well off do you think [*buyer* / *seller* / *trader*] now is?” The scale remained anchored at –5 (“Worse than before”), 0 (“Same as before”), and 5 (“Better than before”).

**Results and Discussion.** As shown in Figure S1, participants’ welfare-change ratings were indeed more positive than in Study 1, reflecting the question asking about benefits without mentioning costs. But critically, the asymmetries among buyers, sellers, and traders remained large in magnitude.

Buyers were seen as losing from the transaction much more often than sellers [*M* = 1.21, *SD* = 1.62 vs. *M* = 0.35, *SD* = 0.77 out of 8; *t*(84) = 5.15, *p* < .001, *d* = 0.68], and this trend was also reflected in the mean welfare-change scores [*M* = 1.88, *SD* = 1.52 vs. *M* = 2.93, *SD* = 1.34; *t*(84) = 7.44, *p* < .001, *d* = 0.73]. While these asymmetries are somewhat less pronounced compared to Study 1, they are large in magnitude and reflect a general concern that buyers fail to benefit from their purchases, which we have also seen in every other study reported in this article.

Also mirroring other studies, traders were much more assigned zero-change scores [*M* = 1.32, *SD* = 2.04 out of 8] much more often than buyers [*M* = 0.19, *SD* = 0.92 out of 8; *t*(84) = 5.42, *p* < .001, *d* = 0.71] or sellers [*M* = 0.16, *SD* = 0.91 out of 8; *t*(84) = 5.42, *p* < .001, *d* = 0.73]. This also resulted in lower mean welfare-change scores for traders compared to monetary transaction parties (collapsing across buyers and sellers) [*M* = 1.21, *SD* = 1.34 vs. *M* = 2.41, *SD* = 1.28; *t*(84) = 7.81, *p* < .001, *d* = 0.92]. This confirms that traders are really seen as failing to benefit from their transactions, not merely as being no “better off” financially afterwards.

*Figure S1*. Proportion of transactions perceived as having negative, zero, or positive impact on buyers, sellers, and traders in Study 1.

Although participants are less likely to deny that buyers “benefit” than that they are made “better off” by their purchases, they still are much more likely to say that buyers, rather than sellers, are harmed by transactions. Likewise, although people are somewhat likelier to say that traders “benefited” from their barters rather than are made “better off,” they are far likelier to say that barters fail to benefit either party compared to monetary transactions. Thus, the key asymmetries documented throughout the studies—and offered as one key piece of evidence for mercantilist intuitions—are not an artifact of the way the dependent measure is worded.

**Study S2: Between-Subjects Replication of Study 2**

A second piece of evidence for mercantilist thinking—beyond the asymmetries among buyers, sellers, and traders—is that reframing transactions in terms of time rather than money tends to reduce the asymmetries between buyer and seller. This follows from mercantilism, which is the systematic tendency to overvalue money relative to the goods and services it can purchase. Sellers are seen as benefitting because they gain money (overvalued relative to the goods they give up) while buyers are seen as losing because they gain goods (undervalued relative to the money they give up). When money is made less salient, it should attenuate these intuitions and reduce asymmetries between buyers and sellers, as we saw in Study 2.

Study S2 aimed to go beyond Study 2 in two respects. First, it used a between-subjects designs. Specifically, Study 2A design was identical to Study 1 except that, rather than framing all purchases in terms of money, it framed them in terms of time (as in the *time-frame* condition of Study 2). That is, comparing Study S2A to Study 1 is equivalent to testing the effect of money-frame versus time-frame (tested within-subjects in Study 2) in a between-subjects design.

Second, Study S2B tested a *money-plus-time* framing in which both the monetary and time value were specified. This helps to tease apart whether the salutary effects of time-framing are due to mentioning time, per se, or instead avoiding the mention of money. According to mercantilism it is the latter avoidance of money-talk that produces the attenuation of the asymmetries between buyer and seller. Thus, we would expect Study S2B to look more like Study 1 than like Study S2A.

**Methods.** We recruited 199 participants (*M*age = 34.4, 55% female, 47% college educated) from Amazon Mechanical Turk (*N* = 99 for Study S2A and *N* = 100 for Study S2B); 55 were excluded from analysis based on the criterion used in the other studies.

Study S2A used the same design as Study 1, except every item appeared in the *time-frame* used in Study 2. That is, the buyers’ side of each monetary transaction was described in terms of labor, as was one of the traders for each of the barter. For example, whereas an item in the money-frame used in Study 1 read “Sally goes to Tony’s clothing store. She pays Tony $30 for a shirt,” the equivalent time-frame item in Study S2A (as well as the time-frame condition of Study 2) read “Sally goes to Tony’s clothing store. Sally worked for 1.5 hours to pay Tony for a shirt,” with a separate paragraph describing Sally’s after-tax wages at $20 per hour. One of the barter items read “Vivian goes to her colleague Tommy’s office. Vivian worked for 20 minutes to pay for a Burger King hamburger, which she trades to Tommy in exchange for Tommy’s McDonald’s hamburger,” as in the time-frame of Study 2.

Study S2B used a *money-plus-time-frame*, which differed subtly from the time-frame in Study S2A by including both the monetary *and* the time cost: “Sally goes to Tony’s clothing store. She pays Tony $30 (which took Sally 1.5 hours of work to earn) for a shirt.” A barter item read “Vivan goes to her colleague Tommy’s office. She trades her Burger King hamburger (which took Vivian 20 minutes of work to earn) for Tommy’s McDonald’s hamburger.” This phrasing does not explicitly mention the monetary value of the good (since the Study 1 phrasing did not do so either) but implicitly cued money by mentioning that it took a given amount of time to “earn” the good.

**Results and Discussion.** As shown in Figure S2, the asymmetries among buyers, sellers, and traders replicate once again, but are generally smaller in Study S2A (in the *time-frame*) compared to Study 1 (the *money-frame*) or Study S2B (the *money-plus-time-frame*). This buttresses the results of Study 2 and further implicates mercantilism as the driver of these effects by showing that it is avoiding money-talk, rather than introducing time-talk, that is driving these framing effects.

First, we note that the key asymmetries among buyers, sellers, and traders remain statistically robust in Studies S2A and S2B. Buyers were likelier to be deemed worse-off compared to sellers [*t*(60) = 4.68, *p* < .001, *d* = 0.78 and *t*(82) = 6.94, *p* < .001, *d* = 1.08 in Studies S2A and S2B, respectively], which led to higher welfare-change scores for buyers than for sellers [*t*(60) = 3.60, *p* < .001, *d* = 0.49 and *t*(82) = 7.61, *p* < .001, *d* = 0.85]. Moreover, traders were seen as likelier to experience no-benefit compared to both buyers [*t*(60) = 5.11, *p* < .001, *d* = 0.84 and *t*(82) = 9.02, *p* < .001, *d* = 1.18] and sellers [*t*(60) = 4.42, *p* < .001, *d* = 0.77 and *t*(82) = 9.22, *p* < .001, *d* = 1.27], resulting in lower welfare-change scores on average for barters versus monetary transactions (averaging across buyers and sellers) [*t*(60) = 7.03, *p* < .001, *d* = 0.60 and *t*(82) = 7.39, *p* < .001, *d* = 0.75]. All of these results qualitatively replicate the asymmetries found in the other studies.

*Figure S2*. Proportion of transactions perceived as having negative, zero, or positive impact on buyers, sellers, and traders in the *money-frame* condition (Study 1), *time-frame* condition (Study S2A), and *money-plus-time* condition (Study S2B).

But quantitatively, there was some evidence for variation in the magnitude of these asymmetries across studies. Just as the asymmetries were smaller in the *time-frame* condition of Study 2 compared to the *money-frame* condition, the asymmetries were smaller in Study S2A than in Study 1. Some of these differences across studies reached significance and others did not, but all were directionally consistent with predictions.

Buyers were seen as directionally (but not significantly) less likely to experience negative welfare-change in Study S2A [*M* = 1.98, *SD* = 2.12 out of 8] compared to Study 1[*M* = 2.53, *SD* = 2.47 out of 8; *t*(145) = 1.41, *p* = .16, *d* = 0.24] or Study S2B [*M* = 2.54, *SD* = 2.58 out of 8; *t*(142) = 1.38, *p* = .17, *d* = 0.23]. These led to comparable (non-significant) trends on mean welfare-change scores for buyers across experiments [*M* = 1.03, *SD* = 1.41 vs. *M* = 0.69, *SD* = 1.48 vs. *M* = 0.74, *SD* = 1.38; *t*(145) = 1.45, *p* = .15, *d* = 0.24 and *t*(142) = 1.28, *p* = .20, *d* = 0.22].

Traders were seen as significantly less likely to experience no-benefit in Study S2A [*M* = 2.00, *SD* = 2.37 out of 8] compared to Study 1 [*M* = 3.29, *SD* = 2.98 out of 8; *t*(145) = 2.81, *p* = .006, *d* = 0.47] or Study S2B [*M* = 3.43, *SD* = 3.01 out of 8; *t*(142) = 3.08, *p* = .002, *d* = 0.52]. However, the differences in mean welfare-change scores for traders across experiments did not reach significance [*M* = 0.71, *SD* = 1.06 vs. *M* = 0.51, *SD* = 0.93 vs. *M* = 0.51, *SD* = 0.94; *t*(145) = 1.26, *p* = .21, *d* = 0.21 and *t*(142) = 1.22, *p* = .22, *d* = 0.21].

Overall, these results—while less statistically robust than the within-subjects Study 2—help to buttress the results of that study in two ways. The money-frame versus time-frame across Studies 1 and S2A produced reduced asymmetries comparable in magnitude (if not larger) than those in Study 2, although some of these reductions did not reach statistical significance. Second, the fact that Study S2B and Study 1 produced very similar results indicates that a money-frame and money-plus-time-frame have similar psychological impact. That is a further piece of evidence for mercantilism, which predicts that cueing money (even if time is also cued) would result in perceived differences in buyer versus seller welfare.

**Study S3: Between-Subjects Replication of Study 3**

We argued that although mercantilism explains the asymmetries between buyers and sellers, it does not necessarily predict that they would need to be as sharp as they are across these studies. Plausibly, other factors may also be at play, and we argued that theory-of-mind or naïve realism is likely to be one such factor (Ross & Ward, 1997; Ross et al., 1977). Supporting this, Study 3 found that cueing participants to consider the buyers’ reasons—even empty reasons such as because the buyer “wanted” the product—reduced win–win denial.

Study S3 had two goals. First, analogously to Study S2, it aimed to replicate Study 3 in a between-subjects design. Whereas Study 3 exposed participants to both the *empty-reason* and *no-reason* condition, Study S3A framed every item in the empty-reason condition. Thus, comparing Study S3A to Study 1 is equivalent to testing the effect of the reason versus no-reason conditions (tested within-subjects in Study 3) in a between-subjects design. Second, Study S3 tested whether different kinds of reasons have differential impact. Thus, whereas Study S3A used the same empty reasons from Study 3, Study S3B gave “content” reasons that appealed directly to the buyer’s preferences.

**Methods.** We recruited 199 participants (*M*age = 38.8; 67% female; 55% college educated) from Amazon Mechanical Turk (*N* = 99 for Study S3A and *N* = 100 for Study S3B); 26 were excluded from analysis based on the criterion used in the other studies.

Study S3A used the same design as Study 1, except every item appeared in the *empty-reason* phrasing used in Study 3. That is, each transaction included an empty explanation of the buyer’s or traders’ choice, such as “Sally made the purchase because she wanted the shirt.” Study S3B was identical, except that a *content-reason* was given instead, which appealed to the buyer’s or traders’ preferences, such as “Sally purchased the shirt because Taylor Swift once wore this kind of shirt at her concert, and Sally loves Taylor Swift very much” or “Eric got the haircut because he finds the environment in Paul’s barber shop to be pleasant and enjoyable.” Similarly, for one of the barters, “Mark traded because he needed vinegar for a recipe, and Fred traded because he happened to run out of soy sauce.”

**Results and Discussion.** As shown in Figure S3, we see yet again the asymmetries among buyers, sellers, and traders predicted by mercantilism. But these are less statistically robust than in other experiments and do not always reach significance—this is the only study in the main or supplementary text that does not produce highly robust asymmetries between traders and monetary transactors. This is because win–win denial was overall much lower in the empty-reason and content-reason versions used in Study S3, compared to the no-reason version used in Study 1. Indeed, these differences among conditions were much larger in magnitude than in the within-subjects Study 3. These results point to theory-of-mind errors as a partial driver of win–win denial and to perspective-taking as a useful corrective.

*Figure S3*. Proportion of transactions perceived as having negative, zero, or positive impact on buyers, sellers, and traders in the *no-reason* condition (Study 1), *empty-reason* condition (Study S3A), and *content-reason* condition (Study S3B).

First, we examine again the asymmetries among buyers, sellers, and traders. Buyers remained likelier to be seen as made worse-off compared to sellers in both Study S3A [*t*(86) = 4.08, *p* < .001, *d* = 0.56] and Study S3B [*t*(85) = 4.95, *p* < .001, *d* = 0.73], with these differences also reflected in the mean welfare-change scores in both studies [*t*(86) = 4.95, *p* < .001, *d* = 0.38 and *t*(85) = 4.06, *p* < .001, *d* = 0.37]. In both studies, traders were likelier to be given no-change scores compared to both buyers [*t*(86) = 3.79, *p* < .001, *d* = 0.57 and *t*(85) = 2.01, *p* = .048, *d* = 0.25] and sellers (though not significantly for Study S3B) [*t*(86) = 4.52, *p* < .001, *d* = 0.61 and *t*(85) = 1.24, *p* = .22, *d* = 0.14]. Unlike the difference between buyers and sellers, the mean welfare-change scores did not significantly differ between barters and monetary transactions (averaging across buyers and sellers) in either experiment [*t*(86) = 1.08, *p* = .28, *d* = 0.08 and *t*(85) = 1.24, *p* = .22, *d* = 0.10]. As we will see, this reflects the much lower rate of win–win denial seen in Study S3.

Next, we directly compare the rates of win–win denial across studies. Buyers were seen as losing from their transactions much less frequently, compared with the no-reason framing in Study 1 [*M* = 2.53, *SD* = 2.47 out of 8], when empty-reasons were given in Study S3A [*M* = 1.06, *SD* = 1.98 out of 8; *t*(171) = 4.34, *p* < .001, *d* = 0.66] or content-reasons were given in Study S3B [*M* = 0.92, *SD* = 1.47 out of 8; *t*(170) = 5.21, *p* < .001, *d* = 0.79]; Studies S3A and S3B did not differ from one another [*t*(171) = 0.52, *p* = .60, *d* = 0.08]. These trends were also reflected in the mean welfare-change scores for buyers, which were significantly more positive in Study S3A [*M* = 2.18, *SD* = 1.79; *t*(171) = 5.98, *p* < .001, *d* = 0.91] and Study S3B [*M* = 2.16, *SD* = 1.35; *t*(170) = 6.82, *p* < .001, *d* = 1.04] compared to Study 1 [*M* = 0.69, *SD* = 1.48].

Similar effects were seen for traders. The perception that traders experience no change from their barters was significantly lower in both Study S3A [*M* = 1.40, *SD* = 2.58 out of 8; *t*(171) = 4.46, *p* < .001, *d* = 0.68] and Study S3B [*M* = 0.84, *SD* = 1.84 out of 8; *t*(170) = 6.50, *p* < .001, *d* = 0.99], relative to Study 1 [*M* = 3.29, *SD* = 2.98]; this effect was slightly larger in Study S3B (with content reasons), reflected in a marginally significant difference between Studies S3A and S3B [*t*(171) = 1.66, *p* =.099]. These effects translated into much higher welfare-change scores for barters in Study S3A [*M* = 2.34, *SD* = 1.92; *t*(171) = 7.98, *p* < .001, *d* = 1.21] and Study S3B [*M* = 2.54, *SD* = 1.45; *t*(170) = 10.96, *p* < .001, *d* = 1.67] compared to Study 1 [*M* = 0.51, *SD* = 0.93].

Overall, the perspective-taking manipulations led to much lower rates of win–win denial and zero-sum thinking. Whereas most participants in Study 1 (88%) believed that at least one of the transactions was non-positive-sum, this proportion was far lower in Studies S3A (39%) and S3B (41%). Likewise, whereas nearly all participants in Study 1 (94%) believed that at least one individual in one of the transactions failed to benefit, this proportion was more modest in Studies S3A (60%) and S3B (65%). Thus, although perspective-taking interventions certainly did not *eliminate* zero-sum thinking, it dramatically lowered its incidence.

Overall, these results attest to the significance of theory-of-mind in win–win denial—providing reasons for buyers’ choices, even if totally devoid of content, cues people to realize that people rarely voluntarily trade at a loss. Nonetheless, theory-of-mind cannot be the whole story, since it does not predict the asymmetries that we are consistently seeing among buyers, sellers, and traders. Theory-of-mind errors and intuitive mercantilism work together to produce our flawed intuitions about exchanges, and both appear to contribute to zero-sum thinking.

**Study S4: Cueing Preferences**

Study S4 further tested the theory-of-mind account by examining whether increasing the saliency of the transactors’ preferences increases the perception that the exchanges are win–win. That is, it is likely that, if asked explicitly, people would recognize that buyers preferred the good they purchased over the money they spent, while sellers prefer the money over the good. Indeed, neoclassical economics goes so far as to *define* preferences as revealed by one’s choices (Mas-Colell et al., 1995). Even if people do not hold this strong assumption of revealed preferences, they probably recognize that choices are typically aligned with preferences. However, from the fact that preferences were satisfied it follows logically that both parties to each exchange benefitted. Hence, cueing preferences as an intermediate step may help to circumvent theory-of-mind limits.

A second goal of Study S4 was to further rule out the concern that participants interpret the dependent variable in exclusively monetary terms. This was addressed partly in Study S1, which used the phrase “benefitted” rather than “better off,” but we put this concern to rest in Study S4 by providing participants with detailed instructions about how we define “better off” and requiring participants to correctly answer a series of check questions to ensure that they can correctly apply this definition.

**Methods.** We recruited 100 participants (*M*age = 43.5, 49% female, 58% college educated) from Mechanical Turk; 19 were excluded from analysis based on the criterion used in the other studies (failing at least 33% of the check questions at the end of the study) or because they answered one or more of the instruction comprehension check questions incorrectly. A further 12 participants were excluded who made an error on any of the preference questions (see below). Hence, all participants included in the final sample acknowledged that all transactions satisfied the preferences of both parties. As shown below, however, excluding participants based on their answers to the preference questions does not affect the results.

The procedure was similar to Study 1, with two changes. First, participants were provided detailed definitions of the terms “better off” and “worse off”:

For each transaction, you will be asked whether each participant is “better off,” “worse off,” or “the same,” relative to how they were before the transaction. When we use these terms, we are asking whether, all things considered, each person has benefited – in the language of economics, whether each person has increased “utility” after the transaction compared to beforehand. We are using a very wide definition of “benefit” or “utility” that includes any monetary gains or losses and any gains or losses in convenience, health, pleasure, or any other reason a person would carry out a transaction.

Then participants completed four sample items that were framed around chance events rather than transactions, so as not to introduce demand characteristics for the main task. These events were “George finds a $10 bill on the ground but loses a $1 bill,” “Rachel finds a $5 bill on the ground but loses a $100 bill,” “Ralph finds a $100 bill on the ground but lost a movie ticket that he would have been willing to pay $10 for,” and “Judy finds a $5 bill on the ground but lost a concert ticket that she would have been willing to pay $50 for.” For each event, participants responded as a forced-choice whether each person was made “better off,” “the same,” or “worse off.” Crucially, two of these items required participants to compare monetary and non-monetary utility, and in one case the correct answer was that the person was made worse-off despite gaining currency. Participants incorrectly answering any of these questions were excluded from data analysis, implementing a maximally strict standard for comprehension of the dependent variable.

Second, for half of the items (in the *preference-cue* condition), participants were first asked to state (as a forced-choice) the preferences of each party to each transaction. For example, for the item asking about Sally’s trip to Tony’s clothing store to buy a shirt for $30, participants were asked “Which do you think Sally preferred?” and “Which do you think Tony preferred?” as a forced-choice (“the shirt” or “$30”). Likewise, for the item asking about Tommy and Vivian’s swap of their Burger King and McDonald’s burgers, participants were asked which Tommy and Vivian preferred (“the Burger King hamburger” or “the McDonald’s hamburger”). Participants were highly accurate in answering these questions. Among participants who were not excluded due to errors on the instruction or memory check questions, the average number of errors on these questions was 0.35 out of 8, with all but 12 participants answering all questions correctly. For the other half of the items (in the *no-cue* condition), these preference questions were omitted. The 4 items in each condition were blocked and the order of the two blocks was counterbalanced.

Because the preference questions are difficult to phrase unambiguously for services (e.g., which did the dog groomer prefer – $45 or the time and labor associated with grooming the dog), we omitted these items to ensure that the manipulation was clear. Hence, participants in Study S4 saw the 4 monetary purchases of goods and 4 barters used in the other studies, with half of the monetary purchases and half of the barters appearing each in the preference-cue and no-cue conditions, counterbalanced across items.

**Results.** Overall, the no-cue condition replicates the mercantilist pattern seen in the other studies. This pattern is attenuated in the preference-cue condition, just as cueing buyers’ reasons for transactions in Studies 3 and S3 reduced win–win denial.

*Figure S4*. Proportion of transactions perceived as having negative, zero, or positive impact on buyers, sellers, and traders across no-cue and preference-cue conditions of Study S4.

We first examine the no-cue condition, which is conceptually similar to Study 1 in the main text, except that participants were inculcated in the meaning of “better off” and “worse off.” The results were very similar to those of Study 1. As shown in Figure S4, in the no-cue condition, participants much more frequently claimed that buyers rather than sellers were harmed by transactions [*M* = 0.83, *SD* = 0.86 vs. *M* = 0.04, *SD* = 0.21 out of 2; *t*(68) = 7.45, *p* < .001, *d* = 1.26]. This led to lower welfare scores for buyers than for sellers [*M* = 0.04, *SD* = 1.80 vs. *M* = 1.85, *SD* = 1.42; *t*(68) = 6.63, *p* < .001, *d* = 1.12].

To adjust for the fact that Study S4 included 4 observations of trader welfare in each condition (2 observations for each of 2 items) but only 2 observations of buyer and seller welfare in each condition (1 observation each of buyer and seller welfare for each of 2 items), we divide the counts of positive, zero, and negative welfare change for traders to create comparable measurements. Participants far more frequently claimed that traders [*M* = 0.70, *SD* = 0.84 out of 2 after adjustment] experience zero change in welfare compared to buyers [*M* = 0.17, *SD* = 0.48 out of 2; *t*(68) = 5.00, *p* < .001, *d* = 0.77] or sellers [*M* = 0.07, *SD* = 0.31 out of 2; *t*(68) = 5.70, *p* < .001, *d* = 1.00]. This manifested in marginally lower welfare-change scores for barters than for monetary transactions (averaging across buyers and sellers) [*M* = 0.69, *SD* = 1.27 vs. *M* = 0.94, *SD* = 1.16; *t*(68) = 1.73, *p* = .088, *d* = 0.20]. Thus, the mercantilist intuitions on evidence in the other studies are replicated in Study S4, even with detailed definitions of “welfare” and instruction check questions to ensure comprehension of this definition.

Next, we compare the preference-cue to the no-cue condition. If the preference-cue assists participants in taking the perspective of the buyers and traders, then we would expect buyers and traders to be seen as more likely to gain, and hence their welfare-change scores to be higher. We would not necessarily expect comparable effects for sellers, for the reasons given in connection with Study 3, which revealed only weak tendencies for mentalizing to increase the perceived welfare of sellers.

As shown in Figure S4, these predictions were borne out. In the preference-cue condition, buyers were less likely to be seen as losing [*M* = 0.52, *SD* = 0.78 vs. *M* = 0.83, *SD* = 0.86 out of 2; *t*(68) = 3.65, *p* < .001, *d* = 0.37] and traders less likely to be seen as experiencing zero change [*M* = 0.94, *SD* = 1.49 vs. *M* = 1.41, *SD* = 1.67 out of 2; *t*(68) = 3.33, *p* = .001, *d* = 0.29], but sellers equally likely to be seen as gaining [*M* = 1.87, *SD* = 0.45 vs. *M* = 1.88, *SD* = 0.37 out of 2; *t*(68) = 0.24, *p* = .81, *d* = 0.04], compared to the no-cue condition. This manifested in more positive welfare-change scores for buyers [*M* = 0.59, *SD* = 1.65 vs. *M* = 0.04, *SD* = 1.80; *t*(68) = 3.01, *p* = .004, *d* = 0.32] and traders [*M* = 1.20, *SD* = 1.63 vs. *M* = 0.69, *SD* = 1.27; *t*(68) = 4.03, *p* < .001, *d* = 0.34], but not sellers [*M* = 1.79, *SD* = 1.43 vs. *M* = 1.85, *SD* = 1.42; *t*(68) = –0.30, *p* = .77, *d* = –0.04].

Plausibly, one might be concerned that excluding participants who made any errors on the preference questions could lead to a differential attrition problem that would bias the results in favor of more positive scores in the preference-cue condition. That is, since the preference questions were only asked in the preference-cue condition, one could argue that we selectively eliminated participants who had particularly negative views of the transactions in that condition. Hence, we repeated all analyses adding back in the participants who were eliminated due to their answers to these questions (*N* = 12). The results were very similar. In the preference-cue condition, buyers were less likely to be seen as losing [*t*(80) = 3.79, *p* < .001, *d* = 0.36] and traders less likely to be seen as experiencing zero change [*t*(80) = 3.29, *p* = .001, *d* = 0.25], but sellers no less likely to be seen as gaining [*t*(80) = 0.00, *p* = 1.00, *d* = 0.00]. Once again this resulted in more positive welfare-change scores for buyers [*t*(80) = 3.43, *p* < .001, *d* = 0.34] and traders [*t*(80) = 4.04, *p* < .001, *d* = 0.31] but not sellers [*t*(80) = 0.44, *p* = .66, *d* = 0.06].

Potentially we might see carry-over effects from one part of the experiment to the next. That is, participants may be more prone to seeing transactions as win–win even in the no-cue condition, if this condition had been preceded by the preference-cue condition. However, the extent to which buyers were seen as losing, sellers as winning, and traders as experiencing zero-change did not differ across the two orders (*t*s < 1.2, *p*s > .27), nor did the mean welfare-change scores for buyers, sellers, or traders (*t*s < 1.7, *p*s > .10).

**Discussion.** Study S4 supports three conclusions. First, participants rarely claimed that the buyers or traders did not prefer the item that they in fact received, suggesting that our minds make a strong intuitive link between preferences and choices. This cuts somewhat against the interpretation that participants believe the buyers or traders are irrational or deceived by the other party. Second, despite detailed instructions and comprehension checks about the definitions of “better off” and “worse off,” participants in the no-cue condition showed similar mercantilist patterns as in the other studies. Third, these mercantilist intuitions were weaker in the preference-cue condition, supporting the idea that theory-of-mind limitations contribute to win–win denial.

**Study S5: Subjective Beliefs and First-Person Perspective**

Given the large effects of cueing reasons and preferences in Studies 3, S3, and S4, we considered two other potential manipulations that might influence the extent of win–win denial.

Study S5A asked participants to judge the buyers’ and sellers’ *beliefs* about their relative welfare. By asking participants to directly judge whether, for example, Sally *believes* she is bettered by her chocolate bar purchase, they may be encouraged to spontaneously consider Mary’s reasons for making the purchase, which would lead to a pattern more like Studies 3 and S3 (where reasons were provided) than Study 1 (where they were not). This would also suggest that people believe that others are *mistaken* when they engage in exchanges—they may believe that they are better off, but they in fact are not. This is the prediction made by the theory-of-mind account, according to which people typically substitute their preferences for that of the buyer. Asking people to explicitly think from the buyer’s perspective by using a subjective question wording should, on this account, have a salutary effect on win–win denial.

Study S5B placed the participant in the position of the buyer by adopting a first-person perspective. On the one hand, people are known to exaggerate their decision-making abilities (e.g., Johnson & Rips, 2014, 2015; Pronin & Kugler, 2010) and might therefore be likelier to think themselves as gaining from their purchases. But on the other hand, people actually know their own preferences, and in many cases the transactions really may not be worth it. You aren’t better-off spending $2 for a chocolate bar if you don’t like chocolate! If theory-of-mind errors are at work, these first-person judgments might look very similar to third-person judgments, since one might impute one’s own anti-chocolate preferences onto the third-person buyer. Thus, this account predicts that Study S5B would produce similar results to Study 1.

**Methods.** We recruited 199 participants (*M*age = 37.9; 61% female; 43% college educated) from Amazon Mechanical Turk (*N* = 100 for Study S5A and *N* = 99 for Study S5B); 31 were excluded from analysis based on the criterion used in the other studies.

For Study S5A, the procedure was identical to Study 1, except that rather than rating the buyers’ and sellers’ welfare, participants rated the buyers’ and sellers’ *perceived* welfare. For example, the dependent measures for Sally and her clothing purchase from Tony were “How well off do you think Sally believes she now is?” and “How well off do you think Tony believes he now is?” The instructions were modified to reflect this change to a subjective wording (“For each transaction, you will be asked whether each participant believes that he or she is better off, worse of, or the same, relative to how they were before the transaction”).

For Study S5B, the procedure was identical to Study 1, except that the buyers in the transactions were first-person (“you”) rather than third-person (e.g., “Sally” or “Eric”). One item had to be lightly rephrased to make it gender-neutral, but otherwise all items were unchanged. For example, one item read “You go to Tony’s clothing store. You pay Tony $30 for a shirt,” and the dependent measures were “How well off do you think you now are?” and “How well off do you think Tony now is?” on the same scale as previous experiments. The instructions were modified to reflect these changes (“…you will read about some transactions, each involving you and another person”).

**Results and Discussion.** As shown in Figure S5, win–win denial was somewhat less common in Study S5A, when explicitly taking the buyers’, sellers’, and traders’ perspectives, compared to other studies where one is not cued to take the transactors’ perspectives. However, the first-person framing in Study S5B led to similar rates of win–win denial as in Study 1, further implicating theory-of-mind. In both studies, asymmetries among buyers, sellers, and traders remained robust.

First, we replicated the asymmetries among buyers, sellers, and traders that we have seen in every other study. Buyers were seen as likelier to lose from transactions compared to sellers in both Study S5A [*t*(81) = 5.41, *p* < .001, *d* = 0.76] and Study S5B [*t*(85) = 7.96, *p* < .001, *d* = 1.15], and these trends were also reflected in mean welfare-change scores [*t*(81) = 6.89, *p* < .001, *d* = 0.65 and *t*(85) = 8.89, *p* < .001, *d* = 0.99]. Likewise, traders were seen as likelier to experience no change in welfare compared to both buyers [*t*(81) = 5.15, *p* < .001, *d* = 0.69 and *t*(85) = 8.08, *p* < .001, *d* = 1.12] and sellers [*t*(81) = 5.11, *p* < .001, *d* = 0.66 and *t*(85) = 8.53, *p* < .001, *d* = 1.22]. This was also reflected in the mean welfare-change scores, which were lower for barters than for monetary transactions (averaging across buyers and sellers) [*t*(81) = 3.59, *p* < .001, *d* = 0.40 and *t*(85) = 7.95, *p* < .001, *d* = 0.77].

The main issue of interest here is the comparison between these studies and Study 1, which used the third-person framing. Win–win denial was somewhat less common in Study S5A than in Study 1, reflecting the effect of the subjective wording. Buyers were seen as less likely to lose in Study S5A [*M* = 1.67, *SD* = 2.22 out of 8] compared to Study 1 [*M* = 2.53, *SD* = 2.47 out of 8; *t*(166) = 2.38, *p* = .019, *d* = 0.37], which was also reflected in the mean welfare-change scores [*M* = 1.30, *SD* = 1.53 vs. *M* = 0.69, *SD* = 1.48; *t*(166) = 2.67, *p* = .008, *d* = 0.41]. Analogously, traders were seen as less likely to experience no change in welfare in Study S5A [*M* = 1.85, *SD* = 2.64 out of 8] than in Study 1 [*M* = 3.29, *SD* = 2.98 out of 8; *t*(166) = 3.30, *p* = .001, *d* = 0.51], also reflected in the mean welfare-change scores [*M* = 1.25, *SD* = 1.27 vs. *M* = 0.51, *SD* = 0.93; *t*(166) = 4.34, *p* < .001, *d* = 0.67]. These results support the theory-of-mind account, in that subjectively wording the questions to emphasize the buyers’ and traders’ perspectives reduced win–win denial.

However, win–win denial differed little between Study S5B and Study 1, which varied only in using first-person versus third-person wording. Buyers were seen as equally likely to lose from their transactions across the two studies [*t*(170) = 0.59, *p* = .56, *d* = 0.09], resulting in similar mean welfare-change scores [*t*(170) = 0.94, *p* = .35, *d* = 0.14]. Traders were seen as somewhat less likely to experience no change in welfare in Study S5B than in Study 1 [*M* = 2.47, *SD* = 2.30 vs. *M* = 3.29, *SD* = 2.98 out of 8; *t*(170) = 2.04, *p* = .043, *d* = 0.31], but this did not translate into a significant difference in mean welfare-change scores [*t*(170) = 1.17, *p* = .24, *d* = 0.18]. Overall, Figure S5 shows that the distributions are fairly similar between Studies 1 and S5B, which supports the theory-of-mind account by suggesting that participants are substituting a question about buyers’ preferences with a question about their own.

*Figure S5*. Proportion of transactions perceived as having negative, zero, or positive impact on buyers, sellers, and traders when transactions were framed in the *third-person* (Study 1), in terms of the transactors’ *subjective* preferences (Study S5A), and in the *first-person* (Study S5B).

Overall, these results provide further support for theory-of-mind errors as a partial explanation of win–win denial. Study S5A tests another way of cueing perspective-taking, finding similar results (albeit smaller in magnitude) to Studies 3 and S3, which provided a more direct route into the buyers’ mental states by giving their reasons. Study S5B, on the other hand, framed transactions in the first person and found that people answer these first-person questions in a very similar way to third-person questions, suggesting that they substitute their own preferences as they evaluate whether others gain or lose from their transactions.

As one of the reasons why markets are possible is that we all place different subjective valuations on things, this finding represents a fundamental misconception about transactions and helps to explain the incidence of zero-sum thinking. If two parties have identical preferences and make a trade, one of them is winning and the other is losing, whereas if they have different preferences (e.g., because one of them can acquire the good at a lower price or because she gains less utility than the other from the product), positive-sum transactions occur. Zero-sum thinking appears to be in part a failure of perspective-taking.

**Part C: Individual Differences**

Each experiment measured participants’ educational background and political views. This allowed us to test hypotheses about what traits are associated with zero-sum thinking. For instance, one might imagine that economics training would attenuate zero-sum thinking, and one could plausibly argue for either liberals *or* conservatives espousing zero-sum attitudes more often (see Davidai & Ongis, 2019).

To test the impact of these variables, we built a hierarchical linear model, using the data from all experiments in the main text and Supplementary Materials (total *N* = 1188), nesting subjects within experiments and allowing a random intercept (but not random slopes) within each experiment. The dependent measure in this model was the frequency with which all parties (buyers, sellers, and traders) were perceived to be non-beneficiaries (i.e., the proportion of individuals rated as having zero or negative gains from trade, multiplied by 100). Dummy-coded variables were entered for experiment comparing these experiments to Study 1. The other variables in the model included educational variables—educational level (on a 7-point scale; *Mdn* = some college), major (dummy-coded with 1 = economics or business; 17% business/econ majors), economics knowledge (on a 0–10 scale; *M* = 4.53, *SD* = 2.49), and number of economics courses (*M* = 1.40, *SD* = 1.84); political views—social conservatism (on a 0–10 scale; *M* = 3.91, *SD* = 2.95) and economic conservatism (on a 0–10 scale; *M* = 4.65, *SD* = 2.93); and basic demographics—gender (dummy-coded with 1 = female; 59% female), age (*M* = 37.9, *SD* = 12.9), and income (square-root transformed; *Mdn* = $35,000). We also measured political party (on a 0–10 scale with higher numbers indicating stronger Republican identification; *M* = 4.03, *SD* = 3.00), but did not enter this variable into the model due to its multicollinearity with the other political variables. However, either adding this variable to the model or replacing the other two political variables with political party do not alter the results.

The model coefficients are reported in Table S2. Model 1 tests the impact of economics knowledge and education. Although formal education did not have an impact on zero-sum thinking (either overall education level or number of economics courses), self-reported economics knowledge had an appreciable effect on zero-sum thinking. For every one point (on a 0–10 scale) of self-reported economics knowledge, participants indicated about one percentage point fewer individuals had failed to benefit from their transactions. However, this effect was somewhat cancelled out by the impact of being a business or economics major, who exhibited significantly *higher* degrees of zero-sum thinking (most of these were business, rather than economics majors, within our sample). This could be (regrettably) an effect of the business curriculum at management schools, but it also may be a self-selection bias. In any event, business majors also reported higher economics knowledge (in a separate model predicting economics knowledge), helping to compensate for this tendency.

Model 2 adds political orientation—separately for economic and social conservatism—to the model. Neither variable was a significant predictor. On the one hand, this may be surprising given current political discourse, which (at the time of writing) seems to include more zero-sum language from conservative politicians, at least within the United States. But given the global resurgence of both left-wing and right-wing economic populism, it may be less surprising that political orientation is not a good predictor of a zero-sum mentality. One limitation of our measure of economic conservatism is that this may be quite conceptually distinct from support of free markets, as Caplan (2007) finds that both political conservatives and liberals are much more skeptical about the effectiveness of markets relative to professional economists.

|  |  |  |  |
| --- | --- | --- | --- |
|  | **Coefficient (SE)** | | |
|  | **Model 1** | **Model 2** | **Model 3** |
| Education | –0.26 (0.41) | –0.22 (0.42) | –0.39 (0.43) |
| Business/Econ Major | 3.82 (1.83)\* | 3.77 (1.84)\* | 3.13 (1.87)º |
| Econ Knowledge | –0.62 (0.28)\* | –0.63 (0.29)\* | –0.58 (0.30)º |
| Econ Courses | –0.43 (0.43) | –0.41 (0.43) | –0.35 (0.44) |
| Social Conservatism |  | 0.22 (0.35) | 0.18 (0.35) |
| Economic Conservativism |  | –0.12 (0.35) | –0.14 (0.36) |
| Female |  |  | 1.49 (1.32) |
| Age |  |  | 0.044 (0.05) |
| Income (square-root) |  |  | 0.008 (0.01) |
| \*\*\* *p* < .001 \*\* *p* < .01 \* *p* < .05 º *p* < .10  **Table S2**. Predictors of zero-sum thinking pooling data across studies. | | | |
| *Note.*Coefficients (SEs)predicting the proportion (multiplied by 100) of parties regarded as non-beneficiaries. | | | |

Another possibility, however, is that the political dynamics are somewhat more complex than these models suggest. Economic conservatism, social conservatism, and political conservatism were all highly correlated in our sample [*r*s > .79, *p*s < .001]. But they do appear to be conceptually distinct. For example, economic conservatism was *positively* associated with both self-reported economics knowledge [*b* = 0.09, *p* = .019] and formal economics coursework [*b* = 0.15, *p* < .001], while social conservatism had no association with economics knowledge [*b* = –0.00, *p* = .93] and a *negative* relationship with economics coursework [*b* = –0.08, *p* = .011]. Given the link between economics knowledge and lower levels of win–win denial, it could be that social conservatism and economic conservatism have opposite effects on zero-sum thinking, which are difficult to detect because they are so strongly correlated in our politically partisan environment.

Finally, Model 3 added demographic variables (gender, age, income) to the model, none of which had any additional predictive value. This is important to the extent that we wish to generalize the current results beyond the sampled population of Mechanical Turk workers, as one might argue that this population is relatively low in socioeconomic status and thus more prone to perceiving themselves as made worse-off by economic forces. But we see little evidence for this story, given no link between income and win–win denial within our sample.

Despite some significant predictors across these models, what is perhaps most striking is how *little* of the variability is explained by individual difference variables, even in cases where the factors had a statistically reliable effect. At least among our American participants, zero-sum thinking appears to generalize across people with very different political beliefs, educational backgrounds, and income levels. Apparently the zero-sum misconception is deeply rooted in human beings, and even extensive economics education does less than one would hope to root it out. The various manipulations explored in the current studies may be a more effective wedge to influence beliefs and behaviors.