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Intransitivity of Consumer Preferences for Privacy

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Consumers frequently exchange private data about themselves for goods or for money. For example, they provide personal data in online interactions with technology companies, in return for information or other goods. We provide a normative criterion to assess whether companies adequately compensate consumers for their private data in these exchanges. Across a series of eight preregistered experiments, we find that individuals place a higher price on their private data when they sell them for money than when they barter them for goods. In an application of the compatibility principle in cognitive psychology, we also find that this effect occurs because money is a more compatible medium for valuing data than goods are, which increases the weight of the data in monetary valuations, raising the prices that participants demand for their data. This discrepancy in valuations constitutes a violation of procedure invariance and amounts to an intransitivity of participants' preferences for privacy. Our findings suggest that companies may not be compensating consumers adequately for their data and that the ubiquitous markets for privacy may not function efficiently.

Keywords: Compatibility Principle; Marketing Automation; Preference Transitivity; Privacy Preferences; Procedure Invariance

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Privacy is an essential human good in many societies, albeit for different reasons (Whitman 2004). Yet aside from labor, perhaps the most common thing for a person to trade today is their private personal data. Companies collect behavioral data such as browsing and purchase histories, geolocation data, demographic and psychographic data, social network data, content and product preferences, and much more. They use these data to segment their customers (Wedel and Kannan 2016), price discriminate between them (Dubé and Misra 2019), target individuals with personalized content, advertising, or promotions (Lambrecht and Tucker 2013), or sell the data to other firms that may use the data for similar purposes. In exchange, companies offer consumers "free" goods. These may include search results, video and music streaming, access to social media, and other information goods (Shapiro and Varian 1999) , along with promotional product and service offers. Researchers have explored whether, why, and when consumers avoid disclosing private information (Hann et al. 2008; White 2004). Yet despite consumers' professed desire to protect their data, technology companies are collecting and trading ever more private data from consumers (Brough and Martin 2021).

We question whether companies compensate consumers adequately for their private data and examine violations of transitivity in consumer preferences for privacy as a normative criterion to answer this question. Prior research raises concerns about whether these market exchanges reflect consumers' true valuations of their private data (Acquisti, Taylor, and Wagman 2016). Research on the *privacy paradox* suggests attitude-behavior inconsistencies between consumers' stated and revealed preferences for privacy (Barth and de Jong 2017; Kokolakis 2017; Norberg, Horne, and Horne 2007). For example, someone might say that they do not want their internet browsing tracked but still have tracking enabled via cookies on their browser. Yet, a discrepancy between stated and revealed consumer privacy valuations need not be paradoxical (Acquisti, Brandimarte, and Loewenstein 2020; Solove 2021). In particular, it does *not* imply that companies compensate consumers *in*adequately for their data. Companies may argue that consumers' revealed preferences in the marketplace reveal their true valuations of their data because consumers exchange these data subject to actual incentives.

We examine whether companies compensate consumers adequately for their private data when exchanged for goods. We note that technology companies typically compensate consumers with goods (i.e., products and services), a form of barter, instead of with money, arguably a more standard medium of exchange. Accordingly, we design experimental tests of whether consumers value their private data the same when they exchange them for money as when they exchange them for goods. A systematic difference between these valuations would suggest that at least one of them cannot be normatively valid as consumers cannot simultaneously hold two different market valuations for the same good (i.e., for their data), all else equal. Yet we find that the same data command a higher value in exchanges for money than in exchanges for goods. To the best of our knowledge, our findings are the first to offer a normative criterion to suggest that consumer preferences for privacy are systematically biased by the exchange medium: Consumers are willing to accept less compensation for their private data when firms compensate them with goods than if firms compensated consumers with the standard medium of exchange, money.

Our participants' valuations systematically violate two essential requirements of rational choice theory. They violate *procedure invariance*, according to which normatively equivalent procedures for measuring preferences should yield the same preference ordering of the choice options (Grether and Plott 1979; Tversky, Slovic, and Kahneman 1990; Tversky and Thaler 1990). The violations of procedure invariance arise because our participants' preferences violate *transitivity*, an axiomatic condition of utility maximization (Edwards 1954; von Neumann and Morgenstern 1944). To detect these violations, we measure participants' willingness to accept (WTA): (1) an amount of money in exchange for a specific amount and type of their private data (i.e., a cash equivalent M), (2) a number of units of goods in exchange for the same data (i.e., a goods equivalent G), and (3) an amount of money to make them indifferent in choosing between that amount and the goods equivalent of their data (i.e., a cash equivalent L of the goods equivalent G). Consumers with transitive preferences would be indifferent between the amount of money (M) and the number of goods (G), either of which they are willing to accept for their data (i.e., $M \sim G$). In contrast, close to half of the participants in our experiments strictly prefer the amount of money to the number of goods, given their cash valuations of those goods ($M \succ G \sim L$). That is, we find that private data command two prices, M and L. This allows firms to pick which of the two to pay, by offering money or by offering goods. While prior research shows context effects on how consumers value their private data (Acquisti, John, and Loewenstein 2012, 2013; John, Acquisti, and Loewenstein 2011; Winegar and Sunstein 2019), pointing to the malleability of privacy valuations, ours is the first to demonstrate the impact of the medium with which buyers compensate sellers of private data, all else constant. We also present evidence of the psychological process that creates these different valuations, a greater compatibility of private data with being valued in money than in goods (Slovic, Griffin, and Tversky 1990).

Our findings are relevant to ongoing debates about the economics and ethics of data collection and use by technology companies (Johnson, Shriver, and Goldberg 2021; Sipior, Ward, and Rongione 2004). The General Data Protection Regulation (GDPR; Parliament and Council of the European Union 2016) and the California Privacy Act (CCPA; California Consumer Privacy Act 2018) limit companies' use of consumers' private data and give consumers rights of control over their data. Large technology companies such as Amazon, Facebook, and Google are experiencing rising pressure from the U.S. Congress concerning how they collect and share consumer data (Romm 2020). There are even discussions of breaking up these companies because of their near-monopsonistic market power in obtaining data (Rainey 2019). The finding that the same data can be bought more cheaply with goods than with money bears on how firms acquire market power based on consumer data and how they should be required to compensate consumers for such data (Arrieta-Ibarra et al. 2018).

THEORY: HOW AND WHY EXCHANGE MEDIA AFFECT VALUATIONS OF PRIVATE DATA

Research has established that consumers are flexible in their data disclosure preferences (Acquisti et al. 2012, 2013; Acquisti, Brandimarte, and Loewenstein 2015; Acquisti et al. 2020; John et al. 2011). For instance, a consumer's willingness to reveal data online depends on the extent to which they trust the firm (Bart et al. 2005; Martin, Borah, and Palmatier 2017; Schlosser, White, and Lloyd 2006) and on the perceived control they have over future usage of their data (Tucker 2014; Xu et al. 2012). Similarly, consumers deem it less acceptable that their data be used in tailored advertising when the data are acquired through a 3rd party rather than explicitly provided by the consumer (Kim, Barasz, and John 2019). Other factors impacting whether consumers disclose their data are whether privacy concerns have been triggered (John et al. 2011), if disclosure is seen as part of a fair exchange (Schumann, von Wangenheim, and Groene 2014), and to what extent they think that other consumers are disclosing sensitive data (Acquisti et al. 2012).

Factors such as these seem reasonable as determinants of data disclosure. They involve adjusting one's willingness to disclose depending on expected psychological and security costs. In contrast, we ask whether the medium with which companies compensate consumers for disclosing their private data influences how much consumers value these data. Technology companies usually do not compensate consumers for disclosing their data with money but with information goods, such as media entertainment and access to social networks. Companies portray these exchanges as "free." Yet when deciding whether to disclose their private data in such bartering exchanges (i.e., revealing their preferences), consumers must at least implicitly value their data in terms of the goods offered, that is, they must map a quantity of data onto a quantity of goods of equivalent value. To determine the value of these goods, they would have to ascribe a monetary value to them, which consumers may not do without being prompted. If, instead, companies compensated consumers directly with money, consumers would value their data directly. We propose a discrepancy between privacy valuations in goods and privacy valuations in money because private data are more compatible with being valued in money than with being valued in goods. In contrast to many other determinants of private data disclosure preferences, such a dependency of privacy valuations on the medium of exchange is not only not reasonable, it violates procedure invariance and implies an intransitivity of consumer preferences for privacy, a violation of an axiomatic foundation of rational choice.

The Compatibility Principle and Procedure Invariance

According to the compatibility principle, "the weight of any input component is enhanced by its compatibility with the output" (Tversky, Sattath, and Slovic 1988, p. 376), including how easy or effortful it is to transform the input into the output (Fitts and Seeger 1953; Slovic et al. 1990; Tversky et al. 1988). In a valuation context, when an output (the valuation scale) is more compatible with the input (the entirety or an aspect of what is being valued), the input will receive more weight in the valuation. Slovic et al. (1990) propose that this is because *in*compatibility between output and input increases effort and shifts attention and weight away from the incompatible input.

The compatibility principle can explain the classic *preference reversal* effects, a well-known violation of procedure invariance in judgment and choice (Grether and Plott 1979; Lichtenstein and Slovic 1971; Lindman 1971). In these studies, participants who face two gambles with financial payoffs, one with a high probability of winning a moderate payoff, the other with a low probability of winning a high payoff, assigned a higher monetary value to the gamble with the higher payoff yet chose the gamble with the higher probability of winning. Because monetary payoffs (the input) are more compatible than probabilities are with assigning monetary values (the output) to the gambles, monetary payoffs weigh more heavily in valuing gambles than in choices among these gambles (Slovic et al. 1990; Tversky et al. 1988; see also Goldstein and Einhorn 1987; Kim, Seligman, and Kable 2012).

We propose that private data are more compatible with being valued in money than with being valued in goods, because money is the scale with which consumers usually value assets, effort, and performance in commercial exchanges. Such an effect of monetary compensation on valuation is consistent with extant research. Heyman and Ariely (2004, Experiment 2; Bauer and Ariely 2021) found that the effort that participants put in solving puzzles was more sensitive to monetary rewards than to non-monetary rewards of similar market value (e.g., candy). Offering participants money signaled that their effort had value, so they exerted less effort at low levels of monetary rewards than at low levels of non-monetary rewards. Even giving people only subtle cues related to money triggers a greater focus on valuation, reducing prosocial helping behaviors towards others and enhancing participants' effort and performance (Vohs 2015; Vohs, Mead, and Goode 2006). Therefore, a monetary scale should be a better, more compatible match for the task of valuing private data, enhancing consumers' sensitivity to the value of their data and, thus, raising the value they place on the data when they value them in money instead of in goods.

Compatibility effects that violate procedure invariance in measuring preferences are more likely to arise when these preferences are not well-articulated and defined (Tversky et al. 1988). For this reason, we expect the discrepancy in valuations measured in money versus in goods to apply to exchanges of private data but not to exchanges of other personal inputs with more clearly defined preferences, such as one's professional services. Therefore, we predict an attenuation of the discrepancy between valuations across exchange media for effort inputs with well-defined wage rates, a boundary condition we test in Experiment 4.

Violations of Transitivity

According to the failure of procedure invariance in valuing private data, which we hypothesize, an individual values their private data at a cash equivalent (M) and also at a goods equivalent (G), but that the goods equivalent is worth less to them (L) than the cash equivalent (i.e., $M > L$ such that $M \succ G \sim L$). Someone with transitive preferences would value the data, their cash equivalent, and their goods equivalent equally. Instead, a higher valuation of private data measured in money than in goods implies a violation of transitivity of consumer preferences for privacy (Regenwetter, Dana, and Davis-Stober 2011; Tversky 1969). Violations of procedure invariance in the classic preference reversals between monetary valuations and choice may arise merely from underpricing the high probability gamble or from overpricing the high payoff gamble, without necessarily implicating violations of transitivity. Underpricing occurs when someone chooses a gamble over their own cash equivalent of that gamble, whereas overpricing denotes the opposite choice, instead of being indifferent between the two. Therefore, we control for under- and overpricing by measuring participants' WTA for their data as indifference points in trades between private data and cash or goods with the same choice-based procedure (Tversky and Thaler 1990). The procedure, described in the next section, involves consecutive binary (trade or no trade) choices to determine cash or goods equivalents, not open-ended responses, which would not allow us to tell whether participants were under- or overpricing. This leaves violations of transitivity as the only explanation for why M > L.

Consider a consumer who is indifferent between retaining an amount of private data P or receiving an amount of money M in exchange for P (i.e., P ~ M); between retaining that amount of private data P or receiving a number of units of a good G in exchange for P (i.e., P ~ G); and finally also between receiving the same number of units of the good G or an amount of money L (i.e., G ~ L), such that M > L. Assuming the consumer monotonically prefers having more money M to having less money L, these indifference relationships imply that one can construct a cyclic, intransitive preference order such that, for any amount of money ε where $0 < \varepsilon < \frac{\mathrm{M}-\mathrm{L}}{2}$,

$$\mathrm{P} \succ \mathrm{M} - \varepsilon \succ \mathrm{L} + \varepsilon \succ \mathrm{P}$$

and, because G ~ L,

$$\mathrm{P} \succ \mathrm{M} - \varepsilon \succ \mathrm{G} + \varepsilon \succ \mathrm{P}.$$

In other words, the consumer would rather keep their private data than give them up in exchange for receiving M minus a small amount ε (i.e., $\mathrm{P} \succ \mathrm{M} - \varepsilon$). Second, they would also prefer to receive an amount of money $\mathrm{M} - \varepsilon$ rather than a number of goods G plus the small amount of money ε (i.e., $\mathrm{M} - \varepsilon \succ \mathrm{G} + \varepsilon$). Yet, they would prefer to receive $\mathrm{G} + \varepsilon$ in exchange for their private data P rather than retain their data (i.e., $\mathrm{G} + \varepsilon \succ \mathrm{P}$), revealing the intransitivity.

While compatibility effects of exchange media on consumer preferences may not be limited to exchanges of consumers' private data, studying them in this context is pertinent and urgent. A systematic discrepancy between consumer valuations in money versus goods and the implied violation of preference transitivity suggest that there is an "incorrect" medium of exchange in the ubiquitous markets for private data, which may induce bias in valuations and thus render market exchanges inefficient (Grether and Plott 1979; Kahneman, Knetsch, and Thaler 1990).

OVERVIEW OF STUDIES AND EXPERIMENTAL PARADIGM

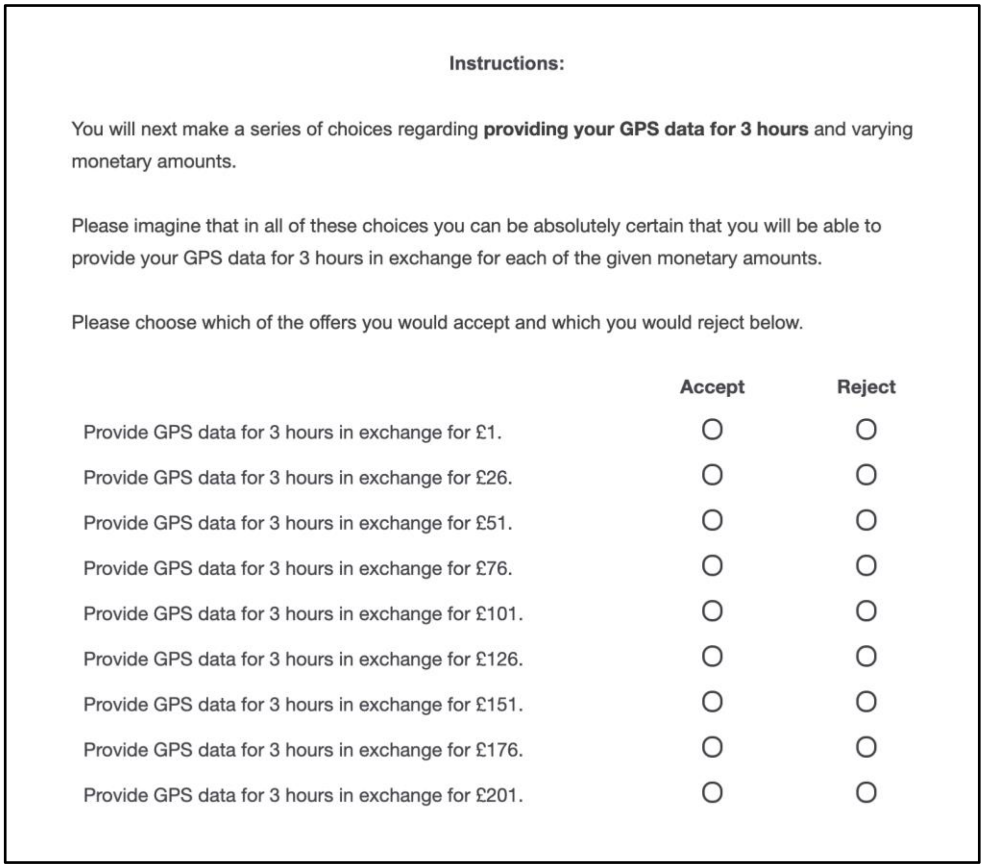
We examine this implied intransitivity in privacy valuations across eight preregistered online experiments. In all our experiments, we compare money and goods as exchange media. Experiments 1a and 1b use an incentive-compatible procedure to demonstrate the basic effect that participants systematically value their private data more when they exchange them for money compared to when they exchange them for goods. We then replicate this effect for multiple types of private data in Experiment 2a, rule out a potential range effect from our value elicitation procedure as an alternative explanation in Experiment 2b, and show that our effect is not driven by our exclusion criteria in Experiment 2c. Experiments 3a and 3b provide process evidence in support of our hypothesis that the intransitivity of privacy valuations arises from greater compatibility of private data with money than with goods. Lastly, Experiment 4 provides additional process evidence by showing that this intransitivity only occurs for private data, not for participants' labor, for which preferences are well-defined.

All studies but Experiment 3 use the same experimental paradigm. Each participant makes three assessments. First, they indicate how much money they would demand in exchange for a certain amount of their private data (privacy to money). Second, they indicate how many units of a good they would demand for those same data (privacy to goods). Third, they indicate how much money they would be equally interested in receiving instead of receiving that number of units of the good (goods to money). This serves as a subjective monetary valuation of the goods and an indirect monetary valuation of the private data (privacy to goods to money). The order of these tasks was randomized for each participant, with the constraint that the privacy-to-goods valuation had to precede the goods-to-money valuation because participants needed to establish the value of their data in goods before they could value that bundle of goods in money.

To motivate realistic and accurate responses, we allowed participants to choose the type of good they wanted to use in the exchanges from a list of eight goods (observed choice shares across all studies in parentheses): Amazon movie rentals (15.7%), Clif Builder's protein bars (6.6%), Hanes socks (4.6%), Kindle E-books (17.4%), Shell gasoline (22.1%), Yellow Tail wine (11.2%), Keurig K-Cups (2.1%), or Starbucks beverages (20.4%). We chose these goods for two reasons. First, we wanted to demonstrate the discrepancy in valuations across different types of barter exchanges, including both digital (e.g., Amazon movie rentals) and physical (e.g., Starbucks beverages) goods to represent a range of offerings participants might receive in exchange for their data (e.g., from digital goods to free samples of products). Second, we wanted to set up a conservative test of our hypothesis by including goods that can be counted so that participants could equate the value of their private data to a specific number of units of a good. While companies often offer goods that are difficult to quantify (e.g., access to a social network) in return for private data, offering our participants goods that can be counted should make these goods more compatible with being valued in money, because both goods and money can be counted. In turn, the greater compatibility should raise participants' monetary valuations of these goods compared to nonquantifiable goods and thus reduce the difference between direct (privacy to money) and indirect (privacy to goods to money) valuations of privacy that we hypothesize.

To ensure the normative equivalence of eliciting participants' preferences for their private data that is required for procedure invariance to hold, we measured all valuations using the same Multiple Price List (MPL) procedure (Andersen et al. 2006; Kahneman et al.1990; Wertenbroch and Skiera 2002). Participants indicated their WTA in money (i.e., the cash equivalent M) and in goods (i.e., the goods equivalent G) in exchange for their private data in an ordered series of accept-reject questions. For instance, in Experiments 1a and 1b we asked participants whether they were willing to exchange three hours of their GPS data for £1, then asked if they would exchange it for £26, £51, etc. on the same screen. The last tradeoff they considered was £201.[1] Likewise, we asked them whether they would exchange their data for a rising number of units of the good they had selected (privacy to goods). In Experiments 1a and 1b, these tradeoffs ranged from 1 to 51 units of their preferred good. We used three price lists for valuations in money and two lists for valuations in goods to increase the precision of our results. For instance, if a participant was willing to exchange their GPS data for £51 but not for £26, they would see a second, narrower price list. This subsequent list would range from £26 to £51, with smaller increments of £5 between the choices, followed by the third list with a range of £5 and increments of £1. We estimated an indifference value for each participant from their responses to the last price list, by taking the midpoint between the lowest amount for which they accepted the exchange and the highest amount for which they rejected the exchange. We treated these indifference values as point estimates of participants' WTA for their private data in money and in goods. Figure 1 illustrates our stimuli.

Figure 1. Sample Stimulus (The Initial MPL in the Direct Elicitation of Cash Equivalents of Private Data in Experiments 1a and 1b).



Instructions:

You will next make a series of choices regarding **providing your GPS data for 3 hours** and varying monetary amounts.

Please imagine that in all of these choices you can be absolutely certain that you will be able to provide your GPS data for 3 hours in exchange for each of the given monetary amounts.

Please choose which of the offers you would accept and which you would reject below.

	Accept	Reject
Provide GPS data for 3 hours in exchange for £1.	O	O
Provide GPS data for 3 hours in exchange for £26.	O	O
Provide GPS data for 3 hours in exchange for £51.	O	O
Provide GPS data for 3 hours in exchange for £76.	O	O
Provide GPS data for 3 hours in exchange for £101.	O	O
Provide GPS data for 3 hours in exchange for £126.	O	O
Provide GPS data for 3 hours in exchange for £151.	O	O
Provide GPS data for 3 hours in exchange for £176.	O	O
Provide GPS data for 3 hours in exchange for £201.	O	O

Once we had estimated a participant's goods equivalent of their private data, they valued these units in money (goods-to-money, i.e., the cash equivalent L of the goods equivalent G), using MPLs with the same increments as in the privacy-to-money task above. For instance, starting at £1 and increasing in increments of £25, participants indicated whether they preferred to receive the amount of money from the price list or the number of units of the good.[2] Based on their lowest acceptable amount and their highest unacceptable amount, participants then saw a second price list with smaller increments of £5 between these two amounts and then a final price list with increments of just £1. We again used the midpoint between the last two amounts on that final list as a point estimate of the participant's indifference value between money and goods. This valuation L of the bundle of goods G serves as a cash equivalent of the value participants placed on their data when trading them for goods (privacy to goods to money). Our analyses compare L to participants' direct WTA in money M (privacy to money). All stimuli, data, and Stata do-files are at: https://osf.io/f2p7u/?view_only=442638c1133142c18a64ba796b15c513.

EXPERIMENTS 1A AND 1B: A HIGHER PRICE FOR PRIVACY IN MONEY THAN IN GOODS

Experiment 1 used the methodology outlined above. Participants valued their data in money, in goods, and then valued those goods in money. As an incentive to respond carefully and truthfully, participants entered a lottery with a chance that one of their choices would be implemented. To test for the hypothesized intransitivity, we compared the means of the direct monetary valuations of the private data to those of the indirect valuations via goods (within participants) and examined the proportion of participants who were intransitive in the predicted direction (between participants). To check the reliability of the hypothesized intransitivity, we ran the same experimental design twice. Experiments 1a and 1b are preregistered at https://aspredicted.org/blind.php?x=gk2s95 and https://aspredicted.org/blind.php?x=ji4hn9.

Method

We recruited N = 301 participants (M_{Age} = 38.53, SD_{Age} = 10.78; 61.3% female) in Experiment 1a and N = 140 (M_{Age} = 38.81, SD_{Age} = 11.36; 59.3% female) participants in Experiment 1b through Prolific Academic for £.45 each. Participants first indicated whether they owned a smartphone, since this study used smartphone location (GPS) data as the private data under consideration. Those who did not own a smartphone were paid and excluded from participating in the rest of the study (N = 4 in Experiment 1a, N = 4 in Experiment 1b).

We told the remaining participants that they would be considering various trade-offs between receiving money or goods on the one hand and providing GPS data collected by their smartphone over a three-hour period on the other. These data would then be given to a company. To induce incentive-compatibility, participants learned that one of them would be randomly selected from all the participants in their respective version of the experiment (301 in Experiment 1a, 140 in Experiment 1b) to have one of the choices they made carried out with real money. This ensured that every single choice that participants made on the MPLs would potentially be binding. We conducted the random selection after the data collection was completed and paid the winners the cash equivalent to a randomly selected choice of theirs without collecting their data.

Participants proceeded by selecting the good from the list of eight goods described above that they would be most interested in receiving. They then started the MPL procedure, counterbalancing the privacy-to-money and privacy-to-goods exchanges as described above. The initial price lists ranged from £1 to £201 (privacy to money, goods to money) and from one unit to 51 units of the goods (privacy to goods). We chose these ranges after pretesting for how much most participants would ever be willing to trade their data.

In each MPL, participants saw a series of binary trade-offs between their GPS data and increasing amounts of cash, or of goods, each of which they could reject or accept (e.g., “Provide GPS data for 3 hours in exchange for £1”) to estimate the cash (M) and goods (G) equivalents of their private data. To determine the value (L) of the goods equivalent, they then selected which in a series of tradeoffs made them indifferent between the goods equivalent (G) and increasing amounts of cash (e.g., “Receive 11 gallons (of gasoline) or £1.”). Lastly, we collected their Prolific ID, thanked, debriefed, and paid them.

As preregistered, we first excluded participants who provided inconsistent responses. For instance, a participant might have rejected £51 for their data on one screen, and then accepted that offer on the subsequent screen where it was the lower anchor on the price list. This makes it impossible to impute a valuation. Second, also as preregistered, we excluded participants whose valuations fell outside of the initial MPL range as they accepted the minimum trading amount for their private data or rejected the maximum amount in at least one of the three valuation tasks. In Experiment 1a, 25 participants were excluded for inconsistencies, 71 for falling outside the range, and 23 for both, yielding a final sample of N = 178. In Experiment 1b, nine participants were excluded for inconsistencies, 25 for falling outside the range, and 18 for both, yielding a final sample of N = 84. Experiment 2c tests whether these exclusion criteria impact our findings.

Results

All results for Experiments 1a, 1b, 2a, 2b, and 2c are presented in Table 1. Unless otherwise noted, all comparisons of mean valuations across medium type were done using t-tests throughout this paper. Likewise, unless stated otherwise, all comparisons of proportions were done using proportions tests. For each test, we set the null hypothesis as the proportion of interest being equal to the observed proportion of comparison. For instance, to test whether the proportion of participants who were intransitive in the direction which our hypothesis predicted was equal to the proportion of participants who were intransitive in the opposite direction, the null hypothesis in Experiment 1a was that the proportion of participants intransitive in the predicted direction equaled 29.8%. Because the tests involved multiple comparisons, we also applied the more conservative Holm-Bonferroni significance (alpha) levels (Holm 1979) to these proportions tests; all relevant proportion results remain significant.

As predicted, mean valuations of private data in both Experiments 1a and 1b were higher when measured in money than when measured in goods, which participants then valued in money. Also as predicted, in both Experiments 1a and 1b a greater proportion of participants exhibited intransitive preferences in the direction predicted by our hypothesis (i.e., they valued their data more in money than in good, with M > L) than the proportion of participants who exhibited intransitive preferences in the opposite direction (M < L). Also as predicted, the proportion of participants whose privacy preferences were intransitive in the predicted direction (M > L) exceeded the proportion of participants whose preferences were transitive (M = L). Finally, a greater proportion of participants were intransitive in the opposite direction than were transitive.

Discussion

In support of our hypothesis, participants valued their private geolocation data more when they considered exchanging these data for money than when they considered exchanging the data for goods in an incentive compatible choice. This effect held both within and between participants by comparing participants' mean valuations of data and by examining the proportion of participants who exhibited the intransitive pattern of preferences that our theory predicts. The effect occurs reliably, as shown by the identical pattern of results in both Experiments 1a and 1b.

Table 1. Study 1a, 1b, 2a, 2b, and 2c Results (Standard Deviations, Degrees of Freedom, and Number of Participants in Parentheses).

	Experiment 1a	Experiment 1b	Experiment 2a	Experiment 2b (Narrow-Range)	Experiment 2b (Wide-Range)	Experiment 2c
Mean Value of Data in Money (Direct Valuation M)	£49.45 (£42.94)***	£47.67 (£39.29)***	£52.23 (£63.34)**	£39.71 (£43.77)**	£43.87 (£43.53)*	$34.80 ($45.01)***
Mean Value of Data in Goods (Indirect Valuation L)	£40.70 (£37.28)***	£41.43 (£34.73)***	£36.14 (£39.81)**	£30.02 (£28.07)**	£37.17 (£33.90)*	$29.46 ($40.05)***
Test Statistic from Analysis	t(177) = 3.49	t(83) = 3.08	F(1, 136) = 9.14,	F(1,193) = 9.80	F(1,193) = 4.46	t(274) = 3.96
Percent Intransitive Predicted (M > L)	54.5% (N = 97)	58.3% (N = 49)	50.0% (N = 73)	55.0% (N = 55)	46.3% (N = 44)	33.8% (N = 93)
Percent Intransitive Opposite (M < L)	29.8% (N = 53)	25.0% (N = 21)	37.0% (N = 54)	31.0% (N = 31)	35.8% (N = 34)	18.2% (N = 50)
Percent Transitive (M = L)	15.7% (N = 28)	16.7% (N = 14)	13.0% (N = 19)	14.0% (N = 14)	17.9% (N = 17)	48.0% (N = 132)
Proportions Test (Intransitive Predicted vs. Opposite)	z = 7.21, *p* < .001	z = 7.06, *p* < .001	z = 3.26, *p* = .001	z = 5.19, *p* < .001	z = 2.14, *p* = .032	z = 6.72, *p* < .001
Proportions Test (Intransitive Predicted vs. Transitive)	z = 14.20, *p* < .001	z = 10.25, *p* < .001	z = 13.28, *p* < .001	z = 11.82, *p* < .001	z = 7.23, *p* < .001	z = 4.71, *p* < .001
Proportions Test (Intransitive Opposite vs. Transitive)	z = 5.15, *p* < .001	z = 2.05, *p* = .040	z = 8.61, *p* < .001	z = 4.90, *p* < .001	z = 4.55, *p* < .001	z = 9.90, *p* < .001
Observations	178	84	146	100	95	275

Notes: We did not preregister proportion analyses for Experiments 1a and 1b. However, preregistered replications of these proportions results are reported in Experiments 2a, 2b, 2c, and 4. In Experiment 2a, participants valued 10 types of private data which had different magnitudes of value (see Web Appendix C).
p* < .05, ***p* < .01, * *p* < .001

EXPERIMENTS 2A, 2B, AND 2C: CHECKS OF EXTERNAL AND INTERNAL VALIDITY

Experiments 2a-c replicate the pattern of data found in Experiment 1 while varying three methodological aspects. In Experiment 2a, participants evaluated 10 different types of private data to show that the discrepancy in valuations generalizes to other types of private data beyond geolocation data. Experiment 2a is preregistered at https://aspredicted.org/blind.php?x=2x69dk.

Experiment 2b addresses a potential limitation of the MPL value elicitation method. MPLs may constrain participants' valuations, because the experimenter sets the range of possible values participants can choose (Bohm, Lindén, and Sonnegård 1997; Poulton 1975). For example, in Experiment 1 participants could not indicate a specific WTA for their private data above 51 units of their chosen good. Even though we had pretested acceptable ranges of MPL values for Experiment 1, the maximum value in the MPLs may have limited the number of goods that some participants demanded for their data, biasing their valuations in goods downwards, which might have artefactually enhanced the effect we had hypothesized. Experiment 2b varies the range of maximum MPL values to address this concern. One condition features the same privacy-to-goods price list range as Experiment 1, while the other almost doubles the range to 101 units. If the range of the number of goods contributed to the effect, we would expect a moderation of the effect in the latter condition. We would also expect fewer participants to reject the maximum privacy-to-goods offer when the range of goods offered is wider. Experiment 2b is preregistered at https://aspredicted.org/blind.php?x=x2fi87.

Experiment 2c addresses a second potential concern with MPL elicitations, that is, the number of participants who are excluded from the analyses due to unimputable, missing valuations. In Experiment 1, we excluded a large number of participants from the analyses because they accepted the minimum valuation or rejected the maximum valuation in a price list, which made it impossible to calculate an indifference point (see method section, Experiment 1). Likewise, we excluded several participants who made inconsistent choices. To check whether the intransitive preference pattern we found in Experiment 1 is sensitive to exclusions, Experiment 2c prescreened participants to yield an experimental sample of individuals who were more likely to provide usable observations. Experiment 2c also accounts for participants who would give up their data without compensation to test the impact of including them in our analyses on our results. Experiment 2c is preregistered at https://aspredicted.org/blind.php?x=t47ea5.

Method

Experiments 2a-c followed the general experimental paradigm with specific adjustments for each experiment. The initial price lists in Experiments 2a ranged from £1 to £276 and from one unit to 51 units of the participant's preferred good. In Experiment 2b, these values ranged from £1 to £201, and one unit to 51 or 101 units. In Experiment 2c, the price lists ranged from \$1 to \$276 and 1 unit to 51 units.

Experiment 2a. We recruited N = 350 participants (M_{Age} = 36.86, SD_{Age} = 12.30; 67.4% female) from the United Kingdom through Prolific Academic to participate for £.40 each. In a mixed experimental design, we randomly assigned each participant to one of 10 different types of private data (between participants) of varying degrees of sensitivity that we asked them to value both in money and in goods (within participants): (1) three hours of their GPS geolocation data; (2) 10 hours of browsing history; (3) details of the last two purchases they had made online; (4) the titles of the last 10 songs they had listened to; (5) a list of the last 20 online studies they had participated in; (6) their height and weight measurements; (7) a list of all the apps on their phone; (8) the last five messages they had sent on any platform; (9) a sample of their saliva; (10) their phone number, e-mail address, and mailing address). As preregistered, we excluded participants who made consistency errors (N = 20), accepted the minimum or rejected the maximum values in any of the three MPL assessments (N = 124), or did both (N = 60; N = 204 total exclusions (see method section, Experiment 1, for why these participants were excluded). Thus, our final sample consisted of N = 146 observations.

Experiment 2b. We recruited N = 362 participants (M_{Age} = 36.63, SD_{Age} = 11.88; 66.8% female) from the United Kingdom via Prolific Academic to participate for £.45 each. In a mixed experimental design, we randomly assigned participants to one of two initial price list conditions (between participants), a narrow-range or a wide-range condition, with which they began valuing their private data in money and in goods (within participants). In the narrow-range condition, we presented participants with an MPL ranging from one unit to 51 units of their preferred good type when assessing whether they would trade their data for goods. In the wide-range condition, the range was from one to 101 units. All other price lists were identical across conditions. After excluding participants who made consistency errors (N = 44), accepted the minimum or rejected the maximum values in any of the three MPL assessments (N = 81), or did both (N = 42; N = 167), our final sample comprised of N = 195 observations.

Experiment 2c. This experiment replicated Experiment 1 with a more select sample of participants that included not only those who would exchange their data for a price but also those who would give up their data for free and participants who could successfully complete an MPL task. To do this, we prescreened our sample prior to running the basic experiment. This allowed us to minimize exclusions during the actual experiment. At the screening stage, we recruited N = 603 American participants on Prolific Academic to participate for £.15. We first screened out participants who would never trade their data and participants who failed at least one of two attention checks. Full details of the preregistered screening procedure are in Web Appendix B.

Two hundred seventy-one participants were eligible to continue to the main experiment, 234 of whom returned and completed the main experiment for £.40 each (M_{Age} = 31.42, SD_{Age} = 11.33; 47.0% female). From these, we excluded participants who made consistency errors (N = 20), rejected the maximum value in any of the three MPL assessments (N = 18), and/or did not complete the experiment (N = 1). If a participant accepted the minimum value when trading their data for both money and goods, we replaced the corresponding WTA values with $0 and 0 units in our analyses (N = 12). Additionally, if a participant accepted the minimum valuation in goods but not money, or vice-versa, we excluded them from analyses (N = 16). This is since, by our coding in this experiment, accepting a minimum valuation indicates a WTA of $0 or 0 units. Such participants would thus be inconsistent; accepting no value in exchange for their data with one medium, but not the other. This left 186 observations with 48 participants excluded based on meeting at least one exclusion criterion during the main experiment. After adding 89 observations with WTAs of $0 and 0 units for those participants who had indicated at the screening stage that they would give their data away for free, our final sample consisted of 275 observations.[3]

Results

Experiment 2a. Mean data valuations and proportions of intransitive and transitive preferences across the different types of data are shown in Table 1; means and proportions for individual data types can be found in Web Appendix C. As predicted, participants valued their private data more when considering exchanging them for money, and this effect generalized across different data types. Eight of the ten data types show directionally greater valuations in money than in goods. A 2 (data valuations in goods or in money) ×10 (type of private data) mixed-design ANOVA revealed this to be a significant within-participants main effect of the exchange medium on valuations ($F(1, 136) = 9.14$, $p = .003$).[4] While there was a between-participants main effect of data type on data valuations across exchange media $F(9, 136) = 3.11$, $p = .002$), there was no significant interaction effect $F(9, 136) = 1.15$, $p = .333$). Also as predicted, proportions tests showed that more participants exhibited preferences that were intransitive in the predicted direction (M > L) than intransitive in the opposite direction (M < L) or transitive (M = L).

Experiment 2b. Mean data valuations and proportions of intransitive and transitive preferences are shown in Table 1. We conducted a repeated-measures ANOVA to test for the basic effect and whether there was any attenuation in this difference across ranges. The dependent variable was the value placed on a participant's data. The independent variables were range and valuation type as the repeated factor. A 2 (range) × 2 (valuation type) mixed-design ANOVA revealed no significant interaction between range and valuation type ($F(1,193) = .45$, $p = .502$; Table 1). There was a main effect such that participants valued their data more in money than in goods ($F(1,193) = 13.66$, $p < .001$), but no main effect of range ($F(1,193) = 1.30$, $p = .255$). Planned contrasts revealed that participants valued their private data significantly more in money than in goods in both the narrow ($F(1,193) = 9.80$, $p = .002$) and the wide range condition ($F(1,193) = 4.46$, $p = .036$). Also as predicted, proportions tests showed that in both conditions more participants exhibited preferences that were intransitive in the predicted direction (M > L) than intransitive in the opposite direction (M < L) or transitive (M = L). To confirm this result, we ran a multinomial logistic regression, which detected no difference in these proportions between the narrow and wide range conditions (Web Appendix E).

In addition to our preregistered analyses, we also tested whether a greater proportion of participants rejected the maximum privacy-to-goods offer in the narrow range condition than in the wide range condition. This test included the sub-sample of participants who met our inclusion criteria (see method section, Experiment 1), minus the requirement that they not reject the maximum number of goods offered. There was no difference in the proportion of participants who rejected the maximum number of goods in the narrow range condition (4.8% of 105 participants) and the wide range conditions (4.0% of 99 participants; z = .25, *p* = .802).

Experiment 2c. Mean data valuations and proportions of intransitive and transitive preferences are shown in Table 1. Replicating our main effect again, participants valued their data more in money than in goods. Also, more participants were again intransitive in the predicted direction (M > L) than were intransitive in the opposite direction (M < L). While more participants were transitive than intransitive in the predicted direction and intransitive in the opposite direction, this is driven by the inclusion of 101 participants who stated they would give their data away for free. These participants alone made up 36.7% of our final data set with 275 participants (see method section, Experiment 2c). Because they valued their data at $0 and 0 units, they were transitive by design. Despite this, 33.8% of our sample was intransitive in the predicted direction, which is viewed as a notable level of intransitivity in the literature (Kivetz and Simonson 2000).

Discussion

Throughout Experiments 2a, 2b, and 2c, we found that our effect generalizes across different types of private data and is robust to methodological variations. In Experiment 2a, participants placed a higher value on their data when exchanging the data for money as opposed to goods across diverse types of private data. In Experiment 2b, participants again valued their data more in money than in goods, regardless of the range of the number of goods from which they could choose. In Experiment 2c, the effect persisted in a more select, prescreened sample, demonstrating the robustness of our effect to our exclusion criteria.

EXPERIMENT 3: PRIVATE DATA ARE MORE COMPATIBLE WITH VALUATIONS IN MONEY THAN IN GOODS

So far, we have shown that consumers value their private data more when they value them in money than when they value them in goods, creating an intransitivity of preferences for private data. We hypothesized that this effect arises because valuations of private data are more compatible with being measured in money than in goods. Consistent with Slovic et al. (1990) and Tversky et al. (1988), consumers should thus find it easier to value their data in money than in goods and place a greater weight and higher value on their data in monetary valuations.

To test this psychological process explanation, Experiment 3 employs an established experimental paradigm that demonstrates compatibility effects in preference reversals (Lichtenstein and Slovic 1971; Tversky et al. 1988; Fischer and Hawkins 1993). In this paradigm, an input variable (e.g., units of private data) is found to be compatible with an output scale (e.g., units of money or goods) if participants weigh the input more when they directly *match* its equivalent on the output scale than when they *choose* between different combinations of levels of the input and the output scale, that is, when they value the input in a choice rather matching it on the output scale.

We adapt this paradigm such that participants in a *choice condition* choose between exchanging a small amount of private data for a small amount of compensation ($-P_L$, $+C_L$) and exchanging a larger amount of private data for a larger amount of compensation ($-P_H$, $+C_H$). Participants in a *matching condition* consider the same trades, but with a missing value for $+C_H$ for how much they would receive as compensation in the trade, in which they would give up the larger amount of private data. They then fill in this missing value with the matching amount of compensation $+C_X$ that would make them indifferent between the two trades. Matching values that are higher (lower) than the corresponding fixed value $+C_H$ in the choice condition imply that participants prefer the trade that involves exchanging the small (larger) amount of private data for the small (larger) amount of compensation, were they in the choice condition. That is, $+C_X > +C_H$ implies $(-P_L, +C_L) \succ (-P_H, +C_H)$, whereas $+C_X < +C_H$ implies $(-P_L, +C_L) \prec (-P_H, +C_H)$.

Therefore, if a smaller proportion of participants provides matching values that imply a preference for exchanging the large amount of private data for more compensation (i.e., $+C_X < +C_H \Rightarrow (-P_L, +C_L) \prec (-P_H, +C_H)$) than the proportion of participants who prefer that option $(-P_H, +C_H)$ in the choice condition (i.e., if participants in matching are less generous with their data than participants in choice), participants value their data more when valuing them in matching than when valuing them in a choice. They would require more compensation to give up their private data in matching than in choice, suggesting that the output variable is "compatible" with the input variable private data (Tversky, Sattah, and Slovic1988).

To test our hypothesis of greater compatibility of private data with money than with goods, Experiment 3 compares valuations in choice and matching between participants who value their private data in money and participants who value their data in goods. We predict that fewer participants will prefer to give up the large amount of data $(-P_H, +C_H)$ in matching than in choice in cash exchanges. That is, we expect a stronger preference in matching than in choice to forego the larger monetary compensation and to give up only a small amount of data, implying higher data valuations in exchanges for money in matching than in choice, a compatibility effect of data and money. In goods exchanges, we expect this difference between matching and choice to attenuate, as we predict no such compatibility effect of data and goods. The preregistration applies to both Experiments 3a and 3b and is at https://aspredicted.org/blind.php?x=uw3ph6.

Method

To ensure the robustness of our findings, we ran the same experiment twice; Experiment 3a and Experiment 3b. For Experiment 3a we recruited N = 499 participants (M_{age} = 37.49, SD_{age} = 12.68; 44.8% female) from the United Kingdom through Prolific Academic to participate for £.10 each. For Experiment 3b, we recruited N = 500 participants (M_{age} = 36.73, SD_{age} = 13.15; 72.4% female) from the United Kingdom. We paid participants £.12 each. We allocated them randomly to four experimental conditions in a 2 (money vs. goods) × 2 (choice vs. matching) between-participants design. The first factor manipulated whether participants considered trading their private data for money or for goods (Starbucks beverages; we chose those due to their popularity in our previous studies).

The second factor, choice vs. matching, implemented the standard scale compatibility test described above. Participants in the *choice conditions* were presented with a choice between two trades, an exchange of three hours of their private GPS data in return for a small amount of compensation ($-P_L$, $+C_L$) or an exchange of 10 hours of their private GPS data in return for higher compensation ($-P_H$, $+C_H$). In the money condition, $+C_L$ was \$60 and $+C_H$ was \$180. In the goods condition, $+C_L$ was 18 medium Starbucks beverages and $+C_H$ was 54 medium Starbucks beverages. That is, in both the money and the goods conditions, we tripled $+C_L$ to determine the compensation $+C_H$ offered in return for the large amount of private data in Experiment 3a. In Experiment 3b, we more than tripled it in the goods condition, raising $+C_H$ to 100 medium Starbucks beverages, to ensure that the proportion of participants who preferred ($-P_H$, $+C_H$) in the goods-choice condition would be comparable to that in the money-choice condition and be high enough in both the money and the goods conditions to allow for a difference between choice and matching. We asked participants "Between the two exchanges below, which one would you rather make, if you had to choose between them?" Participants could also express indifference. Participants in the *matching conditions* were shown the ($-P_L$, $+C_L$) exchange from the choice conditions (i.e., exchange three hours of their private GPS data for $60 or for 18 medium Starbucks beverages). They were also shown an exchange ($-P_H$, $+C_X$) of 10 hours of GPS data for $*X* (money condition) or *X* Starbucks beverages (goods condition) and asked to indicate a value of *X* such that they would be indifferent between the two exchanges.

Table 2 provides the stimuli, experimental design, and dependent measures.

Table 2. Stimuli for Experiments 3a and 3b.

	Choice Conditions	Matching Conditions
Prompt	You will consider two exchanges and be asked to indicate your preference for which one you would rather make. Even if you dislike both exchanges, please choose the option which you dislike the least between the two. If you truly have no preference between the two, please indicate that you are indifferent between them. Between the two exchanges below, which one would you rather make, if you had to choose between them?	Please provide a value for ***X***, such that you would be indifferent between the following two exchanges. Even if you dislike both exchanges, please provide a value for ***X*** so that you would consider these two exchanges equivalent.
Money conditions	**($-P_L$, $+C_L$)**: Exchange three hours of your GPS data for $60 **($-P_H$, $+C_H$)**: Exchange 10 hours of your GPS data for $180 I am indifferent between these two options	**($-P_L$, $+C_L$)**: Exchange three hours of your GPS data for $60 **($-P_H$, $+C_X$)**: Exchange 10 hours of your GPS data for $***X*** The value of ***X*** that would make me equally likely to make either exchange is $[____].
Goods conditions	**($-P_L$, $+C_L$)**: Exchange three hours of your GPS data for 18 medium Starbucks beverages **($-P_H$, $+C_H$)**: Exchange 10 hours of your GPS data for 54* (*100***) medium Starbucks beverages I am indifferent between these two options	**($-P_L$, $+C_L$)**: Exchange three hours of your GPS data for 18 medium Starbucks beverages **($-P_H$, $+C_X$)**: Exchange 10 hours of your GPS data for ***X*** medium Starbucks beverages The value of ***X*** that would make me equally likely to make either exchange is [____] beverages.

* in Experiment 3a, ** in Experiment 3b

Results

Table 3 shows the proportions of participants in Experiments 3a and 3b with a preference for ($-P_L$, $+C_L$), for ($-P_H$, $+C_H$), or who were indifferent. The critical measure, which we compare across conditions in each experiment, is the preference for ($-P_H$, $+C_H$) between matching and choice because it is directly affected by participants' matching responses $+C_X$.

We first ran logistic regression analyses to evaluate whether the task type had an impact on preference distributions within both the money and goods conditions.[5] The dependent variable was which tradeoff a participant preferred (= 1 if ($-P_H$, $+C_H$), = 0 if otherwise). We combined a preference for ($-P_L$, $+C_L$) and being indifferent between the two tradeoffs into one outcome since they both mean that a participant did not prefer ($-P_H$, $+C_H$). The independent variables were task (= 0 if choice, = 1 if matching), medium (= 0 if goods, = 1 if money), and their interaction. The regression coefficients show how the independent variables impact the likelihood of a preference for ($-P_H$, $+C_H$). We observed significant interaction effects in both Experiment 3a (b = -1.45, 95% CI = [-2.19, -.71], $p < .001$) and Experiment 3b (b = -3.04, 95% CI = [-3.89, -2.20], $p < .001$). We then ran a series of contrast analyses in Stata. As predicted, participants in Experiment 3a were less likely to prefer ($-P_H$, $+C_H$) in matching than in choice in the money conditions (b = -1.50, 95% CI = [-2.04, -097], $p < .001$) but not in the goods conditions (b = -.05, 95% CI = [-.56, .46], $p = .843$). In Experiment 3b, participants were also less likely to prefer ($-P_H$, $+C_H$) in matching than in choice in the money conditions (b = -.72, 95% CI = [-1.24, -.21], $p = .006$), while they were actually more likely to prefer ($-P_H$, $+C_H$) in matching than in choice in the goods conditions (b = 2.32, 95% CI = [1.65, 2.99], $p < .001$).

Table 3. Proportions of Preferred Choices in Experiments 3a and 3b.

Experiment 3a

	(-P_L, +C_L): 3 hours of GPS Data for $60 (18 Beverages)	**(-P_H, +C_H)**: 10 hours of GPS Data for $180 (54 Beverages)	Indifferent
Money / Choice	.250	.645	.105
Money / Matching	.680	.288	.032
Goods / Choice	.357	.381	.262
Goods / Matching	.626	.366	.008

Experiment 3b

	(-P_L, +C_L): 3 hours of GPS Data for $60 (18 Beverages)	**(-P_H, +C_H)**: 10 hours of GPS Data for $180 (100 Beverages)	Indifferent
Money / Choice	.405	.492	.103
Money / Matching	.648	.320	.032
Goods / Choice	.366	.463	.171
Goods / Matching	.048	.895	.057

Discussion

These results show that the impact of matching versus choice on valuations of private data is greater in exchanges with money than in exchanges with goods. Put differently, private data are more compatible with being valued in money than in goods. The stronger preference for trading in a large amount of data for a large number of goods in matching than in choice in experiment 3b even suggests that private data may be 'incompatible' with being traded off against goods, lowering their attribute weight.

EXPERIMENT 4: INTRANSITIVITY IS UNIQUE TO PRIVACY

Experiment 3 has shown that consumer valuations of privacy are more compatible with being measured in money than in goods, which is consistent with the violations of procedure invariance and transitivity we have observed in Experiments 1 and 2. Compatibility effects arise when preferences for the input variable—in this case private data—are not well-defined (Tversky et al. 1988). We therefore predict an attenuation of preference intransitivity when consumers exchange assets with well-established values compared to when they exchange their private data. To test this prediction, Experiment 4 asks MTurk workers to value their privacy or to value their labor, again based on MPLs. MTurk labor has a well-established wage rate band which is familiar to MTurk workers because they repeatedly choose to work for rates within that band. Experiment 4 is preregistered at https://aspredicted.org/blind.php?x=qu3zw8.

Method

We recruited N = 603 American participants (53.0% female) through Amazon Mechanical Turk (MTurk) to participate for $.40 each. We randomly assigned participants to one of two conditions, in which we asked them to name their WTA in money and in goods either in exchange for three hours of their GPS data (privacy) or in exchange for completing a three-hour long MTurk image coding task (labor) in a mixed 2 (private data versus labor, between-participants) × 2 (money versus goods, within-participants) design. The procedure of the experiment followed the standard MPL paradigm described in the Overview of Studies and Experimental Paradigm section. As in Experiment 2, the initial price lists ranged from $1 to $276 and from one unit to 51 units of goods. After excluding participants who made consistency errors (N = 52), accepted the minimum or rejected the maximum MPL value in one of three valuations (N = 88), or did both (N = 44; N = 184 total exclusions) the final sample comprised N = 419 observations for the different analyses.

Results

We conducted a repeated-measures ANOVA where the dependent variable was the value placed on a participant's data and the independent variable was condition (private data or labor) with the exchange medium (money or goods) as the repeated factor. Consistent with a compatibility effect of money on valuations, there was a significant main effect of the exchange medium, showing that valuations in money exceeded those in goods ($F(1, 417) = 10.39$, $p = .001$). Unrelated to testing our theory, a marginally significant main effect showed that participants valued providing three hours of their GPS data more than providing three hours of MTurk labor ($F(1, 417) = 2.90$, $p = .090$). Crucially and as predicted, there was also a significant interaction effect of condition and exchange medium ($F(1, 417) = 7.33$, $p = .007$), showing that the intransitivity in the privacy condition was attenuated in the labor condition. Planned contrasts revealed that, as predicted, participants in the privacy condition valued their data more in money than in goods (M_{Money} = \$57.89, SD_{Money} = \$62.84; M_{Goods} = \$48.15, SD_{Goods} = \$47.87; $F(1,417)$ =

15.16, $p < .001$), whereas participants in the labor condition exhibited no such difference (M_{Money} = \$46.09, SD_{Money} = \$40.49; M_{Goods} = \$45.24, SD_{Goods} = \$36.79; $F(1,417) = .16$, $p = .691$).

Consistent with these results and with the hypothesized pattern of compatibility effects, more participants exhibited intransitive valuations for private data than for labor. In the privacy condition, 49.4% (N = 87) were intransitive in the direction predicted by a greater compatibility effect of money than of goods on valuations, compared to 35.2% (N = 62) who were intransitive in the opposite direction ($z = 3.95$, $p < .001$) and 15.3% (N = 27) who were transitive ($z = 12.55$, $p < .001$). The proportion of participants who were intransitive in the opposite of the predicted direction was also greater than the proportion of transitive participants ($z = 7.32$, $p < .001$).

In contrast and also as predicted, the labor condition showed no such difference between the proportion of participants who were intransitive in the predicted direction (40.3%; N = 98) and intransitive in the opposite direction (43.2%; N = 105; $z = .91$, $p = .365$). Participants were both more likely to be intransitive in the predicted direction ($z = 10.03$, $p < .001$) and intransitive in the opposite direction ($z = 11.24$, $p < .001$) than transitive (16.5%; N = 40).[6]

We ran an additional multinomial logistic regression analysis (Web Appendix G), which confirmed that, as predicted, fewer participants were intransitive in the predicted direction in the labor condition than in the data condition.

Discussion

Experiment 4 demonstrates that the difference between valuations expressed in money and valuations expressed in goods that we have hypothesized and shown throughout to apply to participants' private data does not arise for participants' inputs such as labor, which are characterized by well-defined prices. In the within-participants analysis, participants exhibited systematically intransitive preferences in the predicted direction only when they valued their private data, not when they valued their labor. The attenuation of the intransitivity occurs because participants are more certain of the value of other assets they sell than of the value of their private data, in line with Tversky et al.'s (1988) assertion that compatibility effects arise when preferences are not well-defined. This preference uncertainty for private data triggers the effect of the greater compatibility of privacy valuations with money than with goods. The results of Experiment 4 are also further evidence that our findings do not arise as an artefact of the MPL methodology. Otherwise, participants in the labor condition would have exhibited the same systematic preference intransitivity.

GENERAL DISCUSSION

In a series of eight experiments, we documented a robust intransitivity such that participants valued their personal private data more when they considered whether to sell the data for money than when they considered to barter the data for goods. Experiments 1a and 1b reliably demonstrated this violation of procedure invariance in an incentive-compatible setting. In a test of its external validity, Experiment 2a demonstrated the generalizability of this effect across multiple types of private data. In tests of its internal validity, Experiments 2b and 2c showed that the intransitivity is robust to technical characteristics of the MPL methodology (ranges and exclusion criteria).

We hypothesized that this effect occurs because privacy valuations are more compatible with being expressed in money than in goods. Experiment 3 tested this hypothesis by adapting an established design for demonstrating compatibility effects, comparing preferences elicited in matching and in choice tasks. The experiment provides evidence in support of the hypothesized mechanism by showing greater compatibility of privacy valuations with money than with goods. Finally, Experiment 4 again replicated the basic discrepancy in valuations and demonstrated—consistent with the compatibility principle—that preference (un)certainty provides a boundary condition that moderates the effect: The intransitivity of valuations that arises from the greater compatibility of valuations with money than with goods depends on whether participants value resources with uncertain (private data) or well-defined (labor) value.

Limitations and Future Directions

Our experiments were designed to ensure the internal validity of the findings. Moreover, Experiment 2a illustrated their external validity by replicating the intransitivity of privacy preferences across several types of private data. Yet, it also suggested that the extent of the intransitivity may vary for some types of private data. Future research could focus on how and why the discrepancy between privacy valuations in money and in goods depends on the type of private data that consumers give up. More generally, it will also be interesting to examine how other contextual factors might affect this discrepancy, both experimentally and in actual online environments. For example, companies that offer to buy consumers' data might raise suspicion and lead to consumers to demand even more money for their data.

A potential limitation of our research is that the difference in valuations arises because consumers discount the value of the goods, which they consider receiving in exchange for their personal private data, because goods are less fungible than the money, which they consider receiving in exchange for the data. Any such concerns about the lack of fungibility of goods, however, should lead rational consumers to demand more goods in exchange for their private data to begin with and then value these goods in cash according to their fungibility, arguing against this explanation.

Another limitation of our research is that we show the difference between privacy valuations in money and in goods with experimental data. As we discuss below, our findings of an intransitivity of preferences for privacy call into question whether online markets for private data function efficiently. Therefore, future research should devise methods to detect intransitive preferences for private data in actual online environments to confirm our experimental findings in the field and provide direct evidence of the malfunctioning of markets for privacy.

Theoretical and Practical Implications

Our MPL procedure controls for a potential explanation of the privacy paradox—that online bartering exchanges may depress how much consumers value their data because companies do not usually draw explicit attention to the nature of these exchanges (Barth and de Jong 2017). For instance, Facebook neither asks consumers to assess the type and value of the data it collects, nor does it provide an explicit measure of the information goods (e.g., access to social networks) that it offers in return. In contrast, our procedure makes exchanges of private data explicit, by providing measures of both the data and the compensation. Our procedure creates a conservative test of our hypothesis. Not only should making exchanges explicit boost valuations of private data in bartering, raising them above those in actual online environments; according to the compatibility principle, offering countable goods in exchange for data should also enhance the monetary valuations of these goods and thus reduce any gap between direct and indirect monetary valuations. Yet despite these conservative testing conditions, we still detect the gap that our theory of greater compatibility of private data with money than with goods predicts.

Another potential factor contributing to the privacy paradox could be a present bias in how much consumers value the information goods they receive immediately (e.g., video streaming, search results, etc.) relative to suffering the delayed and uncertain consequences of relinquishing private data (Acquisti 2004). Crucially, a present bias for immediate gratification cannot explain the difference in valuations we observed in our experiments, because participants did not receive any immediate payoffs. If anything, a present bias may exacerbate undervaluing private data in real online exchanges with goods compared to selling them for money.

Our studies uncover a bias, or systematic error, in how consumers value their private data, holding contextual factors constant. We show a systematic difference in valuations depending solely on the medium, money or goods, with which consumers are compensated for their private data, regardless of a difference between stated or revealed preferences. This bias constitutes a violation of two fundamental principles of rational choice, procedure invariance (Grether and Plott 1979; Tversky et al. 1990; Tversky and Thaler 1990) and transitivity (Edwards 1954; von Neumann and Morgenstern 1944). While such violations have been amply documented—but also challenged (Regenwetter et al. 2011)—in other domains, usually in the evaluation of gambles and public policy choices (Slovic et al. 1990; Tversky 1969; Tversky et al. 1988), our results imply an intransitivity of consumer preferences for privacy in a specific direction, that is, a bias of valuing private data more in money than in goods.

The practical significance of this finding arises from how technology companies are compensating consumers for their private data in online interactions, such as searches, targeted advertising, purchases, social network participation, streaming and news consumption, etc. Technology companies typically offer consumers information goods while collecting consumer data, portraying the provision of these goods as "free." Our analysis offers a normative criterion to determine whether companies are compensating consumers adequately. The existence of two different prices M and L at which consumers value their private data suggests that companies might not be paying normatively adequate compensation. Our findings thus also point to a possible resolution of the privacy paradox—consumers reveal their privacy preferences in bartering exchanges of private data in return for information goods, whereas their stated preferences may be more representative of their monetary valuations. While we cannot determine which of the two prices is the correct price, our findings imply that consumers would demand higher prices if companies paid them for their data in money rather than in goods.

Our normative analysis of individual consumer choices therefore suggests that a behavioral bias—an intransitivity of consumer preferences for privacy—may allow technology companies to build market power from collecting private consumer data without (inadvertently or deliberately, we cannot tell) paying consumers adequate compensation. When market participants suffer from biased valuations such as intransitive preferences, markets cannot function efficiently (Kahneman et al. 1990), suggesting a need for regulatory intervention. To protect consumer welfare and remedy the possible inefficiencies in markets for privacy, which our findings suggest, policy makers might want to explore devising methods, by which consumers could sell, rather than barter, their private data to technology companies.

A first step in addressing these inefficiencies would be to assign explicit ownership rights to private data to the consumers with whom these data are associated (Coase 1960). The rights to be forgotten and to data portability under GDPR provide consumers with a degree of control of their data and separate data exchanges and goods exchanges (Ke and Sudhir 2020; CCPA has similar provisions), but they stop short of granting legal ownership. Larger firms' better ability to bear compliance costs may even increase industry concentration under such privacy regulation (Johnson et al. 2021). Giving consumers more control but not legal and commercial ownership of their data may constrain data access by firms and thus, in fact, hamper competition (Wertenbroch 2021). To illustrate, Apple's iOS now asks consumers for explicit consent to allow other firms to track their behavior across apps and share their data. Apple frames this feature as a tool to protect consumers' privacy, whereas firms like Facebook complain that it limits their ability to target advertising and compete (The Economist 2021). In contrast, using pricing as a tool, various private enterprises have launched attempts to enable consumers to sell their data (Harrison 2018).

Our findings support a pricing approach. They suggest that allowing, or perhaps requiring, consumers to sell or rent their data for monetary compensation might dampen the market power that technology companies can amass from obtaining consumers' private data in barter exchanges because it would force companies to compete for the data at market prices much like they compete for labor (Arrieta-Ibarra et al. 2018). Such an approach should allow consumers to better protect their privacy and welfare while using market-clearing pricing to enhance competition and innovation in markets for privacy.

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[1] For price list margins across experiments, see Web Appendix A.

[2] We had participants choose between these two options rather than indicate a WTA or willingness-to-pay for the goods, so as to not impact their valuation through an endowment effect for goods or money (Kahneman et al. 1990).

[3] In Experiment 2c, we gave participants a chance to re-complete an MPL if they made a consistency error. Eleven participants in our final sample received, and successfully used, this second chance.

[4] We also conducted the regression analysis, which we had preregistered instead of the mixed-design ANOVA. This analysis yielded similar results (Web Appendix D).

[5] We mistakenly preregistered the use of ANOVA in our analyses; given the binary nature of the dependent variable, we present a logistic regression model here. An ANOVA shows similar results.

[6] We had preregistered analyses both with and without participants who successfully used a second chance after making a consistency error. However, a programming error made us unable to analyze such observations (see Web Appendix F for the full explanation).

Intransitivity of Consumer Preferences for Privacy

WEB APPENDIX

Table of Contents:

WEB APPENDIX A – MARGINS USED FOR PRICE LISTS THROUGHOUT ALL EXPERIMENTS

	Money	**Goods**
Experiment 1a	£1 - £201	1 unit – 51 units
Experiment 1b	£1 - £201	1 unit – 51 units
Experiment 2a	£1 - £276	1 unit – 51 units
Experiment 2b	£1 - £201	1 unit – 51/101 units
Experiment 2c	\$1 - \$276	1 unit – 51 units
Experiment 4	\$1 - \$276	1 unit – 51 units

WEB APPENDIX B – FULL METHODOLOGY FOR EXPERIMENT 2C

First, we tested whether participants were willing to put a price on their data. We asked them to imagine being requested to give away their GPS data to Company A for compensation. We then asked whether they would give the data away for free to Company A and whether they would never give their data to Company A for compensation. If a participant indicated that they would never give their data away (N = 181), we did not invite them to the main study since they were not in our population of interest.

We next tested whether participants could complete an MPL valuation without committing violations of consistency by asking them to financially value a $200 Uber gift card using an MPL. We did not invite participants to the main study if they were unable to complete this task without making a consistency error (N = 41) and/or valued the gift card at over $200 cash (N = 45) since our experimental paradigm relies on the MPL methodology. Lastly, as an attention check, participants saw an image of a U.S. penny and identified its financial value. Those who were unable to successfully do this (N = 47) were not invited to the main experiment. Sixty participants were not invited to the main study uniquely for one of the reasons listed in this paragraph, while 23 participants who made one of these mistakes were also excluded because they had indicated they would never give their data away. We excluded observations from 241 participants in total for these reasons.

Of the remaining participants, 89 said that they would give their data away for free. We added 89 observations to our final dataset with privacy valuations of zero in both money and goods to proxy for these participants. Among the 273 remaining participants who would sell their data for a price, two provided unusable Prolific IDs, making it impossible for us to invite them to the main experiment.

We invited the remaining participants who provided actual Prolific Academic IDs (N = 271) to the main experiment, 234 of whom completed the experiment. Twenty participants exhibited consistency errors in their responses during the main experiment. Additionally, 18 participants rejected the highest WTA in at least one of their responses. We also excluded observations from participants that accepted the lowest trade in money, but not in goods, vice versa (N = 16). This is since, by our coding in this experiment, accepting a minimum valuation indicates a WTA of $0 or 0 units. Such participants would thus be inconsistent; accepting no value in exchange for their data with one medium, but not the other. Additionally, we excluded one participant who did not finish the study. This resulted in 48 excluded observations.

Of the remaining participants (N = 186), 12 accepted the minimum trades of £1 and one unit when trading their private data for both money and goods, respectively. As such, we replaced these participants' privacy valuations with "0" both in monetary and goods valuations. We opted for 0 instead of 1 since accepting the minimum amount of goods means that the highest amount of goods such participants would reject would have to be less than 1. After adding in the 89 observations to represent participants who indicated they would give their data away for free in the first round, the data for analysis come from a final sample of N = 275.

WEB APPENDIX C - RESULTS BY PRIVATE DATA TYPE IN EXPERIMENT 2A

We introduced participants to the study with the following statement followed randomly by one of these types of private data: “In this study, you will be considering various trade-offs between money, products/services, and providing [one of the data types in the table below].” Means and proportions in bolded italics denote results directionally consistent with the research hypothesis of higher valuations of private data in money than in goods and the proportions of participants exhibiting the resulting preference intransitivity.

Data Type	Valuation of Data in Money	Valuation of Data in Goods	Percentage Intransitive Predicted	Percent Intransitive Opposite	Percent Transitive	Observations (After exclusions)
Three hours of GPS data	**£74.06**	**£51.75**	**62.5% (N = 10)**	**18.8% (N = 3)**	18.8% (N = 3)	16
Last 10 hours of browsing history	**£76.33**	**£52.08**	**66.7% (N = 8)**	**8.3% (N = 1)**	25.0% (N = 3)	12
Details of last two online purchases	£15.30	£15.60	40.0% (N = 4)	60.0% (N = 6)	0% (N = 0)	10
Last 10 songs listened to	£33.25	£55.38	12.5% (N = 1)	87.5% (N = 7)	0% (N = 0)	8
Last 20 HITs participated in	**£32.71**	**£21.14**	**50.0% (N = 7)**	**42.9% (N = 6)**	7.1% (N = 1)	14

Height and weight measurements	**£14.68**	**£12.77**	45.5% (N = 5)	45.5% (N = 5)	9.1% (N = 1)	11
List of all apps on one's smart phone	**£39.69**	**£24.17**	**52.4% (N = 11)**	**33.3% (N = 7)**	14.3% (N = 3)	20
Last five messages sent on any platform	**£48.34**	**£29.50**	**57.9% (N = 11)**	**31.6% (N = 6)**	10.5% (N = 2)	18
A saliva sample	**£83.25**	**£61.05**	35.0% (N = 7)	50.0% (N = 10)	15.0% (N = 3)	20
Phone number, e-mail, mailing address	**£71.30**	**£33.30**	**60.0% (N = 9)**	**20.0% (N = 3)**	20.0% (N = 3)	15

WEB APPENDIX D – PREREGISTERED ANALYSIS FOR EXPERIMENT 2A

The preregistered regression analysis yielded similar results with privacy valuation as the dependent variable, valuation type as an independent variable (0 = goods, 1 = money), and dummy control variables for each type of private data. Standard errors were clustered by participant. The coefficient for valuation type was significant (b = 16.09, 95% CI [7.64, 24.53], p < .001).

WEB APPENDIX E – MULTINOMIAL LOGISTIC REGRESSION ANALYSIS FOR EXPERIMENT 2B.

To further test whether the width of the goods range (narrow versus wide) impacted this pattern, we ran a multinomial logistic regression with transitivity type as the dependent variable (0 = intransitive in the opposite of the predicted direction, 1 = transitive in the predicted direction, 2 = transitive) and condition (0 = wide range, 1 = narrow range) as the independent variable. With intransitivity in the predicted direction as the baseline outcome, each coefficient indicates how the condition impacted the likelihood that a participant exhibited a given type of transitivity, relative to being intransitive in the predicted direction. Condition (i.e., narrow versus wide range) did not have a significant impact on participants' likelihood of being intransitive in the opposite of the predicted direction (b = -.32, 95% CI [-.94, .31], p = .325) or being transitive (b = -.42, 95% CI [-1.23, .39], p = .313) compared to being intransitive in the predicted direction.

WEB APPENDIX F – REASON FOR NOT USING SECOND CHANCE PARTICIPANTS IN EXPERIMENT 4

Those who had made such an error were supposed to be allowed to correct it. We then intended to use observations from such participants in an alternative analysis. However, a programming error in the experiment prevented some participants who made a consistency error from proceeding with the experiment and provide a valuation after all. Thus, we only ran the analyses with participants who did not make a consistency error.

WEB APPENDIX G – ADDITIONAL ANALYSES FOR EXPERIMENT 4

We conducted a multinomial logistic regression with intransitivity type as the dependent variable (0 = intransitive in the predicted direction, 1 = intransitive in the opposite direction, 2 = transitive) and condition as the independent variable (0 = private data, 1 = labor). We set intransitivity in the predicted direction for valuations of data/labor as the baseline outcome. The model shows how providing labor instead of private data impacted a participant's likelihood of being intransitive in the opposite of the predicted direction or of being transitive, relative to being intransitive in the predicted direction. To test for an attenuation of the effect of the exchange medium on valuations in the labor condition, we examined the coefficient that indicates how the labor condition (relative to the data condition) impacted the likelihood of being intransitive in the opposite of the predicted direction. As predicted, this coefficient was positive and marginally statistically significant (b = .41, 95% CI [-.02, .83], p = .061). That is, participants in the labor condition tended to be less likely to be intransitive in the predicted direction, relative to the opposite direction, than participants in the privacy condition. In contrast, the coefficient that indicates how the labor condition (relative to the data condition) impacted the likelihood that a participant was transitive relative to being intransitive in the predicted direction was not statistically significant (b = .27, 95% CI [-.29, .84], p = .344). Taken together, these results show that the difference between valuations in money versus goods attenuated when participants valued their labor rather than their privacy. Specifically, participants in the labor condition were more likely intransitive in the opposite of the predicted direction rather than more likely transitive.