Resolving Rabin's Paradox

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Abstract. Controversies and confusions have arisen as to whether Rabin's classical paradox truly violates expected utility and, more generally, reference dependence, partly due to different terminologies in different fields. By providing the proper theoretical model, we resolve the confusions and make it possible to identify the causes of this long-standing paradox. Further, through use of proper experimental stimuli, we can test the empirical relevance of these causes. Based on direct and indirect (excluding all other causes) evidence, we identify violations of reference independence as the true culprit. Thus, Rabin's paradox provides a positive message, underscoring the importance of reference dependence.

Keywords: Rabin's paradox; reference dependence; loss aversion
JEL Classification: C91, D81

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Imagine that you turn down a 50-50 gamble of losing \$10 or gaining \$11, and you happen to be an expected utility maximizer. Then you will find yourself (absurdly) turning down any 50-50 gamble where you may lose \$100, no matter how large the amount you stand to win. This was Rabin's (2000) paradox, which demonstrated how an innocuous preference has a surprising implication that strongly challenges the empirical validity of expected utility. Rabin's thought-provoking paradox led to theoretical discussions about whether such a preference truly violates expected utility and, if so, what might explain this violation. Rabin suggested that his paradox may provide an argument not only against expected utility but, more generally, against reference independence and thus against all traditional decision models. Several authors (referenced later) tried to rescue reference independence by suggesting other explanations, such as probability weighting, disappointment aversion, background risks, or utility of income. The main purpose of our paper is to resolve Rabin's paradox. We show that his suggestion is right, and reference dependence explains his paradox. Other deviations from expected utility do not contribute to explaining the paradox.

The theoretical debate of Rabin's paradox, abbreviated RP henceforth, was complicated by differences in terminology: (a) Rubinstein (2006) suggested that the term "expected utility" incorporate reference dependence;[1] (b) utility of income was an alternative term for reference dependence (see Figure 1). Wakker (2010 pp. 244-245) reviewed early debates and our §VII gives further details. The abundance of theoretical debates and semantic confusions have been barriers to the resolution of RP. Now, 17 years after its appearance, RP has turned into a classic and its meaning should be settled, theoretically and empirically. We will introduce a theoretical model that can disentangle the various potential causes of RP, and then the experimental stimuli that allow to identify the real cause.

Cox et al. (2013), Csvd hereafter, were the first to provide empirical evidence of the assumed preference patterns in RP. They also provided theoretical results showing exactly when Rabin's calibration paradox refutes various reference-independent theories, including expected utility. Thus, they were the first to

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[1] This was stated most clearly in his footnote 5. Rubinstein took expected utility as an abstract mathematical theory without any empirical commitment, rather than as an economic theory about (rational) human decisions with financial or other traditional outcomes. Thus, he proposed to use the term expected utility even for the irrational case of reference-dependent outcomes.

conclusively show that RP is a genuine violation of expected utility. However, they did not identify the causes of RP. Our study does so.

Rabin (2000) already showed that utility curvature cannot completely explain RP. We show that utility curvature does not play any empirical role at all. Several authors showed that other deviations from expected utility, primarily probability weighting may explain RP theoretically, although these deviations have their own problems.[2] Csvd's data did not provide conclusive evidence on probability weighting, and their formal and empirical analyses (of the traditional RP) did not involve reference dependence. We show that probability weighting, like utility curvature, does not play any empirical role at all in explaining RP, and neither do other reference-independent deviations from expected utility. Rabin (2000, p. 1288) conjectured that loss aversion, necessarily involving reference dependence, is the main cause:

> Indeed, what is empirically the most firmly established feature of risk preferences, loss aversion, is a departure from expected-utility theory that provides a direct explanation for modest-scale risk aversion. Loss aversion says that people are significantly more averse to losses relative to the status quo than they are attracted by gains, and more generally that people's utilities are determined by changes in wealth rather than absolute levels.

Other authors also suggested loss aversion as an explanation (Csvd p. 307; Lindsay 2013; Park 2016; Wakker 2010 p. 244), but no analysis to date formalized or tested this conjecture.[3] We do so by incorporating reference dependence in our theoretical model and by empirical tests.[4] Thus we can settle the case. We thus also confirm that utility of income explains RP, in agreement with suggestions by Cox and Sadiraj (2006) and others.

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[2] References include Barberis, Huang, and Thaler (2006), Barseghyan et al. (2013), Csvd (their §4.1), Neilson (2001), and Waakker (2010 p. 244 5th paragraph).

[3] Many papers formalized reference dependence in other contexts, e.g. in auction theory (using utility of income), WTP/WTA discrepancies, narrow versus broad bracketing, and numerous other topics. Lindsay (2013) shows that preference reversals for risk then always occur. We will not survey this literature.

[4] Chapman & Polkovnichenko (2011) argue that reference dependence is intractable in models of financial markets. They show that reference-independent probability weighting, as studied by Neilson (2001), can theoretically accommodate RP by properly restricting the small-scale risk aversion choices and the background risks assumed. Their footnote 8 points out that the empirical measurement of Neilson's weighting function remains as a problem. We will solve this problem.

I. Notation and Definitions

We consider only two-outcome prospects. By $\boldsymbol{\alpha}_p\boldsymbol{\beta}$ we denote a *prospect* yielding outcome $\boldsymbol{\alpha}$ with probability p and outcome $\boldsymbol{\beta}$ with probability $1-p$. *Outcomes* are money amounts. In reference-independent models, outcomes refer to final wealth and are denoted in bold by Greek letters or real numbers. The *initial wealth*, which is the final wealth level when subjects enter the laboratory in our experiment, is denoted $\mathbf{0}$, as has been customary in classical reference-independent models. It is fixed throughout the analysis and experiment.

By $\succcurlyeq$ we denote a *preference relation* over prospects. A *utility function* U maps outcomes to the reals and is strictly increasing and continuous. The *expected utility* (EU) of a prospect $\boldsymbol{\alpha}_p\boldsymbol{\beta}$ is

$$pU(\boldsymbol{\alpha}) + (1-p)U(\boldsymbol{\beta}). \tag{1}$$

Expected utility holds if there exists a utility function U such that preferences maximize EU.

We next define the most general theory considered in this paper, prospect theory (Tversky & Kahneman 1992), and then specify other theories as special cases. Prospect theory assumes that for every choice situation subjects perceive a particular final wealth level as their reference point, which we denote θ. Commonly, the reference point is the status quo, but it can change within the analysis, for instance due to different framings. This is the crucial difference between the reference point and initial wealth, which is fixed throughout the analysis. Under prospect theory, outcomes describe changes with respect to this variable reference point and are denoted by Greek letters or real numbers in normal typeface. For example, outcome α_θ designates final wealth $\boldsymbol{\alpha}+\boldsymbol{\theta}$ with θ the reference point and α the change. The two different notations (bold and nonbold) for different kinds of outcomes serve to clarify the ambiguities that can arise but should be avoided in RP.

A *weighting function* w maps the probability interval $[0,1]$ to $[0,1]$ with $w(0) = 0, w(1) = 1$, and w strictly increasing. It does not have to be continuous. A *loss aversion parameter* λ is a positive number. *Prospect theory* (PT) holds if there exists a utility function u with $u(0) = 0$, two probability weighting functions w^+ and w^-, and a loss aversion parameter λ such that preferences maximize the *prospect theory* value (PT) of prospects:

$$PT(\alpha_{\theta_p}\beta_\theta) =$$

$$w^+(p)u(\alpha) + (1 - w^+(p))u(\beta) \quad \text{if} \;\; \alpha \geq \beta \geq 0; \tag{2}$$

$$w^+(p)u(\alpha) + w^-(1 - p)\lambda u(\beta) \quad \text{if} \;\; \alpha \geq 0 \geq \beta; \tag{3}$$

$$w^-(p)\lambda u(\alpha) + (1 - w^-(p))\lambda u(\beta) \quad \text{if} \;\; 0 \geq \beta \geq \alpha. \tag{4}$$

The parameters u, w^+, w^-, and λ can in principle depend on the reference point θ. However, they will be stable under small changes of θ such as in our experiment, and we therefore assume that they are independent of θ.[5] The loss aversion parameter can be incorporated into utility by writing

$$U(\alpha) = u(\alpha) \text{ for } \alpha \geq 0 \text{ and } U(\beta) = \lambda u(\beta) \text{ for } \beta \leq 0. \tag{5}$$

U will typically have a kink at 0. We usually denote the reference point as a subscript of the preference symbol rather than of the outcomes. If the reference point θ has been specified, we may therefore write α instead of α_θ. *Utility of income* is the special case where there is no probability weighting, i.e., $w^+(p) = w^-(p) = p$. Thus, it generalizes expected utility by incorporating reference dependence, maintaining expected utility given a fixed reference point.

Markowitz (1952) was among the first to propose reference dependence.[6] However, he did not incorporate probability weighting and made empirically invalid conjectures about utility curvature. Prospect theory corrected these points and was the first reference-dependent theory that could work empirically.

We now turn to reference independent special cases of PT. The first special case we consider is *rank-dependent utility* (*RDU*). It assumes $w^+(p) = 1 - w^-(1 - p) = w(p)$ and $\lambda = 1$ (so that $u = U$). The main restriction is that, following EU, RDU assumes *reference independence*: outcomes are described in terms of final wealth. This can be formalized by assuming that the reference point θ is fixed at 0.[7] We get

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[5] Kahneman & Tversky (1979 pp. 277-278) wrote “However, the preference order of prospects is not greatly altered by small or even moderate variations in asset position. … Consequently, the representation of value as a function in one argument generally provides a satisfactory approximation.”
[6] Other early works include Shackle (1949 Ch. 2 on sign-dependence) and Edwards (1954 p. 395 & p. 405). Edwards later influenced the young Tversky. Arrow (1951 p. 432) discussed reference dependence, pointing out that it plays no role when outcomes refer to final wealth, and criticizing it for this reason. An early appearance of loss aversion is in Robertson (1915 p. 135).
[7] Alternatively, it can be formalized by assuming that preferences and, accordingly, the components of the preference functional depend on outcomes α_θ only through the final wealth $\alpha + \theta$. Yet another

$$RDU(\boldsymbol{\alpha}_p\boldsymbol{\beta}) =$$
$$w(p)U(\boldsymbol{\alpha}) + \big(1 - w(p)\big)U(\boldsymbol{\beta}) \quad \text{if } \boldsymbol{\alpha} \geq \boldsymbol{\beta}. \tag{6}$$

Probability weighting under RDU is *sign-independent*. For gains we have $w(p) = w^+(p)$ but for losses we have a dual $w(p) = 1 - w^-(1-p)$. EU is the special case where $w^+(p) = w^-(p) = w(p) = p$. Then the weighting functions are identical to their duals.

For two-outcome prospects as used in our experiment, nearly all existing reference- and sign-independent nonexpected utility theories are special cases of RDU and, consequently, of PT (Wakker 2010 §7.11). Such theories include the reference-independent version of original prospect theory (Kahneman & Tversky 1979) and disappointment aversion theory (Gul 1991). Hence, the analysis of this paper also applies to those theories.

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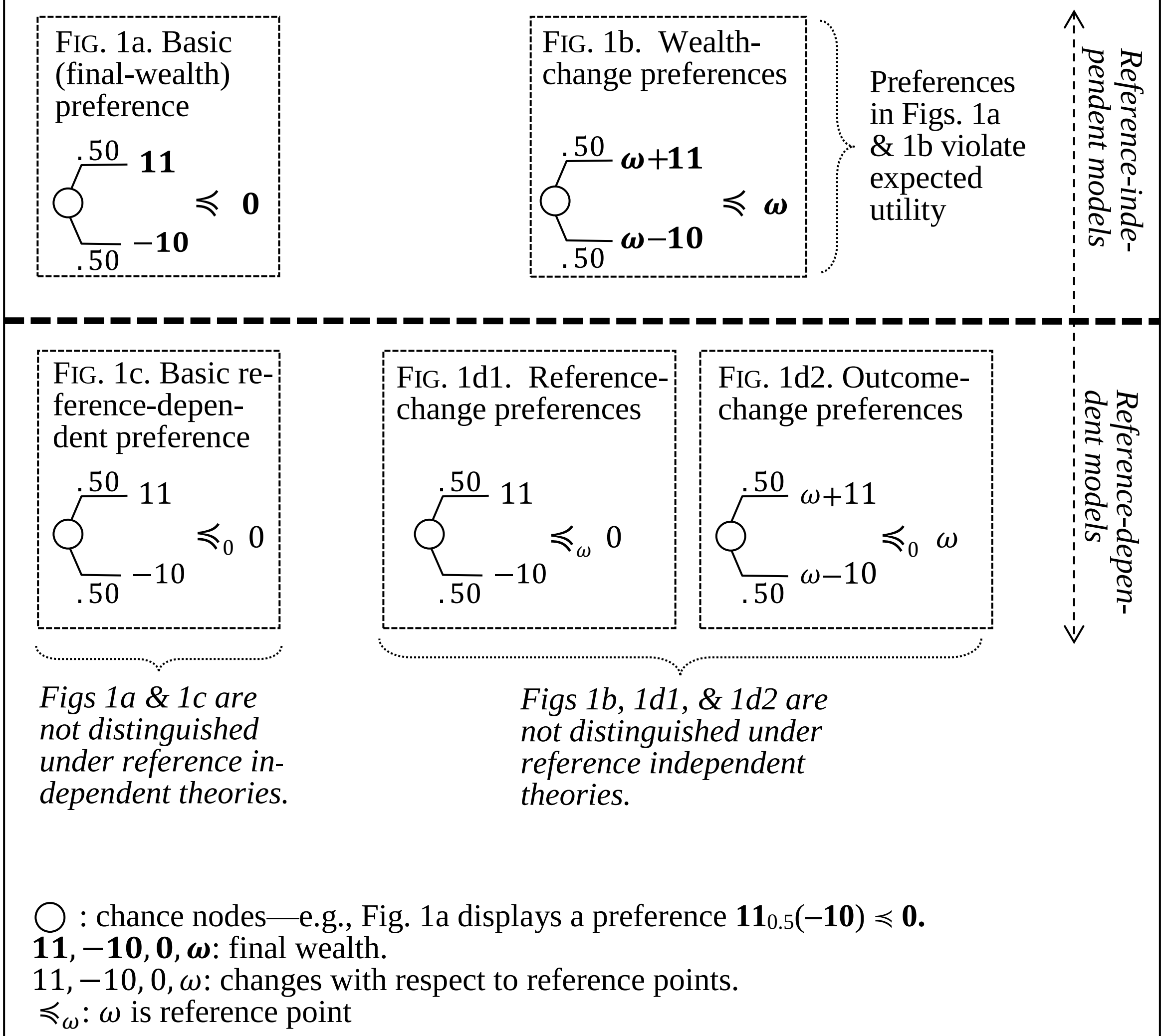
way to interpret RDU as a special case of PT is to assume $\theta = -\infty$, in other words, that all outcomes are gains.

II. The Preferences in Rabin's Paradox: Reference-Dependent versus Reference-Independent Modeling

FIGURE 1. The preferences in Rabin's paradox



Although the formalization of reference-dependence defined in the preceding section has been used in many contexts, it has not yet been used to analyze RP, probably because of the controversial discussions of this paradox. This section shows how, using this formalization, we can identify and isolate potential causes of the paradox. Figure 1 displays the choices in RP. Rabin assumed that people reject a 50-50 prospect of winning **11** or losing **10** (Fig. 1a: *basic (final-wealth) preference*). With the natural status quo of 0, this assumption is empirically plausible for different subjects at different wealth levels; that is, in a "between"-subject sense. It then is also plausible in a "within"-subject sense, i.e., for one subject at different wealth levels. For instance, if for a given subject in our experiment, the basic preference holds for most subjects €11 richer than her, then it is likely to also hold for this subject if she were €11 richer. We call this argument the between-within argument. Under expected utility, it implies the *wealth-change preferences* in Fig. 1b for a range of wealth levels $\boldsymbol{\omega}$. Csvd's experiment covered the range $\boldsymbol{\omega \in [-100, 100000]}$.

Figs 1a and 1b, above the dashed fat line, contain reference-independent presentations. Reference-dependent presentations are below the dashed fat line, in Figs. 1c, 1d1, and 1d2, with reference points specified as subscripts of preferences. Fig. 1c presents the *basic reference-dependent preference,* with reference point 0. The *reference-change preference* of Fig. 1d1 is then plausible for the various reference points ω concerned, say $\omega \in [-100, 100000]$. We will discuss later whether the *outcome-change preference* (Fig. 1d2) is plausible.

EU, as all other reference independent theories, does not distinguish reference-change preference from outcome-change preference (Figs. 1d1 & 1d2), equating them also with the wealth-change preference in Fig. 1b. This is indicated by the brace in the figure below these three figures. It explains EU's "between-within" move from the basic preference to the wealth-change preference. Such a move, via the equivalence between Fig. 1d1 and Fig. 1d2, leads to highly risk averse preferences that cannot be accommodated by EU.

Theoretically, many explanations of the RP have been considered. Under theories that maintain reference independence, one potential cause is that not only utility curvature but also probability weighting contributes to risk aversion (for instance under RDU). Under other theories, such as prospect theory, reference dependence is a potential cause. Then people treat reference-change preferences and outcome-change preferences differently. Then the move from Fig. 1d1 to Fig. 1d2 no longer holds, and therefore preferences observed in Fig. 1a do not necessarily hold in Fig. 1b.

To identify the true cause of RP, it is crucial to model the wealth-change preference (Fig. 1d1) and the reference-change preference (Fig. 1d2) separately, and draw inference by comparing the degree of risk aversion in these two decision situations. For example, if the risk aversion of Fig. 1a mostly shows up in Fig.1d1 and much less so in Fig. 1d2, then reference dependence and loss aversion are the main causes of RP. If it is the other way around, then reference-independent deviations from expected utility, primarily probability weighting—and possibly also utility curvature[8]—are the main causes. If there is no significant risk aversion in Fig. 1d2, then probability weighting and other reference-independent causes play no serious role. In that case, utility of income suffices to explain RP. As emphasized by Buchak (2014 footnote 6), even though Rabin (2000) did not formally distinguish Figs. 1d1 and 1d2, he was careful to always choose framings fitting with Fig. 1d1 and never with Fig. 1d2.

We used a brace below Figs 1a and 1c to indicate that reference-independent theories do not distinguish between these two figures, similarly as they do not distinguish between Figs 1b, 1d1, and 1d2. In particular, background risks play no role if they are incorporated into the reference point ω as in Fig. 1d1 rather than in outcomes as in Fig. 1d2. The impossibility to distinguish between figures above one brace has hampered the debates in the literature using reference-independent theories.

III. Rabin's Paradox as a Violation of Expected Utility

Because framing is central to the resolution of RP, we discuss the different frames that constitute our experimental stimuli jointly with our theoretical analyses. The stimuli were devised based on our theoretical predictions, which is why we present the stimuli and predictions successively.

We use the framing in Figure 2 to test Rabin's basic preference (Figs 1a and 1c). We use an accept-reject ("Yes-No" in the stimuli) formulation because this leads to most reference dependence and loss aversion (Ert & Erev 2013), and gives the strongest possible test of classical theories. Our prediction, in agreement with common views on risk attitudes (Tversky & Kahneman 1992) and Csvd's findings, is:

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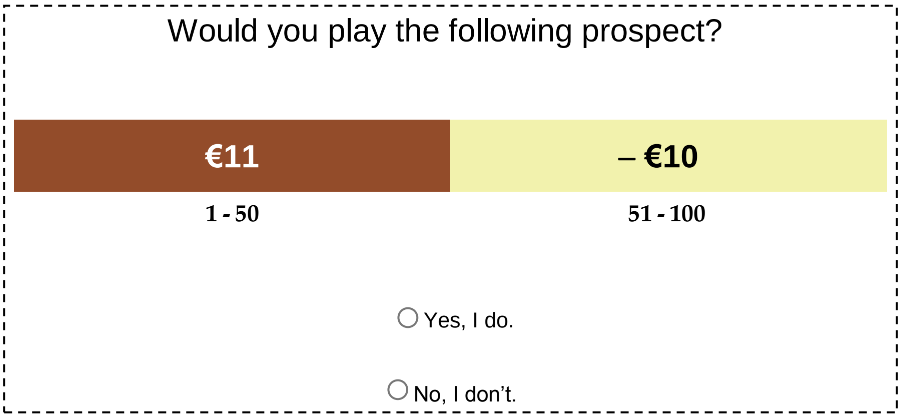
[8] We already know from Rabin's (2000) analysis that utility curvature cannot explain much here and we ignore it in most of our discussions.

FIGURE 2. Presentation of basic preference (Fig. 1a) to subjects



PREDICTION 1. A strong majority will reject (choose "no") in Figure 2.

IMPLICATION. Expected utility with concave utility is falsified.

EXPLANATION. As explained in §II, if the prediction holds true, then the preferences in Fig. 1d1 are also plausible and, hence, the preferences in Fig. 1b follow under expected utility. They imply $U(\boldsymbol{\omega}+\mathbf{11})-U(\boldsymbol{\omega}) \le U(\boldsymbol{\omega})-U(\boldsymbol{\omega}-\mathbf{10})$. Hence the average marginal utility U' over $[\boldsymbol{\omega}, \boldsymbol{\omega}+\mathbf{11}]$ is at most 10/11 times that over $[\boldsymbol{\omega}-\mathbf{10}, \boldsymbol{\omega}]$. For concave utility, it implies that U' falls by a factor of at least 10/11 over every interval $[\boldsymbol{\omega}-\mathbf{10}, \boldsymbol{\omega}+\mathbf{11}]$ of length 21. This is too fast to be reasonable. For example, for every α, no matter how big, it would imply rejection of the prospect $\boldsymbol{\alpha}_{0.5}(-\mathbf{100})$ if the wealth-change preferences (Fig. 1b) hold for all $\boldsymbol{\omega} \in [-\mathbf{100}, \boldsymbol{\alpha}]$ (Rabin 2000 p. 1282). This is absurd. It therefore entails a violation of expected utility. Factors other than utility curvature are needed to explain the rejection in Figure 2. □

IV. Nonexpected Utility Theories as Failed Attempts to Preserve Reference Independence

The main attempt to save reference independence from RP came from explanations based on probability weighting, the other component in prospect theory to deviate from expected utility. That is, RDU was used to explain RP. RDU, like EU, does not distinguish between reference-change (Fig. 1d1) and outcome-change (Fig. 1d2) preference. Consequently, the basic preference (Fig. 1a) implies the wealth-change preferences (Fig. 1b) as it does under EU. Barberis, Huang, and Thaler (2006), Barseghyan et al. (2013), Csvd (their §4.1), Neilson (2001), and Wakker (2010 p. 244 5th paragraph) pointed out that RDU can—in theory—accommodate the final-wealth preferences (Figs. 1a & 1b).[9] For example, a moderate underweighting of $p = 0.5$, with

$$w(0.5) < \frac{10}{21} = 0.476,$$

suffices to accommodate these preferences even when utility is linear. Concave utility reinforces the preferences. Empirical studies have typically found an average of $w(0.5) < 0.476$ (Tversky & Kahneman 1992; Fox, Erner, and Walters 2015), supporting the theoretical explanation. However, violations of RDU have been found in other decision contexts and these cast doubt on the probability weighting explanation. To explore it in more detail, we test RDU by measuring probability weighting. This will provide conclusive evidence.[10]

In a theoretical contribution, Neilson (2001) suggested the following extension of RP that would falsify RDU. We test this falsification empirically. Crucial for Rabin's calibration in §III is that the weight of the gain **11** is the same as the weight of the loss $\mathbf{-10}$. To achieve these equal weights under RDU, for each subject we measured the probability r such that

$$w(r) = 0.5. \tag{7}$$

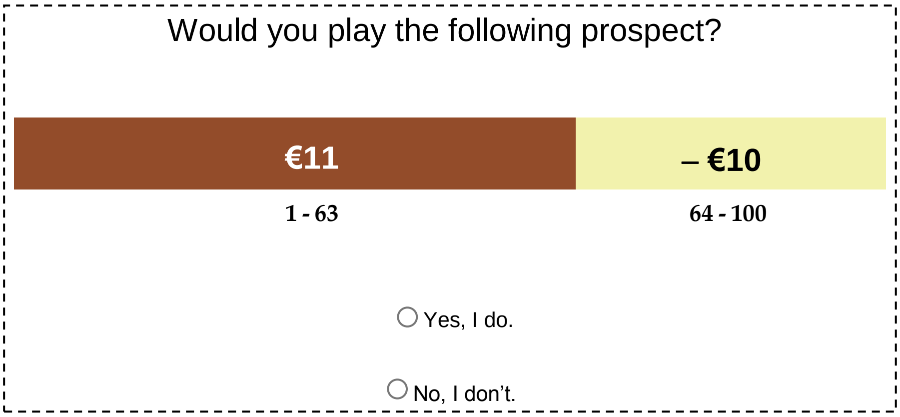
[9] Freeman (2015) showed that this can continue to hold if background risks are incorporated.

[10] Even for the most extreme case in Csvd discussed in their §4 (the second Indian group), strong probability weighting could in theory still explain the observed risk aversion. Our empirical measurements will rule out this theoretical possibility.

Based on existing empirical evidence (Fehr-Duda & Epper 2012; Tversky & Kahneman 1992; Wakker 2010 §9.5), we predict:

PREDICTION 2. The average r in Eq. 7 will exceed 0.5 considerably, entailing considerable risk aversion. □

FIGURE 3. Basic preference with $r = 0.63$ instead of 0.50.



We then offered the prospect $\mathbf{11}_r(-\mathbf{10})$ to each subject, where r was their individual value measured in Eq. 7. This gives the desired equal weighting of outcomes under RDU.[11] The offered prospect was more favorable than Rabin's prospect if $r > 0.5$, which was the typical case. Figure 3 displays the framing used for a subject with $r = 0.63$. The crucial point here is to use a framing that induces the right reference point and loss aversion. For this purpose we again use the accept-reject framing.[12] Hence we have:

PREDICTION 3. A majority will reject ("No") in Figure 3.

IMPLICATION. RDU is fails as an explanation of Rabin's Paradox.

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[11] The condition in Footnote 14 of Csvd is now satisfied and, according to their Corollary 1.1, calibration implications for utility are possible.

[12] Previously, one of us missed this point when he did not distinguish between the reference changes used in our experiment and the outcome changes he had in mind (Wakker 2010 p. 245 2nd para). Such confusions are likely to happen if authors think too much in terms of traditional reference independent models.

EXPLANATION. Under RDU with linear or slightly concave utility, subjects should accept the prospect offered, contrary to Prediction 3. This shows that RDU's correction for probability weighting does not remove all risk aversion. Neilson (2001) showed that utility curvature cannot explain the remaining risk aversion by deriving utility calibration paradoxes for RDU.[13] There must be factors beyond RDU. □

On our domain of two-outcome prospects, nearly all reference-independent nonexpected utility theories agree with RDU (see end of §I). Hence, none of those theories can explain RP either. We therefore turn to reference-dependent theories in the next section, where we will also allow probability weighting to be different for gains and losses, which is empirically desirable. Our experiment will later show that probability weighting plays no empirical role in RP.

To avoid misunderstanding, we clarify here that our study does not claim that probability weighting would be unimportant. Many studies have demonstrated its importance (Barberis and Huang 2008; Barseghyan et al. 2013; Epper and Fehr-Duda 2015; Fehr-Duda & Epper 2012; Steiner and Stewart 2016; Tversky & Kahneman 1992; Wakker 2010), although it has its own problems (Csvd §4.2; Ebert and Strack 2015). We claim only that probability weighting plays no role in RP. To further illustrate our point, consider an alternative paradox, similar to RP and with similar calibration implications for utility. It could be constructed if subjects had preferences $21_{0.5}0 \preccurlyeq 10$ at all or many wealth levels, while perceiving all outcomes as gains. Then loss aversion could play no role and probability weighting would drive the paradox. We will in fact test this preference later (Fig. 5b) and find that it may exist, but is considerably weaker than with Rabin's stimuli. Our only claim about probability weighting is that for the focus of this paper, RP, probability weighting plays no role. This claim is not our main purpose, but only serves as an intermediate tool for what is our main and positive purpose: to show the importance of reference dependence.

[13] Our choice of r rules out the theoretical possibility discussed in §III for the second Indian group in Csvd that strong probability weighting could still explain the risk aversion.

V. Reference-Dependent Theories Can Explain Rabin's Paradox

Many studies have confirmed reference and sign dependence, entailing violations of RDU[14], although there continue to be debates (Isoni, Loomes, and Sugden 2011; Plott and Zeiler 2005). Sign dependence means that risk attitudes are different for losses than for gains. Whereas probability weighting is mostly pessimistic for gains, with prevailing underweighting of the probabilities of best outcomes, for losses the opposite holds, with prevailing optimism and underweighting of the probabilities of worst outcomes. This is called reflection and it falsifies RDU. It also implies that the correction for probability weighting under RDU in Figure 3 is not correct. To obtain Rabin's calibration argument for utility, which involves the same decision weights for the two outcomes, we should, according to prospect theory, measure for each subject the probability p such that

$$w^{+}(p) = w^{-}(1-p). \tag{8}$$

Details are in Appendix A. Because RDU is a special case of prospect theory, it predicts $p = r$. Under RDU, Eq. 8 can be used as an alternative way to find the required r $(= p)$ of Eq. 7. However, based on the common findings of reflection we predict:

PREDICTION 4. $0.5 \approx p < r$. □

We offered the prospect $11_p(-10)$ to subjects. Figure 4 displays this offer for a subject with $p = 0.52$. It is natural to assume that the reference point is the status quo for this choice.

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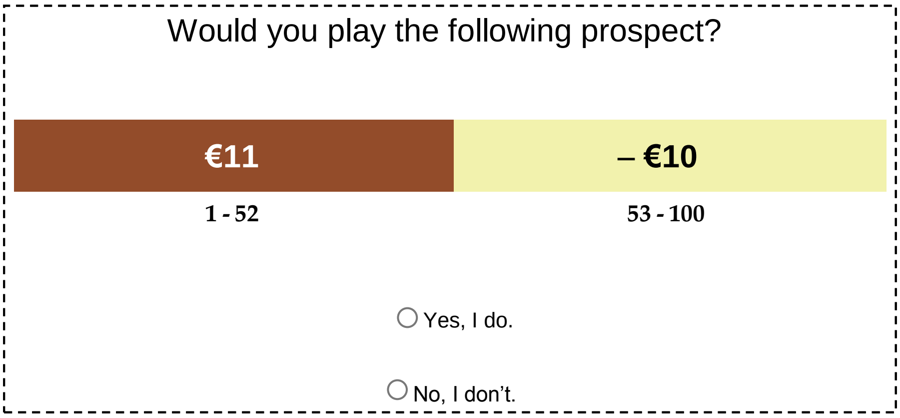
[14] See Apicella et al. (2014), Köszegi & Rabin (2009), von Gaudecker, van Soest, and Wengström (2011), Kahneman (2003b p. 1457), Ok, Ortoleva, and Riella (2015), and Wakker (2010 §9.5).

FIGURE 4. Basic preference with $p = 0.52$ instead of 0.50.



Given Prediction 1 concerning the same choice but with probability 0.5 and given Prediction 4, we have:

PREDICTION 5. A strong majority, as in Figure 2 (Prediction 1), will reject (by selecting "No, I don't") in Figure 4.

IMPLICATION. Probability weighting does not contribute to the explanation of RP. Because $p \approx 0.5$, probability weighting does not capture any risk aversion in RP. After properly correcting for probability weighting (Figure 4) there remains the same unexplained risk aversion as before (Figure 2). □

Under prospect theory, the above prediction gives indirect support to reference dependence, because it is the only explanation left for RP, given that utility curvature and probability weighting (and other nonexpected utilities; see the end of §1) have been ruled out. Loss aversion λ is commonly found to be about 2, although there is much variation (Ert & Erev 2013; Wakker 2010 §9.5). Loss aversion thus leads to strong risk aversion and can readily explain the preference in Figure 3 and the strong preferences in Figures 2 and 4 for any plausible probability weighting and utility curvature. Outside of prospect theory, deviations from expected utility proposed in the literature usually have not considered sign dependence. For our stimuli they mostly agree with RDU. Thus, they concern Prediction 3 in the preceding section and were discussed there.

To obtain direct support for reference dependence, we tested the reference-change and outcome-change preferences. Cohen and Einav (2007 pp. 746-747) emphasized that variations in wealth are needed to test RP, and the following figure does so. In Fig. 5b, the outcome-change preference cannot be formulated as an accept-reject decision and was formulated as a binary choice. To have a clean test of reference dependence, we therefore also framed the reference-change question in Fig. 5a as a binary choice. This change in framing will probably reduce loss aversion and, hence, risk aversion somewhat. To make the framings and procedures as similar as possible, we also added the prior endowment of €1 in Fig. 5b, which by normative standards should be negligible. Finally, we used the probabilities p of Eq. 8 instead of 0.5 to neutralize probability weighting and focus on reference dependence. By Prediction 4 these probabilities p will not have a systematic effect on risk aversion and Figs. 5a and 5b also test Figs. 1d1 versus 1d2.

FIGURE 5. A direct test of reference dependence (with $p = 0.52$ instead of 0.50)

FIG. 5.a. Reference-change preference

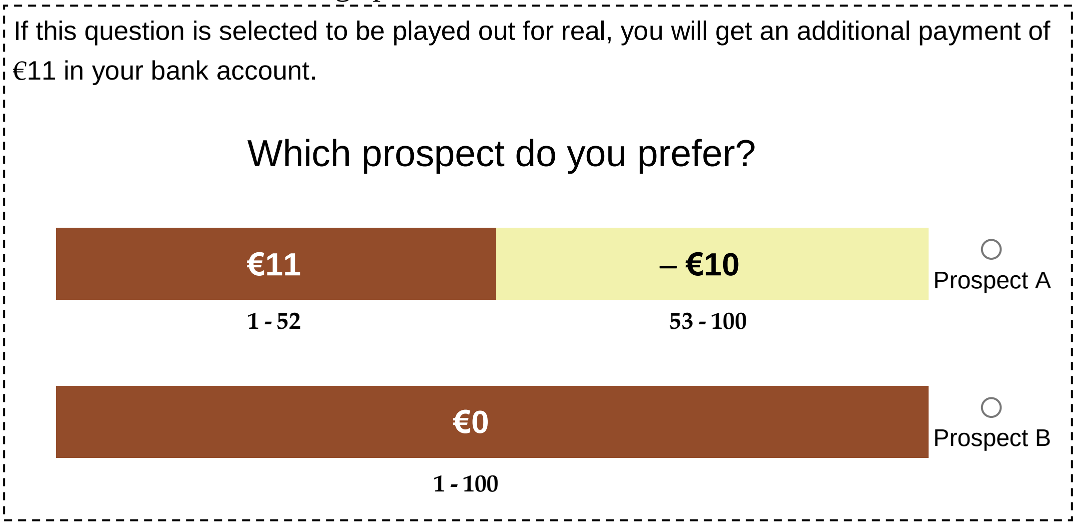
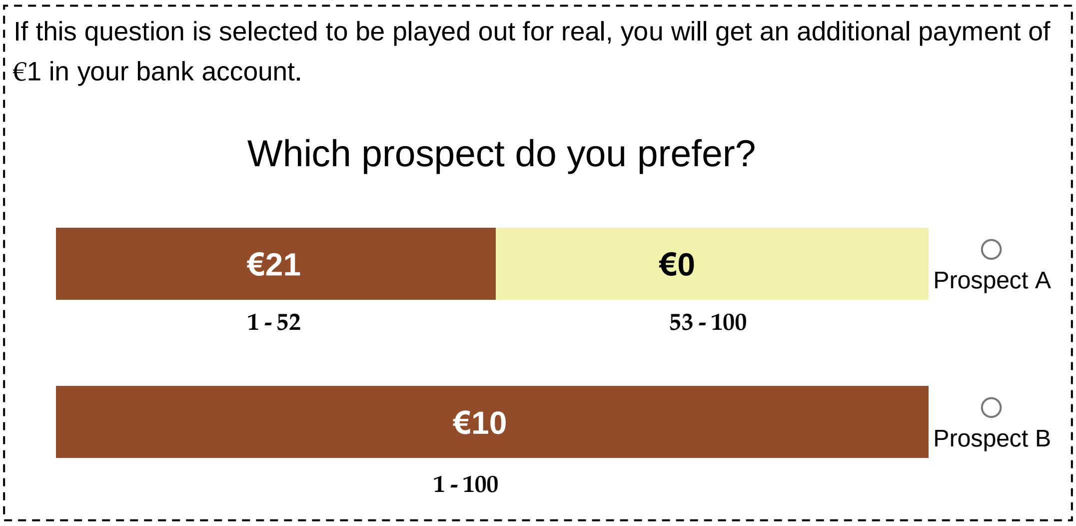


FIG. 5.b. Outcome-change preference



The two figures differ only in the way that final outcomes are split into reference point and change with respect to reference point. Our analysis is based on the assumption that: (a) the reference point in Fig. 5a has the additional payment incorporated; (b) accordingly, the outcome −€10 in Fig. 5a is perceived as a loss; (c) in Fig. 5b, the status quo of €0 is the reference point so that no losses are perceived. Our assumption is the most common one for reference points and for ways to induce them in experiments (de Martino et al. 2006; Fehr-Duda et al. 2010; Kuhberger 1998; Tversky & Kahneman 1992). It is crucial for the common incentivization of losses with prior endowments (Iturbe-Ormaetxe et al. 2011; von Gaudecker, van Soest and Wengström 2011), and for endowment effects such as underlying WTP-WTA discrepancies (Sayman and Öncüler 2005). In the well-known model of Köszegi & Rabin (2006), future expectations serve as reference points, but only if choices have been anticipated sufficiently far ahead in time, and not if they come as a surprise. In our experiment, subjects did not know beforehand what the choices would be. Köszegi & Rabin's (2007) showed that the usual prospect theory and its deterministic reference points then fit choices well. Our assumption will, of course, not hold for all subjects, and several subjects will perceive various other reference points, such as the sure outcome €10 depicted in Fig. 5b. It suffices that our assumption holds for most subjects.

In Fig. 5b, loss aversion does not play a role for most subjects and, therefore, risk aversion will be lower. Yet risk aversion can still be expected because of probability weighting—now modelled correctly—which is pessimistic for gains. Most subjects will take Fig. 5a as Fig. 1d1, and they will be as strongly risk averse as in the basic preferences in Figure 2. Some subjects will integrate payments and take Fig. 5a as Fig. 1d2, which reduces risk aversion. We summarize our claims:

PREDICTION 6. A majority of subjects will reject (choose the sure Prospect B) in Figs. 5a and 5b, but fewer than in Figure 2, and the fewest in Fig. 5b.

IMPLICATION. The difference in risk aversion between Figs. 5a and 5b falsifies reference independence. □

6. Our Experimental Findings

Subjects: N = 77 students (29 female; average age 22) from Erasmus University Rotterdam participated, in four sessions. Most were finance bachelor students. *Incentives:* Each subject received a €10 participation fee. In addition, we randomly (by bingo machine) selected two subjects in each session and for each played out one of their randomly selected choices for real consequences. The selections were implemented in public by a volunteer. The payoff was paid immediately after the experiment. The experiment lasted about 45 minutes and the average payment per subject was €15.70.

Procedure: The experiment was computerized. Subjects sat in cubicles to avoid interactions. They could ask questions at any time during the experiment. Training questions familiarized subjects with the stimuli. Subjects could only start after they had correctly answered two comprehension questions.

Stimuli: Probabilities were generated by throwing two 10-sided dice. Details are in the Online Appendix[15]. We first measured the probability r (Eq. 7). Then we asked the two accept-reject questions of Figures 2 and 3, followed by the measurement of p (Eq. 8). We finally asked the accept-reject question of Figure 4 and the two questions of Figs. 5a and 5b, with the order of these three questions counterbalanced.

Results:

Statistical tests, all two-sided, confirmed our predictions.

PREDICTION 1 [basic preference]: 88% rejected ("No") the prospect in Figure 2 (p-value < 0.001; binomial test).

PREDICTION 2: [$r > 0.5$]: The average and median r were $0.63 > 0.5$ (p-value < 0.001; Wilcoxon test).

As a byproduct in the measurement of r, we also measured utility. We found linear utility, which is plausible for the moderate amounts in our experiment. Thus, whereas Implication 1 shows that utility curvature cannot entirely explain RP, we find that it does not contribute to explaining RP at all.

PREDICTION 3 [basic preference with RDU probability weighting]: 74% rejected the prospect in Figure 3 (p-value < 0.001; Binomial test). This percentage is smaller than in Figure 2 (p-value $= 0.015$; McNemar test).

[15] See http://personal.eur.nl/wakker/pdf/rabinresolved_onl.appdix.pdf .

PREDICTION 4 [$0.5 \approx p < r$]: The average p was 0.52 and the median was 0.48. H_0: $p = 0.5$ is not rejected (p-value = 0.4; Wilcoxon test). $p < r$ is confirmed (p-value < 0.001; Wilcoxon test).

When measuring p, as a byproduct we also measured loss aversion. It was approximately 2 (see Appendix A), in agreement with previous findings in the literature and well suited to explain RP.

PREDICTION 5 [basic preference with PT probability weighting]: 87% rejected the prospect shown in Figure 4 (p-value < 0.001; binomial test). This was not significantly different from Figure 2 (p-value =1; McNemar test).

PREDICTION 6 [reference- versus outcome-change preference]: 78% rejected in Fig. 5a (p-value < 0.001; binomial test), and 62% rejected in Fig. 5b (p-value = 0.08; binomial test). The latter is smaller than the former (p-value = 0.04; McNemar test).

VII. Discussion of Experimental Details

Our experiment involved some adaptive (chained) stimuli, where answers given to some questions affected later stimuli, for instance regarding the probabilities r and p in Figures 3 and 4. It was practically impossible for subjects to see through this procedure. Further, even if the procedure were seen through, it would be practically impossible to then also see if and how manipulation could be beneficial. Hence, manipulation is, in the terminology of Bardsley et al. (2010 pp. 265, 285), only a theoretical possibility but is practically impossible.

Counterbalancing is commonly used to avoid order effects, but can complicate a design for subjects and the analyses done after, and can increase noise. Hence, it is used only to avoid the major risks of order effects. We felt that Figs 5a and 5b were most vulnerable here. We therefore counterbalanced their presentation, combined with Figure 4. For the other stimuli, we saw no concrete reason to expect biases due to order effects, and we did not involve them in counterbalancing. We could also have avoided order effects by using between-subject designs, rather than the within-subject design as used. The pros and cons of these two designs have often been debated Camerer (1989 p. 85), where a between-subject design avoids order effects but a within-subject design gives more statistical power and can test more hypotheses. In our case, there were many practical difficulties for a between-subject design. If it had been embedded in sessions with other experiments, then those other experiments could have induced spillover effects similar to the order effects to be avoided. If we had implemented a between-subject design in isolation in, then, necessarily short experiments, the payoff per subject's time unit would have exceeded the upper bound imposed in our labs to avoid negative externalities for other experiments.

VIII. Preceding Literature

Wakker (2010 pp. 244-245) surveyed early discussions of RP. Since then, Johansson-Stenman (2010) presented a theoretical analysis of RP for life-time consumption, Barseghyan et al. (2013 pp. 2526-2527) discussed an explanation based on probability weighting, and Golman & Loewenstein (2015) suggested a cognitive model to explain it. Csvd investigated RP systematically, following up on their theoretical analysis in Cox & Sadiraj (2006). Csvd were the first to confirm RP empirically and establish it as another falsification of expected utility. They also provided a detailed theoretical analysis under RDU (their Eq. NL-1), with probability weighting as the deviation from EU. Outcomes were taken reference-independent, in terms of final wealth; i.e., they were changes w.r.t. the wealth level upon entering the lab. Csvd pointed out that RDU is a special case of prospect theory (fixed reference point; sign-independent probability weighting), so that this special case of PT is also covered by their analysis.

Csvd provided theorems that exactly identify the utility functions and probability weighting functions that lead to Rabin's calibration paradoxes under RDU for various potential empirical preferences. They thus showed exactly what more is needed to analyze the role of probability weighting in future studies. We followed up on their results. In particular, we measured and corrected for probability weighting in RDU to find out to what extent it accommodates RP empirically.

In their experiments, Csvd used large outcomes, incentivized through an arrangement with a casino with small but positive probabilities of actual implementation. For 41 German students they found majority preferences

$$(\boldsymbol{\omega} + \mathbf{110})_{0.5}(\boldsymbol{\omega} - \mathbf{100}) \preccurlyeq \boldsymbol{\omega}$$

for $\boldsymbol{\omega} = \mathbf{3}\boldsymbol{K}, \mathbf{9}\boldsymbol{K}, \mathbf{50}\boldsymbol{K}, \mathbf{70}\boldsymbol{K}, \mathbf{90}\boldsymbol{K}$, and $\mathbf{110}\boldsymbol{K}$ with $K = 1000$ and Euro as unit. For 30 Indian students they found majority preferences

$$(\boldsymbol{\omega} + \mathbf{30})_{0.5}(\boldsymbol{\omega} - \mathbf{20}) \preccurlyeq \boldsymbol{\omega}$$

for $\boldsymbol{\omega} = \mathbf{100}, \mathbf{1}\boldsymbol{K}, \mathbf{2}\boldsymbol{K}, \mathbf{4}\boldsymbol{K}, \mathbf{5}\boldsymbol{K}$, and $\mathbf{6}\boldsymbol{K}$ with rupee as unit (50 rupees is a one-day salary for the students). Finally, for another group of 40 Indian students they found majority preferences

$$(\boldsymbol{\omega} + \mathbf{90})_{0.5}(\boldsymbol{\omega} - \mathbf{50}) \preccurlyeq \boldsymbol{\omega}$$

for $\boldsymbol{\omega} = \mathbf{50}, \mathbf{800}, \mathbf{1.7}\boldsymbol{K}, \mathbf{2.7}\boldsymbol{K}, \mathbf{3.8}\boldsymbol{K}$, and $\mathbf{5}\boldsymbol{K}$. Thus they overwhelmingly confirmed preferences as in Fig. 1d2 for a wide enough range of wealth levels to imply RP for expected utility and thus establish it as a genuine empirical violation.

The implications of Csvd's findings for probability weighting are not entirely clear. Their Corollary 1.1[16] shows that RDU with nonlinear probability weighting and linear utility can accommodate their findings, and does not lead to calibration paradoxes, if $w(0.5) \leq 10/21$ for the German students, $w(0.5) \leq 2/5$ for the first group of Indian students, and $w(0.5) \leq 5/14$ for the second group of Indian students. To avoid misunderstandings, note that these upper bounds on $w(0.5)$ can be somewhat relaxed under concave utility, offering extra protection against probability calibration paradoxes. Thus, theories that transform both probabilities and outcomes are less prone to calibration problems than theories that transform only one of these two.

Probability weighting is least plausible for the second Indian group of Csvd (requiring $w(0.5) \leq 5/14$). However, it cannot be ruled out without further information about this particular group of subjects, and actual measurement of w is desirable to settle the case. This is why we measured and fully corrected for probability weighting in our experiment.

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[16] Footnote 14 in the proof in their paper points out that, to obtain calibration paradoxes, the weighting-corrected expected value (expected value after replacing $p = 0.5$ by $p = w(0.5)$) of the risky option should exceed that of the safe option.

Csvd did not formalize or test reference dependence with loss aversion, but suggested it as an explanation of the problems of probability weighting.[17] Our study followed up on this suggestion both theoretically and empirically. Reference dependence and loss aversion indeed occur for subjects who perceive the sure outcome ω in Fig. 1b as their reference point—i.e., who perceive the corresponding choice situation as in Fig. 1d1. Then Eq. 3 with loss aversion $\lambda \geq 1.8$ can accommodate all aforementioned findings of Csvd even with linear utility and linear probability weighting. Such loss aversion is plausible. Under pessimistic probability weighting and concave utility, the lower bound for λ can be relaxed somewhat. There is empirical evidence that the aforementioned $\boldsymbol{\omega}$ is a plausible reference point for many subjects (Hershey & Schoemaker 1985; Morrison 2000; Robinson et al. 2001; van Osch & Stiggelbout 2008).

Interestingly, Csvd also tested a dual version of RP, introduced by Sadiraj (2014), in which calibration paradoxes are the result of probability weighting rather than of utility.[18] The dual paradox demonstrates once more that probability weighting alone cannot explain all findings. Masatlioglu and Raymond (2016 Proposition 7) showed that an important implication of probability weighting, first-order risk aversion, may be remodeled as loss aversion, using Köszegi & Rabin's (2007) model. Many other findings further demonstrated the importance of reference- and sign dependence, factors beyond probability weighting. We will not review that literature here and we similarly did not investigate the dual paradox of Csvd, but focused on Rabin's original paradox and its causes.

Summarizing, Csvd were the first to conclusively demonstrate that RP falsifies expected utility. They suggested that probability weighting and reference dependence may accommodate these violations, but the evidence provided was not conclusive. They strongly suggested that probability weighting alone cannot tell the whole story.

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[17] Their §4.2 excludes variable reference points for their dual paradox, but we focus on Rabin's original paradox.

[18] As pointed out by Csvd (§4.2), changing reference points play no role for this dual paradox, unlike for the original one. But sign-dependence and loss aversion still do. Csvd's Corollary 2.1 shows that, for linear utility and probability weighting, calibration paradoxes can be avoided if $\lambda \geq 3$ for German students, $\lambda \geq 3$ for one American sample, $\lambda \geq 14/4$ for another American sample, and $\lambda \geq 5$ for an Indian sample. (In their Corollary 2.2, Csvd do not formalize loss aversion separately but let it be part of their loss utility function μ. That is, their μ is our U of Eq. 5.) Under pessimistic probability weighting and concave utility, the upper bounds on λ can be relaxed. Here, again, theories that transform both probabilities and outcomes are more immune to calibration problems than theories that transform only one of these.

In their introduction, they raised the general question: "Is there a plausible theory for decision under risk?" As we have shown, the main message from RP is that reference dependence is an important part of the descriptive answer to this general question. As regards normative implications, there is wide, though not universal, agreement that reference dependence—taken as a framing effect—is irrational, and that it is more irrational than probability weighting. Probability weighting only violates the von Neumann-Morgenstern independence axiom as in Allais' paradox. Such violations are considered to be rational by Machina (1982) and many others. Hence, RP provides a more serious deviation from classical rationality assumptions than previously thought.

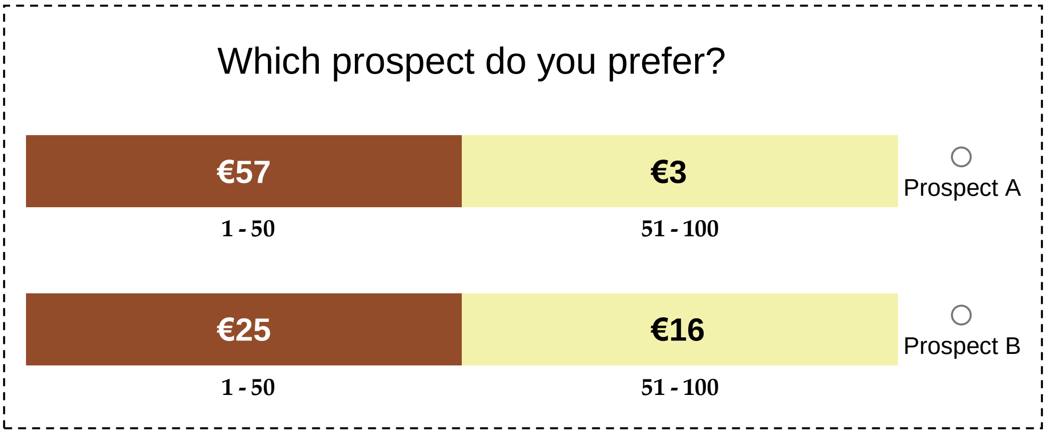
IX. Conclusion

Rabin's (2000) paradox is one of the most famous paradoxes in the modern economic literature. Up to now, it was commonly, although not universally, accepted as negative evidence against classical expected utility (Kahneman 2003a p. 164). Its cause had not yet been identified, so that no positive inference had been derived yet. We identify this cause and provide a positive inference: RP proves that we need reference dependent generalizations of classical models, and it does so more strongly than any other paradox did before. Other deviations from expected utility do not contribute to explaining Rabin's paradox. This confirms that utility of income does explain the paradox.

Appendix A. Measurement of r and P

We derived all indifferences in our experiment from choices through bisection procedures (Online Appendix). To measure the probability r in Eq. 7 and obtain an estimate of utility curvature, we iteratively elicited four indifferences, ${x_i}_{0.5}g \sim {x_{i-1}}_{0.5}G$ ($i = 1, \ldots, 4$), where we chose $g = 3, G = 16$, and $x_0 = 25$. Figure A.1 displays a choice used to elicit x_1.

FIGURE A.1. A choice to elicit x_1



From the indifferences ${x_i}_{0.5}g \sim {x_{i-1}}_{0.5}G$ we obtain, by RDU,

$$U(x_i) - U(x_{i-1}) = \frac{(1 - w(0.5))(U(G) - U(g))}{w(0.5)}$$

for all i, so that the x_i's are equally spaced in utility units. We next elicited probabilities r_i such that

$${x_{i+1}}_{r_i}x_{i-1} \sim x_i$$

for $i = 1,2,3$. By RDU, $w(r_i) = 0.5$ for all i. Note that propagation of errors in the x_i's plays no role here because all that matters is that x_{i+1} is properly placed relative to x_{i-1} and x_i. The three average values of r are 0.67, 0.58, and 0.63. By a Friedman test their differences are significant (p-value = 0.045), which can be taken as a rejection of RDU. For r we took the average of these three r_i.

To measure p of Eq. 8, and obtain an estimate of loss aversion, we first chose a value $L = -10$. We then measured the bold variables in the following four indifferences

$\boldsymbol{G}_{0.5}L \sim 0,\ \ \boldsymbol{x}^+ \sim G_{0.5}0,\ \ 0_{0.5}L \sim \boldsymbol{x}^-$, and $x^+{}_{\boldsymbol{p}}x^- \sim 0$.

Substituting PT, the indifferences imply $PT(G_{0.5}0) = -PT(0_{0.5}L), U(x^+) = -U(x^-)$, and, finally, the required Eq. 8 for p. Table A.1 displays summary statistics.

TABLE A.1: summary statistics of the x_is and the probabilities r and p

	x_i's							probabilities	
	x^0	x^1	x^2	x^3	x^4	x^+	x^-	r	p
Mean	25	59.64	91	125.69	156.3	8.69	-3.69	0.63	0.52
Median	25	58	91	120	151	07	-4	0.63	0.48
Min	25	26	27	28	29	01	-6	0.05	0.05
Max	25	88	151	214	277	31	0	0.95	0.95

The values of x_i $(i = 1, \ldots, 4)$ suggest almost linear utility for gains: the distance $x_{i+1} - x_i$ $(i = 1, \ldots, 3)$ are not significantly different (Friedman test, p = 0.38). Under the plausible assumption of piecewise linear utility for small stakes with only a kink at 0 reflecting loss aversion, the ratio of mean values $\frac{x^+}{x^-} = 2.36$ and the ratio of median values $\frac{x^+}{x^-} = 1.75$ suggest a loss aversion λ of approximately 2, further supporting the presence of loss aversion.

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