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The endowment effect with different possession times and types of items

Domenico Colucci [a], Chiara Franco [b,*], Vincenzo Valori [a]

[a] BEELab+ Behavioral and Experimental Economics Lab, Department of Economics and Management, University of Florence, Italy
[b] Department of Political Science, University of Pisa, Italy

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ABSTRACT

The vast literature dealing with the endowment effect (EE) revolves around untangling the various determinants that may be at work in producing the effect itself. We examine two of these likely determinants that remain under-researched: The first is the effect that the length of possession may have on the EE. The second is the type of good (tangible, intangible or exchange goods) used to test the effect. Using an online questionnaire experiment, we investigate these aspects using three different items – a mug, an Amazon gift card, and a quarterly subscription to Spotify – testing whether the EE occurs when subjects imagine owning the item for different lengths of time. We find that the EE appears clearly for all types of goods, while the results are less clear when considering the duration of possession.

1. Introduction

The idea that whether a subject owns a good or not should not affect its valuation was first challenged by Thaler (1980), who coined the term *endowment effect* (EE henceforth). Indeed, with the experimental evidence first provided by Knetsch (1989) and then refined by Kahneman et al. (1990, 1991), research has shown that endowing a subject with the good typically does matter. Specifically, people seem to place a higher value on what they own and therefore value a good more than when they do not own it and have to buy it. This leads to a significant divergence between the willingness to pay (WTP) to buy the good and the willingness to accept (WTA) to sell it.

In particular, the seminal work of Knetsch (1989) shows that when people are endowed with goods of similar value and are given the opportunity to trade, they make fewer transactions than expected: this result provided the first experimental verification of the existence of EE. Further evidence was provided by Kahneman et al. (1990), who studied the phenomenon in a series of experiments in which goods could be exchanged for money, and presented results suggesting that EE is robust to market experience and that undertrading relative to the predictions of theory is not due to transaction costs.

The main explanation for this effect is traditionally based on loss aversion: the sale of an object is perceived as a loss by sellers with respect to their reference point, to some extent regardless of the compensation received, whereas the purchase is perceived as a gain by buyers — for whom there is no reference point yet. Thus, if individuals are loss-averse, their tendency will be to value the objects they consider selling more than they value the objects they consider buying. This means that the selling price of a good will be higher than the price at which a person is willing to buy the same good.

However, loss aversion can hardly be considered as the only explanation for the effect. For example, a number of relevant studies try to distinguish between loss aversion and an ownership effect. Morewedge et al. (2009), by separating the seller/buyer conditions from the owner/non-owner conditions, suggest that the ownership effect is the main culprit for EE, rather than loss aversion. Chatterjee et al. (2013) fit into the debate emphasizing the association between the self and the owned product, which results in the impending sale appearing as an implicit threat to the seller. As a result, sellers enhance the value of the self-associated object, leading to the EE.[1] In this line of research, the history of ownership and the way in which it occurs can play a very relevant, if largely neglected, role. Wang et al. (2015) investigate how the ownership history of an object – which captures the origins of how an object is acquired – affects its valuation, finding a significant effect in treatments with different sequences of acquisition/loss of ownership. Complementary and alternative explanations

* Correspondence to: Via F. Serafini 3, 56126 Pisa, Italy.
E-mail address: chiara.franco@unipi.it (C. Franco).

[1] Further evidence in this direction is proposed by Dommer and Swaminathan (2013) who stress the role of moderators such as social self-threat, identity associations of the good and gender.

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focus on various psychological underpinnings (e.g. Alexopoulos et al. (2015) and Yeung et al. (2020)).

Another aspect that is perhaps not sufficiently emphasized, is the fact that EE has an important cultural component and may vary depending on the country in which it is tested and measured. In this regard, Maddux et al. (2010) examine the possible impact of cultural differences on the emergence of EE and specifically test whether East Asian cultures and Western cultures show EE to different degrees: the work reports that the effect shown is smaller among East Asians.

It should also be noted that the very existence of the EE phenomenon is disputed by some researchers. Plott and Zeiler (2005, 2007) have argued that the effect found in previous studies has a different explanation, proposing that the observed exchange asymmetries are driven by subjects' misunderstandings and misconceptions about the nature of the experimental task as well as by biases introduced by the experimental procedures. These authors conducted a series of experiments in which they modified the traditional experimental design in an attempt to reduce both confusion and procedure-related influences. This allowed them to show that asymmetries in a given group of subjects could be made to appear or disappear by changing the procedures used. These papers have in turn been criticized (see e.g. Fehr et al. (2015)) and the discussion is still ongoing.

Keeping the lively debate just described on this topic in the background, however, this paper focuses on two specific aspects that have been somewhat neglected, namely the role of the type of good involved in generating EE, and whether the length of possession affects the degree at which EE can be observed.

Regarding which types of goods actually trigger EE, Horowitz and McConnell (2002) illustrate how the type of good used to test the effect can change the final result. They devise a kind of ranking of the goods, with the "non-ordinary goods" showing the highest WTA/WTP ratio at the top. In line with their findings, the meta-analysis by Tunçel and Hammitt (2014) confirms that EE can occur to a lesser extent for ordinary goods than for non-ordinary or public goods, and that the effect is smaller when students are used as subjects and when market experience is higher. The role of the type of good in the EE has also been investigated by Jaeger et al. (2020), who use the time-shifted rationality approach to examine whether and to what extent the effect can vary depending on the good considered. They find that EE can indeed vary in magnitude across goods, largely due to evolutionary mechanisms.[2]

Although EE has now been documented in a wide variety of situations, Kahneman et al. (1990) clearly point out that one implication of EE being caused by loss aversion is that it should not occur for exchange goods. However, this view is at least partially contradicted by Van Dijk and Van Knippenberg (1996), who show – in an experiment in which the value of tokens is induced by lotteries – that EE occurs for exchange goods as long as their value is uncertain. Van de Ven et al. (2005) support this view by investigating (consumer) curiosity effects: if curiosity can only be satisfied by withholding the exchange good, then this can lead to disparities between purchase and sale prices. An indirect argument in favor of the hypothesis that EE does not occur for exchange goods is provided by Novemsky and Kahneman (2005), who develop the idea that the "intentions" of agents define the nature of an item as a consumption good or an exchange good. Such intentions may inhibit (in the case of exchange goods) or generate (in the case of consumption goods) loss aversion. Therefore, if loss aversion is an essential element for the emergence of EE, it must be concluded that it should not occur for exchange goods. More recently, Svirsky (2014) addresses this issue by conducting an experiment with cash, chocolate coins, and chocolate coins described as tokens, finding no EE for either money or a consumer good described as an exchange good. An attempt to resolve this dispute comes from an adversarial collaborative project by Bateman et al. (2005), which tests two different hypotheses – whether or not loss aversion emerges in the case of exchange goods – in a large 10-treatment experiment: the results, although inconclusive, suggest that the WTA/WTP disparity emerges even in the presence of exchange goods. In summary, despite various attempts to settle the controversy, there is not enough evidence to end the debate.

The existence of EE for intangibles is a less studied issue. The case is interesting in the context of the debate on whether it is the physical possession of the good or the abstract property right over it that triggers the WTA/WTP gap. In particular, Reb and Connolly (2007) show in a series of experiments that EE is not determined by the attribution of an abstract property right, but is the result of subjective feelings of ownership induced by physical possession of the object. Later, Bagga et al. (2020) examine different forms of physical possession, that do not imply ownership, such as renting and borrowing, and show that rented objects are valued more than non-owned and borrowed objects. Overall, these studies suggest that if EE is primarily mediated by physical possession, it should be attenuated or absent for intangibles.

Another aspect that has not been sufficiently explored in the literature concerns the effect of prolonged possession on the EE. Traditionally, the literature on this topic refers to instant EE (see Kahneman et al. (1990)), i.e. the adaptation to the new reference point brought about by the acquisition of possession of the good takes place immediately. It is possible that, although some of the adjustment is rapid, the process is more protracted and therefore greater EE occurs over longer periods of time. Indeed, Strahilevitz and Loewenstein (1998) show that the EE does not end immediately after the acquisition of ownership of the good and that it is affected by prolonged ownership. In their experiment, prolonged ownership refers to periods of 20 minutes in one study and one hour in the other, but they do not elaborate on whether longer durations can further influence the EE. As the authors themselves acknowledge, although the effect of such short differences in ownership duration attests to the strength of the effect, it is not possible to generalize the results when much longer ownership durations are considered. More recently Yamamoto and Navarro-Martinez (2022) have investigated the influence of timing on the EE. Their work attempts to elicit WTP and WTA concerning future moments, in which subjects must try to imagine the transaction taking place. They find a negative effect of time delay on WTP, while there is no clear effect on WTA.

The study and in-depth knowledge of EE mechanisms can also be relevant from a policy point of view, e.g. to understand the factors influencing consumer perceptions. One of the most relevant areas of application is the return policy in the retail sector. This type of policy is characterized by different degrees of leniency, which refers to the possibility for the consumer to return the purchased item in an easy and convenient way (see Abdulla et al. (2019)). This research is also crucial for the supply side, since management choices may in turn be influenced by the consumer behavior generated by the adoption of such a policy. In this regard, the role played by the effect of prolonged ownership on EE may also be relevant. For example, Wang (2009) finds that the combination of signals generated by lenient return policies and consumers' purchase rate are positively intertwined with the duration of the EE. Specifically, a longer period "spent with" the purchased item may affect the rate of return, which may be decreasing, thus generating a higher or similar net purchase rate under a more lenient return policy than under a no-return policy. As the author points out, this aspect may have both marketing implications for business decisions and implications for consumer protection policy. More generally, this may also apply to public policies that seek to promote consumption in certain sectors.

Whether these results are mere laboratory artifacts or whether they are actually important in the field, making EE relevant in contexts beyond the laboratory, is still a matter of debate. As an extensive study by Morewedge and Giblin (2015) shows, EE is not only limited to

[2] Huck et al. (2005) were the first to consider the evolutionary view of this concept.

goods usually considered in experimental contexts (such as cups, pens, etc.), but also to many other types of goods. Horowitz and McConnell (2002) in their review refer to a fairly relevant array of goods, ranging from chocolate to nuclear waste repositories; other studies have treated religious items (see Shtudiner et al. (2019)) and intellectual property rights (see Buccafusco and Sprigman (2010)).

The present paper is an attempt to contribute to the literature by investigating whether the EE occurs when considering different periods of possession as well as items of different nature: a mug, an Amazon gift card, and a full subscription to the Spotify streaming service. We approach this question using an online questionnaire experiment, following Jefferson and Taplin (2011), who use a factorial design to manipulate the scenarios in which the EE can occur. Our design aims to test whether the EE is not only present immediately after possession of the item, but also whether the effect is still present after a day, a week and a month. With respect to Yamamoto and Navarro-Martinez (2022) we stick to a definition of the EE that takes current WTP as reference point, as in Strahilevitz and Loewenstein (1998).

The paper is organized as follows: Section 2 presents the aims of the study. Section 3 describes the questionnaire in detail and the subject pool. Section 4 describes the results obtained, while Section 5 offers some comments and concludes.

2. Aims of the study

Building on previous literature, we focus on the possibility that the type of good considered (tangible, intangible or exchange goods) affects the observability of the EE and on the effect of prolonged possession (instant vs. longer term EE).

As mentioned in the previous section, regarding the dependence on the type of good, the emergence of the EE in the case of exchange goods is still debated, while EE in the case of intangible goods has not been sufficiently studied. We examine the issue of exchange goods using an Amazon gift card[3] as an example. For intangible goods, we consider a quarterly subscription to Spotify, a popular online music streaming service.

We address the following questions:

(a) Is there really no EE in the case of an exchange good comparable to legal tender (but different from money), as postulated by standard theory?

(b) Does the EE occur in the case of intangible goods, for which physical possession cannot be ascribed?

With regard to the effect of prolonged possession, the literature on this subject traditionally refers to instant EE, i.e. the adaptation to the new reference point brought about by taking possession of the good takes place immediately. On the other hand, it is possible that although part of the adjustment is rapid, the process is more protracted in time and therefore a greater EE occurs after longer time intervals. In light of these considerations, we ask the following additional questions:

(c) Does WTA continue to increase after periods of one day, one week or longer?

(d) After how long (if so) does prolonged possession cease to affect WTA?

(e) Does this adaptive process result in the valuation of the good gradually approaching a new equilibrium?

In order to answer these research questions, we will make use of an online questionnaire experiment, the details of which are presented in the next section. The study has been pre-registered on aspredicted.org (#71 055, 20 July 2021).

[3] Such cards give the owner the right to make purchases on the Amazon platform up to a predetermined amount.

3. Materials and methods

Data are collected via a questionnaire administered through the Qualtrics^{XM} platform and consisting of the following steps:

1. **Information and consent form.**
2. **Instructions and comprehension check**. A comprehension check question is introduced to familiarize subjects with the "multiple price list" method we use to elicit values, and to minimize noise from confusion.
3. **Main treatment**. The main treatment comes in five different versions, corresponding to the five experimental conditions discussed in detail below.
4. **Attention level check**. The attention check is introduced to ensure a minimum control over the quality of the responses collected. It has shown some effectiveness, as it has led to the cancellation of a percentage of responses of around 15%.
5. **Final survey**. The final survey is in two parts. The first part collects information on some consumption habits relevant to the questionnaire, while the second part collects impressions on the effectiveness of the questionnaire design.

The estimated duration of the questionnaire is 6 minutes and the remuneration is £0.80 (or £8.00 per hour, equivalent to €9.4 or $11.2 at the exchange rates applicable at the time of the questionnaire).

In the online Appendix C all the screenshots related to the Buyer condition are shown, followed by sample screenshots related to other conditions for ease of comparison.

3.1. Treatment conditions

The questionnaire has a between-subjects design for the variable *"duration of possession"* and a within-subjects design for the variable *"nature of the item"*. In total we have five conditions that differ in terms of the duration of possession described.[4]

1. **Buyer condition: No possession**. Subjects are presented with a scenario in which they are shown an item and have the option of receiving that item or, alternatively, some money.
2. **Seller condition: Possession**. Subjects are presented with a scenario in which they are given an item and have the option of keeping it or exchanging it for alternative amounts of money. Four specifications of the seller condition are considered.

   2a **Now**: The choice between keeping or trading must be made *immediately after* after taking possession of the item.
   2b **Day**: Choice to be made *the day after* after taking possession of the item.
   2c **Week**: Choice to be made *one week after* after taking possession of the item.
   2d **Month**: Choice to be made *one month after* after taking possession of the item.

Each subject is randomly assigned to one of the five conditions.

Regarding the within-subjects component related to the nature of the item, each subject is asked questions about each of three good types: tangible (represented by a mug), intangible (represented by a quarterly subscription to Spotify), and medium of exchange (represented by an

[4] Of course, the ideal situation would have been to randomly assign each subject to only one of the 15 experimental conditions: in this case, however, we would have had to recruit three times as many people to generate the same amount of data. Between the two alternatives of allocating "within subjects" either the condition related to the nature of the items or the condition related to the different time scenarios, we chose the former because it implied a lower risk of incurring a significant order effect.

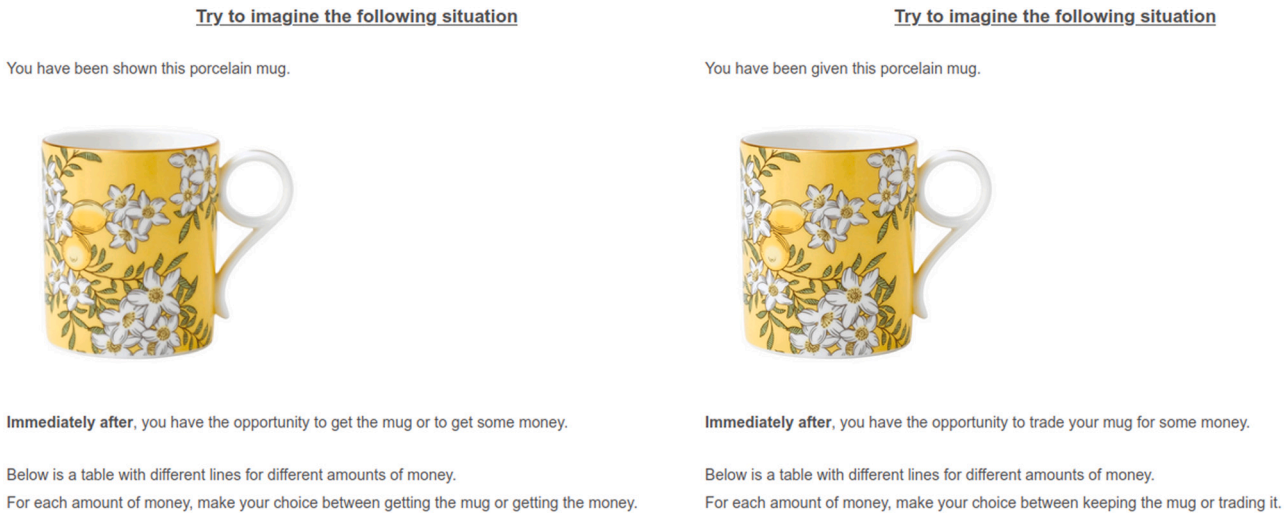

Fig. 1. Comparison between the buyer and the seller conditions.

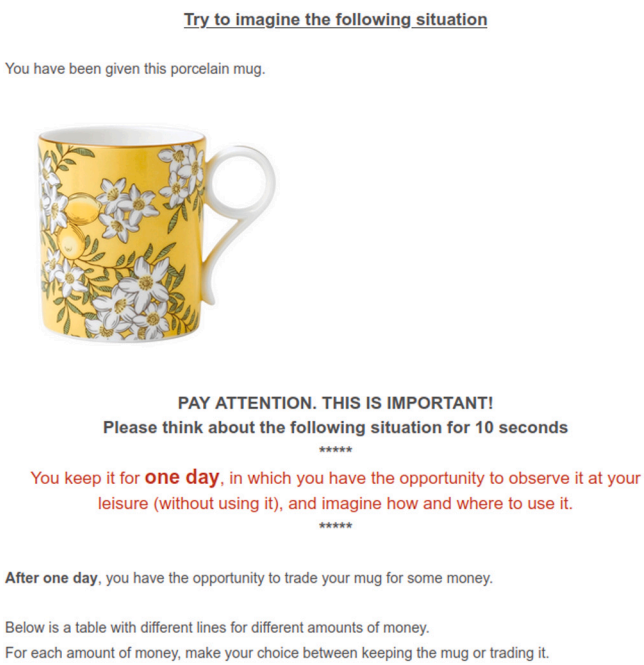

Fig. 2. *Day* condition.

Amazon gift card with a face value of $100). We adopt a counterbalancing design, such that the order of appearance of the three good types (tangible, intangible, medium of exchange) is randomized for each subject to control for possible order effects.

3.2. Conditions implementation

Each item is briefly introduced and accompanied by a picture. We keep the differences between the conditions to a minimum, while trying to induce the necessary identification with the different scenarios. In the baseline conditions (*Buyer* and *Now*) – where we intend to measure the usual WTA-WTP gap – the differences are limited to one word in the description of the context ("You have been shown . . . " vs. "You have been given . . . ") and the verbs describing the possible actions ("Get the item" and "Get the money" vs. "Keep" and "Trade") — see e.g. Fig. 1.

In the "protracted possession" conditions (*Day*, *Week* and *Month*) we add the sentence "You keep it for one day/week/month, in which you have the opportunity to observe it at your leisure (without using it), and imagine how and where to use it" in order to give the necessary salience to the temporal framing of the decision to be made. In addition to presenting the imaginary context, we also ask subjects to pause and think about the situation for a few seconds (10, 20 or 30 s, in the three conditions *Day*, *Week* or *Month*, respectively). Although this pause is not mandatory, we believe that it could help the process of identification which is crucial in an environment such as this one (see e.g. Fig. 2, showing the wording chosen for the Day condition).

3.3. Data collection

Data are collected using a two-step *"multiple price list"* (MPL) method. We decided in favor of this method, as opposed to the alternative of asking subjects to directly provide a value for the items, which is commonly used in incentive schemes à la Becker–DeGroot–Marschak (BDM). The main drawback of MPL is that it requires more time and effort from the subjects: in our setup, each valuation requires subjects to tick 11-12 boxes, whereas a direct revelation would only ask subjects to type a single number. On the other hand, since there is no clear incentive scheme in the questionnaire (since participation is paid at a flat rate), we felt that it would be easier for subjects to achieve the desired goal (i.e., assigning a value to the items) with a mechanism that guides them towards it through successive steps. Furthermore, our decision is also supported by the results of Brebner and Sonnemans (2018) showing that the MPL and the BDM methods produce approximately the same estimate of WTA and WTP in incentivized contexts and suggesting that *"when only few assessments with a relatively low resolution are needed, MPL seems to be a practical choice"*.

In our questionnaire, the first stage of each question asks subjects to choose between receiving the item or receiving the money (in the *Buyer* condition; keeping or trading the item in the *Seller* condition) for five different amounts of money, which remain unchanged across the five treatment conditions. Once the initial choice is made, we ask them to refine their choice in a smaller interval (see Figs. 3 and 4).

Values are elicited in dollars within different intervals and with different steps for the three items:

Mug. Values in 0–18. Steps: 3 in the first stage and 0.5 in the second stage.

Spotify subscription. Values in 0–30. Steps: 5 in the first stage and 1 in the second stage.

Amazon gift card. Values in 40–100. Steps: 10 in the first stage and 2 in the second stage.

Finally, before closing the questionnaire we administer a short survey with two purposes: the first is to collect data on the habit of

Fig. 3. First stage multiple price list for a mug in the Buyer condition.

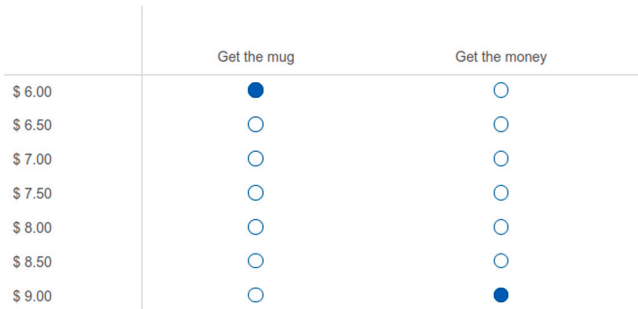

Fig. 4. Second stage, 6–9 interval, Multiple price list for a mug in the *Buyer* condition.

Table 1
Number of subjects, by gender.

Sex	#Subjects
Female	252
Male	260

Table 2
Age of subjects.

Mean	Median	Std Dev
27.36	24	9.5

using the online services involved in the survey (Spotify and Amazon) in order to check for any consequential effect (this part is mandatory).

The second is to gather impressions on the effectiveness of the framing used to set up the questions and, in particular, on the part concerning the duration of possession (this part is optional).

3.4. Subject pool

The questionnaire was administered via the Prolific platform between 12 and 28 July 2021. Overall a total of 516 subjects participated, with slightly over 100 subjects randomly assigned to each of the 5 treatments.[5] We arrived at this sample size after performing a power analysis based on pilot observations (see the online Appendix A for details).

Tables 1 and 2 illustrate the demographic characteristics of the pool.

In addition to balancing by gender, we also imposed a certain allocation balance to the various conditions (see Table 3).

The subjects were all native English speakers, as we wanted to ensure that there was a full understanding of the questions and their logic. The current country of residence of the subjects is shown in Table 4.

It took an average of 5′53″ for subjects to complete the questionnaire, thus making the hourly payment equal to £8.15 (equivalent to €9.57 or $11.40). Funding was provided by the University of Florence.

[5] Of these, for 4 subjects the information about their gender was not available, due to a Prolific situation of "Data Expired".

Table 3
Number of subjects in each condition by gender.

Condition	Female	Male
Buyer	48	52
Now	50	60
Day	53	50
Week	50	49
Month	51	49

Table 4
Number of subjects, by current country of residence.

Country	#Subjects
Australia	20
Canada	53
Ireland	31
Italy	1
New Zealand	10
United Kingdom	146
United States	255

Table 5
Mean values for WTP and WTA at different time scenarios.

	Buyer	Now	Day	Week	Month
Mug	3.86	6.51	7.50	6.52	7.58
Amazon	78.38	88.46	92.18	92.93	92.14
Spotify	12.24	16.60	17.15	19.26	17.24

Table 6
Kruskal–Wallis test: p-values.

	Mug	Amazon	Spotify
Buyer + Seller conditions	0.0000	0.0000	0.0000
Seller conditions only	0.2818	0.105	0.2499

4. Results

We calculated each subject's valuation of the three items on the basis of the questionnaire choices in the Multiple Price List stages, in order to define the variables corresponding to willingness to pay or willingness to accept for the given item for the given treatment condition. Therefore, we calculated the following variables

XXXBuyer_WTP, XXXNow_WTA, XXXDay_WTA, XXXWeek_WTA, XXXMonth_WTA

where in turn XXX can be one of "Mug", "Amazon" and "Spotify". The resulting mean values are shown in Table 5.

A preliminary analysis using the Kruskal–Wallis rank-sum test makes it clear that for all three items there are effects determined by the different conditions studied and that these effects are mainly determined by the difference between WTP and (instant) WTA. Indeed (see Table 6) the K-W test rejects the null hypothesis with very low p-values when all 5 conditions are considered, while it does not reject the null hypothesis when it is applied only to the "seller" conditions.

One clear feature suggested by these average values is that there is a gap between willingness to pay for each item when buying and willingness to accept money when selling: indeed Table 7 shows that there is a statistically significant difference between WTP and WTA for all three items. The Wilcoxon rank-sum test performed clearly rejects the null of equally distributed populations for the valuation of each item between the *Buyer* and the *Now* condition (p-value < 0.001 for all three items).

Fig. 5 shows box plots of the observed WTP and WTA and illustrates, particularly for Mug and Amazon, that there is not only a shift in the distributions (the medians differ) but also a difference in the spread of the distributions between the two conditions.

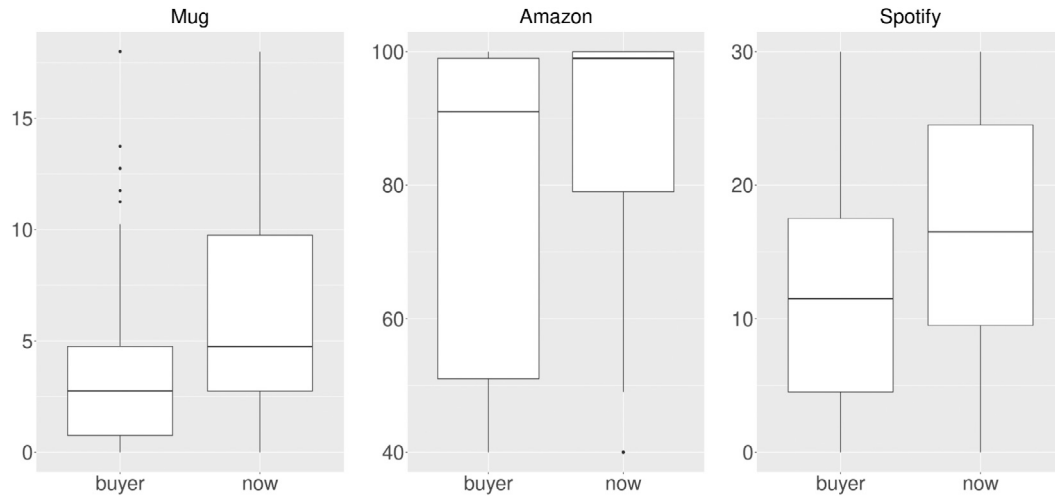

Fig. 5. Instant EE for the different items. Note: horizontal lines indicate the medians and boxes the interquartile range, WTA\WTP on vertical axis.

Table 7
Wilcoxon rank-sum test, p-values.

	Mug	Amazon	Spotify
WTP (Buyer) v. WTA (Now)	0.0001	0.0007	0.0007

Table 8
Wilcoxon test for differences with respect to the "Now" WTA: p-values.

	Day	Week	Month
Mug	0.066	0.513	0.096
Amazon	0.084	0.014	0.026
Spotify	0.340	0.030	0.329

Regarding the differences between the *Now* valuation of the WTA and those across different time scenarios (i.e. between treatment condition 2a and conditions 2b, 2c, 2d), the evidence is mixed, as can be seen from Table 8, where we report the results of the Wilcoxon rank-sum test of the null hypothesis of equal distributions of the WTA the *Day*, *Week* and *Month* conditions with respect to the *Now* condition.

Looking at Table 8 we do not find any clear common pattern across all objects of how the duration of "possession" affects the subjects' valuation in the virtual context of the questionnaire: instead, some differences in the valuation of the three objects for different durations of possession seem to appear.

The three panels in Fig. 6 record for each condition the observations collected on the valuation of the three items (with the mean highlighted in red). They give pictorial insight of the pattern of choices exhibited by the subjects through the various time scenarios. Although, as the period of ownership grows, there appears an initial sign of further growth in valuations (average valuations for Day are higher than for Now, for all three items) the overall trend lacks the expected characteristics of stability and regularity: the average valuation continues to increase for Amazon and Spotify in the Week scenario and then decreases thereafter, while for the cup it decreases in the Week scenario and then resumes growth thereafter.

To support this graphical evidence, Table 9 shows the results of a linear regression in which the valuation of the object depends solely on the duration of ownership. The latter is a categorical variable and is coded using backward difference coding, where the mean of the dependent variable for one level of the categorical variable is compared to the mean of the dependent variable for the previous adjacent level: the coefficients and their significance confirm what is suggested by

Table 9
Items valuations: incremental effect of possession length.

	Dependent variable:		
	Mug (1)	Amazon (2)	Spotify (3)
Now v. Buyer	2.650***	10.088***	4.361***
	(0.0003)	(0.00002)	(0.001)
Day v. Now	0.988	3.720	0.547
	(0.164)	(0.112)	(0.668)
Week v. Day	−0.978	0.746	2.114
	(0.180)	(0.756)	(0.108)
Month v. Week	1.060	−0.790	−2.025
	(0.149)	(0.744)	(0.126)
Constant	6.391***	88.819***	16.495***
	(0.000)	(0.000)	(0.000)
Observations	516	516	516
R^2	0.063	0.091	0.057
Adjusted R^2	0.055	0.084	0.050
Residual Std. Error (df = 511)	5.183	17.075	9.331
F Statistic (df = 4; 511)	8.546***	12.852***	7.738***

Note: P-values in parentheses $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Fig. 6 as well as the results of the Wilcoxon test in Table 7 and in the first column of Table 8.[6]

Overall, the data confirm the existence of instant EE for all three types of goods. On the other hand, the results are less clear when it comes to the effect of the duration of possession on WTA. We can observe that long-term EE does not occur with the same intensity and timing for the three types of goods. For the mug (tangible good), the largest effect is observed after one day, while for the Amazon Gift Card (exchange good) and Spotify (intangible good), the largest effect is observed after one week. It is interesting to note that in none of the three cases is the peak of the WTA reached at the longest possible possession period, nor do we observe the expected adjustment path to the new equilibrium value — which we would have imagined with increasing values and decreasing increments in the transition from one period to the next of longer duration. Indeed, the Jonckheere–Terpstra

[6] In contrast, a direct comparison between the regression and columns 2 and 3 of Table 8 is not possible as the Wilcoxon tests are based on comparisons with respect to the 'now' scenario, whereas the regression is based on incremental comparisons.

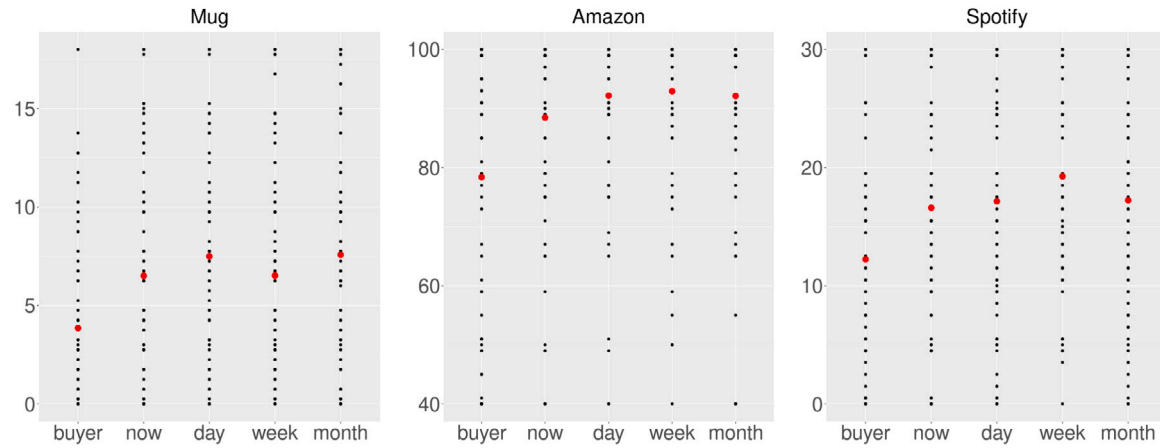

Fig. 6. Empirical distribution of observed valuations. Note: red dots are mean values.

Table 10
Jonckheere–Terpstra test: p-values.

	Mug	Amazon	Spotify
Buyer + Seller conditions	0.0000	0.0000	0.0000
Seller conditions only	0.2563	0.0299	0.2678

test for an ordered alternative hypothesis reveals a (strong) statistically significant trend in the data when all 5 conditions are considered. Instead, when applied only to the "seller" conditions, the J-T test detects a statistically significant trend in only one out of three cases (Amazon), while the null hypothesis is not rejected for the mug and for Spotify (see Table 10 with detailed p-values).

To go deeper into the analysis we test whether further characteristics of the sample played a role in shaping the results by performing a linear regression analysis with the valuations of the item as the dependent variable. As independent variables, in model (1) we included "possession conditions" as well as some socio-demographic variables,[7] while in model (2) we have further added some 'questionnaire variables' specific to the three items.[8] The results are shown in Table 11.

Compared with the regression in Table 9 the number of available observations is reduced by 20% due to missing data (mainly related to the employment status variable). None of the demographic variables seem to play a significant role. Interesting, however, is what emerges in model (2). All three item-specific questions are significant. In particular, for Amazon and Spotify, item evaluation appears to be positively correlated with already being a user of those services: this seems to be consistent with what intuition might suggest. Furthermore, the addition of this variable in the regression does not seem to have an effect on the significance of the coefficients associated with the possession conditions. The situation with regard to the mug, however, is different. Subjects who declare to be influenced by the particular framing of the question evaluate the cup significantly more: in this case the new variable partially absorbs the effect of the conditions of possession. Such change in the significance of the coefficients could be determined by the fact that, in this specific case, the number of available data points is drastically reduced: the mug-specific question was optional and about 20% of subjects preferred not to answer. This fact, adding to the 20% of missing data for demographic variables, reduces the set of available data by 35%.

Finally, as a robustness check, we ran additional regressions using the demographic variables more selectively but none of the changes we did to the benchmark model have a significant impact on the final results: more details are available in the online Appendix B.

5. Discussion and concluding remarks

The main results of our analysis are twofold. Firstly, the emergence of the instant endowment effect is documented for all types of goods. While this is now unanimously accepted in the case of tangible goods, the literature has not yet reached a consensus conclusion with respect to intangible and exchange goods.

The fact that we detect the presence of the EE for intangible goods, in particular, is relevant to the debate on whether mere possession or abstract ownership title is decisive in determining the EE. If one were to infer that from the cited works of Bagga et al. (2020) and Reb and Connolly (2007) the EE should be reduced or absent for intangibles, the present results do not support such an implication.

As far as exchange goods are concerned, the emergence of the EE has so far been attributed to some form of uncertainty about the value of the good itself, which is not present in our case since the nominal value of the Amazon voucher is certain: it remains to be seen whether an element of uncertainty may depend on a lack of familiarity with the electronic marketplace where this voucher can be spent, a hypothesis that deserves further investigation in the future.

Secondly, the results for the effect of longer possession on the EE are mixed. Although there is some upward trend in this effect over time, the results do not show the patterns of regularity that we would expect. Keeping in mind the methodological differences, this result is nevertheless consistent with some of the findings of Yamamoto and

[7] Specifically, the independent variables used are:
– possession condition (categorical, dummy coding, ref. level → buyer condition)
– sex (categorical, dummy coding, ref. level → female)
– age (numeric)
– Country of residence, grouped by continent for ease of presentation (categorical, dummy coding, ref. level → Oceania)
– employment status (categorical, dummy coding, ref. level → unemployed + other residual categories)
– student status (categorical, dummy coding, ref. level → NO).

[8] The questions considered are as follows:
– Mug question, asking whether the subject believes that he was influenced by the particular temporal framing described in the experiment and whether he believes that he would have given a different valuation in alternative scenarios (details in the online Appendix C, Figure 19) (categorical, dummy coding, ref. level → NO)
– Amazon question, asking how often the subject shops on Amazon (details in the online Appendix C, Figure 16) (categorical, dummy coding, ref. level → never + occasionally)
– Spotify question, asking if the subject already subscribes to Spotify or other streaming music services (details in the online Appendix C, Figure 15) (categorical, dummy coding, ref. level → NO + other services).

Table 11
Regression analysis of items valuations.

	Dependent variable: items valuations					
	Mug (1)	Amazon (1)	Spotify (1)	Mug (2)	Amazon (2)	Spotify (2)
Now	2.558**	11.798***	4.452**	1.218	11.006***	4.468**
	(0.002)	(0.00001)	(0.003)	(0.176)	(0.00003)	(0.002)
Day	3.500***	13.728***	5.338***	1.987*	13.026***	5.268***
	(0.00003)	(0.00000)	(0.0004)	(0.027)	(0.00000)	(0.0003)
Week	2.030*	14.509***	7.281***	0.369	13.786***	7.163***
	(0.014)	(0.00000)	(0.00001)	(0.681)	(0.00000)	(0.00001)
Month	3.562***	14.652***	5.240***	2.267*	14.219***	5.317***
	(0.00002)	(0.00000)	(0.0005)	(0.013)	(0.00000)	(0.0003)
Male	0.442	−1.658	−0.285	0.579	−0.863	−0.169
	(0.383)	(0.314)	(0.755)	(0.288)	(0.599)	(0.851)
Age	0.053	−0.018	−0.074	0.051	−0.082	−0.003
	(0.126)	(0.872)	(0.235)	(0.150)	(0.461)	(0.960)
Europe	−1.174	−1.488	−1.595	−0.722	−4.013	−1.354
	(0.277)	(0.672)	(0.413)	(0.524)	(0.257)	(0.480)
North America	0.199	2.267	−3.134°	0.762	−0.497	−2.296
	(0.847)	(0.499)	(0.092)	(0.481)	(0.884)	(0.213)
Full-time	−0.633	1.653	1.655	−0.650	1.391	1.210
	(0.323)	(0.427)	(0.152)	(0.342)	(0.498)	(0.289)
Part-time	−0.044	1.147	0.474	−0.352	0.955	−0.005
	(0.948)	(0.596)	(0.693)	(0.622)	(0.655)	(0.997)
Student	0.891	3.065	−0.005	0.977	3.077	0.105
	(0.169)	(0.146)	(0.997)	(0.156)	(0.139)	(0.928)
Q-Mug				2.765***		
				(0.00001)		
Q-Amazon					6.032***	
					(0.0005)	
Q-Spotify						3.754***
						(0.0002)
Constant	2.613	76.110***	16.136***	2.265	77.661***	11.608***
	(0.108)	(0.000)	(0.00000)	(0.180)	(0.000)	(0.0003)
Observations	417	417	417	339	417	417
R^2	0.089	0.129	0.074	0.142	0.156	0.108
Adjusted R^2	0.064	0.106	0.049	0.110	0.131	0.081
Residual Std. Error	5.058	16.459	9.121	4.851	16.228	8.967
	(df = 405)	(df = 405)	(df = 405)	(df = 326)	(df = 404)	(df = 404)
F Statistic	3.600***	5.464***	2.956***	4.481***	6.204***	4.055***
	(df = 11; 405)	(df = 11; 405)	(df = 11; 405)	(df = 12; 326)	(df = 12; 404)	(df = 12; 404)

Note: P-values in parentheses °p < 0.1; *p < 0.05; **p < 0.01; ***p < 0.001.

Navarro-Martinez (2022). One possible, obvious conclusion is that the effects of prolonged possession may be non-linear or ambiguous. On the other hand, these ambiguities could also be the result of the particular approach we have taken in this paper. Indeed, one possible explanation for the absence of the expected regularities is that the design of the questionnaire was not fully effective in inducing a sufficient degree of identification of the subjects with the situation represented. Indeed, it is well known that individuals have difficulty in predicting the impact that experienced emotions (affects) may have on their decisions (see Loewenstein et al. (2003) on the *projection bias* and Bardsley et al. (2010), §6.4.3 for a more detailed discussion), and the same may be true in our case, where the subtle differences induced by prolonged possession of a good may be difficult to anticipate in the absence of actual experience. A second source of interference, although related to the previous one, could be the lack of incentives, which are often necessary to motivate subjects to engage in tasks of higher cognitive effort, such as envisioning the consequences of different time scenarios.[9] The large variance of the data we collected could be a symptom of this fact: for example, Camerer and Hogarth (1999) and Smith and Walker (1993) show that one of the most robust effects of the presence of incentives in the experimental setting is to reduce the variance of the data collected, especially in judgment tasks that respond to better or more intense effort. These considerations lead us to believe that an interesting possible continuation of this study could be an incentivized experiment, perhaps shedding light on such outstanding questions.

[9] This aspect also affects Yamamoto and Navarro-Martinez (2022).

CRediT authorship contribution statement

Domenico Colucci: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Chiara Franco:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Vincenzo Valori:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The dataset and codes used for this research can be found in Github: https://github.com/domenico-colucci/dataset-CFV-EE.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.socec.2024.102216.

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