Received: 3 March 2022 | Accepted: 27 March 2023

DOI: 10.1002/jcpy.1353

RESEARCH ARTICLE

A meta-analysis on the effects of just-below versus round prices

Eve Sarah Troll[1] | Julius Frankenbach[2] | Malte Friese[2] | David D. Loschelder[1]

[1]Institute of Management and Organization, Leuphana University of Lüneburg, Lüneburg, Germany

[2]Department of Psychology, Saarland University, Saarbrücken, Germany

Correspondence
Eve Sarah Troll and David D. Loschelder, Institute of Management and Organization, Leuphana University of Lüneburg, Universitätsallee 1, 21335 Lüneburg, Germany.
Email: eve.troll@posteo.de and david.loschelder@leuphana.de

Funding information
Deutsche Forschungsgemeinschaft, Grant/Award Number: LO 2201/2-1

Abstract

Marketers' proclivity for just-below prices (e.g., \$9.99) is rooted in an expected higher demand than for round prices (\$10.00). The literature, however, lacks a comprehensive assessment of when and how price endings matter. Three mechanisms might explain price-ending effects on consumers' purchase decisions: just-below prices (1) improve price perceptions, but (2) impair perceived product quality, and (3) cause consumers to underestimate prices. A preregistered meta-analysis (k=69 studies, m=362 effect sizes, N=40,541) established that just-below (vs. round) prices tend to increase purchase decisions (g=0.13, $CI_{95\%}$[0.01, 0.25]), result in an advantageous price image (g=0.28, $CI_{95\%}$[0.09, 0.48]), have no effect on perceived product quality (g=0.00, $CI_{95\%}$[−0.17, 0.18], p=0.96), and are more often underestimated (g=0.67, $CI_{95\%}$[0.04, 1.30]). Participant, study, price, and product characteristics moderate the magnitude of these effects. Overall, the effect sizes are small and highly heterogenous, p-curve analyses revealed a large proportion of nonsignificant effects, and publication bias corrections suggest smaller and, at times, nonsignificant true effects. We discuss theoretical and applied implications for the pricing literature.

KEYWORDS
just-below prices, meta-analysis, price endings, pricing, round prices

INTRODUCTION

Around the globe, it is difficult to walk into a supermarket or to shop online without encountering prices that lie just-below the nearest round figure—\$2.99 for toothpaste, \$59.90 for a sweater, or \$899 for the newest smartphone. Given the pervasiveness of these just-below prices, their efficacy to produce higher demand than a negligibly higher round price is generally taken for granted (Gendall et al., 1997). But does the fact that sellers and managers around the globe use this pricing strategy so consistently necessarily mean that it is effective? Several decades ago, Holloway (1973) was surprised to find that "a strategy so widely used and accepted by merchants and academicians has so little proof behind it" (1973, p. 77). Since then, a number of studies have examined pricing effects (Leone et al., 2012). Nonetheless, Wieseke et al. (2016) concluded recently that "even after 40 years [Holloway's] observation still holds true" (p. 474). A comprehensive meta-analysis that summarizes what is currently known about price-ending effects on consumers' behavior and information processing seems overdue.

This meta-analysis is warranted for another reason: Although researchers have increased their efforts to substantiate pricing effects over the past decades, the scientific evidence is quite heterogeneous—observed effects range from positive to null to even negative. Some studies have shown that consumers indeed prefer just-below over round prices (e.g., Choi et al., 2014; Schindler & Warren, 1988), but other studies have shown no conclusive effects on purchase decisions (e.g., Carver & Padgett, 2012; Georgoff, 1972) or even a preference for round over just-below prices (e.g., Allred et al., 2010;

Accepted by Lauren Block, Editor; Associate Editor, Chris Janiszewski.

Wieseke et al., 2016). This mixed evidence corroborates the need for a meta-analytic synthesis including moderating factors that can potentially account for the effect heterogeneity, thereby possibly advancing theorizing as well (Grewal et al., 2018).

Finally, the present meta-analysis sought to foster the theoretical understanding of price-ending effects by examining the information processing that price endings trigger in consumers. Specifically, we examined three psychological processes that affect purchase decisions: consumers' perception of price image, quality image, and their accuracy in recalling prices.

In the present article, we first review specific characteristics and presumed origins of just-below prices, then review the effects of just-below (vs. round) prices on purchase decisions and elucidate psychological theorizing on the three assumed underlying processes. We synthesize the empirical evidence for each of the four outcomes, examine theoretically derived and methodological moderators that promise insights into variables that influence effect magnitude, and examine the interplay of the three process variables and consumers' purchase decisions.

WHAT ARE JUST-BELOW PRICES AND WHAT IS THEIR ORIGIN?

While just-below prices can be traced back more than a century (Schindler & Wiman, 1989), the pricing strategy itself lacks a consistent name. Scholars have introduced many terms, such as "psychological prices" (Rogers, 1990), "odd prices" (Monroe, 1990), "magic prices" (Sturdivant, 1970), "charm prices" (Gabor, 1977), and "irrational," "intuitive," or "rule-of-thumb" prices (Kreul, 1982). The term "odd prices" has been used frequently, yet all prices that do not end in 0.00 have been subsumed under this category—even "precise" prices such as \$50.77 or \$164.81 (Lambert, 1975). Following recent research (Wieseke et al., 2016), we use "just-below" to refer to prices that lie just-below the nearest round figure (e.g., \$2.99 vs. \$3.00, \$39.90 vs. \$40.00, \$59.95 vs. \$60.00, or \$749 vs. \$750; Gendall et al., 1997), and also differ from "precise" prices (e.g., \$3.11; \$40.23; or \$743.36; see Thomas et al., 2010; Wieseke et al., 2016).

An investigation in 1948 showed that 64.0% of retail store advertisements in various cities across the United States ended in non-round digits—37.0% of these ended in the digit "9" (Rudolph, 1954). Over time, prices ending in 9 became even more prevalent (69%, Levy et al., 2011; Suri et al., 2004). To estimate the current prevalence of just-below prices, we conducted a pilot study using web scraping (Bradley & James, 2019), extracting 12,491 prices from web pages of a large online store and a large supermarket chain in the United States and Germany, respectively (for details, see osf.io/cgke2). The analysis showed that just-below prices remain highly prevalent and make up 64.5% of all prices (11.5% round, 24.0% precise prices). While the prevalence of just-below prices differs across countries (Suri et al., 2004), their general use is widespread and persistent.

Having been raised in environments with frequent just-below pricing, people often axiomatically accept that this is what prices (should) look like. But what is the origin of just-below prices? They have often been described as a historical artifact (McKenzie, 2008) that resulted from an anti-corruption intervention of the retail store Macy's in the early 1900s (Gendall et al., 1997; Twedt, 1965). The implementation of just-below prices helped Macy's management to prevent theft by their own employees. In contrast to round prices, for which consumers often had exact cash, just-below prices required clerks to enter sales into the cash register to issue change. Hence, just-below prices might not be the product of a management masterstroke. Instead, they may have simply been intended to counteract unethical organizational behavior, that is, to prevent employees from pocketing payments without recording sales.

HETEROGENEOUS PRICE-ENDING EFFECTS ON PURCHASE DECISIONS

Price-ending effects have been investigated most frequently for purchase decisions. We review exemplary research to introduce common procedures and to illustrate the effect heterogeneity. We begin with three field studies. In 1972, Georgoff examined sales numbers in six department stores from a leading US chain, randomly manipulating prices as just-below or round for predetermined weekly periods. Results varied drastically: For some products, sales were higher for just-below prices; for others, sales were lower for just-below prices; and for even others, sales were similar for just-below and round prices. Another large-scale study with mail orders of a women's clothing retailer in the United States found that sending customers a catalog with just-below prices produced an 8% higher sales volume than round prices (albeit this difference being statistically nonsignificant; Schindler & Kibarian, 1996). Finally, 12 UK retail stores conducted a field experiment: Six stores raised their usual just-below to round prices, while six other stores served as a control group (Bray & Harris, 2006). For almost all products, sales numbers were higher for round than for just-below prices.

A plethora of laboratory and online studies complements this field research. For instance, participants chose food items for a five-course meal from simulated menus more likely when the items had just-below (e.g., flounder for \$8.95) rather than round prices (e.g., 9.00; Schindler & Warren, 1988). Similarly, individuals preferred just-below over round prices when choosing between different products (Coulter, 2001, 2002; Gendall et al., 1998) or when indicating their purchase intention for a price–product combination (Choi et al., 2014; Quigley & Notarantonio, 2015). Other studies point to potential moderators: For instance, consumers more likely chose a menu featuring round prices when instructed to opt for quality, but preferred just-below prices when imagining they had a tight budget (Manning & Sprott, 2009; Naipaul & Parsa, 2001).

THEORETICAL ACCOUNTS FOR JUST-BELOW-PRICING EFFECTS

From a theoretical perspective, the effect heterogeneity is not entirely surprising. Scholars have proposed that various mechanisms shape consumers' perception and processing of just-below versus round prices. Specifically, the literature distinguishes two main types of theoretical accounts, those based on "image effects" and those based on "level effects" (Figure 1).

Image effects

Image effects (also "meaning effects," Schindler & Kibarian, 2001) are effects of price endings on the meanings consumers infer (Schindler, 1991), particularly regarding price image and quality image. According to theorizing on *price image* (Figure 1[2]), just-below prices signal that a price is a good, discounted, and particularly low price (Schindler & Kibarian, 2001); consumers infer a "good deal" (see Coulter & Coulter, 2005). But why should they consider a difference of only one cent (or a few cents) a significant gain? Schindler and Kirby (1997) suggest that individuals often use round numbers as a reference point when evaluating prices because round numbers are easily accessible (see fluency theory; Oppenheimer, 2008). This might lead consumers to perceive a just-below price of, say, \$9.95 as a round number (\$10.00) along with a small gain of 5¢. Kahneman and Tversky's prospect theory (Kahneman & Tversky, 1979) elucidates how individuals assess and react to (potential) gains and losses: The subjective value of gains (losses) follows a negatively accelerated concave (convex) function. Thus, it indicates that the impact of a change in value diminishes with the distance from the reference point, and that the perception of a small gain (e.g., 5¢) could lead to an improvement in the evaluation of a price that is disproportionate to the gain's relatively small absolute size (Thaler, 1985; Schindler & Kirby, 1997 termed this the "perceived-gain effect," p. 193). Additionally, individuals may have learned to associate a just-below price with a discounted price image via "long-term repeated exposure to price endings in the context of the to-be-learned price, product, or store attributes" (Schindler, 1991, p. 4). Indeed, explicit signals for a discount (e.g., "20% off") are frequently combined with just-below prices (Schindler, 2006).

According to theorizing on *quality image* (Figure 1[3]), a just-below price may signal that the product is of lower quality than a round-priced product (Schindler & Kibarian, 2001). Consumers often use prices as a cue for product quality (Rao & Monroe, 1989; Völckner & Hofmann, 2007). This assumption is embedded in cue utilization theory (Olson, 1972), which posits that consumers use several cues—brand name, store name, and price—as quality indicators. Of the three, price is the most commonly studied quality cue. Corroborating the price-quality link, two meta-analyses found a positive relationship between price magnitude and perceived quality (Rao & Monroe, 1989; Völckner & Hofmann, 2007).

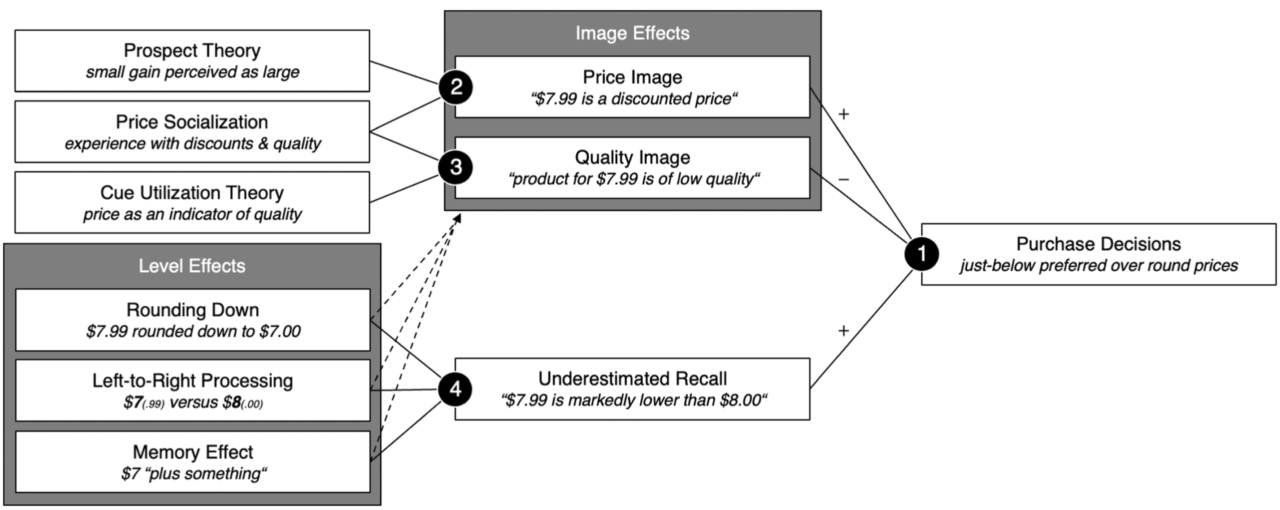

FIGURE 1 Schematic representation of prior theorizing on effects of just-below (vs. round) prices. Psychological effects on consumers' information processing (left) influence their price image, quality image, and underestimation of prices (middle). Specifically, just-below (vs. round) prices are assumed to result in a more positive price image, a more negative quality image, and underestimated price recall. In turn, these three effects are assumed to impact purchase decisions (right). Solid lines represent associations that are based on prior theorizing in the pricing literature. Dashed lines represent further plausible associations.

In other words, people perceive higher prices as signaling greater quality, in line with the adage "you get what you pay for". In addition, consumers may have learned over years to associate a just-below price with lower quality than a round price (Schindler, 1991). Indeed, most managers preferring round over just-below prices believe that round prices are associated with higher quality (Schindler et al., 2011), and companies tend to use round prices for products of higher quality (Stiving, 2000).

In sum, image theorizing suggests that individuals associate just-below (vs. round) prices with a "better deal" (price image) but also with a lower product quality (quality image). Thus, price image effects could increase purchase intentions for items with just-below prices, while quality image effects could decrease that likelihood (Figure 1).

Level effects

Level effects (also "underestimation effects"; Figure 1[4]; Manning & Sprott, 2009) refer to a set of cognitive processes that cause a distorted perception of two prices of essentially the same amount (e.g., \$14.99 and \$15.00; Baumgartner & Steiner, 2007; Wieseke et al., 2016). Proponents of this account elaborate three interrelated processes: First, consumers round prices down to the next full digit (Bizer & Schindler, 2005): \$14.99 is rounded down to \$14.00, while \$15.00 is not rounded at all. Second, consumers process price digits from left to right and place stronger emphasis on earlier digits. In the case of two prices, individuals compare digits one-by-one and stop this comparison once a difference is encountered (Coulter, 2001). A comparison between \$14.99 and \$15.00 stops after the second digit, given that 4 is lower than 5. This leads consumers to judge the difference between prices differently even though objective differences are identical (see "left-digit effect"; e.g., Sokolova et al., 2020); for instance, the difference between \$15.00 and \$14.99 is perceived as larger than that between \$15.01 and \$15.00. Third, to minimize cognitive effort (Kahneman, 2011) when memorizing a price, people exert greater effort for digits farther to the left, which carry a higher monetary value and importance than digits farther to the right (Schindler & Chandrashekaran, 2004). \$14.99 is remembered as \$14 "plus something," while \$15.00 is remembered as \$15. Proponents argued that these interrelated mechanisms jointly cause consumers to underestimate the magnitude of just-below relative to round prices. In turn, this underestimation causes a greater willingness to purchase products with just-below (vs. round) prices, because they appear to be better bargains (Figure 1). The label "level effects" subsumes the three processes because all cause consumers to perceive a just-below price as at a substantially lower price level than the round price that is factually only negligibly larger. To generate empirical evidence for the level effects theorizing, researchers have contrasted recall accuracy for just-below versus round prices (Figure 1[4]). For instance, when participants recall prices for 20 products (e.g., watch or sweatshirt) that they examined 2 days earlier (Schindler & Wiman, 1989), they, as expected, underestimate just-below prices more likely than round prices (see also Schindler & Chandrashekaran, 2004; Schindler & Kibarian, 1993).

THE PRESENT META-ANALYSIS

Our review illustrates the long-standing tradition of research on consumers' perception of and decision-making regarding just-below (vs. round) prices. Previous results, however, have been ambiguous and heterogeneous. In light of the ubiquity of just-below prices, the present meta-analysis promises substantial implications for marketing and retailing, but also for the theorizing on pricing effects. We aimed to organize and synthesize extant findings by pursuing two larger objectives: First, we examined price-ending effects on purchase decisions and on three variables indicative of consumers' processing of just-below (vs. round) prices. The reviewed theorizing suggests (partially opposing) effects on price image, quality image, and underestimated recall. Table 1 illustrates each of these key variables along with their definition, hypotheses, operationalization, and coding. Second, we conducted moderator analyses to examine when just-below pricing effects are stronger and when they are weaker. Third, we applied several methods (e.g., analyses of publication bias) to examine the robustness of these effects. In all, our analyses illuminate both underlying mechanisms and variables that influence the magnitude of these pricing effects as well as the robustness of these effects.

METHODS

We followed reporting guidelines for meta-analyses outlined in the PRISMA statement (Moher et al., 2009) and preregistered the study on the Open Science Framework (OSF, osf.io/nd2am). Following recent recommendations for reproducibility (Lakens et al., 2016), we made all data, code, and supplemental materials publicly available (osf.io/bqdpm).

Inclusion criteria

Studies were eligible for inclusion if they (a) examined effects of prices just-below the nearest round figure, hence ending with 0.90, 0.95, 0.98, or .*x*9 (e.g., \$2.99, \$59.90) or, in the case of an even dollar amount, if the final digit in front of the decimal was 9 (e.g., \$749; for a similar categorization, see Holdershaw et al., 1997); (b) compared outcomes for just-below with round prices; (c) quantified

TABLE 1 Pricing effects and key variables, along with their hypotheses, operationalization, and coding.

Key variable	Definition	Hypotheses	Operationalization	Coding
Behavioral and decision effects				
(1) Purchase Decisions	"[T]heory posits that prices ending in 9 or 99, 'just-below' prices, increase sales" (Wieseke et al., 2016, p. 474)	Just-below prices lead to more purchase decisions than round prices	Including effects on sales volume (Bray & Harris, 2006), purchase intentions (Choi et al., 2014), or consumer choice (Manning & Sprott, 2009)	Larger and more positive effects indicated higher consumer purchase intentions and more choice of just-below over round prices
Image effects				
(2) Price Image	Just-below (vs. round) prices are "more likely to give the impression that the price (a) is low relative to competitors' prices, (b) is a discount or sale price, and (c) has not recently been increased" (Schindler & Kibarian, 2001, p. 96)	Just-below prices lead to more advantageous price images than round prices	Including effects on the perception of a "sale" price (Schindler & Kibarian, 2001) or a particularly "low" price (Thomas & Morwitz, 2005)	Larger and more positive effects indicate more advantageous price images of just-below vs. round prices
(3) Quality Image	Just-below (vs. round) prices are "more likely to give the impression that (a) the advertised item is of low quality, (b) the sponsor's merchandise is generally of low quality, and (c) the sponsor is not a classy retailer" (Schindler & Kibarian, 2001, p. 96)	Just-below prices lead to less advantageous quality images than round prices	Including effects on the perception of the quality of one or more products or of the entire store (Schindler & Kibarian, 2001)	Larger and more negative effects indicate less advantageous quality images of products with just-below versus round prices
Level effects				
(4) Underestimated Recall	"Recalls of just-below prices should be more likely to be underestimates than those of even prices." (Schindler & Wiman, 1989, p. 168)	Just-below prices are more likely underestimated than round prices	Including effects on consumers' likelihood to underestimate just-below versus round prices in a recall task (Schindler & Wiman, 1989)	Larger and more positive effects indicate a higher likelihood of underestimated recall for just-below compared to round prices

Note: Hypotheses were preregistered on the Open Science Framework (osf.io/nd2am).

outcome variables indicative of purchase decisions (e.g., choice, actual purchases, or purchase intention), price image, quality image, or underestimated recall; (d) used an experimental or quasi-experimental design; and (e) were reported in English or German.

Search strategy

We conducted a systematic literature search using several online citation database providers—EBSCO, ISI Web of Science, and ProQuest. In EBSCO, we searched the databases PsycINFO, PsycARTICLES, PSYNDEX, and Business Source Premier with the following search term: TI pric* AND [TI end OR TI ending* OR TI odd OR TI digit* OR TI 9* OR TI nine* OR TI magic OR TI charm OR TI irrational OR TI intuitive OR TI rule-of-thumb OR TI just-below]. The search terms were slightly adapted to fit ISI and ProQuest. For ProQuest, we did not include historical newspapers and journals. We complemented this systematic search with reference harvesting and backward searches in Google Scholar (screening articles listed under "cited from") for the first tranche of matching articles that emerged from the systematic literature search. We also conducted unsystematic searches and issued calls for published or unpublished data (a) via several scientific societies (e.g., Academy of Marketing Science, European Association of Social Psychology), (b) via the international marketing community ELMAR (accessible to all academic members of the American Marketing Association), and (c) by direct request to 91 researchers, who had published articles already included in our meta-analysis and/or had been identified by Leone et al. (2012) as "established authors" in the field.

Screening

Two members of the research team tested the inclusion criteria independently on 100 randomly selected search results. Their conclusions agreed in 99% of the cases, suggesting sufficiently precise inclusion criteria. We then screened titles and abstracts of 4901 search results (see PRISMA flow chart in Figure 2). From $n = 107$ full articles with matching titles and abstracts, a total of 45 articles (all reported in English) were identified as meeting the criteria. When articles did not report the statistical values required to calculate effect sizes, we contacted the corresponding authors. 17 out of 23 author teams responded, and we wish to thank them for this

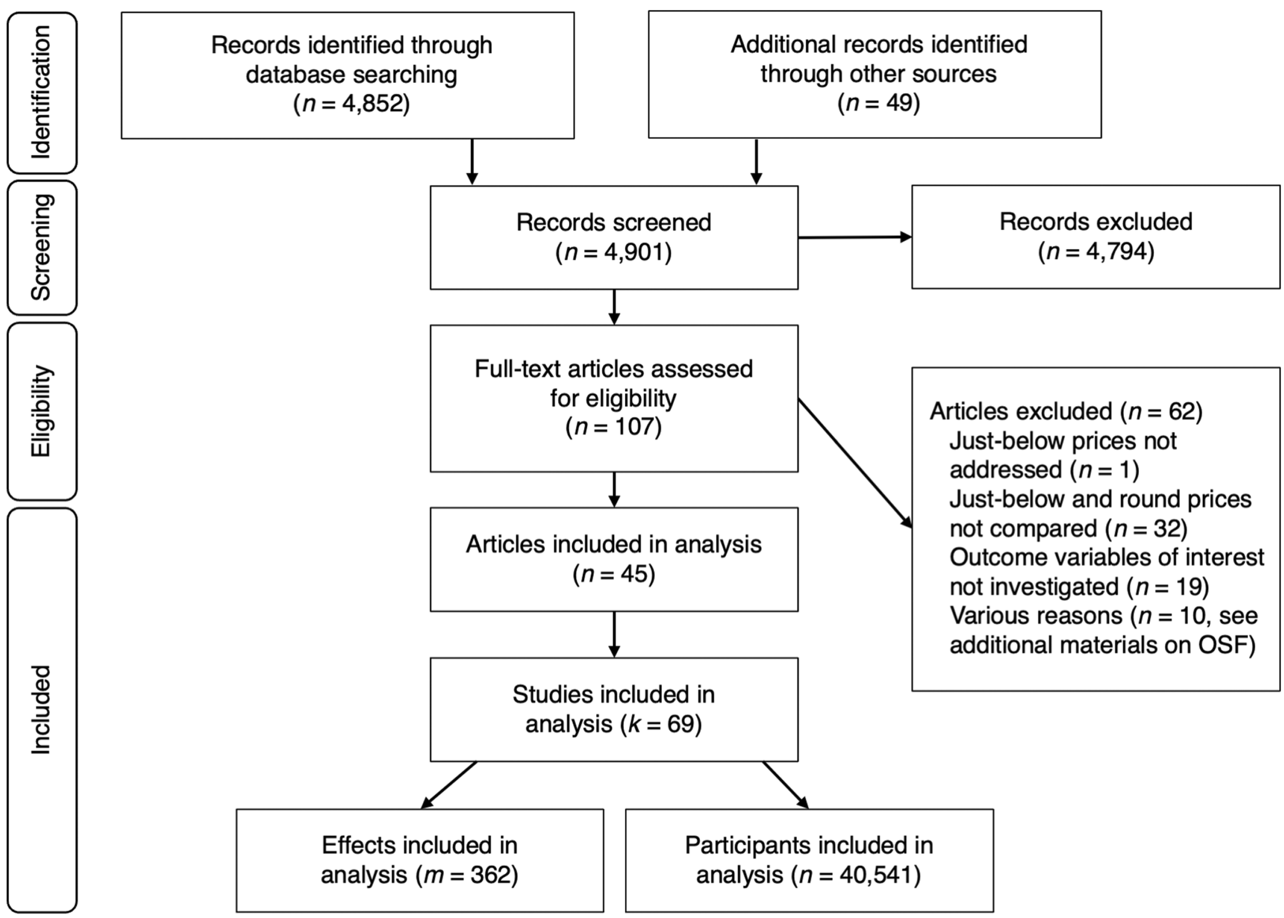

FIGURE 2 PRISMA flow chart of the screening process and study coding.

valuable cooperation. We could not include some prominent studies from the field because, for instance, they did not compare relevant outcomes between just-below and round prices (e.g., Anderson & Simester, 2003; Strulov-Shlain, 2019) or did not investigate one of our key outcomes (e.g., Snir & Levy, 2021). Detailed documentation of all study exclusions is available in the supplemental materials (osf.io/bqdpm).

Outcome coding

The introduction illustrates pricing effects on the main outcome, consumers' purchase decisions, and on the three process-oriented outcomes: price image, quality image, and underestimated recall (Figure 1). Please see Table 1 for details on our key variables.

Moderator coding

To examine variables that influence the magnitude of price-ending effects, we coded moderators organized according to whether they pertain to participants, study, price, or product characteristics. Because of space constraints, we only report the most theoretically promising moderators here. A list of all preregistered moderators (e.g., publication year, impact factor of the journal) and corresponding analyses are available in the supplemental materials. After a pilot coding, two raters coded all moderators independently. Interrater reliability, indexed by the intraclass correlation coefficient (ICC) for continuous moderators (Shrout & Fleiss, 1979) and kappa for categorical moderators (Cohen, 1968), was high according to common standards (Cicchetti, 1994; mean $\kappa=0.97$, mean ICC[2, 1]=0.83).

Study population

Certain effects differ depending on the recruited participant population (e.g., Henrich et al., 2010; Loschelder et al., 2016). Study population was coded as “students” for samples recruited at a university or as “public” for all other populations.

Context culture

Scholars have argued that just-below-pricing effects may differ between high- and low-context cultures (e.g., Jeong & Crompton, 2018). Members of high-context cultures (e.g., China) interpret indirect and implicit information; they “read between the lines” (Hall, 1976). In contrast, members of low-context cultures (e.g., United States) interpret direct and explicit information; they take information at face value and assume unambiguous meaning. Some scholars (Jeong & Crompton, 2018; Nguyen et al., 2007) have suggested that members of high-context cultures perceive a price of \$3.99 as really \$4.00, while members of low-context cultures are more likely affected by price endings. We coded context culture as “high” or “low”.

Prevalence of prices

Proponents of the price-socialization mechanism (Figure 1) might argue that a higher prevalence of just-below prices provides consumers with ample opportunity to learn the association between price endings and discounts, thus leading to larger effects on price image. In contrast, one could predict that an overabundance of just-below prices causes these effects to wear off. For our price-prevalence scores, we coded the percentage of just-below prices among the first 50 hits for a product search on each country's Amazon web page for “lamp” and “computer,” respectively. The rationale for this approach was to maximize comparability across countries by searching the same products in the same marketplace, while covering as many included effect sizes as possible. Indeed, this approach covered 90.3% ($m=327$) of single effect sizes with information on price prevalence in the respective country.

Design

Pricing research has used both between- and within-subject designs. Anticipating that effects may be larger for within-subject designs (e.g., Coulter, 2001) compared to between-subject designs (e.g., Choi et al., 2014) because the former allow for price comparisons, we investigated study design as a moderator (see Coles et al., 2019).

Setting

Data assessed in the laboratory or online may provide more experimental control (i.e., internal validity). In contrast, field data (e.g., from a supermarket) may possess a higher external validity (Berkowitz & Donnerstein, 1982; Schram, 2005) but may be smaller in size due to reduced internal validity (and a flurry of other influential causal factors). We coded this moderator as “laboratory/online” versus “field”.

Response interval

Pricing effects on recall accuracy (e.g., Schindler & Kibarian, 1993) might decline in size as more time elapses between the presentation of the price and recall. We coded response interval as “immediately” when

TABLE 2 Purchase decisions: moderation analyses for just-below-pricing effects ($k = 48$, $m = 160$).

	Summary effect							Test of moderation				
Moderator	g	$CI_{95\%}$	t	df	p	k	m	t	df	p	I^2	τ^2
None	0.11	[0.00, 0.22]	2.08	44.70	0.043	48	160				92.84	0.12
Participants												
Study population								0.36	40.10	0.72	92.67	0.12
Students	0.09	[−0.05, 0.24]	1.30	26.50	0.21	29	91					
Public	0.13	[−0.04, 0.30]	1.63	17.60	0.12	19	69					
Context culture								−0.21	23.90	0.84	92.41	0.12
Low	0.12	[0.01, 0.23]	2.26	31.20	0.031	34	120					
High	0.09	[−0.19, 0.37]	0.69	12.50	0.50	14	40					
Prevalence of prices											93.01	0.14
Intercept	−0.01	[−0.66, 0.65]	−0.03	5.89	0.98	44	147					
Slope	0.00	[−0.01, 0.01]	0.37	6.35	0.72							
Study												
*Study design								−2.74	22.00	0.012	91.68	0.10
Within subject	0.34	[0.11, 0.57]	3.21	11.90	0.008	15	38					
Between subject	0.02	[−0.09, 0.13]	0.32	32.000	0.75	35	122					
Study setting								0.89	8.90	0.40	91.61	0.11
Field	0.04	[−0.14, 0.22]	0.52	5.98	0.62	7	22					
Lab/online	0.13	[−0.001, 0.25]	2.01	37.91	0.05	41	138					
Response interval								−1.67	4.20	0.17	92.80	0.13
Delayed	0.33	[−0.06, 0.72]	2.49	3.50	(0.08)	5	16					
Immediately	0.09	[−0.02, 0.20]	1.58	40.80	0.12	44	144					
Price												
Digit manipulation								−1.51	21.40	0.15	92.73	0.13
Post-decimal	0.16	[0.04, 0.27]	2.77	34.60	0.009	38	129					
Pre-decimal	−0.04	[−0.29, 0.22]	−0.32	13.20	0.75	17	31					
Change first digit								1.42	19.80	0.17	88.58	0.15
No	−0.02	[−0.25, 0.22]	−0.15	11.70	0.88	16	53					
Yes	0.16	[0.02, 0.31]	2.29	30.00	0.030	34	93					
Round comparison price								1.66	42.10	0.10	89.49	0.10
No	0.01	[−0.16, 0.18]	0.12	21.40	0.91	24	52					
Yes	0.19	[0.05, 0.32]	2.83	23.60	0.009	27	108					
Price level (in $100 s)											88.05	0.13
Intercept	0.15	[0.03, 0.28]	2.43	34.59	0.021	41	144					
Slope	−0.03	[−0.07, 0.01]	−1.84	6.22	0.11							
Product												
Type of product											91.43	0.11
Intercept	0.06	[−0.22, 0.34]	0.41	31.20	0.68	48	160					
Slope	0.02	[−0.07, 0.12]	0.47	24.10	0.64							

TABLE 2 (Continued)

Moderator	Summary effect g	$CI_{95\%}$	t	df	p	k	m	Test of moderation t	df	p	I^2	τ^2
Brand								0.70	41.20	0.49	92.28	0.12
No	0.08	[−0.07, 0.22]	1.12	24.70	0.272	27	90					
Yes	0.15	[−0.02, 0.33]	1.85	19.70	0.08	22	70					

Note: Italics in the first column indicate continuous moderators, for which intercepts and slopes are listed. All variables for which the moderation analyses yielded $p<0.10$ are marked with an asterisk. g=Hedge's g effect size; df=small-sample-corrected degrees of freedom; $CI_{95\%}$=95% confidence interval; m=number of effect sizes in the moderator category; k=number of studies per moderator level. Significant test statistics for the moderators indicate significance of the overall model. I^2 is the percentage of true variance in the total observed effect variance after accounting for the indicated moderator. Please note that higher df coincide with higher statistical confidence. When df fall below 4, significance tests should be interpreted with caution. Accordingly, in these cases, we report p values in parentheses.

prices were recalled without delay and as "delayed" when there was a filler task or when participants waited for longer time intervals before providing their response (e.g., Schindler & Wiman, 1989).

Digit manipulation

According to prospect theory's perceived-gain effect (Figure 1), just-below pricing involving the digit before the decimal ("pre-decimal"; e.g., \$9 vs. \$10) should lead to the perception of a larger gain (gain of \$1) compared to just-below pricing involving the digits following the decimal ("post-decimal"; e.g., \$7.99 vs. \$8.00, gain of 1¢; see also Coulter et al., 2012). We coded the moderator digit manipulation according to these two categories.

Change first digit

Authors have reasoned that just-below prices affect price image more strongly when the leftmost digit of a price changes—a phenomenon coined the "left-digit effect" (e.g., Manning & Sprott, 2009). For instance, in one study, when the first digit changed, participants rated the price magnitude significantly lower for just-below prices (\$2.99) than for round prices (\$3.00); when the first digit did not change (i.e., \$3.59 vs. \$3.60), ratings of price magnitude did not differ (Thomas & Morwitz, 2005; see also Chang & Chen, 2014). The authors concluded that a lower price image "is more likely to occur when introducing a nine ending in the price causes a change in the leftmost digit" (Thomas & Morwitz, 2005, p. 63). To test the empirical foundation of this assumption, we coded whether first digits changed or not.

Round comparison price

Round comparison prices might affect consumers' perception of just-below prices in either of two ways: First, price-ending effects could be weaker when consumers become explicitly aware of different price endings. For instance, making participants aware of the difference between a just-below and a round price (i.e., "How different is \$99.95 from \$100.00?") eliminated the effect on purchase decisions (Choi et al., 2014). Second, in contrast, a just-below price (\$7.99 for a product) might become more distinctive (and attractive) when other products feature round prices (\$8.00 for a similar product; see Biswas et al., 2002). We coded the presence versus absence of round comparison prices accordingly.

Price level

While some studies have found larger effects for lower compared to higher price levels (Jaber & Jaber, 2017), others have found the opposite (Manning & Sprott, 2009; see Lin & Wang, 2017). We hence coded products' absolute price level in the round-price control condition as a continuous moderator. Because absolute prices differ as a function of currency, country, and publication year (and inflation), we applied a two-step procedure: First, converting each price to US dollars while accounting for the country's purchase power parity in the publication year (provided by the OECD). Second, we used the Consumer Price Index from the U.S. Bureau of Labor Statistics (Williamson, 2020) to calculate the value of that dollar amount in 2020. The continuous price level index (in \$100 s) is thus comparable across time and currencies.

Type of product

At times, pricing effects appear to differ for hedonic versus utilitarian products (Tripathi & Pandey, 2018a; Wadhwa & Zhang, 2015). For instance, in one study, when participants examined a utilitarian product (laptop) and a similar product with more hedonic attributes (more visually attractive laptop), they were more likely to choose the hedonic over the utilitarian option when it featured a just-below rather than a round price (Choi et al., 2014). For utilitarian products, price endings had no effects on purchase likelihood. To investigate this moderator, we coded products on a scale from 1 (*clearly utilitarian*) to 5 (*clearly hedonic*).

TABLE 3 Price image: moderation analyses for just-below-pricing effects (k = 26, m = 110).

	Summary effect							Test of moderation				
Moderator	g	$CI_{95\%}$	t	df	p	k	m	t	df	p	I^2	τ^2
None	0.25	[0.09, 0.40]	3.25	23.30	0.003	26	110				87.13	0.08
Participants												
*Study population								−3.58	9.75	0.005	85.18	0.07
Students	0.36	[0.16, 0.56]	3.71	17.88	0.002	20	80					
Public	−0.02	[−0.12, 0.09]	−0.40	4.95	0.71	6	30					
*Context culture								5.54	8.73	<0.001	83.56	0.06
Low	0.13	[−0.03, 0.29]	1.68	16.60	0.11	19	75					
High	0.63	[0.51, 0.74]	13.63	5.60	<0.001	7	35					
**Prevalence of prices*											83.75	0.06
Intercept	1.72	[0.53, 2.90]	3.08	16.00	0.007	20	91					
Slope	−0.02	[−0.04, −0.005]	−2.75	15.90	0.014							
Study												
Study design								−0.13	1.23	(0.92)	86.85	0.09
Within subject	0.27	[−1.35, 1.89]	2.11	1.00	(0.28)	2	5					
Between subject	0.25	[0.08, 0.42]	3.01	21.80	0.006	24	105					
Study setting								–	–	–	–	–
Field	–	–	–	–	–	1	3					
Lab/online	–	–	–	–	–	25	107					
Response interval								0.09	2.43	(0.94)	87.46	0.08
Delayed	0.23	[−0.79, 1.24]	1.04	1.87	(0.41)	3	15					
Immediately	0.25	[0.08, 0.42]	3.03	20.61	0.006	23	95					
Price												
*Digit manipulation								4.94	5.83	0.003	84.94	0.06
Post-decimal	0.17	[0.004, 0.33]	2.15	18.46	0.045	21	92					
Pre-decimal	0.62	[0.47, 0.77]	11.44	4.19	<0.001	6	18					
*Change first digit								2.43	5.38	0.06	87.73	0.18
No	0.06	[−0.20, 0.33]	0.70	3.84	(0.52)	6	31					
Yes	0.39	[0.17, 0.61]	3.74	16.28	0.002	19	64					
*Round comparison price								3.95	21.83	<0.001	83.26	0.07
No	0.00	[−0.18, 0.17]	−0.06	9.97	0.96	12	51					
Yes	0.47	[0.27, 0.67]	5.18	12.67	<0.001	15	59					
Price level (in $100 s)											88.71	0.20
Intercept	0.35	[0.11, 0.60]	3.11	15.25	0.007	17	91					
Slope	−0.03	[−0.28,0.23]	−0.50	1.81	(0.67)							
Product												
Type of product											87.55	0.08
Intercept	0.46	[−0.01, 0.93]	2.08	14.20	0.06	26	110					
Slope	−0.11	[−0.36, 0.13]	−0.98	12.80	0.34							

TABLE 3 (Continued)

	Summary effect							Test of moderation				
Moderator	*g*	CI$_{95\%}$	*t*	df	*p*	*k*	*m*	*t*	df	*p*	I^2	τ^2
Brand								1.48	21.70	0.15	86.11	0.08
No	0.15	[−0.06, 0.35]	1.57	12.00	0.14	14	70					
Yes	0.37	[0.11, 0.62]	3.20	10.50	0.009	13	40					

Note: Italics in the first column indicate continuous moderators, for which intercepts and slopes are listed. All variables for which the moderation analyses yielded $p<0.10$ are marked with an asterisk. g=Hedge's g effect size; df=small-sample-corrected degrees of freedom; CI$_{95\%}$=95% confidence interval; m=number of effect sizes in the moderator category; k=number of studies per moderator level. Significant test statistics for the moderators indicate significance of the overall model. I^2 is the percentage of true variance in the total observed effect variance after accounting for the indicated moderator. Please note that higher df coincide with higher statistical confidence. When df fall below 4, significance tests should be interpreted with caution. Accordingly, in these cases, we report p values in parentheses.

Brand

A product's brand is often used to evaluate its quality. Consumers may have prior experience with a brand, which, in turn, shapes their product evaluations (Rao & Monroe, 1989). We coded whether a brand cue was present (or not).

Effect-size calculation

An R script (R Core Team, 2020) detailing all effect-size calculations is available in the supplemental materials. We computed Hedges' g effect sizes and accompanying variances (Var$_g$). Hedges' g, like Cohen's d, indicates the difference between groups in the metric of the pooled standard deviation, but additionally corrects for small sample sizes (Hedges, 1981). The R package *compute.es* (Del Re, 2013) provides a comprehensive set of functions using recommended formulas (Cooper et al., 2009) to calculate effect sizes, along with their variances, confidence intervals, and p values. For within-subject designs, we used formulas that account for the correlation between two measures (Borenstein et al., 2009; Cooper et al., 2009). As this correlation is rarely reported in original within-subject studies, we followed other meta-analysts (Coles et al., 2019) and assumed a correlation of 0.50. Robustness analyses showed that the inferences were not appreciably different when we assumed r=0.20, 0.50, or 0.80 ($-0.01<\Delta g<0.01$; $-1.50<\Delta I^2<0.58\%$). For brevity, we report results for r=0.50 only. For studies with multiple price conditions, we compared each just-below price against the round price. For studies with multiple outcomes, we computed one effect size per outcome. We used robust variance estimation (RVE; detailed below) to account for the resulting effect-size dependency.

For full transparency, we wish to highlight two procedural decisions: First, some data were provided in 2×2 contingency tables (e.g., sales as the number of consumers buying a product out of the total number of consumers; Georgoff, 1972). For these data, we used the *prop.test* function from the *stats* package for R to quantify the extent to which proportions differed as a function of price ending. Second, for one-sample distributions (e.g., number of participants choosing a just-below over a round price; Choi et al., 2014), we calculated chi-squared values (χ^2) by comparing observed means to an equal distribution (Field et al., 2012).

Meta-analytic procedure

We used random-effects meta-analysis models rather than fixed-effects models for all analyses because the types of price manipulations, the research fields, the directions and magnitudes of effect sizes, and the assessed outcome variables varied considerably. It seemed highly unreasonable to expect one true, "fixed" population effect (Borenstein et al., 2009). We relied on RVE (Hedges et al., 2010), a state-of-the-art modeling technique that accounts for various kinds of dependency and allows the inclusion of multiple effect sizes per study in a single model. All RVE models were fitted using the *robumeta* package for R (Fisher et al., 2017). We implemented significance tests that include small-sample-corrected degrees of freedom (df) and adjusted variance–covariance matrices. Specifically, we conducted approximate Hotelling–Zhang tests (HTZ; Tipton & Pustejovsky, 2015) with the *clubSandwich* package for R for multiple parameters (Pustejovsky, 2020) and t tests for single parameters (Tipton, 2015). The statistic for single parameters may provide inaccurate results when degrees of freedom fall below four (Tipton, 2015). Consequently, p values and confidence intervals for estimates with df<4 should be interpreted with caution (Tables 2–5 report these estimates in parentheses).

Dependency correction

When using RVE, meta-analysts need to decide between two weighting schemes to adjust for effect-size dependency (Hedges et al., 2010). "Correlated" effect weights are recommended when dependency results from studies providing multiple effects from the same sample of participants. "Hierarchical" effect weights are

TABLE 4 Quality image: moderation analyses for just-below-pricing effects ($k = 14$, $m = 59$).

	Summary effect							Test of moderation				
Moderator	g	$CI_{95\%}$	t	df	p	k	m	t	df	p	I^2	τ^2
None	0.00	[−0.11, 0.12]	0.10	10.10	0.92	14	59				44.59	0.02
Participants												
Study population								−0.62	6.30	0.56	43.82	0.02
Students	0.03	[−0.15, 0.21]	0.36	7.81	0.73	10	25					
Public	−0.03	[−0.19, 0.13]	−0.61	2.56	(0.59)	4	34					
Context culture								–	–	–	–	–
Low	–	–	–	–	–	14	59					
High	–	–	–	–	–	0	0					
Prevalence of prices											46.92	0.03
Intercept	0.15	[−0.83, 1.14]	0.40	5.05	0.71	13	56					
Slope	0.00	[−0.02, 0.01]	−0.48	3.85	(0.66)							
Study												
Study design								–	–	–	–	–
Within subject	–	–	–	–	–	1	6					
Between subject	–	–	–	–	–	13	53					
Study setting								–	–	–	–	–
Field	–	–	–	–	–	0	0					
Lab/online	–	–	–	–	–	14	59					
Response interval								–	–	–	–	–
Delayed	–	–	–	–	–	0	0					
Immediately	–	–	–	–	–	14	59					
Price												
Digit manipulation								−2.42	2.91	(0.10)	37.82	0.01
Post-decimal	0.04	[−0.07, 0.16]	0.86	7.86	0.42	11	52					
Pre-decimal	−0.39	[−1.00, 0.23]	−2.28	2.41	(0.13)	4	7					
Change first digit								0.76	5.63	0.48	36.30	0.02
No	−0.06	[−0.10, −0.01]	−3.77	3.26	(0.028)	5	23					
Yes	0.01	[−0.21, 0.22]	0.10	5.74	0.93	8	22					
Round comparison price								−1.68	5.70	0.15	37.72	0.02
No	0.05	[−0.10, 0.21]	0.81	7.38	0.44	10	21					
Yes	−0.07	[−0.21, 0.06]	−2.04	2.42	(0.16)	4	38					
Price level (in $100 s)											42.57	0.03
Intercept	0.02	[−0.12, 0.16]	0.36	4.51	0.74	8	44					
Slope	−0.04	[−0.20, 0.12]	−0.77	3.59	(0.49)							
Product												
Type of product											46.53	0.02
Intercept	0.01	[−0.27, 0.29]	0.09	7.86	0.93	14	59					
Slope	0.00	[−0.12, 0.11]	−0.05	8.74	0.96							
*Brand								2.53	6.42	0.042	32.20	0.01
No	−0.10	[−0.22, 0.03]	−2.83	2.53	(0.08)	5	30					
Yes	0.08	[−0.06, 0.22]	1.35	7.50	0.22	10	29					

Note: Italics in the first column indicate continuous moderators, for which intercepts and slopes are listed. All variables for which the moderation analyses yielded $p < 0.10$ are marked with an asterisk. g = Hedge's g effect size; df = small-sample-corrected degrees of freedom; $CI_{95\%}$ = 95% confidence interval; m = number of effect sizes in the moderator category; k = number of studies per moderator level. Significant test statistics for the moderators indicate significance of the overall model. I^2 is the percentage of true variance in the total observed effect variance after accounting for the indicated moderator. Please note that higher df coincide with higher statistical confidence. When df fall below 4, significance tests should be interpreted with caution. Accordingly, in these cases, we report p values in parentheses.

TABLE 5 Underestimated recall: moderation analyses for just-below-pricing effects (k=8, m=33).

	Summary effect							Test of moderation				
Moderator	***g***	**$CI_{95\%}$**	***t***	**df**	***p***	***k***	***m***	***t***	**df**	***p***	**I^2**	**τ^2**
None	0.60	[0.23, 0.98]	3.82	6.94	0.007	8	33				91.55	0.19
Participants												
Study population								0.33	4.21	0.76	92.60	0.23
Students	0.56	[0.10, 1.02]	3.38	3.98	(0.028)	5	26					
Public	0.70	[−0.95, 2.34]	1.83	1.99	(0.21)	3	7					
Context culture								–	–	–	–	–
Low	–	–	–	–	–	7	25					
High	–	–	–	–	–	1	8					
Prevalence of prices											92.55	0.23
Intercept	0.90	[−2.79, 4.58]	0.61	5.33	0.56	8	33					
Slope	0.00	[−0.05, 0.04]	−0.21	5.25	0.84							
Study												
Study design								1.88	1.85	(0.21)	91.07	0.20
Within subject	0.27	[−1.78, 2.32]	1.66	1.00	(0.35)	2	6					
Between subject	0.73	[0.25, 1.22]	3.89	4.97	0.012	6	27					
Study setting								–	–	–	–	–
Field	–	–	–	–	–	1	1					
Lab/online	–	–	–	–	–	7	32					
*Response interval								3.10	4.80	0.028	86.70	0.11
Delayed	0.24	[−0.19, 0.67]	2.36	2.00	(0.14)	3	7					
Immediately	0.86	[0.37, 1.35]	4.92	3.97	(0.008)	5	26					
Price												
Digit manipulation								–	–	–	–	–
Post-decimal	–	–	–	–	–	8	32					
Pre-decimal	–	–	–	–	–	1	1					
Change first digit								2.56	2.50	(0.10)	88.05	0.30
No	0.58	[0.13, 1.02]	5.71	1.96	(0.031)	3	14					
Yes	0.97	[−0.06, 2.00]	6.13	1.42	(0.05)	3	11					
Round comparison price								−1.90	1.70	(0.22)	88.53	0.13
No	1.09	[−2.90, 5.08]	3.46	1.00	(0.18)	2	7					
Yes	0.44	[0.09, 0.79]	3.23	4.90	0.024	6	26					
Price level (in $100 s)											85.26	0.23
Intercept	0.59	[0.29, 0.89]	6.20	2.99	(0.008)	4	25					
Slope	0.07	[0.01, 0.12]	9.77	1.27	(0.038)							
Product												
Type of product						8	33				92.31	0.25
Intercept	0.51	[−0.44, 1.45]	1.52	3.79	(0.21)							
Slope	0.04	[−0.48, 0.56]	0.24	3.33	(0.83)							
Brand								0.33	4.21	0.76	92.60	0.23
No	0.56	[0.10, 1.02]	3.38	3.98	(0.028)	5	26					
Yes	0.70	[−0.95, 2.34]	1.83	1.99	(0.21)	3	7					

Note: Italics in the first column indicate continuous moderators, for which intercepts and slopes are listed. All variables for which the moderation analyses yielded p<0.10 are marked with an asterisk. g=Hedge's g effect size; df=small-sample-corrected degrees of freedom; $CI_{95\%}$=95% confidence interval; m=number of effect sizes in the moderator category; k=number of studies per moderator level. Significant test statistics for the moderators indicate significance of the overall model. I^2 is the percentage of true variance in the total observed effect variance after accounting for the indicated moderator. Please note that higher df coincide with higher statistical confidence. When df fall below 4, significance tests should be interpreted with caution. Accordingly, in these cases, we report p values in parentheses.

recommended when dependency results from authors reporting multiple studies. As both types of dependency often exist simultaneously, Tanner-Smith et al. (2016) recommended choosing the weighting scheme on the basis of the most prevalent type of dependency. Given that many studies in our meta-analysis provide multiple measures from the same sample, we chose correlated effect weights and, as a result, needed to determine a value for the correlation of effect sizes, ρ (rho). In practice, this value plays a relatively minor role, and scholars suggest basing the value on prior empirical work (Tipton, 2015). Accordingly, we followed other meta-analysts in assuming a correlation of $r=0.80$ (Tanner-Smith et al., 2016; Tanner-Smith & Tipton, 2014); again, we conducted robustness analyses, this time varying the correlation from $r=0$ to $r=1$ in steps of 0.2. The value of r did not markedly influence results; summary effects hardly varied ($-0.00009<\Delta g<0.00003$; $\Delta I^2=-0.19\%$). For brevity, we report only results using $r=0.80$.

Main analysis

To estimate effect sizes for the four outcomes, we employed mixed-effects RVE models, analyzing all effect sizes ($m=362$) with type of outcome as a moderator.

Moderation analyses

For subsequent moderation analyses, we created four separate data subsets (one per outcome) and employed mixed-effects RVE models for (a) purchase decisions, (b) price image, (c) quality image, and (d) underestimated recall. As the number of studies was often not large enough to include all moderators in a single model, we initially investigated each moderator separately. We then investigated moderators simultaneously to account for potential conceptual overlap (see Friese et al., 2017). To that end, we fitted models with all possible combinations of up to five moderators. We then selected the 100 models explaining the most heterogeneity in effect sizes (I^2) to determine the relative importance of moderators: If a moderator was included in the best model, it scored 100 points; if it was included in the second-best model, it scored 99 points and so on. Thus, the sum of these scores indicates the relative importance of a moderator. For quality image and underestimated recall, the number of effect sizes was too small to estimate a sufficient number of moderator combinations. For brevity, we report only the main results of this analysis in the manuscript (for more details, please refer to the accompanying OSF project; see osf.io/bqdpm). Exploratorily, we also conducted moderator analyses on the absolute values of all effect sizes. This analysis (a) enabled us to investigate when overall price-ending effects were stronger (vs. weaker)—irrespective of the specific direction of effects—and (b) had higher power to detect moderation effects. For instance, absolute effect sizes might be larger for earlier publication years (Ioannidis, 2005) or might vary by publication status (see Fanelli, 2012). We detail these analyses in the supplemental material.

Effect heterogeneity

We estimated τ^2 and I^2 (Borenstein et al., 2009) for each of the four outcome subsets by fitting intercept-only random-effects RVE models. τ^2 estimates the variance of true effects in the same metric as the original effect size (i.e., Hedges' g). It indicates the absolute amount of variation. I^2 is considered a more interpretable measure of heterogeneity as it reflects the estimated percentage of true variance in the total observed effect variance. To interpret τ^2 and I^2, both can be related to a study investigating heterogeneity estimates reported in *Psychological Bulletin* from 1990 to 2013 (Van Erp et al., 2017). Estimates at the 25th, 50th, and 75th percentiles (i.e., first, second, and third quartiles) serve as references for small, medium, and large heterogeneity estimates, respectively. For τ^2, the quartiles were 0.01, 0.04, and 0.11. For I^2, they were 25.27%, 64.63%, and 88.14% (Van Erp et al., 2017).

RESULTS

We closely followed the preregistered analysis plan (osf.io/nd2am) and transparently highlight exploratory analyses and deviations from the preregistration in the supplemental materials.

Main analysis

For the main analysis, we first estimated an RVE model based on all 362 effect sizes, with the type of outcome as a moderator to estimate the four effects jointly in one model (Figure 3): purchase decisions ($k=48$, $m=160$), price image ($k=26$, $m=110$), quality image ($k=14$, $m=59$), and underestimated recall ($k=8$, $m=33$). Just-below (vs. round) prices increased purchase decisions ($g=0.13$, $CI_{95\%}[0.01, 0.25]$, $p=0.031$), improved price image ($g=0.28$, $CI_{95\%}[0.09, 0.48]$, $p=0.007$), and resulted in underestimated recall ($g=0.67$, $CI_{95\%}[0.04, 1.30]$, $p=0.041$). The effect on quality image was not statistically significant and close to zero ($g=0.004$, $CI_{95\%}[-0.17, 0.18]$, $p=0.96$). For the four subsets, results showed high absolute heterogeneity (indicated by τ^2) and a high percentage of true variation in effect sizes (indicated by I^2) for purchase decisions ($\tau^2=0.12$, $I^2=92.84\%$) and underestimated recall ($\tau^2=0.19$, $I^2=91.55\%$), medium heterogeneity for price image ($\tau^2=0.08$, $I^2=87.13\%$), and small heterogeneity for quality image ($\tau^2=0.02$, $I^2=44.59\%$). In all, these differences among effect sizes called for moderator analyses that might explain some of this heterogeneity.

Moderator analyses

For the moderator analyses, we created four separate data subsets to examine moderator effects on each outcome separately; accordingly, we report moderator analyses separately for the four outcomes. Tables 2–5 contain effect-size estimates for all moderator levels and the corresponding statistical tests. In the text, we focus on statistically significant moderation effects and interpretable results with sufficient data (estimates with $df<4$ need to be treated with caution, we hence do not report p values in these cases).

Purchase decisions

No significant moderation effects emerged for participant, price, or product characteristics (Table 2). With regard to study characteristics, we found significant moderation by study design, $t(22.00)=-2.74$, $p=0.012$: Purchase-decision effects were positive and significant for within-subject designs ($g=0.34$, $CI_{95\%}[0.11, 0.57]$, $p=0.008$), but close to zero and nonsignificant for between-subject designs ($g=0.02$, $CI_{95\%}[-0.09, 0.13]$, $p=0.75$).

Multiple moderation analyses (for details, see osf.io/bqdpm) corroborated this single-moderator finding: Study design emerged as the most important moderator with larger effects for within-study designs (maximum importance score of 5050). Change of the first digit and price level were also identified as influential moderators in that a changing first digit (in 100% of the 100 most influential models; e.g., \$3.99 vs. \$4.00) and larger absolute price levels (in 64%) showed larger effects. A model with these three moderators reduced the true effect variance to $I^2=85.74\%$, compared to 92.84% in a model without moderators.

Price image

Study and product characteristics did not show moderation effects for price image (Table 3). With regard to participant characteristics, price-image effects differed depending on the study population: Effects were markedly larger and significant in student samples ($g=0.36$, $CI_{95\%}[0.16, 0.56]$, $p=0.002$) compared to public samples ($g=-0.02$, $CI_{95\%}[-0.12, 0.09]$, $p=0.71$), $t(9.75)=-3.58$, $p=0.005$. Furthermore, although authors have suggested that consumers in low-context cultures may be more susceptible to price-image effects than consumers in high-context cultures (e.g., Jeong & Crompton, 2018), our meta-analytic results find the opposite: High-context cultures showed price-image effects almost five times as large ($g=0.63$, $CI_{95\%}[0.51, 0.74]$, $p<0.001$) as in low-context cultures ($g=0.13$, $CI_{95\%}[-0.03, 0.29]$, $p=0.11$), $t(8.73)=5.54$, $p<0.001$. The hypothesis that an overabundance of just-below prices causes the price-image effect to wear off was also supported via significant moderation, $b_1=-0.02$, $CI_{95\%}[-0.04, -0.005]$, $t(15.90)=-2.75$, $p=0.014$. Effect size decreased by $\Delta g=-0.02$ when the

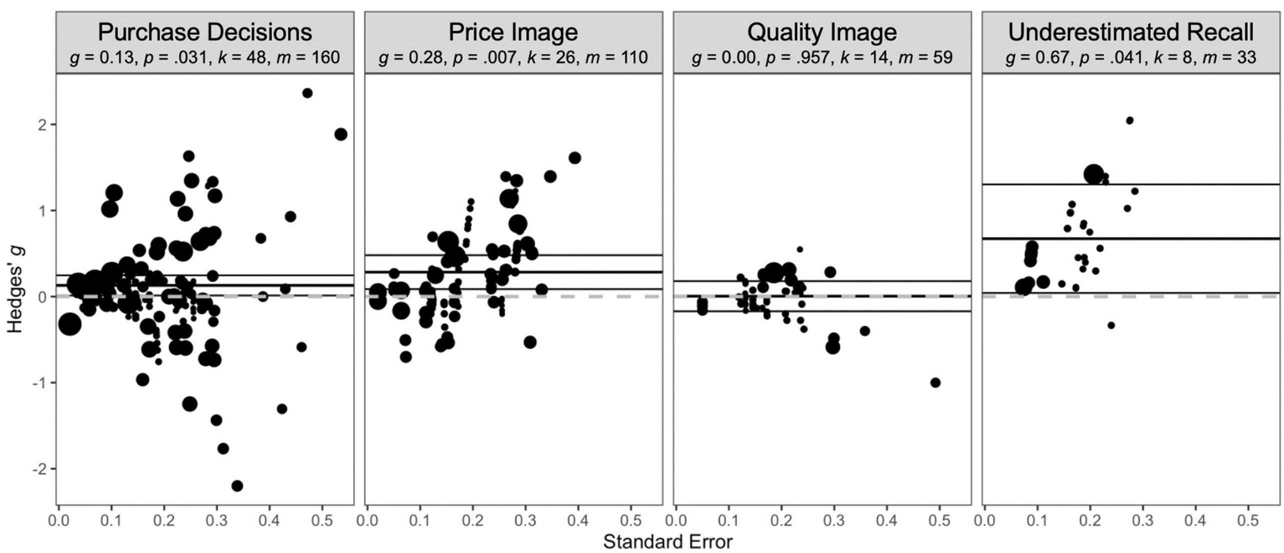

FIGURE 3 Results of the main analysis: effects of just-below versus round prices on the four outcomes. The graphs summarize the results of the main RVE analysis, which included moderation by type of outcome, $HTZ[23.30]=2.48$, $p=0.11$. g=Hedges' g (summary effect for each of the four outcomes); k=number of studies in a subgroup; m=number of effect sizes; p=p value testing the Hedges' g against zero. Black dots represent individual effect sizes; the diameter of each dot represents the weight of the effect in the meta-analytic RVE mixed-effects model. The thick black horizontal lines represent the meta-analytic summary effects. The thin black horizontal lines represent the borders of the $CI_{95\%}$s around the four summary effects. The dashed gray horizontal lines represent the null effect, at $g=0$.

prevalence of just-below prices increased by 1%—thus, $\Delta g=-0.20$ when the prevalence increased by 10%, and $\Delta g=-0.40$ when the increase was 20%.

For price characteristics, analyses showed moderation as a function of digit manipulation. Price-image effects were markedly larger for price endings manipulated before the decimal ($g=0.62$, $CI_{95\%}[0.47, 0.77]$, $p<0.001$) compared with manipulations after the decimal ($g=0.17$, $CI_{95\%}[0.004, 0.33]$, $p=0.045$), $t(5.83)=4.94$, $p=0.003$. Additionally, change of the first digit emerged as a moderator. As predicted, effects were larger when the first digit changed ($g=0.39$, $CI_{95\%}[0.17, 0.61]$, $p=0.002$) than when it did not ($g=0.06$, $CI_{95\%}[-0.20, 0.33]$, $df=3.84$). This moderating effect was close to conventional levels of significance, $t(5.38)=2.43$, $p=0.06$. Furthermore, in line with the assumption that just-below prices become more distinctive (and attractive; Turner et al., 1987) when other products feature round prices, effects were larger when participants saw a round comparison price ($g=0.47$, $CI_{95\%}[0.27, 0.67]$, $p<0.001$) than when they did not ($g=0.00$, $CI_{95\%}[-0.18, 0.17]$, $p=0.96$), $t(21.83)=3.95$, $p<0.001$.

Multiple moderation analysis (for details, see osf.io/bqdpm) largely corroborated these single-moderator findings: The presence of a round price emerged as the most important moderator in that round comparison prices (vs. none) led to larger effects in all of the most influential models. With this moderator, $I^2=83.26\%$ was "true" variance compared to 87.13% in a model without it.

Quality image

Participant, study, and price characteristics did not moderate quality-image effects (Table 4). With regard to product characteristics, we found significant moderation by brand, $t(6.42)=2.53$, $p=0.042$. In line with the reasoning that a detrimental quality image is less pronounced when a price–product combination is associated with a brand (Rao & Monroe, 1989), no difference in quality image emerged when a brand cue was present ($g=0.08$, $CI_{95\%}[-0.06, 0.22]$, $p=0.22$): The perceived quality of just-below-priced products was descriptively even higher than that of round-priced products. In the no-brand condition, the quality image effect ($g=-0.10$, $CI_{95\%}[-0.22, 0.03]$, $df=2.53$) could not be interpreted inferentially because $df<4$. Unfortunately, the number of effect sizes was too small to run multiple moderator analyses for quality image.

Underestimated recall

Participant, price, and product characteristics did not moderate underestimated recall (Table 5). With regard to study characteristics, the response interval moderated effects: As expected (Schindler & Wiman, 1989), more time between presentation of the price and measurement of the outcome decreased the effect-size magnitude—participants more likely underestimated prices when recalling them immediately ($g=0.86$, $CI_{95\%}[0.37, 1.35]$, $df=3.97$) compared to recalling them after a delay ($g=0.24$, $CI_{95\%}[-0.19, 0.67]$, $df=2.00$), $t(4.80)=3.10$, $p=0.028$. The number of effect sizes was too small to run multiple moderator analyses for underestimated recall.

Interim summary

The fact that different moderators emerged in the different subsets underlines the point that purchase decisions, price image, quality image, and underestimated recall are distinct pricing outcomes. However, even after influential moderators were included in the models, effect heterogeneity remained high for purchase decisions, price image, and underestimated recall. This suggests either true effect heterogeneity or further moderation effects that we were not able to detect based on the available information.

Interplay of just-below-pricing effects

Our introductory review suggests that price endings affect purchase decisions via three mechanisms that pertain to consumers' information processing—price image, quality image, and underestimated recall. In theory, the most appropriate method to test this mediation hypothesis would be to run a meta-analytical mediation analysis. To run such an analysis, we would need bivariate correlations between outcomes (e.g., between price image and purchase decisions). Unfortunately, original articles did not report these correlations, precluding a mediation analysis. However, to provide novel meta-analytical evidence on the theorized interplay of price-ending effects, we conducted an exploratory analysis examining the association between purchase-decision effect sizes and effect sizes for each of the three process-oriented variables (Figure 4). Logically, this analysis was constrained to studies that investigated both purchase decisions and at least one of the three process-oriented variables. Consequently, this analysis is based on a smaller, not fully representative subset of effect sizes, does not account for the standard error or interdependence between effect size estimates, and thus should be treated as an exploratory analysis.

Figure 4 shows that price-image ($r=0.43$, $CI_{95\%}[0.17, 0.64]$, $p=0.002$) and quality-image effects ($r=0.36$, $CI_{95\%}[0.06, 0.60]$, $p=0.021$) were positively associated with purchase decision effects. As predicted by image theorizing, larger effects of just-below (vs. round) prices on price image coincided with larger purchase decision effects, and smaller effects for quality image coincided with less pronounced purchase decision effects. Contrary to level-effects theorizing, underestimation effects were negatively associated with purchase decision effects ($r=-0.57$, $CI_{95\%}[-0.79, -0.22]$, $p=0.004$)—the more likely participants underestimated just-below prices in a recall task, the smaller the purchase decision effect. Please note, however, that the six most extreme negative purchase decision effects (all $gs<-0.38$) that contributed to this correlation pattern came from the same study that examined price-ending effects right after the Euro had been introduced as the new currency in Italy (Guido & Peluso, 2004). The authors argue that during these historic circumstances, consumers might have preferred round prices because they were easier to convert into the old Lire currency than just-below prices. Without these six effects, the correlation is still negative but not statistically significant anymore ($r=-0.23$, $CI_{95\%}[-0.63, 0.27]$, $p=0.37$). Given the small number of effects contributing to this correlation and the unique impact of this one study (Guido & Peluso, 2004), we urge caution in readers to refrain from overinterpreting this exploratory analysis for general pricing effects.

ROBUSTNESS TESTS

We applied four methods to examine the (non-)robustness of our findings—(1) outlier analyses, (2) multilevel meta-analysis, (3) *p*-curve, and (4) analyses of publication bias.

Analyses of outliers

To ensure that the meta-analytic results are not biased by one (or a few) extreme effects, sample sizes, or effect weights, we identified outliers based on *z*-transformed values of these three preregistered criteria. We ran analyses with and without outliers ($|z|>3$) to determine Δg and ΔI^2. As we anticipated that the overall database would contain (partially) opposing effects, we conducted outlier analyses separately for the four subsets. Removing outlying effect sizes (5 outliers; $-0.001<\Delta g<0.01$; $\Delta I^2=-0.21\%$), sample sizes (2 outliers; $-0.004<\Delta g<0.02$; $\Delta I^2=-4.31\%$), or effect weights (5 outliers; $-0.003<\Delta g<0.10$; $\Delta I^2=-4.42\%$) did not appreciably influence the summary effects. In addition, the reviewers correctly pointed out that two studies (Georgoff, 1972; Mueller et al., 2020) contribute approximately 25% of effect sizes to the present data ($k=91$ of 362). Sensitivity analyses showed that removing effect sizes from these two studies did not appreciably alter any of the reported summary effects ($0.006<\Delta g<0.009$; $\Delta I^2=+0.42\%$). We therefore did not delete or replace any of these effect sizes.

Multilevel meta-analytical approach

During the review process, an anonymous reviewer and the editor correctly pointed out that—as an alternative to the RVE approach—the present data would also allow for a multilevel (MLM) approach (e.g., Van

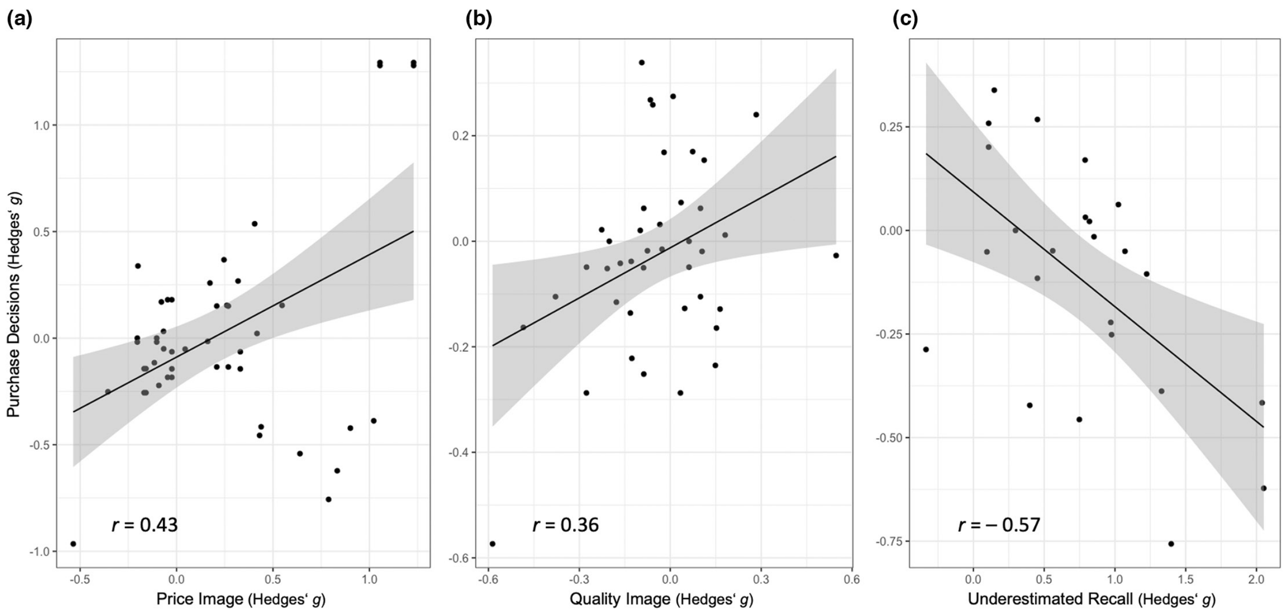

FIGURE 4 Association between effect sizes of the main outcome and of the process-oriented outcomes. The scatterplots show the association between the size of the effect of price endings on purchase decisions (*y*-axis) and the size of the effect of price endings on (a) price image ($k=7$, $m=48$), (b) quality image ($k=7$, $m=41$), and (c) underestimated recall ($k=5$, $m=24$; *x*-axis). Black dots represent individual effect sizes. In each graph, the thick black line represents the linear association between the variables; the gray area represents the 95% confidence interval around this linear association.

den Noortgate et al., 2015). MLM can take into account that individual effect sizes (level 1) are nested within studies (level 2), which are nested within articles (level 3). We had decided for the RVE approach as RVE is known to effectively control for a wider range of dependencies "arising from multiple sources simultaneously, including multiple measures, multiple treatment groups, and multiple time points" (p. 4) and allows to guard inferences against potential misspecification of a meta-analytical model (Tanner-Smith & Tipton, 2014). Nonetheless, we conducted a robustness check using the MLM approach for our main analysis (based on all $k=69$ studies) to contrast both approaches. For moderator analyses, we refrained from using MLM, given that $k<50$ for each data subset (see Moeyaert et al., 2017).

Using the *metafor* package (Viechtbauer, 2010), we estimated a three-level model based on all 362 effect sizes ($k=69$), with the type of outcome as a moderator to again estimate the four pricing effects. This robustness analysis produced similar findings as the RVE approach: Just-below (vs. round) prices slightly increased purchase decisions ($g_{MLM}=0.09$, $CI_{95\%}[-0.02, 0.19]$, $p=0.10$), although this effect was slightly smaller than in RVE ($g_{RVE}=0.13$) and no longer statistically significant. Corroborating the RVE results, just-below (vs. round) prices improved price image ($g_{MLM}=0.16$, $CI_{95\%}[0.05, 0.28]$, $p=0.005$ vs. $g_{RVE}=0.28$) and resulted in underestimated recall ($g_{MLM}=0.68$, $CI_{95\%}[0.52, 0.84]$, $p<0.001$ vs. $g_{RVE}=0.67$). The quality image effect was not statistically significant and essentially zero ($g_{MLM}=-0.0003$, $CI_{95\%}[-0.13, 0.13]$, $p=0.98$ vs. $g_{RVE}=0.004$).

p-Curve analysis

Simonsohn et al. (2014, 2015) have argued that "under conditions of no effect ($d=0$), there will be as many p values between 0.04 and 0.05 as between 0.00 and 0.01, and p-curve's expected shape is uniform" (2015, p. 667). That is, the p-"curve" will actually be a uniform, flat line (see red line in Figure 5). If a true effect exists, however, the likelihood of p-values smaller than 0.01 increases markedly. Consequently, the resulting p-curve becomes right-skewed (see green line in Figure 5). To examine whether there is "evidential value" for a true effect in the price-ending literature, we conducted p-curve analyses for (a) all 362 effect sizes of the entire dataset, and (b) the four outcome subsets (i.e., purchase decisions, price image, quality image, underestimated recall; due to page constraints, please refer to osf.io/bqdpm for [b]). The overall p-curve revealed two things: First, a large number of the included effect sizes was close to zero and nonsignificant ($k=219$ of 362; 60.5%). As the p-curve analysis is, by definition, based only on statistically significant p-values, these effect sizes were excluded. Second, the remaining significant p-values ($k=143$ of 362; 39.5%) showed "evidential value" based on the full p-curve, $Z=-26.94$, $p<0.001$, and the half p-curve, $Z=-26.16$, $p<0.001$, as well as no sign of p-hacking (i.e., p-curve was right-skewed, as expected; Figure 5).

Small-study effects and publication bias

Although a large number of effect sizes in this meta-analysis was statistically nonsignificant (see p-curve), publication bias could nonetheless be present in that significant studies (with smaller samples) were more likely to be published and thus overestimate the meta-analytic effect size estimate(s). Several methods are available to detect and correct for publication bias. As none of these methods clearly outperforms others, we followed recommendations from Carter et al. (2019) and used a variety of methods: In addition to conducting standard publication bias analyses (funnel plot, trim-and-fill) on aggregated effect sizes, we also extended two established methods (Egger's test; PEESE) to the present RVE data with dependent effect sizes.

Publication bias analyses with aggregated effect sizes

We first aggregated the dependent effect sizes from all published studies ($k=59$) for each of the four outcome subsets (using the R package *MAd*; Del Re & Hoyt, 2018). We also conducted these publication bias analyses with *all* effect sizes (including unpublished ones)—these results are highly consistent with the following findings (see osf.io/bqdpm for details). First, we calculated an aggregated effect size and its variance, while taking the correlation among effect sizes into account (default: $r=0.50$; see procedure by Borenstein et al., 2009). We created funnel plots (Sterne et al., 2005) to detect publication bias and applied the trim-and-fill method (Duval, 2005) to correct for publication bias. The random-effects models (Borenstein et al., 2009; "BS") revealed these aggregated effect estimates: purchase decisions: $g_{BS}=0.07$, $CI_{95\%}[-0.03, 0.17]$, $p=0.17$, $k=39$; price image: $g_{BS}=0.27$, $CI_{95\%}[0.15, 0.39]$, $p<0.001$, $k=22$; quality image: $g_{BS}=-0.02$, $CI_{95\%}[-0.16, 0.13]$, $p=0.81$, $k=9$; and underestimated recall: $g_{BS}=0.59$, $CI_{95\%}[0.28, 0.91]$, $p<0.001$, $k=6$.

A "funnel plot" shows a triangle that is centered on the estimate of the meta-analytic effect. In the absence of publication bias, the effect-size distribution should resemble a funnel: Effects with smaller standard errors should cluster symmetrically around the mean effect-size estimate; effects with larger standard errors, closer to the x-axis, should fan out more in both directions (i.e., they should deviate more from true effects). Effects outside this triangle do not inevitably indicate publication bias but could be due to the inadequate assumption of fixed effects (see high effect heterogeneity indicated by τ^2 and I^2). Visual inspection of our funnel plots (Figure 6)

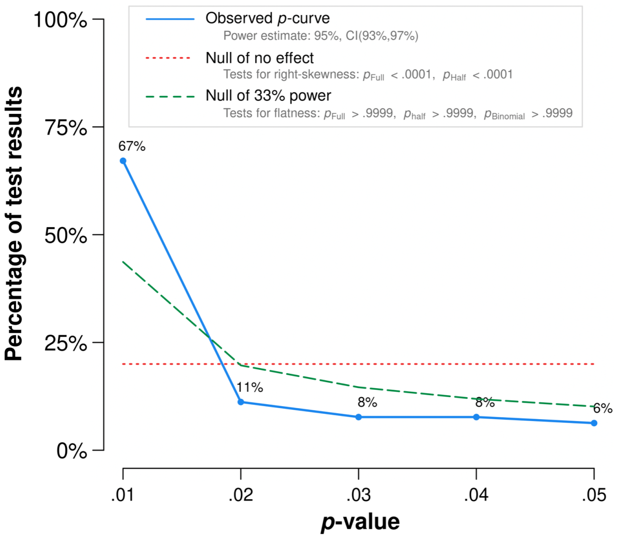

FIGURE 5 Results from p-curve analyses for all $k=362$ effect sizes. The observed p-curve (blue) for all $k=362$ effect sizes showed that (a) $k=219$ p-values (60.5%) were entered but excluded from p-curve analysis because they were nonsignificant, $p>0.05$, and (b) the remaining significant p-values ($k=143$; 39.5%) showed "evidential value" based on the full p-curve, $Z=-26.94$, $p<0.001$, and the half p-curve, $Z=-26.16$, $p<0.001$ compared to the null of no effect (red line), as well as no sign of p-hacking—that is, the p-curve was right-skewed, as expected, and not significantly flatter than the null of 33% power (green line).

revealed asymmetry for purchase decisions and price image—for both, negative effect sizes of low-to-medium precision were missing. For underestimated recall, the funnel plot could not be interpreted conclusively, given the small number of studies.

We used the trim-and-fill method to correct for potential publication bias. In this method, the values of extreme studies that lead to asymmetry in the funnel plot are removed and mirror images are imputed (see unfilled circles in Figure 6). For purchase decisions, six effect sizes were imputed to achieve a symmetric funnel plot. This produced a bias-corrected summary effect that was no longer statistically significant and close to zero: $g=-0.01$, $CI_{95\%}[-0.10, 0.09]$, $p=0.86$. For price image, seven effect sizes were imputed, and this bias correction also produced a smaller and no longer significant summary effect: $g=0.10$, $CI_{95\%}[-0.01, 0.22]$, $p=0.08$. For quality image, one effect size was imputed. The corrected effect remained nonsignificant and close to zero: $g=0.01$, $CI_{95\%}[-0.15, 0.17]$, $p=0.91$. For underestimated recall, we do not report trim-and-fill results as it is underpowered when k is <10 (Kromrey & Rendina-Gobioff, 2006; Sterne et al., 2005).

Publication bias analyses with dependent effect sizes

Egger's regression estimates the association between effect size and the corresponding standard error (SE, an indicator of study precision) in a random-effects meta-regression (Sterne & Egger, 2005). A statistically significant regression coefficient indicates a considerable degree of small-study effects. PEESE (Stanley & Doucouliagos, 2014) estimates the association between effect size and the corresponding squared standard errors (SE^2, an indicator of precision). A statistically significant regression coefficient indicates a considerable degree of small-study effects. We extended the logic of both methods to the RVE approach by investigating the relationship between single dependent effect sizes and corresponding (squared) SEs in mixed-effects RVE meta-regressions. Both methods have been applied to dependent effect-size structures (Coles et al., 2019; Friese et al., 2017), and the Egger's regression test has been validated for these structures (Rodgers & Pustejovsky, 2020).

We used non-aggregated, dependent effect sizes from published studies ($k=59$) for Egger's test and PEESE. For purchase decisions ($b_{SE}=0.54$, $t[19.80]=0.77$, $p=0.45$), quality image ($b_{SE}=-1.23$, $t[2.73]=-0.90$, $p=0.44$), and underestimated recall ($b_{SE}=5.87$, $t[3.42]=2.46$, $p=0.08$), the RVE-based Egger's regression tests did not show significant relationships between standard error and effect sizes, although the latter two analyses should be treated with caution as df<4. For price image ($b_{SE}=2.80$, $t[10.86]=4.55$, $p<0.001$), the Egger's regression test was significant: Larger SEs coincided with larger effects, suggesting small-study effects (and potentially publication bias).

The PEESE method showed no significant relationship between SE^2 and effect size for purchase decisions, $b_{SE^2}=1.93$, $t(8.87)=1.05$, $p=0.32$. For price image, PEESE yielded a significant relationship between SE^2 and effect size, $b_{SE^2}=7.70$, $t(11.70)=4.41$, $p<0.001$. Hence, as did Egger's regression test, PEESE also suggested small-study effects (and potentially publication bias) for price image. PEESE did not suggest small-study effects for quality image ($b_{SE^2}=-3.89$, $t[2.14]=-2.05$, $p=0.17$) or underestimated recall ($b_{SE^2}=17.77$, $t[3.12]=2.19$, $p=0.11$), although based on a small number of studies (Stanley, 2017) and df<4.

Interim summary

We would like to integrate the four robustness tests. First, the present meta-analytic analyses were highly robust to removing outliers and extreme values. Second, multi-level meta-analysis (MLM) produced highly comparable findings as RVE—a small price image effect, a moderate underestimation effect, and a quality image null effect. Only for purchase decisions, the overall effect size was smaller than in RVE and no longer statistically significant. Third, p-curve analyses showed that the majority of included findings were not significant to begin with; yet, the p-curve of only the significant effects showed evidential value and no indication of p-hacking. Finally, several publication bias analyses suggested a

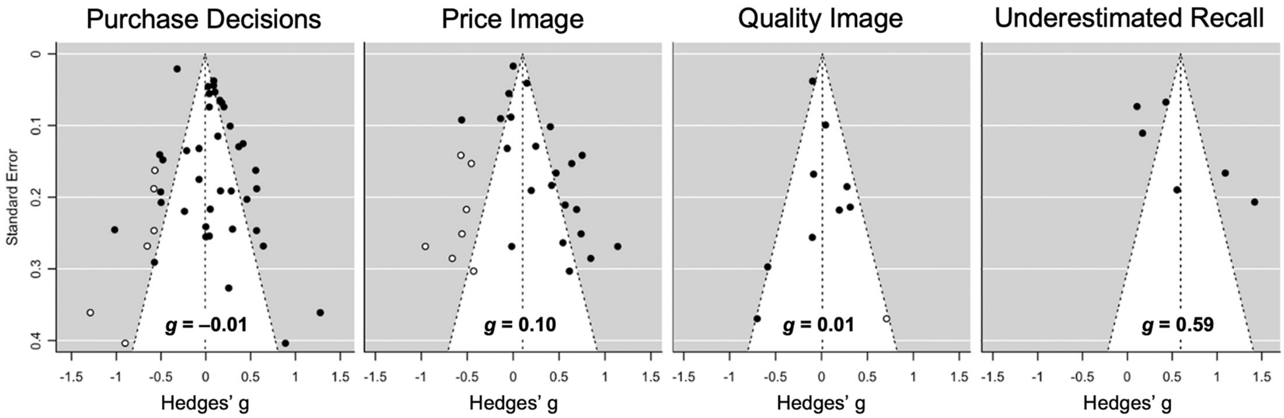

FIGURE 6 Funnel plots for the four just-below-pricing effects after trim-and-fill bias correction. The trim-and-fill bias correction is based on aggregated effect sizes (see Borenstein; "BS"). Filled black dots represent effect sizes from published studies (purchase decisions: $k=39$; price image: $k=22$; quality image: $k=9$; underestimated recall: $k=6$). Unfilled dots represent effect sizes that were imputed to achieve funnel plot symmetry. After trim-and-fill correction, the effect sizes were (1) purchase decisions: corrected $g=-0.01$, $CI_{95\%}[-0.10, 0.09]$, $p=0.86$ (vs. $g_{BS}=0.07$); (2) price image: corrected $g=0.10$, $CI_{95\%}[-0.01, 0.22]$, $p=0.08$ (vs. $g_{BS}=0.27$); (3) quality image: corrected $g=0.01$, $CI_{95\%}[-0.15, 0.17]$, $p=0.91$ (vs. $g_{BS}=-0.02$). No effect sizes were imputed for (4) underestimated recall (uncorrected $g_{BS}=0.59$).

moderate degree of small-study effects (and potentially publication bias) for purchase decisions and price image. The bias-corrected estimates for these outcomes were close to zero and no longer statistically significant. RVE-based analyses (Egger's test and PEESE) corroborated the presence of small-study effects for price image. For underestimated recall, the relatively small database did not allow for a conclusive application of funnel plot and trim-and-fill correction. For quality image, the bias-corrected estimate continued to indicate a nonsignificant effect close to zero. For full disclosure, we note that the true severity of small-study effects and publication bias remains difficult to estimate for three reasons. First, the trim-and-fill correction may underestimate the true effect when there is large effect heterogeneity (Peters et al., 2007)—which was the case for several outcomes. Second, large effect heterogeneity can also reduce the power of regression-based methods (i.e., Egger's test and PEESE; Stanley, 2017). Third, the RVE-based PEESE equivalent has yet to be validated thoroughly. Nevertheless, the analyses suggest small-study effects (and potentially publication bias), particularly for purchase decisions and price image.

DISCUSSION

Given the pervasiveness of just-below prices in retail and e-commerce, it appears that management and marketers take their efficacy to produce higher demand for granted. Our meta-analysis of 362 effect sizes from 69 studies with an overall $N=40{,}541$ found empirical support for the predicted higher demand (purchase decisions: $g=0.13$), advantageous price image effect (i.e., price image: $g=0.28$), and level effect (i.e., underestimated recall: $g=0.67$) of just-below compared with round prices. These effects—particularly for purchase decisions and price image—were rather small according to common conventions (Cohen, 1988) and average effect sizes in marketing research (Eisend, 2015), and showed a very high degree of effect heterogeneity. We found no empirical evidence for the assumed disadvantageous effect of just-below prices on quality image ($g=0.004$). Several methods for detecting and correcting publication bias suggested the presence of small-study effects that may (partly) reflect publication bias and thus an overestimation of true effect sizes. Hence, the reported effect estimates should be treated as likely upper boundaries of the true population effects. While prior research has shown rather large just-below-pricing effects on purchase decisions (e.g., Choi et al., 2014), as well as price image and quality image (e.g., Schindler & Kibarian, 2001), the present meta-analysis suggests that these effects are considerably smaller (or even nonexistent).

Effect heterogeneity

Compared to 705 other meta-analyses (Van Erp et al., 2017), our findings reveal "high" absolute effect heterogeneity for purchase decisions ($\tau^2=0.12$) and underestimation effects ($\tau^2=0.19$), "medium" heterogeneity for price image effects ($\tau^2=0.08$) and "small" heterogeneity for quality image effects ($\tau^2=0.02$). Thus, especially for purchase decisions and underestimation (and partly for price image), effect-size estimates varied markedly. Viewed differently, and akin to the logic of standard deviation (SD) that illustrates variation around the mean ($M\pm$SD), comparably small effect estimates g (close to zero) coincided with comparably large effect heterogeneity τ. Effect heterogeneity was often equal to (or even larger) than the effect itself: purchase decisions ($g=0.13\pm\tau=0.35$), price image ($g=0.28\pm\tau=0.28$), and quality image ($g=0.004\pm\tau=0.14$). This was less the case for underestimated recall ($g=0.67\pm\tau=0.44$). A similar pattern emerged for the proportion of variation due to true effect size differences. True variation was high for purchase decisions ($I^2=92.84\%$) and underestimation ($I^2=91.55\%$), medium for price image ($I^2=87.13\%$), and small for quality image effects ($I^2=44.59\%$). Hence, effect heterogeneity in purchase decisions and underestimation (and partly in price image) appears to result from variation in true effects rather than sampling error.

Real-world implications and underlying theorizing

Our findings have noteworthy implications for real-world applications in marketing, as well as for theorizing in (consumer) psychology. We discuss these for each outcome.

Purchase decisions (main outcome)

The purchase decision effect ($g=0.13$) is approximately a quarter the size of the average effects established in two meta-analyses: (a) a meta-analysis of 176 meta-analyses of marketing research ($d=0.49$; Eisend, 2015), and (b) a meta-analysis of 302 meta-analyses of behavioral, educational, and psychological treatments ($d=0.50$; Lipsey & Wilson, 1993). The effect can also be put into perspective by using the binomial effect-size display to illustrate an effect's practical consequences (Funder & Ozer, 2019). Imagine a sample of 200 consumers who are divided into two equal-sized groups ($n=100$) and presented with a product—say, a pair of headphones: For 100 consumers, the headphones have a just-below price ($79.95); the other 100 consumers see a round price ($80.00). Provided the effect of $g=0.13$ is a true effect, it is equivalent to an absolute difference of six sales (47 for consumers who see the round price vs. 53 for consumers who see the just-below price). This might not be very consequential for a single pair of headphones and 200 customers. Scaling this effect to the actual number of customers and products in a store could illustrate why management and marketers might prefer just-below over round prices (e.g., 2000 customers and 100 different products yield 6500 additional purchases). Note that the present estimate should be treated as upper boundary of the true effect, given the indicated publication bias and multi-level estimate, as well as the high effect heterogeneity. Of course, the smaller the *true* effect, the less pronounced the economic advantage of just-below prices. Without a true effect, there are no such advantages.

Moderator analyses suggested that study design markedly altered the purchase decision effect: The predicted positive effect emerged only for study designs in which participants evaluated both just-below and round prices (within-subject designs; $g=0.34$), but not when participants evaluated only just-below or only round prices (between-subject designs; $g=0.02$). Skeptics might argue that this moderation finding reveals that the price-ending effect is nothing but a demand effect (Zizzo, 2010): that is, researchers show participants a just-below *and* a round price and participants prefer to purchase the just-below-priced product. If true, this would question applied implications of price-ending effects for consumer behavior, because the effect would be driven by characteristics of study designs and be void of a real-world equivalent. Proponents, by contrast, may counter that consumers in the real world face precisely this mix of round *and* just-below prices and that practitioners glean from our findings that just-below prices are particularly effective when contrasted with round ones.

Price image (process-oriented outcome)

As price-image theorizing predicts, just-below prices were more likely than round prices to be perceived as particularly low or discounted. Proponents of this account suggest that consumers perceive a just-below price as a round price along with a small monetary gain (e.g., 5¢) that is seen as disproportionately large and that consumers have learned over years to associate just-below prices with a low-price appeal. At least three moderation findings contribute to and expand this theorizing.

First, effects on price image were consistently larger when pre-decimal digits were manipulated (e.g., $79 vs. $80; $g=0.62$) than when post-decimal digits were manipulated ($79.95 vs. $80.00; $g=0.17$). If, as perceived gain effect reasoning assumes, consumers perceive a just-below price (e.g., $79.95) as a round price (e.g., $80.00) along with a small gain (e.g., 5¢; Schindler & Kirby, 1997), pre-decimal manipulations should indeed lead to the perception of a larger gain (e.g., gain of $1) compared to post-decimal manipulations (e.g., gain of 5¢); the applicability of this finding for real-world marketing is self-evident. Similarly, price image effects were larger when the first digit changed ($3.99 vs. $4.00; $g=0.39$) compared to not changed ($3.49 vs. $3.50; $g=0.06$). Future research should examine whether the size of the discount (e.g., $10 vs. $9 with a 10% discount compared to $100 vs. $99 with a 1% discount) also moderates these pricing effects (for our data $df<4$, precluding these moderator analyses).

Second, moderation analyses suggested that the more prevalent just-below prices are in a country, the smaller the price-image effect. From a price-socialization perspective, this moderation appears somewhat puzzling, as a higher prevalence of just-below prices should allow consumers to internalize the association of just-below prices and low-price appeal more effectively. In contrast, an overabundance of just-below prices may undermine the price-image effect—for each 10% increase in the prevalence of just-below prices in a country, the effect decreased by $\Delta g=-0.20$. While a causal interpretation of this correlational moderation effect is inadequate, another result points in a similar direction: Just-below pricing had an effect on price image when consumers were also shown a round comparison price (g=0.47), but this effect was absent without a round comparison price (g=0.00). Jointly, these findings suggest that just-below prices might become more distinctive (and attractive) when other products feature round prices (Turner et al., 1987; see also subtraction principle, Biswas et al., 2002).

Third, researchers have proposed that people from high-context cultures (e.g., China) would more likely interpret the “true” meaning of \$3.99 as really being \$4.00 than would people from low-context cultures (e.g., United States; Jeong & Crompton, 2018). Hence, members of high-context cultures should be *less* susceptible to price-image effects. Our results suggest the opposite: Price-image effects were almost five times as large in high-context (g=0.63) as in low-context cultures (g=0.13; cf. Kittler et al., 2011). This moderation can be integrated with other moderator findings: Just-below prices are less prevalent in high-context (11.8%) than in low-context cultures (36.0%; Nguyen et al., 2007), and round prices are more prevalent in high-context (49.9%) compared to low-context cultures (30.1%; Nguyen et al., 2007). Thus, members of high-context cultures could be more prone to just-below-pricing effects because they are simply less familiar with this price format and perceive just-below prices as more distinctive (and attractive) than round prices.

Quality image (process-oriented outcome)

In contrast to the results for the price-image mechanism, the results for the quality-image mechanism provide no empirical support for the assumed detrimental effect of just-below prices. The database for quality image was much smaller, however (m=59 effects, k=14 studies). This notwithstanding, other research has shown that the association between price level and perceived quality has decreased over the years (Völckner & Hofmann, 2007). It may be that other factors, such as brand or store name, as suggested by cue-utilization theory (Olson, 1972), play a more influential role for consumers' perception of product quality than the price ending does.

Underestimated recall (process-oriented outcome)

Consumers underestimated just-below prices more often than round prices in recall tasks. This was the largest effect in the present meta-analysis (g=0.67) with the highest proportion of significant effects (i.e., 73% of effect entered into the p-curve), albeit based on the smallest dataset (i.e., m=33 effects, k=8 studies). The underestimation effect was more pronounced when participants recalled prices immediately (g=0.86) than when they recalled prices after a delay (g=0.24). We can only speculate about the reasons for this moderation. Assuming that individuals seek to minimize cognitive effort (Kahneman, 2011), delayed recall should lead to more incorrect recall. This could lead to more unsystematic errors in price recall, which could blur underestimation effects.

Future directions and recommendations

The present findings highlight several possible avenues for future research to further advance our theoretical understanding of just-below pricing effects but also the cognitive processing of numerical information more generally.

Disentangling the level effects

The three processes of level effect theorizing—rounding down, left-to-right comparison, memory effect (Figure 1)—have rarely been investigated directly. We know of only two exceptions that (1) offered indirect evidence for the drop-off mechanism of rightmost digits in that participants overestimated how many items priced at \$2.99 (vs. \$3.00) they could buy for \$73.00 (Bizer & Schindler, 2005), and (2) manipulated the left-to-right processing of prices by presenting digits sequentially (Coulter, 2001). Given this relatively scarce empirical foundation, we would recommend that future research manipulates and measures these psychological mechanisms more directly and with the full range of tools from the psychological methods toolbox. For instance, one could directly investigate the left-to-right price digit processing and the proposedly elevated focus on the leftmost digits by using the eye-tracking methodology: Analyzing the number of fixations, dwell times, time to first fixation, and revisits would allow to determine an overall index of attention allocated to each digit while consumers process a price (Mele & Federici, 2012).

The interplay of just-below pricing effects

To our knowledge, studies have yet to investigate the possible interplay of level and image effects, and whether these effects (jointly) mediate price-ending effects on purchase decisions. If such mediation in parallel does occur, it is not surprising that larger effects emerged for two of the process-oriented variables (i.e., price image and underestimated recall) than for the main outcome (i.e., purchase decisions). In our exploratory examination of the bivariate correlations between effect sizes for the potential mediators and for the main outcome purchase decisions (Figure 4), we found preliminary evidence for the potential interplay of these mechanisms. Future research should investigate these psychological mechanisms not only separately but also examine their (joint) mediation effects on purchase decisions (multiple mediation analysis; Hayes, 2013).

The sequential impact of pricing mechanisms

Future research should also explore whether price image and underestimation effects operate in sequence. Exemplarily, consumers may have learned to associate a just-below price, say \$7.99, with a better deal (price image), which *causes* them to recall a lower price than for \$8.00 (underestimation). In a reversed sequence, individuals who focus predominantly on the leftmost digit, the "7" in \$7.99, underestimate the price magnitude, which in turn could *cause* them to ascribe a better image to just-below prices. Research should examine whether (and how) these mechanisms occur in sequence (sequential mediation analysis; Hayes, 2013) to impact purchase decisions.

Limitations

For full transparency, we discuss several limitations of the present work. When questionable research practices (e.g., *p*-hacking) are applied, meta-analyses inevitably overestimate the true effect magnitude (Friese & Frankenbach, 2020)—especially in the presence of publication bias. We used several methods to detect and correct for publication bias. However, these methods have shortcomings (e.g., Peters et al., 2007; Stanley, 2017) and not all have been validated for dependent effect-size structures. Thus, our corrected effect-size estimates should not be interpreted as providing single estimates that are adjusted for publication bias ex post, but rather provide a gauge of "the range of estimates that result from assuming different forms of and severity of publication bias" (McShane et al., 2016, p. 732). Given that different techniques suggested the presence of publication bias and that corrected effects were smaller and close to null, future preregistered and high-powered research should examine the robustness of price-ending effects, possibly focusing less on statistical significance and more on effect sizes and real-world applicability (McShane et al., 2019).

Similar to the different techniques that detect and correct for publication bias, the *p*-curve procedure is not without limitations and criticism either (e.g., Carter et al., 2019; see also Bruns & Ioannidis, 2016; Ulrich & Miller, 2015). For instance, the *p*-curve method can overestimate the average true underlying effect, particularly when there is effect heterogeneity as in the present case (van Aert et al., 2016)—which is why we opted to use *p*-curve only to establish the "evidential value" for the significant effects. This approach, however, comes with the downside that all nonsignificant findings are disregarded—leading Carter and colleagues to recommend not using *p*-curve "if many studies yielded nonsignificant results" (p. 135). Indeed, our *p*-curve suggests strong evidential value in favor of a true effect (cf. Erdfelder & Heck, 2019), yet this conclusion is based on 143 (39.5%) significant effects, while the 219 (60.5%) nonsignificant effects are disregarded in this approach, causing us to urge readers to not overinterpret the putative support for the price-ending literature, particularly in light of few studies following open science principles.

Indeed, we should note that only one article in our sample featured open-science practices, such as preregistration, open data, and open materials, which are intended to strengthen scientific trust in the robustness of (true) effects (Nelson et al., 2018). In terms of high-powered analyses, novel digital platforms with big data and natural price-ending variations in the field (e.g., *Lyft* or *Uber*; see Pope, 2020) should be conducive to estimating price-ending effects in the real world (at least for purchase decision effects).

Finally, despite the large number of effect sizes ($m=362$), we were not able to interpret results of all moderator and publication bias analyses as the number of studies was at times not large enough. This limitation applies particularly to quality image and underestimated recall. Hence, we caution readers to overinterpret null effects in the moderator analyses, as these results might be due to insufficient power to detect meaningful differences among subgroups (Hedges & Pigott, 2004). Future research should realize high-powered, preregistered studies that ideally follow open-science principles and are aimed at exploring the ability to replicate price-ending effects in the field. In addition, manipulating moderating factors in experimental designs could lead to more powerful tests of moderation (and causality).

CONCLUSION

Just-below prices have dominated retail and e-commerce for over a century. While a plethora of original studies have examined price-ending effects, the literature in marketing and (consumer) psychology has been lacking a systematic meta-analytic synthesis. We sought to fill this lacuna by meta-analytically contrasting just-below with round prices. Overall, we found small effects, with very pronounced effect heterogeneity, on purchase decisions and price image. The presumed disadvantage of just-below (vs. round) prices for perceived product quality did not substantiate meta-analytically. A moderate effect emerged for the underestimated recall of just-below prices. Publication bias corrections suggest smaller and, at times, nonsignificant true effects. Future research, ideally preregistered and sufficiently powered, should examine the robustness of price-ending effects (see Carter et al., 2019) and further illuminate the assumed underlying mechanisms of just-below versus round prices.

ACKNOWLEDGEMENTS

This work was supported by the German Research Foundation (DFG LO 2201/2-1). Open Access funding enabled and organized by Projekt DEAL.

DATA AVAILABILITY STATEMENT

Data, scripts, and supplemental materials are available at https://osf.io/bqdpm/.

ORCID

Eve Sarah Troll https://orcid.org/0000-0001-8206-8487
Julius Frankenbach https://orcid.org/0000-0002-1627-8803
Malte Friese https://orcid.org/0000-0003-0055-513X
David D. Loschelder https://orcid.org/0000-0001-5818-0559

REFERENCES

References marked with an asterisk indicate studies included in the meta-analysis.

*Allred, A., Valentin, E. K., & Chakraborty, G. (2010). Pricing risky services: Preference and quality considerations. *Journal of Product and Brand Management*, *19*(1), 54–60. https://doi.org/10.1108/10610421011018392

Anderson, E., & Simester, D. I. (2003). Effects of $9 price endings on retail sales: Evidence from field experiments. *Quantitative Marketing and Economics*, *1*(1), 93–110. https://doi.org/10.1023/A:1023581927405

*Baumgartner, B., & Steiner, W. J. (2007). Are consumers heterogeneous in their preferences for odd and even prices? Findings from a choice-based conjoint study. *International Journal of Research in Marketing*, *24*(4), 312–323. https://doi.org/10.1016/j.ijresmar.2007.05.003

Berkowitz, L., & Donnerstein, E. (1982). External validity is more than skin deep: Some answers to criticisms of laboratory experiments. *American Psychologist*, *37*(3), 245–257. https://doi.org/10.1037/0003-066X.37.3.245

Biswas, A., Pullig, C., Yagci, M. I., & Dean, D. H. (2002). Consumer evaluation of low price guarantees: The moderating role of reference price and store image. *Journal of Consumer Psychology*, *12*(2), 107–118. https://doi.org/10.1207/153276602760078640

Bizer, G. Y., & Schindler, R. M. (2005). Direct evidence of ending-digit drop-off in price information processing. *Psychology and Marketing*, *22*(10), 771–783. https://doi.org/10.1002/mar.20084

Borenstein, M., Hedges, L. V., Higgins, J. P. T., & Rothstein, H. R. (2009). *Introduction to meta-analysis*. John Wiley & Sons, Ltd.

Bradley, A., & James, R. J. E. (2019). Web scraping using R. *Advances in Methods and Practices in Psychological Science*, *2*(3), 264–270. https://doi.org/10.1177/2515245919859535

*Bray, J., & Harris, C. (2006). The effect of 9-ending prices on retail sales: A quantitative UK based field study. *Journal of Marketing Management*, *22*(5–6), 601–617. https://doi.org/10.1362/026725706777978631

Bruns, S. B., & Ioannidis, J. P. A. (2016). p-curve and p-hacking in observational research. *PLoS One*, *11*(2), e0149144. https://doi.org/10.1371/journal.pone.0149144

Carter, E. C., Schönbrodt, F. D., Gervais, W. M., & Hilgard, J. (2019). Correcting for bias in psychology: A comparison of meta-analytic methods. *Advances in Methods and Practices in Psychological Science*, *2*(2), 115–144. https://doi.org/10.1177/2515245919847196

*Carver, J. R., & Padgett, D. T. (2012). Product category pricing and future price attractiveness: 99-ending pricing in a memory-based context. *Journal of Retailing*, *88*(4), 497–511. https://doi.org/10.1016/j.jretai.2012.05.001

*Chang, H.-H., & Chen, F.-P. (2014). When is a 9-ending price perceived lower than a 0-ending price? The moderating role of price consciousness. *International Journal of Business and Information*, *9*(1), 89–116.

*Choi, J., Lee, K., & Ji, Y. Y. (2012). What type of framing message is more appropriate with nine-ending pricing? *Marketing Letters*, *23*(3), 603–614. https://doi.org/10.1007/s11002-012-9164-7

*Choi, J., Li, Y. J., Rangan, P., Chatterjee, P., & Singh, S. N. (2014). The odd-ending price justification effect: The influence of price-endings on hedonic and utilitarian consumption. *Journal of the Academy of Marketing Science*, *42*(5), 545–557. https://doi.org/10.1007/s11747-014-0369-6

Cicchetti, D. V. (1994). Guidelines, criteria, and rules of thumb for evaluating normed and standardized assessment instruments in psychology. *Psychological Assessment*, *6*(4), 284–290. https://doi.org/10.1037/1040-3590.6.4.284

Cohen, J. (1968). Weighted kappa: Nominal scale agreement provision for scaled disagreement or partial credit. *Psychological Bulletin*, *70*(4), 213–220. https://doi.org/10.1037/h0026256

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Erlbaum.

Coles, N. A., Larsen, J. T., & Lench, H. C. (2019). A meta-analysis of the facial feedback literature: Effects of facial feedback on emotional experience are small and variable. *Psychological Bulletin*, *145*(6), 610–651. https://doi.org/10.1037/bul0000194

Cooper, H., Hedges, L. V., & Valentine, J. C. (2009). *The handbook of research synthesis and meta-analysis* (2nd ed.). Russell Sage Foundation. https://doi.org/10.7758/9781610448864

*Coulter, K. S. (2001). Odd-ending price underestimation: An experimental examination of left-to-right processing effects. *Journal of Product & Brand Management*, *10*(5), 276–292. https://doi.org/10.1108/10610420110401838

*Coulter, K. S. (2002). The influence of print advertisement organization on odd-ending price image effects. *Journal of Product & Brand Management*, *11*(5), 319–334. https://doi.org/10.1108/10610420210442193

Coulter, K. S., Choi, P., & Monroe, K. B. (2012). Comma N' cents in pricing: The effects of auditory representation encoding on price magnitude perceptions. *Journal of Consumer Psychology*, *22*(3), 395–407. https://doi.org/10.1016/j.jcps.2011.11.005

Coulter, K. S., & Coulter, R. A. (2005). Size does matter: The effects of magnitude representation congruency on price perceptions and purchase likelihood. *Journal of Consumer Psychology*, *15*(1), 64–76. https://doi.org/10.1207/s15327663jcp1501_9

Del Re, A. C. (2013). compute.es: Compute effect sizes. https://cran.r-project.org/package=compute.es

Del Re, A. C., & Hoyt, W. T. (2018). MAd: Meta-analysis with mean differences. https://cran.r-project.org/package=MAd

*Diller, H., & Brielmaier, A. (1995). The impact of rounding- up odd prices: Results of a field experiment in German drugstores. *Pricing Strategy & Practice*, *3*(4), 4–13.

*Dodds, W. B., & Monroe, K. B. (1985). The effect of brand and price information on subjective product evaluations. *Advances in Consumer Research*, *12*, 85–90.

Duval, S. (2005). The trim and fill method. In H. R. Rothstein, A. J. Sutton, & M. Borenstein (Eds.), *Publication bias in meta-analysis: Prevention, assessment and adjustments* (pp. 127–144). Wiley.

Eisend, M. (2015). Have we progressed marketing knowledge? A meta-meta-analysis of effect sizes in marketing research. *Journal of Marketing*, *79*(3), 23–40. https://doi.org/10.1509/jm.14.0288

Erdfelder, E., & Heck, D. W. (2019). Detecting evidential value and p-hacking with the p-curve tool. *Zeitschrift für Psychologie*, *227*(4), 249–260. https://doi.org/10.1027/2151-2604/a000383

Fanelli, D. (2012). Negative results are disappearing from most disciplines and countries. *Scientometrics*, *90*(3), 891–904. https://doi.org/10.1007/s11192-011-0494-7

Field, A., Miles, J., & Field, Z. (2012). *Discovering statistics using R*. Sage Publications.

Fisher, Z., Tipton, E., & Zhipeng, H. (2017). robumeta: Robust variance meta-regression. https://github.com/zackfisher/robumeta

*Fortin, D. R., Cleland, S., & Jenkins, A. (2008). Effects of advertised pricing on brand image for an on-line retailer (pp. 263–275).

Friese, M., & Frankenbach, J. (2020). p-Hacking and publication bias interact to distort meta-analytic effect size estimates. *Psychological Methods*, *25*(4), 456–471. https://doi.org/10.1037/met0000246

Friese, M., Frankenbach, J., Job, V., & Loschelder, D. D. (2017). Does self-control training improve self-control? A meta-analysis. *Perspectives on Psychological Science*, *12*(6), 1077–1099. https://doi.org/10.1177/1745691617697076

Funder, D. C., & Ozer, D. J. (2019). Evaluating effect size in psychological research: Sense and nonsense. *Advances in Methods and Practices in Psychological Science*, *2*(2), 156–168. https://doi.org/10.1177/2515245919847202

Gabor, A. (1977). *Pricing: Principles and practices*. Heineman Educational Books Ltd.

*Gaston-Breton, C. (2011). Consumer preferences for 99-ending prices: The mediating role of price consciousness. Working paper.

*Gendall, P., Fox, M. F., & Wilton, P. (1998). Estimating the effect of odd pricing. *Journal of Product & Brand Management*, *7*(5), 421–432. https://doi.org/10.1108/03090569710190541

Gendall, P., Holdershaw, J., & Garland, R. (1997). The effect of odd pricing on demand. *European Journal of Marketing*, *31*(11/12), 799–813. https://doi.org/10.1108/03090569710190541

*Georgoff, D. M. (1972). Odd-even retail price endings: Their effects on value determination, product perception, and buying propensities.

Grewal, D., Puccinelli, N., & Monroe, K. B. (2018). Meta-analysis: Integrating accumulated knowledge. *Journal of the Academy of Marketing Science*, *46*(1), 9–30. https://doi.org/10.1007/s11747-017-0570-5

*Guéguen, N., & Jacob, C. (2005). Odd versus even prices and consumers' behavior. *Psychological Reports*, *96*(3), 1121–1122. https://doi.org/10.2466/pr0.96.3c.1121-1122

*Guéguen, N., Jacob, C., Legoherel, P., & Ngobo, P. (2009). Nine-ending prices and consumer's behavior: A field study in a restaurant. *International Journal of Hospitality Management*, *28*(1), 170–172. https://doi.org/10.1016/j.ijhm.2008.03.009

*Guéguen, N., & Legoherel, P. (2004). Numerical encoding and odd-ending prices. *European Journal of Marketing*, *38*(1/2), 194–208. https://doi.org/10.1108/03090560410511186

*Guido, G., & Peluso, A. (2004). Consumers' perception of odd-ending prices with the introduction of the euro. *Journal of Product & Brand Management*, *13*(3), 200–210. https://doi.org/10.1108/10610420410538096

Hall, E. T. (1976). *Beyond culture* (Vol. *7*, p. 503). Doubleday.

Hayes, A. F. (2013). *Introduction to mediation, moderation, and conditional process analysis*. Guilford Press.

Hedges, L. V. (1981). Distribution theory for Glass's estimator of effect size and related estimators. *Journal of Educational Statistics*, *6*(2), 107–128. https://doi.org/10.2307/1164588

Hedges, L. V., & Pigott, T. D. (2004). The power of statistical tests for moderators in meta-analysis. *Psychological Methods*, *9*, 426–445. https://doi.org/10.1037/1082-989X.9.4.426

Hedges, L. V., Tipton, E., & Johnson, M. C. (2010). Robust variance estimation in meta-regression with dependent effect size estimates. *Research Synthesis Methods*, *1*(1), 39–65. https://doi.org/10.1002/jrsm.5

Henrich, J., Heine, S. J., & Norenzayan, A. (2010). The weirdest people in the world? *Behavioral and Brain Sciences*, *33*(2–3), 61–83. https://doi.org/10.1017/S0140525X0999152X

*Holdershaw, J., Gendall, P., & Garland, R. (1997). The widespread use of odd pricing in the retail sector. *Marketing Bulletin-Department of Marketing Massey University*, *8*, 53–58.

Holdershaw, J., Gendall, P., & Gou, Y. (2005). The effect of odd pricing in China. *Journal of Asia Pacific Marketing*, *4*(1), 53–61.

Holloway, R. J. (1973). Book reviews. *Journal of Retailing*, *49*, 77–78.

Ioannidis, J. P. A. (2005). Contradicted and initially stronger effects in highly cited clinical research. *JAMA*, *294*(2), 218–228. https://doi.org/10.1001/jama.294.2.218

*Jaber, M., Hubbard, N., & Wolverton, J. (2013). Are .99 price endings still an effective way to influence purchase intentions? In *Society for marketing advances conference proceedings*.

*Jaber, M., & Jaber, K. (2017). Currency substitution and price endings: Right digit effect. *Journal of Global Marketing*, *30*(4), 238–255. https://doi.org/10.1080/08911762.2017.1308614

*Jaber, M., & Jaber, K. (2020). Cause-related marketing and the effect of 99-ending pricing. *Journal of Consumer Marketing*, *37*(3), 237–246. https://doi.org/10.1108/JCM-12-2018-2993

*Jeong, J. Y., & Crompton, J. L. (2018). Do subjects from high and low context cultures attribute different meanings to tourism services with 9-ending prices? *Tourism Management*, *64*, 110–118. https://doi.org/10.1016/j.tourman.2017.08.009

Kahneman, D. (2011). *Thinking, fast and slow*. Macmillan.

Kahneman, D., & Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica*, *47*(2), 263–292. https://doi.org/10.2307/1914185

*Kinard, B. R., Capella, M. L., & Bonner, G. (2013). Odd pricing effects: An examination using adaptation-level theory. *Journal of Product and Brand Management*, *22*(1), 87–94. https://doi.org/10.1108/10610421311298740

Kittler, M. G., Rygl, D., & MacKinnon, A. (2011). Beyond culture or beyond control? Reviewing the use of hall's high-/low-context concept. *International Journal of Cross Cultural Management*, *11*(1), 63–82. https://doi.org/10.1177/1470595811398797

*Krämer, A. (2015). Robustness of price perception: How strong are anchoring, left-digit- and framing-effects when promoting sales offers? *Business and Management Studies*, *2*(1), 35–43. https://doi.org/10.11114/bms.v2i1.1137

Kreul, L. M. (1982). Magic numbers: Psychological aspects of menu pricing. *Cornell Hotel and Restaurant Administration Quarterly*, *23*(2), 70–75. https://doi.org/10.1177/001088048202300223

Kromrey, J. D., & Rendina-Gobioff, G. (2006). On knowing what we do not know. An empirical comparison of methods to detect publication bias in meta-analysis. *Educational and Psychological Measurement*, *66*(3), 357–373. https://doi.org/10.1177/0013164405278585

Lakens, D., Hilgard, J., & Staaks, J. (2016). On the reproducibility of meta-analyses: Six practical recommendations. *BMC Psychology*, *4*(1), 1–10. https://doi.org/10.1186/s40359-016-0126-3

Lambert, Z. V. (1975). Perceived price as related to odd and even price endings. *Journal of Retailing*, *51*(3), 13–22.

Leone, R. P., Robinson, L. M., Bragge, J., & Somervuori, O. (2012). A citation and profiling analysis of pricing research from 1980 to 2010. *Journal of Business Research*, *65*(7), 1010–1024. https://doi.org/10.1016/j.jbusres.2011.04.007

Levy, D., Lee, D., Chen, H. A., Kauffman, R. J., & Bergen, M. (2011). Price points and price rigidity. *Review of Economics and Statistics*, *93*(4), 1417–1431. https://doi.org/10.1162/REST_a_00178

*Liang, J., & Kanetkar, V. (2006). Price endings: Magic and math. *Journal of Product and Brand Management*, *15*(6), 377–385. https://doi.org/10.1108/10610420610703702

*Lin, C. H., & Wang, J. W. (2017). Distortion of price discount perceptions through the left-digit effect. *Marketing Letters*, *28*(1), 99–112. https://doi.org/10.1007/s11002-015-9387-5

Lipsey, M. W., & Wilson, D. B. (1993). The efficacy of psychological, educational, and behavioral treatment: Confirmation from meta-analysis. *American Psychologist*, *48*(12), 1181–1209. https://doi.org/10.1037/0003-066X.48.12.1181

*Little, J. D. C., & Ginise, J. (1987). Price-endings: Does 9 really make a difference? Working paper.

Loschelder, D. D., Friese, M., Schaerer, M., & Galinsky, A. D. (2016). The too-much-precision effect: When and why precise anchors backfire with experts. *Psychological Science*, *27*(12), 1573–1587. https://doi.org/10.1177/0956797616666074

*Manning, K. C., & Sprott, D. E. (2009). Price endings, left-digit effects, and choice. *Journal of Consumer Research*, *36*(2), 328–335. https://doi.org/10.1086/597215

McKenzie, R. B. (2008). *Why popcorn costs so much at the movies*. Springer. https://doi.org/10.1007/978-0-387-77001-7

McShane, B. B., Böckenholt, U., & Hansen, K. T. (2016). Adjusting for publication bias in meta-analysis. *Perspectives on Psychological Science*, *11*(5), 730–749. https://doi.org/10.1177/1745691616662243

McShane, B. B., Gal, D., Gelman, A., Robert, C., & Tackett, J. L. (2019). Abandon statistical significance. *The American Statistician*, *73*, 235–245. https://doi.org/10.1080/00031305.2018.1527253

Mele, M. L., & Federici, S. (2012). Gaze and eye-tracking solutions for psychological research. *Cognitive Processing*, *13*(1), 261–265. https://doi.org/10.1007/s10339-012-0499-z

Moeyaert, M., Ugille, M., Natasha Beretvas, S., Ferron, J., Bunuan, R., & Van den Noortgate, W. (2017). Methods for dealing with multiple outcomes in meta-analysis: A comparison between averaging effect sizes, robust variance estimation and multilevel meta-analysis. *International Journal of Social Research Methodology*, *20*(6), 559–572. https://doi.org/10.1080/13645579.2016.1252189

Moher, D., Liberati, A., Tetzlaff, J., Altman, D. G., Altman, D., Antes, G., Atkins, D., Barbour, V., Barrowman, N., Berlin, J. A., Clark, J., Clarke, M., Cook, D., D'Amico, R., Deeks, J. J., Devereaux, P. J., Dickersin, K., Egger, M., Ernst, E., … Tugwell, P. (2009). Preferred reporting items for systematic reviews and meta-analyses: The PRISMA statement. *PLoS Medicine*, *6*(7), e1000097. https://doi.org/10.1371/journal.pmed.1000097

Monroe, K. B. (1990). *Pricing: Making profitable decisions* (2nd ed.). McGraw-Hill.

*Mueller, E. S., Frech, M.-L., & Loschelder, D. D. (2020). Investigating opposing effects and predictions for round ($10.00), just-below ($9.99), and precise prices ($9.87)—two pre-registered, high-powered experiments.

*Naipaul, S., & Parsa, H. G. (2001). Menu price endings that communicate value and quality. *The Cornell Hotel and Restaurant Administration Quarterly*, *42*(1), 26–37. https://doi.org/10.1016/S0010-8804(01)90008-4

Nelson, L. D., Simmons, J., & Simonsohn, U. (2018). Psychology's renaissance. *Annual Review of Psychology*, *69*(1), 511–534. https://doi.org/10.1146/annurev-psych-122216-011836

Nguyen, A., Heeler, R. M., & Taran, Z. (2007). High-low context cultures and price-ending practices. *Journal of Product and Brand Management*, *16*(3), 206–214. https://doi.org/10.1108/10610420710751582

Olson, J. C. (1972). *Cue utilisation in the quality perception process: A cognitive model and an empirical test*. Purdue University.

Oppenheimer, D. M. (2008). The secret life of fluency. *Trends in Cognitive Sciences*, *12*(6), 237–241. https://doi.org/10.1016/j.tics.2008.02.014

*Palmer, A., & Boissy, S. (2009). The effects of airline price presentations on buyers' choice. *Journal of Vacation Marketing*, *15*(1), 39–52. https://doi.org/10.1177/1356766708098170

Peters, J. L., Sutton, A. J., Jones, D. R., Abrams, K. R., & Rushton, L. (2007). Performance of the trim and fill method in the presence of publication bias and between-study heterogeneity. *Statistics in Medicine*, *26*(25), 4544–4562. https://doi.org/10.1002/sim.2889

Pope, D. (2020). Unveiling the law of demand using a large-scale natural field experiment. Presentation Given at Data Colada Seminar Series on September 25, 2020.

Pustejovsky, J. E. (2020). clubSandwich: Cluster-robust (Sandwich) variance estimators with small-sample corrections. https://github.com/jepusto/clubSandwich

*Quigley, C. J., & Notarantonio, E. M. (2015). An exploratory investigation of perceptions of odd and even pricing. In *Proceedings of the 1992 Academy of Marketing Science (AMS) annual conference* (pp. 306–309). https://doi.org/10.1007/978-3-319-13248-8_63

R Core Team. (2020). *R: A language and environment for statistical computing* [Computer software]. R Foundation for Statistical Computing.

Rao, A. R., & Monroe, K. B. (1989). The effect of price, brand name, and store name on buyers' perception of product quality. *Journal of Marketing Research*, *26*(3), 351–357. https://doi.org/10.1021/cen-09504-cover4

Rodgers, M. A., & Pustejovsky, J. E. (2020). Evaluating meta-analytic methods to detect selective reporting in the presence of dependent effect sizes. *Psychological Methods*, *1–25*, 141–160. https://doi.org/10.1037/met0000300

Rogers, L. (1990). *Pricing for profit*. Basil Blackwell.

*Royne, M. B., Martinez, J., Oakley, J., & Fox, A. K. (2012). The effectiveness of benefit type and price endings in green advertising. *Journal of Advertising*, *41*(4), 85–102. https://doi.org/10.1080/00913367.2012.10672459

Rudolph, H. J. (1954). *Pricing for today's market* (pp. 22–24). Printers' Ink.

Schindler, R. M. (1991). Symbolic meanings of a price ending. *Advances in Consumer Research*, *18*, 794–801.

Schindler, R. M. (2006). The 99 price ending as a signal of a low-price appeal. *Journal of Retailing*, *82*(1), 71–77. https://doi.org/10.1016/j.jretai.2005.11.001

Schindler, R. M., & Chandrashekaran, R. (2004). Influence of price endings on price recall: A by-digit analysis. *Journal of Product & Brand Management*, *13*(7), 514–524. https://doi.org/10.1108/10610420410568453

*Schindler, R. M., & Kibarian, T. (1993). Testing for perceptual underestimation of 9-ending prices. *Advances in Consumer Research*, *20*, 580–585.

*Schindler, R. M., & Kibarian, T. M. (1996). Increased consumer sales response though use of 99-ending prices. *Journal of Retailing*, *72*(2), 187–199. https://doi.org/10.1016/S0022-4359(96)90013-5

*Schindler, R. M., & Kibarian, T. M. (2001). Image communicated by the use of 99 endings in advertised prices. *Journal of Advertising*, *30*(4), 95–99. https://doi.org/10.1080/00913367.2001.10673654

Schindler, R. M., & Kirby, P. N. (1997). Patterns of rightmost digits used in advertised prices: Implications for nine-ending effects. *Journal of Consumer Research*, *24*(2), 192–201. https://doi.org/10.1086/209504

Schindler, R. M., Parsa, H. G., & Naipaul, S. (2011). Hospitality managers' price-ending beliefs: A survey and applications. *Cornell Hospitality Quarterly*, *52*(4), 421–428. https://doi.org/10.1177/1938965511421168

*Schindler, R. M., & Warren, L. S. (1988). Effect of odd pricing on choice of items from a menu. *Advances in Consumer Research*, *15*, 348–354.

*Schindler, R. M., & Wiman, A. R. (1989). Effects of odd pricing on price recall. *Journal of Business Research*, *19*(3), 165–177. https://doi.org/10.1016/0148-2963(89)90017-9

Schram, A. (2005). Artificiality: The tension between internal and external validity in economic experiments. *Journal of Economic Methodology*, *12*(2), 225–237. https://doi.org/10.1080/13501780500086081

Shrout, P. E., & Fleiss, J. L. (1979). Intraclass correlations: Uses in assessing rater reliability. *Psychological Bulletin*, *86*(2), 420–428.
Simonsohn, U., Nelson, L. D., & Simmons, J. P. (2014). P-curve: A key to the file-drawer. *Journal of Experimental Psychology: General*, *143*(2), 534–547. https://doi.org/10.1037/a0033242
Simonsohn, U., Simmons, J. P., & Nelson, L. D. (2015). Better P-curves: Making P-curve analysis more robust to errors, fraud, and ambitious P-hacking, a reply to Ulrich and Miller (2015). *Journal of Experimental Psychology: General*, *144*(6), 1146–1152. https://doi.org/10.1037/xge0000104
Snir, A., & Levy, D. (2021). If you think 9-ending prices are low, think again. *Journal of the Association for Consumer Research*, *6*(1), 33–47. https://doi.org/10.1086/710241
Sokolova, T., Seenivasan, S., & Thomas, M. (2020). The left-digit bias: When and why are consumers penny wise and pound foolish? *Journal of Marketing Research*, *57*(4), 771–788. https://doi.org/10.1177/0022243720932532
Stanley, T. D. (2017). Limitations of PET-PEESE and other meta-analysis methods. *Social Psychological and Personality Science*, *8*(5), 581–591. https://doi.org/10.1177/1948550617693062
Stanley, T. D., & Doucouliagos, H. (2014). Meta-regression approximations to reduce publication selection bias. *Research Synthesis Methods*, *5*(1), 60–78. https://doi.org/10.1002/jrsm.1095
Sterne, J. A. C., Becker, B. J., & Egger, M. (2005). The funnel plot. In H. R. Rothstein, A. J. Sutton, & M. Borenstein (Eds.), *Publication bias in meta-analysis: Prevention, assessment and adjustments* (pp. 75–98). Wiley.
Sterne, J. A. C., & Egger, M. (2005). Regression methods to detect publication and other bias in meta-analysis. In H. R. Rothstein, A. J. Sutton, & M. Borenstein (Eds.), *Publication bias in meta-analysis: Prevention, assessment and adjustments* (pp. 99–110). Wiley.
Stiving, M. (2000). Price-endings when prices signal quality. *Management Science*, *46*(12), 1617–1629. https://doi.org/10.1287/mnsc.46.12.1617.12078
Strulov-Shlain, A. (2019). More than a penny's worth: Left-digit bias and firm pricing. Chicago Booth Research Paper No. 19-22. https://doi.org/10.2139/ssrn.3413019
Sturdivant, F. D. (1970). *Managerial analysis in marketing*. Scott Foresman and Company.
Suri, R., Anderson, R. E., & Kotlov, V. (2004). The use of 9-ending prices: Contrasting the USA with Poland. *European Journal of Marketing*, *38*(1–2), 56–72. https://doi.org/10.1108/03090560410511122
Tanner-Smith, E. E., & Tipton, E. (2014). Robust variance estimation with dependent effect sizes: Practical considerations including a software tutorial in Stata and SPSS. *Research Synthesis Methods*, *5*(1), 13–30. https://doi.org/10.1002/jrsm.1091
Tanner-Smith, E. E., Tipton, E., & Polanin, J. R. (2016). Handling complex meta-analytic data structures using robust variance estimates: A tutorial in R. *Journal of Developmental and Life-Course Criminology*, *2*(1), 85–112. https://doi.org/10.1007/s40865-016-0026-5
Thaler, R. (1985). Mental accounting and consumer choice. *Marketing Science*, *4*(3), 199–214. https://doi.org/10.1287/mksc.4.3.199
*Thomas, M., & Morwitz, V. (2005). Penny wise and pound foolish: The left-digit effect in price cognition. *Journal of Consumer Research*, *32*(1), 54–64. https://doi.org/10.1086/429600
Thomas, M., Simon, D. H., & Kadiyali, V. (2010). The price precision effect: Evidence from laboratory and market data. *Marketing Science*, *29*(1), 175–190. https://doi.org/10.1287/mksc.1090.0512
Tipton, E. (2015). Small sample adjustments for robust variance estimation with meta-regression. *Psychological Methods*, *20*(3), 375–393. https://doi.org/10.1037/met0000011
Tipton, E., & Pustejovsky, J. E. (2015). Small-sample adjustments for tests of moderators and model fit using robust variance estimation in meta-regression. *Journal of Educational and Behavioral Statistics*, *40*(6), 604–634. https://doi.org/10.3102/1076998615606099
*Tripathi, A., & Pandey, N. (2018a). Are nine-ending prices equally influential in eastern culture for pricing green products? *Journal of International Consumer Marketing*, *30*(3), 192–205. https://doi.org/10.1080/08961530.2017.1384711
*Tripathi, A., & Pandey, N. (2018b). Does impact of price endings differ for the non-green and green products? Role of product categories and price levels. *Journal of Consumer Marketing*, *35*(2), 143–156. https://doi.org/10.1108/JCM-06-2016-1838
Turner, J. C., Hogg, M. A., Oakes, P. J., Reicher, S. D., & Wetherell, M. S. (1987). *Rediscovering the social group: A self-categorization theory*. Basil Blackwell.
Twedt, D. W. (1965). Does the "9 fixation" in retail pricing really promote sales? *Journal of Marketing*, *29*(4), 54–55. https://doi.org/10.2307/1249703
Ulrich, R., & Miller, J. (2015). p-Hacking by post hoc selection with multiple opportunities: Detectability by skewness test?: Comment on Simonsohn, Nelson, and Simmons (2014). *Journal of Experimental Psychology: General*, *144*(6), 1137–1145. https://doi.org/10.1037/xge0000086
Van Aert, R. C. M., Wicherts, J. M., & van Assen, M. A. L. M. (2016). Conducting meta-analyses based on p values: Reservations and recommendations for applying p-uniform and p-curve. *Perspectives on Psychological Science*, *11*(5), 713–729. https://doi.org/10.1177/1745691616650874
Van den Noortgate, W., López-López, J. A., Marín-Martínez, F., & Sánchez-Meca, J. (2015). Meta-analysis of multiple outcomes: A multilevel approach. *Behavior Research Methods*, *47*(4), 1274–1294. https://doi.org/10.3758/s13428-014-0527-2
Van Erp, S., Verhagen, J., Grasman, R. P. P. P., & Wagenmakers, E.-J. (2017). Estimates of between-study heterogeneity for 705 meta-analyses reported in psychological bulletin from 1990–2013. *Journal of Open Psychology Data*, *5*(1), 1–5. https://doi.org/10.5334/jopd.33
*Van Raaij, W. F., & Van Rijen, C. L. A. (2003). Money illusion and euro pricing. In *Proceedings of the International Association for Research in Economic Psychology*.
Viechtbauer, W. (2010). Conducting meta-analyses in R with the metafor package. *Journal of Statistical Software*, *36*(3), 1–48. https://doi.org/10.18637/jss.v036.i03
Völckner, F., & Hofmann, J. (2007). The price-perceived quality relationship: A meta-analytic review and assessment of its determinants. *Marketing Letters*, *18*(3), 181–196. https://doi.org/10.1007/s11002-007-9013-2
Wadhwa, M., & Zhang, K. (2015). This number just feels right: The impact of roundedness of price numbers on product evaluations. *Journal of Consumer Research*, *41*(5), 1172–1185. https://doi.org/10.1086/678484
*Wieseke, J., Kolberg, A., & Schons, L. M. (2016). Life could be so easy: The convenience effect of round price endings. *Journal of the Academy of Marketing Science*, *44*(4), 474–494. https://doi.org/10.1007/s11747-015-0428-7
Williamson, S. H. (2020). *Purchasing power today of a US dollar transaction in the past*. MeasuringWorth www.measuringworth.com/ppowerus/
Zizzo, D. J. (2010). Experimenter demand effects in economic experiments. *Experimental Economics*, *13*(1), 75–98. https://doi.org/10.1007/s10683-009-9230-z

How to cite this article: Troll, E. S., Frankenbach, J., Friese, M., & Loschelder, D. D. (2024). A meta-analysis on the effects of just-below versus round prices. *Journal of Consumer Psychology*, *34*, 299–325. https://doi.org/10.1002/jcpy.1353