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## Testing the strength and robustness of the attraction effect in consumer decision making

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## **Testing the strength and robustness of the attraction effect in consumer decision making.**

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# Testing the strength and robustness of the attraction effect in consumer decision making.<sup>☆</sup>

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## Abstract

We report the results of an original experiment that was designed to test the strength and robustness of the attraction effect. Rather than the usual simple tests for this effect, we consider a conceptually simple consumer purchasing task where alternatives are however difficult to evaluate. For the attraction effect to be observed, the consumer must go through two steps: the first is to find out that two or more options are comparable, which leads him to exclude the dominated alternatives. The second is to favor the dominant option over those that are not comparable. Our experiment allows us to determine whether and how many individuals stop before each of those two steps. The results confirm the existence of an attraction effect in our setting, but the effect is not strong. Indeed, only a minority of subjects perform the second step. The effect is not robust to introducing larger differences in prices among options and to widening the range of options to choose from. We conclude by showing that our subjects would benefit from relying more on performing asymmetric dominance editing rather than on their skills in the purchasing task.

*JEL Codes:* C91, D12, D83

*Keywords:* Asymmetric Dominance Editing, Attraction Effect, Comparability, Consumer Choice, Experimental Economics, Pricing Formats

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## 1. Introduction

Choosing can be difficult. Consumers often go to great lengths to identify good offers and make thoughtful choices but still make mistakes when facing complex purchasing decisions. Behavioral economics finds that consumers have “inconsistent, context dependent preferences” and may not have “enough brainpower to evaluate and compare complicated products” (Spiegler, 2011). Marketing research (Morwitz et al., 1998; Nunes, 2000; Viswanathan et al., 2005; Zeithaml, 1982) and research from behavioral economics (Ariely, 2008; Iyengar and Lepper, 2000; Iyengar et al., 2004) document several biases in consumer choices.

Consumers manage to navigate complex choice environments by using simple choice heuristics that, while sometimes leading to suboptimal choices and biases, tend to perform well when the situation is too complex to be handled with more “rational” decision processes (Gigerenzer et al., 1999). This is the case of the attraction effect, also known as asymmetric dominance or decoy effect (Huber et al., 1982; Huber and Puto, 1983; Simonson, 1989; Tversky and Simonson, 1993): consumers are more likely to choose a product if it is presented along with a clearly dominated option than if it is presented on its own. The context in which an offer is presented therefore matters even when, by the independence of irrelevant alternatives, it should not matter for a rational decision maker. The literature on the attraction effect is vast (recent papers include Park and Kim, 2005; Malkoc et al., 2013) but the robustness of its results has recently been put into question (Frederick et al., 2014; Yang and Lynn, 2014). Most papers test the attraction effect with designs in which one compares a choice set with no dominated option and a choice set with an additional dominated option. Consumers then tend to choose the now dominating option more often than when it was standing on its own. However, introducing this additional dominated option changes the size of the choice set, which can on its own influence consumer choice. Furthermore, the dominated option is clearly set out as such, in particular by representing all product dimensions (size, price, quality...) as numbers (Frederick et al., 2014). Making the dominated option easy to identify is not realistic however as consumers usually find it difficult to identify dominance relations. Finally, existing experiments do not go beyond establishing the effect or its absence and are not helpful in measuring its strength.

In this paper we break down the attraction effect into two parts and devise a new laboratory task that enables us to measure the strength and

relative importance of each of them. In order for the attraction effect to be observed, two distinct steps must be performed by a decision maker: *recognize* dominance, which leads him to edit out the dominated offers, and give an *extra preference* to the dominant offer. To illustrate these two steps, let us restrict attention to the simplest example, a choice among three items, A, B, and C, in which A dominates B. This is the workhorse of virtually all of the literature on asymmetric dominance.

First, the decision maker must be able to recognize that two or more elements of the choice set are easy to compare (“comparable”) along one or more dimensions. We define “comparable” as “easy to compare”, that is, “equal on at least one product dimension”. Recognizing comparability is a precondition to build a decision heuristic based on dominance. The issue of comparability has traditionally been set aside in the papers about the attraction effect. They adopt a design in which subjects are exposed first to a choice set with no dominated option (A vs. C) and subsequently to a choice set with an additional, clearly identifiable dominated option (B). In most studies, subjects are directly *told* that the new, added option is dominated. Recently, Frederick et al. (2014); Yang and Lynn (2014) showed that when the product dimensions are not clearly spelled out in numbers, the attraction effect is rarely observed, if at all. We will call *naive* those subjects who fail to *see* that two offers are comparable, and hence are unable to apply a decision heuristic based on dominance; in their eyes, B is not dominated. The decision maker who is able to recognize the presence of dominated offers can simplify his choice by eliminating them from the choice set. This holds even when the offers themselves are still intrinsically complex. For example, the switch by Apple from PowerPC processors to Intel x86 processors in 2006 did not make the performance of Apple computers easier to evaluate, but it did make it easier to compare with the performance of most other computers. Since this step involves using dominance to edit some offers out of the choice set, we call consumers who follow this step *dominance editors*. This first step may seem trivial but in most experiments at least some subjects choose dominated options, and in several cases their share is not negligible. A subject who performs dominance editing deletes B from his choice set, and chooses between A and C according to some other rule – for example by relying on his perceived value of the two options.

Second, the decision maker who eliminates dominated offers must give a *preference* to the dominating offer over and above the mere elimination of the dominated options. In an abstract setting, whether A dominates B does

not reveal anything about the ranking of A and C. To observe an attraction effect, at least some subjects must therefore give a preference to A over C *because* A dominates B, or, equivalently, they must penalize C because it is not comparable to (and does not dominate) any other offer. We call the subjects who exhibit this behavior *asymmetric dominance editors*. While their behavior might not make sense in some settings, it can make sense in contexts in which consumers are uncertain about the value of the offers on display. The more difficult it is for consumers to rank different alternatives, the more they should rely on whether an option clearly dominates another to make their choice – we extend on this point later on in the paper. The difficulty of evaluating different options in the real world may therefore explain why the attraction effect has been observed across many situations and even among birds (Schuck-Paim et al., 2004), bees (Shafir et al., 2002) and brainless, unicellular, slime molds (Latty and Beekman, 2010).

We present in this paper a new experimental design consisting of an incentivized purchasing task involving fuzzy comparability. The laboratory experiment allows us to assess whether subjects are *naive*, perform *dominance editing* or perform *asymmetric dominance editing*. It also enables us to measure the strength of the attraction effect in terms of price sensitivity. Participants are asked to buy *paint* to cover an area of a given size with the aim of minimizing expenditure. Prices for each option are displayed in terms of how much it would cost with that option to cover a given area, that is represented graphically in terms of a shape on screen. The task is therefore cognitively complex – it is difficult to compare different shapes of different size, and comparability is not clearly stated but must be inferred by subjects. At the same time, the task is intuitively simple – all a participant has to do is to find the best deal in terms of unit price.

The laboratory provides us with a controlled environment. Goods are homogeneous by design in our experiment so that all consumers would agree that the shape and size of an offer should be irrelevant to their choice. We induce preferences, so that we can abstract away from idiosyncratic preferences of our subjects. For each choice task we know the optimal choice and we can thus quantify how much actual choice deviates away from it in terms of money spent.

Our experimental design allows us to identify and measure the attraction effect in a context of fuzzy comparability while controlling for other aspects of the context in which an offer is presented. We employ this design first to reproduce the design traditionally adopted in the literature: three

items, with one being dominated. We also exploit the flexibility of the task to explore if the effect is robust to changing the characteristics of the choice environment: we vary systematically the number of elements in the choice set and how difficult it is to identify the best option.

Our paper complements the existing literature in several ways. We introduce a novel, rigorous task that can be used to assess performance in the purchasing decision by an absolute standard. We explore the extent of the attraction effect in a context in which comparability and dominance are less focal, and in which we vary the difficulty of the problem and the number of options in the choice set. Finally, our data allows us to *measure* the attraction effect, rather than only *show* that aggregate choice shifts towards the asymmetrically dominant option. Given the richness of the data collected at the individual level in our experiment, we can estimate for each subject how strong the attraction effect is, controlling not only for the objective unit price of each offer but also for individual characteristics and skills.

The attraction effect appears in our experiment, but it is not strong and is present for only a limited number of subjects. At the aggregate level, subjects identify comparable offers (“COs”) and favor the dominant offer. This preference is robust across different demographics and skills, but less so in difficult choice sets and when the number of elements in the choice set moves from 3 to 6. At the individual level, though, we find that 65 to 88% of subjects, depending on conditions, behave as if not taking into account comparability and dominance (some choose at random, others are sensitive to price only). We measure the size of the attraction effect as a price penalty inflicted on non comparable offers (“NCOs”). We find that NCOs suffer a 2 to 5% average price penalty compared to dominant offers, depending on conditions. We then perform simulations to find out the optimal level of the attraction effect as a function of a subject’s level of accuracy in choice among option – *i.e.*, the level of the price penalty to be attached to NCOs that maximizes welfare for our subjects given the choice sets they faced. We find that less precise subjects ought to use high penalty levels, which are much higher than the ones actually measured for the vast majority of subjects. Our subjects would have been better off by choosing more often dominant options rather than relying on their skills in evaluating each option separately.

The remainder of the paper is organized as follows. In Section 2 we survey the reasons given in the literature as to why consumers should exploit dominance relations to inform their choices. Section 3 describes our exper-






imental design and procedures. Section 4 details the results of our study, and Section 5 concludes.

## 2. Comparability and dominance in consumer choice

Evaluating products is a difficult exercise. Even products with standardized quality, *e.g.*, type-00 flour, salt, or electricity, might be offered by different providers with different brands, in packages of different size and shape and using different price formats. A choice among homogeneous products can thus be made difficult by adding spurious complexity to the offers – *i.e.* complexity in dimensions that all consumers would agree should not influence choice.

Consider the example in table 1, with standard black tea chosen from retailers in Germany, with characteristics as described below the respective pictures. Let us assume that the tea contained in each box is of exactly the same quality. Then the differentiation introduced by the different producers can be said to be spurious: offers differ only in the number of tea bags and the quantity of tea in each bag, a difference that has nothing to do with the quality of the product.

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Brand name	Meßmer	Teekanne	Dennree	Marco Polo	Herba
# of tea bags	25	20	20	25	20
Quantity/bag	1.75g	1.75g	1.5g	1.5g	1.5g
Price	€ 1.80	€ 1.55	€ 1.19	€ 1.09	€ 0.89
(Price/100g)	(€ 4.11)	(€ 4.42)	(€ 3.97)	(€ 2.91)	(€ 2.97)

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Table 1: A “simple” consumer choice problem

If the consumer is given the price per 100g of tea, as done in the last row of table 1, then all offers are reduced to a common metric and all become easily comparable. But let us suppose that that information is not given so consumers have to find the best tea deal based on the first four rows only.

Faced with this problem, consumers might compute the price per 100 grams themselves, or else make use of several *shortlisting heuristics* (Tversky, 1972) to reduce the number of calculations to be performed. For instance, consumers might use a *budget heuristic*, that favors small packages,



or equivalently cheaper items (Viswanathan et al., 2005); a *bulk purchasing heuristic*, that favors big packages, as offers in big packages are usually better deals or a *focality-based heuristics*, that may either favor the first, or the last, or in general the most focal item in the choice set. They could choose based on brand, or on the color of the box, or on past choices, or on other people's choices.

We focus here on the two steps, involving comparability and dominance, which were detailed in the introduction. The first step is to recognize that some offers are comparable. In the example, it is straightforward to observe that Herba and Denree are fully comparable (same number of tea bags, same quantity/bag). They are also comparable to some extent with Marco Polo, since their tea bags have the same weight. Having recognized the existence of COs, a subject can edit out the dominated options. Herba dominates Denree, since it is fully comparable and cheaper. Denree is also dominated by Marco Polo, as it offers 5 more tea bags of the same weight and is still cheaper. However, it is not that easy to compare Herba and Marco Polo, nor to compare them with Meßmer and Teekanne, since they do not share a common format. A subject limiting himself to *dominance editing* would eliminate Denree and choose one of the four remaining options based on his evaluation of their unit price.

The second step involves recognizing that there might be further information to be gained from an offer being dominated by another. A subject might give a preference to Herba over the other options because it dominates Denree. An individual who is subject to a strong attraction effect would then directly choose the lowest priced of the COs – in this case, Herba. This heuristic limits considerably the computations to be performed, and is almost optimal in this case – Marco Polo is actually slightly cheaper than Herba.

Choosing Herba in this case thus requires two logical steps: first recognizing COs which leads to deleting dominated offers from the choice set; second, giving a premium to the dominant offer that extends above and beyond the mere fact that Herba dominates Denree.

There are several reasons why adopting this simple heuristic makes sense.

**For simplicity:** Agents faced with complex choices tend to follow simple heuristics, often with good results (Gigerenzer and Brighton, 2009). Restricting attention to the best of the COs simplifies the decision problem and limits its cognitive demand (Ariely and Wallsten, 1995).

An offer being unambiguously better than another provides “one good reason” to choose it (Shafir et al., 1993; Gigerenzer and Goldstein, 1999).

**Statistically:** Assuming that prices are i.i.d. across offers and that the offers’ comparability is due to chance alone, then the cheapest among the COs is cheaper in expectation than all other offers. Indeed, suppose one faces three products with prices  $(p_1, p_2, p_3)$  unknown but all drawn from the same distribution. One is told that product 1 is cheaper than product 2. Its expected price is then  $E(\min(p_1, p_2))$ , which is less than the expected price of product 3,  $E(p_3)$ . As in the Monty-Hall problem (Selvin, 1975; vos Savant, 1990; Friedman, 1998), there is information gained from being told that an option is dominated. This reasoning can be generalized to situations in which consumer can imperfectly assess prices, as explained in appendix C.

**From an understanding of competition:** Firms choosing to offer fully comparable products signal a willingness to compete on price, while firms offering incomparable products might be engaging in collusion. Choosing the lowest priced of the COs is strictly optimal in a competitive context where firms choose strategically whether to offer comparable products and at least some other consumers follow the same heuristic (Gaudeul and Sugden, 2012).

The simple heuristic of choosing the dominant option among COs is based on multiple foundations and can thus be generalized across many settings. This makes it more robust than heuristics that hold only in some settings or that can be justified in only one way (Sugden, 1989).

Products can be comparable or not comparable along several dimensions. It might be difficult then to define what it means for two products to be comparable. In the remainder of the paper, we will operationalize the concept of comparability in terms of a format. A format or frame is, to paraphrase Spiegler (2011, p.151), an aspect of a product’s presentation that is of no relevance to a consumer’s utility and yet affects his ability to make comparisons among alternatives. This can be a way to present prices, the language in a contract clause, but also a unit of measurement, a way of packaging a product, a technical standard and so on. Expressing an offer in terms of a common format does *not* inherently make that offer less complex to understand. That is, comparable offers when standing on their own will not be easier to evaluate than offers that are presented in terms of their

own individual formats. It is only when put in relation with other offers that an offer will be seen to be comparable with some offers and not with others.

### 3. Experimental design

#### 3.1. Paint purchasing task

Subjects were given a budget of 60 ECU (=€48) to buy *gray paint* in order to cover a fixed, square area  $A$ . They had to choose one out of a menu of paint offers, each offer being expressed in terms of its price and a visual representation of the area that the paint could cover for that price. The subjects had the aim of minimizing expenditure. Their payoff was determined as all that remained from their budget once all the paint needed to cover  $A$  had been bought at the cost implied by the chosen offer.

Formally, each offer was a triple  $(s, a, p)$  in which  $s$  is a shape,  $a$  is the area of the shape  $s$ , expressed as a fraction of the total area  $A$ , and  $p$  is the price of the offer. Two offers sharing the same shape *and* size  $(s, a)$  could be said to be easily *comparable*, as price was then the only remaining differentiating factor. The overall price paid for the chosen offer was calculated as  $p/a$ , and the payoff for the subject was  $60 - p/a$ .

While the task is conceptually very simple and relates to everyday activities – subjects must minimize expenditure when buying a product of standardized quality – it is also cognitively quite hard, as evaluating hidden unit prices and comparing areas of different shapes can be difficult. Figure 1 represents one instance of the paint purchasing task.

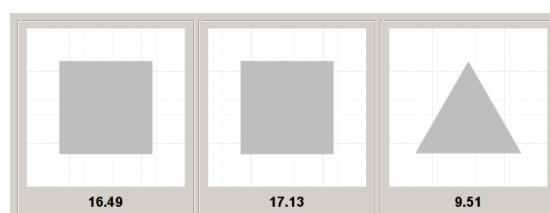


Figure 1: Screen-shot of a menu with three offers, of which two with the same format.

The offers' three dimensions varied in the following way:

1. The shape  $s$  could be a circle, a square, or an equilateral triangle. We considered only those three shapes in order to exploit the existing literature on shape comparisons (Krider et al., 2001), according to which broad based offers such as triangles are usually perceived as bigger

than more compact squares or even more compact circles covering the same area.

2. The area  $a$  took one of 12 possible values. Normalizing the total area  $A$  to 100, these values ranged from 10 to 43, in steps of 3. The step was chosen to be big enough to allow our subjects to determine easily whether an offer was bigger than another of the same shape within a menu, while being small enough to yield a sufficient differentiation in size.
3. The price information conveyed to the subjects,  $p$ , was computed from randomly drawn *unit prices* ( $up$ , the cost to cover 1% of  $A$ ) as  $p = up \cdot a$ . Unit prices  $up$  were drawn from a normal distribution of mean 0.5, while standard deviation varied by treatment. No price was allowed to be so high as to result in a potential loss for the subject choosing the related offer.

The offers were displayed as a gray area centered on a white background representing the total area to be painted. The triangular offers rested on their base while square offers rested on a side. The white background allowed participants to visually appreciate the size of the shape with respect to the total area to be painted. This background was overlaid with a grid of thin light blue lines to ease comparison and make it possible for participants to assess if two offers of the same shape were indeed of the same size.

We chose the above shapes, sizes and prices carefully in order to minimize learning across repetitions on the part of subjects. We did want subjects to accumulate practice and familiarity with the task, but not to rely on past decisions to tackle later repetitions of the task.

### 3.2. Treatments

The menus were randomly generated within a set of constraints about their length, difficulty and presence or not of COs.

**Menu length.** We exposed the subjects to menus of 3 and 6 offers. The 3-menus make it easy to identify COs; comparability is less salient in 6-menus. Different menu lengths capture the effect of proliferation, an increase in the number of products and formats that make consumers more confused (Schwartz, 2003).

**Menu difficulty.** We varied the dispersion of unit prices of the offers within a menu. Unit prices were generated by a random process with mean

0.5 and variance  $\sigma^2 = 0.05$  for *easy menus*, which generated more distance between offers and hence an *easier* problem, or  $\sigma^2 = 0.01$  for *hard menus*, which generated closer offers and thus made it *harder* to identify the best one. This resulted in the best offer being on average 8.5 ECU cheaper than the worst offer in easy menus, 1.9 ECU cheaper in hard menus.

**Comparable offers.** Finally, menus could include COs, *i.e.* offers sharing the same format. Menus without COs were such that a given  $(s, a)$  combination would appear only once. Menus with *one* common format were such that two (and only two) offers featured the same  $(s, a)$  combination.<sup>1</sup>

We summarize the distribution of the menus according to the three criteria above in table 2 and provide a visual representation of each menu as shown to the subjects in our experiment in appendix F.

Table 2: Distribution of menus by length, difficulty and presence of comparable offers.

		Hard menu ( $\sigma^2 = 0.01$ )	Easy menu ( $\sigma^2 = 0.05$ )
3-menu	Without COs	9	9
	With COs	9	9
6-menu	Without COs	9	9
	With COs	9	9

Each participant was exposed to all the menus and menus were presented in a subject-specific random order to avoid order effects. The participants had up to two minutes to choose an offer from each menu and were forced to spend a minimum time of 10 seconds on each menu. The choice was performed by clicking on an offer - in which case it would be highlighted with a light green frame - and could be revised as many times as one wanted within the two minutes limit before being submitted as final. If no final choice was submitted within the time limit the last highlighted offer was submitted as the final choice; if no offer had been highlighted, then the participant received a payment of 3 euros for that trial, which was less than the minimum payment a participant could get even if he made the worst choice out of all our menus.

<sup>1</sup>For simplicity, we do not discuss an additional 8 menus that were presented to subjects. Those had 6 options and *two* common formats whereby one format occurred twice while another occurred thrice.

The participants were given feedback after each menu. This feedback reminded them of the price of their chosen offer, told them the resulting expenditure to paint  $A$ , as well as their payoff in terms of budget minus expenditure. They were not told whether they made the right choice or about the unit price of each offer. The participants were not given the possibility to automatically store and retrieve their payoffs from previous rounds, but were provided with pencil and paper and some did record their payoffs. After receiving feedback, they were given a new budget of 60 and shown the next menu. At the end of the experiment, one random period was selected as payoff-relevant and paid in cash.

### 3.3. Control tasks

After going through the menus participants were exposed to a set of non-incentivized visual perception and computational skills tasks to control for their ability to perform the main task. Three different sets of tasks were chosen:

1. *Shape size comparisons*: The participants were asked to give their estimate of the relative size of a shape (rectangles, circles and triangles) with respect to another. There were four such comparisons, each to be done within one minute.
2. *Mathematical operations*. The participants were asked to solve three sets of 10 operations (sum, subtraction, multiplication, divisions), with one minute for each set.
3. *Simple problems*: The participants were asked to solve four simple problems to test their understanding of the concept of area, of how an area relates to its dimensions, and how a number can be translated from one format to another (here, a currency). They had two minutes for each problem.

Once done with the control tasks, the participants filled in a short demographic questionnaire asking for age, gender, field of study, year of study, level of motivation in completing the tasks, and also what they thought the experiment was about (in order to control for demand effects). Finally, each participant individually drew a number from an urn and was paid according to the result of his purchasing decision in the menu corresponding to that number.

Our whole experiment was computerized. The experimental software, the menu generator and the script we used to collect and organize the raw

data were programmed in Python (Rossum and Python Software Foundation, 1995). The experimental software is available upon request. The English translation of the original German instructions is reported in appendix D.

#### 4. Results

Our experiment took place in June 2011 at the laboratory of the Max Planck Institute in Jena, Germany. The experiment involved 202 subjects over 8 sessions. All subjects were students. Table A.7 in appendix A gives some summary statistics: The average age of our subjects was 24, ranging from 18 to 47. 65% of our subjects were women. Payoffs were on average 11.44€ for an experiment that lasted about 45 minutes for the fastest and 1h15 for the slowest subject. Time spent on each menu declined over time (from an average of 36 seconds for the first choice to 16 for the last). Subjects took on average 5 seconds more to reach a decision among 6-menus than among 3-menus, and the presence of a common format also cut about one second from their decision time. However they did not consistently reach faster decisions when the menu was easy. Women obtained the same payoffs as men on average but performed less well than men in the simple problems and in the shape comparison task.

In the remainder of the paper we will analyze the results of the experiment, basing our analysis on four main questions. We will first check if subjects, on average, were not *naive*, and did benefit, in terms of optimal choices and payoffs, from the presence of COs. Second, we will investigate whether, at the aggregate level and controlling for other possible heuristics, subjects showed behavior consistent with the *attraction effect*, *i.e.*, gave a preference to the lowest priced of the COs over and above editing the dominated offer. Third, we will measure the strength of the attraction effect, both at the aggregate and the individual level, and, fourth, we will compare the estimated level of the attraction effect to the one that would have theoretically made our subjects best off.

##### 4.1. Did subjects benefit from the presence of comparable offers?

Table 3 shows how often consumers made the optimal choice depending on the length of the menu, its difficulty and whether the menu included or not two offers with the same format. As expected, consumers were significantly more likely to choose the best option when the menu was easy. For example, 45% of their choices among easy menus with three options and

no common format were optimal, compared with only 35% in hard 3-menus with no common format, which is barely more than if they had chosen at random among offers.

Subjects did benefit from the presence of a common format. They were significantly more accurate in their choice in menus with COs except in the case of hard menus with 6 options (see Table 3). For example, considering hard 3-menus, the number of optimal choices moved from about 35% in menus without COs to about 46% in menus with COs. This is because the presence of COs allows consumers to avoid the dominated option, thus reducing opportunities to make mistakes.

Table 3: Optimal choices by menu length, difficulty and presence of comparable offers.

		Hard menu			Easy menu		
		Mean	Std Dev	N	Mean	Std Dev	N
3-menu	Without COs	35.48%	47.86%	1818	45.27%	49.79%	1818
	With COs	46.42%*	49.89%	1818	64.63%*	47.82%	1818
6-menu	Without COs	27.89%	44.86%	1818	24.70%	43.14%	1818
	With COs	21.45%*	41.06%	1818	38.44%*	48.66%	1818

\* Difference significant at 5% level *vs.* row above, Fisher's exact test.

Higher accuracy in choices when a menu included COs led individuals to obtain higher payoffs in those menus, as shown in table 4.

Table 4: Payoffs by menu length, difficulty and presence of comparable offers.

		Hard menu			Easy menu		
		Mean	Std Dev	N	Mean	Std Dev	N
3-menu	Without COs	10.41	0.92	1818	11.02	4.56	1818
	With COs	10.45	0.96	1818	13.34*	3.96	1818
6-menu	Without COs	10.14	0.81	1818	11.97	4.11	1818
	With COs	10.04*	0.98	1818	13.84*	5.48	1818

\* Difference significant at 5% level *vs.* row above, Wilcoxon rank-sum test.

Participants obtained significantly higher payoffs and performed significantly better when a menu was easy and included COs, while the effect of the presence of COs in hard menu was either not significant or slightly negative. The presence of COs did not therefore benefit consumers when prices were already close together, but worked to the advantage of consumers when prices varied more widely among options. A regression analysis (not reported) on the determinants of payoffs shows that payoffs improved with experience, were correlated positively with performance in the



math task and negatively with age, and no gender or other individual effect was significant.

#### 4.2. Was there an attraction effect?

We now investigate the main question of our paper, that is, whether in our setting we observe an attraction effect. We therefore have to compare the characteristics of the dominant offer with the **non-comparable offers** (“**NCOs**”) and determine if, everything else equal, the dominant offer is chosen more often. We start by investigating summary statistics before performing regressions to explain the choices made by our subjects.

Table 5: Choice frequencies by menu length and difficulty in menus with comparable offers.

	Hard menus			Easy menus		
	Dominant	Dominated	NCOs	Dominant	Dominated	NCOs
3-menu with COs	53.63%	7.21%	39.11%	59.79%	4.51%	35.70%
(% Lowest priced)	55.56%	0.00%	55.56%*	55.56%*	0.00%	55.56%
6-menu with COs	25.63%	4.07%	17.58%	25.47%	2.97%	17.89%
(% Lowest priced)	33.33%*	0.00%	19.45%*	44.44%*	0.00%	13.89%*

\* Difference significant at 5% level *vs.* row above, Fisher’s exact test.

There were ties in price, so % lowest priced may sum to more than one. % is averaged over the 4 NCOs in 6-menus.

Table 5 shows that consumers took account of the presence of COs by discarding the dominated offer, as the dominated offer was seldom chosen. Consumers were therefore not *naive* overall: they were aware that some offers were easily comparable and discarded the dominated offer. Diversion away from the dominated offer was mainly towards the dominant offer rather than sales being equally distributed across the NCOs and the dominant offer. This led to the dominant offer being chosen more often than any other offer. However, while it was chosen about as often as it was the lowest priced in 3-menus, it was chosen less often than optimal in easy 6-menus. Indeed, while NCOs were avoided in 3-menus *vs.* optimal (chosen less often than they were lowest priced), this was not so in easy 6-menus, where the NCOs were chosen more often than optimal. Whether consumers truly favor the dominant offer is therefore not established when considering 6-menus.

When looking now at the individual level, figure 2 displays the number of subjects that chose the dominant offer 0, 1, 2 ... up to 9 times in the menus. This is disaggregated by menu length and difficulty. For example, 3 subjects chose the dominant offer 9 out of 9 times when exposed to easy

3-menus; 54 subjects chose the dominant offer 5 out of 9 times among hard 3-menus, and so on.

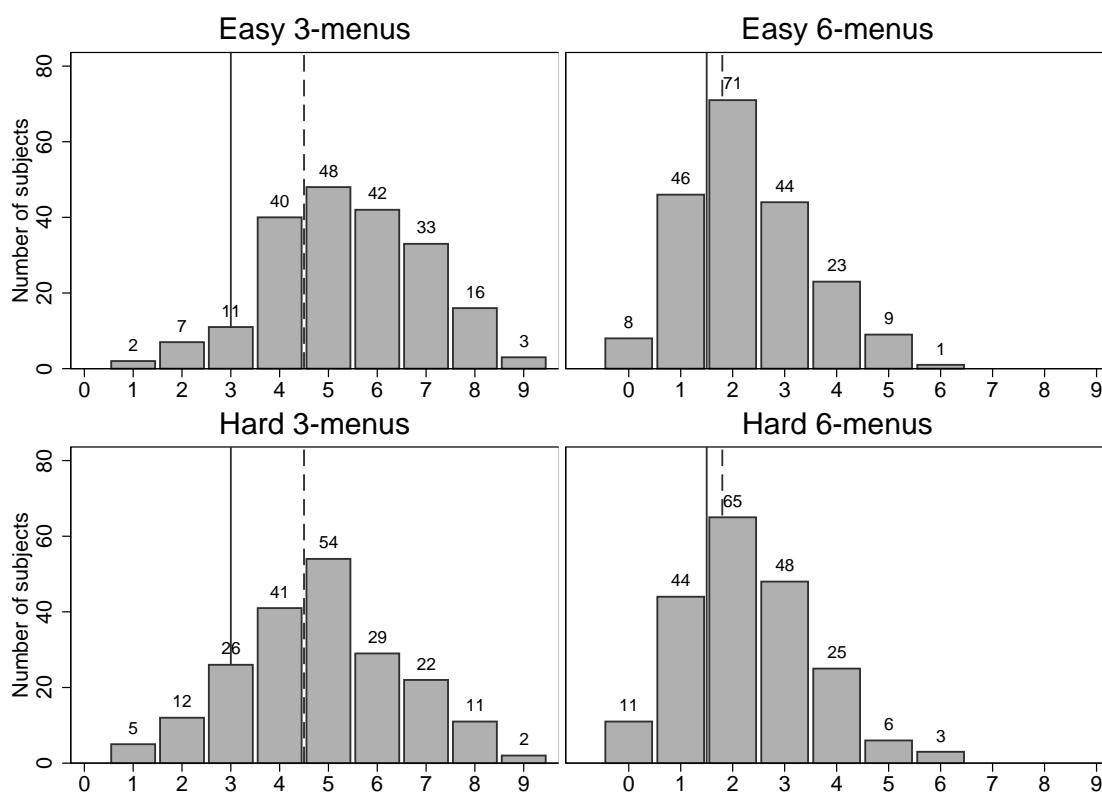


Figure 2: Distribution of the frequency of choice of the dominant offer, by menu length and difficulty.

In each graph, the solid reference line to the left indicates the frequency with which a consumer would choose the dominant offer if he chose among options at random. In the case of 3-menus, it would thus be chosen about a third of the time, that is, 3 times among the 9 hard 3-menus for example. In the case of 6-menus, it would be chosen a sixth of the time, that is between 1 and 2 times among the 9 hard 6-menus. The dashed reference line corresponds to the frequency with which the dominant offer would be chosen if a consumers followed a process of *dominance editing*, *i.e.*, eliminating the dominated offer and choosing at random between the remaining offers. This would lead to the dominant offer being chosen about 50% of the time in 3-menus and 20% of the time in 6-menus. Any subject with a frequency of choice of the dominant offer lower than the solid line can be said to penalize COs. Those with a frequency to the right of the dashed line can be said

to favor COs. We see that a significant number of subjects unambiguously favor the dominant offer in 3-menus, more for easy (70% of subjects) than for hard menus (58%). However, the number of such consumers is smaller in 6-menus (38% for easy and 40% for hard menus).

*How robust was the attraction effect?*

The above descriptive statistics show that the dominant COs were favored by many subjects in 3-menus but not consistently so in 6-menus. However, this could be the result of other characteristics of our menus such as the unit prices of the dominant COs, their shape, size or position in the menu. We hence need to perform a regression analysis to correct for such possible biases.

We performed a maximum likelihood estimation with three different models of consumer choice among options: the alternative-specific conditional logit and probit models and the mixed logit model which allows for preference heterogeneity for all the attributes. The dependent variable is the chosen option for each menu, *i.e.* one of the 3 or 6 available options. Options are identified by their position in the menu, unit price, shape, size, and by whether they are the dominant offer, dominated offer or a NCO in menus with COs. Since shapes that extend more broadly in space are preferred (see Krider et al., 2001), we create a variable coding shapes from most to least attractive: a triangle is assigned a value of 1, a square a value of 2 and a circle a value of 3 (We also ran the same regressions with each shape being a dummy variable; this did not influence results). The variable “position” is coded by lexicographic position in the menu, from 1 if the option is in the top left corner to 6 if it is in the bottom right corner in a 6-menu, otherwise to 3 for the option to the right in a 3-menu.

We crossed unit price with whether an option was a NCO or a dominated offer in a menu with COs so as to assess whether consumers penalized NCOs. We also cross unit price with menu specific variable (whether the menu was “hard” or “easy”), variables that are both menu and case specific (the order in which a specific menu was presented to an individual and the time that individual spent deciding on this menu), and case specific variables such as gender and scores in the control tasks to determine whether menu or individual characteristics make our subjects more or less sensitive to price signals (other individual characteristics such as age and educational background do not vary sufficiently in our sample).

We also introduce individual-specific variables along with alternative-specific variables to determine choice among alternatives. Our individual-

specific variables are scores in the mathematical, shape comparison and simple problems, along with gender, time spent choosing within a menu and motivation. We also consider whether facing a hard menu makes it more likely to favor the dominant offer because following a simple heuristic may be more likely if there appears to be little difference in prices between options. Finally, we consider how close the dominant offer was to the dominated offer with a variable coded as 1 if they were next to each other and on the same row in the menu. This is because it is easier to notice there are COs if COs are close together.

Formally, denote  $y_{ijm}^o$  the utility of option  $j$  in menu  $m$  for individual  $i$ , and denote  $y_{ijm} = 1$  if that option is chosen. We will have  $y_{ijm} = 1$  if  $y_{ijm}^o > y_{itm}^o$  for all  $t \neq j$  in menu  $m$ , 0 else. Latent utility  $y_{ijm}^o$  takes the form

$$\begin{aligned} y_{ijm}^o = & \alpha \times up_{jm} + \beta \times up_{jm} \times \text{NCO}_{jm} + \gamma \times up_{jm} \times \text{dominated}_{jm} + \dots \\ & \dots + \omega \times up_{jm} \times \Omega_i + \mu \times up_{jm} \times M_m + \lambda_o \times \Omega_i + \theta_o \times M_m + \dots \\ & \dots + \psi \times \text{shape}_{jm} + \delta \times \text{size}_{jm} + \phi \times \text{position}_{jm} + u_{ijm} \quad (1) \end{aligned}$$

$up_{jm}$  is the unit price of option  $j$  in menu  $m$ .  $\text{NCO}_{jm}$  takes value 1 if option  $j$  in menu  $m$  is a NCO, 0 else and variable  $\text{dominated}_{jm}$  takes value 1 if option  $j$  in menu  $m$  is dominated, 0 else.  $\Omega_i$  is a  $q \times 1$  vector of case-specific variables,  $\omega$  is a  $1 \times q$  vector of parameters,  $M_m$  is a  $h \times 1$  vector of menu-specific variables,  $\mu$  is a  $1 \times h$  vector of parameters.  $\lambda_o$  is a  $1 \times q$  vector of parameters, different for each type of alternative (dominated or NCO in a menu with COs). This is because case-specific variables may not be linked to the choice of each type of alternative in the same way. Similarly,  $\theta_o$  is a  $1 \times h$  vector of parameters translating the influence of menu characteristics on the choice of an alternative.  $u_{ijm}$  is a random variable of mean 0 that follows either a logistic or a normal distribution. We constrain  $\lambda_o$  and  $\theta_o$  to be the same for all four NCOs in 6-menus with COs. Model selection using the Akaike Information Criterion finds that all of the alternative-specific variables ought to be used, while only score in the shape comparison and in the mathematical tasks, along with gender and whether a menu is hard or easy, ought to be used as case-specific variables. Results are reported in table B.8 of appendix B.

Results show that subjects tend to avoid the dominated offer (negative parameter on “ $up \times dominated$ ”) and that subjects display a less pronounced but still significant aversion to NCOs in menus with COs. This is reflected

as a lower probability to choose a NCO for a given price (negative parameter on “ $up \times NCO$ ”). Choice is more sensitive to unit price when unit prices are close together than when they are far from each other (“ $up \times hard\ menu$ ”). In terms of alternative-specific variables, subjects tend to prefer “broader” shapes (“ $shape$ ”). There is no consistent tendency to prefer smaller sized options or equivalently, those with lower displayed prices (“ $size$ ”). Finally, options either at the beginning or the end of menus do not gain an advantage (“ $position$ ”).

When looking at the determinants of preference against NCOs and the dominated offer (table B.9 of appendix B), individuals that are worse at the shape comparison tasks are more likely to choose the dominated offer (“ $dominated \times shape\ task$ ”, higher scores in this task mean worse performance). Note that those same individuals are also less sensitive to differences in unit prices across options (“ $up \times shape\ task$ ”). Aversion to the NCOs is displayed in particular by women (“ $NCO \times female$ ”), and is also linked to whether the presence of COs is more obvious, that is, whether the COs are next to each other (“ $NCO \times close\ CS$ ”). Hard menus also encourage individuals to reject the NCOs (“ $NCO \times hard$ ”). It may be that subjects rely more on heuristics when it is more difficult to choose on the basis of unit prices.

Preference for the dominant offer *vs.* the NCOs is robust to various alternative specifications, for example when considering preference against the dominated offer or NCOs in terms of the parameter on a constant term associated with those offers rather than as an interaction term with the unit price (regressions not shown).

In conclusion, we confirm the presence of an attraction effect whereby dominant offers are preferred to NCOs over and above the deletion of the dominated offer. This effect is particularly prevalent when COs are presented close to each other (in 3-menus) and when differences in unit price across offers in a menu are small (in 6-menus). A consistent finding across all menus is that women are more sensitive to the attraction effect than men.

#### 4.3. *How strong was the attraction effect?*

Up to now we have shown that subjects did on average benefit from the presence of COs, and that on average they showed a modest attraction effect even after controlling for potential confounding factors due to the characteristics of the subjects, of the menus or of options in the menus. We now turn to quantifying the strength of this effect. Subjects’ choices are modeled

as if they penalize their estimate of the unit price of the NCOs and choose a NCO only if their penalized estimate of its unit price is lower than their estimate of the unit price of the dominant offer. A consumer who adopts a penalty level  $\pi > 0$  does not reject a NCO out of hand. Rather, he does not follow his first impression  $\hat{u}p$  of the unit price of the NCO, but revises that impression upwards  $((1 + \pi) \times \hat{u}p)$  when comparing it to his perception of the unit price of the dominant offer. In other terms, the consumer applies a certain dose of skepticism to his evaluation of offers that are expressed in uncommon terms and chooses to buy it only if it seems sufficiently better than the best of those offers that are expressed in a common format. Higher penalty  $\pi$  means a stronger attraction effect of the dominant offer: subjects choose the dominant offer more frequently and disregard NCOs, thus relying more on comparability and dominance and less on individual estimates of the value of each offer. More details about the optimal choice of penalty  $\pi$  and how we model it is shown in appendix C, where we show that a consumer ought to apply greater penalties to NCOs the more his assessment of unit prices is uncertain. Note that we are *not* stating here that consumers do indeed apply such penalty; we are only measuring the penalty that fits the data best as a way to *quantify* the strength of the attraction effect. The penalty  $\pi$  is merely a summary statistic representing an aspect of consumer choice.

From the regressions in table B.8, we saw that for a given unit price, subjects were on average less likely to choose a NCO. The parameter for sensitivity to unit price is  $\alpha + \beta = -19.0$  for NCOs in 3-menus but  $\alpha = -18.2$  for other offers except the dominated offer, so NCOs suffer a  $\beta/\alpha = 0.8/18.2 = 4.4\%$  price penalty compared to equivalent dominant offers (mixed logit regression). Aversion to NCOs is less pronounced in 6-menus than in 3-menus however ( $\beta/\alpha = 0.4/17.7 = 2.3\%$ ). Those penalties are not negligible: given the low variance in prices in our menus, such penalties can result in many cases in tipping the choice from (one of) the NCO(s) to the dominant offer.

#### 4.4. What decision rule did consumers follow?

The regressions above estimated penalty  $\pi$  for average behavior, but our subjects differ in terms of the penalty  $\pi$  that best fits their behavior. Figure 3 is an histogram of the penalty applied to NCOs, individual by individual, for 3-menus and for 6-menus. This was obtained by running regressions of table B.8 individual by individual. We see that the penalty is close to 0 for

many individuals, and the question is then to determine what rules of behavior those individuals appeared to follow. We do so exploiting the results of individual regressions and using as a criterion the significance of the parameters attached to unit prices ( $\alpha$ ), to the interaction of unit prices and the NCO dummy ( $\beta$ ), and to the interaction of unit prices with the dummy for the dominated option ( $\gamma$ ). The classification we adopt is summarized in Table 6.

Table 6: Parameter estimation and decision rules

	Parameters			Decision rule
	$\alpha$	$\beta$	$\gamma$	
	No	No	No	Random
	Yes	No	No	Naive
Significant?	-	No	Yes	Dominance editing
	-	Yes	Yes	Asymmetric dominance editing
	-	Yes	No	Dislike NCOs

If  $(\alpha, \beta, \gamma)$  were all non significant, then the individual was termed as choosing at *random* (parameters  $(\psi, \delta, \phi)$  may however have been significant). If  $\alpha$  was significant but not  $(\beta, \gamma)$  then the individuals was termed *naive*, that is, he is price sensitive but does not respond to the presence of a dominated offer. If  $\gamma$  was significant but not  $\beta$  then the individual was categorized as doing *dominance editing*. This does not require that  $\alpha$  be significant as a subject may do dominance editing and still be too confused to respond to prices. If both  $\gamma$  and  $\beta$  were significant, then the individual was said to do *asymmetric dominance editing*, that is, he is subject to the attraction effect. Finally, if  $\beta$  was significant but not  $\gamma$  then we simply say that the individual dislike NCOs but does not appear to do dominance editing. We believe that this category is close to following the asymmetric dominance rule because it seems unrealistic that an individual would be able to identify NCOs and yet not be able to choose correctly among the COs.

Figure 3 categorizes individuals by the level of the penalty  $\pi$  that best fits their behavior and by the rule that they were assigned to. We see that subjects who apply a significant penalty to NCOs are assigned to the asymmetric dominance rule and that they are only a minority of subjects in 3-menus (7% do asymmetric dominance editing, 17% dislike NCOs), and almost nonexistent in 6-menus (1% does asymmetric dominance editing, 10% dislike NCOs). As we saw, they are in sufficient number however to result in NCOs being chosen less often on average than COs with the same

unit price. Most of the subjects appear naive (32% in 3-menus and 63% in 6-menus) or random (33% in 3-menus and 25% in 6-menus).

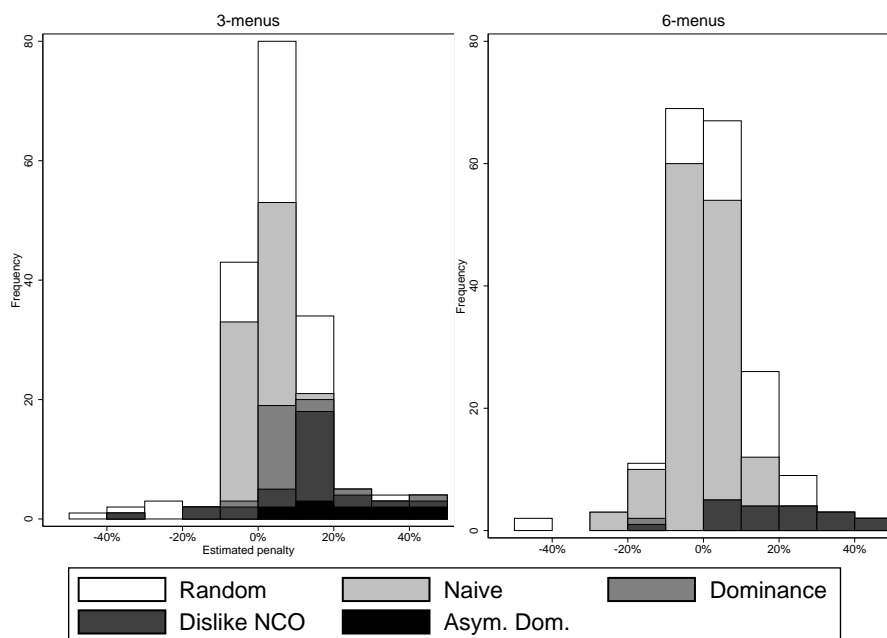


Figure 3: Distribution of subjects by estimated penalty and choice rule.

#### 4.5. Was the attraction effect strong enough?

The exercise carried out above allowed us to estimate the penalties consistent with behavior at the individual level. Using this information it is possible to know if subjects, given their degree of accuracy, could have been better off by imposing higher penalties. Intuitively, the less accurate a subject is in locating the best option, the more he should rely on the information provided by comparability and dominance. Formally, we estimate the level of accuracy of each individual and then calculate, for each level of accuracy, what penalty  $\pi$  maximizes payoffs.

We measure accuracy levels by exploiting choices in menus without COs. We assume that subjects' estimates of the unit price of each offer is a normally distributed random variable with mean the true value of the unit price and standard deviation  $\sigma^2$ . To each value of  $\sigma^2$  corresponds a payoff in menus without COs, with lower  $\sigma^2$  leading to higher payoffs. We thus measure the  $\sigma^2$  by average individual payoffs in menus without COs. We then simulate choice in menus with COs as explained in Appendix C. From graph C.5, we see the optimal penalty  $\pi$  a subject should use given



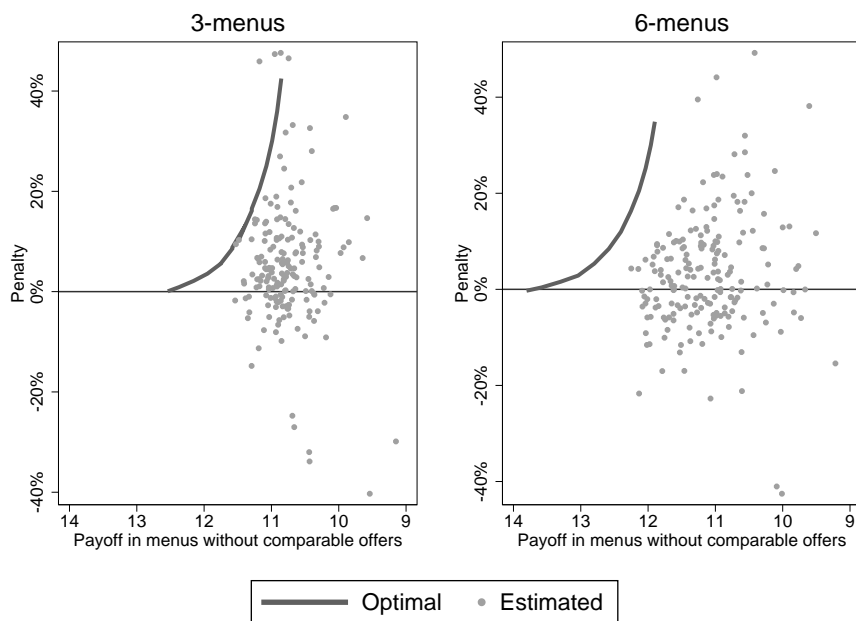


Figure 4: Optimal vs. estimated penalties in 3 and 6-menues.

Note: Dots represent estimated penalties against NCOs in ordinates vs. payoffs obtained from menus without COs in abscissas. The line represents the optimal penalty to be used as a function of accuracy (estimated from payoffs obtained from menus without COs).

a value of  $\sigma^2$  to maximize his payoff. Results are shown in Graph 4. The graph relates average payoffs obtained by subjects in menus without COs (on the horizontal axis), which is a measure of their accuracy in choices, to the penalty that best predicted their choice when choosing within menus with COs (on the vertical axis). We super-impose on this graph the optimal choice of penalty level for a consumer with the accuracy implied by his average payoff when faced with menus without COs. The graph can be read as follows: Consider point (11, 5%) in the plot for 3 menus, which represents a consumer who obtained a payoff of 11 in 3 menus without COs and with estimated penalty of 5% in 3 menus with COs. The curve indicates that a penalty of 20% would have been the optimal choice for this consumer.

Overall results show that subjects' behavior is consistent with much lower penalties than those that would have been optimal given their level of inaccuracy. Almost all subject favor dominant offers *much less* than what would be optimal for them. This can be interpreted as overconfidence in one's ability to make correct evaluations rather than relying on simple

heuristics.

In terms of penalties used by individuals, theory presented in this paper would predict that a rational subject who is beset by an inability to assess offers accurately ought to be using higher penalties than those used by subjects that are more accurate. This is because inaccurate subjects have more to gain from exploiting dominance and restricting their choices to COs (or, conversely, more to lose from relying on their judgment and disregarding comparability). As can be seen in the graph, we find no relation between payoffs when faced with menus without COs and the penalty used by the subject. Furthermore, almost all points on the graph are below the optimal line, meaning that penalties were lower than optimal. This is the case even for those subjects who were deemed to do asymmetric dominance editing. It may be that they chose penalties that were too low because they were overconfident in their own ability to choose the best offers based on signals alone, or they did not make the link between their accuracy and the penalty they ought to be using. We checked whether behavior was consistent over the course of the experiment by re-running estimates of the penalties used while excluding the first 20 menus each consumer was faced with. We did not find any sign that consumers learned to use higher penalties with experience.

## **5. Conclusion**

In this paper we introduced a novel experimental design to the literature on the attraction effect. The bulk of the existing literature relies on a very simple and abstract design. Despite the fact that the basic design has been applied to vastly different contexts and using different methods, the robustness of the effect has only recently been tested, with mostly negative results (Frederick et al., 2014; Yang and Lynn, 2014). Our design allows us to test the attraction effect in a more general setting, in which comparability and dominance relations must be discovered by subjects and are not imposed by the experimenter. Moreover, the presence of induced preferences allows us to abstract away from actual preferences, and to test the behavior of subjects against the benchmark of optimal behavior. Finally, our design allows us not only to check for the presence of an attraction effect in a more general setting, but it allows us to measure its strength under different conditions.

We find, on average and at the aggregate level, evidence for the existence of a small but significant attraction effect. Only about a fifth (in

6-menus) to a third (in 3-menus) of our subjects were found to edit out dominated offers. However, many of those also penalized NCOs which led to the aggregate behavior of our subjects being consistent with an attraction effect. This effect was strongest when COs were presented next to each other (*i.e.*, when dominance was more obvious) and when there was little difference in unit prices among offers. The effect was also stronger among women. Measuring this effect as a penalty given to the perceived price of the NCOs, the attraction effect varied from a small 2.3% in the case of menus with 6 offers, to a slightly higher 4.4%, in the case of menus with three offers, which is the standard case in the literature. While these might seem small numbers, it means that a product that is not comparable to other products can suffer up to a 4% price penalty compared to the dominant option among comparable offers. In several industries in which prices are close together, this can make a difference.

Since our experimental design exposes subjects to a variety of menus with COs and without COs, it allows us to use econometric techniques to measure the strength of the attraction effect at the individual level. For each individual, we hence estimated which model best fit their observed behavior. In menus with 3 options, one third of subjects were deemed to choose at random and another third were classified as *naive*, *i.e.* as price sensitive but not taking into account comparability and dominance. 11% were classified as *dominance editors*, *i.e.*, using dominance only to edit out the dominated offer; finally, 7% were asymmetric dominance editors, *i.e.* favoring the dominant offer over non-comparable offers. Another 17% avoided NCOs. Corresponding numbers for menus with 6 options were 25% random, 63% naive, 1% dominance editing, 1% asymmetric dominance editing and 10% avoiding NCOs.

As many heuristic-based choice rules, favoring dominant offers works best when the task are more complex, or, equivalently, when subjects are particularly bad at the task. In other words, the less one is accurate in his own judgments, the more one should rely on dominance to make his choice. We identified by means of simulations the optimal level by which one ought to penalize NCOs for each level of accuracy in the task. We found out that our subjects applied penalties to NCOs that were much lower than what would have been best given their level of accuracy. This is consistent with overconfidence – *i.e.*, subjects valuing their own judgment more than granted by their skills.

The implication of our findings could be summarized in three main state-

ments. First, we confirmed the existence of an attraction effect in an abstract setting in which comparability and dominance are not self-evident but must be looked for by subjects. Second, the effect – i.e., the preference for dominant offers – is not strong and is shown only by a minority of subjects. The effect is not particularly robust either, as it tends to disappear when the differences in unit prices between offers are larger - leading maybe to being more confident in one's own evaluations - and when the range of offers on display is extended - leading maybe to doubt the wisdom of ignoring a wider range of NCOs in favor of just one dominant offer. Third, subjects would benefit from valuing their skills less and relying on attraction more. Sometimes a simple heuristic is better than intricate reasoning and complex calculations, especially if you are not so good in either.

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### A. Summary statistics

Table A.7: Summary statistics

Variable	Mean	Median	Std Dev.	Skewness	Min	Max	N
Age	23.65	23.00	3.69	2.31	18.00	47.00	202
Gender	0.65	1.00	0.48	-0.64	0.00	1.00	202
Score in shape comparisons	0.25	0.25	0.10	0.35	0.05	0.58	201
Score in simple problems	2.78	3.00	0.96	-0.27	1.00	4.00	202
Score in mathematical tasks	20.92	21.50	2.93	-1.45	6.00	25.00	202
Payoff (in ECU)	11.44	11.48	0.41	-0.80	9.88	12.28	202
Time spent per menu	19.67	18.34	6.36	1.30	11.66	46.27	202

Note: For shape comparisons, higher score is worse.

### B. Regressions

Table B.8: Regressions, 3 and 6-menues

	(1) Logit 3-menues	(2) Probit 3-menues	(3) MixLogit 3-menues	(4) Logit 6-menues	(5) Probit 6-menues	(6) MixLogit 6-menues
main						
unit price (up)	-16.9907 *** (-8.57)	-15.7903 *** (-8.04)	-18.1501 *** (-9.37)	-17.0243 *** (-14.09)	-8.7179 *** (-10.22)	-17.7453 *** (-14.86)
up x hard menu	-2.3166 (-1.00)	-4.0012 + (-1.79)	-3.7198 (-1.52)	-4.8372* (-2.22)	-1.1908 (-1.18)	-5.8081 ** (-2.60)
up x shape task	11.4428 (1.63)	11.6082 + (1.74)	12.0139 + (1.74)	14.3898 *** (3.31)	7.2272 *** (3.86)	15.6980 *** (3.70)
position	0.0257 (1.37)	-0.0579* (-2.38)	0.0302 (1.55)	0.0294 *** (4.04)	0.0417 *** (6.19)	0.0261 *** (3.47)
shape	-0.3091 *** (-14.78)	-0.2985 *** (-12.54)	-0.3335 *** (-10.29)	-0.3178 *** (-19.56)	-0.1541 *** (-11.00)	-0.3671 *** (-9.75)
size	-0.0118 *** (-6.60)	-0.0124 *** (-6.62)	-0.0126 *** (-4.09)	-0.0066 *** (-4.96)	-0.0033 *** (-4.54)	-0.0078* (-2.03)
NCO x up	-0.7075 *** (-8.85)	-1.0146 *** (-8.11)	-0.8082 *** (-9.55)	-0.2677 ** (-3.09)	-0.0342 (-0.49)	-0.3673 *** (-4.14)
dominated x up	-3.6977 *** (-24.61)	-3.0037 *** (-20.88)	-3.6352 *** (-24.17)	-3.1758 *** (-16.38)	-1.9696 *** (-12.34)	-3.1583 *** (-16.28)
SD						
shape			0.3409 *** (11.29)			0.4705 *** (15.06)
size			-0.0348 *** (-13.09)			-0.0523 *** (-16.66)
N menus	7235	7235	7235	7236	7236	7236
ll	-6749.7897	-6731.8500	-6573.5627	-1.179e + 04	-1.171e + 04	-1.125e + 04
df	8	8	10	8	8	10

t statistics in parentheses

+ p < 0.10, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

The base outcome is the dominant offer.



Table B.9: Regressions, 3 and 6-menus

	(1) Logit 3-menus	(2) Probit 3-menus	(3) MixLogit 3-menus	(4) Logit 6-menus	(5) Probit 6-menus	(6) MixLogit 6-menus
main						
unit price (up)	-18.1421 *** (-8.73)	-17.5333 *** (-8.22)	-19.4558 *** (-9.64)	-16.8383 *** (-12.07)	-8.6691 *** (-9.07)	-17.4794 *** (-12.96)
up x hard	2.5259 (0.99)	1.3562 (0.59)	1.5624 (0.59)	-3.4809 (-1.56)	-0.9026 (-0.86)	-4.4468+ (-1.95)
up x shape task	11.5431 (1.61)	9.3121 (1.46)	11.6306+ (1.65)	14.1061 ** (3.11)	6.8876 *** (3.60)	15.5741 *** (3.56)
position	-0.0744 ** (-2.72)	-0.0787 ** (-3.04)	-0.0753 ** (-2.63)	0.0280 *** (3.82)	0.0403 *** (5.79)	0.0248 ** (3.29)
shape	-0.3489 *** (-14.72)	-0.3565 *** (-10.86)	-0.3811 *** (-10.91)	-0.3246 *** (-19.77)	-0.1580 *** (-10.48)	-0.3730 *** (-9.87)
size	-0.0131 *** (-7.01)	-0.0124 *** (-6.49)	-0.0137 *** (-4.30)	-0.0059 *** (-4.42)	-0.0031 *** (-4.19)	-0.0070+ (-1.85)
NCO x up	0.3675 (1.35)	0.9687 ** (3.24)	0.4047 (1.41)	-0.8231 (-1.27)	-0.0169 (-0.05)	-1.0496 (-1.57)
NCO x shape task	-0.1878 (-0.75)	-0.5172* (-1.98)	-0.2397 (-0.88)	0.3783 (0.92)	0.2082 (0.98)	0.3814 (0.90)
NCO x math	0.0205 *** (5.06)	0.0074+ (1.75)	0.0244 *** (5.69)	0.0221+ (1.85)	0.0034 (0.51)	0.0246* (1.98)
NCO x female	-0.1124* (-1.97)	-0.0969+ (-1.69)	-0.1805 ** (-2.89)	-0.2195* (-2.52)	-0.1348 ** (-2.89)	-0.2409 ** (-2.67)
NCO x hard menu	-0.1248* (-2.14)	-0.1895 ** (-3.19)	-0.1531* (-2.51)	-0.3608 *** (-4.03)	-0.1396 ** (-2.87)	-0.3380 *** (-3.69)
NCO x close CS	-0.9181 *** (-7.88)	-0.9300 *** (-6.46)	-1.0102 *** (-8.21)	0.2848 ** (2.66)	0.1008+ (1.79)	0.3239 ** (2.93)
dominated x up	-3.2344 *** (-7.88)	-2.6036 *** (-8.80)	-3.2288 *** (-7.70)	-6.2061 *** (-3.70)	-2.7343 ** (-3.18)	-6.2371 *** (-3.68)
dominated x shape task	0.6004+ (1.81)	0.4948+ (1.80)	0.6102+ (1.80)	2.8257 ** (2.74)	1.2836 ** (2.78)	2.8056 ** (2.92)
dominated x math	0.0090+ (1.86)	-0.0061 (-1.17)	0.0130 ** (2.66)	0.0657* (1.99)	0.0186 (1.17)	0.0671* (2.06)
dominated x female	-0.2191 ** (-2.94)	-0.1665 ** (-2.74)	-0.2745 *** (-3.50)	-0.8407 *** (-4.27)	-0.4491 *** (-4.38)	-0.8422 *** (-4.32)
dominated x hard	-0.2516 ** (-3.27)	-0.3095 *** (-4.54)	-0.3068 *** (-3.82)	-0.2204 (-1.11)	-0.1529 (-1.45)	-0.2247 (-1.12)
dominated x close CS	-0.3772+ (-1.75)	-0.1313 (-0.86)	-0.3675+ (-1.69)	0.0776 (0.30)	0.0336 (0.26)	0.1001 (0.38)
SD						
shape			0.3504 *** (11.45)			0.4722 *** (15.08)
size			-0.0357 *** (-13.21)			-0.0522 *** (-16.67)
N menus	7235	7235	7235	7236	7236	7236
ll	-6696	-6678	-6512	-1.176e + 04	-1.169e + 04	-1.122e + 04
df	18	18	20	18	18	20

t statistics in parentheses

+ p < 0.10, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

The base outcome is the dominant offer.

### C. Optimal penalty simulations

In this we detail the method followed to compute the link between inaccuracy and the optimal penalty that a consumer should have assigned to NCOs.

Consumers may take into account a number of criteria when choosing, including the perceived unit prices of offers, their shape, their position and whether they are expressed in terms of a common format. We assume here, for the sake of simplicity, that consumers only use two pieces of information: their estimation of the unit price of offers, and dominance relations.

Denote  $\hat{u}p_{ij} = up_i + e_{ij}$  the perceived unit price of offer  $i$  by consumer  $j$ .  $up_i$  is the true unit price of offer  $i$  while  $e_{ij}$  is an error term, assumed independent across offers in a menu and across consumers. The standard deviation  $\sigma_i^2$  of the error term depends on the consumer's accuracy and on how difficult it is to compare offers *across* formats.

A consumer who only considers perceived unit prices in his choice will choose the offer with the lowest perceived unit price and will not consider whether that offer may be dominated by another offer expressed in terms of the same format. This is the best rule of action in the absence of dominance information, or if the consumer is unable to assess the comparability of offers. We call this the *naive* rule.

A consumer who makes only use of dominance information, on the other hand, will eliminate all NCO and always choose the dominant offer. This is the best rule of action for a consumer that is extremely inaccurate, that is if  $\sigma_i^2 \rightarrow \infty$ .

A consumer who makes use of both dominance and perceived unit prices, but does not favor the dominant offer *per se* will first eliminate the dominated offer and then compare the dominant offer with the remaining offers, basing her judgment on the perceived unit price. This is dominance editing.

The best rule of action is in-between the two extreme above for a wide range of realistic inaccuracy levels. A consumer is better off first determining the dominant offer, denoted  $k \equiv \arg \min_{i \in COs} p_i$  if there are COs and then choose product  $l(\pi_j) = \arg \min_{i \notin COs} (\hat{u}p_k, \hat{u}p_i \times (1 + \pi_j))$ . The perceived price of NCOs is multiplied by  $\pi_j$ , with  $\pi_j$  being a penalty on NCOs. The optimal choice of penalty is  $\pi_j^* = \arg \min_{\pi_j} E(up_{l(\pi_j)})$ . This is more than one, that is, a consumer is always better off applying a penalty on NCOs.

A consumer's optimal choice of penalty level depends on his accuracy in assessing offers, with less accurate consumers being better off adopting

higher penalties. For example, assuming prices  $a$  and  $b$  are drawn from the same distribution, a consumer with a budget of 60 who makes considerable mistakes obtains  $60 - E(a)$  in expectation if he considers only his perception of prices in his decision (he chooses essentially at random). This is less than  $60 - E(\min(a, b))$ , his expected payoff if he always chooses the dominant offer. He is therefore always better off never choosing NCOs, thus effectively adopting an infinite penalty  $\pi$  on NCOs. At the other extreme, penalty  $\pi_j = 0$  is a special case which corresponds to eliminating the dominated offer and choosing based on the signals from the remaining offers only. This is optimal only if the consumer is perfectly accurate in choosing among offers.

We performed simulations with Octave (Eaton, 2002) to determine the optimal penalty  $\pi^*$  to use as a function of consumers' accuracy. The program is available upon request. We modeled  $e_{ij}$  as following a normal distribution with mean zero and variance  $\sigma^2$ , which we varied between 0 and 0.15. In the same way as in our experiment, unit prices  $up_i$  followed a normal distribution with mean 0.5 and variance 0.01 (hard menus), and 0.05 (easy menus) and the budget was set to 60 as in the experiment. Consumer choice was simulated if consumers only considers price signals in their decision ("naive") as well as when they apply the optimal penalty ("Asymmetric Dominance").

Figure C.5 shows payoffs depending on what choice process the consumer uses and as a function of his accuracy ("error") over 3- and 6-menus, easy and hard. Always choosing the dominant offer always obtains the same payoff, the minimum of two random variables. The naive rule performs worse as the consumer becomes less precise. Its minimum is reached when a consumer chooses at random, whereby he obtains  $60 - 100 \times 0.5 = 10 \text{ ECU}$  on average. Asymmetric dominance editing looks like the naive rule for highly accurate consumers and converges towards always choosing the dominant offer for consumers who are very imprecise and thus use high penalties on NCOs. The graphs also show, on a separate scale, the optimal penalty  $\pi^*$  for every values of the error term. Note that optimal penalties are higher in hard menus for a given error term. This is because even a rather accurate consumer will be confused when offers are close together so that he ought to apply high penalties to NCOs to avoid mistakes.

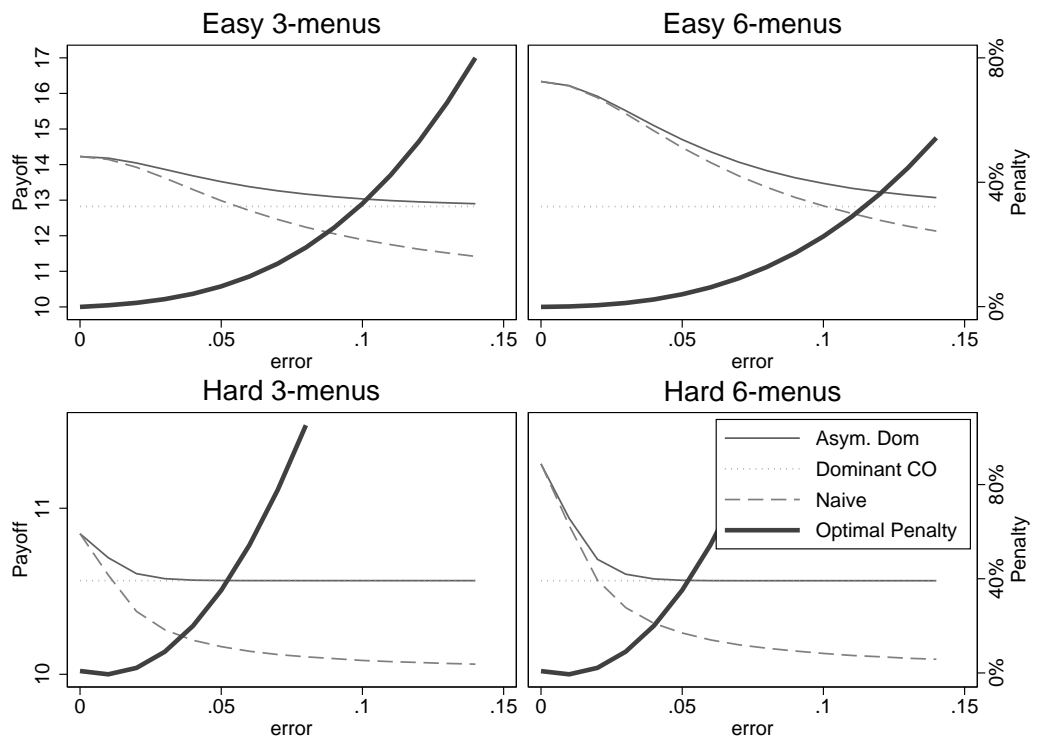


Figure C.5: Consumer payoffs by choice rules and optimal penalties, by menu length and difficulty

## D. Experimental Instructions

Welcome to this experiment!

### 1. General rules/proceedings

During the experiment you are not allowed to talk to other participants. Please switch off your mobile phone. If you have any questions, please raise your hand and refer directly to the experimenters. One of the experimenters will then answer your question in private.

Please read these instructions carefully, as your payment will depend on the decisions that you make during this experiment.

On your desk you will find this instruction sheet, a pen, paper, and a receipt. You can take notes at any time; the receipt will only be used for your personal payment at the end of this experiment. During the experiment, we will not speak of Euro but use ECU (*Experimental Currency Units*) as a currency instead.

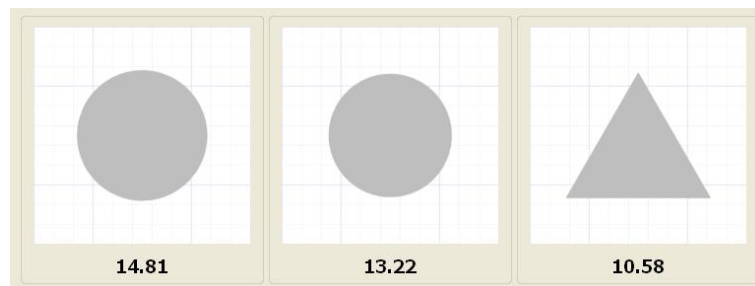
The amount of ECU you earn during the experiment will be converted into Euro at the end of the experiment using the following conversion rate:  $\text{€}0.8 = 1 \text{ ECU}$ . For example, if your earnings amount to 12 ECU, you will receive €9.60. The final payment will be rounded up to the nearest 10 cents.

All participants will remain anonymous, i.e. after the experiment, no one –neither other participants nor the experimenters – will be able to associate your personal information with your decisions or your earnings.

### 2. The Experiment

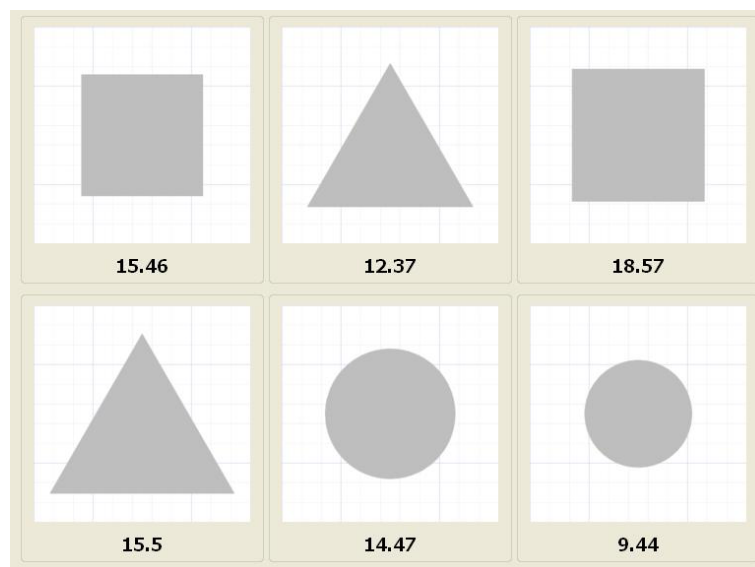
This experiment consists of several tasks. At the beginning of each task, you are endowed with 60 ECU to buy grey paint from a shop in order to paint a specific, given area. Each shop gives a choice between various offers. Each of them is structured in the same way, i.e. it consists of a given shape and its corresponding price. In each offer, the grey shape on display represents the fraction of the total area (which needs to be painted) that you can paint with this specific offer.

**Figure 1** presents the three different offers you are given by a shop. **Figure 2** shows the six different offers made by another shop. The total area which you have to paint is represented by the white square surrounding each of the shapes. The light grid is provided to help you with your task.



**Figure 1**

Once you have chosen one of the offers and submitted your choice, the computer will calculate how much paint you need to cover the entire area (the white square) and will also buy the colour for you. The amount of your initial endowment that you do not spend for buying the paint is yours to keep.

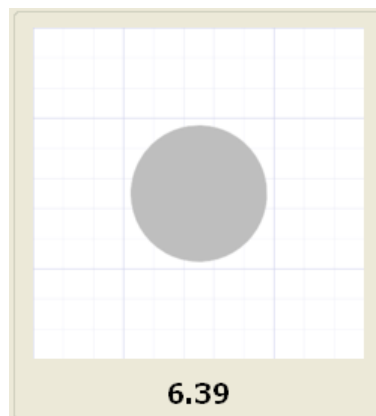


**Figure 2**

### 3. Examples

The following examples should help you understand how the computations made by the computer work in detail.

Suppose you are confronted with the offers in **Figure 3** and the total area you are supposed to paint is  $100m^2$ .



**Figure 3**

In order to paint the area covered by the grey circle, you pay **6.39 ECU**.

However, this circle only covers an area of  $13m^2$ . As you need to paint a square which is  $100m^2$  in size, the computer calculates how much paint you actually need for this offer.

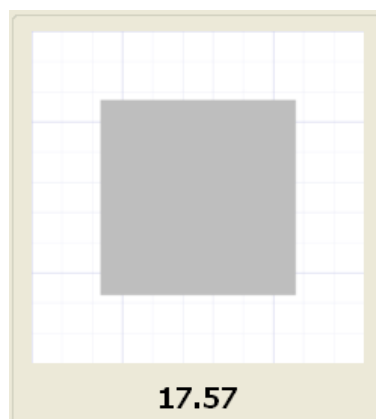
In this case, this amounts to  $100/13 = 7.7$  paint buckets.

Hence, the total price you have to pay for painting the white square amounts to:

$$6.39 \times 7.7 = 49.2 \text{ ECU}$$

Keeping in mind your initial endowment of **60 ECU**, your earnings result as follows:

$$60 - 49.2 = 10.8 \text{ ECU.}$$



**Figure 4**

Consider now the offer in **Figure 4**. In order to paint the area covered by the grey square, you pay **17.57 ECU**.

However, this square only covers an area of  $34m^2$ . As you need to paint a square which is  $100m^2$  in size, the computer calculates how much paint you actually need for this offer.

In this case, this amounts to  $100/34 = 2.94$  paint buckets.

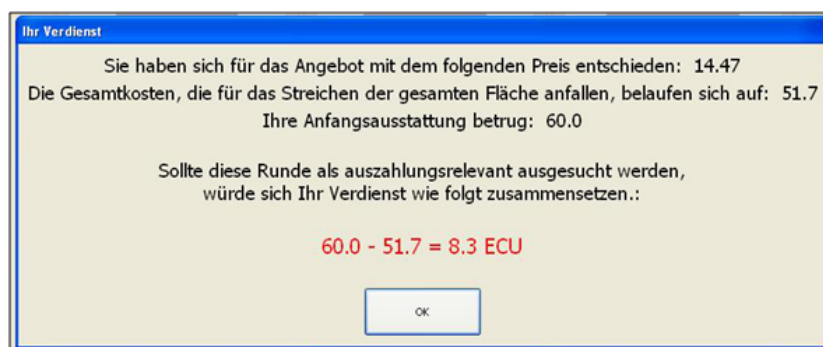
Hence, the total price you have to pay for painting the white square amounts to:

$$17.57 \times 2.94 = \mathbf{51.7 \text{ ECU}}$$

Keeping in mind your initial endowment of **60 ECU**, your earnings result as follows:

$$60 - 51.7 = \mathbf{8.3 \text{ ECU}}$$

A separate pop-up dialog will automatically appear and will tell you the results of each task (see **Figure 5**) including your possible earning of this task; clicking 'OK' will start the next task.



**Figure 5**

You have at most two minutes for each task and can only submit a choice at least ten seconds after you started it. In case you made a choice after two minutes (i.e. clicked on one of the offers), but failed to submit the offer in time (by clicking 'OK'), the computer will nevertheless treat your selected offer as if you had submitted it. In case you did not make any choice after two minutes, you will be paid 3 ECU for this task (if this task is chosen as relevant for your payment).

You will be faced with 36 different tasks with 3 offers, and 44 with 6 offers. At the end of the experiment, only one of the 80 tasks will be randomly selected and you will be paid according to your earnings in this specific task.



*4. Questionnaire and Quiz*

Once you completed the 80 tasks, you will be asked to answer a few questions:

1. Please fill in a simple questionnaire. The answers you submit will be treated confidentially and no data will be disclosed.
2. Please compare different shapes with each other. You have one minute for each of the four comparisons.
3. Please perform some computations. There will be 3 sets of computations and you will have one minute for each.
4. Please solve a number of problems. There will be 4 problems, and you have 2 minutes for each.

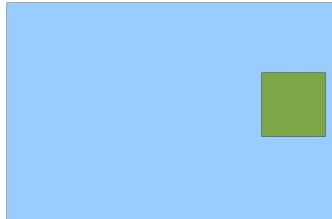
After you completed all the tasks, please raise your hand to signal the experimenters that you finished the experiment and we can start with your payment. One of the experiments will then come to your cabin and ask you to draw a chip out of a bag with 80 chips (which are numbered 1 to 80). This chip will correspond to the task that you will be paid for. The experiment will then enter the number of the chip on your screen and the computer will automatically tell you, how much you earned in this task. Please fill in this amount as well as your name and signature the receipt that you find on your desk. Afterwards, please raise your hand to signal the experimenters that you are finished filling out your receipt. After you received your payment, the experiment is finished and you can leave the laboratory.

**Thank you very much for participating in this experiment!**

**E. Control tasks**

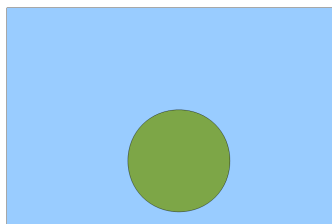
*E.1. Shape comparison task*

- Question 1 (one minute)



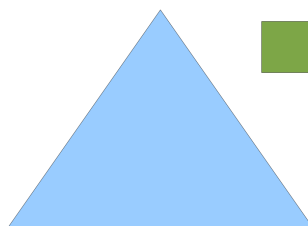
How many times bigger is the area covered by the rectangle compared to the area covered by the square?

- Question 2 (one minute)



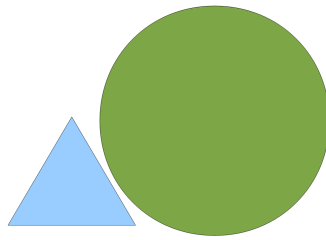
How many times bigger is the area covered by the rectangle compared to the area covered by the circle?

- Question 3 (one minute)



How many times bigger is the area covered by the triangle compared to the area covered by the square?

- Question 4 (one minute)



How many times bigger is the area covered by the circle compared to the area covered by the triangle?

*E.2. Simple computations*

Time given: 1 minute, allow for not answering some questions, put all questions at same time to be answered in box next to each problem

- Question 1 (one minute)

$88 - 45$ ;  $10 + 30$ ;  $57 - 43$ ;  $9 \times 6$ ;  $3 \times 7$ ;  $8 + 45$ ;  $65 - 11$ ;  $2 \times 5$ ;  $8 + 12$ .

- Question 2 (one minute)

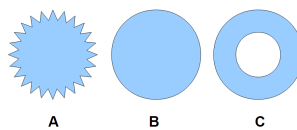
$276 + 177$ ;  $12 / 4$ ;  $106 - 85$ ;  $18 / 6$ ;  $4 \times 10$ ;  $188 - 64$ ;  $106 + 122$ ;  $8 \times 7$ .

- Question 3 (one minute)

$70/10$ ;  $892-179$ ;  $8*8$ ;  $363+93$ ;  $77/11$ ;  $9*5$ ;  $642-193$ ;  $265+108$ .

*E.3. Problems*

- Problem 1 (two minutes)



Which of the figures has the largest colored area?

- Problem 2 (two minutes)

A pizzeria serves two round pizzas of the same thickness in different sizes. The smaller one has a diameter of 30 cm and costs 3 euros. The larger one has a diameter of 40 cm and costs 4 euros.

Which pizza is better value for money?

1. The smaller one
2. The larger one
3. Both are the same value for money

- Problem 3 (two minutes)

Nick wants to pave the rectangular patio of his new house. The patio has length 5 metres and width 3 metres. He needs 80 bricks per square metre. How many bricks does Nick needs for the whole patio?

- Problem 4 (two minutes)

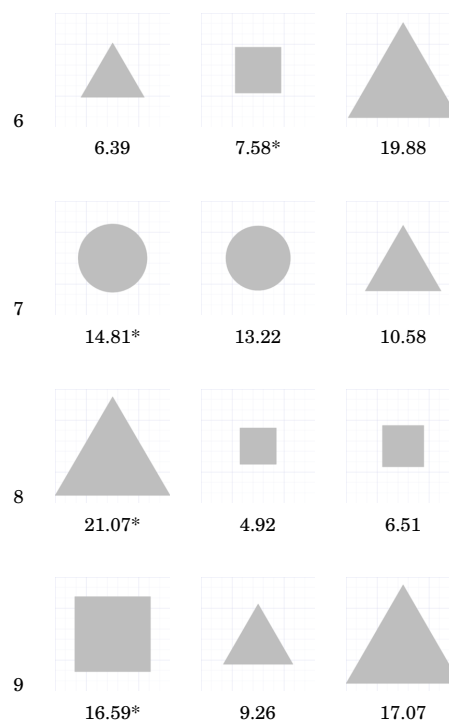
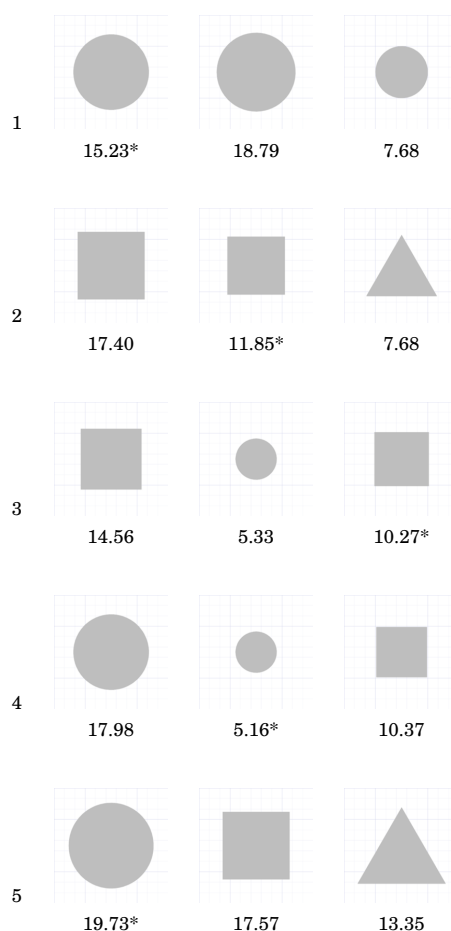
You can buy \$1.40 with one euro. How many dollars can you buy with 50 euros?

**F. Menus used in the experiment**

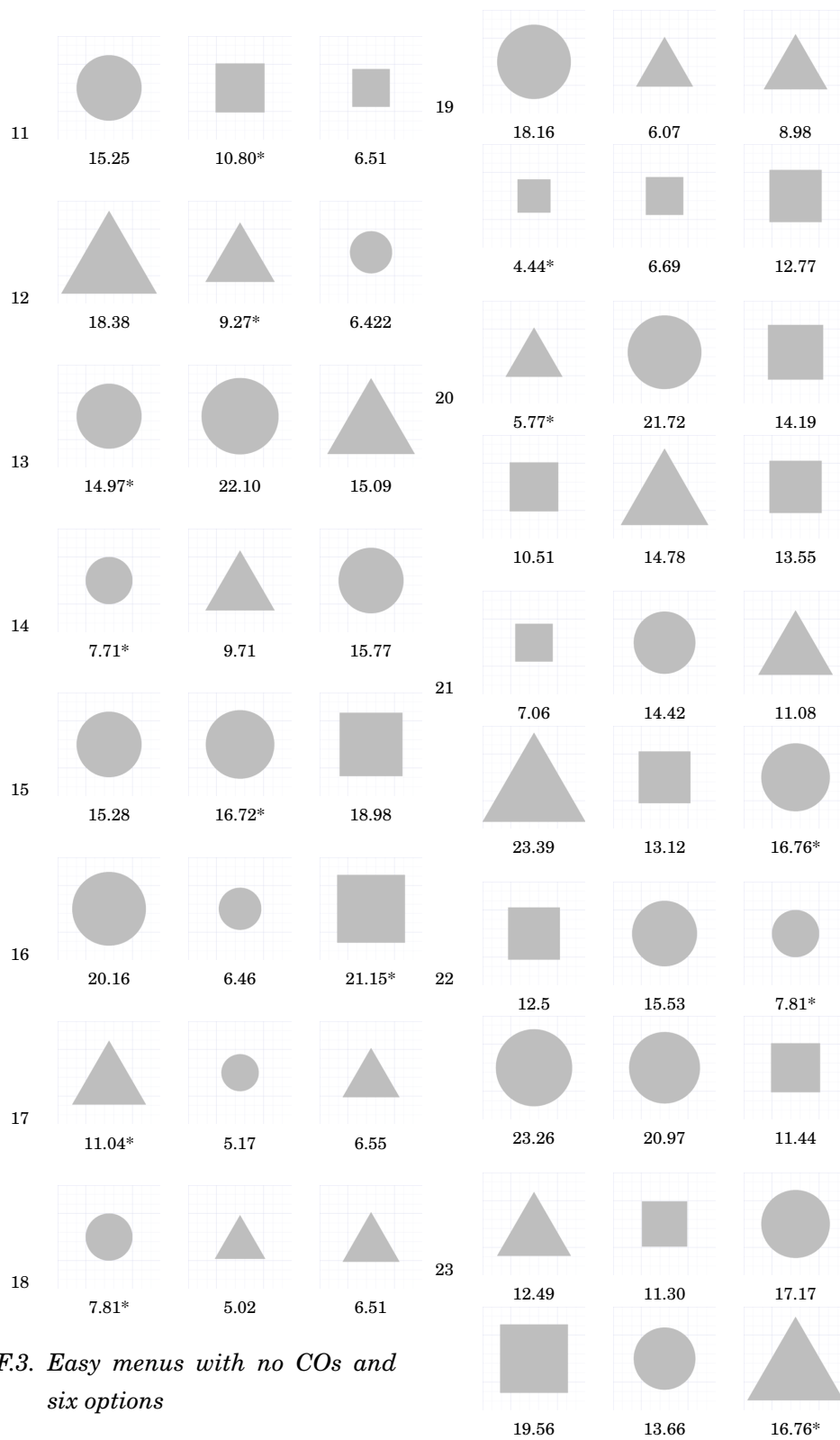
We list below all the menus used in the experiments. The optimal choice in each menu - the one minimizing expenditure - is indicated by an asterisk.

Menus 1 to 18 have three options and no COs, menus 19 to 36 have six options and no COs, menus 37 to 54 have three options and COs and menus 55 to 72 have six options and COs. In each of those categories, the first half are easy menus while the second half are hard. The reader can check that with the exception of menu 25 there are no offers with the same shape but smaller size and higher price than another option in the same menu. This means that other than among COs there were no obviously dominated offers in our menus. It was only in offers of the same size and shape that the dominance relation was obvious.

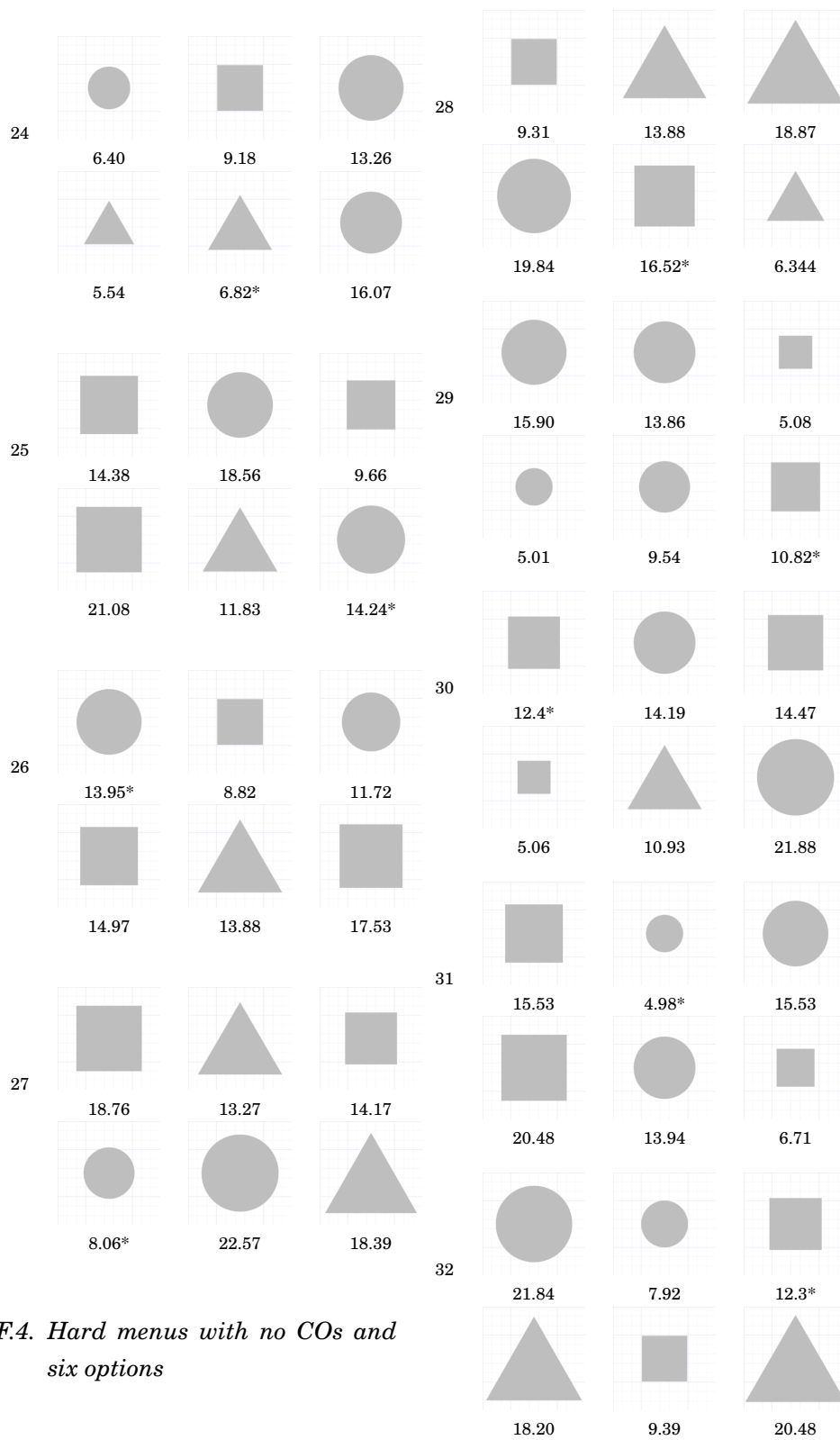
*F.1. Easy menus with no COs and three options*



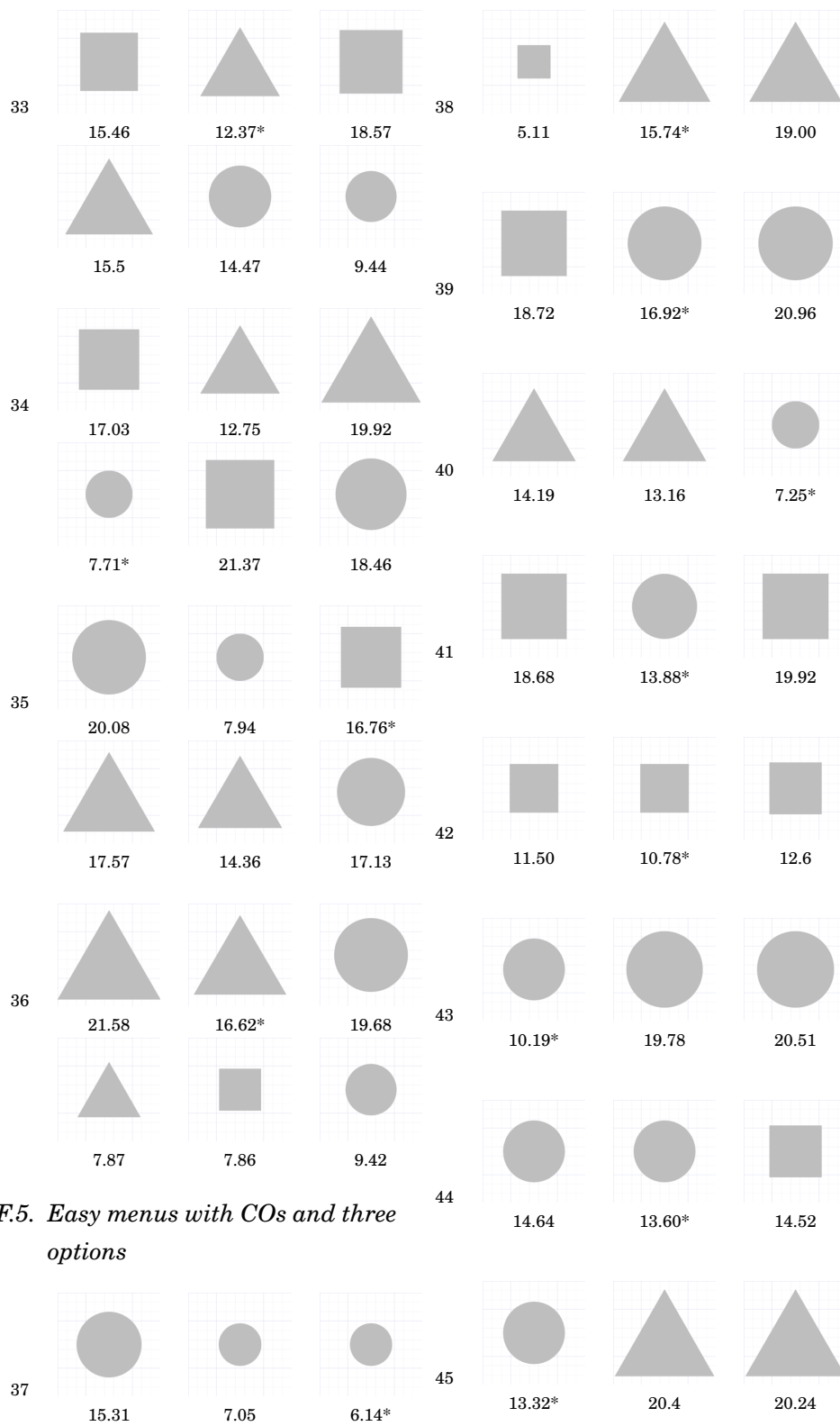
*F.2. Hard menus with no COs and three options*



*F.3. Easy menus with no COs and six options*



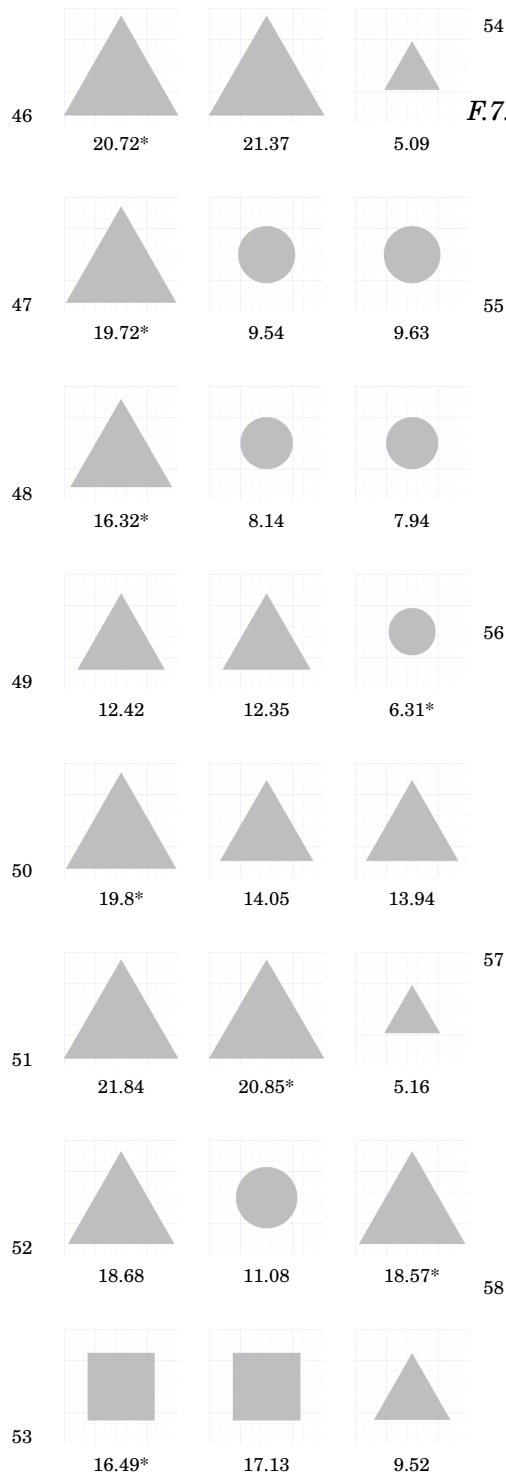
F.4. Hard menus with no COs and six options



F.5. Easy menus with COs and three options



*F.6. Hard menus with COs and three options*



*F.7. Easy menus with COs and six options*

