

# Preference Change through Choice

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## INTRODUCTION

In an unforgettable performance by the English comedian Eddie Izzard, he portrays the Spanish Inquisition as conducted by the wimpy Anglican Church. Playing a supremely domesticated inquisitor, he offers

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the accused heretics the intriguing choice between “Cake or Death?” Apart from the hilariousness of this scene, the social scientist can appreciate it as one of the last safe havens for neoclassical economics, rational choice, and expected utility theory, because people really and truly have stable and identifiable preferences to help them decide between cake and death. There is simply no amount of social-psychology shenanigans that could push this preference around (no matter how little Festinger would pay people for choosing the death sentence, it would not generate enough cognitive dissonance to sway anyone) (Festinger, 1957).

But as soon as we move away from cushy comedic inquisitions towards actual real-world decisions, the situation becomes considerably murkier. In another of his shows, Izzard delivers a brilliant caricature of people’s vocational choices. “Vocation, you got to *go* for it, you can’t just fall into it,” he says. And then he illustrates his point with the fervent calls of the taxidermist and the beekeeper. “You gotta *want* to be a Taxidermist! Yes! I *want* to fill animals with sand! I want to get more sand into an animal than anyone has ever done before /.../ You got to *want* to be a beekeeper! I *want* to be a beekeeper! I *wanna* keep bees! Don’t wanna let them get away; I *wanna* keep them! They have too much freedom.” Here, much of the comedy lies in how difficult it is for us to imagine how, from all the imaginable options available, someone actually decides to become a taxidermist or beekeeper<sup>1</sup>. It is not that there is a reason to believe that stuffing sand into animals or stealing honey from bees are particularly unpleasant occupations to have (except perhaps as Izzard notes, the part where the beekeeper realizes: “oh my god, I’m covered in beeeeeees”), but rather that it seems so unlikely that all those utility calculations line up properly at the many, many decision points that takes a person from a teensy toddler to a towering taxidermist. In contrast to the previous example, and barring ethical concerns, psychologists would have a field day framing and anchoring and frivolously fiddling with vocational trajectories like this.

In this chapter we look at examples of research showing how choice feedback effects may explain people’s progression through a decision space, and we set out a number of desiderata for this type of research to meet in order to fully illuminate the impact of choices in real life.

Work by Ariely, Loewenstein and Prelec (2003, 2006) strongly suggests that arbitrary and irrelevant factors can not only influence participants in their assessment of the utility of different goods (such as when

<sup>1</sup>For all you closeted friends of the conjunction fallacy out there, we have to admit to ourselves that somehow it does not seem all that unlikely that a taxidermist would double as a beekeeper, but consulting the oracle of the Internet, we could only find reference to one such person; a William Jones Weeks, who lived in the hamlet of Yaphank in Longwood Community, Long Island, NY, and who was an inventor, scientist, horticulturalist, beekeeper, and taxidermist).

rumination on the digits of their social security number leads participants to create wildly different anchors for how much they are willing to pay for a bottle of wine), but that these factors can be maintained through longer decision trajectories, and creating a form of “coherent arbitrariness” (i.e. stable market patterns of revealed preferences) (Ariely, 2008). In the words of Norton and Ariely: “These results demonstrate a kind of self-herding, in which people observe their past behavior, infer some amount of utility and act in accordance with the inference of utility, despite the fact that this behavior can be based not on the initial choice driven by hedonic utility but on any host of trivial situational factors that impacted the first decision” (Ariely & Norton, 2008, p.14).

Recent research on the monetary valuation of pain has yielded similar conclusions (Kurniawan, Seymour, Vlaev et al., 2010; Vlaev, Seymour, Dolan & Chater, 2009). Given a fixed amount of “cash in hand,” and two differing levels of pain to which they may be subjected, people will consistently pay more to avoid (or, in a motor task, take more trouble to avoid) the worse pain. But if the level of pains, or the amount of available money, is varied from one person to the next, the monetary “valuations” of any specific pain turn out to be wildly unstable, in highly predictable ways. I might be willing to pay just 20p to avoid a specific pain in one part of the experiment; but might cheerfully pay 40p to avoid the very same pain a few minutes later. One interpretation of these results, consonant with Ariely’s explanation above, is that people have essentially no idea how to relate money and subjective experience; but they do want to be consistent, as far as possible. But such consistency cannot easily be enforced *across* contexts, perhaps because people have no *absolute* representation of subjective experiences (whether pains or other sensory inputs) (Chater & Vlaev, 2011; Stewart, Brown & Chater, 2005).

This, we surmise, is the first aspect to look out for in a framework to deal with feedback effects of choice: that it present itself as a serious contender to deal with both contextualized and realistically scaled time series of choices (even if the long-term effects are mainly implied).

But a limitation of the demonstrations described above is that it remains possible that the arbitrary factors might have fizzled if the starting state had concerned preferences of greater importance for the participants. In such a case, presumably the self-observation that propels self-herding of arbitrary preferences would instead work in concert with the original utility-based choice, making it even more resistant to contextual effects in further instances.

Thus, the second aspect to look out for in relation to feedback effects of choice is a demonstration of arbitrary contextual influences with real punching power. These are not uncommon. For example, in the case of hospital patients reporting on their suffering and life satisfaction, such a simple manipulation as giving out a high or a low frequency scale for

rating the occurrence of physical symptoms markedly effects their judgments of health satisfaction, i.e. they “feel” much worse if the scale has a low frequency skew to make a particular symptom look very rare (the scale ranging from “never” to “more than twice a month”), and much better if it has the opposite skew (see Schwarz & Oyserman, 2001). This is so even if the low frequency scale in itself influences the patients to under-report the occurrence of the symptom in the first place. At the same time, if the patients’ task instead concerns how much their symptoms bother them, then a high-frequency scale (which encourages higher estimates of symptom frequency) elicits reports of greater suffering (Schwarz & Oyserman, 2001). In this way, two very similar introspective reports (of general health satisfaction and subjective suffering) can be made to blatantly contradict each other. Perhaps even more striking is the result reported by Schwarz (1999). In this study participants rated the things that they deemed most important in their lives. When presented with a structured questionnaire format with a long list of alternatives, 61.5% reported that the most important thing for them was to care for and prepare their children for life. In an open-ended format only 4.6% reported similar sentiments. Thus, we find a gross, perhaps even disquieting, discrepancy in what the participants chose to report as being *the most important thing in their lives*, caused by nothing more than a switch between two common survey formats (for further examples, see Winkielman & Schwarz, 2001; Oishi, Schimmack & Colcombe, 2003). On the other hand, this remarkable series of studies made no attempt to track the potential reverberations of these snapshot judgments, or to relate them to economic variables, such as willingness to pay, or to even ask the participants to reason further about their statements. Therefore, this work does not address our first consideration, concerning the ramifications of choice of future beliefs, attitudes or behavior.

The third and perhaps most important dimension to get a grip on choice-feedback dynamics is to pay close attention to the relationship between what economists would call stated and revealed preferences, and what psychologists would call the attitude-behavior gap (Ajzen & Fishbein, 2000; Gross & Niman, 1975; Schelpler, 2010). However, as we see it, the best characterization of this problem comes neither from economists nor psychologists, but from philosophers, who would point out that the crucial thing lacking here is a basic theory of introspection and intentionality (Dennett, 1987). Decades of work on human decision-making, variously stressing either rational or irrational aspects of choice behavior (Krueger & Funder, 2004; Tversky & Kahneman, 1981), have yet to find a compelling method for studying the relationship between choice and introspection (Johansson, Hall, Sikström & Olsson, 2005). The greatest barrier for scientific research in this domain is simply the nature of subjective experience. How can researchers ever corroborate the

reports of the participants involved, when they have no means of challenging them? As philosophers have long noted, incorrigibility is a mark of the mental (Rorty, 1970). Who are *they* (whether “they” are psychologists, economists or philosophers) to say what *my* reasons are?

The most troublesome thing about the dichotomy between stated and revealed preferences is not the fuss over whether actions speak louder than words (they do), but rather that talking is one of the most ubiquitous actions we engage in, and therefore one of the most obvious channels for the study of choice feedback effects. Thus, stated preferences are always prime and potent candidates to change and induce further preferences, particularly as these statements very seldom are allowed to stand undisputed in the social fray of our lives. As Dennett (1993) says: “*human beings have constructed a level that is composed of objects that are socially constructed, replicated, distributed, traded, endorsed..., rejected, ignored, obsessed about, refined, revised, attacked, advertised, discarded*” (p. 230). Here, we find both a glimmer of rationality in the distribution of information traveling between minds – in the asking, judging, revising, and clarifying of critical, communal discourse. But we also find the many social pressures and pitfalls where we unwittingly assume the warped views of parents and peers, and where we latch onto questionable authorities we think know better, or people that are just better known (when Penélope Cruz bats her nine inch L’Oreal-crusted eyelashes and says: “*because I’m worth it!*” we may think we are worth it too). Despite its obvious importance, this incessant discourse of decision-making is left out of the great majority of both economic and psychological models of preference change.

In summary, to forcefully approach the issue of choice feedback effects, we would like to see a focus on all three desiderata described above: (i) to clearly establish the effect as a plausible candidate for influence over multiple extended choice points, (ii) to aim for generalizability beyond preference free starting states by approaching contexts involving manifest preferences, and (iii) to realize the pivotal role individual and social discourse plays in shaping our preferences (whether consciously or not), and thus make an attempt to deal with both stated *and* revealed preferences in the same experimental paradigm.

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## CHOICE BLINDNESS

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In an attempt to investigate self-knowledge and the representational nature of decisions and intentions, we recently introduced the phenomenon of choice blindness (e.g. Johansson et al., 2005). It is a choice paradigm inspired by techniques from the domain of close-up card-magic, which permits us to surreptitiously manipulate the relationship between choice and outcome that our participants experience. In Johansson et al.

(2005) participants were shown pairs of pictures of female faces, and were given the task of choosing which face in each pair they found most attractive. In addition, immediately after their choice, they were asked to verbally describe the reasons for choosing the way they did. Unknown to the participants, on certain trials, a double-card ploy was used to covertly exchange one face for the other. Thus, on these trials, the outcome of the choice became the opposite of what they intended.

From a common-sense perspective, it would seem that everyone would immediately notice such a radical change in the outcome of a choice. But that is not the case. The result showed that in the great majority of trials our participants were blind to the mismatch between choice and outcome, while nevertheless being prepared to offer introspectively derived reasons for why they chose the way they did. When analysing the reasons the participants gave it was also clear that they often confabulated their answers, as when they referred to unique features of the previously rejected face as being the reason for having made their choice (e.g. stating that “*I liked the earrings*” when the option they actually preferred did not have any). Additional analysis of the verbal reports in [Johansson et al. \(2005\)](#) as well as [Johansson et al. \(2006\)](#) also showed that very few differences could be found between cases where participants talked about a choice they actually made and those trials where the outcome had been reversed.

Choice blindness is a robust, replicable, and often dramatic effect. We have demonstrated it for attractiveness of abstract artistic patterns and for male and female faces, both when presented “by hand” as described above ([Johansson et al., 2005, 2006](#)), and when the alternatives are presented on a computer screen ([Johansson, Hall & Sikström, 2008](#)). In [Hall, Johansson, Tärning et al. \(2010\)](#) we examined if choice blindness would extend to choices made in more naturalistic settings, and to modalities other than vision. We set up a sample stand at a local supermarket, where we invited customers to participate in a blind test of two paired varieties of jam and tea. By using a concealed chamber in the jars, we could switch the content before asking the participants to sample again and motivate their choice. The results showed that no more than a third of all manipulation trials were detected by the participants. Even for such remarkably different tastes as spicy Cinnamon-Apple and bitter Grapefruit, or for the sweet smell of Mango and the pungent Pernod, less than half of all manipulation trials were detected.

Recently, we have also established the effect for multi-attribute choices ([Johansson et al., in preparation \(a\)](#)), for self-knowledge of personality traits ([Johansson et al., in preparation \(b\)](#)), and even for moral judgments involving hotly debated topics in the current political debate ([Hall, Johansson & Strandberg \(submitted\)](#)).

Choice blindness as an experimental design is the first to give experimental researchers the opportunity to systematically study how

confabulatory reports are created and how they relate to standard or “truthful” reports about choice behavior. As a general method of investigation choice, blindness elevates inert hypothetical statements to powerful covert counterfactuals (i.e. from what do we think would have happened if we had chosen otherwise, to what actually happens when we get what we did not choose). However, perhaps the greatest potential of choice blindness as an experimental technique lies in the study of preference formation and preference change. Once the participants in a choice blindness experiment have accepted and argued for the opposite of their choice, they have already displayed (at least a stated) reversal of their initial preference. But in the recent studies, we have also started to look at what happens to that preference over time, e.g. when the manipulated choice enters the dynamics of preference formation, will the participants revert to their originally revealed preference, or will they prefer the option they were led to believe they liked?

## CHOICE BLINDNESS AND PREFERENCE CHANGE FOR FACES

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In psychology, an influential tradition has held that choices made influence future preferences; we come to prefer what we have chosen more, while the rejected alternative is liked even less (Brehm, 1956). This effect has been demonstrated for a wide range of choices (e.g. Gerard & White, 1983; Schultz & Léveillé, Lepper, 1999; Sharot, de Martino & Dolan, 2009), and for populations as different as amnesics (Lieberman, Ochsner, Gilbert & Schacter, 2001), young children (Egan, Santos & Bloom, 2007) and capuchin monkeys (Egan, Santos & Bloom, 2010).

But recent studies have questioned the free-choice paradigm (FCP), which is the main methodology used in these experiments (Chen, 2008; Chen & Risen, 2010). In its original form, the participants first rate a number of objects, then make a choice between two alternatives close in rank, and finally rate all the objects a second time. The typical finding is that the distance in rating between the chosen and the non-chosen object has increased when the rating is done at the end of the experiment, which has been interpreted as being due to the choice made between the two alternatives.

But Chen (2008) points out that this and all other versions of the free choice paradigm fall prey to a set of egregious statistical errors. In Brehm (1956), and in subsequent studies using the rating-based version of the FCP, the common procedure has been to remove all participants that are not consistent between the first rating and the choice – i.e. they first rate A over B, but then choose B over A in a direct binary choice between them (e.g. 21% of the participants in Brehm 1956 were removed for this

reason). The remaining participants are then compared to a control condition in which only two ratings are performed without any intermediate choice. The underlying rationale for removing inconsistent participants is that there is no stable preference for the choice to influence. But this procedure unfortunately introduces a bias in the dataset. If the choice is seen as the “true” or revealed preference and the rating as an informative but less stable measure, removing the inconsistent participants effectively removes all participants that may have a weak preference for the chosen object. When comparing the difference between the first and second rating in the control and the choice condition it is thus not that surprising that a greater “spread” between the alternatives is found in the condition in which all the participants with weak preferences are removed.

The objection by Chen and Risen effectively undermines the entire tradition of research using the FCP. Given that the results of FCP research has been taken for granted for so long, and has been cited and relied upon in numerous other related studies, such a conclusion would have far reaching consequences.

In an effort to help fill this void, we recently adapted our choice blindness paradigm to incorporate a measure of preference change. If the participants in our experiments accept the reversed outcome of their choice and then also changed their future preferences in line with the manipulations made, it would serve as firm evidence that choices can indeed influence future preferences.

Using the same methodology as in [Johansson et al. \(2005\)](#), we let participants choose between two faces, and for some trials we reversed their choices using the card-trick. In the first version of the new experiment, to add critical time-series data, we allowed the participants to make a second round of choices using the same face pairs (Hall et al., in preparation (b)). First of all, the detection rate was as low as in [Johansson et al. \(2005\)](#), thus replicating the basic choice blindness effect with a different set of faces. The new measure of preference was choice consistency, i.e. to what extent the participants prefer the same face both the first and the second time they are presented with the pairs. For the non-manipulated trials, the choice consistency was very high (93%). However, in the manipulated trials consistency dropped as low as 56%, which demonstrates that choice blindness clearly influenced the participants preferences, as they were much more likely to pick the originally non-preferred faces the second time they evaluated a pair.

In addition to the first round of choices, the participants also rated the attractiveness of the chosen and the non-chosen picture directly after each verbal motivation. After the second round of choices, the pictures were presented one by one in a random sequence, and the participants rated them one more time. The difference in rating between the chosen



and the non-chosen faces (i.e. a measure equivalent to the “spread” used in the free choice paradigm) also differed dramatically between the manipulated and the non-manipulated trials, both in the first and in the second rating. The originally chosen faces are thus rated much higher than the non-chosen faces for the non-manipulated trials (which makes perfect sense), while this relationship is more or less reversed for the manipulated trials (i.e. they rate the faces they were led to believe they liked higher, and the ones they thought they did not like lower). Of special interest here is the second rating, as the difference in spread indicates that the preference change is also present outside a pairwise comparison between the faces, and is stable enough to last long after the manipulated choice is performed.

Comparing with previous studies using the free choice paradigm, we avoid the statistical issues raised by Chen and Risen, and thereby show that making a choice can indeed influence future preferences for the chosen and the rejected alternative.

Very recently, another study remodelled and re-established the free choice paradigm itself. In Sharot, Velasques and Dolan (2010), the participants first rate a long list of names of holiday destinations, and are then asked to make choices between two destinations subliminally presented on the screen. The choice pairs are constructed from destinations with equal initial ratings, and they are presented for 2msc followed by a mask. After the choice, the two masked alternatives are made visible accompanied by a star marking which alternative the participants had indicated as their choice. Finally, the participants rate all the alternatives one more time. The new feature is that there is actually nothing presented during the “subliminal” presentation – two non-words are displayed before the mask, which means that when the participants make their choice it is not based on any real information. Still, this procedure led to an increase in the rating of the “chosen” alternative (but no reduction in value for the “rejected” alternative). This new methodology is called “blind choice,” which refers to the fact that the participants don’t know what the alternatives are when the “choice” is made.

There are some important similarities and differences between this and our study. Through its elegant design, Sharot et al. (2010) prove that the choice *in itself* can have an impact on subsequent evaluations of the choice alternative. But this very design also makes sure there are no previous preferences involved in *making* the choice, as no choice was actually made. In contrast, when the preferences are changed in our study, they are reversed in relation to what the initial choice revealed was the participants preference, a result that indicates a much stronger potential for preference change as a result of prior choices.

Still, the two studies together clearly point at an interpretation of the choice effect as being due to the induced *belief* in preferring one option

over the other. It has previously been argued that it is the comparison and evaluation of the two alternatives that drives the change in preference (e.g. Shafir, Simonson & Tversky, 1993), but in our studies, that cannot be the case, as the initial evaluative process ended in the opposite direction.

## CHOICE BLINDNESS AND PREFERENCE CHANGE FOR RISKY CHOICES

In the study of risk and decision-making in both economics and psychology, an almost universally employed tool is hypothetical monetary gambles (e.g., Abdellaoui, 2000; Birnbaum, 2008; Brandstätter, Gigerenzer & Hertwig, 2006; Tversky & Wakker, 1995). By studying how people behave when faced with potential losses and gains under different probability conditions, models are constructed for behavior under risk and uncertainty (e.g., Gonzalez & Wu, 1999; Prelec, 1998; Wakker, 2004). This type of modeling gained even more recognition when Daniel Kahneman earned his Nobel Prize in Economics in 2002 for the invention and development of Prospect Theory (Kahneman & Tversky, 1979). And in recent years, the use of monetary gambles has become even more ubiquitous, with the advent of neuroeconomics as a separate and high-profile subfield of economics and cognitive science (e.g. Camerer, Loewenstein & Prelec, 2005; Glimcher, 2003; Kenning & Plassman, 2005).

One assumption that both economic and many psychological theories of decision-making often take for granted is that people's preferences for decisions under risk are stable constructs that stay constant over time and regardless of task (e.g., von Neumann & Morgenstern, 1947; Brandstätter et al., 2006; Tversky & Kahneman, 1992; Tversky & Koehler, 1994; but see Kusev, Van Schaik, Ayton et al., 2009; Stewart, Chater & Brown, 2006; Ungemach, Stewart & Reimers, 2011). For example, one of the main predictions of *Cumulative Prospect Theory* (Tversky & Kahneman, 1992) is the so-called four-fold pattern of risk attitudes: people will be risk averse (prefer the safe bet) for gains and risk seeking (prefer the risky option) for losses of moderate to high probability, and they will be risk seeking for gains and risk averse for losses of low probability. These risk profiles are sometimes thought of as analogous to personality characteristics; they are stable traits that can be used to predict how people will behave in the long-term.

The abundant use and reliance on monetary gambling scenarios, as well as the accompanying assumption of people's stability of preferences for decisions under risk, makes this a very interesting domain for the application of the method of choice blindness. Is it at all possible to change the outcome of a choice without people noticing when the

alternatives are so detailed and explicit? And if the participants would fail to notice when their choices are being manipulated, what will happen to their general preferences over time? Will they change their risk profile from risk seeking to risk averse if this is the direction of the manipulation?

To help answer these questions, we recently constructed a choice blindness experiment using hypothetical gambles (Kusev et al., in preparation (a)). The design closely follows the format of experiments in this tradition. The participants answer a number of gambling questions of the following type: *“what would you prefer: alternative (A) 45% of losing £100, or alternative (B) a certain loss of £50?”* The sums in alternative B are varied around the point of the expected value in alternative A (in this case £45). After the participants have completed their choices, they are presented with all the scenarios again with their previous choices highlighted. The task this time is to indicate if they want to confirm or reject their initial choice, and also to indicate how satisfied they are with what they have chosen. Finally, the scenarios are presented a third time, and the participants have to evaluate the gambles once more. When the participants are asked to confirm or reject their initial choices, some scenarios have the opposite of their first choice highlighted, thereby reversing their initial choices. These changes are consistently made in the opposite direction to participants’ original preferences, e.g., from risk seeking to risk avoiding choices for all gambles with moderate and high probabilities of loss. The results showed that very few participants detected the manipulation in the second phase of the experiment, and when rating how satisfied they were with their previous choices, they were equally satisfied with their manipulated and non-manipulated choices (Kusev et al., in preparation (a)). In addition, we were able to demonstrate an overall significant change in their risk preferences for the repeated choice scenarios, and in some conditions even a complete preference reversal for some of the probability levels.

This result shows that choice blindness can affect not only esthetic preferences, but also supposedly more general preferences like risk aversion. Asymmetries and preference reversals for risk has been demonstrated many times before (see [Lichtenstein & Slovic, 1971, 2006](#)), but this is the first time it follows as a consequence of a manipulation of prior choices. As such, it adds to the accumulated evidence that people do in fact *not* have stable preferences for risk (e.g. [Stewart, Chater, Stoff & Reimers, 2003](#); [Stewart et al., 2006](#)). Instead, a myriad of things can influence a risky choice, like task descriptions and presentation, memory of prior choices, complexity of the gamble, and computational skills (see Kusev, Tsaneva-Atanasova, van Schaik & Johansson, in preparation (b), for our attempt to integrate all these factors into one model of risky choice). In short, the conclusion must be that if it were the case that

people had strong and enduring preferences for risk, it seems unlikely that they would accept a reversal of their choices, endorse and rate them as equally good as the choices in the non-manipulated trials, and then adjust their subsequent choices in line with the manipulations made.

## CHOICE BLINDNESS AND PREFERENCE CHANGE FOR POLITICAL OPINION

The most salient and immovable ideological archetype across the political landscape in the EU and the US is the division between Socialists and Conservatives (left wing vs right wing). Despite a persistent trend towards diminishing and more flexible party affiliation among voters, partisanship across the left-right divide still holds an iron grip on the international western electorate, and has even shown evidence of further polarization in recent years. (For example, see [Abramowitz & Saunders, 1998, 2008](#); [Lewis-Beck, Norpath, Jacoby & Weisberg, 2008](#); [Bafumi & Shapiro, 2009](#); [Carsey & Layman, 2006](#); and [Dodson, 2010](#) for analysis relating to the condition in the US; and [Clarke, Sanders, Stewart & Whiteley, 2009](#); [Kitschelt, 2010](#); [Enyedi & Deegan-Krause, 2010](#); and [Bornschieer, 2010](#) for the EU perspective. See also [Dalton, 2009](#); and [Cwalina, Falkowski & Newman, 2010](#), for cross-cultural comparisons). Given this extraordinary stability of political opinion, we were very keen to investigate the potential impact of choice blindness for voter preferences during the final stretch of the 2010 general election in Sweden.

Despite a tradition of bipartisanship at the national assembly, at the level of voter identity the Swedish electorate is regarded as one of the most securely divided populations in the world, with a small number of voters that “float” or “swing” across partisan lines often having a profound impact on the outcome of the election (see, for example, [Oscarsson & Holmberg, 2008](#) for a succinct analysis of the 2006 election). When we entered into the study, the tracking polls from commercial and government pollsters were polling the electorate at a mere 10% undecided between the two opposing (socialist-green and conservative-liberal) coalitions, with the common wisdom of the political scientist tagging very few additional voters as open for a coalition swing with only a few weeks left of the campaign ([Petrocik, 2009](#); [Holmberg & Oscarsson, 2004](#); [Oscarsson, 2007](#)).

To conduct the study, we approached people in the streets of the cities of Malmö and Lund and asked if they were willing to fill out an “election compass,” a survey-format designed to establish which political coalition (or party) best fits the views of the respondent. The concept of election compasses is well known in Sweden; all major media outlets create their own online versions, often in collaboration with political scientists.

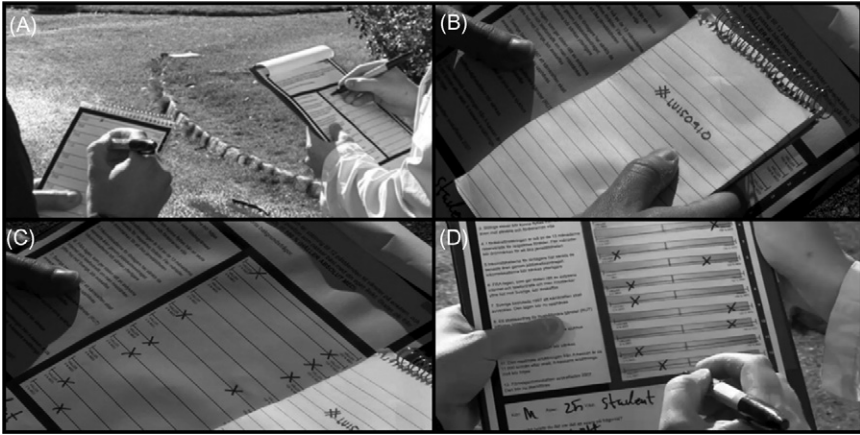
In our case, the questions concerned salient issues from the election campaign where the left- and the right-wing coalition held opposite positions (with a focus on traditional issues in the conservative-socialist divide, such as taxation and privatization).

The people who agreed to participate started by indicating their current voting intention for the election at the coalition-level, and then they proceeded to mark their opinion on our 12 statement election compass. After the participants had completed the survey, we asked them to explain and justify their stance on a few of the issues. However, at this point, we used a sleight-of-hand to alter the sum of the participants' answers in the opposite direction of the stated voting intention – i.e. from leaning socialist to conservative, or from conservative to socialist (see [Figure 6.1](#)). Thus, when the participants were asked to justify their opinion, their position had been reversed (for example, if they previously thought the tax on petroleum ought to be raised, the manipulated answering sheet now appeared to indicate that they had responded that it ought to be lowered).

Next is the crucial step of the experiment. After the participants had worked through the answers to the specific issues in the election compass, we overlaid a color coded semi-transparent correction template on their (manipulated) answering profile. In collaboration with the participants, we then tallied an aggregate “compass” score for the right and left wing side, indicating which political coalition they favored ([Figure 6.1D](#)). We then asked them to explain and comment on the summary score, and as the final step of the experiment, to once again indicate the direction and strength of their voting intention for the upcoming election.

Remarkably, the results showed that only 20% of the manipulated answers were noticed by the participants. Instead, they often volunteered coherent and elaborate arguments why they agreed with the reversed position. For the participants who did detect the changes, they almost invariably attributed the mismatch as a result of them having misunderstood the question the first time. In these cases, they were given the chance to once more express their attitude on the scale. The low rate of detection allowed us to move the aggregate scores of 91% of all participants across the partisan dividing line (i.e. only 9% of the participants detected enough manipulations to adjust their summary score back to the original coalition profile). Thus, we managed to create a situation where an overwhelming majority of the participants accepted a preference reversal across the socialist–conservative divide when aggregating the 12 campaign issues from the compass.

The critical concern now is whether this induced preference reversal managed to leap across the attitude–behavior gap, and impinge on real-world behavior (remember, this is immersed in a live campaign only a few weeks before the election, where stated voter intentions correlate



**FIGURE 6.1** (A) The participant first indicates the direction and strength of his or her voting intention for the upcoming election, and then rates to what extent they agree with 12 statements selected to differentiate between the two political coalitions (e.g. concurring with “the tax on petroleum should be raised” would tally a point in agreement with the left-wing coalition, etc.). Meanwhile, the experimenter (left-most in the image) pretends to take notes, but instead monitors the markings of the participant, and creates an alternative answering profile favoring the opposite view. The new set of ratings is written on a slip of paper identical to the rating section on the questionnaire. (B) After completion, the participant hands over the questionnaire to the experimenter, who has hidden the alternative answer-slip under the notebook. The notebook has a non-permanent adhesive surface, and when the experimenter swipes it over the questionnaire it attaches and occludes the section containing the original ratings. (C) Next, the participant is confronted with the reversed answers, and asked to justify the manipulated opinions. (D) Then the experimenter covers the (manipulated) ratings of the participants with a color-coded, semi-transparent correction template, and sums up the results indicating which coalition the participant favors (8 right-wing vs 4 left-wing, or 2 right-wing vs 10 left-wing, etc.). Finally, the participant is asked to justify his or her aggregate position, and once again indicate the direction and strength of their voting intention for the upcoming election.

extremely well with actual voting, see [Holmberg & Oscarsson, 2004](#)). What we found was that, compared to the initial voting intention, 10% of our participants moved across the full ideological span, and switched their voting intention from firmly right-wing to firmly left-wing. A further 22% went from expressing unequivocal coalition support (left or right), to becoming entirely undecided, and 3% went from being undecided to having a clear voter intention. In addition, 10% of the participants recorded substantial movement in the manipulated direction along the confidence scale – moving from “absolutely sure” to “moderately sure.” If we add to this that around 12% of participants were undecided both before and after the experiment (a figure roughly corresponding

to the category of undecided voters in the traditional opinion polls), we end up with a figure of more than half of all participants being open for movement across the great partisan divide (“in play” as the pollsters would say), a figure dramatically different from the expectations of political scientist, pollsters, party campaign strategists, and not least the voters themselves.

In summary, we have demonstrated considerable levels of self-induced preference change through choice blindness for a highly charged and important domain of political life. As we see it, there is also a robustness to the results, as any potential experimental demand would line up more forcefully on the side of being consistent with the original voting intention (expressed on the exact same scale as the one at the end of the experiment), than being consistent with the compass score – which can always be discounted as not being representative of the interests of the participants, or the campaign focus, etc. It should also be noted that in no part of the experiment did we provide arguments in support or opposition to the expressed views of the participants – the participants did all the cognitive work themselves. This aspect is especially interesting, as a number of recent studies have emphasized how hard it is to influence peoples’ voting intentions with “regular” social psychology tools, like framing (Druckman, 2004) and dissonance induction (Elinder, 2009), or indeed objective arguments, even when the political opinion held is based on factual misconceptions that are demonstrably false (Nyhan & Reifler, 2010).

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## DISCUSSION

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Choice feedback effects have been a focus for psychologists for the better half of a century, and it is time to set some goals and standards for further studies in this domain. In the introduction to this chapter we listed three desiderata for such experiments: (i) to establish the effect as a plausible candidate for influence over multiple choice points, (ii) to aim for generalizability by involving manifest preferences, and (iii) to make an attempt to deal with both stated *and* revealed preferences in the same experimental paradigm. As we see it, the primary impetus for these criteria lies not in the promise of squeaky clean theoretical contrasts between utility and choice-based models (a real problem, admittedly), but rather in an attempt to marry the penetrating inventiveness of social-psychology and decision and judgment research with the crass real world demands of economics and consumer studies.

The studies described above use choice blindness as a novel tool to investigate preference change, and, in particular, consider the impact of modifying people’s beliefs about what they previously chose. Echoing the results of the research on valuation described in the introduction

(e.g., Ariely et al., 2003, 2006; Ariely & Norton, 2008; Vlaev et al., 2009), it appears that people's judgment about their preferences and opinions are unstable, and to some extent "arbitrary." Yet it appears that people also attempt to be consistent with what they believe to be their previously expressed attitudes or preferences: when they are misled about their previous responses, their subsequent responses are modified appropriately.

This presumed drive for consistency provides one possible explanation for the long-term stability of the artificially-induced preferences (for faces, for perceptions of risk, and even for political convictions). What is particularly interesting here is that different probes might have very different powers to promote consistency. For example, using our choice blindness paradigm one might contrast the preference induction arising from choice with verbal explanation, and/or choice with numerical ratings (of the mismatched alternative). Here, the question would be whether the naturalness and fluency of verbal explanation, or the vividness and "concreteness" of numerical ratings, would generate the greatest preference effect. The latter alternative would sit very well with the conjecture of Ariely et al. (2003) that it might be the quantification *as such* that leads participants to form further coherent preferences in the same domain of choice, and thus it might be some of the most quintessential features of economic choices that create the most troubling dynamic effects. Obviously, these results are still mere blips on the experimental radar, and the question remains whether they simply might be swamped or washed out in the competitive consumer and ideological landscape. As Simonson (2008) concludes: "*while the principles governing context, framing, and task effects may be general, the resulting 'preferences' often leave no trace and have little if any effect on subsequent decisions*" (p. 157; see also Yoon & Simonson, 2008). In our view, the attempt to trace such longitudinal and cross-contextual effects of various implicit influences, and to decide whether they represent the norm or just curious exceptions, is one of the most urgent and important quests to pursue for research on preference change.

In relation to the second goal, to use meaningful choices involving actual preferences, the choice blindness experiments we have presented here are more or less on the mark. Deciding which of two faces one finds more attractive, or which hypothetical gamble one prefers, are not the most exciting choices around. But they are simple and intuitive, and it is fair to assume that people have relatively stable guiding preferences for both domains. These are also domains in which the path to possible real-world tests and applications (for faces: the cosmetic and modeling industry, dating and mate selection, shifting cultural standards of beauty, etc., for risk: investment decisions, gambling, insurance, etc.) is relatively short. On the other hand, voting intentions measured at the cusp of the national election must be counted among the diamond league of dependent variables in psychology.



The final goal is also the most difficult. As we said in the introduction, there is currently no good model that can unravel the interplay between introspective reports and preference formation. For example, consumer psychologists and marketing researchers puzzled for some time over an effect they call “The Mere-Measurement Effect” (e.g. Morwitz & Fitzsimons, 2004; Williams, Fitzsimons & Block, 2004). True to its name, the mere-measurement effect concerns the fact that, by simply asking people about their intentions about one or another action (e.g. to buy a specific brand of automobile, to engage in a specific charity, to vote in an election), their behavior is likely to change (compared to a control group who receives no questions about their intentions). But why should the behavior of people change as a result of asking them what they intend to do? These are not questions that are framed to emphasize different benefits or drawbacks with a certain choice, or questions that entice participants into elaborating on reasons they had not thought about earlier, or some other cleverly designed technique of influence – they are just questions that ask people what they intend to do. In surface terms, Morwitz and Fitzsimons (2004) give the answer: *“somehow the act of measuring intentions affects consumers’ thoughts associated with the behavior (e.g. representations of the behavior, attitudes towards the behavior, or thoughts associated with the act of purchasing). These altered thoughts in turn change the consumers subsequent purchase behavior”* (p. 4).

We agree, but to understand the basic process at the heart of the mere-measurement effect, the explanation needs to be framed yet more generally, and with reference to a discussion about introspection and intentionality. For all the intimate familiarity we have with everyday decision-making, it is very difficult to probe the representations underlying this process, or to determine what we can know about them from the “inside,” by reflection and introspection (Nisbett & Wilson, 1977; Jack & Roepstorff, 2004; Johansson et al., 2005, 2006). Mere-measurement creates the effects it does because there is nothing mere about measurement (maybe the situation is not as paradoxical as in physics, but we see the litter of Schrödinger’s cat all over our decision tasks). In fact, for Dennett (1987, 1991, 1996), the “mere-measurement effect” represents something fundamental about decision-making of the kind human beings engage in (and is a centerpiece in the explanation of why the notion of well-specified introspectively observable intentional states is so seductive). He writes: *“What creates the illusion of discrete, separate, individuable beliefs [and preferences] is the fact that we talk about them: the fact that when we go to explain our anticipations, when we move from generating our own private expectations of what people are going to do, to telling others about these expectations we have, and explaining to them why we have them, we do it with language. What comes out, of course, are propositions. /.../ Then... it’s only too easy to suppose that those sentences are not mere edited abstractions or*

*distillations from, but are rather something like copies of or translations of the very states in the minds of the beings we're talking about"* (1991, pp. 88–89).

From an empirical perspective, choice blindness strongly suggests that there is no Archimedean point from which to observe and measure preferences. So, which side of the super sour apple does the economist and psychologist want to bite into? Does choice blindness demonstrate that only 20% of the opinions people held about campaign issues in the Swedish election involved real preferences? Or should we admit that preferences can often be blatantly reversed moments after the decision is made? As the discussion makes clear, we do not believe these questions are well formed, because the very concept of preference as an enduring state, which determines which choices we make, is itself under threat. Yet we do believe choice blindness is a uniquely positioned instrument to pry apart the relative impact of stated and revealed choice on future preferences and behavior.

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