Moving Political Attitudes Through Framing of Numbers

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Abstract

While numbers permeate modern political communication, they are almost entirely absent from the literature on framing effects. This paper shows that numbers are also subject to framing, and using two population based survey experiments, it demonstrates how such numerical framing can have marked effects on political attitudes. Specifically, the paper shows that "ratio bias," a fundamental human tendency to pay too much attention to numerators in ratios and insufficient attention to denominators, means that different representations of the exact same numerical value can move attitudes on politically salient issues substantially.

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Introduction

Framing matters for political attitudes. Public opinion research has seen a host of studies that demonstrate framing effects on a multitude of salient policy issues. By framing a given policy issue with the right words, it is for example possible to affects citizens' attitudes substantially on unemployment and poverty policies (Druckman, 2001; Iyengar, 1991; Slothuus, 2008), public support for war (Mueller, 1973; Zaller, 1992, p. 33), and policies concerning immigrants and minorities (Druckman, Peterson, & Slothuus, 2013; Aarøe & Jensen, 2014). As shown by these and many other studies, frames with different wordings - either presented in questions, vignettes or artificial news stories – result in marked differences in opinions, and these ubiquitous framing effects clearly demonstrate that words often *"do the work of politics"* (Graham, Haidt, & Nosek, 2009).

These studies of framing have contributed greatly to our understanding of citizens' opinion formation, but the current view of framing as a matter of *"winning with words"* (Schaffner & Sellers, 2010), almost entirely disregards the fact that the modern day language of politics is a mixture of words and numbers. The political debates on unemployment and poverty are interspersed with unemployment numbers and the share of people living below the poverty line, media coverage of wars highlight the most recent casualty rates, and immigration policies are assessed on the number of people coming into the country. In short, numbers are a key feature of public discourse (Prévost & Beaud, 2012, p. 1). By disregarding such numbers, the field of framing studies have turned a blind eye to a central component of extant political rhetoric, and, by extension, also paid too little attention to the real life relevance of frames that are logically equivalent (e.g., Tversky and Kahneman 1981).

In this paper I show that numbers can also be framed, and that such, logically equivalent, numerical framing can have a strong impact on citizens' opinion formation on political issues. Specifically, using two survey-experiments, this study demonstrates how numerical framing affects political attitudes substantially through *"ratio bias,"* a fundamental human tendency to pay too much attention to numerators in ratios and insufficient attention to denominators (Reyna & Brainerd, 2008). People generally perceive the same number to be larger when written with a large denominator, and changing the denominator when describing policy relevant numbers can therefore also change attitudes towards a policy.

These results have implications for the way we understand framing effects and public opinion formation: First, the results show that framing can work through other mechanisms than the ones described in previous work. Second, it documents the importance of the ever present numbers in political rhetoric. Furthermore, the results have implications for study of citizens' responses to policy relevant facts (Kuklinski, Quirk, Jerit, Schwieder, & Rich, 2000; Lawrence & Sides, 2014). As shown and discussed in this paper, the study of facts and opinions need to pay more attention to the fact that the numbers in modern political language are not necessarily just simple representations of facts, and the study shows that increased attention to numbers do not necessarily lead to a more rational public.

Frames Without Numbers in a Quantified World

Before we examine the ratio bias phenomenon, it is worth noting that numbers have not always been absent in the framing literature. In fact, in the canonical *"Asian disease"* experiments on framing by Daniel Kahneman and Amos Tversky, numbers played a key role: Using a hypothetical scenario describing the potential consequences of this Asian disease, Kahneman and Tversky showed that the preferences of the participants depended heavily on whether the scenario was framed in terms of the number of human lives lost or number of lives saved (Kahneman & Tversky, 1984; Tversky & Kahneman, 1981, 1982). What made these results so noteworthy was the fact, that the frames presented to the experimental participants were logically equivalent. Hence, differences in preferences were not attributable to different information in the frames but instead due to loss aversion that human beings tend to exhibit when dealing with risky choices, as described in Prospect Theory (Kahneman & Tversky, 1979).

However, with a few exceptions (Druckman & McDermott, 2008; Kam & Simas, 2010), the framing effects investigated within extant public opinion research are based on frames that differ substantially from the frames used by Tversky and Kahneman. First, the definition of frames as being logically equivalent has been relaxed, and most studies within political science and communication now study frames that differ not just in their description but also in content, often termed *"emphasis frames"*(Chong & Druckman, 2007). The use of this less stringent definition of frames has been based on the argument that logically equivalent frames, such as the ones used by Kahneman and Tversky, are rare in policy discourse and political news (Druckman, 2001; Slothuus, 2008; Sniderman & Theriault, 2004). Second, most likely as a corollary to the shift from frames of equivalence to frames of emphasis, numbers now seem to have disappeared almost entirely from the frames investigated in the literature. Instead, frames are now generally based entirely on words, and in many cases frames have become almost synonymous with arguments (see, e.g., Druckman et al., 2013, p. 57). Hence, numbers do not currently play a significant role in the frames used in studies of public opinion formation.

This lack of numbers in the extant framing literature is somewhat remarkable, given the prominent role played by numbers in modern policy discourse and political news. The language of politics may not always have been full of numbers, but they have played a role in public discourse already as early as the 1820s and 1830s, a period sometimes termed the *"great explosion"* of numbers by historians of statistics (Prévost & Beaud, 2012, p. 63), see also, Porter (1986). While the increasing prominence of numbers in public discourse has, somewhat ironically, mostly been described in qualitative terms, there is arguably little doubt that, as of today, political issues are heavily infused with numbers, and that numbers "*have become, over the last two centuries, a central feature of public discourse and a privileged means of founding knowledge and trust in many walks of life"* (Prévost & Beaud, 2012, p. 1). See also, (Desrosiéres, 1998). The quantification of the public discourse, and the world at large, has right from the start often been perceived as a development toward a more rational form of politics and debate (e.g., Prévost & Beaud, 2012, p. 44), and have come to *"epitomize*

objectivity" (Hansen & Porter, 2012). Notably, this perspective on numbers as being a foundation of rational discourse and decision making also seem to be an assumption in current work on citizens' responses to policy relevant facts, where political attitudes based on or impacted by numbers are regarded as being *"enlightened"* (Lawrence & Sides, 2014) However, as the next sections will show, there may still be plenty of room for irrationality in a quantified world.

The Ratio Bias Phenomenon and Political Attitudes

In order to understand the ratio bias phenomenon, imagine for a moment that you are faced with two urns containing a mix of red and white jellybeans. The first urn contains 1 red jellybean and 9 white jellybeans, while the second urn contains 10 red jellybeans and 90 white jellybeans. If you pick a red jellybean, without looking, you will win a price. Should you pick a white jellybean, you will win nothing. Which urn would you prefer to draw from? Obviously, the likelihood of drawing a red jellybean does not differ between the two urns, since 1/10=10/100, but it turns out that most people will show a preference for the larger urn with 10 red jellybeans. (Kirkpatrick & Epstein, 1992; Pacini & Epstein, 1999a). In fact, the tendency to prefer the urn with more jellybeans is so strong that it may in many cases lead to non-optimal choices: Participants often show a preference for the large urn, even when the odds of success are clearly lower, e.g., preferring an urn with 7/100 red jellybeans as opposed to an urn with 1/10 red jellybean. (Dale, Rudski, Schwarz, & Smith, 2007; Denes-Raj & Epstein, 1994; Pacini & Epstein, 1999b study 2).[1]

The jellybean experiment clearly illustrates an effect of the ratio bias phenomenon: The perceived likelihood of an event is greater when it is presented as a ratio with a large denominator than when it is presented as a ratio with a small denominator (Reyna, Nelson, Han, & Dieckmann, 2009). More generally, the ratio bias effect denotes a basic human tendency to pay too much attention to numerators in ratios and insufficient attention to denominators, and, as such, it is not limited to choices concerning probabilities. Across a host of different studies, phenomena equivalent or similar to the ratio bias effect have been studied under terms such as *"denominator neglect"* (Okan, Garcia-Retamero, Cokely, & Maldonado, 2012), *"numerosity effect"* (Reyna & Brainerd, 2008), *"unit effect"* (Pandelaere, Briers, & Lembregts, 2011), and *"the base rate fallacy"* (Bar-Hillel, 1980; Kahneman & Tversky, 1973). As a basic human tendency, ratio bias seems to be a highly general phenomenon, which has been shown to have effects in several different domains (Denes-Raj, Epstein, & Cole, 1995): Ratio bias can influence participants' choices regarding which jobs to apply for (Alonso & Fernandez-Berrocal, 2003) and which goods to buy and

[1] A number of recent studies have criticized the jellybean experiment, arguing that it encourages non-rational decision making by appealing to how respondents feel. These studies have also shown that the ratio bias in the jellybean experiment can be mitigated or made to disappear under certain circumstances (Lefebvre, Vieider, & Villeval, 2011; Passerini, Macchi, & Bagassi, 2012). However, the fact, that this bias, much like other biases, can be suppressed or eliminated by eliciting rational decision making in a number of ways does not necessarily mean that everyday decision-making is not affected by the bias.

consume (Burson, Larrick, & Lynch, 2009; Pandelaere et al., 2011), and the seminal study by Yamagishi (1973) demonstrated that ratio bias can have significant effects on perceptions of health risks: Cancer was, for example rated as riskier by participant when described as *"kills 1,286 out of 10,000 people"* than when described as kills *"24.14 out of 100 people."* Similarly, as a consequence of ratio bias, people rate different potential cases of deaths as being more riskier when given number of deaths per year than when given number of deaths per day (Bonner & Newell, 2008).

While ratio bias thus have been investigated and documented across multiple different domains, the phenomenon is completely unexplored within the domain of politics and public opinion research. Based on the existing studies of ratio bias it might seem reasonable to assume that ratio bias can also impact opinion formation on political attitudes, but this of course rest on the assumption that citizens use numbers when forming political attitudes. This is not necessarily an unproblematic assumption, and extant public opinion research generally seems to give words more persuasive power than numbers. Not only are citizens often ignorant on politically relevant numbers about, e.g., unemployment, public spending and population figures (Herda, 2010; Kuklinski et al., 2000; Lawrence & Sides, 2014; Nadeau, Niemi, & Levine, 1993; Sigelman & Niemi, 2001; Wong, 2007), their opinions also often seem to be unaffected by the numbers even when they do know them: Providing people with the correct numbers on, e.g, welfare spending, unemployment and minority populations (Kuklinki 2000, Sides 2014) generally does not significantly affect their opinions on policies on these issues. Similarly, while the American public had relatively precise beliefs about the casualty rates in the Iraq war, these beliefs did not shape their opinion toward the war (Gaines 2007, 967). Thus, if citizens simply ignore numbers when forming political attitudes, we should of course not expect ratio bias of exert any influence on such attitudes.

However, numbers are not always ineffective in moving opinions: War casualties may, for example, in some cases move support for military action (Gartner, 2008), and information on welfare spending does move people's opinions when it is presented in a way that draws attention to the numbers policy relevance and existing misperceptions. (Kuklinski (2000). Similarly, Gilens (2001) also found that factual information about, e.g., crime rates and share of public spending on foreign aid could move political attitudes significantly in some cases. Furthermore, observational studies have shown that perceptions about the prevalence of a problem such as unemployment seem to have a strong impact on political attitudes (Mutz, 1998 chapter 4). Hence, while the results are mixed, numbers do at least sometimes affect political attitudes. Therefore, if the ratio bias effect can make politically relevant numbers appear larger or smaller, we might also expect that it can affect political attitudes. More specifically, the primary hypothesis, tested both in experiment 1 and 2 below is that *support for a given policy be higher [lower] when the positive [negative] attributes associated with this policy is presented with a larger denominator, relative to a small denominator* (Hypothesis 1).

Who is affected by Ratio Bias? – Mechanisms and Moderators

The degree to which subjects' political attitudes are affected by ratio bias may be contingent on personal characteristics, both cognitive and attitudinal. Starting with the cognitive factors, existing literature on the ratio bias effect have often applied the perspective of dual-process theories of judgment and decision making, e.g., cognitive-experiential self-theory, *CEST* (Mikels, Cheung, Cone, & Gilovich, 2013; Pacini & Epstein, 1999a). According to such dual-process models, human judgment and decision making can be based on two distinct modes of thought. One is the *experiential system*, in similar dual-process models referred to as the *associative system* (Sloman, 1996) or *System 1* (Kahneman & Frederick, 2002), which operates rapidly, affectively and relatively effortless. In contrast, the *rational system* (also known as the *rule-based system* and *System 2*) is slow, logical and effortful. In this dual-process perspective, the ratio bias effect is a result of the intuitive reasoning of the experiential system. Several specific mechanisms, which are not necessarily mutually excluding, have been suggested for why, the experiential system may exhibit ratio bias. First, the experiential system comprehends smaller numbers better than larger numbers, and it may therefore be intuitively easier for the experiential system to realize that a given probability is low when it is presented in small numbers, e.g., 1/10 as opposed to 10/100. Second, raw numbers are easier to process than fractions, and the experiential system may therefore be disposed to attend to the nominator as a number and neglect the denominator (Alonso & Fernandez-Berrocal, 2003; Pelham, Sumarta, & Myaskovsky, 1994; Yamagishi, 1997).[2]

An important point related to dual-process explanations of ratio bias is that individuals differ in their reliance on the two systems. While some individuals tend to rely heavily on their experiential systems, other individuals rely more on their rational system when solving certain tasks, and the latter group should therefore not be expected to show the same degree of ratio bias. In accordance with this perspective, ratio bias has been shown to be moderated by Need for Cognition (Alonso and Fernandez-Berrocal 2003) and the related Cognitive Reflection Test (Liberali, Reyna, Furlan, Stein, & Pardo, 2012). More to the point, in tasks involving numbers, individuals may rely more on their experiential system if they have a low level of *"numeracy,"* i.e. ability to understand and use numerical concepts. In line with this prediction, several experimental results has shown that individuals low on numeracy indeed do tend to exhibit a stronger ratio bias than individuals high on numeracy (Reyna & Brainerd, 2008 - it should also be noted numeracy is not just a reflection of an individuals education or intelligence). Understood in this way, individual numeracy is so far completely unexplored in studies on political attitudes,[3] but based on existing studies from

[2] Additionally, Reyna and Brainberd (2008) argue that ratio bias can be explained by a particular dual-process theory, namely fuzzy-trace theory. For an overview of dual process models, see Samson & Voyer (2012) and Pacini & Epstein (1999a)

[3] Some studies have looked at (in)numeracy and political attitudes, but these studies use the term (in)numeracy as denoting "unfamiliarity with politically relevant numbers" (Lawrence & Sides, 2011), i.e., a question of possession of factual knowledge, whereas the concept of numeracy used in the ratio bias literature and this paper denotes a trait-like ability

other fields, it is reasonable to expect that *numeracy moderates the ratio bias effect on political attitudes/policy preferences* (Hypothesis 2). This hypothesis is tested in the second experiment.

Ratio bias effects may also be affected by attitudinal factors when we move into the domain of politics and political attitudes. In order to get a better understanding of how ratio bias might influence political attitudes, it is therefore worthwhile to approach the issue from the perspective om *"motivated reasoning"* (Kunda, 1990). The basic premise underlying the theory of motivated reasoning is that *" all reasoning is motivated"* (Kunda, 1990), and that the motivations underlying any sort of reasoning can be expected to influence the conclusions reached (Taber & Lodge, 2006). Specifically, people's reasoning may be driven both by accuracy goals and directional goals. An accuracy goal motivates people to evaluate the available information in an evenhanded manner with the goal of reaching a conclusion that is accurate (Kunda, 1990). In contrast, reasoning motivated by a directional goal, is reasoning that is motivated by a desire to arrive at a particular conclusion because this conclusion is in line with already held perceptions or attitudes. Hence, in politics, people will often be motivated to reach a conclusion that is in line with partisan directional goals (Bolsen, Druckman, & Cook, 2014).

The extant literature on ratio bias have not investigated the influence of such directional goals. Rather, when investigating ratio bias in jelly bean drawings or domains such as health (Yamagishi, 1997) and commerce (Pandelaere et al., 2011), the literature have simply assumed an accuracy goal. In contrast, several studies have shown that framing effects on political issues are clearly affected by directional goals, often goals related to partisan identity, and that the efficacy of a frame can depend on the political predisposition of the individual receiving the frame (Druckman & Bolsen, 2011; Slothuus & de Vreese, 2010). We might therefore also at first sight be ready to hypothesize that the ratio bias induced by different framing of ratios would be moderated by the subjects' political predispositions. However, the frames used in the extant studies of policy framing are generally frames which are explicitly in favor or opposition to a certain policy or coming from a certain political party. Ratios on policy relevant , on the other hand, are not in themselves an argument in favor of a particular policy position, no matter how they are framed. According to ratio bias theory, we should expect people to perceive, e.g., 500/10,000 to be relatively larger than 5/100, but that does not in itself imply that either of the ratios would push policy attitudes on a given issue in a certain direction. An individual with a certain political predisposition might perceive, e.g., an unemployment rate of 500/10,000 to be relatively larger than 5/100 but nevertheless still take the numbers as proof that the problem of unemployment is relatively minor. Conversely, a person with another political predisposition might take both ratios as proof that unemployment is a major problem (for a similar line of reasoning, see (Gaines, Kuklinski, Quirk, Peyton, & Verkuilen, 2007)). Hence, on this issue, the study takes a more explorative outlook and asks *how does political predisposition moderate the ratio bias effect on political attitudes/policy preferences?* (Research Question 1). This research question is investigated in the second experiment.

Study 1

The purpose of study 1 was simply to investigate whether numerical framing could move political opinions through ratio bias. To do this, the experiment applied a technique similar to the choice paradigm technique used by Burson et al. (2009), where respondents were asked to choose between two alternatives, which contained trade-offs across two attributes. Respondents were presented with a choice between two future scenarios describing the educational attainments of youths and the respondent's own tax payments. In group one, the problem of youths not completing an education was framed with a relatively small denominator ("8 out of every 100 young people"), while potential future tax increases were framed with a relatively large denominator ("DKK 3,000 more in taxes per year").[4] Respondents in the second group received scenarios where the framing of these numbers were reversed, such that youths without an education was presented as "5,600 young people" (out of 70,000) while tax increases were presented with a smaller denominator as "DKK 250 more in taxes per months." It is important to note that both groups were informed about the total population of youths ("70,000"). With this information included, the respondents in both groups were given logically equivalent information both about the absolute and about the relative prevalence of the problem. The groups were manipulated on both dimensions, ratios of youths and ratios of taxes, in order to maximize the experimental effect in this first study. This is an issue to which we will return in study 2. The exact wording used in study 1 is shown below in table 1.

Table 1: Experimental stimuli, study 1

Group 1 *Small denominator framing of youths ("SD-youths")* *Large denominator framing of taxes ("LD-tax")*	*Group 2* *Large denominator framing of youths ("LD-youths")* *Small denominator framing of taxes ("SD-tax")*
In Denmark, approximately 70,000 young people are attending the 9th grade. Among this group, 8 out of every 100 young people will not complete an education at the level of a high school degree	*In Denmark, approximately 70,000 young people are attending the 9th grade. Among this group, 5,600 young people will not complete an education at the level of a high school degree*
Imagine that the state of society in Denmark three years from now is as described below in either alternative A or alternative B	*Imagine that the state of society in Denmark three years from now is as described below in either alternative A or alternative B*
A: Among the youths, 8 out of every 100 young people will not complete an education, and I pay the same amount of taxes as I do today	*A: Among the youths, 5,600 young people will not complete an education, and I pay the same amount of taxes as I do today*
B: Among the youths, 7 out of every 100 young people will not complete an education, and I pay DKK 3,000 more in taxes per year.	*B: Among the youths, 4,900 young people will not complete an education, and I pay DKK 250 more in taxes per months*

Note: Original text in Danish (author's translation). Differences between the stimuli are underlined here for presentational purposes and were not underlined in the original survey.

[4] "8 out of every 100" could obviously also have been written as "8%" However, several studies have suggested that the choice of percent format may in impact perceptions in a number of ways (Cuite, Weinstein, Emmons, & Colditz, 2008; Dieckmann, Slovic, & Peters, 2009; Peters et al., 2006, study 2; Slovic, Monahan, & MacGregor, 2000; Waters, Weinstein, Colditz, & Emmons, 2006).

It should be noted that the issues of education and taxes are salient issues in Danish politics, as both issues are relatively high on the agenda among the political parties, the news media and the Danish voters (Dahlgaard, Hansen, & Pedersen, 2014; Kosiara-Pedersen, 2014). Denmark spends 8% of GDP on education, the highest share among all OECD countries (OECD, 2013, p. 182), and almost all the political parties are committed to a target set for 2020; that no more than 5% of a youth cohort should end up without an educational attainment below a higher secondary education (OECD, 2014). The share used in the experiment, *8 out of 100*, was the actual current estimate by the Danish Ministry of Education.[5] While there is widespread political agreement on this educational goal, there is, unsurprisingly, a left-right split when it comes to tax policy. While the center-right of the Danish political parties are in favor of lowering income tax, or at least not increasing it, the center-left parties are generally opposed to tax reductions. The choice of DKK 500/DKK 3,000 as values used in the experimental stimuli was loosely based on previous estimates of the Danish citizens' willingness to pay for sociotropic goals such as decreased levels of unemployment (Hansen, Olsen, & Bech, 2014).

Method and respondents

The experiment was fielded as an online-survey in a small survey panel previously established by the author for an experiment on other policy issues. Respondents for the panel had been recruited—two months prior to this experiment—by letter from a representative sample of the Danish population (ages 18-64 years) drawn from the Danish Civil Registration System, which includes data on all citizens in Denmark.[6] The survey was fielded to 202 respondents, of which 108 respondents completed the experiment, yielding a response rate of 53%. More importantly, there was no drop-off after exposure to the experimental stimuli. Furthermore, while the respondents were not fully representative of the Danish population, there was a large degree of variation on demographics and attitudinal variables (gender; 54% female, age; M=47 years, SD=14, voting intention; center-left parties=44%). Median response time for the entire survey was 7 minutes and 10 seconds.[7]

Study 1 Results

In group one, the positive attribute in scenario B (fewer youths without an education) was presented with a smaller denominator than in group two, while the negative attribute (a tax increase) was described with a larger denominator than in group two. If respondents were influenced by ratio bias in their choice between the two scenarios, we would therefore expect that scenario B would be preferred by a smaller proportion in group one than in group two. This is exactly what the results show, in fact the manipulation resulted in a marked shift in preferences: Scenario B was chosen by 39% of the respondents (95%

[5] http://www.uvm.dk/i-fokus/95-procent-maalsaetning

[6] Thirteen percent of the population has chosen to register as not wanting to be contacted for research purposes. This opt-out option is particularly prevalent in certain age groups, and the sample was therefore stratified on age (and gender) in order to resemble the general Danish population.

[7] Following the experiment, the respondents participated in another experiment not included in this paper.

CI[26, 57]) in group one, whereas this scenario was chosen by 63% of the respondents(95% CI[50, 77]) in group two,. Even in this relatively small sample (n=108), this marked difference of 24 percentage points was easily significant (z=2.51, p= 0.012, two-tailed). The results of study one thereby demonstrated that ratio bias can have a marked impact when forming attitudes toward policy issues.[8]

There were, however, limitations to study one. First, the experimental groups in study one differed on two dimensions, ratio framing of youths and ratio framing of taxes, and it was therefore not possible to determine whether the ratio bias effect was attributable to one or both of these manipulations. Second, the small sample size of the study did not allow for a test of possible interactions between ratio framing, numeracy and political predispositions. These limitations of study one lead to study two.

Study 2

Study 2 used the same scenario choice question as in study 1, but in order to investigate the relative impact of framing about youths versus taxes, study 2 employed a 2×2 design, containing all four combinations of scenarios with either a small denominator on taxes (SD-tax) or a large denominator on taxes (LD-tax), *and* a small denominator on youths (SD-youths) or a large denominator on youth (LD-youths).

Method and respondents

To achieve a sufficient sample size, study two was conducted as an online-survey in a commercial survey-panel (YouGov). Among the 1,030 subjects commencing the survey, 1,007 respondents completed it, and survey drop off after exposure to stimuli was negligible (1.1%, n=11). The respondents completing the survey were relatively similar to respondents in study 1 on demographics, (gender; 50% female, age; M=46 years, SD=16), albeit substantially more politically right-leaning (voting intention; center-left parties=30%). Median response time for the entire survey was 3 minutes and 15 seconds.[9] Prior to their exposure to the experimental stimuli, respondents were surveyed on voting intention, numeracy and standard demographic variables.

Numeracy has been measured on several different scales, mostly scales based on objective measures, i.e., scales based on questions in which respondents are asked to perform numerical judgments and calculations. One of the first, the three-item scale constructed by Schwartz et al. (2007), was expanded to include 11 items by Lipkus et al. (2001), containing

[8] Study 1 contained several other experiments, including a similar experiment that included information on unemployment on taxes. While the results of this experiment indicate a similar effect, the effect size was smaller and non-significant (z=0.61, p=.54, two-tailed). Open-ended comments from respondents at the end of the survey made it clear that at least some respondents found this scenario to be unrealistic; an increase in taxes was part of the same scenario as lower unemployment, and some respondents perceived this as a claim that a tax increase would —causally—lead to lower unemployment, a claim to which they objected. Hence, the lack of a significant effect in this study may be due to this problem.

[9] Following the experiment, the respondents participated in a different experiment not included in this paper.

questions such as *"If person A's risk of getting a disease is 1% in ten years, and persons B's risk is double that of A's, what is B's risk?"* This scale has been widely used (Henriksen, 2012), and it was later expanded additionally, leading to a 15-item numeracy scale (Dieckmann et al., 2009; Kuklinski et al., 2000).[10] Such objective measures of numeracy have worked well in so far, as they have been shown to have significant impact on differential risk perceptions and framing effects, and they have been shown to have satisfactory measures of internal reliability (Dieckmann et al., 2009; Tichenor, Donohue, & Olien, 1970).

However, for this study, these objective measures have several disadvantages, compared to subjective measures of numeracy. First, objective numeracy scales are relatively time consuming and respondents generally find them to be stressful and frustrating. (Cuite et al., 2008). Second, most of the items used on the existing objective numeracy scales are focused on health related questions (Dieckmann et al., 2009, pp. 1485-1486), and testing respondents on such questions in a survey on political attitudes would most likely only exacerbate respondent frustration, and thereby also increase the risk of survey drop-off . Third, when conducting numeracy tests in an online survey, it becomes impossible to prevent respondents from using calculators or asking other people for help.

Because of these disadvantages, this study instead used the subjective numeracy scale (SNS) developed by Fagerlin et al. (2008; Waters et al., 2006). The full scale consists of eight items, of which four items measure subjective numerical ability, and four items measure preference for numerical information. Subjective measures of ability always raise questions of respondent's ability to assess their own skills, and SNS may not be completely interchangeable with objective measures (Liberali et al., 2012) . However, SNS has been shown to correlate strongly with objective measures of numeracy and to serve reasonably well as a predictor of success in numerical tasks (Henriksen, 2012; Waters et al., 2006).[11] This study focuses on numerical ability, and hence uses this subscale. The SNS ability scale was translated into Danish, and further modified in two ways. First, one of the items was replaced with a new item: the original SNS asks respondents to assess their own ability to calculate a 15% tip. However, in a Danish context, this item makes little sense, as tipping (at least of this magnitude) is uncommon. Instead, respondents were asked: *"How good are you at figuring out the price of a commodity in Danish Crowns, if the price is stated in a foreign currency, e.g., the Euro"*. Second, *"don't know"* was added as a response option. The four items formed a highly reliable scale (α=.90), and the "don't know" option was used by very few respondents (1.4%-2.6% for each item). The scale was moderately left skewed (skewness=

[10] Several tests of functional health literacy in adults (TOFHLA's) also include a number of questions used to measure numeracy, but the TOFHLA's have not been used to measure numeracy on its own, rather it has been incorporated in an aggregate measure of "health literacy." For an overview, see Reyna et al. (2009).
[11] Nelson et al. (2008) find that subjective measures perform poorly as indicators of objective numeracy. However, their test of SNS is based on just one of the SNS-items, measuring preference for numerical information, and therefore arguably not really performing a proper test of the scale.

-0.72), and, in line with previous research on numeracy (Peters et al., 2006), a median split was subsequently used to divide the subjects into groups with low versus high numeracy.[12]

Political predisposition was measured by the subjects' self-placement on a left-right scale ranging from 0 to 10. Subjects were subsequently classified as being either on the left of the ideological spectrum (self-placement; 0-3), on the center (self-placement; 4-6) or on the right (self-placement; 7-10).[13]

Manipulation check: At the end of the survey, respondents were asked to choose the correct number of youths not receiving an education among four alternatives (The surveys' "back" navigation button was disabled in order to prevent respondents from rereading the question containing the information). The correct answer was chosen by 88.2% of the respondents, indicating that most respondents had paid attention to the numbers presented. (Share of correct answers ranged from 82.9% to 92.8% across the four groups).

Study 2 Results

As expected, the preference for scenario B differed significantly across the four experimental groups, $\chi^2(3, N=1{,}007)=19.55$, $p>.001$. Furthermore, the relative placement of the four groups were exactly as expected, although not all differences were significant at the .05-level. As expected, the proportion of respondents indicating a preference for the tax increase scenario was lowest in the group where youths were framed with a small denominator and taxes were framed with a large denominator, it was higher in the groups were one of these denominators were changed and, finally, highest in the group were youths were framed with a large denominator and taxes framed with a small denominator. The main results are shown in Table 2 below:

Table 2: Proportion of respondents preferring Scenario B

Experimental Group	Proportion choosing Scenario B	95% CI	n
Group 1 (LD-tax, SD-youths)	$27.8\%_{a}$	21.3 - 32.3	250
Group 2 (SD-tax, SD-youths)	$30.2\%_{ab}$	24.5 - 35.8	252
Group 3 (LD-tax, LD-youths)	$35.7\%_{bc}$	29.8 - 41.6	252
Group 4 (SD-tax, LD-youths)	$44.3\%_{c}$	38.1 - 50.4	253

Note: Groups sharing a subscript letter are not significantly different at the 5% level.
The difference between group 3 and 4 is marginally significant at p=0.0501

The difference between the two groups that were also used in study one (LD-tax/SD-youths and SD-tax/LD-youths), was 17.5 percentage points (95% CI[9.2, 25.7], z=4.0, p>0.001), comparable in size to the effect found in study one. The results of study two thereby replicate the results from study one quite nicely, even though subjects of study two

[12] Numeracy items were also included in study one, but not used in this analysis due to the small sample size
[13] 14% of the subjects used the don't know option when asked to place themselves on this scale. These subjects' political predisposition was subsequently set to the median self-placement of the other respondents voting for the same party. [Excluding these subject from the analyses, does not change the results substantially]

were generally less likely to prefer scenario B than subjects in study one. As shown below, this difference is easily explained by the difference in respondents' political leanings in the two experiments.

In order to illustrate the relative magnitude of this effect, model 1 estimates the effects of the experimental manipulation and the subjects' political predisposition, gender, age and numeracy.

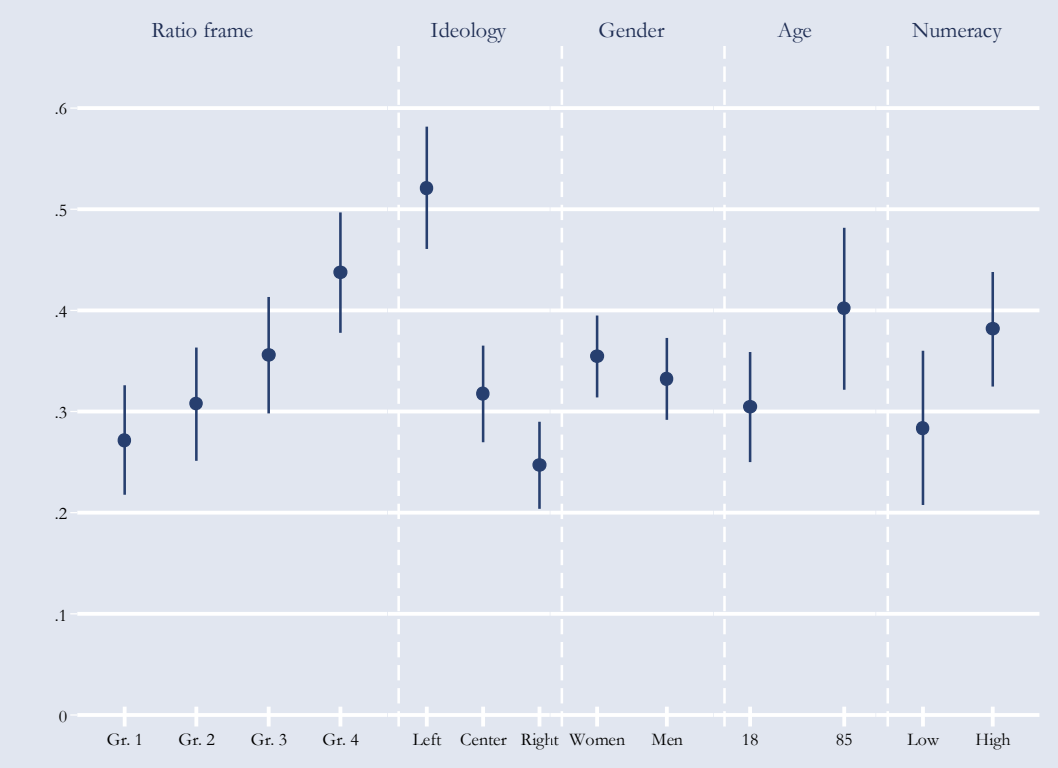
Table 3: Predicting preference for Scenario B

	(1)	
Manipulation		
Group 2	0.18	(0.20)
Group 3	0.42 *	(0.20)
Group 4	0.78 ***	(0.20)
Demographics		
Gender (male)	-0.10	(0.14)
Age	0.01	(0.00)
High numeracy	0.16	(0.14)
Ideology		
Center	-0.87 ***	(0.17)
Right	-1.23 ***	(0.18)
Constant	-0.61 *	(0.27)
Model χ^2	74.21 ***	
Pseudo R^2	0.06	
N	996	

Logistic regression coefficients, standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Based on model 1, the estimated probabilities of preference for Scenario B are illustrated in figure 1 below:

Figure 1: Likelihood of Preference for Scenario B (with 95% CI)



Neither numeracy, age nor gender show significant effects on the respondents' likelihood of choosing scenario B (we will return to the moderating effects of numeracy). As expected, the political leaning of the respondents did have a significant and large impact on preference for Scenario B. Among the left-leaning subjects, a majority of 52% preferred the tax increase in scenario B (95% CI[46,58]), whereas only 25% (95% CI [20,29]) of the right leaning subject preferred this scenario. The preferences of subjects placed at the middle of the ideological spectrum were relative close to the preferences of the right leaning subjects, with 32 % preferring the tax increase in Scenario B (95% CI [27,37]). Compared to these effects of basic political predisposition, the effects of the experimental manipulation are arguably very substantial.

Were the effects driven by the numerical framing of youths or the numerical framing of taxes? In order to estimate the relative effect of the two factors, the average effect of these two factors were estimated in model 2, shown in table 4 below. The model includes dummy variables for the framing of the two attributes, instead of dummies for the four experimental groups.

Table 4: Effects of framing on youths and taxes

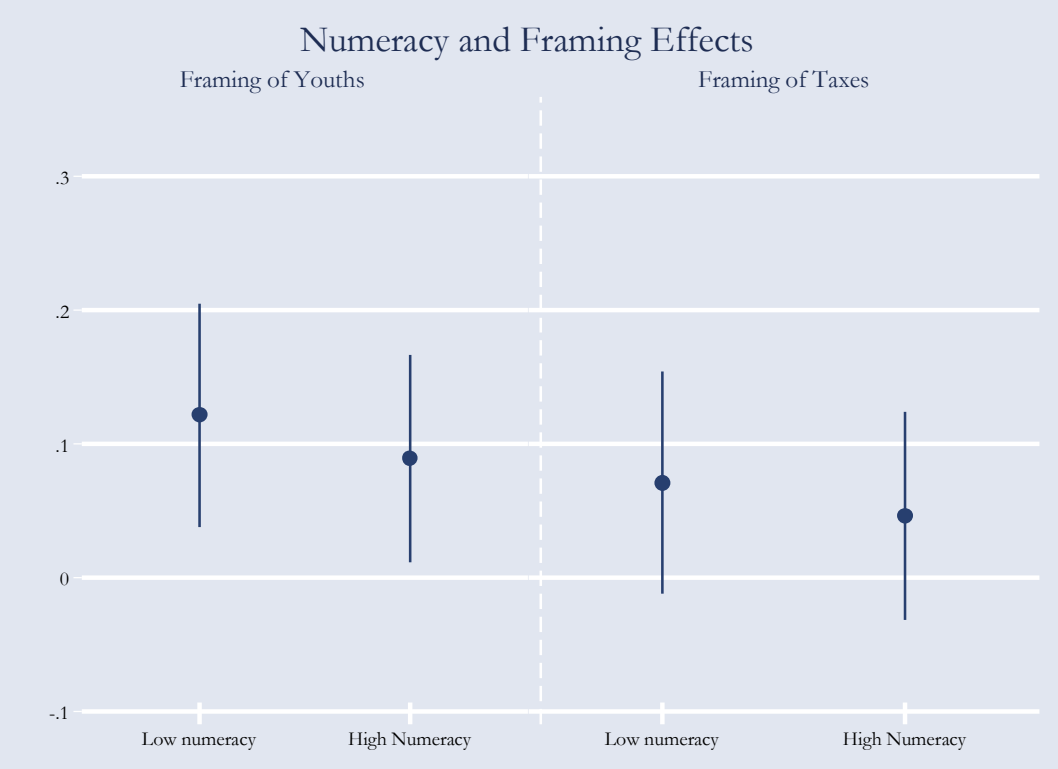
	(2)			(3)		
Manipulation						
LD-youths	0.52	***	(0.13)	0.88	**	(0.30)
SD-tax	0.27	*	(0.13)	0.32		(0.30)
Numeracy						
High numeracy				0.35		(0.25)
LD-youths × High numeracy				-0.18		(0.28)
SD-tax × High numeracy				-0.16		(0.28)
Ideology						
Center				-0.52		(0.30)
Right				-0.126	***	(0.32)
LD-youths × Center				-0.49		(0.34)
SD-tax × Right				-0.30		(0.35)
LD-youths × Center				-0.18		(0.34)
SD-tax ×Right				0.33		(0.35)
Constant						
Model χ^2	18.91	***		75.67	***	
Pseudo R^2	0.01			0.06		
N	1,007			996		

Logistic regression coefficients, standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The estimates of model 2 shows that, across the groups, lowering the denominator of the tax number increased preference for scenario B significantly with 6.0 percentage points (z=2.01, p= 0.045, two-tailed), whereas increasing the denominator used to describe the numbers of youths increased preference for scenario B with 11.5 percentage points (z=3.85, p> 0.001, two-tailed). The difference between the two coefficients is non-significant, $\chi^2(1, N=1007)=1.73$, p=.188. In other words, the effect of manipulating the ratio is significant both for youths and for taxes, and the difference of effects between these two factors is non-significant.

Next, model three was used to investigate how numeracy and political predispositions moderate the effect of numerical framing. In a non-linear model, the sign and significance of the interaction term does not necessarily tell much about the direction or magnitude of the interaction effect (Ai & Norton, 2003; Buis, 2010), and the analyses of interactions below therefore presents plots of the marginal effects, while the statistical significance of these marginal effects were estimated with the procedure suggested by Buis (2010). Starting with the moderating effect of numeracy, figure 2 below plots the effects of the framing of numbers of youths and the framing of taxes, conditional on the level of numeracy.

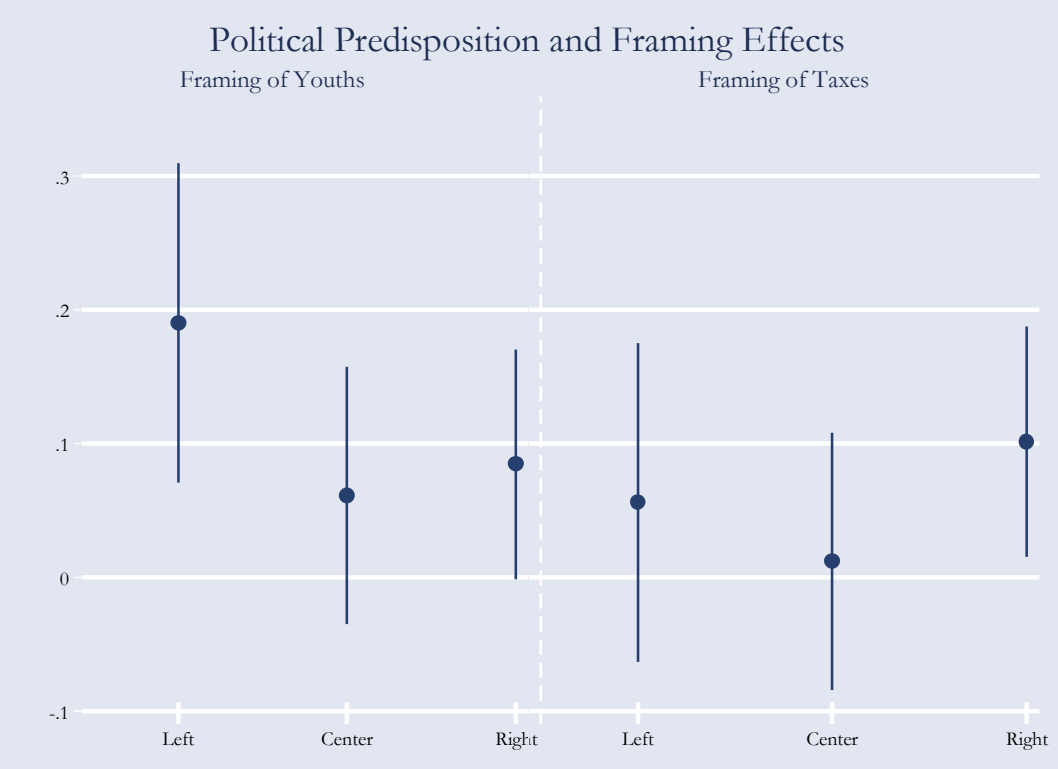
Figure 2: Marginal effects of numeracy (with 95% CI)



As illustrated in figure 2, the framing of numbers of youths, have a significant effect, for subjects both low and high on numeracy. Among the subjects classified as low on numeracy, framing the number of youths as *"4,900"* as opposed to *"7 out of 100"* results in an increase in support for Scenario B of 12 percentage points (95% CI [4,21]). The effect among subject classified as high on numeracy is 9 percentage points (95% CI [1,17]). While these results do suggest that subjects low on numeracy are indeed affected more by ratio bias, the difference in effects between the two groups is far from significant. (p=0.82, estimated using the procedure proposed by Buis 2010). When it comes to the numerical framing of taxes, the increased preference for scenario B associated with lowering of the denominator is non-significant for both subject lows on numeracy, where the difference is 7 percentage point (95% CI[-1,15]), and for subjects high on numeracy, where the difference is 5 percentage points (95% CI[-3,12]). Given these insignificant effects for the subgroups, the difference of effects between them is, unsurprisingly, also non-significant (p=0.73).

As the final step of the analysis, we turn to the question of how political predisposition might moderate the effect of the numerical framing. Based on model 3, figure 3 below plots the marginal effects.

Figure 3: Marginal effects of political predisposition (with 95% CI)



The framing of youths is more effective (in absolute terms) among left-leaning respondents: using the large denominator framing increases the preference for scenario B with 19.0 percentage points [95% CI[7.1,31.0]) from 42.6% to 61.6% among the left-leaning respondents, whereas the increase among right-leaning respondents is just borderline significant (p=0.054) with 8.5 percentage points (95% CI[-0.01,17.1] from 19.7% to 29.9%, and the effect among subject on the center is insignificant at 1.2 percentage points (95% CI[-1.5,18.7]. Hence, left-leaning subjects seem to respond more to this framing than respondents on the center and on the right (difference between left and center significant at p=0.025, while the difference between left and right significant at p=0.029, calculated using the procedure proposed by Buis 2010). When it comes to taxes, the increased preference for scenario B associated with lowering of the denominator is non-significant for both left leaning respondent, where the increase is 5.6 percentage points (95% CI[-6.3,17.5]), and subjects on the center of the left-right spectrum (95% CI[-.8.4,10.8]). However, rightwing subjects show a significant increase in support of 10.1% percentage points (95% CI[1.5,18.7). The effect sizes between the three groups do not, however, differ significantly. (all p>0.2)

General Conclusion and Discussion

Based on the results reported in this paper, there is little doubt that framing of numbers can have an effect on citizens' opinion formation about policy issues through ratio bias. The effect was shown to be of a substantial magnitude, it was successfully replicated across two experiments, and the effect was found across two different domains, namely numerical framing of money (in this case, taxes) and numerical framing of people (in this case, youths). Hence, the ratio bias effect may very well have substantial effects in politics, and future studies might want to investigate the degree to which political actors engage in this sort of numerical framing. Future studies might also want to further investigate whether personal level characteristics can moderate the ratio bias effect. However, the results of this study seem to suggest that ratio bias is a highly general phenomenon, that works across most groups of voters. This paper demonstrated ratio bias both among subject with low numeracy and high numeracy, and the study also found that ratio bias had an effect among subject on the left side of the political spectrum and the right side of the political spectrum. The only group that was not significantly affected by ratio bias was the subjects at the center of the ideological spectrum.

Second, the results of this study is also be important in a more general discussion about the current state and focus of framing research within political science The results suggests that we have been too quick to disregard the empirical relevance of equivalence framing, and therefore numbers, most likely because equivalence framing have been understood too narrowly as the frames used by Tversky and Kahneman (see, e.g, Borah (2012). Seen from the perspective of public opinion research, the experiments by Tversky and Kahneman are arguably rather low on ecological validity (Vraga, Carr, Nytes, & Shah, 2010, p. 2)— Very few of us actually get to choose how to treat deadly Asian diseases—but, as shown in this paper, frames of equivalence and numbers can impact us through other mechanisms than the ones suggested by Prospect Theory. The effect of ratio bias arguably seems to have more real life relevance for framing effects in public opinion research.

Third, the result of this study also have relevance for the literature on citizens' responses to policy relevant facts (Kuklinski et al., 2000; Lawrence & Sides, 2014). As noted in the introduction of this article, citizens' ignorance about policy relevant numbers, and the sometimes very limited impact of these numbers on political attitudes have often been lamented in this literature: The fact that citizens' are sometimes seen as unaffected by these numbers are presented as a lack of rationality in their opinion formation (see, e.g., Lawrence & Sides, 2014). The results of this study challenges this perspective in two ways. First, by showing that in some scenarios, attitudes can be affected significantly by numbers. Second, by showing than when investigating numbers, we should not just treat numbers as raw facts, we should also pay attention to the fact that our perception of policy relevant numbers are affected by numerical framing.

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