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# Selective Exposure, Political Polarization, and Possible Mediators: Evidence From the Netherlands

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## Abstract

One of the main lines of reasoning in the contemporary debate on media effects is the notion that selective exposure to congruent information can lead to political polarization. Most studies are correlational, potentially plagued with self-report biases, and cannot demonstrate time order. Even less is known about the mechanisms behind such an effect. We conducted an online quasi-experiment with a sample matching the characteristics of the Dutch population closely ( $N = 501$ ). We investigate how selective exposure can lead to polarized attitudes and which role frames, facts, and public opinion cues play. While we find that facts learned can help explaining attitude change and that selectivity can influence the perception of public opinion, we cannot confirm that people generally polarize.

In recent years, the topic of selective exposure has increasingly drawn the attention of political communication scholars. Selective exposure theory suggests that people tend to expose themselves to political information they agree with beforehand, which in turn increases political polarization (Stroud, 2011). One reason for the renewed attention for selective exposure is the transformation of the media landscape: It has frequently been argued that the high level of control, which users enjoy in online environments, leads to a predominant exposure to like-minded messages. Still, whether people at all exploit the abundance of information to select ideologically consistent political information is the topic of an ongoing scholarly debate with some arguing that selective exposure is prevalent and on the rise (Stroud, 2011), while others note

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that exposure to partisan media is not all that common (Prior, 2013), and that even those audiences who consume partisan media tend to get political information from both liberal and conservative sources (Gentzkow & Shapiro, 2011).

Whether many or only a few choose ideologically congruent political information, such selectivity could eventually cause the “leveling effects” of mass media to vanish (Bennett & Iyengar, 2008) and at least some polarization to occur. Despite some warnings that the association between selective exposure and political polarization may be an artifact (Prior, 2013), much accumulating evidence finds support for such an association. Theoretically, Bennett and Iyengar (2008) state that the traditional mass media formed a counterforce to political polarization by regularly exposing people to opposite viewpoints, given the journalistic norm of balancing, which dictates that for the most part at least two sides of a political controversy get a say in mainstream coverage. As the era of limited media choices has come to an end and cable television, but most importantly, a huge number of online media have entered the scene, the notion of selective exposure has gained influence in both scientific and societal discourse. But while many have argued that people become more extreme in their attitudes when they expose themselves to reinforcing information, little is known on how this presumed association works exactly (Dvir Gvirsman, 2014). In this article, we outline and investigate three possible mechanisms through which selective exposure may lead to polarization: (1) frame acceptance, (2) perceived opinion climate, and (3) knowledge gain.

Framing theory may offer a first explanation. Exposure to partisan media’s framing could lead to polarization, as the audience accepts the partisan frames (Jamieson & Cappella, 2008). Second, exposure to partisan media might make the users overestimate the amount of people sharing the opinion voiced by the outlet (Tsfati, Stroud, & Chotiner, 2013), which may cause polarization because people adjust their opinion toward the perceived group opinion (Stroud, 2010). Third, polarization may be caused by learning more about the topic: Knowledge can support motivated reasoning and may make people more polarized.

To systematically assess these three possible explanations, we conduct a survey-embedded quasi-experiment to investigate the extent of selective exposure as well as whether the effect of selective exposure on political polarization is mediated by these three components. Knobloch-Westerwick (2014) argues and demonstrates that observational studies are superior compared with self-reported investigation in the study of selective exposure. We test these effects in the context of the Dutch immigration debate. The 1990s are known as a period of increased immigration in many European countries (Boswell, 2005), but especially in the Netherlands (Berkhout & Sudulich, 2011). The

immigration issue has been part of the Dutch political agenda ever since, but in varying levels of prominence (Van Klingereren, Boomgaarden, Vliegenthart, & De Vreese, 2015). It is an issue on which people are likely to have formed an opinion already, which is a precondition for selective exposure to occur.

Our study contributes to the literature not only by investigating the mediated effects we propose, but also by testing selective exposure in the context of a European multi-party system: While many selective-exposure studies are conducted in two-party systems, predominantly in the United States, and operationalize partisanship as party identification, we define exposure to attitude confirming or disconfirming information on the issue level. This helps us better understand the extent, the mechanisms, and the effects of selective exposure: Our issue-specific approach contributes to the generalizability of selective exposure research to a context where clearly identified ideological outlets are not prevalent.

### Theoretical Background

#### Do People Selectively Expose Themselves to Likeminded Content?

The idea that people mainly expose themselves to political information that fits their prior attitudes has been voiced since the beginning of modern communication science (Lazarsfeld, Berelson, & Gaudet, 1944; Sears & Freedman, 1967; Zillmann & Bryant, 1985). Based on the idea that people strive to avoid cognitive dissonance (Festinger, 1957), it was assumed that they choose media outlets that do not confront them with viewpoints that conflicted with their cognitions. However, the practical relevance seemed to be limited by the fact that there were not many media outlets to choose from. Owing to the advent of cable television and the Internet, this has changed radically—and research on so-called selective exposure has relished a revival (Arceneaux & Johnson, 2013; Garrett et al., 2014; Knobloch-Westerwick & Hastall, 2010; Levendusky, 2013; Messing & Westwood, 2011).

Nevertheless, recent data call into question the pervasiveness of likeminded exposure. Stroud (2011, pp. 62–63) found that only 34% of National Annenberg Election Survey respondents who self-identified as conservative or liberals used like-minded partisan media with no cross-cutting exposure. Gentzkow and Shapiro (2011) find only weak evidence for ideologically based selectivity using unobtrusively measured web-tracking data: While more conservatives than liberals visit foxnews.com, many of foxnews.com visitors also visit liberal websites. Omnivorousness, they argue, outweighs ideological segregation over the Internet. In addition, only a slight minority is at all exposed to ideological cable television in the United States (Prior, 2013, p. 112; see also Dvir Gvirsman, Tsfati, & Menchen-Tervino, 2014). In sum, while some

scholars argue that ideological selective exposure is widespread, other evidence casts doubt on whether ideological selectivity is at all common. In light of this debate, we ask:

*RQ1: How widespread is ideologically consistent exposure?*

### Selective Exposure and Political Polarization

While it is one thing to show that such selective exposure occurs, it is another matter to investigate its effects. From a media effects perspective, one of the most interesting questions is to what extent selective exposure leads to increased polarization of the audience. In his model of reinforcing spirals, Slater (2007) argues that partisan content makes people more partisan, which in turn makes them even more likely to consume partisan content—fueling a spiraling process of polarization. But while there is evidence for such a polarizing effect (Iyengar & Hahn, 2009; Stroud, 2010, 2011; Warner, 2010), the spiraling process is partially counteracted by the fact that audiences overlap and few people rely on attitude-reinforcing partisan media exclusively (Gentzkow & Shapiro, 2011; Trilling, 2013). Accordingly, in a comprehensive review, Prior (2013) concludes that selective exposure can have an influence on polarization, but that it is less of a threat than pessimists fear.

Nevertheless, most scholars argue and ample evidence demonstrates, that selective exposure to attitude-consistent political materials can lead to attitude polarization of the individual. Therefore, we proceed to have a closer look at this effect and the possible explanations for its occurrence. It also leads to our first hypothesis:

*H1: Ideologically consistent exposure leads to attitude polarization.*

### Frame Acceptance

The idea that media frames influences audience interpretations is the basis of a branch of scholarship that studies framing as a media effect. Literature suggests that alternative interpretations of events in media texts can influence audience interpretations of events, their attributions of responsibility, and, consequently, their moral judgments (Chong & Druckman, 2007), cognitions, attitudes, and behaviors (Cappella & Jamieson, 1997). For instance, investigating a topic similar to the one under study here, Van Klingeren (2014) has shown how differently framed messages can change people's attitude toward immigration issues.

Research has identified a large variety of different frames, ranging from detailed issue-specific frames to broader generic frames (De Vreese, 2005; Matthes & Kohring, 2008). With regard to the topic under study, this is no

different. Scholten (2011) argues that the nexus surrounding the Dutch immigration issue is diverse and has evolved tremendously over time. He describes that in the late 1970s and early 1980s the topic was mostly discussed in terms of social-cultural emancipation; the problem-frame on citizenship and social-economic participation arose in the late 1980 and early 1990s. The latest change, since the 2000s, has been on transnationalism and assimilation, again stressing the cultural aspects of the issue. As these fluctuations demonstrate, the economic and socio-cultural perspectives have always intrinsically been part of the immigration debate; hence, our focus is on these two frames.

Jamieson and Cappella (2008, p. xiv) argued that exposure to conservative media (like the talk radio show of the conservative host Rush Limbaugh) promotes political polarization by offering and promoting alternative interpretations to those offered by mainstream media. They argue that exposure to the framing of political reality and reframing of mainstream media information makes the audience more likely to interpret reality “in a way that is both systematic and consistent with Limbaugh’s rhetoric.” However, while Jamieson and Cappella offered framing as a mechanism explaining why exposure to conservative media promotes political polarization, their empirical evidence is limited, as they do not investigate whether acceptance of frames mediates the association between ideological exposure and polarization. Yet, Sniderman and Theriault (2004) give some first evidence that exposure to congruent frames leads to more extreme attitudes: Even if two competing frames are offered simultaneously, people tend to accept the frame that is congruent with their prior attitude, which strengthens their attitude even more. Druckman, Fein, and Leeper (2012) contribute to the debate by investigating repeated framing effects. Among other things, they find that when given the opportunity, people tend to seek opinion-consistent information, and additionally they find that the persuasiveness of a counter-attitudinal frame is downgraded when people are repeatedly confronted with attitude consistent frames. If we extrapolate to the context of mainstream and ideological media, even if one is exposed to frames in mainstream media and counter-frames in ideological media (as is the case for most of the ideological media audiences according to the literature as reviewed above; in particular Jamieson & Cappella, 2008), polarization toward prior opinions will likely take place.

*H2: Ideologically consistent exposure leads to frame acceptance, which leads to attitude polarization.*

## Perceived Public Opinion

Another possible mechanism through which selective exposure promotes political polarization is through the potential effects of ideological media on

audience perceptions of the public opinion. First of all, selective exposure might lead to a biased perception of public opinion. For instance, it is not uncommon to overestimate public support for one's preferred candidate (Nir, 2011). As a consequence of this, people's preferences can be reinforced because, as Stroud argues, "people want to be perceived well by their fellow group members and hence adjust their opinions toward the perceived group mean" (2010, p. 558). Based on survey data from both Israel and the United States, Tsfati et al. (2013) demonstrate such an effect empirically: Congruent exposure to partisan media seems to lead to overestimation of public support for one's own position, which in turn leads to polarization (see also Tsfati & Chotiner, 2015).

More in general, we hypothesize:

*H3: Ideologically consistent exposure leads to a perception of public opinion that is in line with the own attitude, which leads to attitude polarization.*

### Knowledge Gain

Yet another possible mechanism explaining how selective exposure can lead to polarization is that the readers of (partisan) information learn new facts that can support their argument. Learning effects resulting from exposure to mediated political information have been documented across a wide variety of genres in a variety of contexts (Chaffee & Kanihan, 1997), and mediated information serves as an important factor shaping political attitudes (Zaller, 1992). At the same time, motivational factors seem to play a big role in political learning (Delli Carpini & Keeter, 1996). If we assume that people with a strong attitude toward a topic are motivated to engage with the topic, we can expect that partisans who use partisan media learn from the (partisan) facts they are exposed to, which might lead to even stronger attitudes. Motivated reasoning (Kunda, 1990) seems to make people ignore counter-attitudinal arguments (Matthes & Valenzuela, 2012)—especially those who Lodge and Taber (2000) call *partisan reasoners*. More in general, Zaller (1992) has argued that knowledge of facts matters for attitude formation. We expect:

*H4: Ideologically consistent exposure leads to knowledge gain, which leads to attitude polarization.*

### Forced Versus Self-Selected Exposure

A central component within the selective exposure framework is the element of *selection*. Although not always explicitly stated, it is assumed that polarization is not merely an effect of exposure to attitude-reinforcing content, but of

exposure to *self-selected* attitude-reinforcing content. One could argue, for example, that the act of *forcing* people to expose themselves to any kind of content might induce some opposition to it. A well-known finding in the cognitive dissonance paradigm shows that the effects of behavior on attitude formation is strongest when the behavior is not forced on experimental participants but self-selected (Hobden & Olson, 1994). The polarizing effect of ideological media could be viewed as a dissonance-reducing effect of exposure behaviors on attitudes. Viewed this way, the effect of congruent exposure on attitude polarization should be stronger when ideological exposure is selected and not forced on participants (see also the arguments put forward by Gaines & Kuklinski, 2011). However, Arceneaux and Johnson's (2013) experiments suggest that forced, and not self-selected, ideological exposure polarizes opinions.

To further investigate whether selective exposure effects are stronger because of people's selectivity or whether they occur simply because of the content people are exposed to, no matter whether they are free to choose or they are forced to use a specific item, we pose a research question (RQ) to test the moderating role of self-selection:

*RQ2: Do the expected effects differ between those who are allowed to select for themselves and those who are assigned an article to read?*

## Method

### Case Under Study

We test our hypotheses in the Netherlands. Owing to its multi-party system, the political landscape in general is not too polarized and, as pillarization has faded since the 1960s, Dutch voters have become pretty volatile in the past decade (Van der Meer, Lubbe, Van Elsas, Elff, & Van der Brug, 2012). Accordingly, some issues have sparked political debates in the past decade, with the issue of immigration and integration being maybe the most prominent one. Owing to the long-standing prominence of the topic in the media, we expect virtually everyone to have an opinion on the topic (Van Klingereren et al., 2015). This makes it a suitable test case for selective exposure research.

### Design

An online quasi-experiment was conducted by drawing a representative quota sample of the Dutch population from a research company's database (Panelclix). The term quasi-experiment relates to a research design that shares with regular experiments many structural elements, such as control groups, and pretest measures—but lack random assignment (Shadish, Cook, & Campbell, 2002). In our design, respondents were randomly assigned to



either choice or forced exposure conditions, but those in the choice condition themselves selected whether to expose themselves to congruent or incongruent information, and thus assignment to these conditions was not fully random. The company's database consists of about 140,000 people. Noncompliance was avoided by giving the respondents financial incentives. The data were collected between 13 and 17 February 2014. A total of 625 respondents started the study. It was agreed with the research company beforehand that only those questionnaires that were completed and where participants took at least 20 s<sup>1</sup> to read the stimulus material would be considered valid data, which resulted in a sample size of 501. The final sample was representative with respect to age (18–81 years), gender, and education (see Appendix for the comparison with population parameters).

### Stimulus Material and Manipulation Check

Two articles were carefully developed and typeset to mimic a real-world newspaper article as closely as possible. One article had a pro-immigration title and was written in a pro-immigration way. A second article was the same in terms of subject, factual content, and order of content as the first article, but was written from an anti-immigration standpoint and showed an anti-immigration title. The control group was exposed to an article on the celestial sphere.

Both the pro and the contra article included an economic frame, a cultural frame, a reference to the public opinion, and factual information. The facts used were always the same (unemployment rates, number of women wearing a headscarf); however, the interpretation (i.e., whether the rates are high or low; whether this is a sign for integration or not) was exactly the opposite. For instance, the sentences “The percentage of Non-Western immigrants without a job increased from 13.1% to 15.5%. Among natives, there was an increase from 4.2% to 5.0%” were followed by “This indicates that immigrants try less hard to connect to the labor market” (in the contra article) and by “This indicates that it is harder for immigrants to connect to the labor market” (in the pro article).

To determine whether the participants perceived the article as pro- or anti-immigration, we asked how they positioned the author of the article with regard to the topic of immigration. The scale ranged from 1 (*being anti-immigration*) to 10 (*being pro-immigration*). Those exposed to an anti-immigration article on average scored 3.4 ( $SD = 1.81$ ); those exposed to the pro-immigration article had an average score of 7.0 ( $SD = 1.47$ ),  $t(437) = -22.64$ ,  $p < 0.001$ , which indicates successful manipulation. Furthermore, we checked whether people noticed the

<sup>1</sup>Instructed to read as fast as possible, it took a convenience sample 25–30 s to read the article. Thus, even a fast reader cannot read it carefully in <20 s.

cultural and the economic frame in the stimulus material, which clearly referred to both aspects of immigration (e.g., unemployment and wearing headscarves). We asked to indicate on a 10-point scale to what degree the article was discussed in terms of health, art, sexuality, economy, and culture. They clearly recognized the cultural ( $M = 6.81$ ,  $SD = 1.78$ ) and economic frame ( $M = 7.63$ ,  $SD = 1.43$ ), and, in line with the expectations, did not recognize the three nonpresent items (respectively,  $M = 3.87$ ,  $SD = 1.75$ ;  $M = 3.48$ ,  $SD = 1.75$ ;  $M = 3.55$ ,  $SD = 1.94$ ). These results indicate successful manipulation.

Past research demonstrates that experimental participants' postexposure assessment of their likelihood to select similar news materials to those they read in the experiment can be of importance (Feldman, Stroud, Bimber, & Wojcieszak, 2013). We asked, "If you saw the headline of the piece you have read on the opinion pages of your newspaper, how likely would it be that you would really read it?" ( $M = 6.26$ ,  $SD = 2.09$ ,  $min = 1$ ,  $max = 10$ ; 69.5% selecting  $\geq 6$ ). However, there was no relationship between the likelihood to read the article in real life and attitude change ( $r = -.00$ ) or the absolute value of attitude change ( $r = -.02$ ), which means that we will also not further consider this variable in our model.

## Procedure

In the first part of the questionnaire, the participants were asked, among other things, about their political position and their opinion on immigration. After a brief distraction, during which people were asked to sketch something unrelated based on a description they were given and tell us what they had drawn,<sup>2</sup>  $n = 145$  of the participants were randomly assigned to one of the two treatments (forced condition),  $n = 62$  to the control group. The remaining  $n = 294$  was asked to select (self-select condition) one of the two articles while being shown the headings only.<sup>3</sup> In the forced condition,  $n = 70$  saw the pro article,  $n = 75$  the contra article. In the self-select condition,  $n = 142$  selected the pro article,  $n = 152$  the contra article. We deliberately oversampled the self-select condition by a factor of 2 to account for the fact that those who self-selected a specific article again can be divided into those whose prior attitude is congruent with the article and those with incongruent attitudes. Our design follows the designs proposed by Arceneaux and Johnson (2013, p. 60), Feldman et al. (2013) and Gaines & Kuklinski (2011). At the same time, we innovate on theirs by adding mediators to the model. After reading the article, the respondents were asked to fill out the second part of the questionnaire, which

<sup>2</sup>We asked to draw or think about what the following description represented: Please draw a square that is open at the top, then draw two half circles next to each other on top of the square. The image would show a slice of bread as commonly sold in the Netherlands.

<sup>3</sup>To keep conditions as similar as possible, also the respondents in the forced condition were shown both headings before (without choice) being directed to the stimulus material.

contained the same questions with regard to immigration, the mediators, a manipulation check, and several control questions.

### Variables<sup>4</sup>

**Dependent variable.** Attitude toward immigration was measured before and after exposure to the stimulus. To maximize reliability and comparability, we relied on items used in the European Social Survey (Meuleman & Billiet, 2012). We slightly adjusted the question wording to abbreviate the items. We used the ESS items<sup>5</sup> that measure *economic threat perceptions*, *cultural threat perceptions*, and *conditions under which immigrants should be admitted*, which taps into someone's general attitude toward immigration. An example for a question tapping into economic threats is, "Most immigrants work here and pay taxes. They also use medical and social services. Altogether, do you think that people who come here, cost more than they contribute, or do they contribute more than they cost?" An example for a cultural threat item is, "Do you think that cultural life in the Netherlands in general is undermined or enriched by people from other countries who come and live here?"

While the first six items (economic and cultural threats) loaded on one factor (eigenvalue<sub>pre</sub> = 2.84, explained variance<sub>pre</sub> = 83.9%, eigenvalue<sub>post</sub> = 3.64, explained variance<sub>post</sub> = 84.2%), the *condition-for-admittance*-items seemed to measure a different construct, which is why we excluded them and averaged the first six items to construct a scale ( $M_{pre} = 5.54$ ,  $SD_{pre} = 0.85$ ,  $min_{pre} = 2.83$ ,  $max_{pre} = 8.0$ ,  $\alpha_{pre} = .80$ ;  $M_{post} = 5.64$ ,  $SD_{post} = 0.84$ ,  $min_{post} = 2.83$ ,  $max_{post} = 8.67$ ,  $\alpha_{post} = .86$ ). A higher value implies a more positive attitude (Figure 1). We subsequently calculated the *attitude change*  $\Delta att = att_{post} - att_{pre}$  ( $M = 0.10$ ,  $SD = 0.64$ ,  $min = -3.17$ ,  $max = 3.33$ ), which we used as dependent variable.

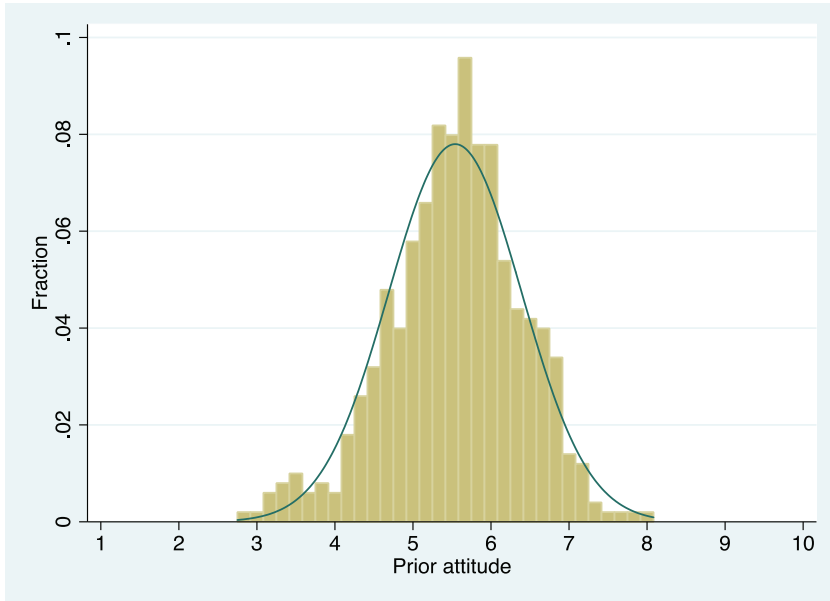
**Mediators. Frame acceptance.** To assess how participants frame the immigration issue after being exposed to the stimulus, we asked an open-ended question ("If you think of immigration, what do you think of foremost? Name up to three aspects and put the most important one on top"). A coding scheme was developed that distinguished between five frames. Each of the first two authors coded each answer independently, resulting in a high inter-coder agreement (frame first open answer: Krippendorff's (nominal)  $\alpha = .75$ , second answer:  $\alpha = .83$ , third answer:  $\alpha = .78$ ). Afterward, both coders discussed the cases on which they disagreed and decided on the final coding. In all, 48.7% used an economic frame in at least one of their answers, 51.5% a cultural frame, 14.6% a humanitarian frame, 20.16% a crime frame, and 5.6% a "the-country-is-full" frame.

<sup>4</sup>For the sake of transparency, we acknowledge that the data set also contained information on need for cognition, need to evaluate, and political knowledge.

<sup>5</sup>We used items D21, D26 and D27; D28, D40 and D41; D10, D12 and D16.

Figure 1

*Distribution of prior attitude toward immigration. Calculated as the average of six items, measured on 10-point scales, the measure of attitude towards immigration at  $t_0$  shows that most participants fancy a moderate, nonpolarized attitude ( $M=5.54$ ,  $SD=0.85$ ,  $\min=2.83$ ,  $\max=8.0$ )*



To find out whether the cultural and economic frames used in the article were *accepted*, we also coded the valence of the given answers for each cultural and economic frame that was mentioned ( $-1$  = anti-immigrant,  $0$  = not mentioned,  $+1$  = pro-immigrant). Coding was reliable: Krippendorff's (ordinal)  $\alpha = .78$ ,  $.77$ , and  $.69$ , respectively. Both coders discussed the cases on which they disagreed and decided on the final coding.

Per respondent we subsequently counted the number of frames *in line* with the framing of the article ( $n_{econ\_in-line}$  and  $n_{cult\_in-line}$ ). We defined frame acceptance as having mentioned more frames in line with the article than not in line with it ( $n_{econ\_not-in-line}$  and  $n_{cult\_not-in-line}$ ). Specifically, we defined economic frame acceptance as  $n_{econ} = 1$  if  $n_{econ\_in-line} > n_{econ\_not-in-line}$  (the frame was accepted),  $n_{econ} = -1$  if  $n_{econ\_in-line} < n_{econ\_not-in-line}$  (the frame was rejected), and  $n_{econ} = 0$  if  $n_{econ\_in-line} = n_{econ\_not-in-line}$  (there was no or an ambiguous acceptance). We did the same for  $n_{cult}$ . Indeed, some frames were valenced opposite to the article's direction (coded as  $-1$ ,  $n_{econ} = 88$  [17.6%],  $n_{cult} = 57$  [11.4%]), and some in line with the article (coded as  $+1$ ,  $n_{econ} = 12$  [2.4%],  $n_{cult} = 42$  [8.4%]). Most people, however, either did not mention one of the frames or mentioned them both in line with

and against the direction of the article (coded as 0,  $n_{econ} = 401$  [80.0%],  $n_{cult} = 402$  [80.2%]).<sup>6</sup>

**Knowledge gain.** We tested how many of the facts that were presented in the article were correctly recalled by the participants. After manipulation respondents were asked to decide whether the seven statements we presented them were correct, incorrect, or whether they did not know (which we counted as incorrect). Examples include “Among immigrants, unemployment rose by 2.4 percentage points” (which is correct), and “Turkish Muslims wear headscarves more often than Moroccan Muslims” (which is wrong). We calculated the share of correct answers ( $M = 0.56$ ,  $SD = 0.24$ ,  $min = 0$ ,  $max = 1$ ).

**Perceived opinion climate.** We measured perceived opinion climate with one item (“How do the Dutch in general think about immigrants?”) on a 10-point scale ranging from “*The Dutch in general are very negative about immigrants*” (1) to “*...very positive about immigrants*” (10),  $M = 4.39$ ,  $SD = 1.48$ ,  $min = 1$ ,  $max = 8$ .

**Independent variables.** We used dummy variables to indicate the condition to which the respondents were assigned, following the approach by Arceneaux and Johnson (2013, p. 176). In addition, to be able to determine whether people are engaging in selective exposure, we measured their political attitude on three 10-point left-right scales (general, economically, culturally), out of which only the cultural left-right position was of interest for this study ( $M = 5.21$ ,  $SD = 2.27$ ). We provided the respondents with the following definition for this dimension: “Left means that you tend to be a proponent of a multicultural society, right means that you tend to be a proponent of a monocultural society.” The scale was afterward reversed and centered so that positive values are consistent with a more pro-immigration attitude.

## Results

RQ1 inquired about the frequency of ideologically congruent selective exposure. To answer this RQ we focused on the self-selected condition only. Those who favor a monocultural society selected in 100 cases (68.5%) the contra article and in 46 cases (31.5%) the pro article, while those who favor a multicultural society showed the opposite pattern and selected the contra article in 52 cases (35.1%) and the pro article in 96 cases (64.9%),  $\chi^2(1) = 32.75$ ,  $p < .001$ . If we remove moderates who score 5 or 6 on the 10-point scale, the difference increases to 73.4% versus 26.6% among the monoculturalists and stays roughly the same among multiculturalists (34.7% vs. 65.3%),  $\chi^2(1) = 31.24$ ,  $p < .001$ . We can

<sup>6</sup>For both variables, among those who score 0, only  $n = 2$  do so because they mention opposing frames, which cancel each other out precisely. Therefore, when we constructed a six-mediator model in which we included one in-line and one not-in-line framing variable for each of the two frames, we did not get substantially different results.

conclude that while not everyone engaged in selective exposure, a clear majority did do so, and the rate of congruent selective exposure was much higher than what would be expected by chance.

When we have a first look at the different values of attitude change between the groups, an analysis of variance (ANOVA) reveals significant differences,  $F(4, 496) = 3.95$ ,  $p = .004$ . Those who self-selected the pro article became more positive ( $M = 0.24$ ,  $SE = 0.05$ ), but also those who were forced to read it did so ( $M = 0.21$ ,  $SE = 0.08$ ). Reading the contra article, be it self-selected ( $M = 0.02$ ,  $SE = 0.06$ ) or forced ( $M = 0.07$ ,  $SE = 0.08$ ), had no substantial effects, neither had being exposed to the control stimulus ( $M = -0.08$ ,  $SE = 0.06$ ).

To test our remaining hypotheses and RQs, we estimated a single structural equation model with attitude change ( $\Delta att$ ) as the dependent variable (see Table 1). We used four condition-dummies (forced exposure to pro-immigration article, forced exposure to contra-immigration article, pro-article selected, contra-article selected; the control group served as the baseline category). In the model, we added the interaction terms of these dummies and the respondent's cultural right-left position; this allows us to be able to see the difference between attitude-congruent and attitude-incongruent exposure. Table 1 shows both the direct effects of the different conditions on attitude change (last column) as well as the effects they have on the four mediators: cultural frame acceptance ( $M_1$ ), economic frame acceptance ( $M_2$ ), public opinion acceptance ( $M_3$ ), and knowledge gain ( $M_4$ ). Table 2 shows us the size and significance of the indirect effects of the conditions on attitude change via the mediators. For example, in Table 1, we see that "pro selected" has an effect of 0.25 on knowledge ( $M_3$ ). Knowledge, in turn, has an effect of  $-0.27$  on attitude change. Table 2, then, specifies that this indirect effect of pro selected via knowledge on attitude change amounts to  $-0.07$  and is significant.

The structural equation model exhibits an acceptable model fit (root mean square error of approximation [RMSEA] = 0.063, 90% CI [0.031–0.098],  $pclose = 0.222$ ). A RMSEA of 0.05 is commonly regarded as good, 0.08 as mediocre, 0.10 as the cutoff point for poor-fitting models (MacCallum, Browne, & Sugawara, 1996). The nonsignificant  $pclose$  value also indicates a rather good model fit. If we construct a model without the two frame-acceptance mediators (which do not affect the model and play no significant role in the selective exposure process, as the descriptive statistics already indicated and as we will further show below), we get an excellent RMSEA  $< 0.001$ . However, for the sake of clarity and consistency with our hypotheses, we chose to present the full model.

H1 predicts an effect of exposure on attitude change. One may expect a positive main effect of the pro conditions and a negative main effect of the contra conditions. If we look at the total effects presented in Table 2, we see

Table 1  
*Effects of independent variables on the mediators and effects of both the independent variables and mediators on the dependent variable.*

Predictors	Effect on M1		Effect on M2		Effect on M3		Effect on M4		Effect on DV: Change of attitudes	
	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	<i>b</i> (SE)	
<b>Independent variables</b>										
Pro selected	-0.02 (0.07)	0.04 (0.06)	0.25 (0.04)***	0.44 (0.22)*	0.39 (0.10)***					
Contra selected	-0.16 (0.07)*	-0.07 (0.06)	0.20 (0.04)***	-0.41 (0.21)*	0.19 (0.10)†					
Pro forced	-0.04 (0.08)	-0.08 (0.07)	0.23 (0.04)***	-0.12 (0.24)	0.36 (0.12)**					
Contra forced	-0.08 (0.07)	-0.04 (0.07)	0.21 (0.04)***	-0.33 (0.24)	0.20 (0.11)†					
Cult_pol	0.03 (0.02)	0.07 (0.02)**	-0.00 (0.01)	-0.00 (0.08)	-0.01 (0.03)					
Pro selected × cult_pol	0.04 (0.03)	-0.04 (0.03)	-0.00 (0.02)	0.13 (0.10)	-0.02 (0.05)					
Contra selected × cult_pol	0.02 (0.03)	-0.05 (0.03)†	-0.02 (0.02)	0.18 (0.09)†	0.05 (0.04)					
Pro forced × cult_pol	0.01 (0.03)	-0.02 (0.03)	-0.00 (0.02)	0.22 (0.11)*	0.05 (0.05)					
Contra forced × cult_pol	0.02 (0.04)	-0.03 (0.03)	0.02 (0.02)	0.27 (0.11)*	0.02 (0.05)					
<b>Mediators</b>										
M1: Cultural frame acceptance					0.02 (0.07)					
M2: Economic frame acceptance					0.05 (0.07)					
M3: Knowledge					-0.27 (0.13)*					
M4: Perceived public opinion					-0.00 (0.02)					
Constant	0.04 (0.05)	-0.13 (0.06)*	0.37 (0.03)***	4.37 (0.18)***	0.04 (0.13)					
R <sup>2</sup>	0.10	0.08	0.13	0.15	0.05					

*Note.*  $N = 501$ . Unstandardized coefficients from structural equation model with standard errors in parenthesis. Cult\_pol = mean-centered cultural political position (higher values: more positive toward multicultural society). Reading aid: The shaded area shows the effect of the independent variables on the mediators. The last column shows the direct effect of the independent variables and the effect of the mediators on the dependent variable.

†  $p < .10$ , \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ .

Table 2  
Direct, indirect, and total effects on the dependent variable of the model

Predictors	Specific indirect effects				Total indirect effects b (SE)	Direct effect b (SE)	Total effect b (SE)	Via economic frame acceptance b (SE)	Via knowledge b (SE)	Via perceived public opinion b (SE)
	Via cultural frame acceptance b (SE)	Via economic frame acceptance b (SE)	Via knowledge b (SE)	Via perceived public opinion b (SE)						
Pro selected	0.33 (0.10)***	0.39 (0.10)***	-0.01 (0.04)	0.00 (0.01)	-0.01 (0.04)	0.39 (0.10)***	0.00 (0.01)	-0.07 (0.03)*	-0.00 (0.01)	
Contra selected	0.13 (0.10)	0.19 (0.10)†	-0.11 (0.03)**	-0.00 (0.01)	-0.11 (0.03)**	0.19 (0.10)†	-0.00 (0.01)	-0.06 (0.03)*	0.00 (0.01)	
Pro forced	0.30 (0.11)**	0.36 (0.12)**	-0.09 (0.03)*	-0.00 (0.01)	-0.09 (0.03)*	0.36 (0.12)**	-0.00 (0.01)	-0.06 (0.03)*	0.00 (0.01)	
Contra forced	0.14 (0.11)	0.20 (0.11)†	-0.09 (0.03)*	-0.00 (0.01)	-0.09 (0.03)*	0.20 (0.11)†	-0.00 (0.01)	-0.06 (0.03)*	0.00 (0.01)	
Cult_pol	-0.01 (0.04)	-0.01 (0.03)	0.05 (0.03)*	0.00 (0.00)	0.05 (0.03)*	-0.01 (0.03)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	
Pro selected × cult_pol	-0.06 (0.05)	-0.02 (0.05)	0.00 (0.03)	0.00 (0.00)	0.00 (0.03)	-0.02 (0.05)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	
Contra selected × cult_pol	0.05 (0.04)	0.05 (0.04)	0.02 (0.02)	0.00 (0.00)	0.02 (0.02)	0.05 (0.04)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	
Pro forced × cult_pol	0.05 (0.05)	0.05 (0.05)	-0.01 (0.03)	0.00 (0.00)	-0.01 (0.03)	0.05 (0.05)	-0.00 (0.00)	0.00 (0.01)	-0.00 (0.01)	
Contra forced × cult_pol	0.02 (0.05)	0.02 (0.05)	0.01 (0.03)	0.00 (0.00)	0.01 (0.03)	0.02 (0.05)	-0.00 (0.00)	-0.01 (0.01)	-0.00 (0.01)	

*Note.* N = 501. Unstandardized coefficients with standard errors in parenthesis. Cult\_pol = mean-centered cultural political position (higher values: more positive toward multicultural society). Coefficients and standard errors for indirect effects were obtained using bootstrapping with 1,000 samples. Reading aid: The variable “pro selected” has a total effect of 0.33 on attitude change (first column). This effect can be split into direct and indirect effects (Columns 2 and 3). The indirect effects can be split into indirect effects via four different mediators (last columns).  
†  $p < .10$ , \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ , based on biased-corrected confidence intervals.



that this is indeed the case for positive conditions. However, those in the negative condition did not become more negative over time. This is in line with the ANOVA analyses presented above and indicates that ideological exposure only in the pro camp leads to more extreme views and therefore to polarization. Furthermore, it does not seem to matter whether the exposure was congruent or not: the total effects of all interactions with cultural right-left position are insignificant, which indicates that articles had a comparable positive effect on everyone, regardless of whether they selected a congruent or an incongruent article.

RQ2 suggests that people who were able to select articles themselves might experience stronger effects. Having a look at the *total* effects (Table 2), we see that being in one of the pro conditions leads to a positive attitude change of +0.33 points on the 10-point scale in the case of self-selection, or +0.30 in the case of forced treatment. As both coefficients are virtually the same (and their standard errors indicate that they do not differ significantly), and as there seems to be no substantial effect of being exposed to a contra stimulus, our results do not indicate that self-selection leads to more polarization than assigned articles, which is also what we saw in the one-way ANOVA. When reestimating the model without the interaction effects to ease interpretation of the main effects, both coefficients remain virtually the same (0.30 and 0.30).

### Mediating Effects

**Frame acceptance.** Self-selecting the contra article is associated with *not* accepting a culture frame in line with the article, independent of one's prior attitude. Regarding acceptance of the economic frame, prior attitude has an influence in the sense that those with a more positive prior attitude were those that were more readily accepting the economic frame, no matter which stimulus was read (see interacted effects in the "effect on M1" column in Table 1)—although those who self-selected the contra article were affected less. In most cases, however, the stimulus does not have any effect on frame acceptance. Moving along to the second part of the mediation (see last column in Table 1), we see that acceptance of neither the cultural nor the economic frame is related to the dependent variable: attitude change. Hence, exposure to the stimulus has little influence on any type of frame acceptance, which in turn does not affect polarization. Table 2 confirms that there are no significant indirect effects via frame acceptance on attitude change. Thereby, our second hypothesis is not confirmed.

**Perceived public opinion.** Contrary to our expectations, perceived public opinion does not predict attitude change (see last column in Table 1), which means that H3 cannot be supported (which is confirmed by the lack of significant effects in the "indirect effects via perceived public opinion" column in Table 2). Not everyone perceives public opinion in a similar way, though:

Those who read the pro article perceived public opinion as more favorable toward immigration, and those who read the con article perceived it as less favorable toward immigration (see “effect on M<sub>4</sub>” column in Table 1). However, this is only true when people were self-selecting the stimulus. As the interaction effects show, especially those who already had been more favorable toward immigration got even more positive even when reading the contra article.

**Knowledge gain.** In all conditions, people acquired knowledge about the topic at a similar rate, while none was acquired in the control group. However, people do not seem to use these facts for *motivated reasoning* in line with the article—the more they got to know, the more negative their attitude became. The magnitude of the effect is almost exactly the same for all stimuli ( $b$ 's between 0.20 to 0.25, all  $SE$ 's = 0.04; column “effect on M<sub>3</sub>” in Table 1). As also the path from knowledge to the dependent variable attitude change is significant ( $b = -0.27$ ,  $SE = 0.13$ ; last column in Table 1), one could say that exposure to any stimulus (compared with the control group) has an indirect effect of approximately  $-0.06$  on *attitude change* via the mediator *knowledge*, which is also confirmed by the bootstrapped coefficients presented in Table 2. As there are no differences between the stimuli, and also prior attitude is completely unrelated, we have to conclude that the knowledge gain cannot be attributed to any kind of selective exposure mechanism. Hence, H<sub>4</sub> cannot be confirmed. Instead of the different effects owing to selective exposure, we find a clear persuasion effect: No matter what someone's prior attitude, no matter what the attitude voiced in the article is, and no matter whether they align or not: People seem to learn at the same rate, and the learning seems to influence their attitude in the same direction.

## Conclusion and Discussion

Our study explored the prevalence of ideologically congruent exposure using a quasi-experimental setting. Our design closely followed the emerging paradigmatic selective exposure design in laboratory studies and in online survey experiments (and was similar to the designs implemented by Arceneaux & Johnston, 2013; Feldman et al., 2014; and Gaines & Kuklinski, 2011), but did so in a multi-party European context, with relatively little predominantly partisan options on citizens' media menus, and focusing on an issue about which a majority of the population held centrist opinions. Consistent with Feldman et al.'s (2014) results, we found evidence for ideological selectivity, and (again consistent with Feldman et al., 2014) even a stronger rate of ideological selectivity when removing moderates from our analysis and focusing on partisans only. Despite the obvious shortcomings of an approach that presents participants with stimuli to study selectivity (Clay, Barber, & Shook, 2013;

e.g., the fact that only relatively few options are presented to participants), our findings regarding the extent of selectivity obtained using a quasi-experimental design in the Netherlands present an important addition to the literature.

The main aim of this study was to take a look inside the black box of selective exposure effects and see how selective exposure leads to polarization. We innovated upon extant research not only by looking at potential mediators that cause voters to polarize, but also by testing the selective exposure mechanism in a European multi-party setting. One of our main findings is that selective exposure occurs, but does not necessarily lead to polarization.

There are some plausible explanations. First of all, the fact that this study was done in a European multi-party system may have contributed to this nonfinding. Whereas most previous selective exposure studies were done in the United States, hence in a two-party system where partisan media play an important role, the context of this study does not have any clearly partisan media. Even when voters in the Netherlands are selective in their media outlets, they are still exposed to messages that are not extreme in their position. Our stimulus material might be discarded as too extreme or simply seen as someone's opinion that should not be taken too seriously. Consequently, short-term effects of a single article are fairly unlikely, even though the setup of our study did not differ too much from that of earlier selective exposure studies. On a related note, one might speculate that the fact that there were little to no effects of exposure on frame acceptance (and, in fact, more people counterargued rather than followed the frames) might be explained by the way these frames are used in the overall discourse: Again, the frames in the material might have been perceived as more extreme than expected, thereby triggering nonacceptance and counterarguing.

Second, although the issue generally is perceived as controversial, most people entertained a relatively moderate opinion. The attitude toward immigrants and the cultural left-right position are approximately normally distributed, which implies that most people hold opinions close to the mean. It makes sense that people of a centrist position do not "radicalize"—they might not even perceive themselves to be in favor of one or the other side. For them, the notion of congruent exposure does not make much sense. This is the opposite of the situation in studies where most people are able to identify themselves as, for example, either Democrats or Republicans. For selective exposure to fuel a process of polarization, thus, it might be necessary that there *is* a certain amount of polarization to start with. To investigate this possibility, we call for more research in countries with multi-party systems, a culture of consensus rather than competition, and comparatively few cleavages in society.

Third, counteracting forces might be at play, which are not fully understood yet. For instance, in contrast to the predominant view that cross-cutting

exposure to divergent viewpoints prevents polarization, Arceneaux and Johnson (2013) argue that polarization is most likely to occur when viewers watch programs that *oppose* their beliefs. They write that “in the process of defending their attitudes, individuals end up adopting more extreme views than those they held before” (p. 88). Future research, we suggest, should try to systematically disentangle whether such counterarguing outweighs the effect of attitude reinforcement.

Having said this, we also found that among those who read an attitude-congruent article, either forced or selected, the pro-condition showed to have a stronger overall effect on attitude change. Although puzzling and in contrast with Soroka’s (2006) asymmetrical influences thesis, which assumes a larger effect of negative (contra) news, this finding is in line with those of previous studies on the impact of immigrant-related news media (Boomgaarden, 2007; Van Klingeren et al., 2015). The stronger effect of positive (or pro) messages in these studies is explained by the notion that there is a slight tendency to discuss the immigration issue in a negative way, and that because people have gotten so used to this negative perspective, any divergence from this norm will have a relatively larger impact.

Furthermore, notwithstanding our general lack of a support for the polarization thesis, we see that especially knowledge can fuel a change in attitude. In our case, knowing more about immigration made people more negative about immigration. We see little evidence, however, that people *selectively* process the new information, as the theory of motivated reasoning would assume: People do not use the information to bolster their preexisting attitudes and consequently polarize. On the contrary, the information seems to have the same effect on everyone, no matter which interpretation is given, and no matter what the preexisting attitudes are. This indicates a straightforward persuasion effect, although it does not preclude that this effect can be overshadowed by counteracting forces (like the direct effect of reading the pro article, which is slightly bigger in size). We can only speculate why knowledge leads to negative, and not positive, attitudes. Although we carefully selected facts that we thought not to favor one side, it might be that the participants perceived this differently. Another explanation might be that more elaboration (reflected by learning more facts) just triggered the participants to perceive the issue as a *problem*.

One of the main characteristics of our quasi-experimental study has to do with the fact that participants in the self-selected condition were not randomly assigned to a treatment but rather selected it themselves. Feldman et al (2014, p. 173) state that “by giving respondents a choice, controlled experimental settings can get closer to the real world where people have many media options.” Forced settings demonstrate potential treatment effects, they argue, while “choice-based

experiments produce more realistic treatment effects but introduce a self-selection bias since randomization cannot be established.” This approach, which modifies the traditional experimental design by supplementing it with a choice component, is gaining more and more influence in selective exposure research (Arceneaux & Johnson, 2013) because “the prototypical randomized experiment, the purported gold standard for identifying cause and effect, falls short when the treatment under study is prone to self-selection in the population and the researcher aims to draw meaningful inferences about its effect” (Gaines & Kuklinski, 2011, p. 724).

Regardless of the fact that it is not fully randomized, it is important to note that our study is superior to correlational studies in three respects. First, causal order is established and thus it is not possible to argue that the polarized attitudes measured after choice and exposure caused the selection. Second, as an observational study it is by definition superior to self-reported accounts of selective exposure that are known to be biased and heavily influenced by prior attitudes (Knobloch-Westerwick, 2014; Prior, 2013). Third, the assignment to the forced and self-selected condition was randomized, and thus we should remember that the comparison between these groups (investigating one of the RQs in this study—RQ2) is still experimentally valid. Despite these considerations, we did not observe a difference between the effects of self-selection and forced exposure on attitude change. Based on Gaines and Kuklinski’s (2011) work one would expect a stronger effect of the former, but not based on Arceneaux and Johnson’s (2013) experiments. It seems necessary to further disentangle the mechanisms underlying self-selected versus forced exposure. One explanation why we did not find a difference in our experiment might be, of course, that we did not observe much polarization in the first place, as we discussed above.

It should also be noted that recently, the way mediation is commonly tested in experiments has been called into question (Green, Ha, & Bullock, 2009; Imai, Keele, Tingley, & Yamamoto, 2011). It is argued that both research design and analysis are not suitable to draw strong causal inferences, because they rest on strong assumptions that are usually hardly met. We therefore do not want to claim that our study can give definitive answers regarding the causality of mediated effects of selected exposure, but at the same time, it is theoretically highly unlikely that some of the proposed relationships run in the other direction. In light of this, we see our study as an important step toward designing subsequent research, by providing both theoretical arguments and some first empirical evidence on the complex interplay between selective exposure, cognitions, and polarization.

## Appendix

Table A1

*Representativeness of the sample compared with the entire population*

Variable	Population	Sample
Men	49.5%	47.9%
Women	50.5%	52.1%
Age (years)		
<20	23.1%	4.6 <sup>a</sup> %
20–39	24.6 [32.0]%	31.7%
40–64	35.5 [46.2]%	49.2%
65–79	12.6 [16.4]%	14.2%
>80	4.2 [5.5]%	0.2%
Low education	29.0%	22.4%
Medium education	43.0%	46.1%
High education	28.0%	31.6%

*Note.* Figures regarding the population were retrieved from the Web site of Statistics Netherlands (Centraal Bureau voor de Statistiek).

<sup>a</sup>Please note that we did not include minors, and therefore, in the sample, the categories range only from age 18 to 20 years, which explains the differences between the two figures. Between square brackets the proportion of the population of  $\geq 20$  years.

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