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THE IMPACT OF AGE AND EDUCATION ON RESPONSE ORDER EFFECTS IN ATTITUDE MEASUREMENT

BÄRBEL KNÄUPER

Abstract This article addresses the impact of age and education on the emergence of response order effects. Previous research has suggested that response order effects are stronger for respondents with less formal education. Education, however, is negatively correlated with age. Therefore, the apparent educational differences in response order effects might in fact reflect differences in age. A series of meta-analyses of experiments originally conducted by Schuman and Presser and subsequent hierarchical logistic regression analyses demonstrate that there is a statistically reliable relationship between response order effects and age and that this relationship is not explained by education. Age-related differences in response order effects may undermine substantive conclusions about age or generational differences in attitudes.

It is well established that the order in which response alternatives are presented in a list can profoundly affect reports of attitudes (e.g., Schuman and Presser 1981; Schwarz and Sudman 1992; Tourangeau and Rasinski 1988) as well as behaviors (Schwarz and Sudman 1994). This has been shown even for the presentation of a small number of response alternatives, such as only two or three items (see Schwarz, Hippler, and Noelle-Neumann 1992 for a review). Schuman and Presser (1981) report on re-

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sponse order effects for several such questions, for example: "Should divorce in this country be easier to obtain, more difficult to obtain, or stay as it is now?" In this question, the endorsement of the last two alternatives depends strongly on the order in which they are presented (Schuman and Presser 1981, p. 65). Response order effects occur as either primacy effects (greater endorsement of an alternative when presented early in the list) or recency effects (greater endorsement when presented late).

In previous research, respondent education has been linked to the emergence and size of response order effects (e.g., Alwin 1998; Alwin and Krosnick 1991; Krosnick 1992; McClendon 1986). A meta-analysis of Howard Schuman and Stanley Presser's attitude experiments recently conducted by Narayan and Krosnick (1996) revealed statistically reliable—though not large—education effects. The authors found larger effect sizes for respondents with low educational attainment as compared to respondents with medium and high education. Building on earlier theoretical reasoning (cf. Schuman and Presser 1981, p. 71) and their own work on satisficing response behavior, Sowmya Narayan and Jon A. Krosnick argued that less formal education is associated with a lack of "cognitive skills" or "cognitive sophistication." According to Krosnick, cognitively less sophisticated respondents are those "for whom complex, abstract, and extensive cognitive activities are difficult and therefore not enjoyable" (Krosnick 1992, p. 203). These respondents should be more likely to conduct "inadequate memory search and superficial evaluation of responses" (p. 205) or to "satisfice." They should be less able to elaborate response alternatives equally and therefore more likely to focus their attention on items presented earlier or later in the list, resulting in primacy or recency effects (see Krosnick 1991; Krosnick and Alwin 1987).

Krosnick and colleagues thus found a reliable relationship between response order effects and education and propose that "cognitive sophistication" explains this relationship. No direct empirical evidence for this reasoning has as yet been provided. Rather, respondents' years of formal education (Krosnick and Alwin 1987), vocabulary test scores (Krosnick and Alwin 1987), or college students' grade point averages (Krosnick 1992) have served as proxies for cognitive sophistication in secondary analyses. Hence, it is unknown whether scoring high on any of these measures indeed indicates a lack of "abilities needed to retrieve information from memory and integrate that information into verbally expressed summary judgements" (Krosnick 1991, p. 222)—that is, the process assumed to mediate educational differences in response order effects.

The present research has the same problem: no direct evidence can be provided for the proposed theoretical reasoning. However, it will be demonstrated that most of the variance previously attributed to education is, in fact, due to age. It will be argued that the age effects are mediated by individual differences in working memory capacity. Educational attain-

ment is negatively correlated with respondent age in the available data sets. Due to a general increase in the level of education over the past century, younger cohorts have received more years of formal education than older cohorts. This article attempts to disentangle the effects of age and education on response order effects and offers lack of working memory capacity as a theoretically reasonable explanation for greater response order effects in the elderly. The data available for the present report do not permit testing the proposed mediational roles of working memory capacity or cognitive sophistication directly—this will be up to future experimental research.

Working Memory Capacity

The emergence and magnitude of response order effects depends critically on the amount of elaboration that each individual response alternative receives (cf. Krosnick and Alwin 1987; Schwarz, Hippler, and Noelle-Neumann 1992). Answering questions with response alternatives requires the simultaneous storage, retrieval, organization, elaboration, evaluation, and manipulation of information. These are precisely the processes that cognitive psychologists assume to operate in a cognitive module called “working memory.” Working memory refers to the ability to simultaneously process and elaborate new incoming information while at the same time executing rehearsal or storage processes on earlier encoded information (cf. Baddeley 1986). The less working memory capacity an individual has available, the less he or she will be able to process, evaluate, and elaborate the presented information. Accordingly, respondents low in working memory should be especially susceptible to response effects in surveys.

The amount of information that can be stored and processed simultaneously at one point in time, that is, working memory capacity, has been found to be significantly smaller for older people than for younger people. Cognitive psychology has demonstrated that the biological processes that accompany normal aging causes this decline (e.g., Craik and Jennings 1992; Stine and Wingfield 1990; see Salthouse, Babcock, and Shaw 1991 for a review). It has been shown to begin in early adulthood and increases slowly and linearly with age. Some research has suggested a more rapid decrease after around age 65 (cf. Schaie 1996). The cognitive decline has been found to result in, among others, text comprehension problems (see Kemper and Kemtes 1998) and problems in learning and reasoning (for overviews, see Craik and Salthouse 1992).

Evidence for the critical role of working memory capacity as a predictor of response quality in surveys has been presented by myself and my colleagues (Knäuper et al. 1997; Knäuper and Wittchen 1994, 1996). Respon-

dents low in cognitive capacity more often give "don't know" responses than respondents high in cognitive capacity (Knäuper et al. 1997), and use heuristic strategies rather than exhaustive memory search strategies to answer complex standardized survey questions (Knäuper and Wittchen 1994, 1996). These results suggest that respondents' low in working memory capacity, for example, older people, should also be more susceptible to response order effects in surveys. This is the proposed theoretical reasoning underlying the age effects presented in this article.

AIMS

Our aim is to determine whether there is a statistically reliable relationship between response order effects and age and whether or not this relationship is explained by education. For this purpose, Schuman and Presser's (1981) response order experiments were, once again, reanalyzed. A series of meta-analyses was conducted to identify independent and shared contributions of age and education to the size of response order effects. Subsequently, hierarchical logistic regression analyses were used to control simultaneously for the effects of age and education and to determine the unique effects of age and education, respectively.

SECONDARY ANALYSES: THE IMPACT OF AGE

Description of the data. The data sources for the meta-analysis are all response order experiments ($N = 14$) originally conducted as part of a larger series of experiments by Schuman and Presser between 1971 and 1980.¹ The authors documented their findings in their 1981 book *Questions and Answers in Attitude Surveys*. All experiments had been conducted in representative national samples, and all were telephone interviews. The design of the experiments was essentially the same for all 14 response order experiments: all respondents received exactly the same question, but the order of the response alternatives was reversed for a random half of the respondents. An overview of the 14 experiments is provided in table 1. In this table, the experiments are ordered in terms of the significance of the overall response order effect (i.e., the effect for the full sample). Experiments 1–7 yielded significant response order effects in the original analyses carried out by Schuman and Presser, while the effects for the remaining experiments did not reach conventional levels of

1. Three experiments ("work values": Survey Research Center [SRC] February 1977, and August 1977; and "most important problem": SRC February 1977) were not included because they were different in style: instead of presenting two or three response alternatives in just two different orders, these experiments involved presenting five response alternatives in five different orders, and three alternatives in three orders, respectively. In accord with Narayan and Krosnick (1996), these experiments were not included in the meta-analysis since there is no simple way to summarize the sizes of the effects.

Table 1. Overview of Experiments and Response Order Effects by Age

Experiment, Year (Page Number) ^a	Response Alternative	Overall Response Effect (%) ^b	Young (18-64 Years)				Old (65+ Years)			
			Response Effect	N		N	Response Effect	N		N
				Form 1	Form 2			Form 1	Form 2	
1. Oil companies, April '79 (p. 60)	Each company	+7.83*	+5.44	383	188		+22.33*	62	26	
2. Oil supply, January '79 (p. 60)	Plenty	+13.81***	+14.50***	267	248		+6.77	26	25	
3. Oil supply, April '79 (p. 60)	Plenty	+8.09*	+8.86*	387	194		+2.38	56	24	
4. Divorce, April '79 (p. 65)	Stay as is	+12.39**	+11.66*	141	299		+18.65	22	60	
5. Divorce, September '79 (p. 67)	Stay as is	+10.14*	+6.77	188	210		+32.10*	34	34	
6. Adequate housing, August '79 (p. 70)	Government	-15.07***	-14.25*	278	293		-21.74*	34	36	
7. Adequate housing, September '79 (p. 71)	Government	-6.93+	-5.56	266	254		-14.58	37	43	
8. Oil companies, January '79 (p. 60)	Each company	+5.68	+4.55	262	244		+16.15	25	27	
9. Vietnam aid, April '79 (p. 65)	Right amount	+3.91	+3.83	57	125		+4.82	6	16	
10. Unions, June '79 (p. 69)	Required	-3.05	-4.81	283	275		+8.83	46	32	
11. Individual/social conditioning, December '78 (p. 69)	Individual	+1.28	+1.82	270	145		-5.32	39	14	
12. Open housing, November '79 (p. 70)	Can decide	+2.29	+4.16	289	316		-5.80	51	46	
13. Divorce, June '79 (p. 66)	Easier	-2.08	-4.29	250	245		+23.74*	26	35	
14. Individual/social conditioning, February '80 (p. 210)	Agree	-2.8	+1.25	420	423		-10.98	60	61	

^a Refers to the page number in Schuman and Presser (1981) on which the results of the specific experiment are reported.

^b This column reports the difference in the percentage of respondents giving the same answer to two different versions of the question. For example, for the oil supply response order item, the overall response effect is the difference between the percentage of respondents saying 'plenty of oil' on the form where it is offered as the first of two options and the form where it is offered as the second of two options. The results in this column agree with the results reported by Schuman and Presser (1981) but not precisely with the results reported by Narayan and Krosnick (1996, app. table A1). The latter authors had to confine their analyses to those respondents whose educational attainment was ascertained, whereas the analyses presented here include all respondents, because no information on respondent age was missing. In this column, a plus sign indicates a recency effect and a minus sign indicates a primacy effect. This notation follows the one used by Narayan and Krosnick (table A1). Some inconsistencies between the present and Narayan and Krosnick's table (Narayan and Krosnick 1996, appendix table A1) are apparently due to incorrect signs in Narayan and Krosnick's table (see n. 2).

+ $p < .10$.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

statistical significance. The sample sizes for the 14 different experiments ranged from $N = 202$ to $N = 915$ participants, and all respondents were age 18 or older. Additional details on the study designs as well as the exact question wordings of each experiment can be found in Schuman and Presser (1981).

Table 1 presents the response effect found for the full sample as well as separately for respondents below and above age 65. A cutoff of age 65 was chosen based on longitudinal studies on intellectual development, which demonstrate that the most pronounced decline in memory functioning has its onset around age 65 (for a review, see Schaie 1996). The response effect is expressed as the percentage difference between response order conditions (first vs. last) for a specific response option. A plus sign indicates a recency effect and a minus sign a primacy effect.²

As can be seen, for 12 out of the 14 experiments the percentage difference by response order is larger for older than for younger respondents (the two exceptions are the "oil supply" experiments conducted in January and April 1979). In some cases older respondents show response order effects that are about six times as large as the effects displayed by younger respondents. Across all 14 experiments the mean absolute difference between response order conditions is about twice as large for older respondents than for younger respondents (13.87 percent vs. 6.56 percent, respectively). This overview provides the first support for the hypothesis that response order effects—whether primacy or recency effects—are more pronounced in older age.

Meta-analysis: age effects. A meta-analysis was conducted in order to test whether the observed age differences are statistically reliable across the individual studies. The advantage of a meta-analytic procedure is that it maximizes statistical power and that it allows it to go beyond the idiosyncratic characteristics of the individual studies (e.g., Rosenthal 1991). The first issue is of particular importance here because the sample sizes employed by Schuman and Presser were planned primarily to allow the testing of two-way interactions (question form by response) but not their interactions with a third or even fourth variable such as respondent age

2. Several of the signs in Narayan and Krosnick's (1996) table appear to be incorrect (app. table A1). Specifically, all signs for the "adequate housing" experiment (September 1979) should be negative (primacy effect). The sign for the medium education group in the "oil companies" experiment (January 1979) should also be negative. The sign for the low education group for the "Vietnam" experiment (April 1979) should be positive. The overall effect for the "union" experiment (June 1979) should be negative. The sign for the high education group for the "open housing" experiment (November 1979) should be positive. The sign for overall effect for the "divorce" experiment (June 1979) should be negative, for the medium education group it should also be negative, and for the high education group it should be positive. The sign for the overall effect for the "individual/social conditions" experiment (February 1980) should be negative, for the low and high education groups the sign should also be negative, and for the medium education group it should be positive.

or education. Due to small samples for the individual studies, existing effects might not be detected (Type II error). It is therefore not surprising that—in spite of clearly observable age-related patterns—log-linear analyses of the effects presented in table 1 yielded statistically reliable or marginally reliable three-way interactions (question form by response by respondent age) for only three out of the 14 experiments. These were the experiments on oil companies (April 1979, $p < .09$), and the two experiments on divorce (September 1979, $p < .09$; June 1979, $p < .03$). Meta-analyses overcome the problem of small sample sizes by combining the effect sizes of a series of studies (cf. Rosenthal 1991).

In order to allow a direct comparison to Narayan and Krosnick's (1996) findings, the same procedure for conducting the meta-analysis was used (e.g., Rosenthal 1991). As described in more detail in Narayan and Krosnick, the meta-analytic procedure involved converting the χ^2 values obtained for each experiment and age group separately into measures of effect size (Cohen's d). The effect sizes d were then combined across all 14 studies within each age group. In a final step, the meta-analysis compares the mean effect sizes d of the two age groups.

An important difference between the present analysis and the one reported by Narayan and Krosnick (1996) has to be noted, however. Narayan and Krosnick argued that it would make no sense to use a study that was not statistically significant for the full sample of respondents to identify a moderator variable such as education. They claimed that the inclusion of nonsignificant experiments could wipe out expected educational differences. They therefore excluded all nonsignificant studies, leaving only seven out of the 14 response order experiments for their analyses.

However, a nonsignificant effect in the full sample could simply be the result of a large subgroup of respondents not being susceptible to variations in response order, whereas another smaller subgroup might show the expected response order effects. Not being able to detect these effects would then simply be due to a lack of statistical power. As a consequence, one would incorrectly accept the null hypothesis. Indeed, table 1 shows that some of the largest differences in effect sizes between age groups are found for experiments that were nonsignificant in the full sample (e.g., experiments 8, 13, and 14). Furthermore, different subgroups might display response order effects in opposite directions (i.e., primacy vs. recency effects). These effects would balance each other out for the full sample, again resulting in nonsignificant overall effects (despite meaningful differences for subgroups). Exclusion of half of the experiments thus contradicts the rationale underlying meta-analytic procedures (e.g., Rosenthal 1991). Consequently, all 14 experiments were included in the present meta-analysis. For the purpose of comparison, however, analyses were also conducted for just the seven significant experiments used by Narayan and Krosnick, and these findings are briefly reported below.

RESULTS OF THE META-ANALYSIS

Table 2 presents the results of the meta-analysis by age groups, using different methods for accounting for the problem of dependent observations (see below). As hypothesized, the mean effect size was reliably larger for older respondents ($d+ = 0.31$) than for younger respondents ($d+ = 0.14$; $Z = 2.46$, $p < .007$, Z -test, Rosenthal 1991). This result strongly supports the assumption that older respondents are more susceptible to response order effects than are younger respondents. Including only the seven studies that were significant for the full sample also revealed a larger mean effect size for older respondents than for younger respondents ($d+ = 0.36$ and 0.21 , respectively). However, in accord with the assumptions outlined earlier, the difference is statistically less reliable than when all 14 experiments were included in the analysis, although it still reaches conventional levels of statistical significance ($Z = 1.64$, $p < .05$).

Accounting for dependent observations. Panel A of table 2 presents the results of two ways to deal with the problem that several of the experiments were conducted on the same survey, that is, within the same sample of respondents (e.g., experiments 1, 3, 4, and 9 were all conducted in April 1979). For the preceding analyses (first row in panel A of table 2), each experiment was treated as though it was conducted in a separate, independent survey. Treating nonindependent studies as independent can, however, lead to significance test errors (cf. Gleser and Olkin 1994; Rosenthal 1991). These errors can be avoided by having each study contrib-

Table 2. Results of the Meta-analysis Using Different Approaches to Account for Dependent Observations and Direction of Effects

	Young (<i>N</i>) <i>d+</i>	Old (<i>N</i>) <i>d+</i>	<i>Z</i>	<i>p</i>
A. All experiments:				
Regular approach	.14 (7,200)	.31 (1,003)	2.46	.007
Average effect size approach ^a	.13 (4,375)	.29 (608)	1.88	.03
Smallest effect size approach	.10 (4,526)	.22 (630)	1.32	.09
B. Direction of effects, experiments 1-9 and 13:				
Regular approach	.18 (4,779)	.28 (654)	2.34	.01
Average effect size approach	.10 (1,985)	.38 (267)	1.32	.09
Smallest effect size approach	.14 (2,993)	.32 (342)	1.51	.07

^a Sample sizes are smaller for the average effect size approach than for the smallest effect size approach because, although conducted within the same sample, different experiments were conducted in differently sized subsamples of the entire sample.

ute only a single effect size and a single significance level to the total (Rosenthal 1984, 1991; see also Gleser and Olkin 1994). There are basically two methods to do so. Both, together with the unadjusted findings, are reported here.

For the first approach, the effect sizes of these experiments were averaged, and the average sample size was used for the meta-analysis. This approach led to a mean effect size $d+$ of 0.13 for young respondents and $d+ = 0.29$ for old respondents, with the difference being significant at the $p < .03$ level ($Z = 1.88, p < .03$; see second row in panel A of table 2). The most conservative approach is to select just the smallest effect size of those effect sizes that are based on one and the same sample of respondents. This approach most likely underestimates the true age difference in effect sizes because only the smallest effect size is retained in the analysis, and all larger effect sizes are excluded. Hence, the true effect size difference can only be larger. However, it was conducted in order to test the robustness of the proposed effects. As shown in the third row of panel A (table 2), the age differences between mean effect sizes (young: $d+ = 0.10$; old: $d+ = 0.22$) are reliable at a $p < .09$ level, even when using this very conservative approach. It can thus be concluded that the age differences presented in tables 1 and 2 are reliable.

Direction of effects. Another methodological problem of the meta-analysis presented here concerns the direction of the response order effects (primacy vs. recency). Ignoring the algebraic sign when combining the effect sizes for the meta-analysis could have resulted in an overestimation of the significance of the mean effect size age difference. In order to rule out this possibility, additional analyses were conducted in which only those experiments were included for which there is a clear overall direction of each effect for the full sample (experiments 1–7) or for which the direction of the effects is consistent in the young and old group (experiments 8 and 9). Experiment 13 was also incorporated, since it is a conceptual replication of the other two “divorce” experiments (see table 1), and the significant effect in the older group is positively signed, like the effects in the other two replications. Hence, experiments 10–12 and 14 were not included in this new meta-analysis because of inconsistent directions in the young and old groups in addition to small and insignificant response order effects in the full sample.

Panel B in table 2 shows that the age difference in mean effect sizes remains significant, even when using this approach to control for the possibility of bias due to chance variation in direction ($d+ = 0.18$ vs. $d+ = 0.28$, for young vs. old respondents, respectively; $Z = 2.34, p < .01$). Furthermore, the age differences remain robust when applying the two methods of accounting for dependent observations (see rows 2 and 3 in panel B).

Differential rates of “don’t know” responses? In the present data, older

respondents provide substantially more "don't know" responses than do younger respondents ($M = 9.13$ percent vs. $M = 2.15$ percent, respectively). Analyses were conducted to test whether the frequency of "don't know" responses is systematically affected by question version in either the younger or the older groups. If so, this would present a problem for the interpretation of the findings reported above, because they could be attributed, in part, to differential rates of item nonresponse. The analyses revealed a significant interaction between age and question version for just one of the experiments ("divorce": September 1979, $p < .04$). This is what can be expected by chance when conducting 14 significance tests (one for each of the 14 experiments). All other interactions were far from reaching statistical levels of significance (all p 's $> .25$). It can therefore be concluded that differential rates of "don't know" responses between age groups cannot account for the findings reported here.³

IS IT AGING OR IS IT EDUCATION?

Could the presented effects simply be a function of differences in respondents' educational attainment? Because of the negative correlation between age and education (ranging in the present experiments from $r = -.23$ to $r = -.32$), the obvious question is: What moderates response order effects in surveys—is it age, is it education, or is it both?

To examine independent and interactive effects of age and education, the magnitude of response effects was estimated for respondents with low (less than high school) and high (high school or more than high school) levels of education separately for younger (lower than age 65) and older (age 65+) respondents. Education was dichotomized this way in order to comply with existing findings from the satisficing literature that the notable difference in various kinds of response effects is usually between the low education groups and the other two groups (see Narayan and Krosnick 1996). The results of these meta-analyses are presented in table 3. The same two methods (average effect size approach and smallest effect size approach) were again used to account for dependent observations. Panel A of table 3 displays the young-old comparison separately for lower and

3. A final methodological issue to consider concerns the so-called file-drawer problem. The file-drawer problem refers to the suspicion that the studies retrievable in a meta-analysis are not likely to be a random sample of all studies actually conducted (e.g., Rosenthal 1984, 1991). Those studies that revealed significant results are more likely to be published than those that did not achieve conventional levels of significance; latter studies tend to be "hidden away in file drawers" (Sterling 1959). In order to estimate the extent of the file-drawer problem, the fail-safe number was calculated (see table 2). The fail-safe number informs about the number of file-drawer studies required to bring the mean effect size down to a defined level. Using Orwin's (1983) fail-safe formula for effect sizes d , the calculation revealed that 17 file-drawer studies with effect sizes of zero would be necessary to reduce the mean effect size of the older group ($d^+ = 0.31$; see first row in panel A) to the mean effect size of the younger group ($d^+ = 0.14$).

Table 3. Tests of Significance of the Differences between Effect Sizes by Respondent Age and Education

Comparisons	Mean Effect Size d^+ Group 1 (N) ^a	Mean Effect Size d^+ Group 2 (N)	Z	p
A. Young (1) versus old (2):				
Within lower education:				
Regular (all 14 experiments)	.24 (1,047)	.45 (404)	1.70	.04
Average effect size approach	.23 (644)	.43 (238)	1.29	.10
Smallest effect size approach	.17 (620)	.37 (246)	1.31	.10
Within higher education:				
Regular (all 14 experiments)	.15 (6,027)	.42 (570)	3.03	.001
Average effect size approach	.14 (3,642)	.43 (348)	2.55	.005
Smallest effect size approach	.11 (3,544)	.30 (351)	1.74	.04
B. Lower (1) versus higher (2) education:				
Within young:				
Regular (all 14 experiments)	.24 (1,047)	.15 (6,027)	1.38	.08
Average effect size approach	.23 (644)	.14 (3,642)	1.06	.14
Smallest effect size approach	.17 (620)	.11 (3,544)	.75	.23
Within old:				
Regular (all 14 experiments)	.45 (404)	.42 (570)	.26	.40
Average effect size approach	.43 (238)	.43 (348)	.01	.50
Smallest effect size approach	.37 (246)	.30 (351)	.41	.34

^a Because of missing data in the education variable, the N s in education-related analyses are smaller than the sums of the columns in table 1.

higher educated individuals. Panel B of table 3 shows the comparison of effect sizes for the two education groups separately for younger and older respondents.

The age comparisons by education presented in panel A demonstrate that the effect sizes are consistently and reliably larger for older respondents than for younger respondents for both levels of educational attainment. This suggests that age might account for the education differences reported by Narayan and Krosnick (1996). However, a closer inspection of table 3 (see panel B) shows that this may be only partly true. Panel B points to the possibility that age and education could both influence the magnitude of response order effects, or that they could even interact. As can be seen in panel B, it appears that there is some indication of an education effect for young respondents, but this effect disappears for older respondents. Younger respondents display larger effect sizes when they are less educated than when they are more educated ($Z = 1.38$, $p < .08$; all 14 experiments). When controlling for dependent observations, however, this difference is no longer statistically significant ($p < .14$ and $p < .23$ for the average and smallest effect size approach, respectively). Within the older group, effect sizes do not differ significantly by level of educational attainment (see panel B of table 3; all $Z < 1$). Hence, the results presented in table 3 may suggest independent effects of age and education on response order effects, although the impact of education appears to be weaker.

The chosen analysis method may not be particularly well adjusted to the nature of the available data. Specifically, dichotomizing age and education—as has been done for the analyses just reported—may not be the best approach when trying to estimate the effects of two variables controlling for each other. Logistic regression analysis does not require dichotomizing of the predictor variables, yielding greater power to detect existing effects. In addition, logistic regression analysis automatically accommodates the possibility that effects on the dependent variable are not constant across the range of the independent variables. Hence, a series of hierarchical logistic regression analyses was conducted in order to investigate whether the separate effects of education and age, respectively, remain stable when controlling for one another.

Hierarchical logistic regression analyses. The outcome variable in the logistic regression analyses reported below is always whether or not a particular response (the one specified in col. 2 of table 1) was selected (i.e., the propensity to choose that response alternative). The predictors of central interest are the order in which the variable appeared (the main effect for order), the interaction of order with age (the degree to which order effects are different for different aged respondents), and the interaction of order with education (the degree to which order effects are different

for different educated respondents). Age (in years) and education (in number of years of formal schooling) are used as continuous variables. In order to test the described interactions, main effects for age and education were controlled for; however, these effects are not of interest to the present article and will therefore not be reported.

The unstandardized logistic regression coefficients (b), standard errors, and significance levels for all regression analyses are presented in table 4. The results of the first regression model (model 1; see col. 3 in table 4) basically replicates, both in size and direction (as indicated by the sign of the coefficients), the overall response order effects as reported by Schuman and Presser (1981; see also col. 3 in table 1). Here, a positive regression coefficient indicates a recency effect, that is, that the ratio of the probability of choosing the specified response alternative to the probability of not choosing it is greater for order 2 (where the alternative is presented in the last serial position) than for order 1 (where the response alternative is presented in the first position). A negative regression coefficient indicates a primacy effect, that is, that the response alternative is relatively less likely to be endorsed for order 2 as compared to order 1. As can be seen, the response order effects for experiments 1–7 are statistically significant—this replicates the results reported by Schuman and Presser (1981) using log-linear analysis.

For models 2 and 3, the order by age (model 2) and order by education (model 3) interactions, respectively, were added to the main effect of order. The coefficients for the interaction terms can be interpreted as follows. The coefficient for “order by age” in model 2 estimates the change of the order effect with increasing age, and in model 3, the change of the order effect with increasing educational attainment. Overall, models 2 and 3 reveal that often, though not always, age and education have similar patterns of coefficients, suggesting that the effects of education could be due to age or vice versa. However, the effects of the order-by-age interaction (increasing order effects with increasing age) appear to be somewhat more consistent across experiments than the order-by-education interaction.

Next, how consistent in terms of size and direction are the age and education effects when simultaneously controlling for each other? The results of this analysis (model 4) are presented in the far right column of table 4. As can be seen, the simultaneous control results in a deflation of the impact of education and produces a number of changes of sign for the education effects. Overall, response order effects tend to increase with age for most experiments (12 out of 14). In contrast, only in five out of 14 experiments do the regression coefficients demonstrate decreases in response order effects with increasing educational attainment. This pattern of results suggests that some of the apparent education-related variation

Table 4. Hierarchical Logistic Regression Analyses: Response Order Effects as a Function of Respondent Age and Education

Experiment Number	Predictors	Model 1: Order			Model 2: Order + Order * Age			Model 3: Order + Order * Education			Model 4: Order + Order * Age + Order * Education		
		b	(SE)	p	b	(SE)	p	b	(SE)	p	b	(SE)	p
1	Order	.337	(.17)	.05	.261	(.66)	.58	2.86	(.95)	.003	3.687	(1.25)	.003
	Order * age				.002	(.01)	.84				-.009	(.01)	.41
	Order * education							-.199	(.07)	.007	-.233	(.08)	.003
2	Order	.711	(.19)	.0002	.134	(.54)	.80	.153	(.99)	.88	-.950	(1.28)	.46
	Order * age				.015	(.01)	.25				.019	(.01)	.16
	Order * education							.044	(.08)	.57	.071	(.08)	.37
3	Order	.392	(.18)	.027	.343	(.49)	.48	-.150	(.98)	.88	-.380	(1.23)	.76
	Order * age				.001	(.01)	.93				.003	(.01)	.78
	Order * education							.043	(.08)	.57	.049	(.08)	.53
4	Order	.604	(.19)	.002	.229	(.54)	.67	.631	(1.03)	.54	-.025	(1.34)	.99
	Order * age				.009	(.01)	.46				.010	(.01)	.44
	Order * education							-.002	(.08)	.98	.017	(.09)	.84
5	Order	.459	(.19)	.02	-.361	(.53)	.49	1.392	(1.01)	.17	.202	(1.27)	.87
	Order * age				.020	(.01)	.09				.018	(.01)	.14
	Order * education							-.074	(.08)	.34	-.038	(.08)	.65
6	Order	-.653	(.17)	.0001	-.114	(.45)	.80	-.641	(.88)	.47	.727	(1.19)	.54
	Order * age				-.013	(.01)	.21				-.018	(.01)	.12
	Order * education							-.002	(.07)	.98	-.053	(.07)	.48

7	Order	-.320	(.17)	.06	-.188	(.46)	.69	-.644	(.89)	.47	-.086	(1.15)	.94
	Order * age				-.003	(.01)	.76				-.005	(.01)	.65
8	Order * education							.025	(.07)	.72	-.003	(.07)	.97
	Order	.247	(.18)	.17	.740	(.50)	.14	-.559	(.93)	.55	-.023	(1.17)	.98
	Order * age				-.012	(.01)	.29				-.011	(.01)	.37
9	Order * education							.063	(.07)	.38	.055	(.07)	.45
	Order	.366	(.31)	.23	-.260	(.82)	.75	.495	(1.83)	.79	-.317	(2.28)	.89
	Order * age				.015	(.02)	.41				.012	(.02)	.56
10	Order * education							-.008	(.14)	.95	.017	(.15)	.91
	Order	-.180	(.20)	.38	-.382	(.53)	.47	1.249	(1.00)	.21	1.031	(1.32)	.43
	Order * age				.005	(.01)	.67				.003	(.01)	.84
11	Order * education							-.118	(.08)	.14	-.110	(.09)	.20
	Order	.059	(.20)	.77	.141	(.55)	.80	-.475	(.50)	.34	-.422	(.85)	.62
	Order * age				-.005	(.01)	.70				-.001	(.01)	.93
12	Order * education							.067	(.08)	.41	.065	(.08)	.44
	Order	.065	(.15)	.67	.513	(.42)	.22	-.270	(.24)	.27	.142	(.45)	.75
	Order * age				-.010	(.01)	.29				-.010	(.01)	.26
13	Order * education							.044	(.02)	.07	.051	(.02)	.04
	Order	-.035	(.17)	.84	-.886	(.46)	.05	-.482	(.93)	.60	-.1633	(1.16)	.16
	Order * age				.022	(.01)	.04				.025	(.01)	.03
14	Order * education							.035	(.07)	.62	.050	(.08)	.50
	Order	-.025	(.13)	.85	-.005	(.35)	.99	-.345	(.44)	.44	-.391	(.62)	.53
	Order * age				-.001	(.01)	.94				.001	(.01)	.94
	Order * education							.140	(.18)	.44	.146	(.19)	.44

in response order effects might be due instead to respondent age. The various changes in signs for the education coefficients suggest that education may in some cases suppress the effects of aging.

Meta-analysis of unique effects. Most of the age and education interactions are not statistically significant in individual experiments. In a final step, then, a meta-analysis was conducted in order to evaluate the significance of the unique effects of age and education, respectively. Partial correlations derived from the logistic regressions were calculated for both the education-by-order and age-by-order interactions, respectively. In terms of variance explained, the unique effects of the age and education interactions are the R^2 increments that are observed when adding an interaction term to the regression equation after the other interaction term has already been entered. Table 5 presents the corresponding partial correlations for age and education for all 14 experiments.

The four bottom rows in table 5 present the results of four different meta-analytic strategies that were employed to address various methodological problems. The first problem is that the direction of effects is not completely clear for experiments 10–14.⁴ The other problem is the exis-

4. For the meta-analysis, it was important to determine which of the correlations (that serve as measures of effect size) point in the theoretically predicted direction. In general, effects that point in the expected direction should receive a positive sign and thereby count toward increasing the magnitude of the mean effect size (cf. Rosenthal 1991; Schwarzer 1989). Effects that point in the opposite-than-expected direction, however, should receive a negative sign so that they count toward weakening the magnitude of the mean effect size. For experiments 1–9, the decision whether an effect points in the expected direction could be derived clearly and unambiguously from the results of the regression coefficients in model 4 (see table 4). For these experiments, the response order effects point in the same direction (primacy or recency, respectively) for all age and education subgroups. In the case of the age-by-order interaction, the partial correlations of experiments 1–9 hence received a positive sign when the regression coefficient in model 4 had the same sign as the overall effect. For the education-by-order interaction, a negative sign was assigned when the regression coefficient had the opposite sign from the overall effect (since the expected effect is that response order effects should decrease with increasing education). Application of this rule results in seven out of nine effects that point in the expected direction for age, but only two out of nine effects that point in the expected direction for education (see table 5). The situation is a little more difficult, though still unambiguous, for the age-by-order effects for experiments 10–14. Because younger individuals show primacy effects while older individuals show recency effects and vice versa for these experiments, the size of the absolute effects in the categorical analyses (table 1) should be taken into consideration. As noted earlier, the signs of the regression coefficients of the age-by-order interactions in experiments 10–14 are in accord with the results of the categorical analyses reported in table 1, and hence we can be rather confident that the effects indeed point in the expected direction. For education, however, the signs of the regression coefficients are not entirely compatible with the results shown in the categorical analyses (presented in table A1 of Narayan and Krosnick 1996), and the real direction of the education effects is hard to determine. To overcome this ambiguity, and in order to apply a consistent rule to these five experiments, we relied on the results of the categorical analyses instead. Applying this decision rule, we derived from table A1 of Narayan and Krosnick (1996) that the response effects for experiments 10, 11, and 14 are largest in the low education group. The partial correlations for these experiments therefore receive a positive sign, counting toward increasing the magnitude of the overall mean effect size in the meta-analysis. For

tence of nonindependent observations within the pool of the 14 experiments that are used for the present meta-analysis. The strategies are described in turn below.

Controlling for uncertain direction of effects. To account for the problem that the direction of the education effects was hard to determine unambiguously for experiments 10–14, the meta-analysis was conducted in two different ways. Once all 14 experiments were included (rows A and C in table 5), and a second time only experiments 1–9 were employed (rows B and D). The results for the unique effects of age show that the magnitude of the mean effect size is statistically significant when all 14 experiments are used for the analysis ($r = .028$, $Z = 2.50$, $p < .006$) and remains significant even when experiments 10–14 are excluded ($r = .025$, $Z = 1.71$, $p < .04$; see table 5). In contrast, the magnitude of the mean effect size of the unique effects of education does not reach statistical significance in any case (all Z 's < 1).

Controlling for nonindependent studies. Two additional meta-analyses were conducted to account for the methodological problem of nonindependent studies. Specifically, effect sizes that were obtained from the same sample were averaged, and the average sample size was used for the meta-analysis. The results of these analyses are presented in rows C and D in table 5. Row C presents the results when all 14 experiments are used and row D for just experiments 1–9. As can be seen, the results remain generally unaffected by controlling for nonindependent studies.

Finally, the meta-analytic procedure can be used to test whether the differences between the mean effect sizes for age and education are statistically significant. This, indeed, is the case, as can be seen in the far right column of table 5. Specifically, the difference between the mean effect sizes of age and education are significant when all 14 experiments are included in the analysis ($Z = 1.63$, $p < .05$) and remain significant when controlling for nonindependent studies (row C; $Z = 1.74$, $p < .04$). This final analysis indicates that the unique effects of age are reliably larger than the unique effects of education.

IMPLICATIONS FOR THE INTERPRETATION OF SURVEY DATA

These results suggest clearly that response order effects are affected by age, and also that the impact of age is not explained by educational differences. The findings have significant implications. An age-related increase in response order effects could, for example, result in misleading conclusions regarding age or generational differences in attitudes.

experiments 12 and 13, the response order effects are largest in the medium- rather than the low-education group. The partial correlations of these experiments hence receive a negative sign and thereby count toward decreasing the magnitude of the overall mean effect size.

Table 5. Meta-analysis of Partial Correlations

Experiment Number	N	Age			Education			Differences between Mean Correlations	
		Mean	Z	p	Mean	Z	p	Z	p
1	651								
2	550								
3	652								
4	515								
5	455								
6	617								
7	585								
8	543								
9	202								
10	620								
11	460								
12	689								
13	543								
14	915								
	N	Age			Education			Differences between Mean Correlations	
		Mean	Z	p	Mean	Z	p	Z	p
Uncorrected:									
(A) All 14	7,997	.028	2.50	<.006	.002	0.19	<.42	1.63	<.05
(B) Experiments 1-9	4,770	.025	1.71	<.04	-.0006	-0.04	<.49	1.25	<.106
Corrected:									
(C) All 14	7,999	.028	2.51	<.006	.0006	0.05	<.48	1.74	<.04
(D) Experiments 1-9	4,771	.025	1.72	<.04	.006	0.38	<.35	0.95	<.17

Schuman and Presser's (1981) "divorce" experiment (conducted in September 1979) serves as an illustration. In this experiment, the authors asked respondents, "Should divorce in this country be easier to obtain, more difficult to obtain, or stay as it is now?" The order of the last two response options was reversed in the two conditions of the experiment. In one form "stay as is" was the last response option, while in the other form it was the second of the three response options and "more difficult" was the last option. Schuman and Presser found a recency effect for the full sample. Both "stay as is" and "more difficult" were endorsed sig-

nificantly more often (by 12 percent more) when presented last rather than second in the response sequence (Schuman and Presser 1981, p. 67).

Reanalyses of this experiment, shown in table 6, indicate a linear increase of the recency effect with respondent age. Response effects are small and not significant if only younger respondents (under 55 years of age) are considered (7.4 percent; $\chi^2 = 2.63$, $df = 2$, n.s.). In contrast, respondents over 55 years of age show a difference of 18.6 percent, and the difference further increases to 45.5 percent for respondents over 70 years of age, with the response effect being significant for all comparisons over age 60. The same age-related pattern emerges for a replication of this study that Schuman and Presser carried out in April 1979 (1981, p. 65).

Importantly, one would draw different substantive conclusions about the relationship of age and attitudes toward divorce depending on the order in which the response alternatives are presented. When the alternative "stay as is" is presented second and "more difficult" is presented last, older respondents apparently report more conservative attitudes toward divorce than younger respondents, with 68.2 percent of the older age group but only 36.4 percent of the younger age group favoring more restrictive divorce laws (see table 6). When "more difficult" is presented as the second alternative, however, responses of the young and old are virtually identical (with 31.8 percent and 31.4 percent for the older and younger age group, respectively). Older respondents are now most likely to endorse "stay as is" (68.2 percent), the alternative read last under this condition. This illustrates that age differences may not only influence the emergence and size of response order effects but may result in misleading substantive conclusions about age or generational effects drawn from the data.

This example also demonstrates that response options do not have to be semantically or grammatically difficult to elicit strong age-related response order effects. None of the three response options (easier, more difficult, stay as is) is problematic to understand or poses syntactical difficulty. Simple forgetting also seems unlikely for the small number of response alternatives. Comprehension difficulty or simple forgetting can thus not explain the strong age-related differences. Rather, what appears to be crucial is that each of the three alternatives presents substantial implications that would need to be equally elaborated before any one of them can be chosen (see Krosnick and Alwin 1987; Schwarz, Hippler, and Noelle-Neumann 1992). Based on cognitive aging research, we would hypothesize that older respondents have less capacity to conduct these extensive elaborations for all alternatives and are more likely, therefore, to select alternatives for a response that received most elaboration. These are the alternatives presented early or late in the list.

Table 6. Divorce Experiment: Percentage Endorsing Divorce Response Alternatives by Order and Age

	18-54		55+		60+		65+		70+	
	Form A	Form B	Form A	Form B	Form A	Form B	Form A	Form B	Form A	Form B
Easier (%)	23.0	25.5	14.9	17.1	11.5	16.1	8.1	17.1	0.0	9.1
More difficult (%)	31.4	36.4	41.4	57.1	38.5	58.9	37.8	61.0	31.8	68.2
Stay as is (%)	45.6	38.2	44.3	25.7	50.0	25.0	54.1	22.0	68.2	22.7
Total (%)	100	100.1	100.6	99.9	100	100	100	100.1	100	100
<i>N</i>	239	220	70	70	52	56	37	41	22	22
χ^2 , <i>df</i> = 2	$\chi^2 = 2.63$, n.s.		$\chi^2 = 5.43$, $p < .07$		$\chi^2 = 7.33$, $p < .03$		$\chi^2 = 8.86$, $p < .01$		$\chi^2 = 10.98$, $p < .007$	
Gamma	-.033		.19		.24		.25		.48	
Difference "stay as is"	7.4		18.6		25.0		32.1		45.5	

NOTE.—Carried out in September 1979, Survey Research Center, University of Michigan. Form A: Should divorce in this country be easier to obtain, more difficult to obtain, or stay as it is now? Form B: Should divorce in this country be easier to obtain, stay as it is now, or be more difficult to obtain? Three-way interaction: 18-64 versus 65+: $\chi^2 = 4.72$, *df* = 2, $p < .09$. Percentages may not total to 100 due to rounding.

Discussion

The present article examined the impact of age and education on the emergence of response order effects. The results of a series of meta-analyses in Schuman and Presser's (1981) response order experiments demonstrate that respondent age accounts for individual differences in response order effects. These effects are not explained by education, which appears, if anything, to have weaker impact than does age.

I have proposed that decreases in working memory capacity among older respondents are responsible for the age-related increase in response order effects. Abilities that are directly associated with formal education or cognitive sophistication might, in contrast, be less relevant for the explanation of response order effects than has previously been assumed (e.g., Krosnick 1991). Krosnick (J. A. Krosnick, personal communication, 1997) suggested that age and education actually might to some extent tap the same underlying construct, that is, what he refers to as "cognitive sophistication" in fact could reflect working memory capacity. Furthermore, Krosnick suggested that decreases in working memory due to age may produce impairment sufficiently great to eliminate cognitive sophistication differences due to education. The data are indeed suggestive of this possibility. I would add to these speculations that the more cognitive operations necessary for responding are knowledge- or practice-dependent, the more the amount of formal schooling should gain relevance for the successful accomplishment of these tasks. This presumably is the case, for example, when the wording of the question or response alternatives is unfamiliar and complicated (see Knäuper et al. 1997).

In the context of the present report, it was not possible to directly test these process assumptions, because neither measures of individual's working memory capacity nor measures of individual's degree of cognitive sophistication are available in Schuman and Presser's data. Future research should directly investigate whether age effects are indeed mediated by working memory capacity and whether what has so far been referred to as cognitive sophistication, in part, reflects individual differences in working memory capacity. Given that well-validated measures exist to assess individuals' working memory capacity (e.g., measures developed by Daneman and Carpenter 1980), this should be easy to accomplish in future studies. In fact, such experiments are currently underway (cf. Schwarz, Park, and Knäuper 1996).

The small number of experiments incorporated in the meta-analyses reported here calls for some caution in drawing too strong conclusions from the findings. While the unique effects of age appear to be rather robust, future studies should be carried out in order to examine the reliability of the reported findings. Results recently presented by Smith and Bishop (1996) for a much larger data set, however, already provide some

reassurance that the age effects are real. Smith and Bishop conducted reanalyses of more than two hundred response order experiments that had originally been carried out by Gallup as part of its American Institute of Public Opinion (AIPO) Survey (the "Gallup Poll"). In both auditory and visual presentation modes, they found statistically reliable differences in effect sizes between older and younger respondents. In auditory mode, older respondents exhibited a greater effect size than younger respondents in 121 of 182 experiments (67 percent). In visual mode, older respondents generated larger effects sizes in 26 out of 32 experiments (81 percent). The mean percentage difference for older respondents (age 65 and older) was about twice as large as for younger respondents (similar to what is found here). Unfortunately, Smith and Bishop did not control for the effects of education in their analysis.

The occurrence of primacy versus recency effects does deserve more attention than it received in the present article. Norbert Schwarz and colleagues (Schwarz, Hippler, and Noelle-Neumann 1992) and Krosnick and Alwin (1987) outline a detailed theory that predicts the direction of response order effects and its dependence on the amount of elaboration that the individual response options receive. All of the present experiments were conducted in an auditory mode, and according to the theories of Schwarz, Hippler, and Noelle-Neumann (1992) and Krosnick and Alwin (1987), this should result in a dominance of recency effects. However, in a considerable number of the presented analyses older respondents show rather strong primacy effects. An interesting issue for future research to investigate concerns the detrimental effects of aging on the propensity to show primacy versus recency effects.

Although obviously more research is needed, the findings nevertheless demonstrate that respondents' age deserves more attention in theoretical and empirical analyses of context effects in surveys (see Schwarz, Park, Knäuper, and Sudman 1998 for a first overview). Bringing the psychological literature on cognitive aging to bear on the emergence of response effects may provide a fruitful avenue for future research, one that promises to enhance our understanding of the cognitive processes underlying response effects in surveys.

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