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Abstract

In this article, we analyze whether the financial strain of poverty systematically alters the allocation of attention. We address two types of attention: attention to unexpectedly occurring events and attention to primary tasks that require focus. We show that the poor are significantly more likely than the rich to notice unexpected events. In addition, we find evidence that the poor notice unexpected events at the expense of attention to the primary task only if the task is sufficiently difficult.

KEYWORDS: Inattentional blindness, limited attention, poverty.
JEL CODES: D91, I32.

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1 Introduction

In this article, we analyze the relationship between poverty and limited attention. Increasing evidence indicates that limited attention plays a major role in decision-making (for an overview, see Gabaix 2019). For example, consumers do not fully attend to odometer mileage of used cars (Lacetera et al. 2012, Busse et al. 2013, Englmaier et al. 2018), age of used cars (Englmaier et al. 2018), shipping costs (Brown et al. 2010), existing taxes (Chetty et al. 2009), or new tax rules (Abeler & Jäger 2015). Yet, few economic studies analyze whether individual differences in attention allocation exist. In this article, we investigate whether differences in attention allocation between the rich and the poor exist.

Specifically, we analyze whether poverty predicts attention allocation in inattentional blindness experiments. Inattentional blindness experiments consist of a primary task and an unexpected event that occurs during the primary task. Inattentional blindness "denotes the failure to see highly visible objects we may be looking at directly when our attention is elsewhere" (Mack 2003). In these experiments, an individual is inattentional blind if, while focusing on the primary task, she does not notice the unexpected event. A famous experiment is the *invisible gorilla* (Simons & Chabris 1999). In that experiment, subjects watch a video, in which two teams each pass a basketball. The primary task of subjects is to count how often one of the teams passes the basketball. In the middle of the video, a person dressed in a gorilla costume walks through the scene (the unexpected event). Although subjects look directly at the gorilla, a significant fraction of subjects fail to notice the gorilla.

Inattentional blindness has not only been demonstrated in abstract laboratory experiments. Inattentional blindness also prevails under realistic primary tasks. For example, radiologists looking at CTs fail to notice superimposed gorillas (Drew et al. 2013) and drivers in simulators fail to notice pedestrians (Murphy & Greene 2016). In addition, inattentional blindness has been demonstrated in field experiments: Cell phone users do not see a unicycling clown during a walk (Hyman Jr. et al. 2010) and running after another person leads to running past a fight without noticing the fight (Chabris et al. 2011).

Inattentional blindness is an economically significant cognitive phenomenon. Depending on the situation, being inattentional blind can be problematic or beneficial. For example, inattentional blindness is problematic when drivers do not notice pedestrians, workers do not notice early signs of machine failure at their workplace, or medical staff do not notice unexpected complications in patients. In these cases inattentional blindness carries high economic costs. In addition, as inattentional blindness illustrates that not all information are processed even if they are freely available, the standard economics' assumption of decision-making under full information is problematic. Without full information, the probability that individuals choose the utility-maximizing option decreases. For example, although very often information (such as whether the next payment will overdraft the account) is freely available, individuals do not attend to this information (Stango & Zinman 2014). In addition, Hanna et al. (2014) show in a field experiment that if individuals do not expect information to be relevant, they do not attend to this information and thus miss opportunities to increase profits. Inattentional blindness experiments capture these inattention problems in a more abstract way. Nevertheless, situations also exist where ignoring unexpected events is beneficial. For example, when facing tasks that require concentration, being able to ignore *irrelevant* distractions, such as noise from a nearby construction site or unwanted advertisements on a website, is beneficial.

A large literature in psychology tries to identify triggers of inattentional blindness. Most of the literature discusses triggers that are task dependent: for example, perceptual load, i.e., the difficulty of the primary task (see, e.g., Simons & Chabris 1999, Cartwright-Finch & Lavie 2007, Chabris et al. 2011, Murphy & Greene 2016), or similarity of the unexpected event to the targets in the primary task (see, e.g., Simons & Chabris 1999, Most et al. 2001). However, independently of the task design, individuals differ in their susceptibility to inattentional blindness. One predictor for inattentional blindness, for instance, is the age of the individual (see, e.g., Graham & Burke 2011, Horwood & Beanland 2016, Stothart et al. 2015). In general, this literature shows that inattentional blindness varies between and within individuals. We contribute to this literature by proposing that poverty reduces the likelihood of being inattentional blind.

Overall, understanding whether groups differ in the attention to new information is relevant for designing better policies. If some groups are less likely to notice new information, we can increase the salience to increase awareness of the information. For example, governmental assistance programs need to be perceived by those who are applicable for such programs. If the target group is unlikely to perceive new information, it is not sufficient for the government just to offer the assistance program. In addition, the government needs to increase the salience of the assistance program to the target group. Similarly, firms need to understand for advertising purposes whether consumers differ in their attention allocation. For example, if the rich are less likely to perceive unexpected events such as advertisement on websites, firms selling luxury products like expensive watches need to adjust their advertising strategies to make the advertisement more salient to their target group compared to firms selling everyday consumption goods.

In this article, we focus on analyzing whether the rich are more inattentional blind than the poor. One predictor for inattentional blindness is the load on working memory experienced by the subject during the experiment (de Fockert & Bremner 2011): Load theory (Lavie & Tsal 1994, Lavie 1995, Lavie et al. 2004) argues that cognitive control functions, and working memory in particular, are necessary to separate between relevant and irrelevant stimuli and to prioritize the processing of relevant stimuli (see Murphy et al. 2016, for an overview of load theory). That means, a subject who has to focus on, for instance, how often one team passes a basketball has to prioritize processing that team's basketball passes. However, if working memory load is high, the ability to filter relevant information decreases. Consequently, increased working memory load increases the probability of noticing unexpected events (de Fockert & Bremner 2011). Recent evidence by Mani et al. (2013) and Shah et al. (2012) indicates that poverty (or scarcity in general) reduces cognitive capacity, especially fluid intelligence, and acts as a distraction that increases working memory load. Poverty triggers intrusive thoughts about financial problems (Shah et al. 2018), leads to greater focus on problems connected to poverty (Shah et al. 2012, 2019), and causes neglect of other information (Shah et al. 2012). Thus as poverty loads working memory and increased working memory load reduces inattentional blindness, poverty should reduce inattentional blindness. This is the main hypothesis we analyze in this article.

In addition, inattentional blindness experiments allow us to measure focused attention. In inattentional blindness experiments, subjects perform a primary task that requires the subjects to focus on a set of targets. Thus the performance on the primary task also provides information about the attention of the subjects. Various economically relevant tasks require such attention: For example, reading, coding, monitoring jobs such as air traffic control, or assembly line jobs require focused attention. Yet, as attention is a limited resource and poverty is distracting (see, e.g., Shah et al. 2012, Mani et al. 2013), we expect that the focus of the poorer subjects is interrupted more often. If the poor exhibit systematically less focused attention, a poverty trap might result. If the poor's focus on tasks is systematically worse, they perform worse at jobs that require focused attention or are less attentive at further trainings for skill enhancements. In turn, they earn less which increases poverty. Whether the poor have systematically less focused attention is the second main hypothesis we analyze in this article.

To test these hypotheses we need data of an inattentional blindness experiment where subjects differ in income. One problem with inattentional blindness experiments is that each subject can only participate once. If subjects know that an unexpected event occurs during the experiment, subjects consciously look for this unexpected event. Thus it is difficult to compile a large data set. In addition, we need a data set that includes enough income variation. Typical laboratory experiments with students do not include enough income variation. However, initiated by Conley, Chabris, and Simons, the Innovation Sample of the German Socio-Economic Panel Study (SOEP-IS) includes an inattentional blindness experiment for a subset of the respondents from the 2014 wave (SOEP-IS Group 2018). Overall, 1370 persons participated in the experiment. In addition, the SOEP-IS is a representative sample of German households and, therefore, includes people with various different backgrounds and thus with various different income levels. Consequently, we use data from the SOEP-IS for our analysis.

In line with our hypothesis, we find evidence for a relationship between poverty and inattentional blindness. In addition, we also provide some evidence that the relationship between poverty and inattentional blindness is causal: Considering that people in Germany are often paid wages at the turn of the month, people are less financially strained after the turn of the month. Our hypothesis about the relationship between poverty and inattentional blindness then suggests that subjects exhibit more inattentional blindness after the turn of the month. We show that changes in inattentional blindness occur around the turn of the month: Subjects in our sample are on average more inattentional blind after the turn of the month.

In addition, we find evidence for a relationship between poverty and the performance in the primary task. However, these findings are not robust. In particular, we do not find evidence for a payday effect. One possible explanation for this could be that the task is too short and not difficult enough to detect an effect. Therefore, we restrict our sample to subjects with the most difficult tasks. For this subsample, the post-payday group performs significantly better at the primary task. Overall, our results thus indicate that poverty decreases inattentional blindness at the expense of performance in the primary task only in difficult tasks.

The remainder of the article is structured as follows: Section 2 discusses our contributions to the literature. Section 3 describes the data. Section 4 covers the analysis of the data and discusses the results. Section 5 examines the limitations of the data and Section 6 highlights policy implications and concludes.

2 Related literature

An increasing literature studies the causal effects of poverty on cognitive functions and the resulting consequences on decision-making. For example, Mani et al. (2013) show in a laboratory study and in a field experiment that poverty reduces cognitive capacity, in particular, fluid intelligence and cognitive control.[1] In the laboratory study, Mani et al. (2013) show that when subjects think about small expenditures, poorer and richer subjects perform similar at cognitive tests. But when subjects think about large expenditures, poorer subjects perform worse at cognitive tests than richer subjects. In the field experiment, Mani et al. (2013) test farmers cognitive capacity before and after the annual harvest. Farmers perform better after harvest, when they are comparatively richer, than

[1]Mani et al. (2013) show that poverty reduces the performance at Raven's matrices, a spatial compatibility task, and the Stroop task. The Raven's matrix measures fluid intelligence (see, e.g., Mani et al. 2013, Dean et al. 2019). Fluid intelligence is "the capacity to think logically and solve problems in novel situations" (Mani et al. 2013, p. 977). The spatial compatibility task and the Stroop task measure cognitive (Mani et al. 2013) or inhibitory control (Dean et al. 2019). "**Inhibitory control** is the ability to control impulses and minimize interference from irrelevant stimuli" (Dean et al. 2019, p. 61). Furthermore, working memory and inhibitory control are closely intertwined and are often not distinguished in the literature (Dean et al. 2019).

before harvest. This is in line with evidence provided by Spears (2011) who finds that economic decision-making reduces cognitive capacity of poor people more than of rich people.

Shah et al. (2018) show that poorer individuals are more likely to think about the costs associated with an experience. In addition, these thoughts about costs arise spontaneously and are persistent. In addition, Shah et al. (2012, 2019) provide evidence that subjects with scarce resources focus more on problems related to scarcity than subjects who do not face scarcity. Zhao & Tomm (2017) confirm this finding in an eye-tracking study. This increased focus on problems of scarcity might explain the increased load on cognitive functions. This increased load is problematic for tasks that require cognitive capacity: Shah et al. (2012, 2019) find that the greater engagement of poorer subjects results in neglect of the future and overborrowing and Kaur et al. (2019) show that scarcity reduces productivity and increases error rates at work. Yet, (perceived) scarcity can also lead to better decision-making because the increased focus reduces context effects (Shah et al. 2015) and endowment effects (Fehr et al. 2019). In addition, at tasks that require proceeduralized processes, such as typewriting, i.e., where too much attention to a task is detrimental, poorer subjects preform better (Dang et al. 2016).

Yet, evidence also accumulates that finds no effect of scarcity on cognitive functions. Carvalho et al. (2016) compare performance on cognitive tests of individuals interviewed before payday with individuals interviewed after payday. Carvalho et al. (2016) find no effect on cognitive functions between the two groups. Using the same data as Carvalho et al. (2016), Mani et al. (2020) show that the null result of Carvalho et al. (2016) is sensitive to the specification. By including distance to payday, Mani et al. (2020) show that the group interviewed before payday performed significantly worse in tests of cognitive functions. Dalton et al. (in press) investigate the effects of priming entrepreneurs with low income to think of financial problems. Dalton et al. (in press) find an effect on risk aversion but not on cognitive performance. In addition, Lichand & Mani (2020) find evidence that income uncertainty affects cognitive functions of all subjects, but that expected income variation only has an effect for the poorest subjects.

We contribute to this literature in three ways. First, the scarcity literature argues that attention is a major driver behind these results. That is, poorer subjects focus more on problems related to their financial problems and neglect other problems. Yet, little research directly focuses on differences in attention allocation between the rich and the poor. One first step in the direction of analyzing whether attention differences between the rich and the poor exist is Goldin & Homonoff (2013) who show that the poor pay more attention to taxes than the rich. Lichand & Mani (2020) focus on trade-offs between money and goods. Bartoš et al. (2018) measure sustained attention and find no effect of priming people to think of hard financial problems on sustained attention (number of inspected options, decision time, and likelihood to stay at default option). Zhao & Tomm

(2017) analyze the attention of subjects to additional information that helps subjects in their primary task. Zhao & Tomm (2017) find that subjects experiencing scarcity use the additional information less if the information is presented away from the center of the spatial focus but not too far away. In general, this literature focuses on attention to expected events and/or on attention to events related to the financial burdens of poverty. Furthermore, to trigger a scarcity mindset, studies often use priming. In contrast, we analyze the attention allocation more abstractly. The task is unrelated to the financial difficulties of the subjects. Yet, we find attention differences between the rich and the poor without priming the subjects. Furthermore, we are the first to analyze the relationship between poverty and attention to unexpected events in a classical inattentional blindness experiment.

Second, we also contribute to the debate that questions the transferability of the results of the scarcity literature from absolute poverty to relative poverty in developed countries. Although Mani et al. (2013) argue that the consequences of scarcity are not limited to developing countries, doubt remains whether these findings are transferable to developed countries. For example, Lichand & Mani (2020) find a payday effect only for the poorest subjects in their sample of Brazilian farmers. We study the effects of relative poverty on attention in a sample of German households. Our results provide evidence for differences in attention allocation between the rich and the poor. We thus add to the debate by providing further evidence for an effect of scarcity also in developed countries.

Third, we also add to the literature that analyzes the relationship between poverty and focused attention, measured in terms of errors (Kaur et al. 2019). In contrast to Kaur et al. (2019), we do not find a robust relationship between poverty and error rates. That is, the poor in the SOEP-IS experiment are not less inattentional blind at the expense of performance in the primary task. The difference to the results of Kaur et al. (2019) could stem from a variety of factors, such as the difference in task. For instance, the subjects in Kaur et al. (2019) were doing a real-world task for the whole day, while the SOEP-IS experiment lasted only 17 seconds. Maybe if the task in the experiment took longer, the differences in error rates would be more pronounced. This explanation is plausible because we find an effect if we restrict the sample to subjects with the most difficult task. Yet, future research is necessary to answer this question conclusively.

3 Data

We use data from the Innovation Sample of the German Socio-Economic Panel Study (SOEP-IS, Richter & Schupp 2015, Goebel et al. 2020). The SOEP-IS exists since 2011 and includes changing questionnaires and behavioral experiments. In 2014, on the initiative of Conley, Chabris, and Simons the SOEP-IS included an experiment on inattentional blindness with 1370 participants (SOEP-IS Group 2018, Bohlender & Glemser 2016). By using this data set, we avoid a common problem of inattentional blindness research. When testing for inattentional blindness, you can only test each individual once. As soon as subjects know that an unexpected event occurs, subjects consciously look for an unexpected event in subsequent rounds and are thus more likely to notice an unexpected event. Consequently, many inattentional blindness experiments have only few subjects. One advantage of the SOEP-IS data set is the large number of participants in the experiment. In addition, in contrast to classical laboratory experiments of inattentional blindness, where students are the majority of subjects, the SOEP-IS is a representative sample of German households and thus provides more variation in, for example, household income and age of respondents.

In the SOEP-IS inattentional blindness experiment (Bohlender & Glemser 2016), subjects watch a video with black and white squares and circles moving around. The primary task consists of counting how often specific objects touch the frame of the video. Subjects are randomly assigned to one of four groups that count how often (i) the squares, (ii) the circles, (iii) the white, or (iv) the black shapes touch the frame of the video.[2] During this task, an unexpected event—an additional black circle—moves through the frame from right to left (see Figure 1).

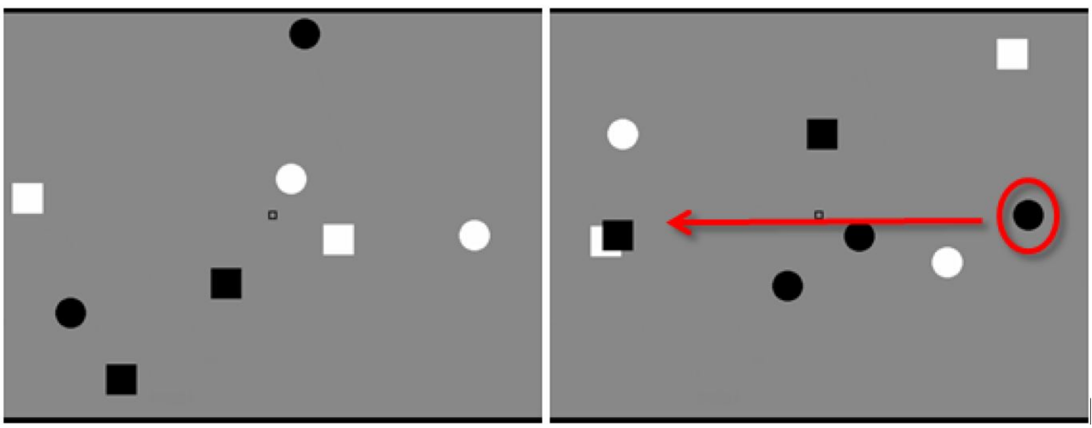

Figure 1: Illustration of the inattentional blindness experiment (source: Bohlender & Glemser 2016).

[2] In the final sample with 1084 respondents, 258 respondents count squares, 265 respondents count circles, 290 respondents count white, and 271 respondents count black shapes.

After watching the video, respondents report their counts in the primary task. We drop the data of respondents who do not report their counts in the primary task. Afterwards, the interviewer asks if the respondents noticed an unexpected event that was not present at the start of the video. We drop the data of respondents who do not answer this question. If respondents affirm this question, they have to state the color, the shape, and the direction of movement of this object. Following Most et al. (2001) and Stothart et al. (2015), we use these additionally reported characteristics to test whether the respondents really observed the unexpected event. If respondents answered that they did not notice the unexpected event or if they answered that they did notice the unexpected event but were unable to state at least one of its characteristics correctly, we define them as inattentional blind (IB=1).

As a test for the respondents' accuracy in the primary counting task, we use the relative counting error (|Counts - True Contacts|/True Contacts). We drop the data of the 5% of respondents who performed the worst in the counting task. This corresponds to respondents with a relative counting error of at least 80%. Such a high relative counting error suggests that the respondents did not understand their task in the experiment. The interviewers' assessment of the respondents' comprehensibility of the experiment provides evidence for this. Interviewers rated comprehensibility as very good, good, intermediate, bad, very bad, or unsatisfactory. This assessment is highly correlated with the relative counting errors. Although the interviewers rated the comprehensibility of only 8.78% of the 1241 respondents[3] as bad, very bad, or unsatisfactory, the interviewers rated the comprehensibility of 26.98% of the 5% with the worst relative counting error as bad, very bad, or unsatisfactory.

The data set provides information about the monthly household net income of subjects as well as household size. We drop all observations without income data which leaves us with 1084 observations (562 female, 522 male). Table 1 contains descriptive statistics on the main characteristics of respondents. The mean age in this sample is 50.29 with a range from 17 to 89. The mean household income in our sample is 2575.54 Euro with a median of 2300 Euro. The lowest reported monthly net household income is 300 Euro; the highest reported net household income is 10 000 Euro. The mean household size is 2.28 with a median of 2. Household size ranges from 1 to 9 persons. We follow Mani et al. (2013) in their definition of poverty: We compute effective household income by dividing the household net income by the square root of the number of persons in the household and use a median split to define the poverty dummy variable. To check the robustness of our results, we use logarithmic effective household income.

[3] After dropping the data of respondents who did not report their counts in the primary task and who did not answer the question whether they noticed an unexpected event, our sample includes 1241 respondents.

Variable	Mean	Median	Std.Dev.	Min	Max
Age	50.29	51	17.72	17	89
Monthly household net income in Euro	2575.54	2300	1455.12	300	10000
Household size	2.28	2	1.21	1	9
Monthly effective household income in Euro	1762.88	1555.64	948.49	257.5	10000
Relative counting error	0.36	0.33	0.20	0	0.79
Inattentional blindness	0.75	1	0.44	0	1

Table 1: Descriptive statistics.

4 Results

Our objective is to investigate whether a relationship between poverty and attention allocation exists. In Section 4.1, we focus on the effect of poverty on inattentional blindness. In Section 4.2, we analyze the performance in the primary task of the poor and the rich to provide evidence for differences in focused attention.

4.1 Poverty and inattentional blindness

We divide our analysis of the effects of poverty on inattentional blindness into two parts. In Section 4.1.1, we compare the prevalence of inattentional blindness for the poor and the rich. To provide evidence for a causal effect of poverty on inattentional blindness, in Section 4.1.2, we analyze payday effects.

4.1.1 Differences in inattentional blindness between the rich and the poor

Our main objective is to investigate whether the poor and the rich differ in their inattentional blindness. As monetary concerns should increase the working memory load and, therefore, decrease the exclusive focus on the targets of the primary task, the poor should be more likely to notice the unexpected event and, consequently, should be less inattentional blind. In line with this first hypothesis, we find that the poor (mean $M = .720$; standard deviation $SD = .450$) are less likely to be inattentional blind than the rich ($M = .773; SD = .419$) (one-tailed $t(1082) = 2.03$; $p = .022$).

We analyze the effect of poverty on inattentional blindness in more detail in Table 2. Table 2 provides the estimates of the effect of poverty on inattentional blindness. The results show that the poor are less likely to be inattentional blind than the rich: In particular, in the baseline specification, the poor are 5.4 percentage points less likely to be inattentional blind than the rich. The literature on inattentional blindness indicates that the difficulty of the primary task (see, e.g., Lavie 1995, Cartwright-Finch & Lavie 2007) and the similarity of the unexpected event to the targets in the primary task (see, e.g., Simons & Chabris 1999, Most et al. 2001) influence inattentional blindness. To rule out that these effects drive our results, we control in column (2) for the primary task of the respondents, i.e., whether the respondents count squares, circles, white, or black shapes. Our results are robust to these additional controls.

Including personal characteristics changes the estimate slightly. The specification with all controls in column (3) shows that poverty significantly decreases inattentional blindness by 7.7 percentage points. The estimates in column (4) are the result of a logit regression. The effect of poverty on inattentional blindess remains highly significant: Poor subjects have a, on average, 0.421 smaller estimated logarithmic chance of inattentional blindness. As a robustness check we use logarithmic effective household income instead of poverty as the dependent variable (see Appendix A.1). Our results are robust to this change in the specification.

	(1)	(2)	(3)	(4)
Poverty	-0.054^{**}	-0.055^{**}	-0.077^{***}	-0.421^{***}
	(0.026)	(0.026)	(0.028)	(0.151)
Experimental controls	-	Yes	Yes	Yes
Person controls	-	-	Yes	Yes
Constant	0.773^{***}	0.821^{***}	0.815^{***}	1.497^{***}
	(0.018)	(0.027)	(0.039)	(0.229)
N	1084	1084	1075	1075
R^2	0.004	0.016	0.041	0.037

Table 2: The effect of poverty on inattentional blindness. (1), (2), and (3) are OLS estimates, (4) reports the estimates of a logit regression (the reported R^2 here is the corresponding Pseudo R^2) of poverty (=1 if subject is poor) on inattentional blindness (dependent variable=1 if subject is inattentional blind). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person controls include the age (centered), the gender, and education (casmin) dummies of the subjects. Robust standard errors in parentheses. $^{*}p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.1.2 Payday effects on inattentional blindness

Section 4.1.1 shows that a relationship between poverty and inattentional blindness exists. To provide evidence for a causal effect of poverty on inattentional blindness, we follow Mani et al. (2013), Carvalho et al. (2016), and Lichand & Mani (2020) in using the timing of the experiment as exogenous variation. A respondent who is interviewed after his payday should be relatively richer than if he would be interviewed before his payday. Thus post-payday, having just received money, a respondent should be less worried about his financial situation and his cognitive load should be lower. Therefore, we hypothesize that respondents who are interviewed after their payday are more inattentional blind than respondents who are interviewed pre-payday. Unfortunately, our sample does not include precise data about the individuals' paydays. However, in Germany, payday is often the last bank working day of the month. Therefore, we assume that the payday of each respondents is the last of each month and correct the payday to the previous bank working day if the last of the month is a holiday or a weekend.[4] We compare respondents who were interviewed in the (a) 8 days, (b) 7 days, (c) 6 days, (d) 5 days, (e) 4 days, (f) 3 days, (g) 2 days, and (h) 1 day period after the payday with the remaining sample. We exclude the data of respondents who were interviewed on the payday. As the unemployed face a series of worries that vary not just with the payday, we exclude them from the analysis.

Table 3 shows the estimates for different post-payday periods (independent variable=1 if the subject was interviewed post-payday) for inattentional blindness (dependent variable=1 if the subject is inattentional blind). For example, in panel (a) we estimate the effect of having been interviewed within 8 days after the payday—so either on day 1, day 2, day 3, day 4, day 5, day 6, day 7, or day 8 after the payday—on inattentional blindess. Our sample includes interviews from September 2014 to April 2015. As financial demands might differ between weekdays or months—for instance, expenditures might increase close to Christmas—we control in column (2) for the the day of the week and the month of the interview. Column (3) adds experimental controls, i.e., whether respondents count circles, squares, black, or white shapes. In Column (4), we furthermore control for person characteristics (age, gender and, education). Column (5) provides the estimates of a logit regression with all controls.

Table 3 provides evidence for a payday effect. First of all, as predicted, all signs of the estimates are positive. That means, respondents interviewed after the payday show higher levels of inattentional blindness compared to the rest of the sample for all specifications. Second, this effect is significant for the period between the payday and 4 to 6 days after the payday for all specifications. For the specifications with more controls, this effect persists until the 8th day after the individuals' payday. For those respondents within the post-payday range in column (4) the estimated probabilities of showing inattentional blindness range from 7 to 13.5 percentage points and, therefore, are higher compared to the rest of the sample. This is robust for the estimated logarithmic chances in column (5) as well. Shorter time periods fail to show significant results due to the small number of subjects within those periods, especially in the presence of controls. For example, only 16 people were interviewed one day after the payday and 33 in the two days after the payday (see Figure 2).

The post-payday effects show that the differences in inattentional blindness are not determined by personal traits that lead to lower income, but that the current financial situation of the individuals influences inattentional blindness. As a robustness check we restrict our sample to symmetric ranges around the payday (see Table 10 in the

[4] We discuss this assumption in more detail in Section 5.

	(1)	(2)	(3)	(4)	(5)
(a) 8 days post-payday	0.05	0.076*	0.075*	0.07*	0.411*
	(0.035)	(0.039)	(0.039)	(0.04)	(0.242)
R^2	0.002	0.028	0.043	0.07	0.064
(b) 7 days post-payday	0.057	0.084**	0.08**	0.074*	0.456*
	(0.037)	(0.04)	(0.04)	(0.041)	(0.258)
R^2	0.002	0.028	0.043	0.07	(0.064)
(c) 6 days post-payday	0.087**	0.113***	0.11**	0.1**	0.656**
	(0.039)	(0.042)	(0.042)	(0.043)	(0.304)
R^2	0.004	0.03	0.045	0.071	0.066
(d) 5 days post-payday	0.116***	0.15***	0.147***	0.135***	0.991**
	(0.043)	(0.045)	(0.046)	(0.048)	(0.425)
R^2	0.005	0.032	(0.046)	0.073	0.068
(e) 4 days post-payday	0.09*	0.139***	0.135**	0.123**	0.863*
	(0.051)	(0.053)	(0.054)	(0.056)	(0.455)
R^2	0.003	0.029	0.044	0.07	0.066
(f) 3 days post-payday	0.057	0.093	0.09	0.08	0.542
	(0.059)	(0.06)	(0.06)	(0.062)	(0.451)
R^2	0.001	0.026	0.041	0.068	0.063
(g) 2 days post-payday	0.035	0.082	0.08	0.057	0.391
	(0.073)	(0.075)	(0.078)	(0.08)	(0.56)
R^2	0.000	0.025	0.04	0.067	0.062
(h) 1 day post-payday	0.186***	0.143*	0.14*	0.116	1.237
	(0.062)	(0.074)	(0.078)	(0.082)	(1.121)
R^2	0.003	0.026	(0.04)	0.068	0.063
N	916	916	916	912	912
Dummies for weekday	-	Yes	Yes	Yes	Yes
Dummies for month	-	Yes	Yes	Yes	Yes
Experimental controls	-	-	Yes	Yes	Yes
Person controls	-	-	-	Yes	Yes

Table 3: The effect of post-payday on inattentional blindness. (1),(2),(3), and (4) are OLS estimates, (5) are estimates from a logit regression (the reported R^2 here is the corresponding Pseudo R^2) with coefficent of (a) 8, (b) 7, (c) 6, (d) 5, (e) 4, (f) 3, (g) 2, and (h) 1 days post-payday (=1 if subject is interviewed post-payday) on inattentional blindness (dependent variable; =1 if subject is inattentional blind). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person characteristics include the age (centered), the gender, and education (casmin) of the subject. Robust standard errors in parenthesis. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Appendix). This reduces the sample size. For example, when we compare the respondents who were interviewed one day before payday with those interviewed one day after payday, our sample includes only 62 respondents. Nevertheless, all our estimates (except three) have the predicted sign. Due to the small sample, however, the estimates are no longer significant.

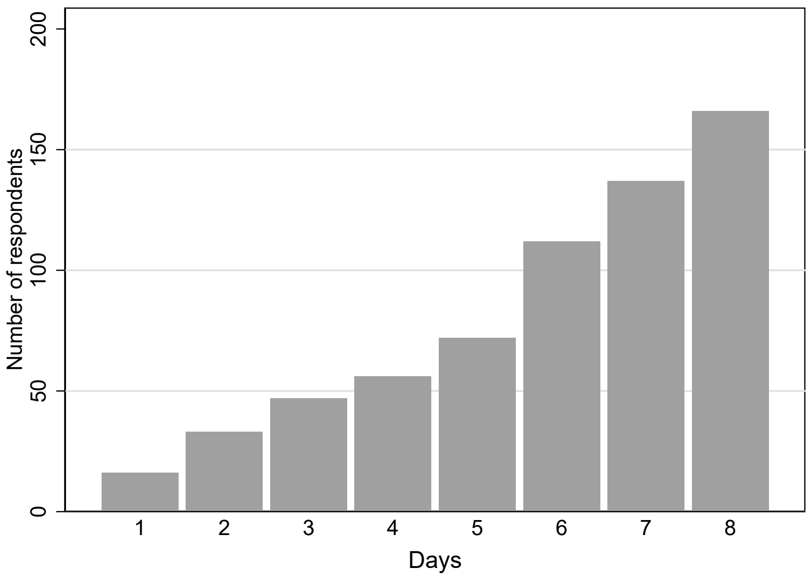

Figure 2: Number of respondents in the post-payday group.

4.2 Poverty and relative counting errors

To address the question whether the lower inattentional blindness of the poor comes at the expense of focused attention, we compare the performance of the poor and the rich in the primary task of the inattentional blindness experiment. To measure performance, we use the relative counting errors: |Counts - True Contacts|/True Contacts.

4.2.1 Differences in relative counting errors between the rich and the poor

We hypothesize that the poor count more often incorrectly, thus showing higher relative errors. The comparison of the means in terms of the relative counting errors shows that the poor ($M = .369; SD = .203$) count more often incorrectly than the rich ($M = .345$; $SD = .2$). This result is significant on a 5 percent level (one-tailed $t(1082) = -1.943$; $p = .026$).

The results in Table 4 show that the effect of poverty on the relative counting errors is not robust in the presence of controls. While poverty increases the relative errors by 2.4 percentage points without further controls, we see no significant effect in column (3), where we control for task, age (centered), gender, and education. The significant results in the absence of these controls seem to refer to the correlation of income and those person characteristics. However, our robustness check where we use a continuous income variable, i.e., logarithmic household income, instead of the median split as the dependent variable, increases the precision of our estimates (see Table 5). This suggests that we lose too much information with a median split compared to the continuous income variable. To test this further, we split our sample into six groups. The poor groups contain the subjects with the lowest effective household income in 10% steps. *Poor 10%* contains the respondents with the 10% lowest effective household income, *Poor 10% - 20%* (*Poor 20% - 30%*, *Poor 30% - 40%*, *Poor 40% - 50%*) contains the respondents with effective household incomes in the range of 10% to 20% (20% to 30%, 30% to 40%, 40% to 50%). The 50% highest effective household income group is the reference category (see Table 6). In line with Kaur et al. (2019), who find that their results are driven by the poorest subjects, we find a significant effect of the poorest 10% in our sample on the relative errors.

	(1)	(2)	(3)
Poverty	0.024*	0.026**	0.015
	(0.012)	(0.012)	(0.012)
Experimental controls	-	Yes	Yes
Person controls	-	-	Yes
Constant	0.345***	0.379***	0.379***
	(0.009)	(0.011)	(0.016)
N	1084	1084	1075
R^2	0.004	0.081	0.155

Table 4: The effect of poverty on relative counting errors. (1), (2), and (3) are OLS estimates with the coefficient of poverty (=1 if subject is poor) for relative counting errors (dependent variable). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person controls include the age (centered), the gender, education (casmin) of the subjects. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.2.2 Payday effects on relative counting errors

Our analysis of the effect of payday on relative counting errors shows that the relative counting error increases after the payday, but this effect is not significant (see Table 7). One reason why we do not find a payday effect could be that the primary task is too easy compared to the overall high living standard in Germany. If that explanation is true, the load on working memory, which leads the poor to be less inattentional blind, is not sufficient to hamper performance in the primary task.

	(1)	(2)	(3)
Log effective household income	-0.029^{**}	-0.032^{***}	-0.023^{*}
	(0.013)	(0.012)	(0.013)
Experimental controls	-	Yes	Yes
Person controls	-	-	Yes
Constant	0.571^{***}	0.624^{***}	0.551^{***}
	(0.092)	(0.088)	(0.094)
N	1084	1084	1075
R^2	0.005	0.083	0.157

Table 5: The effect of logarithmic effective household income on relative counting errors. (1), (2), and (3) are OLS estimates of the effect of logarithmic effective household income on relative counting error (dependent variable). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person controls include the age (centered), the gender, and education (casmin) of the subjects. Robust standard errors in parentheses. $^{*}p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
Poor 10%	0.043^{*}	0.044^{**}	0.041^{*}
	(0.022)	(0.022)	(0.021)
Poor 10% - 20%	0.035	0.035^{*}	0.015
	(0.022)	(0.021)	(0.021)
Poor 20% - 30%	0.007	0.014	0.012
	(0.02)	(0.019)	(0.02)
Poor 30% - 40%	0.03	0.034	0.015
	(0.022)	(0.021)	(0.021)
Poor 40% - 50%	0.005	0.003	-0.004
	(0.02)	(0.019)	(0.018)
Experimental controls	-	Yes	Yes
Person controls	-	-	Yes
Constant	0.345^{***}	0.378^{***}	0.378^{***}
	(0.009)	(0.011)	(0.016)
N	1084	1084	1075
R^2	0.006	0.084	0.157

Table 6: The effect of the lowest effective household income groups on relative counting errors. (1), (2), and (3) are OLS estimates of the coefficient of being in the respective lowest income group (=1 if subject is in the group) for relative counting errors (dependent variable). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person controls include the age (centered), the gender, education (casmin) of the subjects. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

	(1)	(2)	(3)	(4)	(5)
(a) 8 days post-payday	0.007	0.003	0.012	0.001	-0.012
	(0.017)	(0.02)	(0.019)	(0.019)	(0.039)
R^2	0.000	0.014	0.089	0.168	0.204
(b) 7 days post-payday	0.028	0.024	0.03	0.019	0.02
	(0.018)	(0.021)	(0.02)	(0.02)	(0.039)
R^2	0.002	0.015	0.091	0.169	0.205
(c) 6 days post-payday	0.021	0.02	0.025	0.013	-0.037
	(0.019)	(0.022)	(0.022)	(0.021)	(0.039)
R^2	0.001	0.015	0.09	0.168	0.206
(d) 5 days post-payday	0.01	0.004	0.012	0.000	-0.097^{**}
	(0.024)	(0.027)	(0.026)	(0.026)	(0.042)
R^2	0.000	0.014	(0.089)	0.168	0.215
(e) 4 days post-payday	0.029	0.03	0.037	0.026	-0.114^{***}
	(0.028)	(0.031)	(0.031)	(0.03)	(0.042)
R^2	0.001	0.015	0.09	0.169	0.216
(f) 3 days post-payday	0.038	0.038	0.043	0.031	-0.114^{***}
	(0.03)	(0.034)	(0.035)	(0.034)	(0.042)
R^2	0.002	0.015	0.09	0.169	0.216
(g) 2 days post-payday	0.029	0.029	0.033	0.013	-0.092^{*}
	(0.037)	(0.042)	(0.041)	(0.038)	(0.049)
R^2	0.001	0.014	0.089	0.168	0.21
(h) 1 day post-payday	-0.002	-0.004	0.003	-0.003	-0.09
	(0.06)	(0.064)	(0.06)	(0.054)	(0.055)
R^2	0.000	0.014	0.088	0.168	0.208
N	916	916	916	912	229
Dummies for weekday	-	Yes	Yes	Yes	Yes
Dummies for month	-	Yes	Yes	Yes	Yes
Experimental controls	-	-	Yes	Yes	-
Person controls	-	-	-	Yes	Yes

Table 7: The effect of post-payday on relative counting errors. (1),(2),(3), (4), and (5) are OLS estimates of the coefficent of (a) 8, (b) 7, (c) 6, (d) 5, (e) 4, (f) 3, (g) 2, and (h) 1 days post-payday (=1 if subject is interviewed post-payday) on the relative counting error (dependent variable). (5) is restricted to subjects who count squares. Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person characteristics include the age (centered), the gender, and education (casmin) of the subject; Robust standard errors in parenthesis. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Our sample contains four groups: subjects counting the squares, the circles, the white, or the black shapes. Subjectively, counting the squares is the most difficult task. Table 8 confirms this: The relative errors between the four groups differ markedly, which suggests that the tasks differ in their level of difficulty. According to Table 8, counting the squares was the most difficult task. Therefore, in column (5) of Table 7, we restrict our sample to subjects counting how often the squares touch the frame of the video. The results show a significant negative effect of the payday. Consequently, the poor experience less inattentional blindness at the expense of performance in the primary task only if the primary task is sufficiently difficult.

Variable	**Mean**	**Std.Dev.**	**Min**	**Max**	**Obs.**
Squares	0.43	0.187	0	0.765	258
White shapes	0.392	0.170	0	0.733	290
Black shapes	0.314	0.213	0	0.786	271
Circles	0.291	0.205	0	0.75	265

Table 8: Relative errors by primary task.

5 Discussion

The use of the SOEP-IS has the major advantage that it is a *representative* sample of German households. Yet, as German households are typically not subject to absolute poverty, our focus on Germany may give rise to critique. However Mani et al. (2013) explicitly argue that their theory of scarcity refers to subjective scarcity. In other words, people face scarcity when their wants do not align with their financial possibilities. Mani et al. (2013) explicitly state that this makes their theory not only applicable for developing countries but also for developed countries. By focusing on German households, our results on inattentional blindness provide further evidence for the generality of the theory.

Unfortunately, the data set does not include precise data on the individuals' paydays. Nevertheless, to assume the payday to be the last bank working day of each month is reasonable as the payday is the last bank working day (i) for different governmental payments like pensions,[5] (ii) the salaries in the public sector for employees,[6] (iii) the salaries in the public sector for the military and civil servants,[7] and (iv) the salaries of apprentices.[8] This might be true for a majority of employees in other occupations as

[5] § 118 Abs 1 SGB VI.

[6] § 24 Abs 1 (TVöD, TV-L, TV-H).

[7] § 3 Abs 4 BBesG for civil servants of Germany and of the federal states of Saarland, Berlin, and Mecklenburg Western Pomerania; Art 4 Abs 3 BayBesG; § 3 Abs 4 (ThürBesG, LBesG NRW, LBesG LSA, BbgBesG); § 3 Abs 5 HBesG; § 4 Abs 4 (NBesG, BremBesG, HmbBesG, SHBesG); § 5 Abs 1 LBesGBW; § 6 Abs 1 SächsBesG; § 8 Abs 1 LBesG for civil servants of the respective federal states.

[8] § 18 Abs 2 BBiG.

well. Yet, some will have different paydays. This would lead to an underestimation of the payday effect. However, there is no reason to belief that this would lead to a systematic bias.

The scarcity literature argues that poverty has a causal effect on cognitive functions and that other types of scarcity next to scarcity of financial resources (i.e., poverty) have similar effects. Therefore, a fruitful avenue for future research would be to analyze whether our results are generalizeable to other types of scarcity such as time scarcity.

6 Conclusion

Overall our results indicate that the poor and the rich differ systematically in their attention allocation. We find that the poor are more likely to notice unexpected events. This reduced inattentional blindness is at the expense of performance at the primary task if the primary task is sufficiently difficult. In particular, our results provide evidence for a causal link between poverty and inattentional blindness: Exploiting the exogenous variation in the timing of the experiment, we show that the circumstance of being poor influences attention. These results indicate that in line with earlier research (Shah et al. 2012, Mani et al. 2013), the poor are more easily distracted. Consequently, the poor are less likely to focus exclusively on their task and are more likely to notice unexpected events. If the task requires a large amount of focused attention, the reduced focus on the task is insufficient and the poor perform worse at that task.

Understanding the differences in attention allocation is relevant to design better policies. Our results highlight the significance of timing policy interventions well. Inattentional blindness increases after payday. Thus making people aware of new policy programs might be more difficult after paydays. In contrast, difficult tasks like applying for assistance programs might be better timed post-payday, when concentration is comparatively higher.

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A Appendix

A.1 Logarithmic effective household income and inattentional blindness

The estimates of Table 9 confirm the findings of Section 4.1.1. The probability of being inattentional blind increases with the logarithmic effective household income. Figure 3 shows the probabilities of inattentional blindness for different levels of the logarithmic effective household income. The probability of inattentional blindness for a respondent of mean age with a logarithmic effective household income of 6 ($\approx$ 400 Euro) is 63.99%, whereas the probability of inattentional blindness for an respondent of mean age with a logarithmic effective household income of 9 ($\approx$ 8100 Euro) is 84.78%.

	(1)	(2)	(3)	(4)
Log effective household income	0.045^{*}	0.049^{*}	0.073^{**}	0.391^{**}
	(0.027)	(0.027)	(0.03)	(0.16)
Experimental controls	-	Yes	Yes	Yes
Person controls	-	-	Yes	Yes
Constant	0.412^{**}	0.436^{**}	0.246	-1.568
	(0.201)	(0.201)	(0.223)	(1.179)
N	1084	1084	1075	1075
R^2	0.003	0.015	0.04	0.036

Table 9: The effect of logarithmic effective household income on inattentional blindness. (1), (2), and (3) are OLS estimates, (4) are estimates from a logit regression (the reported R^2 here is the corresponding Pseudo R^2) with changes in the logarithmic chance of inattentional blindness (dependent variable=1 if subject is inattentional blind). Experimental controls include whether the subject counts squares, circles, white, or black shapes. Person controls include the age (centered), the gender, and education (casmin) of the subjects. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

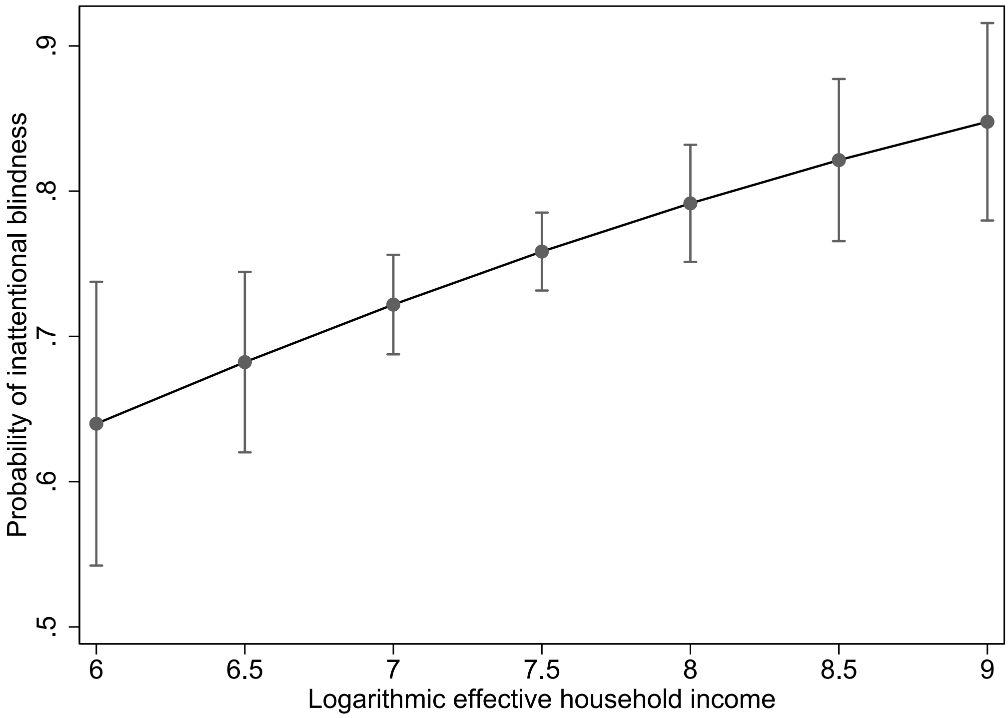

Figure 3: Predictive margins with 95% confidence intervals.

A.2 Symmetric payday-ranges and inattentional blindness

In Table 10, we analyze the effect of the payday in a symmetric setting. That is, whether the respondent is interviewed within a range of days before or after the payday. The coefficients do show an increased probability of inattentional blindness for those interviewed after the payday. But because of the reduced number of cases within those short periods around payday, those results mainly fail to be statistically significant.

	(1)	(2)	(3)	(4)	(5)
(a) 8 days post-payday	0.031	0.035	0.027	0.015	0.085
	(0.041)	(0.05)	(0.050)	(0.052)	(0.317)
N	416	416	416	415	411
R^2	0.001	0.031	0.048	0.087	0.081
(b) 7 days post-payday	0.05	0.078	0.07	0.053	0.316
	(0.046)	(0.053)	(0.053)	(0.054)	(0.33)
N	343	343	343	342	339
R^2	0.004	0.038	0.05	0.101	(0.093)
(c) 6 days post-payday	0.07	0.077	0.06	0.049	0.368
	(0.048)	(0.054)	(0.053)	(0.054)	(0.376)
N	283	283	283	282	267
R^2	0.007	0.04	0.072	0.15	0.15
(d) 5 days post-payday	0.107*	0.125*	0.113*	0.097	0.772
	(0.055)	(0.067)	(0.067)	(0.067)	(0.527)
N	210	210	210	210	198
R^2	0.016	0.071	(0.094)	0.204	0.205
(e) 4 days post-payday	0.087	0.083	0.072	0.078	0.571
	(0.062)	(0.075)	(0.075)	(0.077)	(0.578)
N	185	185	185	185	177
R^2	0.009	0.067	0.09	0.204	0.198
(f) 3 days post-payday	0.074	0.13	0.104	0.133	0.953
	(0.074)	(0.117)	(0.117)	(0.132)	(0.853)
N	141	141	141	141	135
R^2	0.007	0.054	0.065	0.176	0.167
(g) 2 days post-payday	0.045	0.001	−0.081	−0.088	−0.761
	(0.089)	(0.147)	(0.151)	(0.164)	(1.227)
N	103	103	103	103	95
R^2	0.002	0.081	0.138	0.301	0.337
(h) 1 day post-payday	0.155*	0.183	-	-	4.238**
	(0.087)	(0.178)	-	-	(1.786)
N	62	62	-	-	40
R^2	0.032	0.064	-	-	0.556
Dummies for weekday	-	Yes	Yes	Yes	Yes
Dummies for month	-	Yes	Yes	Yes	Yes
Experimental controls	-	-	Yes	Yes	Yes
Person controls	-	-	-	Yes	Yes

Table 10: The effect of pre- vs. post-payday on inattentional blindness. (1),(2),(3), and (4) are OLS estimates, (5) are estimates from a logit regression (the R^2 is the Pseudo R^2) with coefficent for post-payday (=1 if subject is interviewed post-payday; =0 if subject is interviewed pre-payday) on inattentional blindness (dependent variable; =1 if subject is inattentional blind). Experimental controls include whether subjects count squares, circles, white, or black shapes. Person characteristics include the age (centered), the gender, and education (casmin) of the subject. Robust standard errors in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

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